To Grandmother's House We Go: Informal Childcare and Female Labor Mobility*

Childcare and Female Labor Mobility

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Abstract

We document how childcare costs and the location of extended family influence the labor supply and mobility of U.S. women. Women return to their home locations immediately before fertility events, suggesting that informal childcare needs may motivate home migration. Women who live near their parents have lower child earnings penalties. We then build a model of labor supply and migration to assess the impacts of childcare subsidies. Childcare subsidies increase earnings and mobility among U.S. women and ignoring migration can understate the welfare benefits of these policies, especially for college-educated women and those whose parents' locations have poor amenities.

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1 Introduction

How does childcare availability influence the labor force attachment and migration behavior of women in the United States? The cost of childcare is often a financial hardship for families: recent surveys indicate that the average cost of center-based infant care exceeds 27 percent of median income for single parents (Child Care Aware, 2017). An alternative to high-cost private care is to use relative-based care, but this option can only be used if relatives are nearby. Thus, childcare needs may constrain both the labor force *participation* and the geographic labor *mobility* of women.

The goal of this paper is to study how migration choices are constrained by childcare needs and the implications of these location constraints for women's earnings. We first show that women are 33% more likely to move back to their birth state directly prior to their first birth and that women with children exhibit considerably stronger labor force attachment when living in their home state. We then use panel data from the Panel Study of Income Dynamics to analyze how the child earnings penalty varies based on proximity to the child's grandparents and local childcare costs. We show that women who give birth less than 25 miles from their own parents experience a substantially smaller child earnings penalty than women who have a child elsewhere. Additionally, a one-standard deviation increase in the average weekly price of childcare at the county-level increases the child penalty by $2,329 or 11 pp.

These descriptive facts motivate the construction of a structural model to test how additional childcare subsidies would alter women's migration and working decisions. Typically, analyses of the impacts of childcare subsidies focus on the direct impacts of such subsidies on labor force participation and human capital accumulation as the primary mechanism through which such subsides impact women's earnings.[2] However, if the high costs of non-subsidized

[2]For an overview of the literature on the elasticity of women's labor supply to childcare costs, see

childcare prevent households from moving far from their parents and thus from optimally sorting across labor markets, we might expect that there is a secondary effect on earnings and welfare stemming from reduced frictions in labor mobility.

We therefore explore these mechanisms in a model that nests a canonical model of dynamic labor force participation in a model of dynamic migration. Each period, women choose their fertility status, labor force attachment, and where to live. Mothers must balance the trade-off between building experience through labor force participation and paying more in childcare costs, though if they live in their parent's location, they receive a portion of free childcare. The model makes progress in unpacking the "black box" of preferences for one's home location, estimating that informal childcare can account for nearly one quarter of the home location preference for women with children. We additionally estimate considerable racial heterogeneity in informal care usage and find that informal childcare being tied to specific locations reduces its value to agents by approximately 20%. A counterfactual policy that fully subsidizes childcare increases women's lifetime wages by 6.5% overall and additionally increases labor mobility. Finally, we compare this policy to one that offers a "local" subsidy by fully subsidizing childcare *only* in the grandparent location, thus offering free care while not addressing (or exacerbating) the geographic constraints induced by childcare needs. The latter policy does considerably less to improve wages or lifetime utility, especially for women with high socioeconomic status and those with grandparents in locations with poor amenities.

Our paper expands upon and ties together three areas of research: the literature on childcare costs and women's labor supply, the literature on the determinants of migration, and the literature on the implications of family-based ties for labor market outcomes. First, our paper introduces a new mechanism that contributes to the 'child penalty' faced by mothers: increased job mobility frictions caused by location-specific childcare access. Past

(Del Boca, 2015).

research shows that women experience large earnings drops following the birth of a child (Kleven et al., 2019b, Cortés and Pan, 2023, Goldin and Mitchell, 2017, England et al., 2016, Gangl and Ziefle, 2009, Budig and England, 2001). One possible explanation for the dip in earnings post-birth is that women reduce work hours due to high costs of childcare. Analyses of free or subsidized childcare in Canada (Baker et al., 2008, Lefebvre and Merrigan, 2008), Europe (Bauernschuster and Schlotter, 2015, Bettendorf et al., 2015, Lundin et al., 2008), and the United States (Cascio, 2009, Tekin, 2007, Bainbridge et al., 2003, Blau and Tekin, 2007, Borowsky et al., 2022, Landivar et al., 2022) indicate that such programs increases the likelihood that women work, while also crowding out their use of informal care.[3] We argue, however, that this substitution away from informal care is also a mechanism through which these subsidies may improve women's labor market prospects. By allowing women to no longer rely on relative care, they are able to be more mobile and potentially achieve welfare gains by moving to a more productive labor market than their parents live in or to a better job match.

Our analysis of child penalties is most similar to Kleven et al. (2024) and Karademir et al. (2026), which study how child penalties vary with childcare costs in Austria and Canada respectively. Kleven et al. (2024) use variation over time and geography in nursery and kindergarten childcare to show that sudden, unanticipated increases in the number of available childcare seats have no significant impact on the child penalty in the years following a birth. Karademir et al. (2026) explore childcare in the context of grandparent proximity, finding that women are more likely to live close to their parents following their first birth and that the child penalty is 4 p.p. smaller for women living in the same Canadian Census Division as their parents. Our paper contributes to this literature by exploring these questions in the context of the United States, which has uniquely high prices of childcare

[3] Recent papers find more limited impacts of childcare subsidies on employment, possibly due to this crowd-out Fitzpatrick (2010); Havnes and Mogstad (2011).

and low levels of government support for parental leave and childcare relative to Canada and European countries. We find much larger impacts of both proximity to grandparents (20 p.p. difference) and accessible childcare on the child penalty (11 p.p. difference) than both of these papers, possibly due to the greater burden of childcare costs in the United States.

Additionally, we build upon a long-standing strand of the women's labor participation literature which considers how childcare would change women's labor force participation decisions throughout the life cycle rather than just in the immediate aftermath of policy implementation as in the prior papers. Our model builds directly on the frameworks of dynamic labor supply in presence of fertility seen in Eckstein and Wolpin (1989), Francesconi (2002), Bick (2016), and Adda et al. (2017). We extend these models by incorporating informal care from family and migration decisions. By incorporating these components, we are able to show that while the majority of the welfare gains women accrue from childcare subsidies are from increased labor force participation, about one-tenth of the gains are attributable to women being able to sort into their preferred labor markets.

Second, our model also contributes to our understanding of the factors influencing return migration and home-biases in location choices. Older work has studied repeated and return migration (Davanzo, 1983; Dierx, 1988) with the view that such moves are driven entirely by monetary influences. Some more recent work (Diamond, 2016; Kennan and Walker, 2011; Bishop, 2008) considers non-monetary factors agents weigh when making repeated moving and location choices, but these papers typically condense preferences for living in one's home location into a single utility premium. A small literature has documented the role of emotional attachment to places' characteristics and the role of concentration of extended family in location decisions (Boyd et al., 2005; Spilimbergo and Ubeda, 2004; Zabek, 2024; Spring et al., 2017). Through focusing on fertility as a new driver of home migration, we aim to further unpack the specific determinants of return migration and add to the literature that studies how individuals balance pecuniary and non-pecuniary factors when making migration decisions in the United States.

Lastly, our paper incorporates migration decisions into the growing literature on the implications of family-based ties for labor market outcomes. Proximity to family can mitigate child or elder care needs, allowing greater attachment to the labor force. Geographic distance from one's mother or mother-in-law is associated with a greater likelihood of childcare transfers, allowing for higher labor force participation for women (Compton and Pollak, 2015, 2014; Chan and Ermisch, 2015). To identify the effects of access to grandparent care, past research has used variation in pension generosity and retirement age (Dimova and Wolff, 2011; Aparicio-Fenoll and Vidal-Fernandez, 2015; Zamarro, 2020; Bratti et al., 2018; Posadas and Vidal-Fernandez, 2013) and the death of grandparents (Arpino et al., 2014; McMurry, 2021) to show that larger grandparent time transfers are associated with higher earnings for mothers. Beyond the realm of childcare, co-location near parents acts as a buffer against earnings losses for adult children following a job displacement (Krolikowski et al., 2020; Kaplan, 2012).

To our knowledge, the only other paper that assesses the interaction of informal childcare and migration choices is García-Morán and Kuehn (2017), who build a model of residence choice, fertility decisions, and female labor force participation in the context of Germany. Our contribution relative to their paper comes from our focus on dynamics: the authors model migration, working, and fertility decisions as one-shot choices. However, labor force participation and migration are dynamic processes: multiple moves and return migration are salient features of the data (Kennan and Walker, 2011). Because we allow for dynamic location choices, our framework will capture the life-cycle implications of childcare policies, and the richness of our model also allows us to estimate heterogeneity in impacts along considerably more demographic and geographic lines than their work.

The paper is organized as follows: Section 2 motivates our research question by providing descriptive evidence regarding the timing of home migration and fertility events observed in

U.S. data. Section 3 details our model, and Section 4 describes our estimation procedure. Section 5 presents model estimates and evaluates the model's fit, while Section 6 presents the results of counterfactual simulations. Finally, Section 7 considers potential avenues for future research before concluding.

2 Motivation

In this section, we present empirical evidence that U.S. women's location choice is responsive to childcare needs and that women's labor market outcomes following their first birth are tied to proximity to their child's grandparents. For these analyses, we use data from the 2006-2019 waves of the American Community Survey (ACS) (Ruggles et al., 2020) and from the 1970-2019 waves of the Panel Study of Income Dynamics (PSID). Each year of the ACS contains information on one-year migration histories for a 1 percent sample of the entire United States' population, providing a cross-sectional data set with a large enough number of movers to look at heterogeneity by race and marital status in migration outcomes. However, we cannot link observations in the ACS to their parents, nor can we follow observations over time. We therefore use the PSID, a survey which follows a nationally representative sample of approximately 18,000 individuals and their descendants over-time, to link parents to the grandparents' locations and explore lifetime migration patterns and earnings changes at the time of migration and fertility events.

We restrict our ACS sample to women aged 22-40 who were born in the United States. We drop individuals who did not complete at least one year of high school education. The women in our sample are limited to those who are coded as household heads, spouses of household heads, or children/children-in-laws of household heads (to allow for the possibility of "boomerang migration," or individuals moving back into their parents' home). In the ACS, we observe the state of birth of all household members, and we define being in the ‘home state’ as living in the state the respondent or their spouse was born in. The ACS additionally records the youngest own child for all respondents, allowing us to distinguish women who have young children from those who do not. We exclude observations whose age and age of youngest child imply a birth before the respondent was age 14.

We restrict our PSID sample to women whose age at first birth was between 22 and 40 and keep women observed between the ages of 18 and 55 who are either the head of household or the spouse of the head and who we can observe own-location and parent’s location in at least one period. For all analyses using the PSID, we can link across generations within the PSID respondent family, meaning that we can define a woman as living near grandparents if she lives in the same state or county as either her or her spouse’s parents, depending on which spouse is part of the PSID respondent family.

We supplement this data with data on childcare prices sourced from the US Department of Labor’s National Database of Childcare Prices (NDCP) which provides county by year level data on center-based and in-home childcare costs for 2008-2018. We measure childcare costs as the median weekly price of toddler center-based care, aggregated to the state-level as the population-weighted mean across counties. For exact average full-time toddler childcare expenses across U.S. census divisions, refer to Appendix Table A.8.

2.1 Fertility and Return Migration among U.S. Women

The high cost of childcare in the United States may compel women with small children to make different location and working decisions than those without. In particular, we may expect women with young children to be more likely to move back to their parent’s location to take advantage of familial support in raising children and for women with children to work more hours if their parents are in their same location than if not.

We first investigate these hypotheses using data from the ACS whether women are more likely to move home in response to fertility events. We restrict our sample to women who were not living in their or their husband's state of birth in the year before the interview and then run the linear probability model:

$$h_{it} = \beta_0 + \boldsymbol{\beta}_1 \boldsymbol{X}_{it} + \beta_3 f_{it} + \tau_t + \varepsilon_{it},$$

where h_{it} indicates whether individual i moved back to their or their husband's birth state in year t.[4] $\boldsymbol{X}_{it}$ contains a vector of demographic controls as well as birth place, state of residence, and previous state of residence fixed effects, τ_t are year fixed effects, and f_{it} indicates individual i's first fertility status in year t, defined by presence of a child belonging to the respondent that is less than 1 year old while also being the only child of the respondent in the household. Standard errors are consistent under heteroskedasticity, and regressions are weighted using sampling weights provided by the ACS. We focus on the first pregnancy because the presence of additional children may make migration more cumbersome — thus, women may be more likely to move home in response to their first fertility event than subsequent ones. We run our specification for all women as well as for non-married (including never married, divorced, separated, and widowed) and married women separately, as having two potential earners in the household may make married women less likely to move in response to fertility than single women. We also run the specification separately by race.

Table 1 reports the results of this exercise. We find that initial fertility events make women 33% more likely to home-migrate. These effects are also much stronger for single women than married women, and we can reject the null that the effects are equal across

[4]We also can test the converse of this: are women less likely to move *away* from home in response to fertility events? Table A.1 reports the results of a regression of an indicator for moving away from their birth state in year t on the same variables for the sample of households living in their birth state in the year before the interview. We find that women are 15.7% less likely to move away from home at time of first birth.

groups in a t-test of equality of coefficients. Initial fertility events make single women roughly 60% more likely to move home compared to the rest of the sample off a base rate of 3.3 percent, whereas married women's likelihood increases by only 19%. We also find that there are significant differences by race, with White and Black mothers being more likely to move back home than those who are neither White nor Black. When we look fertility patterns for different types of women in Appendix Table A.2, we see that, compared to non-movers in the home location and women who stay in non-home locations, women who have moved back home in the previous year (A) have fewer children, (B) have children who are on average 1.2-1.4 years younger, and (C) are 1.6 p.p. more likely to have had their first birth in the last year.

