Dual-Earner Migration Decisions, Earnings, and Unemployment Insurance

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Abstract

Dual-earner couples' decisions of where to live and work often result in one spouse – the trailing spouse – experiencing earnings losses at the time of a move. This paper examines how married couples' migration decisions differentially impact men's and women's earnings and the role that policy can play in improving post-move outcomes for trailing spouses. I use panel data from the NLSY97 and a generalized difference-in-differences design to show that access to unemployment insurance (UI) for trailing spouses increases long-distance migration rates by 1.9–2.3 percentage points (38–46%) for married couples. I find that women are the primary beneficiaries of this policy, with higher UI uptake following a move and higher annual earnings of $4,500–$12,000 three years post-move. I then build and estimate a structural model of dual-earner couples' migration decisions to evaluate the effects of a series of counterfactual policies. I show that increasing the likelihood of joint distant offers substantively increases migration rates, increases women's post-move employment rates, and improves both men and women's earnings growth at the time of a move. However, unconditional subsidies for migration that are not linked to having an offer in hand at the time of the move reduce post-move earnings for both men and women, with stronger effects for women.

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1 Introduction

How much does the career of one's spouse deter job search at a distance? The difficulty of finding a job outside of one's current labor market is compounded in a household with two earners. More often than not, when one spouse receives a distant job offer, the other spouse will not have secured employment in the new location. The household must decide then whether to turn down the job until they both can move with a job or accept the job and move with only one job-in-hand. In the latter case, the second spouse is a trailing spouse or tied mover – that is, someone who would not choose to move as an individual agent, but moves because the gains of their spouse dominate their individual losses. A large body of literature shows that tied movers are more likely to experience periods of unemployment and/or lower wages following a move (LeClere and McLaughlin, 1997; Cooke et al., 2009; Gemici, 2011; Burke and Miller, 2017) and that women are more likely to be the tied mover who suffers these losses (Mincer, 1978; Nivalainen, 2004; Boyle et al., 2009; Gemici, 2011). These differences in the impacts of a move by marital status and gender have important implications for our understanding of how people in different types of households are more or less able to search for jobs at a distance.

In this paper, I analyze a policy that may mitigate the migration frictions associated with family ties: unemployment insurance (UI) for trailing spouses. Trailing spouses are typically not eligible for UI because they have left their job voluntarily without good cause. As of 2017, 23 states' unemployment laws allow trailing spouses to collect UI. While some states have allowed trailing spouses to collect UI as early as the 1980s, many states implemented this provision throughout the 2000s and 2010s. I use this variation in policy timing to evaluate whether access to unemployment insurance for trailing spouses has a meaningful impact on households' decisions to migrate and the long-run earnings of trailing spouses.

In the first exercise, I use a difference-in-difference-in-differences methodology to estimate the effects of UI for trailing spouses on the likelihood of a long-distance move. I use state-by-year variation in the timing of the policy, along with using single-person households as a natural control group for whom the policy does not change incentives to move. Using panel data from the geo-coded National Longitudinal Survey of Youth 1997, I find that this policy increases the likelihood a married household moves more than 50 miles by approximately 2 percentage points off a base rate of 5 percent and that the effects are increasing in the level of UI generosity that states provide. To provide additional support for this identification strategy, I conduct robustness checks using independent variation unlikely to increase migration (i.e., concurrent UI modernization policies for part-time workers) and outcome variables unlikely to be affected by UI eligibility for trailing spouses (i.e., migration rates within commuting zones). I find no effect of placebo policies on migration and no effect of the policy on short-distance moves.

I next explore post-move outcomes for married households using an event study design, regressing earnings, wages, and UI receipt on indicators for whether a person moved in a given year, along with leads and lags around the time of the move. I do this separately for those living in states with the policy and states without, and the difference in post-move coefficients identifies the effect of the policy on post-move outcomes. I find that the policy has a significant positive impact on women's earnings and wages post-move, but I cannot reject a null effect for men. This aligns with the fact that women are more likely to be trailing spouses and therefore the primary beneficiary of a policy targeting trailing spouses.

Though these results suggest that UI for trailing spouses increases migration rates, it is unclear whether this policy is the optimal way to reduce the frictions associated with joint job search. If a policy maker wishes to incentivize migration, what will be the impacts of linking the migration incentive to employment in a different way? Additionally, it would be useful to evaluate what mechanisms drive the household migration behaviors seen in the reduced form results – are the spatial search frictions that depress migration for dual-earner households driven by gender differences in job-finding rates, wage offers, or some other component of job search?

I therefore turn to a dynamic model of household location choice in the presence of unemployment insurance to better understand the distributional impacts of this UI policy, as well as to estimate the impacts of alternative policy environments. This model extends previous models of migration to incorporate households with two earners, as well as explicitly incorporating unemployment insurance in the household's budget constraints to better under the mechanisms driving the reduced form findings. I estimate this model for a sample of married couples in the geo-coded NLSY97 data using coefficients from the reduced form in an indirect inference analysis, supplemented with additional data moments from the NLSY, American Community Survey (ACS), and Current Population Survey (CPS).

Using the model, I conduct three types of simulations: counterfactual simulations of responses to regional earnings shocks, counterfactual exercises to evaluate the mechanisms behind gender differences in migration outcomes, and counterfactual policy regimes to compare outcomes under different migration subsidy structures.

In the first set of exercises, I simulate household migration behavior in a counterfactual world where state-specific earnings are increased by 10%. I find that earnings shocks are more effective at stemming out-migration than at increasing in-migration, with larger increases in population in states that are losing population in the baseline model.

Next, I compare migration outcomes in the baseline model to scenarios in which I change the spatial search frictions. One reason that dual-earner couples are less likely to move is that it is unlikely both spouses will have simultaneous job offers; to test the importance of this mechanism, I simulate a scenario in which spouses always receive job offers in the same location. I find that this increases migration substantially, increasing the annual migration rate by 1.1 p.p. or 56% and the proportion of those who ever move by 11.6 pp. or 17%. Additionally, I show that women's post-move labor market outcomes improve significantly in this scenario with their post-move employment rate increasing by 8 p.p. off a base rate of 52%.

I then explore how gender differences in earnings contribute to lower migration rates for married households. Mincer (1978) theorizes that households with more equal within-household earnings will move less than households in which one spouse earns significantly more, due to the fact that more equal earnings makes the loss of one income at the time of a move more costly. To evaluate this, I first simulate household decisions in settings where men's and women's earnings are drawn from the same distribution and then in settings in which I increase the leisure value to insure that one spouse never works. I find that equalizing earnings decreases household migration substantially by 12-25%, consistent with Mincer's theory. In the simulations where one spouse never works, households migrate much more: when all women are stay-at-home spouses, the annual migration rate more than doubles, increasing from 2.1% to 4.4%.

Finally, in the policy experiments, I compare movers under a series of counterfactual migration incentives, each designed with different ways of linking migration incentives to employment outcomes. The first subsidy has similar employment incentives to UI for trailing spouses, but standardizes the size of the subsidy to $10,000 to match the other two subsidies. The second subsidy mirrors relocation incentive programs in European countries, in which job-seekers who apply for and accept a job more than a certain distance from home receive a monetary stipend. Lastly, the third subsidy is an unconditional migration subsidy which allows me to explore whether subsidies that do not tie the incentive to employment are more or less effective at inducing migration.

Though all the subsidies increase migration rates, the effects vary across policy designs. The subsidy for moving out of unemployment has little effect on migration rates, but in turn does little to distort post-move earnings for men and women. The unconditional and the trailing spouses subsidies increase migration rates more – by 11.2% and 6.2% respectively – but result in lower earnings gains following a move for both men and women. I find that the unconditional subsidy reduces women's post-move earnings gains more than the trailing spouse subsidy and vice versa for men. The differences in earnings gains are small – in the $500 to $1,100 range annually depending on policy – but suggest that the subsidy induces households to move in situations where the earnings gains in the absence of the policy aren't enough to overcome the costs associated with a move.

Taken together, these analyses demonstrate the important role that income support systems like UI or migration subsidies can play in encouraging geographically distant job search. UI for trailing spouses changes the ways in which moves create gender disparities in earnings within a household. Having access to UI for trailing spouses reduces the income losses that married women tend to experience following a move. The counterfactual exercise of a moving subsidy emphasizes that policymakers should consider how different structures for migration subsidies – tied to moving or tied to employment – result in different outcomes for married male and female movers.

This paper contributes to the existing literature on migration and job search in three ways.

First, this paper evaluates the effect of a previously unstudied migration incentive, contributing to our understanding of the low migration rates for married households and the gender differences in earnings following a move. Both theoretical models of household migration (e.g., Mincer, 1978; Lundberg and Pollak, 2003) and subsequent empirical analyses (e.g., LeClere and McLaughlin, 1997; Cooke et al., 2009; Gemici, 2011; Burke and Miller, 2017; Rabe, 2011; Blackburn, 2010) document the fact that married households move less than unmarried individuals, married women are more likely than married men to be tied stayers, and tied movers typically experience periods of unemployment and/or lower wages following a move. Recent structural models of dual-income couple migration (Gemici, 2011; Guler and Taskin, 2013) demonstrate a link between gender inequalities in earnings within households and the family tie frictions associated with migration.

While these papers discuss the mechanisms behind these facts, they do not consider the role that public policy could play in changing the gender composition of leading vs. trailing spouses. I document the fact that providing UI to trailing spouses significantly increases the likelihood that married households move and that this policy seems to primarily benefit women, providing additional support for past results showing that women are more likely to be the trailing spouse. These results also speak to policies that may encourage domestic migration, a policy concern with increasing relevance in light of the growing literature in economics documenting declining migration rates in recent decades (Kaplan and Schulhofer-Wohl, 2017; Molloy et al., 2011; Johnson and Schulhofer-Wohl, 2019).

Second, this paper adds to a large body of both theoretical and applied research concerned with the effects of unemployment insurance generosity on duration of unemployment, labor supply, and post-separation earnings paths more generally (see Krueger and Meyer, 2002 for a review of the literature). Theory suggests that more generous UI should result in higher reservation wages and post-separation job quality, though applications of this theory to the data find mixed results. Some past research (Marimon and Zilibotti, 1999, Acemoglu and Shimer, 2000, Centeno, 2004; Lalive et al., 2015) suggests that more generous UI policies increase job duration and job quality match post-unemployment spells. However, there are mixed findings about the impacts of UI on earnings post-separation with some studies showing wage gains (e.g., Ehrenberg and Oaxaca, 1976, Nekoei and Weber, 2017), but others only seeing a weak or null effect of UI on wages (Addison and Blackburn, 2000; Card et al., 2007; Van Ours and Vodopivec, 2008; Schmieder et al., 2016; Le Barbanchon et al., 2019).

The majority of these studies on UI generosity focus on access at the intensive margins – increases in replacement rates or in the number of weeks of eligibility – whereas this paper focuses on access on the extensive margin – who is eligible in the first place. Since those at the margin of accessing UI are likely to differ from those typically eligible, one might expect a different behavioral response in this setting. Though extensive margin access based on monetary eligibility requirements has been studied (e.g., Leung and O'Leary, 2020), less is known about how changes to non-monetary eligibility criteria for unemployment insurance would change job search outcomes of workers.

Lastly, this paper contributes to a small but growing literature that extends job search and migration models to consider a household, rather than an individual. The role that UI might play in a joint search model with search at a distance is almost entirely unstudied. Though past papers have considered how earnings gains and government benefits across locations drive migration (e.g., Bishop, 2008; Kennan and Walker, 2010; Kennan and Walker, 2011; Ransom, 2019), papers focusing on dual-earner migration have not examined the role of government benefits in migration decisions (e.g., Braun et al., 2019; Gemici, 2011; Guler and Taskin, 2013). Research on UI in the presence of joint search decisions typically does not incorporate migration (e.g., Cullen and Gruber, 2000; Dey and Flinn, 2008; Ek and Holmlund, 2010 Flabbi and Mabli, 2018; Garcia-Perez and Rendon, 2020). This paper incorporates elements from both the migration and the job search literature to better model how households conduct distant job search.

2 Data

2.1 Institutional Setting and Policy Data

Unemployment insurance provides compensation to full-time workers who are no longer employed through no fault of their own, with eligibility determined partially based on employment and earnings thresholds in the quarters leading up to the separation and partially through non-income based eligibility criteria (e.g., job search requirements). One such non-income based criteria is the reason for separating from employment. Workers who lose their job due to lay offs or for reasons other than misconduct are eligible for unemployment, but voluntary quits are not eligible for unemployment unless the worker can demonstrate that they quit for 'good cause.'

Though UI is governed by federal guidelines under the Federal Unemployment Tax Act, states are given the freedom to implement their UI programs differently, resulting in many different definitions of what constitutes 'good cause' across state lines. As of 2017, 23 states included leaving a job due to a distant move for a spouse or partner's career as one type of good cause for leaving a job. This number is down from a peak of 27 states in 2010 but is much higher than pre-recession levels, when only 11 states had trailing spouse UI provisions (see Figure 1). Many states incorporated this provision as part of the UI modernization requirements associated with receipt of federal funds during the Great Recession under the American Recovery and Reinvestment Act (ARRA).[1]

In Appendix Table A-2, I report the month and year of implementation (and repeal) of provisions granting UI eligibility for job separation due to spousal relocation for each state. Each year, the Department of Labor publishes Comparison of State Unemployment Insurance Laws reports which include a section reporting if a state allowed eligibility for spousal relocation based on either law, regulation, or interpretation. Using these reports, I identify the year that a state starts offering eligibility according to the Department of Labor. I then confirm the date of implementation based on comparisons of language in state statutes available in publicly available state archives, as well as the publicly available applications

[1]More information on the ARRA's UI Modernization program is discussed in Appendix Section A.1.

Figure 1: Change in States with Spousal Relocation UI, 2005, 2010, 2017
Notes. This figure shows the states which had UI for trailing spouse policies in 2005 (beginning of sample), 2010 (after ARRA), and 2017 (end of sample).

for the ARRA modernizations. In cases where the state statutes or UI Modernization applications contradicted the Department of Labor reports, states' implementation dates were coded based on primary source documents, rather than the Department of Labor reports.

2.2 Data Sample Definition

To analyze the effects of UI for trailing spouses, I require data that allows me to observe the same household over multiple periods during the 2000s and 2010s. I therefore use the geocode restricted National Longitudinal Survey of Youth 1997 (NLSY97), a longitudinal survey which began in 1997 and follows a nationally representative cohort of 9000 teenagers who were 12-18 in 1997 annually until 2010 and then biennially until 2018.[2]

Because I am interested in migration rates of married couples of working age, I restrict the NLSY97 sample to individuals who are older than 23, married in the current period and the previous period, and not missing information on completed education, earnings, or state and

[2]Though previous analyses of the effects of unemployment insurance use the Survey of Income and Program Participation (SIPP), the short panels and more limited geographic definitions in the SIPP do not allow for the event study analyses that allow me to look at long-run earnings post-move. Other longitudinal data sets such as the National Longitudinal Survey of Youth 1979 or the Panel Study of Income Dynamics are biennial for the duration of the study period.

county of residence. This leaves me with a primary sample of 18,225 married household-year observations, spanning the years 2004 through 2014 and including 3,810 individual respondents. My secondary sample includes unmarried individuals who are not cohabiting with a romantic partner, are older than 23, and are not missing data. This sample has 27,990 person-year observations with 5,837 individual respondents.[3] Table 1 reports descriptive statistics for treated and non-treated married and unmarried households in the sample.

Table 1: Summary Statistics

	Married			Not Married		
	Full Sample	Treated	Not Treated	Full Sample	Treated	Not Treated
Age	29.56	30.17	29.18	27.91	28.36	27.61
	(3.595)	(3.427)	(3.645)	(3.370)	(3.353)	(3.348)
Female	0.552	0.546	0.557	0.444	0.451	0.439
	(0.497)	(0.498)	(0.497)	(0.497)	(0.498)	(0.496)
White	0.778	0.763	0.787	0.634	0.651	0.623
	(0.416)	(0.425)	(0.410)	(0.482)	(0.477)	(0.485)
BA or more	0.371	0.375	0.368	0.349	0.376	0.331
	(0.483)	(0.484)	(0.482)	(0.477)	(0.484)	(0.471)
Weeks worked last year	40.76	40.90	40.68	39.74	40.14	39.47
	(19.11)	(19.23)	(19.03)	(18.79)	(18.69)	(18.86)
Earnings, 2010$	39447.9	41622.0	38061.3	29171.9	31530.7	27597.8
	(34216.7)	(35474.2)	(33317.9)	(25527.7)	(27459.4)	(24025.5)
Number of kids	1.387	1.422	1.366	0.371	0.328	0.400
	(1.172)	(1.161)	(1.179)	(0.846)	(0.801)	(0.874)
State per capita income	41582.2	44430.2	39793.7	40845.4	43560.8	39073.7
	(7436.9)	(7952.5)	(6483.5)	(7234.7)	(7233.5)	(6663.8)
State unemployment rate	6.570	7.126	6.221	6.448	7.046	6.058
	(2.251)	(2.360)	(2.079)	(2.204)	(2.311)	(2.038)
Moves, State	0.0380	0.0312	0.0422	0.0517	0.0460	0.0554
	(0.191)	(0.174)	(0.201)	(0.221)	(0.209)	(0.229)
Moves, Across CZ	0.0622	0.0523	0.0684	0.0881	0.0733	0.0978
	(0.241)	(0.223)	(0.252)	(0.283)	(0.261)	(0.297)
Moves, Within CZ	0.0297	0.0187	0.0365	0.0399	0.0281	0.0475
	(0.170)	(0.136)	(0.188)	(0.196)	(0.165)	(0.213)
Moves, >50 miles	0.0463	0.0386	0.0512	0.0656	0.0561	0.0718
	(0.210)	(0.193)	(0.220)	(0.248)	(0.230)	(0.258)
Observations	18225	7176	11049	27990	10852	17138
Households	3810	1946	2653	5837	2949	4162

Notes. This table reports descriptive statistics on the full sample of married observations (person-year) in the NLSY97 (col.1), the treated sample of married observations (col.2), control sample of married observations (col.3), the full sample of single observations (col. 4), treated sample of single observations (col. 5), and the control sample of single observations (col. 6). The sample is restricted to individuals 23 or older without missing location, earnings, education, or marital status information and weighted using longitudinal NLSY97 weights.

2.3 Measures of Interest

I look at four outcomes of interest in the regression analyses: annual migration, annual earnings, monthly unemployment insurance receipt, and monthly wages.

I use commuting zones combined with distance to define migration decisions. The NLSY97

[3]Because I follow respondents over time, some households are in the unmarried sample during some years and in the married sample in other years. In total, 7,512 unique households are in the sample.

provides the distance between addresses, allowing me to restrict moves to cross-commuting-zone moves beyond a certain distance. I choose to use commuting zones combined with distance as my proxy for a 'labor market' because of its particular relevance to this setting: eligibility for UI due to spousal relocation is conditional on the spouse's new job making commuting impractical. Using commuting zone moves is likely a better proxy for moves that make one eligible for UI than a cross-state move. When using state moves as the outcome, I am both incorporating some moves that would not constitute good cause under the statutes (i.e., a move from New York City to Hoboken, NJ would be a cross-state move but would not prevent a person from commuting to their previous job) and missing within state moves that require leaving one's job (e.g., a move from San Diego to San Francisco).

Thus, in my primary specification, a household is identified as moving if they are living in a different commuting zone in period t than they were in period $t-1$ and the new address is 50 miles or more from the original address. To identify commuting zone of residence, I use crosswalks developed in Dorn (2009) to convert the county reported by a respondent to commuting zone. I also use moves across state lines and moves across commuting zones unconditional on distance as secondary measures of moves.

For the monthly analyses, I use the NLSY97 retrospective migration and job histories between surveys to measure the exact month of a move. The NLSY97 asks respondents to report a monthly migration history between surveys, asking them the month and year of the move and the state, county, and MSA of the move. I characterize a move event as a month in which the respondent changed commuting zones, once again cross-walking from county to commuting zone using Dorn (2009).[4]

I define annual earnings as the annual earnings from wages and salary. The NLSY97 asks for annual earnings for the year prior from both the respondent and their spouse if present in the household; I then assign earnings to the husband or the wife based on the gender of the respondent. If I never observe earnings for the respondent or spouse, I drop the household from the sample. For the years during the biennial data collection in which annual income is not recorded (2012, 2014, 2016), I impute annual income as the mean of the year prior and the year following if the respondent worked a positive number of weeks in the year and as zero if the respondent worked zero weeks in the year.

UI take up is measured as whether the respondent or spouse received positive income from unemployment insurance in a given month. The NLSY97 asks respondents to report the start and stop dates of each spell of unemployment insurance receipt as well as the amount of unemployment received, allowing me to characterize a person as receiving UI if that month was during the reported spell. As with income, I assign the UI receipt to the husband or wife based on the reported gender of the respondent. A small number of respondents/spouses report working for all 52 weeks in the year and also report receiving unemployment insurance; I re-code their response to be non-receipt of unemployment insurance.

[4] Distance between addresses are only reported at the time of the survey, not for the monthly migration histories. I therefore use commuting zone moves for all monthly analyses.

