Dual Earner Migration, Earnings, and Unemployment Insurance

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Abstract

This paper examines how married couples make internal migration decisions and how policies that encourage migration can impact gender earnings equity. I use a triple differences design to show that access to unemployment insurance (UI) for trailing spouses increases migration by 2.6 pp. Women are the primary beneficiaries, with higher UI uptake and higher annual earnings post-move. I estimate a structural model of household migration and show that migration subsidies linked to trailing spouse status increase household utility by 0.3 percent and increase lifetime income for women impacted by the policy by around $25,000.

JEL Codes: D1, J1, J16, J61, J65, R5.
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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. When one spouse receives a distant job offer, the other spouse may 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; Boyle et al., 2009; Gemici, 2011; Jayachandran et al., 2023). These frictions also reduce mobility, causing individuals not to move if the costs are too high for their spouse and making it harder to move up the job ladder by moving to a productive job at a distance.

This paper analyzes 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. Twenty-three states' unemployment laws allow trailing spouses to collect UI. I use variation in policy implementation timing in a triple-differences methodology to evaluate whether access to UI for trailing spouses has a meaningful impact on households' decisions to migrate and post-move labor market outcomes. Using panel data from the geo-coded National Longitudinal Survey of Youth 1997 (NLSY97), I find that this policy increases the likelihood a married household moves more than 100 miles by 2.6 percentage points off a base rate of 6.4 percent (40%). I then show that this result also holds in the larger, nationally representative sample from the American Community Survey where this policy is associated with a 25% increase in cross-commuting zone migration rates. I next examine post-move outcomes for married households, regressing earnings, employment, and UI receipt in the periods surrounding a period t on indicators for whether a person moved in t interacted with an indicator for having access to the policy. I find that the policy has a significant positive impact on women's earnings, but I cannot reject a null effect for men. Women are also more likely to collect UI following a move in the presence of the policy, whereas men's UI receipt is unaffected. 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. To evaluate this, we must understand what mechanisms drive the household migration behaviors seen in the reduced form results. There are two types of spatial search frictions that might hold back households from moving. First, it is possible that the spouses have different labor markets that are their most productive location. In this scenario, government intervention may not be necessary on efficiency grounds: households are already choosing their optimal location given the constraints of wanting to live together despite individual location preferences. However, if the reason that households do not move to a more productive labor market is due to uncertainty about how long it will take for the trailing spouse to find a job in the new location and the costs associated with extended periods with one-earner (i.e., due to credit constraints), households may not move to a location that is ultimately productive for both spouses. In this case, policies such as UI for trailing spouses have an important role to play in smoothing consumption to allow households to move to more productive job and location matches.

I therefore turn to a dynamic model of household location choice in the presence of UI which extends previous models of migration to incorporate households with two earners, as well as explicitly incorporating UI in the household's budget constraints to better understand 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 and American Community Survey (ACS). I use the model to 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.

First, I compare migration outcomes in the baseline model to scenarios in which I change the spatial search frictions inhibiting dual-earner migration. The model includes two main frictions. First, the quality of spouses' offers may differ across locations, resulting in different individual first-best jobs. Second, it is possible that only one spouse will have an offer, inducing uncertainty about how many periods it will be until the other spouse receives an offer. To test the relative importance of each of these frictions, I first shut-down both mechanisms by simulating behavior if the households only make decisions based on one spouse's job. This scenario more than doubles the annual migration rate, increasing from 2.6% to 5.8%. I then shut down just the timing of offers mechanism by simulating 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 1.0 p.p. which corresponds to about one-third of the change in migration when both mechanisms are shut-down. Additionally, I show that households earn more post-move in the scenario with simultaneous offers and that these moves result in larger earnings gains for both spouses than if they had not moved. This demonstrates that mismatched timing of offers holds households back from moving to locations that are ultimately productive for both spouses.

Second, 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 match the other two subsidies. The second subsidy mirrors relocation incentive programs in Europe, in which unemployed job-seekers who accept a job that require a relocation 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 unconditional subsidies increase migration rates more – by 52% relative to 20% for the trailing spouse subsidy– but result in substantively lower lifetime earnings gains for women (-4.4%). In contrast, the trailing spouse subsidy increases women's lifetime earnings by 0.4%. The unemployed job-seeker subsidy has low takeup, smaller impacts on migration than the other two subsidies, and small negative impacts on both men and women's lifetime earnings, suggesting it is not an effective policy for encouraging unemployed workers to search out-of-state for jobs. Though all three subsidies increase lifetime utility by reducing financial constraints that prevent productive moves, only the trailing spouse subsidy achieves this while not also exacerbating gender inequalities in post-move labor market outcomes.

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 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., Cooke et al., 2009; Gemici, 2011; Burke and Miller, 2017; Rabe, 2011; Blackburn, 2010) document 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. 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 versus trailing spouses. I show 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 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 UI generosity on duration of unemployment, labor supply, and post-separation earnings paths more generally. Theory suggests that more generous UI should result in higher reservation wages and post-separation job quality. Some past research (Marimon and Zilibotti, 1999, 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., Nekoei and Weber, 2017), but others only seeing a weak or null effect (Card et al., 2007; Van Ours and Vodopivec, 2008; Schmieder et al., 2016; Le Barbanchon et al., 2019). The majority of these studies 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 UI 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. My model highlights a key friction unique to dual-earner movers relative to individual movers: one spouse may be moving without a job-in-hand and have uncertainty about the timing of an offer arriving. Single-mover models (e.g., Kennan and Walker, 2011) typically assume the mover has potential offers in all locations. Though past papers have considered how earnings gains and government benefits across locations drive individual migration (e.g., Kennan and Walker, 2010; Ransom, 2022), papers focusing on dual-earner migration have not examined the role of government benefits in migration decisions (e.g., Braun et al., 2021; Gemici, 2011; Guler and Taskin, 2013; Foerster and Ulbricht, 2023). 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; Flabbi and Mabli, 2018). This paper demonstrates that being able to use UI to buffer against a period of job search for a trailing spouse has impacts not just in the short run, but also has long run impacts on lifetime earnings and household welfare.

2 Institutional Setting and Data

Unemployment insurance provides compensation to full-time workers who are no longer employed, with eligibility determined partially through non-income based eligibility criteria. One such non-income 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.'

US states are given considerable 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]

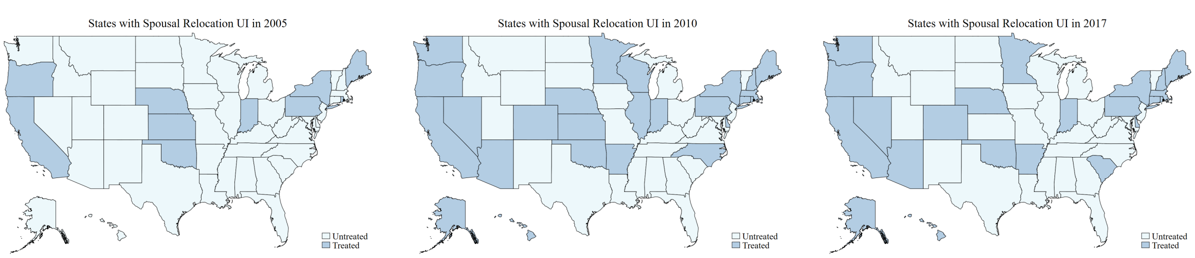

Figure 1: Change in States with Spousal Relocation UI, 2005, 2010, 2017
Notes. This figure shows the states which had UI for trailing spouse policies in 2005 (beginning of sample), 2010 (after ARRA), and 2017 (end of sample).

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

To analyze the effects of UI for trailing spouses, I require data that allows me to observe the same household repeatedly 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.

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 county of residence. My primary sample includes 10,751 married household-year observations and 2,859 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 28,089 person-year observations with 6,008 individual respondents.[2] Table 1 reports descriptive statistics for treated and non-treated married and unmarried households in the sample.

I look at four outcomes of interest in the regression analyses: annual migration, annual earnings, monthly UI receipt, and monthly employment. I use commuting zones combined with distance to define migration decisions. The NLSY97 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. To access UI under this policy, applicants apply for UI and must provide details of the way the move made their old job inaccesible. A state panel then determines if that move would make commuting impractical. Using commuting zone moves is thus likely a better proxy for moves that make one eligible for UI than a cross-state move. When using state moves as the

[2] 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.

outcome, I am both incorporating some moves that would not constitute good cause under the statutes (e.g., a move from New York City to Hoboken, NJ) and missing within state moves that require leaving one's job (e.g., a move from San Diego to San Francisco).

Table 1: Summary Statistics

	Married			Not Married		
	Full Sample	Treated	Not Treated	Full Sample	Treated	Not Treated
Age	27.18	27.64	26.94	28.01	28.46	27.72
	(1.986)	(1.987)	(1.942)	(3.410)	(3.374)	(3.402)
White	0.777	0.752	0.789	0.627	0.642	0.616
	(0.417)	(0.432)	(0.408)	(0.484)	(0.479)	(0.486)
BA or more	0.338	0.333	0.340	0.339	0.365	0.323
	(0.473)	(0.472)	(0.474)	(0.473)	(0.481)	(0.467)
Ind. Earnings, 2012$	37674.7	39754.9	36577.4	32083.9	34544.4	30436.0
	(26223.6)	(27671.8)	(25360.4)	(24862.2)	(26802.1)	(23330.0)
Household income 2012$	77285.8	81807.6	74909.5	63833.0	68270.8	60966.4
	(53480.7)	(59183.3)	(50066.1)	(70995.3)	(74958.7)	(68162.1)
Number of kids	1.210	1.252	1.188	0.391	0.347	0.420
	(1.109)	(1.089)	(1.118)	(0.870)	(0.823)	(0.899)
State per capita income	38137.7	40176.7	37064.1	40936.5	43596.9	39212.2
	(5444.5)	(5470.5)	(5114.2)	(7241.4)	(7238.1)	(6701.6)
State unemployment rate	6.853	7.633	6.443	6.463	7.055	6.079
	(2.514)	(2.534)	(2.404)	(2.200)	(2.289)	(2.050)
Moves State	0.0534	0.0452	0.0578	0.0591	0.0543	0.0622
	(0.225)	(0.208)	(0.233)	(0.236)	(0.227)	(0.241)
Moves Across CZ	0.0880	0.0778	0.0933	0.0977	0.0840	0.107
	(0.283)	(0.268)	(0.291)	(0.297)	(0.277)	(0.309)
Moves > 100 miles	0.0698	0.0613	0.0743	0.0796	0.0706	0.0854
	(0.255)	(0.240)	(0.262)	(0.271)	(0.256)	(0.280)
Observations	10751	3841	6910	28089	10852	17237
Households	2859	1375	2012	6008	3029	4264

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.

Thus, in my primary specifications in which moves are at the annual level, 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 100 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. I define annual earnings as the annual earnings from wages and salary. For the years during the biennial data collection in which annual income is not recorded (2012, 2014, 2016, 2018), 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.