[TABLE 1 ABOUT HERE]

The variation captured in this regression can also be presented visually: in Figure 1, we plot raw means of home migration for women with only one child or no children (N/A in the figure) in the ACS, broken up by the age of their child and marital status. We see a larger spike in home migration for unmarried women with a newborn than for married women, and the increase in migration rates does not extend to older ages, perhaps due to increased costs of migration when children are present as opposed to merely impending.[5] Similar patterns hold when looking at mothers of multiple children and the age of their eldest child, though the decline in migration at older ages is less steep. However, when we look at the likelihood of moving as a function of later births (Appendix Table A.4), there is no effect of later pregnancies on likelihood of a move.

[FIGURE 1 ABOUT HERE]

[5] At older child ages, we see that presence of children is instead correlated with child care prices in the migration destination (see Appendix Table A.3), with parents of young children moving to locations with 1.4% lower child care costs.

Next, we use the PSID data to explore migration trends for mothers over the course of multiple years. Are those who move only moving once, and how much of their parenthood time spent in the state that their parents live in? Here, we split women into 'never-mothers', defined as women who we do not observe having a child, and 'ever-mothers' who have at least one child.[6] For ever-mothers, we then look at migration patterns in six-year time periods surrounding the year of their first birth: pre-birth (-7 to -2 years prior to birth), pregnancy and young childhood (-1 to 4 years around birth), elementary age (5 to 10 year post-birth), teenage (11 to 16 years post-birth), and young adulthood (17 to 22 years post-birth). Table 2 reports the number of moves, the percent of women who are never-movers, one-time movers, and ever-movers, the percent of women who move to the parent's state, and the proportion of years living in the same state as the PSID respondent's parents in all years observed in the data, as well as in each parenthood stage.

[TABLE 2 ABOUT HERE]

Similar to what was shown in the cross-sectional data, ever-mothers are most likely to move during the pregnant/young child stage. 17.1% of women move at least once during this stage, relative to 14.9% of soon-to-be mothers and 6.9-13.7% of mothers of older children. While most movers only move once during each parenthood stage, about 5% move more than once during pregnancy/young child, and 23.7% of ever-mothers move multiple times throughout their entire life. Though mothers are more mobile during the early years of their child's life, we see that moves home disproportionately occur as well. 44.4% of all movers during pregnancy/young childhood are movers to home. A greater proportion of each six-year parenting period is also spent in the same state as their parents: 71% of the pregnant/young child period relative to 69% of pre-pregnancy, and 52-65% of parents of

[6]To insure that we are capturing full lifetime fertility, we restrict our sample for this analysis to women who we observe at both age 25 and at age 50.

older children.

Do women who choose to move home at time of their first pregnancy differ in observables from women who stay in a non-home state location? Table 3 reports the demographic characteristics of women in the ACS who are living in their or their husband's home state in the year prior and post first-birth (col. 1), women who move back to their or their husband's home state (col. 2), and women who are not living in their or their husband's home state following a birth. Women who choose to move home are younger, less likely to be married, less likely to have a college degree, and have lower personal and family income than women who choose to stay in their home state. Notably, while women outside of their birth state have higher personal and family income than women who are living in their birth state, non-mover women[7] that live in the home state are more likely to be working post-birth, though they had similar rates of full-time employment pre-birth. This is consistent with a story in which women who are less resourced and able to afford the costs associating with having children are more likely to move home, and those who are living in their home state have access to familial and social networks that allow them to stay attached to the workforce.

[TABLE 3 ABOUT HERE]

Using the PSID, we can explore how observed earnings change following a move for never-mothers, ever-mothers, and mothers during the pregnancy and young childhood period of parenthood. In Appendix Table A.5, we report changes in income for women from year t to year t+2, conditional on whether the state the woman was living in changed between year t and t-1 (i.e., moved) and whether the state they are in at time t is the same as their parents. The first takeaway from these results is that income gains associated with

[7]Due to the disruption of a recent move to employment, comparing movers to non-movers will likely misstate the relationship between birth state and employment status, thus we compare always stayers in birth vs. non-birth states.

moving differ by parental status. Never-parents get larger income gains from moving than from staying, whereas those with kids experience similar gains regardless of moving status. These average effects, however, mask differences depending on whether the move is to the home state. Moving home is associated with declines in income for those who never have children, whereas ever-parents experience income gains at the time of a move home that are larger than the income gains they experience for moves to a non-home location. We can reject the null that the change in income is the same based on move location for both never-parents and ever-parents. Second, while income gains are generally small for women during the pregnant/young child period, they experience gains if they are either non-movers or stayers in their parent's state but experience earnings declines on average if they stay in or move to a non-home state.

2.2 Grandparent Proximity and the Child Penalty

Our previous analyses suggest that fertility and migration decisions are related, and we hypothesize that this is in part because living near family allows mothers to use family members as sources of childcare. The value of informal childcare may both induce women to move back home and induce women to stay in their home location despite better economic opportunities elsewhere. It is well-documented that women experience a decline in earnings following births, often referred to as the 'child penalty,' which persists for up to ten years post birth (Kleven et al., 2019b). A large factor in the decline in earnings is women's withdrawal from the labor market. Therefore, we might expect that having access to cheaper or free childcare would allow women to work more hours and reduce the child penalty.

To test this, we use data from the Panel Study of Income Dynamics (PSID) to estimate the size of the child penalty for women living near or far from grandparent care and for women living in high vs. low childcare cost regions. We adopt a modified form of the event study specification first proposed by Kleven et al. (2019b). For each mother in the data, we define event time (t) based on the year of their first child's birth. Our outcome of interest is person i's earnings Y_{icst} in year s, event time t, and location c where location is county-state. The regression is as follows:

$$Y_{icst} = \sum_{j \neq -2} \alpha_j^g \mathbb{1}[j = t] + \beta_k^g \mathbb{1}[k = age_{is}] + \gamma_s^g + \theta_c^g + X_{it}' B^g + \epsilon_{icst}. \tag{1}$$

The regression contains event-time dummies with α coefficients, age dummies with β coefficients to control for life-cycle trends, year dummies with γ_s coefficients to control for time trends, location fixed effects, θ_c, to control for county-specific time trends, and demographic controls X_{it} which include dummies for race and education. Event-time $t = -2$ is omitted, so all estimates are relative to the year just prior to birth. All coefficients vary by gender; in our primary specification, men who had a child act as a comparison group, relying on the fact that past papers show that men do not experience the same disruption in income at the time of their first child. We are able to identify effects of all three sets of dummies because of the variation in the age at which women have children.[8] In a secondary specification, we also use a sample that only contains women; the α terms in these specifications thus represent the difference in earnings for a woman of age k in year s who had a child relative to a woman of the same age in the same year who did not.

The parameters of interest are the α parameters, but they will represent differences in levels. To transform them into percent changes, we calculate $P_t = (\hat{\alpha}_t)/(\mathbb{E}[Y_{ics,-2}])$, where the bottom of the fraction is the mean pre-period baseline earnings in the period two years prior to birth.

We estimate this regression separately for women living near or far from the child's

[8]For more details on the identification assumptions needed to assume these are the causal impacts of childbirth, see Kleven et al. (2019b).

grandparents at the time of the child's birth as well as separately for women living in counties with high or low childcare prices.[9] For this analysis, we need a panel of income data for women in the years surrounding their first birth. To create this, we use the geocode restricted PSID's full retrospective history of births and adoptions, which provides the full history of births for those interviewed in the years 1985 onward. We create a data set including the year women's first birth occurred, their age at that birth, and whether they were married at the time of that birth. Following the restrictions used by Kleven et al. (2019a), the panel includes five years pre-birth and ten years post-birth. We restrict the sample to women who had a birth between the ages of 20 and 40 to abstract away from teenage motherhood. Women are also excluded from the sample if they are missing more than 8 years in this period, missing all years pre-birth, or all years post-birth. We then combine this data with information from the PSID family files on earned income in each year of the women's life, the US county they live in each year, and the US county that their parents live in each year. Because we only observe parents of the PSID respondent, households' grandparent status is based on the location of the parents of whichever spouse has an identifiable parent ID.

To compare child penalties across groups, our statistic of interest is the child penalty gap:

$$\frac{\hat{\alpha}_t^1}{\mathbb{E}[Y_{ics,-2}^1]} - \frac{\hat{\alpha}_t^2}{\mathbb{E}[Y_{ics,-2}^2]}$$

where 1 indicates living in the location that is presumed to have lower costs of childcare (near grandparents, low childcare prices) and 2 indicates the opposite. If these lower costs make it easier for women to stay attached to the labor force, we would expect this gap to be positive indicating that women with access to lower costs childcare have smaller earnings

[9]We do not estimate the child penalty separately by those who *move* to be near the grandparents. Near or far is assigned based on location in the year of birth. Due to the small sample size of movers within the PSID, we do not have enough power to separately identify the difference in the child penalty for those who are near to grandparents because they moved home relative to those who are near and never left.

losses post-birth. We use the Delta method to calculate standard errors of this gap and then test whether we can reject the null that the child penalty is equal across types.

Our outcome, earned income, is defined as the reported total income including wages and other income and combines impacts of births on both labor supply and wage.

Note that these estimates should not be interpreted as the causal impact of living near a grandparent or in a childcare cost region on the child penalty. We expect that women are sorting across these locations in part based on their attachment to the labor force; women who want to continue working after a birth for reasons unobservable to us as econometricians are more likely to settle in places with affordable childcare, whether that be relative care or cheaper private care options. We cannot separate these indirect selection effects from the direct effects of having cheaper childcare available. To address differences in pre-birth observables by location, we use inverse probability weighting based on age, race, education, marital status, and pre-birth earnings, following the methods in (Kleven et al., 2024) and (Karademir et al., 2026).[10] While this weighting addresses differences in pre-birth observables, such as the fact that those living in the grandparent's location earn less pre-birth than those living afar, we cannot fully control for selection on unobservables. Nonetheless, these patterns will provide suggestive evidence of whether childcare cost factors are meaningfully related to the long-term child penalty women face following their first birth and motivate the need for a structural model which explicitly models these selection patterns.

Figure 2 plots the coefficients from the event studies described in Equation 1, with panel A plotting the coefficients separately for mothers who live in the same county as the child's

[10]Specifically, we use a Probit model to predict the likelihood π of living within 25 miles of the grandparent's county (in high cost county) as a function of log earnings in the year prior and two years prior to pregnancy, age, race dummies, education dummies, and a dummy for marital status, and we then weight those who live near grandparents (in high cost) $(E[\pi])/(\hat{\pi})$ and those far from grandparents (in low cost) by $(E[1-\pi])/(1-\hat{\pi})$.

grandparents or in a county whose population centroid is less than 25 miles from the centroid of the county of the child's grandparents (near) or more than 25 miles from the county and panel B plotting the size of the gap between these groups. While both types of mothers experience a large child penalty, those living distant from the child's grandparents experience a child penalty that is about 20 percentage points larger than the child penalty for living close to the grandparents.

[FIGURE 2 ABOUT HERE]

Table 4 reports the results of regressions which aggregate the coefficients into post-period, year of birth, and pre-period (excluding two years prior to birth). In these regressions, we vary our definition of 'near' to test that our results are not sensitive to distance or geography cutoffs. Panel A reports results for our primary specification in which we include the male control group; Panel B reports results for the regression including only women. Columns 1 and 2 report the coefficient α_1 for the regression of women's labor income on the post-period separately for those living in the same county and those living in the different county as the child's grandparents. While women who live in different counties from the grandparents earn approximately \$20,590 or 78% per year less post-birth, women who live in the same county only earn \$13,521 or 56% less. This translates to a child penalty that is 22 p.p. greater for mothers 'far' from grandparents relative to baseline pre-birth earnings compared to the child penalty for those near grandparents.

[TABLE 4 ABOUT HERE]

As we add more stringent distance requirements, such as being in a different county and more than 25 miles away (column 3), being in a different state (column 4), or being in a different state and more than 25 miles way (column 5), the child penalty grows in magnitude. Columns 6-8 show the difference in the child penalty for difference distance bins; as before, the child penalty is larger relative to baseline as the distance radius from grandparents increases. In a t-test of equality of coefficients, we cannot reject the null hypothesis that the child penalty differs if we use the distance bin 'different county, 1-25 miles' (col. 6), 'different county, 25-50 miles' (col. 7), or 'different county, >50' (col. 8), but we can reject that all differ significantly from being within the same county (col 1). In our secondary specification omitting men, the child penalty is smaller in magnitude across all specification, indicating that some of the child penalty in panel A is due to men who have children having slight increases in earnings post-birth. However, despite the difference in magnitude, there is still a larger child penalty for women living far from grandparents.

We next do a similar exercise for those living in high or low childcare cost states. Using the US Department of Labor's National Database of Childcare Prices, we calculate the average weekly price for center-based toddler care by county and assign each county a percentile value indicating where in the distribution that county falls relative to the full distribution of county-level prices. The average respondent in our PSID sample lives in the 83rd percentile of the national distribution. We compare those who are living at or above the 75th percentile of the child price distribution ('High Cost') to women living below the 75th percentile ('Low Cost').

In Table 5, we report the aggregated post-birth effects of a child by childcare cost region for those above (column 1 and 4) and below (column 2 and 5) the 75th percentile in terms of childcare costs. We also run a specification that interacts the level of weekly childcare costs with the event study indicators (columns 3 and 6). Here, we see that a one-standard-deviation increase in childcare prices – \$48 per week – increases the child penalty by \$2,329, which is an 11% child penalty relative to the baseline earnings. Appendix Figure A.2 reports the coefficients for event study specification. The difference across childcare cost regions is of similar magnitude to the difference in the child penalty for those near vs. far from the child's grandmother but is not statistically significant until the age of two, consistent with the fact that children are less likely to receive out-of-home care in the first year of life.

[TABLE 5 ABOUT HERE]

The declines in income following a woman's first child are in part driven by changes in labor force participation. To test whether the declines are due to women working fewer hours (i.e., part-time) or not working at all, we repeat the child penalty regressions with indicators for working full time (i.e., usual weekly hours greater than 37.5 hours), part-time (i.e., 1-37.5 usual hours), or not working (i.e., 0 usual hours) as the outcome. Appendix Figure A.1 show the results of these regressions. For both distance bins, women experience a statistically significant decline in likelihood of working full-time (-22 to -34 pp), a statistically insignificant increase in the likelihood of not working (+21 to +29 pp), and a non-statistically significant and small increase in the likelihood of working part-time (+0.9 to +4 pp). For both full-time work and non-work, there is a stronger effect for those living far from grandparents: women's likelihood of full-time work post-birth is 12 pp lower if they live more than 25 miles away from grandparents.

