Unemployment Insurance Extensions, Labor Market Concentration, and Match Quality

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Unemployment insurance (UI) extensions can improve the bargaining power of job seekers relative to employers by improving workers' outside options. In this paper, I investigate whether the effects of UI extensions are different for workers exposed to higher levels of local labor market concentration, a potential source of employer market power. I exploit measurement error in state unemployment rates that led to quasi-random assignment of UI durations in the U.S. during the Great Recession. Using matched employer-employee data from the Longitudinal Employer-Household Dynamics program, I find that UI extensions lengthen nonemployment durations by one week and cause economically meaningful but not statistically significant increases in earnings. The UI-earnings effect is significantly lower at higher levels of concentration, while there is no difference in the UI-duration effect. The lower UI-earnings effect is driven by differences at the extremes of the distribution of concentration. Workers exposed to higher concentration also are slightly more likely to change workplaces, local labor markets, and industries following an extension, but they are not induced to match into less-concentrated markets. My results imply that the benefits of more generous UI, in terms of match quality, are attenuated at higher levels of concentration, and so UI policy that accounts for local concentration is warranted.

Keywords: Unemployment insurance, labor market concentration, local labor markets, earnings, nonemployment duration

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1 Introduction

Unemployment insurance (UI) policy in the U.S. is set at the state and federal level, but labor markets are local. When studying the effect of UI, and job search outcomes in general, it therefore is crucial to understand the role of local labor market conditions. For example, Kroft et al. (2013) show that the effect of a worker's nonemployment duration on callback rates varies substantially with local labor market tightness. Other studies also have found that the effect of UI depends on labor market conditions (Kroft and Notowidigdo, 2016; Schmieder et al., 2012; East and Kuka, 2015). Another dimension along which local conditions influence labor demand and supply is local labor market concentration. Concentration is associated with lower posted pay, wages, earnings, and total compensation (e.g., Azar et al., 2022; Marinescu et al., 2021; Rinz, 2022; Qiu and Sojourner, 2019). In addition, higher levels of concentration are associated with lower flows out of nonemployment (Berger et al., 2022a) and worse outcomes for workers following a nonemployment spell (Dodini et al., 2020). Despite extensive literatures documenting the separate importance of both UI and concentration for workers' outcomes, this paper is the first to examine their interaction. This interaction has substantial implications related to the cost and effectiveness of UI because benefit extensions create an additional outside option that subsidizes longer search. This additional search either could counteract or exacerbate the negative outcomes associated with labor market concentration.

I fill this gap in the literature by studying how the effect of UI benefit extensions on the duration of nonemployment spells and subsequent earnings varies across the distribution of local labor market concentration. I use quarterly matched employer-employee data from the Longitudinal Employer-Household Dynamics (LEHD) program at the U.S. Census Bureau and an empirical strategy that is new to the study of UI and worker-level outcomes. Specifically, I exploit measurement error in state unemployment rates that led to the quasi-random assignment of potential UI durations in the U.S. during the Great Recession. My sample covers the period 2004-2014, during which policymakers enacted then-unprecedented expansions to UI, and includes over 25 million nonemployment spells from more than 19 million workers spread across 28 states. I do not observe UI eligibility or receipt, and so my estimates represent the treat effect from additional weeks of potential UI benefits. In order to focus on those likely to be eligible for UI at the time of separation, I restrict my attention

to prime-age workers (aged 25-54) who were employed for at least two consecutive quarters prior to the start of a nonemployment spell.

Ex-ante, it is not obvious whether the effects of UI extensions would be different in more vs. less concentrated labor markets as no theoretical work has addressed this question. On the one hand, longer UI benefits provide an additional outside option that allows nonemployed workers to be more selective in their job search and potentially improves their bargaining power relative to employers (e.g. Acemoglu and Shimer, 1999). Workers exposed to higher levels of concentration, a potential source of monopsony power, stand to benefit differentially from even a small increase in their bargaining power.[1] Indeed, policy makers wanting to curb labor market power could design UI policy to provide longer benefits for those in more concentrated labor markets if UI helped counteract the negative outcomes associated with concentration.

On the other hand, workers in more concentrated labor markets might be limited in their ability to become more selective following a UI extension because, conditional on the size of their labor market, fewer firms are directly competing for them. In addition, at baseline, labor markets with higher levels of concentration appear to have lower job-finding rates as they are associated with fewer new hires and lower flows out of nonemployment (Marinescu et al., 2021; Berger et al., 2022a). If the effect of UI extensions on nonemployment durations is the same across the distribution of concentration, then the benefits of selectivity could be offset by longer nonemployment durations in more concentrated labor markets due to baseline differences in job-finding rates and the effects of negative duration dependence. Negative duration dependence corresponds to the fact that job prospects, skills, and outcomes all decrease the longer someone is out of work (Kroft et al., 2013; Schmieder et al., 2016; Nekoei and Weber, 2017).[2]

My empirical strategy allows me to overcome the biggest challenge to identifying the effect of UI extensions: benefit durations become more generous when labor market conditions worsen. Starting in 2008, in response to the Great Recession, Congress extended the duration of UI benefits through two programs, the Extended Benefits and Emergency Unemployment Compensation programs. These programs provided up to 72 weeks of additional benefits, beyond the 26 weeks of regular

[1] While local labor market concentration is a potential source of employer market power, concentration, in general, is not a perfect index of market power (Berry et al., 2019; Syverson, 2019; Eeckhout, 2021).

[2] Negative duration dependence can arise due to stigma, human capital depreciation, falling reservation wages, or other sources.

UI benefits, in states with with temporarily high levels of unemployment as captured by real-time measurements of state unemployment rates. To avoid the endogeneity problem of the availability of longer benefits in states with worse labor market conditions, I exploit measurement error in the state-level unemployment rates used to assign maximum potential benefit durations. This approach was first used by Chodorow-Reich et al. (2019) to study the contribution of UI extensions to the aggregate unemployment rate.[3] I am the first to use this method to examine UI and worker-level outcomes. More specifically, I construct as the treatment variable the difference in actual UI benefit duration as assigned using the real-time unemployment rate, which is partly estimated using survey data and subject to measurement error, and the counterfactual duration as assigned using subsequent revisions to the unemployment rate. The resulting "UI error" is the amount of UI benefit duration attributable to measurement error and not the underlying labor market conditions.

Figure 1 provides an example of the UI error from January 2009, comparing the cases of New York and New Jersey. The real-time unemployment rate in New York at this time was 5.8%, while in New Jersey it was 6.0%. According to the relevant policy parameters, eligible individuals in states with an unemployment rate of at least 6.0% could receive UI benefits for an additional 13 weeks beyond the 34 weeks of benefits available to unemployed workers in states with unemployment rates below that threshold. Qualifying individuals in New York, therefore, were eligible for a total of 34 weeks of UI while those in New Jersey were eligible for 47 weeks. However, the unemployment rates for both states were subsequently revised upwards to 6.1%. If the revised (correct) unemployment rates had been used to assign UI benefit durations, then UI recipients in New York would have received 13 additional weeks of benefits, and potential benefit duration for those in New Jersey would have been unchanged. In this example, the UI error equals -13 weeks, or one quarter, in New York and zero weeks in New Jersey. Over time and across all states, the UI error is positive and negative, meaning that UI duration could be longer (strictly positive UI error), shorter (strictly negative UI error), or unchanged (zero UI error) as a result of the measurement error.

This measurement error approach allows me to identify the effect of UI extensions on worker outcomes using weaker identification assumptions than has been employed in prior research in the U.S. context. Most studies of UI benefit extensions in the U.S., as opposed to changes in benefit

[3] Chodorow-Reich et al. (2019) estimate that UI extensions had a very small effect on the unemployment rate during the Great Recession, increasing it by at most 0.3 percentage points.

levels, rely on variation in potential benefit duration across states and time (e.g. Rothstein, 2011; Farber and Valletta, 2015; Farooq et al., 2020). This identifying variation requires the assumption that the controls fully account for any differences in underlying labor market conditions across treatment and control states, which is difficult to establish because differences in labor market conditions generate the variation in benefits (Schmieder and von Wachter, 2016). A small set of papers avoids this endogeneity issue by using either politically-motivated changes to UI generosity or kinks in benefit schedules, but they are limited to one or two states and so their findings do not necessarily generalize to a broader population (Card and Levine, 2000; Landais, 2015; Johnston and Mas, 2018). Instead, I overcome the endogeneity of benefit extensions by isolating changes in UI durations that are due to plausibly exogenous measurement error from a much larger number of states.

I first estimate the overall impact of UI extensions, without accounting for concentration, to provide new estimates of how extensions affect workers and as a comparison with prior work. My preferred approach includes fixed effects for the start of a nonemployment spell, the worker's state of employment, and local labor market of employment prior to the spell. I define local labor markets as the combination of a commuting zone and industry, so the local labor market fixed effects are, in practice, commuting zone-by-industry fixed effects.[4] This fixed effects structure isolates variation in UI duration occurring through the UI errors that is orthogonal to systematic differences in labor market conditions over time and across places. I also control for worker characteristics and lags of the state's revised unemployment rate in order to compare similar workers in states on the same labor market trajectory prior to a UI error-induced benefit extension.

I find that extending UI benefits by one quarter (13 weeks) causes nonemployment durations, as measured by the number of full quarters with zero earnings, to be longer by 0.063 quarters, or 0.8 weeks. This estimate is statistically significant at the 5% level, demonstrating that my approach and sample allow me to identify small effects. The point estimate represents a 2.8% increase relative to the mean nonemployment duration. OLS estimates that do not use the measurement error approach show an effect about half as large (1.4%).

This difference in the magnitude of the duration effect may seem counterintuitive because the

[4] I use this definition of local labor markets because I do not observe occupations. Other researchers using the same or similar data have also used this definition (e.g. Rinz, 2022). Local labor markets based on occupations also exhibit substantial variation over time and across places (Handwerker and Dey, 2022).

endogeneity of UI extensions and labor market conditions should cause the state-time variation to overstate the effect of UI on re-employment probabilities (Card and Levine, 2000). The UI errors, however, capture both reductions and extensions of UI duration, while the estimates using state-time variation only reflect the treatment effect of additional potential benefits (not reductions). For a better comparison, I separately estimate the effects of positive and negative UI errors. Positive UI errors, which assign a longer potential benefit duration, lead to a nonemployment duration effect about one-third the size of that from OLS, although the estimates are not statistically significantly different from zero. This difference in the size of the effects across methods is consistent with prior evidence showing that estimates based solely on state-time variation at least partially capture the endogenous relationship between benefit extensions and underlying labor market conditions (e.g. Card and Levine, 2000). Negative UI errors cause nonemployment durations to decrease by approximately 0.5 weeks or 1.6% relative to the mean, though this is only significant at the 10% level. The effect size for negative UI errors is substantially smaller than that implied by Johnston and Mas (2018), who study a politically motivated, permanent cut in UI benefits in a single state.

In terms of earnings, my preferred measure of match quality, I focus on the change in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter prior to a nonemployment spell. I use this definition to avoid the influence of partial quarters of employment at either end of a nonemployment spell. I estimate that extending UI benefits by one quarter causes earnings at re-employment to increase by 0.9%, but this effect is not statistically significantly different from zero. I obtain very similar estimates of the duration and earnings effects using a slightly different specification that uses state-time variation in potential UI benefit duration while controlling flexibly for the concurrent revised unemployment rate. This model could not be estimated in the absence of measurement error because the UI duration would be perfectly collinear with the unemployment rate. The similarity of the estimates from my preferred approach and this alternative model confirms that the underlying identifying variation in my estimates is due to measurement error and not labor market conditions.

I next estimate how the effect of UI extensions differs across the distribution of local labor market concentration. Specifically, I interact the UI error with the Herfindahl-Hirschman Index (HHI) in the quarter in which the worker separates from employment. I construct a quarterly payroll version of the HHI, which is the sum of squared payroll market shares across firms in the

same local labor market, using data from the Longitudinal Business Database (LBD) from the U.S. Census Bureau.[5] The LBD, unlike the LEHD, contains consistent firm identifiers across states, and so these data allow me to correctly measure concentration in markets that span multiple states. I do not instrument for the HHI because concentration is an equilibrium object, and so no valid instrument exists that would allow me to shift concentration and not worker outcomes like earnings (Berry et al., 2019). My approach, which is a standard triple-difference model, is similar to other types of heterogeneity analyses where we might be interested in, for example, how UI extensions differ by gender or earnings prior to separation. It identifies the effects of UI extensions across the distribution of HHI with the additional assumption that the effect of UI extensions in more concentrated markets is an accurate counterfactual for their effect in less concentrated markets.

I find that the effect of UI extensions on the length of nonemployment spells is not statistically significantly different across the distribution of local labor market concentration. Specifically, for a 0.10 point change in HHI, the difference between a low-concentration market and a high-concentration market according to the U.S. *Horizontal Merger Guidelines*, the effect of a one quarter UI extension on nonemployment duration increases by 0.01 weeks (U.S. Department of Justice and Federal Trade Commission, 2010). This difference represents just 0.03% of the mean nonemployment duration. The interaction effect is precisely estimated, as I can rule out increases in the UI-duration effect larger than 0.16 weeks for a change in HHI of 0.10.