The NLSY97 includes a weekly job history in which the wage is reported in three situations. First, the respondents are asked to report the start and stop date of each job as well as the hourly wage for the job at that time. Also, if a job carries over across interviews, they are asked about the wage for that job again in each interview. If a person reports wage w_t in an interview in year t, I assign that wage to all weeks in year t pre- and post- interview unless the start/stop dates occured in that year (in which case, the wage switches at the time of job change). I then aggregate this to the monthly level by taking the average wage in a given month. I impute missing wages as the mean of the most recent previous reported wage and the next reported wage. Missing values during periods in which the respondent is unemployed or out of the labor force are imputed as zero. I deal with extreme values by top-coding any wages above the 99th percentile of reported wages as 1.5 times the 99th percentile of the reported wage distribution.

2.4 Supplementary Data

I also use supplementary data on state-level characteristics that vary over time. Data on seasonally unadjusted unemployment rates by state and year are from the publicly-available Bureau of Labor Statistics Local Area Unemployment Statistics data from 2004 through 2014. Per capita income comes from the publicly-available U.S. Bureau of Economic Analysis Local Area Personal Income accounts, 'Annual Personal Income by County.'

I use American Community Survey (ACS) 2004-2016 (Ruggles et al., 2019) as an alternative sample to measure the effects of the policy on migration in a non-panel data setting with a larger sample and a greater range of ages as well as to calculate moments on employment post-move for the structural model. In this sample, I define a long-distance move as a move across commuting zones for the reduced form exercises.

To calculate a supplementary estimate of the effects of the policy on actual UI use, I use a data set published by the Department of Labor on the number of claims at the state level that are eligible for UI based on a non-monetary determination. This data includes a measure of the annual voluntary separations that receive non-monetary determinations between the years 2000 and 2017 (Department of Labor, 2019), which includes separations that are eligible for UI under the policy of interest.

Lastly, to measure average state generosity in UI, I create "simulated UI replacement rates", a measure of the generosity of the state UI program that depends only on state policy variation using a UI calculator developed in Kuka (forthcoming). This calculator uses the 1996, 2001, 2004, and 2008 panels of the Survey of Income and Program Participation (SIPP) to identify individuals who have lost their job through no fault of their own and calculates the replacement rate of the UI that they receive. It then uses this sample to calculate average replacement rates by state-year-household type, defined as the UI payment divided by weekly earnings, for each state, year and number of children cell.

3 Empirical Strategy

This section describes the empirical strategy for identifying the effects of the policy in the reduced form analyses.

3.1 Migration Rates

To identify the effects of access to UI on migration rates, I use a generalized difference-in-difference-in-differences framework. I rely on variation in when a state implemented the policy as well as the fact that the policy should only impact married household migration decisions. The key identifying assumption is that conditional on observables and state-year fixed effects, the likelihood of moving for the treated households in the absence of the policy would be the same as that of the untreated households in absence of the policy.

To estimate this, I regress an indicator for moving between year $t-1$ and year t on an indicator for whether a person's state in year $t-1$ allowed for UI receipt, state fixed effects for state in $t-1$ (S_{t-1}), year fixed effects (T_t), and time-varying characteristics of the sending state (Z_{st}: per capita income and unemployment rate). I first estimate this regression including individual covariates (X_{it}: a quadratic of age, indicator for college, number of children, race indicators); I then add individual fixed effects (θ_i); and then I restrict the sample to individuals who were working in the previous year to remove spouses who would not be eligible due to non-participation in the labor force. Additionally, because this policy should have no impact on the benefits available to non-married individuals who move long distances, I am able to compare the effects for individuals who were married over the time period of the move (t and $t-1$) to individuals who were unmarried and not cohabiting with a partner.

The regression is specified as follows:

$$
\begin{aligned}
\mathbb{1}(\text{Move})_{it} \quad = \quad & \mathbb{1}(Married)_{it} \times \left[\beta_0^M + \beta_1^M \mathbb{1}(State_{t-1} = Treated)_{it} + X_{it}'\beta_2^M + Z_{s,t-1}'\beta_3^M + S_{t-1}^M + T_t^M\right] + \\
& \mathbb{1}(NotMarried)_{it} \times \left[\beta_0^{NM} + \beta_1^{NM} \mathbb{1}(State_{t-1} = Treated)_{it} + X_{it}'\beta_2^{NM} + Z_{s,t-1}'\beta_3^S NM + S_{t-1}^{NM} + T_t^{NM}\right] \\
& + \theta_i + \epsilon_{it} \qquad (1)
\end{aligned}
$$

where β_1^M is the coefficient of interest, representing the average treatment on the treated of access to UI for trailing spouses for a married household, identified off of within-person variation in whether they were married and living in a state during a year in which the policy was in place. All covariates are interacted with marital status, excluding the individual fixed effect which can be thought of as controlling for an individual's fixed propensity of moving in a given year across the time period in the sample regardless of marital status.

While the state, year, and individual fixed effects control for within-state, within-year, or within-person characteristics that make the treated households different from the untreated, we might be concerned that there are macro-level factors co-varying at the state-year level with the policy. Notably, since much of the variation comes from UI modernization in 2009, one might be concerned that treated states differed from untreated states in a systematic way during the recession.

To address this concern, I do three things. First, I use unmarried individuals as a plausible comparison group; though they face the same state-year conditions as married households at the time of policy implementations, the policy should not affect their migration decisions. This assumes that $\mathbb{E}[\beta_1^{NM}] = 0$, but that the expectation of the differenced unobservables (i.e., the ϵ term) are equal for single and married households. I run the regression as specified, allowing β_1^{NM} to soak up anything changing concurrently with the policy, and then I run a specification where I omit the treatment for single households and include state by year fixed effects which captures anything that changes at the state-year level that affects both single and married households.

Second, I test the assumption of parallel trends using an event study analysis, where I regress the indicators for moving on a series of indicators equal to one in the year of the policy going into place and leads and lags surrounding the year of implementation, along with the same set of controls used in the most restrictive version of equation (1). Figure 2 plots the coefficient on these leads and lags for married households and single households. This analysis shows that there are parallel trends in the likelihood of moving prior to the policy for single and married households, but the patterns diverge following policy implementation with treated married couples not experiencing the same decline in migration that single households experienced during the recession.

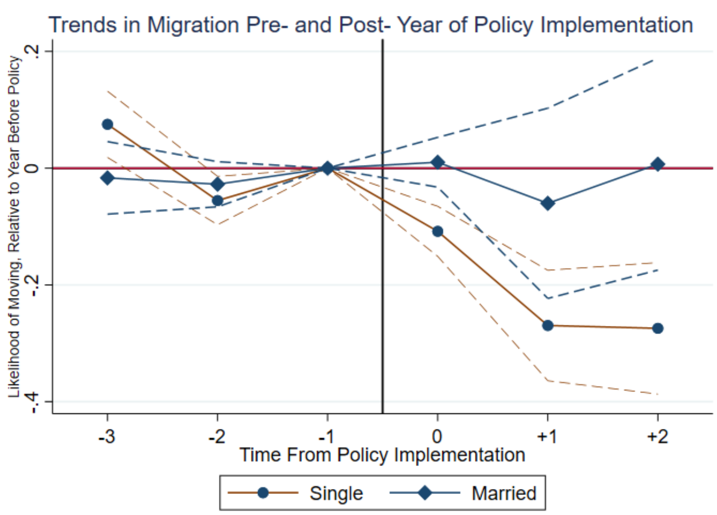

Figure 2: Pre-Trend Comparison
Notes. This figure plots the coefficients from a regression of likelihood of moving more than 50 miles on indicators for the year the UI policy goes into place (T= 0) and leads and lags surrounding that year. Brown lines with circles connect coefficients for single households; blue lines with diamonds connect coefficients for married households. All regressions include individual, state, and year fixed effects, controls for a quadratic of age, number of children, state unemployment rate, and state per capita income and are restricted to households with the same marital status the year prior to T= 0 who worked in the year prior to policy implementation.

Last, I estimate equation 1 for a placebo treatment (UI eligibility for part-time workers) and a placebo outcome that should not be affected by the move (within commuting zone moves). Appendix Section 4.1.2 reports the results of these robustness checks.

3.2 Post-Move Labor Market Outcomes

Next, I turn to the effects of the policy on post-move labor market outcomes. One would expect this policy to impact post-move earnings in two ways.

First, there is a direct effect on job search behavior of the trailing spouse. For a trailing spouse moving without a job-in-hand, this policy will theoretically let the spouse search for longer post-move and have a higher reservation wage, resulting in lower earnings in the short run due to a longer period of unemployment but higher earnings and/or wages in the long run. This effect will hold regardless of whether this household is an 'always mover' who moves in the presence or absence of the policy or a 'marginal mover' who is induced to move due to the policy.

Second, there is an indirect effect of changing who selects into migration. For the leading spouse, it changes job search behavior pre-move, increasing their willingness to search for jobs at a distance and lowering their long-distance reservation wage. For the trailing spouse, it changes which trailing spouses will be willing to give up their pre-move earnings for an uncertain post-move labor market outcome – because the option value of non-employment is more valuable, trailing spouses with higher earnings potentials will be willing to move.

The goal of this exercise is to identify the direct effect – how does access to UI for trailing spouses change post-move UI take up, earnings, and wages?[5] However, measuring the effect of the policy on, for example, post-move wages is complicated by the fact that moving itself affects wages and that the decision to move is endogeneous to the policy. The following econometric model illustrates this identification problem:

$$\overbrace{W_{i,t+1}}^{\text{wage}} = \underbrace{f(X_{it})}_{\text{state FE, Year FE, observables}} + \overbrace{\alpha M(D)_{it}}^{\text{Mover, conditional on D}} + \phi \underbrace{D_{it}}_{\text{Treated}} + \gamma \overbrace{[D \times M(D)]_{it}}^{\text{Treated Mover}} + e_{it}$$

A household's earnings in the coming period are a function of whether a household moves this period $\big(\mathrm{M(D)}\big)$, whether they have access to UI for trailing spouses (D), and the interaction between the terms, as well as observable characteristics of the household. γ is the parameter of interest: the difference in earnings next period for movers with access to UI for trailing spouses relative those who don't have access to the policy. In an ideal world, in which I observe the migration and labor market outcomes of households in all states of the world,

[5]Note that the goal is not to use the treatment as an instrument to identify the causal impacts of a move on earnings. Because the treatment impacts post-move earnings not only through increasing the likelihood of a move but also through changing job search behavior, it does not meet the exclusionary restriction necessary to instrument for moving.

irrespective of realized treatment status, I could estimate γ as follows:

$$\begin{aligned}\hat{\gamma} \;=\;& \big(\mathbb{E}[W_{i,t+1}|X_{it}, D=1, M(1)=M(0)=1] - \mathbb{E}[W_{i,t+1}|X_{it}, D=1, M(1)=M(0)=0]\big) \\ & -\big(\mathbb{E}[W_{i,t+1}|X_{it}, D=0, M(1)=M(0)=1] - \mathbb{E}[W_{i,t+1}|X_{it}, D=0, M(1)=M(0)=0]\big)\end{aligned}$$

That is, I would estimate the difference in earnings between always movers and always stayers in the presence and the absence of the policy. The identification relies on the assumption that differences in UI for trailing spouse policies within sending states over time are not correlated with other factors that affect job search behavior of movers.[6] However, I cannot observe the same household in both states of the world and therefore cannot identify always movers/stayers. I can estimate the following instead:

$$\begin{aligned}\widetilde{\gamma} \;=\;& \big(\mathbb{E}[W_{it}|X_{it}, D=1, M(1)=1] - \mathbb{E}[X_{it}, W_{it}|D=1, M(1)=0]\big) \\ & -\big(\mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1] - \mathbb{E}[W_{it}|X_{it}, D=0, M(0)=0]\big)\end{aligned}$$

For $\widetilde{\gamma}$ to be equal to $\hat{\gamma}$, it would have to be the case that the wage gains/losses to moving in the untreated state are the same for those who move (don't move) in the presence of the policy and those who move (don't move) in the absence of the policy. These identifying assumptions rely on the idea that movers and stayers in the presence of this policy are plausibly similar to those in states which do not implement UI for trailing spouses. Given that one might expect the policy to not only change post-move outcomes but also change the composition of movers, part of the estimated effects may come from the selection patterns into migration described above.

To deal with this identification problem, I do three things.

First, I use a rich set of controls. To estimate this econometric model, I parametrize the model described in equation 2 as an event study style analysis with annual wage and salary earnings, monthly UI take-up, and monthly average hourly wages. For the earnings analysis, I regress earnings on lead and lag indicators of whether a person moved in a given year defined as previously ($\mathbb{1}(Move)_{i,t+\tau-1}$), along with the same set of controls as in the migration regressions along with an added control for earnings one-year prior to the move. I do this separately for individuals who were in states that had the policy in place in year t and those in states that did not have the policy in place in year t. The specification is as follows:

$$Earn_{it} = \beta_0 + \sum_{\tau=-3}^{\tau=3} \alpha_\tau \mathbb{1}(Move)_{i,t+\tau-1} + X'_{it}\beta_1 + S_{t-1} + T_t + \theta_i + \epsilon_{it} \tag{2}$$

In this regression, the coefficients of interest are the vector of α_τ, which represent the earnings growth of movers relative to stayers normalized to be zero in the year prior to the move,

[6] Alternatively, I could also estimate $\hat{\gamma} = \mathbb{E}[W_{i,t+1}|X_{it}, D=1, M(1)=M(0)=1] - \mathbb{E}[W_{i,t+1}|X_{it}, D=0, M(1)=M(0)=1]$ or the difference in earnings across treatment status for always movers. The downside to estimating this version is that it obfuscates the interpretation of the effect. A positive γ could indicate that trailing spouses in the presence of the policy have higher earnings post-move than stayers whereas trailing spouses in the absence have flat wages, or it could indicate that trailing spouses who move in the absence of the policy earn less than a similar stayer would. I do run these analyses as a robustness check and report results in appendix section A.4.

with standard errors clustered at the state-year level. This vector is estimated separately for individuals who are in a treated state at time t and those in a control state. The difference between the α_τ across treatment states then indicates the effect of the policy on income growth at the time of a move – that is, the difference $\alpha_{D=1} - \alpha_{D=0}$ is the $\mathbb{E}[\gamma]$ described in the econometric model.

For both UI and wages, I regress the outcome of interest ($Y_{i,m}$: UI, wages) on a similar specification as the earnings specification with moves measured at the monthly level. Covariates are the the same as in the annual specification with the addition of UI take-up three months prior and log wages three months prior. The specification is as follows:

$$Y_{im} = \beta_0 + \sum_{\tau=-12}^{\tau=24} \alpha_\tau \mathbb{1}(Move)_{i,m+\tau-1} + X'_{im}\beta_1 + S_{m-1} + T_m + \theta_i + \epsilon_{im} \tag{3}$$

I omit the lead for three months prior in the monthly regressions. The coefficient vector of interest in these event studies, α_τ, then represents the difference in the outcome of interest in the months surrounding month m relative to $m - 3$ for those who move in period m compared to those who do not. As before, the treatment of interest is the difference in α_τ for treated versus untreated households.

Second, I use propensity score weighting methods (e.g., Rosenbaum and Rubin, 1983; Imbens, 2000; Hirano et al., 2003) to re-weight observations to be observably similar to treated movers. In particular, I estimate a respondent's likelihood of moving and being treated as a function of a set of observable characteristics, including observable characteristics that would impact migration but would plausibly be unrelated to labor market outcomes other than through migration propensity. I show that the effects of the policy are substantively similar to the primary specification, though the effects are attenuated. Further discussion of this specification and the results are described in Appendix Section A.3.

Even in this analysis, however, the assumption fails if there are unobserved characteristics of the marginal mover that impact earnings post-move that change labor force attachment/ job search behavior simultaneously with the move. For example, one might be concerned that always movers are more likely to have trailing spouses who were timing an exit from the labor market for the same year as the move happens, such as a family intending to have a child and then move.

Since I cannot control for all possible scenarios that would violate this assumption, the final robustness check I do is a bounding exercise adapted from Lee (2009). In this exercise, I calculate a lower bound on the effects of the policy by estimating the proportion of the sample who are marginal movers (q) and then assuming that the marginal movers are the most positively selected in terms of earnings, meaning that the top q earners post-move are marginal movers and should be excluded. This method is described in more detail in the Appendix Section A.4. These bounds suggest that γ is positive for women, and the lower bound estimates are statistically significantly greater than zero three years post-move.

4 Empirical Results

4.1 Migration Rates

In the first set of regressions, I explore the impacts of access to UI for trailing spouses on likelihood of a household move either across state lines or across commuting zones. I estimate the regression specified in equation 1. Table 2 reports the coefficient of interest, β_1^M, which represents the effect of having access to UI for trailing spouses on likelihood of moving for married households. The first column does not include individual fixed effects; the second column adds individual fixed effects; the third column restricts the sample to households where both spouses worked in the year prior to the move; the fourth column omits the treatment for unmarried households and includes state-year fixed effects. Panel A reports results for cross-commuting zone moves greater than 50 miles, Panel B reports results for cross-commuting zone moves irrespective of distance, and Panel C reports results for cross-state moves.

Table 2: Likelihood of Move Given UI Eligibility

		(1) OLS	(2) Ind. FE	(3) Dual Earner Only	(4) State × Year FE
Panel A: > 50 Mile Move	Single × Treated	-0.00575	-0.00938	-0.0136^{+}	
		(0.00593)	(0.00729)	(0.00786)	
Base Rate: 5.0%	Married × Treated	0.0183*	0.0230*	0.0187	0.0236*
		(0.00844)	(0.0103)	(0.0114)	(0.0110)
Panel B: Cross-CZ Move	Single × Treated	0.00128	-0.00220	-0.00708	
		(0.00698)	(0.00791)	(0.00829)	
Base Rate: 6.6%	Married × Treated	0.0231*	0.0252*	0.0182	.0242*
		(0.00998)	(0.0115)	(0.0127)	(0.0123)
Panel C: Cross-State Move	Single × Treated	-0.00483	-0.00913	-0.00886	
		(0.00585)	(0.00647)	(0.00713)	
Base Rate: 4.1%	Married × Treated	0.0127	0.0161^{+}	0.0119	0.0127
		(0.00895)	(0.00974)	(0.00988)	(0.0101)
	State FE	yes	yes	yes	yes
	Year FE	yes	yes	yes	yes
	Covariates	yes	yes	yes	yes
	Ind. FE	no	yes	yes	yes
	Worked Last Year	no	no	yes	no
	State X Year FE	no	no	no	yes
	N	46220	46220	37841	46220

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficients for regressions of likelihood of moving on an indicator for access to UI eligibility for trailing spouses. Column 1 includes state and year fixed effects and controls including dummies for race and education, an indicator for kids, state-year unemployment rates, and state-year per capita income. Column 2 adds individual fixed effects. Column 3 restricts the sample to households where both spouses worked in the previous year. Column 4 omits the non-married treatment indicator and includes state-year fixed effects. Standard errors are clustered at the state-year level.

Focusing first on my preferred definition of moves – cross-commuting zone moves greater than 50 miles – the results suggest that access to unemployment insurance for trailing spouses is associated with significantly higher migration rates for married respondents and no increase in migration for single respondents. The effect is positive and statistically significant in the both most parsimonious model without fixed effects (column 1), and with the addition of individual fixed effects (column 2). In this latter specification, access to UI for trailing spouses is associated with a 2.3 percentage point (46%) increase in migration rates. When the sample is restricted to individuals who were working in the previous period (column 3), the effect is of similar magnitude, but no longer significant: 1.9 percentage points, a 38% increase relative to the base migration rate of 5.0 percent for married treated households pre-implementation. Later analyses of unemployment receipt surrounding timing of the move suggest that leading spouses are more likely to be unemployed pre-move, suggesting that this third specification is actually a conservative estimate of the true effects of the move as it excludes this type of household.

The coefficients for unmarried individuals are negative and either non-significant or only marginally significant. This is consistent with the expectation that the policy only increases migration decisions of married individuals and provides support for the assumption that there are no other state changes happening concurrently with implementation that encourage distant migration.[7] Effect sizes for married households are similar in column 4 when I replace the treatment coefficient for single households with state by year fixed effects.

The effects are similar in magnitude when I focus on cross-commuting zone moves (panel B) or cross-state moves (panel C). Depending on the specification, living in a treated state is associated with a 1.8 to 2.5 percentage point increase in the likelihood that one moves to a new commuting zone if married, relative to a base rate of 6.6%. The analyses show that married individuals in treated states are also more likely to move across state-lines (1.2-1.6 percentage point higher likelihood), though the effects are more noisily estimated in the cross-state specification. As before, the effect of the treatment on singles' migration is not significantly different than zero.

For all estimates, it should be noted that the confidence intervals are large, meaning that while I can reject a null effect, the magnitude of these estimates should be treated with caution. A forty percent increase in migration rates in response to such a policy arguably stretches the limits of plausibility. A more measured interpretation of the primary specification effects with individual fixed effects (Column 2, Table 2, Panel A) is that the 95% confidence interval ranges from 0.0025 to 0.044, suggesting that the migration increased anywhere from 5 percent to 88 percent.