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).[3] Monthly UI receipt is measured as whether the respondent or spouse received positive income from UI in a given month. A small number of respondents/spouses report working for all 52 weeks in the year and also report receiving UI; I re-code their response to be non-receipt of UI. Employment at the monthly level is based on weekly job history which I aggregate this to the monthly level by reporting a person as employed in a month if they worked at least one week of a job in a given month.

I supplement the NLSY97 data with a number of alternative data sets. 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. For the analysis using the ACS, I cannot use migration distance to define moves or look at labor market outcomes over time, but can look at one-year migration rates across commuting zones as a robustness check that allows for a larger sample size and wider age coverage.

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) 2004-2019 (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.

3 Empirical Exercise #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.

[3]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.

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), individual fixed effects (θ_i), and time-varying characteristics of the sending state (Z_{st}: per capita income and unemployment rate). 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 use unmarried individuals as a plausible comparison group in a triple differences regression specified as follows:

$$\begin{aligned}\mathbb{1}(\text{Move})_{it} \quad = \quad & \sum_{M=0,1} \mathbb{1}(\text{Married}_{it} = M) \times \left[\beta_0^M + \beta_1^M \mathbb{1}(S_{t-1} = \text{Treated})_{it} + X'_{it}\beta_2^M + Z'_{s,t-1}\beta_3^M + S_{t-1}^M + T_t^M\right] \\ & + \theta_i + \epsilon_{it} \end{aligned} \tag{1}$$

where β_1^1 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.

The identifying assumption is that while single households face the same state-year conditions as married households at the time of policy implementations, the policy should not affect their migration decisions.[4] I run the regression as specified, allowing β_1^0 to soak up anything changing concurrently with the policy, and then I run a specification where I include state by year fixed effects which captures anything that changes at the state-year level that affects both single and married households. Finally, I adjust for bias from the staggered treatment in stacked cohort triples differences design and using the method from Borusyak et al. (2021). I also test whether my results are robust to a series of alternative specifications including analyses using the ACS, tests or placebo outcomes, and alternative outcomes that support the findings. Additional detail of these robustness checks are discussed in further details in Section 3.2.

[4] An alternate control group would be single-earner married households. In practice, I do not have the statistical power to use this sample in the NLSY because the number of persistently single-earner households (defined as households for whom I observe at least one of the spouses is not working for 75% or more of the observed years) is too small.

3.1 Results

I first test the impacts of access to UI for trailing spouses on likelihood of a household move. Table 2 reports the coefficient of interest, β_1^1, which represents the effect of having access to UI for trailing spouses on likelihood of moving for married households. The first column is two-way fixed effects model including covariates and individual fixed effects with separate treatment effects for married and unmarried couples; the second column omits the treatment for unmarried households and includes state-year fixed effect; the third column implements a stacked triple differences specification in which I construct a separate data set for each valid cohort and stack them to estimate the treatment effect as proposed in Cengiz et al. (2019) and Deshpande and Li (2019); and the fourth column implements the bias adjustment from Borusyak et al. (2021). Panel A reports results for cross-commuting zone moves greater than 100 miles in the NLSY and Panel B reports results for cross-state moves which are used in the structural model estimation.

Table 2: Likelihood of Move Given UI Eligibility

		(1) DD	(2) DDD	(3) Stacked DDD	(4) Borusyak et al. (2021)
Panel A: NLSY, > 100 Mile Move	Treated	-0.015			
		(0.011)			
Base Rate: 6.4%	Married × Treated	0.042^{**}	0.027^{+}	0.038^{*}	0.026^{**}
		(0.016)	(0.015)	(0.018)	(0.009)
	N	38,840	38,840	44,519	45,329
Panel B: NLSY, Cross-State Move	Treated	-0.014			
		(0.012)			
Base Rate: 5.5%	Married × Treated	0.037^{*}	0.023	0.036^{*}	0.024^{*}
		(0.014)	(0.014)	(0.017)	0.012)
	N	38,840	38,840	44,519	45,329

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. Column 2 omits the non-married treatment indicator and includes state-year fixed effects. Column 3 is a stacked triple difference regression. Column 4 is a triple difference specification adjusted using Borusyak et al. (2021). Controls include individual fixed effects, an indicator for kids, state-year unemployment rates, and state-year per capita income. Standard errors are clustered at the state level.

Focusing first on my preferred definition of moves – cross-commuting zone moves greater than 100 miles – the results suggest that access to UI 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 all specifications. In the fourth specification which adjusts for bias from the staggered treatment, access to UI for trailing spouses is associated with a 2.6 percentage point or 40% increase in migration rates off a base rate of 6.4%. The coefficients for unmarried individuals are negative and non-significant in column 1. 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.[5] Results are similar but more noisily estimated when using state moves as the outcomes

Next, I supplement these results with an analysis in the ACS in which I can look at cross-commuting zone migration for all ages, as well as looking separately by age and education level. Table 3 reports the results of the triple difference specification, analogous to column two of Table 2. I show that the effects of the policy are smaller in levels than those seen in the NLSY97 sample, but are still statistically significant and substantive in terms of percent change relative to base migration rates. There is a significant positive impact for the full sample who have a 0.3 p.p. (25%) higher likelihood of moving in the presence of the policy. Effects are slightly larger in levels for those younger and college-educated, but are similar in percent terms and I cannot reject the null that effects are equal across age and education groups.

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

	(1) All	(2) <35	(3) >35	(4) College	(5) Non-College
Treat × Married	0.003^{*}	0.004^{+}	0.002^{*}	0.004^{*}	0.003^{*}
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Observations	14,078,827	2,572,818	11,506,009	4,823,730	9,255,097
Baseline Mean	0.012	0.026	0.008	0.013	0.011

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

Note. This table reports the coefficient on a triple difference regression 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 interacted with marriage. Regressions include pre-move-state by year fixed effects and current-state 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. Column 1 is the full sample, Column 2 is individuals 35 or under, Column 3 is individuals older than 35, Column 4 is college-educated, and Column 5 is non-college educated.

3.2 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 are given in Appendix Section A.2.

First, because my identification strategy relies on staggered implementation of a policy, I test whether the migration rates in periods prior to implementation exhibit pre-trends using the two-way fixed effect bias correction proposed by Borusyak et al. (2021) in an event study framework.[6] Figure 2 plots the coefficients on the interaction of married and

[5] 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.

[6] In this specification and in the stacked triple differences specification discussed above, I omit the post-reversal years for states which reversed the policy three years after implementing it, as the estimation methods do not allow for non-monotonicity in the policy turning on or off.

treated from the methodology from Borusyak et al. (2021); I see no evidence of differential trends for married households relative to single households in the pre-period and the post-treatment effects, though noisy, are consistent with the average increase in migration rates post-implementation equal to 2.6 p.p. reported previously.

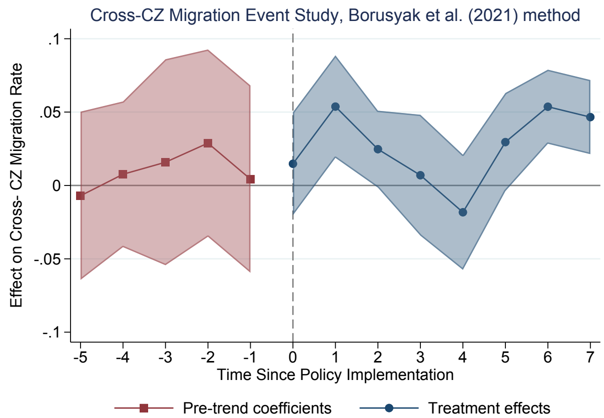

Figure 2: Effects of UI Eligibility on Likelihood of Moving, Borusyak et al. Adjustment
Notes. This figure plots the coefficients on the triple difference coefficient from the bias-adjusted regression of likelihood of moving across commuting zones on an indicator for UI for trailing spouses, adjusted for bias in staggered treatment adoption designs using the Borusyak et al. (2021) design. 95% CI are shown.

Second, I estimate two placebo tests: a policy implemented as part of UI modernization that should not increase 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 UI eligibility for part-time workers, the policy most often implemented simultaneously with UI for trailing spouses as part of the ASSA, decreases the likelihood that a household moves more than 100 miles (see Appendix Table A-3 Panel A), which provides support for the identification argument that the implementation of this policy as part of the ARRA does not bias the results. This suggests that other policies which increased overall UI generosity for non-movers induced households to be less likely to move at the time of the policy changes. I then show that UI eligibility has no impact on moves within a commuting zone (see Appendix Table A-3 Panel B), 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). In 2019, there were approximately 58 million married households in the United States and the ACS baseline migration rate implies then that there were around 812,000 married households that moved. If all 3713 of these increased claims were due to this policy, 3,713 additional moves would be consistent with a 0.4 pp increase in the migration rate which is of slightly larger magnitude than the effects seen in the ACS. In percent terms, this would be a 28.5% increase relative to the ACS commuting zone migration rate, consistent in levels with the percent change in migration seen in the NLSY. This provides evidence that the magnitudes of my estimates are plausible.

4 Empirical Exercise #2: 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 in the long run. This effect will hold regardless of whether this household is an 'always mover' who moves regardless 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.

While I cannot directly observe which spouse is the leading or trailing spouse in the data, I use gender as a proxy for leading spouses, with men assumed to be more likely to be the leading spouse.[7] The goal of this exercise is to identify the direct effect of this policy on women– how does access to UI for trailing spouses change women's UI take up, earnings, and wages? To test this, I regress annual earnings, monthly UI receipt, and monthly employment status in the years and months surrounding a move on indicators for moving interacted with

[7]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. 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.

treatment status in the following specification:

$$Y_{i,t+n} = \beta_0 + \beta_1 \mathbb{1}(\text{Move})_{it} + \beta_2 \mathbb{1}(Treat)_{it} + \beta_3 \mathbb{1}(\text{Move})_{it} \times \mathbb{1}(Treat)_{it} + X'_{it}\beta_4 + S_{t-1} + T_t + \epsilon_{it} \quad (2)$$

The coefficient of interest is β_3 with standard errors clustered at the household level because the 'treatment' in this specification – moving – is at the household level. Subscript t is the period in which migration is measured; the outcome is then measured in n periods pre or post the focal year of the move. When the outcome of interest is earnings, periods are years, and when the outcome of interest is employment status or UI receipt, periods are months. The sample is restricted to married households. Covariates X_{it} are the the same as in the migration regressions.

However, measuring the direct effect of the policy on post-move wages is complicated by the fact that moving itself affects wages and that the decision to move is endogeneous to the policy. To reduce the effect of selection (i.e., the indirect effect), I use propensity score weighting methods to re-weight observations to be observably similar to treated movers. 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 be unrelated to labor market outcomes other than through migration. Further detail on how I estimate the propensity 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.[8] Since I cannot control for all possible scenarios that would violate this assumption, I conduct a bounding exercise adapted from Lee (2009) in which I assume that the selection into migration is either the very highest or lowest earners and calculate the effects omitting these observations, giving me lower and upper bounds on my estimate. This method is described in more detail in the Appendix Section A.4.