The effect of UI extensions on earnings decreases at higher levels of concentration. The estimated interaction term is statistically significant at the 5% level and is economically meaningful. For a local labor market with an HHI of 0.15, the UI-earnings effect is 0.96%. At an HHI of 0.25, meanwhile, the UI-earnings effect is about half as large: 0.49%. This UI error-HHI interaction term also is precisely estimated: for a change in the HHI of 0.10, I can rule out interaction effects for earnings outside of the interval ranging from -0.89% to -0.06%. This lower UI-earnings effect in more concentrated markets is driven by effects at the extremes of the HHI distribution: when I use $\log(HHI)$, the UI-HHI interaction term is smaller in magnitude and no longer significant.

I additionally examine how UI impacts worker outcomes at different parts of the distribution of local labor market concentration. In particular, I estimate separate regressions within each tercile of the distribution of HHI. The marginal effect of UI extensions on nonemployment durations

[5] The payroll HHI was first used by Berger et al. (2022b).

for workers exposed to the lowest levels of concentration is half as large as that for workers in the middle- and top-third of the HHI distribution. A similar pattern exists for earnings. The marginal effect on earnings is positive across the distribution of HHI but nearly twice as large for workers in the bottom two-thirds of the distribution compared to those exposed to the highest levels of concentration. While none of the point estimates for earnings is statistically significant, they suggest that the overall effect of UI extensions on earnings is attenuated at higher levels of concentration, consistent with my estimate of a negative UI error-HHI interaction term.

Are there ways other than the immediate earnings effect that extended UI benefits might impact workers in more concentrated labor markets? To answer this question, I make use of the linked employer-employee data and examine how the mobility of nonemployed workers differs across the distribution of local concentration following a UI extension. I estimate that the likelihood of switching workplaces, local labor markets, and industries as a result of a UI extension increases with local labor market concentration. The magnitudes of these interaction effects for mobility, however, are not economically meaningful. In addition, longer UI benefits do not induce workers to match into less-concentrated labor markets, regardless of the degree of concentration.

This paper provides the first estimates of the interaction of UI and concentration, which inform not only UI policy but also policy related to labor market power, as concentration is a potential source of monopsony. My results have important implications for the cost and effectiveness of UI. I find that, overall, UI extensions during the Great Recession did not lead to large moral hazard costs by subsidizing unproductive search. Instead, nonemployed workers used the additional benefits to avoid decreases in earnings. These findings align with existing research, which I discuss in Section 2, that indicates the moral hazard cost of UI is low during downturns (e.g. Rothstein, 2011; Kroft and Notowidigdo, 2016).

In terms of concentration, and the potential for employer market power, I find that the effect of UI extensions on earnings is more negative at higher levels of concentration. This smaller UI-earnings effect is not driven by longer nonemployment durations in more concentrated markets. These results imply that the UI extensions I study did not lead to large enough increases in the bargaining power of workers to offset the negative outcomes associated with concentration. However, they also imply that policies aimed at reducing labor market concentration, such as antitrust enforcement, might have complementary benefits in the form of improving the effectiveness of UI.

2 Literature Review

This paper is related to a large literature on how changes to UI parameters affect outcomes for workers (Krueger and Meyer, 2002). I focus on how extensions of potential UI benefit duration, as opposed to changes in the level of benefits, affect the duration of nonemployment spells and earnings at re-employment.[6] Most recent papers in the U.S. context study the effect of UI extensions on labor supply. These papers typically rely on state-time variation in the duration of UI benefits to estimate changes in the hazard rate out of unemployment and find that UI extensions cause small reductions in the likelihood of exiting unemployment (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015; Kroft and Notowidigdo, 2016).

The state-time variation, however, is potentially subject to issues of endogeneity because extensions occur in states where labor market conditions are worse. And so, despite the care taken to address this potential problem, these studies require relatively strong identification assumptions in order to isolate the causal effects of UI extensions (Schmieder and von Wachter, 2016). In contrast, the measurement error approach, which is new to the literature on UI and worker-level outcomes, allows me to relax these identification assumptions because I can focus on conditionally exogenous changes to potential UI benefit durations. My estimates, while not directly comparable in magnitude to these other studies, also indicate that UI extensions have small impacts on the duration of nonemployment.

Others have avoided the endogeneity problem inherent to state-time variation by examining either politically-motivated changes to UI duration (Card and Levine, 2000; Johnston and Mas, 2018) or kinks in the schedule of UI benefits in certain states (Landais, 2015). These papers estimate smaller effects for UI on nonemployment durations compared to those relying solely on state-time variation in benefits. However, while these papers are well-identified, their designs limit researchers to studying the experience of workers in one or two states. My approach allows for well-identified estimates of the effect of the UI from a much larger sample of workers. Compared to these studies of one or two states, I estimate a smaller effect of UI extensions on nonemployment duration (0.8 weeks compared to 4-6 weeks). This difference could reflect effects from my wider

[6] Schmieder and von Wachter (2016) review the recent literature on the effect of changes in UI benefit levels. In general, the same identification issues in the U.S. exist for these studies, with regression kink designs possible in a handful of states (Landais, 2015; Card et al., 2015).

sample or the temporary nature of the extensions I study.

Outside of the U.S. context, researchers have exploited discontinuities in European UI policies based on the age, experience, or geography of affected workers in order to isolate changes in potential UI duration that are orthogonal to labor market conditions. These studies, which are summarized in Schmieder and von Wachter (2016), estimate marginal effects on the duration of nonemployment in the range of 0.08-0.65. My estimate is at the lower end of these findings. These results might not generalize to the U.S. context because of differences across countries in the non-UI social safety net available to those unable to find work, which could lead job seekers to respond differently to similar extensions in benefits. In addition, recession-induced UI extensions may have different effects.

Only two recent studies in the U.S. context examine the effect of additional UI benefits on subsequent match quality. Johnston and Mas (2018) use a time-based regression discontinuity to study a politically-motivated 16-week reduction in UI benefit duration in Missouri. They find an increase in earnings at re-employment that is not statistically significant. Farooq et al. (2020) study the effect of UI extensions during the Great Recession using state-time variation in benefits and the same data (but a different method) as this paper. Using data from 2000-2013, they estimate that an additional quarter of UI benefits leads to a 2.9% increase in quarterly earnings at re-employment. When examining a time period more comparable to this paper (2008-2013), they find that a one quarter UI extension causes quarterly earnings to increase by 0.5%, an effect that is not statistically significantly different from zero. This point estimate is similar to my preferred estimate of 0.9%, which also is not statistically significant.

My estimates of the earnings effect of UI have distinct advantages over these studies. First, unlike Johnston and Mas (2018), my results come from both UI extensions and reductions, as well as a much larger sample of workers, which improves external validity. I also use different variation in potential UI duration. Second, in addition to using state-time variation in UI benefits, Farooq et al. (2020) compare workers who exit nonemployment in the same quarter, which differs from my strategy of comparing workers entering nonemployment at the same time. The approach in Farooq et al. (2020), therefore, introduces bias into their estimates as it conflates the effect of exiting nonemployment at different points in the business cycle effects with the treatment effect from additional UI benefits.

European-based studies of UI and match quality primarily find that additional UI benefits cause

small decreases in wages at re-employment. These negative effects range from -0.1% to -2.0% (Card et al., 2007; Lalive, 2007; Schmieder et al., 2016). Only Nekoei and Weber (2017) have found a positive effect of additional UI benefits on earnings at re-employment in Europe. They estimate a statistically significant increase in earnings of 0.5% following a nine-week extension in Austria. As with the European studies on duration, each of these papers are based on specific discontinuities in UI policy. In addition to issues with interpreting these estimates for workers far away from the discontinuities, it is not clear how well these estimates would translate to the U.S. context where wage determination is much more decentralized.

A key question at the center of the literature on UI and match quality is why theoretical predictions of positive effects on re-employment earnings are typically not found in empirical studies. Depending on the context, researchers have estimated significant negative effects (Schmieder et al., 2016), significant positive effects (Nekoei and Weber, 2017; Farooq et al., 2020), or effects that are statistically indistinguishable from zero (Card et al., 2007; Lalive, 2007; van Ours and Vodopivec, 2008; Johnston and Mas, 2018). Nekoei and Weber (2017) suggest one reason for this variety of results is that heterogeneity in the matching function across samples can lead to larger drops in the UI-induced job-finding rate for some populations following an extension, which can lead to negative duration dependence canceling out any positive impact of UI.

I argue that concentration is another important source of heterogeneity that can lead to different effects of UI across labor markets. I go beyond the existing UI literature to estimate how UI extensions and local labor market concentration interact to affect worker-level outcomes. No prior research has examined this interaction either empirically or theoretically. Studies of local labor market concentration find that higher levels of concentration are associated with lower posted pay (Azar et al., 2022), wages (Marinescu et al., 2021), earnings (Benmelech et al., 2022; Rinz, 2022), and non-wage compensation (Qiu and Sojourner, 2019). These results are consistent with the an interpretation of local labor market concentration as a potential source of monopsony.

Further evidence in favor of this interpretation comes from Azar et al. (2019), who estimate that the employment elasticity of the minimum wage is statistically significantly more positive in local labor markets with higher levels of concentration, consistent with predictions from monopsony theory. This result also indicates that local labor market concentration can influence the effectiveness of labor market policies, which suggests I should expect to find different effects of UI extensions

across the distribution of concentration.

The majority of the existing literature on concentration has focused either explicitly on job-stayers (Bassanini et al., 2021) or on a mix of workers remaining employed and those separating from employment. In contrast, I focus on nonemployed workers who are likely searching for employment. A handful of other papers study how concentration relates to job search outcomes. Both Jarosch et al. (2019) and Berger et al. (2022a) embed concentration in a search model in which the size of an employer endows them with labor market power. In both cases, higher levels of concentration are associated with worse outcomes for workers due to their limited outside options. Dodini et al. (2020) study the impact of exogenous separations on workers exposed to different levels of concentration, finding that workers initially exposed to higher levels of concentration have lower earnings at re-employment. I go beyond these papers by studying how local labor market concentration interacts with UI, which is independently important for job search outcomes.

Finally, I contribute to the literature on the then-unprecedented expansions in UI during the Great Recession. The majority of the papers in this literature focus on how UI affected the likelihood of workers exiting nonemployment, finding that these extensions had modest effects on the likelihood of re-employment (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015). Other studies examine the macro effects of these extensions and conclude that they contributed little to aggregate unemployment rates (Marinescu, 2017; Chodorow-Reich et al., 2019; Dieterle et al., 2020; Boone et al., 2021). The methods used in these papers do not allow for a direct comparison with my estimates, but I find qualitatively similar effects on nonemployment durations using a much larger sample of workers.

3 Conceptual Framework

It is theoretically ambiguous how the effect of UI extensions on the quality of new matches would vary with local labor market concentration. In general, UI extensions cause workers to be out of work for longer as a result of reduced search effort (the duration effect) but also may induce workers to be more selective in their search and find a higher paying job (the selectivity effect). The sign and magnitude of the UI-earnings effect is governed by the relative sizes of the elasticity of duration and the elasticity of selectivity, with larger duration effects associated with more negative UI-earnings

effects (Schmieder and von Wachter, 2016; Nekoei and Weber, 2017). In this section, I first discuss how concentration could change the effect of UI extensions on nonemployment duration. I then provide intuition for how the UI-earnings effect could vary with concentration while abstracting from duration effects.

Concentration is associated with several features of local labor markets that could influence the duration effect of UI extensions. For example, local labor markets with higher degrees of concentration have fewer new hires (Marinescu et al., 2021), firms in these markets post fewer vacancies, and average flows out of nonemployment are lower (Berger et al., 2022a) compared to less concentrated markets. This evidence implies that, in theory, the baseline job-finding rate for nonemployed workers is lower in more concentrated markets compared to less concentrated markets.

It is unclear whether or how additional UI benefits would differentially change job-finding rates across the distribution of concentration. If the UI-induced drop in the job-finding rate is constant across the distribution of concentration, then, for the same change in potential UI duration, we should expect the UI-duration effect to be larger in more concentrated markets due to the lower baseline job-finding rate in those markets.[7] This prediction corresponds to a positive UI-HHI interaction effect with respect to nonemployment duration. The same prediction would hold if the UI-induced drop in the job-finding rate is larger in more concentrated labor markets.

There is no clear prediction if workers in more concentrated labor markets reduce their search effort by less than those exposed to lower levels of concentration following a UI extension. In such a scenario, the sign of the UI-concentration interaction effect depends on the size of the difference in baseline job-finding rates and the relative drop in the job-finding rate across the distribution of HHI. Because both the choice of search effort and the degree to which UI extensions change that effort are unobserved, it is not possible to determine how the UI-duration effect changes at different levels of concentration. Adding to this ambiguity, it also is possible that, during recessions, the baseline job-finding rate is constant across the distribution of concentration as the existing evidence does not account for differences across the business cycle.