We use the cross-sectional ACS data to corroborate the finding that women work more in their home-states than outside their home-state. We regress usual hours worked per week in the previous year on a variety of covariates to do with the presence of children, location, childcare costs, and marital status. Intuitively, higher childcare costs ought to decrease hours worked by women because it makes working relatively more expensive. Being proximal to parents ought to increase labor force attachment if parents primarily provide time transfers in child-rearing. Appendix Table A.6 presents the results of this exercise. The presence of children decreases usual weekly hours worked by women substantially, and the effects are noticeably stronger for married women. However, women who have children in their birth state work more than women who do not, while women with children in states with higher childcare costs also working relatively less.

Lastly, we test whether there are spillover impacts on the grandparents' earnings. If women living nearer grandparents are using relative care, we also might expect that the women's first birth decreases grandparents' labor supply and earnings as well. Past research in Austria (Frimmel et al., 2022), Canada (Karademir et al., 2026), and Denmark (Gørtz et al., 2025) document that grandmothers experience earnings declines post-birth. While Frimmel et al. (2022) shows that this grandmother penalty is larger for proximal grandparents, Gørtz et al. (2025) finds no difference based on distance to grandchildren. In our setting, we re-run our child penalty regression with the earnings of the grandmother as the outcome and see a 15% child penalty for the full sample of grandmothers but cannot reject the null that the effects are similar for those in the same county vs. different county than their daughter. This, along with the fact that grandparents are much less mobile than their children,[11] suggests that while informal childcare may impact the grandparent generation's labor supply, it is less likely to impact the grandparent generation's location choice.

3 Model

Taken together, these analyses demonstrate the importance of geographic proximity to affordable childcare for women's long-run labor market outcomes– whether it be informal care from a grandparent or less expensive private childcare. However, in both the analyses, we are not fully accounting for the joint selection process of location, fertility, and labor force participation. For example, when we observe that mothers living in high childcare cost regions earn less than those in low childcare cost regions, it may be that the mothers in low cost regions were motivated to select into those regions due to higher ability or attachment to

[11] In our PSID sample, we see that the cross-state mobility rate of women of child-bearing age is 4 times higher than that of their parents and that women are significantly more likely to move to their parent than vice versa.

the labor force that is known to them but unobserved to us as econometricians. Therefore, a model that places some assumptions on the selection process will be required to account for the endogeneity of migration decisions and to evaluate the impact of policy counterfactuals.

In particular, we are interested in how policies that may substitute for intergenerational time transfers (such as subsidized childcare) would influence the migration decisions and subsequent earnings of women who might otherwise rely on their parents to assist in child-rearing. Using our model, we will be able to explore the effectiveness of such policies in improving welfare for different types of parents, as well as decompose any effects on earnings into a direct effect of changes in attachment to the labor force due to childcare policies versus the secondary effects of the policies such as allowing households to sort into better paying labor markets.

Lastly, we estimate the model separately by race to explore heterogeneity in the value of these policies for Black mothers relative to White mothers. The frictions associated with childcare access may be particularly important in explaining racial gaps in migration rates and wages, as single motherhood is more common for Black mothers. Our descriptive analyses suggest that single mothers are more dependent on geographic proximity of family for access to care. Moreover, the descriptive analyses suggest that home migration at time of first pregnancy is more common for White and Black women, but not non-White/Black women. The model will allow us to precisely quantify the extent to which fertility events drive migration across demographic groups in the United States.

3.1 Setup and Timing of Decisions

Our model adapts the dynamic labor force participation of Eckstein and Wolpin (1989) and nests it in a simple framework of dynamic migration (Kennan and Walker, 2011) while incorporating multiple dimensions of family structure. The model is a dynamic discrete choice model that follows the fertility, labor force participation, and migration decisions of women.

Figure 3 presents a summary of the phases of the model and the decisions made in each period. A period is one year. Agents enter the model at age 22 and may choose to conceive each period until age 40. Between ages 40 to 45, though agents cannot get pregnant, they may either have young children or have no children. After age 40, we additionally assume that the agent's current marital status remains fixed for the rest of the lifecycle. Agents choose whether to conceive, whether to supply labor and, afterward, whether and where to move until making a final labor force decision at age 65, after which they accrue no further utility. We select age 22 as the starting point to allow the bulk of higher education choices to be made while pre-empting the prime fertility years of U.S. women.

[FIGURE 3 ABOUT HERE]

At the beginning of each period, the women in our model observe the location of their parents and stochastic realizations of their marital status. The women choose whether to conceive, which we model as entering a state wherein the woman knows with certainty she will have a child aged 0 in the subsequent period. Agents then choose whether to participate in the labor force, weighing increased utility from consumption should they choose to vs. preferences for leisure and savings on childcare expenditures should they not. Participation also increases future expected earnings through accumulating work experience. The women then choose where to live — in particular, their options include staying in their current location, moving to their parents' location, or moving anywhere else, which we collapse into the nine Census divisions.[12] The parent's location is defined as the same state within a Census division as the parents; agents can live in the same division as their parents while

[12] See Appendix Table A.7 for division definitions. 72% of cross-state moves observed in the data involve cross-divisional moves.

not being in the parent's state, in which case they do not receive the time transfers or utility benefits of parent's state. There are thus 10 total locations an agent can choose between. Following their migration decision, women enter the subsequent period.

3.2 State Variables and Value Functions

Table 6 presents a complete summary of state variables and notation in the model, which are described in more detail in this section. Locations are indexed by ℓ, with ℓ^P denoting an agent's parent location. The other locations represent the nine Census divisions, each of which vary by childcare costs δ^ℓ, wage effects η^ℓ, and cost of living κ^ℓ. Static college attainment is indexed by $e \in \{0, 1\}$, endogenously determined years of experience by x, and deterministic age by a.

[TABLE 6 ABOUT HERE]

We now turn to describing notation for family structure.[13] Marital status is denoted by $m \in \{0, 1\}$ and is assumed to evolve entirely stochastically as a function of other state variables, described further in Section 4. Men make no decisions in our framework and inelastically provide monetary and childcare time transfers to their wives. The variable a_c captures the age of the youngest child in the household, provided that they are less than 5 years old.[14] The state $a_c = \emptyset$ stands for when the household has no children aged 5 or younger.

[13] We omit individual and time subscripts in this section for readability.

[14] We currently do not keep track of the number of young children and instead focus on the presence of any at all. Rosenzweig and Wolpin (1980) study the effects of twins on labor force participation and find that women with twins exhibit a labor force participation rate 0.371 pp lower than women without. While these effects are meaningful, we view their magnitude as small enough to permit the omission to ease computation. This almost certainly means that we are understating the costs of childcare and the potential effects of subsidies to them in terms of labor force participation and wages.

Meanwhile, the variable f captures the fertility status of the woman: if $f = 1$ in year t then $a_c = 0$ in year $t + 1$ with certainty. Having pregnancy be a known state allows our women to make migration and labor force participation decisions *in anticipation* of fertility events. Women are allowed to have multiple children in that their f state may equal 1 even if the household currently contains a young child, in which case a_c will be reset to zero in the subsequent period. We shut down fertility events at age 40, meaning that when women leave the model at age 45 all children have aged out of early childhood.

Women are endowed with a single unit of time each period and may choose to work full time ($h = 1$) or not at all ($h = 0$). Subsuming all the state variables outside of the agent's fertility status and current location into the vector Ω, the agent begins the period by choosing whether or not to conceive:

$$\begin{aligned} V^1(\Omega, \ell) &= \max_{f\in\{0,1\}} \left\{ \mathbb{E}_{\varepsilon_w}[V^2(\Omega, \ell; f)] + f \cdot (\theta_1 + \theta_2 a + \theta_3 m + \theta_4 am + \theta_5 e + \varepsilon_f) \right\}, \\ &\qquad \varepsilon_f \sim N(0, \sigma_f), i.i.d. \end{aligned} \tag{2}$$

If the agent is older than 40, conception is no longer an option, and we have $V_1(\Omega, \ell) = \mathbb{E}_{\varepsilon_w}[V_2(\Omega, \ell; 0)]$ and assume that $\varepsilon_f = 0$. Conceiving involves utility costs that include a fixed component along with variable costs that depend on the agent's age, marital status, an age-marital-status interaction, and an education effect. To allow for individual heterogeneity in fertility decisions, we also include a random component ε_f – thus, the agents in the model will only choose to conceive if their draw of ε_f is above a certain threshold.

The agent also considers how happy they expect to be when they move to the labor supply phase of the period, given by V_2. We first discuss the labor supply decision in the case of a woman with no young children to attend to:

$$
\begin{aligned}
V^2(\Omega, \ell; f) &= \max_{h \in \{0,1\}} \Big\{ \alpha_1(c) + (1-h)(\alpha_2 + \alpha_e e + \alpha_x + \alpha_c c + \alpha_\mu \mu) + \alpha_3 \mathbb{1}(h \neq p) \\
&\quad + \alpha_4 \mathbb{1}(\ell = \ell^P) + \boldsymbol{\alpha_\Gamma \Gamma} + \mathbb{E}_{\zeta_{\ell'}}[V^3(\Omega, \ell; f, h)] \Big\}; \\
\kappa^\ell c &= w_S \mathbb{1}(m = 1) + wh.
\end{aligned} \tag{3}
$$

Thus, α_1 rescales utility over consumption in dollars to util terms[15], and α_2 represents a preference for leisure. Preferences for leisure are further modified based on experience (α_x) or if the agent has a college degree (α_e), and α_c represents a consumption-leisure complementarity that makes married women less likely to work. Finally, we allow for leisure preferences to depend on unobserved earnings potential μ (see below) via α_μ. The parameter α_3 constitutes a penalty borne from changing one's labor force participation status, allowing the model to account for frictions individuals face in moving in and out of the labor force. The utility premium for currently being in one's parent's location is captured by α_4. Locations differ in amenities $\boldsymbol{\Gamma}$ that include average distance to shore taken from Lee and Lin (2017), average number of warm-weather days in a calendar year taken from Kennan and Walker (2011), and an index of other amenities related to government provisions and quality of life taken from Diamond (2016) which have been identified as salient drivers of location decisions. While we do not explicitly model decisions over housing or consumption of other location-specific, non-childcare products to impact utility, we allow the price of consumption κ^ℓ to differ by location, allowing for differences across Census divisions in costs for goods like housing, transportation, food, etc. to influence location choice.

Consumption here is given by the wages of the woman's spouse (assumed to be supplied

[15]We scale consumption so that one unit of consumption corresponds to $2,080. This can be thought of as one hourly wage unit, since working 40 hours per week, 52 weeks pear year would imply that an additional dollar increase in hourly wages would increase one's annual budget by $2,080.

inelastically and equal to zero if the woman is unmarried) and the earnings of the woman herself. Log wages of the woman and her spouse are given by the following equations:

$$\log(w) = \beta_0 + \boldsymbol{\beta X} + \mu + \eta^\ell + \beta_{e,\eta} e \cdot \eta^\ell + \varepsilon_w + \xi;$$

$$\log(w_S) = \beta_{S,0} + \boldsymbol{\beta}_S \boldsymbol{X}_S + \mu_S + \eta^{\ell,S};$$

$$\varepsilon_w \sim N(0, \sigma_w) \text{ i.i.d}; \quad \xi \sim N(0, \sigma_\xi) \text{ i.i.d.}$$

The vector of observables of the spouse $\boldsymbol{X}_S$ contain a college dummy and a quadratic in experience. We also include a high-earning and low-earning spouse type, $\mu_S \in \{\mu_S^L, \mu_S^H\}$. The agent's observables $\boldsymbol{X}$ contain the same standard Mincerian combination along with dummies for having a child aged 0-1 or a child aged 2-4.[16] With the assumption that husbands supply labor inelastically, the terms of the husband's wage equations can be uncovered directly from data if we assume husbands to be identical to their wives in age, and husband earnings types can be inferred from the individual fixed effects we estimate for them. We estimate the components of the woman's wage process within the model. Location fixed effects η^ℓ are also assumed to be constant across time and are estimated separately for men and women, which with the assumption of exogenous male labor supply will allow us to estimate values for $\eta^{\ell,S}$ outside the model using observed male wages. Location fixed effects for women will be estimated internally while also including an interaction between these effects and education level ($\beta_{e,\eta}$) to account for the possibility of larger variation in geographic wage returns for more educated individuals. Wages for women additionally include an unobserved fixed effect

[16]We abstract away from an explicit part-time choice. The child penalty analysis shows only small, not statistically significant changes in part-time work, and these changes do not vary by distance to grandparents, unlike the large and significant changes for full-time and non-work. However, we include these dummy terms to allow the model to be consistent with mothers of young children preferring more flexible/ lower-paying jobs.

μ, a transient component ε_w, and measurement error ξ assumed to be uncorrelated with other wage determinants.[17]

The final term of Equation (3), $\mathbb{E}_{\zeta_{\ell'}}[V^3(\Omega, \ell; f, h)]$, represents the expected continuation value associated with choosing a location given the woman's labor force participation decision. Following her choice of h, the woman receives a series of location preference shocks that will determine her location choice:

$$(4)\quad V^3(\Omega, \ell; f, h) = \max_{\ell'} \left\{ \beta \sum_{\Omega'} \mathbb{E}_{\varepsilon_f}[V^1(\Omega', \ell')]\mathbf{Pr}(\Omega'|\Omega, f, h, \ell') - \Delta(\Omega, \ell')\mathbb{1}\{\ell' \neq \ell\} + \zeta_{\ell'} \right\}.$$

The agent takes into account possible state transitions Ω' and expected next-period utility after solving her optimal labor supply problem and optimizes her choice of next-period location following a series of location preference shocks $\zeta_{\ell'}$ distributed Type 1 EV with location 0 and the scale parameter normalized to 1. Allowing for such shocks to influence migration decisions is critical, given that moves for apparently non-pecuniary reasons are a salient feature of the data (Kennan and Walker, 2011). Marriage transitions are governed by stochastic functions that we calibrate directly from the data. We assume that the woman can no longer become pregnant at age 40 and that their marriage state at age 40 carries on for the remainder of the life cycle. The agent's next value of p (past-period labor force participation) depends on her selection of h. The agent's experience x increments by 1 should she choose to work and 0 if she does not, and the agent's age a increments by 1 with certainty. Next-period utility is discounted by the factor β.

