Sources of Regional Variation in Intergenerational Mobility: Evidence from the Netherlands

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August 25, 2024

Abstract

In this paper, I investigate the impact of regions where people grow up on the transmission of socioeconomic status from parents to children, using rich Dutch administrative data. I disentangle place effects from other confounding factors by exploiting variation across children's ages at the time their parents move across regions (Chetty and Hendren, 2018a). I document a place effect for educational attainment at the time that children choose a high school track (i.e., age 14): every additional year spent in a place with a one percentage point higher probability of enrollment into a high secondary education track, increases children's own probability of following such a track by 5 percentage points. I identify selective location choices of parents that depend on their children's ages at the time of move in terms of children's high school tracks observed before moving. After controlling for such age-dependent migration and family fixed effects, I document no place effect for outcomes measured between age 24 and 28.

Keywords: movers-exposure design, higher education, place effects, early tracking

JEL Codes: J62, R12, I24, R23

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†I make use of microdata from *Statistics Netherlands*. I thank Bertrand Melenberg, Marike Knoef and Anja De Waegenaere for their useful comments and support throughout this project. Moreover, I thank Bettina Siflinger for her very valuable advice, support, and time. Furthermore, I am thankful to Jeffrey Campbell and Kristin Butcher for their invaluable advice and guidance during my research visit to the Federal Reserve Bank in Chicago and the University of Notre Dame (funded by Tilburg University). I thank Alexandros Theloudis, Jaap Abbring, Jochem de Bresser, Bart Bronnenberg, Bhashkar Mazumder, Kasey Buckles, Jeffrey Denning, Derek Neal, Mary Kate Batistisch, Daniel Hungerman, and Albert Saiz for very helpful comments.

1 Introduction

The transmission of socioeconomic status across generations − formally called *intergenerational mobility* − is a perennial topic within economics. In order to better understand the state and development of societal inequalities, recent literature has started to consider the mechanisms through which disparities in intergenerational mobility emerge (Black and Devereux, 2011). Previous research highlights that the place where a child grows up affects the intergenerational transmission of income and educational attainment (Chetty and Hendren, 2018a; Alesina et al., 2021; Deutscher, 2020). For instance, regions with better school quality tend to produce better socioeconomic outcomes, for a given level of parental endowment (Chetty and Hendren, 2018b). Equal access to high-level education across regions is therefore considered to be one of the key paths to upward socioeconomic mobility (Bailey and Dynarski, 2011; Dynarski, 2003).

In this paper, I investigate the causal impact of childhood location on the intergenerational transmission of income and education at multiple stages in life, using high-quality Dutch administrative data. The Netherlands employs a national education system that tries to mitigate regional disparities in mandatory school attendance, curriculum standards, and school financing (Snyder and Dillow, 2013; Owings et al., 2015). Nevertheless, empirical research shows that children's expected earnings and educational attainment, conditional upon parental endowment, vary across regions in the Netherlands (Atav et al., 2023). These geographical differences can be explained in two ways: place effects and sorting (Abowd et al., 1999). Place effects measure the extent to which a neighborhood affects children's expected outcomes, such as the quality of schooling and peer effects. This implies that randomly moving children to a different neighborhood will affect their future economic outcomes (Durlauf, 1993; Benabou, 1996). Sorting refers to the possibility that this geographic variation is due to family types living in each region, such as differences in skills, demographics or wealth (Behrens et al., 2014; Diamond, 2016). The main econometric challenge in this paper is to consistently estimate causal effects of place in isolation from a (potential) sorting bias.

I employ a quasi-experimental framework proposed by Chetty and Hendren (2018a) to assess the importance of place of residence during childhood on intergenerational income and education mobility. In particular, I exploit variation across the ages of move of children whose parents migrated to a region with higher rates of upward mobility − as measured by the outcomes of the children already living there − during their childhood. Identification of place effects rests on the key assumption that sorting does not vary with the child's age at move. This framework allows for families with better unobservables to sort themselves into regions with higher upward mobility. However, the identifying assumption can be violated if the extent to which this selection takes place depends on the child's age at move. Since I observe children's education-related decisions and outcomes during childhood, I am able to test and account for non-constant selection effects around targeted ages.[1] To the best of my knowledge, this approach has not been feasible to date, since data on children's earnings and educational at-

[1]Given the Dutch institutional setting regarding education and data availability, I consider three ages for which I measure children's outcomes: 14, 19, and 28. In particular, I measure children's track choices at age 14, next whether they got a degree they need to access higher education at age 19, and finally whether individuals have obtained a bachelor degree at age 28.

tainment are only available after secondary education has been finished, but not at earlier ages.[2]

Similar to Chetty et al. (2014), Chetty and Hendren (2018a) and Deutscher (2020), my analysis starts by estimating intergenerational mobility at the regional level, using socioeconomic outcomes of individuals whose families have stayed in the same region during their entire childhood.[3] As a measure of region-specific intergenerational mobility, I document geographic variation in individuals' expected outcomes conditional on their parents' household income rank with respect to the national distribution.[4] These results show different spatial patterns for income and educational attainment depending on age at measurement. Particularly, when I measure an individual's educational attainment observed at age 14, regional differences in intergenerational mobility become larger, compared to an individual's income and education observed from age 24 onwards. In this paper, I focus on explaining whether this regional variation is driven by causal effects of place and/or sorting, and to which extent the effect of place on early schooling decisions translates to individual's socioeconomic outcomes observed after childhood.

This paper documents four main findings. Replicating the design as developed by Chetty and Hendren (2018a), I document a downward sloping age-regional income mobility gradient: children whose families move at younger ages to regions with a higher upward income mobility, have a higher household income at age 28 as compared to children whose parents move at later ages. This finding is consistent with ter Weel et al. (2019) who also use Dutch administrative data on individuals' household income. However, when I exploit variation between siblings (i.e., controlling for all unobservable determinants of an individual's income that are constant within a family), I find that this downwards sloping relationship between age at move and a destination's level of intergenerational mobility disappears. This suggests that families with better (unobserved) parental inputs tend to systematically migrate to regions with higher upward mobility at earlier ages. This is in sharp contrast with the US, where the estimates including family fixed effects are very similar to the baseline estimates (Chetty and Hendren, 2018a).

Second, for children's educational attainment at age 28, I again do not find evidence of place effects. This suggests that all of the regional variation in intergenerational mobility post-childhood can be explained by systematic sorting of families into higher-educated regions. My findings are very different from the US and Australia, where place of residence during childhood explains most of the observed geographical variation in individual's income and educational at-

[2]By definition, intermediate outcomes on income are generally not available, since the majority of children starts to work (full-time) only after they finished education. However, intermediate outcomes in terms of educational attainment are generally available in terms of test scores or, if a country employs a track-based education system, secondary education track choices.

[3]Throughout my main analysis, a region is defined as a COROP region. This is a geographical division that is equivalent to the European NUTS 3 level, and the definition of commuting zones (CZs) in the US.

[4]I use this measure of intergenerational mobility at the regional as this is consistent with the previous literature (Chetty et al., 2014), and because the data on parents' educational attainment is less generally available for this generation.

tainment after childhood (Chetty and Hendren, 2018a; Deutscher, 2020). [5]

Third, the gradient of exposure effects across children's ages at move follows a similar pattern as found for the US in terms of college attendance rates, if I evaluate children's track choices *during* childhood (i.e., at age 14) instead of their (Bachelor) degrees *after* childhood (i.e., at age 28). In particular, I find that the magnitude of yearly place effects converges to zero when I measure children's educational attainment at later ages: from approximately 5pp for age 14, to 1.9pp for age 19, to a null effect for age 28. My results suggest that the place effect for a child's first schooling decision does not translate to a place effect for outcomes after childhood, on average.

Lastly, I show that the average relationship between intergenerational mobility measured after childhood in the destination area and children's track choices made earlier in life varies with the child's age at move. This observation could potentially violate the key identifying assumption underlying the framework by Chetty and Hendren (2018a). After controlling for this age-dependent selection for children who moved after age 14, I still find a flat relationship between age at move and a regions' level of upward mobility. This result provides additional evidence that places only seem to have a causal effect on children's educational attainment during childhood, but not for outcomes observed directly after childhood. One potential explanation for this would be that there is a nationwide educational adaptability, as disadvantageous regions that offer lower opportunities to high-level secondary education tracks are compensated by equalized higher education prospects across the Netherlands.

This paper contributes to three large strands of literature. First, my results complement the classic literature about the role of neighborhoods on the transmission of socioeconomic status from parents to children. Observational studies generally document substantial variation across regions in intergenerational mobility in income (Deutscher and Mazumder, 2019; Sampson et al., 2002; Sharkey and Faber, 2014). Prominent research has shown that this variation in intergenerational income mobility is strongly correlated with geographical variation in college attendance rates (Chetty et al., 2014; Card et al., 2022). Motivated by this observation, both experimental and quasi-experimental studies have shown that the neighborhood where a child grows up has a large causal effect on future outcomes in earnings and education outcomes observed later in life (Chetty and Hendren, 2018a; Deutscher, 2020; Nakamura et al., 2022; Chyn, 2018; Gould et al., 2004), and that one important mechanism explaining this effect of place is quality of schooling in a region (Chetty and Hendren, 2018b; Laliberté, 2021). I estimate place effects on socioeconomic mobility in a country with a tracking-based education system, less inequality, and higher intergenerational mobility at the national level. My results suggest that the place effects as documented for the US by Chetty and Hendren (2018a) and Australia by Deutscher (2020) might be driven by differences in institutions.

Second, I add to the literature using within-country migration to identify place effects, as

[5] In the US, the results by Chetty and Hendren (2018a) suggest that children who relocate to high income mobility areas shortly after birth and reside there for two decades would experience an improvement of approximately 80% of the observed outcomes in the destination location in their own long-term family income, compared to individuals who remain in their original locations.

initiated in the seminal work by Abowd et al. (1999). For example, using movers' variation in outcomes in an event-study design is popular in multiple fields in economics, such as health economics (Finkelstein et al., 2016; Finkelstein et al., 2021; Moura et al., 2019), the formation of preferences (Bronnenberg et al., 2012; Cagé et al., 2022), and labor economics (Bonhomme et al., 2019), comparing outcomes before and after the move took place. However, as recently argued by Chetty and Hendren (2018a), pre-move data is not available when estimating place effects during (or just after) childhood. As a consequence, they developed the so-called *movers-exposure design*, which rests on the key identifying assumption of constant selection across ages of exposure. This quasi-experimental framework has also been used for other outcomes than socioeconomic mobility ever since, such as innovation (Bell et al., 2019), racial earnings (Derenoncourt, 2022), and crime rates (Finlay et al., 2023). In this paper, I show that the relationship between the destination choice in terms of intergenerational mobility, and children's pre-move track choices varies with age. I argue that a causal effect of place is overestimated (in other studies) in case families with better parental inputs and other determinants of children's outcomes tend to systematically migrate into better regions at earlier ages.

Third, my findings contribute to the ongoing discourse in the literature regarding the impact of early tracking on outcomes later in life (Hanushek and Wößmann, 2006; Dustmann, 2004). Using Dutch administrative data, Stans (2022) documents long-lasting consequences of emotional distress due to unexpected grandparental death during a critical educational stage on children's test scores during secondary education, suggesting that first schooling decisions persist to children's educational careers before age 20. My findings for age 19 are similar to her findings, but suggest that these first schooling decisions do not translate to socioeconomic outcomes at age 28. Pekkarinen et al. (2009) show that a Finnish comprehensive school reform, that shifted the two-track system to a comprehensive schooling system, substantially reduced the intergenerational earnings elasticity. Similarly, Meghir and Palme (2005) demonstrate that a similar reform has led to an increase in educational attainment and lifetime income of high ability students in Sweden. In high contrast with these findings for these Scandinavian countries, this paper shows that regions that accommodate higher track opportunities in the Netherlands do not generate higher income or education outcomes after childhood, on average. This finding is in line with studies that document no effect of being enrolled in above-average schools on students' test scores and college quality in the US (Abdulkadiroğlu et al., 2014).

The rest of this paper is organized as follows. Section 2 describes the institutional setting regarding (higher) education in the Netherlands, and discusses the administrative data I use. Section 3 documents regional variation in intergenerational mobility, and postulates the parametric form describing intergenerational income and education mobility. Section 4 explains the movers exposure design that I use to disentangle selection effects from causal effects of place. Section 5 presents the main results, under the identifying assumption of constant selection across ages at move (Chetty and Hendren, 2018a). Section 6 assesses the validity of this key assumption, by performing placebo tests, controlling for pre-move outcomes, and including family fixed effects. Section 7 concludes.

2 Institutional setting and data

The focus of my work is to investigate the role of place during childhood on the intergenerational transmission of income and education, in a country which employs a tracking based education system. In this section, I elaborate on the Dutch educational system, and how it differs from more comprehensive schooling systems like in the US. Furthermore, I describe the administrative data from *Statistics Netherlands*.

2.1 Education in the Netherlands

For all children who are between 5 and 13 years old, it is mandatory to follow primary education in the Netherlands. Thereafter, children are tracked into ability-dependent programs. This is very different from the US, which employs a more comprehensive secondary school system (Bol and Van de Werfhorst, 2013; Hanushek and Wößmann, 2006). Tracking refers to the existence of multiple ability-dependent secondary education programs at more than one age during the educational path. The Dutch education system can be decomposed into three educational tracks.[6] Moreover, almost all schools (in primary, secondary, and tertiary education) are publicly financed, and tuition fees are relatively low as compared to other countries (Snyder and Dillow, 2013; Owings et al., 2015).

Tracking into different secondary education programs happens after age 13, and typically determines a child's opportunities to access different types of tertiary education. Children will be classified into one out of three levels of education after primary school, (i) pre-vocational training (duration of 4 years), (ii) pre-higher education (duration of 5 years), or (iii) pre-academic education (duration of 6 years), based on their teachers' advice and their performance in a national test.[7] The latter two tracks are called high-tracks throughout the rest of the paper, as they are perceived as diplomas that lead to a Bachelor and/or a Master diploma. After secondary education, individuals are again subject to three different types of tertiary education, (i) secondary vocational training (*Dutch:* MBO), (ii) education at a university of applied sciences (*Dutch:* HBO), and (iii) education at a university (*Dutch:* WO). I consider all institutions which are either a university of applied sciences, or a university as higher educational institutions. The accessibility of each type of tertiary education depends on the degree an individual has obtained during secondary education. For example, to have immediate access to higher education, it is necessary to have a high-track degree (or have a degree with a similar or higher academic achievement).[8] Figure A.1 in the Appendix presents a visual overview of the educational system in the Netherlands.

[6] Here, I follow the general definition of educational programs as defined by UNESCO (2006).

[7] This national test at the end of primary school consists of questions on language skills, reading comprehension, and math, and is known as the "CITO-test". In some cases, an additional entry exam is asked for, depending on a child's performance on this test.

[8] There exist some exemptions to these rules. However, the fastest path to a degree obtained at a higher educational institution is via pre-higher education (HAVO) or pre-academic education (VWO). It is also possible to access higher education via secondary vocational training in some cases. For example, for some programs a degree obtained from such vocational training programs is sufficient to start a study at a university of applied sciences.

2.2 Variable definitions

Using a unique identification number, I link parents' characteristics to their children's (long-run) outcomes. In particular, I link parents' income level, and their location when their children are young to children's outcomes on education and income observed later in life. All monetary values are measured in 2015 euros, adjusted for inflation using the consumer price index (CPI).[9]

Parents' Income. Following the existing literature, my main measure of parents' income is equal to the average gross household income of both parents between 2003 and 2007 (Solon, 1999; Lee and Solon, 2009). Parents' gross household income for a given year is equal to the average of their mother's household income and their father's household income.[10] A household's gross income consists of primary income received from employment, including unemployment benefits, and other social security benefits (i.e., pension benefits, disability insurance benefits, unemployment insurance benefits), but also includes rent subsidies and child-related benefits. An individual's household income is equal to the joint income of their own income and their partner's personal income, where a partner is the person he/she is (i) married, and/or (ii) has a registered partnership, and/or (iii) lives together with.[11] I use data on parents' income for the earliest years possible, to obtain a proxy for socioeconomic circumstances for children during their childhood. Since I only observe income from the year 2003 onwards, and the children in my sample are born before 1990, this means that the earliest child's age for whom I can match their parents' household income is 13 years (see Figure A.2 in the Appendix). I average income over five consecutive years to obtain a measure for parents' household income that is less influenced by fluctuations in lifetime income, as commonly done in the existing literature (Solon et al., 2000; Chetty et al., 2014). I delete individuals from whom the parents' gross household income is strictly negative, since these families are generally associated to be relatively wealthy, and therefore their family income is considered to be a bad measure for their real socioeconomic status (Deutscher, 2020). In order to best compare my results with the results obtained by Chetty and Hendren (2018a) and Deutscher (2020), I also construct a parent's income measure which is solely based on the marital status of each parent.[12]

Parents' Location. In each year from 1995 until 2018, I observe an identification number which is unique to a particular address of residence. These addresses consist of all available

[9] The CPIs used in this paper can be found here: `https://www.cbs.nl/nl-nl/reeksen/tijd/consumentenprijzen`.

