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

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 two types of simulations: 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 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.

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 signi cantly increases the likelihood that married households move and that this policy seems to primarily bene t 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 e ects of unemployment insurance generosity on duration of unemployment, labor supply, and post-separation earnings paths more generally (see Krueger and Meyer, 2002 for a revieTtW 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 layoffs 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 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.

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

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.

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).

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 e ects 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 de ne a long-distance move as a move across commuting zones for the reduced form exercises.

To calculate a supplementary estimate of the e ects 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, de ned 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 e ects 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 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.

I also test whether my results are robust to a series of alternative specifications, discussed in further details in Section 4.1.2. First, a growing literature suggests that staggered adoption of a treatment in two-way FE models can result in biased estimates (see (De Chaisemartin and D'Haultfoeuille, 2022) for a survey of literature on this topic). I thus re-estimate my specification using a stacked event study design (Cengiz et al., 2019, Deshpande and Li, 2019) which aligns policy changes by event-time addressing the concern of negative weighting in two-way FE models noted by Sun and Abraham (2021). This exercise also provides evidence in support of the assumptoin of parallel trends. Second, 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). Lastly, I interact treatment with state-level UI generosity to show that the effects of the policy are stronger in the presence of higher potential UI benefits.

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 econometric model illustrates this identification problem:

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

A household's earnings in the coming period are a function of whether a household moves this period $M(D)$, whether they have access to UI for trailing spouses (D), and the interaction between the terms, as well as observable characteristics of the household. 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, irrespective of realized treatment status, I could estimate as follows:

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

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.[5] 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:

$$\begin{aligned} e \;=\;& E[W_{it}|X_{it}, D=1, M(1)=1] \quad E[X_{it}, W_{it}|D=1, M(1)=0] \\ & E[W_{it}|X_{it}, D=0, M(0)=1] \quad E[W_{it}|X_{it}, D=0, M(0)=0] \end{aligned}$$

For e to be equal to ^, 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 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

[5] Alternatively, I could also estimate $\hat{} = \mathbb{E}[W_{i,t+1}|X_{it}, D=1, M(1)=M(0)=1] \quad \mathbb{E}[W_{i,t+1}|X_{it}, D=0, M(1)=M(0)=1]$ 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 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.3.

indicators of whether a person moved in a given year defined as previously ($1(Move)_{i;t+\tau-1}$), 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:

$$Earn_{it} = \alpha_0 + \sum_{\tau=-3}^{3} \beta_\tau 1(Move)_{i;t+\tau-1} + X'_{it-1} + S_{t-1} + T_t + \gamma_i + \epsilon_{it} \quad (2)$$

In this regression, the coefficients of interest are the vector of β_τ, which represent the earnings growth of movers relative to stayers normalized to be zero in the year prior to the move, 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 β_τ across treatment states then indicates the effect of the policy on income growth at the time of a move – that is, the difference $\beta_{D=1} - \beta_{D=0}$ is the E[ ] 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:

$$Y_{im} = \alpha_0 + \sum_{\tau=-12}^{24} \beta_\tau 1(Move)_{i;m+\tau-1} + X'_{im-1} + S_{m-1} + T_m + \gamma_i + \epsilon_{im} \quad (3)$$

I omit the lead for three months prior in the monthly regressions. The coefficient vector of interest in these event studies, β_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 β_τ for treated versus untreated households.

In this analysis, we may be concerned that 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.

To address this, I conduct 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.3. These bounds suggest that β is positive for women, and the lower bound estimates are statistically significantly greater than zero three years post-move.

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.

4.1.2 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 use the ACS data to estimate a stacked event study design (Cengiz et al., 2019, Deshpande and Li, 2019) which serves the dual purpose of addressing concerns about negative weighting in the fixed effects specification and providing a test of parallel trends. In this specification, I assign all policy changes in which the UI for trailing spouse policy `turns on' in four treatment cohorts (2006, 2009, 2010, and 2011) and create stacked cohorts of two years prior and two years post policy implementation.[7] I then stack these cohorts, using never-treated states as the control group in these same years. Appendix Figure **??** plots the coefficients on the interaction of married and treated; I see no evidence of differential trends for married households relative to single households in the pre-period and I see a

[7] I use this window because some states reversed the policy three years after implementing it and including those years would contaminate the estimates.

statistically significant increase in migration rates post-implementation equal to 0.4 p.p. or a 15% increase relative to the base-rate.

Third, 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 men assumed to be more likely to be the leading spouse. Alternate specifications in which I use household income contributions in the year prior to the move to determine the primary or secondary spouse result in similar findings as around 70% of households have husbands making more than half of household income.