The parameter $\Delta(\Omega, \ell')$ captures moving costs that the agent faces should they have

[17] It is possible that individuals may enjoy some advantage in the job market of the grandparent location due to better networks or referrals. At the same time, such factors may not be helpful for wages if they demotivate the agent from searching for a high-quality job. Including a term for living in the home location in the women's wage function does little to improve the model's fit or change the main results qualitatively or quantitatively, which is consistent with our mixed findings of the impacts of a home move on wages earlier.

chosen to do so, which itself depends on other elements of the state space. If a woman moves across locations in a period, she must incur moving costs given by

$$\Delta(\Omega, \ell') = \gamma_0 - \gamma_1 e + \gamma_2 \mathbb{1}\{a_c \neq \emptyset\} + \gamma_3 m + \gamma_4 N^{\ell'} + \gamma_5 a. \tag{5}$$

Moving costs involve a fixed cost and costs that vary with college education, marriage, age, and presence of young children. While the purely monetary costs of moving are relatively small, the non-pecuniary burdens of moving may be large, and these terms capture these costs while allowing them to vary along demographic lines. Furthermore, we allow for moves to larger locations (N^{ℓ} represents the population of division ℓ in tens of millions) to be less costly as in Kennan and Walker (2011), representing that more populous locations may be more likely to contain social contacts for movers or easier-to-navigate job markets.

Finally, a woman with young children in the working stage of the model enjoys utility:

$$\begin{aligned} V^2(\Omega, \ell; f) &= \max_h \Big\{ \alpha_5(c) + (1-h)(\alpha_6 + \alpha_e e + \alpha_x x + \alpha_c c + \alpha_\mu \mu) + \alpha_3 \mathbb{1}(h \neq p) \\ &\quad + \alpha_7 \mathbb{1}(\ell = \ell^P) + \boldsymbol{\alpha}_{\boldsymbol{\Gamma}} \boldsymbol{\Gamma} + \mathbb{E}_{\zeta_{\ell'}}[V^3(\Omega, \ell; f, h)] \Big\}; \\ \kappa^\ell c &= w_S \mathbb{1}(m=1) + wh - \delta^\ell \cdot \max \Big\{ 0, h - \tau^S \mathbb{1}(m=1) - \tau^{P,m} \mathbb{1}(\ell = \ell^P) \Big\}. \end{aligned} \tag{6}$$

The specification thus flexibly allows women with young children to have different preferences for consumption, leisure, and location. When young children are present, the agent must also either dedicate time to caring for their children or absorb childcare costs, which depend on their current location, the current location's type, and the woman's marital status. Women never pay for childcare costs if they do not work ($h = 0$), and spouses and grandparents contribute fixed time transfers to childcare (τ^S and $\tau^{P,m}$) if the woman is either married or living in her parent's location. The grandparents' contribution varies based on the marital status of the woman — this, along with women differing in their budget constraint and moving costs along marital status, allows the model to potentially capture differential behavioral patterns among married and unmarried women, as was suggested by the empirical analysis. Differences in labor supply for married and unmarried women outside of the grandparent location will be useful for identification of τ^S, while labor supply patterns of women within marital types located in and out of the grandparent location will be informative for $\tau^{P,m}$. Furthermore, we allow for unobserved heterogeneity in grandparent helpfulness, such that with probability P_τ the agent's parents will provide time transfers of zero.

3.3 Model Solution

The model is solved via backward induction. If the agent is in the phase of the model where fertility is possible, her fertility decisions will be governed by whether her fertility utility shocks ε_f are sufficiently high. Afterward, labor force participation is governed by whether the transient component of the wage offer ε_w is sufficiently high. We compute cutoff values of ε_w for each element in the state space, after which continuation values can be computed by applying the usual type-1 extreme value formula and using the cutoff values in conjunction with properties of the normal distribution to solve for an agent's expected flow utility in the next period. A more detailed description of the procedure and the algebraic details for solving cut-off values is described in Appendix B.1.

4 Estimation

4.1 Data

We use data on non-Hispanic White and Black women aged 22-40 in the 2001-2019 waves of the PSID.[18] All women must be observed at least through ages 22 to 25 to be included

[18]Sample size limitations prevent us from looking at additional demographic subgroups.

in our sample. The PSID shifted to a biennial schedule starting in 1997 — however, in years following 2000, respondents were asked their income and hours worked for both the preceding year and the year before. Furthermore, if the respondent had moved across states since their most recent interview, they were asked in which year the move was made. This information, combined with marital and childbirth histories for all respondents, allows us to construct yearly data from the biennial survey with minimal assumptions. Importantly, the PSID additionally allows for intergenerational linkages, through which we can track the location of the parents of the respondent. For additional details on the sample construction, refer to Online Appendix B.2.

Our sample construction leaves us with a sample of 932 women and 10,122 person-year observations. The median woman in our sample is observed for seven years (i.e., up through age 28), and Appendix Table A.9 for a complete tabulation of ages in our analysis sample. Appendix Table A.10a presents descriptive demographic and economic statistics broken down by age ranges and race, while Appendix Table A.10b presents migration statistics in our estimation sample with additional breakdowns by race.[19] White women in the sample are considerably more mobile than their Black counterparts, even when conditioning on factors such as the presence of young children and marital status. However, among movers, the share who move back to the parent location across race is roughly constant across race.

4.2 Parameters Estimated Outside the Model

Table 7 describes the values of all parameters taken from outside the model. We assume a discount rate of $\beta = 0.95$. Childcare costs levels δ for an hour of care are at the division level using previously described data from the NDCP. We average center-based prices across age groups and collapse to the division level weighted by the number of households with children

[19] We do not use sample weights when creating these statistics or when estimating our model. Including longitudinal sample weights available in the PSID does little to change our parameter estimates.

under six per county, keeping data before 2016 to be consistent with our estimation sample. Costs of living κ^ℓ are taken from the American Chamber of Commerce Research Association's Cost of Living Index.[20] The parameters governing spousal wages are taken from comparable ACS and PSID samples to our analysis sample. With the assumption that husbands supply labor exogenously and are of the same age as their wives, these parameters can be estimated directly from Mincerian wage regressions. This also allows for the recovery of location wage effects for spouses $\eta^{\ell,S}$, which are again grouped at the division level. Division populations N^ℓ come from year-2000 Census population estimates.

[TABLE 7 ABOUT HERE]

Appendix Table A.8 in the online appendix reports division-level childcare costs, wage effects of spouses, and living costs.[21] Unsurprisingly, these measures are highly correlated across locations, with high-wage divisions also usually having high costs of living and high childcare costs. However, the relationships are not exact, with the correlation of wage effects and childcare prices (both being adjusted for living costs) being approximately 0.75. This will result in contrasting migration incentives for women with and without children, and the extent to which we observe these types of women behave differently in the data will

[20]The ACCRA index is a weighted average of costs of food, housing utilities, transportation, health care, and miscellaneous goods and services among different metro areas in the United States. State-level indices have been published from 2016 onward by the ACCRA, and a state-level index constructed by Kennan and Walker (2011) for around 1980 is also available. Given high correlation of local cost-of-living across years ($\rho = 0.8$), we take the midpoint of the 1980 and 2016 indices while normalizing the cost-of-living level of Iowa to be zero before averaging by division with population weights.

[21]When estimating the model separately by race, we also estimate race-specific values of η^ℓ and β_S. We also raise childcare costs for married, college-educated women by a quantity consistent with Berlinski et al. (2023) to reflect this group on average selecting higher-quality and more expensive options, though the childcare quality choice is not explicitly modeled here.

be crucial in identifying preferences for consumption and will prevent us from mechanically overstating the role of childcare costs in influencing labor mobility.

We estimate marital transitions via linear probability models using our estimation sample that admit as inputs whether the agent is currently married, a parent to young children, and age. Probabilities of marital dissolution and formation also vary over spousal and agent college attainment, and the likelihood that a woman marries a man with a college degree depends on her own educational attainment.[22] We calculate marriage processes separately for women with and without a college degree as well as by race.[23] Appendix Figure A.3 presents the fit of our model with regards to life-cycle profiles of marriage rates and indicates that our model fits the data well.

4.3 Estimation and Identification

We use maximum likelihood to estimate the remaining parameters of our model using the Sbplx algorithm.[24] The joint likelihood function for labor force participation, wages, and

[22]We acknowledge that other factors, such as the ease of childcare arrangements, could influence marital stress and consequently marital formation/dissolution. However, accounting for this factor is difficult due to childcare arrangements not being directly observable, and given other mixed evidence on the elasticity of marital decisions with respect to broader policy contexts (e.g. Bitler et al. (2004), we expect such responses to be second-order in importance.

[23]Disparities in spousal quality by race are a salient feature of the data — for example, approximately 60% of college-educated white women marry with college-educated men in our estimation sample, while the corresponding statistic for Black women is around 30%.

[24]A variation on the Subplex algorithm, which itself applies the Nelder-Mead method on a sequence of subspaces; see https://nlopt.readthedocs.io/en/latest/NLopt_Algorithms/.

migration for the N women in our sample, each observed for T_i periods, is given by:

$$L = \prod_{i}^{N} \sum_{\tau,\mu} \mathbf{Pr}(\tau,\mu) \prod_{t=1}^{T_i} \mathbf{Pr}(f = f_{it}|\Omega_{it}, \ell_{it}) \cdot \mathbf{Pr}(h = h_{it}|\Omega_{it}, \ell_{it}, f_{it}) \cdot \\ \mathbf{Pr}(w = w_{it}|\Omega_{it}, \ell_{it}, f_{it}, h_{it}) \cdot \mathbf{Pr}(l' = l'_{it}|\Omega_{it}, \ell_{it}, f_{it}, h_{it}).$$

Further details on the functional form of the probabilities are given in Appendix Section B.3.

We employ a mixture model over unobserved heterogeneity in grandparent transfers (i.e. allowing grandparents to be helpful ($\tau^P > 0$) or unhelpful ($\tau^P = 0$)) and individual earnings potential, letting $\mathbf{Pr}(\tau, \mu)$ denote the probability of the agent being unobserved type τ and μ. We allow for two unobserved wage types μ, with the value of the lower type normalized to zero. We assign the educational state based on whether women in our sample *eventually* obtain a college degree, but since in practice some individuals who obtain a college degree do so in their mid-20s, we exclude observations age less than 25 with a college degree when evaluating the likelihood and start their simulation at age 25 as well. The relationship between labor force participation and migration decisions in our model are identified from jointly observing participation, earnings, and location choices for women, conditional on demographic characteristics and location of grandparents.

First, we assume that the shocks drawn in the model – location preferences, earnings shocks, fertility realization, marriage realization – are all independently and identically distributed across individuals and time. While this may seem a strong assumption at first, we do allow the likelihood of pregnancy and marriage to vary on observable characteristics, including many of the factors that contribute to a woman having a higher or lower earnings potential. This means that this assumption relies only on the weaker assumption that preferences for pregnancies and marriages are not correlated across time with the transitory component of earnings that varies idiosyncratically across time. Due to high rates of unintended and mistimed births in the US, we believe this is a reasonable assumption. The assumption of independence of location preference shocks and earnings shocks is a stronger assumption: we assume that wage differences across time/individuals are not place-specific and not correlated with amenities in a location in a given year. While the inclusion of amenities along with a reasonably rich wage process in the model helps justify this assumption, allowing for additional heterogeneity in idiosyncratic wage match effects may be helpful as well.

With these distributional assumptions in place, identification of the structural parameters derives from variation in labor force outcomes and choice of location. Following Eckstein and Wolpin (1989) and using equations for reservation wages in Appendix B.1, the reservation wages ε^*, the wage parameters ($\beta_0, \boldsymbol{\beta}$, and μ), and σ_w and σ_ξ are all identified from data on participation and wages.[25] We can then use the identified ε^*_w and our equation for the definition of the reservation wage described in Appendix B.1 to identify $(\alpha_2)/(\alpha_1), (\alpha_e)/(\alpha_1), (\alpha_\mu)/(\alpha_1), (\alpha_c)/(\alpha_1)$, and $(\alpha_3)/(\alpha_1)$. Based on the similar equation for women with young children, we can identify $(\alpha_6)/(\alpha_5), (\alpha_e)/(\alpha_5), (\alpha_\mu)/(\alpha_5), (\alpha_c)/(\alpha_5)$, $(\alpha_3)/(\alpha_5), \tau_{p1}, \tau_{p0}$, and τ_s. Using any combination of pairs in which the leisure parameter is the same across the presence of children (e.g., $(\alpha_e)/(\alpha_1), (\alpha_\mu)/(\alpha_1), (\alpha_e)/(\alpha_5), (\alpha_\mu)/(\alpha_5)$) would allow us to separately identify α_1 and α_5 and thus separately identify all α parameters governing leisure. Similar arguments allow us to identify the θ parameters that govern fertility decisions in the model.

The identification of the parameters governing unobserved heterogeneity comes from the use of wage and labor force participation data across time and geography. The dynamic structure of the data allows us to observe women's income repeatedly over time conditional

[25] While we could rely on structural form assumptions to identify these parameters as is done in Eckstein and Wolpin (1989), it is also the case that presence of spouse acts as an exclusion restriction. Being married changes your likelihood of participating through spouses' earnings, but we assume that spouses' earnings are uncorrelated with the unobserved component of wife's wages.

on labor force participation and location choices, allowing us to identify a discrete approximation of the full distribution of individual fixed effects. Identification of the proportion of helpful grandparents and the time transfer comes from the fact that we use the full distribution of earnings when forming the likelihood rather than just first moments. Increasing the proportion of helpful grandparents and increasing the time transfer both increase the likelihood of working and change the mean earnings of those who work, meaning that first moments alone cannot identify these parameters separately. However, how responsive labor force participation and wages are to amount of transfer and probability of transfer differs across locations based on variation in childcare prices across regions, meaning that we can uniquely identify the two parameters based on relative changes in wages and shares of women working across home locations.

