Where You Grow Up Matters
Effects of Childhood Municipal Exposure on Intergenerational Mobility*

By

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Abstract

This paper studies whether childhood location causally affects intergenerational mobility in Denmark. Using population-wide administrative data for cohorts born 1969–1989, I replicate the mover design of Chetty and Hendren (2018a) in a Danish setting, comparing children who move between municipalities at different ages to identify exposure effects on adult outcomes, isolating the effect of an extra year of childhood exposure while controlling for family background. I find that children who relocate to municipalities where local children have better long-run outcomes go on to achieve higher adult income, educational attainment, and hours worked, with gains increasing in proportion to the time they spend growing up there. The magnitude of the effects implies that a substantial share of the differences in intergenerational mobility across municipalities of upbringing reflects causal childhood exposure effects, rather than selection in parental residential sorting alone. Effects are robust, largest for children from low-income families, and suggestive of asymmetry, with especially large losses from moves to worse areas. These results point to the relevance of policies aimed at improv-ing conditions in low-opportunity municipalities.

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1. Introduction

Intergenerational mobility varies markedly across geographical settings. Even within Denmark, children who grow up in different local areas face very different long-run prospects, conditional on their parents' position in the income distribution (Eriksen and Munk, 2020; Cholli et al., 2024). A central question in recent research is therefore whether these geographic differences reflect causal effects — meaning that where a child grows up directly shapes their future outcomes — or whether they primarily capture selection of families into different locations (Chetty and Hendren, 2018a; Deutscher, 2020; Laliberté, 2021; Cholli et al., 2024).

I find that the municipality in which a Danish child grows up has a causal impact on their adult income, educational attainment, and hours worked. These results are obtained by implementing the empirical framework of Chetty and Hendren (2018a) on comprehensive Danish administrative data, where *place* is defined as municipalities. The main contribution of the paper is to provide the first quasi-experimental estimates of childhood exposure effects on intergenerational mobility in Denmark. By estimating an exposure-effect parameter that is directly comparable to those in existing literature, the paper enables a clean benchmark of the magnitude of place-based childhood effects in a Nordic welfare-state context. Using population-wide register data, I consistently measure outcomes for the full population and conduct extensive robustness tests alongside complementary heterogeneity analyses. A key advantage of the Danish setting is that individuals can be followed from birth, allowing exposure to place to be measured over the entire childhood period, in contrast to Chetty and Hendren (2018a), who observe moves only from age 9 onward. Moreover, the use of administrative data eliminates reliance on self-reported earnings and provides consistently measured income information for both parents and children.

I construct a population-wide panel of more than one million individuals born between 1969 and 1989, linking detailed childhood residential histories to adult outcomes. Intergenerational mobility at the municipal level is measured using the long-run adult outcomes of children who spend their entire childhood in each municipality. Following Chetty and Hendren's empirical framework, I then use movers to identify causal municipal exposure effects. Specifically, I exploit variation in the age at which children move across Danish municipalities and compare otherwise similar children who move to municipalities with different levels of intergenerational mobility at different ages. This design isolates the effect of an additional year of childhood exposure to a place where children attain better or worse adult outcomes conditional on parental income. By focusing on differences in exposure timing rather than on the move itself, the methodology addresses the endogeneity of moving decisions. The key identifying assumption is that selection into moving does not vary with the age at which children move. While this is a strong assumption, the main results are robust to validation, including sibling fixed effects and controls for contemporaneous changes in parental income and marital status.

Children who move to better municipalities experience statistically and economically meaningful gains in adult outcomes, with effects increasing with the number of years spent in the destination area. Under a linear exposure model, each year spent in a municipality where local children rank one percentile higher in adult income raises a child's own adult income rank by approximately 0.031-0.037 percentile points, conditional on parental income rank. These effects imply that a move at birth would close more than half of the origin–destination gap in intergenerational mobility. Despite operating in a markedly different institutional and policy context, the estimated causal childhood exposure effects are strikingly similar to those documented in other countries. In particular, the implied annual convergence rate is close to the magnitude reported by Chetty and Hendren (2018a) for the U.S., and comparable to estimates from Deutscher (2020) for Australia. Further, the magnitude of the municipality exposure effects on education closely mirrors findings from Canada obtained by Laliberté (2021). I also find pronounced heterogeneity in these effects. Municipal exposure effects are substantially larger for children from low-income families, indicating that local environments can play an important compensatory role for disadvantaged children, even in an egalitarian welfare state. Conversely, this also implies that municipalities that fail to provide supportive institutions and opportunities may reinforce rather than mitigate existing inequalities. I also find suggestive evidence of asymmetry, with larger losses from moves to worse areas than gains from moves to better areas, although these differences are estimated with limited statistical precision. Taken together, these findings suggest that place-based childhood environments matter not only on average, but differentially across income distribution, and that adverse local conditions may be particularly difficult to undo. Local context thus remains a key determinant of long-run outcomes in Denmark, especially for children growing up in low-income families.

In the remainder of the paper, I proceed as follows. I begin by reviewing the theoretical background and related literature. I then describe the data in Section 2. In Section 3, I outline the empirical framework from Chetty and Hendren (2018a) and explain how I adapt it to the Danish context. Section 4 presents the main results and robustness checks. Finally, in Section 5, I discuss the findings and conclude.

1.1. Background

This section outlines the theoretical mechanisms linking childhood environments to intergenerational mobility and situates the paper within the related empirical literature, with a particular focus on the Danish context.

1.1.1. *Theoretical Framework*

Municipal resources and local environments can shape how parental background translates into adult outcomes through several interconnected channels. Following Brandén et al. (2023), these mechanisms can be grouped into three overarching channels: (i) Peer effects imply that children's aspirations and behaviors are shaped by those of their classmates and neighbors, such that the prevailing behaviors in each area influence the children exposed to them (Jencks et al., 1990). (ii) Collective socialization refers to the transmission of local norms and aspirations through adult role models and social pressure within a neighborhood, whereby, for example, highly educated adults can shape children's attitudes and expectations beyond their own families. (Galster, 2008). (iii) Institutional resources, in turn, emphasize that differences in local institutions — such as schools, libraries, and sports clubs — can affect educational performance and longer-run outcomes (Jencks et al., 1990; Hermansen et al., 2020). In the Danish context, municipalities constitute a relevant unit for studying these mechanisms, as they are responsible for key inputs into children's development, including primary schooling, early childhood education and care, and a wide range of social services (Finansministeriet, 2025). At the same time, municipalities differ in their socio-demographic composition, which generates systematic variation in peer groups and local social environments (VIVE, 2021). The literature has long emphasized that it is difficult to disentangle the relative importance of these mechanisms in shaping children's outcomes (Brandén et al., 2023; Pickett and Pearl, 2001; Galster, 2012). Accordingly, this paper does not seek to separately identify these channels; rather, it uses this conceptual framework to interpret and discuss the estimated exposure effects.

A central insight in the literature is that children differ in their susceptibility to geographical influences, and these differences are closely linked to family background (Brandén et al., 2023). In models of intergenerational mobility, the transmission of advantage is often framed in terms of parental endowments and investments (Becker and Tomes,1986; Cunha and Heckman, 2007). Endowments include knowledge, norms, abilities, and access to networks, while investments refer to deliberate inputs of time and money aimed at shaping children's outcomes. When such family resources are limited, children may rely more heavily on external environments, potentially making them more responsive to the quality of local institutions, peer groups, and role models (Jencks et al., 1990; Brandén et al., 2023). This interaction between family resources and local context suggests that exposure effects may be heterogeneous across socioeconomic groups. Several studies emphasize cumulative disadvantage, arguing that children from low-SES families may be particularly sensitive to growing up in deprived areas, as adverse family and neighborhood conditions can reinforce one another over time (Brandén et al., 2023). Consistent with this view, long-term exposure to disadvantaged neighborhoods has been shown to reduce educational and labor-market outcomes, especially for children from less advantaged backgrounds (Ludwig et al., 2013; Wodtke et al., 2016; Morrissey and Vinopal, 2018). At the same time, this framework also implies that children from low-SES families may benefit disproportionately from exposure to more favorable environments, while a contrasting hypothesis is that children from more advantaged families may have more to lose in deprived settings (Levy, 2019).

1.1.2. Related Literature

A large body of research across a wide range of countries has documented correlations between children's geographic area of upbringing and their economic outcomes in adulthood (Page and Solon, 2003; Sharkey and Faber, 2014; Black and Devereux, 2011; Aydemir and Yazici, 2019; Kenedi and Sirugue, 2023; Dodin et al., 2024). These associations may reflect genuine place-based effects, but they may also arise from selection, whereby families with certain characteristics choose – or are constrained to – particular residential areas. To obtain a more causal understanding of the role of local areas, earlier studies have exploited exogenous variation generated by housing policy experiments. One of the most prominent examples is the Moving to Opportunity (MTO) program, a U.S. initiative launched in 1994 that offered low-income families the opportunity to move to less disadvantaged areas through targeted housing vouchers. The aim was to examine whether changes in neighborhood conditions could improve children's long-term mobility. Early results showed positive effects on mental health and educational outcomes — especially for girls — but did not provide clear evidence of higher adult incomes (Kling et al., 2007; Ludwig et al., 2013). Later analyses with longer follow-up horizons showed that children who moved at an early age achieved substantially better economic outcomes in adulthood, including higher incomes (Chetty et al., 2016).

More recently, a growing body of research has revisited place-based effects using improved identification strategies, richer sources of quasi-experimental variation, and large-scale administrative data (e.g. Chetty and Hendren, 2018a; Chetty et al., 2016; Chyn, 2018; Damm and Dustmann, 2014). Chetty and Hendren (2018b,a) develop a large-scale mover design using U.S. tax records, showing that each additional year spent in a higher-opportunity area raises a child's adult income rank (conditional on their parents' income rank) by about 4 percent of the gap between origin and destination. These exposure effects accumulate roughly linearly with childhood residence. The authors conclude that a large share of geographic variation in intergenerational mobility reflects causal place effects rather than selective sorting, and document similar patterns for outcomes such as college attendance and teenage birth rates.

A key open question in this literature is whether similarly large exposure effects exist outside the United States. The U.S. is characterized by relatively high levels of income inequality and residential segregation, which may amplify place-based differences. Several recent studies have adapted the design of Chetty and Hendren (2018a) to explore neighborhood effects in other institutional contexts (Alesina et al., 2021; Britto et al., 2022; Deutscher, 2020; Laliberté, 2021). Deutscher (2020) applies the mover-based framework to Australian data and finds that childhood exposure to higher opportunity areas increases adult earnings, with effect sizes that vary systematically by age at move. Convergence toward the outcomes of permanent residents in the destination is more modest in early childhood (around 0.011) but substantially larger during adolescence (around 0.042). Similarly, Laliberté (2021) implements the design using Canadian data and shows that movers' educational attainment converges toward that of permanent residents at an annual rate of approximately 4.5 percent.

Within Denmark, direct evidence of causal place-based effects on intergenerational mobility remains limited. Most studies have examined place-based influences through specific natural experiments or policy interventions rather than estimating broad causal effects on income mobility. For example, Damm (2014) exploits quasi random refugee dispersal and finds no overall negative impact of disadvantaged neighborhoods on refugees' labor market outcomes once family sorting is accounted for, though she identifies important network effects from highly educated co-ethnic peers. Billings et al. (2024) use a quasi-random assignment of public housing and show sizable neighborhood effects on children's educational achievement and mental health, driven mainly by very local conditions such as nearby unemployment and education levels. However, these findings are harder to generalize to the broader population, as both interventions apply to highly specific groups rather than the general population.

Considering the relationship between intergenerational mobility and geography in Denmark, Eriksen and Munk (2020) show that mobility is highest in middle-income rural municipalities in Western Jutland, while both urban areas and low-income rural municipalities exhibit substantially lower mobility. More recently, Cholli et al. (2024) decompose cross-neighborhood variation in intergenerational mobility in Denmark using a generalized mobility model that separates selection effects from residual location effects. They find that selection is the dominant driver, explaining roughly two to three times as much variation as location effects. Importantly, their analysis seeks to account for cross-sectional differences in neighborhood mobility parameters, whereas my study focuses on estimating the causal effect of childhood exposure to a given location.