In terms of earnings, a large and growing literature has shown that concentration is associated with lower posted pay (Azar et al., 2022), wages (Marinescu et al., 2021), earnings (Rinz,

[7] In this stylized example, I assume that concentration arises exogenously. In reality, however, concentration is determined in equilibrium. For the purposes of exposition this is a reasonable assumption because UI extensions are unlikely to directly affect concentration.

2022; Benmelech et al., 2022), and total compensation (Qiu and Sojourner, 2019), consistent with concentration being a potential source of monopsony power. Many search models, meanwhile, predict that UI extensions can increase worker bargaining power and lead to better match quality (higher earnings) by providing an additional outside option that allows workers to search for longer (e.g. Acemoglu and Shimer, 1999; Chetty, 2008). These models do not account for labor market concentration, but their predictions imply that workers exposed to higher levels of concentration will differentially benefit if the extensions provide a sufficient improvement in their outside option and strengthen their bargaining power relative to employers, reducing or offsetting the negative outcomes associated with concentration.[8]

There is limited direct evidence in the UI literature on how UI policy changes impact the bargaining power of affected workers. Dahl and Knepper (2022) study large UI reforms in several U.S. states that reduced both the level and duration of UI benefits. Using data on establishments located in differently-treated states but within the same firm, they find that starting salaries decrease by 7% and posted salaries for the same job in the same firm decrease by 6% in reform states. These results for posted pay indicate that reductions in UI generosity cause worker bargaining power to fall when the outside option of remaining nonemployed becomes less valuable.[9] The theoretical predictions discussed above, as well as this empirical evidence of UI generosity influencing worker bargaining power, imply that the UI-earnings effect will be more positive in local labor markets with higher levels of concentration.

There also are reasons to expect that the UI-earnings effect will be more negative in markets with higher levels of concentration. As discussed in Section 2, concentration is associated with a variety of negative labor market outcomes, including lower earnings. If UI extensions do not sufficiently increase the bargaining power of workers exposed to higher levels of concentration relative to employers, then, following a change in UI policy and holding everything else constant,

[8] I use the term "outside option," which is relevant for wage bargaining models, loosely. In wage posting models with random search such as Burdett and Mortensen (1998), additional UI benefits enter as an increase in the reservation wage. In directed search models such as Acemoglu and Shimer (1999), more generous UI may allow workers to search in a "higher" submarket.

[9] Jäger et al. (2020) also study UI reforms in Austria that increased the level but not the duration of benefits for some, but not all, nonemployed workers. They find that wages for eligible incumbent workers are insensitive to changes in UI benefit levels, implying that the value of nonemployment as an outside option has little bearing on wages. They further show that this insensitivity persists among groups thought to have limited bargaining power such as women, blue-collar workers, and younger workers. It is reasonable to expect a different effect in the U.S. context, however, where worker bargaining power is lower and wage determination is more decentralized.

we would expect the earnings at re-employment for the average nonemployed worker in a highly concentrated market to be lower than that of the average nonemployed worker in a less concentrated market. This prediction follows not only from the papers discussed above but also static models of labor market concentration (Boal and Ransom, 1997; Arnold, 2020) and dynamic models of labor market power in which concentration is embedded (Jarosch et al., 2019; Berger et al., 2022b,a).

A similar prediction holds if we consider the trade-off faced by nonemployed workers following a UI extension. These workers can become more selective in their job search, which pushes their expected re-employment earnings upward. But the additional benefits also lead to reduced search effort, which pushes their expected earnings downward as a result of negative duration dependence. Nonemployed workers exposed to higher levels of concentration, however, might be limited in their ability to become more selective as, conditional on the size of the local labor market, fewer employers directly compete for them relative to workers exposed to less concentration. These predictions suggest that the UI-earnings effect should be more negative in local labor markets with higher levels of concentration.

Taken together, the arguments in this section illustrate why it is theoretically ambiguous how the effect of UI extensions would vary with local labor market concentration. The question of the sign and magnitude of the interaction between UI extensions and local labor market concentration is therefore an empirical one. This ambiguity holds for both the UI-duration effect and the UI-earnings effect, both of which are important parameters for assessing the effectiveness of UI policy. If the effect of UI extensions on match quality is more positive in more concentrated labor markets, then this suggests that UI can bolster worker bargaining power and can serve as an additional policy tool beyond antitrust enforcement and the minimum wage for counteracting monopsony. If, instead, the UI-earnings effect is more negative in more concentrated labor markets, then policies intended to reduce high levels of concentration could have the additional benefit of improving the effectiveness of UI.

4 Policy Background and Data

In this section, I first discuss the policy context for my empirical strategy, followed by a description of the specific features of the LEHD data and the creation of my analysis sample.

4.1 UI Policy During the Great Recession

UI benefits in the U.S. typically last for 26 weeks, with some states offering more or less generous benefit durations. Additional UI benefits become available to qualifying unemployed workers during periods of high unemployment in their state through two programs. First, the Extended Benefits (EB) program provides 13 or 20 weeks of benefits depending on the state's unemployment rate. EB is a permanent but optional program that is funded by both the federal and state governments, and not all states participate. During the Great Recession, however, the American Recovery and Reinvestment Act of 2009 temporarily changed EB to be fully federally funded, which induced several states to opt into the program (Rothstein, 2011).

The second program is the Emergency Unemployment Compensation program (EUC). Unlike EB, EUC is not permanent: Congress must authorize and fund it each time the need arises. During the Great Recession, EUC was initially authorized in June 2008 and provided up to 13 weeks of additional UI benefits for workers who exhausted their regular benefits at any point before the end of March 2009. As the recession deepened, Congress continued to re-authorize the program and expand the benefits offered. In November 2008, EUC was expanded to include two tiers: tier I provided eligible unemployed workers in all states with 20 weeks of benefits (7 beyond the original 13 weeks provided by the program), and tier II provided an additional 13 weeks for those in states with an unemployment rate above 6%. Another expansion in November 2009 changed tier II benefits to 14 weeks and created tiers III (13 weeks if the state unemployment rate exceeded 6%) and IV (6 weeks if the state unemployment rate exceeded 8.5%). At its most generous, EUC provided benefits for a total of 53 weeks beyond regular and EB benefits, for a potential total duration of 99 weeks.

This initial gradual expansion of benefits would be uncharacteristic of the remainder of EUC's existence, which Rothstein (2011) describes as "proceed[ing] in fits and starts." The program was reauthorized eight times after the November 2009 expansion, but also was allowed to expire three times between reauthorizations (in April, June, and November 2010). After each lapse, Congress eventually made benefits retroactive to the previous expiration of the program, though this feature was not always predictable: it took almost two months for retroactive benefits to be approved following the June 2010 expiration. In September 2012, EUC entered its final phase with benefits

unchanged or less generous in tiers I through III and expanded in tier IV. The program ended in late December 2013.

This series of alterations, lapses, and reauthorizations of EUC motivates the assumption in my empirical strategy that nonemployed workers expect, at most, to receive the maximum potential benefit duration available in their state at the time they separate from employment. Under program rules, higher tiers of benefits could only be accessed after qualifying for and exhausting benefits on all lower tiers. In addition, unemployed workers who exhausted benefits on lower tiers were immediately eligible for benefits on a higher tier as soon as their state triggered onto that tier. As noted by Rothstein (2011), however, beginning in 2010, the EUC program was consistently nearing expiration and even lapsed three times. Unemployed workers, therefore, could not reasonably expect the continued availability of higher tiers of benefits.[10] Moreover, as the recovery progressed, states triggered off of higher EUC tiers as their labor markets gradually improved. Once a state no longer qualified for higher tiers of benefits, unemployed workers immediately lost access to those higher tier benefits. For these reasons, I argue that total potential UI benefits at the time of separation is the relevant policy parameter for the expectations formed by nonemployed workers about UI availability.

Eligible workers could claim benefits on a given EUC tier depending on their state's unemployment rate. EUC contained a series of triggers that determined which tier of benefits a state qualified for based on either of two measures of its unemployment rate. The first measure was the insured unemployment rate (IUR), which is the share of UI-covered workers in the state who are receiving regular UI benefits. The second measure is the total unemployment rate (TUR), which is the standard measure of the unemployment rate: the share of the labor force not working but who are ready to work and actively searching for a job. Each of these measures entered into the EUC trigger formulas as three-month moving averages to avoid high frequency movements between tiers.

Both the IUR and TUR were measured in real-time, and each week the Department of Labor issued notices of both the latest readings of these unemployment rates and which states, if any, were eligible for the various benefit tiers (Chodorow-Reich et al., 2019). The IUR was measured using administrative data from the state's UI system and therefore was subject to minimal measurement

[10] Once EUC expired, UI beneficiaries could receive the remaining benefits on any tier that they had already started but could not access additional benefits on higher tiers.

error. The TUR, on the other hand, was measured using a combination of administrative and survey data. The Bureau of Labor Statistics (BLS) constructs state-level unemployment rates by combining estimated counts of employed and unemployed workers that are produced by a state-space filter. This state-space model uses as inputs the count of unemployed workers from the Current Population Survey (CPS), counts of insured unemployed workers, counts of employed workers from the CPS, and state-level payroll employment from the Current Employment Statistics (CES) program.[11]

The estimated state unemployment rates are subject to revision. As described in Chodorow-Reich et al. (2019), BLS revises the estimates because of: revisions to administrative data inputs (insured unemployed counts and CES employment), incorporation of the full time series of input data, and occasional methodological improvements. As discussed in Chodorow-Reich et al. (2019), from 1996 to 2015, the majority of cases in which state unemployment rates measured in real-time differed from the revised estimates were because of methodological changes to the BLS model. These differences between the real-time and revised data led to more than 600 instances, almost exclusively during the Great Recession, in which the maximum potential UI benefit duration for a given state and month would have been different if the revised unemployment rate had been used instead of the real-time unemployment rate.

The most relevant major methodological improvements for my sample period occurred in 2015, when BLS introduced better handling of state-specific outliers as well as improved seasonal adjustment. These updates are described by Bureau of Labor Statistics (2015) as leading to "more accurate and reliable estimates." Chodorow-Reich et al. (2019) estimate the importance of the various components of the revision process for the UI errors. They find that the 2015 methodological update and state-specific outlier detection explain about half of the variation in UI errors, while the incorporation of the full time series explains very little of the variation.

I use revised unemployment rates as of 2015 to construct my treatment variable, which is discussed in more detail in Section 5.1.[12] Specifically, I follow Chodorow-Reich et al. (2019) and calculate UI errors, which are equal to the difference between potential UI durations using the real-time unemployment rate and counterfactual durations derived from the revised unemployment

[11] Additional details about the estimation of state-level unemployment rates by BLS can be found in Chodorow-Reich et al. (2019) Online Appendix A.

[12] I thank John Coglianese for generously sharing these data.

rate.[13] These UI errors isolate the cases when a state experienced a UI extension because of quasi-random measurement error and not a change in labor market fundamentals, under the assumption that the revised unemployment rate reflects true labor market conditions. As I discuss in more detail below, this measurement error approach allows me to invoke weaker identification assumptions than are typically required in the UI literature on benefit extensions in the U.S. context.

4.2 Data

Understanding how the effects of UI extensions vary across the distribution of labor market concentration is a data-intensive exercise, requiring data linking workers to their employers in order to measure concentration, heterogeneity in local concentration, and variation in UI benefit duration. In the U.S. context, the primary source of variation in UI benefits is across states and over time. Therefore, I use data from the LEHD program at the U.S. Census Bureau, which is the most comprehensive linked employer-employee dataset for the U.S. that contains information from all states. The LEHD includes administrative data on employment and earnings reported by state UI agencies. A subset of states participates in any given LEHD project, and my sample consists of data from 28 states.[14] The data also include information on the age, geography, education, race, and ethnicity of workers. Data on education, race, and ethnicity are imputed for much of the sample. I also observe information about employers, such as industry, employment, and the number and location of establishments. In order to assign workers to a local labor market, which I define as the combination of a commuting zone and industry, I need to assign each worker to one industry. Appendix B describes how I assign workers to a single industry when multiple industries are associated with the same firm. In all cases, I use four-digit NAICS industries.

I observe quarterly data on employment and earnings, but my identification strategy requires comparing workers in different states who begin nonemployment spells in the same quarter. To that end, I collapse the LEHD panel into nonemployment spells defined as at least one quarter

[13] A natural assumption about the revision process is that UI errors are systematically correlated with state populations because they arise from survey-based measures with larger sample sizes in more populous states. Appendix Figure A.1, however, shows little correlation between state populations and UI errors.

[14] A subset of states participates in any given LEHD project. The states in my sample are: Alabama, Arizona, California, Colorado, Connecticut, Delaware, Idaho, Indiana, Kansas, Maine, Maryland, Nevada, New Jersey, New Mexico, New York, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Utah, Virginia, Washington, Wisconsin, and Wyoming.

without earnings in any state.[15] For each spell, I also track quarterly earnings before and after the spell, the worker's employer and industry before and after the spell, and the state in which the worker was previously employed. In addition to this information on nonemployment spells, I add data on worker characteristics, commuting zone, and how long I observe them in the LEHD, which corresponds to actual labor market experience for many workers. I also make use of the linked employer-employee data to include information on the worker's tenure at their last job prior to a nonemployment spell.