4.1.1 Heterogeneity by State UI Generosity

The above analysis assumes a constant impact of the policy on migration rates, despite the fact that the actual impact of the policy is likely to vary depending on the amount of UI a household is eligible for if they use the policy. To test whether the policy is more effective when UI covers a larger portion of lost income, I next estimate a model in which I interact the treatment with the income replacement rate that a household might expect, conditional

[7] The negative effects are likely due to concurrent policies that *discourage* migration, such as simultaneous expansions of monetary eligibility requirements inducing households to be less likely to leave the state.

on state, year, and household size.

To do this, I simulate the average replacement rate at the state-year level using code for a UI calculator developed in Kuka (forthcoming) and data from the 2001, 2004, and 2008 panels of the SIPP. This UI calculator identifies individuals between the ages of 18 to 60 in the SIPP who lost a job through no fault of their own and uses information on pre-unemployment earnings in the 12 months prior to losing their job and their number of children to calculate UI eligibility for each individual in each state and year. These simulated UI weekly payments are then divided by their weekly earnings pre-layoff to calculate a simulated replacement rate for each individual. I then collapse the data to the year, state, and household size level to calculate a simulated replacement rate for each state-year-household size cell. For more details on the sample and simulated rates, see Kuka (forthcoming).[8]

I then re-estimate equation 1 with the treatment variable interacted with the replacement rate, where the replacement rate is a variable that ranges from 0 to 100, meaning that the coefficient on the interaction term refers to the effect of the treatment coming from a state with a 1 percentage point higher replacement rate. Table 3 shows the results of this regression, reporting the coefficient on being treated and married and the coefficient on the interaction between being treated and married and the replacement rate. As before, column 1 omits individual fixed effects, column 2 adds individual fixed effects, and column 3 restricts the sample to households in which both spouses worked in the previous year.[9]

There is a marginally statistically significant effect of higher replacement rates on the likelihood of moving in the presence of the treatment, with a 1 percentage point increase in the replacement rate associated with a 0.4 percentage point increase in the likelihood of moving more than 50 miles. Though the Married $\times$ Treated coefficient is now negative and non-significant, recall that this is the effect in a state with a zero replacement rate. In the sample of treated states, the replacement rate varies from around 29 percent to 74 percent, with a mean value of around 41 percent. Taking the interaction term and the main effect together from the preferred specification (column 2), these coefficients imply that a married household in a state with a replacement rate above 32% will have positive impacts of the policy on migration, and households at the average replacement rate will be 3.4 percentage points more likely to move in presence of the policy than in the absence.

[8] Thanks to Elira Kuka for generously sharing the code to calculate these replacement rates.

[9] I omit from the table the results for replacement rate generosity for unmarried households for the sake of space; the coefficients of more generous replacement rates interacted with treatment are negative for unmarried households, meaning that unmarried households in more generous states are less likely to move when this policy is in place than not. This is consistent with the fact that the ARRA UI modernizations would bias downwards the effect of this policy on migration due to states simultaneously increasing state UI generosity and implementing UI for trailing spouses.

Table 3: Effect of UI Access By State Generosity

	(1) No FE	(2) Ind. FE	(3) Dual Earners Only
Married× Treated	-0.140*	-0.114	-0.139
	(0.0600)	(0.0883)	(0.0990)
Married× Treated × Replacement Rate	0.00420**	0.00360^{+}	0.00292
	(0.00141)	(0.00211)	(0.00236)
State, Year FE	yes	yes	yes
Covariates	yes	yes	yes
Ind. FE	no	yes	yes
Worked Last Year	no	no	yes
N	41583	41583	34485

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficient from a regression of likelihood of moving more than 50 miles on an indicator for access to UI eligibility for trailing spouses interacted with marital status and the income replacement rate of UI (0-100). Column 1 includes state and year fixed effects and controls including dummies for race and education, an indicator for kids, state-year unemployment rates, and state-year per capita income. Column 2 adds individual fixed effects. Column 3 restricts the sample to households where both spouses worked in the previous year. Standard errors are clustered at the state-year level.

4.1.2 Robustness Checks

I estimate a series of additional regressions to supplement the previous evidence in support of the hypothesis that UI for trailing spouses increases long-distance migration rates for married couples. Additional details on these specifications are given in Appendix Section A.2.

First, I test whether these results hold in a different data set, the American Community Survey, which also allows me to compare the effects of the policy across age cohorts. While the panel data structure and rich migration histories from NLSY97 is preferable for the main analyses, I am limited to a cohort between the ages of 23 to 34 in the NLSY97. Using the ACS, I show that the effects of the policy are smaller than those seen in the NLSY97 sample, but are marginally significant ($p < 0.10$) for those in the same age range as the NLSY97 sample and within the bounds of the confidence interval of those estimates (see Appendix Table A-3). Effects are not statistically significant for older Americans, possibly related to lower rates of migration and job switching later in life.

Second, I estimate two placebo tests: a policy implemented as part of UI modernization that should have no effect on migration (UI eligibility for part-time workers) and an outcome that should be unaffected by the policy (short-distance moves within a commuting zone). I first show that there is no statistically significant effect of UI eligibility for part-time workers on the likelihood that a household moves more than 50 miles (see Appendix Table A-5), which provides support for the identification argument that the implementation of this policy as part of the ARRA does not bias the results. I then show that UI eligibility has no impact on moves within a commuting zone (see Appendix Table A-6), which is consistent with the fact that moves that allow a person to continue commuting to their old job are not covered under this policy.

Last, I test whether this policy is associated with higher numbers of state-level claims, focusing in particular on non-monetary determinations due to voluntary separations, which is the category under which UI for trailing spouses would fall. I show that having the policy in place is associated with 3,713 more eligible claims due to voluntary separations per year, whereas the policy has no effect on the number of eligible claims for non-voluntary separations (see Appendix Table A-4). This provides additional evidence suggesting that households were aware of and used this component of the UI system.

4.2 Event Study Analysis

A second way of exploring the impacts of this policy is an event study style analysis in which I regress earnings, wages, and unemployment take-up on indicators for leads and lags around the year (month) of a move. By doing this separately for individuals living in treated states and those living in untreated states at the time of a move, I can ascertain whether movers had different earnings trajectories pre-move in treated states. As previously, I include individual fixed effects, meaning that I can interpret the coefficients as the difference in the change in earnings when moving relative to staying, controlling for an individual's typical income growth in the absence of a move. While I cannot directly observe which spouse is the leading or trailing spouse in the data[10], I use two proxies: gender, with men assumed to be more likely to be the leading spouse, and household income contributions in the year prior to the move, with the primary earner being the spouse who earned more than 50 percent of earnings.

4.2.1 Unemployment Insurance Take Up

First, I test whether this policy results in higher UI take up. One would expect that this policy should result in higher take-up of UI post-move for trailing spouse.

Figure 3 plots the coefficients from the regression of monthly UI take up on indicators for leads and lags around the move for married men and women. All regressions are on a balanced panel of individuals age 23 or higher and employed for at least one week three months prior to the move. The treatment group is defined as an individual living in a state that has the policy at the time of the move; the comparison group is defined as an individual living in a state that does not have the policy at the time of the move. I normalize the

[10] Though I observe which spouse enters a job first post-move, I would need to observe a counterfactual 'leading' spouse in couples who choose not to move.

coefficient in three months prior to a move to be 0, meaning that the point values can be interpreted as the difference between movers and stayers, adjusted to have equal levels of UI take-up prior to the move. Appendix Figure A-1 plots the same for primary and secondary earners.

Though the estimates for men and primary earners (the theorized 'leading spouse' proxies) are noisy estimates, they demonstrate two things. First, there is no significant difference in UI take-up post move for leading spouses who are treated, consistent with what one would expect. Second, there is marginally significant higher UI take up in the two months prior to the move for treated men, which then disappears post-move. This is consistent with a story in which the treatment allows households with a laid off primary earner to increase their search radius to jobs that would require their spouse to give up a local job.

Though treated and untreated female movers are no more likely to be unemployed prior to the move than stayers, treated movers have a higher UI take-up post-move relative to stayers than comparison movers. This effect persists for three months post-move and then dissipates, suggesting that these women move back into the workforce at this point. The estimates are noisier and not significant for secondary earners, but secondary earners do experience a sharp up-tick in UI take-up at the time of the move in the treatment states that is much more pronounced than the gradual up-tick seen for comparison state secondary earners.

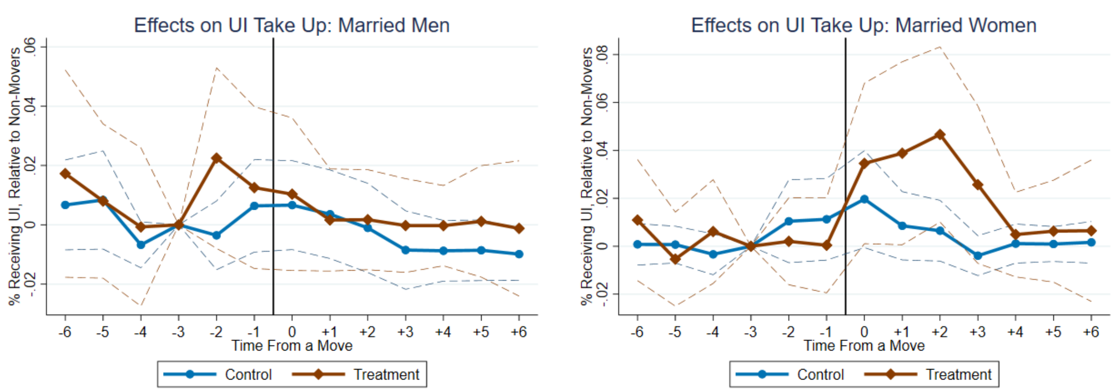

Figure 3: Effects of UI Eligibility on UI Take-Up for Men (left) and Women (right)

Notes. This figure plots the coefficients of a regression of an indicator for if a person receives UI on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. Sample restricted to married, age 23+ and those working three months prior to move. The three months prior to a move T= -3 is omitted, and the points plotted thus indicate change in unemployment take up for movers relative to stayers, normalized to be zero three months before the move. All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI shown.

4.2.2 Annual Earnings

Panel A of Figure 4 plots coefficients in the regression of annual earnings for married men; Panel B for married women; and Panel C for household income for married couples. All regressions are on a balanced panel of individuals age 23 or higher in the period 2004 through 2009.[11] The treatment and comparison group are defined as in previous analyses. I normalize the coefficient in the year prior to a move to be 0, meaning that the point values can be interpreted as earnings relative to the level of earnings prior to the move.

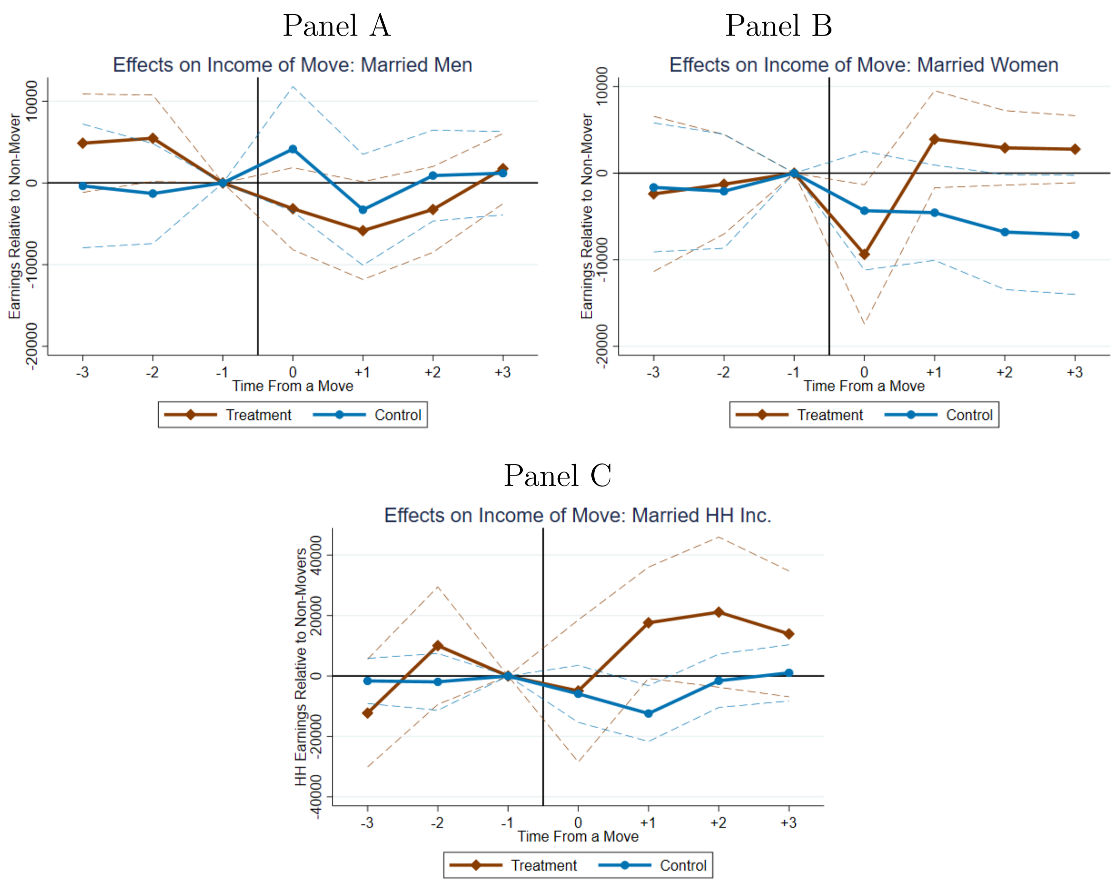

Figure 4: Effects of UI Eligibility on Earnings for Married Men, Women and Households
Notes. This figure plots the coefficients of regressions of income from wages and salary on indicators for leads and lags surrounding the year of a move across commuting zones, denoted as T= 0 in the figure. The brown diamonds indicate households with access to UI for trailing spouses; the blue circles indicates households without access. The year prior to a move (T= -1) is omitted. All regressions include individual, state, and year fixed effects. All earnings are in real 2012 dollars and standard errors are clustered at the state-year level, and 95% CI are shown.

While there are not statistically significant differences in the post-move earnings patterns for married men, there are significantly different patterns for married women with access to

[11] Because I must observe earnings three years pre- and post- move, the switch to biennial collection post-2012 means that I cannot have a balanced panel that includes moves post 2009.

spousal relocation UI and those without. While both groups experience a dip in earnings at the time of a move, women without access to UI continue to have significantly lower earnings than stayers up to three years post-move whereas those with access to UI rebound. Female movers from treated state have earnings gains that remain marginally higher (significant at the $p < 0.05$ level) than movers from comparison states three years post-move.

I also look at household income. In this analysis, human capital theories of migration predict that I should explicitly see an increase in earnings post-move if households are moving to better economic opportunities for the household as a whole (even if one spouse experiences an earnings decline). This event study in figure 4 shows a more pronounced divergence in post-move earnings for treated households relative to untreated households. While households in treated states see a significant and sustained increase in earnings post-move relative to stayers, households in untreated states see a slight decline in earnings in the initial period post-move that then rebounds back towards a net change of zero two years post-move. This is consistent with unemployment insurance for the secondary earner removing some of the frictions that prevent households from moving to higher paying jobs at a distance.

4.2.3 Wages

The results for annual earnings suggest that access to UI for trailing spouses is associated with better quality jobs post-move for women, but I cannot separate out whether this is due to individuals accepting higher wage jobs or merely working more hours. I therefore look at the post-move wages for married men and women in the presence of the policy.

Figure 5 plots coefficients in the regression of monthly wages for married men (left) and married women (right). The sample, treatment definition, and normalizations are the same as in the monthly UI take-up analysis. Appendix Figure A-4 plots the same for primary and secondary earners.

For both men and primary earners, the coefficients in the event study for wages are imprecisely estimated and do not exhibit a clear pattern post-move. Interestingly, treated male movers seem to have significantly lower wages than treated stayers in the two months pre-move, and this difference is also significantly larger than the difference between movers and stayers in untreated states. This is consistent with the evidence from the unemployment take up event studies that men who move from the treated states seem to have lost their job prior to the move.

For women and secondary earners, the coefficients are consistent with the theory that trailing spouses are unemployed or in lower wage jobs directly following a move, and access to UI allows them to get marginally better jobs upon re-entering the workforce. To better illustrate these effects, I bin the months into three-month groups in Figure 6. The graph shows a significant dip in wages for movers relative to stayers in both treated and untreated states. Six months post-move, wages have bounced back and I show that there are significantly higher wages for those with access to the policy one-year and two-years post-move, with

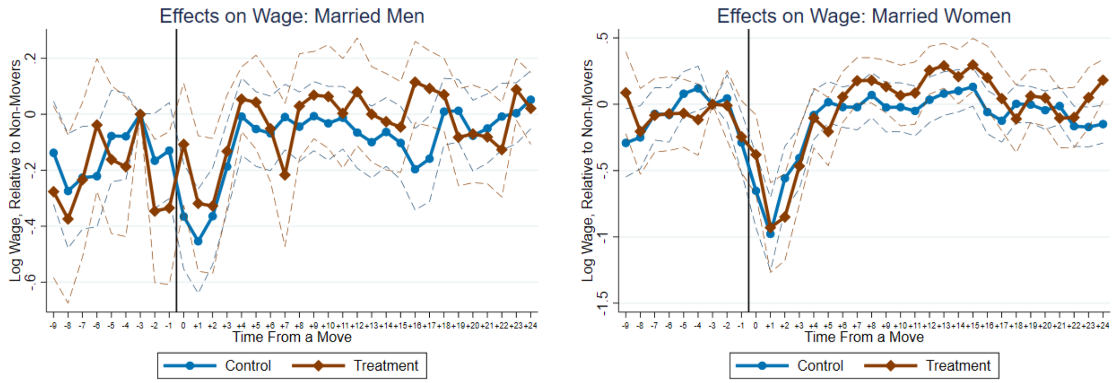

Figure 5: Effects of UI Eligibility on Wages for Men (left) and Women (right)
Notes. This figure plots the coefficients of a regression of ln(wages+0.1) on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. The brown diamonds indicate households with access to UI for trailing spouses; the blue circles indicates households without access. The period three months pre-move is omitted (T= -3). All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI are shown.

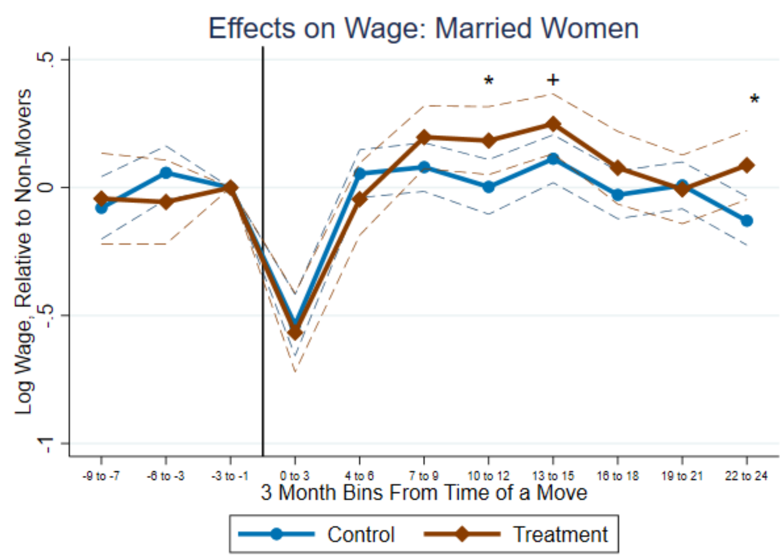

Figure 6: Effects of UI Eligibility on Wages Post-Move, Three-Month Bins
Notes. This figure re-runs thre regression in 5 using three month bins as indicators. 95% CI are shown, and stars indicate significant differences between treated and control at the 0.05 level (*) and 0.10 level (+).

stars indicating significant differences between treated and control at the 0.05 level (*) and 0.10 level (+). The average of the coefficients for treated women from six months to 24 months post-move is 0.131 and for untreated women is 0.007, which is consistent with a 13.1 percent wage gain to moving for married women from treatment states relative to a 0.7 percent wage gain for married women from untreated states. This implies that access to UI improves post-move wages one-year post move by 12.4 percent.

5 Model

When considering policies that might affect migration, it is important to consider who will respond to incentives to move and the distributional impacts of migration. As the reduced form analysis shows, policies that change migration decisions for dual-earner households can have different impacts on men's and women's post-move labor market outcomes. However, though the reduced form exercise suggests that women are more likely to be trailing spouses, this exercise cannot isolate which mechanism contributes to this phenomenon: is it because women are more likely to be the secondary earner? Are women in jobs that have less potential for earnings growth across locations? In light of these results, one might want to understand how linking migration incentives to labor market participation may reinforce or lessen the negative impacts of migration on women's earnings.

