From Parents' Cradle to Children's Career: Intergenerational Effects of Parental Investments

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Abstract

There is a clear consensus that childhood experiences shape adult success, yet there is limited understanding of their impact on future generations. We proxy parental investments during childhood with birth order and study whether disadvantages due to lower investments are transmitted to future generations. Birth order effects on the first generation are large, apply to 80% of the population, and can be identified with relatively mild assumptions. Using cousin comparisons in Dutch administrative data, we find that around 20 percent of the income disadvantages are transmitted. Additionally, we find sizeable decreases in children's education and increases in boys' criminal behavior.

Keywords: intergenerational mobility, birth order, extended family, education, crime

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1 Introduction

Although numerous studies investigate how childhood experiences shape adult success, there is limited understanding of their impact on future generations. This is an important gap to fill for two reasons. First, causal intergenerational effects provide valuable insights into the drivers of intergenerational mobility (Black and Devereux (2011)). Second, intergenerational spillovers of childhood experiences can have important consequences for policymakers. If childhood experiences affect not only the first but also subsequent generations, then conventional estimates of the returns to childhood interventions may be considerably underestimated (Bennhoff et al. (2024)).

Aided by improved data availability, a small but growing literature studies the intergenerational consequences of childhood experiences. Earlier studies mostly focused on education (Currie and Moretti (2003), Black et al. (2005a), Oreopoulos et al. (2006)), while a more recent literature includes the effects of other childhood experiences like prenatal exposure to pollution, access to health insurance before birth, increased tuberculosis testing and vaccination during school, preschool enrollment, and parental access to disability insurance during childhood (Black et al. (2019); Bütikofer and Salvanes (2020); East et al. (2023); Rossin-Slater and Wüst (2020); Barr and Gibbs (2022); García et al. (2023); Dahl and Gielen (2024)).[1] This literature advances our understanding of which types of childhood experiences can generate long-lasting change.

The main contribution of this paper is to document the intergenerational effects of a very different type of childhood experience: birth order. Birth order is a unique 'treatment' affecting multiple important dimensions of human capital. Children with a higher birth order have considerably less education, a lower IQ, unfavorable personality traits and leadership quali-

[1] For other papers studying education see also Maurin and McNally (2008), McCrary and Royer (2011), Pekkarinen et al. (2009), Holmlund et al. (2011), De Haan (2011), Carneiro et al. (2013), Chevalier et al. (2013), Lundborg et al. (2014), Sikhova (2023), Barrios Fernández et al. (2024), and Akgündüz et al. (2024). The discussion above focuses on Western countries. There are a few studies in non-western countries, such as the intergenerational effects of school construction (Mazumder et al. (2023), Akresh et al. (2023b)), deworming (Walker et al. (2023)), and war exposure (Akresh et al. (2023a)).

ties, higher crime rates, and lower subsequent earnings (Black et al. (2005b); Kantarevic and Mechoulan (2006); Black et al. (2011); Black et al. (2018); Breining et al. (2020),Houmark (2023)).[2] These effects come from sibling comparisons and can thus not be attributed to factors shared by siblings such as schools, neighborhoods, or genes. The prevailing hypothesis suggests that they stem from differences in parenting, based on compelling evidence indicating that birth order effects are driven by larger parental investments or higher levels of stringency toward earlier-born children (Price (2008), Averett et al. (2011), Hotz and Pantano (2015), Pavan (2016), Black et al. (2018), Lehmann et al. (2018)).[3] Consequently, our findings may offer particularly important insights into the potential for parental investments to make long-lasting intergenerational impacts.

Additionally, the prior literature studying causal intergenerational effects focuses predominantly on specific groups, such as disadvantaged children or those complying with particular reforms, and the outcomes of the second generation are often measured at young ages. This makes it unclear whether these childhood experiences truly 'break the cycle' of income persistence and the findings are more difficult to generalize. A second key contribution of this paper is to accurately quantify the degree of intergenerational transmission due to birth order (dis)advantages for an exceptionally large share of the population. This is possible because birth order effects apply to roughly 80 percent of the population (everyone with at least one sibling) and because our data includes long-run income information for over 3 million Dutch individuals *and* their children.