I do not observe UI eligibility or receipt. However, all workers in my sample are potentially eligible for UI at the time of hiring because all jobs included in the LEHD are covered by UI. The workers in my sample can lose UI eligibility if they voluntarily leave their job or fail to meet certain "look back" criteria such as length of employment or total earnings, which vary by state. I make two sample restrictions in order to focus my analysis on workers likely to be eligible for UI at the time of separation. First, I only include prime-age workers (aged 25-54). This restriction reduces the possibility that a given nonemployment spell is actually the start of retirement for older workers or schooltime for younger workers. Second, I limit my attention to individuals who worked for at least two consecutive quarters at their prior job before the start of a nonemployment spell. I include this sample screen because most states require that unemployed workers have approximately six months of employment history in order to be eligible for UI benefits.

A key issue for any paper studying nonemployment durations is how to deal with spells that are ongoing at the end of the sample window. I do not model the hazard rates for these right-censored spells. Instead, I follow Nekoei and Weber (2017) and report estimates of the nonemployment duration effect for all spells as well as those that are completed within my sample window.[16] This approach shows the importance of ongoing spells for my estimates of the duration elasticity. To reduce the influence of outliers in terms of the length of nonemployment spells, I top-code all nonemployment spells at 12 quarters (three years), which is a similar cap as employed by other papers in this literature (e.g. Nekoei and Weber, 2017). For estimates of the earnings elasticity, I only include spells where I observe at least two quarters of re-employment earnings.[17]

[15] The LEHD includes an indicator for the presence of positive earnings in all states, both those included in my sample and those outside of my sample.

[16] I observe at least one quarter of re-employment for 96% of the spells included in my sample.

[17] I do not require that these two quarters of re-employment earnings be from full quarters of employment. I also do not require that they be at the same employer.

My analysis sample includes approximately 25,930,000 nonemployment spells experienced by 19,690,000 workers. Table 1 provides summary statistics for this sample. These workers are, on average, 39 years old and slightly more than half are women.[18] The workers are predominantly white (76%), with Black workers (14%) and Asian workers (6%) comprising the next largest groups. More than 80% of the sample holds a high school degree and 23% have at least a Bachelor's degree.[19] Appendix Table A.1 provides demographic statistics for prime age unemployed workers from the Current Population Survey (CPS), including those likely to be eligible for UI. The workers in my sample have qualitatively similar characteristics as those in the CPS, with the exception of women, who are over-represented, and high school graduates, who are under-represented. On average, these workers are still relatively early in their careers, having about 12 years of total labor market experience. They are somewhat new to the jobs held prior to their nonemployment spells with an average tenure of just under 2.5 years.

The workers in my sample experienced an average of 2.6 nonemployment spells between 2004 and 2014. Because workers can separate from employment or start a new job at any point in a quarter, I measure nonemployment durations as the number of full quarters without any earnings. The average nonemployment spell in my sample is 2.2 quarters or 6.6 months, and the standard deviation is 1.9 quarters. The existence of partial quarters of employment at either end of a nonemployment spell also introduces noise into the comparison of earnings before and after nonemployment. For example, average earnings in the last quarter of employment before the start of a nonemployment spell is $\$6,378$, which is 22% lower than average earnings in the second-to-last quarter before a nonemployment spell ($\$8,205$). This difference could reflect partial quarters of employment, an Ashenfelter dip, or a combination of the two. A similar difference (25%) exists between average earnings in the first quarter of re-employment ($\$6,038$) and the second quarter of re-employment

[18] The fact that slightly more than half of the nonemployment spells in my sample are experienced by women is at odds with the fact that men were hit harder by the Great Recession (Kochhar, 2011). This likely is a result of my inability to distinguish between nonemployment spells due to unemployment versus other causes, such as parental leave, which are much more likely to affect women. Appendix Table A.1 provides descriptive statistics for unemployed prime age workers from the Current Population Survey. Only 46% of these workers are women, and this proportion falls to 39% when looking at unemployed workers who are likely to be eligible for UI.

[19] The education information in the LEHD is a point-in-time snapshot and represents the highest level of education that the worker has completed at that time. Some nonemployed workers might enroll in further education in order to increase their human capital while searching for jobs, and this skill investment can translate into higher earnings once they are re-employed (Leung and Pei, 2020). Unfortunately, I do not observe the effects of further education, which potentially biases my results if there is differential re-enrollment along the distribution of UI errors and local labor market concentration.

($8,092). In order to avoid issues related to partial quarters of employment at either end of a spell, my preferred outcome variable for earnings is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a spell.

My empirical strategy uses variation in the UI error at the time of separation from employment. As described above, the UI error is measured in quarters and is the additional UI benefit duration available to nonemployed workers as a result of quasi-random measurement error. The average UI error experienced in my sample period is 0.0086 per quarter. This small average value is due to the quasi-random nature of the UI error and the fact that errors could be either positive or negative. Even with each of these factors, there is still considerable variation in the UI error: its standard deviation in my sample is 0.1737 per quarter. Figure 2 shows the distribution of UI errors during my sample period, both including (Panel A) and excluding zeros (Panel B). Focusing on the distribution of nonzero UI errors in Panel B, there is considerable mass beyond 0.5 quarters (both positive and negative).

I measure labor market concentration using the Herfindahl-Hirschman Index (HHI) of quarterly payroll:

$$HHI_{z,j,t-1} = \sum_{k=1}^{K} s^2_{z,j,k,t-1}, \tag{1}$$

where $s_{z,j,k,t-1}$ is firm k's share of total payroll in the local labor market defined as the combination of commuting zone z and industry j in the quarter prior to the start of the nonemployment spell $t-1$. Payroll for firm k is equal to the number of workers at k times their earnings. I construct the payroll HHI using data from the Longitudinal Business Database (LBD). Unlike the LEHD, the LBD contains consistent firm identifiers across states, which allows me to accurately measure concentration within commuting zones that span multiple states. This version of the HHI was first used by Berger et al. (2022b). The average worker in my sample is exposed to an HHI of 0.14 in the quarter in which they separate from employment. This degree of concentration is just below the level of HHI (0.15) at which U.S. Department of Justice and Federal Trade Commission (2010) classifies markets as moderately concentrated. There is, however, considerable variation in concentration across local labor markets as the standard deviation is 0.21.

In addition to studying how UI extensions and labor market concentration influence earnings

after nonemployment, I examine mechanisms related to the mobility of workers that potentially influence both the immediate and long-term effects of UI on earnings. In particular, I look at the extent to which longer UI benefits allow workers to switch workplaces and local labor markets. On average, 56% of nonemployment spells end with the worker switching SEINs, and 52% of spells end with the worker employed in a different local labor market. These switches across markets are more likely to be the result of changing industry (48%) than moving to a different commuting zone (4%), though I cannot observe moves to states outside of my sample.[20] Because concentration is associated with a variety of negative outcomes for workers, including lower earnings, this mobility is important because it might allow workers to match with an employer in a less concentrated market. Among the individuals that return to work within my sample window, the HHI in their local labor market of re-employment is 0.14, on average, which previews my finding that UI extensions do not enable workers to move to less concentrated labor markets.

5 Empirical Strategy

The goal of my empirical strategy is to identify how local labor market concentration mediates the effect of UI extensions on labor market outcomes for nonemployed individuals. Before doing so, I follow the existing literature and first estimate the effect of UI extensions on match quality without taking into account local concentration.

5.1 UI Extensions and Match Quality

I begin by estimating models that relate changes in UI benefit duration to outcomes for nonemployed workers using models of the form:

$$Y_{itszj} = \delta T^*_{st} + \sum_{k=1}^{4} \pi_k UR_{s,t-k} + \theta X_{it} + \alpha_t + \rho_s + \gamma_{zj} + u_{itszj}, \tag{2}$$

where Y_{itszj} are labor market outcomes for worker i who began a nonemployment spell in quarter t after working in state s and in the local labor market defined by the combination of commuting zone z and industry j. The main outcomes I study are nonemployment duration and the change

[20] Estimates of changing industries after a nonemployment spell are among workers who remain in the same commuting zone.

in earnings at re-employment, which is my preferred measure of match quality. The variable of interest is potential UI duration T^*_{st}: the maximum number of quarters of UI benefits for which a qualifying worker is potentially eligible after losing their job in quarter $t-1$ in state s. As discussed in Section 4.1, this UI duration is the most relevant for capturing workers' expectations about the availability of benefits.

The fact that labor market outcomes and the generosity of UI benefits are both correlated with labor market conditions motivates the set of fixed effects and controls used in the model. I include fixed effects for the first quarter of the nonemployment spell (α_t) in order to compare outcomes for workers beginning a nonemployment spell at the same time. I also include state of employment fixed effects (ρ_s) to eliminate pre-existing differences across states in potential outcomes. These "spell start" and state fixed effects account for any correlation between outcomes, potential UI benefit duration, and labor market conditions that varies systematically over the course of the business cycle or across individual states.

Even after accounting for these factors, there still can be residual correlation between labor market outcomes and the length of UI benefits due to labor market conditions. For example, while UI policy is set at the state-level, local labor market conditions matter for how quickly workers return to work and their earnings once reemployed (e.g. Kroft et al., 2013). To control for systematic differences across local labor markets, I include commuting zone-by-industry fixed effects (γ_{zj}), which serve as local labor market fixed effects given how I define these markets. I also include lags of the revised unemployment rate ($UR_{s,t-k}$), which allow me to compare outcomes for workers in states on similar labor market trajectories prior to a UI error. Worker-level controls (X_{it}) measured at the onset of a nonemployment spell are also included in order to absorb any remaining variation in the outcome variables. Specifically, I use fixed effects for age, education, gender, race and ethnicity, prior job tenure, and total observed labor market experience, all of which are key potential determinants of nonemployment durations and earnings. Finally, u_{itszj} is the error term, and I cluster the standard errors at the state level because that is the unit at which UI policy varies.

This model will identify the effect of UI extensions on nonemployment duration and earnings under the assumption that, conditional on the controls, UI duration is not systematically correlated with workers' potential outcomes as they enter nonemployment. This assumption is unlikely to hold,

even with the rich set of controls included in (2). As many researchers have noted, UI extensions occur when labor market conditions worsen. In the case of EUC and its triggers, the relationship between UI generosity and the unemployment rate is written into the law. The implication of this relationship is that the primary reason why UI durations vary across states is due to underlying differences in labor market conditions (Schmieder and von Wachter, 2016). The resulting mechanical correlation means that potential outcomes for nonemployed workers will be worse in states with extended benefits, and so the effects of UI identified off of cross-state variation can be biased by differences in underlying labor market conditions.

The endogenous relationship between potential benefit durations and labor market outcomes has motivated researchers to use empirical designs in which UI duration is orthogonal to labor market conditions. Almost all recent studies exploit discontinuities or kinks in the UI benefit schedule based on the age or experience of workers in several European countries (e.g. Card et al., 2007; Lalive, 2007; Schmieder et al., 2016; Nekoei and Weber, 2017). These research designs are generally not feasible in the U.S., where UI benefit duration is typically constant across all qualifying workers within a given state (Card et al., 2015; Johnston and Mas, 2018), and so researchers studying the U.S. context have relied on state-time variation in benefits instead (Rothstein, 2011; Farber and Valletta, 2015; Farber et al., 2015; Farooq et al., 2020).

In contrast, I use the measurement error approach, which was first developed by Chodorow-Reich et al. (2019) and is new to the literature on worker-level outcomes, in order to overcome the endogeneity of UI extensions and labor market conditions. This approach allows me to control for actual labor demand and exploit plausibly exogenous variation in potential UI benefit durations, conditional on that demand. By isolating differences in potential benefit durations that are due to measurement error and not underlying differences in labor market conditions, I can relax the assumption that UI duration and potential outcomes are conditionally uncorrelated.

To implement this strategy, I modify (2) to have the form:

$$Y_{itszj} = \beta_1 E_{st} + \sum_{k=1}^{4} \pi_k UR_{s,t-k} + \theta X_{it} + \alpha_t + \rho_s + \gamma_{zj} + \varepsilon_{itszj}. \tag{3}$$

The only difference between (2) and (3) is that the treatment variable is now E_{st}, which is the UI error in the quarter of separation from employment, and the coefficient of interest is β_1. The

UI error E_{st} is equal to the difference in potential UI benefit duration when using the revised state-level unemployment rates as opposed to the real-time state-level unemployment rate. It is measured in quarters, and so the estimated coefficients from this model can be directly compared to those from (2). As described in Chodorow-Reich et al. (2019) but applied at the worker-level, this model approximates the experiment comparing outcomes for similar workers who separated from employment at the same time, worked in states with similar labor market and UI policy paths, but were randomly assigned to different UI benefit durations because of measurement error. The value of this design, as opposed to (2), is that the variation in potential benefit duration is purged of the component due to underlying labor market conditions, which allows for cleaner identification of the effect of UI extensions.

