Is Zip Code Destiny?

Re-visiting Long-run Neighborhood Effects

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Abstract

Chetty & Hendren (2018a), Chetty & Hendren (2018b), and Chetty *et al.* (2020a) argue that 60%-100% of the geographic variation in upward mobility in the US is due to the causal effect of place of residence. This paper demonstrates that this argument is built on too strong assumptions not supported by the data. I provide evidence that the identifying assumptions, underlying the recent influential studies in the literature, are untenable using detailed Danish registry data. I document life cycle heterogeneity in the neighborhood sorting process, which invalidates the assumption of constant selection effects by the child's age when the family moves. This selection introduces a sizable upward bias to the estimates of childhood exposure effects in the most notable studies of the literature. Consequently, the exposure effect estimates in these studies simply reflect the correlational estimates of place effects in Chetty *et al.* (2014).

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1 Introduction

Children's incomes in adulthood vary remarkably by the local region where they grow up, even after conditioning on their parental income. Previous studies have documented substantial spatial variation in intergenerational mobility within the US and many other developed countries.[1] The recent social mobility literature has revived interest in the yet unsettled question concerning the causal status of the link between neighborhood characteristics and residents' economic well-being. A series of related papers that aim to determine the extent to which the differences in income mobility across geographical areas reflect causal effects of place have received growing attention recently (Chetty & Hendren (2018a); Chetty & Hendren (2018b); Chetty *et al.* (2020a); Chetty *et al.* (2020b); Chetty (2021)).

Using US tax data, Chetty & Hendren (2018a) and Chetty & Hendren (2018b) aim to identify the causal effects of place on children's long-term outcomes. To overcome the identification problem caused by the endogeneity of neighborhood choice, they exploit variation in the timing of children's moves across US commuting zones. They compare the outcomes of children who moved to a new area at different ages to identify the exposure effects of spending an additional year of childhood in each area. To draw causal inferences, they assume that unobservable determinants of children's adulthood outcomes are uncorrelated with the age at which they move to a different neighborhood. In other words, they assume that selection biases are constant in the child's age when the family moves. Chetty & Hendren (2018a) find that the adult incomes of children who moved converge to the adult incomes of children of permanent residents in the destination at a rate of 4% per year of exposure. Under their identifying assumption, Chetty & Hendren (2018a) interpret this result as the causal impacts of neighborhoods on later life outcomes. Chetty *et al.* (2020a) repeat the same analysis at the Census tract level rather than commuting zone level and find similar exposure effects for various outcomes such as earnings, college attendance, marriage, teenage birth

[1] See Chetty *et al.* (2014) for the US; Heidrich (2017) for Sweden; Acciari *et al.* (2019) for Italy, Corak (2020) for Canada; Deutscher & Mazumder (2020) for Australia; Eriksen & Munk (2020) for Denmark; Chuard & Grassi (2020) for Switzerland; Kenedi & Sirugue (2021) for France; and Buscha *et al.* (2021) for the UK.

rates, and incarceration. A growing number of studies have recently adopted the same estimation strategy from Chetty & Hendren (2018a) and replicated their findings using data from other countries, including Australia and Canada (Deutscher (2018); Laliberté (2021)).

Using a similar but stronger identifying assumption, Chetty & Hendren (2018b) present the causal effect of each county and commuting zone in the US.[2] They argue that the correlations between area-level characteristics and upward mobility are due to the causal effects of place and that "there is substantial scope for households to move to areas within their commuting zones that are opportunity bargains—places that produce better outcomes for children without paying higher rents." Moreover, Chetty *et al.* (2020a) construct an "Opportunity Atlas" that provides estimates of the long-term outcomes of children who grew up in each US Census tract.[3]

This set of results has been touted as "zip code destiny" or "power of place" (Badger & Bui (2015); Brooks (2018); Kristof (2019); Vedantam (2019)) and has been advocated to inform the design of housing policies (Chetty *et al.* (2020a)).[4] These efforts have given rise to advocacy for relocation policies to promote upward mobility. Creating Moves to Opportunity in Seattle and King County is an example of such housing public programs motivated by these popular studies of neighborhood effects.[5]

This paper presents a body of evidence suggesting that the estimates of exposure effect in certain prominent studies of neighborhoods and intergenerational mobility are driven by the sorting of heterogeneous agents across areas rather than by causal effects of place.

Furthermore, this paper conducts a placebo test to examine the credibility of the estimation strategies for identifying long-run neighborhood effects in Chetty & Hendren (2018a), Chetty & Hendren (2018b), and Chetty *et al.* (2020a). For this purpose, I exploit the data on

[2] To this end, they assume that the selection effect is constant in the child's age at the time of the move for *each* origin-destination pair. Hence, the identifying assumption must hold for every origin-destination pair.

[3] See Andrews *et al.* (2019) and Mogstad *et al.* (2020) for a discussion of the statistical uncertainty surrounding neighborhood upward mobility estimates in Chetty *et al.* (2014) and Chetty *et al.* (2020a).

[4] The findings of these studies are sometimes summarized as "a better address can change a child's future."

[5] Bergman *et al.* (2019) conducted an experience urging the recipients of the housing vouchers to move to high-opportunity neighborhoods selected based on the Opportunity Atlas in Chetty *et al.* (2018).

birth characteristics such as birth weight, which are realized at age zero, i.e., before neighborhood exposure comes into play. For the sample of children whose families move across neighborhoods during childhood, the destination area cannot impact children's birth characteristics. Otherwise, the effect would be preceding the cause. Hence, one expects to obtain insignificant estimates if they use the methodology of Chetty & Hendren (2018a) to investigate the relationship between characteristics realized at birth and later moves across neighborhoods during childhood. Nonetheless, the emerging pattern is similar to Chetty & Hendren (2018a) and Chetty *et al.* (2020a). The similarities between the placebo estimates and the exposure effect estimates suggest that the estimates of exposure effects on adulthood outcomes in Chetty & Hendren (2018a) and Chetty *et al.* (2020a) pick up the sorting of heterogeneous families across areas rather than neighborhood causal impacts.

I demonstrate that what Chetty & Hendren (2018a) and Chetty *et al.* (2020a) interpret as the causal impact of neighborhood exposure on children is an artifact of the sorting process of households across areas. Individuals move to the areas whose residents share common characteristics with them the most.[6] Moreover, the sorting pattern is more pronounced for earlier movers. A self-selection pattern by the child's age exists: those who move when their children are younger are, on average, more affluent, more educated, and more likely to be a nuclear family. It follows that children of early movers tend to earn more in adulthood, regardless of their area. At the same time, early movers sort into more expensive areas with more educated, more stable, and wealthier residents whose children earn more in adulthood. Neglecting these patterns misleads researchers to conclude that exposure to a better destination neighborhood, which is longer for children who moved at earlier ages, is the cause for better outcomes observed for children later in their life.

This paper exploits a rich longitudinal administrative data set from Denmark to analyze the credibility of the primary identifying assumptions in the recent popular studies of neighborhood exposure effects. I replicate the results of Chetty & Hendren (2018a) and Chetty

[6] Residential segregation by race and income are prominent, persistent features of urban America (Lee *et al.* (2008); Reardon *et al.* (2009); Reardon & Bischoff (2011); Graham (2018)).

et al. (2020a) in a Danish context. Despite significant differences in social mobility between the US and Denmark (Landersø & Heckman (2017)), I find similar estimates to those of Chetty & Hendren (2018a) and Chetty *et al.* (2020a). Then, I present evidence suggesting that, contrary to what Chetty & Hendren (2018b) assume, selection biases are not constant in the child's age when the family moves. Instead, the selection bias decreases in the child's age, which results in an overestimation of the exposure effects.

This paper contributes to the prolific literature in economics and sociology on neighborhood effects (Julius (1987); Katz *et al.* (2001); Wodtke *et al.* (2011); Altonji & Mansfield (2018)).[7] The present article is related to a strand of literature that evaluates the puzzling, seemingly contradictory results from the experimental studies and the results of nonexperimental studies of neighborhood effects on economic opportunities (Burdick-Will *et al.* (2011); Harding *et al.* (2021)). Experimental research, mainly using the Moving To Opportunity (MTO) experiment, find no effect or modest effects of relocating from high-poverty neighborhoods to low-poverty communities on economic opportunities for disadvantaged families (Katz *et al.* (2001); Goering *et al.* (2003); Kling *et al.* (2007); Gennetian *et al.* (2012)).[8,9] These studies usually estimate the intention-to-treat effect by comparing the average outcomes of the treatment group (who were being offered a voucher) to the average outcome of the control group (who were not being offered a voucher).[10] Because of the random assignment of families to treatment and control groups, this approach arguably eliminates the problem of self-selection bias, i.e., the nonrandom nature of the relationship between the neighborhood attributes and the preexisting characteristics of the residents who choose to live in the neighborhood, that plagues the estimates of observational studies.[11,12] In contrast

[7] Jencks & Mayer (1990) review earlier studies on neighborhood effects in economics and sociology. More recent reviews include Sampson *et al.* (2002), Durlauf (2004), Harding *et al.* (2010), Sharkey & Faber (2014), Galster & Sharkey (2017), Graham (2018), Chyn & Katz (2021), and Mogstad & Torsvik (2021).

[8] The MTO experiment randomly assigned housing vouchers that required moving to a lower-poverty area.

[9] Chyn (2018) is a notable exception. Using the public housing demolitions in Chicago (which forced low-income households to relocate to less disadvantaged areas), he finds that displaced children have better labor market outcomes at age 26 than their non-displaced peers.

[10] Pinto (2018) distinguishes neighborhood effects from voucher effects. He finds that neighborhood effects are statistically significant even though voucher effects are not.

[11] See Heckman (2001) and Manski (1995) for overviews of self-selection bias.

[12] Chetty *et al.* (2016) revisited the MTO experiment using Internal Revenue Service data on later life out-

to the findings of the MTO studies, a growing number of studies employ quasi-experimental strategies and find sizable causal effects of place on children's long-term outcomes (Chetty & Hendren (2018a); Chetty & Hendren (2018b); Chetty *et al.* (2020a); Chetty *et al.* (2020b); Chetty (2021)). This paper questions the validity of the estimation strategies and the identifying assumptions underlying the prominent quasi-experimental studies in the literature. The findings of this paper also provide new insights into the lifecycle heterogeneity in the neighborhood sorting process.

This paper is close to Harding *et al.* (2021), who use a within-study comparison design and compare experimental and nonexperimental estimates from the MTO and parallel analysis of the Panel Study of Income Dynamics (PSID). They consider several explanations to reconcile the results of experimental studies to those from nonexperimental ones. The first candidate is selection bias which they cannot test directly. Instead, they test several other common hypotheses, which all fail to explain the different results between experimental and observational studies.[13] They, therefore, conclude that selection bias is the most likely driver of the neighborhood effects on adult outcomes found in nonexperimental studies. However, Harding *et al.* (2021) focus only on adults. On the other hand, this paper studies the long-run impact of neighborhoods on children. It provides direct evidence of selection bias in prominent observational studies of neighborhood effects, rooted in their untenable assumptions.

This paper proceeds in the following way. Section 2 describes the data. Section 3 critically reviews the estimation procedures and underlying assumptions of the most notable works in the literature. Section 4 replicates these studies in a Danish context. Section 5 tests the reliability of the common estimation strategies for identifying long-run neighborhood effects. Section 6 conducts a placebo test to gauge the empirical relevance of the violations of the identifying assumptions on the estimates of neighborhood effects. Section 7 concludes.

comes. The treatment-on-the-treated estimates presented by Chetty *et al.* (2016) indicate that children who move to lower-poverty areas when under age 13 experience significant gains in their adulthood income, while those who move after age 13 experience no gain or a loss.

[13] Specifically, they find no clear evidence that different estimates are related to the duration of adult exposure to disadvantaged neighborhoods, nonlinear effects of neighborhood conditions, the magnitude of the change in neighborhood context, frequency of moves, treatment effect heterogeneity, or measurement.

2 Data

This paper uses administrative data for the entire population of Danes provided by Statistics Denmark. The registers cover the years 1980 through 2019 and provide an extensive account of individual income, assets, and the structure and demographics of households and details on various characteristics of the neighborhood of residence for each individual in each year.[14]

2.1 Sample Definition

Income Analysis: The final analysis sample includes all children born between 1970–1982 in Denmark and for whom I can identify the parents. The sample is divided into permanent residents (non-movers) and movers. The permanent residents (PR) are defined as the subset of parents living in a single municipality in all years between 1982–2000. The movers are those individuals in the sample who are not permanent residents. With these sample choices, the age range of children when their families move is 1-30. To get higher precision, I only focus on ages 1-25 when most moves occur. I measure children's annual income at age 30.

Placebo Analysis: For the placebo analysis, I make use of the data set on birth characteristics. The data is available only for individuals born in 1997 or later. Therefore, the placebo analysis includes all children in the data set born between 1997–2005 and for whom I can identify the parents. The sample period for the placebo analysis is 1997-2019. The permanent residents are defined as the subset of parents living in a single municipality in all years between 1997–2019. The movers are those in the sample who are not permanent residents.

2.2 Variable Definitions

Income: As the primary measure of income, I use annual disposable income, which is income after taxes, interests, and rental value of owner-occupied housing.[15,16] For parents, I

[14]For most of the analyses in this paper, I focus on broad geographic units, i.e., municipalities, to maximize statistical precision. I observe similar patterns when using more granular neighborhood levels, such as parishes.

[15]The main results of this paper are robust to alternative measures of income, such as wage income and market income.

[16]The disposable income is computed as follows: The following items are added: total salary income, remuneration, social security contributions, net profits from self employment, public transfers (social assistance,

use disposable income averaged over the sample period.[17] For children, I use their disposable income at age 30.

Neighborhood Units: In the primary analysis, I use municipality as the neighborhood unit.[18] I also use data on smaller neighborhood units, such as parish and housing clusters.[19]

Moves: Each year, parents are assigned municipality (parish) codes of residence. I define a move across neighborhoods when the municipality of residence changes from a year to the next.[20]

Marriage: For each individual, I observe the partner each year. I use cohabitation (inclusive of marriage) as the primary measure for analyzing the dynamics of family structures.

Birth Weight: I observe the birth weight for each person in the sample born in 1997 or later. The variable is measured and registered immediately after birth.

Birth Length: I observe the birth length for each person in the sample born in 1997 or later. The variable is measured and registered immediately after birth.

2.3 Summary Statistics

Table 1 presents summary statistics of the movers and permanent residents.

Figure A.1 of Appendix A shows the education level (measured as years of schooling) for permanent residents and compares it to the education level of movers who move exactly once. Figure A.1 suggests that, compared to the sample of movers, permanent residents, on

unemployment benefits, labor market leave, sick leave assistance, labor market activation, child benefits, education grants, housing support, early retirement pension, disability pension, and retirement pension), private pensions paid, interest income and realized gains on securities, and residual income including child support. The following items are subtracted: interest expenses, taxes, labor market contributions and special pension, maintenance (contributions) paid to a former spouse as well as to children under age 18. Finally, the estimated rental value of own home is added.

[17] I use the mother's income plus the father's income in each year from 1982 to 2000 divided by 19.

[18] There were 271 municipalities in Denmark during the sample years (i.e., 1982-2000). These 271 municipalities were merged into 98 large new municipalities in 2007. For the placebo analysis, in which sample years cover 2007 and later years, I use the post-2007 definition of municipalities.

[19] In constructing the clusters, I build on the methodology implemented in Damm & Schultz-Nielsen (2008), which satisfies the following criteria: (1) clusters correspond to geographical areas within which an individual has social contact; (2) clusters should be unaltered over time; (3) clusters are allowed to be combined with administrative register data, which enforces cluster sizes to have at least 150 and 600 households for residential segregation analyses and descriptive purposes, respectively, as required by Statistics Denmark.

[20] Like Chetty & Hendren (2018a), when parents are separated, I always track the mother's location.

average, tend to have lower education levels.

The family structure of movers is also different than that of permanent residents. As Table 1 shows, while the fraction of intact families is 0.62 among permanent residents, it is only 0.45 and 0.39 for those who move exactly once and those who move between 1-3 times, respectively. Compared to permanent residents, movers are more likely to be single parents, as Figure A.2 of Appendix A shows. Figure A.2 also shows how the family structure evolves over the age of children, separately for permanent residents and movers. The differences in family structure between permanent residents and movers become more pronounced in later childhood years.

3 Exposure Effects: A Review of the Methodology

In this section, I briefly review the estimation strategy for assessing the neighborhood exposure effects originally proposed by Chetty & Hendren (2018a) (denoted by CH from now on) and subsequently adopted by the most notable empirical works in the literature. The notation is that of CH.

Let y_i denote the child's percentile rank in adulthood based on her position in the national income distribution relative to all others in her birth cohort.[21] Also, for the child i, let $p(i)$ be the parents' percentile rank in the national distribution of parental income for child i's birth cohort.[22] Now, let $\bar{y}_{pcs}$ denote the mean rank of children with parents at percentile p of the income distribution, residing in neighborhood c, and birth cohort s. The mean children's rank, given their parents' rank in each neighborhood c and birth cohort s can be approximated by a linear form as follows:[23]

$$y_i = \alpha_{cs} + \psi_{cs} p_i + \epsilon_i \tag{1}$$

[21]The disposable income at age 30 is used in the primary analysis.

[22]I compute parents' percentile ranks based on parents' family disposable income averaged over the sample period, i.e., 1982-2000.

[23]Figure B.1 of Appendix B provides evidence for a linear relationship. Figure B.1 illustrates how I estimate $\bar{y}_{pcs}$ for children born in 1970 to permanent residents of Copenhagen (who never left Copenhagen to other municipalities during the sample years, i.e., 1982-2000). The figure plots the mean child rank at age 30 within each percentile bin of the parent income distribution, $E[y_i|p(i) = p]$.

CH suggest obtaining estimates of $\bar{y}_{pcs}$ from the fitted values of the linear regression below:

$$\bar{y}_{pcs} = \hat{\alpha}_{cs} + \hat{\psi}_{cs} p \tag{2}$$

3.1 Definition of Exposure Effects

CH define the exposure effect at age m as "the impact of spending year m of one's childhood in an area where permanent residents' outcomes are one percentile point higher." They consider a thought experiment in which children are randomly assigned to new neighborhoods d starting at age m for the rest of their childhood. The best linear predictor of children's outcomes y_i in the experimental sample, based on the permanent residents' outcomes in destination neighborhood d $(\bar{y}_{pds})$, is as follows:

$$y_i = \alpha_m + \beta_m \bar{y}_{pds} + \theta_i \tag{3}$$

The random assignment guarantees that $\theta \perp \bar{y}_{pds}$. The exposure effect at m can be obtained as $\gamma_m = \beta_m - \beta_{m+1}$, i.e., the effect on y_i of spending the year from age m to age $m+1$ in the destination neighborhood. However, the observational data yields a regression coefficient $b_m = \beta_m + \delta_m$ where $\delta_m = \frac{cov(\theta_i, \bar{y}_{pds})}{var(\bar{y}_{pds})}$ is a standard selection effect measuring the extent to which parental inputs and other determinants of children's outcomes (of movers) covary with children's outcome of permanent residents. For identification purposes, CH impose Assumption A.1:

Assumption A.1. (***CH***)**:** *Selection effects do not vary with the child's age at the time of the move:* $\delta_m = \delta$ *for all m.*

Under A.1, consistent estimates of exposure effects can be obtained from observational data as

$$\gamma_m = \beta_m - \beta_{m+1} \tag{4}$$
$$= (b_m - \delta_m) - (b_{m+1} - \delta_{m+1})$$
$$= (b_m - b_{m+1}) - (\delta_m - \delta_{m+1}) \tag{5}$$
$$= b_m - b_{m+1} \tag{6}$$

where the first equation holds by the definition of exposure effects. The second one is derived by replacing β with its empirical counterpart from observational data, b, and correcting for the selection bias, δ. Equation (5) is obtained by rearranging terms. The last equation holds under the maintained identifying assumption, i.e., $\delta_m = \delta_{m+1}$ for each age, m.

The regression coefficient from the observational data is $b_m = \frac{cov(y_i, \bar{y}_{pds}|m)}{var(\bar{y}_{pds}|m)} = \frac{corr(y_i, \bar{y}_{pds}|m)\sigma_{y_i|m}}{\sigma_{\bar{y}_{pds}|m}}$, where $cov(y_i, \bar{y}_{pds}|m)$ is the covariance between the income ranks of children who moved across areas at age m and the average income rank of permanent residents' children at the destination, and $var(\bar{y}_{pds}|m)$ is the variance of the mean income rank of permanent residents' children at the destinations for children whose parents moved across areas when they were m years old. CH identify the exposure effect, γ, from the variation of the covariance, $cov(y_i, \bar{y}_{pds}|m)$, in the child's age when they move across neighborhoods. They assume that the covariance between all factors influencing children's income rank and income ranks of permanent residents' children at the destination is constant in the child's age at the time of the move. Children's innate ability, parental education, and school quality are a few examples of such factors, which are not fully captured by parent income rank. The covariation between these factors and permanent residents' outcomes may depend on the child's age when moving across neighborhoods. For example, those parents who are less credit-constrained or are more informed about the impact of school quality (local amenities) on child development might move across areas at earlier ages of their children. These affluent parents are also more likely to move to more expensive neighborhoods where children of permanent residents earn relatively more in adulthood. In this scenario, the correlation between outcomes of movers and permanent residents at the destination is higher for early movers than late movers due to the sorting of heterogeneous families into neighborhoods. Nevertheless, CH assume that, after conditioning on parents' income ranks, the covariation between all the inputs to the child development process and permanent residents' outcomes is uncorrelated with the child's age when moving across areas. Under this assumption, they attribute the differences in $cov(y_i, \bar{y}_{pds}|m)$ between children who move across regions at different ages to the impact of neighborhood.

Using a similar but stronger identifying assumption, Chetty & Hendren (2018b) present the causal effect of each county and commuting zone in the US. They assume that the selection effect is constant in the child's age at the time of the move for *each* origin-destination pair. In other words, Chetty & Hendren (2018b) assume that the selection effect does not vary with the child's age at the move within origin-destination pairs. Their findings, built on these identifying assumptions, lead them to conclude that "every additional year spent growing up in Salt Lake City will increase a child's income by 0.17 percentiles relative to an average commuting zone", while "on the other hand, every additional year spent growing up in New Orleans is predicted to reduce a child's income by 0.21 percentiles."

3.2 Discussion of the Identifying Assumptions

Equation (4) implies that consistent estimates of exposure effects can be obtained from observational data using equation (6) because the selection effect, δ, cancels out when estimating the exposure effect.

Assumption A.1 rules out differential preferences among parents by the child's age for local amenities, such as school quality, that are not fully captured in adult income percentile rank $\bar{y}_{pds}$.

Next, I elaborate on the implications of a potential violation of Assumption A.1 in CH for the exposure effect estimates from observational data.

In case of a violation of Assumption A.1, one of the three different cases below can emerge:

1. If selection intensity decreases with the child's age at the time of the move: $\delta_m >$

$\delta_{m+1} \quad \forall m \in \{\underline{m}, ..., \overline{m}\}$, then equation (6) overestimates the exposure effect, γ_m.

2. If selection intensity increases with the age of the child at the time of the move: $\delta_m < \delta_{m+1} \quad \forall m \in \{\underline{m}, ..., \overline{m}\}$, then equation (6) underestimates the exposure effect, γ_m.

3. If selection intensity changes non-monotonically over the age support, the direction of the bias in the average exposure effect is unknown.