[10] As discussed in Chetty and Hendren (2018a), this implies that I average gross household income from both parents over the years $2003-2007$, and divide by 10 in case both parents are identified. In case only one parent is identified, this number is divided by 5. In case the parents are not married with each other (in a given year), the household income of both the father and mother is unequal by definition. In that case, I track the household income of both parents over time.

[11] I consider individuals to be living together if they are linked to each other in the tax administration, and therefore are (i) married, and/or (ii) have at least one child together and live at the same address, and/or (iii) a couple obtaining healthcare allowance, housing allowance, or a joint provisional assessment for the tax authorities. This information is obtained via backward induction. For example, in case a couple has a child in year $t+3$, but were already living together in year t, they will be classified as cohabiting in year t.

[12] Following Chetty and Hendren (2018a), I consider a parent's household income as the joint income of their own income, together with the income of their spouse. As such, this alternative measure does not account for cohabitation, as opposed to my main measure of parents' income.

addresses in the Geographical Basis Registration, the Municipal Basis Registration, and the Housing Register. I can link this identification number to regions of residence for all parents. I use the COROP region level as the level of location throughout the main analyses in this paper. A COROP region consists of one or more contiguous municipalities.[13] I use the most recent definition of municipalities and COROP regions in the Netherlands, as established in 2021. I base the parents' location on the address of the mother. This implies that in case the mother and the father do not live together the assigned location is based on the mothers' region of residence (Chetty and Hendren, 2018a). In case the mother's residence information is unavailable, I use the father's location information.

Child's Education. I consider three measures of educational attainment. These measures correspond to children's educational attainment during and after childhood. First, I measure whether a child has chosen and followed the pre-higher education and/or pre-academic education track at (or before) age 14.[14] This type of secondary school track is called a high-level track throughout the paper. Second, I consider whether an individual has obtained a degree from this high-level track at age 19 (i.e., finished the high-level track). Lastly, I define a child's long-run outcome on education based on the highest obtained degree at age 28. I consider a degree from a university (of applied sciences) to be a higher educational degree (called a Bachelor degree hereafter).

Child's Income. Similar as for parents, my main measure of a child's income is equal to gross household income ranging from age 24 till 28, which is equal to their own personal gross income together with the personal income of their (cohabiting) partner.[15] In order to test the robustness of my results, I also consider children's personal gross income as a measure for income. Additionally, in order to replicate the results obtained by Chetty and Hendren (2018a) as closely as possible, I also consider children's household income based on their marital status at the point at which income is measured. I also present my main results for personalized household income.[16]

2.3 Sample selection

For my primary sample, I select all children (i) who are born between 1985 and 1990 and that can be linked to their parents, (ii) with valid information on parents' location throughout the entire period 1995 until 2018, (iii) whose parent(s) have available information on their household income between 2003 and 2007, (iv) from whom I can observe the highest obtained education

[13] A COROP region (*Dutch:* Coördinatiecommissie Regionaal Onderzoeksprogramma) is a geographical division done for statistical purposes, and is the equivalent of the European NUTS 3 level, and the definition of commuting zones (CZs) in the United States.

[14] I measure the highest *followed* track at age 14. This means that children who first have chosen a lower track (i.e., pre-vocational training) at age 13, but then switched to the higher track at age 14 are classified as high trackers in my analysis. Furthermore, children who followed a high track at age 13, but then switched to a lower track at age 14, are also classified as high trackers in my analysis.

[15] Again, an individual's partner is defined in a similar way as described above for parents. This means that in case a child is single, household income is equal to personal income. Moreover, in case an individual is living together with other persons who are not their partner (i.e., (one of) their parents), I consider the children's household income to be equal to their personal income at age 28.

[16] That is, I divide the household income by 2 in case an individual is living together with their spouse.

degree at ages 14, 19, and 28, and (v) from whom I can observe their (household) income from age 24 onwards. From all children who were born between 1985 and 1990 in the Netherlands, approximately 82% is still in my primary sample after applying these selection criteria.

I distinguish between two types of children, namely (i) *permanent residents*, who are children whose parents do not move across different regions between 1995 and 2018, and (ii) *one-time movers*, who are children whose parents moved exactly once across different regions during the same period. This implies that I exclude all children from the primary sample whose parents moved more than once to another region between 1995 and 2018, and all children whose parents did not live in the Netherlands for (part of) the period between 1995 and 2018. Table B.1 in the Appendix presents an overview of the number of observations belonging to each category. For all geographies, the majority of children are permanent residents. For example, at the COROP regional level, approximately 88% of the individuals in the primary sample are permanent residents, as opposed to 10% who are one-time movers.

2.4 Descriptive statistics

Table 1 presents the descriptive statistics for my main analysis sample. Panel A reports these statistics for the 876,742 permanent residents in my sample. Panel B considers the children whose families moved exactly once across different COROP regions. There are 101,144 children in this subsample. The mean yearly gross household income for children aged 28 (aged 26) is approximately equal to 58,500 euros (46,500 euros) for permanent residents, compared to approximately 57,500 euros (44,500 euros) for one-time movers, respectively. The average household income for parents is approximately equal to 89,000 euros for permanent residents, compared to approximately 91,000 euros for one-time movers. Furthermore, 43.5% of the permanent residents has a Bachelor degree or higher at age 28, compared to 44.8% for one-time movers. At age 19 (age 14), approximately 34% (35.2%) of the permanent residents has (followed) a high-level degree (track), opposed to 37.1% (37.9%) of the one-time movers.

3 Regional variation in intergenerational mobility

This section presents descriptive statistics on spatial differences in intergenerational mobility in the Netherlands. To do so, I make use of permanent residents only. Section 3.1 defines the relationship I use to estimate intergenerational mobility. I present regional variation in intergenerational mobility, for several outcomes (evaluated at different ages) in Section 3.2.

3.1 Definition of intergenerational mobility

Let $p_i \in [0, 100]$ denote child i's parents' household income rank with respect to the national household income distribution, for all children born in year $s(i)$.[17] Thus, a child with $p_i = 0$ belongs to the country's poorest households, and a child with $p_i = 100$ belongs to the country's richest households of their birth cohort. For every child i born in year $s(i)$, whose parents

[17] That is, a child with $p_i = 75$ has parents who belong to the 75th percentile rank at the national household income distribution in their birth cohort $s(i)$.

Table 1: Descriptive Statistics

	Mean (1)	Std. dev. (2)
Panel A: Permanent residents		
Parents' household income	88,968	50,638
Child's household income at age 28	58,518	44,420
Child's household income at age 26	46,475	45,862
Child's household income at age 24	34,785	30,368
Child's personal income at age 28	34,700	20,108
Child's corrected household income at age 28	31,912	22,145
Having a Bachelor degree at age 28	43.66	49.60
Having a high-level degree at age 19	34.07	47.40
Having followed a high-level track at age 14	35.27	47.78
Num. of obs.	876,742	
Panel B: One-time movers		
Parents' household income	91,077	61,625
Child's household income at age 28	57,427	46,977
Child's household income at age 26	44,689	49,286
Child's household income at age 24	33,253	31,195
Child's personal income at age 28	33,726	29,894
Child's corrected household income at age 28	30,670	29,067
Having a Bachelor degree at age 28	44.82	49.73
Having a high-level degree at age 19	37.13	48.31
Having followed a high-level track at age 14	37.85	48.50
Num. of obs.	101,144	

Notes. This table presents the summary statistics for the two subsamples in our main analysis (at the COROP regional level), for children who are born between 1985 and 1990. I report descriptive statistics for two subsets of the full sample. Panel A shows statistics for the permanent residents (i.e., children whose parents do not move across COROP regions for the period 1995 – 2018). Panel B shows statistics for children whose parents only moved once across distinct COROP regions during the period 1995 – 2018. Parents' percentile ranks on average gross household income are calculated with respect to the national gross household income over the years 2003 – 2007, for parents whose children were born in the same birth year. Children's percentile ranks on the income measures are calculated with respect to the national income distribution for their birth cohort. For more information on the variables presented here, see Section 2.2.

lived in region $c(i)$ during the child's childhood, I consider the following relationship describing intergenerational mobility:

$$y_i = \mu_{c(i)s(i)}\big(p_i\big) + \eta_i \quad \text{with} \quad \mathbb{E}\Big(\eta_i \Big| p_i, c(i), s(i)\Big) = 0 \tag{1}$$

for $i \in \mathcal{P}$, where $\mathcal{P}$ denotes the set of permanent residents in our primary sample. y_i denotes a child's socioeconomic outcome $O \in \{I, E\}$ measured at age A. For income, $y_i \in [0, 100]$, which denotes individual i's (family) income rank with respect to the national (family) income distribution, for all individuals born in the same year $s(i)$. For educational attainment, $y_i \in$

$\{0, 100\}$, where $y_i = 100$ if a child has obtained or followed a specific high-level degree (track) at age A, and $y_i = 0$ otherwise.[18] The unobserved error term η_i is mean-independent of parents' income rank p_i, birth cohort $s(i)$, and region of residence $c(i)$. Given this model specification, the conditional mean function (CMF hereafter) is given by:

$$\mu_{cs}(p) = \mathbb{E}\Big(y_i \Big| p_i = p, c(i) = c, s(i) = s\Big) \tag{2}$$

for $i \in \mathcal{P}$. The next section motivates the choice of the functional form of $\mu_{cs}(\cdot)$ for every socioeconomic outcome and age at measurement combination.

3.2 Descriptives on intergenerational mobility

Figure 1 plots children's mean outcomes at several ages within ordered bins of the parent income distribution for two selected COROP regions: Groot-Amsterdam (an urban region) and Flevoland (a rural region). To capture the uncertainty in estimating this conditional mean function, we also present the 95% confidence band (shaded area), together with the best-fit line using the underlying microdata for each region using an OLS regression (solid line).

The figures show an increasing relationship in both children's outcomes with respect to their parents' income rank. At first glance, both Panel A and Panel B in Figure 1 show differences in mobility across Groot-Amsterdam and Flevoland for outcomes measured after childhood (at age 28). For a fixed level of parental family income, individuals who grew up in Groot-Amsterdam are more likely to have a Bachelor degree at age 28 than individuals who grew up in Flevoland. On the contrary, individuals who grew up in Flevoland have a higher household income rank at age 28, compared to individuals who grew up in Groot-Amsterdam.[19] When I measure children's outcomes at earlier ages (i.e., age 14 and 19), the differences in intergenerational mobility become more apparent: for any fixed parental endowment, individuals who spent their childhood in Groot-Amsterdam are more likely to have (followed) a high-level (track) at age 19 (age 14), as depicted in Panel C (Panel D) in Figure 1, than individuals who grew up in Flevoland.

Because the confidence bands contain the best-fit linear line for most values of parents' household income rank, I conclude that linearity of the functional form in $\mu_{cs}(\cdot)$ cannot be rejected (Cattaneo et al., 2024).[20] Throughout my main analysis, I will assume a linear relationship to model intergenerational mobility at the regional level, thereby following the work by Chetty and Hendren (2018a) and Deutscher (2020), as follows:

$$y_i = \beta_{0,cs} + \beta_{1,cs} p_i + \varepsilon_i \quad \text{with} \quad \mathbb{E}\big(\varepsilon_i | p_i\big) = 0 \tag{3}$$

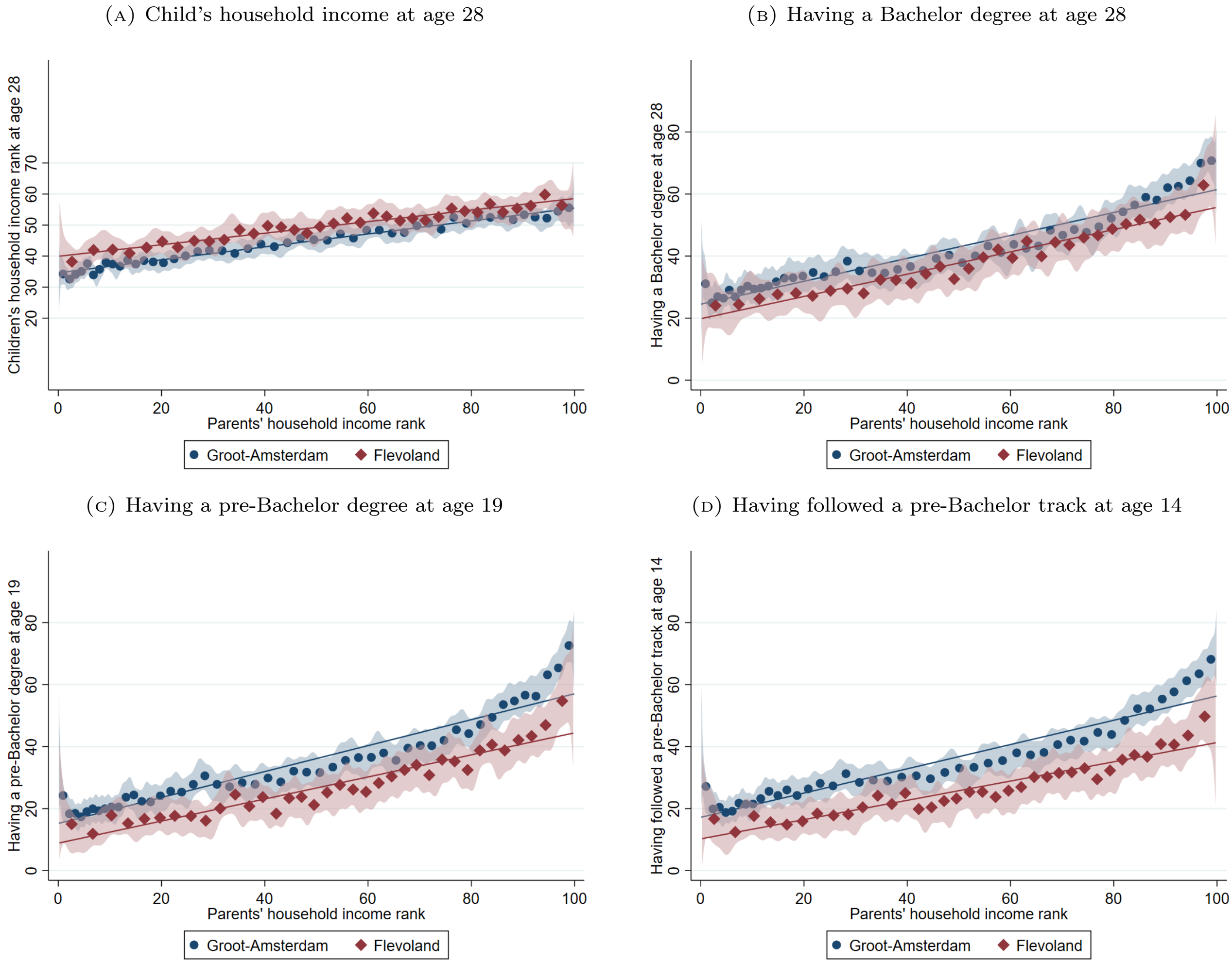
for $i \in \mathcal{P}$. I estimate the expression above using OLS for every birth cohort and region of residence. I use the obtained coefficient estimates $\hat{\beta}_{0,cs}$ and $\hat{\beta}_{1,cs}$ to predict intergenerational

[18] I observe a child's educational attainment E, at three different ages $A \in \{14, 19, 28\}$. For income I, I only observe a child's outcome at later ages $A \in \{24, 26, 28\}$. I denote the binary indicator in terms of $\{0, 100\}$ instead of the usual outcome space {0,1} to enhance the interpretability of my results.

[19] This suggests that an individual's income rank at age 28 might not be a good proxy for an individual's lifetime income rank (Haider and Solon, 2006). I stick to this income measure to compare my results to other studies.

[20] This linear property is robust across regions.