By estimating a structural model of household migration and labor force participation, I am able to test different counterfactual policy regimes under the assumption that these policy changes will impact behavior, but not the preference structure governing behavior. First, I explore how sensitive households are to region-specific earnings shocks and compare the migration elasticities found in these exercises to similar estimates from the individual migration literature. Second, I evaluate which mechanisms in the model are most important for explaining dual-earner couples' lower migration rates and women's tendency to be the trailing spouse by simulating household behavior in settings where men and women receive simultaneous distant job offers and in settings where men's and women's earning potential across locations are equalized. Lastly, I use the model to compare the migration and earning patterns for husbands and wives under a relocation subsidy tied to the leading spouse's employment, a relocation subsidy not tied to employment, and a subsidy that mirrors the incentives of UI for trailing spouses. Through these exercises, I can evaluate how different policies meant to induce migration impact men's and women's labor market outcomes, even though these policy settings have not been implemented in the US in practice.

The following section describes the model setting, the household's decision problem, and the model solution.

5.1 Model Timing

The model begins with an already-coupled household at the age of 25. Each period, the household jointly makes decisions about where to live, whether each spouse works, and which occupation each spouse works in. This decision repeats until they reach the age of 64, which is the terminal period of the model. This assumes that any future utility from the location chosen or future consumption is subsumed in the location preferences for the final period. These decisions can be summarized as each household choosing a vector of locations $J = (j_1, j_2, ...j_T)$ and labor supply choices $K = ([k_1^1, k_1^2], ..., [k_T^1, k_T^2])$ where each spouse chooses from set $k \in$ [work, receive UI if eligible, quit w/o UI] that maximizes their lifetime household utility function.

At the beginning of each period, there is some probability that each member of the household receives an offer of a job and corresponding income draw in location j. If they receive an offer in their current location and it is better than their previous job-location match, this income draw replaces their previous draw from the location-match distribution. There is also some probability that their current job is destroyed.

The realization of these probabilities then determine the choice set of the household. After receiving offers for period t, a household chooses a location-labor supply pairing (j_t, k_t^1, k_t^2). If a person's job is not destroyed, they may stay in their current job in their current location. If it is destroyed, they may be unemployed in their current location and receive an unemployment benefit. If they receive an offer in a new location, they can then choose to move to that location if it is preferred to their current draw. However, if they move and only one spouse has an offer in that location, the spouse without an offer will not be employed and will not receive UI unless their sending state has in place UI for trailing spouses. A person can always choose to leave their job and be unemployed without UI benefits in any period – including the possibility of both spouses choosing to be unemployed in a new location.

At the time of the decision, households know their current period's job offers, their current period preferences over location, and the costs associated with a move but have uncertainty over their future job and preference draws. Because the location and job one accepts changes the choice set the couple will face in future periods, the household's value function is made up of three components: known flow utility for the period of the decision, a known location preference shock component that is household-location-year specific, and expectations over future periods' utility.

5.2 Value Function

A household's decision problem in a given period is given as the following:

$$
\begin{aligned}
V &= \sum_{t=25}^{t=64} \mathbb{E}[\max_{d_t \in \mathbb{J}_t} \beta^{t-24} u(d_t, d_{t-1}, X_t) + \epsilon(d_t)] \\
& \quad \text{which can be specified in recursive form as} \\
V_t(d_{t-1}, X_t) &= \max_{d_t \in \mathbb{J}_t} u(d_t, d_{t-1}, X_t) + \beta \mathbb{E}[V_{t+1}(d_t, X_{t+1})] + \epsilon(d_t) \qquad (4)
\end{aligned}
$$

where d_t represents a household's choice of location and labor supply for each spouse (j_t, k_t^1, k_t^2) in period t. X_t represents the deterministic and stochastic state variables that a household has coming into the period including observable characteristics such as age and home location, individual-specific earnings components, their current realization of offers, and their current realization of the job destruction shock. $u(d_t, d_{t-1}, X_t)$ represents flow utility; $\mathbb{E}[V_{t+1}(d_t, X_{t+1})]$ represents expectations over future utility discounted at rate β; and $\epsilon(d_t)$ is a preference shock that is i.i.d. across time and location. Following a large strand of the discrete choice literature, I assume $\epsilon(d_t)$ is distributed Type I extreme value.

I split the state variables into two distinct groups (the choice variables and the deterministic/stochastic variables) to illustrate more clearly how choices carry across periods. Last period's choice, d_{t-1} only affects the current period's utility through the flow utility term, $u(d_t, d_{t-1}, X_t)$ (described in more detail in section 5.3). It does not carry over into expected value of utility next period, $\mathbb{E}[V_{t+1}(d_t, X_{t+1})]$, nor does it affect the location preference shock you receive this period. This is a necessary simplification to deal with an already large state space of the model; by limiting the memory to a single period, I do not need to track all previous location and job-match components.

The choice set, denoted $\mathbb{J}_t$, varies by period and depends on the choice made in the previous period along with the draws from the offer distribution and the job destruction shock in the current period. While households can choose to live in any location, they can only work in locations they have an offer in that location, and they can only receive UI if they were laid off or are eligible for UI for trailing spouses following a move.

5.3 Flow Utility

In each period, the household's flow utility is a function of their consumption, leisure, non-pecuniary utility from their location, and costs associated with a move if relevant. I assume a unitary model of the household, rather than a collective model. This is a simplifying assumption; non-unitary models typically incorporate decisions to remain in a marriage, where the likelihood of being married helps identify the solution to the Nash bargaining problem. Due to the size of the state space implicit in a migration model in which households can choose to move across states and the fact that the decision to marry or divorce are not the primary mechanisms at work in my model, I abstract away from the marriage decision, making estimation of a unitary model more applicable.[12] This is consistent with past models of household migration which allow the choice of all 50 states (i.e., Guler et al., 2012; Guler and Taskin, 2013), though Gemici (2011) uses a collective model of the household and restricts the choice set to Census regions rather than states.

For a household that chooses location j, supplies labor k_1 and k_2, previously lived in location j_0, and has observable characteristics defined by X, flow utility with time subscripts omitted can be expressed as follows:

$$\begin{aligned} u(j, k_1, k_2, j_0, X) &= \alpha_0 ln(c) + M(j, k_1, k_2, j_0, X) \\ \text{s.t. } p_j c &= w_1(j, X)\mathbb{1}(k_1 = \text{work}) + w_2(j, X)\mathbb{1}(k_2 = \text{work}) + b(j_0, k_1, k_2) + A \end{aligned} \tag{5}$$

In this function, c is household consumption and is determined fully by the household's choice of location and work. Individuals have three possible labor market statuses: working, not working with UI, or not working without UI. The cost of consumption, p_j, varies by location, capturing different costs of living across states. If they work, spouse $g \in [1, 2]$ receives earnings $w_g(j, X)$ which varies as a function of where they choose to live and individual characteristics. If they do not work and were laid off, a spouse can choose to receive benefit b; the next period, they do not receive benefits if still unemployed. Regardless of work status, households have some non-labor income A that they consume every period, acting as a consumption floor for households without employment or UI.[13]

I assume a log-consumption functional form to help match the labor force participation decisions of men and women across the earnings distribution. As shown in the reduced form exercises and in past research, women are more likely to be the trailing spouse and forgo earnings for their spouse's long-distance move. In a linear-consumption model, an extra dollar earned by one spouse is equivalent to one less dollar earned by the other spouse. In a log-consumption model, the secondary earner – often the wife – will be more willing to forgo income than the primary earner, since the marginal cost of losing a dollar of income is smaller at higher household income levels.

This functional form also means that the role of UI as a consumption smoother is one of the mechanisms through which the policy induces migration. Because I do not include a savings decision in this model, this functional form may overestimate the importance of access to UI for smoothing consumption. However, this model is primarily of migration and labor force participation decisions of young adults, who are unlikely to have large amounts of financial assets. In the NLSY97 sample, the median financial assets of the married sample are $800

[12] See Browning, Chiappori, and Weiss (2011) for an overview of unitary and non-unitary models of the households. In practice, 19.8% of the married couple households in the NLSY97 do divorce at some point in the NLSY97 sample. Comparison of the data moments used to estimate the model including and excluding couples that divorce suggest that there are not substantive differences in migration rates. Couples that eventually divorce do have significantly lower earnings on average for both men and women. Since I do not use the earnings moments in the method of simulated moments estimation and households are similar on migration dimensions, I do not exclude couples that will eventually divorce from my sample.

[13] This can be thought of as an amalgamation of all other resources, such as government transfers net of unemployment insurance or familial transfers, that a household receives and is included to prevent households from hitting a corner solution of zero consumption.

at age 25, \$2,500 at age 30, and \$6,800 at age 35. Given these low asset levels, I choose to abstract away from including savings for model tractability.

In addition to receiving utility from consumption, each household receives a location-specific non-pecuniary utility flow represented by $M(j, j_0, X)$:

$$\begin{aligned} M(j, j_0, X) &= \overbrace{-(\alpha_1 + \alpha_2(\text{age}_t - 24)\mathbb{1}(j \neq j_0)}^{\text{one-period costs of moving}} \\ &+ \underbrace{\alpha_3 \mathbb{1}(j = \text{home})}_{\text{permanent component}} + \sum_{g=M,F} \underbrace{\ell_g \mathbb{1}(k_g \neq \text{work})}_{\text{leisure preference}} + \underbrace{S_g \mathbb{1}(b_g > 0)}_{\text{UI take-up cost}} \end{aligned}$$

Location-specific utility can be split into two parts: a one-period component that only enters if a household moves (i.e., a moving cost) and a permanent component that a household receives every period they live in the location. The moving cost includes a fixed cost to moving (α_1) and a cost that is a function of age (α_2), meant to capture the fact that households move more at younger ages. The permanent component (α_3) includes a preference for living in one's home location (defined based on location where one grew up).

The other components of non-pecuniary utility relate to labor force participation (LFP) choices. To proxy for the costs/stigma associated with accessing UI and the fact that many who are eligible for UI do not receive UI, I include a constant utility cost associated with receiving benefits, S_g. This costs varies by gender of spouse who is eligible for UI and allows me to explain why individuals who are eligible for UI often do not receive UI. Also, in all periods where a spouse does not work, they receive utility from leisure, denoted ℓ_g.

5.4 Earnings Parameterization

A person's earnings are a function of where they choose to live and their individual characteristics. I parameterize earnings for spouse of gender g in household i[14] living in location j in period t as follows:

$$ln(w_{ijgt}) = \underbrace{\gamma_1^g A_{g(i),t} + \gamma_2^g A_{g(i),t}^2 + \mu_{jg}}_{\text{observed}} + \overbrace{\eta_{g(i)} + e_{g(i),t} + \theta_{g(i),j}}^{\text{unobserved to econometrician}}$$

Earnings are a function of observable characteristics of a person (γ_1^g, γ_2^g: coefficients on quadratic of age; μ_{jg}: location-gender premium) and an individual-specific residual. Due to concerns about extrapolating earnings patterns for later in life from the NLSY97 data, I assume that the age-earnings profile is flat following age 45. Following Kennan and Walker (2011), I assume that this residual term can be divided into three distinct components: an individual fixed effect, a transitory component, and a location-specific fixed effect. The first term can be thought of as capturing permanent individual sources of heterogeneity in

[14] To indicate an individual rather than gender specific component, I subscript with the term $g(i)$ to differentiate from terms that vary across gender but not individual.

earnings, such as ability or educational attainment. I assume that the terms are drawn from a discrete approximation of a normal distribution with a mean of zero and a variance, $\sigma^2_{\eta_g}$, using the method from Kennan (2006) to discretize this distribution to two points of support.[15] The second component is a transitory income shock that occurs each period, $e_{g(i),t}$, which I assume to be normally distributed with mean of zero and variance, $\sigma^2_{e_g}$, which varies by gender.

The third term, $\theta_{g(i),j}$, is an individual-location specific term and can be thought of as representing an individual's "job" match[16] which remains as long as one stays in a location-job pair but is replaced when one changes location or is laid off/voluntarily separates. This component of earnings is the primary earnings parameter that creates uncertainty about migration decisions in the model. While an individual knows the average earnings premium for someone in a distant location (μ_{jg}), they do not know how well-matched they individually will be to such a job and will not know until they receive an offer to work in that job. This uncertainty is particularly important in the dual-earner household's decisions relative to a single-earner's decision because migration decisions often happen with one member of the household moving without a job-in-hand, meaning that they have uncertainty both about how long it will take to receive an offer *and* the quality of the offer they will eventually receive. Similar to the individual fixed effect, I assume that the distribution of location-match components is drawn from a normal distribution with mean zero and variance $\sigma^2_{\theta_g}$, which can be approximated by a discrete distribution with three points of support symmetric around zero and governed by the parameter θ.

5.5 Job Offers, Job Destruction, and Preference Shocks

In addition to the stochastic components of earnings, households also receive stochastic draws from distributions that govern their location/labor supply choice set. At the beginning of the period, there is some probability that each spouse's job is destroyed and they are laid off. When laid off, they lose the location-job-match component of earnings (θ) and cannot work in that location until they receive a new offer. I parameterize this as a draw from a uniform distribution for each spouse in which a draw less than δ results in a lay off.

Each spouse also receives a draw from a job offer distribution in each location, which I again parameterize as a uniform distribution. Draws less than λ are considered an offer if in the home location and draws less than $\rho \times \lambda$ are considered an offer if in a distant location, where ρ is a value greater than zero that allows distant offers to be either more or less likely than home offers. There is an equal chance that this offer will be attached to a high, medium, or low location-job-match. These offers are independent across location and across spouses,

[15] I omit educational attainment from the observable characteristics purely for computational tractability as each additional household type increases the state space exponentially. I weight the points of support for the η term such that the proportion of individuals with the 'high' draw is equal to proportion with a college degree in the population.

[16] This is a slight abuse of the term "job" as I will not be measuring distinct job tenures across terms. Here I use job to refer to one's tenure within a location uninterrupted by a period of unemployment.

meaning that there is a fairly low probability that both spouses will have an offer in the same location simultaneously.

Each period, households also receive a preference shock draw in each location (ϵ) which is drawn from a Gumbel distribution with a location of zero and scale normalized to one.

5.6 Model Solution

Because there are only a finite set of periods, the household's optimal decision can be solved recursively starting in period T, where $\mathbb{E}[V(d_T, X_{T+1})] = 0$. In period T, a household has full information over all realizations that will affect their utility, making their decision a simple discrete choice problem:

$$V(d_{T-1}, X_T) = \max_{d_T \in \mathbb{J}_T} u(d_T, d_{T-1}, X_T) + \epsilon(d_T) \tag{6}$$

where

$$\begin{aligned} d_T^* &= \{\hat{j}, \hat{k}_1, \hat{k}_2\} \text{ if} \\ u(\hat{j}, \hat{k}_1, \hat{k}_2, j_{T-1}, X_T) + \epsilon(\hat{j}, \hat{k}_1, \hat{k}_2, X_T) &> u(d_T, j_{T-1}, X_T) + \epsilon(d_T) \ , \forall d_T \in \mathbb{J}_T \backslash \{\hat{j}, \hat{k}_1, \hat{k}_2\} \end{aligned}$$

Moving backwards, I then can use the functional form assumptions previously described for the stochastic elements of utility, along with the decision rule for period T to rewrite the expectation in period $T-1$ as:

$$\begin{aligned} V(d_{T-2}, X_{T-1}) = & \max_{d_{T-1} \in \mathbb{J}_{T-1}} u(d_{T-1}, d_{T-2}, X_{T-1}) \\ & + \beta \sum_{\mathbb{J}_T} P(\mathbb{J} = \mathbb{J_T} | \lambda, \delta, d_{T-1}) \sum_G \sum_G \int_{N(0,\sigma_1^2)} \int_{N(0,\sigma_2^2)} ln \Big[\sum_{d_T \in \mathbb{J}_T} exp\big(u(d_T, d_{T-1}, X_T)\big) \Big] \\ & + \epsilon(d_{T-1}) \end{aligned} \tag{7}$$

The household is taking expectations over:

1. $P(\mathbb{J} = \mathbb{J_T} | \lambda, \rho, \delta, d_{T-1})$: The likelihood of having a given choice set $\mathbb{J}$ in period T, which depends on their choice this period and their likelihood of job offers and destruction
2. $\sum_G$: Realization for the job-match earnings component for their future offer, for each spouse
3. $\int_{N(0,\sigma_g^2)}$: Realization for the transient earnings component, for each spouse
4. $ln\Big[\sum_{d_T^* \in \mathbb{J}_T} exp\big(u\big)\Big]$: Realization of Type I EV location shock

The functional form assumptions allow me to solve out the expected continuation value for every possible choice in period $T-1$ only as a function of the state variables for period $T-1$. This then becomes, once again, a discrete choice problem of observed values where d^*_{T-1} is the location-LFP combination that results in the highest possible value out of all combinations in the choice set. In practice, I estimate the expectations over the choice set, the job-match earnings components, and the transient earnings components using Monte Carlo simulations, drawing r=100 combinations of shocks and taking the average continuation value over those 100 draws. This process continues recursively back to period 1.

5.7 State Space and Initial Conditions

In the first period, a household enters the model in a starting location (j_0), has starting labor force participation states (k^1_0, k^2_0), has job-match components for their previous job if working ($\theta_{j_0,1}$, $\theta_{j_0,2}$), has permanent earnings components (η_1, η_2), and has observable characteristics (age$_1$, age$_2$, home location j_h).

In periods $t > 1$, a person's state space evolves based on a household's choice, deterministic values, and stochastic processes. The previous location and previous LFP decision update to be the choice made in the previous period, as does the job match component of earnings. Home location and the individual fixed effect component of earnings are permanent and carry over from the previous period. Age increases deterministically. The stochastic elements that affect the choice but are not carried across periods include job offers, job destruction, and the location preference set, which the household receive as a new draw from known distributions each period.

The size of the state space in a given period is then

$$\underbrace{N^2_{\text{loc}}}_{\text{Start Location, Home}} \times \overbrace{(4 \times N_{\text{age}} \times 2) \times (4 \times N_{\text{age}} \times 2)}^{\text{For each spouse: types of LFP (Unemployed, Low, Medium, High } \theta\text{), Age Types, } \eta \text{ types (High and Low)}}$$

If I allow mainland US states to be the unit of location and have all individuals start at the same age for both the husband and wife, there are 147,456 states to solve value functions for in each period.

6 Structural Model Empirical Strategy

Table 4 lists the model parameters to be estimated. Theoretically, I could estimate all of the parameters simultaneously using indirect inference. However, the number of parameters makes this computationally intensive. I therefore determine the parameters in three steps. First, I estimate the parameters governing the earnings equations outside the model using a selection-corrected OLS regression and the covariance structure of the earnings residual for individuals across time and location, using a method from Kennan and Walker (2011). Second, I take the policy parameters such as the price index, UI benefits, and lay-off rate from data outside the model. Finally, I estimate the remaining 11 parameters using indirect inference. The following section describes the data sources and estimation methods.

Table 4: Parameter Definitions

Parameter	**Description**	**Estimation Type**
μ_{jg}	Location Wage Premium	ACS data, Selection-corrected OLS
$\gamma_1^M, \gamma_2^M, \gamma_1^F, \gamma_2^F$	Age-earnings Profile	ACS data, Selection-corrected OLS
θ_M, θ_F	Earnings Residual, Job-location Match	NLSY97 data, residual decomposition
η_M, η_F	Earnings Residual, Individual FE	NLSY97 data, residual decomposition
e_M, e_F	Earnings Residual, Transitory	NLSY97 data, residual decomposition
p_j	Location Price Index	ACCRA Cost of Living Index (Q1 2019)
b_{jgt}	UI benefit level	Kuka (forthcoming) and UI for trailing spouses data
δ	Annual layoff rate	JOLTS 2005-2018
α_0	Consumption Scaling	Indirect Inference
α_1, α_2	Moving Cost	Indirect Inference
α_3	Home Preference	Indirect Inference
ℓ_M, ℓ_F	Leisure Value	Indirect Inference
S_M, S_F	UI Take Up Cost	Indirect Inference
λ	Local Offer Rate	Indirect Inference
ρ	Scaling for Distant Offer	Indirect Inference
A	Non-labor Income	Indirect Inference

Notes. This table lists the parameters in the model, descriptions of the parameters, and the estimation technique/data source for estimating the parameters.

6.1 Step #1: Estimation of Earnings Parameters

I estimate the wage parameters outside of the model of migration in two steps.

As a reminder, earnings are specified as follows:

$$ln(w_{ijgt}) \quad = \quad \gamma_{1g}A_{g(i),t} + \gamma_{2g}A^2_{g(i),t} + \mu_{jg} + \eta_{g(i)} + e_{g(i),t} + \theta_{g(i),j}$$

While I would ideally estimate the earnings parameters in the model, the number of location fixed effects make this computationally infeasible. Because a simple linear regression of earnings on age and state fixed effects would be biased by selection into location, I use the method described in Dahl (2002), where selection correction takes the form of an unknown function of the first best probability of location choices. In this method, one classifies people into 'cells' based on observable characteristics and calculates the probability that a person within that cell chooses to move from location j to location k to get a distribution-free estimate of the selection probability. Then, this first-best probability is included in the regression using a flexible functional form (i.e., a polynomial approximation of the unknown function).