For the sake of exposition, here, I use parents' education level, edu_i^p, as an example of an omitted variable affecting both child's outcomes and the choice of the destination neighborhood. To elaborate, suppose that the true model is as follows:

$$y_i = \alpha_m + \beta_m \bar{y}_{dps} + \beta_e edu_i^p + u_{im}, \tag{7}$$

while we estimate equation (3), repeated here: $y_i = \alpha_m + \beta_m \bar{y}_{pds} + \theta_i$. Then,

$$\begin{aligned} Plim\ \hat{\beta}_m &= \beta_m + \beta_e \frac{cov(edu_i^p, \bar{y}_{pds}|m)}{var(\bar{y}_{pds}|m)} \\ &= \beta_m + \beta_e \delta_m \end{aligned}$$

$$Plim\ \hat{\gamma}_m = (\beta_m - \beta_{m+1}) + \beta_e(\delta_m - \delta_{m+1}) \tag{8}$$

The second term in equation (8) indicates that if Assumption A.1 is violated (i.e., $\delta_m \neq \delta_{m+1}$), then $b_m - b_{m+1}$ (equation 6) is an inconsistent estimator of the exposure effect parameter. Equation (8) suggests that if the extent to which the unobserved parental inputs covary with permanent residents' outcomes depends on the child's age when her family moves, the estimates of exposure effects from observational data are inconsistent. The direction of the bias in the estimates of exposure effects depends on the sign of the second term in Equation (8), $\beta_e(\delta_m - \delta_{m+1})$. If, for example, the covariance between omitted factors and permanent residents' outcomes is decreasing in the child's age at the time of the move, i.e., $\delta_m - \delta_{m+1} > 0$, then, assuming $\beta_e > 0$, there is an upward bias in estimates of exposure effects from equation (6). Section 5 analyzes selection patterns over the child's age at the time of the move to identify which of the three cases mentioned above emerges in the data. The empirical results prove that the selection intensity decreases with the child's age at the time of the move (case 1 discussed above). As a result, there is an upward bias in estimates of exposure effects from Equation (6).

3.3 Empirical Implementation

For the estimation sample, CH use both the subsample of permanent residents and the subsample of families who move across neighborhoods exactly once during the sample period.

CH first consider the children whose families moved when they were exactly m years old. They analyze how children's incomes in adulthood are related to those of permanent residents in their destination neighborhood using the linear regression below, which they interpret as an observational analog of the specification in Equation (3):

$$y_i = \alpha_{qos} + b_m \Delta_{odps} + \epsilon_{1i}, \tag{9}$$

where y_i denotes the child's income rank at age 30 and α_{qos} is a fixed effect for the origin neighborhood o by parent income decile q by birth cohort s. Also, $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ is the mean difference in permanent residents' income ranks (at age 30) between the destination and origin for the relevant parent income rank p and birth cohort s. CH describe Δ_{odps} as the difference in mean *predicted* income rank of permanent residents in the destination versus origin for the relevant birth cohort and parent income rank.[24] A regression coefficient estimate of $b_{13} = 0.5$, for example, implies that conditional on $\bar{y}_{pos}$, a one percentile increase in $\bar{y}_{pds}$ is associated with a 0.5 percentile increase in income rank (at age 30) for the children who move at age 13.

Generalizing Equation (9), CH estimate equivalent regression coefficients b_m for children

[24] Figure B.2 of Appendix B presents a nonparametric binned scatter plot corresponding to the regression in Equation (9) for children who first move at age $m = 13$. To construct Figure B.2, as in CH, I first demean both y_i and Δ_{odps} within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $y_i^r = y_i - E[y_i|q, o, s]$ and $\Delta^r_{odps} = \Delta_{odps} - E[\Delta_{odps}|q, o, s]$. I then divide the Δ_{odps} residuals into 20 equal-size groups and plot the mean value of y_i^r versus the mean value of Δ^r_{odps} in each bin. Consistent with the findings of CH, Figure B.2 suggests that income at 30 is higher for those children who move to neighborhoods where children of permanent residents have a higher income at 30.

whose families move at each age m as follows:

$$y_i = \alpha_{qosm} + \sum_{m=1}^{25} \beta_m I(m_i = m)\Delta_{odps} + \sum_{s=1970}^{1981} \kappa_s I(s_i = s)\Delta_{odps} + \epsilon_{2i}, \tag{10}$$

where α_{qosm} is an origin neighborhood by parent income rank (decile) by birth cohort by age at the time of the move fixed effect, and $I(x_i = x)$ is an indicator function that is one when $x_i = x$ and zero otherwise.[25]

Equation (10) entails thousands of fixed effects (α_{qosm}). Therefore, CH suggest a parametric counterpart of Equation (10) as follows:

$$\begin{aligned} y_i = &\sum_{s=1970}^{1982} \kappa_s I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}) + \sum_{m=1}^{25} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \\ &+ \sum_{m=1}^{25} \beta_m I(m_i = m)\Delta_{odps} + \sum_{s=1970}^{1981} \kappa_s^d I(s_i = s)\Delta_{odps} + \epsilon_{3i}, \end{aligned} \tag{11}$$

Now, parameterized linearly, CH replace the nonparametric $\sum_{m=1}^{25} \beta_m I(m_i = m)\Delta_{odps}$ term in Equation (11) with an intercept and slope. They also allow for a different slope after age 23, as follows:

$$\begin{aligned} y_i = &\sum_{s=1970}^{1982} \kappa_s I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{y}_{pos}) + \sum_{m=1}^{25} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \\ &+ \sum_{s=1970}^{1981} \kappa_s^d I(s_i = s)\Delta_{odps} + I(m_i \leq 23)(b_0 + (23 - m_i)\gamma)\Delta_{odps} \\ &+ I(m_i > 23)(\delta_0 + (23 - m_i)\delta')\Delta_{odps} + \epsilon_{3i}, \end{aligned} \tag{12}$$

Estimating this specification, CH find an average annual exposure effect, γ, in the range of $[0.031, 0.043]$ at the commuting zone level (CH, Table II) and in the range of $[0.022, 0.037]$ at the county level (CH, Online Appendix Table A.5). In a subsequent study, Chetty *et al.* (2020a) find an average annual exposure effect of 0.027 at the Census tract level (Chetty *et al.* (2020a), Table IV).

Chetty *et al.* (2020a) argue that the average convergence rate of 2.7% per year of exposure between the ages of 0 and 23 implies that children who move at age 0 would pick up about $23 \times 0.027 = 62\%$ of the observed difference in permanent residents' outcomes between their origin and destination Census tracts. Using this back-of-the-envelop calculation, they con-

[25]While CH only study the sample of children (born between 1980 to 1988) whose parents move when the child is older than 9 years of age, I use the sample of children (born between 1970 to 1982) whose parents move when the child is between 1 to 25 years old.

clude that 62% of the observational variation in outcomes across Census tracts reflects the causal effects of neighborhoods rather than sorting. Similarly, Chetty & Hendren (2018b), based on their estimate of 0.04, argue that "many of the correlations between area-level characteristics and upward mobility are driven almost entirely by causal effects of place."

4 Replicating Chetty & Hendren (2018a)

Figure 1 plots the coefficients b_m obtained from estimating Equation (11), replicating Figure IV of CH using data from Denmark. Table 2 replicates Table II of CH and presents estimates of the exposure effect parameter, γ, in Equation (12). Unlike CH and Chetty *et al.* (2020a), who are interested in children's family income rank in adulthood, I mainly focus on children's individual income rank later in life. As a robustness check, in column 5, I also use children's family income, i.e., children's income plus their spouse's income.[26] The results presented in Table 2 suggest an exposure effect of 2.3% per year, which is close to the 2.7% in Chetty *et al.* (2020a) using the Census tract data, but it is smaller than 4% in CH using the commuting zone data. There are two remarks worth making about these estimates. First, similar to CH, the estimate of childhood exposure is robust to various specifications in columns 2-6 of Table 2. Second, akin to CH and Chetty *et al.* (2020a), I obtain similar exposure estimates using the family fixed-effect model that exploits variations in exposure among siblings. Columns 7-9 of Table 2 present the estimates of exposure effect using a family fixed-effect model. The estimates range from 1.7% to 2.3%. These estimates are similar to the 2.1% in Chetty *et al.* (2020a) but smaller than the 3.1%-4.4% in CH.[27]

It is noteworthy that, like CH, I observe a pattern similar to Figure 1 when using other outcomes of children such as marriage.

[26] Both individual income and family income might be of interest. Section 6 presents placebo tests that exploit birth characteristics, such as birth weight. These measures are not well-defined at the family level as most of the individuals in the sample, for whom data on birth characteristics is available, are still single in the last year of the data. I, therefore, focus on individual-level rather than family-level outcomes for both income analyses and placebo tests.

[27] As in CH, the standard deviations of family fixed effect estimates are about four times higher than those of the baseline.

4.1 Heterogeneity of Effects

Homeownership status is one of the most crucial dimensions of heterogeneity among movers, which is overlooked in the previous studies in the literature. Housing is a form of committed consumption good, which requires substantial expenditures and is difficult to adjust in the short run. Also, buying a house is a commitment to future cash outflows (see Banerjee (2011) and Alpanda & Zubairy (2019)). In this section, I first explore how the timing of moves across neighborhoods varies by parents' homeownership status for the sample of movers. Figure B.3 of Appendix F presents the results, suggesting that homeowners tend to move across neighborhoods when their children are older, when compared to renters. They also tend to be more educated and wealthier. Neighborhood exposure effect estimates presented earlier in this section are plagued when researchers overlook this critical dimension of heterogeneity across movers, which is the case in previous studies of exposure effects.

I assess the sensitivity of the exposure effect estimates presented earlier in Table 2 to the homeownership status of parents.[28] Table 3 presents the exposure effect estimates, separately for the sample of homeowners (panel A) and renters (Panel B). The results of the baseline specification, presented in columns 1 to 6, suggest that, compared to renters, the exposure effect estimates for the sample of homeowners are about 50% larger (0.18-0.27 vs. 0.11-0.18).

On the other hand, the family fixed-effect estimates, presented in columns 7 to 9, draw a completely different picture, i.e., the estimates are about zero for homeowners while they are sizable for the sample of renters (ranging from 0.026 to 0.035).

5 The Identifying Assumptions: Empirical Evidence

CH, Chetty & Hendren (2018b), and Chetty *et al.* (2020b) are unique in their approach for

[28] I define homeowners (renters) as those who were homeowners (renters) just before and right after moving across municipalities. Defined so, homeowners and renters cover more than 80% of the movers. I abstract away from those who change their ownership status (from renter to owner and vice versa) during the transition across municipalities.

identifying neighborhood effects. They abstract away from modeling individuals' sorting behavior based on observable and unobservable characteristics and the spatial distribution of local amenities and taxes, which determine the equilibrium distribution of families across neighborhoods. The neighborhood quality in these studies is defined by adult outcomes of children of permanent residents (non-movers) of each neighborhood given birth cohort and parent income rank. In other words, these studies use an output of the skill formation process (i.e., outcomes of children of permanent residents in each neighborhood) to define an input to the child development process (i.e., neighborhood quality). If plausible, this is an intriguing approach. One would not need to exploit data on local public goods, such as school quality, safety, housing stock, air quality, water quality, local amenities including parks and medical centers, and neighbor peer-groups.

There are several possible issues with this approach. First, neighborhood causal impacts are identified solely by comparing outcomes of children of families who move across areas (self-select into neighborhoods) to those who never moved across neighborhoods after their initial sorting into neighborhoods. Second, this approach does not explain why place matters in shaping outcomes of children and what policy implications of findings are.

Moreover, CH's claim to causality rests on their constant-in-age selection assumption, which is empirically analyzed later in this section. I show this assumption to be too strong. The results of CH suggest that *conditional* on moving, the earlier the move occurs in childhood, the more similar is the expected adulthood outcomes of children (of movers) to the outcomes of children of permanent residents in the destination, all else (i.e. parent income rank) equal. While CH argue that this relationship reflects the causal effect of neighborhoods on child outcomes, I provide evidence suggesting that the relationship cannot be interpreted as causal. Instead, it reflects that families sort into neighborhoods, and the sorting pattern across the lifecycle is heterogeneous; i.e., the extent to which there is a selection into better neighborhoods is not orthogonal to the child's age when parents move. I provide evidence that the identification assumption in CH is violated, and the exposure effect estimates mirror the correlational estimates of place effects in Chetty *et al.* (2014).

In Section 3.2, I discussed three different scenarios in which the identifying assumption, i.e., the constant selection effect, is violated. This section empirically investigates which scenario is supported by the data. Second, I elaborate on another estimation strategy of CH. Specifically, I discuss their family fixed effect model in detail.

5.1 Selection and the Age of the Child at the Time of the Move

To investigate the constant-in-age selection effect assumption, one would need to investigate if parent inputs and observed (or unobserved) determinants of children's outcomes covary with permanent resident's outcomes. In what follows, I analyze the relationship between the child's age when parents move and parental characteristics, parental sorting behavior, and family structure. I also investigate the heterogeneity of such relationships by the homeownership status of parents. Focusing on factors related to both children's outcomes and the quality of the move, I explore how such relationships vary by the child's age when parents move. This analysis uses the same sample used for the exposure effect estimation analysis presented earlier in Table 2 (Section 4), which consists of those families who move across municipalities exactly once during the sample years.

5.1.1 Parental Characteristics and the Age of the Child at the Time of the Move

(I) Education: Panel (a) of Figure 2 presents a binned scatter plot of the relationship between the child's age when parents move and parental education level, which illustrates that the two variables are inversely related.[29]

Next, I discuss the impact of the negative correlation between the child's age at the time of the move and parental education level, presented in Figure 2, on the exposure effect esti-

[29]Figure C.1 of Appendix C repeats the analysis in Figure 2, separately for homeowners and renters. A similar pattern emerges, suggesting that the child's age at the time of the move and parental education level are inversely related for both homeowners and renters. However, parental education level decreases with the child's age when parents move with a higher rate for the sample of renters (a reduction of 0.061 per year vs. 0.045 per year). Figure C.1 also suggests that, compared to renters, homeowners tend to be more educated.

mates, γ_m, in Equation (8) in Section 3.2. From Equation (8), we have:

$$Plim\ \hat{\gamma}_m = (\beta_m - \beta_{m+1}) + \beta_e(\delta_m - \delta_{m+1})$$

where the second term, $\beta_e(\delta_m - \delta_{m+1})$ is the by-age selection bias, and $\delta_m = \frac{cov(edu_i^p, \Delta opds|m)}{var(\Delta opds|m)}$. As discussed before, the direction of the bias in the exposure effect estimates, γ_m, depends on the sign of $\beta_e(\delta_m - \delta_{m+1})$. Using Equation (7) to estimate β_e, I find values close to one. Therefore, I focus on $(\delta_m - \delta_{m+1})$. Panel (b) of Figure 2 plots the estimates of the δ terms over the child's age when parents move, m. The pattern indicates that the extent to which parental education level of movers covaries with children's outcome of permanent residents is not constant over the child's age when parents move, which means the assumption of a constant selection bias is violated. Panel (b) of Figure 2 suggests that the selection intensity decreases with the child's age at the time of the move. The slope of the fitted line, -0.005, provides a linear approximation of the bias term $(\delta_m - \delta_{m+1})$ in the exposure effect estimates solely due to the omitted variable bias arising from overlooking parental education when estimating the childhood exposure effect in the manner of CH.

(II) Income: Panel (a) of Figure 3 shows a binned scatter plot depicting the relationship between the age of the child when parents move and parental income rank in the national income distribution, suggesting that parental income rank is negatively correlated with the child's age when parents move across municipalities.[30] The inverse association between parental income rank and the child's age when moving holds for both homeowners and renters.[31]

(III) Family Structure: Now, I examine how the likelihood of being raised in an intact family varies with the child's age when parents move. For each child, I define an intact family as follows: the family is intact if the child's parents live together during the first 18

[30] The relationship holds till age 20 of the child when parents move. After age 20, parental income rank slightly increases with the child's age when parents move.

[31] Figure C.2 of Appendix C repeats the analysis in Panel (a) of Figure 3, separately for homeowners and renters. The results suggest that parental income rank is negatively correlated with the child's age when parents move for both homeowners and renters. However, the relationship is not linear for the sample of homeowners; parental income rank decreases sharply with the child's age among early movers (up to age 11 of the child when parents move) and decreases with a lower rate after age 11. Also, approximated linearly, parental income rank decreases with the child's age with a higher rate for the sample of renters (a reduction of 0.9 per year vs. 0.5 per year).

years of childhood, regardless of their legal marital status. Panel (b) of Figure 3 shows a binned scatter plot depicting the relationship between the child's age when parents move and the fraction of intact families. The pattern indicates that the fraction of intact families is negatively correlated with the child's age when parents move up to 20 years old of children.[32]

Altogether, Figures 2-3 suggest that, compared to late movers, families who move when their children are younger tend to be more affluent, more educated, and more stable.

5.1.2 Sorting to Neighborhoods and the Age of the Child

Earlier in this section, I explored the relationship between parents' characteristics (income rank, education level, and family structure) and the child's age when parents move. In what follows, I analyze the relationship between the child's age when parents move and parental neighborhood choice.

Quality of the Moves: Figure 4 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the mean income rank of children of permanent residents when moving from the origin to the destination neighborhood (i.e., $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ defined earlier in Section 3.3).[33] Figure 4 suggests that, on average, the quality of the moves (approximated by the difference between the mean income rank of children of permanent residents of the destination neighborhood vs. the origin neighborhood) that take place later in childhood years are lower than those made in early childhood. This pattern is not surprising given the sorting patterns documented earlier in Section 5.1.1, which show earlier movers tend to be more educated, more affluent, and more stable.[34]

[32]Figure C.3 of Appendix C presents the relationship between the likelihood of an intact family and the child's age when parents move, separately for homeowners and renters. The general pattern is similar to Figure 3. Approximated linearly, the fraction of intact families decreases with the child's age with a higher rate for the sample of renters (a reduction of 0.013 per year vs. 0.003 per year).

[33]Figure D.1 of Appendix D shows the distribution of the quality of the moves for the sample of movers who moved exactly once across municipalities between 1982 and 2000.

[34]Figure C.6 of Appendix C depicts the relationship between the child's age when parents move and the increase in the income rank of children of permanent residents when moving from the origin to the destination neighborhood, separately for homeowners and renters. Consistent with previous results presented in this paper, homeowners, on average, make moves of higher quality (proxied by the mean income rank of children of the permanent residents of the neighborhood).

While Figure 4 shows how the difference in income ranks of permanent residents in the destination versus origin varies with the child's age when moving, Panel (a) of Figure 5 focuses only on the income rank of children of permanent residents of the origin area (i.e., $\bar{y}_{pos}$ defined earlier in Section 3.3). By doing so, Panel (a) of Figure 5 presents the heterogeneity in the initial sorting of families into neighborhoods by the child's age when parents move to a new neighborhood. Panel (a) of Figure 5 suggests that early movers initially sort into higher-quality neighborhoods (where quality is proxied by the income rank of children of the permanent residents of the neighborhood) at a rate of 0.07 per year.

On the other hand, Panel (b) of Figure 5 focuses on the income rank of children of permanent residents of the destination neighborhood (i.e., $\bar{y}_{pds}$ defined in Section 3.3). It presents the heterogeneity in neighborhood selection by the child's age at the time their parents move to a new area. Panel (b) of Figure 5 suggests that early movers tend to self-select into better destination neighborhoods at a rate of 0.12 per year.[35] The results suggest that the quality of the moves, on average, decreases as the move happens later in childhood years. Also, controlling for the neighborhood of origin and parent income ranks is not enough to capture the lifecycle dynamics of the neighborhood sorting process. In other words, the child's age at the time of the move is not orthogonal to the extent to which there is self-selection into a better neighborhood.[36]

Alternative Measures of Neighborhood Unit and Neighborhood Quality: Above, I analyzed the relationship between the age of the child when parents move and the quality of the moves across neighborhoods (Figures 4-5). Now, I use alternative measures of neighbor-

[35] I also investigate if controlling for the neighborhood of origin interacted with parental income decile and birth cohort, as CH do, changes the relationship between the child's age and the quality of the moves across areas observed in Figure 4. Nevertheless, the pattern is similar to Figure 4.

[36] Figures C.4 and C.5 of Appendix C depict the relationship between the age of the child when parents move and the income rank of children of permanent residents of the origin and the destination neighborhood, separately for homeowners and renters. Consistent with previous results presented in this paper, homeowners, on average, sort into better neighborhoods and move to higher-quality neighborhoods when moving to a new area. Also, compared to families who move across areas when their children are older, early movers sort initially into better neighborhoods and self-select into higher-quality neighborhoods when moving to a new area. Figures C.4 and C.5 of Appendix C suggest a nonlinear relationship between neighborhood quality and the age of the child when parents move for the sample of homeowners. There is also a tiny uptick after age 20.

hood quality. For a given neighborhood, I compute the average income rank of permanent residents in the national distribution of household income in Denmark. I also use the average house price rank in each area and a measure of school quality. Moreover, I use alternative neighborhood units, i.e., parish and neighborhood clusters to evaluate the quality of the origin and destination areas. I use contiguous clusters of around 600 households that exhaust the whole population of Denmark. Hence, instead of focusing on municipalities, I focus on a more granular neighborhood unit, including 600 households. These alternative units of neighborhoods might better capture the heterogeneity in the quality of the moves within the benchmark neighborhood unit, i.e., at the municipality level. The reason is that it is plausible that families sort into neighborhoods at different layers. They sort into municipalities (commuting zones in CH), and given the municipality (commuting zone), they still sort into different areas according to their characteristics and preferences for local amenities, such as school quality.

(I) Neighborhood Average Income: Panel (a) of Figure D.2 in Appendix D presents a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the parish income rank (where all parishes are ranked based on their average household income) during the year the family moves.[37] Panel (b) of Figure D.2 shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the cluster income rank (where all clusters are ranked based on their average household income) during the year the family moves.[38] Figure D.2 suggests that those who move when their children are younger, on average, move to areas whose residents are more affluent.

(II) Neighborhood Average House Price: Figure D.3 of Appendix D shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the neighborhood house price rank (measured at parish level) when moving

[37] Figure C.7 of Appendix C repeats the same analysis, separately for homeowners and renters.
[38] Figure C.8 of Appendix C repeats the same analysis, separately for homeowners and renters.

from the origin to the destination.[39] Also, Figure D.4 of Appendix D presents the results of a similar exercise where house price ranks are computed at the municipality level (compared to parish-level ranks in Figure D.3).[40]

Figure D.3 suggests that those who move when their children are younger, on average, move to more expensive areas. The more expensive areas may have better local amenities, such as school quality. I test this hypothesis by investigating the relationship between the school quality at the destination and the age of the child when parents move.

(III) Neighborhood Average School Quality: Figure D.5 of Appendix D shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the neighborhood school quality rank (at parish level), during the year the family moves.[41,42] In general, Figure D.5 indicates an inverse relationship between school quality and the age of the child at the move across municipalities.

It is noteworthy that the patterns in Figures 2-5 and the supplementary figures presented in Appendix D are robust to various specifications and controls for cohort effects, origin neighborhood effects, and parent income ranks.[43]

5.1.3 Timing of the Moves and Lifecycle Shocks

In this section, I investigate the relationship between lifecycle events, such as marital status changes and income shocks, and the age of the child when parents move. Panel (a) of Figure 6 presents the fraction of parents living together just before the move but not right after. The fraction increases in the child's age when moving till age 20.[44]

[39] Figure C.9 of Appendix C repeats the same analysis, separately for the samples of homeowners and renters.