4.1 Annual Earnings

I first look at whether earnings post-move differ in the presence of the policy. Typically, moves are associated with earnings gains for married men and earnings losses for married

[8]Fertility is often timed simultaneously with moves. However, when I use number of children as an outcome instead of earnings, I find that there is no significant difference in likelihood of having a child post-move for those with or without access to UI for trailing spouses.

women who are more likely to be trailing spouses. In the year of the move, we would expect the effect of UI for trailing spouses to be negative for trailing spouses who are more likely to have zero earnings from labor if they can access UI. We would expect the direct effect of UI on trailing spouses' earnings to be ambiguous in the long-run; it might increase earnings by increasing a job searcher's reservation wage or it might decrease earnings if the workers' increased time out of employment results in human capital depreciation.

Figure 3 plots lower and upper bounds of estimates of the effects of the policy on moving (i.e., β_3 from equation 2) for men in Panel A and women in Panel B. For men, we cannot reject a null effect which is consistent with the fact that men are less likely to be the trailing spouse. In contrast, we see that women earning significantly less in the year of the move – consistent with remaining out of the labor force directly post-move – and more in the presence of UI for trailing spouses one and three years post-move. I find that the lower bound of the effect on women's earnings for one-year and three-years post move is positive and statistically significant, showing that the lower bound of estimates is that women earn around \$4,700 more annually three years post-move in the presence of the policy. Appendix Table reports the unbounded coefficients from the regression of annual earnings in year prior to and post period t on indicators for moving in period t interacted with originating in a state that offers UI for trailing spouses. Female movers from treated state have earnings gains that are \$10,795 higher (significant at the $p < 0.05$ level) than movers from comparison states three years post-move. The bounding exercise halves this effect size but is still significant, meaning that even if trailing spouses who select into move in the presence of the policy are positively selected, there is still a significant direct effect of receiving UI on earnings post-move.

4.2 Employment Outcomes

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 being more likely to re-enter employment than women who move without access to UI. I therefore look at the unemployment receipt and post-move employment status for married women in the presence of the policy. Table 4 reports the coefficients from the regression of monthly UI take up in the months following period t on indicators for moving in period t interacted with originating in a state that offers UI for trailing spouses. Table 5 reports the coefficients from the regression of an indicator for being employed in the months following period t on indicators for moving in period t interacted with originating in a state that offers UI for trailing spouses. All regressions are

Panel A Panel B

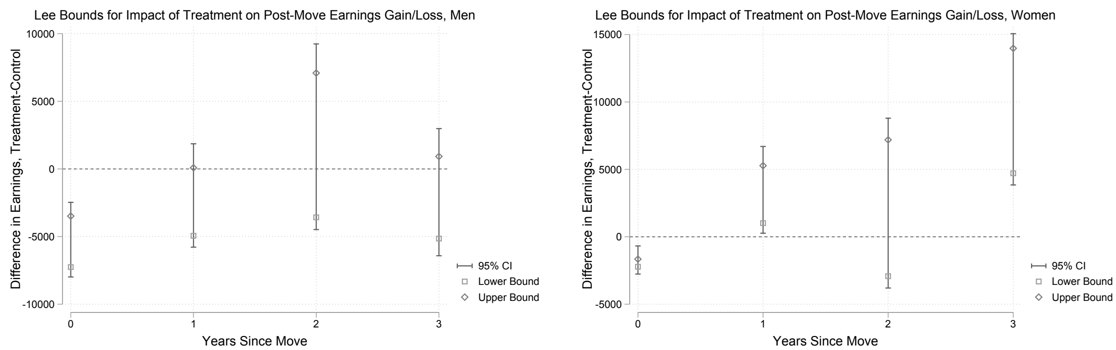

Figure 3: Lee (2009) Bounds on Effects of UI Eligibility on Earnings for Married Men and Women

Notes. This figure plots the bounds on the coefficients of regressions of income from wages and salary in years following the year of a move across commuting zones. The points plotted indicate the difference in earnings movers vs. staters for treated relative to non-treated. Panel A shows the bounds for men; Panel B shows the bounds for women. Confidence intervals are bootstrapped estimates of the bounds.

on a balanced panel of married individuals age 23 or higher and employed for at least one week two months prior to the move.

Table 4: Impact of UI for Trailing Spouses and Move on Monthly UI Receipt

		(1)	(2)	(3)	(4)	(5)
		UI, t+1	UI, t+2	UI, t+3	UI, t+4	UI, t+5
Panel A: Married Men	Treat	0.00647*	0.00690*	0.00592	0.00413	0.00271
		(0.00289)	(0.00346)	(0.00376)	(0.00403)	(0.00436)
	Move	0.0144+	0.0134+	0.00578	0.00341	0.000489
		(0.00749)	(0.00738)	(0.00627)	(0.00623)	(0.00583)
	Treat X Move	-0.00751	0.00128	0.00169	0.00258	0.0140
		(0.0126)	(0.0144)	(0.0129)	(0.0122)	(0.0138)
	Observations	97826	96422	94990	93537	92073
Panel B: Married Women	Treat	0.00282	0.00405	0.00520	0.00643	0.00675
		(0.00231)	(0.00285)	(0.00344)	(0.00394)	(0.00451)
	Move	0.00713	0.000463	0.0109	0.00489	-0.00331
		(0.00769)	(0.00545)	(0.00977)	(0.00826)	(0.00417)
	Treat X Move	0.0373*	0.0418*	0.0244	0.0402*	0.0472**
		(0.0186)	(0.0176)	(0.0186)	(0.0192)	(0.0177)
	Observations	92831	91635	90439	89224	87995

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

Notes. This table reports the coefficient on a regression of men's (panel A) and women's (panel B) lagged monthly UI receipt on indicators for moving interacted with living in a state with UI for trailing spouses, as well as controls for age, race, education, children, earnings prior to the move, and state and year FE. Sample restricted to married couple. Standard errors are clustered at the household level. All regressions are propensity score weighted; see appendix section A.3 for description of weight.

Table 5: Impact of UI for Trailing Spouses and Move on Monthly Employment

		(1)	(2)	(3)	(4)	(5)	(6)
		Emp, t+1	Emp, t+2	Emp, t+3	Emp, t+6	Emp, t+9	Emp, t+12
Panel A: Married Men	Treat	-0.00194	-0.00241	-0.00189	-0.000117	0.00339	0.00483
		(0.00435)	(0.00537)	(0.00626)	(0.00852)	(0.0108)	(0.0122)
	Move	-0.108***	-0.0697***	-0.0576***	-0.0256+	-0.0359+	-0.00945
		(0.0182)	(0.0152)	(0.0145)	(0.0154)	(0.0210)	(0.0160)
	Treat X Move	0.0107	-0.00813	-0.0112	0.000494	0.0230	-0.0180
		(0.0291)	(0.0265)	(0.0257)	(0.0249)	(0.0288)	(0.0262)
	Observations	98202	96959	95714	91995	88297	88297
Panel B: Married Women	Treat	0.000621	0.00202	0.00116	0.00278	0.00441	0.00256
		(0.00557)	(0.00694)	(0.00798)	(0.0110)	(0.0133)	(0.0157)
	Move	-0.239***	-0.163***	-0.118***	-0.101***	-0.0835**	-0.0852**
		(0.0301)	(0.0266)	(0.0253)	(0.0257)	(0.0265)	(0.0259)
	Treat X Move	-0.0483	-0.0958*	-0.103*	-0.0481	-0.0504	-0.0407
		(0.0437)	(0.0419)	(0.0411)	(0.0389)	(0.0407)	(0.0417)
	Observations	93152	92045	90960	87689	84361	84361

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

Notes. This table reports the coefficient on a regression of men's (panel A) and women's (panel B) lagged monthly employment status on indicators for moving interacted with living in a state with UI for trailing spouses, as well as controls for age, race, education, children, earnings prior to the move, and state and year FE. Sample restricted to married couple. Standard errors are clustered at the household level. All regressions are propensity score weighted; see appendix section A.3 for description of weight.

The results for UI are consistent with the expectation that men are the leading spouse and women are the trailing spouse. There is no significant difference in UI take-up post move for men who are treated, consistent with them not being the trailing spouse who uses the policy. There is marginally significant higher UI take up among all male movers relative to non-movers. Because this exists for men who do not have access to UI for trailing spouses, this is consistent with households moving with a husband who was laid off prior to the move rather than men leaving their job for a move. For married female movers, there is a positive and statistically significant impact of access to the policy on UI receipt. Married women who move in the presence of this policy are 4-5 p.p. more likely to be collecting UI following a move than movers without access to the policy. The base rate for unemployment receipt one month post-move for untreated married women is 1.1%, meaning that treated women are four to five times as likely to receive UI post-move. This effect persists for up to five months post-move. This is consistent with a story in which women are more likely to be a trailing spouse and have quit their job for the move, thus making them the spouse primarily impact by the policy.

For both men and women, moves are associated with a significantly lower likelihood of being employed in the first three months post-move, regardless of treatment status. This is consistent with households being more likely to move when searching for a job, as well as households moving with the intention of starting a job after they have had a short period to adjust to the location. While men are no longer significantly less likely to be employed one year post-move (column 6 of Table 5), women who move are still 8.5 p.p. less likely to be employed than non-movers one-year post-move.

Consistent with the findings that treated women are more likely to be collecting unemployment insurance, we do see that a greater proportion of female movers are not employed in the presence of the policy, with treated female movers being about 10 p.p. less likely to be employed than untreated female movers two months and three months post move (col. 2 and 3.). However, by six months post-move, women are no longer significantly less likely to be employed in the presence of the policy than in the absence. More importantly, they are not *more* likely to be employed one year post-move, meaning that the finding that women earn more in the presence of the policy is likely attributable to better quality jobs rather than higher employment rates.

5 Structural Model of Dual Earner Migration

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. 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 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. Next, I use the model to compare the migration and earning outcomes 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, including long-term outcomes such as lifetime income and lifetime welfare that could not be measured in the reduced form.

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 whether the household will save. This decision repeats until they reach the terminal period of the model at age of 64, assuming 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 $J = (j_1, j_2, ...j_T)$, savings $S = (s_1, s_2, ..., s_T)$ 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, each member of the household receives an offer of a job in their current location with probability λ and in all distant locations with probability $\rho \times \lambda$. Offers occur for both the employed (i.e., on-the-job undirected search) and for the unemployed. This offer contains their wage offer which includes a location-match component unknown prior to receiving an offer. If the offer in their current location 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, s_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 and Flow Utility

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

$$V_t(d_{t-1}, X_t) \;\; = \;\; \max_{d_t \in \mathbb{J}_t} u(d_t, d_{t-1}, X_t) + \beta \mathbb{E}[V_{t+1}(d_t, X_{t+1})] + \epsilon(d_t) \qquad (3)$$

where d_t represents a household's choice of location and labor supply for each spouse (j_t, s_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. 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.

