Occupational Mobility, Occupational Distance & Unemployment Insurance

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Abstract

I study occupational mobility and the effect of unemployment insurance (UI) on mobility using a skill-based distance measure between occupations based on the O*NET Program's classification of occupational skill requirements. I first show using the skill-based distance and observed job transitions in the US Current Population Survey (CPS) that unemployed workers tend to find jobs with skill requirements that are close to the occupation of their previous job. Also, using the Displaced Worker Supplement of the CPS I show that a larger occupational distance is associated with lower re-employment wages. Exploiting state and time variation in UI generosity during the Great Recession of 2007-2009 and the subsequent recovery, I show that more generous UI decreases the occupational distance in observed unemployment to employment transitions, so that unemployed workers end up taking jobs with skill requirements closer to their previous job.

Keywords: occupational mobility, occupational distance, job search, unemployment insurance, occupational switching, O*NET, mismatch, reallocation.

JEL codes: J24, J64, J65.

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1 Introduction

An unemployed worker faces an important trade-off when searching for work: she may accept whatever job that comes her way and quickly find work. However, if occupations differ in, for example, their task content, there may be a switching cost associated with accepting the offer. The worker may have to incur retraining costs or accept a lower wage due to lower productivity on the job. To reduce these switching costs, she can be more patient and wait for a more suitable offer. However, being unemployed for longer is costly in itself, and many unemployed workers face liquidity constraints that interfere with this trade-off. As shown by Chetty (2008), there are large and important liquidity effects of unemployment insurance (UI) on the duration of unemployment spells. Indeed, as he points out, most unemployed report having little liquidity to remain unemployed for prolonged periods of time. During recessions, when jobs are harder to come by, this effect may be even more important.

One common policy instrument during economic downturns is the extension of the duration of UI. During the general economic decline in the late 2000s and early 2010s, policy makers in the United States enacted unprecedented extensions of the duration of UI. Individuals in some states could qualify for as many as 99 weeks of UI, whilst the Federal baseline of UI under normal economic conditions is merely 26 weeks. Leveraging this expansion, this paper studies the effect of the duration of UI on occupational mobility.

To better understand the patterns of occupational mobility in the data and the effects of UI on mobility, I use the descriptors in the O*NET database to create a task-based measure of distance between occupations. Occupations are classified in a range of skills, tasks, and knowledge, indicating the required level of each element in every occupation. Similar to Guvenen, Kuruscu, Tanaka, and Wiczer (2018), I let these elements form a vector that describes an occupation. A measure of (dis)similarity between any two occupations may then be calculated by applying a metric to the two vectors.

I combine these task vectors with observed job transitions in the Current Population Survey (CPS) to calculate the distances between workers' old and new occupations. I show a number of facts based on these distances. First, unemployed workers tend to find jobs with skill requirements similar to their previous job or, in other words, jobs that are *close* to their previous job. Second, using the Displaced Worker Supplement (DWS) to the CPS, I show that longer moves are associated with lower re-employment wages. Third, older workers tend to take jobs with lower task-based distance than younger workers.

Next, I merge the observed job moves with detailed data on UI benefits compiled by Farber, Rothstein, and Valletta (2015). These give the number of weeks of UI available to a worker in any state and month, conditional on his duration of unemployment from January 2004 to December 2013. To identify the effect of UI on occupational mobility I rely on variation generated by states' choices of participation, expirations, re-authorizations, and rollback of UI extension programs, as in e.g. Howell and Azizoglu (2011), Rothstein (2011), Farber, Rothstein, and Valletta (2015), and Farber and Valletta (2015).

There are two sources of variation that I rely on. First, two individuals in the same state and month may expect different UI generosity, depending on when they became unemployed as extension programs expire and are rolled back. Second, differences in state participation decisions in the federal extension programs enable me to compare workers in similar labor markets with similar unemployment duration but with different UI length. Using these data, I show that more generous UI decreases the occupational distance in observed unemployment to employment transitions, so that unemployed workers end up taking jobs with skill requirements closer to their last held job. The estimated effect of UI length on occupational distance is sizable. In the trough of the Great Recession (May 2009) the newly unemployed had on average 30 more weeks of UI available across all states. For an unemployed worker previously occupied as a *Real Estate Agent*, this amounts to being hired in *Claims Examiners, Property and Casualty Insurance* rather than a *Lodging Manager*. Or, a *Securities and Commodities Trader* may become a *Credit Authorizer* rather than a *Librarian*.

The results are robust to using alternative specifications of the task-based distance, and only looking at workers above 35 years of age. Moreover, including or excluding individuals out of labor force when identifying employment transitions in the CPS does not affect the results. The result are also unaffected by only considering transitions where the worker remains employed for at least two consecutive months. All regressions include demographic controls and flexible state-by-month fixed effects.

Finally, I calibrate a standard search model, as in McCall (1970), augmented with costly occupational switching to assess the plausibility of the estimated effects and to account for other empirical regularities that I uncover in the data. The switching cost is modeled through job offers having a lower wage relative to an *ideal* job, where the wage discount is increasing in the task-based distance between the job offer and the *ideal* job of the worker. The model shows that with longer UI, workers search for closer jobs longer, before becoming inclined to accept less close offers, in line with the main empirical result of this paper. A calibrated version of the model can also account quantitatively for the empirical results.

The rest of the paper is organized as follows. I start by giving an overview of the literature, before proceeding to describe how I construct the distance measure and show the main descriptive facts about mobility. Section 3 presents my sample from the Current Population Survey and lays out the empirical strategy. I present the UI results in Section 4. Next, to better understand the estimated effects, Section 5 calibrates a simple search model with costly occupational switching and time limited unemployment benefits. Finally, Section 6 discusses the results and concludes.[1]

1.1 Related literature

This paper relates to the literature studying human-capital specificity and occupational mobility. Autor, Levy, and Murnane (2003) use occupation descriptors from the Dictionary of Occupational Titles (DOT) to study how technological change affects skill demands. Since then, a growing number of papers have used task or skill descriptors of occupations for various purposes. For example, Ingram and Neumann (2006) and Yamaguchi (2012) studied the returns to skill. Some papers use skill descriptors to calculate measures of (dis)similarity or distance between occupations. Cortes and Gallipoli (2018) use the DOT to create a measure of distance between occupations to study the costs

[1] In the appendix I provide omitted proofs, more detailed data sample descriptions, additional results, and robustness checks.

of occupational mobility. Other examples include Poletaev and Robinson (2008), Gathmann and Schönberg (2010), and Robinson (2011). Some recent papers use the DOT's successor, the O*NET database. Guvenen, Kuruscu, Tanaka, and Wiczer (2018) study mismatch and use worker chacteristics matched to the wider set of occupation descriptors available from the O*NET to calculate a measure of mismatch between the worker and an occupation.

The task-based distance constructed in this paper does not deviate significantly from these papers. One difference is, however, to use the explained variances from a principal component analysis to weight the different descriptors in calculating the distance between occupations. I contribute to the literature by using the distance measure to provide facts about occupational mobility and by applying it to study the effects of UI on occupational mobility.

There is an extensive literature that studies the effects of UI on labor market outcomes. One strand is mainly concerned with the interaction of moral hazard and UI generosity, namely that workers receiving longer UI may decrease their search intensity. A large literature has, therefore, studied the effect of UI on e.g. unemployment duration and exit rates from unemployment. Notable papers include Shavell and Weiss (1979) and Hopenhayn and Nicolini (1997). See Nicholson and Needels (2006) for a summary.

On the other hand, another issue central to this literature is the realization that if there is heterogeneity of skill (requirements) among both workers and jobs, a frictional job search process can lead to mismatch between the skills of the worker and the needs of the job. Theoretical contributions exploring this effect includes Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000). Two strands of the UI literature study this effect empirically.

The first explores the effect of UI duration on re-employment wages, relying on wage changes as a proxy for match quality. Several papers have taken this approach, but the results are mixed. Several find positive and statistically significant effects of UI on re-employment wages.[2] On the other hand, a range of papers find no effects on wages.[3] Finally, Price (2018) finds modest effects on re-employment wages, while Schmieder, von Wachter, and Bender (2016) find a statistically significant negative effect.

The second strand of the literature explores how UI duration affects re-employment job tenure, proxying for mismatch by relying on the idea that "good matches endure" from Jovanovic (1979). Böheim and Taylor (2002), Centeno (2004), Tatsiramos (2009), and Caliendo, Tatsiramos, and Uhlendorff (2013) find results in the direction of longer UI duration giving longer re-employment tenure. Belzil (1995), who leverages a reduction in UI eligibility rules in Canada finds insignificant but positive effects of maximum UI duration on job tenure. Moreover, Card, Chetty, and Weber (2007), Van Ours and Vodopivec (2008), Portugal and Addison (2008) find either no effect, negative, or insignificant results.

Finally, in a recent important contribution, Chetty (2008) bridges the moral hazard and mismatch literature by showing that an increase in unemployment duration due to longer UI not only stems from reduced search intensity, but in large part also from a "liquidity effect". He finds that liquidity-constrained households react more sharply to increases in UI.

My paper directly contributes to this large literature by exploring the effects of UI on mismatch, as measured by a task-based distance between a worker's old and new occupation. To the extent that a larger distance between old and new occupation measures misalignment between the skill profile of a worker and a job, and is associated with a larger wage decline, my evidence suggests that longer UI reduces mismatch.

There is also a growing literature that studies the general equilibrium effects of UI. Contributions include Millard and Mortensen (1997), Shi and Wen (1999), and Krause and Uhlig (2012). These papers perform analysis based on estimation of structural models à la Mortensen and Pissarides (1994) to identify general equilibrium effects of UI policy. A recent contribution is that of Hagedorn, Karahan, Manovskii, and Mitman (2015), who use a novel empirical method to identify general equilibrium effects. They

[2] E.g. Burgess and Kingston (1976), Ehrenberg and Oaxaca (1976), Holen (1977), Barron and Mellow (1979), and more recently Caliendo, Tatsiramos, and Uhlendorff (2013) and Nekoei and Weber (2017).

[3] Classen (1977), Blau and Robins (1986), Kiefer and Neumann (1989), Addison and Blackburn (2000), Card, Chetty, and Weber (2007), Lalive (2007), and Van Ours and Vodopivec (2008).

leverage policy discontinuities at US state borders and avoid reliance on a fully specified structural model. They find that benefit extensions raise equilibrium wages, contract vacancy creation and employment, and ultimately raise unemployment. In a different approach, Chodorow-Reich, Coglianese, and Karabarbounis (2018) use random variation in UI duration generated by measurement error in reported unemployment rates to find only limited effects on macroeconomic variables.

While my paper does not deal with general equilibrium effects, the results of my empirical investigation point to two countervailing effects on job creation decisions. First, workers' reservation wages increase, which should decrease firms' job creation incentives. Second, workers may find jobs for which they are more productive, which should increase firms' job creation incentives.

2 Task-based occupational distance

When discussing occupational mobility one often thinks about a worker's cost of switching between different occupations. Realizing that occupations differ in what tasks and skills are necessary to perform them, it is natural to think that some occupation switches will be less costly for a worker. To capture that heterogeneity in switching costs and that occupational mobility is not binary, an existing literature relies on task-descriptors to create a proxy for these switching costs. I follow that literature and use the task-descriptors in the US Department of Labor's O*NET database to measure the degree of (dis)similarity between occupations.

The O*NET database provides a classification of 994 occupations in their occupation taxonomy O*NET-SOC 2010. Each occupation is classified in several dimensions, ranging from particular skills needed to values typically inherent to each occupation. O*NET labels each characteristic as an *element*, and places each element in a *domain*. The classification is performed by combining data from targeted job incumbents, occupational experts, and occupational analysts.

The database has been continuously developed since its release in 1998, when it re-

Table 1: Summary of O*NET classification

Domain	Elements	Example elements
Knowledge	33	Chemistry; Clerical; Fine Arts
Skills	35	Writing; Time Management; Quality Control Analysis
Abilities	52	Perceptual Speed; Multi-limb Coordination
Interests	6	Realistic; Artistic; Enterprising
Work Activities	41	Processing Information; Interacting With Computers
Work Context	57	Consequence of Error; Frequency of Conflict Situations
Work Styles	16	Leadership; Cooperation; Persistence
Work Values	6	Achievement; Recognition
Sum	246	

placed the Dictionary of Occupational Titles. I rely on the latest available version of the database, version 22.1 dated October 2017. From this version I use the complete domains given in Table 1. The table also gives the number and some examples of the elements in each domain.