This model identifies the effect of UI extensions under the assumption that, conditional on the controls, the UI errors are exogenous with respect to the true underlying labor market conditions, which are accurately measured by the revised unemployment rate. Put another way, the UI errors are conditionally uncorrelated with potential outcomes for workers. In order to assess the validity of this identification assumption, I regress the UI error on both the contemporaneous and lagged state unemployment rates. The results of this test are presented in columns (1) and (2) of Table 2 and confirm that this assumption is likely to hold. In both cases, labor market conditions can explain at most 2.5% of the variation in the UI errors. As another test of the identification assumption, I regress the UI error on a set of worker characteristics. The results of this test are presented in columns (3) and (4) of Table 2 and indicate that the assumption is also likely to hold: the worker characteristics can explain at most 3.8% of the variation in the UI errors.

I also provide additional evidence that the UI errors are uncorrelated with potential outcomes for workers. In Table 3, I regress the UI error on predicted earnings while controlling for fixed effects for spell start, state of employment, and local labor market of employment, as well as lags of the revised unemployment rate. Earnings are predicted in each of the two quarters prior to and following a nonemployment spell, using the same set of worker characteristics as in (3). This exercise provides a summary measure of residual selection conditional on the fixed effects and lags of the revised unemployment rate. The results indicate no residual selection, as the coefficients, which are measured in dollars, are neither economically nor statistically significant: the coefficient with the largest magnitude is \$29 for predicted earnings in the second-to-last quarter before a

nonemployment spell.

As a final check on the validity of the identification assumption that UI errors are uncorrelated with potential outcomes for workers, I regress the UI errors on each of the worker characteristics included in (3) while controlling for the fixed effects and lags of the unemployment rate. The results are presented in Appendix Table A.2. The estimated coefficient for each characteristic is small, and it is only significant for gender. I also regress the UI error on the total number of nonemployment spells that the worker experiences across the full sample, both before and after the focal UI error. This exercise can also be thought of as testing for selection in terms of otherwise unobservable worker quality. The coefficient from this regression is small and neither economically nor statistically significant.[21] The evidence presented here strongly implies that the identification assumptions I invoke are likely to hold.

In a supplementary analysis, I estimate a similar regression to (3) in which the treatment variable is given by the actual UI benefit duration T^*_{st}, and I additionally include fixed effects for different levels of the revised unemployment rate. In the absence of measurement error, including potential benefit duration and the contemporaneous unemployment rate would lead to perfect collinearity and I could not estimate the model. The coefficient on potential benefit duration from this alternative approach is therefore identified solely off of measurement error. As discussed in Section 6, the estimates from this strategy are very similar to those from my preferred specification.

5.2 UI Extensions, Concentration, and Match Quality

The primary goal of this paper is to move beyond the existing literature and study how local labor market concentration modifies the effect of UI extensions on match quality. I study this question by estimating a model in which I interact a measure of concentration with the UI error:

$$Y_{itszj} = \beta_1 E_{st} + \beta_2 HHI_{zjt} + \beta_3 E_{st} \times HHI_{zjt} + \sum_{k=1}^{4} \pi_k UR_{s,t-k} + \theta X_{it} + \alpha_t + \rho_s + \gamma_{zj} + \varepsilon_{itszj}. \quad (4)$$

Here, HHI_{zjt} is the payroll HHI measured in local labor market $z \times j$ in the quarter in which the worker separated from employment $(t-1)$. This model is identical to (3) except for the

[21] This coefficient is -0.0213 with a standard error of 0.0201.

HHI_{zjt} and $E_{st} \times HHI_{zjt}$ terms. In addition to the identification assumption invoked in the previous subsection, the effect of UI extensions at different levels of local concentration is identified under the assumption that the effects of UI extensions in less concentrated local labor markets are an accurate counterfactual for the effects of UI extensions in more concentrated local labor markets. There are two potential threats to this identification assumption. First, there could be secular trends in outcomes around UI extensions that differ by concentration.[22] Second, there could be unobserved shocks that correlate with the timing of UI extensions and that differ across the distribution of concentration within the same state.

While the second identification assumption cannot be tested directly, I argue that such secular shocks to labor market conditions are unlikely to exist. These shocks must be timed to align perfectly with the timing of UI errors, affect only a subset of local labor markets, and somehow be correlated with HHI. These requirements rule out any events or changes that affect an entire commuting zone or industry because the local labor markets within which I measure concentration are a combination of the two. Put differently, a commuting zone-wide shock would impact many local labor markets and would not necessarily be isolated to only high- or low-concentration markets. Similarly, an industry-wide shock would threaten identification only if it has differently sized effects in different commuting zones and those effects are correlated with concentration. In addition to these scenarios, which are unlikely to exist systematically, these shocks must also occur at the same time as the UI errors and be in the same direction. Given that overall labor market conditions explain very little of the variation in UI errors, it is also unlikely that any unobserved shocks correlate with the timing of these errors.

6 Results

In this section, I begin by discussing my estimates of the effect of UI extensions on nonemployment duration and earnings and compare them to the existing literature. I then show how local concentration mediates these effects and discuss some mechanisms that help to explain this interaction. Finally, I explore the heterogeneity and robustness of my results.

[22] I am awaiting approval from Census to disclose the results of a test for possible violations of this identification assumption.

6.1 The Overall Effect of UI Extensions

Before showing results from the measurement error approach, I present OLS estimates of the effect of UI extensions on nonemployment duration in Table 4. There are six columns that sequentially add controls in order to asses their importance for the estimates. Column (1) shows the correlation between nonemployment duration and potential UI duration, measured in quarters. Column (2) adds fixed effects for the quarter in which the nonemployment spell starts as well the state in which the worker was employed. This is my most basic model because these are the minimal controls necessary for my empirical approach. The point estimate is positive but not statistically significant at conventional levels. I next add four lags of the revised state unemployment rate in column (3). The point estimate doubles and is now significant at the 1% level, which highlights the importance of comparing workers in states on similar labor market trajectories. Column (4) introduces commuting zone-by-industry (i.e., local labor market) fixed effects, and the point estimate becomes slightly smaller but is qualitatively similar.

My preferred specification includes controls for worker characteristics and is shown in column (5). The point estimate is 0.0306, meaning that a one quarter (13 week) extension of UI benefits causes nonemployment spells to increase by 0.031 quarters or 0.4 weeks. This effect is statistically significant at the 1% level and represents an increase in nonemployment duration of 1.4% relative to the mean. The point estimate is very similar to that in column (4), which implies that controlling for worker characteristics contributes little additional explanatory power when estimating the effect of UI extensions on nonemployment durations.[23] In order to assess the influence of spells that have not yet concluded (i.e., are right-censored) at the end of the sample window, I estimate duration effects among completed spells for which I observe at least one quarter of re-employment. This result is presented in column (6). The point estimate is barely changed, indicating that right-censored spells are not influencing my estimates.

Table 5 presents estimates using the measurement error approach, which overcomes the endogeneity of labor market conditions discussed in Section 5.1. This table has the same structure as Table 4. The point estimates in columns (2) and (3), which include spell start and state fixed effects and controls for lags of the revised unemployment rate, respectively, are nearly identical

[23] The same conclusion can be drawn from the R^2 values of these two models: adding worker characteristics increases the R^2 by less than 1%, conditional on the other controls in the model.

and show that additional UI benefits lengthen nonemployment duration. After including the local labor market fixed effects in column (4), the point estimate is approximately 20% smaller than in column (3) but tells a qualitatively similar story.

My preferred specification controls for worker characteristics and is shown in column (5). The point estimate, which is essentially unchanged from the specification without worker characteristics, indicates that extending UI benefits by one quarter causes nonemployment spells to be longer by 0.0629 quarters, or 0.8 weeks, which represents a 2.8% increase relative to the mean. This effect, while quite small, is statistically significantly different from zero at the 5% level. My ability to precisely estimate such small effects highlights the statistical power generated by my approach and data. In addition, the 95% confidence interval allow me to rule out duration effects longer than 1.4 weeks. This estimate also is not driven by right-censored spells. The point estimate in column (6) removes such spells and is nearly identical.

The point estimates in Table 5 are larger than the OLS estimates in Table 4. At first this may appear to be an unexpected result, because OLS estimates for nonemployment duration based on state-time variation tend to overstate the effect of UI duration (Card and Levine, 2000). The UI errors, however, capture both reductions and extensions of UI duration, while the OLS estimates only capture the effect of extensions. A better comparison between the two methods, therefore, is between positive UI errors and the OLS estimates. In Appendix Table A.3, I separately show the effects of positive and negative UI errors, which correspond to UI duration extensions and reductions, respectively. The point estimates in column (1), which give the effect on nonemployment duration, show that positive UI errors cause nonemployment durations to be 0.01 quarters longer. This point estimate is much smaller than my OLS estimates, confirming that my results using the UI errors avoid the endogeneity of weak labor demand present in studies relying solely on state-time variation in benefits.

Looking at the negative UI errors, I find that measurement error-induced benefit reductions cause nonemployment spells to be shorter by 0.04 quarters (0.5 weeks), a decrease of 1.6% relative to the mean, which is significant at the 10% level. It is also substantially smaller than the findings in Johnston and Mas (2018), who estimate that a one-quarter decrease in UI benefit duration in Missouri caused nonemployment durations to decrease by 0.25 quarters (3.3 weeks).[24]

[24]The change in potential UI benefit duration studied by Johnston and Mas (2018) is actually a 16-week reduction in

My estimate of the overall effect of UI extensions on nonemployment durations is smaller than most existing studies. Much of this literature is from the European context, where the majority of papers exploit UI policy that allows for age-based regression discontinuity (RD) studies in a handful of countries. As summarized in Schmieder and von Wachter (2016), estimates of the UI duration effect from Europe range from 0.05 to 0.65 (0.08-0.65 when only considering RD studies), implying that a one-quarter UI extension increases nonemployment spells by 0.7-8.5 weeks. My estimate effect of 0.8 weeks, therefore, is at the bottom end of this range.

In the U.S. context, most studies of UI extensions have focused on hazard probabilities and not marginal effects on nonemployment durations because of data limitations. Among the studies that have examined duration effects, their estimates range from 0.15-0.45 (2.0-5.9 weeks), with all recent studies (since 2000) estimating effects of at least 3.3 weeks (Schmieder and von Wachter, 2016). These recent studies are well identified as they use either RD/RK designs (Johnston and Mas, 2018; Landais, 2015) or a politically motivated change in benefits (Card and Levine, 2000). However, they are also limited to studying one or two states, meaning that it is unclear how well their effects generalize beyond the studied populations. My preferred point estimate using the measurement error approach is much smaller than their estimates, potentially suggesting that effects among a broader population are different from these case studies.

I next investigate how these UI extensions impact the quality of new matches. My preferred measure of match quality is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. This measure implicitly introduces a worker fixed effect into the regression, which controls for possible selection on unobservables, and the coefficients can be interpreted as approximately equal to the percent change in earnings. I present estimates using other earnings outcomes in Section 6.4.

Table 6 presents estimates of the effect of UI extensions on the earnings of workers who exit nonemployment using the measurement error approach, and the corresponding OLS estimates are in Appendix Table A.4. Using the measurement error approach and my preferred specification in column (5), I find that extending UI benefits by one quarter causes earnings to increase by 0.9%. This estimate is not statistically significant at conventional levels, but is relatively precise as I can

benefits. They calculate an expected difference in average nonemployment duration between treatment and control groups of 1.1 weeks for a one-month reduction in benefits. I re-scale this one-month estimate to its one-quarter equivalent by multiplying their estimate by 3.

rule out negative effects beyond -1.1%. While not statistically significant, this point estimate is notable relative to the existing literature on match quality because only two other studies have estimated a positive effect of UI extensions on match quality. Nekoei and Weber (2017) estimate a statistically significant increase in earnings of 0.5% following a nine-week extension in Austria, and Farooq et al. (2020) estimate a statistically significant 2.9% increase in quarterly earnings for one additional quarter of potential UI benefits using the same data and a longer time frame as this paper.[25]

This point estimate represents the effect for workers experiencing nonemployment spells of various lengths. If there is selection in terms of productivity across nonemployment spells of different lengths, then this point estimate might mask different earnings effects at different nonemployment durations. To address earnings differences driven by this potential selection into employment, column (6) includes a fixed effect for the length of the nonemployment spell. This fixed effect is technically a "bad control" but allows me to diagnose the degree to which unobserved productivity differences drive my result. The effect size is essentially unchanged at 1.2% (not statistically significant), which supports the use of the log difference earnings measure to account for unobserved heterogeneity.

The results presented in this section indicate that the measurement error approach provides credible estimates of the effect of UI extensions. I next use this method to determine how the effects of UI vary across the distribution of local labor market concentration.