This study contributes to the literature by providing a direct replication of the Chetty–Hendren mover design in a Danish setting, thereby delivering causal estimates of childhood residential exposure effects that are directly comparable to those from other countries. In addition, the analysis offers an alternative perspective on the relative importance of selection versus causal effects by focusing on marginal exposure effects rather than cross-sectional mobility differences.

2. Data

I utilize register data provided by the Danish Ministry of Employment, specifically the Register of Households and Families (FAIN) from 1980–2007, the Population Register (BEF) from 1985–2023, the Education Register (UDDA) from 1980–2023, the Income Register (IND) from 1980–2023, and the Register-based Labor Force Statistics (RAS) from 2008–2022. The section proceeds in three parts. First, I present the data sources and the sample. Second, I review the variables used, and the definition of lifetime income and its ranking is outlined. Third, I document similarities and differences between permanent residents and one-time movers.

2.1. Sample Selection

I identify all individuals born 1969–1989 from FAIN (1,391,225 in total), which form the core sample. After restrictions, the final dataset covers 1,134,634 individuals (81.5%; Table 1). Individuals are required to (i) reside in Denmark throughout childhood (ages 0–17), (ii) reside in Denmark in adulthood (ages 32–40), and (iii) have at least one parental income observation when the father was aged 35–50. See definitions below.

Table 1. Sample Restrictions and Reamaining Individuals

Restriction step	**Individuals**	**Percent**
All individuals born 1969–89	1,391,225	100 %
Reside in Denmark, ages 0–17	1,253,742	90.1 %
Reaide in Denmark, ages 32–40	1,166,926	83.8 %
Parental income observed (father ages 35–50)	1,134,634	81.5 %
Final analysis sample	1,134,634	81.5 %

Note: The table documents how the final analysis sample is restricted from the full population of individuals born 1969–1989. Childhood municipality of residence is observed from 1980 onward, implying partial coverage for the 1969–1979 cohorts but full coverage for those born 1980 or later.

The first step is to identify the municipality of residence during ages 0–17, observed from 1980 onward. Consequently, those born in 1969 enter the data from age 11, while cohorts born in or after 1980 have full childhood coverage.[1] Roughly 140,000 individuals are excluded due to incomplete childhood residence, leaving 90.1 % of the initial sample. Next, BEF is used to exclude individuals not residing in Denmark during ages 32–40, the period when adult income (from IND) is measured (see Section 2.2.1), reducing coverage to 83.8%. Information on parents, partners, and other individual characteristics is then merged (see Section 2.2).

1. I account for this by interacting the exposure effects with birth cohort indicators in the estimation model; see Section 3.2.1.

Parents are defined as the registered birth mother and father from the FAIN-register.[2] This definition remains fixed regardless of later family changes. Step-parents and new partners are thus not included in the parental income measure, as the focus is on long-term endowment and investment from registered parents at birth. This approach aligns with standard practice in intergenerational mobility research and ensures consistency across families experiencing divorce or re-partnering (Chetty and Hendren, 2018a; Deutscher, 2020; Björklund et al., 2012). Parental income is defined as the average of both parents' incomes in years when the father was aged 35–50 (see Section 2.2.1). Observations are dropped if valid parental income is unavailable, for instance due to migration, early death, or incomplete coverage for older fathers who are not observed because the data begin in 1980.

Requiring continuous residence in Denmark during the relevant childhood (ages 0–17) and adulthood (ages 32–40) windows implies that individuals who spend parts of these periods abroad are excluded. If international mobility during these years is systematically related to unobserved ability or family resources, this may affect the composition of the analysis sample. The estimates should therefore be interpreted as describing municipal exposure effects for individuals whose life courses remain observable within Denmark during the relevant periods.

2.2. Variable Definitions

The analysis relies on three sets of variables: location, income and work hours. FAIN and BEF provide the core identifiers that allow individuals to be linked to their parents, siblings, and registered partners, as well as to their municipality of residence. IND and RAS supply information on income, wages, and working hours, while UDDA contains detailed information on educational attainment.

1. Location. Municipalities serve as the definition of location. FAIN records each individual's municipality of residence at year-end through the variable KOM. Denmark currently has 98 municipalities responsible for local welfare, education, planning, and infrastructure. A 2007 reform reduced their number from 271 to 98, requiring a minimum population of 20,000 (Indenrigs- og Sundhedsministeriet, 2005).[3] I recode pre-2007 municipalities to this structure. For the 13 that were split, individuals are assigned to the municipality with the largest share of prior residents. This generates only minor discontinuity, as merged or neighboring municipalities are generally similar in socioeconomic conditions and mobility, thereby preserving the meaningful variation central to the analysis.

2. Birth parents are defined as the mother and father registered for the child in the FAIN-register at age 0.
3. With a few exceptions.

2. Income, wage and work hours. I use both total income and wage income from the IND register, while working hours are obtained from the RAS register. All monetary values are adjusted for inflation using Statistics Denmark's consumer price index, such that they are expressed in 2015 prices. Both income and wages are measured as household averages during the period when the individual in the main sample is aged 32 to 40. The household is defined as the individual from the main sample and, if applicable, their registered partner. Parental household income is defined as the combined income of the mother and father during the years when the father is aged 35 to 50. See Section 2.2.1 for further details on income definitions.

Individual income is measured using the variable PERINDKIALT, which captures total personal income, including wages, public transfers, capital income (excluding imputed rent from owner-occupied housing), and other income sources attributable to the individual. Income is reported before taxes, labour market contributions, and pension contributions. It excludes items such as health-related subsidies for medicine, dental care, physiotherapy, etc.; lottery winnings; employer pension contributions and early pension withdrawals; and any income not reported to public authorities. As an alternative income measure, I use the variable LOENMV, which captures the individual's annual wage income before taxes and after the labor market contribution (AM-bidrag). This variable includes all types of A-income reported by employers, such as wages, paid holiday allowances, bonuses, and certain types of fees. To measure work intensity, I use the variable LOENTIMER, sourced from the RAS register. As with income, I calculate average annual hours worked for individuals in the main sample between the ages of 32 and 40 (regardless of any partner).

3. Individual characteristics. Characteristics are drawn from UDDA and BEF. Sex is defined using the variable KOEN from BEF. Education is measured by HFAUDD in UDDA, recorded as the highest level obtained up to 2023 and grouped into seven categories from »primary school or below/unknown« to »PhD or equivalent« (see Appendix table A2). Ethnic origin is captured by two BEF variables: IE_TYPE, distinguishing natives, immigrants, and descendants (born in Denmark to non-citizen parents), and OPR_LAND, recording country of origin. For immigrants, origin is determined by country of birth (or citizenship if unknown); for descendants, by the mother's origin (or the father's if the mother's is unknown). I classify origin into three groups: (1) Danish origin, (2) other Western, and (3) non-Western.[4] Last, the variable CIVST is used to indicate the parents' marital status

4. Immigrants are underrepresented in the sample because individuals without full residence records during childhood (ages 0–17) are excluded, disproportionately affecting those who arrived as children or were first observed after 1980.

in the year before and after the move. I construct four categories, treating married and cohabiting as a single group: (1) from married to married, (2) from married to unmarried, (3) from unmarried to unmarried, (4) from unmarried to married.

2.1.1. *Proxy for Lifetime Income and Income Ranking*

Studies of intergenerational income mobility rely on lifetime income measures across generations, but limited data often require using income at specific ages as proxies. This can introduce bias, particularly errors-in-variables bias, which arises when short-term income poorly reflects lifetime income (Haider and Solon, 2006; Mazumder, 2005). Additionally, life-cycle bias occurs because the correlation between current and lifetime income varies systematically with age (Haider and Solon, 2006; Böhlmark and Lindquist, 2006; Grawe, 2006). Transitory income shocks that persist over time can further distort estimates (Mazumder, 2005). To reduce these biases, the literature recommends averaging income over several years, ideally when individuals are aged 32 to the late 40s — a strategy supported by findings in Böhlmark and Lindquist (2006), whose Swedish context is highly comparable to Denmark. Guided by this evidence and the structure of the Danish registers, I measure children's adult income as the average household income between ages 32 and 40. By doing this, the data allows me to average over the same interval of years for almost all cohorts. Further, all these years lie in the non-biased interval, which makes them a good proxy for lifetime income (Böhlmark and Lindquist, 2006). I do not extend the window beyond age 40, as doing so would require further restricting the sample (see Section 2.1) while yielding only limited improvements in measurement accuracy. The chosen window, therefore, balances measurement quality against sample representativeness. For parents, a symmetric income window is not feasible. Because the Danish income registers begin in 1980, many parents — especially those of older child cohorts — are already past their early 30s at first observation. I therefore measure parental income as average household income when the father is aged 35 to 50. This interval falls within the literature-recommended range for approximating permanent income, generates a meaningful multi-year average, and maximizes sample coverage by ensuring that most families contribute at least one, and often several, valid income observations (see Section 2.1). Averaging over a relatively broad age span is particularly important given substantial variation in parental ages across cohorts and the fact that the estimation model does not directly condition on parental cohort (see Section 3.2.1).

Despite these efforts, the measurement strategy mechanically generates uneven income coverage across cohorts because income data are only available for the period 1980–2023. For example, individuals born in 1989 are observed only at ages 32–34, while earlier cohorts cover the full 32–40 window. Parents also differ in the number of observed years within the 35–50 age range, and some of this variation follows trends in children's cohorts. Following Chetty and Hendren

(2018a), I include cohort fixed effects and their interactions with the municipal exposure measure in all estimation models to account for cohorts entering the sample at different points in childhood (see Section 3.2.1). This specification will also mitigate potential bias arising from cohort specific differences in the number of income years used to construct average outcomes.

My income definitions differ from those in Chetty and Hendren (2018a) in terms of both timing and data consistency. They measure parental income over a fixed five-year period (1996–2000), regardless of the parent's age, and define income using a mix of sources: self-reported 1040 tax returns and third-party reports such as W-2 and SSA-1099. In contrast, I measure parental income based on the father's age (35–50) using consistent administrative register data, which avoids variation in measurement quality across individuals. For children, Chetty and Hendren (2018a) assess family income between ages 24–30 (baseline 24), while I use income from ages 32–40, aligning more closely with the period shown to best reflect life-time earnings. These differences aim to reduce life-cycle and measurement bias in my analysis.

Table 2 presents the income and wage characteristics of the sample. The table reports individual level means for key economic variables, including both individual and household income, as well as coverage rates for partner and parental information, measured as the share of possible years in which these could have been observed.

The main analysis uses *adjusted* household income (hereafter: household income). For children — the individuals whose mobility is analyzed — household income is calculated as an average across annual observations. In years when the individual has no partner, household income equals individual income. In years with a partner, household income is defined as the sum of the individual's and the partner's incomes divided by two. This simple adjustment method reflects that a partner's income contributes to the resources available to the individual while also accounting for variation in household size, which is substantial in the sample. As a robustness check, alternative specifications are estimated using (i) unadjusted household income and (ii) individual income. This allows for an assessment of how sensitive the results are to the chosen income definition. For parents, household income is calculated as the sum of the mother's and father's personal incomes, divided by the number of parents with income information each year regardless of whether the parents live together.[5]

5. This applies irrespective of cohabitation status, as both parents' incomes are considered relevant for the long-term intergenerational mobility.

Table 2. Income and Labor Market Characteristics of the Sample

Variable	**Mean**	**Share of possible years**
Income (1,000 DKK, 2015 prices)		
Household income (adjusted)	390,748	
Household wage income (adjusted)	323,094	
(i) Household income (unadjusted)	689,540	
(i) Household wage income (unadjusted)	574,880	
(ii) Individual income, women	354,849	
(ii) Individual income, men	426,919	
(ii) Individual wage income, women	279,655	
(ii) Individual wage income, men	366,488	
Labor supply		
(ii) Annual work hours	1,530	
Partnership status		
Years with partner	5.8	72.1 %
Years with partner, women	6.0	74.6 %
Years with partner, men	5.6	69.6 %
Parental background		
Parental household income (adjusted)	330,180	
Parental wage income (adjusted)	254,375	
Years with parental income information	14.6	91.2 %

Note: Income is measured in 1,000 DKK (2015 prices). Negative incomes are set to zero to avoid distortions from capital losses (affecting about 1.3 % of the sample). Partnership status reports the number of years an individual is observed with a registered partner (spouse or cohabiting). The main income measure is adjusted household income. Alternative specifications use (i) unadjusted household income and (ii) individual income. Work hours are based on the 1,056,008 individuals observed in RAS in at least one relevant year.