Figure 1: Relationship between parents' outcomes and children's outcomes



Notes. Panel A presents the binned scatter plot of the relationship between parents' household income rank and children's income rank at age 28, for all individuals born in 1985. Panel B, Panel C and Panel D present the binned scatter plot of the relationship between parents' household income rank and whether children have a Bachelor degree at age 28, have a pre-Bachelor at age 19, and have followed a pre-Bachelor track at age 14, respectively. Parent's income is the average household income from 2003 to 2007. Children's income is the average household income measured at age 28. The (blue) dots and (red) diamonds present the average outcomes of all children within each bin, for the COROP regions Groot-Amsterdam and Flevoland, respectively. For both panels, I make use of the binned scatter method and corresponding confidence bands provided by Cattaneo et al. (2024). Following this approach, the number of bins for each COROP region is chosen in an data-driven way. The 95% confidence band is presented for both regions, capturing the uncertainty in estimating the conditional mean functional form. The figures also present the estimated linear line underlying the individual-level data, using OLS. Children's income rank is constructed relative to all other children who were born in the same year. Parent's income rank is constructed relative to other parents of children born in the same year. For more information on the construction of these variables, see Section 2.2.

mobility for a given parent income rank for every region and birth cohort, separately.

Panel A in Figure 2 maps children's household income rank at age 28 at the median of the parental endowment distribution (i.e., $p_i = 50$) for children born in 1985, where darker (black and/or purple) colors represent lower predicted household income ranks, and lighter (yellow and/or orange) colors represent higher predicted ranks. I document significant variation in the predicted income ranks across COROP regions, with northern regions showing lower income ranks than southern regions. A similar map is constructed in Panel B, Panel C, and Panel D in Figure 2 for educational attainment at age 28, 19, and 14, respectively. These figures show different spatial patterns in the predicted values than for income, where darker (blue) areas correspond to regions with lower rates of high educational attainment, and lighter (green and/or yellow) areas correspond to regions with higher rates of high educational attainment, at any age. This descriptive evidence shows that more children have entered a high-level track at age 14 (Panel D in Figure 2), than finished a high-level track at age 19 on average (Panel C in Figure 2). However, Panel D in Figure 2 suggests that some individuals are able to catch up on initial schooling decisions, as the average rate of individuals having obtained a Bachelor degree (or higher) at age 28 is higher than track decisions observed at earlier ages for every region. Similar regional patterns are observed at the 25th and 75th percentile rank of the national parental household income distribution, as presented in Figure A.4 and Figure A.5 in the Appendix, respectively.

Following Chetty et al. (2014) and pooling all regions for children's income rank with $p = 25$ (Panel A in Figure A.4 in the Appendix), I find that the unweighted standard deviation in $\hat{\mu}_{cs}(25)$ is equal to 2.46, as opposed to 5.68 in the US. The unconditional standard deviation of children's income ranks (which is uniformly distributed) is equal to $\frac{100}{\sqrt{12}} = 28.9$. As such, a one standard deviation increase in $\hat{\mu}_{cs}(25)$ is associated with a $\frac{2.46}{28.9} \approx 0.09$ standard deviation increase in the expected income rank at age 28 for children whose parents are at the 25th percentile rank of the national income distribution. This association is equal to 0.20 standard deviation in the US (Chetty et al., 2014), suggesting that regional disparities in intergenerational mobility are half as large in the Netherlands than in the US. Table B.2 in the Appendix presents the distribution across regions of the expected income rank at ages 24 till 28, and educational attainment at age 14, 19, and 28 for all children with parents at the 25th percentile rank of the national distribution. Panel B in Table B.2 shows that the regional dispersion at age 28 in the Netherlands is approximately equal to the regional dispersion at age 24 in Australia, but always strictly smaller than in the US. Nevertheless, Deutscher (2020) identifies a similar magnitude of the place effect for Australia as Chetty and Hendren (2018a) for the US.

Figure 3 presents the estimated correlation between different age-outcome (O, A) combinations of intergenerational mobility at the median of the parental household distribution (i.e., $p_i = 50$) across regions. These results show that the regional variation in intergenerational mobility is heavily correlated across different ages of measurement: regions with lower predicted outcomes in high-track choices at age 14, also have higher predicted educational attainment at age 19 (i.e., correlation coefficient equal to 0.956), and a higher probability of having a Bachelor degree at age 28 (i.e., correlation coefficient equal to 0.598). On the contrary, the correlation gets closer to zero when one compares income with education mobility: the correlation coefficient between individuals' predicted income at age 28 and high-track choices at age 14 is equal to -0.343, but equal to -0.010 when compared with final educational attainment at age 28. Furthermore, educational attainment measured at age 14 and income at age 24 are negatively correlated, but this strong negative correlation across regions gets weaker once income is measured at later ages. One possible explanation for the latter observation is that individuals who have a Bachelor degree start their first job later than individuals without such a degree, and that this is reflected in measuring income at earlier ages (Haider and Solon, 2006). I observe similar correlation patterns at the 25th and 75th percentile of the national parental household income distribution (see Figure A.6 and Figure A.7 in the Appendix, respectively). The main goal of this paper is to investigate what explains the variation in these predicted values across regions: systematic sorting of family types based on their unobservables, and/or causal effects of places.

4 Empirical framework

I use the quasi-experimental framework introduced in Chetty and Hendren (2018a). This approach consists of two steps. In the first step children's outcomes are predicted for every region using permanent residents only. I use these predicted outcomes in the second step to estimate the average place effects in isolation from a (potential) sorting bias.

4.1 Definition of place effects

I denote children's outcomes O at age A and their parents' income rank of one-time movers again by y_i and p_i, respectively. Consider a child i born in year $s = s(i)$ whose parents moved from origin region $o = o(i)$ to destination region $d = d(i)$ when the child was $m = m(i)$ years old. Then I consider the following model specification to determine the best linear predictor of children's outcome O measured at age A, given the level of mobility in destination region d, as denoted by $\mu_{ds}(p_i)$:

$$y_i = a_m + \gamma_m \mu_{ds}(p_i) + f_m(W_i) + \theta_{im} \tag{4}$$

for $i \in \mathcal{M}_m$, where $\mathcal{M}_m$ denotes the set of children in our sample whose parents are one-time movers, and moved at the time when the child was m years old. The total set of movers is equal to $\mathcal{M} = \cup_m \mathcal{M}_m$. Furthermore, W_i is an unobserved vector of covariates capturing all factors which determine children's long-run socioeconomic outcomes, such as ability, their parents' latent wealth, or other (family) inputs which are correlated with the decision and/or the possibility for a child to obtain a (pre-) higher education degree or earn a relatively high (household) income later in life. The functional form of $f_m(\cdot)$ is unknown. I assume that the unobserved confounders captured by $f_m(W_i)$ are additively separable, and I do not allow for interaction terms of $f_m(W_i)$ and other regressors. Furthermore, θ_{im} is a mean-independent error term, i.e., $\mathbb{E}(\theta_{im}|p_i, W_i) = 0$.

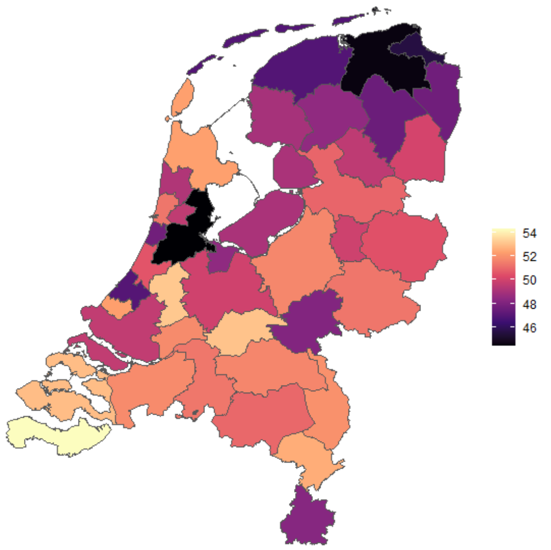
I do not observe $f_m(W_i)$, but instead I consider the following model specification for $i \in \mathcal{M}_m$:

$$y_i = A_m + \Gamma_m \mu_{ds}(p_i) + \varepsilon_{im} \tag{5}$$

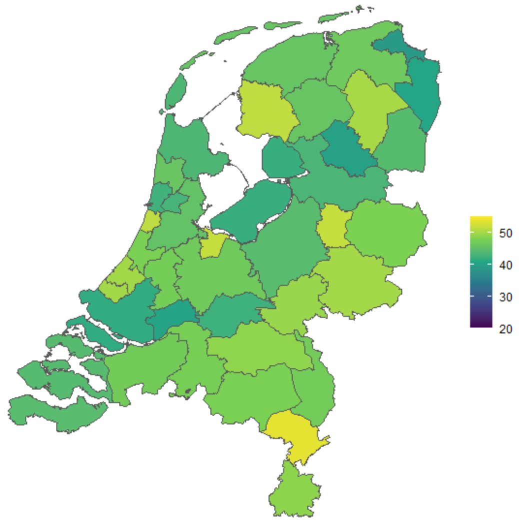
where ε_{im} is assumed to be uncorrelated with $\mu_{ds}(p_i)$. Given the model specification as presented in (4), one can interpret the OLS coefficient estimates Γ_m as place effects in equation (5) if the unobserved confounders of children's (long-run outcomes) captured by $f_m(W_i)$ are uncorrelated with $\mu_{ds}(p_i)$. If this is true, then $\Gamma_m = \gamma_m$, which represents the average impact of spending $A - m$ years in destination region d where children's outcomes who have been

Figure 2: Predicted Outcomes for Permanent Residents at Median Parental Endownment

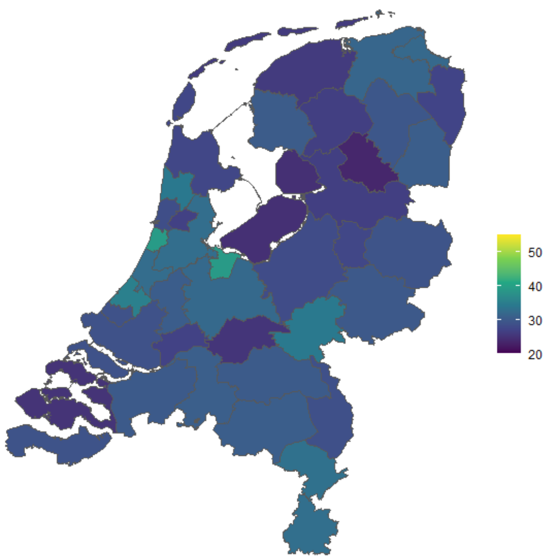
(a) Household Income Age 28 ($p = 50$)
Mean: 50.07, Std. dev.: 2.13



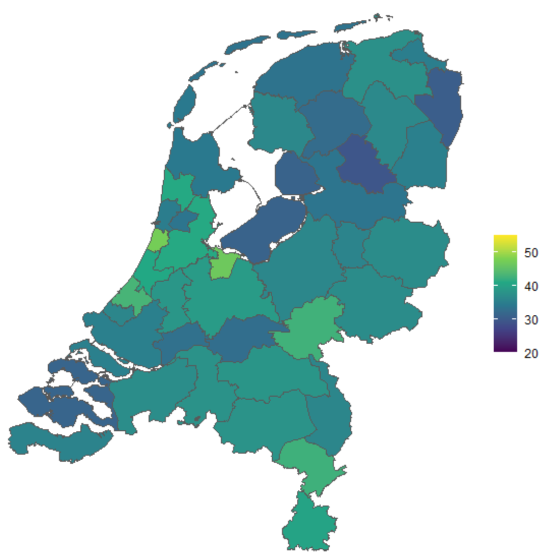
(b) Bachelor Degree Age 28 ($p = 50$)
Mean: 46.50, Std. dev.: 3.10



(c) Pre-Bachelor Degree Age 19 ($p = 50$)
Mean: 30.00 , Std. dev.: 3.14

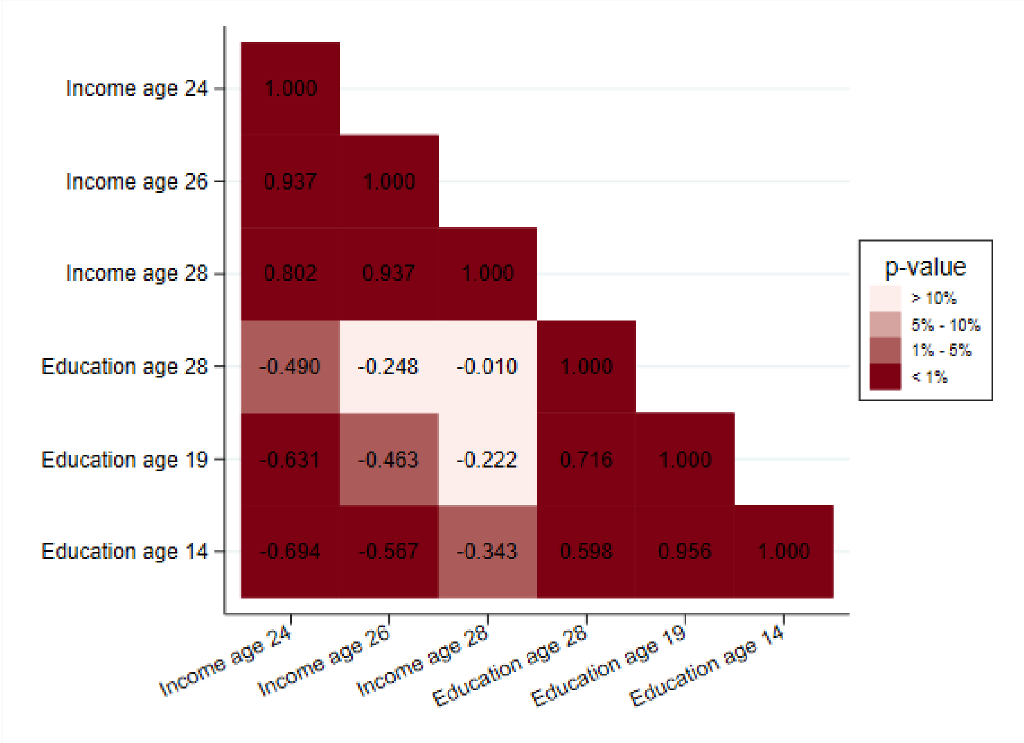


(d) Pre-Bachelor Track Age 14 ($p = 50$)
Mean: 37.33 , Std. dev.: 3.63



Notes. This figure maps the predicted outcomes for children's household income rank (Panel A) and children's probability of having a bachelor degree (or higher) at age 28 (Panel B), at the median of the parental household income distribution for all parents whose children are born in 1985. Similarly, Panel C and Panel D map children's predicted educational attainment at ages 14 and 19. I construct all of these predicted outcomes by estimating equation (3) using OLS, and setting $p = 50$. For income (Panel A), darker (black and/or purple) colors represent areas with lower predicted outcomes, and lighter (pink and/or orange) colors represent areas with higher educational attainment outcomes. For educational attainment (Panel B, Panel C, and Panel D), darker (blue) colors represent areas with lower predicted outcomes, and lighter (green) colors represent areas with higher predicted outcomes. The estimation sample includes all children in the 1985 birth cohort whose parents did not move across different COROP regions between 1995 and 2018 (i.e., permanent residents). I use the resulting estimated coefficients to construct predicted values for children whose parents' have $p = 50$. For more details on the construction of these variables, see Section 2.2.

Figure 3: Regional correlation in Predicted Outcomes at Median Parental Endowment



Notes. This figure shows the estimated regional correlation between combinations of intergenerational mobility measures, at the median of the parental household income distribution for all parents whose children are born in 1985. This figure considers the linear relationship (3) between parents' household income rank and children's family income at age 28, educational attainment at ages 14, 19, and 28 as measures for intergenerational mobility, as also presented in Figure 2 for every COROP region. Every cell presents the estimated correlation coefficient between two measures of intergenerational mobility across regions. Dark red cells correspond to combinations of mobility measures for which the null hypothesis that the correlation between two mobility measures is equal to zero, is rejected at the 1% significance level.

living there during their entire childhood are one unit higher.[21] This unit is equal to percentile points in case the child's outcome is equal to their income rank (i.e., the coefficient $\gamma_m^{28,I}$), and percentage points in case the child's outcome is equal to a binary indicator representing their educational attainment at age A. Moreover, $\gamma_m = 0$ for all $m \geq A$, since moving to better regions after age A cannot have a causal effect on a child's outcomes measured at age A. Yearly place effects are defined by $\psi_m = \gamma_m - \gamma_{m+1}$. Then, the measure $\gamma_0 = \sum_{m=0}^{A} \psi_m$ denotes the degree of which differences in permanent residents' outcomes across regions are due to causal effects of place.[22]

[21]Some studies are able to use a (natural) experimental setting to disentangle causal effects of place from individual determinants, which implies that such unobserved confounders are independent of the destination of movers. Some examples of such studies are Ludwig et al. (2013), Oreopoulos (2003), and Gould et al. (2004).