[40] Figure C.10 of Appendix C repeats the same analysis, separately for homeowners and renters.

[41] I rank parishes based on their average school quality level while school quality is proxied by the average test scores of 9th-grade students who attended the school between 2002 and 2015. I assume that the school quality ranks of parishes are time-invariant, so the exact ranking is valid for previous years (1982-2000), for which data on test scores is not available.

[42] Figure C.11 of Appendix C repeats the same analysis, separately for homeowners and renters.

[43] Specifically, the patterns remain the same when I control for the factors mentioned above in the same fashion presented in Equation 9, i.e., by including α_{qos}, which is a fixed effect for the origin neighborhood o by parent income decile q by birth cohort s.

[44] Figure C.12 of Appendix C repeats the same analysis, separately for the samples of homeowners and renters.

Panel (b) of Figure 6 presents a binned scatter plot depicting the relationship between the child's age when parents move and the increase in parental rank in the national income distribution during the year they move. It suggests that, in general, the older the child when parents move, the smaller the (positive) shock to the parental income around the time parents move across neighborhoods.

Figure 6 suggests that the lifecycle shocks to family status and income of parents are not orthogonal to the child's age when parents move. These shocks most likely affect parental investments in children and children's outcomes in adulthood through channels other than neighborhood choice. Neglecting these dynamics makes the estimates of exposure effects from Equations 9-12 biased.

5.2 The Family Fixed Effect Model of Exposure Effects

To mitigate concerns about the validity of constant-in-age selection effects (Assumption A.1), as a robustness check, CH also use a family fixed effect regression, which uses variations in the age gap between siblings to identify the neighborhood exposure effect.[45] The idea is that when a family moves to a new neighborhood, the younger sibling will be exposed to the new neighborhood for a longer time. One of the implicit identification assumptions here is that the age space between siblings is exogenous, which is too strong an assumption. Also, one cannot separately estimate the impact of the age space from the exposure effect as the two are perfectly collinear.[46] The determinants and consequences of birth spacing have been documented in several studies in the literature (Zajonc (1976); Galbraith (1982); Rosenzweig (1986); Rosenzweig & Wolpin (1988); Buckles & Munnich (2012); Broman *et al.* (2017); Golsteyn & Magnée (2017); Joensen & Nielsen (2018)).

To demonstrate these points in a formal context, consider Equation (11) after adding a family-fixed effect component to the equation. Now, I can write $\epsilon_{3i} = \hat{\theta}_{f,i} + e_i$, where $\hat{\theta}_{f,i}$

[45] While the point estimates from the family fixed-effect model are close to their baseline model, the standard deviations of family fixed effect estimates are about four times higher than those of the baseline.

[46] See Durlauf (2004) for the econometrics issues associated with using sibling data to uncover neighborhood effects.

would capture fixed family inputs, such as culture and parents' human capital for individual i in family f, and e_i is variable inputs, such as wealth shocks.

The constant-in-age selection assumption requires $\delta_m = \frac{cov(\epsilon_{3i}, \Delta opds)}{var(\Delta opds)}$ to be constant in the child's age when parents move, m. Including family fixed effects controls for $\hat{\theta}_f$ component of ϵ. For example, higher-skill families might choose better neighborhoods. To interpret results as causal, $\frac{cov(e_i, \bar{y}_{pds})}{var(\bar{y}_{pds})}$ needs to be constant in the child's age at the time of the move. This condition, however, is too strong. To demonstrate how strong this condition is, I give an example in which this condition is violated even when the arrival of children (the timing of parenthood) is random. Everything else constant, the arrival of a second child leads to lower resources (per capita) available for investments in children. It may then act as a negative wealth (investment) shock to the first child. The magnitude of the shock is correlated with the first child's age when the second child arrives. The dependence of wealth shocks on the timing of births, among other factors, is due to the economy of scale. There might be meaningful differences between families where children are two years vs., for example, eight years apart. When the age gap between the first two children is smaller, they are more likely to share some resources from which both children can benefit; some investments by parents are not specific to the older (younger) child.[47] This is just an example of a possible violation of the constant-in-age selection effects in a family fixed-effect model.

In what follows, using the sample of permanent residents, I provide empirical evidence suggesting that the age gap is endogenous. First, panel (a) of Figure 7 presents a binned scatter plot depicting the relationship between the age gaps among siblings and the differences in their educational attainments, i.e., the years of schooling of the older child minus the years of schooling of the younger one. Figure 7 suggests that, when benchmarked against their older siblings, younger siblings born after longer intervals attain higher education than those born after shorter intervals. These results are consistent with the findings of Buckles & Munnich (2012) and Broman *et al.* (2017). Broman *et al.* (2017) find that younger siblings

[47]For instance, when the age space is two years, parents might read the same book to both children once, which may not be possible when children are eight years apart.

born after longer intervals scored higher on the Stanford-Binet intelligence scale than those born after shorter birth intervals. Buckles & Munnich (2012) use miscarriages as instrumental variables and find that a one-year increase in spacing increases reading test scores for older siblings by about 0.17 standard deviations.[48]

Second, Panel (b) of Figure 7 presents a binned scatter plot depicting the relationship between the age gap among siblings and the difference in their adult income rank, i.e., the income rank of the older child minus the income rank of the younger one. The income rank (at age 30) is computed relative to all others in the child's birth cohort. Figure 7 suggests that the difference in siblings' outcomes is negatively correlated with the age gap between them, which means that the difference in outcomes between siblings is correlated with the difference in the duration of exposure to the destination neighborhood between siblings through channels other than neighborhood choice. This correlation provides evidence for an endogeneity problem when estimating exposure effects using variations in the age space between siblings.

Overall, the results presented in this section point to a violation of the identifying assumptions in the previous studies of exposure effects. In the next section, I conduct a placebo test to examine the impact of this violation on the estimates of neighborhood effects.

6 A Placebo Test

The challenge in estimating neighborhood impacts on adults and children is the nonrandom selection of individuals into neighborhoods. Individuals have an opportunity to move to neighborhoods where their idiosyncratic preferences are best satisfied by the neighborhood's bundle of local services, and the local property tax is the price of the services (Tiebout (1956); Rosen (1974)). This leads to correlate neighborhood attributes to individual characteristics. As a result, the underlying sorting pattern of individuals across neighborhoods plagues the estimates of neighborhood impact in nonexperimental studies (Ludwig *et al.* (2008)).

[48] Also, Rosenzweig & Wolpin (1988) find negative consequences for birth weight of short birth intervals.

CH argue that they present "estimates of exposure effects, addressing the concerns about selection and omitted variable bias that arise in observational studies." In this section, I conduct a placebo test to examine the credibility of the estimation strategies for identifying the causal impact of neighborhoods in Chetty & Hendren (2018a), Chetty & Hendren (2018b), and Chetty *et al.* (2020a).

A placebo test is the most direct way to gauge the extent to which the neighborhood exposure estimates in CH are driven by the sorting of heterogeneous families across neighborhoods with different amenities rather than by causal impacts of neighborhoods on children's outcomes in their adulthood. To this end, I exploit the data on birth characteristics of children born between 1997-2005 in Denmark.[49] While CH investigate how children's earnings in adulthood are related to the quality of the destination neighborhood and the child's age when moving, I examine how a child's birth weight is related to such factors (i.e., the quality of destination neighborhood and the age of the child at the time of the move). Previous papers have established that birth weight is a factor that is positively correlated with later outcomes in adulthood (see Black *et al.* (2007)). In this framework, I pretend that an individual's birth weight is an outcome observed in adulthood while allowing it to be influenced by the quality of the neighborhoods where individuals were living during childhood and the duration of their exposure.

Nevertheless, we know that an individual's birth weight is realized and measured at age zero, i.e., before neighborhood exposure comes into play (i.e., long before parents move from one neighborhood to another). Hence, the neighborhoods of residence in childhood cannot have a causal impact on a child's birth weight. Otherwise, the effect would be preceding the cause. By applying the same methodology suggested by CH to investigate the relationship between characteristics realized at birth and later moves across neighborhoods during childhood, I expect to obtain insignificant estimates. On the other hand, if CH's estimates are not due to a causal impact of neighborhoods on children but picking up sorting patterns across neighborhoods by heterogeneous parents, I may observe a pattern for the impact of neigh-

[49] Table E.1 of Appendix E presents the summary statistics of the sample.

borhoods on birth characteristics similar to the one I observe for later life outcomes, such as earnings at age 30.

Following CH, I identify the permanent residents of each municipality and the sample of families who moved exactly once across municipalities during the sample years (1997-2019). The outcome of interest is the child's percentile rank on her position in the national birth weight distribution relative to all others in her birth cohort. CH characterize neighborhoods based on the mean adult outcomes (measured as percentile rank) of children spending their entire childhood in an area (i.e., permanent residents) conditional on parental income rank. Similarly, I characterize each neighborhood by the mean birth weight of permanent residents' children conditional on parental income rank.

To this end, I modify Equations 11 and 12 as follows:

$$\begin{aligned} bw_i = &\sum_{s=1997}^{2005} \kappa_s I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{bw}_{pos}) + \sum_{m=1}^{20} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \\ &+ \sum_{m=1}^{20} \beta_m I(m_i = m)\Delta_{odps}^{bw} + \sum_{s=1997}^{2004} \kappa_s^d I(s_i = s)\Delta_{odps}^{bw} + \epsilon_{3i}, \end{aligned} \tag{13}$$

and

$$\begin{aligned} bw_i = &\sum_{s=1997}^{2005} \kappa_s I(s_i = s)(\alpha_s^1 + \alpha_s^2 \bar{bw}_{pos}) + \sum_{m=1}^{20} I(m_i = m)(\zeta_m^1 + \zeta_m^2 p_i) \\ &+ \sum_{s=1997}^{2004} \kappa_s^d I(s_i = s)\Delta_{odps}^{bw} + I(m_i \geq 0)(b_0 + m_i\gamma)\Delta_{odps}^{bw} \\ &+ I(m_i < 0)(\delta_0 + m_i\delta')\Delta_{odps}^{bw} + \epsilon_{3i}, \end{aligned} \tag{14}$$

where bw_i denotes the child's percentile rank on her position in the national birth weight distribution relative to all others in her birth cohort, and $\Delta_{odps}^{bw} = \bar{bw}_{pds} - \bar{bw}_{pos}$ is the mean difference in permanent residents' birth weight ranks between the destination and origin for the relevant parent income rank p and birth cohort s.

Figure 8 plots the coefficients b_m obtained from estimating Equation (13). Table 4 presents estimates of the placebo effect parameter, γ, in Equation (14). CH find that the incomes of children who move across US commuting zones converge to the incomes of permanent residents in the destination at a rate of 4% per year of childhood exposure. Table 4 implies that the extent to which birth weight ranks of children whose parents move across areas covary with the birth weight ranks of permanent residents in the destination decrease by 2.7% per year of age of children when their parents move. In other words, birth weight ranks of children covary 2.7% more with the birth weight ranks of permanent residents in the destination for those children whose parents move one year earlier in the childhood, e.g., at age seven vs. age eight of their child. This result is strikingly similar to the exposure effect estimates in Chetty *et al.* (2020a) when they focus on movers across US Census tracts. They found a childhood exposure effect of 2.7% per year (2.1% using the family fixed effect model).

There are three remarks worth making about the placebo effect estimates. First, Table 4 indicates that the estimates are robust to various specifications proposed by Chetty & Hendren (2018a), which were replicated earlier in Table 2 using child income as the measure of adulthood outcome. Table 4 shows that the placebo estimates are close to the exposure estimates using the income data presented earlier in Table 2. This is especially interesting as the two analyses use two different sets of birth cohorts who are, on average, 25 years apart.

Second, Table 4 also presents the results of the family fixed effect model that exploits variations among siblings in their birth weight and the duration of exposure to neighborhoods. Similar to Chetty *et al.* (2020a) and Chetty & Hendren (2018a), the family fixed-effect model estimates are both slightly smaller and less precisely estimated.

Third, clearly, these results cannot be interpreted as causal impacts of neighborhood exposure in childhood as the outcome of interest is realized and measured at age zero of children. Instead, the results demonstrate how the correlation between movers and permanent residents crucially depends on the child's age when parents move; the correlations are higher for those who move across areas earlier in their childhood. These results demonstrate that the identifying assumptions of CH are untenable, as discussed earlier in Section 5. The violation of the identifying assumptions results in an upward bias to the estimates of place effects.

6.1 Heterogeneity of Placebo Effects

Table F.1 of Appendix F presents the placebo exposure effect estimates separately for the sample of homeowners (Panel A) and renters (Panel B). Two interesting patterns emerge, resembling the heterogeneity results presented for the income analysis in Section 4.1. First, compared to renters, the exposure effect estimates for homeowners are significantly larger (0.30-0.37 vs. 0.17-0.31). Second, the family fixed-effect estimates show a different pattern, i.e., the estimates are lower for homeowners than the sample of renters. These patterns are similar to those obtained from the heterogeneity analysis in Section 4.1. For subsamples of homeowners and renters, the placebo estimates mimic the patterns for the exposure effects on income presented earlier in Section 4.

6.2 Discussion

As discussed earlier in this paper, previous research has used untenable assumptions to estimate the neighborhood effect, which leads them to overestimate the contribution of neighborhoods in shaping children's later life outcomes. Following CH, the central assumption imposed in many recent works on exposure effects is a common selection bias independent of the age when child moves across areas (Assumption A.1), which I discussed previously in more detail in Section 5.

Figure 9 presents a binned scatter plot of the relationship between children's birth weight ranks (among their cohort) and the age of the child when parents move. Figure 9 suggests that, on average, those children whose parents moved earlier to the destination neighborhood tend to belong to the upper part of the birth weight distribution (i.e., higher percentile ranks in the national birth weight distribution). This suggests that children's potential outcomes are not orthogonal to their age when moving across areas.

Figure E.2 of Appendix E shows the positive correlation between birth weight and children's academic achievement in different subjects in the national exam at the 9th grade. Also,

Figure E.1 of Appendix E, using data on earlier cohorts[50], presents the significant positive correlation between the 9th grade and earnings of the children at age 29. Altogether, these results suggest that a child's birth weight, which is highly correlated with her later life outcomes, is not randomly distributed across children who move at different ages during their childhood.

The relationship between the child's age when parents move and preexisting characteristics of the child, observed in Figure 9, is not exclusive to children's birth weight. Figure E.3 of Appendix E shows a similar, negative relationship between the child's age when parents move and birth length. Moreover, Figure E.4 of Appendix E presents a binned scatter plot of the positive relationship between children's age at the time of the move and the probability of being a low birth weight as an infant. The pattern regarding the likelihood of LBW is especially important because previous studies have established that LBW infants face many complications in their lives, some of which persist into adult life.[51, 52]

Finally, while CH focus on children moving across US commuting zones from age nine onward, Figure E.6 of Appendix E shows that most moves in childhood occur before children reach age 10.

The placebo estimates in Table 4 demonstrate that the estimation strategy proposed by CH is not suitable for assessing causal effects of place. The results of the placebo test illustrate how the heterogeneity in the sorting dynamics leads to an overestimation of the exposure effect. Taken together with the estimates of exposure effects in Section 5, the results of this paper suggest that the neighborhood causal effects in CH, Chetty & Hendren (2018b), and Chetty *et al.* (2020b) mirror the correlational estimates of place effects in Chetty *et al.* (2014).

[50]This analysis uses children born in 1990, for whom both 9th-grade test scores and income at age 29 are observed. The birth weight data is not available for the individuals in this sample.

[51]LBW infants are more likely to suffer from weaknesses in attention and hyperactivity, anxiety and depression, and poor social skills, which affect their cognitive outcomes (Hack *et al.* (2009)). Ribeiro *et al.* (2011) report language problems in LBW children. Conley & Bennett (2000) report that LBW children are 74% less likely to graduate from high school by age 19 when compared with their siblings.

[52]Also, Figure E.5 of Appendix E presents a binned scatter plot of the relationship between the education of parents and the age of the child when parents move, suggesting that the child's age at the time of the move and parental education level are inversely related.

7 Conclusion

Earlier studies in sociology and economics document the relationship between the neighborhood of residence in childhood and various outcomes in adulthood. Several recent studies have exploited quasi-experimental strategies to identify the causal impact of neighborhoods on children's long-run outcomes.

One of the major econometric challenges in estimating the causal impact of neighborhoods on child outcomes is the endogeneity of neighborhood quality. Area of residence is a choice, and individuals sort into areas based on a wide variety of characteristics.

This article investigates the main estimation strategies and identifying assumptions of the most notable studies in the literature. This study exploits rich, longitudinal admin data from Denmark and documents that the recent popular quasi-experimental approach in the literature fails to assess the causal impact of the neighborhood of residence on later life outcomes.

In this article, I demonstrate that, due to the lifecycle heterogeneity in the neighborhood sorting process, the assumption of constant selection effects by the child's age at the time of the move is rejected. I document that the intensity of the sorting of households across neighborhoods is not constant in the child's age when moving. This means that the constant-in-age selection effects assumption in certain celebrated empirical studies is untenable.

This paper also conducts a placebo test showcasing that neighborhood exposure estimates obtained by growing quasi-experimental research conflates sorting and selection processes by heterogeneous agents with neighborhood causal impacts.

Future research should address the methodological implications of the findings of this paper. Neighborhoods might matter for different reasons. School quality, exposure to crime and violence, and social interactions are channels through which neighborhoods might affect children's economic opportunities. While isolating the aforementioned factors from family characteristics, researchers should take into account the impact of the sorting behavior, which confounds the relationship between neighborhood quality and children's outcomes in adulthood.

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Tables

Table 1: Summary Statistics for Municipality Permanent Residents and Movers

Variable	Mean (1)	Std. dev. (2)	Median (3)	Num. of obs. (4)
Panel A: Permanent residents: Families who do not move across municipalities				
Child individual income at 30	25,495	9,710	25,415	536,993
Child family income at 30	43,090	19,368	44,476	536,072
Child cohabiting at 30	0.67	0.47	1.00	537,801
Child years of schooling by 30	14.68	2.37	14.50	524,959
Child individual property value at 30	81,794	99,120	69,070	529,849
Parent family income	43,832	13,272	42,660	527,670
Parent property value	109,882	79,499	106,692	525,677
Nuclear (intact) Family	0.62	0.49	1.00	484,164
Panel B: Families who move 1-3 times across municipalities				
Child individual income at 30	24,880	10,007	24,846	258,295
Child family income at 30	41,732	19,911	42,257	257,744
Child cohabiting at 30	0.65	0.48	1.00	258,592
Child years of schooling by 30	14.50	2.55	14.50	251,296
Child individual property value at 30	69,105	92,740	47,726	255,337
Parent family income	43,586	13,549	41,948	252,652
Parent property value	94,273	77,781	86,069	251,903
Nuclear (intact) Family	0.39	0.49	0.00	234,262
Panel C: Families who move exactly once across municipalities				
Child individual income at 30	25,197	10,066	25,146	157,428
Child family income at 30	42,313	19,955	42,968	157,119
Child cohabiting at 30	0.65	0.48	1.00	157,633
Child years of schooling by 30	14.63	2.51	14.50	153,221
Child individual property value at 30	72,892	94,934	54,975	155,601
Parent family income	44,180	13,879	42,528	154,143
Parent property value	100,761	78,964	94,480	153,667
Nuclear (intact) Family	0.45	0.50	0.00	143,172

Notes: This table presents summary statistics for the samples used in the main analysis of this paper, which consists of all children born in Denmark between 1970 and 1982. The table reports the summary statistics for three different subsets of this sample. Panel (A) presents the statistics for the sample of permanent residents, i.e., children whose parents never moved from 1982 to 2000. Panel (B) shows the statistics for those who moved across municipalities once, twice or three times between 1982 and 2000. Panel (c) reports the statistics for the sample of movers who moved only once across municipalities between 1982 and 2000. I use the Consumer Price Index (CPI) to adjust for inflation. All dollar values are in 2010 US dollars (using an exchange rate of 6.7 DKK per US dollar). See Sections 2.1 and 2.2 for further details on variable and sample definitions.

Table 2: Childhood Exposure Effect Estimates

Dependent Variable: Child's Income Rank in Adulthood (Age 30)

							Family FE		
Specification:	Pooled	Age <= 23	Age < 18	No cohort controls	Family Income	Child nbhd FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Exposure Effect (γ)	0.023	0.023	0.019	0.016	0.016	0.021	0.019	0.017	0.023
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.013)	(0.009)	(0.015))
Number of Obs.:	107,289	102,521	80,237	107,289	107,123	107,252	107,289	107,289	107,289

Notes: This table reports estimates of annual childhood exposure effects on children's income ranks at age 30 (γ). The estimates are interpreted in CH and Chetty *et al.* (2020b) as the impact of spending an additional year of childhood in a neighborhood where children of permanent residents have one percentile point higher ranks of income at age 30. Standard errors are shown in parentheses. Each column reports estimates from a regression of a child's income rank at age 30 on the mean difference in permanent residents' income ranks between the destination and origin for the relevant parent income rank p and birth cohort s, interacted with the child's age at the time of the move (m). I permit separate linear interactions for $m \leq 23$ and $m > 23$ and report the coefficient on the interaction for $m \leq 23$. Each regression also includes additional controls specified in Equation (12). Permanent residents' predicted ranks are constructed using linear regressions of children's ranks on parents' ranks in each neighborhood and birth cohort. Column (1) reports the estimate of γ from Equation (12) using all children in the primary analysis sample of one-time movers, defined in the notes to Table 1 (Panel C). Columns (2) and (3) restrict the sample to those who move at or before age 23 or 18. In column (4), I exclude the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead include a single control for the predicted outcomes of permanent residents in the origin. Column (5) replicates column (1), using household income ranks (rather than individual income ranks) to measure both the child's outcome and the predicted outcomes of permanent residents in the origin and destination. Column (6) adds fixed effects for the child's neighborhood in the last sample year to column (1) specification. Column (7) adds family fixed effects to the baseline specification in column (1). Column (8) adds family fixed effects to column (4) specification that does not include cohort-varying intercepts. Column (9) adds controls for changes in parental marital status and income rank in the year before versus after the move, along with their interactions with the child's age when moving, and indicators for moving above and below age 23, to column (7) specification. See Section 3.3 for details.