A key measure of intergenerational mobility is the extent to which children's relative position in the income distribution differs from that of their parents (Black and Devereux, 2011; Chetty et al., 2014). I define children's and parents' relative positions within their respective national income distributions in terms of percentiles. These form the basis for analyzing *intergenerational mobility*, defined as children's upward or downward movement within the ranked distribution, conditional on their parents' position. Specifically, children are ranked by percentile within their own birth cohort, while parents are ranked by percentile relative to other parents of children in the same cohort. This procedure ensures comparability across generations despite changes in income levels over time, following the approach in Chetty and Hendren (2018a).[6] Using ranks rather than log incomes avoids problems with zeros and extreme values (Chetty et al., 2014; Bratberg et al., 2017). On the other hand, it does not capture shifts in *absolute* inequality within generations. In other words, a child who attains the same rank as their parents is considered »stationary«, regardless of whether the income gap between those ranks has widened significantly from one generation to the next.

2.3. Summary Statistics: Permanent Residents and Movers

The analysis sample consists of 1,134,634 individuals born in Denmark between 1969 and 1989 who meet the inclusion criteria (see Table 1). To study municipal exposure effects, I divide the population into two main groups: permanent residents, who never changed municipality during childhood (ages 0–17), and one-time movers, who changed municipality exactly once. These groups contain 842,357 and 178,056 individuals, respectively. Individuals with multiple childhood moves are excluded from the estimation sample due to ambiguous exposure patterns but are included in the descriptive statistics to reflect overall population characteristics.

6. I apply the same ranking approach to wage income and hours worked (the latter only for the mainsample individuals, not their parents).

Table 3. Comparison of Permanent Resisents and Movers

	Permanent	Movers	Difference	Full sample
Parents				
Household income rank	49.5 (28.2)	52.7 (30.0)	3.17^{***}	49.5 (28.8)
Household wage income rank	49.9 (28.2)	51.9 (30.1)	1.97^{***}	49.5 (28.8)
Father with university degree	0.06 (0.2)	0.11 (0.3)	0.05^{***}	0.07 (0.3)
Mother with university degree	0.03 (0.1)	0.06 (0.2)	0.03^{***}	0.03 (0.2)
Children				
Household income rank	50.7 (28.4)	48.9 (29.5)	-1.83^{***}	49.5 (28.8)
Household wage income rank	50.8 (28.4)	48.7 (29.6)	-2.11^{***}	49.5 (28.8)
Share female	0.49 (0.5)	0.50 (0.5)	0.01^{***}	0.49 (0.5)
University degree	0.18 (0.4)	0.21 (0.4)	0.03^{***}	0.18 (0.4)
Vocational degree	0.77 (0.4)	0.76 (0.4)	-0.01^{***}	0.76 (0.4)
Work hours rank (annual avg.)	50.5 (28.4)	48.3 (29.3)	-2.21^{***}	49.5 (28.8)
Observations	842,357	178,056		1,134,634

Note: Standard deviations in parentheses. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$. The table is based on the final analysis sample. Work hours are based on the 1,056,008 individuals observed in RAS in at least one relevant year.

One-time movers form the primary analysis group, as their single municipal transition allows for clear identification of childhood exposure effects. To validate the quasi-experimental design of Chetty and Hendren (2018a), it is important that movers and permanent residents do not differ fundamentally. They need not be identical, but they must be sufficiently comparable for permanent residents' outcomes to serve as proxies for the place-specific opportunities to which movers are exposed. Table 3 reports descriptive statistics of the two groups.[7] Parents of movers have higher income ranks and education levels than those of permanent residents on average. In contrast, movers themselves attain slightly lower income and work-hour ranks, consistent with findings in the U.S. (Chetty and Hendren, 2018a). These differences likely reflect that movers are exposed to their destination municipality for only part of childhood, that some moves are to lower-opportunity areas, and that relocation may entail short-run disruptions such as school changes. Overall, however, the distributions of both child and parent income ranks overlap substantially across the two groups (see Appendix Figure A1).

7. The distributions by origin and by education for the two groups are shown in Table A1 and Figure A2.

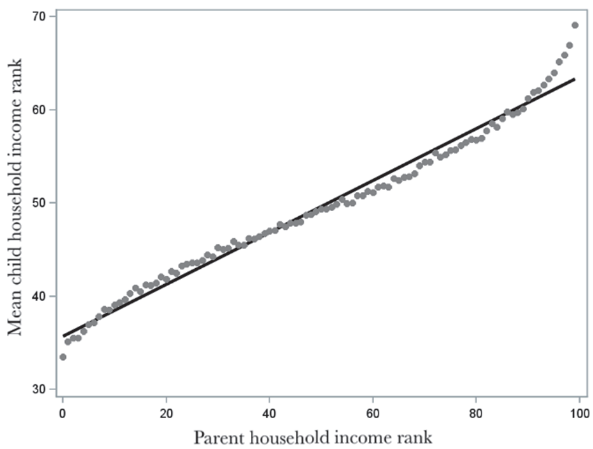
3. Empirical Framework

In this section, I describe intergenerational mobility across Danish municipalities and outline the observational research design, following the framework of Chetty and Hendren (2018a). The objective is to estimate the extent to which spending an additional year of childhood in a higher-mobility location increases a child's adult income rank, conditional on parental income rank. This captures the effect of childhood municipal exposure on intergenerational mobility.

3.1. Geographic Variation in Intergenerational Mobility of Permanent Residence

I rank each child's adult household income, y_i, within the national income distribution of their birth cohort. Parents' household income rank, $p(i)$, is defined analogously among parents of children in the same cohort (see Section 2.2.1). Figure 1 displays an almost linear relationship between parent and child ranks.[8] Under full mobility the slope would be 0, yet children of parents at the 25th percentile attain an average rank of 43.6, compared to 55.6 for children of parents at the 75th percentile. I exploit this near linearity to estimate municipal variation in intergenerational mobility.

Figure 1. Mean Child Income Rank versus Parent Income Rank, Birth Cohorts 1969-1989



Note: Child rank y_i is based on average (adjusted) household income at ages 32-40 and ranked within birth cohort. Parent rank $p(i)$ is based on average household income when the father is aged 35-50 and ranked among parents of children in the same cohort.

8. Appendix Figures A3–A6 provides complementary figures for permanent residents, municipality-specific plots for Copenhagen and Aarhus, and a map of mean parental ranks.

Estimating municipal exposure effects requires substantial geographic variation in intergenerational mobility across Danish municipalities. To document such variation, I restrict attention to *permanent residents*[9] and estimate a rank–rank regression at parental percentile p for each municipality, o, and cohort (cluster), c_c,

$$y_i = \alpha_{oc_c} + \psi_{oc_c} p_i + \varepsilon_i \tag{1}$$

and use the estimated coefficients to construct the predicted mean child rank,

$$\bar{y}_{poc_c} = \hat{\alpha}_{oc_c} + \hat{\psi}_{oc_c} p \tag{2}$$

Because some municipality-cohort cells are thin, I group birth cohorts into three clusters (1969–1975, 1976–1982, 1983-1989) and estimate the predicted mean for each municipality, o, within each cohort cluster, c_c.[10] In this setup, $\hat{\alpha}_{oc_c}$ is the expected child rank at $p = 0$, while $\hat{\alpha}_{oc_c} + 99\hat{\psi}_{oc_c}$ is the expected rank at $p = 99$. With no mobility, $(\hat{\alpha}, \hat{\psi}) = (0,1)$, meaning children's ranks mirror their parents'. With perfect mobility, $(\hat{\alpha}, \hat{\psi}) = (50,0)$, implying all children converge to the mean rank of 50 regardless of parental rank. A higher value of $\hat{\psi}_{oc_c}$ therefore indicates lower mobility, whereas $\hat{\alpha}_{oc_c}$ reflects both the general income level in the municipality and the average income for children from the least advantaged families.

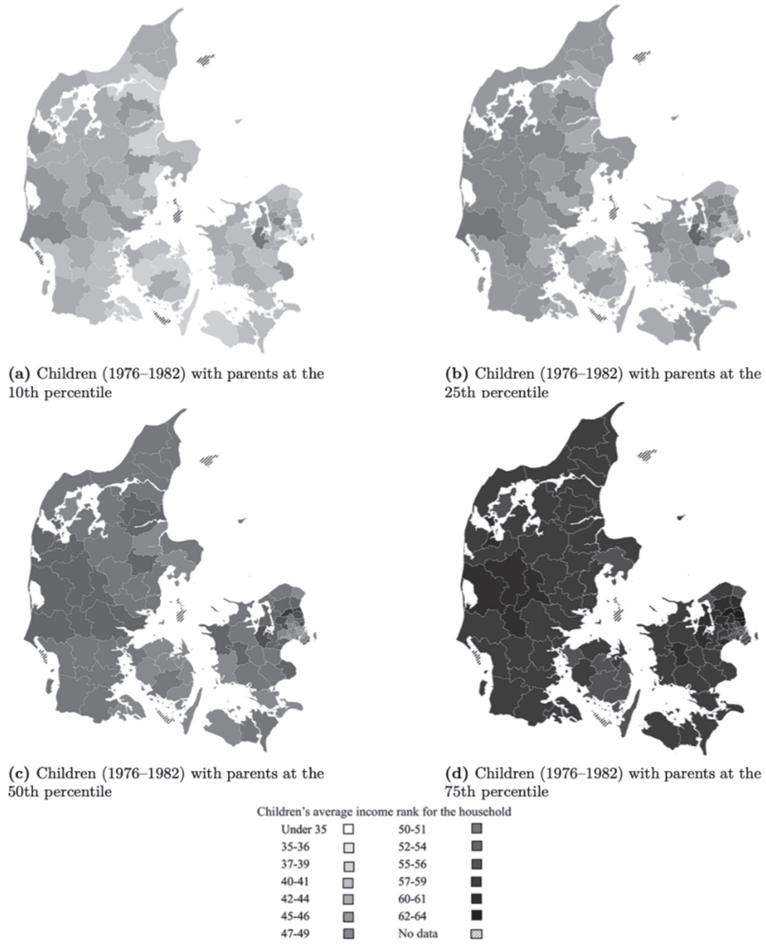
Figure 2 displays predicted mean child income ranks for the 1976–1982 cohort cluster at parental percentiles 10, 25, 50, and 75 (panels 2a–2d).[11] The maps reveal pronounced spatial heterogeneity in intergenerational mobility. Among children of parents at the 10th percentile, predicted adult ranks range from just 32.0 in Ishøj to 49.7 in Lejre — a gap of 17.7 ranks. For children of parents at the 75th percentile, the spread is smaller but still substantial: from 53.7 in Copenhagen to 62.8 in Hørsholm, a difference of 9.1 ranks. These patterns highlight a strong correlation between intergenerational mobility — measured as children's outcomes conditional on parental background — and municipalities.

9. Permanent resident refers to the municipality of childhood residence at ages 0-17.
10. Alternative clusterings were tested, the three-way split balances precision and coverage. Very small islands (Læsø, Fanø, Ærø, Samsø) are excluded due to cell size, removing 1,796 individuals (≈1% of movers).
11. Municipality-level schedules for all clusters, and additional municipality-level details and correlates of the mobility measures at the national level are provided in Appendix Tables A3–A4.

Variation in rank–rank slopes, $\hat{\psi}_{o1976-1982}$, further illustrates these dynamics (see Appendix Table A4). The national average slope for the 1976–1982 cohorts is $\hat{\psi} = 0.26$, but it spans from low in Lejre and Varde $(\hat{\psi} = 0.16)$ to high in Høje-Taastrup $(\hat{\psi} = 0.35)$. Recall, a lower slope implies weaker dependence of children's outcomes on parental rank and therefore greater mobility. This distinction is clearly reflected in predicted ranks: children from the 10th percentile in Varde reach an average adult rank of 47.1, compared to just 36.0 in Høje-Taastrup — a difference of 11.1 points. For children of parents in the 75th percentile, however, the same comparison yields a gap of only 0.8 points (57.6 vs. 58.4), which reflects Varde's lower slope and thus higher mobility.

