Childhood Place and Motherhood Penalties*

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Abstract

Motherhood penalties vary widely across places. Do these differences reflect the causal effect of the environment in which women grow up, or the sorting of families across places? Using Danish administrative data, we compare women who moved at different ages to municipalities with different motherhood penalties. Each additional year of childhood exposure to a municipality with a one-percentage-point larger employment penalty raises a woman's own penalty by 0.034 percentage points; we find no such effect for men. Childhood place also shapes the characteristics women bring into motherhood, such as education and partner traits, but these account for less than 40% of the exposure effect. Exploiting the staggered expansion of childcare that followed Denmark's 1964 move toward universal provision, we show that greater expansion raised the labor supply of mothers at the time and, decades later, reduced the motherhood penalties of the women who grew up exposed to it, with no effect on educational attainment. Childhood environment thus causally shapes women's labor market response to motherhood, and policy-induced changes to it can have lasting effects across generations.

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1 Introduction

Motherhood is the largest driver of the remaining gender gap in earnings in high-income economies: women's employment and earnings fall sharply after first childbirth, while men's labor market trajectories are largely unaffected (Bertrand et al., 2010; Fitzenberger et al., 2013; Angelov et al., 2016; Adda et al., 2017; Goldin and Mitchell, 2017; Chung et al., 2017; Goldin et al., 2017; Kleven et al., 2019a,b; Cortés and Pan, 2020). Understanding what determines the size of motherhood penalties is therefore central to understanding gender inequality. Existing work has emphasized women's human capital, occupational choices, and earnings potential (Goldin, 2014; Goldin and Katz, 2016; Goldin, 2021; Andresen and Nix, 2022), as well as family background and the intergenerational transmission of female labor supply and gender roles (Fernández et al., 2004; Fernández and Fogli, 2009; Fernandez, 2007; Fogli et al., 2007; Fogli and Veldkamp, 2011; Kleven et al., 2019b). In this paper, we ask whether childhood place, the municipality where a woman grows up, causally shapes her labor market response to motherhood.

Whether childhood place itself matters is difficult to establish because families sort across places. Kleven (2022) shows that women from high-penalty origin areas experience larger motherhood penalties after migration, documenting the persistence of geographic differences in motherhood penalties across generations and locations. Kleven et al. (2024) isolate a specific component of childhood exposure, showing that quasi-random exposure to peers with working mothers during adolescence reduces subsequent child penalties. Building on this evidence, we identify the causal effect of the broader residential environment in which women grow up. Using variation in age at residential move, we ask whether longer exposure to a municipality shifts women's later motherhood penalties toward those prevailing there. We then exploit a major childcare expansion to ask whether policy-induced changes in childhood environment can have lasting effects across generations.

We report three main findings. First, childhood place has a substantial causal effect on women's motherhood penalties. Each additional year of childhood exposure to a municipality with a one-percentage-point larger motherhood employment penalty increases a woman's own penalty by 0.034 percentage points; we find no corresponding effect for men. We find suggestive evidence that the exposure effect is asymmetric, with larger effects for moves to lower-penalty than to higher-penalty municipalities. Second, childhood place shapes several characteristics women bring into motherhood, including education and partner characteristics, but observable characteristics account for around 40% of the exposure effect. Third,

exploiting cross-municipality variation in childcare expansion following Denmark's 1964 transition toward universal childcare, we find that greater childcare expansion increased the labor supply of women who were mothers at the time of the reform and reduced the motherhood penalties of women who grew up exposed to this expansion decades later by approximately DKK 2,100 (about €280), equivalent to 1.8% of average pre-birth labor income, with no effect on educational attainment.

We use Danish administrative data covering 1975–2018. We begin by documenting substantial variation in motherhood employment penalties across municipalities and birth cohorts. High-penalty places have fewer college-educated women, lower pre-birth earnings, and female employment concentrated in retail, hospitality, and agriculture rather than business and finance, manufacturing, education, and healthcare. They also have lower labor supply among women of the previous generation. These patterns point to differences in both economic opportunities and exposure to female labor supply and gender roles, but cannot distinguish causal place effects from family sorting.

We separate these forces using the movers design of Chetty and Hendren (2018). We compare women who move at different ages from the same origin to destinations with different motherhood penalties. Women who move at ages 0–5 to a municipality with a 10-percentage-point larger motherhood penalty experience an employment penalty that is approximately 5.5 percentage points larger. This relationship declines steadily with age at move and is close to zero for moves at ages 16–20 and older. Formalizing the age gradient, we estimate an annual exposure effect of 0.034 percentage points for each percentage-point difference in motherhood penalties between destination and origin. Eighteen years of exposure to a municipality with a 10-percentage-point larger motherhood penalty imply a 6.1-percentage-point larger motherhood penalty in adulthood, a magnitude equivalent to approximately 61% of the destination-origin gap. The annual exposure effect for men is close to zero. Decomposing the outcome into employment before and after childbirth shows that the effect is concentrated after childbirth. We also find suggestive evidence that exposure effects are larger for moves to lower-penalty than to higher-penalty municipalities.

Identification requires that selection into destinations with different motherhood penalties be constant across ages at move. We provide several tests of this assumption. Among movers aged 21–30, who receive little or no childhood exposure to the destination, the age gradient is close to zero. The estimates are also stable when we control for changes in family circumstances around the move and predetermined family characteristics, and when we restrict attention to moves associated with large local displacement shocks. These

patterns provide support for the assumption that selection does not vary systematically with age at move.

We next examine characteristics that women bring into motherhood. Childhood exposure affects education, partner characteristics, and fertility choices. A predictive decomposition shows that observable intermediate outcomes account for around 40% of the exposure effect on motherhood penalties, leaving most of the effect unexplained by these observables.

In the second part of the paper, we ask whether policy-induced changes in childhood environment can have lasting effects on motherhood penalties. We exploit cross-municipality variation in childcare expansion following Denmark's 1964 transition toward universal childcare. Childcare availability increased substantially nationwide, but municipalities expanded at different rates. We trace the consequences across two generations. Among women who were already mothers when childcare expanded, greater expansion increased cumulative labor supply. Among girls who grew up exposed to greater childcare expansion, it reduced motherhood penalties decades later, without affecting pre-birth income or educational attainment. Placebo estimates for men and childless women in the adult generation show no corresponding labor-supply effects, reducing concerns that contemporaneous local labor-demand shocks drive these results.

These results provide complementary policy evidence on the role of childhood environment. While the movers design identifies the effect of childhood place as a whole, the childcare reform changes a specific component of that environment. Childcare expansion increased maternal labor supply in the adult generation and reduced motherhood penalties among girls exposed to the reform, while leaving their educational attainment unchanged. This pattern is consistent with intergenerational transmission through maternal labor supply and role models as a channel of this particular policy intervention. More broadly, the reform shows that policies that alter the environments in which girls grow up can have lasting effects on their labor market response to motherhood.

Our results contribute to three literatures. First, we contribute to the literature on the determinants of motherhood penalties, which has emphasized human capital and occupational choice (Goldin, 2014; Goldin and Katz, 2016; Goldin, 2021), biology (Kleven et al., 2021; Andresen and Nix, 2022), and family policy (Olivetti and Petrongolo, 2017; Kleven et al., 2020). Most closely related are Kleven (2022) and Kleven et al. (2024), discussed above. We contribute by identifying the causal effect of the broader childhood residential environment on women's motherhood penalties, separating childhood exposure from family sorting.

Second, we contribute to the literature on the role of culture, gender norms, and social

environments in shaping female labor supply (Fernández et al., 2004; Fernández and Fogli, 2009; Fernandez, 2007; Fogli et al., 2007; Fogli and Veldkamp, 2011; Olivetti et al., 2020; Meluzzi, 2024; Boelmann et al., 2025). This literature highlights the role of family background and exposure to social environments in shaping women's labor market choices. We contribute by showing that the broader residential environment during childhood has a lasting causal effect on women's employment response to motherhood, beyond the sorting of families across places.

Third, we contribute to the literature on the long-run effects of childcare (Berlinski et al., 2008, 2009; Gupta and Simonsen, 2010; Felfe et al., 2015; Cornelissen et al., 2018; Baker et al., 2019), including Havnes and Mogstad (2015) on long-run earnings effects. Most closely related, Sander (2026) shows that access to universal daycare in Denmark has persistent effects on mothers' labor supply and earnings over their working lives. We extend this literature by studying the effects of childcare expansion across generations on women's motherhood penalties. We find that greater childcare expansion increased maternal labor supply in the adult generation and reduced motherhood penalties among women who grew up in the affected environments, with no effect on their educational attainment.

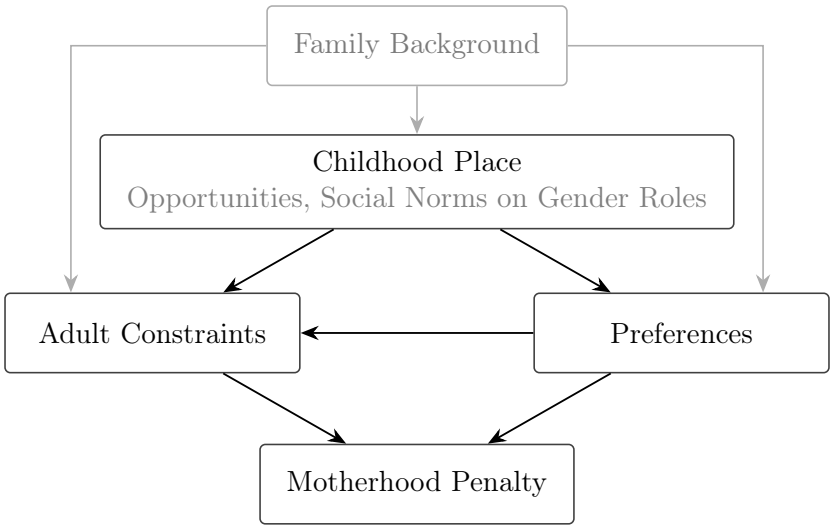
The rest of the paper is organized as follows. Section 2 presents the conceptual framework. Section 3 describes the data and institutional setting. Section 4 documents motherhood penalties across places. Section 5 presents the movers design and childhood exposure effects. Section 6 presents the childcare reform evidence. Section 7 concludes.

2 Conceptual Framework

Childhood place may shape women's later response to motherhood by influencing both the constraints they face when they become mothers and their preferences over work and family, as summarized in Figure 1. Growing up in different places exposes girls to different economic opportunities and prevailing gender norms. The opportunities available during childhood may influence women's educational and career choices, and thereby the constraints they face in adulthood. Education, occupational choice, and workplace characteristics are important determinants of motherhood penalties (Goldin, 2014; Goldin and Katz, 2016; Goldin, 2021). Exposure to female labor supply and gender roles may also shape girls' expectations and attitudes toward work and family (Fernández et al., 2004; Fernández and Fogli, 2009; Fernandez, 2007; Fogli and Veldkamp, 2011; Olivetti et al., 2020). Preferences may affect motherhood penalties both directly, by shaping women's responses to a given set of con-

straints, and indirectly, through educational, occupational, and partner choices that shape those constraints.

Figure 1: Conceptual Framework



Notes: The figure summarizes the conceptual framework. Childhood place may shape motherhood penalties by exposing girls to different economic opportunities and prevailing gender norms, which may influence their adult constraints and preferences over work and family. Observable adult constraints include education, industry, and partner characteristics. Preferences may affect motherhood penalties both directly, conditional on constraints, and indirectly, through choices that shape those constraints. Family background (in gray) affects residential sorting and may independently shape adult constraints and preferences.

Family background may influence both where children grow up and the adult constraints and preferences that shape their later labor market outcomes. Geographic differences in motherhood penalties may therefore reflect both causal effects of childhood place and the sorting of families across places. We separate these forces using a movers design that compares women who leave the same origin for destinations with different penalties at different ages. Families may still sort into destinations on unobserved characteristics, but such sorting generates an age gradient in the estimated coefficients only if it varies systematically with the child's age at move. Identification therefore requires the selection component to be constant across ages at move, an assumption we assess in Section 5.3.

The movers design identifies the overall effect of childhood place, but does not separately identify the channels through which place operates. The two are empirically difficult to separate: municipalities offering different economic opportunities to women may also differ in prevailing gender roles and female labor supply. We provide complementary evidence on these channels in two ways. First, we examine how childhood exposure affects observable dimensions of adult constraints—including education, industry, and partner characteristics—

and assess how much of the exposure effect is statistically accounted for by these observables.

Second, we exploit Denmark's 1964 childcare reform, which changes a specific component of the childhood environment rather than the environment as a whole. Childcare expansion may affect girls' future opportunities through investments in human capital and may also shape their preferences by changing the female labor supply and gender roles to which they are exposed while growing up. We therefore trace the reform across two generations: first, its effect on the labor supply of mothers in the adult generation; and second, its effect on the educational attainment and motherhood penalties of girls who grew up exposed to the expansion. Together, the two designs provide complementary evidence on how childhood environments shape motherhood penalties and how policies that alter these environments can have lasting effects across generations.

3 Data

We use individual data from Statistics Denmark covering 1975 to 2018. Every person in Denmark is assigned a unique personal identification number at birth, and we merge multiple registers built around this identifier: place of residence, income and employers, children and parents, and other demographics.

The identifier lets us link each Dane born from 1960 onward to their mother and father, and in turn to their own children, giving us three generations of family linkages together with background information such as gender and date of birth from 1986. We combine this with geographic data: individuals' zip codes from the 1970 Census, and a unique address identifier from 1975 onward in the registers, which let us identify residences and cohabitants and define place of residence at the region, municipality, parish, and zip-code level. We also observe each individual's birth parish: for those born before 1978, this is the parish of birth itself; for those born in 1978 or later, it is their mother's parish of residence in the year of birth.