Table 3: Heterogeneity of Childhood Exposure Effect Estimates

Dependent Variable: Child's Income Rank in Adulthood (Age 30)

							Family FE		
Specification:	Pooled	Age <= 23	Age < 18	No cohort controls	Family Income	Child nbhd FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel A: Homeowners				
Exposure Effect (γ)	0.027	0.027	0.024	0.026	0.018	0.025	0.001	0.000	-0.028
	(0.006)	(0.006)	(0.008)	(0.005)	(0.006)	(0.006)	(0.020)	(0.016)	(0.031))
Number of Obs.:	37,503	33,122	24,544	37,503	37,444	37,494	37,503	37,503	37,503
					Panel B: Renters				
Exposure Effect (γ)	0.018	0.018	0.011	0.015	0.013	0.016	0.035	0.026	0.031
	(0.006)	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.019)	(0.012)	(0.024))
Number of Obs.:	56,646	52,574	43,459	56,646	56,547	56,621	56,646	56,646	56,646

Notes: This table reports estimates of annual childhood exposure effects on children's income ranks at age 30 (γ). The estimates are interpreted in CH and Chetty *et al.* (2020b) as the impact of spending an additional year of childhood in a neighborhood where children of permanent residents have one percentile point higher ranks of income at age 30. Standard errors are shown in parentheses. Each column reports estimates from a regression of a child's income rank at age 30 on the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with the child's age at the time of the move (m). I permit separate linear interactions for $m \leq 23$ and $m > 23$ and report the coefficient on the interaction for $m \leq 23$. Each regression also includes additional controls specified in Equation (12). Permanent residents' predicted ranks are constructed using linear regressions of children's ranks on parents' ranks in each neighborhood and birth cohort. Column (1) reports the estimate of γ from Equation (12) using all children in the primary analysis sample of one-time movers, defined in the notes to Table 1 (Panel C). Columns (2) and (3) restrict the sample to those who move at or before age 23 or 18. In column (4), I exclude the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead include a single control for the predicted outcomes of permanent residents in the origin. Column (5) replicates column (1), using household income ranks (rather than individual income ranks) to measure both the child's outcome and the predicted outcomes of permanent residents in the origin and destination. Column (6) adds fixed effects for the child's neighborhood in the last sample year to column (1) specification. Column (7) adds family fixed effects to the baseline specification in column (1). Column (8) adds family fixed effects to column (4) specification that does not include cohort-varying intercepts. Column (9) adds controls for changes in parental marital status and income rank in the year before versus after the move, along with their interactions with the child's age when moving, and indicators for moving above and below age 23, to column (7) specification. Panel A restricts the sample to children whose parents were homeowners before and after moving across municipalities. Panel B restricts the sample to children whose parents were renters before and after moving across municipalities. See Section 3.3 for details.

Table 4: Placebo Effect Estimates

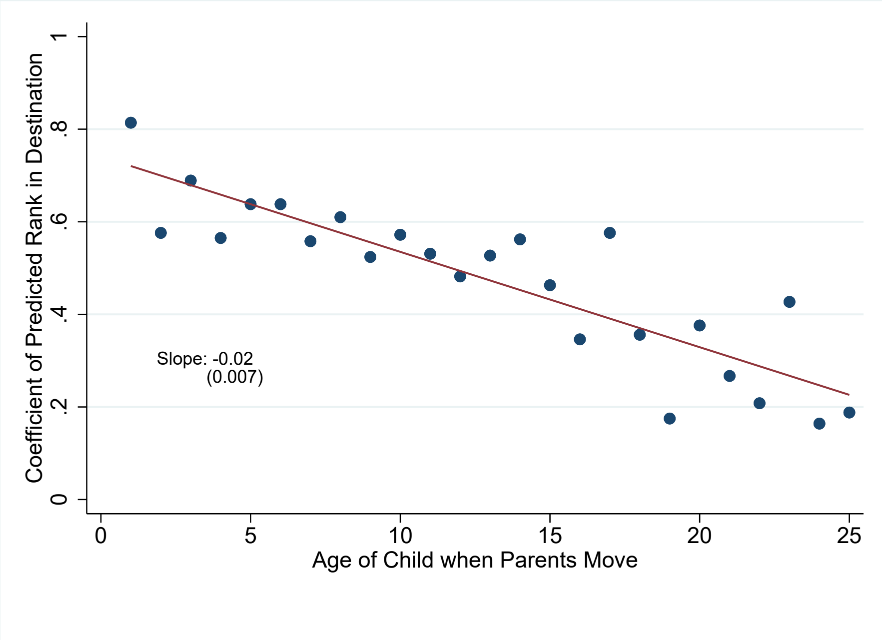
Dependent Variable: Child's Birth Weight Rank

							Family FE		
Specification:	Pooled	Age $>= 0$	Age < 18	No cohort controls	Family Level	Child nbhd FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Placebo Effect (γ)	0.027	0.027	0.032	0.024	–	0.024	0.012	0.017	0.008
	(0.007)	(0.007)	(0.009)	(0.007)	–	(0.006)	(0.016)	(0.016)	(0.017)
Number of Obs.:	134,303	80,087	129,982	134,303	—	130,149	131,916	131,916	131,916

Notes: This table reports estimates of the placebo annual childhood exposure effects on children's birth weight ranks (γ). The estimates can be interpreted as the rate at which birth weight ranks of children who move one year earlier covary more with the birth weight ranks of permanent residents in the destination. Standard errors are shown in parentheses. Each column reports estimates from a regression of a child's birth weight rank on the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with the child's age at the time of the move (m). I permit separate linear interactions for $m \leq 0$ (when the mother moves across municipalities before the arrival of her child) and $m > 0$ and report the coefficient on the interaction for $m \leq 0$. Each regression also includes additional controls specified in Equation (14). Permanent residents' predicted ranks are constructed using linear regressions of children's ranks on parents' ranks in each neighborhood and birth cohort. Column (1) reports the estimate of γ from Equation (14) using all children in the primary analysis sample of one-time movers, defined in the notes to Table E.1 (Panel C). Columns (2) and (3) restrict the sample children moved at or after age 0 (Column 2) and at or before age 18 (Column 3). In column (4), I exclude the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead include a single control for the predicted outcomes of permanent residents in the origin. Column (6) adds fixed effects for the child's neighborhood in the last sample year to column (1) specification. Column (7) adds family fixed effects to the baseline specification in column (1). Column (8) adds family fixed effects to the specification in column (4) that does not include cohort-varying intercepts. Column (9) adds controls for changes in parental marital status and income rank in the year before versus after the move, along with their interactions with the age of the child at the time of the move and indicators for moving above and below age 0, to the specification in column (7). See Section 6 for details.

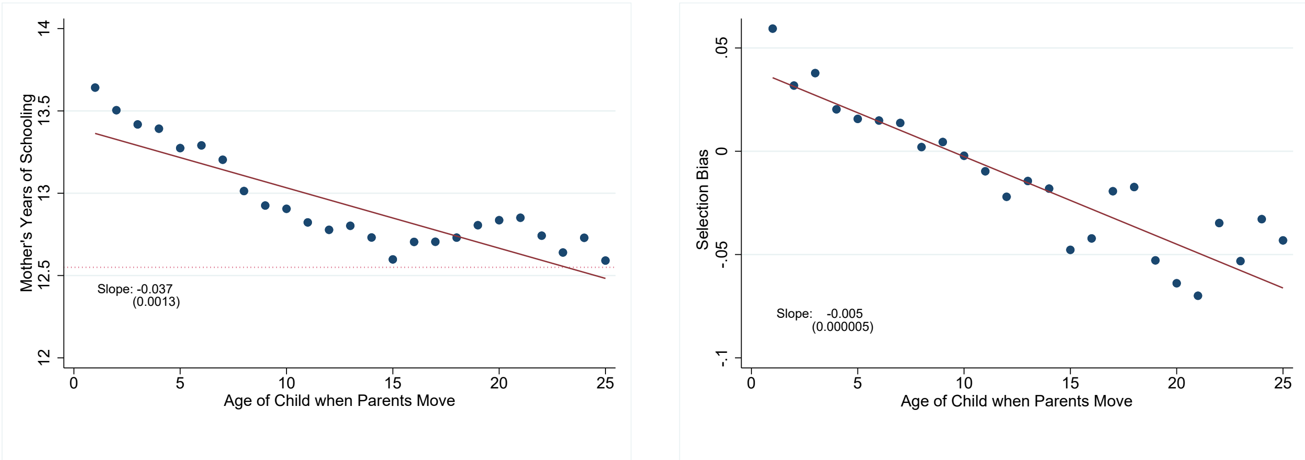
Figures

Figure 1: Childhood Exposure and Income Ranks in Adulthood



Notes: This figure plots the regression coefficients, $\{b_m\}$, versus the child's age when the parent moves (m) using the specification in Equation (11), measuring children's incomes at age 30. The sample includes all children in the primary analysis sample whose parents moved exactly once between 1982 and 2000. The b_m coefficients are interpreted in CH as the effect of moving to an area where permanent resident outcomes are one percentile higher at age m. They are estimated by regressing the child's income rank in adulthood y_i on $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$, the mean difference in permanent residents' income ranks between the destination and origin for the relevant parent income rank p and birth cohort s, interacted with each age of the child at the time of the move m. As in CH, I include indicators for the child's age when moving interacted with parent income rank and predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects. See Section 3.3 for details.

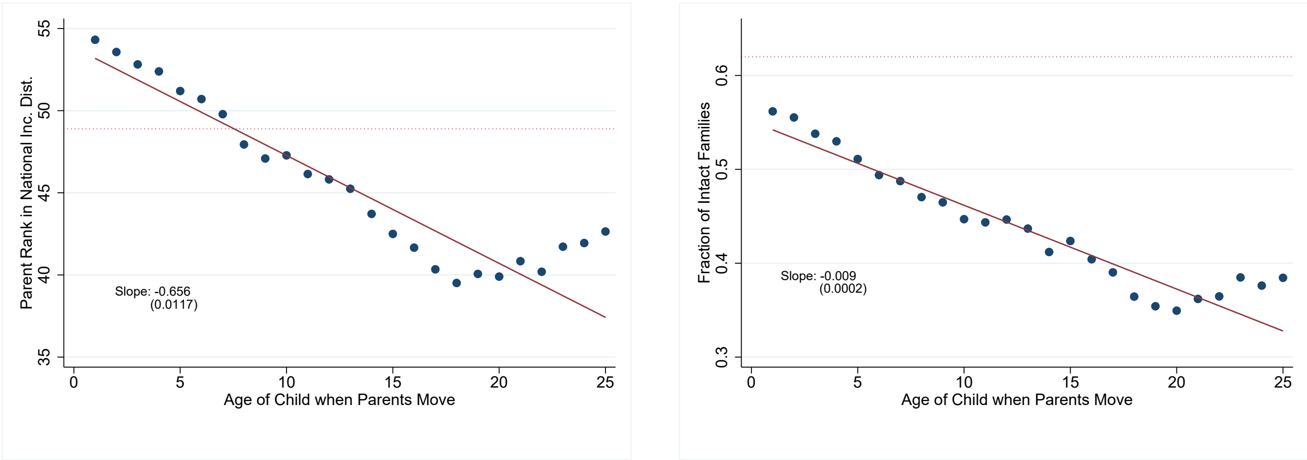
Figure 2: Age of Child at the Time of the Move and Parental Education



(a) Parental Education (b) Selection Bias (δ_m)

Notes: Panel (a) presents a binned scatter plot depicting the relationship between the education level of mothers and the age of the child when parents move. The horizontal dotted line shows the average education level of permanent residents, i.e., those who never moved across municipalities between 1982 and 2000. Panel (b) presents a binned scatter plot depicting by-age selection bias, δ, arising from parental education level being omitted when estimating the childhood exposure effects. See Sections 3.2 and 5.1.1 for details.

Figure 3: Parental Characteristics and the Age of the child when Parents Move

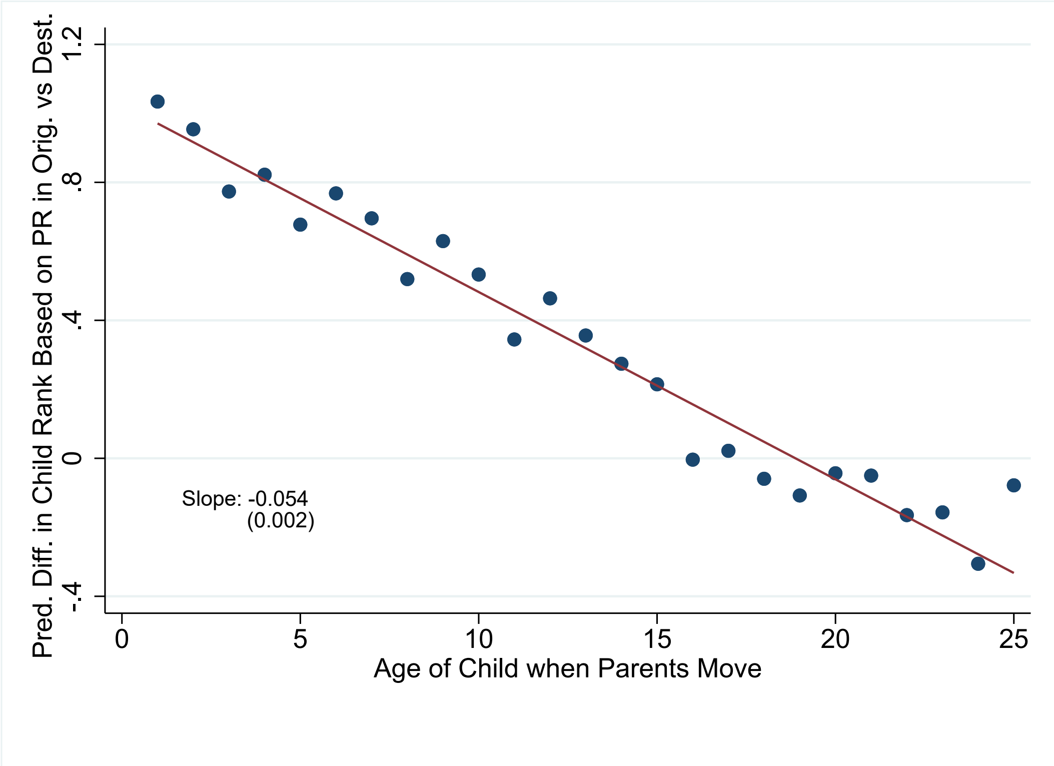


(a) Parental Income

(b) Family Intactness

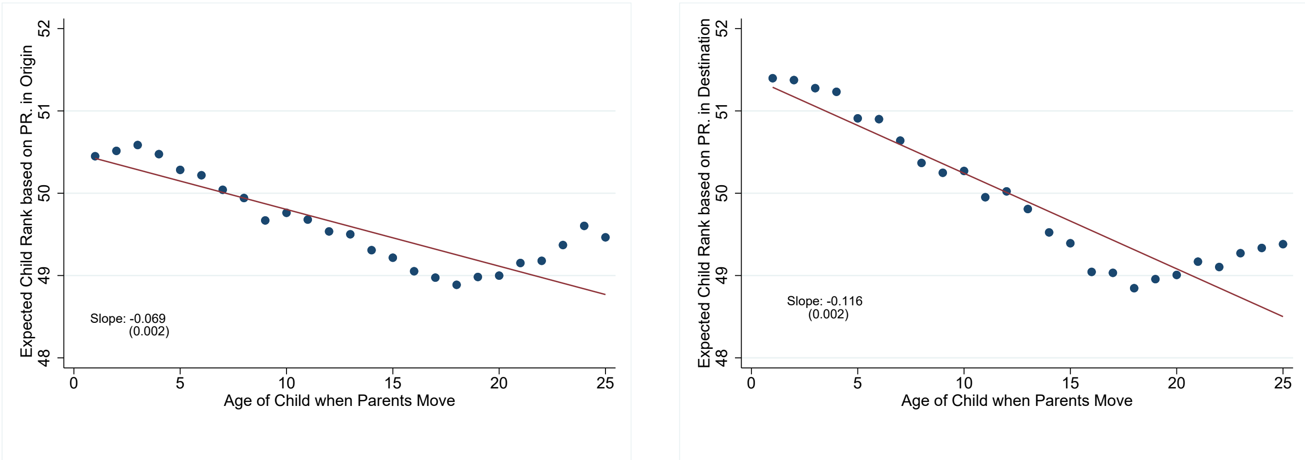
Notes: Panel (a) presents a binned scatter plot depicting the relationship between the child's age when parents move and parent (disposable) income rank in the national income distribution. Panel (b) shows a binned scatter plot depicting the relationship between the child's age when parents move and the fraction of intact families. The horizontal line shows the fraction of intact families for the permanent residents (non-movers) sample. For each child, I define an intact family as follows: the family is intact if the child's parents live together (cohabit) during the first 18 years of childhood. The horizontal dotted lines show the average income rank (Panel a) and the fraction of intact families (Panel b) for the sample of permanent residents, i.e., those who never moved across municipalities between 1982 and 2000.

Figure 4: Quality of Moves (Δ_{odps}) and the Age of the Child when Parents Move



Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the mean income rank of children of permanent residents when moving from the origin to the destination neighborhood conditional on parent income rank p and birth cohort s (i.e., $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ defined in Section 3.3 of this paper).

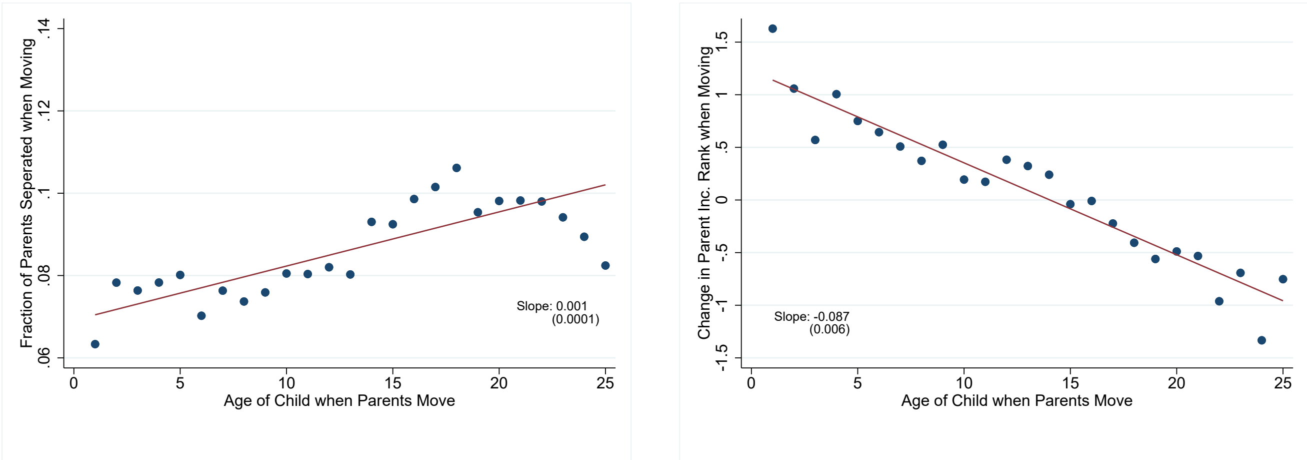
Figure 5: Age of Child when Parents Move, Sorting, and Selection



(a) Sorting to Origin Neighborhood (b) Selection into Destination Neighborhood

Notes: Panel (a) presents a binned scatter plot depicting the relationship between the child's age when parents move and the income rank of children of permanent residents of the origin neighborhood (i.e., $\bar{y}_{pos}$ defined in Section 3.3 of this paper). Panel (b) shows a binned scatter plot depicting the relationship between the child's age when parents move and the income rank of children of permanent residents of the destination neighborhood (i.e., $\bar{y}_{pds}$ defined in Section 3.3 of this paper).

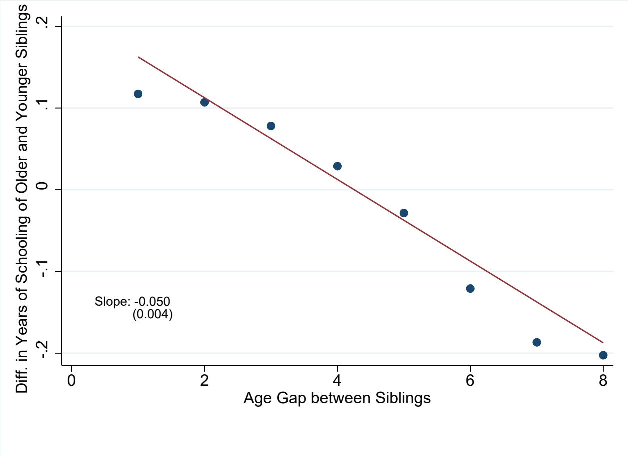
Figure 6: The Age of Child at the Time of the Move and Lifecycle Shocks



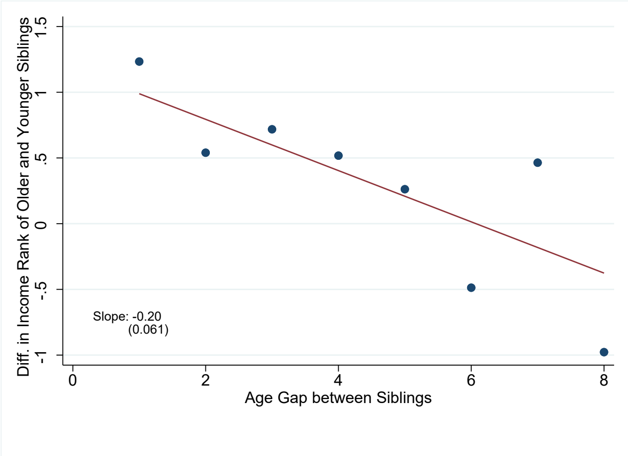
(a) Fraction of Parents Separated (b) Income Shocks

Notes: This figure presents a binned scatter plot depicting the relationship between the child's age when parents move and lifecycle events. Panel (a) shows a binned scatter plot depicting the relationship between the child's age when parents move and the fraction of parents separated during the year they moved across municipalities. Panel (b) presents a binned scatter plot depicting the relationship between the child's age when parents move and the increase in parental rank in the national income distribution during the year they moved across municipalities.