Following the literature we use sibling comparisons to estimate birth order effects on the first generation. The main identifying assumption underlying this approach is that parents do not consider the quality of their existing children when deciding to have another child.

[2] In terms of magnitude, birth order effects on education are similar to the effects of compulsory schooling laws often examined to estimate the causal effect of parental education (Black et al. (2005a), Oreopoulos et al. (2006), Holmlund et al. (2011)).

[3] Two alternative theories suggest that birth order effects may be driven by (i) older siblings learning from being a role model or (ii) younger siblings being exposed longer to changes in family structure such as parental divorce. Contrary to (i), multiple papers (see Section 3) find no effects of having siblings or having more siblings, suggesting that role modeling is of limited importance in siblings' development. As for (ii), Black et al. (2005b) exclude families with such disruptions and find similar estimates.

This assumption is supported by Domingue et al. (2015), Muslimova et al. (2020), and Isungset et al. (2022), who show that children of different birth orders do not systematically differ in their polygenic score for education.[4] Given this assumption, siblings' genes are randomly drawn from the same gene pool, making their birth order unrelated to their initial endowments. To estimate *parental* birth order effects we follow the same framework, but replace the outcomes of siblings with those of their children.[5]

Our main finding is that around 20 percent of the long-run income disadvantages due to a higher birth order are transmitted to the next generation. For example, the income of third-born parents is 3 ranks lower than that of first-born parents, whereas their children's income is 0.6 ranks lower than that of children from first-born parents. While the absolute effects increase with each successive parental birth order, the degree of intergenerational transmission is centered at 20 percent across different birth orders and family sizes. A higher parental birth order also reduces educational attainment and increases boys' criminal behavior, particularly violent crime, consistent with declines in both cognitive and non-cognitive skills.

Our setting allows us to consider two additional open questions. First, are paternal and maternal advantages transmitted similarly? We find slightly larger (but not significantly different) paternal birth order effects, which is consistent with birth order effects having a larger impact on fathers. In relative terms, fathers transmit as much of their income disadvantage as mothers. Second, are there intergenerational complementarities in early life advantages? [6] We show that children benefit from a lower own birth order and a lower parental birth order, but these effects do not interact, providing no support for the existence of intergenerational complementarities.

[4]A polygenic score aggregates the effects of many genetic variants to estimate an individual's genetic predisposition for a certain trait.

[5]Some additional complexities arise when estimating intergenerational effects, such as selective fertility and assortative mating. We discuss these in Section 3.

[6]Dettmer et al. (forthcoming) estimate the intergenerational complementarity of early life advantage with Rhesus monkeys. To our knowledge, we are the first to estimate an intergenerational complementarity with humans.

We also explore potential mechanisms. While birth order does influence fertility decisions and neighborhood choices, we find that these channels explain only a small share. Birth order also affects partner selection, potentially introducing genetic differences between cousins. However, prior research indicates that birth order effects on spouses' genetic propensity for education are modest (Abdellaoui et al. (2022)). Based on this evidence, we argue that genetic differences are also unlikely to be a major driver of the intergenerational effects.

We are not the first to study the intergenerational effects of birth order. Havari and Savegnago (2022) and Barclay et al. (2021) study parental birth order effects on children's education. We complement their results by (i) considering income and crime, (ii) accurately quantifying the degree of intergenerational income transmission, (iii) zooming in on gender differences and intergenerational complementarities, and (iv) considering the roles of neighborhoods, fertility, and assortative mating. Also, although Barclay et al. also use cousin comparisons, their specifications do not fully control for parents' year of birth. As a result, it is unclear whether their findings are driven by parental birth order effects or differences between children whose parents are born in different years or have children at different ages.[7] To avoid this issue, we flexibly control for a parent's year and month of birth.