The remaining parameters include the parameters governing preferences for the parent's location, amenities, and the moving cost parameters. We can identify the parent's location preference parameter off the difference in the likelihood of moving to the parent's location ℓ^p from some location k and the likelihood of moving to a non-parent's location from that same location k for agents who are similar on all demographic characteristics. The same logic applies for identifying the amenity utility parameters $\boldsymbol{\alpha}_{\boldsymbol{\Gamma}}$. The moving cost parameters are identified off the differences in likelihood of moving from location j to k versus staying in location j by demographic group. The parameter on population is identified off of the relative likelihood of moving from a small division to a large division vs. from a large division to a small division.

5 Results

5.1 Parameter Estimates

Table 8 reports the parameter estimates. Standard errors are computed via inverting the numerical Hessian of the likelihood function and taking its diagonal. The estimates of fertility parameters suggest that being married makes having children more likely, as does being younger, but the extent of racial heterogeneity across these parameters is limited, and many of the fertility preference parameters are estimated with considerable noise.

[TABLE 8 ABOUT HERE]

The estimation recovers preferences for consumption and leisure that increase and decrease respectively over the presence of a small child. The disutility associated with changing one's labor force participation status is substantial, and we find higher leisure preferences for women with high earnings potential, which rationalize their rates of labor force participation that, while higher than low-earning women, are still lower than those of men. The leisure-consumption complementary α_c is positive, reflecting women being less likely to work with higher-earning spouses, all else held equal. The estimates of wage returns to a college degree and experience are all in line with previous estimates in the literature. We also estimate a meaningful reduction in wages associated with having a child between 0 and 1 year old, but the effect of older children on wages is statistically insignificant. Additional estimates of the wage process (see Appendix Table A.11) show that geographic wage returns to women are positively correlated but slightly more spread out than those for men. We also estimate broadly similar degrees of unobserved earnings potential for Blacks and whites.

The estimates of time transfers τ suggest that informal care considerably offsets the direct cost of childcare for women with children — indeed, helpful grandparents cover close to half of childcare expenditures. Moreover, we estimate statistically and substantively significant differences in informal care usage by race, with Black women being approximately 15 p.p. more likely to have informal care available than their white counterparts.

We estimate positive preferences for residing in the parent location, suggesting that agents place a premium in being in their parent's location even after accounting for informal childcare transfers. We also estimate a version of the model without these transfers to quantify how important the informal childcare channel is in informing home preferences. Without childcare transfers, we estimate parent location preference parameters of 0.122 and 0.244 for women without and with children, respectively, suggesting that grandparent informal care can explain up to 22.5% of the home preference among U.S. women with children. Amenity preference estimates indicate that the agents in our model prefer higher levels of the Diamond (2016) amenity index, shorter distances to shores, and warmer weather, but only the last of these factors is estimated to be statistically significant. The moving cost estimates suggest that moving is less expensive (in terms of utility) for college graduates, but more expensive for married and older women. Moreover, moving to larger populations is meaningfully cheaper in terms of utility. Somewhat surprisingly, the model estimates that the presence of children makes moving slightly cheaper, though the standard errors are such that the estimate cannot be distinguished from zero statistically.

Because utility is linear in consumption, we are able to convert the moving parameters into dollars by dividing by the consumption scaling parameter and then multiplying by the consumption equivalence unit. For the "average" mover, the moving cost is about \$96,923, ignoring the value of the payoff shocks.[26] To construct a point of comparison for this figure,

[26] To calculate, we sum Δ for all individuals who move, discounted by the relevant consumption scaling. That is

$$\bar{\Delta} = 2080 \times \frac{1}{N_{move}} \sum_{i=1}^{N_{move}} \big[\big(\frac{\mathbb{1}(a_c \neq \emptyset)_i}{\alpha_5} + \frac{\mathbb{1}(a_c = \emptyset)_i}{\alpha_1} \big) \times (\gamma_0 + \gamma_1 e_i + \gamma_2 \mathbb{1}(a_c \neq \emptyset)_i + \gamma_3 m_i + \gamma_4 N_i^{\ell'} + \gamma_5 a) \big].$$

we compute cost-of-adjusted wages for working women in our simulated data and regress these real wages on the women's relevant characteristics and a vector of location indicators. We estimate that moving from the lowest-real-wage location (the East South Central Census division) to the highest-real-wage location (the New England Census division) would increase a woman's yearly real wage by $1,650, which would correspond to roughly $72,600 in lifetime earnings and a present value of $29,546 if she worked every year from age 22 to 65 and $\beta = 0.95$. Though this is the most extreme example of the potential earnings gains from a move and does not fully account for factors such as childcare costs and selection into the labor force in the first place, it demonstrates that our moving costs net of payoff shocks are of a comparable magnitude to the potential earnings gains. However, we will note that these moving costs are the estimated costs for a hypothetical move to an arbitrary location, whereas in the model people will only choose to move to high pay-off locations. Thus, these average costs are higher than the costs that households which actually choose to move will face once pay-off shocks are accounted for.[27] We also estimate racial heterogeneity in moving costs in that Black women have smaller moving costs than white women associated with being married.

5.2 Goodness of Fit

To assess our model's ability in approximating the true data generating process, we randomly simulate the outcomes of each woman in our estimation sample ten times, starting at age 22 (25, for college-educated women) and ending at the final age the given woman is

[27] See Kennan and Walker (2011) for further discussion of the distinction between average moving costs versus average moving costs conditional on moving. Kennan and Walker (2011) show that while the moving costs for households that choose to move to their home location are large, moving costs to non-home locations are actually negative, representing the fact that these moves are ones with large expected future payoffs for the households who make them.

observed in the data, using Bayes' rule to draw unobserved types. We then compare key moments in the estimation sample to those in our simulated data. We use our separate parameter estimates for White and Black women when simulating data for all model fit and counterfactual evaluations.

Figure 4 presents our model's fit of lifecycle profiles of labor market outcomes separately for women with and without a college degree. The model fits the data well, reproducing profiles of wages and experience accumulation that look very similar to the data. The model slightly understates labor force participation and earnings for women with a college degree at the beginning of the lifecycle, but the fit for non-college-educated women is nearly exact.

[FIGURE 4 ABOUT HERE]

We evaluate the model's fit of labor force participation in more detail in Table 9 by breaking up labor force participation by fertility status, marital status, and proximity to parents. Qualitatively, the model can reproduce patterns of lower participation rates for mothers of young children and unmarried women. Across all women, the profile of labor force participation the model outputs over different locations and fertility statuses is reasonable. However, the model does understate participation for pregnant women as a whole.

[TABLE 9 ABOUT HERE]

Next, we assess the model's fit of migration decisions by breaking down moves according to sending location, destination, and fertility status in Table 10. Since the PSID can have very small samples of movers and migration rates have wide confidence intervals, we supplement the table with statistics from the ACS sample used in Section 2 as well. Among all women and women without children, the model predicts rates of migration both out and into the parent location that are similar, albeit slightly lower, to those observed in the data, especially the ACS. Moreover, the model is able to match the pattern observed in the ACS of pregnant women moving back to their parents' location more frequently, though it also overpredicts the rate at which women with young children engage in such moves. We also evaluate the model's fit of fertility profiles in Figure 5. The model's fit of lifecycle fertility profiles by both race and education is excellent.

[TABLE 10 ABOUT HERE]

[FIGURE 5 ABOUT HERE]

An analysis of the frequency of repeat movers in our simulation also allows us to speak to the importance of allowing for dynamics when modeling migration decisions. While the probability of a move in any given year is small, the proportion of individuals who move at least twice in our estimation sample is non-trivial (8.26 percent in the data vs. 8.23 percent in the simulation). Moreover, this measure is severely understated due to our often ceasing to observe agents directly in our estimation sample by their late 20s. When we simulate all individuals through the entire life cycle, we observe that over 20% have moved multiple times by age 45. This indicates that agents will indeed re-optimize location decisions if allowed to do so, and a dynamic model is clearly needed to capture this richness of behavior.

Our incorporation of unobserved heterogeneity in wages also allows us to provide direct estimates of selection into migration, presented in Appendix Table A.12. When simulating unobserved types, we assign type by taking the likelihood-weighted probability of being each type, meaning that we allow the estimated likelihood to up-weight types that an observation's earnings, labor force participation, and migratory behavior predict is a more likely type. The estimates suggest that agents who move at least once are modestly positively selected relative to never-movers — however, women who *start* the model outside the grandparent location are considerably positively selected compared to the overall sample. This heterogeneity in initial conditions is an additional important factor that would be difficult to capture satisfactorily in a static, one-shot model. However, the fact that we do not explicitly model this initial "moving-away" decision means that our counterfactual policies cannot impact it. To the extent that childcare policies could encourage migration in this especially mobile point of the life cycle, either for education or other early-career considerations, this likely results in our estimated policy impacts being more conservative in nature.

Finally, we evaluate the frequency of grandparent childcare usage observed in our simulated data and compare it to external statistics. Specifically, we compare our informal care usage rates to the share of parents who use relative care as their main form of childcare the 2001 Early Childhood Longitudinal Study, as reported by Berlinski et al. (2023). The authors report that 18 and 23 percent of married and single parents, respectively, use relatives as their main source of childcare. We measure informal care usage in our simulated data as the share of women with young children who 1) live in the same location as their own parents, 2) are of the "helpful" grandparent type, and 3) choose to supply labor. Using this metric, we estimate informal care usage rates for married and single women of 20 and 28 percent, respectively, which compares well to Berlinski et al. (2023), particularly given that our measure doesn't explicitly observe grandparent informal childcare and that our PSID sample over represents low-income individuals who are more likely to rely on informal care in general. We also note that our sample uses later birth cohorts of children than Berlinski et al. (2023), and as formal childcare prices have increased over time, so too has informal care usage likely become more frequent.

6 Counterfactual Analyses

Having evaluated our model's performance, we now turn to comparative statics exercises.

6.1 The Role of Grandparents

We begin by evaluating the role of grandparents in wage formation, fertility, and migration by setting grandparent time transfers $\tau^{P,m}$ to zero. Conceptually, the impact of the presence of grandparents on wages is ambiguous, since residing with them may increase labor supply and experience in the short run but may also impact wages negatively by discouraging moving to higher-paying locations.

Table 11 presents the results of this exercise in Panel A. For this counterfactual as well as for those upcoming, we estimate impacts in terms of average effects on lifetime real wages, years of experience, fertility, number of lifetime moves, and time spent in the grandparent location. We also calculate willingness-to-pay metrics by taking the difference in ex-ante utility (that is, utility at the start of the model) and ex-post utility (that is, total utility over the simulated lifecycle, weighting each age equally) resulting from the counterfactual scenario and dividing it by α_1, the utility scaling parameter for consumption for women without children. The ex-ante measure is useful in assessing how better-off women in the model expect the policies to make them, which may be informative for willingness-to-pay metrics. The ex-post measure, in contrast, allows us to more completely evaluate the lifetime impact of the policies while fully incorporating their interactions with the various wage and preference shocks throughout the model.

[TABLE 11 ABOUT HERE]

We conduct demographic heterogeneity analyses by assessing impacts separately for women with an informal childcare option and also conduct splits by race, education, and whether women start the model in the grandparent location ℓ^P. Finally, we evaluate the importance of geography in influencing the value of grandparents in Panel B of the table, where we compute the impacts of the removal of grandparents in an alternate world where the grandparent time transfer is not conditional on location — that is, agents with an informal care option receive the time transfer regardless of where they are, which allows us to understand how much the location lock-in caused by grandparents dampens their benefits. Wages and WTP measures are reported in thousands of dollars.

The removal of grandparents substantially reduces fertility, years in the labor force, and wages, with women who have an informal care option working nearly 1.3 fewer years and earning over $34,000 less over their lifecycle. Across the entire sample, the existence of grandparents as a potential source of childcare is associated with half a year of experience (+ 2.2% off baseline). Despite Black women having more access to informal care according to our estimates, we find that the removal of informal care does *less* to decrease their earnings, mainly due to having lower wages on average than their white counterparts. However, the ex-ante and ex-post utility of Black women falls by a greater amount due to the higher prevalence of informal care in this subgroup. Looking at heterogeneity by education, we find that college graduates exhibit larger responses than high school graduates to the removal of grandparents in terms of migration and fertility behavior — as a result, high school graduates bear the negative impacts of the counterfactual more directly and experience greater losses in utility.

Additionally, these results demonstrate that parents' mobility is not only influenced by grandparents but also by regional costs for childcare. Eliminating the pull of the parent location results in increased migration for the same groups of women who see the largest declines in earnings — namely, those who had informal childcare available in the baseline model — suggesting that they are substituting from staying in their home location towards either higher paying or lower childcare cost locations when they can no longer take advantage of free relative care in their parent location. For women who have an informal care option in the baseline simulation, the removal of said option increases the total number of lifecycle moves by 0.05, or roughly 4 percent, while decreasing the length of time spent in the grandparent location by approximately 2/3 of a year on average.

To provide an alternate measure of the importance of geography in the model, we next consider the impacts of removing grandparents if their childcare assistance was not tied to a specific location — that is, agents with an informal childcare option receive the time transfer regardless of where they are. The impacts of removing grandparents from this counterfactual world are presented in Panel B of Table 11, and the difference in impacts between Panels A and B provide a measure of how much *more* valuable grandparents would be if their childcare assistance was unconditional on location. Realized utility gains from grandparents are larger by $1,280 without location lock-in, suggesting that geographic frictions caused by the use of informal childcare reduce utility by approximately 20%. Earnings shift by comparable quantities. Moreover, relaxing the geographic constraint is valuable even for women who start the model in the grandparent location and would be unaffected if they do not move: for such women, eliminating the grandparent geographic constraint increases the ex-ante WTP for grandparents by over 10 percent.