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 2 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 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.

Though the estimates for men 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 husband 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.

4.2.2 Annual Earnings

Panel A of Figure 3 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.[9] 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

[9]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.

interpreted as earnings relative to the level of earnings prior to the move.

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 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 3 shows a more pronounced divergence in post-move

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 unemperiod'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, $E[V_{t+1}(d_t; X_{t+1})]$, 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 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 lo... they have an offer in that location, and they can only receive UI if they were laid o... ...gible for UI for trailing spouses following a move.

5.3 F... ...lity

In each perio... ...sehold's flow utility is a function of their consumption, leisure, non-pecuniary utili... ...heir location, and costs associated with a move if relevant. I assume a unitary mode... ...household, rather than a collective model. This is a simplifying assumption; non... ...models typically incorporate decisions to remain in a marriage, where the likeliho... ...ing married helps identify the solution to the Nash bargaining problem. Due to t... ...the state space implicit in a migration model in which households can choose to mov... ...states and the fact that the decision to marry or divorce are not the primary m... ...s at work in my model, I abstract away from the marriage decision, making es... ...of a unitary model more applicable.[10] This is consistent with past models of hous... ...gration which allow the choice of all 50 states (i.e., Guler et al., 2012; Guler and Tas... ..., though Gemici (2011) uses a collective model of the household and restricts the ch... ...Census regions rather than states.

For a household tha... ...location j, supplies labor k_1 and k_2, previously lived in location j_0, and has observab... ...teristics defined by X, flow utility with time subscripts omitted can be expressed as...

$$u(j; k_1; k_2; j_0; X) \ldots n(c) + M(j; k_1; k_2; j_0; X)$$
$$\text{s.t. } p \ldots (j; X)1(k_1 = \text{work}) \ldots)1(k_2 = \text{work}) + b(j_0; k_1; k_2) + A \quad (5)$$

In this function, ...hold consumption an... ...ined fully by the household's choice of location and ...52 e552 ek1tan68 11.9... ...nork.0 Td 81 Tf J/F68 1 Ttrailinork.0 Td 81ice

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 $`_g$.

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[12] living in location j in period t as follows:

$$ln(w_{ijgt}) = \underbrace{{}^{g}_{1}A_{g(i);t} + {}^{g}_{2}A^{2}_{g(i);t} + {}_{jg}}_{\text{observed}} + \overbrace{{}_{g(i)} + e_{g(i);t} + {}_{g(i);j}}^{\text{unobserved to econometrician}}$$

Earnings are a function of observable characteristics of a person (${}^{g}_{1}$, ${}^{g}_{2}$: coefficients on quadratic of age; ${}_{jg}$: 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 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, ${}^{2}_{g}$, using the method from Kennan (2006) to discretize this distribution to two points of support.[13] 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, ${}^{2}_{e_g}$, which varies by gender.

The third term, ${}_{g(i);j}$, is an individual-location specific term and can be thought of as representing an individual's "job" match[14] 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 (${}_{jg}$), 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

[12] 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.

[13] 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 term such that the proportion of individuals with the `high' draw is equal to proportion with a college degree in the population.

[14] This is a slight abuse of the term "job" as I will not be measuring distinct job tenures across terms.

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 ${}^2_g$, which can be approximated by a discrete distribution with three points of support symmetric around zero and governed by the parameter .

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 ( ) 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 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 are considered an offer if in the home location and draws less than are considered an offer if in a distant location, where 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, 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 ( ) 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 $E[V(d_T; X_{T+1})] = 0$. In period T, a household has full information over all realizations that will affect their utility, making their decision a simple discrete choice problem:

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

where

d_T

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:

$$
\begin{aligned}
V(d_{T-2}; X_{T-1}) = & \max_{d_{T-1} \in \mathbb{J}_{T-1}} u(d_{T-1}; d_{T-2}; X_{T-1}) \\
& + \sum_{\mathbb{J}_T} P(\mathbb{J} = \mathbb{J}_{\mathbb{T}} | \ ; \ ; d_{T-1}) \sum_{G} \sum_{G} \int_{N(0; \ ^2_1)} \int_{N(0; \ ^2_2)} \ln \sum_{d_T \in \mathbb{J}_T} \exp u(d_T; d_{T-1}; X_T) \\
& + \ (d_{T-1})
\end{aligned}
\qquad (7)
$$

a ect the choice but are not carried across periods include job o ers, 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

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

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 3 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 bene ts, and lay-o 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.