All elements are measured on a given interval, but not all with the same bounds. Those elements that are not already scaled to $[0, 1]$ are rescaled. The literature has taken two approaches to treating the scores. The first approach uses raw dimensions.[4] The second uses some transformation of the characteristics.[5] I follow that second approach and apply Principal Components Analysis (PCA) to the matrix of occupation characteristics. Using these transformed dimensions, I calculate the distance between any two occupations i and j as

$$d_{ij}^F = \sum_{f \in F} \lambda_f |f^i - f^j|, \tag{1}$$

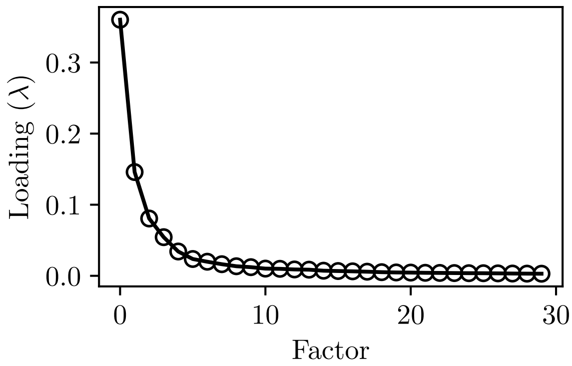
where F is the PCA transformed matrix of occupation characteristics, f^i is the value of factor f for occupation i, and λ_f is the fraction of variance in F explained by factor f.

The PCA transformation finds new factors that are linear combinations of the original variables. The factors are orthogonal to each other and, as such, each factor describes a unique dimension of an occupation. Using the fraction of explained variances of each

[4] See e.g. Gathmann and Schönberg (2010) and Cortes and Gallipoli (2018).

[5] See e.g. Ingram and Neumann (2006), Poletaev and Robinson (2008), and Yamaguchi (2012). With the purpose of computing a metric see e.g. Robinson (2011) and Guvenen, Kuruscu, Tanaka, and Wiczer (2018).

Figure 1: Explained variances (largest 30 factors)



factor as weights ensures that factors that are good in terms of distinguish occupations from each other are given a high weight, whilst factors that do not (e.g. a characteristic constant across occupations) get little weight.[6] Figure 1 shows the estimated weights. The first five factors explain 68 percent of the variance and the first 30, as plotted, explain 88 percent. The first two and the fourth factors all put greatest weight on *Oral comprehension* abilities, the third on *Social* interest, and the fifth on *Artistic* interest.

The smallest computed task-based distance, between two different occupations, is 0.0438 between *Biomass Power Plant Managers* and *Biofuels Production Managers*. The largest is 2.4686 between *Neurologists* and *Fallers* (lumberjacks).

Tables 2 and 3 show the nearest neighbors of two occupations, *Economists* and *Rotary Drill Operators, Oil and Gas*. Intuitively, some of the listed occupations should be similar in task and skill demands. This indicates that the task distance captures a degree of (dis)similarity between occupations.

Figure 2 plots the distances between all occupations using multidimensional scaling to reduce the occupation space to two dimensions. Each point is an occupation and is color coded according to which aggregate occupation group it belongs to in the 1990 CPS occupation classification. There is clustering by CPS aggregate occupational groups. For example, occupations within *Managerial Professional Specialty* cluster together. The clustering confirms that occupations that are classified as being close to each other in the

[6] Similar, but not identical, applications of PCA may be found in e.g. Poletaev and Robinson (2008), Robinson (2011), and Guvenen, Kuruscu, Tanaka, and Wiczer (2018). This paper's approach differs from earlier work in using all the factors and letting their explained variances determine their weight.

Table 2: Nearest occupations of *Economists*

Occupation	**Distance**
Actuaries	.0892
Business Intelligence Analysts	.1085
Environmental Economists	.1352
Operations Research Analysts	.1455
Climate Change Analysts	.1507
Biostatisticians	.1596
Market Research Analysts and Marketing Specialists	.1642

Table 3: Nearest occupations of *Rotary Drill Operators, Oil and Gas*

Occupation	**Distance**
Service Unit Operators, Oil, Gas, and Mining	.1397
Tank Car, Truck, and Ship Loaders	.1534
Millwrights	.1876
Farm Equipment Mechanics and Service Technicians	.1992
Sailors and Marine Officers	.2007
Mobile Heavy Equipment Mechanics, Except Engines	.2058
Roofers	.2101

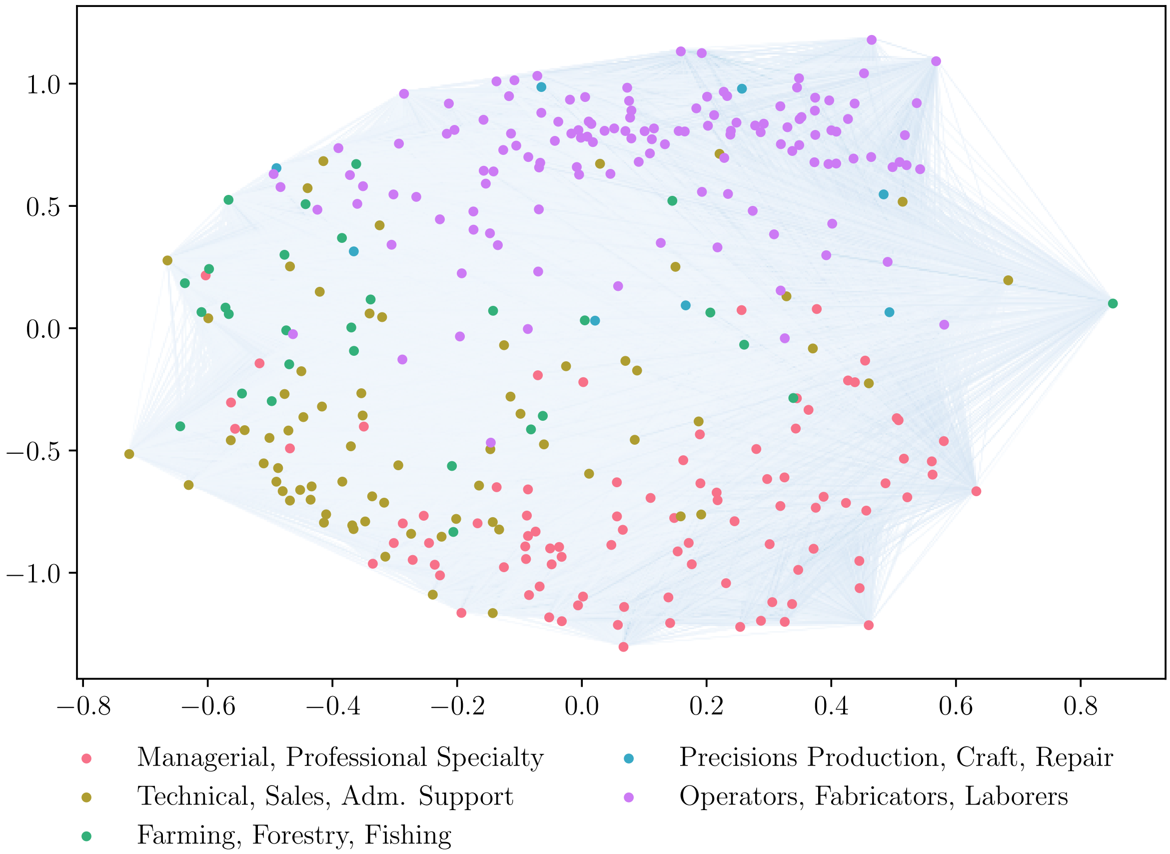
CPS, i.e. in the same aggregate group, are in fact also close according to the task-based distance.

2.1 Measurement issues

The metric is based on a weighted rectilinear norm, as in Guvenen, Kuruscu, Tanaka, and Wiczer (2018).[7] The rectilinear norm is symmetric (i.e. $d_{ij} = d_{ji}$). For firms' hiring decisions this implies that a worker can be overqualified, since being a unit of measure better at e.g. mathematics than the occupation requires results in an equal distance to being a unit worse at it. With respect to switching costs, this implies that a worker switching to a job requiring, for example, more maths has the same switching cost as moving to an occupation requiring symmetrically less maths. For some dimensions, such

[7] Some papers, e.g. Gathmann and Schönberg (2010) and Cortes and Gallipoli (2018), rely on the norm angular separation. They justify it by referring to the innovation and trade literature, but it appears to have two significant issues. First, the angular distance between two occupations with vectors $[2, 1]$ and $[20, 10]$ is zero since angular separation measures the angle between the vectors. Second, the distance between $[2, 3]$ and $[3, 2]$ is the same as between $[16, 24]$ and $[24, 16]$. Moreover, this is the same distance as between $[2, 3]$ and $[24, 16]$ (and vice versa). In the context of occupational switching, this means that switching to an occupation requiring proportionally more of everything (e.g. moving from a junior to senior position) incurs no switching costs, which is a priori not very satisfactory. This makes it difficult to compare distances between different occupation pairs.

Figure 2: Multidimensional scaling of occupations in the occupation space



as education, that may make sense. However, for others it may not. In addition, this simplifies calculations of the distances and, because the distance measure is then a proper metric, I can use techniques such as multidimensional scaling.[8]

Note that job-to-job (J-t-J) transitions that follow career ladders may be attributed a positive distance, as for example moves into managerial positions require less in-depth technical knowledge and more leadership skill. Such a career ladder move would mean that moves with positive distances would not reflect higher switching costs. However, climbing a career ladder move is relevant for J-t-J transitions, but much less so for unemployment-to-employment (U-t-J) transitions, since moves to unemployment tend to be associated with a resetting of the career ladder.

The metric is not invariant to the p-norm chosen when using the proposed weights. Therefore, in the appendix, I compute and report robustness results using a skill distance based on the rectilinear norm without applying PCA and also using the Euclidean norm.

2.2 Task-based distance and occupational transitions

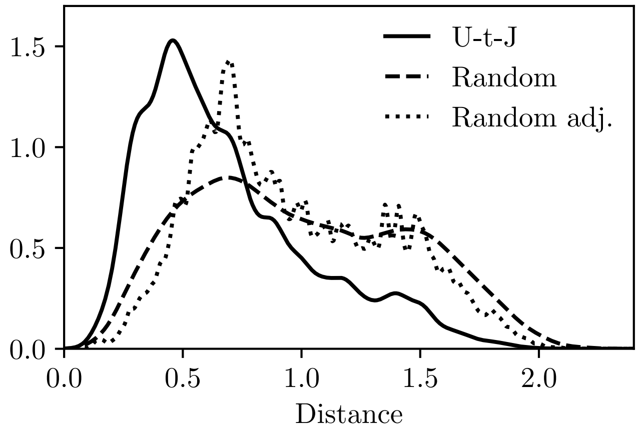
I use a sample of employment transitions from the Current Population Survey (CPS), described in more detail in Section 3, to show a set of facts about occupational switching using the task-based distance. These facts also show that the task-based distance captures relevant information about observed occupational transitions and hence serves as a good proxy for occupational switching costs. Appendix B presents additional facts.

2.2.1 Distribution of distances

First, I estimate densities for task-based distances on the sample of observed job transitions. Figure 3 compares the density of U-t-J transitions with a counterfactual density based on workers getting and accepting job offers to random occupations. The solid line plots the density of observed U-t-J transitions in the CPS. The dashed line is a counterfactual density if there were random transitions in the economy with equal probability

[8] I use the rectilinear norm as a benchmark measure since some evidence from observed job transitions using the DOT skills characteristics indicate that there is always a larger deviation in at least one characteristic. A higher exponent in the p-norm would punish such deviations with a disproportionately larger distance. For robustness I also show results with the Euclidean norm in Appendix C.

Figure 3: Observed job transitions in the CPS and counterfactuals



Note: Densities are conditional on switching, estimated with Gaussian kernels, and bandwidth selected using Scott's rule (Scott, 1992) independently for each density.