[22]For example, Chetty and Hendren (2018a) find that the average yearly place effect is equal to 0.04 percentiles. In particular, this means that every additional year a child spends in a commuting zone (CZ) during his/her childhood, their outcome on the income rank measured at age 24 converges to the outcomes on income rank of permanent residents with a rate of 4 percent per year during childhood. More specifically, they argue that if this rate of convergence remains 4% for ages below 9 (the earliest age from which they can measure place effects), children who would move at birth to an area with higher outcomes would pick up approximately $4\% \times 23 = 92\%$ of the differences between origin and destination region.

Using observational data, $f_m(W_i)$ potentially depends on permanent residents' outcomes captured by the degree of mobility $\mu_{ds}(p_i)$ in the destination area they move to. If this is the case, one can show that the regression coefficient Γ_m is biased for γ_m, i.e.:

$$\Gamma_m = \gamma_m + \underbrace{\frac{Cov\Big(f_m(W_i), \mu_{ds}(p_i)\Big)}{Var\Big(\mu_{ds}(p_i)\Big)}}_{=\delta_m}. \tag{6}$$

Thus, the estimated coefficient Γ_m does not solely represent a yearly place effect γ_m, but in addition includes a selection effect δ_m which measures the extent to which the unobserved confounders captured by $f_m(W_i)$ are related to the estimated regional mobility. For example, a child's unobserved family inputs $f_m(W_i)$ are correlated with a destination region's measure of mobility $\mu_{ds}(p_i)$ if specific types of families (i.e., families with higher wealth or educational attainment) move into areas with higher upward mobility.

In order to disentangle these causal effects of place from the selection effects, Chetty and Hendren (2018a) assume that selection into better regions (i.e., regions associated with a higher level of mobility for a given parental endowment) does not vary with the child's age at the time of the move. Following this assumption, it is not required that *where* families move to is independent of a child's potential outcomes on education and income, but *when* people move to a specific region with relatively high or low intergenerational mobility is independent of a child's potential outcome. This assumption is formalized below.

Assumption 1. $\delta_m = \delta$ for all m.

Given Assumption 1, one obtains the causal effect of place by taking the differences between the coefficient estimates Γ_m evaluated at different values of ages at move m:

$$\psi_m = \gamma_m - \gamma_{m-1} = \Gamma_m - \Gamma_{m-1}. \tag{7}$$

Specifically, I estimate the average yearly place effect, denoted by ψ, by regressing the obtained coefficient estimates $\{\Gamma_m\}$ on age at move m:

$$\Gamma_m = \psi_0 + \psi m + \varepsilon_m \tag{8}$$

with $\mathbb{E}(\varepsilon_m|m) = 0$. Throughout my results, I will discuss three main implications from this regression as in Chetty and Hendren (2018a).

First, I will test the null hypothesis that the estimated slope coefficient ψ for ages at move $m \geq A$ is equal to zero. Recall that for $m \geq A$, the coefficient estimates $\{\Gamma_m\}$ merely reflect a selection effect, as moving to better areas after age A when children's outcomes are measured cannot have a causal effect on children's outcomes. In case I do not reject this null hypothesis, this suggests that there exist a flat relationship between selection and age at move for $m \geq A$ which is consistent with Assumption 1. Second, I will report the average selection effect, which is equal to the average value of the coefficient estimates $\{\Gamma_m\}$ for $m \geq A$. Under Assumption

1, this average selection effect can then be extrapolated to earlier ages. Lastly, I will report the slope coefficient ψ for ages at move $m < A$ representing the average yearly place effect. The next section describes which regression specifications I use to obtain the coefficient estimates $\{\Gamma_m\}$ in (5).

4.2 Regression specifications

Consider a child i born in year $s = s(i)$, whose parent(s), with income decile $q = q(i)$ and exact income percentile rank p_i, moved when he/she was $m = m(i)$ years old to destination region $d = d(i)$ from origin region $o = o(i)$. First, I estimate the relationship between these children's outcomes y_i and the regional measure of mobility, using the following model specification:

$$\begin{aligned} y_i = \sum_q \sum_o \sum_s \sum_m \alpha_{qosm} \mathbb{1}\{q(i) = q, o(i) = o, s(i) = s, m(i) = m\} \\ + \sum_o \sum_d \sum_s \sum_m \Gamma_m \mathbb{1}\{o(i) = o, d(i) = d, s(i) = s, m(i) = m\} \hat{\Delta}_{ods}(p_i) + \\ \sum_o \sum_d \sum_s \kappa_s \mathbb{1}\{o(i) = o, d(i) = d, s(i) = s\} \hat{\Delta}_{ods}(p_i) + \theta_i \end{aligned} \quad (9)$$

for all children $i \in \mathcal{M}$, where α_{qosm} denotes a parent's income rank decile q, origin o, birth cohort s, and age at move m fixed effects.[23] The independent variable(s) of interest is the predicted difference in intergenerational mobility the destination region and the origin region, given by:

$$\hat{\Delta}_{ods}(p_i) = \hat{\mu}_{ds}(p_i) - \hat{\mu}_{os}(p_i) \quad (10)$$

where $\hat{\mu}_{cs}(p_i)$ is estimated assuming a linear relationship between parents' income and children's outcomes, as represented in expression (3) for region c. I only observe parents' locations starting from age 10 for the 1985 birth cohort, and age 5 for the 1990 birth cohort. Similarly, I only observe parents' income from age 18 for the 1985 birth cohort, and age 13 for the 1990 birth cohort. I control for such potential systematic birth cohort-specific measurement errors via the third term in equation (9) by $\{\kappa_s\}$. The coefficients of interest in equation (9) are represented by $\{\Gamma_m\}$ for $m = 5, \ldots, 33$.

The regression equation as denoted in (9) consists of approximately 11,000 fixed effects. Such a general model specification appears to be inconvenient in case the researcher wants to estimate similar regressions on smaller subsamples or include family fixed effects. For that reason, I also estimate a more parametric model specification, in which I closely follow the work by Chetty

[23] Therefore, this first model specification contains a large amount of fixed effects. In particular, I consider income deciles labeled by the subscript q. I use income deciles instead of exact income percentiles to reduce the number of fixed effects α_{qosm} as also done by Chetty and Hendren (2018a) and Deutscher (2020). Increasing the number of income categories as indexed by q (i.e., 20 income categories) does not change the results.

and Hendren (2018a) and Deutscher (2020) as follows:

$$y_i = \sum_s \sum_o (\alpha_s^0 + \alpha_s^1 \hat{\mu}_{os}(p_i)) \mathbb{1}\{s(i) = s, o(i) = o\} + \sum_m (\zeta_m^0 + \zeta_m^1 p_i) \mathbb{1}\{m(i) = m\}$$
$$+ \sum_s \sum_o \sum_d \kappa_s \hat{\Delta}_{ods}(p_i) \mathbb{1}\{s(i) = s, o(i) = o, d(i) = d\}$$
$$+ \sum_o \sum_d \sum_s \sum_m \Gamma_m \hat{\Delta}_{ods}(p_i) \mathbb{1}\{o(i) = o, d(i) = d, s(i) = s, m(i) = m\} + \theta_i \quad (11)$$

for all $i \in \mathcal{M}$. Here, I explicitly control for five key factors that are potentially captured by the fixed effects as presented in equation (9), namely: (i) birth-cohort effects, (ii) birth-cohort effects interacted with origin quality effects, (iii) disruption costs of moving effects, (iv) age-at-move effects interacted with parents' household income rank p_i, and (v) and cohort-specific measurement error as defined previously.

5 Results

This section presents my main estimates of the coefficients $\{\Gamma_m\}$ for $m = 5, \ldots, 33$, for different outcome-age (A, O) combinations. Section 5.1 discusses the results for children's outcomes after childhood. Section 5.2 presents the results for intermediate outcomes on educational attainment (i.e., from age 14 onwards).

5.1 Children's outcomes after childhood

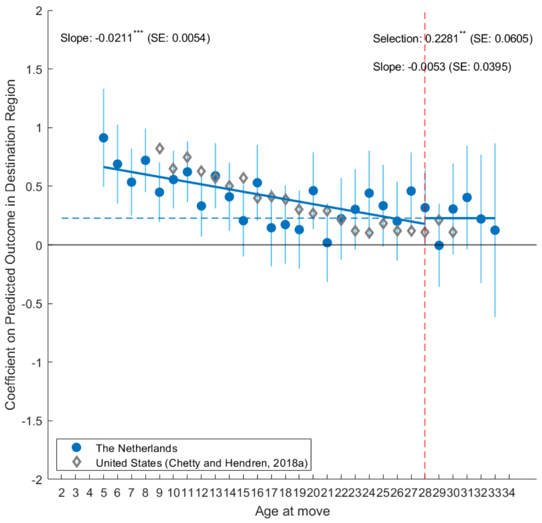
Figure 4 presents the estimated regression coefficients for every age of move m, for income (Panel A) and educational attainment (Panel B) measured at age 28, as represented by the blue dots, using the model specification as presented in (11).

Children's household income rank at age 28. Figure 4 Panel A shows my estimated exposure effects (blue dots) $\{\Gamma_m\}$ for every age at move m, where intergenerational mobility and the outcome variable (i.e., independent variable) are measured in terms of a child's household income rank (i.e., $O = I$) at age $A = 28$, using the general model specification as denoted in equation (11). Children's household income rank is the main outcome in Chetty and Hendren (2018a). The estimated coefficients measure to which extent a child's household income rank at age 28 is associated with a move at age m to a region with a one percentile point higher household income rank for children who are permanent residents. In order to compare my results to other contexts, I plot similar coefficients reported for the US by the grey diamonds.

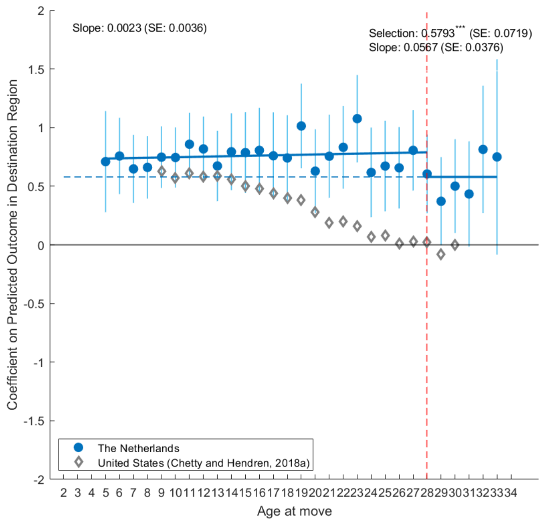
First, I find evidence of selection effects. All coefficient estimates obtained for ages $m \geq A$ can only represent a selection effect, since the causal effect of place is equal to zero in case the move happens after the outcome is measured. I determine selection effects by the average level of the coefficients $\{\Gamma_m\}$ for $m \geq 28$, since selection effects are constant across ages of move (Assumption 1). I find that the average selection effect is equal to 0.2281 (SE: 0.0605). Under the validity of Assumption 1, I then extrapolate this average selection effect to earlier ages, thereby disentangling selection effects from place effects in $\{\Gamma_m\}$ for $m < 28$. Following Chetty and Hendren (2018a), I then regress the coefficients $\{\Gamma_m\}$ on $m \geq 28$ yields an insignificant

Figure 4: Estimated Effects on Earnings and Educational Attainment

(a) Household Income Age 28



(b) Having a Bachelor Degree Age 28



Notes. These figures plot the coefficient estimates $\{\Gamma_m\}$ versus the child's age at move m using the model specification presented in (11), for different outcomes O and ages A at time of measurement of the outcome variable (blue dots). The estimation sample includes all children whose parents have moved exactly once throughout the period $1995-2018$. The best-fit lines are estimated by a simple OLS regression of the coefficients $\{\Gamma_m\}$ on m for two different samples: $m < A$ and $m \geq A$. The slope coefficient for $m < A$ and $m \geq A$ are presented, as well as the average selection effect for $m \geq A$. The vertical red dashed line presents the age at which the outcome variable is measured, and therefore visualizes this split for estimation of the best-fit line for different samples. Under validity of constant selection effects across age of move (Assumption 1), the average selection constant measured for $m \geq A$ can be extrapolated to earlier ages $m < A$, as presented by the dashed horizontal blue lines. 95% confidence intervals are presented, using robust standard errors. Panel A plots the coefficient results obtained by Chetty and Hendren (2018a) (i.e., Figure IV, Panel A), denoted by grey diamonds. Similarly, Panel B, Panel C, and Panel D plot the coefficient results on college attendance between age 18 and 23, obtained by Chetty and Hendren (2018a) (i.e., Figure VIII, Panel A), again denoted by grey diamonds.

slope of -0.0053 (SE: 0.0395), confirming Assumption 1 for $m \geq 28$.

Second, I find evidence on the existence of causal effects of place when exploiting regional differences in intergenerational income mobility (Figure 4 Panel A). Regressing the coefficient estimates $\{\Gamma_m\}$ on $m < 28$ yields a slope equal to -0.0211 (SE 0.0054), suggesting that children's household income at age 28 converges to the household income of children whose parents are permanent residents of the destination region at a rate of 2.1% per year. This result is in line with the results documented by ter Weel et al. (2019) who uses similar Dutch data, but less than half of the effect as documented for the US and Australia (Chetty and Hendren, 2018a; Deutscher, 2020), where the magnitude of convergence is equal to 4.4% per additional year. I obtain similar exposure estimates, all approximately equal to 2%, using different model specifications and different measures of children's income, as reported in Table B.3 in the Appendix. Moreover, I find a similar average yearly place effect of -0.0232 (SE: 0.0074) when using the general model specification as presented in equation (9), presented in Panel A in Figure A.8 in the Appendix.

Children's educational attainment at age 28. Panel B in Figure 4 show the estimated exposure effects $\{\Gamma_m\}$ for every age at move m, where intergenerational mobility and the independent variable are measured in terms of a child's educational attainment measured at age 28. I also plot the coefficient estimates as documented by Chetty and Hendren (2018a) who consider children's college attendance rates between age 18 and 23 (grey diamonds) as outcome variables. As opposed to income, this figure shows a different age pattern than in the United States. First, I find evidence of statistically significant selection effects for $m > 28$, with an average of 0.5793 (SE 0.0719). This implies that families who move to a region where the predicted outcomes of children who spent their entire childhood in that area are ten percentage points higher, will have a 5.8 percentage points higher probability of having a Bachelor degree themselves, as a consequence of selective sorting of their families only.

Second, in stark contrast with the pattern observed for income and the pattern observed for the United States, there is no gradient in the relationship between the age of move and the effect of regional mobility with a child's probability of having Bachelor degree, for children who moved at ages $m < 28$. In particular, this relationship between age and regional mobility is flat and statistically insignificantly different from zero for ages $m < 28$, i.e., the estimated slope is equal to 0.0023 (SE 0.0036). This implies that children who move earlier to better high-upward education mobility regions, measured as educational attainment at later ages, do not have higher outcomes than children who move later during their childhood.[24] I find a similar average yearly place effect of 0.0069 (SE: 0.0067) when using the general model specification as presented in (9), as presented in Panel B in Figure A.8 in the Appendix.

5.2 Children's education outcomes during childhood

I also evaluate the effect of place on children's intermediate outcomes at ages 14 and 19 to measure to which extent place effects on early schooling decisions translate to individuals' socioeconomic outcomes after childhood. Specifically, I use children's (intermediate) track choices to better understand at what stage in the educational trajectory regional inequalities in schooling emerge. If individuals who grow up in disadvantaged regions experience lower opportunities to enter into a high-ability secondary education track, this could in turn lead to continuing regional disparities in higher education and/or income at older ages as well. Panel A and Panel B in Figure 5 presents the estimated coefficients $\{\Gamma_m\}$ when I estimate equation (11) for educational attainment measured at ages 19 and 14.

I find evidence of selection effects for both intermediate outcomes in educational attainment. At age 19, the average selection effect for ages at move $m \geq 19$ is equal to 0.6060 (SE 0.0365) (Figure 5 Panel A). In line with Assumption 1, regressing the coefficients $\{\Gamma_m\}$ on m for $m \geq 19$

[24] Strictly speaking, under the validity of Assumption 1, my results suggest that there is a constant effect of place. This effect is equal to the difference between the average exposure effect for $m < 28$ and the average exposure effect for $m \geq 28$. In other words, irrespective at which age a child moves to a higher mobility region, places have a positive effect on children's final educational attainment. However, this jump around the age of move 28 seems peculiar, since it is unlikely that differences in causal effect of place emerge exactly around this targeted age. If I assume that children whose families move around a similar age are more likely to be similar in terms of unobserved determinants of socioeconomic outcomes after childhood, it seems more natural to compare coefficient estimates Γ_m for ages at move m close to each other.

yields an insignificant slope of -0.0011 (SE 0.0088). On the contrary, at age 14, I report suggestive evidence that selection varies across children's ages at move: regressing the coefficients $\{\Gamma_m\}$ on m for $m \geq 14$ yields an statistically significant slope equal to -0.0193 (SE 0.0075). This indicates that the relationship between region's upward mobility in terms of early track choices and children's track choices varies with age for moves that took place after children's track choices were made. The latter finding invalidates Assumption 1 for outcome-age combination (A, O) equal to $(14, E)$.