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 speci ed as follows:

$$ln(w_{ijgt}) = {}_{1g}A_{g(i);t} + {}_{2g}A^2_{g(i);t} + {}_{jg} + {}_{g(i)} + e_{g(i);t} + {}_{g(i);j}$$

While I would ideally estimate the earnings parameters in the model, the number of location xed e ects make this computationally infeasible. Because a simple linear regression of earnings on age and state xed e ects 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 rst best probability of location choices. In this method, one classi es people 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 $_{jg}$ 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.[15] 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. I then define ^ as the coefficients on age and ^ 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:

$$Y_{g(i);t} = ln(w_{ijgt}) \quad \hat{}_{1g} A_{g(i);t} \quad \hat{}_{2g} A^2_{g(i);t} \quad \hat{}_{jg} = {}_{g(i)} + e_{g(i);t} + {}_{g(i);j}$$

I then stack these residuals to be the vector $Y_{g(i)}$, and for each individual, I define a covariance matrix $!_{g(i)} = Y_{g(i)} Y^{0}_{g(i)}$.[16] 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 = ${}^2_g + {}^2_{e_g} + {}^2_g$
2. Same-location off-diagonal terms: covariance of earnings within location across period $= {}^2_g + {}^2_g$
3. Different-location off-diagonal terms: covariance of earnings across location and period $= {}^2_g$

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 , A3-

15

calculates the annual discharge and layo rate as the number of layo s 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 o in a given period.

To account for di erences 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 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 coe cient 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 de ne a move as living in a di erent 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, $^U = [\ _0, \ _1, \ _2, \ _3, \ell_M, \ell_F, A, \ , \ , S_M, S_F]$ 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 $_{\{H;L\}}$.

The parameter estimate is given by the expression:

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

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 $_m$ in the range $[0.5\ _m; 1.5\ _m]$, 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 $\frac{\partial \hat{m}^s}{\partial \ _m}$ 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 4 reports the parameters governing the age earnings pro le and the variances of the unobservable components for men and women.

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 4 reports the parameters governing the distributions of the three residual terms: the individual xed e ect (

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

si3 ecc32

In all counterfactual policies, the subsidy level is $10,000. The first counterfactual has similar incentives to UI for trailing spouses in terms of encouraging the household to move with only one job-in-hand, 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.[22] 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 7 reports the effects of these policies on migration rates for the sample aged 25 to 35 to make the effect sizes 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.

Next, I evaluate the effects of these subsidies on earnings outcomes for movers. Table 8 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.

The earnings declines found in this exercise suggest that the mechanisms induced by the `single-earner' subsidy are not fully capturing the earnings patterns induced by UI for trailing spouses. As is, this counterfactual primarily captures the selection effect of the policy in moves.

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-

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 Tables and Figures

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).

Figure 2: 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.

Panel A

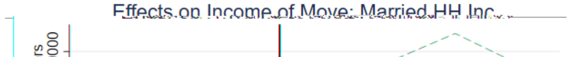
[redacted]

Panel B

[redacted]

Panel C

Effects on Income of Move: Married HH Inc



[redacted]

Figure 4: E ects of UI Eligibility on Wages Post-Move, Three-Month Bins

Notes. This gure regresses wages in three month bins as indicators on indicators for leads and lags surrounding the month of a move across commuting zones, denoted as T= 0 in the gure. 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. . 95% CI are shown, and stars indicate signi cant di erences between treated and control at the 0.05 level (*) and 0.10 level (+).