Western Jutland stands out with distinctive patterns, where several municipalities function as favorable environments for children from disadvantaged families. This finding aligns with Eriksen and Munk (2020). Municipalities such as Varde, Ringkøbing-Skjern, and Billund combine low rank–rank slopes with high average outcomes for children whose parents are at the bottom of the income distribution. This indicates that these municipalities not only exhibit high intergenerational mobility but also provide children from the most disadvantaged families with better chances of attaining relatively high positions in the national distribution as adults. Conversely, children from more affluent families do not perform as well here, since the higher intergenerational mobility means that their parents' high incomes translate less strongly into their own outcomes.

Figure 2. Predicted Mean Child Rank for Permanent Residents across Municipalities (Cohort 1976-1982), by Parental Percentile



Note: Each map shows predicted adult income rank (cohort-percentile scale) for children who grew up permanently in a municipality, conditional on parental rank p and cohort cluster c_c, based on (1)– (2).

These municipality-by-cohort schedules (based on permanent residents) provide the exposure measure for movers in the estimation models. For a child moving from origin o destination d, the origin–destination contrast at parental rank p and cohort cluster c_c is

$$\Delta_{odpc_c} = \bar{y}_{dpc_c} - \bar{y}_{opc_c}$$

which I interact with age-at-move to identify childhood exposure effects (see Section 3.2.1).

3.2. Childhood Exposure Effects: Identification and Estimation

My objective is to estimate how much a child's adult income rank rises, on average, if the child spends one additional year of childhood in a location where permanent residents have a one–percentile-point higher expected outcome, conditional on parents' rank. Thus, the primary **outcome** is the child's adult income rank conditional on parental income rank. The key **exposure measure** is the parental-income–conditional income rank of permanent residents in each municipality, which reflects the place-specific opportunities available in the destination.

Consider first a randomized assignment in which children are placed into a new municipality, d, from age a onward. The best linear predictor of adult outcomes, y_i, in this experimental sample — using the predicted outcome, $\bar{y}_{pdc_c}$, for permanent residents in d — is

$$y_i = \alpha_a + \beta_a \bar{y}_{pdc_c} + \theta_i \tag{3}$$

where the error term, θ_i, collects family inputs and other determinants of y_i and is independent of $\bar{y}_{pdc_c}$ under random assignment. Estimating Equation 3 yields β_a, the average effect of spending ages a and onward in d. Now, the municipal exposure effect at age a can be defined as

$$\gamma_a = \beta_a - \beta_{a+1}$$

i.e., the causal payoff of one year of exposure at exact age a. This coefficient is interesting for three reasons. (i) If $\gamma_a > 0$ for some a, I can reject the null that municipal environments do not matter. (ii) The profile $\gamma_a$reveals which ages are most sensitive to municipal exposure. (iii) The cumulative effect of assignment from birth, $\beta_0; =; \sum_{a=0}^{17} \gamma_a$, quantifies how much cross-area differences reflect causal municipal exposure effects (if $\beta_0 = 0$ they reflect selection only; if $\beta_0 = 1$ they are fully causal).

Because the ideal experiment is not feasible, I estimate exposure effects by exploiting variation in the age of children when families move between municipalities, using Danish administrative register data. In this setting, the error θ_i in Equation 3 may correlate with $\bar{y}_{pdc_c}$ (e.g., parents with bigger ambitions for their children choose higher-opportunity places), so estimating the equation on one-time movers yields an estimate, b_a, that reflects the sum of the causal exposure effect and embedded selection bias

$$b_a = \beta_a + \delta_a, \qquad \delta_a = \frac{\operatorname{cov}(\theta_i, \bar{y}_{pdc_c})}{\operatorname{var}(\bar{y}_{pdc_c})},$$

where δ_a captures selection into destinations. Identification of exposure effects does not require that the destination area be orthogonal (independent) to the child's potential outcomes. Rather, it requires that *the timing* of moves to higher- versus lower-opportunity areas are orthogonal to the child's potential outcomes, as formally stated in the following assumption.

ASSUMPTION 1. Selection does not vary with age at move: $\delta_a = \delta$ for all a. This allows families who move to »better« areas to differ from those who move to »worse« areas but requires that the timing of moves relative to the child's age is orthogonal to potential outcomes. Given this assumption, I identify the exposure effect by comparing children who move at different ages. Formally, consistent estimates are $\gamma_a = b_a - b_{a+1} = (\beta_a + \delta) - (\beta_{a+1} + \delta) = \beta_a - \beta_{a+1}$, since the selection term δ is constant and therefore cancels when differencing adjacent ages. Assumption 1 is strong — families may time moves for family-specific reasons — so I assess its plausibility using robustness analyses reported in Section 4.2.

3.2.1. Parametric Estimates of Childhood Exposure Effects

I estimate the exposure effects embedding selection, $\{b_a\}_{a=1}^{17}$, using the parametric baseline model specification from Chetty and Hendren (2018a) to study how adult income rank, education, and work hours vary with childhood exposure to location.[12] This model implements Equation 3 in observational data by relating children's realized adult outcomes to the difference in predicted outcomes between permanent residents in their destination and origin, interacted with the child's age at move (Chetty and Hendren, 2018a). By conditioning flexibly on parental income, origin municipality, and birth cohort, the model exploits variation in the timing of moves across otherwise comparable children. The estimation sample consists of children from the 1969–1989 cohorts who move exactly once between municipalities at ages $a \in 1, \dots, 17$ (see Table 3). For a child i who moves once from origin, o, to destination, d, at age, a, the adult outcome y_i is estimated as

$$\begin{aligned} y_i = & \sum_{c=1969}^{1989} I(c_i = c)\big(\alpha_{1c} + \alpha_{2c}\bar{y}_{opc_c}\big) + \sum_{a=1}^{17} I(a_i = a)(\zeta_{1a} + \zeta_{2a}p_i) \\ & + \sum_{a=1}^{17} b_a I(a_i = a)\Delta_{odpc_c} + \sum_{c=1969}^{1989} \kappa_c I(c_i = c)\Delta_{odpc_c} + \varepsilon_{1i} \end{aligned} \qquad \text{(Model 1)}$$

where Δ_{odpc_c} is the origin–destination opportunity contrast at parental rank p and cohort cluster c_c based on permanent residents (see Section 3.1). The first two terms together control for differences in parental income, origin municipality, and birth cohort: $I(c_i = c)$ and $I(a_i = a)$ are cohort and age-at-move indicators;

12. Owing to Denmark's smaller population size, the fully semi-parametric fixed-effects specification in Chetty and Hendren (2018a) is estimated too imprecisely to be informative in this setting. I therefore focus on the parsimonious parametric specification of the exposure model.

$(\alpha_{1c}, \alpha_{2c})$ absorb cohort-specific levels and the quality of the origin location, which is modeled by interacting the predicted outcomes for permanent residents in the origin at p with the birth cohort fixed effects, $\bar{y}_{opc_c}$; (ζ_{1a}, ζ_{2a}) capture age-specific move disruptions and their dependence on parental income rank, p_i. $\{b_a\}_{a=1}^{17}$ are the coefficients of interest, representing the age-specific municipal exposure effects still including selection, δ. The terms κc allow these effects to vary across birth cohorts, accounting for the fact that some cohorts enter the sample later in childhood (see Section 2.1) and that later cohorts are observed over fewer income years (see Section 2.2.1). The *causal* municipal exposure effect at age a is then identified as $\gamma_a = b_a - b_{a+1}$ as outlined above. ε_{1i} is an idiosyncratic error term.

Motivated by prior evidence of approximately linear exposure estimates in the U.S., I estimate a *linear* version of the above model in which each childhood year contributes the same marginal effect. This specification is similar to the linear model presented in Chetty and Hendren (2018a), with a key difference: while Chetty and Hendren estimate both exposure effects and selection effects jointly — by leveraging moves that occur after income is measured — I estimate only the exposure effect, γ, since post-outcome mobility is not observed in my data. I replace the set of age-specific coefficients, $\{b_a\}_{a=1}^{17}$, with a single parameter γ, scaled by the remaining childhood years in the destination

$$A_i \equiv 18 - a_i, \qquad \text{and} \qquad \sum_{a=1}^{17} b_a I(a_i = a)\Delta_{odpc_c} \longrightarrow \gamma A_i \Delta_{odpc_c}$$

The estimating equation then becomes

$$\begin{aligned} y_i = \sum_{c=1969}^{1989} I(c_i = c)\left(\alpha_{1c} + \alpha_{2c}\bar{y}_{opc_c}\right) + \sum_{a=1}^{17} I(a_i = a)(\zeta_{1a} + \zeta_{2a}p_i) \\ + \gamma A_i \Delta_{odpc_c} + \sum_{c=1969}^{1989} \kappa_c I(c_i = c)\Delta_{odpc_c} + \varepsilon_{2i} \end{aligned} \qquad \text{(Model 2)}$$

Here, γ denotes the annual causal municipal exposure effect, A_i represents the number of remaining childhood years spent in the destination municipality, and the remaining are defined as in Model 1. Under linearity, the cumulative effect of assignment from birth equals $\beta_0 = \sum_{a=0}^{17} \gamma = 18\gamma$. Hence, the theoretical upper bound in a purely causal, no-selection benchmark — where a child assigned at birth fully inherits the destination's opportunity — implies $\beta_0^{max} = 1$, so that $\gamma^{max} = \frac{\beta_0^{max}}{18} \approx 0.056$ i.e., about 5.6 percent of the cross-area gap per childhood year. This bound provides a scale for interpreting estimated γ.

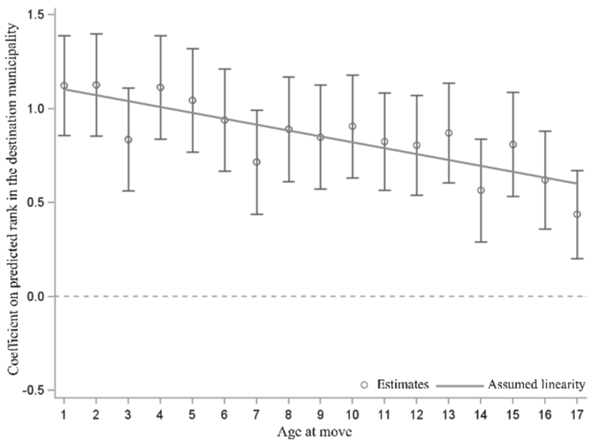
4. Results

This section quantifies how childhood municipal exposure shapes adult outcomes. I first present baseline parametric estimates of age–at–move exposure shares, b_a. I then impose linearity to obtain the average annual *causal* exposure effect, γ, across income rank, wageincome rank, educational attainment, and work-hours. Next, I examine the identifying assumption using sibling fixed effects and controls for contemporaneous parental changes. Finally, I document heterogeneous effects by cohort, move direction, gender, and parental income rank.

4.1. Estimates of the Childhood Exposure Effects to Municipalities

Figure 3 reports the baseline parametric estimates. The coefficients b_a for $a \in 1,2,\ldots,17$ measure the share of Δ_{odpc_c} — the gap between predicted outcomes in destination and origin municipalities, conditional on cohort cluster and parental income rank — that movers on average realize. These coefficients also capture selection, δ. The causal municipal effect at age a is thus identified as $\gamma_a = b_a - b_{a+1}$ (see Section 3.2). I also estimate a specification Model 1b which omits cohort interactions, and Model 1c which additionally controls for the sex of the child and parental education (see Appendix Table A5).

Figure 3. Parametric Municipal Effects on Income (Model 1)



Note: The figure plots point estimates $\hat{b}_a$ for ages at move $a - 1, \ldots, 17$ based on the parametric specification (Model 1). Vertical lines show 95% confidence intervals.