6.2 Childcare Subsidies

While Table 11 presented the impacts of removing informal care in the model, Table 12 presents the impacts of instead removing childcare *costs*. Though the introduction of such a policy would clearly have general equilibrium implications on the labor market, we evaluate the policy in partial equilibrium and analyze how it may impact the wages, work, and welfare of an individual woman in the model, which in turn may be informative for the potential impacts of more policy-relevant targeted reductions in childcare costs. We begin by discussing the results in Panel A, which report the impacts of removing childcare costs from the model entirely (the "national subsidy"), before comparing these impacts to those in Panel B, which correspond to a policy that removes costs *only* in the location of the agents' parents (the "local subsidy"). These two exercises allow us to decompose what portion of the value of the policy comes from reducing the migration frictions created by the need for grandparent care. The second exercise provides the same full subsidization of childcare costs as the national subsidy but does not remove the labor market lock-in effect.

[TABLE 12 ABOUT HERE]

For all subgroups, this policy increases years of experience, labor mobility, fertility, and lifetime wages. Fully subsidizing childcare increases the lifetime earnings of women by about $47,290 (6.5% off baseline). For comparison, in the reduced form estimates, we saw that the child penalty for women living in low childcare cost states was about 10% lower than for women in high-cost states. Because Black women have more access to informal care than White women and face larger wage reductions from the presence of young children in their wage function, the childcare cost reductions do markedly less to encourage their labor force participation.

The fertility responses to the full subsidy suggest that women would have, roughly, an additional 0.65 children on average if childcare costs were removed entirely. Labor force participation and earnings increase despite these increases in childbearing. While not shown directly, we also evaluate fertility responses to other subsidies to demonstrate that our model can make predictions that are in line with other literature entries.[28]

Among all women in our sample, the complete removal of childcare costs raises lifetime moves by 0.01, or roughly 1 percent. While the effects of the policy on earnings and wages

[28] Specifically, we consider a specification in which we halve childcare costs, which is equivalent to a transfer of around $25,000 and increases fertility by 0.25 relative to the model baseline. This elasticity is similar to one found by Zhou (2022), who estimates that a $30,000 "baby bonus" would raise US fertility rates by about 0.15 points. We also compare our elasticity to Haan and Wrohlich (2011), who estimate a roughly 4.6% increase in fertility in response to a ∼$500 transfer. Our estimated elasticity for a similar transfer is 2%, which may be lower due to different country and time contexts.

are stronger for women who do *not* have helpful grandparents, since informal childcare does crowd out the labor force participation effects of the policies, the migration effects are largest for women who *do* have helpful grandparents, since it is these women for whom the geographic constraint induced by grandparent childcare applies. Among these women, the average time spent in the grandparent location decreases by close to one quarter of a year in response to the policy, with much smaller or even slightly positive effects for other subgroups, such as Blacks and women without informal care available. This happens because the national subsidy makes all locations substantially more affordable from a childcare perspective, including the home location for women without the informal care option. As a result, it is very possible that they would opt to spend more time there given the continued presence of the home preference parameter. Additionally, the most frequent home locations also tend to be relatively high-value in terms of amenities or real wages (see Table A.8; discussed more in Section 6.3), such as the South Atlantic, (especially for Blacks), East North Central, and West South Central divisions, which further draws certain natives to stay in them when the national policy makes doing so more affordable.

We study the welfare generated by this policy in the final columns of the table. Across all individuals, the average ex-ante willingness to pay for the full removal of childcare costs at age 22 is approximately $23,100.[29] An alternate measure is ex-post utility, or the change in lifetime utility, in dollars, induced by the policy. We find that the full childcare subsidy increases lifetime utility by approximately $52,700 — it is notable that this effect is larger than the impact on lifetime real wages, which arises from the policy enabling women to derive more non-pecuniary utility from making more preferred fertility and/or migration

[29] To put this in context of other policies, we can compare this to the willingness to pay calculated in Hendren and Sprung-Keyser (2020) for early child education programs. Across six different pre-school programs, the average WTP is approximately $45,000. Omitting Perry Preschool and Abcedarian, which have particularly large impacts on children's future earnings, the average WTP is approximately $10,500.

decisions. The base levels of WTP for the childcare policies is higher for women without an informal care option, since, as with wages, informal childcare can partially crowd out the benefit of the policies. Compared to the wage effects, the impacts on either ex-ante or ex-post utility are considerably larger than the impacts on utility relative to wages for the removal of grandparents in Table 11. This happens because

6.3 The Role of Geography

The quite modest impacts the childcare subsidy has on migration and years spent living in the parent location in Panel A of Table 12 may suggest that geography in our model is relatively unimportant in influencing the benefits of childcare policies. However, both of these figures are imperfect in assessing the importance of geographic constraints induced by the need for childcare: for instance, the national childcare subsidy can both increase moves out of the grandparent location while *decreasing* the number of moves back to it, leaving its effect on total lifetime migration ambiguous. Likewise, while it slackens the geographic constraint for women who had an informal childcare option, it may also make the parent location, ℓ^P, much more appealing for agents who do not have such an option, so the impacts on time spent in the parent location can also go in either direction.

To better understand the role of geography in our model in informing policy impacts, we compare the impacts of the policy that removes childcare costs entirely (the "national subsidy") to a policy that removes childcare costs *only* in ℓ^P (the "local subsidy"), effectively giving all agents maximally helpful grandparents. Conceptually, this policy is similar to the national subsidy in offering agents free childcare in their most common location, but it crucially differs in that it does not alleviate (and in fact may exacerbate) the geographic constraint induced by lower childcare costs being available only in a single place.

In a world with no migration and all agents starting in the grandparent location, the impacts of the national and local subsidy would obviously be identical. However, Panel B of Table 12 presents the impacts of the local subsidy and indicates that this clearly is not the case. Across the entire sample, the local subsidy increases lifetime wages by $39,250, which is roughly $8,000 (or 16%) less than the national subsidy. The impacts on ex-ante or ex-post willingness to pay are reduced comparably, and these differentials are also much larger for college-educated and white women.

Comparing these policies demonstrates how tying childcare provision to location creates geographic frictions which reduce women's earnings and welfare. The policies differ clearly in how they influence migration, with the local policy decreasing lifetime moves by 0.17 on average (17%) and leading to agents spending multiple additional years in the grandparent location. Notably, these migratory and welfare impacts also clearly manifest for women who begin the model in the grandparent location — for instance, moving from the local to the national subsidy increases the ex-ante WTP of women who start in ℓ^P from $21,600 to $23,880, or roughly 10%.

As a final analysis, we evaluate *where* in the model are the differences in impacts between local and national policies the most stark. In Table 13, separately for each possible grandparent location, we compute the average lifetime wage and utility impacts of the national and local childcare subsidies and then report the difference between the two. The national childcare subsidy tends to outperform the local subsidy in terms of utility gains *more* in home locations which are lower value, whether because they have low amenity values such as New England division, low real wages such as East South Central division, or both such as Pacific and Mid-Atlantic divisions.[30] This demonstrates that the value of relaxing geographic constraints varies systematically with location characteristics, which would be difficult to

[30]Refer to Table A.7 for divisional groupings and Table A.8 for division-level characteristics. Amenities are valued at between $400-$1700 per year, meaning that the value of moving between high and low amenity locales is similar to the value of moving between high and low wage locales.

demonstrate without our model's reasonably rich geographic structure.

[TABLE 13 ABOUT HERE]

7 Conclusion

This paper studies how childcare costs, the location of extended family, and fertility events influence both the labor force attachment and labor mobility of women in the United States. We document trends in the data that suggest that the need for informal childcare exerts considerable pull for U.S. women, especially those without a spouse. This manifests in women being substantially more likely to move back to their home location when faced with an imminent fertility event. These short-run behavioral margins are complemented by long-run reductions in the child earnings penalty for women who have nearby grandparents.

These analyses provide evidence consistent with past research that documents that the presence of grandparents and informal care meaningfully impacts the labor market outcomes of women in the United States. Our primary contribution comes from taking seriously the choice to locate nearby to grandparents in the first place. If women are sacrificing labor market opportunities by locating in places where they can use informal care, then policies that encourage substitution from informal to formal care need not be wage or welfare-neutral, as they may enable women to move to more productive or preferred locations.

We present a tractable model of dynamic labor force participation and migration that aims to capture the geographic constraints imposed by childcare costs and grandparent locations that U.S. women face. The presence of grandparents reduces childcare costs and thus reservation wages, allowing women to maintain their participation in the labor force and to continue building work experience that will subsequently raise wages for the remainder of their life. However, these benefits only apply if women are located in the same place as their grandparents, which is a notable constraint given the extent to which migration plays a role in wage growth (Kennan and Walker, 2011). We find that these geographic constraints meaningfully reduce the benefits that informal childcare offers relative to an unconditional time transfer, and policies that subsidize childcare costs have markedly different impacts on wages and welfare depending on both where they are implemented and whether they are implemented at the national or local level.

While these welfare estimates point to the importance of location choices in calculating the benefits of childcare subsidies, there are limitations to our model's ability to measure these benefits. First, the assumption that women only have one child at a time means that our model almost certainly understates childcare costs and the potential effects of childcare subsidy policies on labor force participation, experience, and wages. Second, the welfare calculations only consider the mother's utility; to the extent to which childcare subsidies allow grandparents more time for work or leisure as well, we are underestimating the societal benefits of these policies. Past research (Gørtz et al., 2025; Karademir et al., 2026; Frimmel et al., 2022) shows that grandmothers experience declines in their own earnings at the time of their first grandchild in part due to provision of informal care. Introducing childcare subsidies may therefore improve grandparents' well-being by allowing them to stay attached to the labor market and increase their lifetime earnings. While we show that the welfare gains of subsidized private care are highest for mothers who use less informal care (e.g., White mothers, married mothers), we might expect grandmothers who provide more informal care (e.g., Black grandmothers) also benefit from these policies in ways not measured by our model.

Third, while our model allows women to adjust to anticipated fertility through migration, we abstract away from other margins of adjustment that women may make in anticipation of decreased labor supply in the future, such as switching occupations to allow for more hours flexibility. While our child penalty estimates do not show significant differences in likelihood of part-time work post-birth by proximity to grandparents, we do see that those living far from grandparents are more likely to work part-time pre-birth than those nearby, suggesting that future work could explore how family proximity allows women to choose less flexible or full-time jobs. Finally, the discretization of geography into nine locations means that we may be suppressing the role that more granular geography plays in wages, which may also have implications for our counterfactual policy predictions. Extending the model to account for more locations or additional unobserved heterogeneity in wages such as location-specific match effects would increase the variation in potential earnings/welfare gains across locations, perhaps increasing the role that location choice plays in WTP for childcare.

Nonetheless, our model is able to match migration and labor supply behavior well and demonstrates that informal care needs substantively impact women's location choices and welfare. We estimate meaningful racial differences in rates of informal care use. For those women who do have access to informal care, the informal care option plays a large role in encouraging labor force participation and increasing lifetime earnings at the cost of constraining where these women choose to work. As a result, policies that reduce the cost of childcare substantially increase the labor mobility of these women, and a non-trivial component of the increase in earnings enjoyed by them following the policies comes from their being able to locate to labor markets with higher real wages. Our findings make clear that the non-pecuniary factors in location decisions confer considerable welfare benefits, and childcare subsidies reduce mobility frictions. As such, analyses that do not consider the welfare consequences of increased mobility will understate the benefits of childcare policies.

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Data Availability Statement

The public-use data that support the findings of this study are openly available in Harvard Dataverse at http://doi.org/10.7910/DVN/PNIO8H. Data from the PSID geocode are available from the University of Michigan. Restrictions apply to the availability of these data, which were used under license for this study and can be applied for at https://simba.isr.umich.edu/restricted

8 Tables

Table 1: Timing of First Pregnancy and Home Migration Probability (HMP)

	(1) All Mean = 3.57	(2) Non-Married Mean = 3.29	(3) Married Mean = 3.84	(4) White Mean = 3.70	(5) Black Mean = 3.24	(6) Other Races Mean = 3.52
First Pregnancy (FP)	1.179***	2.072***	0.783***	1.403***	2.051**	-0.313
	(0.186)	(0.426)	(0.203)	(0.216)	(0.666)	(0.405)
Observations	933,489	441,608	491,881	707,994	94,990	105,107
R-squared	0.051	0.049	0.061	0.058	0.079	0.125

Notes: The sample includes US-native-born women aged 22-40 in the 2006-2019 ACS who completed at least one year of high school and were not located in birth state or birth state of their husband if married the previous year. Additional controls include fixed effects for birth state, current state, and previous state, and calendar year, a quadratic in age, indicators for education (HS drop out (omitted), HS, Some College, College, More than College), indicator for marital status, and indicators for race (White (omitted), Black, Non-White/Black). First pregnancy indicator defined by presence of a child less than one year old while being the only own child of the respondent in the household. Regressions weighted by sampling weights and robust standard errors in parentheses.$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Migration Patterns Across Stages of Parenthood

	Num. of Moves	% Non-Mover	% Move Once	% > 1 Move	% Move Home	% of Years in Home
Never Mother, Lifetime	1.12	0.552	0.143	0.305	0.234	0.550
Ever Mother, Lifetime	0.85	0.640	0.123	0.237	0.187	0.571
Pre-birth	0.21	0.851	0.102	0.047	0.045	0.690
Pregnant/Young Child	0.24	0.829	0.119	0.052	0.076	0.708
Elementary	0.19	0.863	0.090	0.047	0.051	0.651
Teenage	0.11	0.911	0.072	0.017	0.025	0.583
Young Adult	0.08	0.931	0.062	0.007	0.016	0.517

Note. This table reports the average migration statistics for never-mothers and ever-mothers, calculated for a sample of women in the PSID observed at both age 25 and 50. Migration is defined as having switched states in year t relative to t-1. We report statistics for mothers separately based on periods surrounding first birth including pre-birth (2-7 years pre birth), pregnancy/young child (1 year prior to 4 years post-birth), elementary-aged (5-10 years post-birth), teen-aged (11-16 years post-birth) and young adulthood (17-23 years post-birth). Home is defined as the state that the PSID respondent's parent is living in.