Finally, our design is also closely related to papers that compare cousins whose parents are monozygotic twins to study the intergenerational transmission of schooling (Behrman and Rosenzweig (2002), Antonovics and Goldberger (2005), Holmlund et al. (2011), Pronzato (2012)). An advantage of our approach is that by focusing on the intergenerational transmission of birth order effects, we can study intergenerational transmission in a more isolated setting than in twin designs, where the origins of the differences between the twin-parents' schooling are unobserved.

[7] Barclay et al. group the parents' year of birth into bins of 5 years (Table S3). When they do not control for the children's year of birth, they compare children born in different years, and when they do, they compare children whose parents are of different ages. Moreover, their preferred specification includes several after-treatment variables which can induce a bias. Havari et al. use multi-country survey data and control for a range of factors, including parent's family size and year of birth.

2 Data

Sample. We use administrative data from Statistics Netherlands covering the full population. We select all individuals born in the Netherlands between 1945 and 1970, dropping families with migrants (5.7%), at least one missing birth date (8.1%), twins (5.6%), only one child (9%) or six or more children (17%).[8] We call the remaining individuals the first generation; this sample includes 64 percent of all individuals born between 1945 and 1975. We establish birth order by ranking all individuals with the same mother and father by their birth dates, so our analysis focuses solely on birth order among full siblings. Next, we link the first generation with their children.[9] We refer to the children of the first generation as the second generation. In the core analysis, we focus on children born before 1991.

Income. The income register records the gross personal income extracted from tax statements spanning the period between 2003 and 2023, which encompasses all income from employment, entrepreneurship, income insurance payments, and social security benefits. We measure income in 2024 euros, adjusting for inflation using the consumer price index. We define household income as the total income of all household members.[10] Household income provides a reliable measure of economic resources even in the case of non-participation in the labor market and is commonly used in other intergenerational mobility studies (Chadwick and Solon (2002)). We have household income records for 97.1% of the second generation sample and 97.5% of the first generation sample.

A well-known challenge is that income snapshots are prone to measurement error due to transitory income shocks (Mazumder (2005)) and life-cycle bias arising from heterogeneous age-income profiles (Haider and Solon (2006)). We use income only as an outcome, so our

[8] We drop migrants because family links are poorly observed and because these families often arrive simultaneously, creating a correlation between birth order and age at arrival of siblings. Single-child families are not used for identification; we drop larger families for conciseness of our results.

[9] We rely on legal relationships between parents and children. Consequently, the identified parent is not necessarily the biological parent, but rather one who has most likely raised the children. Parents do not need to be together for the (full) period in which they raise their children.

[10] If children still live with their parents the parents' household income is defined as the joint parental income, and the child's household income as their personal income.

main concern is life-cycle bias, as opposed to attenuation bias due to (classical) transitory income shocks. We aim to mitigate such bias by calculating the average income over the four years that are nearest to the age of 35, and within the age range of 30 to 60 (Nybom and Stuhler (2017)).[11] We next define individuals' income ranks based on their positions in the distribution of long-run household income in their respective cohorts. To investigate the sensitivity of our results to these choices of measure and ages we also present results for various alternative measures.

Education and crime. We construct an indicator for a higher education degree based on the education register that contains tertiary education degrees since 1986. The primary crime outcome is an indicator of whether a child has been suspected of any crime at ages 18 to 20, which are the prime ages at which individuals commit crimes in the Netherlands. We distinguish between property crime, violent crime, and other types of crime based on the reported offense types and restrict the analysis to boys only. As the crime outcomes are available for a limited period, we restrict the crime sample to children born between 1986 and 2001.

Table 1 presents descriptive statistics for the parents and the children separated by the parent's birth order. Panel A shows descriptives for the fist-generation sample with non-missing incomes. Panel B includes all their children born before 1991, which are the children we will use for the income and education analyses.[12] Many children in the analysis sample occur once for each parent, and thus twice in the dataset.[13]

The first generation's outcomes show that individuals with a higher birth order on average have a lower income.[14] From the children's outcomes in panel B we observe that children of higher-birth-order parents have lower income and are less often enrolled in higher education.