All estimated exposure effects are statistically different from zero at the 1 pct. significance level. This holds across all three specifications and for all age groups (see Appendix Table A5). The estimates lie in the interval 0.44–1.13, with the highest values in the early years of life (e.g., $\hat{b}_1 = 1.12$) and a gradual decline toward the end of childhood ($\hat{b}_{17} = 0.44$). For example, $\hat{b}_6 = 0.94$ means that a child who moves at age 6, on average, realizes 94 pct. of the difference in outcomes of permanent residents between their origin and destination municipality. By contrast, if they had moved at age 16, they would, on average, realize only 62 pct., since $\hat{b}_{16} = 0.62$. When controlling for the child's sex and the parents' educational attainment in Model 1c, most estimates decline slightly, indicating that a small part of the effect on children's outcomes is explained by differences in family background not captured by parental income rank alone. Model 1b, which omits cohort controls, clearly reveals the model's sensitivity to variation in data coverage, which underscores the importance of accounting for variation in measurement accuracy across birth cohorts.

The differences between the point estimates are insignificant, which limits the statistical power to precisely identify *causal* age effects, $\gamma_a = b_a - b_{a+1}$. This result is consistent with the findings in Chetty and Hendren (2018a). Although it is not possible to draw a clear conclusion from the differences between age-specific estimates, a clear declining trend emerges.

Assuming municipal effects are linear, γ can be estimated as the slope of the line in the figure, as captured by Model 2. Table 4 reports the linear estimates, along with specifications Model 2b and Model 2c.[13] The baseline linear exposure effect is estimated at $\hat{\gamma} = 0.037$. This estimate is robust to alternative income definitions and sample restrictions. Notably, the estimate is identical when using wage income instead of total household income, as reported in the main specification. Additional robustness checks show the estimate remains unchanged when using individual income and is only slightly higher with unadjusted household income (Appendix Table A6). The results are also robust to variation in the number of years with observed parental income (Appendix Table A7). For comparison, the theoretical maximum effect is $\gamma^T = 0.056$ (see Section 3.2.1). Given that **Assumption 1** holds, this implies the following: On average, a child moving from one municipality to another realizes 3.7 pct. of the rank difference between permanent residents in the destination and origin municipality per year of exposure, interpreted as the *causal effect of exposure*. More concretely, the effect can be illus-

13. As above, (b) omits cohort interactions. (c) additionally controls for sex and parental education.

trated as follows: Imagine a child moving from a municipality where the expected rank is 50 to another municipality where the expected rank is 60 — conditional on the parents' rank in the income distribution. This yields a 10-percentile difference in area opportunity. If the child moves at age two, they will spend 16 childhood years in the new area. They would thus realize 16 years × 3.7 pct. = 59.2 pct. of the difference, corresponding to an expectedrank of 55.9. [14]

Consistent with findings from the U.S. (Chetty and Hendren, 2018a), my estimates suggest that most of the variation in intergenerational mobility across Danish municipalities can be attributed to causal municipal exposure effects. The estimated convergence rate of 3.7 percent per year of exposure implies that children who relocate at birth can experience substantial improvements (loses) in expected adult outcomes. Specifically, full exposure to a municipality with significantly better (worse) outcomes among permanent residents from ages 0 to 17 can increase (decrease) the child's expected adult income rank by up to $\beta_0 = \sum_{a=0}^{17} \hat{\gamma} =$ 0.67pct. of the origin–destination gap.

Table 4 also reports the parametric estimates of the linear municipal effect for two alternative outcomes: (i) the probability of obtaining a qualifying (competency-conferring) education and (ii) the percentile rank with respect to total hours worked (measured at ages 32–40). The effects on education are sizable. The estimates range from 0.025 to 0.027 and are statistically significant at the 1 pct. level. This implies that children exposed to municipalities with a higher share of individuals holding a qualifying education have a significantly greater probability of attaining a similar education themselves. The effect is largest in the baseline specification (Model 2) but changes only marginally when controlling for child gender and parental education in Model 2c, suggesting that the municipality's role in educational mobility cannot be explained solely by these background factors. The effects on hours worked are slightly smaller but still clear. The estimates range from 0.008 to 0.009 and are statistically significant at the 5-pct. level. This indicates that the municipal environment's influence on labor-market attachment — measured by hours worked — is positive, though more muted than for earnings and educational outcomes.

14. This example is, of course, a simplification, since the effect — if the assumption holds — is an average effect at the national level and therefore not necessarily applicable to a specific move between two municipalities.

Table 4. Parametric Estimates of Linear Municipal Exposure Effects ($\hat{\gamma}$)

	Model 2	**Model 2b**	**Model 2c**
Income rank, $\hat{\gamma}$	0.037^{***}	0.050^{***}	0.031^{***}
	(0.004)	(0.002)	(0.004)
Observations	176,260	176,260	176,246
Wage income rank, $\hat{\gamma}$	0.037^{***}	0.049^{***}	0.032^{***}
	(0.004)	(0.002)	(0.002)
Observations	174,802	174,802	174,788
Competency-qualifying education, $\hat{\gamma}$	0.027^{***}	0.021^{***}	0.025^{***}
	(0.003)	(0.002)	(0.003)
Observations	175,995	175,995	175,981
Work-hours rank, $\hat{\gamma}$	0.008^{**}	0.015^{***}	0.009^{**}
	(0.003)	(0.002)	(0.003)
Observations	163,853	163,853	163,841
Cohort–interaction controls	yes	no	yes
Controls for parental education & child sex	no	no	yes

Note: Standard errors in parentheses. Significance: $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$. $\hat{\gamma}$ is the linear exposure effect: the average change in the outcome from one additional year spent in a municipality with higher expected opportunity. Model 2b omits interactions between birth cohort and Δ_{odpc_c}; Model 2c adds controls for parents' highest education and child sex. For education and work-hours outcomes, Δ_{odpc_c}, is replaced with $\Delta^{C}_{odpc_c}$ where C is the share with competency-qualifying education or the work-hours rank among permanent residents at parental rank, p, and cohort cluster, c_c.

4.2. Validation of the Critical Assumption

I investigate whether potential outcomes for children who move to higher – or lower – opportunity areas vary with the age at which they move, as required by **Assumption 1**. I decompose the unobserved determinant in Equation (3) as $\theta_i = \bar{\theta}_i + \tilde{\theta}_i$, where $\bar{\theta}_i$ captures family-invariant factors (e.g., genetics) and $\tilde{\theta}_i$ captures time-varying shocks (e.g., parental employment changes). To probe this assumption, I (i) estimate specifications with *sibling fixed effects*, which absorb $\bar{\theta}_i$ and identify exposure effects from within-family timing differences, and (ii) include controls for *changes around the move* in parents' marital status and in parental income rank as a way of capturing $\tilde{\theta}_i$. The robustness checks are conducted and summarized in Table 5.

First, sibling comparisons are used to test whether unobserved family factors bias the estimates. The baseline effect (0.037) is replicated for siblings who moved together and remains positive and significant (0.033) and decreases only slightly when including sibling fixed effects (0.027). The differences across specifications are modest and not statistically significant, indicating that the estimated municipal effect is not primarily driven by family-level selection.[15]

Second, potential time-varying confounders related to parental circumstances at the time of moving are examined. When controlling for parental marital-status changes in the year before and after the move, the estimate is 0.034; adding controls for both marital-status and parental income-rank changes around the move yields 0.035 — both statistically significant — suggesting that contemporaneous changes in parents' socioeconomic conditions do not either drive the results. Further, moves are fairly evenly distributed across childhood (see Appendix Table A8), suggesting that move timing is not systematically linked to children's potential outcomes. There is a mild concentration before school age (a few percentage points higher than other ages) and a notable spike at age 17 (11.2 pct.of all moves), which should be interpreted more cautiously because these moves can reflect the child's own choices (e.g., upper-secondary enrollment) rather than parental housing decisions.[16] The distribution of moves across birth cohorts is highly stable (see Appendix Table A9), reducing concerns about cohort-specific bias. Overall, the robustness checks support the validity of the identifying assumption and strengthen the causal interpretation of the exposure effects.

15. The inclusion of sibling fixed effects reduces the sample size by more than half, which may partly explain the lower precision and the attenuation of the coefficient, with statistical significance now at the 95-percent level.
16. Unlike Chetty and Hendren (2018a), who include moves up to age 24, this analysis restricts to moves before age 18. This focuses on moves typically decided by parents, strengthening the causal interpretation.

Table 5. Robustness Checks of Parametric Municipal Effects

Panel A: Sibling Fixed Effects

	Model 2	Siblings only	Sibling FE
(1) Income rank, $\hat{\gamma}$	0.037^{***}	0.033^{***}	0.027^{**}
	(0.004)	(0.008)	(0.013)
Number of observations	176,260	77,424	77,424
All baseline controls	yes	yes	yes
Sibling fixed effects	no	no	yes

Panel B: Changes in Parental Marital Status

	Model 2	Marital status observed	Marital status control
(1) Income rank, $\hat{\gamma}$	0.037^{***}	0.035^{***}	0.034^{***}
	(0.004)	(0.004)	(0.004)
Number of observations	176,260	135,257	135,257
All baseline controls	yes	yes	yes
Control for marital status × age at move	no	no	yes

Panel C: Changes in Parental Income Rank

	Rank registrered	Rank control	Rank and marital status controls
(1) Income rank, $\hat{\gamma}$	0.034^{***}	0.035^{***}	0.035^{***}
	(0.005)	(0.005)	(0.005)
Number of observations	122,103	122,103	122,103
All baseline controls	yes	yes	yes
Control for change in parental rank × age at move	no	yes	yes
Control for marital status × age at move	no	no	yes

Note: Standard errors in parentheses. Significance levels: $* \, p < 0.10, ** \, p < 0.05, *** \, p < 0.01$. Panel A reports estimates of the linear municipal effect utilizing variation in age at move among siblings to include sibling fixed effects. Panel B assesses robustness to contemporaneous changes in parental marital status around the move, including interactions with the child's age at move. Panel C examines robustness to changes in parents' income rank (and their interaction with age at move), and combinations with marital-status controls. Samples differ by data availability as indicated.

4.3. Heterogenous Effects

Table 6 summarizes subgroup estimates of the linear municipal exposure effect, $\hat{\gamma}$, on adult income rank. Across the full sample, the parametric specification yields $\hat{\gamma} = 0.037$ (significant at the 1 percent level), confirming a robust and persistent role of geographic exposure in shaping economic mobility. To assess whether this effect varies across populations, I examine differences by cohort, move direction, gender, and parental background.

Birth cohorts. Estimates are remarkably consistent across the three cohorts: 0.045(1969–1975), 0.032 (1976–1982), and 0.036 (1983–1989). Although each estimate is highly significant, pairwise differences are not statistically distinguishable. The results therefore suggest a broadly stable municipality–outcome relationship across cohorts. While the point estimates are slightly larger for the oldest cohort, these differences are imprecisely estimated and do not provide evidence of meaningful cohort heterogeneity.

Move direction. Here, a sharper pattern emerges. Moves to lower-opportunity areas are associated with substantially larger effects (0.057) compared to moves to higher-opportunity areas (0.033). Both estimates are precise, and their difference is marginally significant at the 10 percent level. This asymmetry suggests that downward moves impose stronger and more immediate penalties than the gains from upward moves, though the modest significance level calls for cautious interpretation.

Gender. When measured in terms of adjusted household income, exposure effects are nearly identical for women (0.035) and men (0.037). Restricting outcomes to individual earnings, however, reveals a somewhat larger estimate for men (0.046 vs. 0.036). Yet, the difference is not statistically significant, implying that gender-based disparities in effects remain limited.

Parental income rank. By contrast, heterogeneity is most striking among the parental-income gradient. Children from the bottom quintile experience the strongest municipal exposure effects (0.055), which decline monotonically through the middle quintiles and become negligible at the top (0.012, insignificant). This pattern underscores that municipal environments matter most for children from disadvantaged families, reinforcing that place-based resources and constraints disproportionately shape the opportunities of those growing up with fewer economic advantages.

Overall, the subgroup analyses indicate that municipal exposure effects are stable across cohorts and sex but display two asymmetries: negative shocks from moving to lower-opportunity areas seem more consequential than gains from upward moves, and children from low-income families are disproportionately affected.