Table 3: Characteristics of Women in Year Following First Birth

	Non-Mover, Home	Mover, Home	Non-Home	All
Age	28.56	27.91	29.72	28.83
	(4.376)	(4.485)	(4.744)	(4.506)
White	0.748	0.815	0.768	0.755
	(0.434)	(0.389)	(0.422)	(0.430)
Black	0.104	0.0895	0.0905	0.100
	(0.305)	(0.286)	(0.287)	(0.300)
Married	0.739	0.749	0.756	0.744
	(0.439)	(0.434)	(0.430)	(0.437)
College	0.307	0.293	0.328	0.312
	(0.461)	(0.455)	(0.469)	(0.463)
Currently Employed	0.743	0.553	0.698	0.725
	(0.437)	(0.497)	(0.459)	(0.447)
Worked Full-Time, Prev. Year	0.563	0.494	0.568	0.562
	(0.496)	(0.500)	(0.495)	(0.496)
Family Income, Prev. Year	88820.8	92027.5	102768.8	92301.4
	(76973.5)	(84769.2)	(96817.3)	(82848.8)
Own Income, Prev. Year	32061.5	26716.5	37781.8	33314.9
	(35939.8)	(38669.4)	(46545.9)	(39036.3)
Observations	67,930	1,504	24,153	94,543

Notes: This table reports mean and s.d. (in parentheses) of demographic characteristics of US-native women aged 22-40 in the 2006-2019 ACS who completed at least one year of high school and have a child less than one year old which is the only own child of the respondent in the household. Col. 1 includes women living in their birth state in the prior and current year. Col. 2 includes women living in a non-birth state in the prior year and in their birth state in the current year. Col. 3 includes women living in a non-birth state in the prior and current year. Col. 4 includes all women in the sample.

Table 4: Aggregated Child Penalty, by Distance to Grandparent

Panel A: Male Control Group	Same County	Diff. County	Diff. County & > 25 mi.	Diff. State	Diff. State & > 25 mi.	1-25 mi.	26-50 mi.	> 50 mi.
Mean Baseline	24219.6	26360.7	26015.7	27755.1	27506	30715.5	24737.1	26382.2
Post-Period	-13521.3***	-20589.5***	-20666.3***	-27354.3***	-27546.7***	-19682.0**	-10698.0***	-23428.6***
	(1684.7)	(1759.5)	(1844.6)	(2987.2)	(3014.0)	(5885.2)	(2919.3)	(2164.3)
Num. Households	727	1024	922	409	398	102	146	776
HH-Year Obs.	9014	11312	10104	4751	4623	1208	1800	8304
Panel B: Women Only, No Control Group	**Same County**	**Diff. County**	**Diff. County & > 25 mi.**	**Diff. State**	**Diff. State & > 25 mi.**	**1-25 mi.**	**26-50 mi.**	**> 50 mi.**
Mean Baseline	21299.3	21834.3	21419.6	21884.2	21751.9	26642.4	22609.3	21066.7
Post-Period	-7759.0***	-12410.1***	-11900.6***	-11793.7***	-11671.9***	-15153.1***	-12797.0***	-11561.6***
	(1190.6)	(1195.4)	(1249.2)	(1658.1)	(1664.4)	(3353.5)	(2143.6)	(1380.4)
Num. Households	723	1008	907	400	389	101	146	761
HH-Year Obs.	4925	5994	5360	2496	2434	634	941	4419

Note. This table reports the coefficients of a regression of earnings measures on indicators for years surrounding a woman's first birth, collapsed into the pre-period (3 to 5 years pre-birth),year prior to birth, year of the birth, and post-period (1 to 10 years post-birth). Two years prior to birth is omitted. Controls for year of survey, county fixed effects and age of mother are also included. Panel A is a specification which uses men as a control group; panel B only includes women. Distance is measured as the distance between the population centroid of the focal woman's county and the population centroid of the grandparent's county. Standard errors clustered at the individual level in parentheses.$^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Aggregated Child Penalty, by Childcare Costs

	No Control			Male Control		
	> 75th Pct.	<75th Pct.	All	> 75th Pct.	< 75th Pct.	All
Baseline Mean	22837.4	16937.8	20393.3	28350.7	21385.6	25465.5
Post-Period	-12700.2***	-7171.8***	-1418.6	-19745.7***	-11170.8***	892.7
	(1074.6)	(934.4)	(1604.9)	(1607)	(1615.5)	(3443.5)
Post X CC Price (10s)			-496.5***			-970.5***
			(86.07)			(194.4)
Num. Households	1234	720	1708	1270	2269	1708
Num. HH-Year	7071	3943	10663	13137	7231	19706

Note. This table reports the coefficients of a regression of earnings on indicators for years surrounding a woman's first birth, collapsed into the pre-period (3 to 5 years pre-birth), year of the birth, year prior to birth, and post-period (1 to 10 years post-birth). The year two years prior to birth is omitted. Col. 1-3 include do not include men as a control group; Col. 4-6 do. Controls for year of survey, age of mother, and county are also included. Columns 1 and 4 are for women living in counties with center-based childcare prices above the 75th percentile of the national distribution. Columns 2 and 5 are for women living in counties below the 75th percentile. Columns 3 and 6 are for the full sample and interact the event study indicators with the average weekly childcare price (unit= \$10). Standard errors clustered at the individual level in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Model Notation

Description		Values	Description		Values
Locations	ℓ	$\ell^P, \{1, ..., 9\}$	Years of Experience	x	[0,40]
Location Daycare Cost Types	δ	$\delta^\ell, \ell \in \{1, ..., 9\}$	Marital Status	m	0,1
Location Wage Effects	η	$\eta^\ell, \ell \in \{1, ..., 9\}$	Fertility Status	f	0,1
Location Costs of Living	κ	$\kappa^\ell, \ell \in \{1, ..., 9\}$	Wage Effects	μ, μ_S	Various
College Attainment	e, e_S	0,1	Previous LFP Status	p	0,1
Age	a	[22,65]	Hours	h	0,1
Age of Youngest Child	a_c	$\emptyset$, [0,4]	Time Transfers; Spouse/Parents	τ	τ^S, $\tau^{P,m}$

Note. This table presents model notation. Column 1 and 3 list the description of model components; column 2 and 4 list the symbolic representations of the components; column 5 and 6 list the potential values the variables can take.

Table 7: Parameters Estimated Outside the Model

Parameter		Value
Discount rate	β	0.95
Location populations	N^{ℓ}	Various
Spouse wage: Constant	$\beta_{S,0}$	2.234
Spouse wage: College	$\beta_{S,1}$	0.571
Spouse wage: Age	$\beta_{S,2}$	0.047
Spouse wage: Age Sq.	$\beta_{S,3}$	-0.007
Spouse wage, fixed effects	μ_S^L, μ_S^H	-0.39, 0.39

Note. This table reports values of parameters that are estimated outside the model. Columns 1 and 2 describe the parameters and present their symbolic representation. Column 3 reports parameter values. Census region population levels are set to match the 2000 Decennial Census population estimates. Spouse wage coefficients are estimated by regressing log wages for husbands in the PSID estimation sample on indicators for college, age, age-squared, and an indicator for individual unobserved type. We assign unobserved type based on quantile of age and year residualized earnings within education group. See text for details on model and sample construction. See Table A.8 for division-level childcare costs, wage effects, and living costs.

Table 8: Parameters Estimated via Maximum Likelihood

Parameter		Value	S.E.	Value	S.E.	Value	S.E.
Fertility							
Fixed cost	θ_1	-0.624	0.717	-0.602	1.180	-0.331	1.258
Age effect	θ_2	-0.105	0.019	-0.118	0.035	-0.109	0.041
Marriage effect	θ_3	3.586	1.175	3.625	1.781	3.028	3.927
Age-marriage interaction	θ_4	-0.101	0.037	-0.090	0.056	-0.096	0.130
Education effect	θ_5	0.405	0.128	0.512	0.180	0.199	0.231
Fertility shock SD	σ_f	2.621	0.444	2.552	0.671	2.611	1.445
Utility							
Consumption, no children	α_1	0.090	0.009	0.090	0.020	0.092	0.007
Leisure, no children	α_2	0.938	0.104	0.935	0.231	0.938	0.078
LFP switch penalty	α_3	-0.074	0.007	-0.092	0.021	-0.052	0.004
Parent preference, no children	α_4	0.129	0.014	0.131	0.019	0.122	0.027
Consumption, with children	α_5	0.097	0.009	0.097	0.023	0.097	0.006
Leisure, with children	α_6	0.722	0.081	0.718	0.193	0.708	0.043
Parent preference, with children	α_7	0.194	0.028	0.175	0.035	0.215	0.050
Consumption/leisure complementarity	α_c	0.002	0.000	0.002	0.000	0.000	0.001
College leisure preference modifier	α_e	0.460	0.048	0.458	0.128	0.433	0.021
Experience leisure preference modifier	α_x	0.004	0.001	0.004	0.003	0.004	0.002
Amenity preference: distance to shore	$\alpha_{\Gamma,1}$	-0.001	0.003	-0.002	0.004	-0.004	0.007
Amenity preference: amenity index	$\alpha_{\Gamma,2}$	0.008	0.018	0.017	0.024	0.030	0.030
Amenity preference: warm days	$\alpha_{\Gamma,3}$	0.024	0.017	-0.013	0.023	0.100	0.030
Time Transfers							
Spouse time transfer	τ^S	0.057	0.045	0.025	0.064	0.039	0.081
Parent time transfer, unmarried	$\tau^{P,0}$	0.386	0.035	0.316	0.071	0.393	0.035
Parent time transfer, married	$\tau^{P,1}$	0.363	0.037	0.363	0.045	0.245	0.082
Probability of $\tau^P = 0$	P_τ	0.573	0.059	0.643	0.064	0.509	0.093
Wages							
Wage intercept	β_0	1.870	0.019	1.870	0.025	1.863	0.026
College effect	β_1	0.483	0.014	0.474	0.023	0.467	0.020
Experience effect, linear	β_2	0.068	0.003	0.068	0.003	0.068	0.001
Experience effect, quadratic	β_3	-0.002	0.000	-0.002	0.000	-0.002	0.000
Child aged 0-1	β_4	-0.093	0.012	-0.074	0.014	-0.100	0.016
Child aged 2-4	β_5	-0.039	0.010	-0.020	0.014	-0.057	0.016
Wage shock SD	σ_ε	0.193	0.007	0.197	0.008	0.167	0.013
Wage measurement error	σ_ξ	0.282	0.004	0.289	0.004	0.264	0.007
Moving Costs							
Fixed cost	γ_0	4.732	0.224	4.756	0.282	4.833	0.418
College effect	γ_1	0.330	0.116	0.319	0.145	0.298	0.221
Child effect	γ_2	-0.061	0.123	-0.136	0.157	0.034	0.234
Marriage effect	γ_3	0.451	0.126	0.578	0.158	0.270	0.284
Population effect	γ_4	-0.204	0.052	-0.215	0.064	-0.222	0.097
Age effect	γ_5	0.045	0.014	0.034	0.021	0.052	0.028
Sample		All		Whites		Blacks	
N		10,122		6,108		4,014	
Individuals		932		549		383	
Log Likelihood		-10,922		-6,538		-4,288	

Note. This table presents estimates and standard errors of parameters estimated via maximum likelihood (see equation B.3 for detail of likelihood function). The model is estimated on a sample of non-Hispanic White and Black women aged 22-40 in the 2001-2019 waves; number of women-year observations used indicated by 'N' and number of individual women included indicated by 'Individuals'. Column 1 and 2 report results for the full estimation sample; Column 3 and 4 for the White estimation sample; Column 5 and 6 for the Black estimation sample.

Table 9: Model Fit: LFP by Location, Marital Status, and Fertility

Panel A: Data

	Overall			In Parent Location			Not In Parent Location		
Marital Status	No Kids	Pregnant	Kids	No Kids	Pregnant	Kids	No Kids	Pregnant	Kids
All	0.627	0.681	0.406	0.617	0.676	0.422	0.663	0.696	0.339
$m = 0$	0.647	0.655	0.453	0.630	0.635	0.458	0.711	0.773	0.416
$m = 1$	0.593	0.696	0.372	0.594	0.707	0.391	0.588	0.671	0.314
Observations	5140	385	4597	4017	293	3726	1123	92	871

Panel B: Model

	Overall			In Parent Location			Not In Parent Location		
Marital Status	No Kids	Pregnant	Kids	No Kids	Pregnant	Kids	No Kids	Pregnant	Kids
All	0.640	0.558	0.413	0.634	0.569	0.440	0.661	0.515	0.285
$m = 0$	0.661	0.596	0.453	0.652	0.596	0.469	0.696	0.593	0.336
$m = 1$	0.610	0.529	0.381	0.607	0.546	0.413	0.619	0.475	0.261
Observations	47580	3874	41096	37200	3095	34149	10380	779	6947

Notes: This table reports labor force participation in the PSID estimation sample data (Panel A) and in the data simulated form the model (Panel B). Row 1 in each panel reports moments for the full sample, row 2 for unmarried women, and row 3 for married women. Columns report moments separately by presence of children and by whether the household is in the same state as the grandparent household. 'Pregnant' corresponds to woman being pregnant with their first child and 'Kids' corresponds to having at least one child between 0-5 in the household. See text for details on estimation sample construction.

Table 10: Model Fit: Migration by Fertility

Panel A: Data

Direction	All	No Kids	Pregnant	Kids
ℓ^p Out-Migration Rate	1.88	2.28	2.72	1.43
ℓ^p In-Migration Rate	4.77	4.58	6.58	4.82

Panel B: Out of Sample (ACS)

Direction	All	No Kids	Pregnant	Kids
ℓ^p Out-Migration Rate	1.80	2.03	1.64	1.31
ℓ^p In-Migration Rate	4.04	4.15	4.52	3.45

Panel C: Model

Direction	All	No Kids	Pregnant	Kids
ℓ^p Out-Migration Rate	1.55	1.66	0.90	1.75
ℓ^p In-Migration Rate	3.24	2.89	3.82	3.71

Note. This table reports annual migration rates out of parent's location (ℓ^P) and into the parent's location in the estimation sample data (Panel A), in the ACS sample of 22-40 year White and Black women (Panel B), and in the data simulated from the model (Panel C). We report migration separately by presence of children where 'Pregnant' corresponds to woman being pregnant with their first child and 'Kids' corresponds to having at least one child between 0-5 in the household.. See text for details on sample construction.