[11]We observe parental incomes around ages 45 to 60, and for children around ages 30 to 45. Only 1.6 percent of the children and 2.6 percent of the parents have less than four income observations.

[12]For the crime analysis we rely on a different sample of children born between 1986 and 2001. Summary statistics for crime can be found in Table A6.

[13]We explain this in greater detail in Section 3.

[14]The average income percentile differs from 50 because we also include dropped individuals such as migrants when calculating it.

However, these are just correlations. We next explain how we can identify causal (parental) birth order effects.

3 Identification

Our identification strategy relies on within-family variation in birth order. To estimate birth order effects for the first generation, we estimate the following Sibling Fixed Effects (SFE) model

$$Y_{pf} = \alpha_f + \sum_{k=2}^{5} \beta_k^{FG} I[BO_p = k] + \tau_{t(pf)} + \epsilon_{pf}, \tag{1}$$

where Y_{pf} is the outcome of a child p in a family f, α_f are family fixed effects, $I[BO_p = k]$ is an indicator that equals 1 if the birth order of child p equals k, and $\tau_{t(pf)}$ are year of birth $\times$ month of birth $\times$ gender $\times$ family size fixed effects. The family fixed effects ensure that we only compare siblings. By including $\tau_{t(pf)}$, we flexibly control for different trends in the outcome between cohorts by gender and family size. We model the fixed effects by year and month to ensure that even when two siblings are born in the same year (but are not twins), their difference in birth timing is controlled for. The coefficients β_k^{FG} capture the birth order effects on the first generation.

Although the family fixed effects rule out confounders that differ between families, there can still be within-family confounders. In particular, birth order effects can arise mechanically if parents' fertility decisions are related to the quality of their earlier children. When parents stop having children after having a particularly 'bad draw', then birth order effects are the result of the last child being negatively selected. Domingue et al. (2015), Muslimova et al. (2020), and Isungset et al. (2022) find that children of different birth orders do not structurally differ in their polygenic score for education. This suggests that, at least genetically, children of different birth orders are of similar 'quality'.[15] Moreover, in our main results, we

[15]Generally, it can be hard to infer an earlier child's 'quality' at the time that parents decide about another

also compare first and second-born children in families of three or more, who should not be impacted by such an optimal stopping rule. Our results are virtually unchanged for these comparisons.

By construction, birth order effects also capture the effect of having older or younger siblings. For example, in a family of size two, the effect of being born second includes the effect of having an older sibling. As a result, birth order effects may arise from spillovers between siblings that are correlated with birth order. However, most studies find that if there are spillovers, these are typically in the same direction as the direct effect (e.g. Dahl et al. (2014), Nicoletti et al. (2018), Bharadwaj et al. (2022)). This is contrary to birth order patterns, which are a measure of siblings' differences rather than similarities.[16] Second, multiple papers show that the effect of having a sibling or having more siblings does not affect children's outcomes (Black et al. (2005b), Angrist et al. (2010), Ilciukas et al. (2025)) This suggests that sibling spillovers are of limited importance in children's development.

To estimate our effect of interest, the intergenerational effect of birth order, we replace the outcomes of children p with the outcomes of their children, indexed by cp. This results in the following Cousin Fixed Effects (CFE) model

$$Y_{cpf} = \alpha_f + \sum_{k=2}^{5} \beta_k^{SG} I[BO_p = k] + \tau_{t(pf)} + \epsilon_{cpf}, \tag{2}$$

where Y_{cpf} is the outcome of child c of parent p in extended family f, α_f are extended family fixed effects, $I[BO_p = k]$ is an indicator that equals 1 if the birth order of parent p equals k, and $\tau_{t(pf)}$ are parent's year of birth $\times$ month of birth $\times$ gender $\times$ family size fixed effects. The extended family fixed effects ensure that we only compare *the children* of siblings. These children are cousins, but only from one side of the family. The regression are weighted by the number of children so that all parents receive equal weight.

child. One of the few signals parents have at this early stage is children's health. However, unlike most outcomes later in life, firstborn children tend to have worse health outcomes relative to later-born siblings during their first years (Brenøe and Molitor (2018)). This contradicts the idea of an optimal stopping rule.