6.2 The Interaction of UI Extensions and Local Labor Market Concentration

Table 7 presents estimates of equation (4), which interacts the UI error with the HHI, both of which are measured in the quarter of separation. Column (1) shows estimates of the duration effect. The point estimate in the first row is the main effect of UI extensions and corresponds to the case of a perfectly competitive labor market in which the HHI is approaching zero. In such a market, a one quarter extension of UI benefits causes nonemployment durations to increase by 0.0617 quarters (0.80 weeks), which is very similar to the overall effect of UI extensions without

[25] Farooq et al. (2020) also report estimates for a more comparable time frame (2008-2013): a one-quarter extension leads to a 0.5% increase in quarterly earnings (not statistically significant). As discussed above, their approach compares workers exiting nonemployment at the same time and not entering nonemployment at the same time, and so their estimates are not directly comparable to mine.

accounting for concentration (0.0629 quarters in Table 5).

The coefficient on the interaction term, in row 2 of Table 7, is 0.0072, meaning that the effect of UI extensions on nonemployment durations is more positive at higher levels of concentration. The magnitude of this change, however, is neither economically nor statistically significant. For example, the effect of UI extensions when the HHI is 0.15, the upper bound for a market to be considered unconcentrated (U.S. Department of Justice and Federal Trade Commission, 2010), is 0.0628 quarters or 0.82 weeks. The same effect when the HHI is 0.25, the lower bound for highly concentrated markets, is 0.0635 or 0.83 weeks. While this interaction effect is small, it is precisely estimated: for a difference in the HHI of 0.10, the difference between an unconcetrated and highly concentrated market, I can rule out effects larger than 0.16 weeks.

In contrast to the effect on duration, I find that the UI-earnings effect is statistically significantly different across the distribution of HHI. The main effect of UI extensions on earnings in the case of perfect competition (first row) is 1.7%, nearly twice as large as the overall effect on earnings without accounting for concentration (0.9% in Table 6). This point estimate is not statistically significant but is meaningfully large when compared to existing estimates of the UI-earnings effect discussed in the previous subsection. The interaction term is -0.0474 and statistically significant at the 5% level. This interaction term means the effect of UI on re-employment earnings is more negative at higher levels of local labor market concentration. For example, the UI-earnings effect is 1.0% in unconcentrated local labor markets (HHI = 0.15) and 0.5% in highly concentrated local labor markets (HHI = 0.25). In other words, the size of the UI-earnings effect is approximately 50% smaller in highly concentrated labor markets compared to unconcentrated labor markets. The interaction term also is precisely estimated, as I can rule out changes in the UI-earnings effect outside of the interval from -0.89% to -0.06% for a change in HHI of 0.10.

Taken together, the results in Table 7 indicate that the effect of UI extensions on nonemployment durations is constant across the distribution of local labor market concentration but that their effect on re-employment earnings declines at higher levels of concentration. This pattern of results implies that the smaller UI-earnings effect in more concentrated labor markets is not driven by longer nonemployment spells and negative duration dependence. Instead, my results suggest that the change in bargaining power provided by the UI extensions I study is insufficient to counteract the negative outcomes associated with higher levels of concentration. In terms of policy, these

results imply that efforts to reduce high levels of local labor market concentration can additionally help the UI system facilitate better matches.

I also examine how UI affects outcomes at different parts of the distribution of concentration by estimating separate regressions within each tercile of the distribution of HHI.[26] The results from this exercise are presented in Table 8. Panel A includes results for the effect on nonemployment duration. I find suggestive evidence that UI extensions cause nonemployment spells to be longer throughout the distribution of concentration, but that the magnitude of this effect differs non-linearly with the level of HHI. The effect of UI extensions on the duration of nonemployment is about twice as large for workers in the middle (3.0% relative to the mean duration) and top of the HHI distribution (2.9%) compared to workers separating from the least concentrated labor markets (1.7%). This pattern suggests that local concentration magnifies the effect of UI extensions on nonemployment duration above a particular level of concentration. Each of these estimates is statistically significant, though the estimates for the first and third terciles are significant at the 10% level while that for the middle of the distribution is significant at the 5% level.

Turning to effects on match quality, Panel B of Table 8 presents estimates of the effect of UI extensions on re-employment earnings across terciles of local concentration. UI extensions cause increases in earnings throughout the distribution of concentration, and the point estimates suggest that concentration modifies the effect of UI in a non-linear fashion. Workers in the middle third of the HHI distribution saw their earnings increase the most (1.5%) following a UI extension, while earnings for workers exposed to the least amount of concentration (1.2%) increased by twice as much as the earnings of workers exposed to the highest levels of concentration (0.6%). These results are not statistically significantly different from zero but are economically meaningful. As discussed in Section 6.1, only two recent studies have found a positive and significant effect of UI duration on earnings. Nekoei and Weber (2017) estimate a 0.5% increase in Austria, and Farooq et al. (2020) estimate a 2.9% increase in the U.S. using the same data as this paper. While my 95% confidence intervals include effects of the size found by Nekoei and Weber (2017), my point estimates are substantially larger among workers in the bottom two-thirds of the distribution of local concentration. One reason why my point estimates, as well as that of Farooq et al. (2020), might be larger than those in Nekoei and Weber (2017) is that wage determination in the U.S. is

[26] Terciles are constructed across the full sample period.

much more decentralized than in Austria, meaning that, on average, U.S. workers likely benefit more from UI-induced changes to their bargaining power.

As a whole, these tercile-based results suggest that the effects of UI extensions are concave with respect to local labor market concentration. Comparing effects for workers at the bottom of the HHI distribution to those in the middle, UI extensions cause nonemployment durations to be longer and earnings to increase modestly. In contrast, compared to workers in the middle of the distribution, workers exposed to the highest levels of concentration have longer spells but only small increases in earnings. This pattern of results suggests that workers exposed to higher levels of concentration are able to search productively with additional UI benefits as both the length of their nonemployment spells and earnings increase as a result of UI extensions. This benefit to earnings, however, fades at higher levels of concentration. In terms of policy, these results suggest that UI extensions do not harm workers exposed to higher levels of concentration and might also lead to higher earnings after a nonemployment spell. More speculatively, they also imply that policies aimed at reducing extreme levels of labor market concentration could have the added benefit of improving the effectiveness of UI.

A potential concern with my preferred approach is the influence of extreme values of the HHI on my estimates. I prefer to use the level of HHI because the "outlying" values of the HHI distribution correspond to the cases with the least potential for employer market power (the perfectly competitive limit as the HHI approaches zero) and the greatest potential for such power (the monopsonistic limit as the HHI approaches one). But it is still important to understand the influence of observations at either end of the range of HHI. To further explore how extreme values of the HHI impact my results, Table A.6 shows estimates from a version of equation (4) using $\log(HHI)$. The coefficients in this table have a different interpretation as a result of the log transformation. The point estimates in the first row now correspond to the effect of UI extensions in the case of true monopsony, where $HHI = 1$. In such a market, a one quarter extension of UI benefits causes nonemployment durations to increase by 0.0766 quarters (1 week) and earnings at re-employment to remain unchanged (0.03%). The sign of the interaction term in row 2, for both duration and earnings, is the same as that using the level of HHI but the magnitude is smaller. At an HHI of 0.15, the marginal effect of an additional quarter of UI benefits is an increase in nonemployment duration of 3.0% relative to the mean duration and an increase in earnings at re-employment of

0.6%. These marginal effects are essentially unchanged at an HHI of 0.25: the UI-duration effect is 3.1% and the UI-earnings effect is 0.5%. These point estimates, together with the estimates in Table 7, imply that the significantly lower UI-earnings effect in more concentrated markets is driven by differences in the UI-earnings effect at the extremes of the HHI distribution. This pattern of effects also suggests that labor market interventions aimed at reducing the highest levels of concentration might improve the ability of UI to deliver better matches.

6.3 Mechanisms

The estimates in Section 6.2 show that the effect of UI extensions on earnings is more negative at higher levels of concentration. Are there ways other than the immediate impact on earnings through which UI extensions could affect match quality for these workers? To answer this question, I investigate the mobility of workers across the distribution of local concentration.

To the extent that concentration does translate into monopsony power, nonemployed workers exposed to higher levels of concentration might use extended UI benefits to switch employers or local labor markets to escape such power. Table 9 presents estimates of the effect of UI extensions on different measures of worker mobility. Overall, I find that the effect of UI extensions on the likelihood of changing workplaces and local labor markets increases at higher levels of concentration, but these effects are not economically significant. Among workers that do switch local labor markets following a nonemployment spell, there is no difference in the concentration of their new labor market compared to their old one.

6.4 Robustness

In this section, I explore the robustness of my results to an alternative empirical strategy as well as different earnings measures. My empirical strategy isolates the UI extensions caused by quasi-random errors in the real-time measurement of state-level labor market conditions. I exploit these errors by constructing counterfactual maximum potential UI durations using revisions to state unemployment rates and comparing them to the actual potential UI durations faced by workers in real-time. I also can isolate the effect of this measurement error by using as the treatment variable the actual potential UI durations based on the real-time unemployment rate and controlling flexibly for the concurrent real-time unemployment rate. In the absence of measurement error, these

potential UI durations and unemployment rates would be perfectly collinear because UI duration is a direct function of the unemployment rate. Therefore, the coefficient on potential UI benefit duration with this approach is identified solely off of measurement error.

The results using this approach are given in Table 10. In terms of the duration effect, the coefficient in column 1 is 0.0434, which is between that from my OLS results and UI error results. This represents an increase in nonemployment duration of 1.9% relative to the mean, and is statistically significant at the 1% level. The estimate for earnings also is similar to my estimates using the UI errors: an additional quarter of UI benefits causes earnings to increase by 0.8%. It is reassuring that I obtain similar estimates from this alternative approach because it emphasizes that the underlying identifying variation in my estimates using the UI error is coming from measurement error and not underlying labor market conditions.

I also examine the overall effect of UI extensions on alternative measures of earnings. These estimates are shown in Table A.5. In the first two columns, I estimate the effect of UI extensions on log earnings in the first quarter (column 1) and the second quarter (column 2) after a nonemployment spell, while controlling for log earnings two quarters prior to the start of the nonemployment spell. These estimates do not implicitly include a worker fixed effect and therefore include additional unobserved worker heterogeneity not present in my preferred measure of match quality. My results indicate that, without controlling for unobserved worker heterogeneity, UI extensions led to decreases in earnings of 1.4-2.3%. These effects, however, are quite imprecise, and neither point estimate is statistically significant.

In column (3) of Table A.5, I use as the outcome variable the difference in log earnings between the first quarter of re-employment and the last quarter before a nonemployment spell. Like my preferred measure of match quality, this outcome implicitly controls for unobserved heterogeneity across workers. My results are very similar to my preferred specification: earnings increase by 0.8% following a UI extension, an effect that is not statistically significant. Taken together, the results using these alternative earnings measures point to the importance of accounting for unobserved heterogeneity and the robustness of my results to a related measure of match quality.

7 Conclusion

This paper provides estimates of the effect of UI extensions using a measurement error approach, which is new to the literature on UI and worker-level outcomes. I also show the first estimates of how additional UI benefits affect workers across the distribution of local labor market concentration. I argue that it is theoretically ambiguous how the effect of UI on match quality will vary across the distribution of concentration. UI extensions provide an additional outside option that can improve the bargaining power of workers exposed to higher levels of concentration, enabling them to be more selective in their search and leading to a more positive UI-earnings effect in more concentrated labor markets. Workers in highly concentrated local labor markets, however, might be limited in their ability to become more selective, and UI extensions could lead to longer nonemployment durations and a more negative UI-earnings effect among these workers compared to those in less concentrated markets.

I identify the effects of UI extensions using measurement error in state unemployment rates that assigned conditionally random potential UI benefit durations during the Great Recession. Overall, I find that nonemployment durations are 2.8% longer and re-employment earnings are 0.9% higher following a UI extension, though only the duration estimate is statistically significantly different from zero. My estimated duration effect is comparable to recent estimates using regression discontinuity designs in Europe, but substantially smaller than U.S. studies based on policy changes in one or two states. My estimated earnings effect, meanwhile, is not significantly different from zero but nearly twice as large as the upper range of existing estimates. The sign and magnitude of this point estimate is notable given that the majority of the prior literature finds small and negative effects of UI extensions on earnings.

I then estimate how these effects differ across the distribution of local labor market concentration. I find that the impact of UI extensions on earnings is significantly lower in more concentrated labor markets, an effect that is driven by differences at the extremes of the HHI distribution. As a result, the UI-earnings effect in unconcentrated labor markets is twice as large as that in highly concentrated labor markets. This difference is not the result of UI extensions having larger effects on nonemployment durations in more concentrated labor markets. I find suggestive evidence that the effects of UI on the length of nonemployment spells is nonlinear with respect to concentration,

but I estimate that the effect of UI extensions on nonemployment duration is neither economically nor statistically significantly different across the distribution of concentration. This pattern of results suggests that UI extensions during the Great Recession did not provide a sufficiently large increase in worker bargaining power to offset the negative outcomes associated with labor market concentration. In terms of mechanisms, I find limited evidence of differences in worker mobility across workplaces, labor markets, and industries across the distribution of concentration. UI extensions also do not induce workers to move to less concentrated labor markets.