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.[9] 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 $d = [j, s, k_1, k_2]$, previously lived in location j_0, previously chose savings s_0 and has observable characteristics defined by X, flow utility with time

[9] 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 ever divorce suggest that there are not substantive differences in migration rates while married.

subscripts omitted can be expressed as follows:

$$\begin{aligned} u(d, j_0, s_0, X) &= \alpha_0 ln(c) + M(j, k_1, k_2, s, j_0, X) \\ \text{s.t. } p_j c - s &= w_1(j, X)\mathbb{1}(k_1 \neq 0) + w_2(j, X)\mathbb{1}(k_2 \neq 0) + b(j_0, k_1, k_2) + A + s_0 \end{aligned} \quad (4)$$

In this function, c is household consumption and is determined fully by the household's choice of location, work, and savings. Individuals have three possible labor market states: 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 such as differences in housing costs. If they work, spouse $g \in [1, 2]$ receives earnings $w_g(j, X)$ which varies with location and individual characteristics. If they do not work and were laid off, a spouse receives benefit b; the next period, they do not receive benefits if still unemployed. Those who quit receive no benefits (unless the UI for trailing spouse applies). 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.[10]

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, women are more likely to be the trailing spouse and forgo earnings for their spouse's long-distance move. A linear-consumption model results in independent optimization by spouse, making it difficult to replicate the fact that the secondary earner is less likely to lead a move. A log-linear functional form also means that the role of UI as a consumption smoother is one of the mechanisms through which the policy induces migration, and it is therefore important to consider alternative ways that households can smooth consumption across periods through savings. Because allowing for continuous savings would be computationally infeasible in this setting, I instead assume that households can either choose not to save or can choose to save a percent of their income chosen from a discrete choice of options to be estimated in the model.[11]

In addition to receiving utility from consumption, each household receives a location-

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

[11] I have also estimated versions of the model in which I do not allow for savings. While the exact magnitudes of the model predictions change, the substantive direction of the impacts of counterfactual scenarios is unchanged.

specific non-pecuniary utility flow represented by $M(j, j_0, X)$:

$$\begin{aligned} M(j, j_0, X) &= \overbrace{-(\alpha_1 + \alpha_2(\text{age}_t - 24) + \alpha_3(\text{age}_t - 24)^2)\mathbb{1}(j \neq j_0)}^{\text{one-period costs of moving}} \\ &\quad + \underbrace{\alpha_4 \mathbb{1}(j = \text{home})}_{\text{permanent component}} + \overbrace{\alpha_5 \mathbb{1}(s > 0)}^{\text{savings preference}} + \sum_{g=M,F} \underbrace{\ell_g \mathbb{1}(k_g \neq \text{work})}_{\text{opp. cost of work}} \end{aligned}$$

Location-specific utility can be split into two parts. The one-period moving cost includes a fixed cost to moving (α_1) and a cost that is a function of age, meant to capture the fact that households move more at younger ages. The permanent component (α_4) includes a preference for living in one's home location (defined based on location where the focal NLSY respondent was in at age 14).

The other components of non-pecuniary utility relate to savings and labor force participation (LFP) choices. The savings coefficient, α_5, represents non-pecuniary utility that the household derives from savings, included to help match the proportion of households that actual save and representing aggregate tendencies towards savings for reasons other than consumption smoothing.[12] The coefficients related to LFP, denoted ℓ_g, represent the opportunity cost of working, which can be thought of as an amalgamation of preferences for leisure and home production (e.g., child care, chores, etc.) and is allowed to vary by gender.

5.3 Earnings and Savings 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[13] living in location j in period t as follows:

$$ln(w_{ijgt}) = \underbrace{\gamma_1^g A_{g(i),t} + \gamma_2^g A_{g(i),t}^2 + \gamma_3^g \mathbb{1}(\text{College})_{g(i)t} + \mu_{jg}}_{\text{observed}} + \overbrace{\eta_{g(i)} + e_{g(i),t} + \theta_{g(i),j}}^{\text{unobserved to econometrician}}$$

Earnings are a function of observable characteristics of a person (γ_1^g, γ_2^g: quadratic of age; γ_3^g: college; 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

[12] In practice, savings levels are very low among the population that are most migratory – those under the age of 35 – and unlikely to account for a large portion of consumption smoothing. In the NLSY97 sample, the median financial assets of the married sample are \$800 at age 25, \$2,500 at age 30, and \$6,800 at age 35. Movers do not have savings substantively different than non-movers.

[13] 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.

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, $\sigma^2_{\eta_g}$, using the method from Kennan (2006) to discretize this distribution to two points of support. The second component is a period-specific transitory income shock, $e_{g(i),t}$, 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/job specific term and can be thought of as representing an individual's job/location match 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.

To make the savings decisions computationally feasible, I make two simplifying assumptions in the savings parameterization. First, households face a discrete choice between saving nothing or saving π_s of household income. Second, they only choose to save out of the components of earnings that are stored in memory across time periods. This means that the household does not include the transitory shock $e_{g(i),t}$ in the income that they are saving out of. This means that the state space only needs to additionally store the state of saver vs. non-saver as the amount saved is then a function of observed state characteristics and parameters. A household that chooses to save in period t chooses to save the following:

$$s_{it} = \pi_s\Big(\sum_{g=M,F} \mathbb{1}(k_g = \text{work})exp(\gamma_1^g A_{g(i),t} + \gamma_2^g A^2_{g(i),t} + \gamma_{3g}\mathbb{1}(\text{College})_{g(i)t} + \mu_{jg} + \eta_{g(i)} + \theta_{g(i),j})$$

5.4 Job Offers, Job Destruction, and Preference Shocks

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, meaning that there is no guarantee 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.

5.5 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 over known values:

$$d_T^* \quad = \quad \text{argmax}_{d_T \in \mathbb{J}_T} u(d_T, d_{T-1}, X_T) + \epsilon(d_T)$$

Moving backwards, I then rewrite the expectation in period $T-1$ using distribution assumptions and the period T decision rule as:

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

The household is taking expectations over: 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 ($P(\mathbb{J} = \mathbb{J}_{\mathbb{T}}|\lambda, \rho, \delta, d_{T-1})$); each spouse's realization of the job-match component of earnings ($\sum_G$); each spouse's transient earnings component ($\int_{N(0,\sigma_g^2)}$); and next period's Type I EV location preference shock ($ln\left[\sum_{d_T^*\in\mathbb{J}_T} exp\left(u\right)\right]$).

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 $T-1$ and solve for the optimal d_{T-1}^* . In practice, I estimate 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 to period 1.

5.6 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_0^1, k_0^2), 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 (both spouses' ages and education, home location j_h). I assume all households start with savings of 0 at the beginning of their working life.

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, education, 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 time period is product of the number of start location, number of home locations, types of labor market outcomes for each spouse (employment status and wage-location match), saving status (saver, non-saver), age, education, and individual wage fixed effect type. 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 23,592,960 possible states to solve value functions for each point in the savings grid.

The large size of this state space limits the heterogeneity I can build into the model. For example, I discretize the earnings and savings components to reduce the states of the world that are carried in the model's memory. I omit household characteristics such as the presence of children or individual occupational choices. While fertility is undoubtedly linked to household's decisions of whether each spouse should work, it is not the first order mechanism that this model is written to understand. To the extent to which women are less likely to work due to fertility decisions, this will be captured in their opportunity cost of work parameter and its magnitude relative to the variance of the transitory shock to income. Similarly, while some occupations may be more mobile than others or a better fit to a given labor market, the current model cannot capture that type of heterogeneity and the impacts in men and women's likelihood of sorting into these occupations will load onto the unobserved permanent component and the location match components of income.

Because I start the model at age 25, individuals already having a number of years in which they could have moved to their optimal labor market resulting in selection biasing the location parameters in the wage equation. While migration papers often deal with this by starting the model at age 18 when individuals are still in their place of birth, this would require me to either start individuals as unmarried and model behavior for unmarried and married households, doubling my state space. Rather than do this, I do two things. First, in estimating the wage equation, I adjust for selection into location to estimate the parameters net of selection. Second, in Appendix Section A.5, I re-estimate all model parameters separately for those who start in the home location at age 25 and those who do not to allow for the fact that those who have left home may be different in unobservable ways. Re-estimating the counterfactual exercises in this split model result in substantively similar findings.

6 Structural Model Empirical Strategy

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 observable parts of the earnings equations outside the model using a selection-corrected OLS regression. 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 parameters using indirect inference. Appendix Table A-7 lists the full set model parameters to be estimated and their sources. The following section describes the data sources and estimation methods.

6.1 Step #1: Estimation of Earnings Parameters

I estimate the wage parameters related to observable characteristics outside of the model. While I would ideally estimate these parameters in the model, the number of location fixed effects (96 μ_g) make this computationally infeasible. Because a simple regression of earnings on age, college 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).

I use my structural model to inform the characteristics used to form the cells and categorize people based on the model components which should impact migration but not own earnings other than through location choice: 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, coefficient on college, and μ_{jg} using ACS data, restricted to individuals 25 to 45 who are married in the year of the survey and the year prior to the survey.[14] 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, indicator for having a college degree, 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 $\hat{\gamma}$ as the coefficients on age and $\hat{\mu}$ values as the fixed effects plus the constant.

6.2 Step #2: Calibrated Parameters

To calibrate the UI benefit level, I simulate the average replacement rate at the state-year level using a UI calculator developed in Kuka and data from the 2001, 2004, and 2008 panels of the SIPP. I define benefits for a person of age a, college education e, gender g, and living in state j as:

$$b_{aegj} = 0.5 \times reprate_{1982+a,j} \times exp(\gamma_{1g}a + \gamma_{2g}a^2 + \gamma_{3g}e + \mu_{jg})$$

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 UI (i.e., one-half of a year). 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

[14] 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.

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.

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 (0.163) as the probability that a person is laid off in a given period. 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. Lastly, I set the discount rate to be $\beta = 0.95$.

6.3 Step #3: Utility Parameters

I use indirect inference for estimation of the remaining parameters using 38 moments, including migration rates, employment rates pre- and post-move, savings likelihoods, earnings moments, and a regression coefficient from the reduced form exercise. The full list of moments and their data values are listed in Appendix Table A-8.

I calculate the vector of data moments, m^d, from data from the NLSY97 and the ACS, as well as data moments on savings from existing research. I therefore use the NLSY97 for variables that require panel data: the likelihood of moving, likelihood of living in the home location, likelihood of moving in and out of the home location, and 18 earnings moments including mean, standard deviation, and one-period change over time in earnings for movers, non-movers with a job change, and non-movers without a job change, separately by gender. I use households where the respondent is between the ages 25 to 34, 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 1,936 households who are used to calculate these moments.