I use my structural model to inform the characteristics used to form the cells and categorize people based on the components of my model which should impact migration likelihood but not own earnings other than through location and LFP decisions: location at birth, location in year prior, employment status of one's spouse, age (25-30, 30 to 35, 35 to 40, 40 to 45), and whether the state in the year prior offered UI for trailing spouses. I estimate the parameters governing the age distribution and μ_{jg} using ACS data from 2005 through 2016, restricted to individuals 25 to 45 who are married in the year of the survey and the year prior to the survey.[17] I drop individuals in cells in which the number of observations in the ACS is less than 50. I regress log earnings from salary and wages on a constant, a quadratic of age, indicators for state separately by gender, and a quadratic polynomial of the first-best probability of choosing a location for one's cell.[18] I then define $\hat{\gamma}$ as the coefficients on age and $\hat{\mu}$ values as the fixed effects plus the constant.

To identify the error distributions, I need to observe earnings over time and location. Because the job match term is constant for individuals who do not move locations, the individual permanent effect is constant across locations and periods, and the transitory shock varies across periods but not locations, I can use the panel structure of the NLSY to separate out the variances of each component. For each individual, I calculate the residual earnings for person $g(i)$ in year t as:

$$Y_{g(i),t} = ln(w_{ijgt}) - \hat{\gamma}_{1g} A_{g(i),t} - \hat{\gamma}_{2g} A^2_{g(i),t} - \hat{\mu}_{jg} = \eta_{g(i)} + e_{g(i),t} + \theta_{g(i),j}$$

I then stack these residuals to be the vector $Y_{g(i)}$, and for each individual, I define a covariance matrix $\omega_{g(i)} = Y_{g(i)} Y'_{g(i)}$.[19] These matrices can be split into three parts that correspond to three expressions that help me identify the distributions of the error terms:

1. Diagonal terms: variance of unobservable term over time $= \sigma^2_{\eta_g} + \sigma^2_{e_g} + \sigma^2_{\theta_g}$
2. Same-location off-diagonal terms: covariance of earnings within location across period $= \sigma^2_{\eta_g} + \sigma^2_{\theta_g}$
3. Different-location off-diagonal terms: covariance of earnings across location and period $= \sigma^2_{\eta_g}$

By taking the sample average of the unbalanced panel of these elements, I get three estimates A1, A2, and A3, where A3 is a consistent estimator of the population-wide variance of η, A3-A2 is a consistent estimator of the population-wide variance of θ, and A1-A2 is a consistent estimator of the population-wide variance of e.[20]

[17] I use the ACS rather than NLSY97 to estimate the μ terms because the small sample size of NLSY97 does not have enough observations per state in some cases to accurately gauge mean earnings by state.

[18] I also estimate this for a cubic and quartic; it does not substantively change the estimates.

[19] I assume that the residual terms are independent across spouses. I have estimated a version in which I use the joint covariance of husband's and wife's residuals to recover the correlation in these earnings components across spouses due to assortative matching. I find $\rho_\eta = 0.19$, $\rho_e = 0.10$, and $\rho_\theta = -0.43$. These correlation terms are consistent with a story in which there is slight positive assortative matching, and spouses tend to up mismatched in terms of location. Future iterations of the model will relax this independence assumption.

[20] For a more detailed discussion of the intuition behind this identification method in the context of migration, see Kennan and Walker (2011).

I then discretize the distributions of the θ and η terms using the method put forth in Kennan (2006). Kennan (2006) shows that the best discrete approximation of a distribution $F(x)$ has n equally-weighted support points $x_i \in \{x_1, ..., x_n\}$ where

$$F(x_i) = \frac{2i-1}{2n}$$

For the η terms, I assume there are two support points, which gives me $\eta^g_{low} = \hat{\sigma}^2_{\eta_g} \times \Phi^{-1}(0.25)$ and $\eta^g_{high} = \hat{\sigma}^2_{\eta_g} \times \Phi^{-1}(0.75)$.[21] For the θ terms, I assume there are three support points, which gives me $\theta^g_{low} = \hat{\sigma}^2_{\theta_g} \times \Phi^{-1}(0.167)$, $\theta^g_{mid} = 0$ and $\theta^g_{high} = \hat{\sigma}^2_{\theta_g} \times \Phi^{-1}(0.833)$.

6.2 Step #2: Calibrated Parameters

I calibrate the discount rate to be $\beta = 0.95$.

6.2.1 UI Benefits Schedule

To calibrate the UI benefit level, I simulate the average replacement rate at the state-year level using the same method as in the design-based analysis (see section 4.1.1): a UI calculator developed in Kuka (forthcoming) and data from the 2001, 2004, and 2008 panels of the SIPP.

I define benefits for a person of age a, gender g, and living in state j as:

$$b_{agj} = 0.5 \times reprate_{1982+a,j} \times exp(\gamma_{1g}a + \gamma_{2g}a^2 + \mu_{jg})$$

I assign the replacement rate to approximately match the age profiles of the NLSY97. I multiply the replacement rate by half of the average predicted annual earnings for someone of that age and location, which captures the fact that most states offer 26 weeks of unemployment insurance (i.e., one-half of a year).[22]

Workers who are laid off in the model receive this benefit for the first year following their layoff. If the person continues to not work for more than one year, they receive a benefit of 0. To incorporate UI for trailing spouses, I create a secondary UI benefit calibration which people receive if they move to a new location with only one spouse working. This benefit calibration sets the level of benefits equal to 0 for sending states which do not have the policy and equal to the formula above for states that do have this policy.

[21] I weight the sample to have fewer high types than low types, using the proportion of individuals in the sample with a college degree as the weighting for high types.

[22] I abstract away from the fact that UI was extended during part of this time period due to the Great Recession, making this an underestimate for some years. However, since not all recipients would use all 26 weeks, this is also an overestimate for some observations.

6.2.2 Lay Off Distribution

To calibrate the parameter governing the lay off distribution, I use the annual layoff rate from the Job Openings and Labor Turnover Survey (JOLTS). The U.S. Bureau of Labor statistics calculates the annual discharge and layoff rate as the number of layoffs and discharges during the entire year as a percent of annual average employment. I take the average of this value across years 2005 through 2018 and assign this value as the probability that a person is laid off in a given period.

6.2.3 Price Index

To account for differences in cost of living, the price of consumption varies by location. To calibrate these prices, I use the ACCRA cost of living composite index for all metro/micropolitan areas in the United States, which incorporates costs of housing, utilities, groceries, transportation, health care, and miscellaneous goods/ services. I use the 2019 Q1 through 2020 Q1 index, averaged across all cities within a state. I normalize prices to be 1 in Pennsylvania.

6.3 Step #3: Utility Parameters

I use indirect inference for estimation of the remaining parameters using the following 21 moments:

- Likelihood of move each age 25-35 (1 moment $\times$ 11 periods)
- Average likelihood of living in home location between 25-35 (1 moment)
- Percent of moves that are to and from home location between 25-35 (2 moments)
- Percent working by mover type and gender, age 25-35 (4 moments)
- Percent eligible for UI who are receiving UI, by gender, age 25-44 (2 moments)
- Reduced form coefficient from regression of likelihood of move on UI treatment (1 moment)

I calculate the vector of data moments, m^d, from data from the NLSY97, the ACS, and the CPS. Because of the small proportion of households who move, the sample of movers in the NLSY97 is too small to calculate the percent working by mover type. I therefore use the NLSY97 only for the likelihood of moving, likelihood of living in the home location, and likelihood of moving in and out of the home location. I use households where the respondent is between the ages 25 to 35, married in the year of the interview, the year prior to the interview, and the year following the interview, and has non-missing location, earnings, and employment status data. This gives me a sample of 1936 households who are used to calculate these moments.

I use the ACS for the employment status for movers and for the full sample. The ACS asks households where they lived in the previous year; I define a move as living in a different state the year prior to the survey. To make the data comparable to the NLSY97 data, I restrict the sample to individuals who were in the same age ranges as the NLSY97 cohort, keeping only those who were aged 25 between aged 35 between 2005-2017. I also restrict the sample to households that are married in the year of the survey. The full sample of men and women include 455,188 observations across all years and ages, and percent employed are tabulated by age, gender, and migration status.

I use the Current Population Survey's Unemployment Non-Filers Supplement to calculate moments on the percent of unemployed individuals who are receiving UI. I calculate the percent of non-working individuals who were working in the last 12 months who receive UI among those between the ages of 25 to 45. Both individuals who did not apply and those who applied but did not receive UI are considered to be non-recipients. 25.6 percent of men received UI and 24.8 percent of women received UI.

Lastly, I take advantage of the policy variation in UI for trailing spouses to try to match the effect of the policy on cross-state moves, as estimated in the first reduced form exercise. I regress likelihood of a move in my simulation on an indicator for having access to the policy along with state, year, and individual fixed effects. The coefficient on the treatment then corresponds to the coefficient on the treatment in Column 2 of Panel C of Table 2.

I then calculate the vector of simulated moments, m^s, for each guess of the parameter vector, $\psi^U =$[α_0, α_1, α_2, α_3, ℓ_M, ℓ_F, A, λ, ρ, S_M, S_F] by solving the model backwards for each guess and then simulating the decisions of a sample of 10,000 households. The starting states are a sample in which I draw the starting location, home location, and starting employment status for each spouse by drawing with replacement from the NLSY97 household sample at age 25 and randomly assigning spouse type $\eta_{\{H,L\}}$.

The parameter estimate is given by the expression:

$$\hat{\psi}^U = \text{argmin} \frac{1}{N_{\text{moments}}} \sum_{i=1}^{N_{\text{moments}}} \left(\frac{m_i^s(\psi^U) - m_i^d}{m_i^d} \right)^2$$

I find the minimizer using the Nelder Mead algorithm and choosing a starting point for the algorithm by drawing 1000 draws from a Sobol hypercube.

Standard errors are computed using the standard GMM formula. Because simulation error in method of simulated moments impacts the smoothness of the moment function and can thus induce bias in the standard errors, I follow the procedure used in Lise and Robin (2017). I evaluate each moment at an equally spaced grid of 101 points around each parameter ψ_m in the range $[0.5\psi_m, 1.5\psi_m]$, holding all other parameters constant at their estimated values. I then fit the predicted moments and the grid point to a polynomial of degree 9. The predicted derivative $\frac{\partial \hat{m}^s}{\partial \psi_m}$ is then used in place of the numerical differentiation of the moments in the standard formula.

7 Model Results

7.1 Wage Parameter Estimates

Table 5 reports the parameters governing the age earnings profile and the variances of the unobservable components for men and women.

Table 5: Estimates of Earnings Parameters

Panel A: Age Parameters	γ_{1M}	γ_{2M}	γ_{1F}	γ_{2F}		
	0.153***	-0.00183***	0.108***	-0.00138***		
	(0.00155)	(0.00002)	(0.00227)	(0.00003)		
Panel B: Variances	η_M	η_F	θ_M	θ_F	e_M	e_F
	0.5878***	0.2331***	0.0409	0.0294	0.2958***	0.1947***
	[0.1444]	[0.0411]	[0.0342]	[0.0222]	[0.0275]	[0.0230]
Panel C: Discretized Parameter	0.3965	0.1572	0.0395	0.0284		

OLS Estimates SE in parentheses; Bootstrapped Estimates SE in brackets, using 5000 draws of 2000 obs.; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the estimates of the structural parameters for the wage equations for men and women. Panel A reports the parameters governing the age-earnings profile, estimated using selection-corrected OLS in a sample of married men and women aged 25 to 35 in the ACS between 2005 and 2016. Panel B reports the variances of the unobservable components of earnings, estimated using the covariance structure of NLSY97 sample. Panel C reports the parameter governing the discretized version of the distributions of η and θ.

Panel A reports the parameters governing returns to age estimated using OLS for men in column 1 and 2 and for women in column 3 and 4. Panel B of Table 5 reports the parameters governing the distributions of the three residual terms: the individual fixed effect (η), the location-match component (θ) and the transient component (e). Panel C reports the parameter governing the discretized version of η and θ. I assume η is drawn from a discrete approximation with two support points equal to the parameter and the negative of the parameter. I assume θ is drawn from a discrete approximation with three support points equal to the parameter, zero, and the negative of the parameter.

To make these parameters more interpretable, I convert the values for η and θ to dollar value rather than leaving them in terms of log-earnings. To do this, I calculate how much more a person would make if the parameter was added to a base salary of \$40,000. Adding η_H would increase earnings by \$19,500 for men and by \$6,800 for women. Adding θ_H would increase earnings by \$1,600 for men and by \$1,200 for women.

7.2 Utility Parameters

Table 6 reports the parameters estimated using indirect inference, as well as the calibrated lay-off rate, δ.

Table 6: Estimates of Utility Parameters

	α_0	α_1	α_2	α_3
Cons. Scaling and Moving Costs	0.64×10^{-2}	0.0239	-0.01×10^{-2}	0.0045
	(0.022×10^{-2})	(0.0001)	(0.36×10^{-2})	(0.0019)
	ℓ_M	ℓ_F	S_M	S_F
Leisure and UI Costs	-0.0037	0.0018	-0.0038	-0.0010
	(0.0065)	(0.0096)	(0.0017)	(0.0032)
	A	ρ	λ	δ
Non-labor Income and Offer Distribution	0.299	0.860	0.520	0.163
	(0.116)	(0.120)	(0.088)	

Notes. This table reports the parameters estimated within the model. The first row reports the consumption scaling parameter (α_0) and moving cost parameters including the fixed cost (α_1), the parameters on the age-varying cost (α_2), and the preference for home (α_3). The second row reports the values of leisure by gender (ℓ_M, ℓ_F) and the UI take up costs by gender (S_M, S_F). The third row reports the non-labor income parameter (A) and the parameters governing employment: scaling for likelihood of a distant offer (ρ), likelihood of a offer (λ), and likelihood of layoff (δ, calibrated outside the model). Standard errors in parentheses.

For all utility parameters other than A (which is in \$1000 increments), the value given is in utility units rather than dollars units. To interpret these values, I can convert the moving costs and leisure values into dollar terms using the consumption scaling parameter, $\alpha_0 = 0.0064$. Because utility for consumption is non-linear, I can express costs only as a function of a base income/consumption level rather than as an exact dollar equivalent. This implies that the costs associated with moving and/or not working are by assumption larger for households with higher incomes. The form of utility implies that any costs in utility units denoted with X, is equal to Y dollars lost based on the following formula, where C_0 is consumption from income:

$$\begin{aligned} 0.0064\ln(C_0+Y) &= 0.0064\ln(C_0)+X \\ Y &= C_0\left(\exp\left[\frac{X}{0.0064}\right]-1\right) \end{aligned}$$

For example, the leisure value for women of 0.0018 implies that they benefit more from not working and consider its value equal to $0.32 \times C_0$, which at age 25 is \$16,500 in a household with average earnings. Conversely, the men's leisure value of -0.0037 implies that the household values men's leisure negatively and would be willing to take a 43% paycut to keep his job and remain in the workforce. This leisure values are, however, fairly noisily estimated. This negative 'leisure' value for men is likely actually tapping into the fact that the model does not induce gendered patterns of labor supply other than through differences in earnings and the 'leisure' parameter. Notably, I do not include the role that fertility might play in why women are less likely to work, and the relative value of these parameters are therefore sink parameters for unobserved factors such as this.

Turning to the moving costs, the fixed costs of moving at age 25 is equal to 0.0239 utility units, which in dollar terms for a household with average earnings at age 25 in the sample would be $2.1 million. However, these are only the non-pecuniary costs of forcing a random household to move, ignoring the fact that households that actually move are moving in part due to the location preference shocks, ϵ. The switching costs of moving conditional on actually moving are much lower. Since a mover from location j_0 to j_1 exchanges ϵ_{j_0} for ϵ_{j_1}, the average moving cost will then be:

$$\mathbb{E}[MC|j_0, D^* = j_1] = \alpha_1 + \alpha_2(\text{age}_t - 24) + \mathbb{E}[\epsilon_{j_0} - \epsilon_{j_1}|j_0, D^* = j_1]$$

Because my estimation method involves simulating the ϵ realizations, I can "observe" the ϵ each simulated household receives in their first-best location choice and the ϵ they would have received that period if they were to stay in the location they start the period in. I use these to calculate the moving costs inclusive of the location preference shock change and find that the average moving cost conditional on moving at age 25 is negative: -0.396 utility units, which corresponds to -$20,500 for a household with average earnings. This negative moving cost conditional on moving implies that the move is being driven by a household having either a very large ϵ draw in the new location or a very small ϵ draw this period in their current location, rather than the prospect of high future utility flows from that destination. This low, negative cost to moving conditional on a move is consistent with findings in individual migration models, such as Kennan and Walker (2011) which also finds high fixed costs to move but negative costs conditional on migration.

7.3 Model Fit

The model matches the data moments well. Figure 7 plots the data moments against the moments for the sample simulated for estimation. I am able to fit the general pattern of migration rates by age, capturing the higher migration rate at age 25 which declines over time and the fact that the annual migration rate averages around 3.5 percent. I am able to match the fact that the treatment increases migration rates, with the simulation predicting an increase in cross-state moves of 1.8 p.p. in states with the treatment. Given that this quasi-natural policy experiment is part of what helps me identify the decision for households to move, matching this parameter is particularly important. I also capture the fact that the migration rate to one's home location is higher than migration from home, though I slightly underestimate the movement to the home location and thus slightly underestimate the proportion in the home location.

I match the employment rates fairly well for women but somewhat overestimate employment for men post-move. I do, however, capture the overall gendered pattern of employment of movers and stayers, with men's employment rate being high regardless of mover status and women's employment post-move declining relative to overall employment.

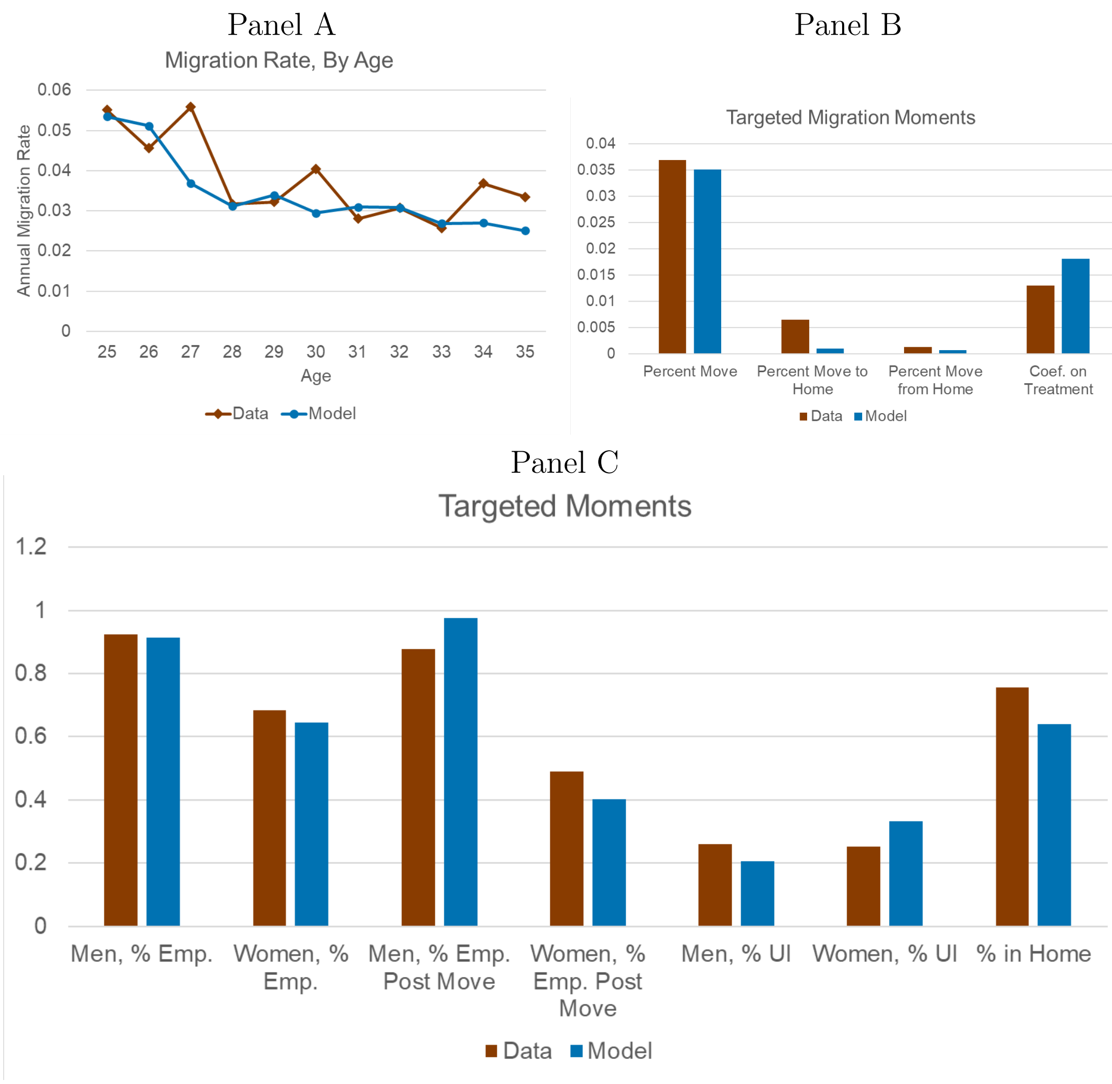

Figure 7: Model Fit: Targeted Moments

Notes. This figure plots simulated data moments and the NLSY97 data moments used to calibrate the utility parameters. Panel A shows the migration rate, defined as the percent of households that move across state lines at a given age, both overall and moves from the home location. Panel B reports migration moments. Panel C reports the remaining model moments (percent employed overall, percent employed post-move, percent of non-employed receiving UI, and percent in home location).