Figure 7: Age Space and Sibling Outcomes' Differences



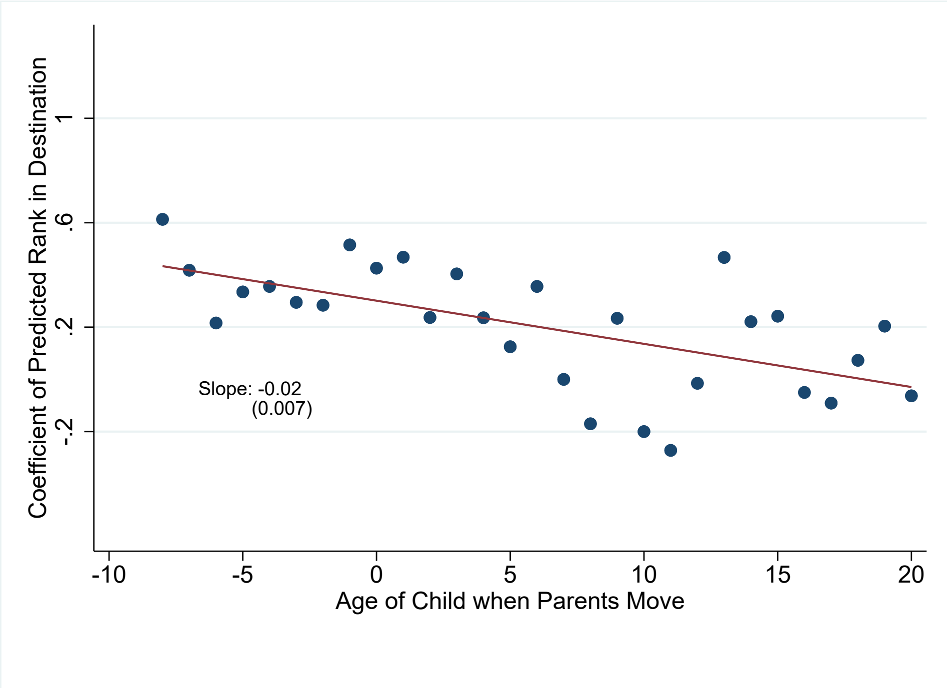
(a) Years of Schooling



(b) Income Ranks

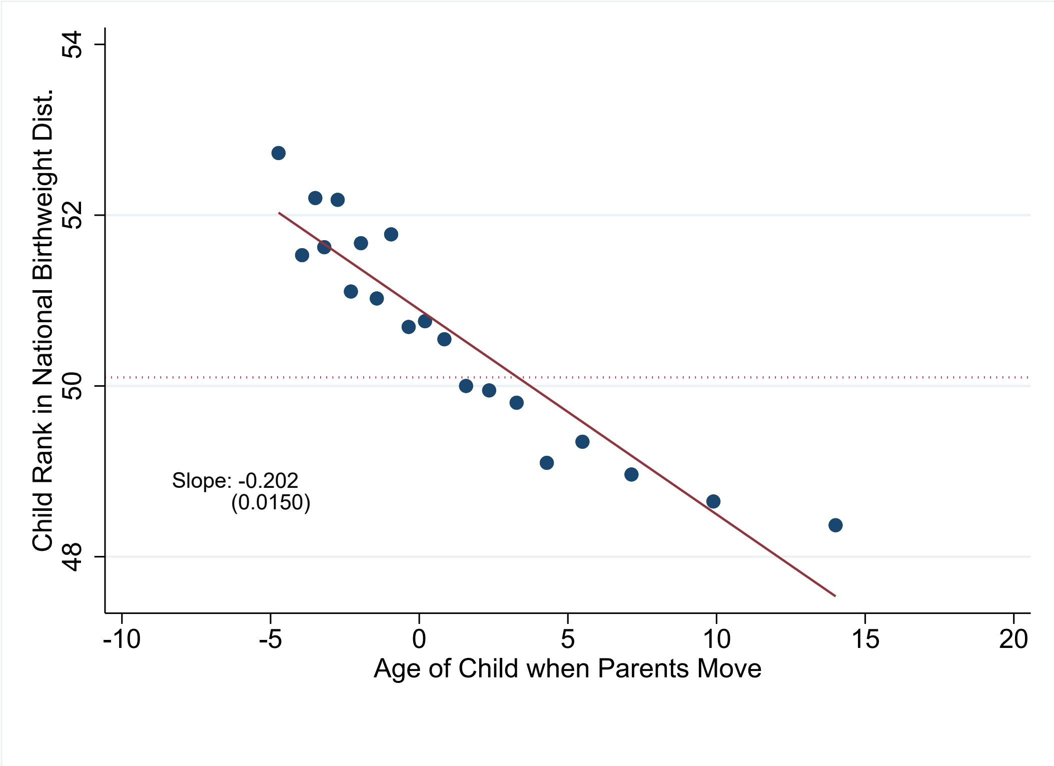
Notes: This figure shows a binned scatter plot depicting the relationship between the age space between siblings and the difference between their outcomes, i.e., the outcome of the older sibling minus the outcome of the younger one. Panel (a) considers years of schooling. Panel (b) examines income ranks. Children's income ranks (at age 30) are computed relative to all others in their birth cohort. The sample is restricted to children of permanent residents, i.e., those who never moved across municipalities between 1982 and 2000.

Figure 8: Placebo Effects Using Birth Weight



Notes: This figure plots the regression coefficients, $\{b_m\}$, versus the child's age when the parent moves (m) using the specification in Equation (13), measuring children's birth weight rank. The sample includes all children in the analysis sample whose parents moved exactly once between 1997 and 2019. The b_m coefficients are estimated by regressing the child's birth weight rank bw_i on $\Delta_{odps} = \bar{bw}_{pds} - \bar{bw}_{pos}$, the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with each age of the child at the time of the move m. As in CH, I include indicators for the child's age when moving interacted with parent income rank and predicted outcomes for permanent residents in the origin interacted with birth cohort fixed effects. See Section 6 for details.

Figure 9: Birth Weight Rank and the Age of the Child at the Time of the Move

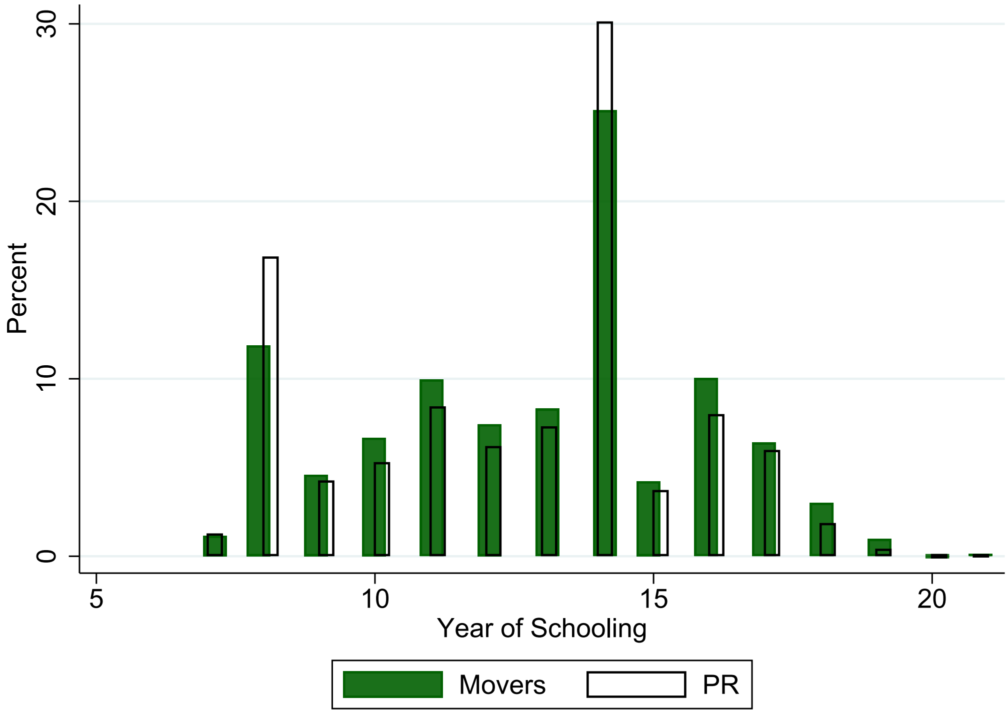


Notes: This figure plots a binned scatter plot depicting the relationship between the age of the child when parents move and her rank in the national birth weight distribution relative to all others in her birth cohort. The sample includes all children in the placebo analysis sample whose parents moved across municipalities exactly once between 1997 and 2019. The horizontal dotted line shows the average birth weight rank for children of permanent residents, i.e., those who never moved across municipalities between 1997 and 2019.

A Additional Statistics

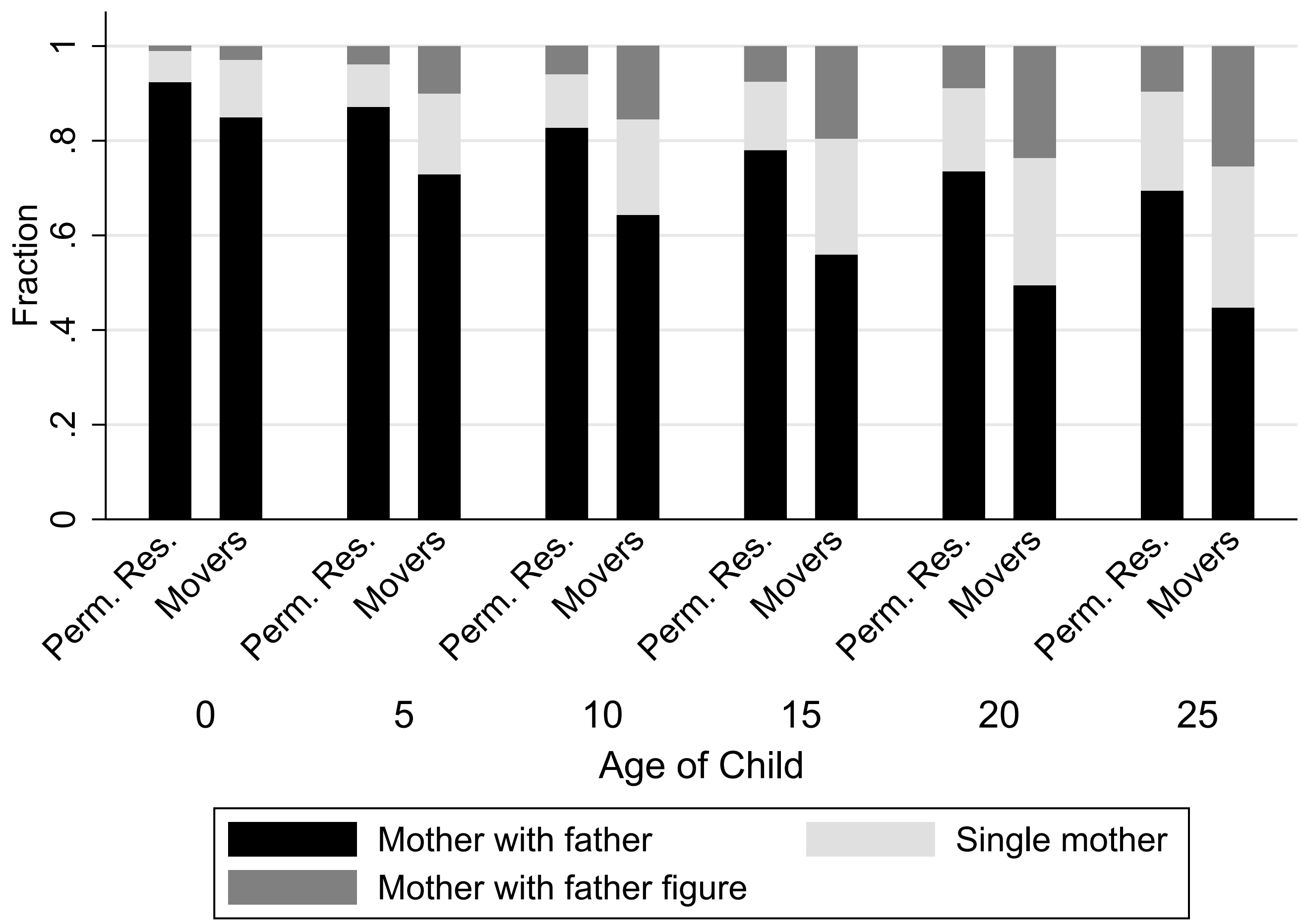
Figure A.1 shows the education level (measured as years of schooling) for permanent residents and compares it to movers' education level.
Figure A.2 shows how the family structure evolves over the age of children, separately for permanent residents and movers.

Figure A.1: Distribution of Years of Schooling by Permanent Residence Status



Notes: This figure compares the distribution of education level (years of schooling) between movers (who moved across municipalities exactly once between 1982 and 2000) and permanent residents (i.e., those who never moved across municipalities between 1982 and 2000).

Figure A.2: Family Structure over the Lifecycle- by Permanent Residency Status



Notes: This figure presents the family structure statistics over the age of children, separately for permanent residents (i.e., those who never moved across municipalities between 1982 and 2000) and movers.

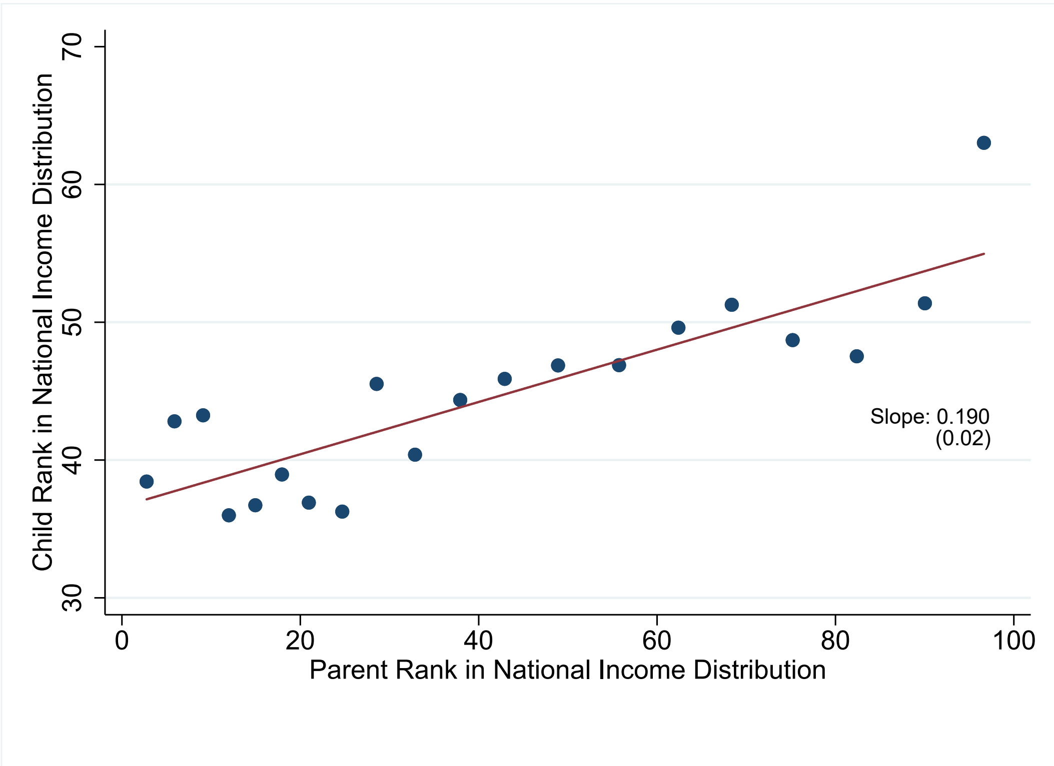
B Additional Analyses

Figure B.1 presents the linear approximation for the relationship between the child's income rank and her parents' rank, given her neighborhood of residence and birth cohort. Figure B.1 illustrates how I estimate $\bar{y}_{pcs}$ for children born in 1970 to permanent residents of Copenhagen (who never left Copenhagen to other municipalities between 1982 and 2000). This figure plots the mean child rank at age 30 within each percentile bin of the parent income distribution, $E[y_i|p(i) = p]$. See Section 3 for details.

Figure B.2 presents a nonparametric binned scatter plot corresponding to the regression in Equation (9) for children who first move at age $m = 13$. To construct Figure B.2, I first demean both y_i and Δ_{odps} within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $y_i^r = y_i - E[y_i|q, o, s]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q, o, s]$. I then divide the Δ_{odps} residuals into 20 equal-size groups and plot the mean value of y_i^r versus the mean value of Δ_{odps}^r in each bin. See 3 for details.

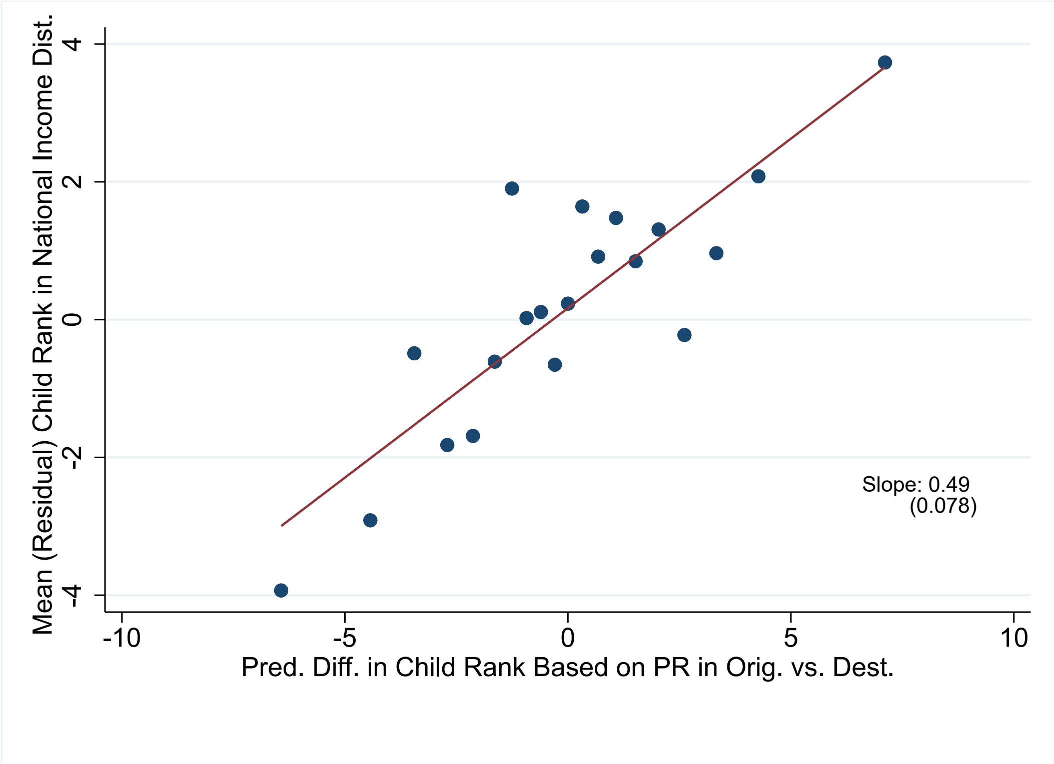
Figure B.3 explores how the timing of moves across neighborhoods varies by the homeownership status of families for the sample of movers. Figure B.3 suggests that among the sample of one-time movers, compared to renters, homeowners tend to move across neighborhoods when their children are older.

Figure B.1: Mean Child Income Rank versus Parent Income Rank for Children Raised in Copenhagen



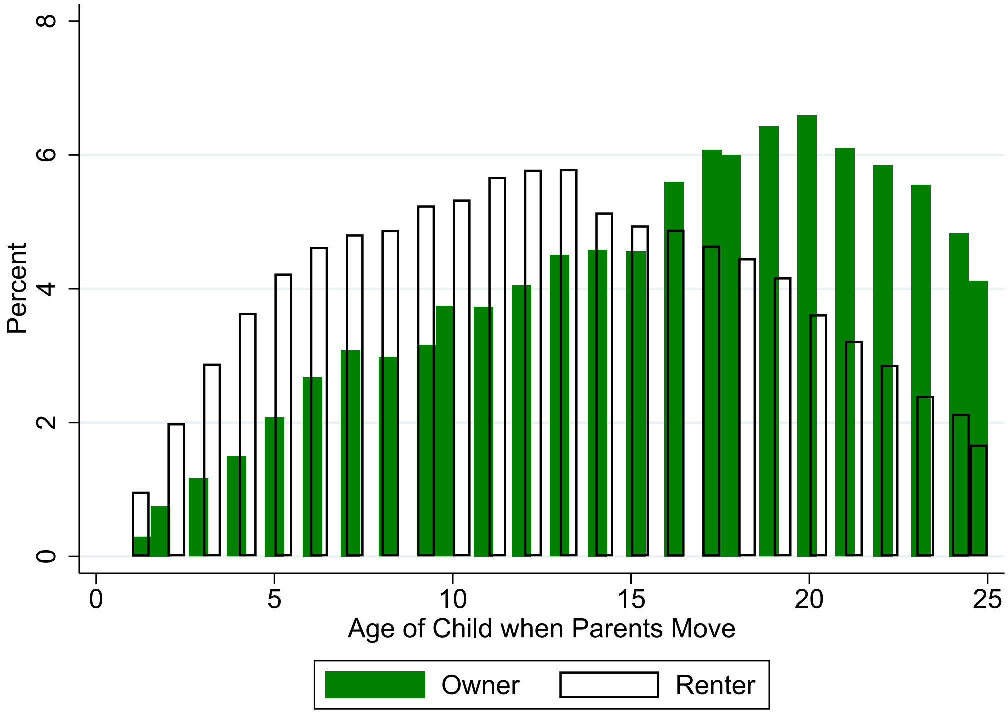
Notes: This figure replicates Figure I of CH using Danish data. It presents a binned scatter plot of the relationship between children's disposable income ranks and parent disposable income ranks for children raised in Copenhagen. The figure plots the mean rank of children within each parental income percentile bin (20 bins used for this figure). The best-fit line is estimated using an OLS regression. The figure also reports the slope of the linear fit (the rank-rank slope), along with the standard error (in parentheses). The sample includes all children in the 1970 birth cohort in the analysis sample whose parents were permanent residents of the Copenhagen municipality during the sample period (1982–2000). Children's disposable incomes are measured at the individual level at age 30; parents' incomes are defined as mean family income from 1982 to 2000. Children are assigned ranks based on their incomes relative to all other children in their birth cohort. Parents' are assigned ranks based on their incomes relative to other parents of children in the same birth cohort.

Figure B.2: Movers' Outcomes versus Predicted Outcomes Based on Permanent Residents in Destination



Notes: This figure replicates Figure III of CH using Danish data. It presents a binned scatter plot depicting the relationship between the (disposable) income ranks of children who moved to a different municipalities at age 13 and the differences in the outcomes of permanent residents in the destination versus origin municipality. The sample includes all children in the 1970–1982 birth cohorts whose parents moved when the child was 13 years old and moved only once between 1982 and 2000. Children's family (disposable) income ranks y_i are measured at age 30. Permanent residents' predicted ranks for each parent income percentile p, municipality c, and birth cohort s ($\bar{y}_{pcs}$) are constructed using the methodology described in the notes to Figure I. To construct the figure, I demean both y_i and $\Delta_{odps} = \bar{y}_{pds} - \bar{y}_{pos}$ within the parent decile (q) by origin (o) by birth cohort (s) cells in the sample of movers at age $m = 13$ to construct residuals: $y_i^r = y_i - E[y_i|q, o, s]$ and $\Delta_{odps}^r = \Delta_{odps} - E[\Delta_{odps}|q, o, s]$. I then divide the Δ_{odps} residuals into 20 equal-size groups and plot the mean value of y_i^r versus the mean value of Δ_{odps}^r in each bin. The slope of the best-fit line, which corresponds to $b_1 3$ in Equation 9, is estimated using an OLS regression using individual data, with standard errors in parentheses.

Figure B.3: Timing of Moves across Neighborhoods by Homeownership

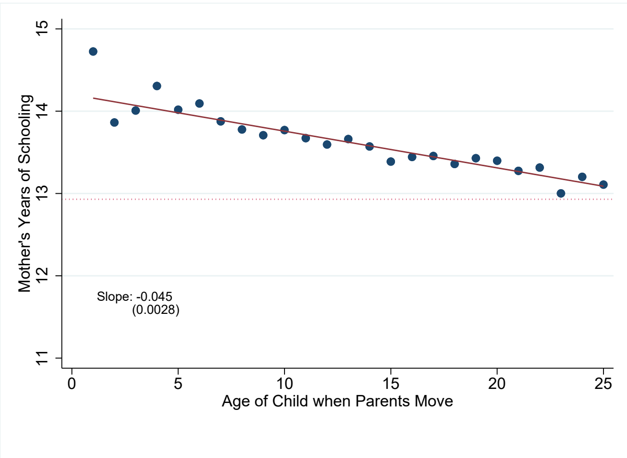


Notes: Here, I plot the histogram of the child's age when parents move across municipalities between 1982 to 2000 by parents of children born between 1970 to 1982, separately by the homeownership status of parents over the years 1982-2000. I restrict the sample to parents who moved only once between 1982 and 2000.