[16] Generally, it is difficult to think of the type of spillover that could result in the same pattern as birth order effects.

In our analysis, we compare children once to cousins from their mother's side and once to cousins from their father's side. Thus, some children occur twice in the dataset.[17] These results are averaged into a single treatment effect that captures both paternal and maternal birth order effects. We also explore heterogeneity in the effects by studying the effects for fathers and mothers separately.

Our empirical design captures not only the direct effects of treatment but also any indirect effects operating through assortative mating. Moreover, birth order may influence fertility behavior, including the timing and likelihood of having children. Because these channels could affect our results, we return to these issues in section 6.

4 Main Results

Table 2 displays the estimates of birth order effects on the income ranks of the first and second generation. Column 1 includes the full analysis samples; columns 2 to 5 present (parental) birth order effects separately by the first generation's family size.

Panel A shows that parental disadvantages due to their birth order transmit to their children. For example, column 1 shows that the income rank of children of a third-born parent is 0.5 ranks lower than their cousins of a first-born parent. The effects increase with each additional birth order of the parent. Columns 2 through 5 show consistent patterns across different extended family sizes. These results highlight that birth order effects have considerable intergenerational spillovers.

While the effects in panel A are interesting on their own, they are not informative about *how much* of the parental disadvantage is transmitted to their children. To shed light on this, we present the first-generation birth order effects on income in panel B. These estimates show that birth order has large effects on income.[18] Although there is limited prior evidence

[17]Some children are sampled only once because one of the parents does not meet the sample selection criteria.

[18]The sample in panel B includes all individuals from the first generation, including those without children born before 1991. Using the sample of parents with children in our core sample yields very similar estimates

on birth order effects on income, these results are in line with Black et al. (2005b), who report birth order effects on earnings using specifications with rich control variables. We also observe that the effects of being second or third-born are similar for all family sizes. This is inconsistent with optimal stopping models of fertility, which would suggest that only the last-born has particularly bad outcomes.

We use the estimates in panel B to obtain the degree of intergenerational transmission, which we compute as the ratio of intergenerational birth order effects (β^{SG}) to birth order effects on the first generation (β^{FG}). A transmission coefficient above (below) one indicates that the intergenerational birth order effects are greater (smaller) than those on the first generation. This ratio is reported in Panel C. The estimates in column 1 are around 0.18 and columns 2 to 4 show that the transmission estimates are relatively consistent across families of different sizes and various parental birth orders. Aggregating these estimates into one coefficient using 2SLS yields a transmission estimate equal to 0.18 (standard error 0.04). We conclude that on average about 20 percent of the income disadvantages due to a higher birth order are transmitted to the next generation.

For comparison, the rank-rank correlation between parental and child income is 0.25 in our sample. This implies that the persistence of income disadvantages due to birth order is slightly weaker than the overall persistence of income across generations.

Whether the transmission of income (dis)advantages due to birth order effects of 0.2 is considered high or low depends on one's priors. On the one hand, multiple papers show that treatments that affect parents' education or income do not always result in intergenerational spillovers, or result in considerably smaller spillovers than conventional intergenerational mobility estimates suggest (Holmlund et al. (2011), Page (Forthcoming)). In this light, the intergenerational spillovers of birth order are relatively large. On the other hand, these studies often focus on exogenous changes in education or income at a relatively old age. Given that birth order affects both cognitive and noncognitive skills already at young ages (Houmark

(Table A2).

(2023)), initial disadvantages might compound over time and result in relatively large effects for subsequent generations (Becker et al. (2018)). An extreme example comes from Barr and Gibbs (2022), who show that the intergenerational effects of attending preschool education even surpassed the initial impact on the subjects. This is clearly not the case for birth order.