I use the ACS for the employment status moments that can be calculated in the cross-section since they do not require observations over multiple years. 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. 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.

To match the proportion living hand-to-mouth, I use a data moment from Aguiar et al. (2024) which calculates the proportion of households with zero wealth (40.6%). To match the average annual savings rate, I use 2010 Bureau of Economic Analysis data on personal income and its disposition to calculate the personal savings rates as a percentage of disposable personal income for the year 2010 (5.9%).[15]

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 B of Table 2.

I then calculate the vector of simulated moments, m^s, for each guess of the parameter vector, ψ^U =[α_0, α_1, α_2, α_3, α_4, α_5, π_s, ℓ_M, ℓ_F, A, λ, ρ] 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 choose a starting point for the algorithm by drawing 1000 draws from a Sobol hypercube. Because simulation error in method of simulated moments impacts the smoothness of the moment function and can 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

[15] I use data moments from other papers in part because the NLSY does not provide annual savings information, but instead only provides accumulated assets measured at age 25, 30, 35, and 40. Because I need to be able to be able to link savings relative to income in a given year, these measures would not be enough to identify the savings grid point.

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.

6.4 Identification

Although all of the model parameters are identified jointly when using indirect inference, I provide here a discussion of which data and auxiliary model moments are most informative about each parameter.

The likelihood of moving by age (10 moments) helps pin down the parameters governing moving costs (α_1, α_2, α_3). The likelihood of moving is decreasing in the fixed cost of moving. By including multiple years of data, I pin down the changes in migration with age; in the data we see that migration is decreasing with age at a decreasing rate which pushes α_2 to be positive and α_3 to be negative. These moments are also linked to the value of consumption α_0; because there are large earnings gains that can be achieved by moving locations, if households put more weight on consumption relative to non-pecuniary preferences for a location, the migration rate will go up. Thus, the migration rate is increasing in α_0. This relationship will be stronger for younger ages because earlier movers have more future years of earnings in the new location, meaning that a smaller change in α_0 will have a larger impact on migration rates at earlier ages.

To separately identify the cost of moving from the preference for living in the home location (α_4), I include the moments for the proportion living in the home location and the flows in and out of the home location. The proportion in the home location is determined both by how much one likes the home location and the costs of leaving that location. The relative likelihood of moving to the home location (i.e., gain the utility from living at home but have to pay the one time moving cost) versus moving from the home location (i.e., lose the home preference utility and pay the moving cost) identifies the magnitude of home preference compared to the moving cost.

To identify the earnings 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 or change jobs, the individual permanent effect is constant across location/jobs and periods, and the transitory shock varies across periods but not locations/jobs, I can use the panel structure of the NLSY, along with location and job transitions, to separate out the variances of each component. For each gender, I observe nine earnings moments that help me identify this distribution: mean, standard deviation, and one-period average change in earnings for those who do not change location and do not change jobs, those who do not change location and do change jobs, and those who move across locations and are employed pre- and post-move. The mean and change over time moments for those who do not change jobs are key for identify the transitory terms whereas these same terms help identify the job match term. The variances help identify the size of the individual permanent effects.

The four data moments about likelihood of working help identify the opportunity cost of work parameters (ℓ_F and ℓ_M) as well as the parameters governing the relative likelihood of receiving an offer locally versus at a distance λ and ρ. The likelihood that a person works is determined by whether they currently have an offer in a location and the value of accepting that offer in terms of consumption relative to the opportunity cost of work. As the opportunity cost increases, the likelihood that each spouse works goes down in the overall population, regardless of migration status. As the local offer rate increases, the likelihood that both genders work increases among non-movers. ρ is a scaling parameter indicating how much more/less likely a person is to get an offer at a distance. The ratio employment rates for non-movers to movers goes up at this scaling parameter goes towards zero.

However, these moments alone are not enough to identify the offer rate. Because we don't observe offers, I cannot separately identify a model with low distant offer rates from a model with high migration costs/value of leisure that cause people to turn down lucrative offers at a distance and primarily move based on the ϵ term. Both will result in low employment post-move. To address this, I include the coefficient from a regression model that regresses migration on the UI for trailing spouses policy along with state and year fixed effects. Because a trailing spouse is only eligible for this policy if they cannot find a job at a distance, spouses that receive an offer at a distance and turn it down do not receive the benefit. Only those who move without having a received an offer are eligible, both in real life and in the model. Thus, how much the policy increases migration rates depends on the likelihood of receiving an offer at a distance; as ρ increases, the likelihood you will be eligible for UI as a trailing spouse declines and the coefficient on the policy declines as well. My empirical setting is thus uniquely suited to be able to separately identify offer rates at a distance from the parameters governing preferences for migration and leisure.

The value of A combined with α_0 are related to the moments about likelihood of working and the moments related to savings. Because utility is log-linear in consumption, the relative likelihood that a household will move by employment status is governed by the weight the household puts on consumption and by the baseline utility households receive if their only source of consumption is non-labor income. As A increases, it increases the differences in the proportion of those who are employed overall relative to those who are employed following a move, with stronger effects for the spouse who is more likely to be out of the labor force due to either lower wage parameters or higher opportunity cost of work parameters. Similarly, lower values of A make it more costly for a household to not save, increasing the average amount saved and the proportion of households who save. Since there is the possibility that both spouses will experience a lay-off in the same period next period, households are incentivized to save more as A decreases because a lower value of A makes the possibility of that bad state in the future more costly.[16]

The average level of savings and the proportion who save are also closely linked to the parameters on preferences for saving α_5 and the percent of income save π_s. As the preferences for savings increases, this drives up the proportion of households who save. As the percent of income saved increases, the average level of savings goes up.

7 Parameter Estimates and Model Fit

Table 6 reports the parameters estimated using indirect inference in the first three columns and wage parameters estimated outside the model in the last three columns.

The wage parameters estimated in the model have two important takeaways. First, the location/job match components are sizeable for both men and women. For comparison, the difference in the largest location fixed effect μ and the smallest is about 0.4, meaning that shifting from the a low type job to a high type job can provide earning gains of a similar magnitude to moving from the lowest paid state to the highest paid state. Second, the variance of women's earnings are greater than that of men, both across locations and in terms of the idiocsyncratic shock. These combined imply that uncertainty about the wage offer available for a spouse without a job-in-hand will inhibit joint migration and that this uncertainty is greater for women.

For all non-pecuniary utility parameters, 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, $\alpha_0 = 2.142$. Because utility for consumption is non-linear, 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 base consumption: $Y = C_0 \left(\exp\left[\frac{X}{2.142}\right] - 1\right)$.

For example, the opportunity cost of work for women of 0.163 implies that they benefit more from not working and consider its value equal to \$5,233 in a household of 25-year olds with average earnings (i.e., $C_0 = 66{,}200$). Conversely, the negative opportunity cost for men is likely actually tapping into the fact that the model does not induce gendered patterns of

[16] How sensitive savings are to non-labor income sources will also depend on how likely it is that both spouses get laid off in the same period and the likelihood that they receive offers. An alternative estimation of this model could calibrate A to be a set value based on data and instead estimate the lay-off rate with the identification argument working similarly.

Table 6: Model Parameter Estimates

Estimated in Model			Earnings Parameters, Est. Outside Model		
	Parameter	S.E.		Parameter	S.E.
Cons. Scaling (α_0)	2.142	0.143	Age Linear, M (γ_{1m})	0.0851	0.002
Move Cost (α_1)	2.149	0.046	Age Quadratic, M(γ_{2m})	-0.0009	0.00002
Move Cost, Age (α_2)	-0.205	0.003	College, M(γ_{3m})	0.493	0.001
Move Cost, Age Sq. (α_3)	0.015	0.030	Age Linear, F (γ_{1f})	0.0744	0.002
Home Preference (α_4)	0.953	0.216	Age Quadratic, F (γ_{2f})	-0.0009	0.00003
Assets, \$1000 (A)	0.008	0.00003	College, F (γ_{3f})	0.549	0.002
Opp. Cost of Work, M (ℓ_M)	-0.102	0.00005			
Opp. Cost of Work, F (ℓ_F)	0.163	0.002			
Long Distance Offer Scaling (ρ)	0.838	0.084			
Offer Rate (λ)	0.629	0.023			
Savings Grid Point (π)	0.296	0.182			
Savings Preference (S)	0.189	2.071			
Loc Match, M (θ_M)	0.123	0.024			
Loc Match, F (θ_F)	0.214	0.001			
Ind FE, M (η_M)	0.526	0.027			
Ind Fe, F (η_F)	0.494	0.028			
SD of Trans. Shock, ε_M	0.123	0.004			
SD of Trans. Shock, ε_F	0.165	0.018			

Note. This table reports the parameter estimates with the first three columns reporting parameters estimated within the model using indirect inference and the last three columns reporting parameters estimated outside the model in a selection corrected regression.

labor supply other than through differences in earnings and the opportunity cost of work 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 2.149 utility units, which in dollar terms for a household with average earnings at age 25 in the sample would be \$114,332. This is significantly smaller than the average moving cost found in a model of individual migration, such as Kennan and Walker (2011) which had average moving costs of \$312,146. This suggests that large moving costs found in individual migration models are possibly driven by the un-modeled dual-earner spatial frictions.

The model matches the targeted data moments well. Figure 4 plots the data moments against the moments for the sample simulated for estimation as well as a line of best fit; the left panel shows the non-earnings moments which are all percentages between 0 and 1 and the right panel shows the earnings moments which are scaled in the 1000s. Notably, in both cases, the line of best fit has a slope close to one and an intercept close to zero.[17] I am able to fit the general pattern of migration rates by age, capturing the decline in migration with age 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

[17] Most moments lie close to this line of best fit. The exception is women's post-move earnings which are underpredicted in the model.

increase in cross-state moves similar to as seen in the motivating empirical exercises. 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 capture the overall gendered pattern of employment and earnings of movers and stayers: in my model, men's employment rate is high regardless of mover status and their earnings increase post-move substantively more than for stayers who change jobs whereas women's employment post-move decline relative to overall employment and the typical earnings change is smaller for movers than stayers who change jobs.