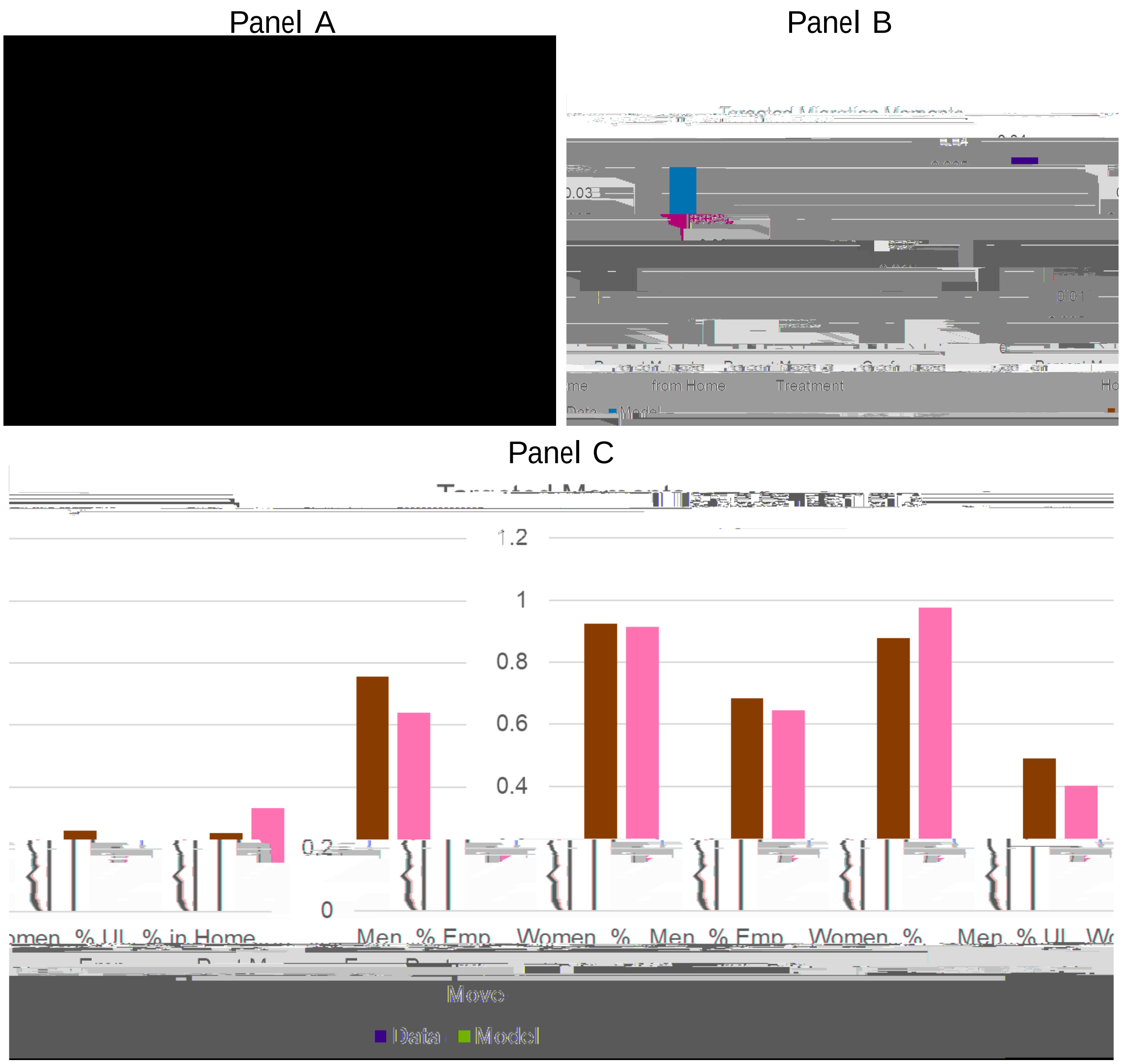


Figure 5: Model Fit: Targeted Moments

Notes. This gure plots simulated data moments and the NLSY97 data moments used to calibrate the utility parameters. Panel A shows the migration rate, de ned 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).

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)

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 coe cients for regressions of likelihood of moving on an indicator for access to UI eligibility for trailing spouses. Column 1 includes state and year xed e ects 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 xed e ects. 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 xed e ects. Standard errors are clustered at the state-year level.

Table 3: Parameter De nitions

Parameter	Description	Estimation Type
$_{jg}$	Location Wage Premium	ACS data, Selection-corrected OLS
$^{M}_{1}; ^{M}_{2}; ^{F}_{1}; ^{F}_{2}$	Age-earnings Pro le	ACS data, Selection-corrected OLS
$_{M}, _{F}$	Earnings Residual, Job-location Match	NLSY97 data, residual decomposition
$_{M}, _{F}$	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 bene t level	Kuka (forthcoming) and UI for trailing spouses data
	Annual layo rate	JOLTS 2005-2018
$_{0}$	Consumption Scaling	Indirect Inference
$_{1}, _{2}$	Moving Cost	Indirect Inference
$_{3}$	Home Preference	Indirect Inference
ℓ_M, ℓ_F	Leisure Value	Indirect Inference
S_M, S_F	UI Take Up Cost	Indirect Inference
	Local O er Rate	Indirect Inference
	Scaling for Distant O er	Indirect Inference
A	Non-labor Income	Indirect Inference

Table 4: Estimates of Earnings Parameters

Panel A: Age Parameters	$_{1M}$	$_{2M}$	$_{1F}$	$_{2F}$		
	0.153	-0.00183	0.108	-0.00138		
	(0.00155)	(0.00002)	(0.00227)	(0.00003)		
Panel B: Variances	$_{M}$	$_{F}$	$_{M}$	$_{F}$	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 e320 Td [(ationfor)-443(the)3494(w)23494ers for3494gometevl w

Table 7: Migration Subsidies: E ects on Migration

	No Subsidy	Single-Earner	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 $

11 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 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,

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 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, as well as look at heterogeneity by education in a larger sample.