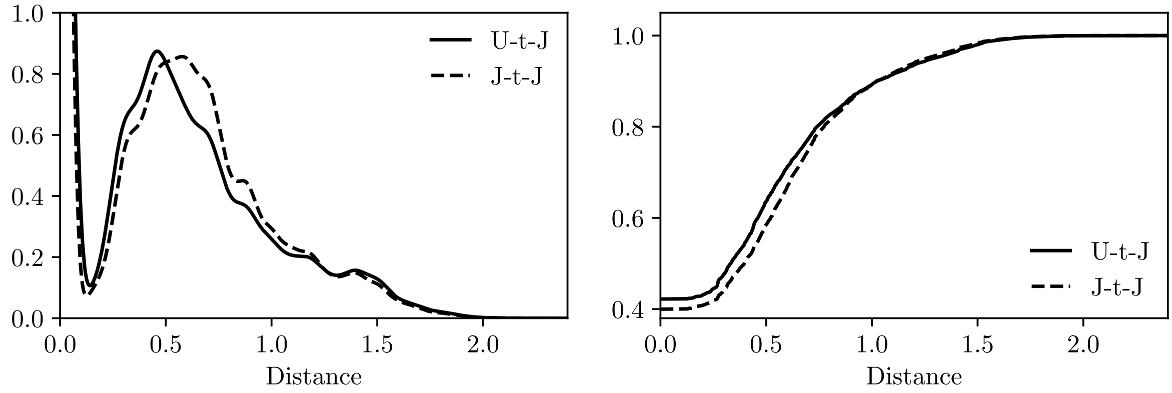
of moving from any one occupation to another. Hence, this is also the distribution of distances between all occupations. The dotted line is constructed in the same way as the previous, except that the transition probabilities are given by each occupation's share of total employment. The observed density differs substantially form the other two plots. Therefore, observed transitions do not appear to be random with respect to the distance measure. Indeed, the distribution is more left skewed, indicating that workers switch to jobs that are close relative to their previous occupation. Moreover, 42.2 percent of observed moves are within occupation compared to 0.32 percent if moves are random, providing further support to individuals searching for jobs of the same or close occupations.[9]

Figure 4a shows two kernel density estimates. Again, the solid line is a density estimate of observed U-t-J in the CPS sample. The dashed line plots the density of observed J-t-J in the CPS, of which 39.9 percent are within occupation.[10] J-t-J transitions appear very similar to U-t-J transitions. With two small differences. First, there are more within occupation moves for J-t-J compared to U-t-J. Second, given a move, there is more mass for shorter moves for U-t-J, which may reflect career job ladder moves in J-t-J transitions. Overall, however, the two distributions are very similar, as shown in Figure 4b.

[9] 1.47 percent of transitions are within occupation given random moves by employment shares.

[10] J-t-J transitions are identified by workers reporting having switched employer since the last interview.

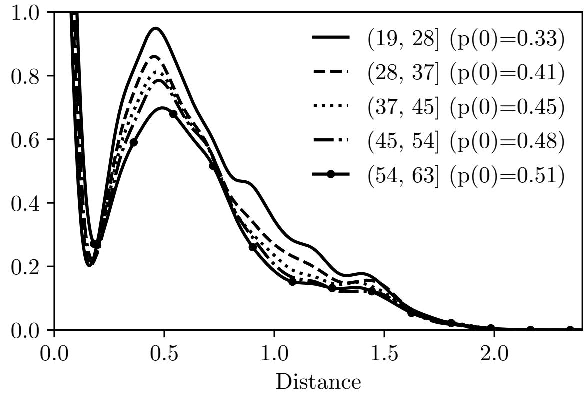
Figure 4: Observed job transitions in the CPS



(a) Empirical PDF (b) Empirical CDF

Note: The densities are estimated with Gaussian kernels and bandwidth selected using Scott's rule (Scott, 1992) independently for each density.

Figure 5: Observed job transitions in the CPS: Densities by age group (U-t-J)



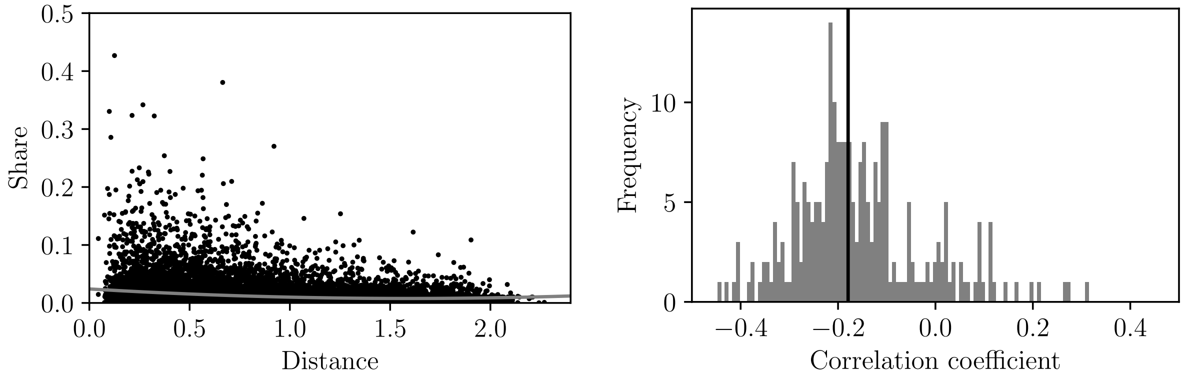
Note: The densities are estimated with Gaussian kernels and bandwidth selected using Scott's rule (Scott, 1992) independently for each density.

2.2.2 Age and distance

Figure 5 plots five densities estimated by bins of age, for U-t-J transitions. It is interesting to note that the densities become more left skewed with age. This is consistent with Fredriksson, Hensvik, and Skans (2018), who showed that there is more mismatch and learning about own skills among the inexperienced. Similarly, Guvenen, Kuruscu, Tanaka, and Wiczer (2018) argue for workers learning about own skills over time and eventually converging to their *ideal* occupation.[11]

[11] In the appendix, a linear regression shows that the coefficient of age on distances in observed U-t-J job moves is negative.

Figure 6: Correlations of occupation move shares and task-based distance



(a) Scatter plot (b) Histogram

Note: An observation in the left panel is the share of moves to occupation y from occupation x along with the distance between them. An observation in the right panel is the correlation of said shares for a given occupation y and their distances. Shares with less than 30 moves to the destination occupation in total are dropped. Subplot (a) is invariant to this choice, and subplot (b) is unaffected as long as you drop those with no less than 10 transitions.

2.2.3 Flow shares and distance

Another way to see that workers tend to move close to their original occupation is to consider the correlation between the distance and fraction of observed moves between two occupations. Let s_{ij} be the share of observed transitions to occupation i that come from occupation j. In that case, the task-based distance should correlate negatively to the share s_{ij}, so that moves to an occupation are predominately from occupations that are close to it. Figure 6a shows a scatter plot of all such shares, s_{ij}, against the distance between them, d_{ij}. There is a clear negative relationship overall. The correlation coefficient between the shares and distances is $-.179$ ($p = .000$). Figure 6b plots the distribution of correlation coefficients between the shares and distances, when the correlations are calculated for each destination occupation. The solid vertical line is the aggregate correlation. For most destination occupations there is a negative correlation, and for some occupations the correlation is as low as $-.449$.

2.2.4 Re-employment wages and distance

I next show that a larger distance is associated with a lower re-employment wage. I use the Displaced Worker Supplement (DWS) of the CPS. This is a biennial supplement that asks additional questions to workers who have experienced displacement, including collecting details on their wage.[12] I relate the current wage of an individual who experienced job displacement in the recent past to my distance measure, estimating the following regression

$$\log w_{ismt} = \beta_0 + \beta_1 d_i + \zeta_{sm} + X_i + \varepsilon_{ist} \tag{2}$$

where w_{ismt} is the worker's current wage, d_i is the distance between the displaced and current occupation, ζ_{sm} is a set of state and time fixed effects, and X_i are worker specific control variables.

Table 4: Log of hourly re-employment wages and occupational distance in the DWS

	(1)	(2)	(3)	(4)	(5)	(6)
Skill-based	-0.2153***	-0.2125***	-0.1462***	-0.1417***	-0.1339***	-0.1419***
distance	(0.0135)	(0.0118)	(0.0098)	(0.0086)	(0.0090)	(0.0079)
Demographics	No	Yes	Yes	Yes	Yes	Yes
From occupation FE	No	No	Yes	Yes	No	Yes
To occupation FE	No	No	Yes	Yes	Yes	Yes
State FE	No	No	No	Yes	Yes	No
Month FE	No	No	No	Yes	No	No
To occ. × month FE	No	No	No	No	Yes	No
State × month FE	No	No	No	No	No	Yes
Observations	9,055	9,051	8,997	8,997	8,205	8,992
Adjusted R^2	0.037	0.219	0.450	0.606	0.602	0.609

Note: Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Each observation is an individual currently employed, but has experienced displacement in the recent past. The left-hand side variable is the wage in the current job. The skill-based distance is a measure of dis-similarity of the displaced and current occupation, with respect to the skill requirements of those jobs. Demographic controls includes age, age squared, sex, race, marital status, and education. Occupation fixed effects are occupation dummies for both from and to occupation.

Table 4 presents the regression results. These show that a larger change in occupation is associated with a lower current wage. More specifically, compared to other displaced workers arriving in the same occupation, those that switched to it from an occupation further away have lower wages than their colleagues. For example, an individual occupied as a *Steel Worker* switching to being a mail man has an 8.2 percent lower hourly wage compared to the re-employment wage of a displaced worker that did not switch

[12] Appendix F gives details on the sample and shows that it is representative for my full CPS sample in terms of observed job switches. The DWS is not rich enough for me to link a wage to the UI availability during unemployment spells.

occupations. In Appendix B.1, I extend this analysis using wage data from the Outgoing Rotation Groups in the CPS, to show that this effect is significantly more negative for U-t-J transitions than for J-t-J transitions.

As the DWS contains workers that recently experienced job displacement, the current wage is likely related to the re-employment wage after displacement. If lower re-employment wages are associated with larger mismatch, as assumed in parts of the mismatch literature, these results show that a larger task-based distance is associated with larger mismatch. For robustness, Table 17 in the appendix reports the same regression where the sample is limited to those who were displaced in the previous year, and a sample where the individual is still in her first post-displacement job.[13]

3 Data and empirical strategy

The Current Population Survey (CPS) is a rotating survey giving a representative sample of US households.[14] Households are interviewed for four consecutive months, then after a four months break they are interviewed for another four months before exiting the survey. The CPS records detailed information on employment and demographic variables.

Using individuals' reported employment status I identify those who move from unemployment at time t to employment at $t+1$. Observed sequences consisting of unemployed-missing-employed are discarded. Sequences of unemployed-not in labor force-employed are included in the main specifications, but I also report results where also these sequences are discarded. As a benchmark, I consider only unemployed who are job losers.[15] I restrict the sample to those not self-employed and not in the military. I restrict the sample to those aged 18-64 and follow Rothstein (2011) by further restricting the sample to unemployed individuals with up to 99 weeks of unemployment. Discarding sequences with missing observations leaves 75,031 observed transitions for 71,440 unique individuals for the period 1989-2018. Table 5 gives summary statistics of the main variables for this

[13] The table also reports results with controls for unemployment duration. Also in the appendix, Table 18 reports results for nominal, and CPI and GDP deflated wages.

[14] I use the IPUMS-CPS database to access the CPS (Flood, King, Ruggles, and Warren, 2017).

[15] Reason for unemployment is not reported when an individual is not in labor force. For sequences including not in labor force observations I may, therefore, not be able to remove the job leavers.

Table 5: Summary statistics of U-t-J transitions in CPS sample

	Average	St.dev.
Age	38.43	11.96
Unemployment duration (weeks)	11.19	14.24
Occupation distance (d^F)	0.39	0.43
Share same 3-digit occupation	0.43	0.50
Share same 2-digit occupation	0.47	0.50
Share same 1-digit occupation	0.60	0.49

	Percentage
Female	39.26
Education:	
High school >	16.83
High school	38.82
High school <	44.34

sample.

The 1990 occupation code in the CPS data reports the current occupation if employed, or if not employed it reports the occupation of the respondent's last held job. I match the occupation reported whilst unemployed and employed to the task-based distance via the 1990 census codes harmonized by Autor and Dorn (2013) (*occ1990dd*).[16] Harmonizing occupation codes leaves 74,996 observations, of which I have occupational distance for 71,200.[17] Figure 4a shows a density estimate of the observed distances for the transitions in the CPS sample.

To identify the effect of UI on occupational mobility I exploit differences in UI availability at the individual level, as in Rothstein (2011), Farber and Valletta (2015), and Farber, Rothstein, and Valletta (2015). I use the simulated availability of unemployment benefits to the unemployed by Farber, Rothstein, and Valletta (2015), who give weeks of UI for a worker in any state and month conditional on when he became unemployed, and hence also conditional on his unemployment duration. The data covers the years 2004 through 2014, capturing the expansion, expirations, re-authorizations, and contraction of UI surrounding the Great Recession of the late 2000s. Variation in UI comes from two federal programs in the United States that extended basic UI of 26 weeks up to 99 weeks in some states. First, Extended Benefits (EB) and then Emergency Unemployment Compensation (EUC).[18]

[16]I thank Destin Royer for sharing his crosswalk file for O*NET SOC 2010 to Census Occupation Codes 1990dd (*occ1990dd*).

[17]If multiple O*NET occupation pairs map into the same harmonized occupation pair, I use the average for original occupation codes that differ and zero if identical.

[18]The following discussion relies heavily on Rothstein (2011) and Fujita et al. (2010).