When I measure educational attainment at earlier ages, I find evidence on the existence of causal effects of place at age 14 and 19. The gradient of coefficient estimates across age of move follows a similar pattern like the documented in the US for college attendance rates between age 18 and 23 (Chetty and Hendren, 2018a), if I evaluate children's track choices at age 14 instead of their Bachelor degrees at age 28 (Figure 5 Panel A). Regressing the estimated coefficients $\{\Gamma_m\}$ on age at move m, I observe that the magnitude of this slope gets closer to zero once the age at measurement A increases: from -0.0460 (SE: 0.0114) for $A = 14$ (see Panel B in Figure 5), to -0.0193 (SE: 0.0075) for $A = 19$ (see Panel A in Figure 5), to an insignificant flat slope of 0.0023 (SE: 0.0036) for $A = 28$ (see Panel B in Figure 4). These results show that place effects are larger when educational attainment is measured at younger ages, and become less apparent once educational attainment is measured at older ages. Under Assumption 1, this observation could point towards the rationale that there is a nationwide equalized opportunity to higher education that compensates children who grew up in disadvantaged regions during high school.

6 Validity of constant selection effects across age of move

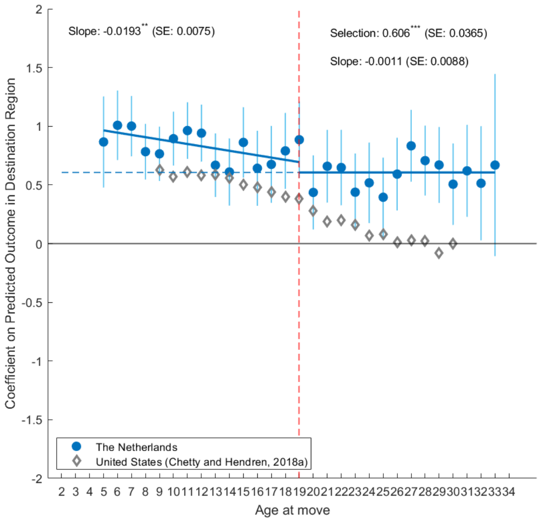
The previous conclusions on the magnitude and existence of place effects rest on the key identifying assumption that families' sorting decisions do not vary with the child's age at move (Assumption 1). However, my results on early educational attainment raise doubts about the validity of this assumption, as it suggests that the association between a region's upward mobility and individual's track choices varies with age. In this section, I test and account for violations of this assumption in more detail. I perform a test for non-constant selection in Section 6.1 by investigating to which extent pre-move track choices for children whose families moved after they were 14 years old, are related to a region's mobility in educational attainment at age 28. Section 6.2 identifies exposure effects on outcomes measured after childhood by explicitly controlling for pre-move characteristics related to track choices. Section 6.3 exploits the variation in the age difference between siblings.

6.1 Test for non-constant selection

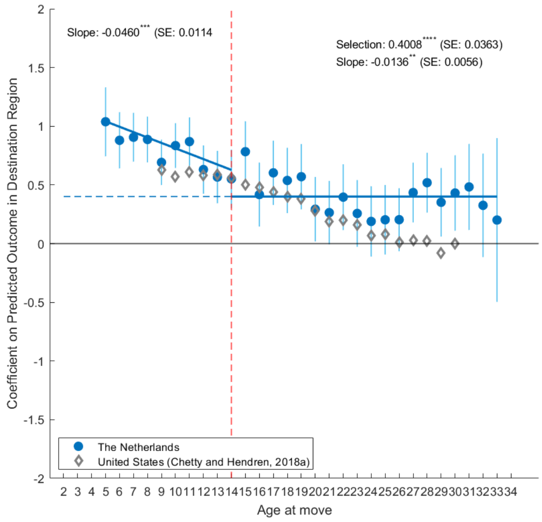
I revisit the main results as presented in Panel B in Figure 4, and test for balance in track choices made at earlier ages using the same movers sample. That is, I again estimate the exposure effects $\{\Gamma_m\}$ as represented in equation (11), but I replace the dependent variable y_i by $y_i^{14,E}$ and $y_i^{19,E}$, respectively, and the measure of a region's mobility $\hat{\Delta}_{ods}(\cdot)$ by $\hat{\Delta}_{ods}^{28,E}(\cdot)$ and $\hat{\mu}_{os}(\cdot)$ by $\hat{\mu}_{os}^{28,E}(\cdot)$. Here, the super-indices are used to clearly distinguish the difference in the measure of regional mobility and children's outcomes (at different ages). The resulting

Figure 5: Estimated Effects on Educational Attainment at Early Ages

(a) Educational Attainment Age 19



(b) Educational Attainment Age 14



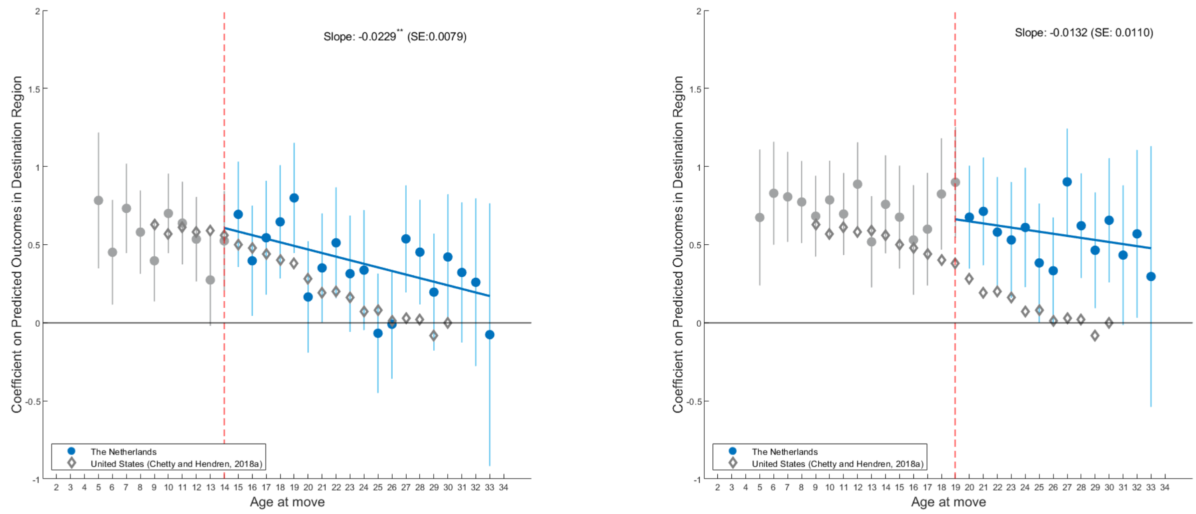
Notes. These figures plot the coefficient estimates $\{\Gamma_m^{A,E}\}$ versus the child's age at move m using the parametric model specification presented in equation (11), for educational attainment outcomes measured at ages $A = \{14, 19\}$. The point estimates are presented by the blue dots. For both figures, the estimation sample includes all children whose parents have moved exactly once throughout the period $1995 - 2018$. The best fit lines (blue straight lines) are estimated by a simple OLS regression of the coefficients $\{\Gamma_m^{A,E}\}$ on m for two different samples: $m < A$ and $m \geq A$. The slope coefficient for this best fit line is presented for $m < A$, as well as the average selection effect for $m \geq A$. The vertical dashed line presents the age A at which the outcome is measured, and therefore visualizes the split for estimation of the best-fit line for different samples. Both figures also plot the estimated exposure coefficients as documented in Chetty and Hendren (2018a) (i.e., Figure IV, Panel A in their paper) for college attendance rates, by the grey diamonds.

coefficient estimates for $m \geq 14$ can be interpreted as a test for non-constant selection across age at move: families who move to better regions with higher education mobility *after* their children have made a track choice around age 14 cannot experience a causal effect of place of such upward mobility on their children's track choice decisions at age 14.

Figure 6 presents the results of this test for age-dependent selection. I show that the average relationship between the degree of intergenerational mobility in higher education in the destination region and individual's track choices made earlier in life varies with the age of move (as represented by the blue dots). Specifically, regressing the coefficients $\{\Gamma_m^{28,E}\}$ against m for $m \geq 14$ yields a slope equal to -0.0229 (SE: 0.0079) which suggests that families who move to areas at age 14 with higher upward mobility in educational attainment at age 28 are more likely to have children with a high track choice than families who move at later ages (Panel A in Figure 6). If this type of age-specific selective migration is correlated with an individual's educational attainment measured after childhood, Assumption 1 is violated, and the exposure effects as presented in my main results are biased. Hence, these pre-move outcome tests on the basis of track choices provide suggestive evidence that violates the key identifying assumption underlying the quasi-experimental framework as proposed by Chetty and Hendren (2018a) in the Netherlands.

FIGURE 6: TEST FOR AGE-DEPENDENT SELECTION

(A) Educational Attainment Age 14 (B) Educational Attainment Age 19



Notes. These figures present the coefficient estimates $\{\Gamma_m^{28,E}\}$ versus the child's age at move m using the general model specification as presented in equation (11), replacing the dependent variable by $y_i^{14,E}$ (Panel A) and $y_i^{19,E}$ (Panel B), which denotes a binary indicator equal to one if a child has chosen or finished a high track at age 14 or 19, respectively. For both figures, the estimation sample includes all children whose parents have moved across COROP regions exactly once throughout the period $1995-2018$. The point estimates of this placebo test are presented by the blue dots. The red vertical dashed line presents the age A at which the dependent variable outcome is measured, and therefore visualizes the split for estimation of the best-fit lines for different samples (and the placebo test). The best fit lines (blue straight line for the placebo test) are estimated by a simple OLS regression of the coefficients $\{\Gamma_m^{28,E}\}$ on m for $m \geq A$. The point estimates before the age A are denoted by grey dots, as they do not represent a placebo test: the exposure effect measured for children whose families moved to higher mobility areas (in terms of expected outcomes in educational attainment measured at age 28) before age A could include the causal effect of place via the realized track choices at age A. Both figures plot the estimated exposure coefficients as documented in Chetty and Hendren (2018a) (i.e., Figure IV, Panel A in their paper) for college attendance rates, represented by the grey diamonds.

6.2 Controlling for intermediate education outcomes

There exist two very different explanations that could explain the evidence of no causal effect of place into higher education (Figure 4 Panel B), and a causal effect of place documented for earlier track choices (Figure 5 Panel A and Panel B). First, if Assumption 1 is true, children are able to catch up on the opportunity to obtain a Bachelor degree later in life. Thus, despite evidence of a causal effect of place for age 14, a place effect does not emerge for individual's final educational attainment at age 28. This could happen if children can easily progress to a high track after age 14, and that this opportunity is equal across all regions.[25]

Second, as argued in Section 6.1, it could be that families who move to high mobility regions in higher education after age 14, are systematically different in their children's past tracking

[25]This phenomenon is called "degree stacking". Degree stacking is sometimes argued to be a way to correct for the negative effects of selection into lower secondary education tracks at early ages (Visser et al., 2022).

decisions.[26] If such age-variant selective mobility in track choices matters for educational attainment later in life, this observation would violate the key identifying assumption of constant selection across age of move (Assumption 1). This could lead to an overestimation of the place effect as described in my main results, if parents whose children enrolled into higher tracks systematically migrate to higher upward mobility regions at earlier ages, as opposed to parents whose children enrolled in lower tracks. Since I observe children's education choices and outcomes at various ages during their life, I can control for these intermediate outcomes. Formally, I estimate an extension of the main regression specification as denoted in expression (11), by additionally controlling for children's *pre-move* tracking decisions (educational attainment) at age 14, (age 19), respectively. This specification is given by:

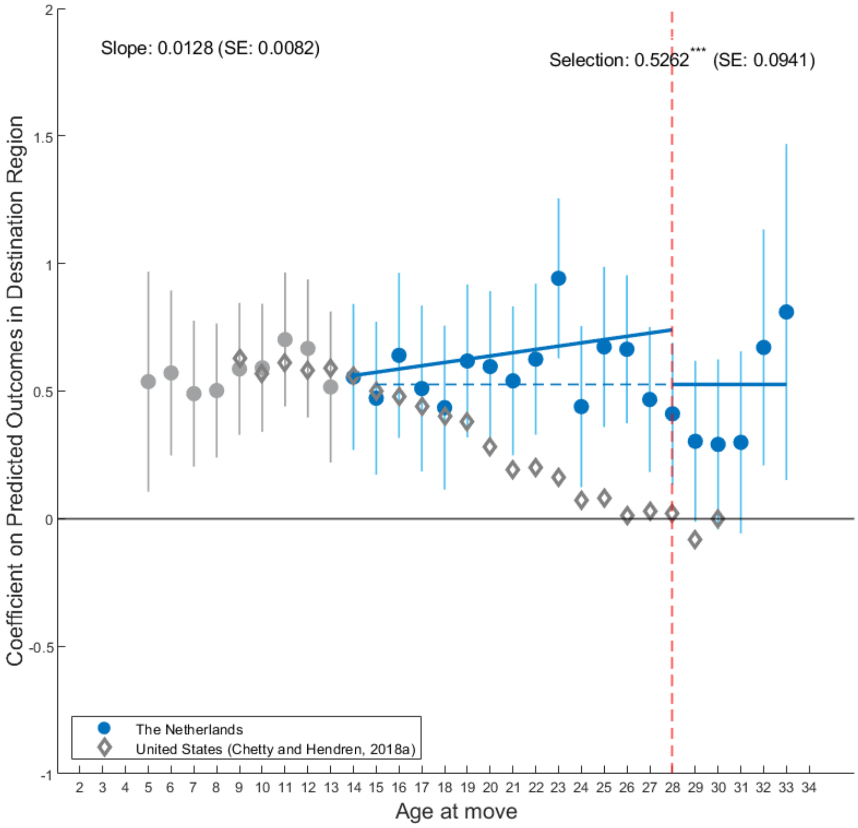
$$
\begin{aligned}
y_i^{28,E} = \sum_o \sum_s (\alpha_s^0 + \alpha_s^1 \hat{\mu}_{os}^{28,E}(p_i)) \mathbb{1}\{o(i) = o, s(i) = s\} + \sum_m (\zeta_m^0 + \zeta_m^1 p_i) \mathbb{1}\{m(i) = m\} \\
+ \sum_o \sum_d \sum_s \kappa_s \hat{\Delta}_{ods}^{28,E}(p_i) \mathbb{1}\{o(i) = o, d(i) = d, s(i) = s\} \\
+ \sum_o \sum_d \sum_m \Gamma_m^{28,E} \hat{\Delta}_{ods}^{28,E}(p_i) \mathbb{1}\{o(i) = o, d(i) = d, m(i) = m\} \\
\underbrace{\sum_m \Gamma_m^{14,E} \mathbb{1}\{m(i) = m, m(i) \geq 14\} y_i^{14,E}}_{=\text{Pre-move selection after age 14}} + \underbrace{\sum_m \Gamma_m^{19,E} \mathbb{1}\{m(i) = m, m(i) \geq 19\} y_i^{19,E}}_{=\text{Pre-move selection after age 19}} + \theta_i \quad (12)
\end{aligned}
$$

for $i \in \mathcal{M}$. The indicator function $\mathbb{1}\{m(i) = m, m(i) \geq A\}$ is equal to one if $m(i) \geq A$ and $m(i) = m$, and zero otherwise. The coefficient of interest is $\{\Gamma_m^{28,E}\}$ for ages at move $m = 5, \ldots, 33$. Here, I explicitly include super-indices on a region's estimated measure of higher education mobility, $\hat{\mu}_{os}^{28,E}$ and $\hat{\Delta}_{os}^{28,E}$, and children's outcomes $y_i^{28,E}$, $y^{19,E}$, and $y_i^{14,E}$ to clearly distinguish at which age children's outcomes are measured, and from which age onwards I can interpret intermediate outcomes as pre-move outcomes. This specification controls for systematic age-variant sorting of family types based on observed children's pre-move decisions, by incorporating variation in track decisions (track degrees) for children who moved *after* age 14 (and age 19), presented by $y_i^{14,E}$ (and $y_i^{19,E}$).