To measure women's income, we use all taxable income from 1980 to 2018. The income registers are third-party reported, except for self-employment income, and provide yearly labor and capital income linked to each individual through their unique identifier. We define a person as working if their yearly labor income exceeds zero. Because income data are unavailable before 1980, we instead rely on ATP payments, mandatory pension contributions closely tied to labor force participation, which we observe cumulatively over 1965–1979 and use to construct accumulated labor supply over that period.

We complement these income measures with several job and education characteristics. Industry is measured annually using a 6-digit code for the workplace accounting for the largest share of an individual's income, which we aggregate into 9 and 27 industry groups; we also observe contracted hours annually, though for part-time workers we cannot distinguish part-time work for the full year from full-time work for part of the year. For education, we aggregate a four-digit attainment code into two tiers: at most high school, and college.

Sample Definition: Permanent Residents and Movers Our sample consists of women born in Denmark between 1960 and 1990 who had their first child between 1985 and 2013. The latter restriction lets us follow women for several years before and after first childbirth.

We split the sample into permanent residents and movers. Movers are women with children whose own mother moves to a new municipality while the child is aged 1–30, with the move occurring between 1975 and 2018, the period over which we observe residential address.[1] Permanent residents are women whose mother never changes municipality of residence over the same age range.

We drop the four smallest municipalities, each with fewer than 600 births among permanent residents, as the resulting estimates would be too noisy. The sample contains 396,407 permanent residents and 199,359 movers. Table A1 reports summary statistics for both groups around first childbirth.[2] Although our empirical strategy does not require permanent residents and movers to be similar, we find that they are: most variables are comparable, with the largest difference being a higher likelihood of living in the capital region among movers.

4 Motherhood Penalties across Places

We measure motherhood penalties using the change in women's employment following first childbirth. For woman i in the sample of permanent residents, we define

$$P_i^c = \frac{1}{6}\sum_{j=0}^{5} E_i^j - 1, \tag{1}$$

where E_i^j is an indicator equal to one if individual i has positive labor income in year j relative to first childbirth. The sample is restricted to women employed in both years before

[1] Following the childhood-exposure literature, we assign children's location and moves based on the residence of their parents (Chetty and Hendren, 2018; Britto et al., 2026; Brown et al., 2023).

[2] Measured in year -2 relative to childbirth, for all time-varying variables.

childbirth. Hence, P_i^c measures the average change in employment during the first six years after childbirth relative to pre-birth employment, with more negative values corresponding to larger motherhood penalties. We focus on extensive-margin employment rather than earnings to isolate the labor supply response to childbirth from differences in wages and wage growth across occupations and places.

For the movers analysis, we additionally define the unconditional employment penalty as $P_i = \frac{1}{6}\sum_{j=0}^{5} E_i^j - \frac{1}{2}\sum_{j=-2}^{-1} E_i^j$. We use P_i as the outcome in the movers analysis, while using P_i^c to characterize municipality-level motherhood penalties. Conditioning on pre-birth employment makes municipality-level penalties comparable across places with different baseline employment rates. Figure A10 shows that the main results are similar when using the unconditional measure to characterize places.

Figure A1 shows employment around first childbirth for women and men. Consistent with the child penalty literature (Kleven et al., 2019a), women's employment falls sharply after childbirth, by approximately 10 percentage points after five years, while men's employment falls by about 5 percentage points.

4.1 The Geography of Penalties

We study geographical variation in motherhood penalties by computing the average penalty P_{ms}^c for permanent-resident women in each municipality m and 10-year birth cohort s:

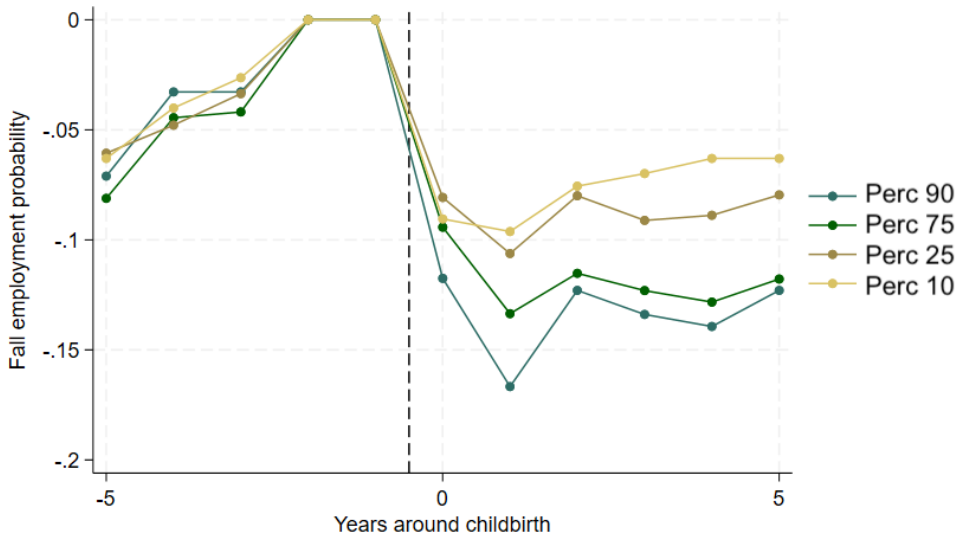
$$P_{ms}^c = \mathbb{E}\left[\frac{1}{6}\sum_{j=0}^{5} E_i^j - 1 \,\middle|\, m, s\right].$$

Figure 2 plots average employment around first childbirth for municipality-cohort cells at different percentiles of penalty magnitudes. Before childbirth, women across places follow similar employment trends. After childbirth, their trajectories diverge sharply: employment falls by approximately 13 percentage points in municipality-cohort cells at the 90th percentile of the penalty-magnitude distribution, compared with 6 percentage points at the 10th percentile.

Figure A4 reports the precision of the estimated motherhood penalties across municipality-cohort cells. We assess sampling variation using a non-parametric bootstrap procedure.[3] We compute noise variance as the average squared standard error and total variance as the variance of estimated penalties across municipality-cohort cells. The resulting noise-to-

[3]We draw 200 bootstrap samples with replacement within each municipality-cohort cell and recompute the motherhood penalty in each draw. We report the 2.5th and 97.5th percentiles as confidence intervals.

Figure 2: Motherhood Penalties of permanent resident women in high vs low penalty places



Notes: The figure shows the evolution of employment around childbirth, relative to average employment in the two years before childbirth, for women working in both years prior to childbirth in the municipality–cohort cells (out of 294) at the 10th, 25th, 75th, and 90th percentiles of the distribution of motherhood penalties. The sample includes women who are permanent residents.

total variance ratio is approximately 0.13, indicating that most of the observed variation reflects signal. To address the remaining measurement error, we implement a split-sample instrumental-variables strategy in the movers analysis, following Alesina et al. (2021).[4]

We next characterize high- and low-penalty places. Figure 3 reports univariate correlations between the absolute value of the motherhood penalty, $|P^c_{ms}|$, and municipality-cohort characteristics.[5] A positive coefficient indicates that a characteristic is more prevalent in places with larger motherhood penalties.

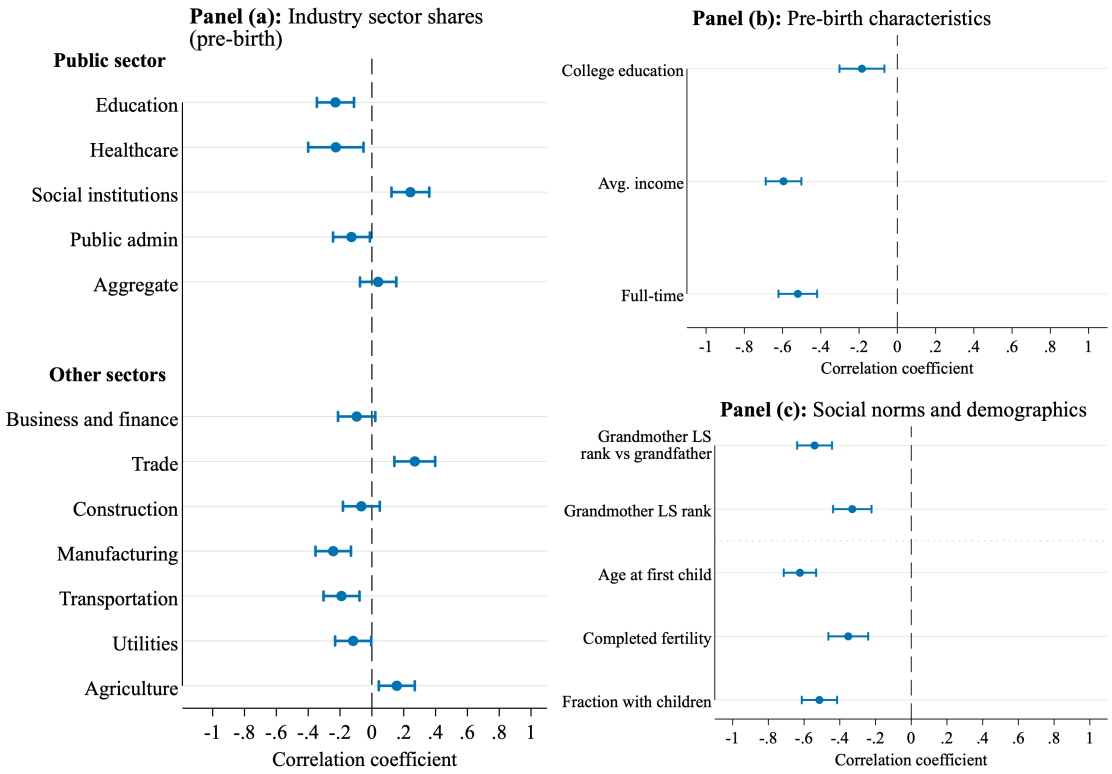
Panel (a) considers industry composition. Larger penalties are associated with higher female employment shares in trade and agriculture and lower shares in manufacturing, transportation, utilities, and business and finance.[6] The correlation with aggregate public-sector employment is close to zero, masking substantial heterogeneity across subsectors. High-penalty municipalities have lower female employment shares in education and healthcare

[4]We construct $\Delta_{do,i}$ using a random half of the sample and instrument it with the corresponding measure constructed from the other half.

[5]Motherhood penalties are negative in all municipality-cohort cells. Taking their absolute value therefore reverses their sign without changing their dispersion, so that larger values correspond to larger penalties and the signs of the reported correlations are easier to interpret.

[6]The positive correlation with trade is driven primarily by retail trade and accommodation and food services rather than wholesale trade (Figure A2).

Figure 3: Correlates of motherhood employment penalties across places



Notes: The figure reports univariate correlations across municipality–cohort cells between the absolute value of the motherhood employment penalty, $|P^c_{ms}|$, and the characteristics listed on the vertical axis. Positive coefficients indicate that a characteristic is associated with larger motherhood employment penalties. Panel (a) reports correlations with industry composition, measured by the share of women employed in each sector; the public sector is further decomposed into four subsectors. Panel (b) reports correlations with women's pre-birth characteristics, measured two years before first childbirth. Panel (c) reports correlations with proxies for gender norms and demographic characteristics. Grandmother labor supply is measured as cumulative years worked between 1964 and 1979. Grandmother LS rank vs. grandfather denotes the rank of grandmother labor supply relative to grandfather labor supply within the same cohort. Horizontal bars report 95% confidence intervals.

and higher shares in social institutions.

Panel (b) considers pre-birth characteristics. High-penalty municipalities have fewer college-educated women, lower average pre-birth earnings, and lower rates of full-time employment. Women in these places therefore enter motherhood with weaker labor market attachment.

Panel (c) considers proxies for gender norms and demographic characteristics. Larger penalties are associated with lower grandmother labor supply, both in absolute terms and relative to grandfather labor supply.[7] Larger penalties are also associated with younger age at first birth and lower completed fertility.

[7]Following Kleven et al. (2019b), we proxy local gender norms using grandmother labor supply relative to grandfather labor supply.

Taken together, high-penalty places are characterized by lower female educational attainment and pre-birth earnings, lower labor supply among women of the previous generation, and employment concentrated in retail, hospitality, and agriculture rather than business and finance, manufacturing, education, and healthcare. These correlations are descriptive and may reflect both differences in childhood environments and the sorting of families across municipalities. Section 5 separates these forces using variation in childhood exposure among movers.

5 Childhood Exposure Effect: Movers' Design

5.1 Empirical Strategy

We now estimate the causal effect of childhood exposure to places with different motherhood penalties. We adopt the movers design of Chetty and Hendren (2018), comparing women whose mothers move when the women are at different ages, from the same origins to destinations with different motherhood penalties. If childhood environment matters, moving earlier to a higher-penalty municipality should lead to a larger motherhood penalty in adulthood.

To illustrate the identification strategy, suppose children moving at age a were randomly assigned to destination municipalities with motherhood penalties P_d^c. The effect of moving at age a to municipality d could then be estimated from $y_i = \alpha_a + b_a P_d^c + \nu_i$, where y_i is the individual's motherhood penalty. Without random assignment, $b_a = \beta_a + \delta_a$, where β_a captures the causal effect of exposure and δ_a captures selection into destinations. Following Chetty and Hendren (2018), identification requires that the selection component δ_a be constant across ages at move. Under this assumption, differences in b_a across adjacent ages identify the effect of an additional year of exposure to the destination environment.