Table 6. Parametric Estimates of the Municipal Exposure Effect, $\hat{\gamma}$, across Subgroups

	$\hat{\gamma}$	Std. error	Observations
Model 2	0.037***	(0.004)	176,260
Cohorts			
1969–1975	0.045***	(0.010)	53,234
1976–1982	0.032***	(0.006)	62,274
1983–1989	0.036***	(0.006)	60,752
Direction of move			
To lower-opportunity areas	0.057***	(0.010)	77,118
To higher-opportunity areas	0.033***	(0.008)	99,142
Sex			
Women	0.035***	(0.005)	88,557
Men	0.037***	(0.006)	87,703
Sex (individual income)			
Women	0.036***	(0.006)	88,557
Men	0.046***	(0.007)	87,703
Parental income percentile			
0–20	0.055***	(0.008)	33,791
20–40	0.040***	(0.010)	30,437
40–60	0.035***	(0.011)	31,149
60–80	0.031***	(0.011)	35,714
80–100	0.012	(0.008)	45,169

Note: Standard errors in parentheses. Significance: $* \, p < 0.10, ** \, p < 0.05, *** \, p < 0.01$. The table reports linear municipal exposure effects, $\hat{\gamma}$, on income estimated separately by subgroup. All models include baseline controls.

5. Discussion and Concluding Remarks

This paper provides quasi-experimental estimates of childhood municipal exposure effects on intergenerational mobility in Denmark. By adapting the mover design from Chetty and Hendren (2018a) to comprehensive Danish administrative data, the analysis isolates the causal effect of an additional year of childhood exposure to a given municipality, conditional on parental income rank. The results show that where a child grows up in Denmark has a statistically and economically meaningful impact on adult income, educational attainment, and hours worked. Under a parametric linear exposure model, each additional year spent in a municipality where permanent residents' children rank one percentile higher raises a mover's own adult income rank by approximately 0.031-0.037 percentile points. This estimate is nearly identical to the annual convergence rate of about four percent of the cross-area gap reported by Chetty and Hendren (2018a) for the United States, and well within the range of 0.01–0.04 documented by Deutscher (2020) for Australia, depending on age at move. The similarity of these estimates across such different welfare regimes suggests that causal place-based effects are not a peculiarity of high-inequality settings, but a more general feature of childhood environments in modern economies. Importantly, the estimated exposure effects remain large and statistically significant in robustness tests, most notably with sibling fixed effects, which absorb all time-invariant family characteristics. This strengthens the causal interpretation of the results. In cumulative terms, a child who moves at birth realizes roughly two-thirds of the origin–destination gap in intergenerational mobility by adulthood, implying that a substantial share (most) of geographic inequality in opportunity reflects genuinely causal differences in childhood environments rather than selective sorting alone.

This finding contrasts with the conclusion in Cholli et al. (2024) that most cross-sectional variation in neighborhood-level mobility can be attributed to selective sorting. However, the two approaches address related but distinct estimands, which helps explain the differences in emphasis across the two studies. While Cholli et al. (2024) decompose observed level differences in mobility across neighborhoods at a point in time, the present study identifies marginal causal exposure effects using movers and then aggregates these effects over the full childhood period. In this sense, my results suggest that although sorting plays a central role in explaining cross-sectional mobility differences, sustained exposure to place exerts economically meaningful causal effects that are large enough to account for much of the geographic inequality in opportunity observed in Denmark.

A second key finding concerns heterogeneity. The subgroup analyses point to two noteworthy asymmetries that are in line with patterns documented in international literature. First, consistent with theories of cumulative disadvantage and differential susceptibility, municipal exposure effects are largest for children from low-income families and decline monotonically across the parental income distribution. For children in the bottom quintile, the estimated annual effect is roughly 0.055, compared to a small and statistically insignificant effect for children from the top quintile. As discussed in Section 1.1.1, this asymmetry is consistent with findings in prior research (e.g. Jencks et al., 1990; Brandén et al., 2023) and supports the notion that when family resources are limited, children rely more heavily on external environments. Second, moves to lower-opportunity areas appear to be associated with larger effects than the corresponding gains from moves to higher-opportunity areas, although these differences are estimated with limited precision. As discussed in the theory section, the literature offers two main interpretations of such asymmetries. One explanation emphasizes that exposure to disadvantaged contexts is particularly consequential for children from low-SES families, as unfavorable family circumstances and local conditions may interact and compound over time (e.g. Brandén et al., 2023; Wodtke et al., 2016; Ludwig et al., 2013). An alternative view stresses that children from more advantaged backgrounds may experience relatively larger losses when growing up in deprived settings, given their greater potential to benefit from favorable environments (Brandén et al., 2023; Levy, 2019). My results lend more support to the first channel: municipal exposure effects are concentrated among children from low-income families, while outcomes for children of high-income parents are only weakly responsive to place. This pattern suggests that disadvantaged children are particularly sensitive to local conditions, whereas the second mechanism — disproportionate losses among high-SES children — appears less consistent with the evidence.

A particularly important contribution of this paper is the demonstration that municipal exposure effects in Denmark extend beyond earnings to educational attainment and hours worked. The estimated effect on the probability of obtaining a competency-qualifying education is large and robust, and comparable in magnitude both to the estimated income effects and to the educational convergence rates documented by Laliberté (2021). This result points to education as a plausible mediating channel through which place affects long-run economic mobility. Differences in local school quality, peer composition, and access to educational resources are likely to shape human capital accumulation early in life, with consequences for labor-market attachment and earnings capacity. This interpretation is consistent with evidence from Billings et al. (2024), who document sizable neighborhood effects on educational achievement in a Danish social-housing experiment, and with the broader literature emphasizing the centrality of schooling environments in explaining long term outcomes (Jargowsky and El Komi, 2009; Brandén et al., 2023). While the present design does not separately identify the contribution of specific mechanisms – such as institutions, peers, or social networks – the strong educational effects suggest that educational environments play a central role in translating municipal differences into long-run economic inequality.

The findings have several implications for policy. First, the results indicate that local conditions during childhood play a meaningful causal role for intergenerational mobility even in a comparatively egalitarian welfare state. This suggests that national redistribution and universal welfare policies, while important, do not fully offset the influence of place. Municipalities therefore constitute a relevant and potentially powerful margin for intervention. Second, the strong heterogeneity by parental income implies that place-based policies are likely to matter most for children from disadvantaged families. Efforts that improve the quality of local schools, youth services, and early childhood institutions in low-opportunity municipalities may generate disproportionately large gains for these children. Conversely, the results caution that allowing disadvantaged areas to lag behind can have long-lasting consequences, as negative local conditions appear particularly harmful for those with the fewest family resources. Third, the sizable effects on educational attainment point to education as a central policy lever. Interventions that strengthen school quality, reduce segregation across schools, and support transitions into competency-qualifying education maybe effective channels for mitigating geographic inequality in opportunity. While the present study cannot isolate specific mechanisms, the alignment between income and education effects suggests that policies targeting human capital formation are likely to yield long-run returns.

Finally, several directions for future research follow from these results. While municipalities provide a natural and policy-relevant unit of analysis, examining childhood exposure at more disaggregated spatial levels – such as neighborhoods or school catchment areas – could help identify where place-based effects are most pronounced. In addition, further work could explore the mechanisms underlying these effects, for instance by linking childhood environments to variation in schools, peer composition, or other local institutions.

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Appendix

Supplementary Materials for Section 2 – Data and Sample

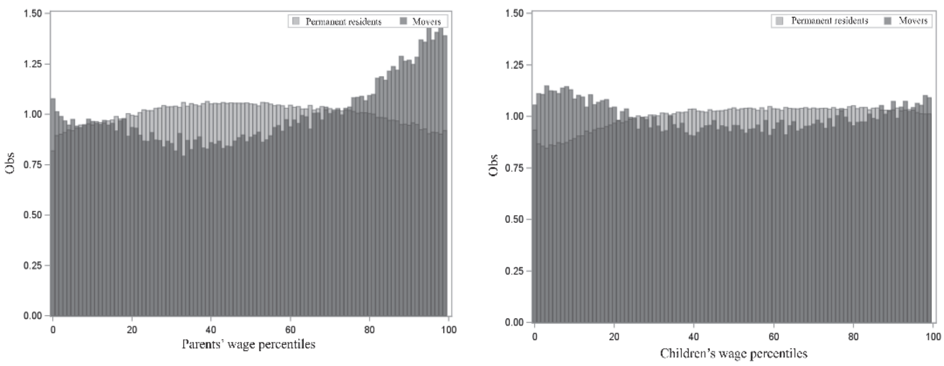
Table A1 reports the distribution of countries of origin, and Figure A1a shows income rank distributions for children and parents among movers and permanent residents. Table A2 outlines the seven education categories, with Figures A2a–A2c displaying their distributions.

Table A1. Origin Distribution for Permanent Residents, Movers, and the Full Sample

Origin	Permanent residents	Movers	Full sample
Denmark	825,975	173,843	1,112,042
Western countries	1,949	597	2,924
Non-Western	14,433	3,616	19,668
Total	**842,357**	**178,056**	**1,134,634**

Note: The table shows country-of-origin distributions for children in the full sample and for permnent residents and movers separately. Origin follows Statistics Denmark's classification-based on IE_TYPE and OPR_LAND. For immigrants, origin is determined by country of birth (or citizenship if unknown); for descendants, by the mother's origin (or the father's if the mother's is unknown). Children are categorized as (1) Danish origin, (2) other Western, and (3) non-Western. Permanent residents had an unchanged municipality of residence throughout childhood; movers made exactly one inter-municipal move at ages 0–17.

Figure A1. Comparison of Income Ranks for Permanent Residents and Movers

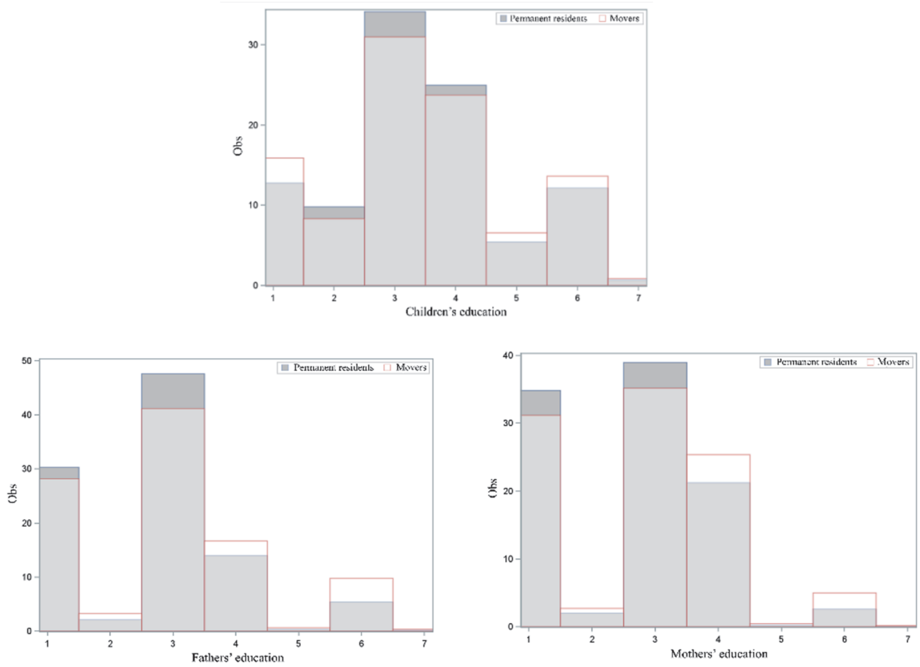


Note: The left panel shows parental income ranks; the right panel shows children's income ranks, for movers and permanent residents. Ranks are percentile positions within birth cohorts, measured using adjusted household income.

Table A2. Classification of Education Levels Used in the Analysis

Code	Description
1	Preschool/primary school/unknown
2	Upper-secondary education
3	Vocational education and training
4	Short/medium-cycle higher education
5	Bachelor’s degree
6	Long-cycle higher education (Master’s)
7	PhD or equivalent

Figure A2. Highest Completed Education: Distributions for Children, Fathers, and Mothers



Note: Table A2 defines the seven-category classification of highest completed education (HFAUDD). Education is measured as the highest level attained by 2023, from primary school (1) to PhD (7). Figures A2a–A2c plot the distributions for children, fathers, and mothers, respectively, split by movers and permanent residents.