8 Counterfactuals

This model now allows me to conduct a series of analyses to test how sensitive household's migration rates are to different counterfactual policy regimes.

8.1 Migration Elasticities

In the first set of counterfactuals, I test how elastic migration is to state-specific labor market shocks. Do households' location choices respond to increases in local wages? To answer this question, I increase the location-specific earnings components (μ) by 10% in one state at a time and simulate how households would behave if this shock happens in the first period of the model and persists. I then calculate the percent change in the population residing in that location, $\frac{\Delta \text{Pop}}{\text{Pop}}$, and the elasticity is then this value divided by 0.10.

Figure 8 reports the percent change for three large representative states: California, New York, and Illinois. The elasticity of location choice to earnings varies across locations. California's population is the least elastic, converging around an elasticity of 0.25 within 7 years of the earnings shock. Illinois is more responsive, with the population increasing 10 percent in response to the 10 percent earnings shock. New York is slower to respond with the percent in population change remaining low in the first few years of the shock, but then increasing until it flattens out 13 years post-shock at an elasticity of around 1.6.

These differences across states demonstrate that the effects of earnings shocks in different parts of the country vary, in part due to differences in how the earnings shock affects population. In the absence of the shock, households are more likely to leave Illinois and New York than enter and vice versa for California. The shock causes Illinois and New York to retain households that would have left without the earnings gain whereas the shock induces more in-migration in California. My findings suggest that retention is more sensitive to earnings shocks than in-migration.

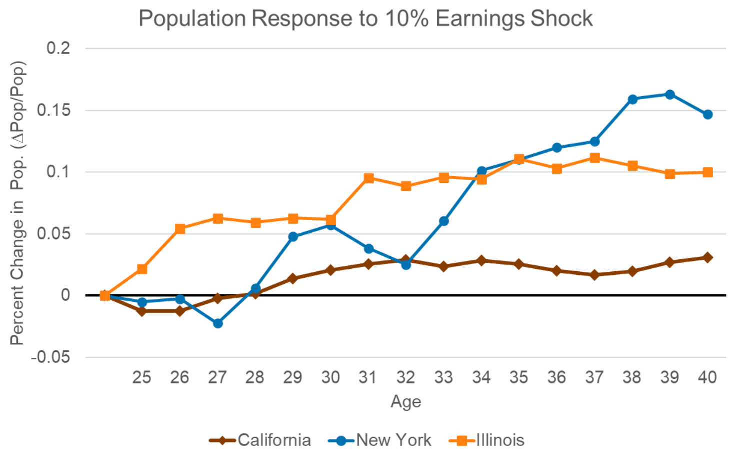

Figure 8: Responses to 10% Earnings Shock
Notes: This figure plots the percent change in the population in California, Illinois, and New York in response to a 10% increase in location-specific earnings component μ.

8.2 Evaluating Mechanisms

In the second set of counterfactuals, I explore how households behave in a series of hypothetical scenarios which change the spatial frictions associated with dual-earner moves. Table 7 reports the effects of these counterfactuals on migration measures. I report the annual migration rate, the number of lifetime moves, and the percent of households who ever move under each counterfactual. Column 1 reports the baseline migration rates.[23]

First, I evaluate how gender differences in earnings contribute to the tendency of women to be trailing spouses in two exercises. In counterfactual 1, I equalize men's and women's earnings by setting both men's earnings parameters equal to women's earnings parameters. In the counterfactual 2, I do the opposite, setting women's earnings parameters equal to men's. Both of these counterfactuals test whether having more equal earnings within a household makes it less likely to move – one would expect that households in which both spouses are equal contributors will be more likely to end up a in a 'tied stayer' situation where one spouse is unwilling to accept a distance offer due to their spouse's career. The two counterfactuals which equalize earnings result in lower migration rates, consistent with the prediction from Mincer (1978)'s model of migration in which households with more equal earnings face greater spatial search frictions. Setting men's earnings distribution equal to women's earnings distribution (CF1) has the larger effect, reducing overall migration rates by 0.49 p.p. or 23.7%

In the third and fourth counterfactual scenarios, I ask the question: what would happen to migration rates if we remove the possibility that one spouse wants to work? To simulate

[23]Annual migration rates are averaged across all ages 25 to 64, resulting in a lower average migration rate than was used in a matched moments .

scenarios where each gender always prefers being a stay-at-home spouse, I set the leisure parameter equal to 5 for women in counterfactual 3 and men in counterfactual 4. This makes leisure always preferable to working in my model. I find that households move much more frequently when one spouse does not work, likely due to the fact that there is no longer any disagreement between spouses in terms of the most preferred location earnings-wise. When all women are stay-at-home spouses, the annual migration rate more than doubles, going from 2.1% of households moving annually to 4.4%. There are similar, though smaller, effects in CF 4 in which all men are stay-at-home spouses (3.3% annual migration). Though much of the earlier analyses focused on the negative impacts of moves on female spouses who were tied movers, these counterfactuals indicate that there are also a great deal of spouses who are tied stayers who would move if their spouse's earnings were not a consideration.

Finally, I evaluate the importance of joint offers in explaining lower migration rates for dual-earner couples. This is the primary spatial search friction that I build into the model – how much of married couples' low migration rates can be explained by the fact that they are hesitant to move without a job offer for both spouses? In counterfactual 5, I simulate how households would behave if both spouses received distant job offers simultaneously, rather than receiving independent job offer draws across locations. To do this, I use the men's job offer location draws as the draws for both spouses.[24] Incorporating simultaneous job offers has a large positive effect on migration, confirming that the difficulty of finding two jobs at the time of the move contributes to low migration rates for married households. This counterfactual increases the overall annual migration rate by 1.09 p.p. or 56%.

Table 7: Equalizing Labor Market Opportunities Across Genders: Effects on Migration

	Model	CF1	CF2	CF3	CF4	CF5
% Move: All	2.07	1.58	1.81	4.35	3.26	3.16
Number of Moves: All	0.83	0.63	0.72	1.74	1.30	1.26
% Ever Move: All	69.63	58.49	63.73	89.47	81.04	81.21

Note. This table reports the results of the first set of counterfactuals. Column 1 shows the baseline model results, column 2 sets both earnings distributions equal to women's distribution, column 3 sets both earnings distributions equal to men's distribution, column 4 makes all women stay-at-home spouses, column 5 makes all men stay-at-home spouses, and column 6 implements simultaneous job offer draws.

I also look at the effects of these scenarios on men's and women's labor market outcomes at the time of a move. Table 8 reports the proportion of movers employed one-year post move by gender and the change in earnings for movers by gender, where the change is the difference between earnings one year post-move and one-year pre-move. In the baseline outcomes, men are on-average almost always employed post-move whereas only half of women are. Men gain on average \$9,800 with a move whereas women gain \$2,100. For comparison, the average earnings gain for non-mover men in the baseline is \$1,600 and for women is \$50, so while both male and female movers are gaining more at the time of a move than stayers, the gain is much larger for men.

[24] Specifically, men's offers remain unchanged from the baseline, but women's offers are dropped and instead replaced with an offer in any location that the husband received an offer in.

Obviously, men's post-move earnings decline when they draw earnings from the women's distributions (CF1), and women's post-move earnings increase when they draw earnings from the men's distribution (CF2). Interestingly, however, both scenarios increase the likelihood that women work post-move. This is especially true when men have lower earnings in CF1; households compensate for men's lower earnings by adding a second earner. Women remain more likely to be tied movers than men, but the decline in employment post-move relative to non-movers is smaller when earnings are equalized.[25]

In the stay-at-home spouse counterfactuals (CF3 and CF4), both men and women are better able to move to a high-earning location than in the baseline. When all women do not work, men's post-move earnings gain increases to \$25,000 from \$9,800. Women's earnings gain at the time of a move increases from \$2,000 to \$17,000 when men do not work. The size of these gains speak to the fact that spouses' location preferences over jobs prevent moves, resulting in individuals not being able to sort into high paying labor markets

There is also a large impact in the final hypothetical (CF5) in which spouses receive offers simultaneously. The proportion of women employed post-move increases by 8.7 p.p or 16.7%. Women's earning gains grow to \$8,500 and men's earnings gains also grows to \$17,600. Similar to counterfactuals 3 and 4, these results demonstrate that weakening the spatial search frictions associated with moving two jobs rather than one can allow households to move to high-paying locales.

Table 8: Equalizing Labor Market Opportunities Across Genders: Effects on Labor Market Outcomes

	Model	CF1	CF2	CF3	CF4	CF5
% Emp. Post-Move, Men	91.62	91.57	90.33	93.56	0.00	92.51
% Emp. Post-Move, Women	51.67	72.06	65.59	0.00	77.05	60.32
Δ Earnings at Move, Men	9.82	3.69	9.40	25.09	0.00	17.63
Δ Earnings at Move, Women	2.12	3.52	9.98	0.00	17.18	8.52

Notes. This table reports the results of the first set of counterfactuals for movers only. Column 1 shows the baseline model results, column 2 sets both earnings distributions equal to women's distribution, column 3 sets both earnings distributions equal to men's distribution, column 4 makes all women stay-at-home spouses, column 5 makes all men stay-at-home spouses, and column 6 implements simultaneous job offer draws. Row 1 and 2 report percent employed in the year following a move. Row 3 and 4 show the average change in earnings (\$1000) between one year prior to the move and one year post move.

Taken together, these scenarios suggest three things. First, the declines in migration in counterfactuals 1 and 2 confirm that more equal within-household earnings make joint distant job search more difficult. Second, the scenarios with stay-at-home spouses demonstrate that spatial search frictions do not just contribute to worse outcomes for tied movers, but also cause both men and women to be tied stayers who miss out on large potential earnings gains.

[25] This can be seen by comparing post-move employment to non-mover employment; 75.2% of non-mover women are employed in CF2 relative to 65.6% of non-movers in the baseline. This means that employment is only 9.6 p.p. lower post-move in the counterfactual relative to 13.9 p.p. lower in the baseline.

Lastly, though it is unrealistic to consider policies that force one spouse to be a stay-at-home spouse, similar gains in terms of increased migration rates and improved post-move labor market outcomes are achieved when offer rates are equalized. This counterfactual has more policy-relevance; one could imagine a number of public or private policies that could achieve this, such as job search assistance for spouses as part of relocation packages.

8.3 Alternative Policies

In the third set of counterfactuals, I evaluate different designs for subsidies meant to induce migration. I use these counterfactuals to compare how migration incentives with different employment requirements change migration and post-move labor market outcomes for men and women. UI for trailing spouses incentivizes moving with only one spouse employed and subsidizes search for the trailing spouse after the move has happened. One reason that states might want to have UI for trailing spouses is because it allows a spouse who is out of work to increase their search radius for jobs without being as concerned about their spouse needing to quit their job. However, providing UI to the spouse who quits at the time of the move is not the only way to encourage job search as a distance. An alternative option is to provide relocation subsidies to movers who were unemployed pre-move and find a job in a distant location or to provide relocation assistance regardless of employment status. I test three possible subsidy designs:

1. Subsidy for trailing spouses, eligible if one spouse works and one spouse doesn't post-move
2. Subsidy for distant job search, eligible if individual is unemployed pre-move and employed post-move
3. Subsidy for moving, eligible regardless of employment status

In all counterfactual policies, the subsidy level is $10,000. The first counterfactual has similar incentives to UI for trailing spouses, but removes the UI take up cost associated with receiving the benefit, does not have any pre-move eligibility requirements, and standardizes the benefit level across genders/locations to make it more comparable to the other counterfactuals. The second counterfactual policy mirrors relocation incentives for job-seekers that exist in multiple European countries in which benefits are given to those who accept jobs in regions different from their current region.[26] Lastly, the final subsidy counterfactual explores the effects of a policy that de-links the benefit from any employment requirements.

All three subsidy policies have positive effects on the migration rate. Table 9 reports the effects of these policies on migration rates for the sample aged 25 to 35 to make the effect sizes

[26]Evaluations of the effects of these programs on inter-region mobility in Germany (Caliendo et al., 2017) and France (Glover and Roulet, 2019) suggest that take-up of the relocation assistance is typically associated with long term gains in earnings.

comparable to those discussed in the reduced form exercise. The policy with the largest effect is the unconditional subsidy (0.38 p.p., or 11.2% increase). In this setting, the effect of the migration subsidy tied to trailing spouses is smaller than the UI policy effect sizes though within the confidence interval of the reduced form exercise (0.27 p.p. or 6.2% increase). Effects of the policy are largest in percent terms for households in which the wife typically works, with the trailing spouse subsidy increasing migration rates by 16.7% in female-headed households, 7.0% in dual-earner households, and 6.2% in male-headed households.

Table 9: Migration Subsidies: Effects on Migration

	No Subsidy	Trailing Spouse	Job Relocation	Unconditional
% Move: All	3.37	3.58	3.54	3.75
Dual-Earner	3.13	3.35	3.33	3.51
Male Single-Earner	3.69	3.92	3.85	4.07
Female Single-Earner	3.60	4.20	3.90	4.44
Number of Moves: All	0.87	0.89	0.89	0.91
Dual-Earner	0.82	0.83	0.84	0.86
Male Single-Earner	0.96	0.98	0.98	1.01
Female Single-Earner	0.71	0.77	0.71	0.77
% Ever Move: All	68.17	69.80	69.58	70.91
Dual-Earner	66.01	67.80	67.66	68.94
Male Single-Earner	72.58	74.17	73.79	75.19
Female Single-Earner	58.58	62.57	59.51	62.05

Notes. This table reports the results of the second set of counterfactuals. Column 1 shows a scenario in which there are no subsidies or UI for trailing spouses; column 2 provides a $10,000 subsidy for households who move with one spouse unemployed; column 3 provides a $10,000 subsidy for households in which an unemployed spouse accepts a job at a distance; and column 4 provides a $10,000 subsidy for a move regardless of employment. Dual-earner households are households where both spouses work in more than half of periods; male single-earner have only the husband working in more than half of periods; female single-earner have only the wife working in more than half of periods; and other are households which do not fit any of these categories for more than half of the periods.

Next, I evaluate the effects of these subsidies on earnings outcomes for movers. Table 10 reports the earnings growth one-year, two-year, and three-years post-move, relative to one year pre-move. Though the job relocation subsidy results in similar earnings patterns following a move as the baseline, the trailing spouse subsidy and the unconditional subsidy both result in lower earnings post-move for women and men. Interestingly, the trailing spouse policy impacts women's post-move earnings by less than the unconditional subsidy whereas the opposite is true for men. Though the earnings gains are smaller under the subsidies than in the baseline, recall that these earnings gains are still larger than what the average household gains in the absence of a move.

Table 10: Migration Subsidies: Effects on Labor Supply

	No Subsidy	Trailing Spouse	Job Relocation	Unconditional
Δ Earnings, $t+1$, Women	1.47	0.18	1.52	-0.28
Δ Earnings, $t+2$, Women	2.92	2.18	2.93	2.00
Δ Earnings, $t+3$, Women	2.70	2.17	2.87	1.90
Δ Earnings, $t+1$, Men	6.09	4.70	6.17	5.55
Δ Earnings, $t+2$, Men	9.16	8.12	9.10	8.73
Δ Earnings, $t+3$, Men	10.87	9.82	10.91	10.20

Notes. This table reports the results of the second set of counterfactuals in terms of earnings change relative to one-year pre-move. Rows 1-3 report women's earnings (in $1000) change one-year, two-years, and three-years post-move; Rows 4-7 show the same for men. Column 1 shows a scenario in which there are no subsidies or UI for trailing spouses; column 2 provides a $10,000 subsidy for households who move with one spouse unemployed; column 3 provides a $10,000 subsidy for households in which an unemployed spouse accepts a job at a distance; and column 4 provides a $10,000 subsidy for a move regardless of employment.

The earnings declines found in this exercise suggest that the mechanisms in the model are not fully capturing the earnings patterns induced by UI for trailing spouses. As is, the model primarily captures the selection effect of the policy in which households are willing to move for lower earnings gains due to the income from the subsidy, but does not capture the direct effect of UI on post-move search behavior. Part of why trailing spouses with access to UI have higher earnings post-move than those without UI likely stems from a change in search behavior – searching longer and increasing their reservation wage. In the current model, periods are a year, meaning that individuals cannot receive UI for more than a period and thus do not have incentives to change their search behavior post-move. Future extensions of this model should incorporate costly search as well as shorter period lengths to address these limitations.

Nonetheless, the differences in migration rates and post-move labor outcomes across the subsidies do suggest that how governments design migration policy should differ depending on their goals. If a government is implementing migration subsidies to ameliorate spatial search frictions, they must consider how household ties will complicate the effectiveness of the policy for different groups. The relocation subsidy distorted post-move labor market outcomes the least, though this was in part because it did not have as large of an effect on who chose to move. An unconditional subsidy increased migration rates the most, but resulted in the earnings losses in the year following a move for women and the smallest long-term earnings gains for women.

9 Conclusions

This study explores how dual-earner households make decisions about where to work and live. I evaluate the impacts of a specific component of the unemployment insurance program – UI for trailing spouses – on a household's decision to move and the consequences of these moves for men's and women's labor market outcomes. I show that access to UI is associated with significantly a higher likelihood of distant moves for married couples, with effects in the range of 16 to 46 percent, depending on sample and age cohort. Results from an analysis of post-move UI take-up also show that this policy resulted in the expected uptick in receipt of unemployment insurance following a move, with effects concentrated on take up rates for married women and secondary earners. Lastly, this policy is associated with significantly different post-move income trajectories for married women, with female movers in treated states having higher earnings and wage gains relative to stayers one-year post-move than those in comparison states. In contrast, this policy has null or negative effects on men's earnings. These reduced form exercises demonstrate evidence of the benefits of unemployment insurance programs in a new context – distant job search– and suggest that the difficulty of moving two jobs rather than one acts as a substantial barrier to migration for married couples.

Motivated by these analyses, I then estimate a structural model of household migration which sheds some light on the mechanisms underpinning the migration behavior of dual-earner households. I show that equalizing earnings distributions across genders reduces the likelihood that married couple households move, consistent with theory showing that households with two equal earners are more likely to end up tied stayers when spouses only have one offer. In a separate counterfactual, I implement simultaneous distant offers for the spouses, removing the primary spatial search friction that dual-earner couples face. I find that this substantially increases migration rates and results in better post-move outcomes for women in particular. Lastly, I compare the effects of three different subsidy designs, demonstrating that the efficacy of a migration subsidy will depend on how it is tied to household job search. Unconditional subsidies increase migration rates the most, but reduce post-move earnings more than subsidies that encourage migration with a job.

These analyses provides evidence consistent with past research on dual-earner migration, suggesting that women are more likely to be the trailing spouse in distant moves and experience earnings losses due to the move. The findings in both the reduced form and the structural exercises demonstrate the particular importance of the trailing spouse's ability to find a job in the new location as the primary mechanism driving these gender inequalities. Since moves across both locations and jobs can provide one way for individuals to climb the earnings ladder, the fact that women are more likely to accommodate their husband's career path rather than initiate a move themselves speaks to one channel through which gender gaps in earnings open up. Policies such as UI for trailing spouses which mitigate the costs of moves for trailing spouses are therefore one policy lever that can be used to help address gender inequalities in earnings.

Future analyses should explore further the process of job search at a distance, comparing and contrasting how different household structures influence the geographic radius over which individuals search for jobs. The current model does not incorporate search effort nor does the timing of the model allow for receipt of UI in multiple periods. These limitations restrict the model's ability to understand how migration subsidies and UI for trailing spouses might change search effort following a move. To better understand the reduced form results, future work should focus on these mechanisms as potential factors impacting how households conduct distant search.

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10 Appendix

A.1 Institutional Setting for UI Eligibility for 'Compelling Family Reasons' under ARRA

In an effort to address the burden on states' UI funds during the Great Recession, the federal government made a total of $7 billion in incentive payments available to states to use to cover all benefits paid through the Extended Benefits (EB) program, provided they could demonstrate that their UI laws, regulations, or policies included a set of modernization provisions. Before the ARRA, EB programs were typically split evenly between federal and state funds. To access the first third of the incentives, states had to implement an alternative base period for establishing monetary eligibility for UI. The second two-thirds were contingent on implementing at least two of four possible modernizations:

1. Extending eligibility to individuals seeking part-time work if they have a history of part-time work.
2. Extending what constitutes good cause for leaving a job to include 'compelling family reasons,' defined as quitting to care for an ill or disabled immediate family member, following a spouse who is relocating due to a change in location of the spouse's employment such that commuting is impractical, or leaving a job due to domestic violence that makes continued employment at that job hazardous.
3. Extending benefit time period by 26 weeks for UI exhaustees who enroll in state-approved training programs.
4. Adding a dependents' allowance provision where eligible recipients can collect an allowance of at least $15 per week per dependent on top of the regular benefits.