We implement this design using the following specification:

$$P_i = \mu_{ao} + \eta_s + \gamma_e + \sum_{k=1}^{6} \beta_k \mathbf{1}(a_i \in A_k)\Delta_{do,i} + \varepsilon_i, \tag{2}$$

where P_i is the unconditional employment penalty defined in Section 4, and $\Delta_{do,i} = P_{d,is}^c - P_{o,is}^c$ is the difference in motherhood penalties between the destination and origin mu-

nicipalities for individual i's birth cohort.[8] Because larger motherhood penalties correspond to more negative values of P^c, a negative $\Delta_{do,i}$ corresponds to a move to a higher-penalty municipality.[9]

We divide age at move into six five-year bins, A_k, covering ages 0–30. The fixed effects μ_{ao} interact age-at-move bins with municipality of origin, so that the coefficients β_k compare individuals who move from the same origin at similar ages to destinations with different motherhood penalties. We additionally include 10-year birth-cohort fixed effects, η_s, and fixed effects for the mover's mother's education, γ_e, distinguishing between college and non-college education.

Estimated municipality-cohort penalties contain sampling error. To address the resulting measurement error in $\Delta_{do,i}$, we use a split-sample instrumental-variables strategy following Alesina et al. (2021). We construct $\Delta_{do,i}$ using one random half of the permanent-resident sample and instrument it with the corresponding measure constructed from the other half.

The coefficients β_k trace how the relationship between the destination-origin penalty gap and individuals' own motherhood penalties varies with age at move. Their levels may reflect selection into destination municipalities, while the age gradient in the β_k coefficients identifies childhood exposure effects under the assumption that selection is constant across ages at move. We assess this identifying assumption in Section 5.3.

5.2 Main Results

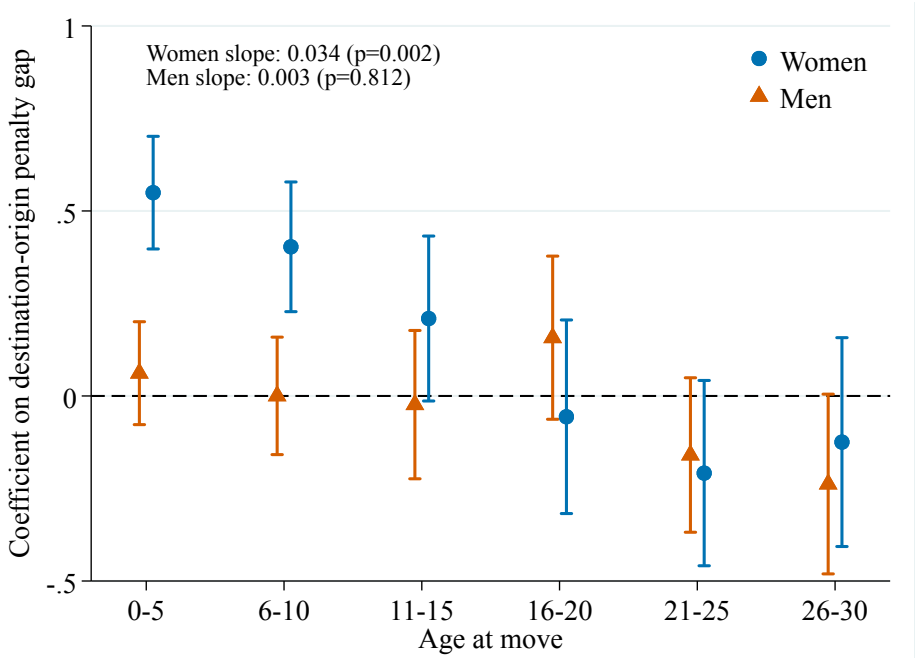
Figure 4 plots the β_k coefficients from equation (2) separately for women and men. For women, moving at ages 0–5 to a municipality with a 10-percentage-point larger motherhood penalty increases the individual's own employment penalty by approximately 5.5 percentage points. The effect declines steadily with age at move and is close to zero for moves at ages 16–20 and older. In contrast, the coefficients for men are close to zero at all ages, indicating that childhood exposure to higher-penalty municipalities does not affect men's employment responses to childbirth.

Figure A5 decomposes the effect for women into average employment before and after childbirth. Moving at ages 1–5 to a municipality with a 10-percentage-point larger motherhood penalty reduces average employment by 9.3 percentage points after childbirth and by

[8] We separately report results for pre-birth employment, $\frac{1}{2}\sum_{j=-2}^{-1} y_i^j$, and post-birth employment, $\frac{1}{6}\sum_{j=0}^{5} y_i^j$. We use the unconditional penalty P_i, rather than P_i^c, as the outcome because conditioning on pre-birth employment would restrict the movers sample based on a variable that childhood exposure to place may itself affect.

[9] Figure A3 shows the distribution of this variable.

Figure 4: Childhood exposure effects on motherhood employment penalties, by gender



Notes: The figure reports estimates of β_j from specification 2 by age at move, separately for women and men. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. The outcome is the motherhood employment penalty P_i, measured as average employment in the six years after first childbirth minus average employment in the two years before childbirth. The destination–origin penalty gap, $\Delta_{do,i}$, is the motherhood employment penalty in the destination municipality minus that in the origin municipality. Since larger penalties are more negative, a negative $\Delta_{do,i}$ denotes a move to a higher-penalty municipality. A positive β_j therefore means that moving at age j to a higher-penalty municipality is associated with a larger penalty for the mover herself. The reported slopes are the average exposure effects γ_1 from specification 3, estimated separately for women and men. Estimates include origin-municipality-by-age-at-move-bin fixed effects, 10-year birth-cohort fixed effects, and mother's education fixed effects. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

3.8 percentage points before childbirth. Both effects decline with age at move and are close to zero for moves at ages 16 and older.

Under the assumption that selection is constant across ages at move, the levels of the coefficients may reflect selection, whereas their slope identifies the causal effect of exposure to higher-penalty places. We therefore focus on the slope of the age-at-move profile. We follow Chetty and Hendren (2018) and estimate the annual exposure effect using a piecewise-linear specification that replaces the age-at-move-bin coefficients in equation (2) with a linear age profile on either side of age 18:[10]

[10]We place the kink at age 18, the midpoint of the 16–20 age-at-move bin. Table A3 shows that the results are robust to alternative choices.

$$P_i = \mu_{ao} + \eta_s + \gamma_e + 1\{a_i \leq 18\}\left(\gamma_0 + (18 - a_i)\gamma_1\right)\Delta_{do,i} + 1\{a_i > 18\}\left(\delta_0 + (18 - a_i)\delta_1\right)\Delta_{do,i} + \epsilon_i, \tag{3}$$

Table 1 reports the estimated exposure effects. For women's employment penalty around childbirth, the estimated slope is 0.034. This implies that each additional year of childhood exposure to a municipality with a one-percentage-point larger motherhood penalty increases a woman's own employment penalty by 0.034 percentage points. Equivalently, the estimate implies that each additional year of childhood exposure closes approximately 3.4% of the gap between the motherhood penalties in the origin and destination municipalities. Over 18 years of childhood exposure, this corresponds to approximately 61% convergence toward the destination penalty.[11] The corresponding slope for men, reported in Figure 4, is 0.003 ($p = 0.81$). Childhood exposure to higher-penalty places thus shapes women's labor market response to parenthood specifically, rather than reflecting features of the childhood environment that affect men's and women's responses similarly.

The post-birth effect is nearly three times as large as the pre-birth effect and statistically significant at the 1% level, while the pre-birth effect is only marginally significant. The difference between the two, reported in the first row, is itself statistically significant, indicating that exposure to high-penalty places operates primarily through employment after childbirth rather than through pre-birth labor market attachment.

Column 2 restricts the sample to birth cohorts from 1975 onward. Because residential histories are observed only from 1975, moves for earlier cohorts may correspond to second or later moves, and moves at young ages are not observed. The estimated exposure effect for the overall employment penalty is nearly unchanged relative to the baseline, while the estimates for pre- and post-birth employment become somewhat larger. Columns 3–7 add the controls discussed in Section 5.3.

We also explore whether childhood exposure effects differ by the direction of the move by re-estimating equation (2) separately for moves to higher- and lower-penalty municipalities. Figure A11 shows larger coefficients for moves to lower-penalty destinations throughout childhood: for moves at ages 0–5, the coefficient is close to one, compared with approximately

[11] The convergence-rate interpretation is approximate because the baseline specification uses conditional municipality-cohort penalties to construct the destination–origin penalty gap, while the dependent variable is the unconditional motherhood penalty. We obtain a very similar convergence rate when we construct the destination–origin gap using unconditional municipality-cohort penalties—the same measure used for the dependent variable (see Figure A10).

Table 1: Movers Design: Main Result on Employment Penalty

	(1) Main	(2) Born ≥1975	(3) Income change	(4) Cohabitation change	(5) Income & cohab. change	(6) Selection 1	(7) Selection 2
Penalty (post − pre)	0.0340	0.0327	0.0321	0.0269	0.0241	0.0283	0.0247
	(0.0109)	(0.0145)	(0.0112)	(0.0115)	(0.0115)	(0.0109)	(0.0108)
Post (after childbirth)	0.0524	0.0781	0.0502	0.0435	0.0440	0.0475	0.0396
	(0.0117)	(0.0166)	(0.0121)	(0.0123)	(0.0123)	(0.0117)	(0.0116)
Pre (before childbirth)	0.0184	0.0455	0.0181	0.0166	0.0199	0.0192	0.0149
	(0.0101)	(0.0137)	(0.0103)	(0.0109)	(0.0108)	(0.0100)	(0.0100)
Observations	199,359	86,133	199,359	199,359	199,359	189,421	189,421

Notes: Each column reports estimates of γ_1 from specification 3. The three rows correspond to three outcomes: the motherhood penalty in employment P_i (the difference between average employment after and before childbirth), average employment after childbirth, and average employment before childbirth. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Column (1) reports the baseline specification in the main sample, as described in Section 5.1. Column (2) restricts the sample to individuals born in 1975 or later. Column (3) adds a time-varying control for the change in the income of the mover's mother between the year before and the year after the move; Column (4) adds a time-varying control for the change in her cohabitation status over the same window; Column (5) includes both. Time-varying controls enter together with their interaction with Δ_{do} and with indicators for moving before or after age 18. Column (6), "Selection 1," adds fixed effects for quartiles of the mover's mother's age at first birth and age at move, the mover's father's education, and the maximum education attained by either of the mover's parents. Column (7), "Selection 2," adds to Column (6) fixed effects for quartiles of the 1964–1979 labor supply of the mover's mother and father. Standard errors, clustered by interactions of origin municipality, 10-year birth-cohort bins, age-at-move bins, and education of the mover's mother, are in parentheses.

0.4 for moves to higher-penalty municipalities. We interpret this pattern as suggestive evidence of stronger exposure effects for moves to lower-penalty environments, consistent with evidence on asymmetric responses to more gender-egalitarian social environments (Meluzzi, 2024; Boelmann et al., 2025).

5.3 Identifying Assumption

The key identifying assumption is that selection into destinations with different motherhood penalties is constant across ages at move. Families moving to high- and low-penalty municipalities may differ systematically, but these differences must not vary with the age at which the move occurs. We require this assumption conditional on origin by age-at-move fixed effects, cohort fixed effects, and fixed effects for the education of the mover's mother.

We assess this assumption in several ways. First, we exploit moves at ages 21–25 and 26–30. Because movers are defined based on their mother's relocation, individuals in these age groups rarely move with their mother and therefore receive little or no childhood exposure to the destination. The age gradient among these older movers should therefore be zero; a non-zero gradient would instead indicate that selection into destinations varies with age at

move. Figures 4 and A5 show that the coefficients for older movers are small and statistically insignificant, with a slope close to zero. A potential concern with this placebo is that families moving when children are young may select into different types of destinations than families moving when children are older. Figure A7 compares average destination-origin penalty gaps for moves at ages 1–20 and 21–30 within cells defined by origin municipality, birth cohort, and the education of the mover's mother. The correlation is 0.84, suggesting that families from similar backgrounds tend to select destinations with similar motherhood penalties regardless of the child's age at move.

We conduct three additional tests following Chetty and Hendren (2018). First, we control for changes in family circumstances around the move, to rule out that changes coinciding with the move itself drive the exposure effect. We add the change in the mover's mother's income and cohabitation status between the year before and the year after the move, together with their interactions with Δ_{do} and indicators for moving before or after age 18. Columns 3–5 of Table 1 show that the estimated exposure effect declines slightly but remains statistically significant at the 5% level.

Second, we control for predetermined characteristics of the mover's family: her mother's age at first birth and age at move, her father's education, and the maximum education level of her parents. Column 6 of Table 1 ("Selection 1") adds fixed effects for these characteristics.[12] Column 7 ("Selection 2") additionally controls for the 1964–1979 labor supply of the mover's parents, a well-established predictor of motherhood penalties (Fernández et al. (2004); Fernández and Fogli (2009); Fernandez (2007); Kleven et al. (2019a)). The estimated exposure effect declines slightly in both specifications but remains statistically significant at the 5% level.

Finally, we exploit displacement shocks, following Chetty and Hendren (2018). Let F_{pt} denote the number of families leaving parish p in year t, F_p the parish-specific average across years, and $f_{pt} = F_{pt}/F_p$ the relative outflow. Restricting the sample to moves occurring at progressively higher percentiles of f_{pt} focuses the analysis on moves associated with larger local displacement shocks, where individual sorting is plausibly less important. If differential sorting were driving the exposure effect, we would expect the estimate to attenuate as we restrict attention to these moves. Figure A8 shows instead that the estimated exposure effect remains stable across increasingly restrictive samples, while confidence intervals widen.

[12]Unlike Chetty and Hendren (2018), we cannot implement family fixed effects due to limited power and the small number of sibling pairs.

5.4 Intermediate outcomes

We next examine whether childhood exposure affects characteristics that women bring into motherhood. We focus on education, industry of employment, place of residence, partner characteristics the year before childbirth, age at first childbirth, and completed fertility.[13] These outcomes capture observable dimensions through which childhood environment may shape women's labor market outcomes around childbirth.