Table 11: Impacts of Removal of Informal Care

Panel A: Relative to Baseline World

Sample	Wages	Years x	# Moves	Years in ℓ^P	Fert. f	E-A WTP	E-P WTP
Baseline levels	738.7	23.95	1.08	32.38	1.22	918.0	3075
All	-13.99	-0.54	0.02	-0.26	-0.05	-1.57	-5.81
$\tau^P \neq 0$	-34.20	-1.32	0.05	-0.63	-0.11	-3.83	-14.21
Whites	-16.07	-0.54	0.02	-0.26	-0.04	-1.21	-5.69
Blacks	-11.00	-0.54	0.02	-0.25	-0.05	-2.08	-5.98
High-School	-12.66	-0.58	0.02	-0.21	-0.04	-2.11	-5.92
College	-16.94	-0.46	0.03	-0.36	-0.06	-0.35	-5.56
Start in ℓ^P	-16.17	-0.63	0.02	-0.22	-0.05	-1.84	-6.70

Panel B: Relative to Unconditional Transfer

Sample	Wages	Years x	# Moves	Years in ℓ^P	Fert. f	E-A WTP	E-P WTP
Baseline levels	738.7	23.95	1.08	32.38	1.22	918.0	3075
All	-17.58	-0.66	0.00	-0.04	-0.06	-1.97	-7.09
$\tau^P \neq 0$	-42.98	-1.62	0.01	-0.09	-0.15	-4.83	-17.34
Whites	-21.62	-0.71	0.00	-0.03	-0.06	-1.63	-7.50
Blacks	-11.77	-0.59	0.00	-0.05	-0.06	-2.46	-6.50
High-School	-14.58	-0.66	0.00	-0.03	-0.05	-2.63	-6.89
College	-24.25	-0.66	0.01	-0.06	-0.08	-0.50	-7.53
Start in ℓ^P	-17.51	-0.68	0.00	-0.03	-0.06	-2.09	-7.15

Note. The table presents impacts of removal of informal childcare in terms of mean change in lifetime real wages, mean change in years of experience, mean change in number of moves, mean change in years spent in grandparent location, and changes in ex-ante and ex-post utility. E-A indicates Ex-Ante Utility, calculated as utility for the household in the first period of life prior to making any choices, and E-P indicates Ex-Post Utility, calculated as realized utility after simulating out all decisions. We compute impacts relative to baseline simulated data (Panel A) and relative to a scenario where informal childcare is unconditional on agent location (Panel B). Wages and utility measured in thousands of dollars. See text for details on estimation sample and procedure.

Table 12: Impacts of Childcare Subsidies

Panel A: Impacts of a National Subsidy

Sample	Wages	Years x	# Moves	Years in ℓ^P	Fert. f	E-A WTP	E-P WTP
Baseline levels	738.7	23.95	1.08	32.38	1.22	918.0	3075
All	47.29	2.44	0.01	-0.01	0.65	23.10	52.70
$\tau^P \neq 0$	21.78	1.43	0.03	-0.27	0.70	21.37	42.52
$\tau^P = 0$	64.94	3.13	0.00	0.17	0.70	24.30	59.74
Whites	69.75	3.03	0.03	-0.24	0.68	23.06	60.36
Blacks	15.10	1.59	-0.02	0.32	0.59	23.16	41.71
High-School	33.3	2.32	0.00	0.05	0.60	29.72	49.57
College	78.42	2.69	0.03	-0.15	0.74	8.39	59.67
Start in ℓ^P	41.97	2.35	0.01	-0.03	0.64	23.88	51.59

Panel B: Impacts of a Local Subsidy

Sample	Wages	Years x	# Moves	Years in ℓ^P	Fert. f	E-A WTP	E-P WTP
Baseline levels	738.7	23.95	1.08	32.38	1.22	918.0	3075
All	39.25	2.09	-0.17	2.49	0.54	18.65	43.83
$\tau^P \neq 0$	15.24	1.13	-0.14	2.00	0.48	17.06	34.49
$\tau^P = 0$	55.85	2.76	-0.20	2.83	0.59	19.74	50.29
Whites	56.7	2.51	-0.19	2.76	0.55	17.58	47.21
Blacks	14.23	1.49	-0.15	2.10	0.53	20.18	38.99
High School	29.88	2.08	-0.15	2.18	0.53	24.33	43.79
College	60.09	2.11	-0.22	3.17	0.58	5.99	43.91
Start in ℓ^P	40.75	2.26	-0.17	1.88	0.59	21.60	48.32

Notes. The table presents impacts of removal of childcare costs at the national (all locations) in Panel A or local (only in ℓ^P) level in Panel B. We measure impacts in terms of mean change in lifetime real wages, mean change in years of experience, mean change in number of moves, mean change in years spent in grandparent location, and changes in ex-ante and ex-post utility.E-A indicates Ex-Ante Utility, calculated as utility for the household in the first period of life prior to making any choices, and E-P indicates Ex-Post Utility, calculated as realized utility after simulating out all decisions. Wages and utility measured in thousands of dollars. See text for details on estimation sample and procedure.

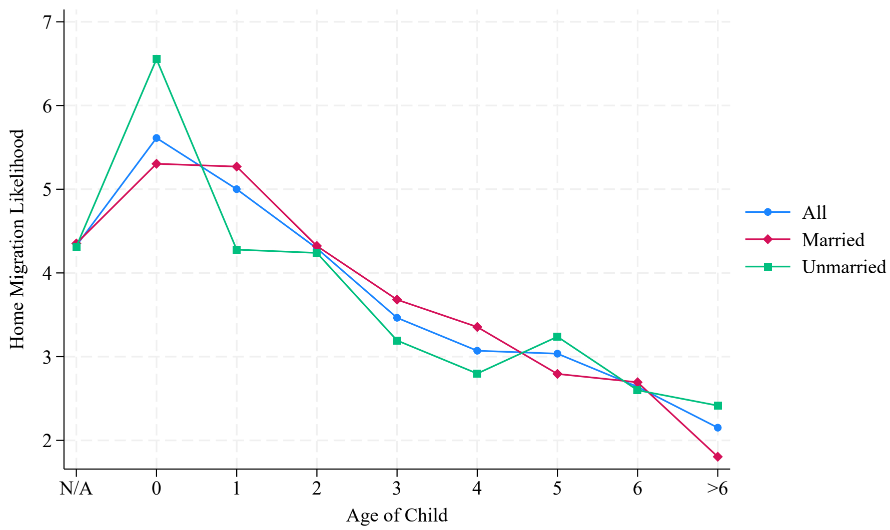
Table 13: Geographic Heterogeneity in Effects of Childcare Subsidies

	Impacts on Wages			Impacts on Ex-Post WTP		
Grandparent Location ℓ^P	National	Local	Δ	National	Local	Δ
New England	23.70	23.29	0.41	65.68	50.18	15.50
Mid-Atlantic	56.93	33.57	23.36	53.69	33.01	20.68
East North Central	35.29	33.45	1.84	51.89	47.57	4.32
West North Central	15.68	15.35	0.33	54.37	49.16	5.21
South Atlantic	26.57	22.59	3.98	47.07	40.37	6.70
East South Central	56.73	40.85	15.88	46.13	36.68	9.45
West South Central	69.12	65.73	3.39	55.14	51.31	3.83
Mountain	113.72	92.98	20.74	67.41	55.11	12.30
Pacific	77.85	60.13	17.72	58.43	39.36	19.07

Notes: This table presents impacts of national and local childcare subsidy on the change in lifetime wages and change in ex-post utility separately by location of agent's parent ℓ^P. Wages and utility measured in thousands of dollars. See text for details of model estimation, Table A.7 for Divisional groupings, and Table A.8 for Division-level characteristics.

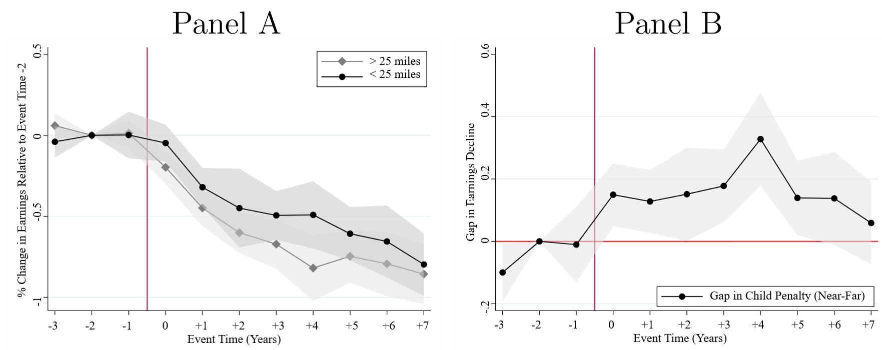
9 Figures

Figure 1: Home Migration Rates by Age of Child



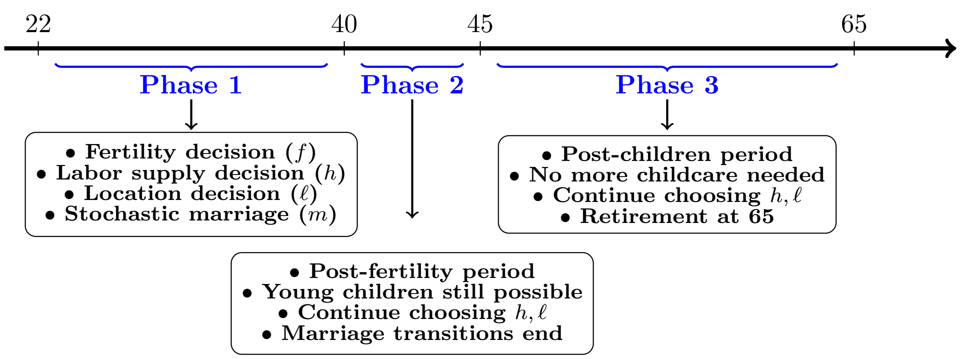
Notes: This figure plots the annual home-migration rate (scale: 0-100), calculated as the proportion of women living outside their location of birth in the year prior to the survey who are living in the birth location in the year of the survey, separately by age of youngest child and marital status. This This is estimated on US-native women aged 22-40 in the 2006-2019 ACS who completed at least one year of high school, have one or fewer children, and were not located in their or their husband's birth state the previous year. N/A indicates that observation in data has no children in household.

Figure 2: Child Penalty for Women Living Near or Far from Grandparents



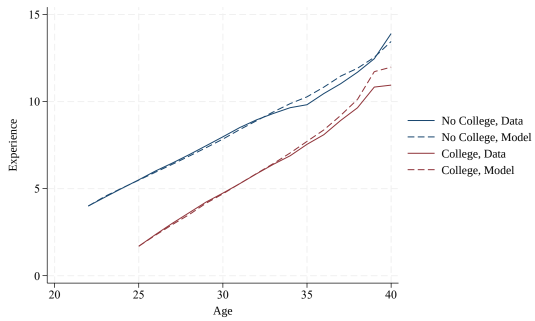
Notes: Figure 2A (left) plots coefficients and 95% confidence intervals from event studies of earnings on indicators for years surrounding a woman's first birth for both women who live less than 25 miles from their grandparent's counties (near) or more than 25 miles (far). The units are percent changes (0 to 1) in earnings relative to two years prior to birth. The regression includes men as a control group and includes controls for age of mother at first birth, year of birth, and county time trends. Figure 2B (right) calculates the gap for those near vs. far and reports 90% confidence intervals for a test of the null that this gap is equal to zero.

Figure 3: Model Timing

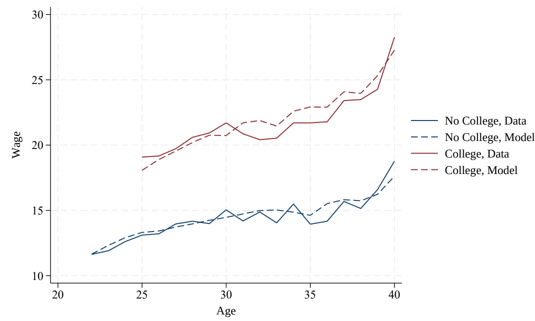


Notes: This figure presents the timing of phases of model and summarizes decisions made in each period. A period in the model is one year.

Figure 4: Model Fit: Labor Force Lifecycle Profiles



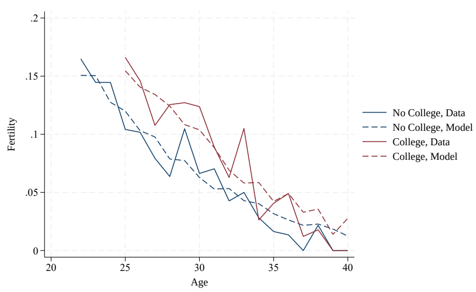
(a) Experience



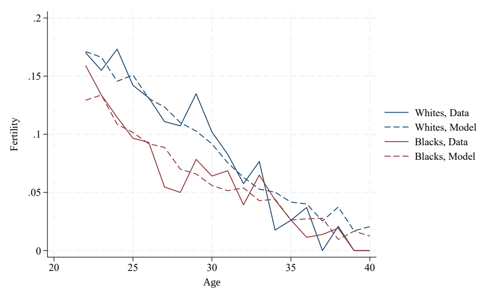
(b) Hourly Wage

Notes: This figure compares model fit for life-cycle trends of experience and annual wages (in $1000) for women with and without a college degree in PSID estimation sample (solid line) and data simulated from model (dotted lines). Fit reported for all ages for women with a high school degree and ages 25-onward for women with a college degree. See text for details on sample construction.

Figure 5: Model Fit — Fertility Life-Cycle Profiles



(a) By Education



(b) By Race

Notes: This figure presents model fit of fertility rates over the lifecycle for women in PSID analysis sample (solid line) and in data simulated from model (dotted line) separately by women's education level (panel A) and by race (panel B). Fertility is defined as proportion of women pregnant at each age. Fit reported for all ages for women with a high school degree and ages 25-onward for women with a college degree. See text for details on sample construction.

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