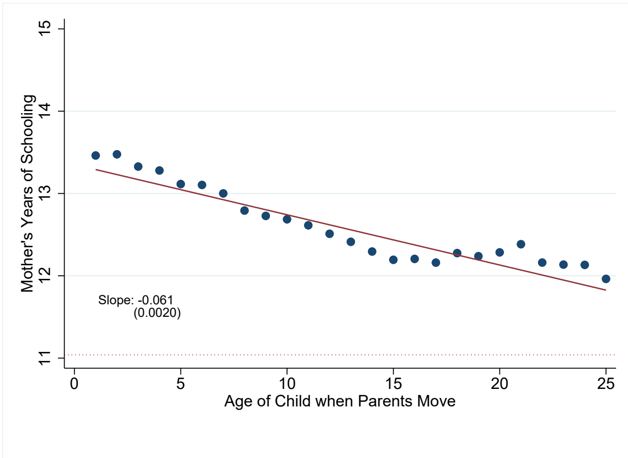
C Heterogeneity of Results by Ownership Status

Figure C.1 presents a binned scatter plot of the relationship between the child's age when parents move and parental education level, separately for homeowners and renters.
Figure C.2 shows a binned scatter plot depicting the relationship between the child's age when parents move and parental income rank in the national income distribution, separately for homeowners and renters.
Figure C.3 presents the relationship between the likelihood of an intact family and the child's age when parents move, separately for homeowners and renters.
Figure C.4 depicts the relationship between the child's age when parents move and the income rank of children of permanent residents of the origin neighborhood, separately for homeowners and renters.
Figure C.5 depicts the relationship between the child's age when parents move and the income rank of children of permanent residents of the destination neighborhood, separately for homeowners and renters.
Figure C.6 depicts the relationship between the child's age when parents move and the increase in the income rank of children of permanent residents when moving from the origin to the destination neighborhood, separately for homeowners and renters.
Figure C.7 presents a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the parish income rank (where all parishes are ranked based on their average household income), during the year the family moves, separately for the samples of homeowners and renters.
Figure C.8 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the cluster income rank (where all clusters are ranked based on their average household income), during the year the family moves, separately for the samples of homeowners and renters.
Figure C.9 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood house price rank (measured at parish level) when moving from the origin parish to the destination parish, separately for the samples of homeowners and renters.
Figure C.10 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood house price rank (measured at municipality level) when moving from the origin parish to the destination municipality, separately for the samples of homeowners and renters.
Figure C.11 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood school quality rank (at parish level), during the year the family moves, separately for the samples of homeowners and renters.
Figure C.12 presents the fraction of parents who were married just before the move but single right after, separately for the samples of homeowners and renters.
Figure C.13 presents a binned scatter plot depicting the relationship between the child's age when parents move and the increase in parental rank in the national income distribution during the year they move, separately for the samples of homeowners and renters.

Figure C.1: Age of Child at the Time of the Move and Parental Education by the homeownership status



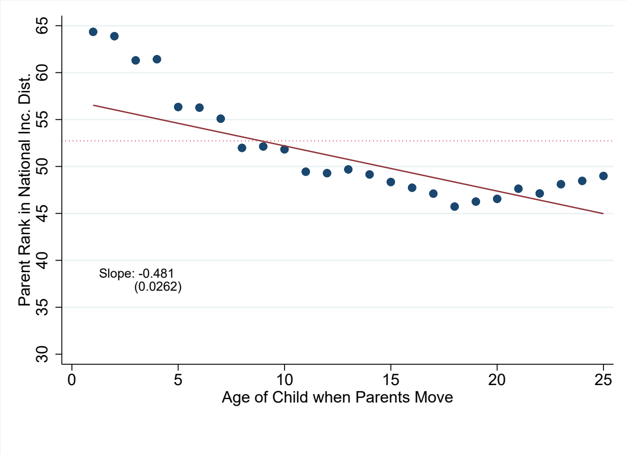
(a) Owners



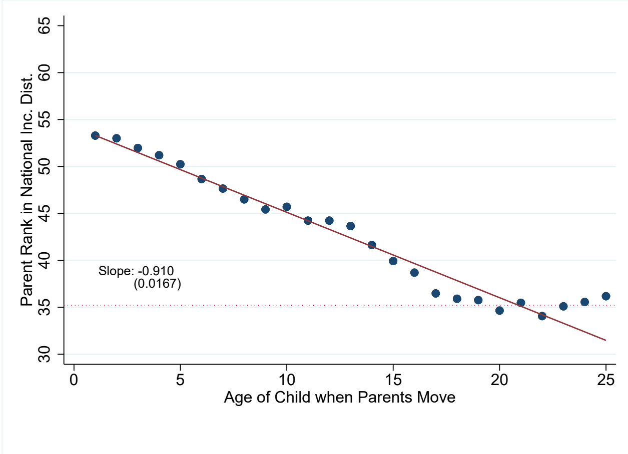
(b) Renters

Notes: Panel A shows a binned scatter plot depicting the relationship between the education level of mothers and the age of the child when parents move for owners. Panel B shows the relationship for the sample of renters. The horizontal dotted lines show the value for the sample of permanent residents (i.e., those who never moved across municipalities between 1982 and 2000).

Figure C.2: PARENTAL INCOME RANK AND THE AGE OF THE CHILD WHEN PARENTS MOVE BY OWNERSHIP STATUS



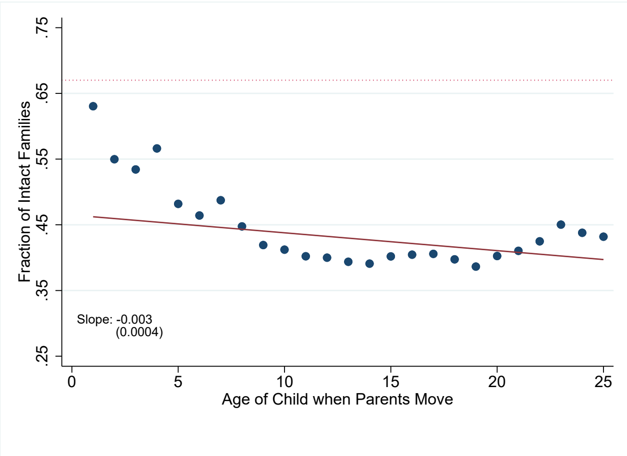
(a) Owners



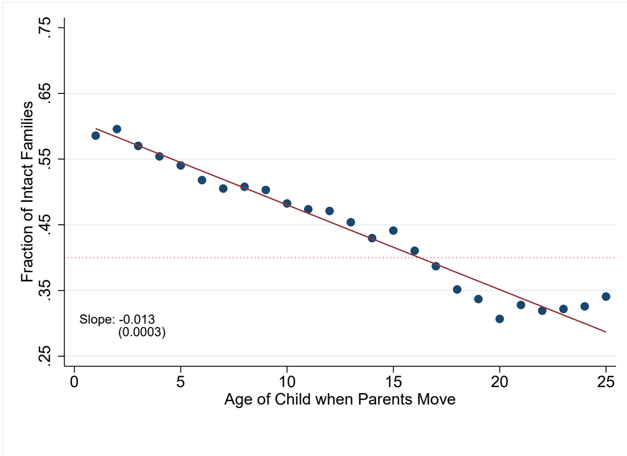
(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the child's age when parents move and parent (disposable) income rank in the national income distribution. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters. The horizontal dotted lines show the value for the sample of permanent residents (i.e., those who never moved across municipalities between 1982 and 2000).

Figure C.3: Fraction of Intact Families and the Age of the Child when Parents Move by the homeownership status



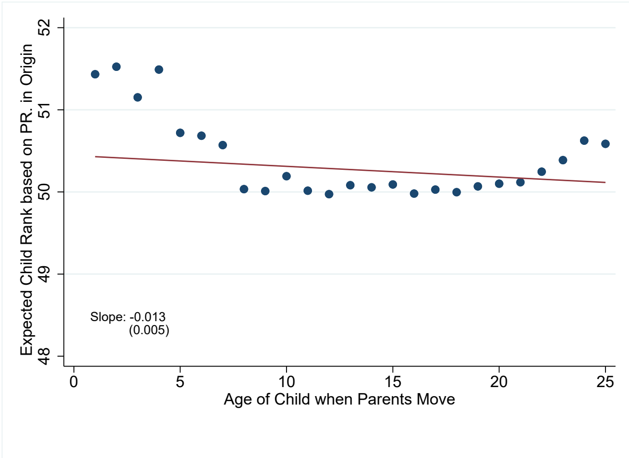
(a) Owners



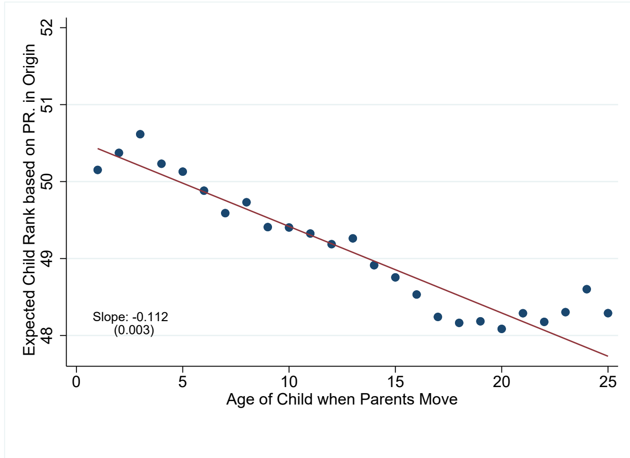
(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the child's age when parents move and the fraction of intact families. The horizontal line shows the fraction of intact families for the permanent residents (non-movers) sample. For each child, I define an intact family as follows: the family is intact if the mother and father of the child live together (cohabit) during the first 18 years of childhood. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters. The horizontal dotted lines show the value for the sample of permanent residents (i.e., those who never moved across municipalities between 1982 and 2000).

Figure C.4: Initial Sorting and the Age of the Child when Parents Move by the homeownership status



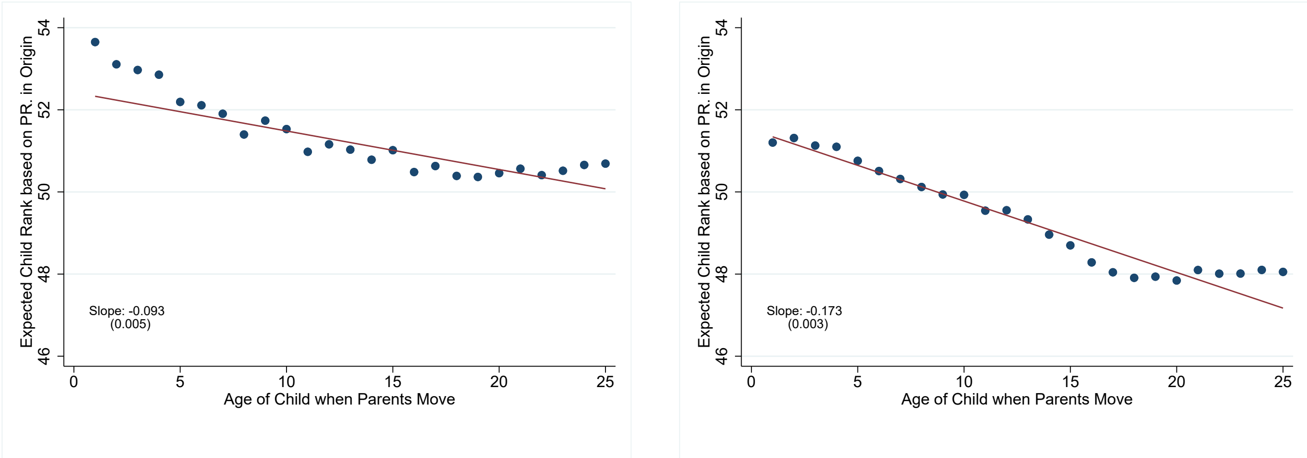
(a) Owners



(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the income rank of children of permanent residents of the origin neighborhood. The income rank of children of permanent residents of each neighborhood is computed conditional on the child's birth cohort and parental income rank. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

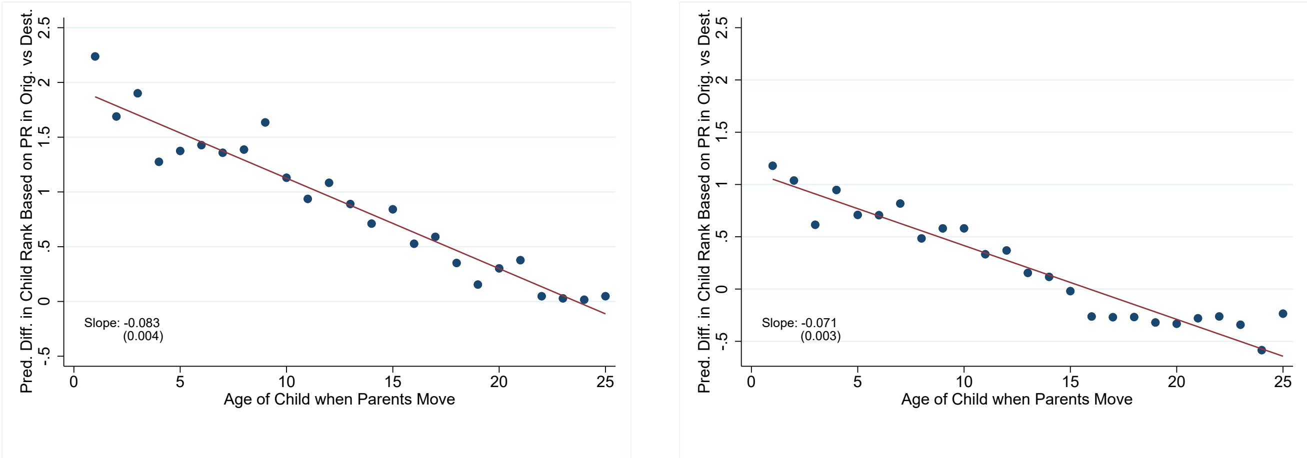
Figure C.5: Neighborhood Selection and the Age of the Child when Parents Move by the homeownership status



(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the income rank of children of permanent residents of the destination neighborhood. The income rank of children of permanent residents of each neighborhood is computed conditional on the child's birth cohort and parental income rank. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

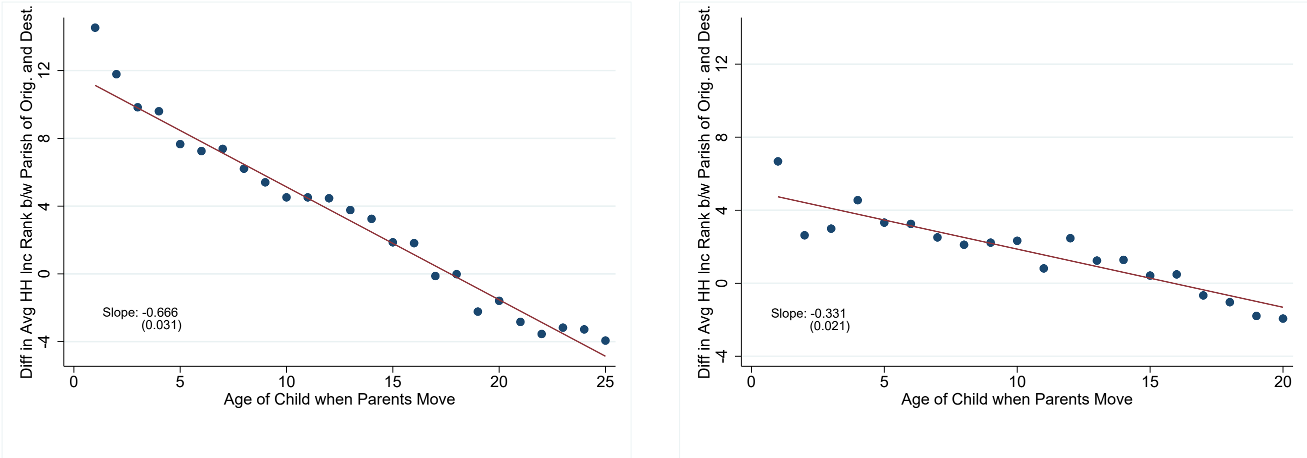
Figure C.6: Quality of Moves (Difference in Predicted Outcomes of Children) and the Age of the Child when Parents Move by the homeownership status



(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the predicted income rank of children by moving to the destination neighborhood. The prediction is based on the relationship between parental income rank and child outcomes for the permanent residents of each neighborhood conditional on the child's birth cohort. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

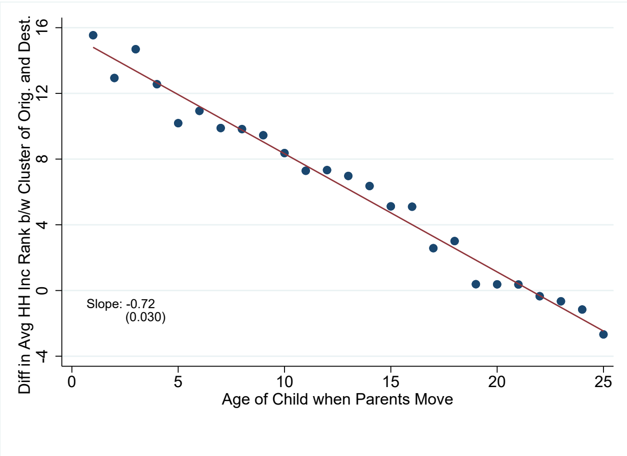
Figure C.7: Quality of Moves (by Parish Income) and the Age of the Child when Parents Move by the homeownership status



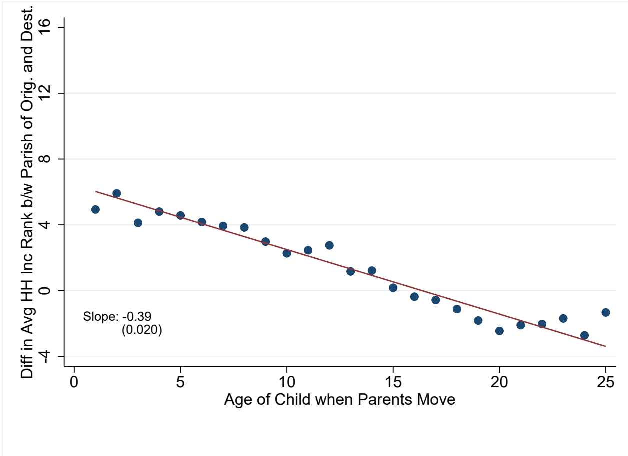
(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the neighborhood average family income (at parish) rank among all neighborhoods, during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000). Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

Figure C.8: Quality of Moves and the Age of the Child when Parents Move by the homeownership status



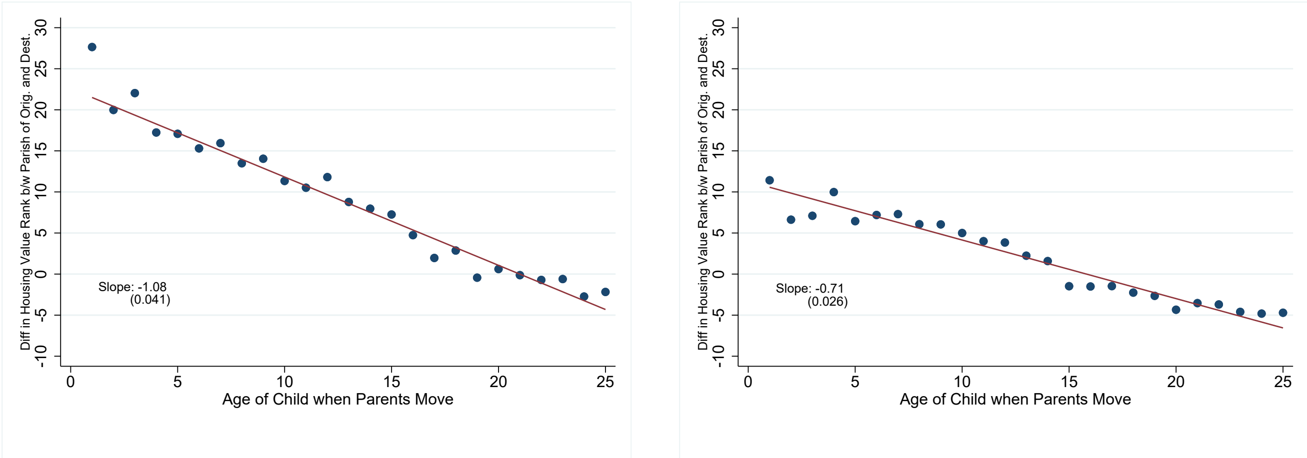
(a) Owners



(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the neighborhood average family income (at 600 house clusters) rank among all neighborhoods, during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000). Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

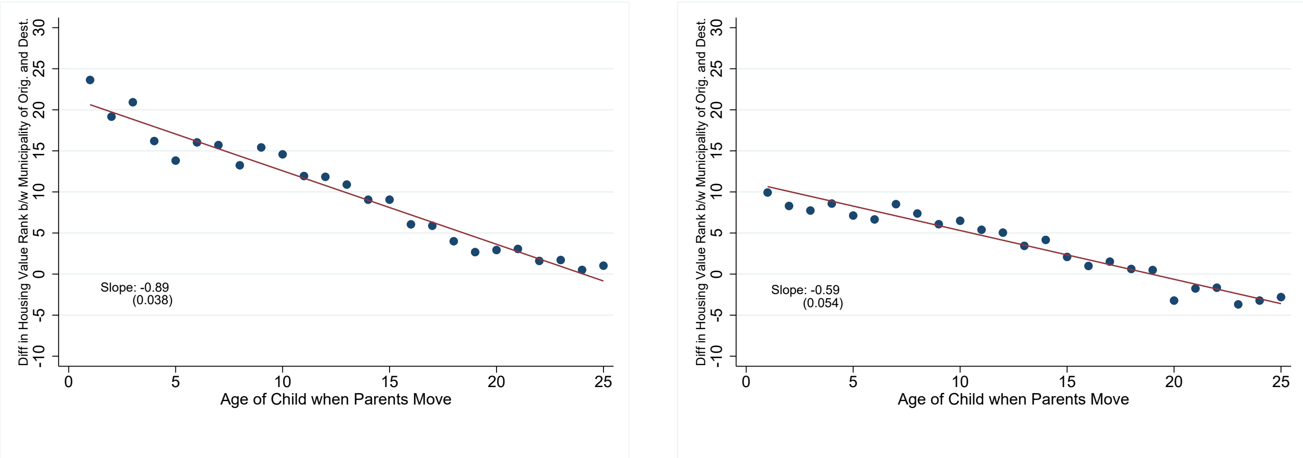
Figure C.9: Changes in Neighborhood House Price Rank and the Age of the Child when Parents Move



(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move across municipalities and the increase in the neighborhood house price rank (measured at parish level) when moving from the origin parish to the destination parish. The rank is calculated based on parish-level house prices averaged over the sample years (1982-2000). Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

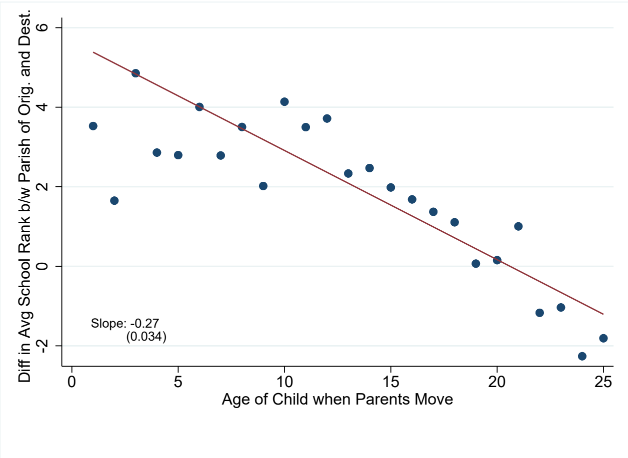
Figure C.10: Changes in Neighborhood House Price Rank and the Age of the Child when Parents Move