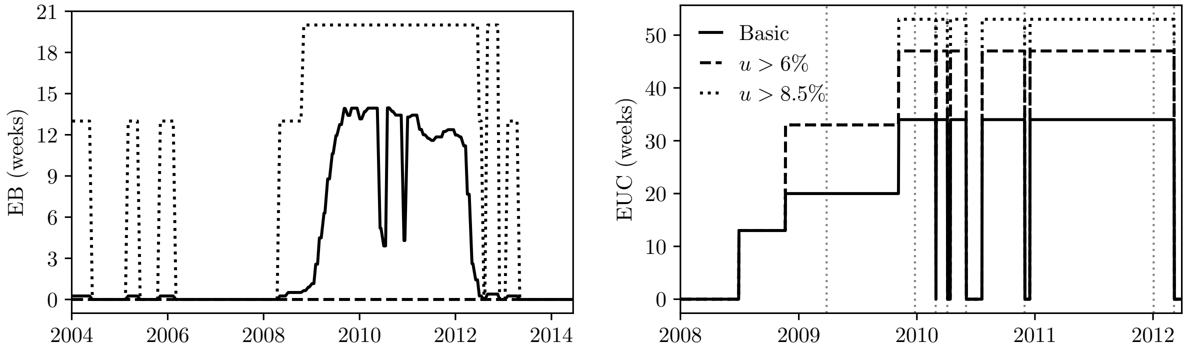
Extended Benefits (EB) was a preexisting UI extension program that provided additional weeks of UI in states with elevated unemployment rates. There are three sources of variation in UI from this program. First, states had the choice of whether or not to participate. Before the American Recovery and Reinvestment Act of 2009, states had to bear a significant part of the cost. Indeed, before 2009 only three states participated in the program, whilst in July the same year 32 more states had opted in. Second, after choosing to participate, states had to choose between either 13 or 20 weeks of extra UI. Third, the states had to select a trigger rule for when EB benefits would kick in. Many states adopted strict trigger rules, so that EB benefits were not available in practice. Figure 7a shows the evolution of EB benefits that were in action (i.e. states had opted in and met the trigger rule) as the maximum, average, and minimum across states.

The EB program generates within state variation as they are triggered. Moreover, they generate cross-sectional variation through states' choice of participation and trigger rules.

Emergency Unemployment Compensation (EUC) was first introduced by Congress with an additional 13 weeks in June 2008. In successions, Congress expanded the program from 13 weeks in June 2008 to additional tiers of 20, 14, 13, and 6 weeks. The exact eligibility rules for each tier changed slightly over time. For most of the recession there was basic coverage that applied to all states, along with two additional tiers that were only available in states with unemployment rates above 6 and 8.5 percent. For some states the EUC program provided up to 53 weeks in addition to any preexisting coverage. Figure 7b shows how the availability of EUC evolved over time.

The EUC extensions were only in place for short periods. The vertical lines denote scheduled expirations. Re-authorization and further extensions of the program were often done just before scheduled expiration, making it difficult for the unemployed to predict and expect the continuation of extended UI. On three occasions, the program came to an end before Congress would agree on re-authorization. In June 2010, it took one and a half months to reauthorize the program. The EUC program generates within state time

Figure 7: UI extensions throughout the Great Recession



(a) EB (b) EUC

Note: Panel (a) shows the availability of EB as the maximum, average, and minimum across states over time conditional on actual unemployment rate. Generated from simulated UI availability of Farber, Rothstein, and Valletta (2015). Panel (b) shows the evolution of the EUC program.

variation through expiration and re-authorization. It also generates cross state variation through differences in state unemployment rates.

Finally, in the event of program expiration individuals who had already started on a tier, were allowed to complete that tier. Upon extension or re-authorization, the program paid retroactive benefits.

An important implication of the structure of the extensions is that workers who are unemployed for longer receive more UI. Workers who remain unemployed longer mechanically have a higher probability of being covered by extensions and re-authorizations. If there is dynamic selection into longer unemployment durations, this could generate a positive correlation between distance and UI. Accounting for the correlation between UI and unemployment duration will then be important for part of the empirical analysis in section 3.1.

It is not clear how to model workers' expectation of UI duration available to them. First, as each expansion and re-authorization were highly controversial it seems unlikely that worker's would expect these. Moreover, because benefits paid retroactively are much less valuable for a liquidity constrained worker it seems unlikely that a worker would take into account a possible extension or re-authorization. Thus, when simulating UI availability Farber, Rothstein, and Valletta (2015) assume that workers do not foresee EUC extensions or trigger events.

To illustrate the importance of the expectations and the variation it generates, consider the following example. In Alabama in June 2011, an individual that had been unemployed for 16 weeks expected to enjoy a total of 66 weeks of UI; 26 weeks of regular benefits, 20 weeks of EUC tier 1 benefits, and finally 20 weeks of EB benefits. EUC was set to expire on January 3, 2012, and this worker did, therefore, not expect to be able to draw out the remaining EUC tiers, as they would expire before she could start them. Compare that to an individual with an unemployment duration of 17 weeks. He expected to enjoy a total of 80 weeks of UI. He would start EUC tier 2 before expiry and, therefore, enjoy an additional 14 weeks of UI. Finally, compare that to an individual of 31 weeks of unemployment duration. This worker would, in addition, enjoy EUC tier 3 benefits of 13 weeks, for a total UI duration of 93 weeks.

As the example illustrates, this variation allows me to include state by month fixed effects to control for effects of local labor market conditions. The remaining variation in UI then stems from differences in when individuals became unemployed, and, hence, their unemployment duration.

Matching the CPS sample to Farber, Rothstein, and Valletta (2015)'s simulated UI availability for the period 2004-2014 leaves 33,325 observations. Unless otherwise stated, regressions are estimated only for individuals that have not exhausted benefits since there should be no effect of UI on those who have.

3.1 Model specifications

To identify the effect of UI on occupational mobility I estimate the following regression.

$$d_{istm} = \beta\tau_{stm} + \zeta_{sm} + X_i + \varepsilon_{istm}, \tag{3}$$

where d_{istm} is observed distance in transition for individual i, τ_{stm} is the total number of UI weeks available to unemployed individuals in state s in month m with unemployment duration t, ζ_{sm} is a vector of month-by-state fixed effects, and X_i is a vector of individual control variables.[19] β will then give the average effect of one more week of UI on occupational mobility, as measured by the task-based distance. In the subsequent analysis, the task-based distance (d) will be standardized by dividing it by its own standard deviation in observed unemployment-to-employment transitions (see Table 5).

State-by-month fixed effects are included in all specifications to absorb the effects of time varying state labor market conditions. If local labor market conditions are not constant within these groups, this might bias my estimates. It is not clear in which direction the bias affects my results, as economic models can predict both mismatch increasing and decreasing in recessions.

Abstracting from this possible bias, the remaining variation in UI should then stem from the variation in workers' unemployment duration. Controlling for unemployment duration will account for the positive correlation between UI and unemployment duration discussed previously. However, this might lead to a bias in β caused by over-controlling. Therefore, I also estimate equation (3) using a sample that includes UI ineligible individuals as a control group. These individuals have $\tau = 0$, but any unemployment duration. The non-UI eligible serve to absorb the effects of unemployment duration by weakening the correlation between UI and unemployment duration present in the group of UI eligible workers.[20,21]

Finally, if dynamic selection effects are indeed important, then the true effect of UI should be measured at shorter unemployment durations. To see this, consider a simple model with two types of workers: a *normal* type, and a *low* type that, for some reason, will be unemployed for a prolonged period of time. As t increases and *normal* types exit the pool of unemployed, the pool will gradually be composed of a higher fraction of *low* types. To the extent that the effect of UI differs between the two groups and there is a mechanical correlation between UI and unemployment duration (and, hence, type),

[19] The regressions are not weighted.

[20] I assume that those labeled as "job loseres" in the CPS are eligible, whilst "job leavers" are not.

[21] These two groups might differ on important variables. For example, job leavers are likely to have weakly better liquidity to remain unemployed, or could considers themselves to have good employment prospects. Appendix D compares summary statistics for the two groups, and indeed finds they are different. However, as this approach to dealing with the UI-unemployment duration correlation suffers from different limitations than when controlling linearly for unemployment duration, I consider it to be a useful exercise.

the dynamic selection can distort the measured effects of UI, as the pool asymptotically consists of only *low* types. To investigate this empirically, I estimate the effect of τ on distance by unemployment duration. Specifically,

$$d_{istm} = \sum_{g} \beta_g \tau_{stm|t\in g} + \zeta_{sm} + X_i + \varepsilon_{istm} \tag{4}$$

where β_g is the parameter for τ for individuals who have unemployment duration in bin g. If dynamic selection effects are present and the true effect of UI is to lower distances, we should find that β_g is negative and significant for small g's, but become larger and potentially insignificant as g increases.

4 Results

Table 6, columns (1) and (2) present the regression results using the main sample described in the previous section. The coefficient for weeks of UI is negative and significant across all specifications, showing that longer UI makes individuals find jobs closer to their original occupation. Ten more weeks of UI corresponds to a decrease in (non-standardized) distance of .012, according to specification (2). To give an example of what this effect means in terms of actual occupational switching, consider the following. For 20 weeks of additional UI, it corresponds to a *Ship Engineer* becoming an electrician, rather than a *Commercial driver*. In the trough of the Great Recession (May 2009), the newly unemployed had on average 30 more weeks of UI available across all states. For an unemployed worker previously occupied as a *Real Estate Agent*, this amounts to being hired as a *Claims Examiners, Property and Casualty Insurance* rather than a *Lodging Manager*. Or, a *Securities and Commodities Trader* may become a *Credit Authorizer* rather than a *Librarian*.

Next, column (3) includes non-UI eligible in the sample. The non-UI eligible serve as a control group that absorbs the effect of unemployment duration by weakening the correlation between UI and unemployment duration. The estimated coefficient is similar to that in the baseline specification, suggesting that the bias from (over-)controlling for

Table 6: UI duration on standardized distances

	(1) Baseline 1	(2) Baseline 2	(3) Non-UI eligible	(4) 2004-2010
UI duration	-0.0032**	-0.0029**	-0.0014***	-0.0057***
	(0.0015)	(0.0014)	(0.0002)	(0.0017)
State × month FE	Yes	Yes	Yes	Yes
Unemployment duration	Yes	Yes	No	Yes
Demographics	Yes	Yes	Yes	Yes
Occupation FE	No	Yes	Yes	Yes
Observations	30,846	30,831	39,793	20,252
Adjusted R^2	0.045	0.163	0.148	0.164

Note: Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Demographic controls includes age, age squared, sex, race, marital status, and education. Unemployment duration is a linear term of weeks of unemployment. Occupation fixed effects are occupation dummies for both from and to occupation. Each observation is an individual who has found a job. The left-hand side variable is a measure of dis-similarity of the pre- and post-unemployment occupation, with respect to the skill requirements of those jobs. It is standardized by dividing by its own standard deviation of observed U-t-J transitions (see Table 5). UI duration is the total number of UI weeks the worker expected to receive in the week finding a job, as simulated by Farber, Rothstein, and Valletta (2015). All columns use data for full sample years 2004-2014, except column (4) which is limited to 2004-2010.

unemployment duration is small.

Finally, column (4) reports results when estimated on data for the years 2004-2010, capturing the peak through the trough of the recession with the UI expansions and expirations.

Table 7 reports additional robustness results. In column (1) I drop all individuals who are less than 35 years of age, as an additional control for any learning effects. Fredriksson, Hensvik, and Skans (2018) showed that learning is most pronounced for workers with less than five years of work experience. Setting the threshold at 35 years should also account for any time spent in education, in addition to the five years. The estimate is slightly larger in magnitude than similarly specified regressions from the main table, implying that older workers end up in jobs more similar to their previous job's occupation.

In the next specification, I drop all transitions with a not-in-labor force observation prior to becoming employed. Column (3) keeps only those observations where the individual remains employed for at least two consecutive months. Columns (4) and (5) uses the rectilinear norm and the Euclidean norm to calculate the task-based distance, respectively.

Finally, in column (6) I perform a placebo test by assigning UI to non-UI eligible as if they were eligible and estimate the model on those workers only.[22] As expected, this yields a non-significant coefficient.