The coefficient estimates $\{\Gamma_m^{28,E}\}$ are plotted in Figure 7. This result shows that after controlling for pre-track decisions and outcomes, the relationship between age of move and regional education mobility is flat: regressing the coefficients $\{\Gamma_m^{28,E}\}$ on m for $14 \leq m < 28$ yields an insignificant slope equal to 0.0128 (SE: 0.0082), which is of a similar magnitude as the effect I find in my main results as presented in Panel B in Figure 4. This exercise suggests that place only seem to matter for children's educational attainment *during* childhood, but not *after* childhood, even after controlling for age-specific sorting in individual's intermediate educational attainment.

[26] The movers-exposure design as developed by Chetty and Hendren (2018a) does not require that families who move at older ages are similar in terms of their children's track choices, than families who move at younger ages. However, if the association between the degree of mobility in the destination region and children's track choices varies with the age at move (i.e., which is true in the particular case of selective mobility on the basis of past decisions/(revealing) information regarding children's educational attainment), the key identifying assumption (Assumption 1) is violated.

Figure 7: Controlling for intermediate outcomes: Educational Attainment



Notes. This figure presents the coefficient estimates $\{\Gamma_m^{28,E}\}$ versus the child's age at move m using the alternative model specification as presented in expression (12), in which I additionally control for pre-move outcomes of children who moved after age $m \geq 14$. These point estimates are presented by the blue dots. The estimation sample includes all children whose parents have moved across COROP regions exactly once throughout the period $1995 - 2018$. The red vertical dashed line presents the age A at which the dependent variable outcome is measured, and therefore visualizes the split for estimation of the best-fit lines for different samples of children: $14 \leq m < 28$ and $m \geq 28$. The point estimates before age 14 are denoted by grey dots, as they cannot be directly compared with the blue coefficient estimates. The figure plots the estimated exposure effects as documented in Chetty and Hendren (2018a) (i.e., Figure IV, Panel A in their paper) for college attendance rates, represented by the grey diamonds.

6.3 Family fixed effects

In order to control for unobservable confounders that are fixed within families, I re-estimate equation (11) by additionally controlling for family fixed effects, as also suggested by Chetty and Hendren (2018a). Such a specification then exploits variation in the timing of move across siblings on income and educational attainment.

The results of this exercise are plotted in Figure 8. For educational attainment (Panel B, Panel C and Panel D in Figure 8), I find a similar exposure effect as presented in my main results. Exploiting variation in ages at move and outcomes between siblings, I find that regressing the coefficient estimates $\{\Gamma_m\}$ on m for $A = 14$ yields a significant slope equal to -0.0542

(SE: 0.0142), which is approximately the same as presented in my main results in Figure 5. For $A = 28$ (Panel B Figure 8), I report a flat age-regional mobility gradient, with a slope equal to 0.0074 (SE: 0.0083). For $A = 19$ (Panel C Figure 8), I find a significant slope equal to -0.0286 (SE: 0.0114). Moreover, the average estimated coefficients for $m \geq A$ are statistically indifferent from zero, which implies that all of the selection effects are captured by the family fixed effects.

For income (Panel A in Figure 8), I find that the exposure effect of approximately 2.1% as found in my main results (Panel A in Figure 4) decreases to zero, after controlling for family fixed effects. When regressing the estimated coefficients $\{\Gamma_m\}$ on ages at move m, I document a insignificant slope equal to 0.0062 (SE: 0.0108). I can reconcile the downwards sloping age-regional mobility gradient as documented in my main results, with the flat gradient documented in this section, by arguing that families with better (unobserved) parental inputs tend to systematically migrate to regions with higher upward income mobility at earlier ages than families with worse determinants. Again, this rationale provides suggestive evidence of non-constant selection across ages at move into areas with a higher income mobility, which violates Assumption 1.

6.4 Discussion

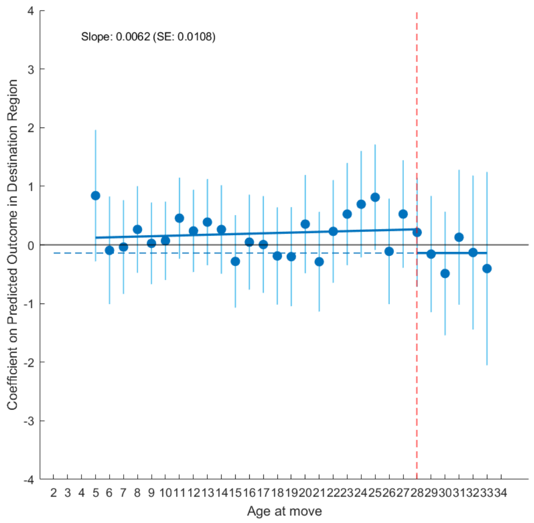
In this paper, I aim to shed light on whether the place of residence during childhood matters for the intergenerational transmission of income and education outcomes at both young and older ages in life. I document a place effect for educational attainment at ages 14 and 19, but not at age 28. I reconcile these different place effects by controlling for intermediate education outcomes as a measure for age-dependent selection. I show that place of residence during childhood only matters for individual's track choices made early in life, but not for final outcomes on educational attainment and (family) income. This observation is very different from the United States, where exposure to better place early in life significantly affects both individual's probability of attending college between ages 19 and 23 and their family income from age 24 onwards (Chetty and Hendren, 2018a).

One potential reason for this difference between the Netherlands and the US could be the different institutional setting. The Netherlands employs a national education system that manages to mitigate regional disparities in equality in opportunity to higher education, as opposed to countries like the US which utilize a more state-based schooling system. The latter system is heterogeneous in school quality, curriculum standards, and school financing, across and within regions (Snyder and Dillow, 2013; Owings et al., 2015).

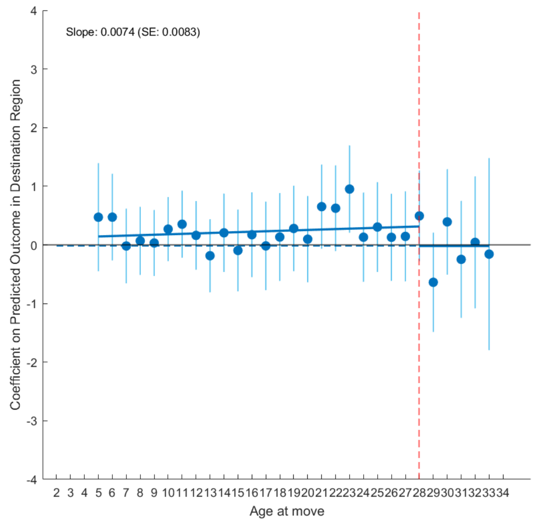
Another line of argumentation touches the ongoing discussion on relative benefits of a selective (the Netherlands) versus a comprehensive schooling system (US). A selective education system selects students into hierarchically structured levels by their ability, both within and across schools at relatively early ages. A comprehensive system employs more heterogeneous classrooms, consisting of both low and high ability students. One argument in favor of selective education systems is that both the curriculum and pace of instruction can be optimally set such that students gain maximally given their (ability-related) constraints. As a consequence,

Figure 8: Family Fixed Effects

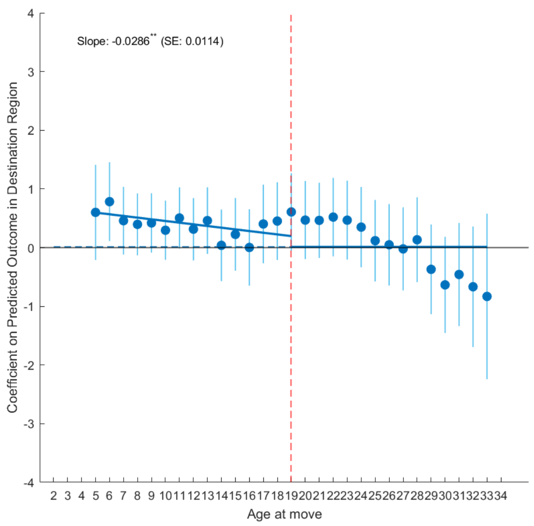
(a) Household Income Age 28



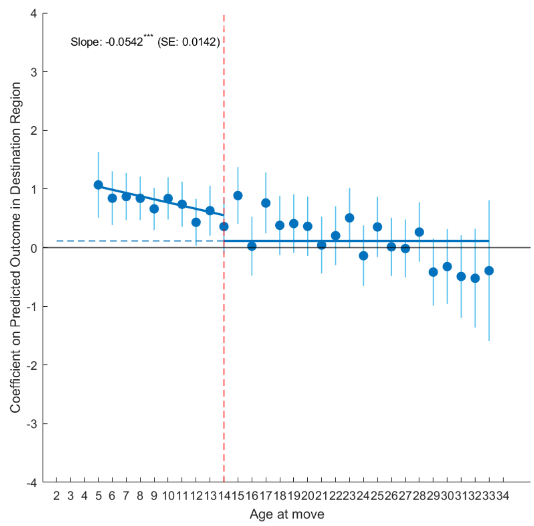
(b) Having a Bachelor Degree Age 28



(c) Having a pre-Bachelor Degree Age 19



(d) Having followed a pre-Bachelor track Age 14



Notes. These figures present the coefficient estimates $\{\Gamma_m^{A,O}\}$ versus the child's age at move m using the general model specification as presented in equation (11), and additionally controlling for family fixed effects. The point estimates are denoted by blue dots. The estimation sample includes all children whose parents have moved across COROP regions exactly once throughout the period $1995 - 2018$. The red vertical dashed line presents the age A at which the dependent variable outcome is measured, and therefore visualizes the split for estimation of best-fit lines for different samples of children: $m < A$ and $m \geq A$. The slope for regressing the coefficients $\{\Gamma_m^{A,O}\}$ for $m < A$ is presented. The average value of the coefficients $\{\Gamma_m^{A,O}\}$ for $m \geq A$ (i.e., selection effect) is presented.

individuals can be better accommodated in developing the necessary skills to successfully follow and complete higher education (Hanushek and Wößmann, 2006). However, other studies find that early tracking negatively affects the intergenerational transmission of earnings and/or children's outcomes later in life (Pekkarinen et al., 2009; Dustmann, 2004; Meghir and Palme, 2005; Stans, 2022).

Another reason that could reconcile the results found for Netherlands and the US is the existence of age-dependent selective migration of families into higher mobility regions. I provide evidence on the existence of such non-constant sorting across ages at move in individual's track choices in the Netherlands. Specifically, I show that families with children who entered high schooling tracks before they move tend to systematically sort themselves into higher upward mobility regions right after age 14, rather than later. For example, if such age-dependent selection intensity of sorting into upward mobility areas also decreases in age in the US, the yearly place effect as documented in Chetty and Hendren (2018a) could be overestimated.

7 Conclusion

This paper documents and examines regional variation in intergenerational mobility in family income and educational attainment using Dutch administrative data. I disentangle place effects from sorting of families by exploiting variation across children's ages at the time their parents move to another region, as also done by Chetty and Hendren (2018a). I extend on their design by testing and controlling for age-varying targeted migration of families, based on children's pre-move track choices.

I find that the place where a child grows up does not affect their socioeconomic outcomes (i.e., income and educational attainment) observed directly after childhood. However, a place effect is present for educational attainment observed during childhood: children who move to regions with a higher average probability of following a high track in secondary education, are themselves more likely to follow such tracks, than children who move at older ages.

Furthermore, my results suggest that controlling for past-track decisions in the *movers-exposure design* as developed by Chetty and Hendren (2018a) might be important, especially in a country employing a track-based educational system.[27] I provide evidence on age-variant selective mobility: families with children who chose higher secondary education tracks *before* they move, systematically sort themselves into higher upward education mobility regions after age 14. Conditional upon this track-based sorting, I find that moving to a better area at a young age (in terms of upward education mobility) does not generate larger long-term gains, as compared to individuals who move at later ages.

[27] Examples of countries, next to the Netherlands, employing a track-based educational system include: Germany, France, Sweden, Italy, Greece, Czech Republic, Slovak Republic, and Hungary (Hanushek and Wößmann, 2006).

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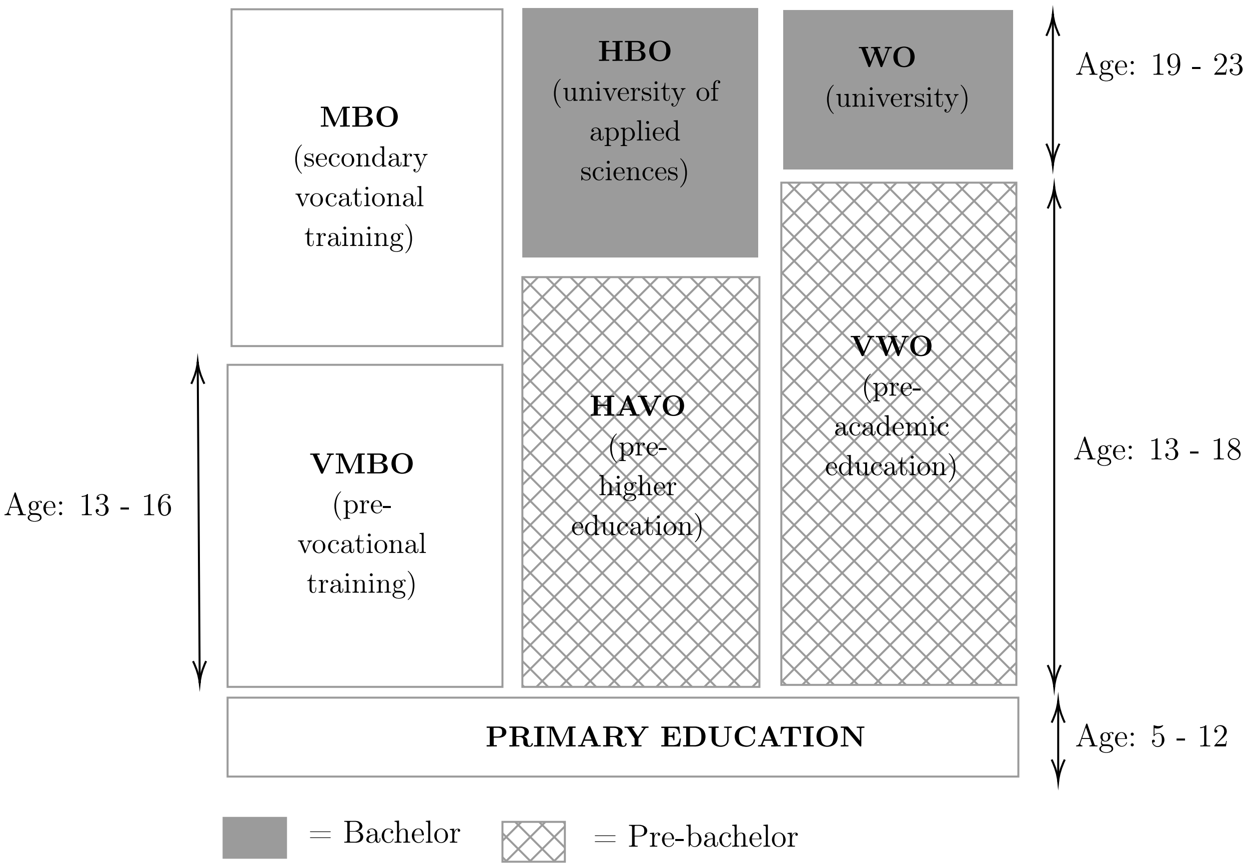
Appendices