Robustness. Table A3 shows that the degree of intergenerational transmission is very similar when we use the log of household income or personal income ranks. Table A4 shows that the estimates remain stable when varying the number of income observations. Figure A1 shows that birth order effects are similar when income is measured at any age between 33 and 60. At ages 30 to 32, however, the effects are slightly attenuated, suggesting that income measured this early may not accurately reflect lifetime income. In our baseline specification, these younger income observations are used only for the most recent cohorts (comprising 20 percent of the sample). Reassuringly, the results hardly change when we exclusively use income observations above age 32 (Table A4).[19]

Education and Crime Recognizing that intergenerational effects can be multidimensional, we proceed by estimating parental birth order effects on education and crime. In contrast to the analysis for income, we do not information on the parents' university enrollment or criminal activity at the same ages as their children, thus precluding a direct calculation of intergenerational transmission. Instead, we compare the parental birth order effects to the second generation's birth order effects. For completeness, we also report results for income.

Figure 1 shows parental birth order effects, birth order effects, and its ratio for these outcomes. Parental birth order significantly decreases children's higher education attainment. For example, children of third-born parents are 1.6 percentage points (four percent) less likely to have a higher education degree than children of first-born parents. A higher

[19]This restriction reduces the number of income observations for cohorts born between 1988 and 1990 to fewer than four. However, since the number of observations has little influence on the estimates, this is unlikely to bias our results.

parental birth order also increases boys' likelihood to be suspected of a crime.[20]

The degree of intergenerational transmission is roughly between 20 to 30 percent for all three outcomes. Even though the parental birth order effects are smaller than birth order effects, their magnitude is non-trivial. For example, parental birth order increases boys' crime by up to 10 percent for third-born children (relative to the sample mean). In Table A7 we show that the rise in crime is primarily driven by violent offenses. In this category, effect sizes are as high as 20 percent.

We interpret the findings above, in particular for violent crime, as significant evidence that parental birth order influences children's human capital beyond cognitive abilities. Given the high societal costs of (violent) crime, these results highlight that taking into account the non-monetary intergenerational effects of childhood experiences are important for a proper evaluation of their effects.

5 Gender Differences and Intergenerational Complementarities

Our large sample and identification strategy allows us to provide new insights on two open questions in the intergenerational mobility literature. First, we examine whether economic disadvantage is transmitted differentially through fathers or mothers. Second, we test for intergenerational complementarities in early-life advantage.

Gender differences. Some prior research suggests that fathers are less influential in shaping child outcomes than mothers, possibly due to their more limited involvement in raising children (e.g. Black et al. (2005a), Lundborg et al. (2024)). If this is the case, then we would expect children's incomes to respond stronger to maternal disadvantage than paternal disadvantage. This section tests this hypothesis empirically using birth order as a proxy for

[20] Estimates by family size are reported in Tables A5 and A6.

parental disadvantage.

We start by showing how a higher paternal or maternal birth order translates into paternal and maternal disadvantage. To do so, we estimate birth order effects on the incomes (in levels) of the first-generation individuals and their partners by gender. We have converted birth order into a numeric variable from 1 to 5. The results do not differ much if we impose this restriction and it makes the estimates more precise and readable.

Column 1 in panel A of Table 3 shows that birth order has a larger effect on the personal income of males. It also affects partner choice: partners of individuals with a higher birth order have a considerably lower income, and these effects are now larger for females (column 2). Columns 3 and 4 show the effect on household income in absolute value and in ranks. Because the effect on male's personal income is relatively large, the overall birth order effect on household income is slightly larger for men than for women. This implies that children with a higher paternal birth order grow up with an overall parental household income that is somewhat lower. If fathers affect children differently, then we would expect these differential effects on partner composition also to result in differential intergenerational effects.

Panel B tests for this by interacting parental birth order with the gender of the corresponding parent. Consistent with the larger income effect for males, Column 1 in Panel B shows a somewhat greater absolute effect of paternal birth order on child outcomes. These effects do not differ much depending on the gender of the child. However, Column 2 shows that the ratio of second-generation to first-generation effects —the intergenerational transmission rate — does not differ substantially by parent gender. Overall, these results are inconsistent with large gender differences in the transmission of economic (dis)advantage.