Table 7: UI duration on distances, robustness

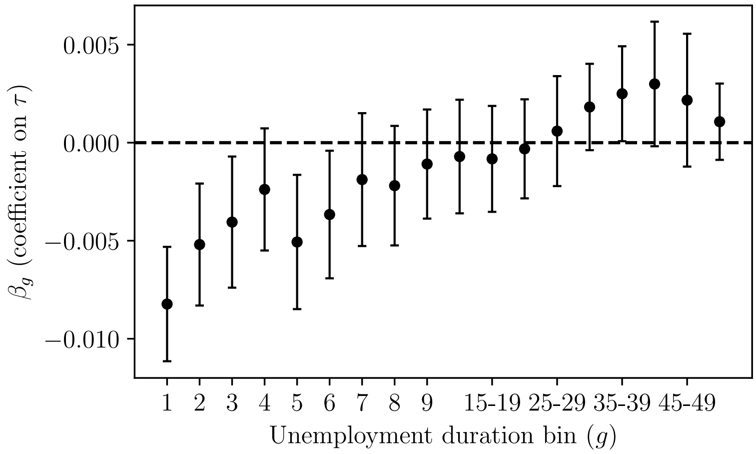
	(1) Age ≥35	(2) Drop nilf	(3) Emp. ≥2m.	(4) Rect.	(5) Eucl.	(6) Placebo
UI duration	-0.0040**	-0.0029**	-0.0025*	-0.0407**	-0.0036**	0.0030
	(0.0020)	(0.0013)	(0.0013)	(0.0193)	(0.0016)	(0.0037)
State × month FE	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment dur.	Yes	Yes	Yes	Yes	Yes	Yes
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	17,170	31,568	18,306	32,658	32,658	4,509
Adjusted R^2	0.169	0.164	0.174	0.186	0.191	0.107

Note: Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Demographic controls includes age, age squared, sex, race, marital status, and education. Unemployment duration is a linear term of weeks of unemployment. Occupation fixed effects are occupation dummies for both from and to occupation. Each observation is an individual who has found a job. The left-hand side variable is a measure of dis-similarity of the pre- and post-unemployment occupation, with respect to the skill requirements of those jobs. In columns (1) through (3) and (6), is standardized by dividing by its own standard deviation of observed U-t-J transitions (see Table 5). UI duration is the total number of UI weeks the worker expected to receive in the week finding a job, as simulated by Farber, Rothstein, and Valletta (2015). All columns use data for full sample years 2004-2014. Column (1) drops those below 35 years. Column (2) ignores not-in-labor-force observations. Column (3) keeps only those observations where the individual remains employed for at least two consecutive months. Columns (4) and (5) uses the rectilinear norm and the Euclidean norm to calculate the skill-based distance, respectively.

Figure 8 shows estimates of β_g from equation (4). As expected, effects are most prominent at shorter unemployment durations, indicating that dynamic selection effects are indeed important, and that controlling for unemployment duration in equation (3) is warranted.

[22] A similar exercise is done by Rothstein (2011) and Farber and Valletta (2015), who originally built on Valletta and Kuang (2010).

Figure 8: Distance and UI duration by unemployment duration



Note: The 95% confidence intervals are based on standard errors clustered on state. The figure shows the estimates of β_g in equation (8). Each observation is an individual who has found a job. The left-hand side variable is a measure of dis-similarity of the pre- and post-unemployment occupation, with respect to the skill requirements of those jobs. UI duration is the total number of UI weeks the worker expected to receive in the week finding a job, as simulated by Farber, Rothstein, and Valletta (2015). The effect of UI duration on distance is estimated by bins of unemployment duration (g), as reported by the survey respondent in the week finding a job. Demographic controls include age, age squared, sex, race, marital status, and education. Time fixed effects are state-by-month. Occupation fixed effects are occupation dummies for both from and to occupation. The full sample (2004-2014) is used.

5 Calibration exercise

The empirical estimates may be affected by, for example, non-linear effects, insufficient controls for local labor market conditions, or using unemployment duration as a control variable. Therefore, to better understand the estimated effects of UI generosity, I perform a simple calibration exercise using a standard search model as in McCall (1970) augmented with costly occupational switching and time limited unemployment benefits. I first present the model, and then proceed to show the parametrization in section 5.2 and the results in section 5.3.

5.1 Theoretical framework

An infinitely lived hand-to-mouth worker maximizes lifetime utility

$$\mathbb{E}\left[\sum_{t=0}^{\infty}\delta^t u(c_t)\right]$$

where δ is a discount factor. At $t = 0$, the worker is unemployed. Whilst unemployed, the worker receives unemployment benefits giving direct utility $b > 0$ and an allowance from the household $h > 0$, which also can be considered as containing the flow value of leisure. The unemployment benefits are received for $t \leq \tau$. With probability γ, the worker receives an offer to work in a job with distance d, where a greater d indicates a higher occupational switching cost. The ideal job, with no switching cost, is given by $d = 0$. If he accepts the offer he enters the next period as employed, and remains employed forever.

After accepting a job the worker gets $V_t(d)$, which is stationary so that $V_t(d) = V_{t+1}(d) = V(d)$ for all t. To capture that d is a switching cost, I assume that $V(d)$ is decreasing in d. Finally, d is distributed according to a probability density function, $f(d)$.

Whilst unemployed, the worker's problem is summarized by the non-stationary Bellman equation

$$U_t = h + \mathbb{1}_{t \leq \tau} b + \gamma \delta \mathbb{E} \max \{V(d_t),\ U_{t+1}\} + (1 - \gamma)\delta U_{t+1}. \tag{5}$$

The optimal search behavior of the worker is summarized by a reservation distance, d_t^R, such that the worker accepts any job offer with a distance below it.[23] Figure 9 illustrates the reservation distance as a function of time. Consider an individual with UI lasting τ weeks, given by the dashed line in the figure. He starts out with a low reservation distance, meaning he is more *picky* and only accepts jobs with a low distance. If he remains unemployed, he becomes less and less picky as time progresses. After exhausting UI at τ weeks his reservation distance becomes constant. Consider another individual with $\tau' > \tau$ weeks of UI, given by the solid line. She starts out being even more selective, and remains more picky at all unemployment durations up to exhausting UI. Thereafter, she has the same reservation distance as the first worker.

The model predicts that longer UI makes workers search for jobs that are closer to their *ideal* occupation for a longer time. If the previous occupation of an unemployed worker is close to his ideal occupation, we should expect to see that individuals with

[23] I characterize the reservation distance, d_t^R, in Appendix A.

Figure 9: UI and reservation distance in a model

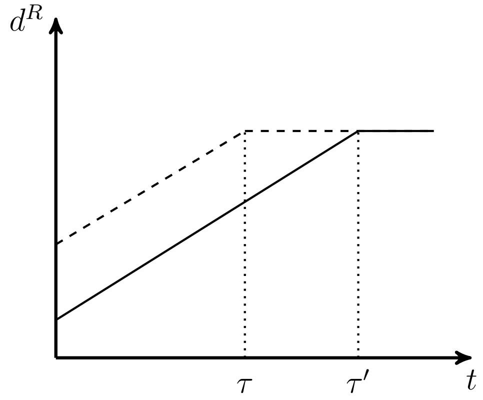


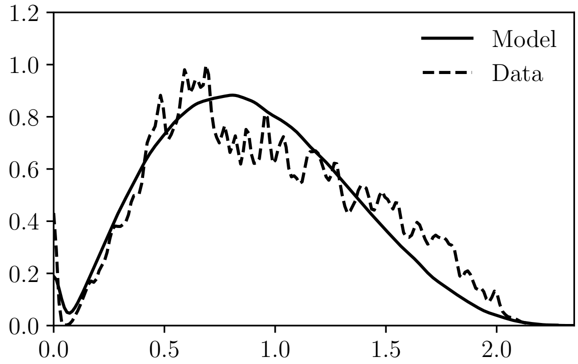
Table 8: Parametrization in calibration exercise

Parameter	Value	Description	Calibration target
p	0.0147	Probability of drawing same job	Distribution of jobs
α	2.4	Beta distribution shape	Distribution of jobs
β	4	Beta distribution shape	Distribution of jobs
$\overline{d}$	2.35	Beta distribution scalar	Largest d
$\underline{d}$	0.045	Beta distribution scalar	Lowest $d > 0$
w_1	2.0648	Wage function (max salary)	$w_1 = \beta_0^{\text{dws}}$
w_2	-0.1653	Wage function (slope)	$w_2 = \beta_1^{\text{dws}}$
δ	0.999	Discount factor	Yearly .95
h	0.42745	Household stipend	$h = 0.25\mathbb{E}[w]$
b	0.8549	Unemployment benefits	$b = 0.5\mathbb{E}[w]$
γ	0.3	Probability of job offer	

longer UI end up in jobs closer to their previous occupation. On the other hand, they will also search a longer time on average.

In the data, the previously held job of a worker might not correspond to her *ideal* occupation. In that case, the previous occupation will be a bad proxy for a worker's ideal occupation. However, my data covers job losers during the Great Recession, when arguably good matches were destroyed at a higher rate. Furthermore, the estimated wage effects from section 2.2 supports this assumption by showing that workers with a larger task-based distances got lower re-employment wages. Moreover, the empirical results show that the effect of UI on the task-based distance is as strong for workers over 35.

Figure 10: Calibration of offer distribution



5.2 Parametrization

Table 8 shows the parameterization of the model. The parameters are calibrated assuming $\tau = 26$. The offer distribution is set to match the distribution as if workers randomly draw offers with probabilities that equal the share of employment of each occupation, as given by the CPS sample. That distribution has a mass point at zero, then a dip followed by a right skewed distribution. Let the cumulative distribution function of the offer distribution be given by

$$F(d) = p + (1-p)\left(\underline{d} + (\overline{d} - \underline{d})G(d;\alpha,\beta)\right) \tag{6}$$

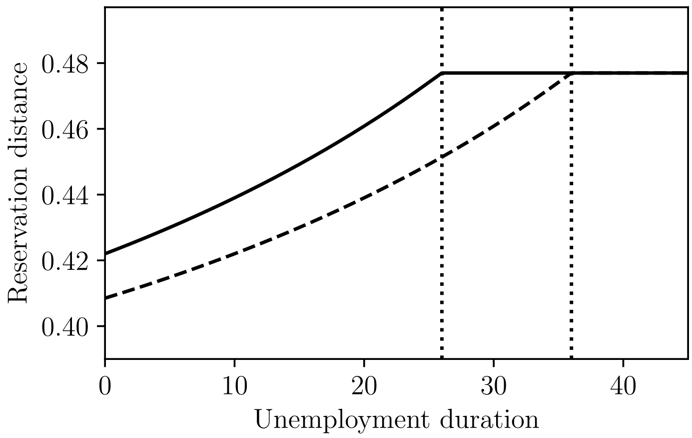
where p is the probability of drawing the same occupation, and G is a beta distribution with shape parameters α and β. p is set equal to the probability of drawing the same occupation by chance, 1.47 percent. The two latter parameters are set to match the shape. Figure 10 shows kernel density estimates of the calibrated and target distributions.

I assume linear utility in wage. The wage function is

$$w(d) = w_1 \exp\{w_2 d\}, \tag{7}$$

so that I can directly use the estimated coefficients from Section 2.2 on the link between distance and the re-employment wage. I set w_2 equal to the average of the estimated slopes in Table 4. Similarly, I set w_1 equal to the average of the fixed effects from the samle table. This gives $\mathbb{E}[w] = 6.7992$. Finally, I follow Chetty (2008) in setting $h = 0.25\mathbb{E}[w]$

Figure 11: Reservation distance for $\tau = 26$ (solid) and $\tau = 36$ (dash)



and $b = 0.5\mathbb{E}[w]$.

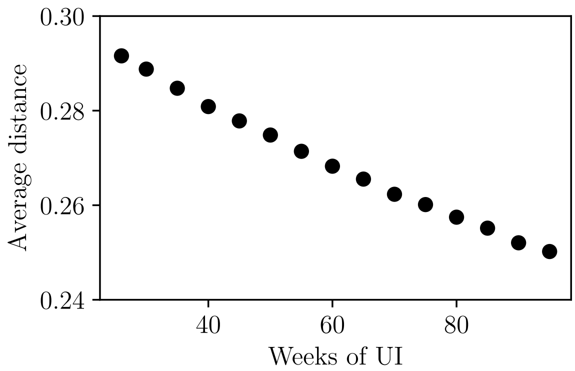
Figure 11 shows the reservation distance as a function of unemployment duration, t, for $\tau = 26$ and $\tau = 36$ weeks of UI. Whilst receiving UI the worker accepts jobs that are relatively close. When nearing exhaustion of UI, he begins accepting less suitable jobs.

5.3 Results

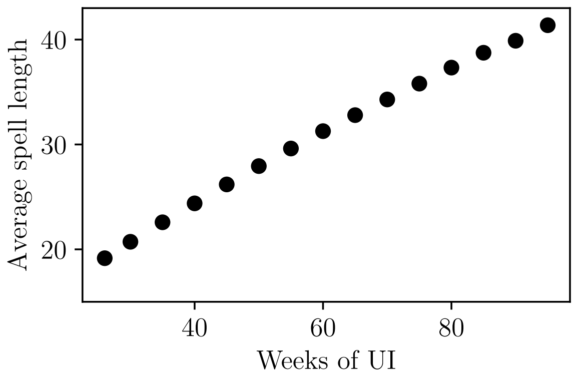
Figure 12 shows, for a range of weeks of UI, the average distance of accepted jobs and the average unemployment duration when accepting them. The relationships appear almost linear, with a slope of -0.0006 and 0.3045 for distance and duration, respectively. The effect of UI on distance is very similar in size to some of the estimates from the empirical regressions, which, when not standardized, ranged from -0.0008 to -0.0012. The slope of UI on unemployment duration implies that a one week increase in UI lengthens unemployment spells by 0.3045 weeks. This is higher than typical estimates, for example Moffitt (1985) finds that an increase in UI duration from 26 to 27 weeks lengthens spells by 0.15 weeks.