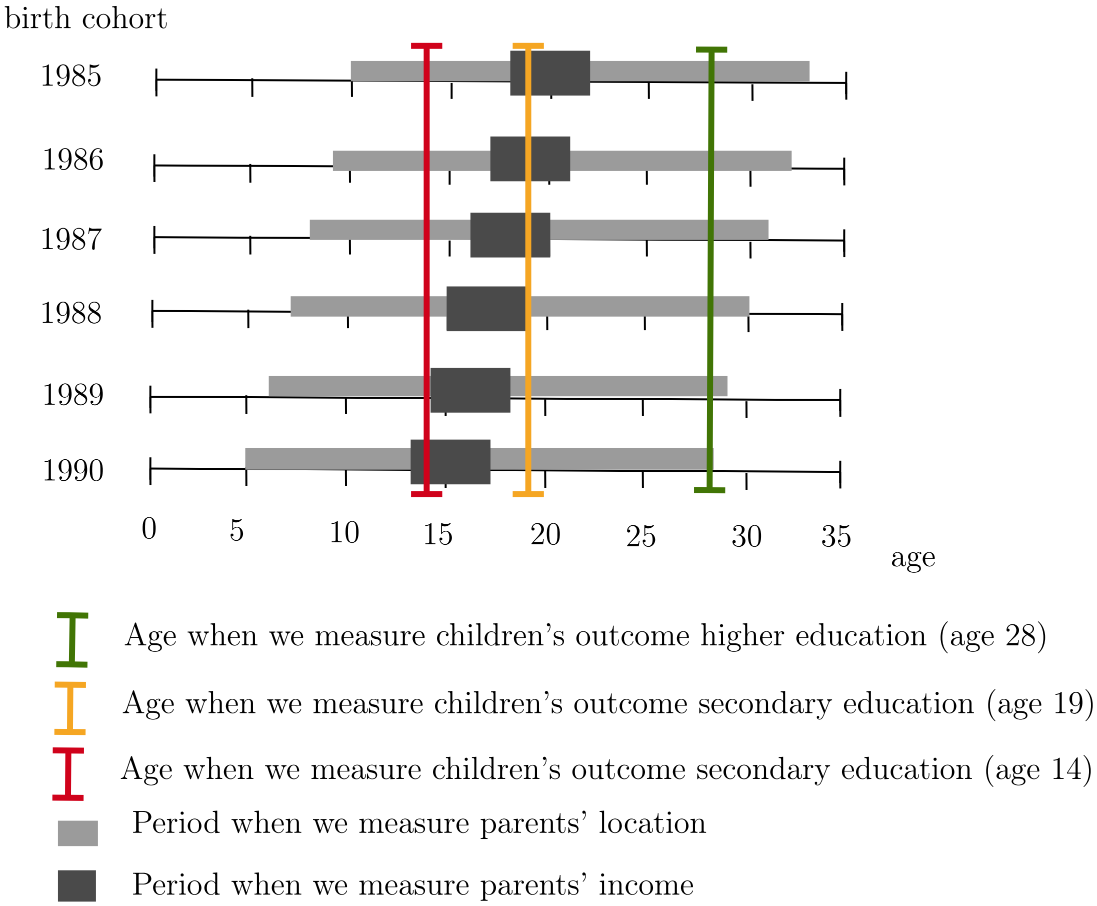
A Additional figures

Figure A.1: Overview (Higher) Education in the Netherlands



Notes. This figure provides a broad overview of the education system in the Netherlands. It is mandatory for children aged between 5 and 16 years to go to school. This means that all children who are living in the Netherlands need to follow primary education, and (part of) pre-vocational training, pre-higher education, or pre-academic education. After primary education, children take a test and get advice from their teacher which of the three general tracks in secondary education to take. Notice that the duration of this secondary education tracks are different: pre-vocational training (*Dutch:* VMBO) has a minimum duration of 4 years, pre-higher education (*Dutch:* HAVO) has a minimum duration of 5 years, and pre-academic education (*Dutch:* VWO) has a minimum duration of 6 years. After secondary education, there exist three types of tertiary education, namely (i) secondary vocational training (*Dutch:* MBO), (ii) education followed at a university of applied sciences (*Dutch:* HBO), and (iii) education followed at a university (*Dutch:* WO). Any education offered by a university of applied sciences or a university is known as higher education. In general, it takes a minimum of four years to obtain a bachelor degree at a university of applied sciences. Next to that, it takes a minimum of three years to obtain a bachelor degree at a university. In order to access higher education, it is necessary to have obtained a degree obtained by having followed pre-higher education, or pre-academic education. In order to access secondary vocational training, it is necessary to either (i) have proof that you have completed the first three years of pre-higher or pre-academic education, (ii) have a degree pre-vocational training, or (iii) have another degree that is acknowledged by the Dutch government. Furthermore, after age 16, it is partly mandatory to go to school. That is, individuals aged between 16 and 23 years old, who do not have a secondary vocational training degree, or no pre-higher education/pre-academic education degree, are obliged to go to school.

Figure A.2: Sample Overview

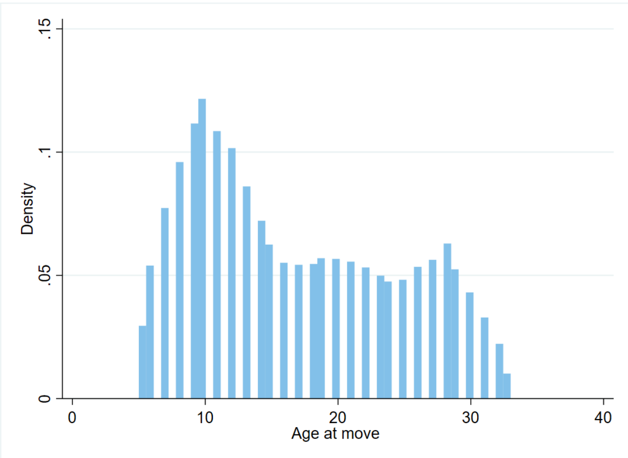


Notes. This figure presents an overview of the sample I am using in this paper. I select six different birth cohorts (1985 − 1990), and I am only able to measure income from 2003 onwards, as represented by the dark grey area. I measure parents' location from year 1995 onwards, as represented by the light grey area. I measure a child's outcome on income in the year that they turn 28 (the green line), and a child's education level at three points in time: the year they turn 14 (the red line), the year they turn 19 (the orange line), and the year they turn 28 (the green line).

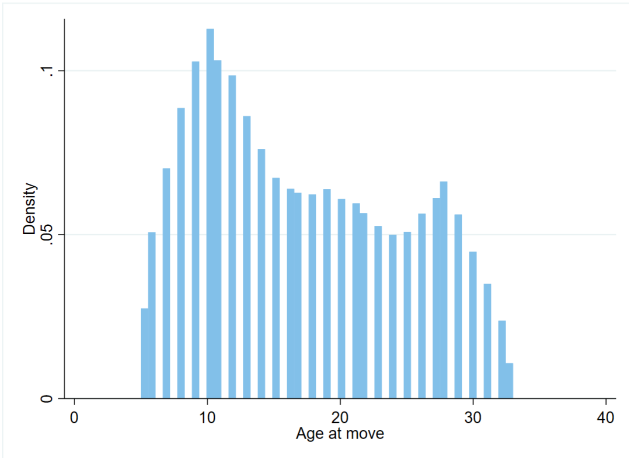
B Additional Tables

Figure A.3: Move frequencies

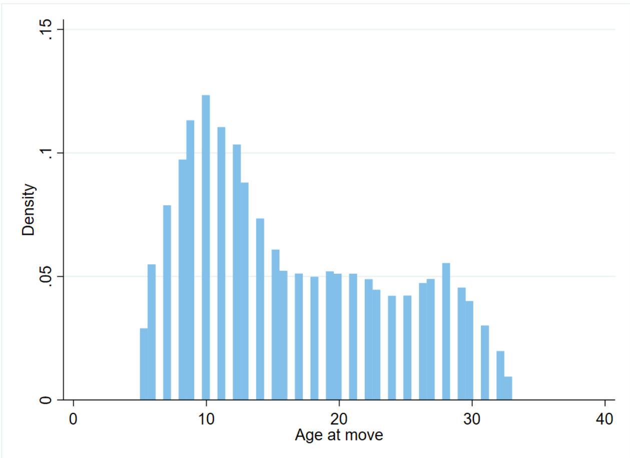
(a) COROP



(b) Municipality



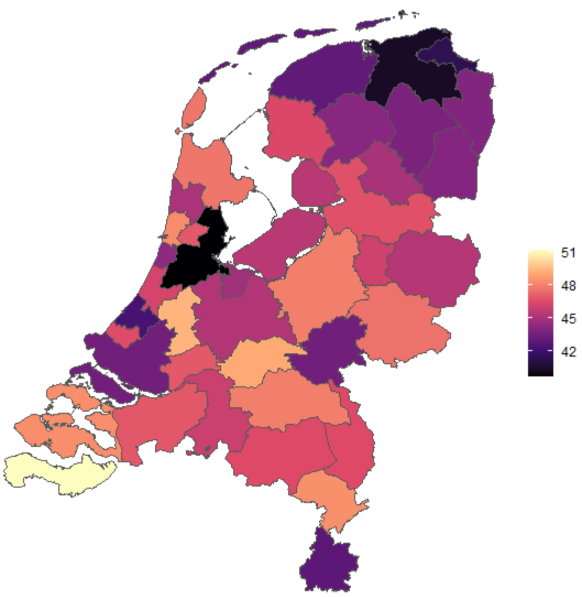
(c) Province



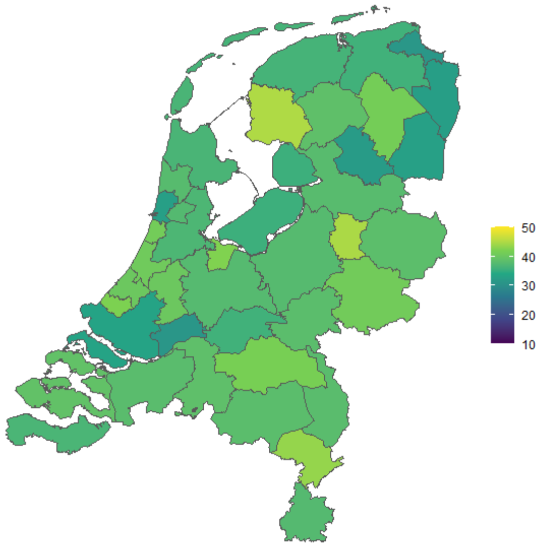
Notes. These histograms present the frequency of children's age at the time their families moves to another region. The sample on which these histograms are based includes all children whose parents moved to another region exactly once throughout the period 1995 – 2018. Panel A shows the distribution of ages at move when the geographical unit of regions is defined at the COROP level, as used in the main analysis of this paper. Panel B and Panel C show the distribution of ages at move when this unit is defined at the municipality and province level, respectively.

Figure A.4: Predicted Outcomes for Permanent Residents at 25th percentile

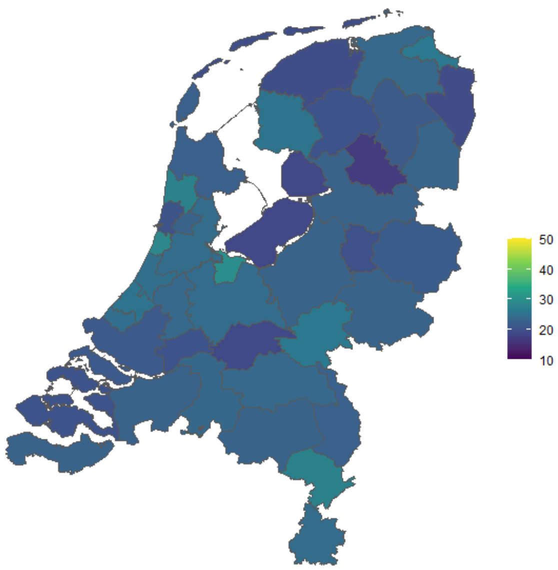
(a) Household Income Age 28 ($p = 25$)



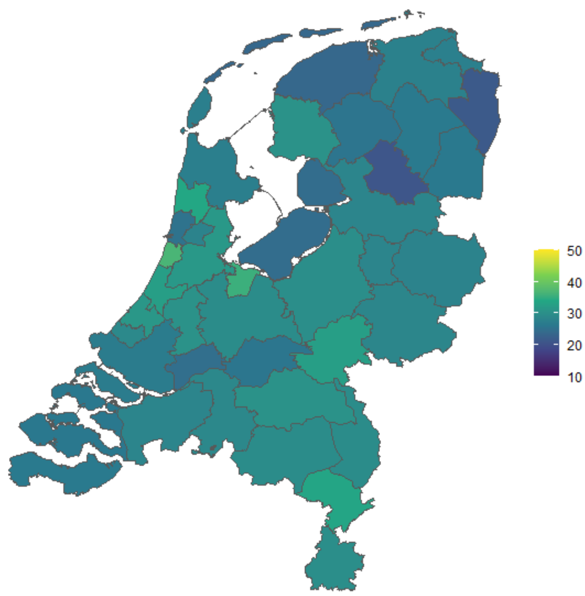
(b) Bachelor Degree Age 28 ($p = 25$)



(c) Pre-Bachelor Degree Age 19 ($p = 25$)



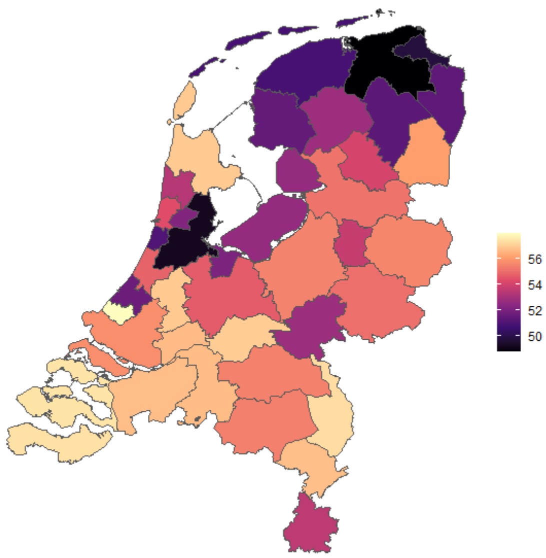
(d) Pre-Bachelor Track Age 14 ($p = 25$)



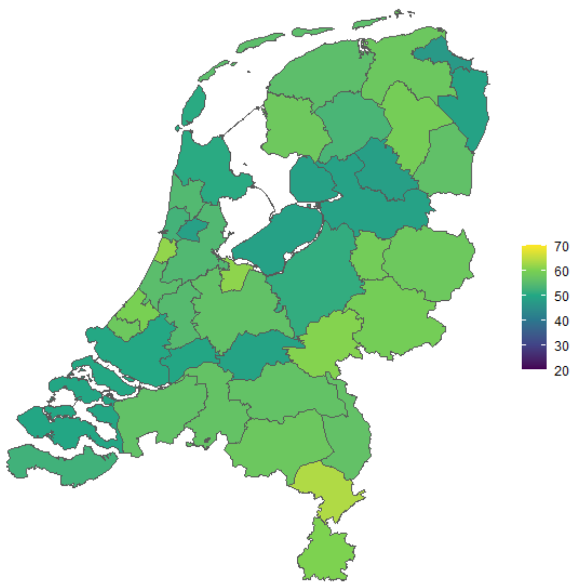
Notes. This figure maps the predicted outcomes for children's household income rank (Panel A) and children's probability of having a bachelor degree (or higher) at age 28 (Panel B), at the median of the parental household income distribution for all parents whose children are born in 1985. Similarly, Panel C and Panel D map children's predicted educational attainment at ages 19 and 14. I construct all of these predicted outcomes by estimating equation (3) using OLS, and setting $p = 25$. Darker (blue) colors represent areas with lower predicted outcomes, and lighter (green) colors represent areas with higher predicted outcomes. The estimation sample includes all children in the 1985 birth cohort whose parents did not move across different COROP regions between 1995 and 2018 (i.e., permanent residents). I use the resulting estimated coefficients to construct predicted values for children whose parents' have $p = 50$. For more details on the construction of these variables, see Section 2.2.