Intergenerational complementarity. Another open question is whether the returns to investments are larger for children with higher initial endowments (Cunha and Heckman, 2007). Establishing whether such complementarities exist is crucial for understanding whether interventions can mitigate early childhood disadvantage or amplify the benefits of childhood investments, but identifying them is challenging due to the need for exogenous variation in two treatments (Almond et al. (2018)).[21] Dettmer et al. (forthcoming) study Rhesus monkeys and find that maternal rearing yields larger benefits when the mother herself was also maternally reared. In doing so, they are the first to study an *intergenerational* complementarity. To our knowledge, we are the first to study intergenerational complementarities in early life advantage for humans.

Specifically, we test whether children benefit disproportionally from higher parental investments when the parent also received higher parental investments. As before, we use birth order as a proxy for parental investment in both generations.

A challenge is that birth order effects are identified using sibling comparisons, but among siblings, parental birth order is constant. To solve this issue we rely on a two-step approach. We first regress a child's birth order on sibling fixed effects and cohort fixed effects. The residual of this regression measures within-family variation in birth order that is uncorrelated with birth timing, which is precisely the exogenous variation we use to estimate birth order effects (equation 1). In the second step, we use the cousin fixed effects model (equation 2) again, but now we interact parental birth order with the residualized birth order variable. This interaction effect measures whether birth order effects differ by parental birth order.

Panel C in Table 2 shows the results. Consistent with the prior results, we see that children benefit from a lower birth order and a lower parental birth order, and the magnitude of the intergenerational effect is about 20% of the direct birth order effect. However, we find a precise null effect for the interaction. Thus, we find no evidence supporting intergenerational complementarities in early life advantage.

[21] A particularly closely related example comes from Muslimova et al. (2020), who also employ a lower birth order as a proxy for increased parental investments and demonstrate that first-born children disproportionately benefit from a high polygenic score (the initial endowment).

6 Fertility, Neighborhoods, and Genetic Endowments

Fertility. To examine whether differential fertility patterns might drive our results, Table A8 reports birth order effects on the likelihood of having any children, the total number of children, and age at first birth. For the extensive margin, we observe a non-monotonic pattern: second-born individuals are slightly more likely to have children than first-borns, but this pattern reverses at higher birth orders, with fourth- and fifth-borns being less likely to do so.

Such fertility differences could drive our results when the types of individuals who opt in or out of parenthood due to their birth order systematically differ. However, two reasons suggest this is unlikely. First, the effect sizes are very small: for instance, second-borns are only 0.8 percentage points more likely to have children than first-borns, and third-borns just 0.3 percentage points more likely, relative to a mean of 78 percent. Second, if selective fertility were a key driver, we would expect similarly non-monotonic patterns in our main outcomes, which we do not observe.

At the intensive margin, higher birth order decreases the number of children and decreases the age at first birth. As a result, children of higher–birth-order parents are born earlier and have a lower birth order themselves. Ideally, we would like to compare children of similar birth order and birth year. Directly controlling for children's year of birth or birth order, however, may lead to a bad control problem because these are 'after-treatment' variables that are affected by the parents' birth order. We therefore did not control for these mediators in our main results.

Nevertheless, such indirect effects via children's birth year or birth order do not generalize well across settings. We therefore try to gauge their importance in two ways. First, Column 2 of Table A9 replicates our main specification while adding children's birth year and birth order as (bad) controls. Comparing Columns 1 and 2, the estimates decrease somewhat but remain highly significant.

To deal with this problem more rigorously, we also present a two-step estimator in Appendix B that consistently recovers an intergenerational treatment effect net of birth year and birth order effects without adding bad control variables. This approach is broadly applicable in settings where a treatment affects the timing or number of children.[22] We describe the estimator in detail and validate it through a Monte Carlo simulation. When applied to our data, the two-step estimates closely match those obtained using direct controls (Table A9), confirming that our main findings are not driven by differences in children's birth order or birth year.