The direction of the effects are given by the structure and assumptions of the model, and does not depend on the calibration itself. However, the size of the effects is determined by the calibration of the wage equation and the distribution of job offers. The wage equation is plausibly calibrated by outcomes estimated in a Mincer regression. The job offer distribution, however, relies on the assumptions that individuals search randomly

Figure 12: Calibration results



(a) Average task-based distance



(b) Average unemployment duration

and that their last held job were their *ideal* job. The latter assumption, in particular, is important for the size of the effect. To avoid this assumption, one possible extension of the model is to add job destruction and track the underlying skills of the worker, rather than assuming they correspond to those of the worker's last held job.

5.4 Additional results

A prediction of the model is that reservation distances flatten out after exhausting benefits. Table 9 presents the results of a regression of unemployment duration on observed distance, including an indicator function for benefits exhaustion to capture such a potential kink. The coefficient for unemployment duration is positive and significant across all specifications, suggesting that the unemployed accept - and possibly search for - less relevant jobs as they remain unemployed. Once UI is exhausted, the interaction term shows that unemployment duration does not affect distance, and distances stabilize at the level shown by the coefficient on the UI exhaustion indicator.

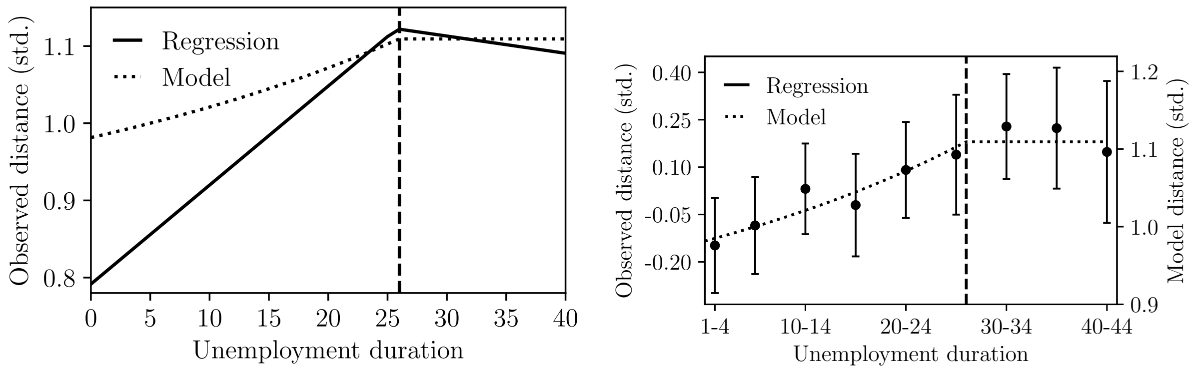
Figure 13a plots the regression coefficients of unemployment duration from specification (4) in Table 9, but only estimated on those with 26 weeks of UI available such that the location of the kink is constant. There we clearly see that the shape matches the quantitative prediction of the calibrated model. To further investigate the curvature, I estimate a more flexible model. In Figure 13b I have estimated the effect of unemployment duration on observed distance for bins of unemployment duration. Controls and

Table 9: Dynamic search behavior

	(1)	(2)	(3)	(4)	(5)
Unemployment	0.0100***	0.0103***	0.0074***	0.0076***	0.0078***
duration	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
UI exhausted	0.4333***	0.4358***	0.3439***	0.3556***	0.3919***
indicator	(0.0791)	(0.0772)	(0.0801)	(0.0778)	(0.0874)
Interaction	-0.0104***	-0.0104***	-0.0087***	-0.0092***	-0.0101***
	(0.0018)	(0.0017)	(0.0018)	(0.0017)	(0.0019)
Demographics	No	Yes	Yes	Yes	Yes
Occupation FE	No	No	Yes	Yes	Yes
State FE	No	No	No	Yes	No
Month FE	No	No	No	Yes	No
State × month FE	No	No	No	No	Yes
Observations	33,325	33,323	33,314	33,314	32,658
Adjusted R^2	0.021	0.042	0.160	0.163	0.162

Note: Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Each observation is an individual who has found a job. The left-hand side variable is a measure of dis-similarity of the pre- and post-unemployment occupation, with respect to the skill requirements of those jobs. It is standardized by dividing by its own standard deviation of observed U-t-J transitions (see Table 5). Unemployment duration is the worker's reported spell length (weeks) in the week of finding a job. UI exhausted indicator is equal to one if the worker had exhausted unemployment benefits, as simulated by Farber, Rothstein, and Valletta (2015). Demographic controls includes age, age squared, sex, race, marital status, and education. Occupation fixed effects are occupation dummies for both from and to occupation. All columns use data for full sample years 2004-2014.

Figure 13: Regression estimates of unemployment duration



(a) Regression line

(b) Regression coefficients by bins

Note: Subplot (a) is the regression line of Table 9 specification (4) with the sample restricted to $\tau = 26$. *Subplot (b) shows* β_j*'s from the regression* $d_{ist} = \sum_{j \in J} \beta_j \mathbb{1}_{t \in j} + \varepsilon_{ist}$, *where* j *are predefined bins (J) of unemployment duration. Both estimations include the full set of controls with month and state fixed effects and with a sample restricted to* $\tau = 26$. *The 95% confidence intervals are based on standard errors clustered on state.*

standard errors are as in specification (4) of Table 9. Notice also that the bins flatten out after week 26, without imposing a cut-off at that point through an indicator function. Again, the calibrated model matches the empirical findings.

6 Concluding discussion

In this paper I develop a measure of occupational distance and combine it with observed job transitions in the Current Population Survey, to show that unemployed workers target their search to, or selectively accept jobs of, occupations close to their previous occupation. Moreover, I showed that a larger distance in an occupation switch is associated with a lower re-employment wage. Finally, I have shown that workers with longer UI end up in jobs that were more similar to their previous occupations.

One interpretation of the task-based distance is that it captures mismatch between the skill set of an unemployed worker and a new job. A positive distance indicates a worker not working in her *ideal* occupation, and a larger distance corresponds to larger mismatch. This is particularly the case for older workers for whom the original occupation is very close to (or even is) the *ideal* occupation of the worker that perfectly reflects his skills and abilities. Given this interpretation, my results imply that longer UI decrases mismatch. In addition, the increases in unemployment duration associated with UI is to a large extent the outcome of the worker taking longer time to search for jobs and minimize mismatch rather than the result of reduced search intensity.

Existing literature on UI and mismatch using job tenure and re-employment wages to proxy for mismatch has provided inconclusive results. The results in my paper calls for using more direct measures of mismatch, such as the task-based distance measure.

In addition, more extensive study of the patterns and nature of occupational mobility can be done with task-based distance measures. My examination of occupational mobility using the task-based distance revealed interesting relationships to age, level of education, labor market conditions, and household characteristics. These deserve further exploration, in future research.

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A Model

This section shows some results about the model and gives details on the calibration exercise.

A.1 Results

Recall the non-stationary Bellman equation of the unemployed worker

$$U_t = h + \mathbb{1}_{t \leq \tau} b + \gamma \delta \mathbb{E} \max \{V(d_t),\ U_{t+1}\} + (1-\gamma)\delta U_{t+1}. \tag{8}$$

The solution of the model is a sequence of reservation distances, $\{d_t^R\}_t$. The policy is to reject offers above the reservation distance. The reservation distances must satisfy

$$V(d_t^R) = U_{t+1} \ \forall \ t. \tag{9}$$

Due to the time limited unemployment benefits the reservation distance is a function of time. I make the following assumptions.

(1) $V'(d) < 0$

(2) $V(\underline{d}) > U_{t+1}$, where $\underline{d}$ is the lower support of $f(d)$

(3) $V(\overline{d}) < U_{t+1}$, where $\overline{d}$ is the upper support of $f(d)$

The first assumption, presented in the main text, makes the worker prefer lower task-based distances. The second assumption implies that there exists at least one job that the worker desires, compared to remaining unemployed. Finally, the third assumption implies there exists at least one job the worker considers undesirable. I can show the following results about the model.

Corollary 1. *The reservation distance exists and is unique for all t.*

Proof. See Appendix A.2.1. □

Corollary 2. *The reservation distances have the following ordinal properties* *(i)* $d_k^R = d_{k+t}^R$ *for all* $k \geq \tau$ *and* $t > 0$*.*

(ii) $d_k^R < d_{k+1}^R$ *for all* $k < \tau$*.*

Proof. See Appendix A.2.2. $\square$

Corollary 3. d_t^R *is decreasing in* τ *for all* $t < \tau$ *and independent of* τ *for all* $t \geq \tau$*.*

Proof. See Appendix A.2.3. $\square$

A.2 Proofs

A.2.1 Proof of Corollary 1

By assumptions (1) through (3) the Intermediate Value Theorem applies, such that equation (9) has a solution. Moreover, assumption (1) implies that equation (9) is injective such that any solution of it must be unique.

A.2.2 Proof of Corollary 2

For (i). Since the reservation distance must satisfy $V(d_t^R) = U_{t+1}$ for all t, and that for $t > \tau$ we have $U_t = U_{t+1}$ by Lemma 1, then $V(d_t^R) = V(d_{t+1}^R)$ for $t \geq \tau$. Finally, as $V'(d) < 0$ (s.t. $V(\cdot)$ is injective) we have that $d_t^R = d_{t+1}^R$ for $t \geq \tau$.

For (ii). Since $V(d_t^r) = U_{t+1}$ for all t, and $U_t > U_{t+1}$ for $t \leq \tau$ then $V(d_{t-1}^R) > V(d_t^R)$ for $t \leq \tau$. Finally, as $V'(d) < 0$ we have $d_{t-1}^R < d_t^R$ for $t \leq \tau$.

A.2.3 Proof of Corollary 3

Note that d_t^R is only a function of $\tau - t$ for $t < \tau$. Since we know that d_t^R is independent of τ for $t \geq \tau$ and $d_{t-1}^R < d_t^R$ for any $t \leq \tau$ by Corollary 2, it follows that $\hat{d}_t^R < d_t^R$ if $\hat{\tau} > \tau$.

The following Lemmas have been used in preceding proofs.

Lemma 1. *For all* $t > \tau$ *we have that* $U_t = U_{t+1}$*.*

Proof. It follows trivially from the stationarity of U_t, for $t > \tau$, that $U_t = U_{t+1}$ for $t > \tau$. $\square$

Lemma 2. *For all $t \leq \tau$ we have $U_t > U_{t+1}$.*

Proof. **For $t = \tau$.** Compare the Bellman equation for $t = \tau$ and $t = \tau + 1$. Notice that by Lemma 1, U_τ exceeds $U_{\tau+1}$ only by b. As $b > 0$, $U_\tau > U_{\tau+1}$.

For $t = \tau - 1$. Compare the Bellman equation for $t = \tau$ and $t = \tau - 1$. Notice that the difference of the two is

$$U_{\tau-1} - U_\tau = \gamma\delta \left(\mathbb{E} \max \{V(d),\, U_\tau\} - \mathbb{E} \max \{V(d),\, U_{\tau+1}\}\right) + (1-\gamma)\delta \left(U_\tau - U_{\tau+1}\right).$$

By the previous point, $U_\tau > U_{\tau+1}$, such that also $U_{\tau-1} > U_\tau$.

For $t < \tau - 1$. By identical argument as for $t = \tau - 1$, we have $U_t > U_{t+1}$ for $t < \tau - 1$. □

B Extended validation of task-based distance

This sections expands Section 2.2 with additional validation exercises.

B.1 Wages in the CPS's DWS and Outgoing Rotation Group

First, I rerun the analysis of wage effects in Table 4 on a sample of J-t-J transitions in the CPS, matched to the wages available in the outgoing rotation groups (ORG). Columns (1) and (2) of Table 10 show the results. For these transitions as well, those who arrived in an occupation by switching occupations have a lower wage than those who did not switch.[24] Columns (3) and (4) show results for a regression on a sample containing both the DWS and ORG transitions. It includes an indicator for whether an individual was displaced from the previous job, in contrast to it being a J-t-J transition, along with an interaction term between the indicator and the task-based distance. The effect on wages is significantly larger for the displaced workers.[25]

[24] Note that these results say nothing about whether the individuals experienced a wage increase or decrease after switching.