Supplementary Materials for Section 3 – Empirical Framework

Figures A3-A5 plot cohort–percentile averages of children's income rank against parents' ranks for permanent residents: overall (Figure A3), Copenhagen (Figure A4), and Aarhus (Figure A5). A map of mean parental income ranks across Danish municipalities appears in Figure A6.

Figure A3. Mean Child vs. Parent Income Rank, Cohorts 1969–1989 (Permanent Residents)

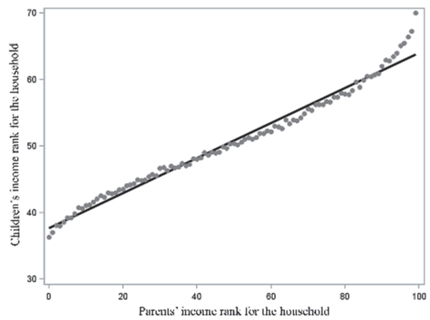


Figure A4. Mean Child vs. Parent Income Rank (Copenhagen)

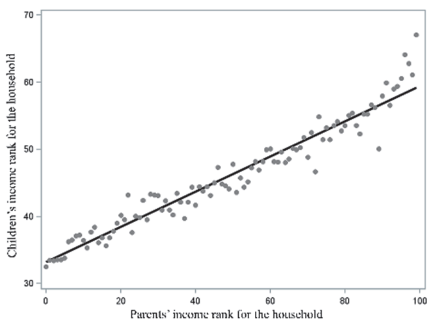
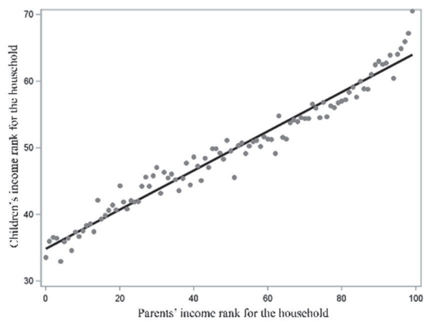


Figure A5. Mean Child vs. Parent



Note: Each panel plots cohort–percentile averages of children's income rank against parents' income rank for the 1969-1989 birth cohorts. Child rank y_i is based on average (adjusted) household income at ages 32-40 and ranked within birth cohort. Parent rank p_i is based on average household income when the father is aged 35-50 and ranked among parents of children in the same cohort.

Figure A6. Mean Parental Income Rank for Permanent Residents, Cohort 1976-1982

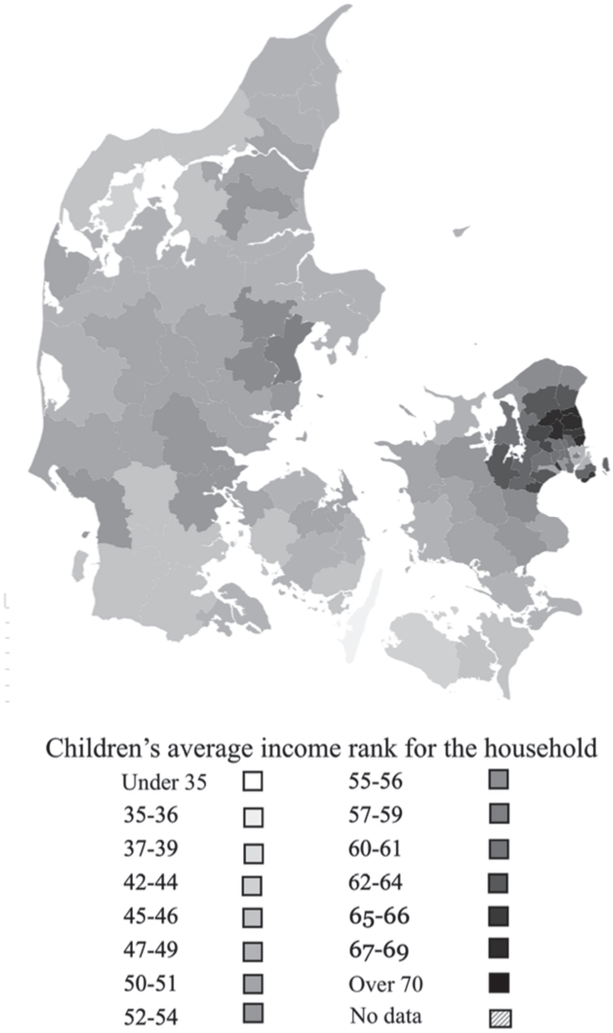


Table A3 reports simple correlations between selected municipal characteristics and the estimates from the mobility regressions in equation (2) for the 1969–1975 cohort cluster. include both the municipality-level rank–rank slope and intercept.

Table A3. Correlations of Municipal Characteristics with Mobility Measures

	Rank–rank slope, $\hat{\psi}_{o,1969-75}$	Intercept, $\hat{\alpha}_{o,1969-75}$
Mean parental rank	0.13	0.37
SD of parental rank	0.19	−0.55
Share of parents in ranks 1–20	−0.07	−0.43
Share of parents in ranks 21–40	−0.14	−0.29
Share of parents in ranks 41–60	−0.18	−0.10
Share of parents in ranks 61–80	−0.05	0.32
Share of parents in ranks 81–100	0.18	0.31
Share ethnic Danish	−0.24	0.38
Share from Western countries	0.18	−0.23
Share from non-Western countries	0.24	−0.39

Note: A higher *rank–rank slope* implies lower mobility; hence a negative correlation indicates higher mobility. For each municipality, we estimate a rank–rank regression among permanent residents in the 1969--1975 cohorts, regressing children's adult income rank (ages 32--40) on parents' income rank. Entries are simple correlations between, respectively, the slope $\hat{\psi}_{o,1969-75}$ intercept $\hat{\alpha}_{o,1969-75}$ and the listed municipal characteristics, all measured on the same population of permanent residents.

Table A4 reports the fitted parameters from equation (1) for every municipality and cohort cluster in the data, along with the municipality's mean parental rank within each cluster.

Table A4. Estimated Parameters and Mean Parental Rank for Three Cohort Clusters

	1969-1975			1976-1982			1983-1989		
	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean
Municipality									
Aabenraa	37.2	0.28	41.5	37.7	0.26	43.4	39.0	0.26	45.3
Aalborg	35.8	0.29	46.9	35.8	0.29	48.9	35.1	0.29	50.0
Aarhus	35.7	0.28	54.0	34.4	0.31	55.2	34.2	0.30	54.8
Albertslund	38.4	0.24	59.4	35.6	0.28	56.2	37.2	0.22	48.6
Allerød	41.2	0.29	69.7	40.9	0.29	71.7	40.3	0.28	71.6
Assens	34.9	0.28	43.7	34.2	0.30	43.4	34.9	0.31	44.0
Ballerup	38.2	0.29	58.1	36.7	0.26	57.4	36.4	0.29	57.6

	1969-1975			1976-1982			1983-1989		
	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean
Municipality									
Billund	41.5	0.21	46.3	43.3	0.21	46.6	42.3	0.22	48.4
Bornholm	35.4	0.23	41.3	37.8	0.24	38.6	38.1	0.24	37.8
Brøndby	36.9	0.27	50.7	32.9	0.30	49.9	33.5	0.31	49.1
Brønderslev	39.4	0.22	42.7	40.1	0.22	44.3	36.5	0.32	44.1
Dragør	41.5	0.21	68.9	44.2	0.18	68.8	33.1	0.35	68.7
Egedal	43.7	0.23	66.7	44.5	0.21	66.4	41.7	0.23	66.9
Esbjerg	38.5	0.24	48.5	37.8	0.27	49.3	38.0	0.27	50.0
Faaborg-Midtfyn	36.5	0.25	43.6	39.6	0.22	44.4	38.7	0.25	44.6
Favrskov	40.5	0.23	51.0	44.0	0.17	51.6	43.8	0.19	51.6
Faxe	40.6	0.23	48.8	37.0	0.28	49.4	39.3	0.25	50.2
Fredensborg	40.5	0.26	65.0	36.6	0.31	62.2	37.2	0.29	60.4
Fredericia	37.7	0.27	49.7	35.4	0.30	50.2	36.3	0.25	51.1
Frederiksberg	34.4	0.26	56.5	32.9	0.33	55.7	32.5	0.32	57.9
Frederikshavn	37.3	0.22	44.3	38.8	0.24	44.7	38.5	0.24	46.2
Frederikssund	42.1	0.22	57.0	43.3	0.21	56.8	38.6	0.26	54.7
Furesø	37.6	0.31	72.8	30.5	0.38	71.6	40.3	0.24	66.9
Gentofte	39.8	0.28	75.2	40.7	0.29	77.6	35.4	0.34	78.5
Gladsaxe	42.7	0.19	57.1	37.1	0.26	59.3	34.8	0.30	60.7
Glostrup	39.6	0.28	57.9	38.6	0.27	58.9	37.1	0.30	59.1
Greve	41.7	0.25	64.3	40.7	0.26	64.0	38.2	0.27	63.6
Gribskov	40.4	0.22	54.8	36.1	0.29	53.8	39.5	0.22	52.7
Guldborgsund	35.4	0.26	44.2	37.3	0.28	43.6	36.5	0.29	42.9
Haderslev	39.0	0.25	42.7	38.2	0.28	43.2	39.5	0.26	44.8
Halsnæs	41.1	0.21	48.9	40.7	0.20	49.4	38.6	0.26	51.3
Hedensted	42.1	0.20	45.0	44.0	0.19	47.1	42.4	0.21	49.5
Helsingør	37.8	0.26	57.2	33.1	0.29	53.6	36.2	0.25	54.1
Herlev	38.2	0.28	58.8	34.6	0.30	58.1	35.5	0.30	57.9
Herning	40.6	0.25	46.6	40.5	0.25	47.1	42.6	0.20	47.9
Hillerød	44.6	0.21	61.8	40.7	0.26	62.3	38.7	0.29	63.4
Hjørring	35.5	0.26	44.5	38.8	0.25	44.8	39.9	0.23	43.8
Holbæk	38.4	0.27	48.3	37.6	0.28	49.6	39.6	0.25	51.0
Holstebro	40.4	0.26	44.4	40.8	0.24	45.9	43.2	0.20	48.0
Horsens	37.3	0.26	47.0	36.5	0.27	48.9	35.7	0.29	48.2
Hvidovre	38.8	0.24	56.3	36.6	0.26	56.4	35.6	0.28	56.1
Høje-Taastrup	36.6	0.31	59.7	32.6	0.35	57.9	36.6	0.27	53.8
Hørsholm	45.3	0.24	77.5	39.9	0.31	75.6	42.1	0.26	76.7
Ikast-Brande	39.8	0.22	45.4	42.6	0.22	46.7	41.5	0.25	45.5
Ishøj	28.5	0.39	54.9	28.6	0.35	48.1	32.9	0.29	41.9