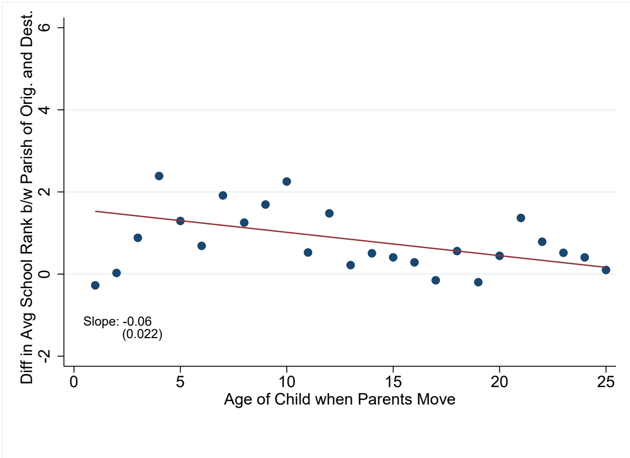
(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move across municipalities and the increase in the neighborhood house price rank (measured at municipality level) when moving from the origin municipality to the destination municipality. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

Figure C.11: Changes in School Quality Rank and the Age of the Child by the homeownership status



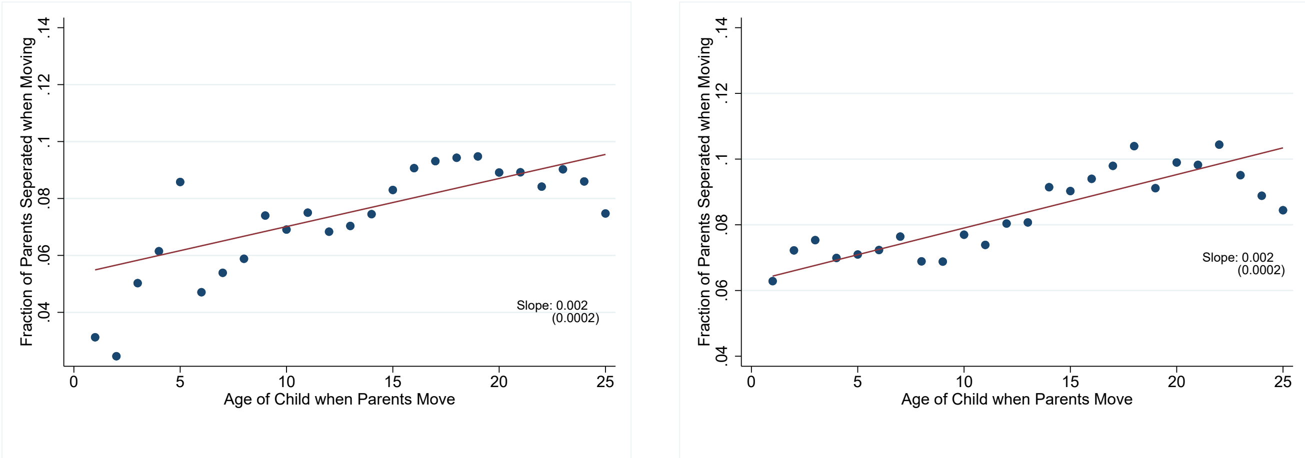
(a) Owners



(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in the neighborhood average school quality rank (at parish level) among all neighborhoods, during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000). Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

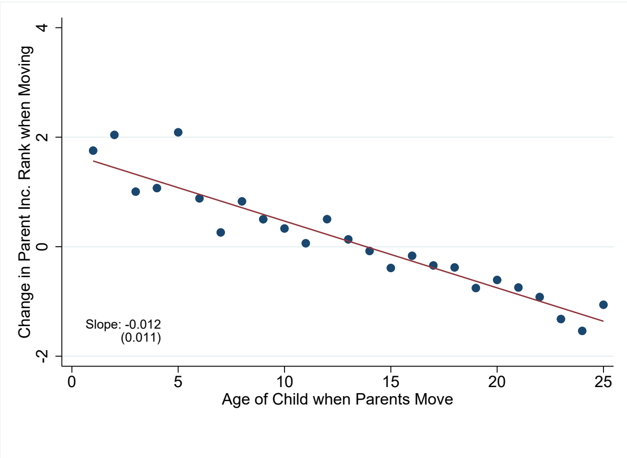
Figure C.12: Age of Child when Parents Move and Fraction of Parents Separate when Moving by the homeownership status



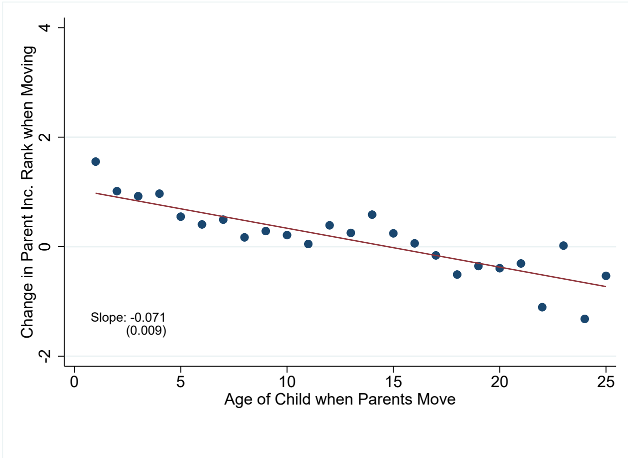
(a) Owners (b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the fraction of parents getting a divorce during the year they move to a new neighborhood. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

Figure C.13: Age of Child when Parents Move and the Change to Family Income Rank by the homeownership status



(a) Owners



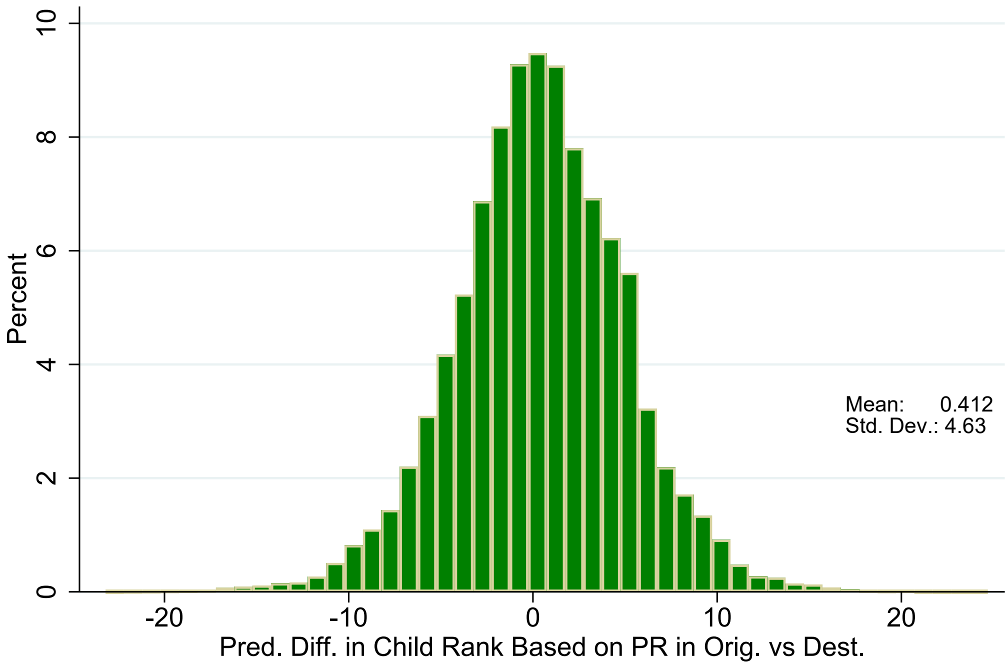
(b) Renters

Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the increase in parental rank in the national income distribution during the year they move. Panel A shows the relationship for the sample of homeowners. Panel B shows the relationship for the sample of renters.

D Quality of Moves

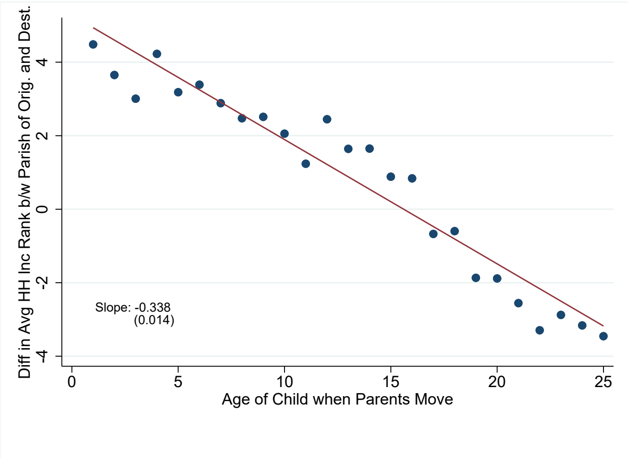
Figure D.1 presents the distribution of the quality of the moves for the sample of movers who moved exactly once across municipalities between 1982 and 2000.
Figure D.2 presents a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the parish income rank (where all parishes are ranked based on their average household income) during the year the family moves.
Figure D.3 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood house price rank (measured at parish level) when moving from the origin parish to the destination parish.
Figure D.4 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood house price rank (measured at municipality level) when moving from the origin parish to the destination municipality.
Figure D.5 shows a binned scatter plot depicting the relationship between the child's age when parents move and the increase in the neighborhood school quality rank (at parish level) during the year the family moves.

Figure D.1: Distribution of the Quality of Moves (Difference in Predicted Outcomes of Children)

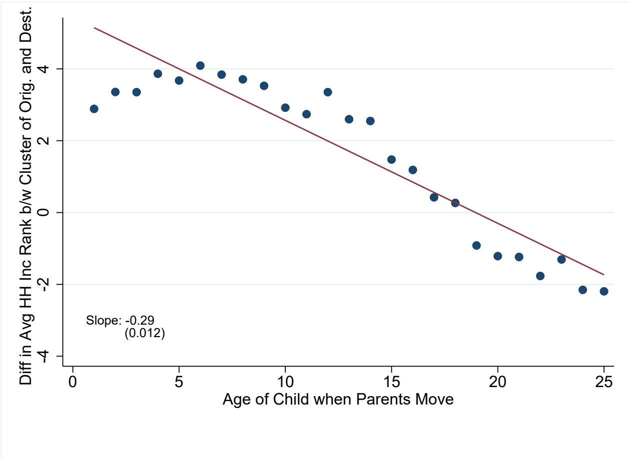


Notes: This figure shows the distribution of the quality of the moves, measured by the mean difference in permanent residents' income ranks between the destination and origin for the relevant parent income rank and birth cohort.

Figure D.2: Quality of Moves (by Average Neighborhood Income) and the Age of the Child when Parents Move



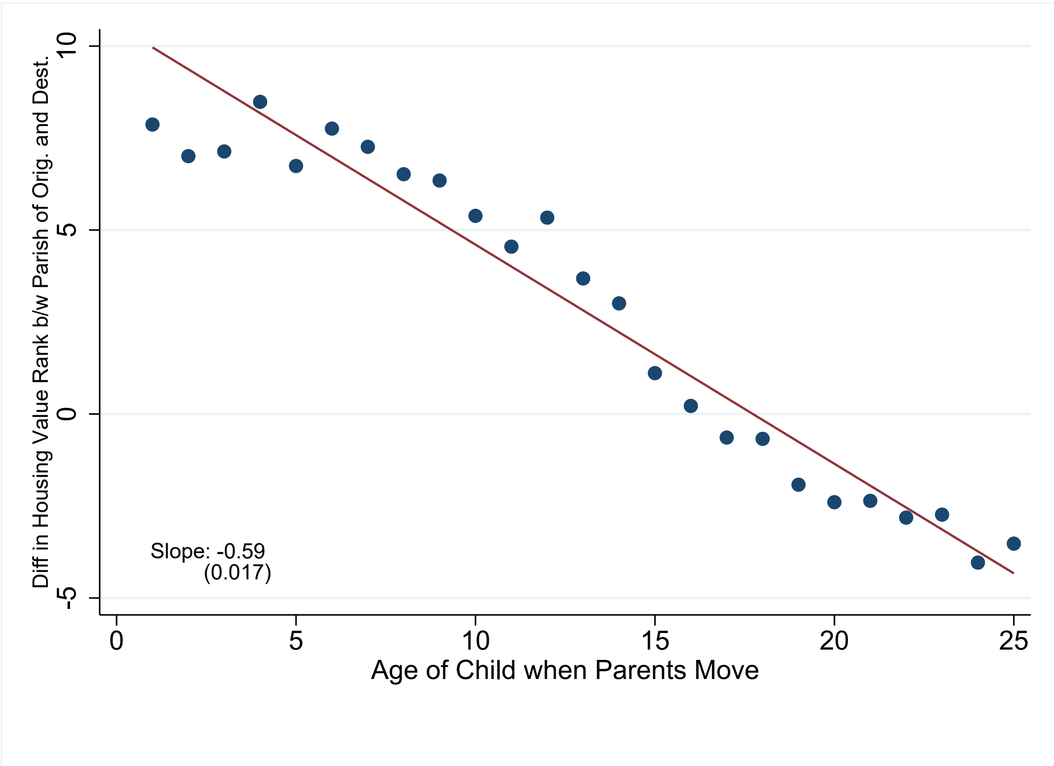
(a) Parish Level



(b) Cluster Level

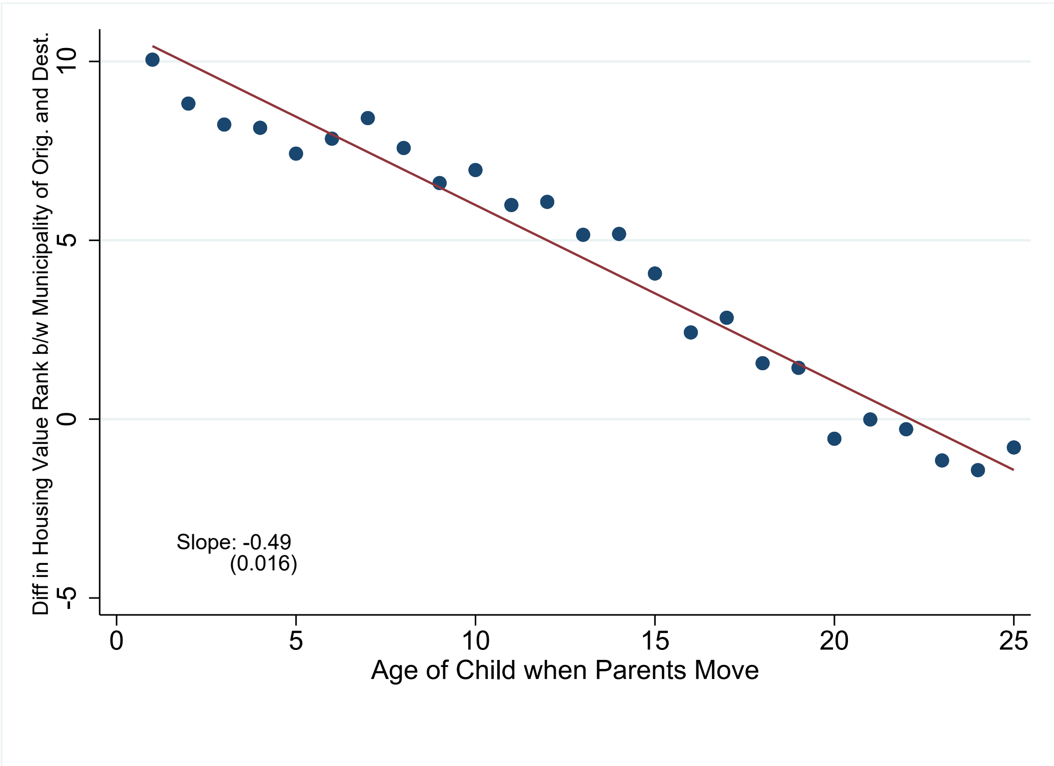
Notes: Panel (a) presents a binned scatter plot depicting the relationship between the child's age when parents move and the neighborhood average family income (at parish level) rank among all neighborhoods during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000). Panel (b) shows a binned scatter plot depicting the relationship between the child's age when parents move, and the neighborhood average family income (at 600 house clusters) rank among all neighborhoods during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000).

Figure D.3: Changes in Neighborhood House Price Rank and the Age of the Child when Parents Move



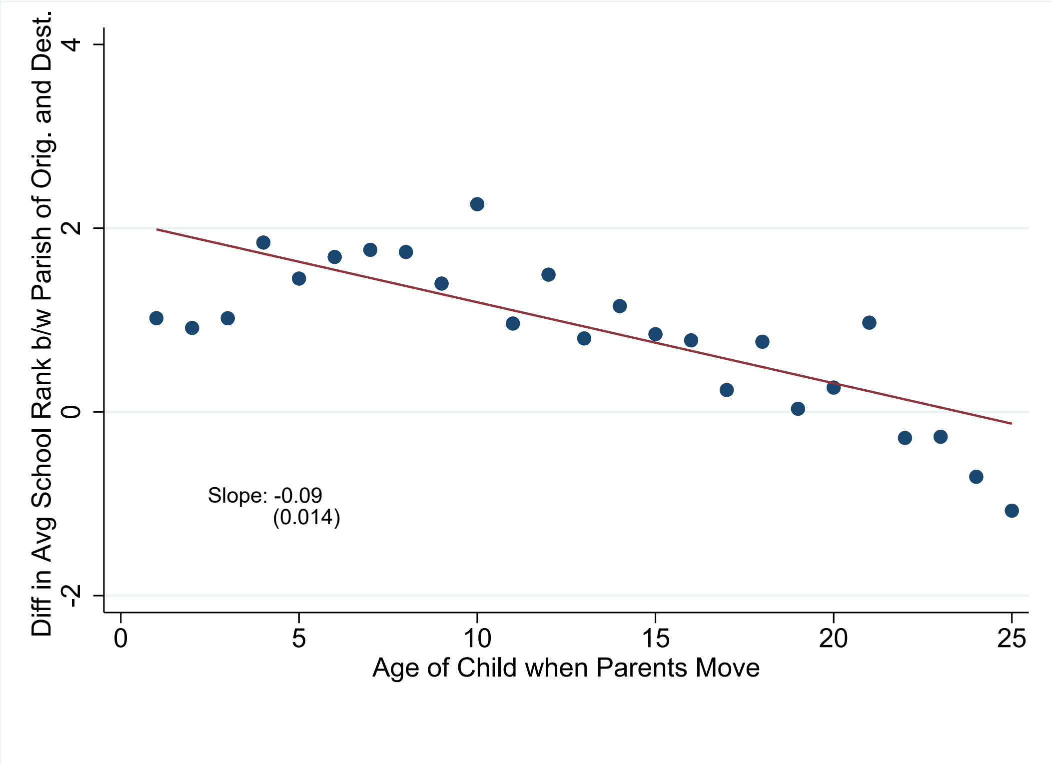
Notes: This figure shows a binned scatter plot depicting the relationship between the child's age when parents move across municipalities and the increase in the neighborhood house price rank (measured at parish level) when moving from the origin parish to the destination parish. The rank is calculated based on parish-level house prices averaged over the sample years (1982-2000).

Figure D.4: Changes in Neighborhood House Price Rank and the Age of the Child when Parents Move



Notes: This figure shows a binned scatter plot depicting the relationship between the child's age when parents move across municipalities and the neighborhood house price rank (measured at municipality level) when moving from the origin municipality to the destination municipality. The rank is calculated based on municipality-level house prices averaged over the sample years (1982-2000).

Figure D.5: Changes in School Quality Rank and the Age of the Child when Parents Move



Notes: This figure shows a binned scatter plot depicting the relationship between the age of the child when parents move and the neighborhood's average school quality rank (at parish level) among all neighborhoods during the year the family moves. The rank is calculated based on neighborhood-level house prices averaged over the sample years (1982-2000).

E Birth Characteristics and Later Life Outcomes

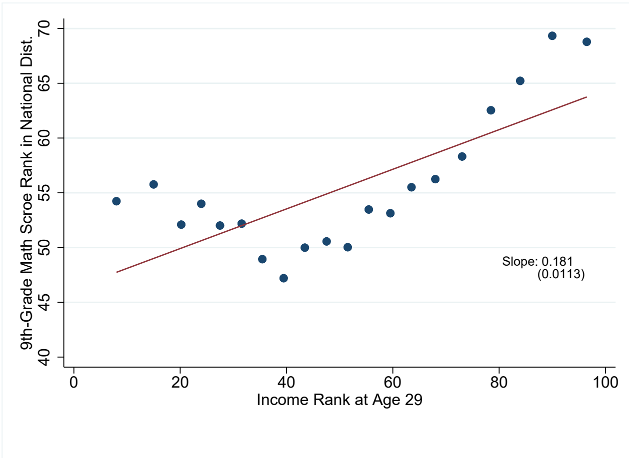
Table E.1 presents the summary statistics of the sample used for the placebo analysis in Section 6, i.e., the sample of children born between 1997-2005 in Denmark.
Figure E.1 presents a binned scatter plot of the relationship between birth weight and children's test scores in different subjects at the national exam at the 9th grade (around age 15) for 1997-2004 birth cohorts.
Figure E.2 presents a binned scatter plot of the relationship between children's test score rank and their adulthood income at age 29 for the sample of 1990 birth cohort whose income is measured at age 29 (observed in the last year of the data).
Figure E.3 presents a binned scatter plot depicting the relationship between the child's age when parents move and her birth length rank for the placebo analysis sample (1997-2005 birth cohorts).
Figure E.4 presents a binned scatter plot of the relationship between children's age at the time of the move and the probability of being a low birth weight as an infant for the placebo analysis sample (1997-2005 birth cohorts).
Figure E.5 presents a binned scatter plot of the relationship between the education of parents and the age of the child when parents move for the placebo analysis sample (1997-2005 birth cohorts).
Figure E.6 presents the fraction of movers over the age of the child for children born in 2000-2001 whose parents moved exactly once across municipalities between 1997 and 2019.