Effects on Adult Characteristics. We estimate equation (3) using each intermediate outcome as the dependent variable. Table 2 reports the estimated exposure effects. Each additional year of childhood exposure to a municipality with a one-percentage-point larger motherhood penalty reduces the probability of completing college by 0.10 percentage points. Exposure to higher-penalty places also increases the probability of working in the trade sector, consistent with the industry composition of high-penalty municipalities documented in Section 4.1. It also reduces the probability of having a partner and partner income, and leads women to have their first child earlier and to complete fewer births.

Table 2: Childhood Exposure Effects on Intermediate Outcomes

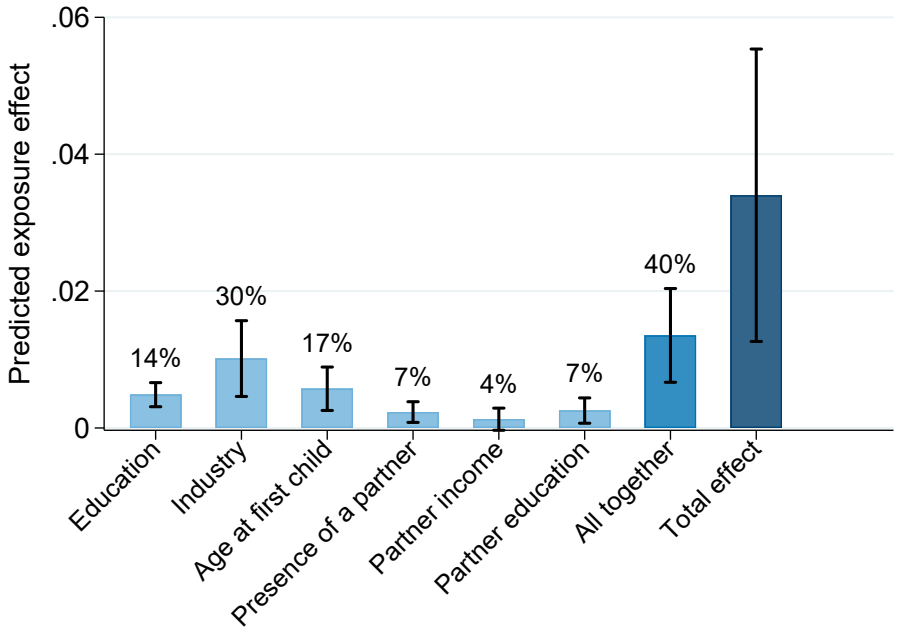
	(1) Education	(2) Trade	(3) Finance	(4) Completed fertility	(5) Age at first child	(6) Has partner	(7) Partner's income	(8) Partner's education
Penalty gap ($-\Delta_{do,i}$)	-0.0010	0.0002	0.0001	-0.0015	-0.0047	-0.0006	-225.45	-0.0003
	(0.0002)	(0.0001)	(0.0001)	(0.0002)	(0.0016)	(0.0002)	(86.22)	(0.0002)
Observations	198,991	199,359	199,359	199,359	199,359	199,359	146,994	132,585

Notes: The table reports estimates of the average exposure effect γ_1 from specification 3, using each intermediate outcome as the dependent variable. The outcomes are an indicator for having completed college (Column 1), indicators for working in the trade and finance sectors (Columns 2–3), completed fertility, measured as the number of children (Column 4), age at first birth (Column 5), an indicator for having a partner (Column 6), the partner's labor income in DKK (Column 7), and an indicator for the partner having completed college (Column 8). Education, sector of employment, and partner characteristics are measured in the year before first childbirth. For readability, the right-hand-side variable is the destination–origin penalty gap with the sign reversed, $-\Delta_{do,i}$, so that larger values denote moves to municipalities with larger motherhood penalties. For example, the coefficient in Column (1) implies that one additional year of exposure to a destination whose motherhood penalty is one percentage point larger than the origin's is associated with a 0.10-percentage-point lower probability of completing college. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors, clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and education of the mover's mother, are reported in parentheses.

[13] Although completed fertility is not itself a pre-birth characteristic, we include it because of its close relationship with women's labor supply over the life cycle.

Predictive Decomposition We next assess how much of the exposure effect on motherhood penalties is statistically accounted for by observable intermediate outcomes. For each intermediate outcome x, we first divide movers into categories of x (27 categories for industry, deciles for partner income, 4 categories for age at first child) and assign each mover the average motherhood penalty in her category, denoted by $\widehat{P}(x)$. We then re-estimate equation 3 using $\widehat{P}(x)$ as the dependent variable. The resulting coefficient captures the component of the exposure effect associated with differences in x.[14] Figure 5 reports the results. Industry, education, and age at first childbirth account for the largest components of the exposure effect. Taken together, the observable pre-birth characteristics account for around 40 percent of the total exposure effect on the motherhood penalty.

Figure 5: Exposure Effect Accounted for by Observable Characteristics



Notes: The figure reports how much of the average exposure effect is statistically accounted for by observable characteristics women bring into motherhood. For each intermediate outcome x, we divide movers into categories of x (27 categories for industry, deciles for partner income, and four categories for age at first childbirth) and assign each mover the average motherhood employment penalty among movers in her category, denoted by $\widehat{P}(x)$. We then re-estimate equation 3 using $\widehat{P}(x)$ as the outcome. Each bar reports the resulting average exposure effect γ_1. The rightmost bar reports the baseline exposure effect on the motherhood employment penalty, estimated from equation 3. Labels above the bars express each estimate as a percentage of the baseline exposure effect. Confidence intervals are obtained by bootstrapping, with resampling clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

[14]This exercise is descriptive and does not identify causal mediation effects (e.g., Bolt et al., 2021), which would require stronger assumptions and complete measurement of the relevant mediators.

Figure A12 repeats the exercise for employment before childbirth. Observable intermediate outcomes account for 97% of the exposure effect on pre-birth employment, compared with 40% of the effect on motherhood penalties. Thus, while childhood exposure affects several observable characteristics before childbirth, these characteristics account for only part of its effect on women's employment response to motherhood.

6 Policy Evidence: Childhood Exposure to Childcare

The previous sections show that childhood environment has persistent effects on women's motherhood penalties. We now ask whether policy can durably alter this environment. We study Denmark's 1964 childcare reform, which marked the transition toward universal childcare by assigning municipalities responsibility for expanding provision, as discussed in Section 6.1. Childcare availability increased substantially nationwide, but the pace of expansion varied considerably across municipalities. We exploit this variation to study whether greater childhood exposure to childcare affects women's labor market trajectories around childbirth later in life.

We proceed in two steps. First, we examine whether greater childcare expansion increased the labor supply of women who were adults at the time of the reform (the *adult generation*). Second, we study the long-run effects on women who were children at the time of the reform (the *child generation*) and therefore grew up with different levels of childcare availability. We estimate the effects of childhood exposure to childcare expansion on their educational attainment and motherhood penalties in adulthood.

6.1 Institutional Background

In 1964, the Danish central government initiated the transition toward a universal public childcare system, aiming to make childcare accessible to families across socioeconomic backgrounds. Municipalities were assigned responsibility for providing sufficient childcare services. The costs of new facilities were shared between the central government and municipalities, while operating costs were financed jointly by the central government, municipalities, and parents. The reform also established national quality standards, including central government approval of locations and buildings, trained staff requirements, and an emphasis on educational activities.

The reform triggered a rapid expansion of childcare capacity for children aged 3–7 (Figure A14), with substantial variation across municipalities (Figure A15). Between 1963 and

1974, the average municipality increased childcare capacity by approximately 16 seats per 100 children aged 3–7. As Figure A14 shows, childcare availability increased in both high- and medium-growth municipalities but diverged substantially from the early 1970s onward. By 1974, childcare capacity reached approximately 35 seats per 100 children aged 3–7 in top-quartile growth municipalities, compared with 15 in the middle two quartiles.

Several factors may have contributed to this heterogeneity, including the availability of suitable buildings, shortages of trained childcare workers, and local administrative capacity. A key concern, however, is that municipalities with stronger labor demand may also have expanded childcare more rapidly. We address this concern in Section 6.4 by showing that differential childcare expansion did not affect the labor supply of men or childless women, a pattern not consistent with contemporaneous labor demand shocks driving our results.

6.2 Data and Sample

We digitized facility-level records from Tvenstrup (1975), which documents all childcare facilities operating in 1974. The data report each facility's zip code, opening date, maximum capacity, accepted age groups, and operating hours (see Figure A13 and Table A2).

We merged these records with Danish administrative registers using zip codes. Because individual zip codes are unavailable in the registers before 1975, we assigned childhood location using each individual's residence in the 1970 Census. For individuals born after 1970, we used their mother's municipality of residence in 1970. We discuss potential concerns arising from endogenous residential sorting before 1970, and the corresponding robustness checks, in Section 6.5. We do not use place of birth to assign childhood location because, prior to 1978, births were recorded in the municipality of the hospital rather than the family's municipality of residence, introducing systematic measurement error.

Sample. The analysis uses two samples, corresponding to the two generations affected by the reform. The *adult generation* consists of mothers and fathers born between 1910 and 1960 whose first child was born between eight years before and fifteen years after the reform became operative in their municipality (event time $e_i \in [-8, 15]$, as defined in Section 6.3). This sample is used to estimate the direct effect of childcare expansion on the labor supply of women who were adults at the time of the reform, with men serving as a comparison group.

The *child generation* consists of women and men born between 1950 and 1985 whose own birth falls between eight years before and fifteen years after the reform became operative in their childhood municipality. For this sample, event time is defined relative to the individual's

birth year rather than the birth of a first child. This sample is used to estimate the long-run effects of childhood exposure to childcare expansion on educational attainment and, for women, motherhood penalties. Summary statistics for both samples are reported in Tables A4 and A7.

6.3 Empirical Design

We study the effects of childcare expansion on women in the adult and child generations by exploiting cross-municipality variation in the growth of childcare capacity. We measure treatment intensity as the change in childcare seats per child between 1963 and 1974:

$$A_m = \mathbb{E}\left[\frac{\text{3–7 seats}_z}{\text{3–7 population}_z} \,\middle|\, m,\ \text{year} = 1974\right] - \mathbb{E}\left[\frac{\text{3–7 seats}_z}{\text{3–7 population}_z} \,\middle|\, m,\ \text{year} = 1963\right],$$

where z indexes zip codes within municipality m. We focus on childcare for children aged 3–7 because this group accounts for approximately 80 percent of newly created facilities. Figure A15 documents substantial cross-municipality variation in childcare expansion.

Following the approach of Bailey et al. (2024), we exploit variation across municipalities in the timing of childcare expansion and across cohorts in exposure to the reform. We define individual event time as:

$$e_i = b_i - \mathrm{c}_m$$

where b_i denotes the year of first childbirth for the adult generation and the year of birth for the child generation. The variable c_m denotes the first birth cohort in municipality m eligible to benefit from the first post-1964 childcare facility throughout ages 3–7. Since these facilities serve children aged 3–7, a facility opening in year o_m can accommodate children born in year $o_m - 3$ from age three onward, so that $c_m = o_m - 3$. Event time therefore distinguishes cohorts born before and after childcare became available from age three in their municipality. For the adult generation, this distinction is based on the birth year of the woman's first child; for the child generation, on the individual's own birth year.

Women in the adult generation living in municipalities with no childcare expansion or expansion in the bottom quartile differ systematically from those living in municipalities in the upper three quartiles (Table A5). We therefore restrict the analysis to municipalities in the upper three quartiles of the childcare-growth distribution. We define

$$T_m = 1\{A_m \in Q_4^A\},$$

so that treated municipalities are those in the top quartile of childcare growth, while municipalities in the second and third quartiles form the control group. Our main estimating equation is:

$$y_i = \alpha_m + \gamma_{er} + \sum_{\substack{j=-2 \\ j\neq -1}}^{3} \beta_j 1\{e_i \in B_j\} T_m + X_i\eta + \varepsilon_i, \tag{4}$$

where α_m are municipality fixed effects, γ_{er} are event-time-by-region fixed effects, and B_j denotes four-year event-time bins spanning $e_i \in [-8, 15]$, with B_{-1} (event times -4 to -1) omitted. X_i includes birth year, education, and age at first birth, measured for the woman herself in the adult generation and for her mother in the child generation. Standard errors are clustered at the municipality level.

Each β_j compares women in municipalities with higher and lower childcare growth—the top quartile versus the middle two—in event-time bin B_j, relative to the same comparison in the omitted bin. Bins with $j < 0$ cover cohorts for whom the new facilities arrived too late to be used throughout ages 3–7, so β_{-2} serves as a pre-trend test, while β_j for $j \geq 0$ measures the effect of greater childcare expansion on exposed cohorts.

Identification requires that, absent differential childcare expansion, outcomes would have evolved similarly in treated and control municipalities. We provide three pieces of supporting evidence. First, the pre-treatment coefficients are small and statistically indistinguishable from zero (Figure 6). Second, Figure A14 shows that childcare availability begins to diverge sharply between treated and control municipalities from 1971 onward, consistent with the timing of the estimated effects. Third, Section 6.4 reports placebo estimates for men and childless women, for whom we find no effects, reducing concerns that contemporaneous local labor demand shocks drive the results.

6.4 Effects on Mothers in the Adult Generation

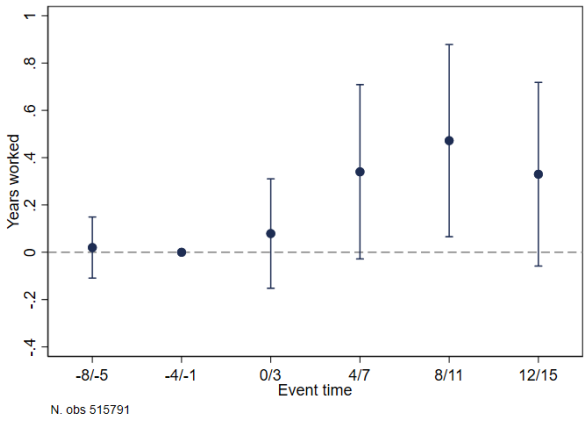
We first examine whether the reform affected the labor supply of mothers in the adult generation, a potential mechanism through which childcare expansion could affect the child generation. Because administrative income data are unavailable before 1980, we use cumulative extensive-margin labor supply between 1964 and 1979 as our primary outcome.