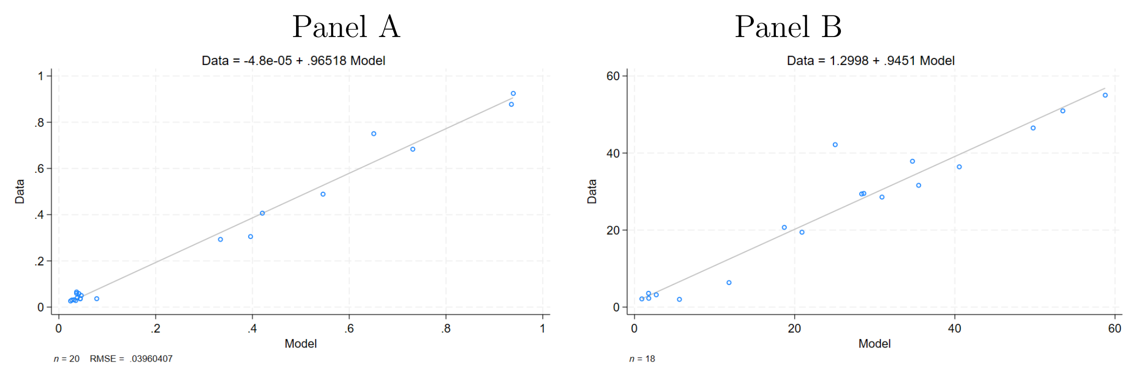

Figure 4: Comparison of Targeted Data Moments and Model Moments
Notes. This figure plots the data moments against the simulated moments from the model. Panel A shows the moments that are less than 1; Panel B shows the earnings moment. I report the line of best fit, calculated by regressing data moments on model moments.

While I targeted the coefficient in the migration exercise, I did not target the post-move labor market outcome moments. I therefore test model fit by comparing the effect of access to UI for trailing spouses on post-move unemployment insurance use and employment. As a reminder, the empirical exercises above showed that access to unemployment insurance for trailing spouses significantly increased take-up of unemployment insurance for married female movers by around 375-450% and had a non-statistically significant effect on men of about +10%. While mean levels of UI take-up is higher in my model than the data,[18] the percent change in UI take-up for movers with access to UI for trailing spouses show similarly gendered patterns in which UI for trailing spouse increases take-up for female movers more than male movers. For women, the post-move UI take-up rate increases by 510% whereas men's increases by 24%.

[18] Baseline levels of UI take-up in my model is about 5% rather than 1% because all workers who are eligible (i.e., those laid off) receive UI. In reality, many people who are eligible for UI do not receive UI. Previous versions of the model incorporated a distaste parameter for UI and matched UI take-up rates; this version of the model did not result in substantively different predictions for the counterfactuals, so I omit these parameters in the final version.

The model is also able to replicate patterns in post-move employment by gender. In the regression analysis, I found that women experience reduced employment (10 pp) directly following a move during the period in which they are covered by UI , but they have similar employment rates one-year later. In contrast, men's employment was unaffected by access to UI for trailing spouses. Directly following a move, my model predicts that 69% of treated women will be employed compared to 81% of untreated women, a difference of 12 pp. In contrast similar rates of of treated men (92%) and untreated men (93%) are employed directly following a move. One-year post-move, the gap in employment for married female movers shrinks to 4.5 pp and it is almost completely eliminated by two years post-move when the gap is only 0.2 pp.

8 Counterfactuals

In the first set of counterfactuals, I test 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 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. Column 1 reports the baseline migration rates.[19] In the baseline outcomes, men are on-average almost always employed post-move whereas only 60% of women are. Men gain on average $13,350 with a move whereas women earn $7,010.

Table 7: Equalizing Labor Market Opportunities Across Genders

	Model	CF1: Equal Earnings	CF2: Single Earner	CF3: Simultaneous Offers
Annual % Move	2.55	2.18	5.82	3.58
% Ever Move	33.97	30.79	75.94	53.26
% Emp. Post-Move, Men	94.01	88.16	99.08	96.23
% Emp. Post-Move, Women	58.24	75.97	-	63.61
Δ Earnings at Move, Men	13.35	11.74	5.76	11.40
Δ Earnings at Move, Women	7.01	13.60	-	9.77

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 men's distribution, column 3 makes all women stay-at-home spouses, and column 4 implements simultaneous job offer draws. Row 1 reports the annual migration rate; row 2 reports the proportion of households who ever move between ages 25 and 40. Row 3 and 4 report percent employed in the year following a move. Row 5 and 6 show the average change in earnings ($1000) between one year prior to the move and one year post move. All percentages are in range 0-100.

First, I evaluate how gender differences in earnings contribute to the tendency of women

[19] Annual migration rates are averaged across all ages 25 to 40 to mirror the sample used in the reduced form analysis.

to be trailing spouses. In counterfactual 1, I equalize men's and women's earnings by setting women's earnings parameters equal to men's (CF1). This counterfactual tests 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. These counterfactuals which equalize earnings result in a 16.9% lower annual migration rate, consistent with the prediction from Mincer (1978)'s model of migration in which households with more equal earnings face greater spatial search frictions.

In CF2, I remove all spatial frictions related to dual earner households by shutting down women's employment, meaning that households make choices only about the husband's labor supply and the household location choice. 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 2.6% of households moving annually to 5.8%. When all women do not work, men's post-move earnings gain decreases to $5,760 from $16,690. This is consistent with positive selection into migration in the baseline: those who move in the baseline are only those with particularly large gains which offset the losses faced by their wives. These moves do, however, increase earnings more than staying in place as the average earnings gain for a man who doesn't move in baseline is around $2,330. This suggests that the joint spatial frictions prevent moves that would be individually beneficial for men.

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? One can interpret the difference between the baseline and CF2 as the effects of two mechanisms: mismatched time of offers (i.e., having to wait a period for your spouse to have an offer in the new location) and mismatched preferred location (i.e., a location with a good offer for spouse 1 is a bad offer for spouse 2). In CF3, 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.[20] This removes the friction associated with mis-matched timing while maintaining the possibility that the location one spouse receives an offer in has a sub-optimal offer for the other spouse (i.e., a low individual-location draw in wages, θ).

[20] 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.

I find that 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 1.0 p.p. This increase is equivalent to 31% of the increase in migration that occurs in CF2, suggesting that the timing friction accounts for about one-third of the additional migration frictions face by dual-earner couples relative to single-earner households. There is also a large impact in the final hypothetical on labor market outcomes. The proportion of women employed post-move increases by 5.4 p.p or 9.2%. Women's earning gains grow to \$9,770 and men's earnings gains decline to \$11,400. The average household income grows by \$21,165 which is about \$1,000 more than moving households gain in the baseline and significantly higher than stayers households' earnings gains. Similar to counterfactual 2, 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.

Taken together, these scenarios suggest three things. First, the declines in migration in counterfactual 1 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 men to be tied stayers who miss out on potential earnings gains. 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 and demonstrates that a key friction in dual-earner migration is one of timing of offers. 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.

In the second 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) Trailing Spouse Subsidy: eligible if one spouse works and one spouse doesn't post-move; (2) Distant Job Search Subsidy: eligible if unemployed pre-move and employed post-move; (3) Unconditional Subsidy: 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 in terms of encouraging the household to move with only one job-in-hand, but 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. Lastly, the final subsidy counterfactual examines the effects of a policy that de-links the benefit from any employment requirements. In all cases, I compare to a world in which UI for trailing spouses has been turned off in all states. Table 8 reports the effects of these policies on migration rates for those 25-40, post-move earnings for those 25-40, and total income and utility summed over ages 25 to 65.

Table 8: Evaluating Migration Subsidy Designs

	Baseline	CF1: Trailing Spouse	CF2: Distant Search	CF3: Unconditional
% Move	2.49	2.99	2.75	3.80
% Ever Move	33.47	37.82	36.28	45.66
Earnings 1-yr Post-move, Men	51.08	50.97	48.82	43.53
Earnings 1-yr Post-move, Women	27.23	26.23	27.68	25.92
% Change in Total Income, Men	-	-0.11	-0.01	5.60
% Change in Total Income, Women	-	0.44	-0.15	-4.73
% Change in Total Utility	-	0.26	0.001	1.09
% Change in Total Utility, Excl. 0s	-	0.95	0.004	1.44

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. Row 1 reports the average annual migration rate from 25 to 40. Row 2 reports the proportion of households who ever move between ages 25 and 40. Row 3 and 4 report the level of earnings in \$1000 for male and female movers respectively 1 year following the move. Row 5 and 6 report the average percent change in total income from age 25 to 65 for men and women relative to the baseline model. Row 7 reports the average percent change in total household utility from 25 to 65 relative to the baseline model. Row 8 reports the average percent change in total household utility relative to the baseline model, excluding those whose behavior was unchanged. All percentages are in range 0-100.

All three subsidy policies have positive effects on the migration rate (row 1 of Table 8). The policy with the largest effect is the unconditional subsidy (1.3 p.p., or 52% 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 (0.5 p.p. or 20% increase).

Next, I evaluate the effects of these subsidies on earnings outcomes for movers. Row 3 and 4 of Table 8 reports the earnings level one-year post-move. In almost all cases, both men and women's earnings post-move are smaller in the presence of the subsidy policies relative to the baseline. The reductions in post-move earnings are strongest in the unconditional subsidy setting: men earn \$7,500 less post-move in this counterfactual relative to in the baseline and women earn about \$1300 less. While these movers still experience larger average earnings gain relative to the year pre-move than the average male stayer, it suggests that the men who are induced to move by the policy would be negatively selected on earnings gains relative to those who move in the absence of a subsidy. This is consistent with the earnings gain for the primary earner not being large enough without the subsidy to induce a move (i.e., marginal mover is negatively selected). The trailing spouse linked subsidy has almost no impact on men's earnings and is associated with about -\$1,000 less in earnings for women directly following a move. This is consistent with the effects we saw in the reduced form exercise: women experience lower earnings directly post-move as they are the spouse induced to leave the labor market following a move. In contrast, the distant search policy which incentivizes moving with a job rather than without a job results in a small increase in average earnings directly following a move for women (\$450). This policy reduces men's average earnings post-move by around \$2,200. This suggests the subsidy allows couples to accept distant jobs that otherwise would not be worth paying the moving cost.

While earnings directly post-move are lower for women in the trailing spouse subsidy, there are stronger overall income gains for women under this subsidy than the other two subsidies, more consistent with the long-term earnings gains seen in the reduced form. Total income and utility are calculated as the sum of all period's income/utility from age 25 to 65. Row 5 to 7 of Table 8 reports the percent change in income and utility. Row 8 reports the percent change in utility for those whose behavior deviated from baseline.[21] The only subsidy which increases women's total income is the trailing spouse policy which increases women's total income by 0.44 pp or around \$5,000 on average. If we exclude households whose behavior and utility were not impacted by the policy, total income increases by around \$24,261 on average for women who move and receive the subsidy at least once in their life.[22] In contrast, the distance search policy decreases women's total income by 0.2 pp and 4.7 pp respectively. This demonstrates the value of testing the long-term effects of policies which

[21] Because all shocks are identical across models, many individuals did not make decisions originally that triggered subsidy receipt (e.g., never movers) and did not change their decisions in response to the policies, resulting in zero change in utility. 80.6% of households in the trailing spouse subsidy, 89.3% in the distant search subsidy, and 62.5% in the unconditional subsidy were unaffected.