Table E.1: Summary Statistics for The Placebo Analysis

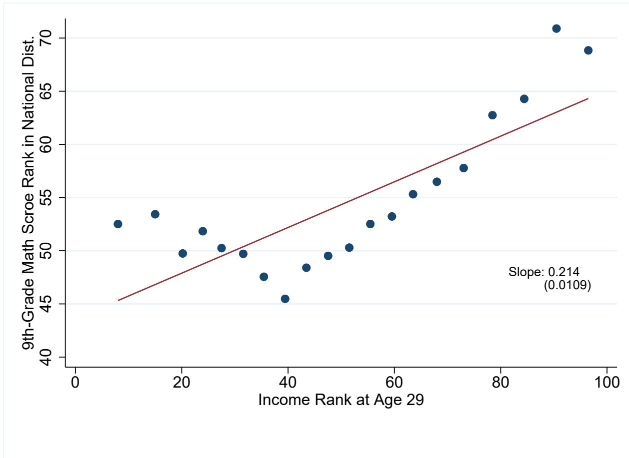
Variable	Mean (1)	Std. dev. (2)	Median (3)	Num. of obs. (4)
Panel A: Permanent residents: Families who do not move across municipalities				
Child weight at birth (gram)	3,508	591	3,530	265,545
Child length at birth (cm)	51.96	2.54	52.00	262,483
Child math problem-solving ability	0.03	0.92	0.06	124,045
Child math knowledge	0.02	0.93	-0.02	124,513
Child Danish writing ability	0.10	0.87	0.17	125,457
Child Danish reading ability	-0.00	0.90	0.07	124,923
Parent family income	50,886	16,549	49,100	263,432
Parent years of schooling	14.08	2.37	14.00	257,361
Parent property value	63,906	68,695	53,331	265,088
Panel B: Families who move 1-3 times across municipalities				
Child weight at birth (gram)	3,508	582	3,530	273,145
Child length at birth (cm)	51.99	2.53	52.00	270,049
Child math problem-solving ability	0.10	0.94	0.16	93,886
Child math knowledge	0.10	0.95	0.01	94,242
Child Danish writing ability	0.17	0.89	0.17	95,044
Child Danish reading ability	0.12	0.92	0.12	94,658
Parent family income	52,074	17,978	49,468	270,523
Parent years of schooling	14.76	2.50	14.50	267,280
Parent property value	72,773	70,342	61,637	272,878
Panel C: Families who move exactly once across municipalities				
Child weight at birth (gram)	3,518	583	3,540	139,567
Child length at birth (cm)	52.03	2.52	52.00	137,981
Child math problem-solving ability	0.14	0.93	0.16	54,640
Child math knowledge	0.15	0.94	0.01	54,836
Child Danish writing ability	0.20	0.88	0.18	55,267
Child Danish reading ability	0.14	0.91	0.14	55,062
Parent family income	53,451	18,333	50,773	138,237
Parent years of schooling	14.75	2.49	14.50	136,004
Parent property value	76,642	72,893	65,972	139,363

Notes: This table presents summary statistics for the samples used in the placebo tests in Section 6, which consists of all children who were born in Denmark between 1997 and 2005. I report the summary statistics for three different subsets of this sample. Panel (A) presents the statistics for the sample of permanent residents, i.e., children whose parents never moved from 1997 to 2019. Panel (B) shows the statistics for those who moved across municipalities once, twice or three times between 1997 and 2019. Panel (C) reports the statistics for the sample of movers who moved only once across municipalities between 1997 and 2019. I use the Consumer Price Index (CPI) to adjust for inflation. All dollar values are in 2010 US dollars (using an exchange rate of 6.7 DKK per US dollar). Academic achievement is measured using 9th-grade standardized scores in Mathematics (problem-solving and knowledge) and Danish (reading and writing). See Sections 2.1 and 2.2 for further details on variable and sample definitions.

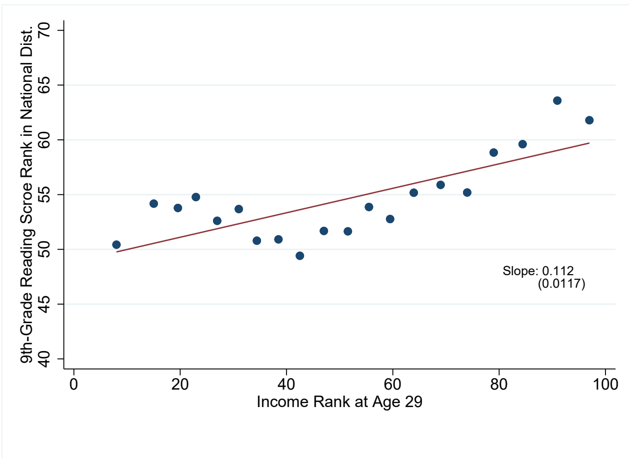
Figure E.1: Test Scores and Adulthood Income



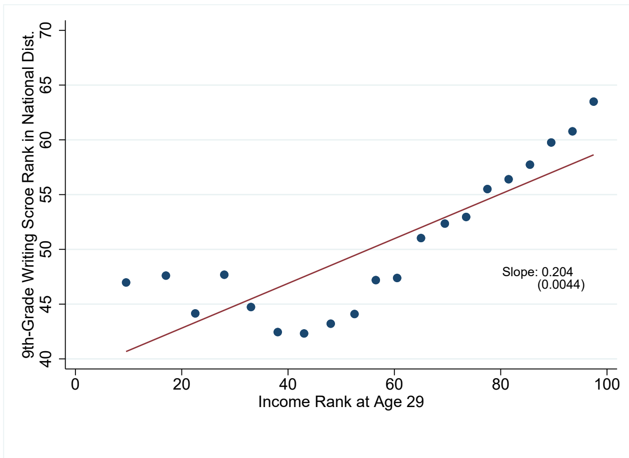
(a) Mathematics Knowledge



(b) problem-solving



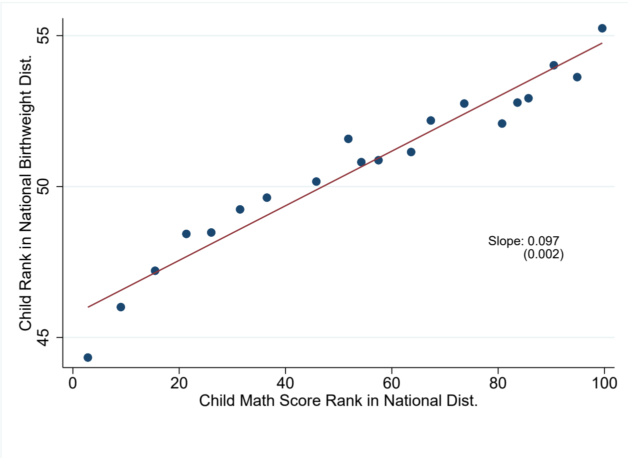
(c) Reading



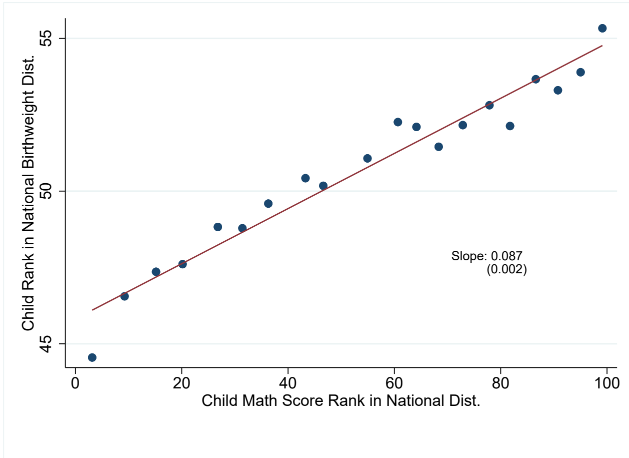
(d) Writing

Notes: This figure presents a binned scatter plot of the relationship between children's test score rank and their adulthood income at age 29. Panel (a) shows the relationship for mathematics knowledge scores, Panel (b) for mathematics problem-solving score, Panel (c) for the Danish reading score, and Panel (d) for Danish writing score. The sample consists of 1990 birth cohorts. Gross income is measured at age 29 in 2019.

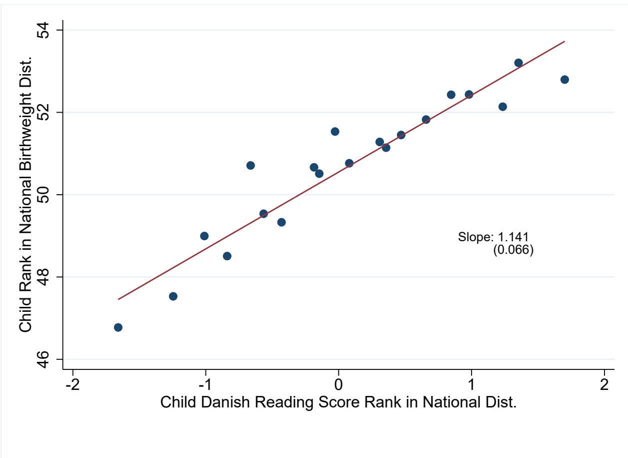
Figure E.2: BIRTH WEIGHT AND TEST SCORES



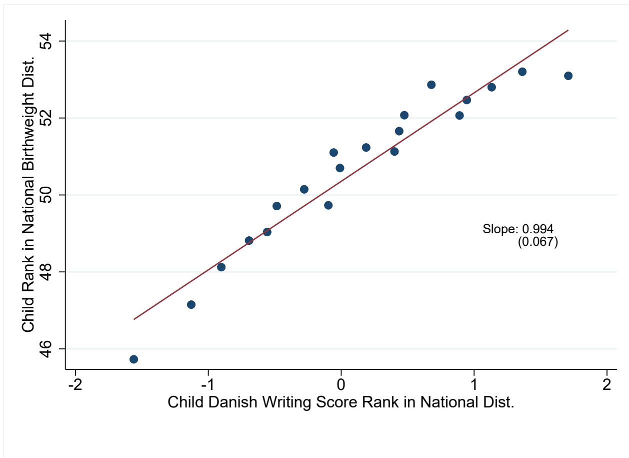
(a) Mathematics Knowledge



(b) problem-solving



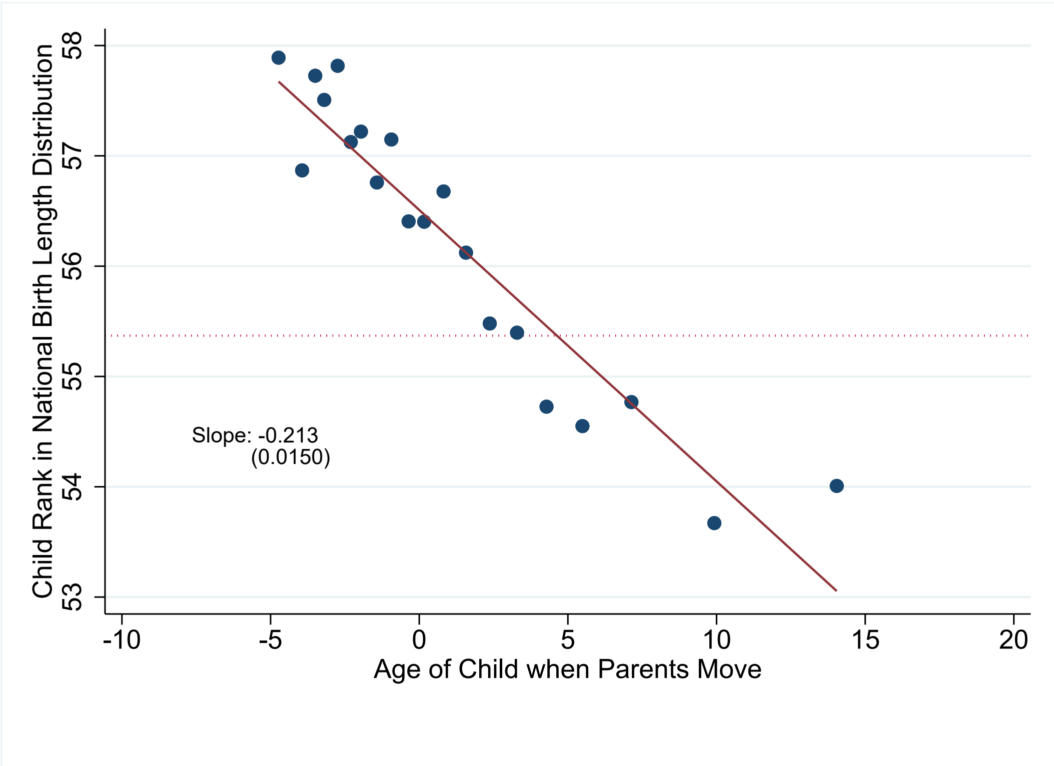
(c) Reading



(d) Writing

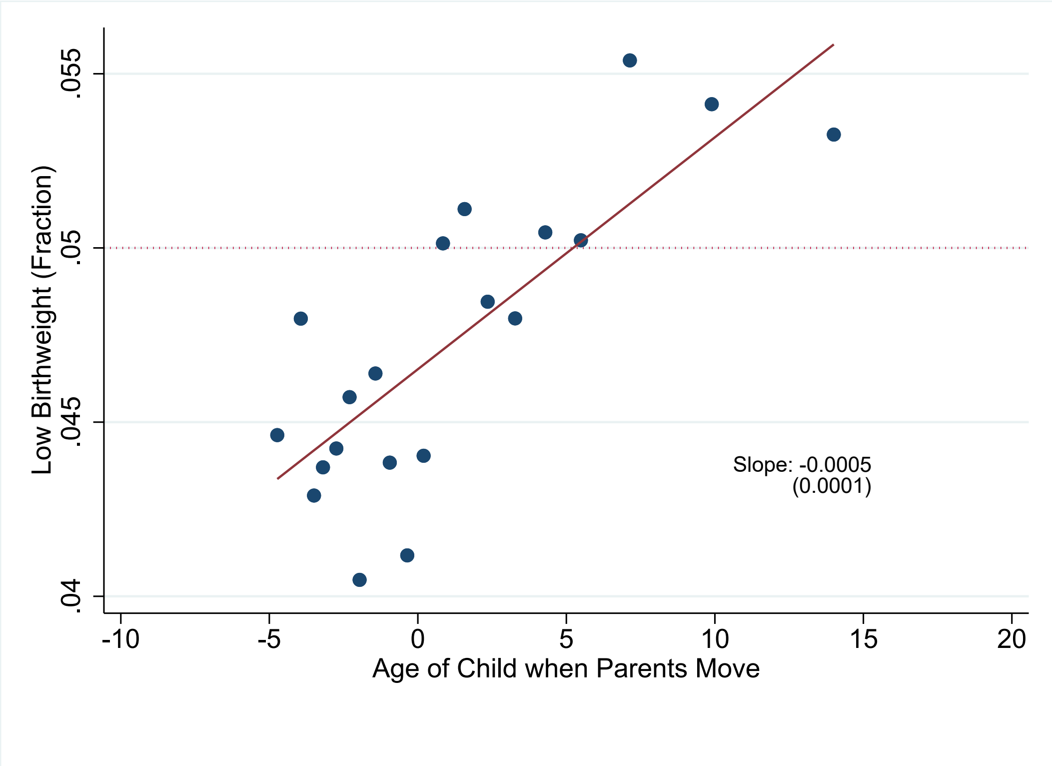
Notes: This figure presents a binned scatter plot of the relationship between children's birth weight rank and their test scores in different subjects in 9th grade. Panel (a) shows the relationship for mathematics knowledge scores, Panel (b) for mathematics problem-solving score, Panel (c) for the Danish reading score, and Panel (d) for Danish writing score. The sample consists of 1997-2004 birth cohorts.

Figure E.3: BIRTH LENGTH AND THE AGE OF THE CHILD WHEN PARENTS MOVE



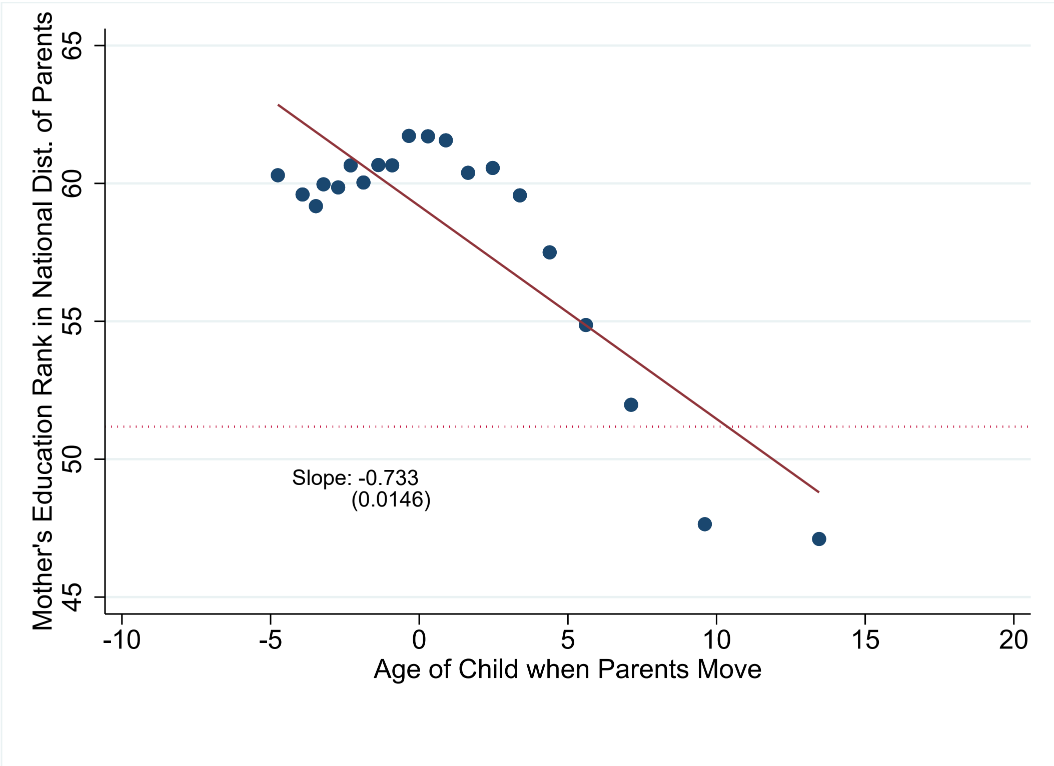
Notes: This figure plots a binned scatter plot depicting the relationship between the age of the child when parents move and her rank in the national birth length distribution relative to all others in her birth cohort. The sample includes all children in the placebo analysis sample whose parents moved across municipalities exactly once between 1997 and 2019. The horizontal dotted line shows the average birth length rank for children of permanent residents, i.e., those who never moved across municipalities between 1997 and 2019.

Figure E.4: Low Birth Weight and the Age of the Child When Parents Move



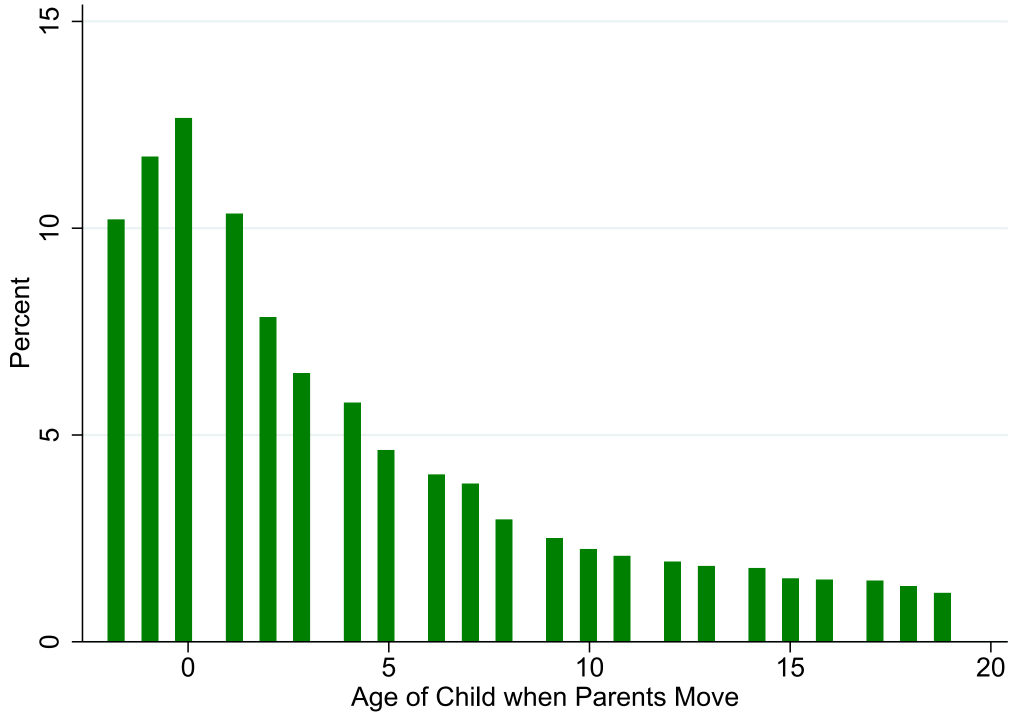
Notes: This figure plots a binned scatter plot depicting the relationship between the age of the child when parents move and the fraction of children who were low birth weight infants. Low birth weight is defined as a birth weight of less than 2500 grams (5 pounds and 8 ounces). The sample includes all children in the placebo analysis sample whose parents moved across municipalities exactly once between 1997 and 2019. The horizontal dotted line shows the fraction of low birth weight infants among permanent residents, i.e., those who never moved across municipalities between 1997 and 2019.

Figure E.5: Parental Education Level and the age of the child When Parents Move



Notes: This figure presents a binned scatter plot of the relationship between the education of parents and the age of the child when parents move. The education of parents is measured as the years of schooling of mothers. The sample is restricted to the main estimation sample, i.e., families who moved exactly once during the sample period. The horizontal dotted line shows the average education of permanent residents, i.e., those who never moved across municipalities between 1997 and 2019.

Figure E.6: Distribution of One-time Movers by the Child's Age



Notes: This figure presents the percentage of movers by the age of the child at the time of the move for those families who moved exactly once during 1997-2019 (which is the main estimation sample for the placebo analysis). The sample is restricted to families of children born between 2000-2001.

F Heterogeneity of Effects- Placebo Tests

Table F.1 presents the placebo exposure effect estimates separately for the sample of home-owners (panel A) and renters (Panel B). See Section 6 for details.

Table F.1: Placebo Exposure Effect Estimates- by the Homeownership Status

Dependent Variable: Child's Birth Weight Rank

							Family FE		
Specification:	Pooled	Age >= 0	Age < 18	No cohort controls	Family Level	Child nbhd FE	Baseline	No cohort controls	Time-varying controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Homeowners									
Placebo Effect (γ)	0.034	0.033	0.037	0.030	–	0.034	-0.011	-0.016	-0.010
	(0.010)	(0.010)	(0.012)	(0.011)	–	(0.011)	(0.025)	(0.024)	(0.017)
Number of Obs.:	66594	43955	65384	66594	—	65115	66594	66594	66594
Panel B: Renters									
Placebo Effect (γ)	0.024	0.025	0.031	0.023	–	0.017	0.036	0.039	0.031
	(0.011)	(0.011)	(0.012)	(0.010)	–	(0.011)	(0.025)	(0.024)	(0.025)
Number of Obs.:	48,918	29,897	47,652	48,918	—	46,802	48,918	48,918	48,918

Notes: This table reports estimates of the placebo annual childhood exposure effects on children's birth weight ranks (γ). The estimates can be interpreted as the rate at which birth weight ranks of children who move one year earlier covary more with the birth weight ranks of permanent residents in the destination. Standard errors are shown in parentheses. Each column reports estimates from a regression of a child's birth weight rank on the difference between permanent residents' predicted ranks in the destination versus the origin, interacted with the child's age at the time of the move (m). I permit separate linear interactions for $m \leq 0$ (when the mother moves across municipalities before the arrival of her child) and $m > 0$ and report the coefficient on the interaction for $m \leq 0$. Each regression also includes additional controls specified in Equation (14). Permanent residents' predicted ranks are constructed using linear regressions of children's ranks on parents' ranks in each neighborhood and birth cohort. Column (1) reports the estimate of γ from Equation (14) using all children in the primary analysis sample of one-time movers, defined in the notes to Table E.1 (Panel C). Columns (2) and (3) restrict the sample children moved at or after age 0 (Column 2) and at or before age 18 (Column 3). In column (4), I exclude the cohort interactions with the predicted outcomes of permanent residents in the origin and destination location and instead include a single control for the predicted outcomes of permanent residents in the origin. Column (6) adds fixed effects for the child's neighborhood in the last sample year to column (1) specification. Column (7) adds family fixed effects to the baseline specification in column (1). Column (8) adds family fixed effects to the specification in column (4) that does not include cohort-varying intercepts. Column (9) adds controls for changes in parental marital status and income rank in the year before versus after the move, along with their interactions with the age of the child at the time of the move and indicators for moving above and below age 0, to the specification in column (7). Panel A restricts the sample to children whose parents were homeowners before and after moving across municipalities. Panel B restricts the sample to children whose parents were renters before and after moving across municipalities. See Section 6 for details.