Panel A of Figure 6 reports the event-study estimates from equation 4. The pre-treatment coefficient is small and statistically insignificant. Positive effects on cumulative labor supply emerge at event times 4–7 and increase further at event times 8–11, consistent with

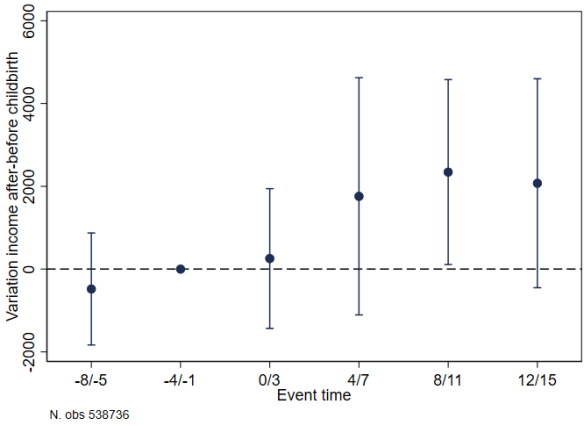
the gradual expansion of childcare availability documented in Section 6.3. The estimate remains positive at event times 12–15 but is not statistically significant. Table 3 reports the corresponding estimates, which are similar across specifications with progressively richer controls.

Figure 6: Effects of Childcare Availability on Mothers in the Adult and Child Generations

(a) Mothers in the adult generation



(b) Mothers in the child generation



Notes: The figure reports estimates of β_j from specification 4, comparing women in municipalities in the top quartile of childcare growth with those in the middle two quartiles, relative to the omitted event-time bin (-4 to -1). Panel A reports effects on cumulative labor supply between 1964 and 1979 for women in the adult generation (adult women at the time of the reform), measured in years worked. A coefficient of one corresponds to one additional year worked in top-quartile relative to middle-two-quartile municipalities, compared with the omitted event-time bin. Panel B reports effects on the change in labor income around first childbirth for women in the child generation (women who were children at the time of the reform), defined as the average labor income in the five years after childbirth minus average labor income in the two years before childbirth, measured in DKK. Positive coefficients therefore correspond to a reduction in the motherhood labor-income penalty. Event time is defined relative to the first birth cohort fully eligible for childcare throughout ages 3–7 in each municipality, using the year of first childbirth for the adult generation and the individual's own birth year for the child generation. Standard errors are clustered at the municipality level.

A potential concern is that local economic shocks may have simultaneously increased labor demand and childcare provision. If so, greater childcare expansion should also increase labor supply among groups not directly affected by childcare availability. Table A6 shows no statistically significant effects on years worked for men or childless women in the adult generation. These placebo results reduce concerns that contemporaneous local labor demand shocks drive the estimated effects.

Table 3: Effect of Childcare on the Labor Supply of Mothers in the *Adult Generation*

	(1)	(2)	(3)	(4)
	Years worked, 1964–1979			
Event time $-8/-5$	0.0200	0.0261	0.0087	0.0298
	(0.0654)	(0.0673)	(0.0763)	(0.0693)
Event time 0/3	0.0791	0.0672	0.0737	0.0821
	(0.1172)	(0.1113)	(0.1403)	(0.1349)
Event time 4/7	0.3403	0.2956	0.2848	0.3053
	(0.1867)	(0.1676)	(0.2013)	(0.2022)
Event time 8/11	0.4721	0.4529	0.4384	0.4701
	(0.2059)	(0.1957)	(0.2209)	(0.2342)
Event time 12/15	0.3301	0.3314	0.3278	0.3845
	(0.1968)	(0.1995)	(0.2218)	(0.2441)
Age at first birth		✓	✓	✓
Birth cohort			✓	✓
Education				✓
Observations	515,791	515,791	515,791	515,791

Notes: The table reports estimates of β_j from specification 4 for women in the adult generation. The outcome is cumulative labor supply between 1964 and 1979, measured in years worked. Event time is defined as the year of first childbirth relative to the first year in which families in the municipality could fully benefit from the first post-1964 childcare facility. Each coefficient reports the difference between women in municipalities in the top quartile of childcare growth and women in municipalities in the middle two quartiles for the indicated four-year event-time bin, relative to the omitted event-time bin $-4/-1$. Column (1) includes the fixed effects in specification 4. Column (2) additionally controls for age at first birth, Column (3) adds birth cohort, and Column (4) adds education (all variables related to the adult generation). Age at first birth enters as quartile fixed effects and birth cohort as five-year-bin fixed effects. A coefficient of one corresponds to one additional year worked over 1964–1979 in top-quartile relative to middle-two-quartile municipalities. Standard errors are clustered at the municipality level and reported in parentheses.

6.5 Effects on the Child Generation's Motherhood Penalties

We now turn to the child generation. Panel B of Figure 6 reports the event-study estimates from equation 4. The outcome is the change in labor income around first childbirth, measured as average labor income during the five years after childbirth minus average labor income during the two years before childbirth. The estimated effects are close to zero through event times 0–7, and become positive and significant at event times 8–11. This pattern mirrors

the dynamics for the adult generation and the gradual expansion of childcare availability shown in Figure A14. At event times 8–11, growing up in a municipality in the top quartile of childcare growth, relative to the middle two quartiles, reduces the motherhood penalty by approximately DKK 2,100 (about €280). We summarize these dynamics using the following coarser specification:

$$y_i = \alpha_m + \gamma_{er} + \beta_0 1\{e_i \in B_{-8/-5}\}T_m + \beta_1 1\{e_i \in B_{0/7}\}T_m + \beta_2 1\{e_i \in B_{8/15}\}T_m + X_i\eta + \varepsilon_i, \quad (5)$$

Table 4 reports estimates for pre-birth labor income and the change in labor income around childbirth. We find no statistically significant effect on pre-birth income: the point estimate at event times 8–15 is negative and small relative to its standard error. By contrast, greater childcare expansion reduces the motherhood labor-income penalty by approximately DKK 2,100 at event times 8–15, with similar estimates across specifications with additional controls. Table A9 shows no statistically significant effects on educational attainment for either women or men. Taken together, these results suggest that the long-run effects are concentrated in women's income response to childbirth, with no detectable effects on their educational attainment or pre-birth earnings.

A potential concern is endogenous residential sorting. For cohorts born before 1970, childhood municipality is assigned using municipality of residence in 1970, after childcare expansion had begun.[15] We address this concern by excluding individuals whose migration patterns are consistent with sorting toward municipalities with greater childcare expansion. We first exclude women who moved from municipalities below the 25th percentile of childcare growth to municipalities above the 75th percentile, and then exclude moves from below-median to above-median growth municipalities. Table A8 shows that the estimates are similar across specifications.

The absence of effects on educational attainment gives little support to a human capital channel. Instead, the results are consistent with an intergenerational transmission mechanism operating through the labor supply of women in the adult generation: childcare expansion increased their labor supply, while girls exposed to this change subsequently experienced smaller motherhood penalties. This interpretation is consistent with Barigozzi et al. (2018), who model social norms as shaped by the childcare choices of the previous generation, which in turn affect women's subsequent career and childcare decisions.

[15] See Section 6.2 for details.

Table 4: Long-Run Effect of Childhood Exposure to Childcare on Women's Labor Income Around Childbirth (Women in the *Child Generation*)

	(1)	(2)	(3)	(4)
	Income before childbirth		Change, after − before childbirth	
Event time $-8/-5$	−1,490.0	−1,360.0	−432.4	−336.2
	(1,257.4)	(1,200.1)	(694.0)	(734.4)
Event time 0/7	−403.8	−848.4	1,049.2	894.1
	(1,083.3)	(954.7)	(1,012.5)	(985.9)
Event time 8/15	−751.9	−449.5	2,134.8	2,145.6
	(1,126.0)	(1,141.9)	(720.7)	(789.9)
Additional controls		✓		✓
Observations	538,736	538,736	538,736	538,736

Notes: The table reports estimates from specification 5 for women in the child generation. Event time is defined as the woman's year of birth relative to the first year in which families in her childhood municipality could fully benefit from the first post-1964 childcare facility. Each coefficient compares women in municipalities in the top quartile of childcare growth with women in municipalities in the middle two quartiles for the indicated event-time bin, relative to the omitted event-time bin $-4/-1$. Columns (1)–(2) use average labor income in the two years before first childbirth as the outcome. Columns (3)–(4) use the change in labor income around first childbirth, defined as average labor income in the five years after childbirth minus average labor income in the two years before childbirth. Columns (2) and (4) additionally control for characteristics of the adult generation: birth cohort of the mother in the adult generation, her age at first childbirth, her education. Birth cohort enters as five-year-bin fixed effects and age at first childbirth as quartile fixed effects. Outcomes are measured in DKK. A coefficient of 1,000 therefore corresponds to a 1,000 DKK difference between top-quartile and middle-two-quartile municipalities, relative to the omitted event-time bin. Positive coefficients in columns (3)–(4) correspond to a reduction in the motherhood labor-income penalty. Standard errors are clustered at the municipality level and reported in parentheses.

7 Concluding Remarks

This paper shows that motherhood penalties have important roots in childhood environment. Using variation in age at move across Danish municipalities, we find that longer childhood exposure to a higher-penalty municipality increases women's own motherhood penalties in adulthood. Each additional year of exposure to a municipality with a one-percentage-point larger motherhood employment penalty increases a woman's own penalty by 0.034 percentage points. The effect is absent for men.

Childhood place also shapes characteristics that women bring into motherhood, including education and partner characteristics. These observable intermediate outcomes account for around 40% of the exposure effect. Thus, the environments in which women grow up shape both characteristics determined before motherhood and their subsequent employment response to childbirth.

We complement the movers design with policy evidence from Denmark's transition toward

universal childcare. Greater childcare expansion increased the labor supply of women who were mothers at the time of the reform and reduced the income penalty of women who grew up exposed to this expansion decades later, without affecting their educational attainment. While the movers design identifies the effect of childhood place as a whole, the childcare reform changes a specific component of that environment. The resulting pattern is consistent with intergenerational transmission through maternal labor supply and role models as a channel of this particular policy intervention.

Together, these findings show that geographic differences in motherhood penalties do not simply reflect the sorting of families across places: childhood environment has a lasting causal effect on women's labor market response to motherhood. They also show that these environments are policy-malleable, and that changes in them can persist into the next generation. Our results point to both economic opportunities and exposure to female labor supply and gender roles as relevant features of childhood environment. Distinguishing their causal contributions, and identifying the policies that most effectively alter them, remains an important direction for future research.

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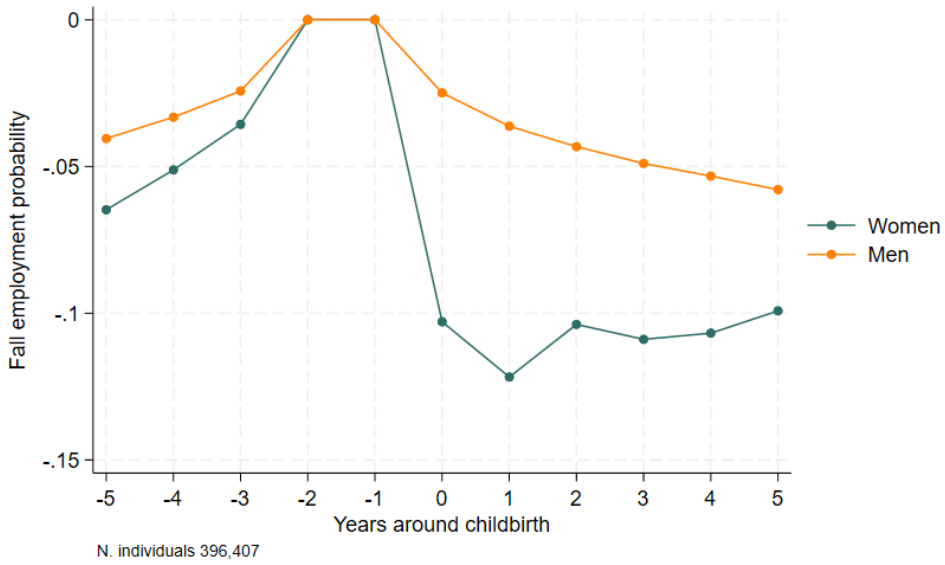
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Appendix

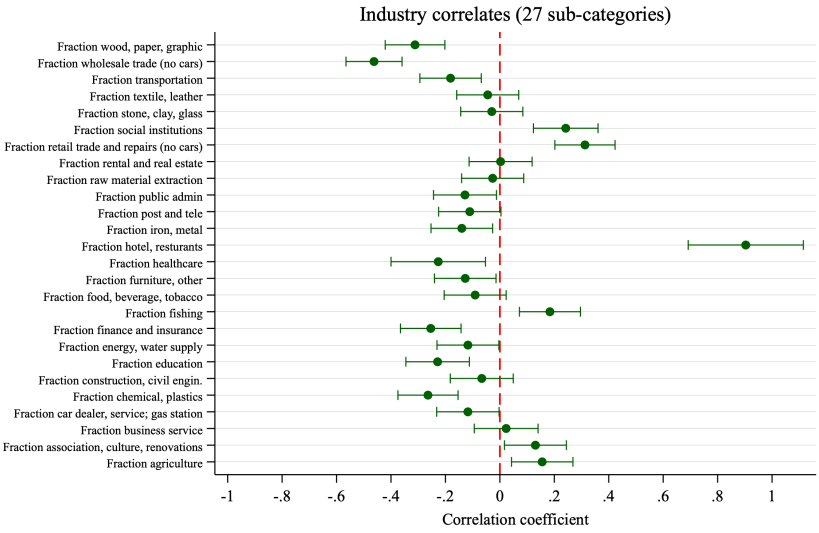
A Additional figures and tables

Figure A1: Employment Around First Childbirth, by Gender



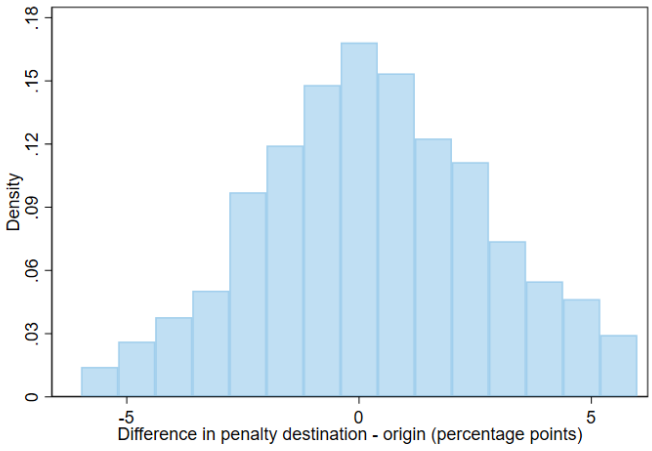
Notes: The figure shows the evolution of employment around first childbirth, separately for women and men. Employment is measured as an indicator equal to one if annual labor income is positive. The figure uses the conditional employment-penalty measure, restricting the sample to individuals employed in both years before first childbirth. Employment at each event time is expressed relative to average employment in those two pre-birth years. Event time zero denotes the year of first childbirth.