My results have important implications for policy. In particular, they indicate that UI extensions during the Great Recession had a small impact on the time workers spent out of work, and that this additional time searching led to positive but not significant changes in their earnings. These findings imply very little scope for moral hazard as a result of longer UI benefits, consistent with prior research on the trade-off between moral hazard and liquidity at the heart of UI policy during recessions, and the Great Recession in particular (e.g., Kroft and Notowidigdo, 2016; Rothstein, 2011).

In addition, by providing the first estimates of the interaction of UI and concentration, I help to inform policy related to labor market power, of which concentration is a potential source. My results imply that UI extensions neither hurt nor help workers exposed to higher levels of concentration, which is important for policy makers concerned about unintentional negative effects of additional UI benefits during the next recession. Moreover, I find that the positive impact of UI extensions on earnings decreases at higher levels of concentration. If other policies, like antitrust enforcement, are able to reduce local labor market concentration, they might also have complementary benefits in the form of improving the effectiveness of UI.

This study is limited by the type of variation in UI benefits that I exploit. These extensions are temporary in nature and almost exclusively occur during a recessionary period. To the extent that permanent changes to UI benefits have different interactions with local concentration, my estimates will be limited in their ability to speak to such policy changes. I am also unable to separately observe nonemployed workers who are eligible for UI and those who, for example, voluntarily leave their job. It is likely that changes in UI policy have different effects for these two groups, and so being able to distinguish between these workers would be beneficial. Additionally, other research has shown that the effects of UI can differ across the business cycle. My estimates, therefore, would

likely be different during expansions, potentially leading to alternative policy implications. For these reasons, future research in this area should focus on how permanent changes in UI interact with concentration, and the role of UI receipt in that interaction.

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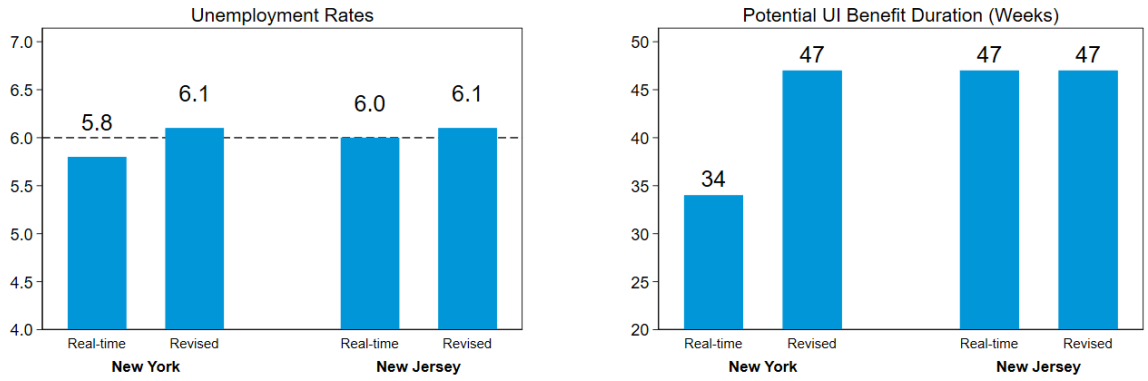
Figures

Figure 1: Example of UI Error: January 2009

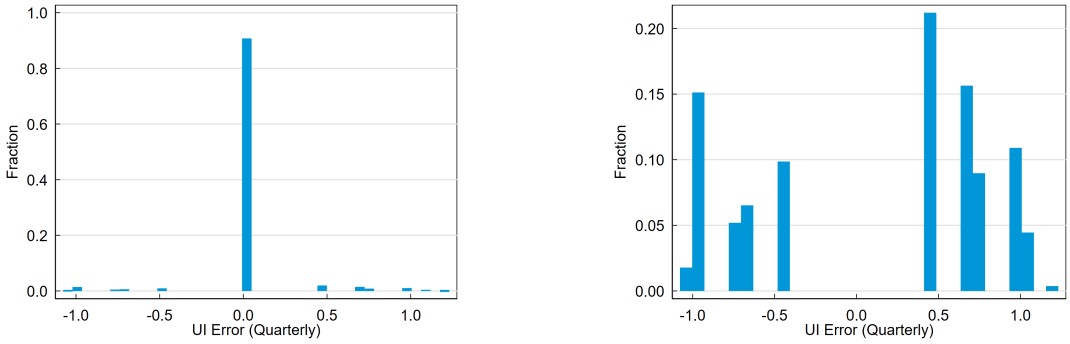


(a) Unemployment rates

(b) Potential benefit duration (weeks)

This figure provides an example of the UI error, which is calculated as the difference in potential UI benefit duration under the real-time state-level unemployment rate and the counterfactual benefit duration using the revised state-level unemployment rate. The Emergency Unemployment Compensation Program (EUC) from 2008 provided an additional 13 weeks of benefits in states with an unemployment rate of at least 6.0%. In this example, using the real-time unemployment rates, UI-eligible workers in New York were eligible for 34 weeks of UI benefits, while those in New Jersey were eligible for 47 weeks of benefits. Using the revised unemployment rates, workers in both states would have been eligible for 47 weeks of benefits. The UI error for New York is, therefore, $34 - 47 = -13$ weeks. In New Jersey, it is $47 - 47 = 0$ weeks.

Figure 2: Distribution of UI Errors



(a) Quarterly UI Error

(b) Nonzero Quarterly UI Errors

Histograms of the size of UI Errors in the 28 states in my sample from 2004-2014. Panel A includes all state-quarter observations. Panel B includes all state-quarter observations with nonzero UI Errors. UI Error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter (measured in quarters).

Tables

Table 1: Summary Statistics

	(1) Mean	(2) SD
Age	38.78	8.872
Women	0.5282	0.4992
White	0.7626	0.4255
Black	0.1405	0.3475
Asian	0.0600	0.2375
Other races	0.0369	0.1885
Hispanic	0.2060	0.4044
Less than high school	0.1852	0.3884
High school	0.2786	0.4483
Some college/Associates degree	0.3092	0.4622
Bachelors degree or higher	0.2270	0.4189
Earnings in final quarter before nonemployment spell[a]	6,378	8,551
Earnings in second-to-last quarter before nonemployment spell[a]	8,205	8,414
Earnings in first quarter after nonemployment spell[a]	6,038	8,003
Earnings in second quarter after nonemployment spell[a]	8,092	8,155
Total labor market experience[b]	49.38	20.86
Prior job tenure[b]	9.555	11.39
Number of nonemployment spells	2.628	1.801
Nonemployment spell length[b]	2.249	1.921
UI error	0.0086	0.1737
Payroll HHI in quarter of separation	0.1383	0.2078
Payroll HHI at re-employment	0.1368	0.2064
Total Payroll in Local Labor Market[a]	47,000	247,000
Different SEIN	0.5641	0.4959
Different local labor market	0.5194	0.4996
Different industry (same commuting zone)	0.4806	0.4996
Different commuting zone	0.0388	0.1930
Observe at least 1 quarter of re-employment	0.9551	0.2072
Observe at least 2 quarters of re-employment	0.7563	0.4293
Number of nonemployment spells	25,930,000	
Number of workers	19,690,000	

a) 2018$, winsorized at the 1% and 99% levels. b) Measured in full quarters of nonemployment (top-coded at 12). Sample sizes and estimated values have been rounded for disclosure avoidance.

Table 2: Selection: Exogeneity of UI Errors and Labor Market Conditions

	(1)	(2)	(3)	(4)
R^2	0.0004	0.0242	0.0144	0.0384
Concurrent revised unemployment rate	X			
Lagged revised unemployment rate		X		X
Worker characteristics			X	X
Observations	25,930,000	25,930,000	25,930,000	25,930,000

Each column is a separate regression. In column 1, the UI error is regressed on the revised unemployment rate in the quarter of separation from employment. In column 2, the UI error is regressed on four lags of the revised unemployment rate. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance.

Table 3: Selection: UI Errors and Predicted Earnings

	(1) 2 Quarters Before Spell	(2) 1 Quarter Before Spell	(3) 1 Quarter After Spell	(4) 2 Quarters After Spell
UI error	28.51	12.59	-11.05	-5.93
	(54.75)	(43.53)	(33.85)	(38.34)
Spell start FE	X	X	X	X
State FE	X	X	X	X
Local labor market FE	X	X	X	X
Lagged revised unemployment rate	X	X	X	X
Observations	25,930,000	25,930,000	20,370,000	16,700,000
R^2	0.1393	0.1385	0.1462	0.1506

Each column is a separate regression. The outcome variable is predicted quarterly earnings in the quarter indicated in the column title. Earnings are predicted using a model consisting of fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. The coefficients are measured in dollars Sample sizes and estimated values have been rounded for disclosure avoidance.

Table 4: Nonemployment Duration – OLS Estimates

	(1)	(2)	(3)	(4)	(5)
Potential UI Duration	0.0162	0.0361***	0.0313***	0.0306***	0.0304***
	(0.0137)	(0.0108)	(0.0091)	(0.0090)	(0.0091)
% Effect	0.72	1.61	1.39	1.36	1.49
Spell start FE	X	X	X	X	X
State FE	X	X	X	X	X
Local labor market FE			X	X	X
Lagged unemployment rate		X	X	X	X
Worker characteristics				X	X
Spell length FE					
Completed Spells Only					X
Observations	25,930,000	25,930,000	25,930,000	25,930,000	24,760,000
R^2	0.5389	0.5389	0.5506	0.5542	0.1213
Dep. Var. Mean	2.249	2.249	2.249	2.249	2.037

In each column, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). Potential UI Duration is the maximum number of quarters of UI benefits available to qualifying workers who separate from a job in the quarter before the onset of a nonemployment spell. Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 5: Nonemployment Duration – Measurement Error Approach

	(1)	(2)	(3)	(4)	(5)
UI error	0.0825***	0.0821**	0.0639**	0.0629**	0.0641**
	(0.0293)	(0.0311)	(0.0254)	(0.0243)	(0.0239)
% Effect	3.67	3.65	2.84	2.80	3.15
Spell start FE	X	X	X	X	X
State FE	X	X	X	X	X
Local labor market FE			X	X	X
Lagged unemployment rate		X	X	X	X
Worker characteristics				X	X
Spell length FE					
Completed Spells Only					X
Observations	25,930,000	25,930,000	25,930,000	25,930,000	24,760,000
R^2	0.5389	0.5390	0.5506	0.5542	0.1214
Dep. Var. Mean	2.249	2.249	2.249	2.249	2.037

In each column, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 6: Re-employment Earnings – Measurement Error Approach

	(1)	(2)	(3)	(4)	(5)
UI error	-0.0091	-0.0059	0.0004	0.0093	0.0122
	(0.0084)	(0.0080)	(0.0077)	(0.0106)	(0.0099)
% Effect	-0.91	-0.59	0.04	0.93	1.22
Spell start FE	X	X	X	X	X
State FE	X	X	X	X	X
Local labor market FE			X	X	X
Lagged unemployment rate		X	X	X	X
Worker characteristics				X	X
Spell length FE					X
Completed Spells Only	X	X	X	X	X
Observations	16,700,000	16,700,000	16,700,000	16,700,000	16,700,000
R^2	0.0019	0.0020	0.0235	0.0816	0.0834
Dep. Var. Mean	-0.1213	-0.1213	-0.1213	-0.1213	-0.1213

In each column, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 7: Interaction Effects of UI Extensions and Local Concentration

	(1) Nonemployment Duration	(2) Re-employment Earnings
UI error	0.0617**	0.0167
	(0.0229)	(0.0100)
UI error × HHI	0.0072	-0.0474**
	(0.0576)	(0.0211)
Marginal Effect at DOJ Thresholds		
Low Concentration Market (HHI = 0.15)	2.8%	0.96%
High Concentration Market (HHI = 0.25)	2.8%	0.49%
Spell start FE	X	X
State FE	X	X
Local labor market FE	X	X
Lagged unemployment rate	X	X
Worker characteristics	X	X
Spell length FE		
Completed Spells Only		X
Observations	25,930,000	16,700,000
R^2	0.5543	0.0816
Dep. Var. Mean	2.249	-0.1213

Each column is a separate regression. In column 1, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). In column 2, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Department of Justice (DOJ) thresholds correspond to the upper bound for an unconcentrated market and the lower bound for a highly concentrated market according to U.S. Department of Justice and Federal Trade Commission (2010). Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 8: Effect of UI Extensions by Terciles of HHI Distribution