For the purposes of this paper, the second option, henceforth known as the Compelling Family Reasons provision, is the relevant modernization, though it encompasses a broader set of eligibility criteria than this paper focuses on. Twenty-one states chose to implement the Compelling Family Reasons provision, of which one state already had all three provisions (Nevada), seven modified existing provisions to fulfill the requirements of the ARRA specifications, and thirteen added it as a new provision (Mastri, Vroman, Needels, and Nicholson, 2016). Appendix Table A-1 shows which states implemented which of the four provisions, as well as which states opted out of the federal UI incentives. Since then, three states which implemented the Compelling Family Reason provision have since removed the provision allowing for trailing spouses to receive UI: Illinois, North Carolina, and Wisconsin.

In an ideal natural experiment, a states' choice of whether to implement UI eligibility for spousal relocation would be random, both in terms of whether they have this provision and when they decide to enact the provision. Though identification stems partially from the states which independently implemented UI eligibility for trailing spouses in years other than 2009-2010, one might be concerned that identification of the effects of this policy are coming primarily from the bulk of states changing their policy at the same time as the Great Recession and concurrently with other UI policy changes. This is less of a concern if the states which chose the Compelling Family Reasons provision are plausibly similar to states which chose other provisions or did not take up the UI modernization provisions at all in 2009-2010.

In an analysis of states' decisions to adopt the UI modernization provisions as part of ARRA, Mastri et al. (2016) conduct a survey of UI administrators in all 50 states and DC, asking them to describe the key factors in favor or against implementing each provision for the state, the expected costs the state considered when deciding on adoption, and any challenges in implementing the provisions. For both adopters and non-adopters of the modernization provisions, states reported that the most important factor considered in favor of implementation was the desire to access the federal incentive funds. For adopters only, the fact that they already had all or parts of one or two provisions in place was the next highest rank in favor implementation. The most important factor against adopting the modernization were higher expected benefit pay outs or administrative costs. Similarly, when states that did adopt UI modernization were asked why they chose a given provision over the other options, the most common response was that they already had a conforming provision in place. Notably, the majority of states did not perform a cost-benefit analysis of all of the provisions and had not estimated how many residents would be newly eligible under the different provisions. This suggests that the decision to choose the Compelling Family Reasons provision was not driven by pre-existing differences across states in out-migration rates or expectations about the how the policy would change migration patterns.

Though a small number of papers have explored the impacts of the ARRA's UI modernization components on eligibility or take-up of UI (e.g., Callan et al., 2015; O'Leary, 2011), this paper focuses only on the trailing spouse component of this provision, allowing me to take advantage of additional variation in state provisions. While only five states had all three components of the Compelling Family Reasons provision pre- 2009 (Mastri et al., 2016), there are more states which had the trailing spouses policy pre-2009 and the exact month/year of variation post-2009 varies. Therefore, I am able to separately identify the effects of trailing spouse provisions from the legislative package as a whole.

To further address these identification concerns, I conduct a series of robustness checks, including exploring the effects of a different component of UI Modernization, eligibility for part-time workers, which should have no impact on migration but was also implemented as part of the ARRA. If the effects of this policy are similar to those seen for UI for trailing spouses, this would suggest that the estimates are actually tapping into effects of the policy being implemented during the Great Recession. However, there is no significant effect of UI for part-time workers on long-distance migration, suggesting that the effects seen for UI for trailing spouses is not driven by the timing of implementation in specific states due to the ARRA. These robustness checks are discussed further in Section A.2.

A.2 Robustness Checks

A.2.1 Alternative Sample: American Community Survey

To test whether these results hold in a larger sample and across age groups, I re-estimate the regressions from panel B and panel C of table 2 column 1 using cross-sectional data from the American Community Survey (ACS) 2004-2016 (Ruggles et al., 2019). Though the panel structure of the NLSY97 allows me to control for individual-level fixed effects, labor force participation in previous periods, and the distance of a move, it is limited in both the size of the sample and the cohort-based design of the survey. The ACS allows me to compare the effects of the policy on moves for older respondents, who typically move less and therefore may have a smaller response to the policy.

I regress an indicator for moving between year $t-1$ and t on an indicator for being in a state with the UI policy in year $t-1$, interacted with an indicator for if the respondent is married, along with state and year fixed effects, individual level characteristics (quadratic of age, indicator for college degree, race indicators, number of kids, indicator for if state in $t-1$ is state of birth) and state level characteristics (unemployment rate, per capita income, index of housing prices). Table A-3 reports the effect of the treatment on cross-commuting zone moves (col. 1 and 3) and cross-state moves (col. 2 and 4) for a sample of individuals age 23 to 35 and a sample of individuals age 23 to 65.

There are larger impacts of the treatment on migration rates for younger respondents in absolute terms, with the treatment being associated with 0.5 percentage point higher cross-commuting zone migration rates for those under 35 and 0.3 percentage point higher cross-commuting zone migration rates for all respondents. The base rate for cross-commuting zone moves for married individuals under 35 in the ACS is 2.4 percent, making this increase a 16 percent increase for the sample that corresponds to the age range in the NLSY97. The base rate for all married individuals is 1.0 percent, meaning that the increase for the full age range is larger in percent terms than for younger Americans (30%). These effects are smaller than those seen in the NLSY97 sample, but are within the bounds of the confidence interval of those estimates.

A.2.2 Placebo Test #1: Alternative UI Modernization Option

Given the number of states that changed their UI provisions through the Compelling Family Reasons component of UI modernization, one might be concerned that this policy is implemented at the same time as a set of other UI policies as well as at a time when economic conditions are particularly poor in the sending state. I address some of these concerns by controlling for economic conditions in the state at the time of the move (unemployment level, per capita income) as well as by using single households as a comparison. Alternatively, because the UI Modernization included other possible ways of modernizing, I am able to test whether it was the package of policies that induced state out-flows rather than the spousal relocation component by using one of the other modernization options, the part-time eligibility option, as a type of placebo test. Because this option increases the benefits available to those who stay in the state and does not help those who leave the state, this policy should not induce people to move out of state or change the earnings trajectories of movers. If there is any effect, the part-time eligibility component would encourage people to stay in-state because it reduces the cost of unemployment for part-time workers, making a household less likely to move to improve their job prospects if a member of the household in part-time job loses their job.

To test this, I collect information from the Department of Labor's State UI Comparison reports on which states allowed workers who are searching for a part-time job to collect UI if they have a history of part-time work. In the early 2000s, 31 states allowed this; at the peak of the UI modernization, 39 states had this policy. I then re-estimate the regression model from equation 1 using UI eligibility for part-time workers as the treatment of interest rather than UI eligibility for tied movers. Table A-5 shows the results of this regression.

As expected, there is no statistically significant impact of part-time worker eligibility on a person's likelihood of moving across commuting zones for either married individuals or single individuals.[27] This suggests that the effects of the spousal relocation policy previously estimated are not simply the result of this policy being implemented at the same time as other UI policies such as the alternate base period or the other modernization criteria, as I would then see a similar effect from the part-time eligibility policy which is also bundled with the ARRA changes in terms of timing for many states.

A.2.3 Placebo Test #2: Within-Commuting Zone Moves

In addition to testing the effects of a policy change that should not affect migration, I am also able to benchmark my results against an outcome that would not be affected by the policy: within-commuting zone migration rates. A key component of the statute is that the job change of the person's spouse must make commuting impractical. I therefore would not expect to see an impact of this policy on the likelihood that a household moves within a commuting zone. For example, though a move from Newark, NJ to Hartford, CT for a New York City worker is a cross-state move, it would not make the worker eligible for UI since their ability to commute into the city would be unchanged.

To test this, I characterize a move as within-commuting zone if the respondent was living in a different state or county in the previous year, but was living in the same commuting zone. I then repeat the regressions from equation 1 with an indicator for experiencing a within-commuting zone move as the dependent variable. Table A-6 shows the results of this regression. There is no statistically significant impact of UI eligibility for trailing spouses on the likelihood that one moves within a commuting zone for either married or unmarried

[27] Regressions results for the effect of the part-time worker eligibility on changes in earnings for movers also show no significant differences in earnings for movers from states with this policy versus movers from states without this policy.

households.

A.2.4 Effects on State-Level Claims

Given the magnitude of effects on moves, I ideally would like to observe a large enough increase in UI applications associated with being a trailing spouse to justify the increase in moves. This would require access to data on the number of UI claims made by married individuals who claim UI due to 'compelling family reasons,' which is not reported at either the federal or state level in public records. However, states are required to report to the federal government the number of non-monetary determinations they accept and deny in each quarter, as well as whether the non-monetary determination was related to a non-separation, voluntary separation, a discharge separation, or any other type of separation. Claimants who are eligible due to compelling family reasons are automatically required to go through the determination process and would be categorized as a voluntary separation.

While not all non-monetary determinations for voluntary separations will be trailing spouses, one would expect that implementing UI for trailing spouses should increase the number of non-monetary determinations. To test this, I combine the data set on legislative changes to UI access for trailing spouses with a measure of the annual voluntary separations that receive non-monetary determinations between the years 2000 and 2017 (Department of Labor, 2019) and estimate the following regression:

$$\text{NMD}_{st} = \beta_0 + \beta_1 \mathbb{1}(Treated)_{st} + Z'_{st}\beta_3 + S + T + \epsilon_{st} \tag{A-1}$$

where NMD_{st} is the number of eligible non-monetary determinations; $\mathbb{1}(Treated)_{st}$ is a dummy equal to one if the state allowed trailing spouses to collect UI, Z_{st} are state time-varying characteristics including unemployment rate, per capita income, index of housing prices, average age, percent college-educated, and percent non-white, and S and T are state and year fixed effects.

Table A-4 shows the results of this regression for three outcomes: total non-monetary determinations due to separations (col. 1); total non-monetary determinations due to voluntary separations (col. 2); and total non-monetary determinations due to discharges (col. 3). Column 2 is the measure that is closest to the preferred measure – determinations due to quits for compelling family reasons; column 1 is a broader measure that encompasses all possible non-monetary determinations and column 3 is a placebo test since eligibility if discharged is not dependent on being a trailing spouse. There is a marginally significant increase in total number of non-monetary determinations in states with UI for trailing spouses and a more precisely significant increase in total number of non-monetary determinations due to voluntary separations. States with UI for trailing spouses have 3713 more determinations than states without the policy. In contrast, there is not a significant increase in the number of UI determinations associated with discharges.

A.3 Propensity Score Matching for Post-Move Labor Market Outcomes

Though the results of the primary specification provide evidence suggesting that access to UI for trailing spouses increases women's earnings and UI takeup, I cannot rule out the possibility that these differences in post-move earnings stem from different types of households moving in the presence of the policy. To address this concern, I use the propensity scores to re-weight observations to be similar to those of treated movers.

Since this application has multiple groups rather than a simple treatment-control typically seen in propensity scoring matching applications, I follow the reweighting scheme for multiple groups highlighted in DiNardo et al. (1996) and Fortin et al. (2011), which has previously been used in the context of migration event studies by Coate et al. (2017). Specifically, I calculate the following weight, W_{it}, for each person i in month t grouped according to whether they move or not (M or N) and whether they are in a treated or comparison state (T or C), $j_{it} \in \{TM, TN, CM, CN\}$, as follows:

$$W_{it} = \frac{P(j_{it} = TM|X_{it})}{P(j_{it} = TM)} \frac{P(j_{it})}{P(j_{it}|X_{it})}$$

I estimate the unconditional probabilities using sample averages and estimate the conditional probabilities using a logit regression based on predictors related to the characteristics of the respondent and the jobs they hold in the months prior to the move. For the monthly analyses, X_{it} contains a quadratic of age, indicators for race, an indicator for having a bachelor's degree or more, number of children, log of wages three months prior, an indicator for if employed three months prior, and an indicator for if the household is living in the respondent's home location, defined as their state in the first wave of the survey). For the annual analysis, it contains the above variables excluding the monthly measures and adding earnings for both husband and wife a year prior. The last control, home location, is the key variable that meets the exclusionary restriction necessary for identification; while living in one's home location is likely associated with a lower likelihood of moving, it should not be associated with earnings other than through moving likelihood.

Table A-7 reports descriptive statistics on the sample of movers and stayers in treated and untreated states before weighting (panel A) and after weighting (panel B). Prior to weighting, movers were slightly more educated than stayers, less likely to have children, and less likely to be employed three months prior. Treated individuals were less educated and more likely to be collecting UI three months prior than untreated individuals. With weights, the respondents now are similar on observables.

I next re-estimate the event study analyses with observations weighted by these propensity scores.

Figure A-2 shows UI take-up for matched married men and women. For men, the pre-move trend is more pronounced here than in the un-matched analyses. Treated stayers who are similar on observables in period T= -3 to treated movers are significantly less likely to be collecting UI in period T = -2 and this difference is large than the difference between untreated movers and stayers with similar observables. For women, treated movers are more likely to be collecting UI in the months following the move than similar treated stayers and this difference is significantly larger than that of observably similar untreated movers and stayers. However, observably similar female movers in the comparison states were more likely to have started collecting UI in the months leading up the move. This, along with the pre-trends for married men, suggest that some of these moves are pre-dated by a displacement for one spouse rather than the move causing the displacement.

The results for the matched earnings regressions depict a clear relationship between access to spousal UI and men's and women's earnings, with the policy being associated with significantly smaller earnings gains for male movers, smaller earnings losses for female movers, and larger household income gains overall. Figure A-3 Panel A shows results for men; figure A-3 Panel B shows results for women; figure A-3 Panel C shows results for household income. While observably similar comparison men see earnings gains relative to stayers, treated men see flat earnings profiles. Once matched on observables to treated movers, married women movers in comparison states no longer see a dip in earnings post-move, consistent with there being negative selection on which women move in the absence of the policy. Nonetheless, women in comparison states see lower income long run than women in treated states. This higher income for treated married women then translates into higher overall household income gains for treated households relative to those moving from comparison states.

Results for wages (shown in figures A-5 are similar in magnitude and direction to the non-matched data. However, wage gains are no longer significantly higher at a $p < 0.05$ level for women one-year post move. The magnitude in the average gap in coefficients over this time period is nonetheless similar to those in the unmatched sample. While I cannot reject that the effects of access to UI on future wages are zero, these results are at least suggestive of a positive impact of these policies on women's post-move job match.

A.4 Bounding Exercise for Post-Move Labor Market Outcomes

When estimating the effect of access to UI for trailing spouses on post-move outcomes, the econometrician faces an endogeneity problem in which I do not observe the counterfactual post-move outcomes for treated movers and treated stayers if they were to move/stay in the absence of the policy. I instead only observe the post-move outcomes for untreated movers and untreated stayers, who may differ from those who move/stay in the presence of the policy. Because the treatment changes which households decide to move, it is difficult to separate the effects of the policy on selection into migration from the effects of the policy on the earnings one receives post-move.

In this section, I use the methods described in Lee (2009) to develop estimates which can be thought of as bounds on the true effect of the policy, net of selection effects. Because the event study design has two potential selection problems – selection into moving the presence of the policy and selection into staying in the absence – I turn to the simpler method of estimating γ for this exercise in which I look only at the difference in outcomes for movers rather than the difference in movers relative to stayers. Recall that the parameter of interest γ, could be estimated as follows in a world of perfect information about all possible states of the world:

$$\mathbb{E}[\gamma] = \mathbb{E}[W_{i,t+1}|X_{it}, D=1, M(1)=M(0)=1] - \big(\mathbb{E}[W_{i,t+1}|X_{it}, D=0, M(1)=M(0)=1]$$

However, I cannot observe a single household in both states of the world. I instead can estimate the following:

$$\widetilde{\mathbb{E}[\gamma]} = \mathbb{E}[W_{it}|X_{it}, D=1, M(1)=1] - \mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1] \quad \text{(A-2)}$$

In this exercise, I demonstrate that $\mathbb{E}[\gamma]$ can be bounded from below if I make some assumptions about the composition of always movers vs. marginal movers.

To see this, consider the terms that we can observe. The observed term

$$\mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1]$$

is expected earnings for individuals who don't live in a treated state and do move when they live in an untreated state. We can split this group into two sub-groups: 'always movers', who move in the presence of the policy or in the absence of the policy and 'untreated movers,' who move when untreated and don't move when treated. If we denote the percent of this group who are always movers as q, we can rewrite this term as follows:

$$\begin{aligned}\mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1] &= q \times \mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1, M(1)=1] \\ &\quad +(1-q) \times \mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1, M(1)=0]\end{aligned}$$

We can rewrite the expectation for earnings for the treated group similarly:

$$\begin{aligned}\mathbb{E}[W_{it}|X_{it}, D=1, M(1)=1] &= p \times \mathbb{E}[W_{it}|X_{it}, D=1, M(0)=1, M(1)=1] \\ &\quad +(1-p) \times \mathbb{E}[W_{it}|X_{it}, D=1, M(0)=0, M(1)=1]\end{aligned}$$

Then, I make an assumption about the effect of the treatment on migration that allow us to simplify these expressions:

Assumption # 1: The probability that you move in the presence of the treatment is greater than or equal to the probability that you move in the absence of the treatment, conditional on observables.

This implies that $P[M(1)=1] > P[M(0)=1]$, meaning that in the above expressions, q must equal 1 – that is, if a household moves in the absence of the policy, they always move in the presence of it. This means that $\mathbb{E}[W_{it}|X_{it}, D=0, M(0)=1]$ is the expected value of earnings for always movers in the absence of the policy.

Lee (2009) proposes an estimate for the upper and lower bounds of the average treatment effect in which one assumes that the effect of the treatment is bounded by the assumption that the sample whose outcomes are observed are either the highest p-th percentile of the outcome variable or the lowest, where p is the probability that a respondent's outcome is observed conditional on observing the outcome in the presence of the treatment. In the Lee (2009) setting, the treatment was a job training program and the outcome, wages, was observed if the person was working. In the current paper, while I always observe earnings, I only observe earnings post-move if one moves. That is, the relevant 'observed' outcome is earnings conditional on selecting into moving and the treatment is access to UI for trailing spouses.

Lee defines the trimming property as follows, where $Z* > 0$ is the selection decision which in my scenario is selecting into migration:

$$p = \frac{Pr[Z^* \geq 0|D=1, X=x] - Pr[Z^* \geq 0|D=0, X=x]}{Pr[Z^* \geq 0|D=1, X=x]}$$

While Lee (2009) estimates these probabilities non-parametrically using a binned estimator, my reduced form work provides a natural parametric estimator for this. The top of the fraction is the treatment effect estimated in the first reduced form exercise: the effect of UI for trailing spouses on the likelihood of a move. The bottom of the fraction is the predicted probability of moving conditional on treatment and covariates from the same analysis. Using the estimates from the comparable regressions, I find that p = 0.31.[28]

I then can compare the effects of the treatment on post-move earnings for the full sample and for the sample trimmed to only include the bottom 69th percentile, which assumes that the marginal movers all have the highest post-move outcomes. Because the event study design has two potential selection problems – selection into moving the presence of the policy and selection into staying in the absence – I turn to the simpler method of estimating γ for this exercise, a regression of earnings on the treatment, state fixed effects, year fixed effects, and controls (quadratic of age, college dummy, race dummy, number of kids, pre-move earnings). I also only conduct the bounding exercise for women's estimates as men's estimates are noisy null values even in the absence of bounding. For each $\tau \in [t, t+1, t+2, t+3]$, I run the following regression:

$$(Earn_{i,\tau}|Move_{it} = 1) = \beta_0 + (Treat)'_{i,t}\gamma + X'_{it}\beta_1 + S^{t-1} + T_t + \epsilon_{it} \tag{A-3}$$

Table A-8 reports the treatment effect for the full sample of married female movers in panel A; the lower bound estimate based on the sample restricted to the 69th percentile and below of the outcome variable in panel B; the full sample of married female movers in panel C with propensity score weights to match on observables; and the lower bound with propensity score matching in panel D.

In the full sample, I see similar effect sizes as shown in the event study, with women earning significantly more in the presence of UI for trailing spouses two and three years post-move. When I restrict my sample to those in the bottom 69 percentiles of married female earners,

[28] Because I will be restricting the sample only to movers, I do not include individual fixed effects in this exercise and thus am using the estimates from column 1 of table 2.

I find that the lower bound is not significantly different from zero two-years post the move, but that the lower bound for three-years post move is positive and marginally significant, showing that the lower bound of estimates is that women earn around $4500 more annually three years post-move in the presence of the policy.[29] This halves the effect sizes seen in the main analysis, consistent with my assumption that trailing spouses who select into move in the presence of the policy are positively selected.