Figure A.5: Predicted Outcomes for Permanent Residents at 75th percentile

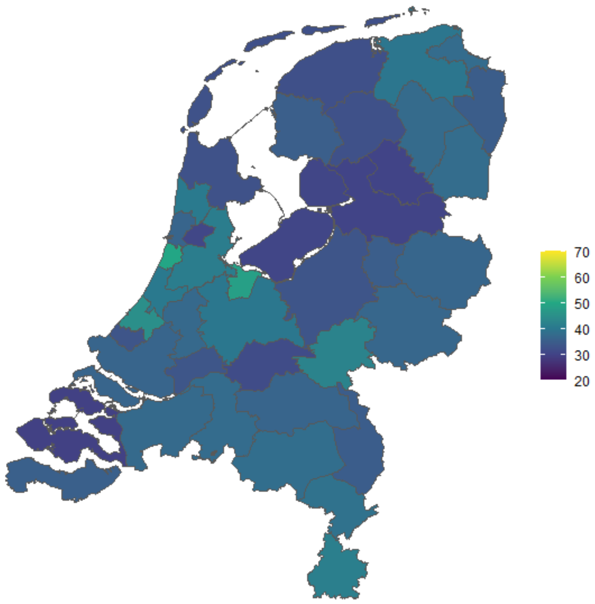
(a) Household Income Age 28 ($p = 75$)



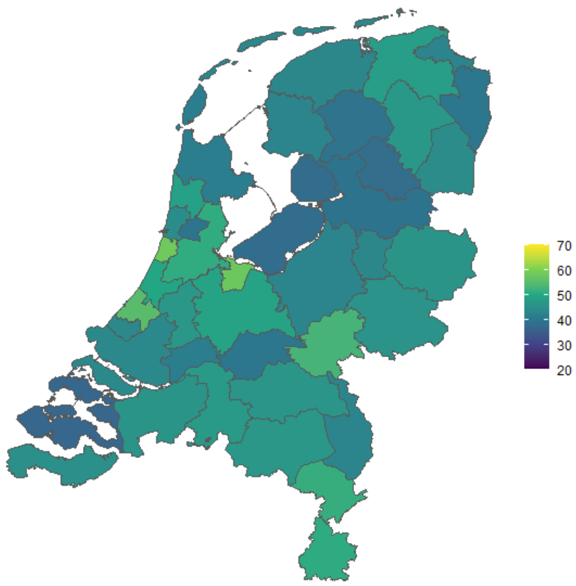
(b) Bachelor Degree Age 28 ($p = 75$)



(c) Pre-Bachelor Degree Age 19 ($p = 75$)

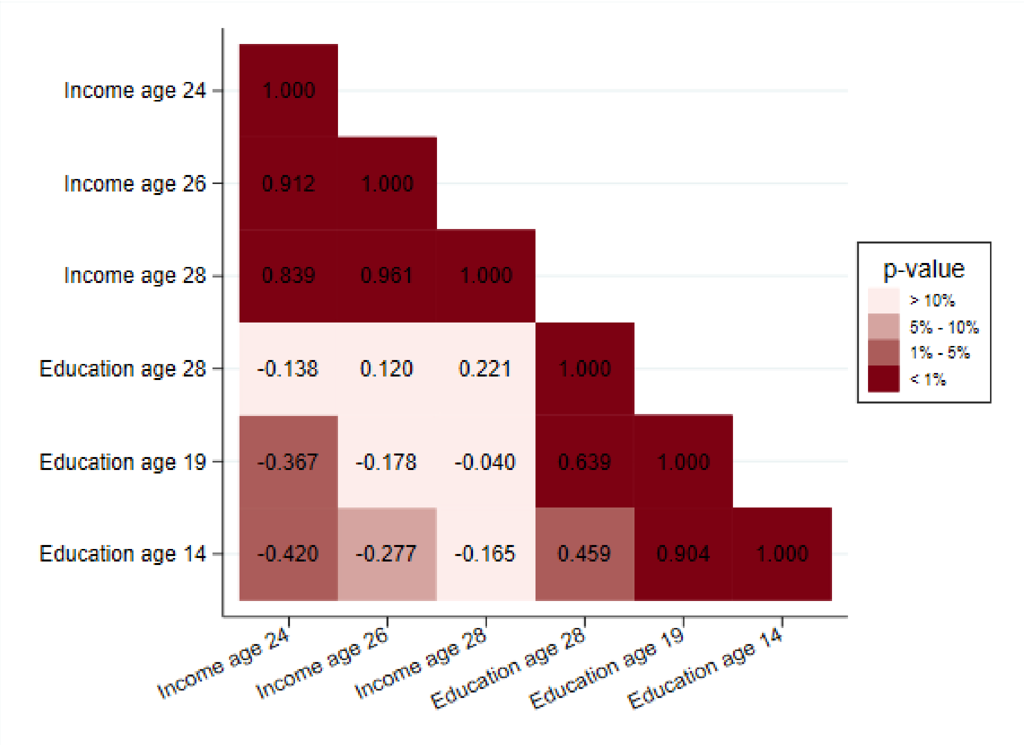


(d) Pre-Bachelor Track Age 14 ($p = 75$)



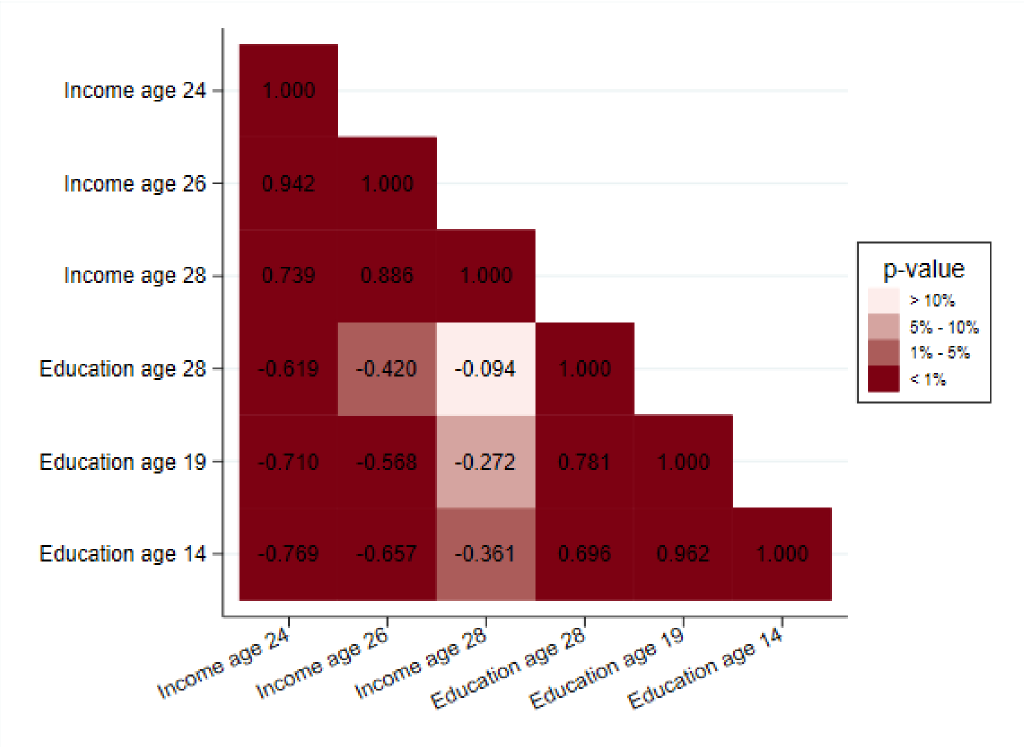
Notes. This figure maps the predicted outcomes for children's household income rank (Panel A) and children's probability of having a bachelor degree (or higher) at age 28 (Panel B), at the median of the parental household income distribution for all parents whose children are born in 1985. Similarly, Panel C and Panel D map children's predicted educational attainment at ages 19 and 14. I construct all of these predicted outcomes by estimating equation (3) using OLS, and setting $p = 75$. Darker (blue) colors represent areas with lower predicted outcomes, and lighter (green) colors represent areas with higher predicted outcomes. The estimation sample includes all children in the 1985 birth cohort whose parents did not move across different COROP regions between 1995 and 2018 (i.e., permanent residents). I use the resulting estimated coefficients to construct predicted values for children whose parents' have $p = 50$. For more details on the construction of these variables, see Section 2.2.

Figure A.6: Regional correlation in Predicted Outcomes at 25th Percentile Parental Endowment



Notes. This figure shows the estimated regional correlation between combinations of intergenerational mobility measures, at the 25th percentile of the national parental household income distribution for all parents whose children are born in 1985. This figure considers the linear relationship (3) between parents' household income rank and children's family income at age 28, educational attainment at ages 14, 19, and 28 as measures for intergenerational mobility, as also presented in Figure 2 for every COROP region. Every cell presents the estimated correlation coefficient between two measures of intergenerational mobility across regions. Dark red cells correspond to combinations of mobility measures for which the null hypothesis that the correlation between two mobility measures is equal to zero, is rejected at the 1% significance level.

Figure A.7: Regional correlation in Predicted Outcomes at 75th Percentile Parental Endowment

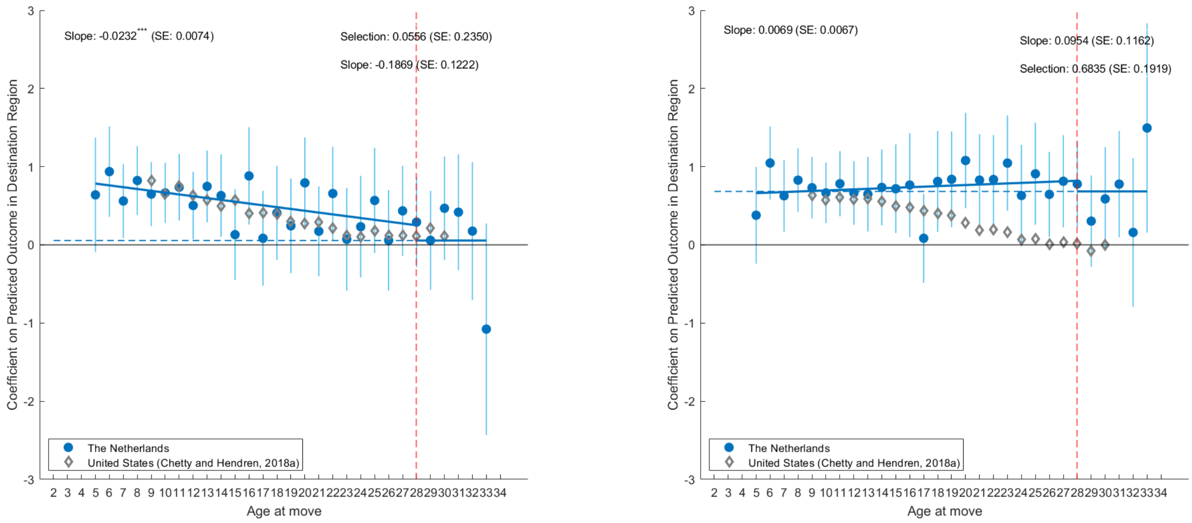


Notes. This figure shows the estimated regional correlation between combinations of intergenerational mobility measures, at the 25th percentile of the national parental household income distribution for all parents whose children are born in 1985. This figure considers the linear relationship (3) between parents' household income rank and children's family income at age 28, educational attainment at ages 14, 19, and 28 as measures for intergenerational mobility, as also presented in Figure 2 for every COROP region. Every cell presents the estimated correlation coefficient between two measures of intergenerational mobility across regions. Dark red cells correspond to combinations of mobility measures for which the null hypothesis that the correlation between two mobility measures is equal to zero, is rejected at the 1% significance level.

Figure A.8: Estimated Effects on Earnings and Educational Attainment

(a) Household Income Age 28

(b) Having a Bachelor Degree Age 28



Notes. These figures plot the coefficient estimates $\{\Gamma_m\}$ versus the child's age at move m using the model specification presented in (9), for different outcomes O and ages A at time of measurement of the outcome variable (blue dots). The estimation sample includes all children whose parents have moved exactly once throughout the period 1995−2018. The best-fit lines are estimated by a simple OLS regression of the coefficients $\{\Gamma_m\}$ on m for two different samples: $m < A$ and $m \geq A$. The slope coefficient for $m < A$ and $m \geq A$ are presented, as well as the average selection effect for $m \geq A$. The vertical red dashed line presents the age at which the outcome variable is measured, and therefore visualizes this split for estimation of the best-fit line for different samples. Under validity of constant selection effects across age of move (Assumption 1), the average selection constant measured for $m \geq A$ can be extrapolated to earlier ages $m < A$, as presented by the dashed horizontal blue lines. 95% confidence intervals are presented, using robust standard errors.

Table B.1: Fraction of Permanent vs. Non-permanent Residents

	Num. of obs.	Percentage
Panel A: Municipality level		
Permanent residents	781,049	78.4%
One-time movers	152,601	15.3%
Moved more than once	61,974	6.3%
Panel B: COROP level		
Permanent residents	876,742	88.1%
One-time movers	101,144	10.2%
Moved more than once	17,738	1.7%
Panel C: Province level		
Permanent residents	919,202	92.3%
One-time movers	76,422	7.7%

Notes. This table shows the percentage of individuals who are considered permanent residents, one-time movers, or individuals who moved more than one time. In Panel A, individuals are classified into one of the three groups based on their parents' location information at the municipality level. For Panel B (Panel C), this classification happens at the COROP (province) level. Individuals are considered to be permanent residents in case their parents' have stayed within the same location (i.e., municipality, COROP region, or province) throughout the entire period $1995 - 2018$. Individuals are considered to be one-time movers in case their parents have moved to another location (i.e., municipality, COROP region, or province) exactly once throughout the period $1995 - 2018$. According to the 2021 definitions, there are 352 municipalities, 40 COROP regions, and 12 provinces in the Netherlands.

Table B.2: Regional distribution of expected outcomes of children with $p = 25$

Panel A: Household Income Age 24, $p = 25$	
Mean region	50.2 (US: 43.9, Australia: 45.3)
p10 region	47.0 (US: 37.3, Australia: 42.4)
p90 region	53.3 (US: 52.0, Australia: 50.4)
Panel B: Household Income Age 28, $p = 25$	
Mean region	45.6
p10 region	42.6
p90 region	48.3
Panel C: Household Income Age 26, $p = 25$	
Mean region	47.9
p10 region	44.9
p90 region	50.7
Panel D: Educational Attainment Age 14, $p = 25$	
Mean region	28.4
p10 region	24.5
p90 region	32.9
Panel E: Educational Attainment Age 19, $p = 25$	
Mean region	23.0
p10 region	19.4
p90 region	26.8
Panel F: Educational Attainment Age 28, $p = 25$	
Mean region	37.8
p10 region	32.4
p90 region	42.2

Notes. This table presents statistics on the regional variation (based on COROP regions) of the expected outcomes in both earnings and educational attainment, measured at different ages. Statistics are calculated for children whose parents are at the 25th national household income percentile rank. The sample used to construct this table is based on permanent residents only. For children's household income measured at age 24 (Panel A), age 26 (Panel C), and age 28 (Panel B)) are presented, together with comparable statistics for Australia (Deutscher, 2020) and the United States (Chetty et al., 2014) are presented. For more information on these statistics, I refer to Table 2 in Deutscher (2020).

Table B.3: Estimated Income Exposure Effects

	Dependent variable: Child's income rank at age A						
	General model specification		Parametric model specification				
	10 income categories $A = 28$	5 income categories $A = 28$	Household income (marital status) $A = 28$	Personal income $A = 28$	Corrected family income $A = 28$	Household income $A = 24$	Household income $A = 26$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Yearly exposure effect							
Slope $m < A$	-0.0207^{**}	-0.0232^{***}	-0.0235^{***}	-0.0174^{***}	-0.0263^{***}	-0.0186^{***}	-0.0278^{***}
	(0.0076)	(0.0074)	(0.0039)	(0.0058)	(0.0053)	(0.0045)	(0.0043)
Panel B: Average selection effect							
Intercept $m \geq A$	0.0992	0.0557	0.3952^{***}	0.3866^{*}	0.2169^{**}	0.6928^{***}	0.3503^{**}
	(0.2783)	(0.2350)	(0.0664)	(0.1815)	(0.0808)	(0.0645)	(0.1084)
Panel C: Slope selection effect							
Slope $m \geq A$	-0.2642	-0.1869	-0.0005	0.1332	0.0112	-0.0201	0.0821^{*}
	(0.1255)	(0.1222)	(0.0435)	(0.0984)	(0.0526)	(0.0227)	(0.0385)
Num. of obs.	101,201	101,201	101,201	101,201	101,201	101,201	101,201

Notes. This table presents the estimated income exposure effect, using the estimated coefficients $\{\Gamma_m^{A,I}\}$, for age at move $m = 5, \dots, 33$, for different model specifications and outcomes. The estimation sample includes all children whose parents moved exactly to another COROP region between 1995 and 2018 (i.e., one-time movers). A denotes the age at which the child's income (i.e., the dependent variable) is measured. The estimated exposure effects denoted in Panel A are constructed using an OLS regression of the estimated coefficients $\{\Gamma_m\}$ on age at move m for $m < A$, and reporting the resulting slope of this regression. This slope coefficient then represents the magnitude of the yearly childhood exposure effect of a region's income mobility on children's income rank at age A. Panel C presents the resulting slope of a similar OLS regression of the regression coefficients $\{\Gamma_m\}$ on m for $m \geq A$. Panel B presents the average selection effect, which is defined as the mean value of the estimated exposure effects $\{\Gamma_m\}$ for $m \geq A$. Columns 1 and 2 are constructed using the estimated coefficients from the most general model specification presented in equation (9), using 10 and 5 income categories to construct the fixed effects, respectively. Columns 3 till 7 are estimated using the parametric model specification as presented in equation (11), using different income outcomes. Column 3 considers a child's household income based on their marital status only, to best replicate the results obtained by Chetty and Hendren (2018a). Column 4 presents the results using a child's personal income. Column 5 uses an individual's corrected household income, which divides the child's household income by 2 in case this individual is cohabiting with their partner, and by one otherwise.