Panel A: Nonemployment Duration			
	(1)	(2)	(3)
	HHI Tercile 1	HHI Tercile 2	HHI Tercile 3
UI error	0.0381*	0.0688**	0.0645*
	(0.0193)	(0.0263)	(0.0362)
% Effect	1.67	3.03	2.93
Observations	7,929,000	8,687,000	9,311,000
R^2	0.6223	0.5235	0.5185
Dep. Var. Mean	2.286	2.270	2.198
Panel B: Re-employment Earnings			
	(1)	(2)	(3)
	HHI Tercile 1	HHI Tercile 2	HHI Tercile 3
UI error	0.0124	0.0152	0.0063
	(0.0140)	(0.0116)	(0.0148)
% Effect	1.24	1.52	0.63
Spell start FE	X	X	X
State FE	X	X	X
Local labor market FE	X	X	X
Lagged unemployment rate	X	X	X
Worker characteristics	X	X	X
Spell length FE			
Observations	4,962,000	5,610,000	6,125,000
R^2	0.0697	0.0818	0.0950
Dep. Var. Mean	-0.1129	-0.1171	-0.1320

Each column is a separate regression. The sample in each column is a tercile of the distribution of HHI, measured over the entire sample period (2004-2014). In Panel A, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). In Panel B, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Samples in Panel B limited to completed spells only. Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 9: Interaction Effects of UI Extensions and Local Concentration: Mobility

	(1)	(2)	(3)	(4)	(5)
	Different SEIN	Different LLM	Different Industry	Different Commuting Zone	Next HHI
UI error	0.0059	0.0053	0.0016	0.0038*	-0.0024
	(0.0098)	(0.0100)	(0.0104)	(0.0022)	(0.0015)
UI error × HHI	0.0386*	0.0403*	0.0501***	-0.0098	-0.0015
	(0.0219)	(0.0236)	(0.0170)	(0.0093)	(0.0024)
% Effect: ΔHHI = 0.10	0.68	0.78	1.04	-2.53	-0.12
Spell start FE	X	X	X	X	X
State FE	X	X	X	X	X
Local labor market FE	X	X	X	X	X
Lagged unemployment rate	X	X	X	X	X
Worker characteristics	X	X	X	X	X
Spell length FE					
Completed Spells Only	X	X	X	X	X
Changed LLMs					X
Observations	20,370,000	20,370,000	20,370,000	20,370,000	6,000,000
R^2	0.1895	0.1707	0.1495	0.0456	0.1866
Dep. Var. Mean	0.5641	0.5194	0.4806	0.0388	0.1294

Each column is a separate regression. In column 1, the dependent variable is an indicator for changing SEINs following a nonemployment spell. SEINs can correspond to individual or multiple establishments. In column 2, the dependent variable is an indicator for changing local labor markets (LLMs), either as a result of changing industry or commuting zone or both. In column 3, the dependent variable is an indicator for changing industry but not commuting zone. In column 4, the dependent variable is an indicator for changing commuting zones. In column 5, the dependent variable is the HHI for the local labor market of re-employment in the first quarter after a nonemployment spell. The sample in column 5 is limited to spells in which the worker changed local labor markets. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table 10: Alternative Specification – Measurement Error Approach

	(1) Duration	(2) Earnings
UI error	0.0434***	0.0075
	(0.0149)	(0.0075)
% Effect	1.93	0.75
Spell start FE	X	X
State FE	X	X
Local labor market FE	X	X
Lagged unemployment rate	X	X
Worker characteristics	X	X
Spell length FE		
Completed Spells Only		X
Revised unemployment rate FE	X	X
Observations	25,930,000	16,700,000
R^2	0.5556	0.0822
Dep. Var. Mean	2.249	-0.1213

Each column is a separate regression. In column 1, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). In column 2, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. Potential UI Duration is the maximum number of quarters of UI benefits available to qualifying workers who separate from a job in the quarter before the onset of a nonemployment spell. Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Revised unemployment rate fixed effect corresponds to the revised unemployment rate in the quarter of separation. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

A Additional Results

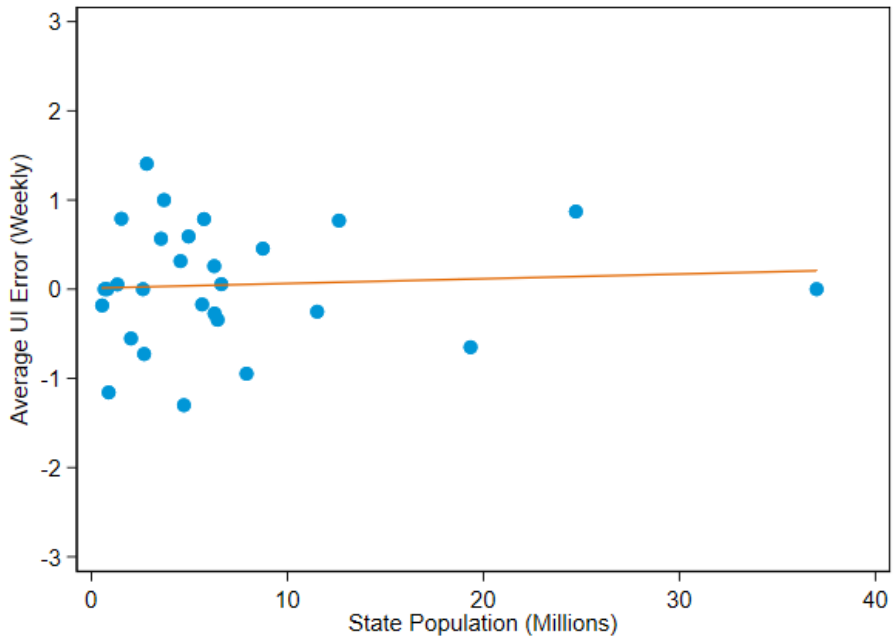


Figure A.1: UI Errors and State Populations

Scatter plot of the average weekly UI error and average state populations (in millions) for the 28 states in the sample from the period 2004-2014. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in weeks).

Table A.1: Summary Statistics - Current Population Survey

	(1) All Unemployed	(2) Unemployed and Likely UI Eligible
Age	38.7574	39.6563
	(8.8032)	(8.6946)
Women	0.4637	0.3912
	(0.4987)	(0.4880)
White	0.7377	0.7613
	(0.4399)	(0.4263)
Black	0.1785	0.1660
	(0.3829)	(0.3721)
Asian	0.0369	0.0318
	(0.1885)	(0.1755)
Other races	0.0469	0.0409
	(0.2115)	(0.1980)
Hispanic	0.1664	0.1693
	(0.3725)	(0.3750)
Less than high school	0.1670	0.1639
	(0.3730)	(0.3701)
High School	0.3622	0.3822
	(0.4806)	(0.4859)
Some college/Associates degree	0.2828	0.2743
	(0.4504)	(0.4462)
Bachelors degree or higher	0.1880	0.1796
	(0.3907)	(0.3839)
Unemployment spell length[a]	2.1587	2.0500
	(2.4274)	(2.3191)
Observations	329,288	214,570

a) Measured in quarters. Data from the January 2004-December 2014 waves of the Current Population Survey. Likely UI eligible means respondent indicated that they are unemployed as a result of job loss. Sample limited to prime age workers (aged 25-54).

Table A.2: Selection: UI Errors and Worker Characteristics

	(1) Age	(2) Women	(3) White	(4) Black	(5) Asian
UI error	0.0019	0.0039**	0.0019	-0.0019	0.0005
	(0.0354)	(0.0017)	(0.0017)	(0.0017)	(0.0005)

	(6) Other Races	(7) Hispanic	(8) < High School	(9) High School	(10) Some College
UI error	-0.0005	-0.0015	0.0010	-0.0002	-0.0010
	(0.0004)	(0.0016)	(0.0009)	(0.0006)	(0.0008)

	(11) College	(12) LM Experience	(13) Tenure	(14) # Spells
UI error	0.0001	-0.1044	0.0902	-0.0213
	(0.0.0007)	(0.2430)	(0.1640)	(0.0201)

Observations = 25,930,000. All models control for lagged unemployment rate and fixed effects for spell start, state, and local labor market.

Table A.3: Positive and Negative UI Errors

	(1) Spell Length	(2) Earnings
Positive UI error	0.0103	0.0006
	(0.0142)	(0.0049)
Negative UI error	-0.0357*	-0.0088
	(0.0190)	(0.0079)
% Effect: Positive	0.46	0.06
% Effect: Negative	-1.59	-0.88
Spell start FE	X	X
State FE	X	X
Local labor market FE	X	X
Lagged unemployment rate	X	X
Worker characteristics	X	X
Spell length FE		
Completed Spells Only		X
Observations	25,930,000	16,700,000
R^2	0.5542	0.0816
Dep. Var. Mean	2.249	-0.1213

Each column is a separate regression. In column 1, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). In column 2, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table A.4: Re-employment Earnings – OLS Estimates

	(1)	(2)	(3)	(4)	(5)
Potential UI Duration	0.0082*	0.0052	0.0071*	0.0098*	0.0111**
	(0.0047)	(0.0046)	(0.0040)	(0.0053)	(0.0053)
% Effect	0.82	0.52	0.71	0.98	1.11
Spell start FE	X	X	X	X	X
State FE	X	X	X	X	X
Local labor market FE			X	X	X
Lagged unemployment rate		X	X	X	X
Worker characteristics				X	X
Spell length FE					X
Completed Spells Only	X	X	X	X	X
Observations	16,700,000	16,700,000	16,700,000	16,700,000	16,700,000
R^2	0.0019	0.0020	0.0235	0.0816	0.0834
Dep. Var. Mean	-0.1213	-0.1213	-0.1213	-0.1213	-0.1213

Each column is a separate regression. In each column, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. Potential UI Duration is the maximum number of quarters of UI benefits available to qualifying workers who separate from a job in the quarter before the onset of a nonemployment spell. Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table A.5: Alternative re-employment Earnings – Measurement Error Approach

	(1) Log Earnings: 1st Quarter	(2) Log Earnings: 2nd Quarter	(3) Log Difference: 1st Quarter
UI error	-0.0226	-0.0139	0.0075
	(0.0226)	(0.0109)	(0.0137)
% Effect	-2.26	-1.39	0.75
Spell start FE	X	X	X
State FE	X	X	X
Local labor market FE	X	X	X
Lagged unemployment rate	X	X	X
Worker characteristics	X	X	X
Spell length FE			
Completed spells only	X	X	X
Earnings 2 quarters before spell start	X	X	X
Observations	16,700,000	16,700,000	16,700,000
R^2	0.3056	0.2993	0.0299
Dep. Var. Mean	8.034	8.425	-0.0606

Each column is a separate regression. In column 1, the dependent variable is log earnings the last quarter before the onset of a nonemployment spell. In column 2, the dependent variable is log earnings the second-to-last quarter before the onset of a nonemployment spell. In column 3, the dependent variable is the difference between log earnings in the first quarter after a nonemployment spell and the first quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

Table A.6: Interaction Effects of UI Extensions and Log Local Concentration

	(1) Nonemployment Duration	(2) Re-employment Earnings
UI error	0.0766*	0.0003
	(0.0430)	(0.0172)
UI error × Log(HHI)	0.0048	-0.0031
	(0.0079)	(0.0035)
Marginal Effect at DOJ Thresholds		
Low Concentration (HHI = 0.15)	3.0%	0.6%
High Concentration (HHI = 0.25)	3.1%	0.5%
Spell start FE	X	X
State FE	X	X
Local labor market FE	X	X
Lagged unemployment rate	X	X
Worker characteristics	X	X
Spell length FE		
Completed Spells Only		X
Observations	25,930,000	16,700,000
R^2	0.5543	0.0816
Dep. Var. Mean	2.249	-0.1213

Each column is a separate regression. In column 1, the dependent variable is the length of nonemployment spells measured in quarters (top-coded at 12). In column 2, the dependent variable is the difference in log earnings between the second quarter after a nonemployment spell and the second-to-last quarter before a nonemployment spell. UI error is the difference in potential UI benefit duration using the real-time and revised state unemployment rate in the quarter before the nonemployment spell onset (measured in quarters). Department of Justice (DOJ) thresholds correspond to the upper bound for an unconcentrated market and the lower bound for a highly concentrated market according to U.S. Department of Justice and Federal Trade Commission (2010). Local labor market fixed effects are commuting zone-by-industry (4-digit NAICS level) fixed effects. Worker characteristics include fixed effects for gender, education, race and ethnicity, age, total labor market experience, and prior job tenure. Sample sizes and estimated values have been rounded for disclosure avoidance. Standard errors clustered at the state level in parentheses: significant at *10%, **5%, and ***1%.

B Data Appendix

The variable identifying employers in the LEHD is the State Employer Identification Number (SEIN). SEINs do not necessarily correspond to establishments for firms with multiple locations, and multiple industries can be associated with the same SEIN if it happens to have multiple establishments (SEINUNITs). In order to assign workers to a local labor market, which I define as the combination of a commuting zone and industry, I need to assign each worker to one industry. This assignment presents two challenges. First, workers may hold multiple jobs. For these workers, I assign them to a "dominant employer" in each quarter. I follow Hyatt et al. (2014) and define the dominant employer as that with the highest combined earnings in two consecutive quarters. Second, firms with multiple establishments might have one SEIN, and I do not observe the actual SEINUNIT for workers at firms that happen to report separately by establishment. I address this challenge by assigning workers employed by a SEIN associated with multiple industries to the one with the most employment in their county of residence.