[22] These results are likely conservative estimates as I am assuming for all counterfactuals that agent's search efforts are unchanged by the policies, meaning that arrival rates of jobs at a distance do not respond to the subsidy. We would expect that these policies should increase the likelihood that both spouses put more effort into search at a distance and change the search effort of trailing spouses in the period directly post-move, increasing the likelihood that a 'good' job match arrives. In particular, I noted that the large earnings gains in the regression analysis is partially due to women being able to target their search effort to higher quality jobs.

tie benefits to employment post-move: while the one-year post-move earnings results would suggest a policy which incentivizes distant search reduces gender inequality more than a subsidy for trailing spouses, it misses the long run benefits derived from the trailing subsidy policy allowing household to sort to more productive locations for both spouses and search upon reaching the location.

The unconditional subsidy counterfactual demonstrates that different policy designs have gender equity implications even when overall utility improves. Both the trailing spouse subsidy policy and the unconditional subsidy increase lifetime household utility. The unconditional subsidy increases utility by more than the trailing spouse subsidy, in part because a greater proportion of the population receives the subsidy. Conditional on the policy impacting behavior at all, the unconditional subsidy increases lifetime utility by 1.4% compared to the trailing spouse subsidy which increases utility by about 1%. However, the unconditional subsidy reduces married women's lifetime income by 4.7% and increases married men's lifetime income by 5.6%. Unlike the trailing spouse subsidy which decreases the role that migration might play in gender gaps in earnings, an unconditional subsidy would exacerbate the gap.

The results of these counterfactuals highlight the importance of understanding how linking migration subsidies to employment differentially impact men and women. While these analyses do not necessarily imply that governments should subsidize migration, it is nonetheless the case that governments implement policies such as these with the goal of reducing geographic frictions and allowing people to potentially move to more productive labor markets. While policies such as CF2 that encourage those out-of-work to increase their search radius may improve job outcomes for a small portion of job seekers, there is trade-off in terms of their spouses then being uprooted from their jobs. In contrast, subsidies which ease the transition for households in which one spouse is uprooted, such as CF1, results in short-term larger income losses for the trailing spouse, but long-term income gains. As women are more likely to be the secondary earner and are more likely to end up a trailing spouse, considering the impacts of program design of mobility policy is particularly key for married women.

9 Conclusions

This study examines how dual-earner households make decisions about where to work and live. I evaluate the impacts of a specific component of the UI 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 20-58 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 UI following a move, with effects concentrated on take up rates for married women. 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 UI 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 policies linked to subsidizing households with trailing spouses result in the highest lifetime income gains for women.

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.

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10 Online-Only 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 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. Panel A of Table A-3 shows the results of this regression.

As expected, there is no positive impact of part-time worker eligibility on a person's likelihood of moving across commuting zones for either married individuals or single individuals.[23] 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. The UI for trailing spouse parameters are of similar magnitude to that in the primary specification, though the triple difference design coefficient is more noisily estimated.

A.2.2 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. Panel B or Table A-3 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 households.

A.2.3 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-

[23] 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.

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. In 2019, there were 58 million married households in the United States. Of those, 1.4% or 812,000 moved across commuting zones. An increase of 3,713 movers would increase the migration rates by 0.4 pp or 28.5%. This is consistent in levels to the results seen in the ACS sample and in percent terms to the results seen in the NLSY sample.

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). 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-6 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.

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. The following econometric model illustrates this identification problem:

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

A household's earnings in the coming period are a function of whether a household moves this period $\big(\text{M(D)}\big)$, 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, irrespective of realized treatment status, 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. However, I cannot observe the same household in both states of the world and therefore cannot identify always movers/stayers.

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. 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:

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

In this exercise, I demonstrate that $\mathbb{E}[\gamma]$ can be bounded 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:

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

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

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

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.

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 percentiles, which assumes that the marginal movers all have the highest post-move outcomes. Similarly, I can calculate an upper bound for a sample trimmed to the top percentiles, assuming that the marginal movers all have the lowest post-move outcomes. I then calculate a 95% confidence interval for the upper bound and the lower bound by bootstrapping as proposed in Lee (2009).

A.5 Model Robustness to Starting State

When modeling dynamic decisions, it is important to consider whether the starting state of the world in the model is exogenous. Typically, models of migration start at age 18 under the assumption that the location a person is in as a child was out of their own control and thus this location is an exogenous starting location. In contrast, the current model starts at 25. This is a necessity of my setting: because agents in my model are already married couples and the typical American is not yet married at 18, starting the model earlier would require the addition of a marriage decision to the model. Given the computational complexity of modelling a joint location decision over the 48 mainland US states, I choose this starting age to A) eliminate the need to model marriage and B) eliminate the need to model decisions about college.

This, however, leads to potential bias in the estimation of wages. Those who choose to leave their home location between 18 and 25 may have chosen locations based on the kinds of selection forces described in this paper, meaning that they are choosing locations that are more productive for them in both observable and unobservable ways. If this is the case, my estimates of the parameters governing the wages will be biased by this selection, even accounting for selection using the Dahl (2002) selection correction method. To address these concerns, I re-estimate the model on two separate samples: those who are in their home-location at age 25 and those who are not. I allow all parameters to vary by type and match type-specific moments.

While not all moments in the original estimation have a comparable analog for 'home at 25' and 'not home at 25' types, I choose moments that approximate those used in the primary specification. For the migration moments and wage moments taken from the NLSY97, I can observe location at age 25 and calculate the same moments for the sub-samples in home and non-home locations. For the moments taken from the ACS, I can only see current location, not location at age 25. I thus estimate the employment for movers and non-movers on a sample of those between the ages of 25-27, based on location during that age range. For the wage parameter estimation down outside the model, I use the same methodology, but restrict the ACS regression for home-types to those living the home location at time of survey and vice-versa for non-home types. Finally, for the savings moments and the regression coefficient, I use the same moments as in the primary estimation method as I cannot separately identify these moments for the two groups. Appendix Table A-8 reports the moments used to estimate the model. Those who are in the home location at age 25 have lower lifetime migration rates overall and have a wage distribution that is shifted downwards in mean levels.[24]

I then use the two sets of parameters to run the same counterfactuals as in the primary specification. Table A-9 reports the results of the mechanism counterfactuals and Table A-10 report the results of these exercises. Each table reports results separately by type as well as for a joint sample which combines the two samples into one for better comparison to the main sub-sample.

A couple clear patterns emerge from this exercise. First, those who were not in their home location at age 25 are substantially more mobile than those that start in the home location. As a result, in the mechanisms analysis, the counterfactuals which reduce frictions to migration (i.e., single earner household and simultaneous offers) have smaller impacts

[24]The full set of parameter estimates as well as model fit by group are omitted for space, but are available upon request.

in percent terms for those who were already far from home than those who are far from home. However, in the combined sample, the aggregate effects are very similar: shutting down one spouse's earnings increases migration rates by 2.6 pp (compared to 3.2 pp in the baseline) and simulating simultaneous offers increases migration rates by 0.8 pp (compared to 1.0 in the baseline model), meaning that timing frictions are about one-third of the total dual-earner penalty to migration in both versions. Second, the impacts of the policy counterfactuals on migration are broadly consistent with the baseline model, though the magnitude of the effects are smaller. As in the baseline model, the trailing spouse subsidy is associated with the largest lifetime income gains for women (0.7% in the joint sample, 1.4% for those who start outside the home location). In contrast to the baseline model, there are smaller impacts on men's lifetime income of all subsidies and we see less negative impacts of the unconditional subsidy on women, suggesting that this version of the model exhibits less selection on earnings when subsidized to move. Nonetheless, the results are consistent with the conclusion that migration subsidies that are attached to trailing spouse subsidies are particularly beneficial for women.

A.6 Appendix Figures and Tables

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

	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 end of sample, 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: Robustness Checks: Part-Time UI and Within CZ Moves

	(1) DD	(2) DDD
Panel A: Part-Time UI		
Part-Time UI	0.0004	
	(0.0066)	
Trailing Spouse UI	-0.0087	
	(0.0115)	
Married X Part-Time UI	-0.0228^{+}	-0.0236*
	(0.0127)	(0.0116)
Married X Trailing Spouse UI	0.0454*	0.0287
	(0.0196)	(0.0199)
Panel B: Within Commuting Zone Move		
Treat	0.0013	
	(0.0088)	
Married X Treat	-0.0011	-0.0007
	(0.0152))	(0.0183)

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

Notes. This table reports robustness checks to our primary specification. Panel A reports the the coefficient of a regression of moving more than 100 miles on an indicator for whether the state had UI eligibility for part-time workers, interacted with marital status, along with the primary policy. Panel B 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 includes state and year fixed effects, controls for state characteristics including state-year unemployment rates and per capita income, and individual fixed effectss. Column 2 includes state X year FE. Standard errors are clustered at the state level.

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: Impact of UI for Trailing Spouses and Move on Annual Earnings

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Inc, t-3	Inc, t-2	Inc, t-1	Inc, t	Inc, t+1	Inc, t+2	Inc, t+3
Panel A. Married Men	Treat	3515.2+	19.48	-2333.5	1446.2	1226.6	1593.0	-41.71
		(2041.3)	(1898.3)	(2204.7)	(1292.6)	(1806.7)	(2800.1)	(3145.7)
	Move	3660.3	-2024.6	-3135.0	1967.3	1223.3	-903.9	4946.2
		(5734.3)	(2012.9)	(2319.4)	(1850.6)	(3014.6)	(2384.9)	(5147.4)
	Treat X Move	-7145.8	-4410.9	-1643.8	-5650.3*	-7346.2+	79.55	-2073.1
		(6692.7)	(3567.1)	(3921.9)	(2670.8)	(4387.3)	(4942.2)	(6796.1)
	Observations	3512	4423	5966	5375	4049	3953	2899
Panel B. Married Women	Treat	3739.7**	2397.8+	1011.4	-663.7	-141.0	2056.4	1957.8
		(1426.3)	(1248.2)	(1076.2)	(763.3)	(1417.3)	(2029.7)	(2083.5)
	Move	1010.0	2254.9	67.83	-551.4	-4028.1+	529.0	-6832.7*
		(3180.7)	(2671.2)	(1984.2)	(1538.7)	(2367.1)	(2942.9)	(3057.2)
	Treat X Move	-521.4	-2202.2	-12.59	-3407.5	-215.1	-1092.7	10795.2*
		(4040.5)	(3449.0)	(2816.6)	(2174.9)	(3631.6)	(4604.4)	(4710.0)
	Observations	3244	4176	5966	4958	3591	3473	2501

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

Notes. This table reports the coefficient on a regression of men's (panel A) and women's (panel B) lagged annual earnings on indicators for moving interacted with living in a state with UI for trailing spouses, as well as controls for age, race, education, children, earnings prior to the move, and state and year FE. Sample restricted to married couple. Standard errors are clustered at the household level. All regressions are propensity score weighted; see appendix section A.3 for description of weight.