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 for those less than 35 (columns 1-3) and those older than 35 (columns 4-6).

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 older 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.

For those younger than 35, the effects of the policy are twice as large for college-educated individuals (0.8 p.p. or 26% increase) than for non-college educated individuals (0.3 p.p. or 13% increase). This suggests that loosening dual-earner migration frictions has greater

A.2.2 Stacked Event Study Design

A growing literature in applied econometrics has documented the fact that identification strategies relying on staggered treatment adoption and two-way fixed effects results in biased estimates of the coefficients (see (De Chaisemartin and D'Haultfoeuille, 2022) for a summary). These biases are a result of the staggered timing putting different weights on each treated unit, depending on whether it was treated early in the sample or late in the

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-5 shows the results of this

There is a marginally statistically signi cant e ect 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 Treated coe cient is now negative and non-signi cant, recall that this is the e ect 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 e ect together from the preferred speci cation (column 2), these coe cients 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.

A.2.6 E ects on State-Level Claims

Given the magnitude of e ects 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:

$$\mathrm{NMD}_{st} = {}_0 + {}_1 1(Treated)_{st} + Z'_{st}\,{}_3 + S + T + {}_{st} \tag{A-1}$$

where NMD_{st} is the number of eligible non-monetary determinations; $1(Treated)_{st}$ 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 xed e ects.

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 e ect of this policy on migration due to states simultaneously increasing state UI generosity and implementing UI for trailing spouses.

Table A-7 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 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   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 , could be estimated as follows in a world of perfect information about all possible states of the world:

$$E[\;] = E[W_{i,t+1} \mid X_{it}, D = 1, M(1) = M(0) = 1] - E[W_{i,t+1} \mid X_{it}, D = 0, M(1) = M(0) = 1]$$

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

$$E[\tilde{\;}] = E[W_{it} \mid X_{it}, D = 1, M(1) = 1] - E[W_{it} \mid X_{it}, D = 0, M(0) = 1] \qquad \text{(A-2)}$$

In this exercise, I demonstrate that $E[\;]$ 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

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

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:

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

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

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 $p = 0.31$.[27]

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

Figure A-1: E ects of UI Eligibility on Migration (ACS), Stacked Event Study

Notes. This gure re-runs the primary speci cation using ACS data for individuals age 23 to 65 in a stacked event study frameworks. Control states are states which never implemented the policy. The baseline migration rate is 2.7 percent annually. 95% CI are shown, clustered at the state-level.

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
O908mr406Option 1 (PT) and and O239.2776(OptiDi.936 w 86(n8as9rMo)-27)-429(.162 cm q .84387 0 0 .843-1259	

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	-	-

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

	(1) All	(2) College	(3) Non-College	(4) All	(5) College	(6) Non-College
Treated, Unmarried	-0.000859	-0.00206	-0.000369	-0.00138	-0.00117	-0.00140
	(0.00282)	(0.00413)	(0.00238)	(0.00129)	(0.00152)	(0.00121)
Treated, Married	0.00482^{+}	0.00784^{+}	0.00326^{+}	0.00218^{+}	0.00193	0.00223^{+}
	(0.00248)	(0.00435)	(0.00163)	(0.00114)	(0.00123)	(0.00112)
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Ind. Cov.	yes	yes	yes	yes	yes	yes
State Cov.	yes	yes	yes	yes	yes	yes
Age < 35	yes	yes	yes	no	no	no
N	2,798,158	1,070,557	1,727,601	12,539,743	4,130,299	8,409,444

Standard errors clustered at state-level in parentheses; $^{+}$ $p < 0{:}10$, $p < 0{:}05$, $p < 0{:}01$, $p < 0{:}001$

Note. This table reports the coe cient on a state-level regressions of percent movers cross-CZ 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 xed e ects, 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: 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 coe cient 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 xed e ects, 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 xed e ects 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-5: Robustness Check: Likelihood of Within Commuting Zone Move Given UI Eligibility

	(1)	(2)	(3)	(4)
	No FE	Ind. FE	Dual-Earners	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 coe cients 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 xed e ects, 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

Table A-7: E ects 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