Figure A2: Correlates of Motherhood Employment Penalties with Detailed Industry Composition



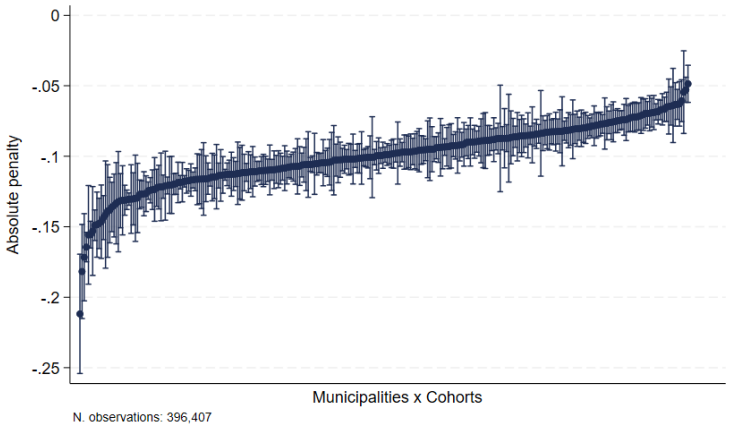
Notes: The figure reports univariate correlations across municipality–cohort cells between the absolute value of the motherhood employment penalty, $|P^c_{ms}|$, and detailed industry composition. Industry composition is measured by the share of women employed in each industry. Positive coefficients indicate that a larger employment share in the corresponding industry is associated with a larger motherhood employment penalty. Industries are disaggregated into 27 categories. Horizontal bars report 95% confidence intervals.

Figure A3: Distribution of Destination–Origin Motherhood Penalty Gaps



Notes: The figure shows the distribution of the destination–origin motherhood employment penalty gap, $\Delta_{do,i} = P^c_{d,is} - P^c_{o,is}$, among female movers. Municipality penalties are computed from permanent residents using the conditional employment penalty, separately by 10-year birth cohort. Because penalties are negative, $\Delta_{do,i} < 0$ denotes moves to higher-penalty municipalities and $\Delta_{do,i} > 0$ moves to lower-penalty municipalities.

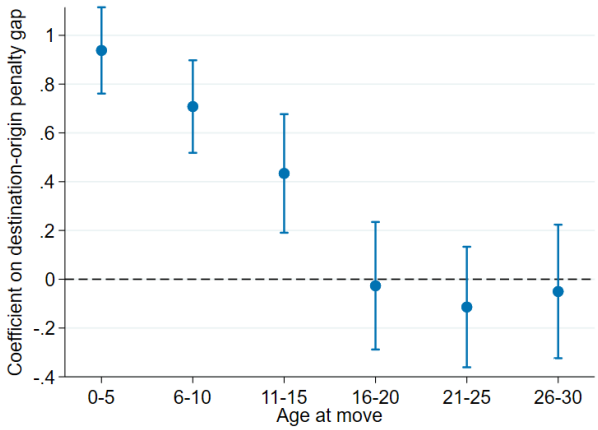
Figure A4: Variation in Motherhood Employment Penalties Across Places



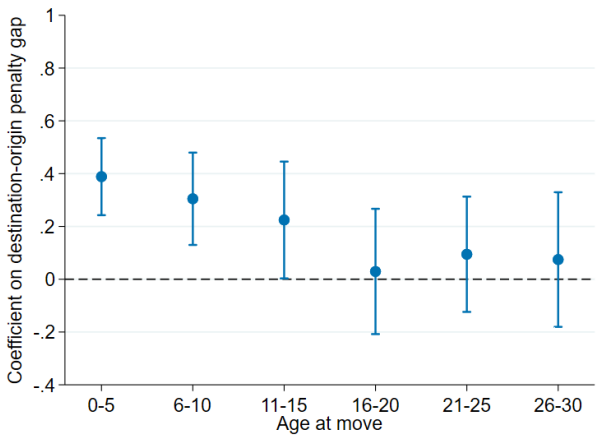
Notes: The figure shows motherhood employment penalties across municipality–cohort cells, ordered by penalty size. Penalties are computed among permanent-resident women using the conditional employment-penalty measure, which restricts the sample to women employed in both years before first childbirth. The penalty is defined as average employment in the six years after first childbirth relative to employment in the two years before childbirth. Vertical bars report 95% confidence intervals based on 200 non-parametric bootstrap replications within municipality–cohort cells.

Figure A5: Childhood Exposure Effects on Employment Before and After Childbirth

(a) Average employment after childbirth



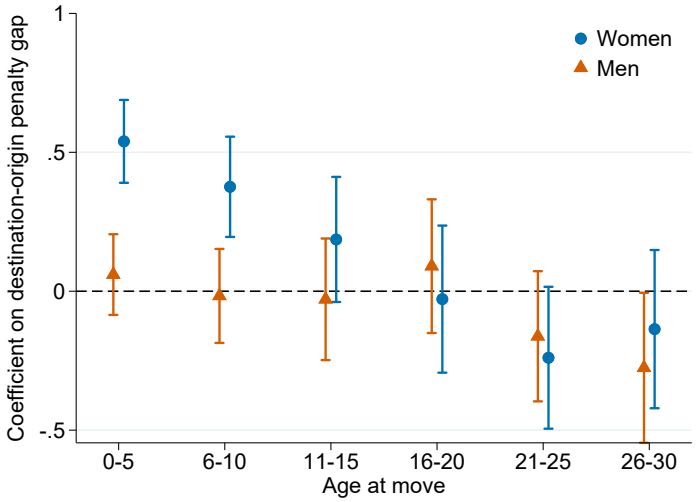
(b) Average employment before childbirth



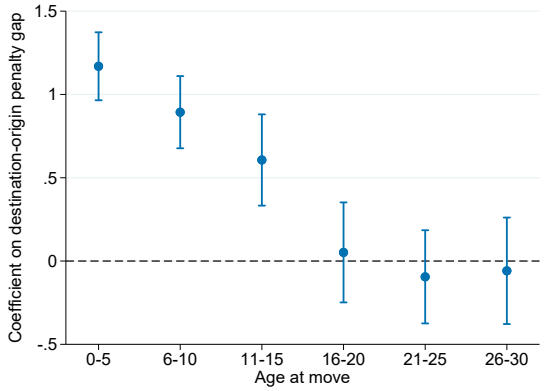
Notes: The figure reports estimates of β_j from specification 2 for female movers, by age at move. Panel (a) uses average employment in the six years after first childbirth as the outcome, while Panel (b) uses average employment in the two years before first childbirth. The destination–origin penalty gap, $\Delta_{do,i}$, is negative for moves to higher-penalty municipalities. A positive β_j therefore indicates that moving to a higher-penalty municipality at age j is associated with lower average employment over the period shown in the corresponding panel. Estimates include origin-municipality-by-age-at-move-bin fixed effects, 10-year birth-cohort fixed effects, and mother's education fixed effects. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A6: Movers' Design Using Log-Labor Income

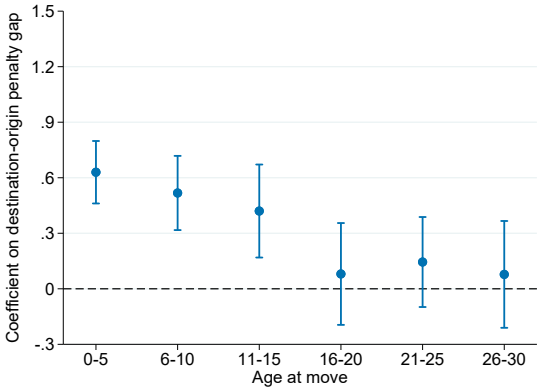
(a) Log-Income penalty



(b) Average log-income after childbirth

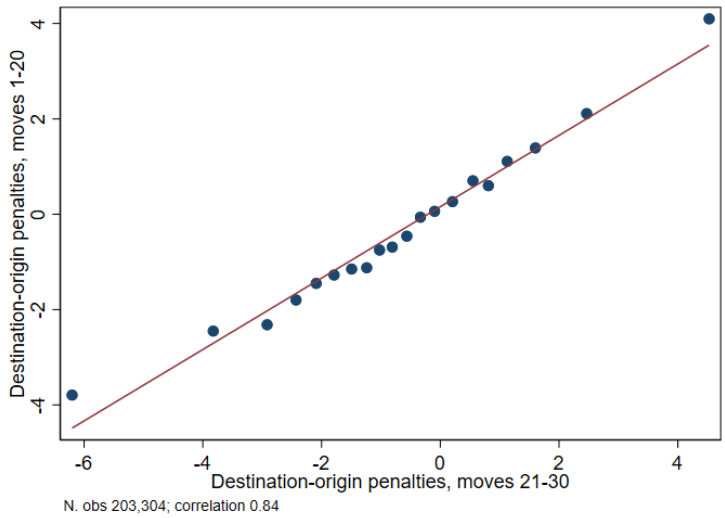


(c) Average log-income before childbirth



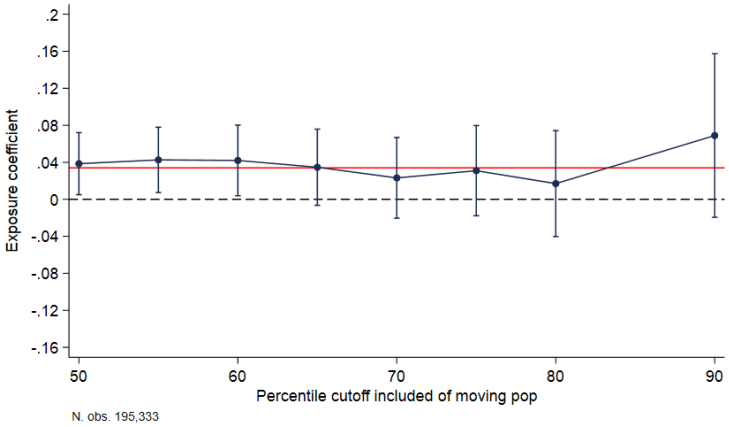
Notes: The figure reports estimates of β_j from specification 2 using log labor income instead of employment. Panel (a) reports estimates separately for women (blue) and men (orange) and uses the change in average log labor income around first childbirth as the outcome, defined as average log labor income in the six years after childbirth minus average log labor income in the two years before childbirth. Panels (b) and (c) use average log labor income in the six years after and the two years before first childbirth, respectively. Destination and origin municipality penalties are also constructed using log labor income. Because larger income penalties correspond to more negative values, a negative destination–origin penalty gap denotes a move to a higher-penalty municipality. A positive β_j therefore indicates that moving to a higher-penalty municipality at age j is associated with a larger income penalty in Panel (a), lower post-birth income in Panel (b), and lower pre-birth income in Panel (c). Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Estimates include origin-municipality-by-age-at-move-bin fixed effects, 10-year birth-cohort fixed effects, and mother's education fixed effects. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A7: Destination Penalties for Younger and Older Movers



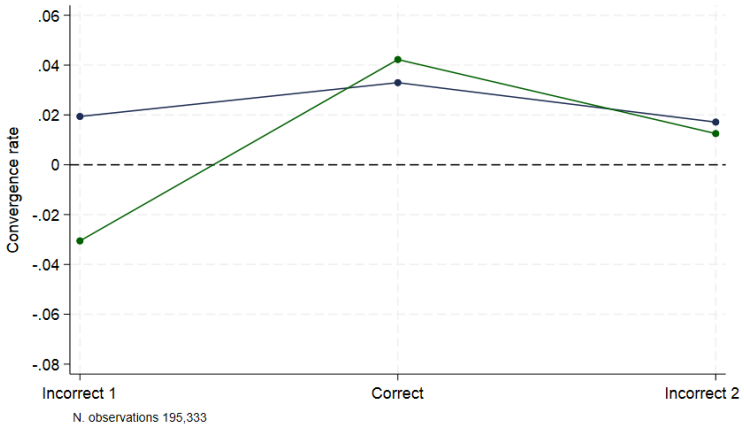
Notes: The figure shows the relationship between average destination–origin motherhood employment penalty gaps, Δ_{do}, for moves at ages 1–20 and moves at ages 21–30. Averages are computed within cells defined by origin municipality, birth cohort, and mother's education. The figure groups cells into 20 equal-sized bins based on the average penalty gap for moves at ages 1–20 and plots the corresponding average penalty gap for moves at ages 21–30. The correlation between the two measures is 0.84.