	1969-1975			1976-1982			1983-1989		
	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean
Municipality									
Jammerbugt	37.4	0.24	41.4	38.0	0.28	42.4	39.0	0.23	42.2
Kalundborg	42.1	0.23	47.7	41.2	0.24	48.8	41.6	0.21	48.7
Kerteminde	35.8	0.26	45.3	33.4	0.33	48.2	35.9	0.22	47.4
Kolding	38.4	0.24	47.8	36.3	0.29	49.1	37.3	0.27	51.1
København	33.5	0.25	46.3	31.8	0.29	45.2	34.2	0.25	45.9
Køge	40.8	0.23	54.7	36.2	0.29	53.6	37.7	0.26	54.8
Langeland	34.0	0.24	37.8	35.1	0.26	35.1	41.4	0.15	34.4
Lejre	44.9	0.19	59.2	48.1	0.16	61.1	41.8	0.21	61.6
Lemvig	40.7	0.21	48.1	41.9	0.22	47.8	44.1	0.19	47.8
Lolland	34.6	0.28	41.0	34.2	0.33	40.4	36.9	0.28	38.2
Lyngby-Taarbæk	40.5	0.25	66.3	38.4	0.30	68.1	38.2	0.30	71.9
Mariagerfjord	39.3	0.22	43.1	39.1	0.26	44.7	41.1	0.21	46.0
Middelfart	36.4	0.25	48.4	40.9	0.19	48.0	39.6	0.23	47.6
Morsø	35.2	0.29	39.0	38.6	0.23	39.3	40.9	0.20	40.6
Norddjurs	37.7	0.25	43.3	38.6	0.24	45.9	39.9	0.20	45.1
Nordfyns	38.0	0.20	43.3	37.0	0.26	44.6	39.6	0.22	44.6
Nyborg	38.7	0.19	45.9	35.2	0.26	44.7	35.4	0.27	44.1
Næstved	37.5	0.28	47.4	38.8	0.26	48.5	37.9	0.27	49.9
Odder	38.6	0.25	50.6	39.5	0.23	49.1	39.7	0.21	50.7
Odense	32.0	0.32	47.7	33.3	0.29	47.8	33.3	0.30	49.4
Odsherred	36.2	0.27	44.4	38.8	0.25	44.5	39.5	0.23	43.3
Randers	36.1	0.27	43.5	34.6	0.31	45.3	35.3	0.28	45.5
Rebild	38.6	0.23	45.1	44.1	0.19	49.1	43.2	0.20	48.3
Ringkøbing-Skjern	43.3	0.19	45.0	43.9	0.20	45.7	44.8	0.21	47.2
Ringsted	39.6	0.27	47.9	37.8	0.28	50.7	41.0	0.20	50.4
Roskilde	40.7	0.26	59.9	39.9	0.25	60.8	36.8	0.27	60.7
Rudersdal	40.7	0.30	75.0	39.7	0.31	74.5	36.5	0.32	75.1
Rødovre	37.9	0.25	56.5	37.4	0.24	56.5	36.7	0.26	55.8
Silkeborg	38.1	0.25	46.8	39.2	0.23	47.1	39.4	0.24	49.0
Skanderborg	40.4	0.22	51.8	41.1	0.20	53.4	39.7	0.23	53.2
Skive	37.9	0.27	42.9	40.5	0.22	44.7	41.7	0.20	45.4
Slagelse	37.2	0.25	45.6	36.8	0.27	46.0	37.5	0.27	46.2
Solrød	44.9	0.20	64.8	40.7	0.23	64.5	41.2	0.22	65.2
Sorø	40.9	0.23	46.2	39.1	0.27	47.3	42.0	0.20	49.1
Stevns	40.2	0.25	50.2	42.6	0.19	48.9	38.0	0.29	49.1
Struer	40.6	0.23	43.7	39.2	0.28	45.3	42.3	0.22	46.9
Svendborg	36.2	0.25	42.8	37.3	0.24	43.2	36.0	0.27	44.6
Syddjurs	39.5	0.20	44.5	39.5	0.20	45.6	40.4	0.19	46.2

	1969-1975			1976-1982			1983-1989		
	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean	$\hat{\alpha}$	$\hat{\psi}$	Mean
Municipality									
Sønderborg	36.3	0.28	43.5	34.6	0.29	45.8	36.0	0.28	45.6
Thisted	37.4	0.22	41.7	40.4	0.22	41.9	42.3	0.19	43.3
Tårnby	41.2	0.19	58.1	37.1	0.25	59.9	38.9	0.25	61.4
Tønder	37.7	0.24	42.4	38.7	0.25	43.0	42.0	0.20	41.6
Vallensbæk	45.6	0.20	69.3	31.5	0.37	68.1	48.0	0.16	71.4
Varde	43.7	0.16	44.7	45.5	0.16	46.4	43.6	0.22	47.3
Vejen	41.6	0.19	42.8	40.4	0.22	44.0	43.6	0.18	45.1
Vejle	39.7	0.25	49.0	39.0	0.26	49.6	41.2	0.23	50.3
Vesthimmerlands	38.7	0.23	41.3	40.7	0.21	41.8	40.0	0.22	42.9
Viborg	40.0	0.22	44.1	40.7	0.23	45.2	40.7	0.22	46.6
Vordingborg	36.4	0.25	44.7	37.2	0.26	44.2	39.8	0.24	42.7
min	28.5	0.16	37.8	28.6	0.16	35.1	32.5	0.15	34.4
max	45.6	0.39	77.5	48.1	0.38	77.6	48.0	0.35	78.5
mean	38.9	0.25	50.9	38.3	0.26	51.2	38.8	0.25	51.4

Note: The table reports estimates from equation (1) for each municipality and three birth cohort clusters: 1969--1975, 1976--1982, and 1983--1989. For each municipality and cluster, we show the intercept $\hat{\alpha}$, the rank–rank slope $\hat{\psi}$, and the mean parental rank among permanent residents. Higher slopes indicate lower intergenerational mobility, whereas lower slopes indicate greater mobility.

Supplementary Materials for Section 4 – Results

Table A5 reports parametric estimates, $\hat{b}$. Table A6 presents lineary estimates using (i) unadjusted household income and (ii) individual income. Table A7 shows the lineary estimates sensitivity to the number of years with observed parental income. Table A8 and Table A9 provide additional insight into the composition of the moving sample used in the analysis.

Table A5. Baseline Parametric Estimates of Municipal Effects

	Model 1		Model 1b		Model 1c	
	Coef.	Std. err.	Coef.	Std. err.	Coef.	Std. err.
$\hat{b}_1$	1.12^{***}	(0.14)	0.76^{***}	(0.07)	1.05^{***}	(0.14)
$\hat{b}_2$	1.13^{***}	(0.14)	0.77^{***}	(0.08)	1.08^{***}	(0.14)
$\hat{b}_3$	0.84^{***}	(0.14)	0.49^{***}	(0.08)	0.79^{***}	(0.14)
$\hat{b}_4$	1.11^{***}	(0.14)	0.77^{***}	(0.08)	1.08^{***}	(0.14)
$\hat{b}_5$	1.04^{***}	(0.14)	0.72^{***}	(0.08)	1.01^{***}	(0.14)
$\hat{b}_6$	0.94^{***}	(0.14)	0.63^{***}	(0.08)	0.89^{***}	(0.14)
$\hat{b}_7$	0.71^{***}	(0.14)	0.43^{***}	(0.08)	0.69^{***}	(0.14)
$\hat{b}_8$	0.89^{***}	(0.14)	0.62^{***}	(0.09)	0.87^{***}	(0.14)
$\hat{b}_9$	0.85^{***}	(0.14)	0.60^{***}	(0.09)	0.83^{***}	(0.14)
$\hat{b}_{10}$	0.91^{***}	(0.14)	0.66^{***}	(0.09)	0.88^{***}	(0.14)
$\hat{b}_{11}$	0.82^{***}	(0.13)	0.60^{***}	(0.09)	0.82^{***}	(0.13)
$\hat{b}_{12}$	0.81^{***}	(0.14)	0.58^{***}	(0.09)	0.80^{***}	(0.13)
$\hat{b}_{13}$	0.87^{***}	(0.14)	0.64^{***}	(0.09)	0.85^{***}	(0.13)
$\hat{b}_{14}$	0.56^{***}	(0.14)	0.33^{***}	(0.10)	0.57^{***}	(0.14)
$\hat{b}_{15}$	0.81^{***}	(0.14)	0.57^{***}	(0.10)	0.84^{***}	(0.14)
$\hat{b}_{16}$	0.62^{***}	(0.13)	0.38^{***}	(0.09)	0.64^{***}	(0.13)
$\hat{b}_{17}$	0.44^{***}	(0.12)	0.20^{**}	(0.06)	0.47^{***}	(0.12)
Cohort interaction controls	yes		no		yes	
Controls for parental education & child sex	no		no		yes	
Adjusted R^2	0.084		0.084		0.098	
Observations	176,260		176,260		176,246	

Note: Standard errors in parentheses. Significance: $*\, p < 0.10, **\, p < 0.05, ***\, p < 0.01$. The table reports estimated exposure effects $\hat{b}_a$ the effect of spending one additional year in a municipality with higher expected mobility — by age at move $(a - 1, \ldots, 17)$. Estimates are based on the parametric Model 1 and are identified from variation in age at move among children who move exactly once in childhood. Results are presented for three specifications: The baseline model includes interactions between birth cohort and differences in mobility potential; Specification b omits these inte actions; Specification c additionally controls for child sex and parents' highest completed education. All models include fixed effects for origin and destination municipality, birth year, and parental income rank.

Table A6. Estimates of the Linear Municipal Effect by Income Definition

	Model 2	(i) Unadjusted household income	(ii) Individual income
(1) Income rank, $\hat{\gamma}$	0.037***	0.045***	0.037***
	(0.004)	(0.004)	(0.004)
Observations	176,260	176,260	176,260
All baseline controls	yes	yes	yes

Note: Standard errors in parentheses. Significance: $* p < 0.10, ** p < 0.05, *** p < 0.01$. Outcome in column (i) is *unadjusted household income* (individual income plus any partner income in years with cohabtation with another adult). Outcome in column (ii) is *individual income* in adulthood (excluding any partner income).

Table A7. Definition Estimates of the Linear Municipal Effect by Number of Years with Parental Income

	Model 2	Over 8 years	Over 10 years	Over 12 years	Over 15 years
(1) Income rank, $\hat{\gamma}$	0.037***	0.037***	0.037***	0.038***	0.038***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	176,260	169,107	165,574	158,084	140,656
All baseline controls	yes	yes	yes	yes	yes

Note: Standard errors in parentheses. Significance: $* p < 0.10, ** p < 0.05, *** p < 0.01$. The table reports parametric estimates of the linear municipal effect, $\hat{\gamma}$, by minimum number of years with observed parental income (father observed at ages 35–50).

Table A8. Age at Move for Children in the Moving Sample

Child's age at move	Observations	Percent	Cumulative percent
1	14,060	7.90	7.90
2	12,710	7.14	15.03
3	11,728	6.59	21.62
4	10,518	5.91	27.53
5	10,780	6.05	33.58
6	10,877	6.11	39.69
7	9,732	5.47	45.16
8	9,176	5.15	50.31
9	9,237	5.19	55.50
10	9,249	5.19	60.69
11	8,996	5.05	65.75
12	8,377	4.70	70.45
13	7,880	4.43	74.88
14	7,351	4.13	79.00
15	7,351	4.13	83.13
16	9,446	5.31	88.44
17	20,588	11.56	100.00
Total	**178,056**	**100.00**	

Note: The table shows the age distribution at the time of moving among the 178,056 children in the moving sample. Exactly one move per individual is included, defined as the first year in which the child changes municipality of residence between ages 0 and 17. The »Percent« column reports the share of moves at each age; the »Cumulative percent« column reports the cumulative share up to and including that age.

Table A9. Distribution of Observations by Birth Cohort in the Moving Sample

Birth cohort	Observations	Percent	Cumulative percent
1969	5,894	3.31	3.31
1970	6,696	3.76	7.07
1971	7,362	4.13	11.21
1972	7,961	4.47	15.68
1973	8,065	4.53	20.21
1974	8,618	4.84	25.05
1975	9,273	5.21	30.25
1976	8,676	4.87	35.13
1977	9,004	5.06	40.18
1978	9,407	5.28	45.47
1979	9,581	5.38	50.85
1980	9,065	5.09	55.94
1981	8,500	4.77	60.71
1982	8,656	4.86	65.57
1983	8,303	4.66	70.24
1984	8,133	4.57	74.80
1985	8,272	4.65	79.45
1986	8,506	4.78	84.23
1987	8,836	4.96	89.19
1988	9,211	5.17	94.36
1989	10,037	5.64	100.00
Total	**178,056**	**100.00**	

Note: The table shows the distribution of the 178,056 children in the moving sample across birth cohorts 1969–1989. The moving sample consists of children who made one and only one municipality move between ages 0 and 17 and otherwise satisfy the inclusion criteria. The distribution is relatively even across cohorts, with a mild increase for the younger cohorts, reflecting better coverage of early childhood moves for these cohorts. The cumulative percent column reports the accumulated share up to and including each cohort.