Table A-6: Pre- and Post- Propensity Score Match Demographics

		Treated, Mover	Treated, Non-Mover	Untreated, Mover	Untreated, Non-Mover
Non-Matched	Age	27.27	29.46	26.26	28.87
		(3.015)	(3.674)	(2.858)	(3.950)
	% Black	0.154	0.190	0.257	0.314
		(0.361)	(0.392)	(0.437)	(0.464)
	% BA or more	0.152	0.0891	0.149	0.0821
		(0.359)	(0.285)	(0.356)	(0.275)
	% w/ kids	0.257	0.262	0.254	0.287
		(0.437)	(0.440)	(0.435)	(0.452)
	% Emp. 3 mos prior	0.689	0.653	0.686	0.672
		(0.451)	(0.470)	(0.451)	(0.463)
	% Log Wages 3 mos prior	2.590	2.758	2.458	2.636
		(0.716)	(0.659)	(0.700)	(0.664)
	% on UI 3 mos prior	0.0477	0.0275	0.0261	0.0185
		(0.213)	(0.163)	(0.159)	(0.135)
Matched	Age	27.33	27.28	27.46	27.21
		(0.0714)	(0.00522)	(0.0590)	(0.00404)
	% Black	0.139	0.134	0.144	0.137
		(0.00831)	(0.000631)	(0.00570)	(0.000401)
	% BA or more	0.162	0.157	0.146	0.141
		(0.00884)	(0.000863)	(0.00683)	(0.000652)
	% w/ kids	0.242	0.232	0.252	0.226
		(0.0103)	(0.000790)	(0.00902)	(0.000609)
	% Emp. 3 mos prior	0.932	0.945	0.945	0.943
		(0.00531)	(0.000498)	(0.00349)	(0.000415)
	% Log Wages 3 mos prior	2.591	2.534	2.609	2.596
		(0.0172)	(0.00380)	(0.0148)	(0.00110)
	% on UI 3 mos prior	0.0317	0.0164	0.0208	0.0125
		(0.00421)	(0.000257)	(0.00293)	(0.000190)
	Month-HH Observations	1733	354467	3165	515231

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 and restricts to those employed 2 months prior to focal year (Panel B: Matched).

Table A-7: 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
γ_3^M, γ_3^M	College Wage Coef.	ACS data, Selection-corrected OLS
θ_M, θ_F	Earnings Residual, Job-location Match	Indirect Inference
η_M, η_F	Earnings Residual, Individual FE	Indirect Inference
e_M, e_F	Earnings Residual, Transitory	Indirect Inference
p_j	Location Price Index	ACCRA Cost of Living Index (Q1 2019)
b_{jgt}	UI benefit level	Kuka (2020) and UI for trailing spouses data
δ	Annual layoff rate	JOLTS 2005-2018
α_0	Consumption Scaling	Indirect Inference
α_1, α_2, α_3	Moving Cost	Indirect Inference
α_4	Home Preference	Indirect Inference
α_5	Saving Preference	Indirect Inference
ℓ_M, ℓ_F	Leisure Value	Indirect Inference
λ	Local Offer Rate	Indirect Inference
ρ	Scaling for Distant Offer	Indirect Inference
A	Non-labor Income	Indirect Inference
π_s	Savings Grid Point	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.

Table A-8: Moments Used for Estimation

Moment	Source	Full Sample	Home Sample	Non-Home Sample
Migration Rate, Age 25	NLSY97	0.066	0.030	0.192
Migration Rate, Age 26	NLSY97	0.060	0.019	0.120
Migration Rate, Age 27	NLSY97	0.051	0.027	0.074
Migration Rate, Age 28	NLSY97	0.038	0.017	0.054
Migration Rate, Age 29	NLSY97	0.037	0.016	0.051
Migration Rate, Age 30	NLSY97	0.040	0.024	0.051
Migration Rate, Age 31	NLSY97	0.029	0.025	0.032
Migration Rate, Age 32	NLSY97	0.032	0.020	0.040
Migration Rate, Age 33	NLSY97	0.031	0.027	0.034
Migration Rate, Age 34	NLSY97	0.027	0.008	0.038
% Employed, Men	ACS	0.925	0.919	0.907
% Employed, Women	ACS	0.684	0.723	0.617
% Employed Post-Move, Men	ACS	0.878	0.810	0.888
% Employed Post-Move, Women	ACS	0.489	0.400	0.559
% in Home Location	NLSY97	0.751	0.950	0.593
% of Moves to Home Location	NLSY97	0.293	0.329	0.293
% of Moves from Home Location	NLSY97	0.306	0.562	0.233
Coefficient on UI for Trailing Spouses	Regression	0.037	0.037	0.037
% Hand-to-Mouth	Aguiar, Bils, and Boar (2023)	0.407	0.407	0.407
% of Income Saved	BEA	0.059	0.059	0.059
Mean Earnings, M, Job Stayers	NLSY97	46.542	44.354	47.467
Mean Earnings, W, Job Stayers	NLSY97	31.651	27.925	34.621
Mean Earnings, M, Job Changers	NLSY97	50.980	47.767	53.582
Mean Earnings, W, Job Changers	NLSY97	36.455	31.937	40.130
Mean Earnings, M, Movers	NLSY97	55.057	52.439	53.796
Mean Earnings, W, Movers	NLSY97	42.212	49.438	38.847
Earnings Change, M, Job Stayers	NLSY97	3.227	3.068	4.400
Earnings Change, W, Job Stayers	NLSY97	3.616	3.038	4.380
Earnings Change, M, Job Changers	NLSY97	2.324	2.324	2.452
Earnings Change, W, Job Changers	NLSY97	2.168	2.363	2.313
Earnings Change, M, Movers	NLSY97	6.398	5.174	6.165
Earnings Change, W, Movers	NLSY97	2.042	1.613	2.677
Earnings SD, M, Job Stayers	NLSY97	29.550	29.119	29.506
Earnings SD, W, Job Stayers	NLSY97	20.733	17.033	22.353
Earnings SD, M, Job Changers	NLSY97	28.598	25.787	30.441
Earnings SD, W, Job Changers	NLSY97	19.473	16.487	21.409
Earnings SD, M, Movers	NLSY97	37.903	33.948	38.016
Earnings SD, W, Movers	NLSY97	29.414	41.299	22.567

Notes. This table reports the data moments used to calibrate the utility parameters separately for the full sample, for those starting at home at age 25 and for those not at home at age 25, sourced from NLSY97, ACS, and other sources.

Table A-9: Equalizing Labor Market Opportunities Across Genders, Two-Sample Model

	Model	CF1: Equal Earnings	CF2: Single Earner	CF3: Simultaneous Offers
Panel A: In Home Location at 25				
Annual % Move	1.38	0.79	4.01	2.11
% Ever Move	20.50	12.72	54.53	32.98
% Emp. Post-Move, Men	94.63	93.17	96.91	95.11
% Emp. Post-Move, Women	57.38	73.63	-	66.14
Δ Earnings at Move, Men	34.34	33.88	25.17	34.38
Δ Earnings at Move, Women	6.99	23.24	-	18.02
Panel B: Not In Home Location at 25				
Annual % Move	3.03	2.30	5.56	4.00
% Ever Move	41.84	33.79	69.62	56.80
% Emp. Post-Move, Men	95.66	95.28	98.06	96.22
% Emp. Post-Move, Women	38.05	55.24	-	45.34
Δ Earnings at Move, Men	17.65	18.63	13.39	13.71
Δ Earnings at Move, Women	8.07	20.05	-	20.80
Panel C: Combined Sample				
Annual % Move	2.21	1.55	4.78	3.05
% Ever Move	31.17	23.25	62.08	44.89
% Emp. Post-Move, Men	95.34	94.74	97.58	95.84
% Emp. Post-Move, Women	44.11	59.92	-	52.52
Δ Earnings at Move, Men	21.68	21.85	18.36	19.65
Δ Earnings at Move, Women	6.48	18.12	-	17.42

Note. This table reports the results of the first set of counterfactuals, separately for the ‘home’ sample (Panel A), the ‘not home’ sample (Panel B), and the joint sample which uses both group’s parameters (Panel C). Column 1 shows the baseline model results, column 2 sets both earnings distributions equal to men’s distribution, column 3 makes all women stay-at-home spouses, and column 4 implements simultaneous job offer draws. Row 1 reports the annual migration rate; row 2 reports the proportion of households who ever move between ages 25 and 40. Row 3 and 4 report percent employed in the year following a move. Row 5 and 6 show the average change in earnings ($1000) between one year prior to the move and one year post move. All percentages are in range 0-100.

Table A-10: Migration Subsidies: Effects on Migration, Two-Sample Model

	Baseline	CF1: Trailing Spouse	CF2: Distant Search	CF3: Unconditional
Panel A: In Home Location at 25				
% Move	1.36	1.47	1.42	1.55
% Ever Move	20.53	21.71	21.36	22.97
Earnings 1-yr Post-move, Men	73.16	73.15	72.32	71.44
Earnings 1-yr Post-move, Women	25.91	25.07	25.13	24.84
% Change in Lifetime Income, Men	-	0.04	-0.03	0.09
% Change in Lifetime Income, Women	-	0.07	-0.05	0.04
% Change in Lifetime Utility	-	0.01	0.02	0.05
Panel B: Not In Home Location at 25				
% Move	2.99	3.20	3.06	3.32
% Ever Move	41.38	43.46	42.30	44.88
Earnings 1-yr Post-move, Men	61.21	60.98	60.58	58.68
Earnings 1-yr Post-move, Women	28.34	27.51	28.02	26.10
% Change in Lifetime Income, Men	-	-0.07	-0.02	-0.03
% Change in Lifetime Income, Women	-	1.41	-0.03	0.53
% Change in Lifetime Utility	-	0.17	0.12	0.44
Panel C: Combined Sample				
% Move	2.17	2.34	2.24	2.44
% Ever Move	30.95	32.59	31.83	33.92
Earnings 1-yr Post-move, Men	62.95	62.82	62.41	60.96
Earnings 1-yr Post-move, Women	25.75	24.98	25.32	23.96
% Change in Lifetime Income, Men	-	0.04	-0.02	0.04
% Change in Lifetime Income, Women	-	0.65	-0.05	0.19
% Change in Lifetime Utility	-	0.09	0.07	0.25

Notes. This table reports the results of the second set of counterfactuals, separately for the 'home' sample (Panel A), the 'not home' sample (Panel B), and the joint sample which uses both group's parameters (Panel C). 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. Row 1 reports the average annual migration rate from 25 to 40. Row 2 reports the proportion of households who ever move between ages 25 and 40. Row 3 and 4 report the level of earnings in $1000 for male and female movers respectively 2 years following the move. Row 5 and 6 report the average percent change in lifetime income for men and women relative to the baseline model. Row 7 reports the average percent change in lifetime household utility relative to the baseline model. All percentages are in range 0-100.