Figure A8: Exposure Effects for Moves Associated with Displacement Shocks



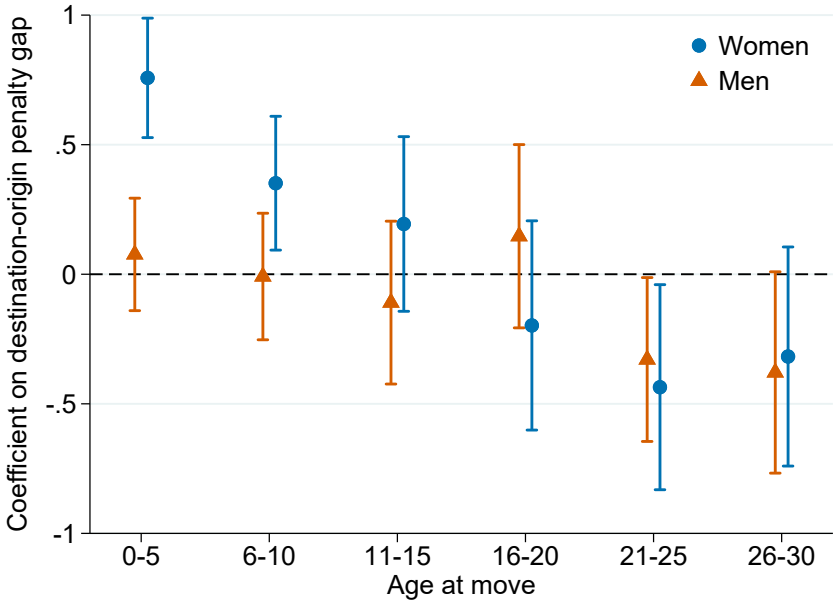
Notes: The figure reports average exposure effects for moves occurring in parish–year cells with progressively larger displacement shocks. Displacement is measured as the number of families leaving parish p in year t, relative to the parish-specific average number of families leaving across years. Each estimate restricts the sample to moves above the percentile of the displacement distribution indicated on the horizontal axis; for example, the estimate at 50 restricts the sample to moves above the median. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A9: Placebo Tests Using Adjacent Birth Cohorts



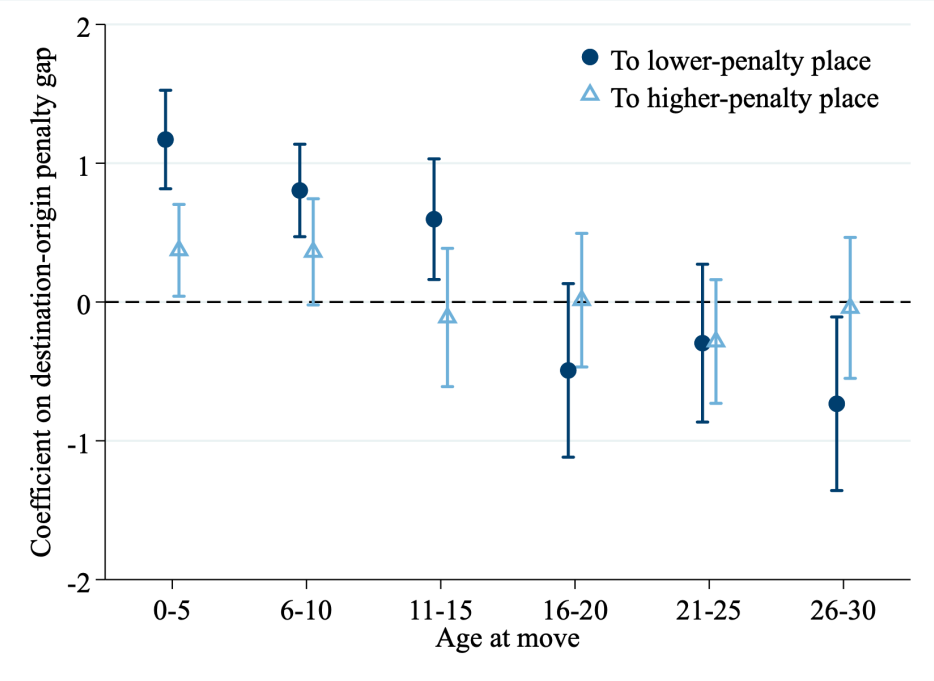
Notes: The figure reports average exposure effects using motherhood employment penalties from the mover's own 10-year birth cohort and from the adjacent 10-year birth cohorts in the same municipality. Green coefficients are estimated jointly, including the own-cohort and adjacent-cohort penalties in the same specification. Blue coefficients are estimated from separate regressions using each cohort-specific penalty in turn. All specifications otherwise follow equation 3. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A10: Childhood Exposure Effects Using Unconditional Motherhood Penalties



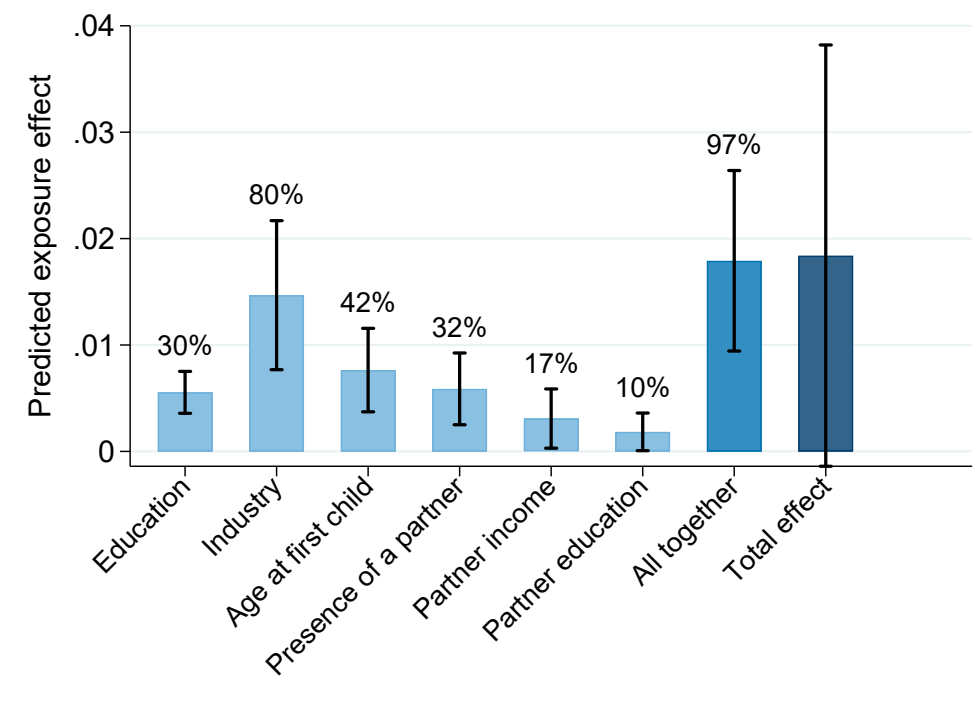
Notes: The figure reports estimates of β_j from specification 2 by age at move, separately for women and men. The outcome is the employment penalty around first childbirth, measured as average employment in the six years after childbirth minus average employment in the two years before childbirth. Unlike the baseline specification, destination and origin municipality penalties are computed using the unconditional employment-penalty measure. The destination–origin penalty gap is defined as the unconditional employment penalty in the destination municipality minus that in the origin municipality. Because larger penalties correspond to more negative values, a negative destination–origin gap denotes a move to a higher-penalty municipality. A positive β_j therefore indicates that moving to a higher-penalty municipality at age j is associated with a larger employment penalty around childbirth. Estimates include origin-municipality-by-age-at-move-bin fixed effects, 10-year birth-cohort fixed effects, and mother's education fixed effects. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A11: Childhood exposure effects by direction of move



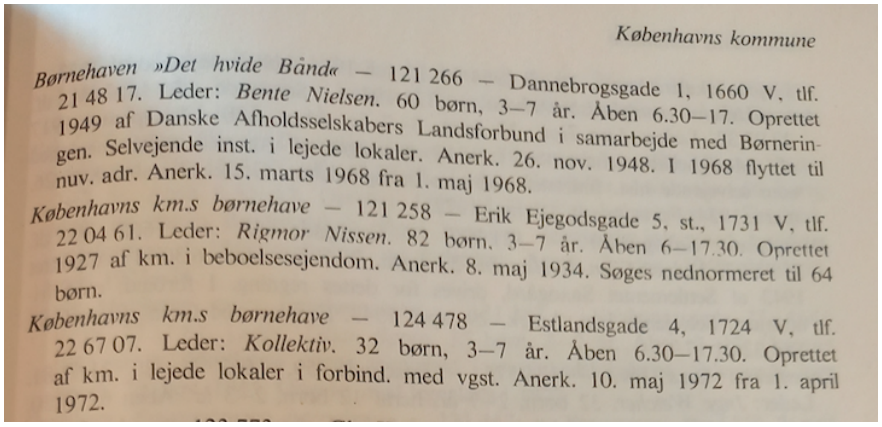
Notes: The figure reports estimates of β_j from specification 2 by age at move for female movers, separately for moves to lower-penalty municipalities (dark blue circles) and moves to higher-penalty municipalities (light blue triangles). Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. The outcome is the motherhood employment penalty P_i, measured as average employment in the six years after first childbirth minus average employment in the two years before childbirth. The destination–origin penalty gap, $\Delta_{do,i}$, is the motherhood employment penalty in the destination municipality minus that in the origin municipality. Since larger penalties are more negative, a negative $\Delta_{do,i}$ denotes a move to a higher-penalty municipality. A positive coefficient β_j therefore indicates that movers' motherhood penalties vary in the same direction as the destination–origin penalty gap. Estimates include origin-municipality-by-age-at-move-bin fixed effects, 10-year birth-cohort fixed effects, and mother's education fixed effects. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A12: Average exposure effect on pre-birth employment accounted for by observable characteristics



Notes: The figure reports how much of the average exposure effect on pre-birth employment is statistically accounted for by observable characteristics women bring into motherhood. Pre-birth employment is measured as average employment in the two years before first childbirth. For each intermediate outcome x, we divide movers into categories of x (deciles for continuous variables) and assign each mover the average pre-birth employment among movers in her category, denoted by $\widehat{E}^{pre}(x)$. We then re-estimate equation 3 using $\widehat{E}^{pre}(x)$ as the outcome. Each bar reports the resulting average exposure effect γ_1. The rightmost bar reports the baseline exposure effect on pre-birth employment, estimated from equation 3. Labels above the bars report each estimate as a share of the baseline exposure effect on pre-birth employment. Confidence intervals are bootstrapped, clustering by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education.

Figure A13: Information on Childcare Facilities



Københavns kommune

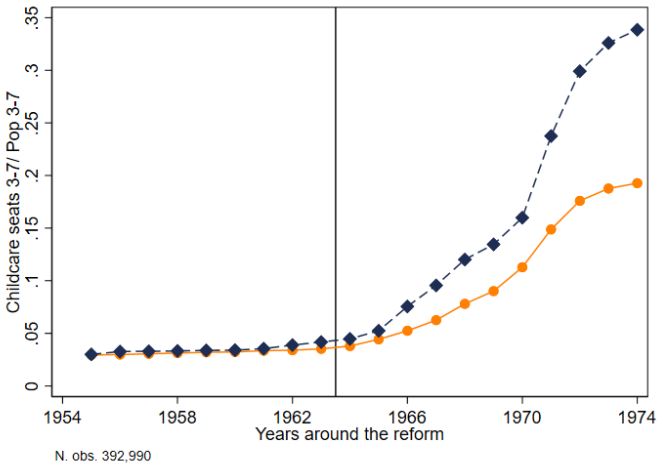
Børnehaven »Det hvide Bånd« — 121 266 — Dannebrogsgade 1, 1660 V, tlf. 21 48 17. Leder: *Bente Nielsen*. 60 børn, 3–7 år. Åben 6.30–17. Oprettet 1949 af Danske Afholdsselskabers Landsforbund i samarbejde med Børnerinnen. Selvejende inst. i lejede lokaler. Anerk. 26. nov. 1948. I 1968 flyttet til nuv. adr. Anerk. 15. marts 1968 fra 1. maj 1968.

Københavns km.s børnehave — 121 258 — Erik Ejegodsgade 5, st., 1731 V, tlf. 22 04 61. Leder: *Rigmor Nissen*. 82 børn, 3–7 år. Åben 6–17.30. Oprettet 1927 af km. i beboelsesejendom. Anerk. 8. maj 1934. Søges nednormeret til 64 børn.

Københavns km.s børnehave — 124 478 — Estlandsgade 4, 1724 V, tlf. 22 67 07. Leder: *Kollektiv*. 32 børn, 3–7 år. Åben 6.30–17.30. Oprettet af km. i lejede lokaler i forbind. med vgst. Anerk. 10. maj 1972 fra 1. april 1972.