[25] The results also point to a level difference between the two groups, but it is unclear in which direction the effect goes.

Table 10: Log of hourly wages and occupational distance in the ORG vs. DWS

	(1)	(2)	(3)	(4)
Distance	-0.0832***	-0.0875***	-0.0906***	-0.0905***
	(0.0065)	(0.0056)	(0.0051)	(0.0059)
Displaced worker			-0.2017***	0.1006***
			(0.0138)	(0.0126)
Displaced worker			-0.0473***	-0.0517***
× Distance			(0.0111)	(0.0103)
Demographics	Yes	Yes	Yes	Yes
From occupation FE	Yes	Yes	Yes	Yes
To occupation FE	No	Yes	No	Yes
State FE	Yes	No	Yes	No
To occ. × month FE	Yes	No	Yes	No
State × month FE	No	Yes	No	Yes
Observations	37,268	48,097	47,725	57,265
Adjusted R^2	0.576	0.582	0.570	0.576

Note: Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$. Columns (1) and (2) only use data from the ORG. Columns (3) and (4) use data from both the ORG and DWS. Each observation is an individual currently employed, but has experienced displacement in the recent past or just had a job-to-job transition. The left-hand side variable is the wage in the current job. The skill-based distance is a measure of dis-similarity of the displaced and current occupation, with respect to the skill requirements of those jobs. Demographic controls includes age, age squared, sex, race, marital status, and education. Occupation fixed effects are occupation dummies for both from and to occupation. Differences in number of observations between (1) and (2), and (3) and (4) is caused by dropping singleton observations (Correia, 2015).

B.2 Other demographics

Now I extend the analysis to other observable variables. Using the U-t-J transitions in the CPS I estimate the following regression

$$d_{ij}^F = \alpha_0 + \beta \mathbf{X} + \varepsilon \tag{10}$$

where d_{ij}^F is the theoretical distance between occupation i and j, α_0 is a constant and $\mathbf{X}$ a vector of observables and β a vector of its coefficients. Table 11 presents the results.

Observed distance is falling in age. This is in line with earlier literature, e.g. Guvenen, Kuruscu, Tanaka, and Wiczer (2018), that have shown that workers learn about their own skills over time. A second order term is positive and cancels out the negative first order effect by age 55. If learning is the underlying mechanism, then either the intensity of learning decreases over time, workers have converged to an occupation with good skill match, or both.

Females seem to switch to jobs closer to their original occupation. A potential explanation is variation in risk preferences and perception between genders. Switching to an occupation further away could be seen as a more risky occupation move, since expected productivity might have large variance. There is a literature indicating that males perceive less risk than females and take more risks than females[26], which can be interpreted as consistent with our observation that females switch to jobs closer to their original occupation.

The regression includes the complete set of racial categories in the CPS. Of these, only the *black* category is significant with *white* as the baseline, with the exception of a few narrow and small categories. It indicates that individuals identifying themselves as *black* find jobs further away. Without disregarding the fewer observations available when including wage and wealth proxies in the regression, it is important to note that the effect disappears when controlling for these factors.

Married workers get jobs closer to their original occupation. It is tempting to attribute

[26] See e.g. DeJoy (1992), Byrnes, Miller, and Schafer (1999), Garbarino and Strahilevitz (2004), Harris, Jenkins, and Glaser (2006), and Olsen and Cox (2001).

Table 11: Regressing observables on d_{ij}^F using observed job transitions

	(1)	(2)	(3)	(4)	(5)
Log age	-0.0154***	-0.0150***	-0.0143***	-0.0140***	-0.0149***
	(0.0023)	(0.0020)	(0.0020)	(0.0022)	(0.0034)
Age^2	0.0001***	0.0001***	0.0001***	0.0001***	0.0001**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Female	0.0274**	-0.1352***	-0.1344***	-0.1365***	-0.1486***
	(0.0117)	(0.0113)	(0.0120)	(0.0115)	(0.0169)
Number of children	-0.0297***	-0.0110***	-0.0112***	-0.0106***	-0.0085**
	(0.0037)	(0.0027)	(0.0028)	(0.0029)	(0.0041)
NBER recess. month	0.0022	0.0035	0.0000	0.0000	0.0000
	(0.0076)	(0.0082)	(.)	(.)	(.)
Race (white):					
Black	0.0805***	0.0481***	0.0486***	0.0502***	0.0060
	(0.0148)	(0.0105)	(0.0120)	(0.0119)	(0.0172)
Marital status (single):					
Married	-0.0505***	-0.0303**	-0.0315***	-0.0321***	-0.0081
	(0.0121)	(0.0115)	(0.0106)	(0.0119)	(0.0124)
Widowed or divorced	0.0479***	0.0621***	0.0574***	0.0524***	0.0582***
	(0.0136)	(0.0126)	(0.0120)	(0.0139)	(0.0165)
Wealth proxies:					
Log household income					-0.0009***
					(0.0001)
Medicaid recipient					0.0372
					(0.0245)
Occupation FE	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	No	Yes
Month FE	No	No	Yes	No	Yes
State × month FE	No	No	No	Yes	No
Observations	92,717	92,715	92,715	90,722	32,399
Adjusted R^2	0.016	0.151	0.158	0.161	0.175

Note: Household income is in thousands. All specifications include education fixed effects. Occupation fixed effects are occupation dummies for both from and to occupation. Categorical variables are given with omitted group (baseline) in parenthesis. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

it to a within household risk sharing argument. More wealth or higher income seem to give shorter distances.

The task-based distance varies significantly with observable characteristics, indicating that such a measure contains relevant information for job moves. In the following section I reproduce the table above with other sample restrictions.

B.3 Robustness

Table 12 reproduces Table 11 with quantitative variables in logarithms. In Table 13 I report regression coefficients where only unemployed-employed sequences are included (columns (1) through (3)) in the sample and those who are employed for at least two months consecutively (columns (4) through (6)). I calculate an alternative task-based distance. From Equation (1) I weights set to unity and use untransformed dimensions (d_{ij}^1). Second, I use untransformed dimensions but with the Euclidean norm (d_{ij}^2). These are presented in Table 14 and 15, respectively.

C Employer matching success

Autor, Levy, and Murnane (2003) use the occupational skills requirements from the Dictionary of Occupational to construct five characteristics for occupations. I match these five characteristics to my observed job transitions in the CPS. For every observed job transition I compute the absolute deviation for each characteristic separately.

Figure 14 plots density estimates for each of the smallest to largest observed distance in each of the five dimensions. I.e., the first density, "Smallest", is the density of the smallest deviation across all five dimensions in each observed job transition. The first dimensions are extremely left skewed thus showing that firms successfully match in most dimensions. However, the final density, shows that there is very relatively little mass at small values and it does not converge to zero as the others do. This implies that there is often a large deviation in at least on dimension, indicating it would be inappropriate to punish such deviations with e.g. a Euclidean norm.

Table 12: Regressing observables on d_{ij}^F: Variables in log

	(1)	(2)	(3)	(4)	(5)
Age	-0.2746***	-0.2794***	-0.2883***	-0.2974***	-0.1915***
	(0.0213)	(0.0210)	(0.0203)	(0.0243)	(0.0345)
Female	-0.0032	-0.1441***	-0.1424***	-0.1475***	-0.1508***
	(0.0137)	(0.0135)	(0.0140)	(0.0145)	(0.0218)
Log children	-0.0451***	-0.0105	-0.0116	-0.0084	-0.0103
	(0.0112)	(0.0078)	(0.0082)	(0.0098)	(0.0141)
NBER recess. month	0.0110	0.0107	0.0000	0.0000	0.0000
	(0.0146)	(0.0142)	(.)	(.)	(.)
Race (white):					
Black	0.1126***	0.0730***	0.0865***	0.0845***	0.0428
	(0.0192)	(0.0154)	(0.0157)	(0.0166)	(0.0278)
Marital status (single):					
Married	-0.0576***	-0.0637***	-0.0531***	-0.0607***	-0.0230
	(0.0173)	(0.0155)	(0.0150)	(0.0211)	(0.0212)
Widowed or divorced	0.0572***	0.0376**	0.0424**	0.0319	0.0459*
	(0.0188)	(0.0168)	(0.0163)	(0.0202)	(0.0245)
Household income					-0.0683***
					(0.0106)
Medicaid recipient					0.0394
					(0.0288)
Occupation FE	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	No	Yes
Month FE	No	No	Yes	No	Yes
State $\times$ month FE	No	No	No	Yes	No
Observations	42,800	42,793	42,793	38,270	14,934
Adjusted R^2	0.010	0.160	0.167	0.176	0.183

Note: Household income is in thousands. All specifications include education fixed effects. Occupation fixed effects are occupation dummies for both from and to occupation. Categorical variables are given with omitted group (baseline) in parenthesis. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 13: Regressing observables on d_{ij}^F: Alternative sample restrictions

	(1)	(2)	(3)	(4)	(5)	(6)
Log age	-0.0166***	-0.0126***	-0.0079	-0.0149***	-0.0110**	-0.0040
	(0.0048)	(0.0041)	(0.0057)	(0.0051)	(0.0049)	(0.0074)
Age^2	0.0001*	0.0001	0.0000	0.0001	0.0000	-0.0000
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Female	-0.0079	-0.1474***	-0.1911***	-0.0114	-0.1452***	-0.1805***
	(0.0132)	(0.0143)	(0.0250)	(0.0156)	(0.0199)	(0.0371)
Number of children	-0.0291***	-0.0067	-0.0152	-0.0348***	-0.0120**	-0.0164
	(0.0059)	(0.0046)	(0.0099)	(0.0052)	(0.0052)	(0.0121)
NBER recess. month	0.0160	0.0000	0.0000	0.0176	0.0000	0.0000
	(0.0142)	(.)	(.)	(0.0202)	(.)	(.)
Race (white):						
Black	0.1044***	0.0521***	0.0354	0.0900***	0.0437*	0.0566
	(0.0150)	(0.0168)	(0.0385)	(0.0199)	(0.0223)	(0.0533)
Marital status (single):						
Married	-0.0248	-0.0198	0.0318	-0.0308	-0.0164	0.0012
	(0.0169)	(0.0169)	(0.0330)	(0.0201)	(0.0190)	(0.0444)
Widowed or divorced	0.0726***	0.0597***	0.0873***	0.0689***	0.0621**	0.0884**
	(0.0204)	(0.0188)	(0.0294)	(0.0248)	(0.0246)	(0.0419)
Wealth proxies:						
Log household income			-0.0006***			-0.0004*
			(0.0002)			(0.0002)
Medicaid recipient			-0.0296			
			(0.0443)			
Occupation FE	No	Yes	Yes	No	Yes	Yes
State FE	No	Yes	No	No	Yes	No
Month FE	No	Yes	No	No	Yes	No
State × month FE	No	No	Yes	No	No	Yes
Observations	32,290	32,280	10,845	19,674	19,651	6,327
Adjusted R^2	0.014	0.155	0.189	0.014	0.159	0.200

Note: Household income is in thousands. All specifications include education fixed effects. Occupation fixed effects are occupation dummies for both from and to occupation. Categorical variables are given with omitted group (baseline) in parenthesis. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 14: Regressing observables on d^1_{ij}

	(1)	(2)	(3)	(4)	(5)
Log age	-0.4102***	-0.3134***	-0.2986***	-0.2953***	-0.2660***
	(0.0377)	(0.0337)	(0.0338)	(0.0387)	(0.0633)
Age^2	0.0029***	0.0021***	0.0019***	0.0019***	0.0016**
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0008)
Female	-0.7194***	-2.0040***	-1.9927***	-2.0584***	-2.1690***
	(0.1992)	(0.2024)	(0.2095)	(0.1940)	(0.3073)
Number of children	-0.4074***	-0.1882***	-0.1866***	-0.1722***	-0.1613**
	(0.0585)	(0.0453)	(0.0477)	(0.0516)	(0.0772)
NBER recess. month	-0.0622	-0.0194	0.0000	0.0000	0.0000
	(0.1414)	(0.1492)	(.)	(.)	(.)
Race (white):					
Black	2.0494***	1.0448***	0.9920***	0.9682***	0.0473
	(0.2669)	(0.1829)	(0.2050)	(0.2203)	(0.2995)
Marital status (single):					
Married	-1.2299***	-0.6620***	-0.7131***	-0.7344***	-0.5017**
	(0.2200)	(0.2040)	(0.1856)	(0.2044)	(0.2366)
Widowed or divorced	0.9338***	1.0338***	0.9185***	0.8363***	0.7801**
	(0.2285)	(0.2014)	(0.1901)	(0.2184)	(0.3004)
Wealth proxies:					
Log household income					-0.0159***
					(0.0024)
Medicaid recipient					0.8900**
					(0.4343)
Occupation FE	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	No	Yes
Month FE	No	No	Yes	No	Yes
State × month FE	No	No	No	Yes	No
Observations	92,717	92,715	92,715	90,722	32,399
Adjusted R^2	0.024	0.169	0.175	0.180	0.189