A.5 Appendix Figures and Tables

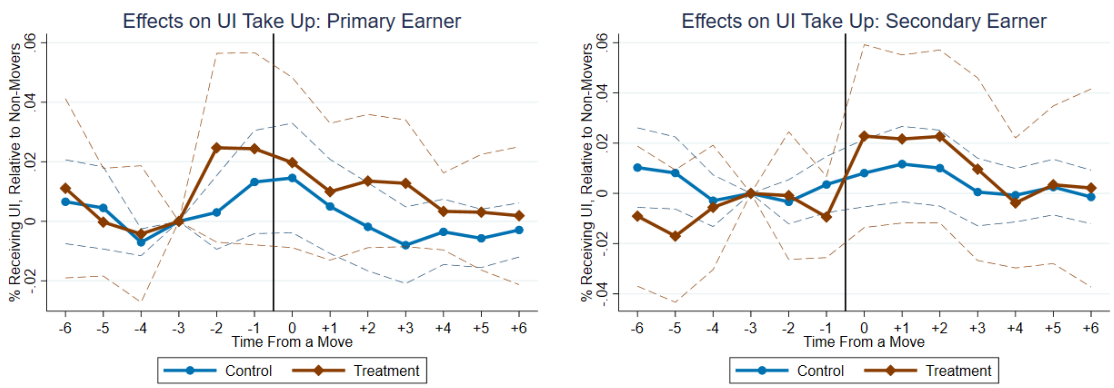


Figure A-1: Effects of UI Eligibility on UI Take-Up for Primary Earner (left) and Secondary Earner (right)

Notes. This figure plots the coefficients of a regression of receiving UI on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. Sample restricted to married, age 23+ and those working three months prior to move. The three months prior to a move T= -3 is omitted. All regressions include individual, state, and year fixed effects. Brown line indicates effects of a move with access to UI for trailing spouses; blue line shows effects without the policy. Standard errors are clustered at the state-year level, and 95% CI are shown

[29] Note that γ is not statistically significant in the year of or the year post-move even in the regular regression without bounding assumptions.

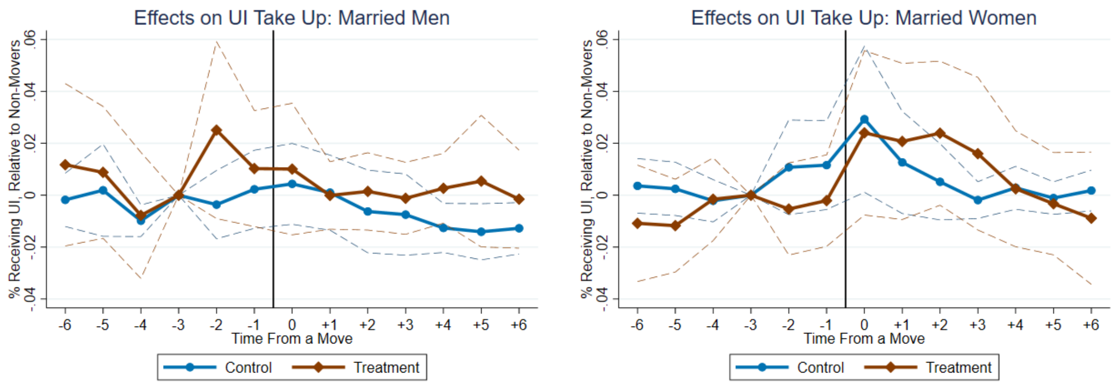
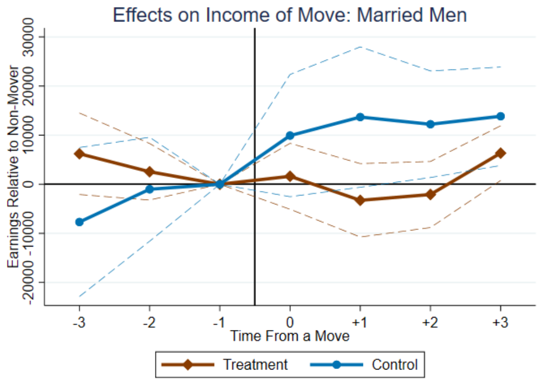


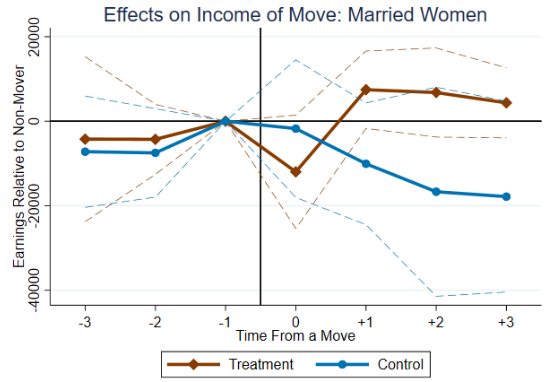
Figure A-2: Effects of UI Eligibility on UI Take-Up for Men (left) and Women (right), with Propensity Score Weights

Notes.This figure plots the coefficients of a regression of receiving UI on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. Sample restricted to married, age 23+ and those working three months prior to move. Responses are weighted using propensity score matching based on age, race, college education, number of children, employment status in T= -3, and log wages in T-3. The three months prior to a move T= -3 is omitted. All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI are shown

Panel A



Panel B



Panel C

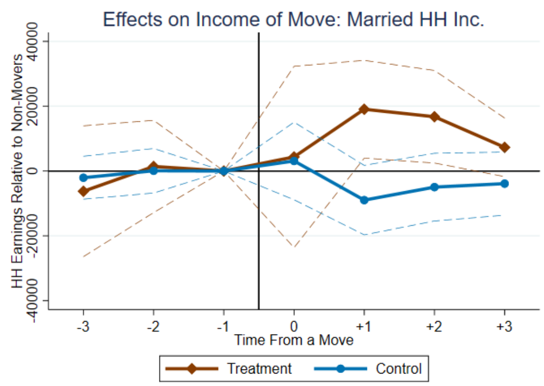


Figure A-3: Effects of UI Eligibility on Earnings for Married Men (top left), Women (top right), and Households (bottom), Post Cross CZ Move with Propensity Score Weights

Notes: This figure plots the coefficients of a regression of income from wages and salary on indicators for leads and lags surrounding the year of a move across commuting zones, denoted as T= 0 in the figure. Responses are weighted using propensity score matching based on age, race, college education, number of children, and own and spouse earnings in year T=-1. The year prior to a move T= -1 is omitted and the points plotted thus indicate change in earnings relative to the year before the move. All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI are shown.

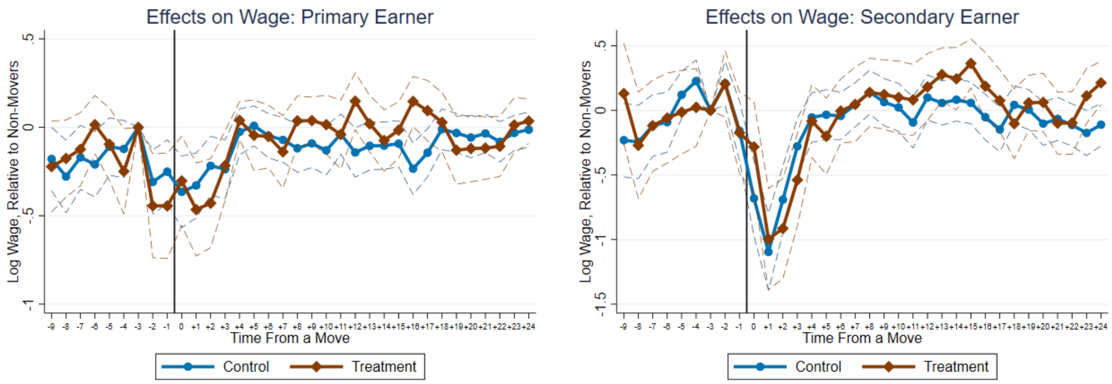


Figure A-4: Effects of UI Eligibility on Wages for Primary Earner (left) and Secondary Earner (right)

Notes. This figure plots the coefficients of a regression of an indicator for if a person receives UI on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. Sample restricted to married, age 23+ and those working three months prior to move. The three months prior to a move T= -3 is omitted. Brown line indicates effects of a move with access to UI for trailing spouses; blue line shows effects without the policy. All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI are shown.

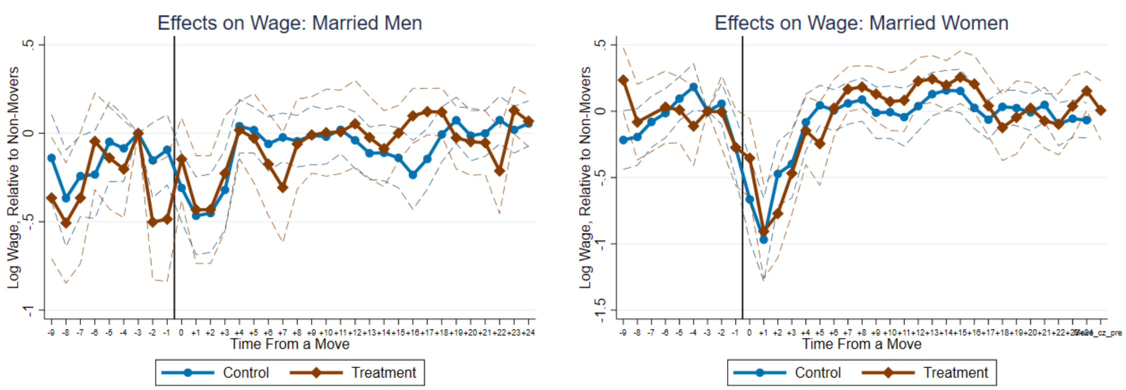


Figure A-5: Effects of UI Eligibility on Wages for Men (left) and Women (right), with Propensity Score Weights

Notes. This figure plots the coefficients of a regression of ln(wages+0.1) on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the figure. Responses are weighted using propensity score matching based on age, race, college education, number of children, employment status in T= -3, and log wages in T-3. Sample restricted to married, age 23+ and those working three months prior to move. The three months prior to a move T= -3 is omitted. All regressions include individual, state, and year fixed effects. Standard errors are clustered at the state-year level, and 95% CI are shown.

Table A-1: Combinations of Modernization Options Chosen As Part of ARRA Incentives

Option 1 (PT) and Option 2 (CFR)	Arkansas, California, Colorado, Delaware, Hawaii, Minnesota, Nevada, New Hampshire, New York, North Carolina, Oklahoma, South Carolina
Option 1 (PT) and Option 3 (Training)	Georgia, Idaho, Iowa, Kansas, Maine, Maryland, Montana, Nebraska, New Jersey, South Dakota, Vermont
Option 1 (PT) and Option 4 (Dependent)	New Mexico, Tennessee
Option 2 (CFR) and Option 3 (Training)	Maine, Oregon, Washington, Wisconsin
Option 2 (CFR) and Option 4 (Dependent)	Alaska, Connecticut, Illinois, Rhode Island
Option 3 (Training) and Option 4 (Dependent)	Massachusetts
Did Not Take Incentives	Alabama, Florida, Kentucky, Louisiana, Michigan, Ohio, Pennsylvania, Texas, Utah, Virginia, West Virginia, Wyoming

Notes. This table lists the combination of modernization incentives chosen by each state to be eligible for increased federal funding for UI under the ARRA, as well as the states which did not accept federal assistance. PT stands for eligibility for part-time workers; CFR stands for eligibility for compelling family reasons; Training stands for extended benefits for enrollment in training programs; and Dependent stands for adding a dependents' allowance.

Table A-2: State Spousal Relocation Policies, 2000-2017

	Date of Implementation	Date of Repeal		Date of Implementation	Date of Repeal
Alabama	-	-	Montana	-	-
Alaska	April 2010	-	Nebraska	Pre-2000	-
Arizona	pre-2000	-	Nevada	March 2006	-
Arkansas	July 2009	-	New Hampshire	Sept. 2009	-
California	Pre- 2000	-	New Jersey	-	
Colorado	July 2009	-	New Mexico	-	-
Connecticut	April 2009	-	New York	Pre-2000	-
Delaware	July 2009	-	North Carolina	Aug. 2009	July 2013
Florida	-	-	North Dakota	-	-
Georgia	-	-	Ohio	-	-
Hawaii	July 2009	-	Oklahoma	Pre-2000	-
Idaho	-	-	Oregon	Pre-2000	-
Illinois	July 2009	Jan 2013	Pennsylvania	Pre-2000	-
Indiana	Pre-2000	-	Rhode Island	Pre-2000	-
Iowa	-	-	South Carolina	Jan. 2011	-
Kansas	Pre-2000	July 2012	South Dakota	-	-
Kentucky	-	-	Tennessee	-	-
Louisiana	-	-	Texas	-	-
Maine	Pre-2000	-	Utah	-	-
Maryland	-	-	Vermont	-	-
Massachusetts	-	-	Virginia	-	-
Michigan	-	-	Washington	1: Pre-2000; 2: Sept. 2009	1: Jan. 2004; 2: -
Minnesota	August 2009	-	West Virginia	-	-
Mississippi	-	-	Wisconsin	May 2009	July 2013
Missouri	-	-	Wyoming	-	-

Notes. This table lists the date of implementation of a policy designating spousal relocation as good cause for leaving a job and the date of repeal for states which removed the policy. States which had the policy prior to the beginning of the sample are listed as implementing it Pre-2000; states which have never implemented it are denoted with a dash. If the policy was not repealed by 2017, the date of repeal is designated with a dash as well. One state, Washington, implemented the policy, repealed it, and then re-implemented it. Dates of implementation are collected by the author from state archives of legislation, Department of Labor applications for ARRA Modernization of UI, and Department of Labor annual report of UI Law Comparisons. In cases were the three sources disagreed, priority was given to primary source documents (i.e., legislation first, applications second, and DOL reports last).

Table A-3: Likelihood of Move Given UI Eligibility, ACS

	(1) Cross-CZ	(2) Cross-State	(3) Cross-CZ	(4) Cross-State
Treated, Unmarried	-0.000859	0.000344	-0.00156	-0.000528
	(0.00282)	(0.00178)	(0.00166)	(0.00102)
Treated, Married	0.00482^{+}	0.00170	0.00299^{+}	0.00114
	(0.00248)	(0.00133)	(0.00154)	(0.000783)
State FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Ind. Cov.	yes	yes	yes	yes
State Cov.	yes	yes	yes	yes
Age ¡35	yes	yes	no	no
N	2798158	2798158	15337901	15337901

Standard errors in parentheses; $^{+}$ $p < 0.10$ * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficient on a state-level regressions of percent movers cross-CZ (col. 1 and 3) or cross-state (col. 2 and 4) between year t-1 and t on an indicator for whether the state in time t-1 had UI eligibility for trailing spouses, separately for married. Regressions include state and year fixed effects, as well as individual-level controls for a quadratic of age, indicator for college degree, race dummies, and an indicator for living in home location and state-level controls for state unemployment rate, per capita income, and an index of state-level housing costs.

Table A-4: Effects of UI Eligibility for Trailing Spouses on Claims Determinations

	(1) All	(2) Voluntary Separations	(3) Discharges
Treat	6774.5^{+}	3713.6^{*}	2747.6
	(3465.2)	(1818.4)	(2131.8)
State FE	yes	yes	yes
Year FE	yes	yes	yes
State Cov.	yes	yes	yes
N	765	765	765

Standard errors in parentheses; $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Notes. This table regresses the number of non-monetary determinations in a state in a year on an indicator for whether the state allowing UI eligibility for trailing spouses, along with controls for state unemployment rate, per capita income, housing prices, average age, percent college educated, and percent non-white, along with year and state fixed effects. Column 1 is all non-monetary determinations for separations; column 2 is for voluntary separations only; and column 3 is for discharges only.

Table A-5: Likelihood of Move Given Part-time Workers UI Eligible

	(1) No FE	(2) Ind. FE	(3) Dual-Earners	(4) State X Year FE
Part-Time UI Eligible, Unmarried	0.000189	0.00182	0.00231	
	(0.00514)	(0.00560)	(0.00616)	
Part-Time UI Eligible, Married	-0.00688	-0.0108	-0.000718	-0.0137
	(0.00882)	(0.00979)	(0.0102)	(0.0101)
State, Year FE	yes	yes	yes	no
Covariates	yes	yes	yes	no
Ind. FE	no	yes	yes	no
Worked Last Year	no	no	yes	no
State X Year FE	no	no	no	yes
N	45220	45220	39361	45220

Standard errors in parentheses;$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficient of a regression of moving more than 50 miles on an indicator for whether the state had UI eligibility for part-time workers, interacted with marital status. Column 1 includes state and year fixed effects, controls for individual characteristics including dummies for race and education, and controls for state characteristics including state-year unemployment rates and per capita income. Columns 2 adds individual fixed effects and no longer control for individual characteristics. Column 3 restricts the sample to those who worked at least 1 week in the previous year. Standard errors are clustered at the household level.

Table A-6: Robustness Check: Likelihood of Within Commuting Zone Move Given UI Eligibility

	(1) No FE	(2) Ind. FE	(3) Dual-Earners	(4) State X Year FE
Treated 0.000991	0.00234	0.00402		
	(0.00440)	(0.00529)	(0.00592)	
Married × Treated	0.00586	0.00106	-0.00545	0.000663
	(0.00658)	(0.00758)	(0.00855)	(0.00762)
State, Year FE	yes	yes	yes	yes
Covariates	yes	yes	yes	yes
Ind. FE	no	yes	yes	yes
Worked Last Year	no	no	yes	no
State X Year FE	no	no	no	yes
N	46220	46215	38448	46215

Standard errors in parentheses; $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficients from regressions of moving within commuting zones on an indicator for whether the state had UI eligibility for trailing spouses, interacted with marital status. Column 1 included state and year fixed effects, controls for individual characteristics including dummies for race and education, and controls for state characteristics including state-year unemployment rates and per capita income. Column 2 adds individual fixed effects and no longer control for individual characteristics. Column 3 restricts the sample to those who worked at least 1 week in the previous year. Column 4 includes state X year FE. Standard errors are clustered at the state-year level.

Table A-7: Pre- and Post- Propensity Score Match Demographics

		Treated, Mover	Treated, Non-Mover	Untreated, Mover	Untreated, Non-Mover
Non-Matched	Age	27.6	27.9	26.4	27.1
		(2.63)	(2.58)	(2.50)	(2.62)
	% Black	0.118	0.104	0.172	0.178
		(0.323)	(0.305)	(0.378)	(0.383)
	% BA or more	0.126	0.097	0.144	0.093
		(0.332)	(0.295)	(0.351)	(0.290)
	% w/ kids	0.604	0.670	0.507	0.647
		(0.489)	(0.470)	(0.500)	(0.478)
	% Emp., 3 Mos. Prior	0.579	0.760	0.637	0.764
		(0.488)	(0.419)	(0.468)	(0.417)
	% on UI, 3 Mos. Prior	0.041	0.030	0.017	0.017
		(0.198)	(0.172)	(0.129)	(0.129)
	Log Wages, 3 Mos. Prior	2.71	2.77	2.62	2.66
		(0.673)	(0.607)	(0.698)	(0.653)
Matched	Age	28.0	27.6	27.8	27.3
		(0.170)	(0.014)	(0.141)	(0.011)
	Black	0.095	0.073	0.105	0.068
		(0.019)	(0.001)	(0.020)	(0.001)
	BA or more	0.170	0.176	0.205	0.185
		(0.024)	(0.002)	(0.020)	(0.002)
	% w/ kids	0.537	0.471	0.528	0.419
		(1.093)	(0.002)	(0.024)	(0.002)
	% Emp., 3 Mos. Prior	0.974	0.968	0.964	0.968
		(0.008)	(0.001)	(0.006)	(0.001)
	% on UI, 3 Mos. Prior	0.008	0.011	0.003	0.008
		(0.006)	(0.001)	(0.002)	(0.0004)
	Log Wages, 3 Mos. Prior	2.73	2.68	2.77	2.71
		(0.043)	(0.007)	(0.033)	(0.001)
	Month-HH Observations	1763	254879	3684	404329

Notes. This table reports descriptive statistics on the treated movers (col. 1), treated non-movers (col. 2), untreated movers (col. 3) and untreated non-movers (col. 4), at the month-person level observation level for the primary sample (Panel A: Non-Matched) and for the weighted sample which uses propensity scores to weight samples to match treated movers on observables (Panel B: Matched).

Table A-8: Bounding Exercise: Lower Bound of Post-Move Earnings Effects for Women

	(1)	(2)	(3)	(4)
	$Earn_{i,t}$	$Earn_{i,t+1}$	$Earn_{i,t+2}$	$Earn_{i,t+3}$
γ (Treatment Coef.)	-961.0	436.2	3745.3	9728.2*
	(1604.2)	(3085.7)	(4133.8)	(3853.2)
γ , Lower Bound	-531.2	-1219.4	-839.0	4558.9^{+}
	(1115.2)	(1268.1)	(1844.5)	(2332.9)
γ, PSM	-564.4	1036.8	6030.5^{+}	11618.8**
	(1569.8)	(3244.7)	(3202.0)	(3566.6)
γ Lower Bound, PSM	-1332.3	-1890.2	-1162.0	4926.5^{+}
	(1208.5)	(1391.4)	(2093.9)	(2608.8)

Standard errors in parentheses;$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes. This table reports the coefficient from regressions of earnings in the year of the move (col. 1), one-year post-move (col. 2), two-years post-move (col. 3), and three-years post-move (col. 4) on an indicator for access to UI for trailing spouses, state and year fixed effects, and controls for age, race, college education, kids, and earnings one-year pre-move. The sample is restricted to married, female movers over the age of 23. Panel A and C are for the full-sample, Panel B and D report the lower bound of estimates, assuming Lee (2009) trimming parameter of q =0.69. In panels C and D, observations are weighted using propensity scores. Standard errors are clustered at the state-year level.