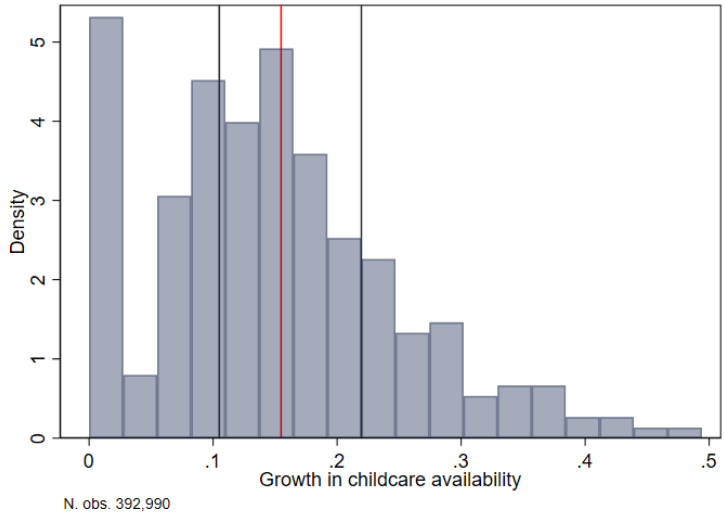
Notes: The image shows an example of childcare facilities opened in the Copenhagen municipality. In the image, it can be seen how information about the address where the facility was opened (including zip code) is present. In addition, there is information on the number of children the facility can host, the age range of children hosted, daily opening hours, and the date when it first opened.

Figure A14: Childcare Availability in High and Low Growth Municipalities



Notes: The figure shows the evolution of childcare availability for children aged 3–7 in municipalities in the top quartile of childcare growth and in the middle two quartiles. Childcare availability is measured as the number of childcare seats for children aged 3–7 relative to the population in the same age group. The vertical line denotes 1964, when Denmark initiated the transition toward universal public childcare.

Figure A15: Distribution of Childcare Growth Across Municipalities



Notes: The figure shows the distribution across municipalities of the change in childcare availability for children aged 3–7 between 1963 and 1974. Childcare availability is measured as the number of childcare seats for children aged 3–7 relative to the population in the same age group. The red vertical line denotes the median, and the black vertical lines denote the 25th and 75th percentiles of the distribution. Municipalities with zero childcare growth, shown in the first bar, are excluded when computing these percentiles.

Table A1: Summary Statistics for Permanent Residents and Movers

	(1) Permanent Residents		(2) Movers	
	Mean	SD	Mean	SD
Agricolture (year -2)	0.009	0.096	0.010	0.100
Manufacturing (year -2)	0.119	0.324	0.092	0.289
Construction (year -2)	0.013	0.111	0.010	0.100
Trade (year -2)	0.206	0.404	0.203	0.402
Business and finance (year -2)	0.118	0.322	0.114	0.317
Public sector (year -2)	0.468	0.499	0.448	0.497
Average income before birth (2 years)	168964.475	102824.370	148413.453	117829.237
Average income rank before birth (2 years)	57.672	20.658	51.323	25.330
Less than high school	0.107	0.309	0.161	0.367
High school	0.453	0.498	0.404	0.491
At least some college	0.439	0.496	0.434	0.496
Age at first birth	27.642	4.377	26.877	4.708
Completed fertility	2.191	0.777	2.213	0.854
Capital region (year-2)	0.287	0.453	0.394	0.489
Observations	396407		199362	

Notes: The table reports means and standard deviations for permanent-resident women and female movers. Permanent residents are women whose mothers do not change municipality of residence while the women are aged 1–30. Movers are women whose mothers move to a different municipality while the women are aged 1–30. For time-varying characteristics, year -2 denotes two years before first childbirth, while "2 years" denotes the average over the two years before first childbirth.

Table A2: Summary Statistics for Childcare Facilities

	Age group 0–2		Age group 3–7	
	Median	SD	Median	SD
Opening year	1970	11.22	1968	12.31
Number of children	32	18.40	40	16.86
Observations	517		2,090	

Notes: The table reports summary statistics for childcare facilities operating before 1975, separately for facilities serving children aged 0–2 and 3–7. Opening year denotes the year in which the facility began operating, and number of children denotes facility capacity. The data are from Tvenstrup (1975).

Table A3: Robustness to the Choice of Kink in the Exposure Specification

	Age 16	Age 17	Age 18	Age 19	Age 20
Penalty (post − pre)	0.0313	0.0353	0.0340	0.0369	0.0351
	(0.0119)	(0.0111)	(0.0109)	(0.0096)	(0.0092)
Post-birth employment	0.0454	0.0523	0.0524	0.0570	0.0560
	(0.0130)	(0.0121)	(0.0117)	(0.0104)	(0.0098)
Pre-birth employment	0.0141	0.0169	0.0184	0.0201	0.0209
	(0.0113)	(0.0103)	(0.0101)	(0.0088)	(0.0084)
Observations	199,359	199,359	199,359	199,359	199,359

Notes: The table reports estimates of the average exposure effect γ_1 from specification 3 for alternative choices of the kink in the piecewise-linear age-at-move specification. Columns correspond to kink ages 16 through 20; age 18 is used in the baseline specification. Rows report estimates for the motherhood employment penalty, average post-birth employment, and average pre-birth employment. Estimates use the split-sample instrumental-variables strategy described in Section 5.1 to address measurement error in the estimated destination–origin penalty gap. Standard errors are clustered by interactions of origin municipality, 10-year birth-cohort bin, age-at-move bin, and mother's education and are reported in parentheses.

Table A4: Childcare Analysis: Summary Statistics for the Adult Generation

	(1)		(2)		(3)	
	Woman, children		Man, children		Woman no children	
	Mean	SD	Mean	SD	Mean	SD
LS 1964-1979	5.604	4.124	10.736	5.111	8.541	5.435
Childbirth 1 year	1966.098	6.560	1966.924	6.549	1964.861	9.990
Birth year	1941.694	7.531	1940.287	7.516	1939.851	8.629
Policy start year	1965.414	1.940	1965.529	2.032	1965.112	1.722
Age first child	24.404	4.322	26.636	4.476	25.010	5.029
high_school_least	0.546	0.498	0.670	0.470	0.552	0.497
Observations	515791		471407		48576	

Notes: The table reports summary statistics for mothers, fathers, and childless women in the adult-generation sample used in the childcare analysis. For childless women, a placebo age at first childbirth is drawn from a normal distribution with the same mean and standard deviation as the observed age at first childbirth among mothers and is used to assign placebo event time.

Table A5: Childcare Analysis: Characteristics by Childcare-Growth Group

	0–Q1		Q2–Q3		Q4		Standardized difference	
	Mean	SD	Mean	SD	Mean	SD	0–Q1 vs. Q4	Q2–Q3 vs. Q4
Labor supply, 1964–1979	4.827	3.921	5.618	4.116	5.566	4.144	-0.183	0.013
Year of first childbirth	1966.840	6.688	1966.027	6.561	1966.284	6.553	0.084	-0.039
Birth year	1942.498	7.606	1941.746	7.541	1941.557	7.501	0.125	0.025
Policy start year	1967.312	2.439	1965.335	1.926	1965.609	1.962	0.769	-0.141
Age at first childbirth	24.342	4.298	24.281	4.319	24.727	4.316	-0.089	-0.103
High school or more	0.468	0.499	0.532	0.499	0.582	0.493	-0.230	-0.101
Observations	135,306		373,329		142,462			

Notes: The table reports means and standard deviations for mothers in the adult generation, separately by childcare growth in their municipality of residence. Childcare growth is the change in childcare availability for children aged 3–7 between 1963 and 1974. Group 0–Q1 comprises municipalities with zero growth or growth in the bottom quartile of the distribution, Q2–Q3 the middle two quartiles, and Q4 the top quartile. The last two columns report standardized differences relative to the top-quartile group, computed as the difference in group means divided by the square root of the average of the two group variances. The estimation sample compares Q4 with Q2–Q3; group 0–Q1 is reported for completeness and is excluded from the analysis. Standardized differences between the two estimation groups are below 0.15 for all characteristics, whereas municipalities with zero or bottom-quartile growth differ substantially in the timing of childcare expansion and in maternal education.

Table A6: Placebo Effects on Men and Childless Women in the Adult Generation

	(1)	(2)
	Years worked	Years worked
-8/-5	-0.1085	0.0329
	(0.0848)	(0.2087)
0/3	0.1745	0.0306
	(0.0957)	(0.1693)
4/7	0.0454	-0.1712
	(0.1726)	(0.1678)
8/11	0.0210	-0.1660
	(0.2589)	(0.2223)
12/15	0.0120	-0.1652
	(0.3169)	(0.1916)
Additional controls	Yes	Yes
N	471407	48576

Notes: The table reports estimates from specification 4 for men (column 1) and childless women (column 2) in the adult generation. The first column report estimates for men, while the second column report estimates for childless women. The outcome is cumulative labor supply between 1964 and 1979, measured in years worked. Event time is defined relative to the first year in which families in the municipality could fully benefit from the first post-1964 childcare facility. For men, event time is based on the year of first childbirth. For childless women, a placebo year of first childbirth is drawn from a normal distribution with the same mean and standard deviation as the observed age at first childbirth among mothers and is used to assign placebo event time. Each coefficient compares individuals in municipalities in the top quartile of childcare growth with individuals in municipalities in the middle two quartiles for the indicated event-time bin, relative to the omitted event-time bin. Additional controls include birth cohort, age at first childbirth, and education. Standard errors are clustered at the municipality level and reported in parentheses.

Table A7: Childcare Analysis: Summary Statistics for the Child Generation

	(1) Woman		(2) Man	
	Mean	SD	Mean	SD
Policy start year	1965.645	2.106	1965.619	2.089
Birth year	1967.221	6.442	1966.770	6.672
Childbirth 1 year	1995.217	8.177	1996.719	8.357
Age first child	27.922	4.859	29.970	5.112
Fraction high school	0.417	0.493	0.477	0.499
Fraction college	0.422	0.494	0.304	0.460
Labor income before childbirth	119180.614	112592.138	147162.003	143249.291
Labor income after childbirth	125293.070	115730.611	186158.118	165724.541
Birth year mother	1940.964	8.193	1940.545	8.339
Fraction high school mother	0.337	0.473	0.329	0.470
Fraction college mother	0.169	0.375	0.163	0.370
Age first child mother	26.257	5.075	26.225	5.064
Observations	538736		583414	

Notes: The table reports summary statistics for women and men in the child-generation sample used in the childcare analysis. The child generation consists of individuals who were children around the time of the childcare expansion.

Table A8: Robustness to Residential Sorting in the Child Generation

	(1) Change income	(2) Change income	(3) Change income	(4) Change income	(5) Change income	(6) Change income
-8/-5	-432.4001	-336.2494	-150.2113	-59.2153	-207.0595	-100.2849
	(694.0337)	(734.3730)	(693.9525)	(734.5818)	(738.7552)	(771.3874)
0/7	1049.2368	894.1344	1118.3120	970.8942	1355.5211	1212.0058
	(1012.4708)	(985.8963)	(1046.4875)	(1021.8377)	(1042.4959)	(1015.1480)
8/15	2134.8066	2145.5908	2281.8250	2282.6843	2359.2690	2364.7440
	(720.7358)	(789.8882)	(729.7906)	(809.1295)	(732.6631)	(809.1043)
Additional controls	No	Yes	No	Yes	No	Yes
N	538736	538736	534157	534157	512866	512866

Notes: The table reports estimates from specification 5 for women in the child generation. Columns (1)–(2) use the full sample. Columns (3)–(4) exclude women who moved from municipalities in the bottom quartile of childcare growth to municipalities in the top quartile; Columns (5)–(6) exclude women who moved from municipalities in the bottom half of the distribution to municipalities in the top half. Even-numbered columns additionally control for characteristics of the adult generation: birth cohort of the mother in the adult generation, her age at first childbirth, and her education. Birth cohort enters as five-year-bin fixed effects and age at first childbirth as quartile fixed effects. Event time is defined as the woman's year of birth relative to the first year in which families in her childhood municipality could fully benefit from the first post-1964 childcare facility. The outcome is the change in labor income around first childbirth, defined as average labor income in the five years after childbirth minus average labor income in the two years before childbirth. Each coefficient compares women in municipalities in the top quartile of childcare growth with women in municipalities in the middle two quartiles for the indicated event-time bin, relative to the omitted event-time bin $-4/-1$. Outcomes are measured in DKK. A coefficient of 1,000 therefore corresponds to a 1,000 DKK difference between top-quartile and middle-two-quartile municipalities, relative to the omitted event-time bin. Positive coefficients correspond to a reduction in the motherhood labor-income penalty. Standard errors are clustered at the municipality level and reported in parentheses.

Table A9: Effects of Childcare Expansion on College Attainment in the Child Generation

	(1)	(2)	(3)	(4)
	College education, women		College education, men	
Event time $-8/-5$	-0.0133	-0.0110	-0.0125	-0.0089
	(0.0124)	(0.0104)	(0.0078)	(0.0067)
Event time 0/7	0.0003	-0.0047	0.0030	-0.0006
	(0.0076)	(0.0058)	(0.0053)	(0.0041)
Event time 8/15	-0.0045	-0.0043	-0.0042	-0.0036
	(0.0109)	(0.0073)	(0.0134)	(0.0077)
Additional controls		✓		✓
Observations	538,736	538,736	583,414	583,414

Notes: The table reports the effect of childcare availability on college completion for women (columns (1)–(2)) and men (columns (3)–(4)) who were children at the time of the reform, from specification 5 with different controls. The outcome is an indicator for holding a college degree; coefficients are therefore in probability units. A coefficient of -0.01, for example, means that being in a municipality in the top 25% of the childcare growth distribution, relative to the middle of the distribution (25th–75th percentile), is associated with a 1-percentage-point lower probability of completing college. Each row corresponds to a different event time. Additional controls (columns (2) and (4)) include the adult generation mother's birth year, her age at first child, and her education, entered non-parametrically as fixed effects for bins: 5-year bins for birth year, and quartile bins for age at first child. Standard errors, clustered at the municipality level, are in parentheses. No coefficient is statistically significant at conventional levels.