Note: Household income is in thousands. All specifications include education fixed effects. Occupation fixed effects are occupation dummies for both from and to occupation. Categorical variables are given with omitted group (baseline) in parenthesis. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table 15: Regressing observables on d_{ij}^2

	(1)	(2)	(3)	(4)	(5)
Log age	-0.0351***	-0.0266***	-0.0253***	-0.0251***	-0.0223***
	(0.0033)	(0.0028)	(0.0028)	(0.0033)	(0.0053)
Age^2	0.0002***	0.0002***	0.0002***	0.0002***	0.0001**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)
Female	-0.0404**	-0.1684***	-0.1671***	-0.1720***	-0.1846***
	(0.0167)	(0.0173)	(0.0179)	(0.0167)	(0.0262)
Number of children	-0.0360***	-0.0164***	-0.0163***	-0.0150***	-0.0142**
	(0.0051)	(0.0038)	(0.0040)	(0.0043)	(0.0065)
NBER recess. month	-0.0042	-0.0018	0.0000	0.0000	0.0000
	(0.0122)	(0.0127)	(.)	(.)	(.)
Race (white):					
Black	0.1821***	0.0906***	0.0859***	0.0849***	0.0074
	(0.0234)	(0.0157)	(0.0172)	(0.0186)	(0.0249)
Marital status (single):					
Married	-0.1023***	-0.0538***	-0.0582***	-0.0601***	-0.0394*
	(0.0188)	(0.0174)	(0.0158)	(0.0175)	(0.0207)
Widowed or divorced	0.0805***	0.0880***	0.0781***	0.0702***	0.0651**
	(0.0194)	(0.0172)	(0.0163)	(0.0185)	(0.0254)
Wealth proxies:					
Log household income					-0.0014***
					(0.0002)
Medicaid recipient					0.0778**
					(0.0371)
Occupation FE	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	No	Yes
Month FE	No	No	Yes	No	Yes
State $\times$ month FE	No	No	No	Yes	No
Observations	92,717	92,715	92,715	90,722	32,399
Adjusted R^2	0.025	0.173	0.179	0.183	0.194

Note: Household income is in thousands. All specifications include education fixed effects. Occupation fixed effects are occupation dummies for both from and to occupation. Categorical variables are given with omitted group (baseline) in parenthesis. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Figure 14: Density estimates of observed distances for the smallest to largest deviation

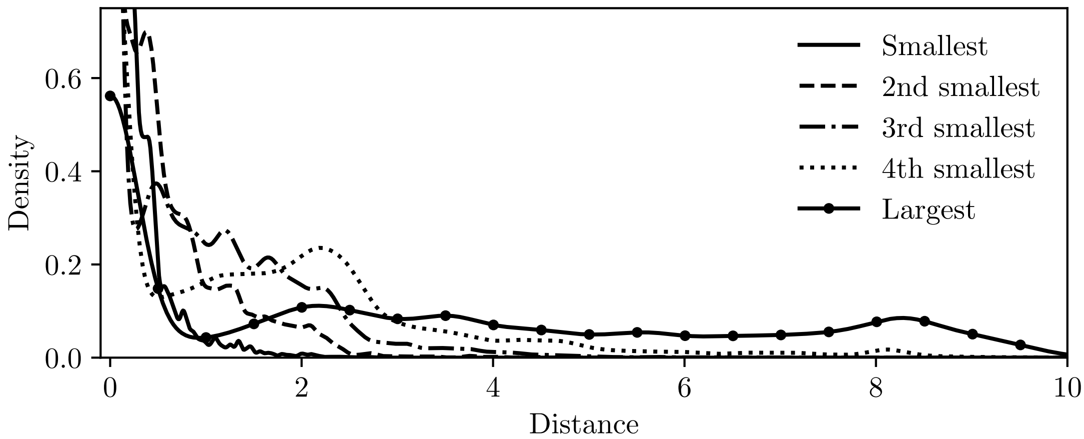


Table 16: Comparing summary statistics of U-t-J transitions in CPS sample for UI eligible and ineligible

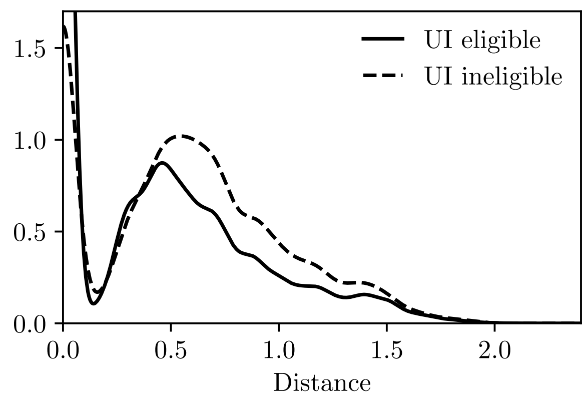
	UI eligible		UI ineligible	
	Average	St.err.	Average	St.err.
Age	38.43	0.0448	32.78	0.0847
Unemployment duration (weeks)	11.19	0.0534	9.22	0.0917
Occupation distance (d^F)	0.39	0.0016	0.57	0.0033
Share same 3-digit occupation	0.43	0.0019	0.23	0.0032
Share same 2-digit occupation	0.47	0.0019	0.28	0.0034
Share same 1-digit occupation	0.60	0.0018	0.43	0.0037
Share Female	0.39	0.0018	0.51	0.0037
Education:				
Share high school $>$	0.17	0.0014	0.13	0.0025
Share high school	0.39	0.0018	0.37	0.0036
Share high school $<$	0.44	0.0018	0.50	0.0038

Note: All averages are significantly different at the 1% level in a two-sample two-sided t-test.

D Comparing U-t-J transitions for the UI eligible and ineligible

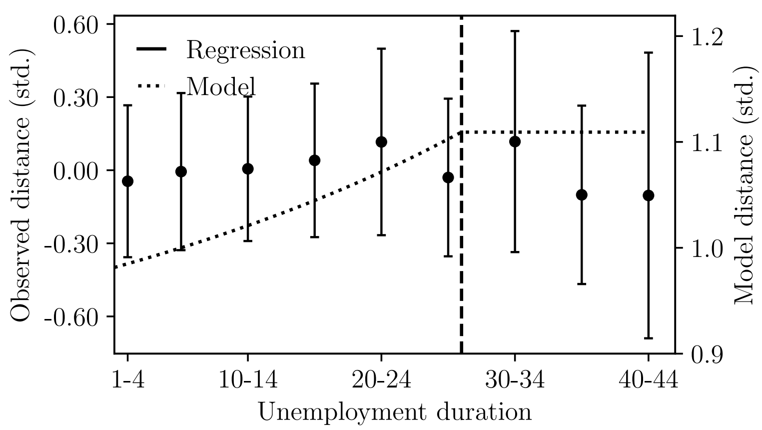
UI ineligible individuals are used as a control group as an alternative to linear controls for unemployment duration. Table 16 compares summary statistics for the two groups. Figure 15 compares the distribution of task-based distances for the two groups.

Figure 15: Observed job transitions in the CPS (U-t-J): Densities by UI eligibility



Note: The densities are estimated with Gaussian kernels and bandwidth selected using Scott's rule (Scott, 1992) independently for each density.

Figure 16: Duration on distance for non-UI eligible workers



Note: The figure reproduces Figure 13a for a sample of non-UI eligible workers. The estimation includes the full set of controls. The 95% confidence intervals are based on standard errors clustered on state.

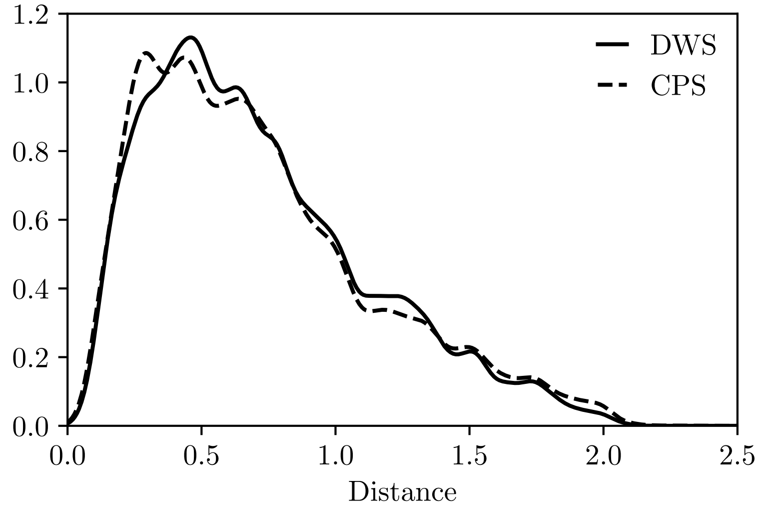
E Additional results

Figure 16 reproduces Figure 13b for the non-UI eligible in. There is no clear pattern in the effect of duration on distance, even when disregarding the large confidence intervals.

F Displaced Worker Supplement

The Displaced Worker Supplement (DWS) within the CPS consists of a set of questions for workers who have experienced a job loss. The supplement survey is conducted biennially. Only workers 20 years of age or older are included. Up to 1994, displaced workers were those who left or lost a job some time the last five years. After 1994, displaced workers

Figure 17: Kernel density estimates of observed distances



Note: Densities are truncated to not include zero distances. The densities are estimated with Gaussian kernels and bandwidth selected using Scott's rule on CPS sample, approximately equal to 0.1.

would have to have left or lost a job due to layoffs or shutdowns some time the last three years. 1998 and onwards, the survey added a requirement that workers were not self-employed and did not expect to be recalled by previous employer within six months.

In addition to the default restrictions described above, I can only use observations where occupation and hourly wage are reported for both the displaced and current job. Of the 988,569 original DWS workers 77,593 observations are discarded since the individual is either unemployed, self-employed, or in the military at time of interview or last job. 904,548 observations have missing values[27]. This leaves 6,003 observations.

Each observation is a pair of occupations; the displaced job and the worker's current occupation. I do not observe any (un)employment spells in-between.

Regardless of the limited sample size the DWS offers, the sample exhibits the same properties as the full CPS sample. Figure 17 shows estimated densities for distances in observed job switches, for both the full CPS sample and the DWS sample. These densities appear identical.

[27]This also includes wages that are top-coded, which are removed from the sample.

Table 17: Wage changes and mismatch in the DWS - Robustness

	(1)	(2)	(3)	(4)	(5)	(6)
Task-based	-0.1558***	-0.1614***	-0.1282***	-0.1396***	-0.1499***	-0.1501***
distance	(0.0128)	(0.0119)	(0.0162)	(0.0170)	(0.0108)	(0.0100)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Unempl. duration	No	No	No	No	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No	Yes	No
Year $\times$ month FE	Yes	No	Yes	No	Yes	No
State $\times$ month FE	No	Yes	No	Yes	No	Yes
Adj. R^2	.615	.620	.614	.620	.603	.607
Observations	6,079	6,061	3,525	3,460	8,997	8,992

Note: Demographic controls includes age, age squared, sex, race, marital status, and education. Occupation fixed effects are occupation dummies for both from and to occupation. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, *and* *** $p < 0.01$.

F.1 Robustness

Table 17 reproduces the results of Table 4. All columns have the hourly re-employment wage in logs as the regressand. Columns (1) and (2) have the sample restricted to those who are still in their first post-displacement job. The following two columns only look at those displaced in the previous year. Finally, columns (5) and (6) include a linear control for unemployment duration.

All columns of Table 18 have the hourly re-employment wage in levels as the regressand. (3) and (4) have w_{ist} deflated using a CPI, whilst (5) and (6) are deflated with GDP.

Table 18: Wage changes and mismatch in the DWS - Robustness to price level

	(1)	(2)	(3)	(4)	(5)	(6)
Task-based	-2.2515***	-2.2374***	-2.4529***	-2.4221***	-2.4074***	-2.3786***
distance	(0.1724)	(0.1508)	(0.1849)	(0.1687)	(0.1804)	(0.1648)
Demographics	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No	Yes	No
Year × month FE	Yes	No	Yes	No	Yes	No
State × month FE	No	Yes	No	Yes	No	Yes
Adj. R^2	.538	.541	.494	.496	.496	.499
Observations	8,997	8,992	8,608	8,604	8,608	8,604

Note: Demographic controls includes age, age squared, sex, race, marital status, and education. Occupation fixed effects are occupation dummies for both from and to occupation. Standard errors in parenthesis are clustered on state. * $p < 0.1$, ** $p < 0.05$, *and* *** $p < 0.01$.