Neighborhoods and genetic endowments. Finally, we discuss the role of neighborhoods and the partner's genetic endowments. The implications of our results differ when intergenerational effects operate primarily through neighborhood selection or changes in genetic endowments because the pool of residential locations or potential partners is effectively fixed. In that case, our intergenerational effects reflect a reallocation of scarce resources, and extending the 'first-born' treatment to all individuals would not lead to aggregate intergenerational gains. As noted by Abrahamsson et al. (2025), these dimensions are often overlooked in studies of intergenerational effects.

Our data enables us to study the role of neighborhoods in great detail. In the Dutch administrative data, neighborhoods are defined at a very granular level, with average and median neighborhood sizes of 1160 and 560 residents, respectively.[23] Table A10 shows that a higher parental birth order decreases neighborhood income, but the effect size is economically small. To evaluate the importance of this sorting, Table A10 replicates our main specification with the inclusion of neighborhood fixed effects. By adding these fixed effects we account for all unobserved differences in neighborhood quality. The estimates decrease only marginally, providing strong evidence that neighborhoods do not drive our main findings.

[22] An alternative strategy used in prior work is to restrict the sample to first-born children to avoid variation in child birth order (e.g., Currie and Moretti (2003); Rossin-Slater and Wüst (2020)). The two-step method avoids this sample restriction and additionally adjusts for differences in birth timing.

[23] We first observe parental neighborhood of residence in 1995, when the children are 14 years old on average.

Understanding the role of genetic endowments is more challenging because we do not observe them. However, Abdellaoui et al. (2022) use data from the UK Biobank to estimate the causal effect of birth order on partners' polygenic scores (PGIs) for educational attainment. They find statistically significant but small effects: a one-unit increase in birth order reduces the partner's PGI by just 0.03 standard deviations. Given that parents transmit only half of their genes, this implies that a one-unit increase in parental birth order lowers children's PGIs by only 0.015 standard deviations on average, which is far too small to explain our effects.[24] These findings suggest that genetic transmission via assortative mating plays only a limited role, consistent with other evidence that matching on genes is modest in magnitude (Collado et al. (2023), Sunde et al. (2024)).

7 Conclusion

Our study provides insight into how parents' own childhood experiences shape the future of their children. Leveraging data on the full population of the Netherlands and the widespread applicability of birth order effects we provide precise estimates of transmission effects and explore gender differences and mechanisms. This advances the understanding of how human capital is transmitted across generations. Our findings also highlight the potential of childhood interventions targeted toward the family to make a long-lasting impact. Our results suggest that the benefits of such interventions may be larger than previously thought.

[24] In their data, a one standard deviation increase in the PGI predicts a 9 percentage point increase in university attendance, implying that a 0.015 standard deviation decrease corresponds to only a 0.13 percentage point decline. Even if this estimate understates the true relationship because of measurement error in the PGI, the effect remains an order of magnitude smaller than our estimates for higher education completion in Figure 1.

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Tables and Figures

Table 1: Descriptive Statistics

A. First Generation	Birth Order				
	1	2	3	4	5
Year of birth	1959.1	1959.1	1958.9	1958.9	1959.2
Male	51.3	51.2	51.1	51.2	51
Household income percentile	55.2	53.5	52.5	51.4	50.5
Has child	79	78.2	78.3	78.2	77.6
Age at first child	29	28.8	28.7	28.5	28.4
Number of children	1.7	1.7	1.7	1.8	1.7
N	1,227,725	1,139,220	561,332	240,279	78,777

B. Second Generation	Parental Birth Order				
	1	2	3	4	5
Year of birth	1981.6	1981.5	1981.5	1981.7	1981.9
Male	51	50.9	50.9	51	50.8
Household income percentile	55.1	54.9	54.9	54.9	54.7
Higher education completion	41.2	40.6	40.2	39.8	39.8
N	1,080,162	990,103	512,855	226,227	72,418

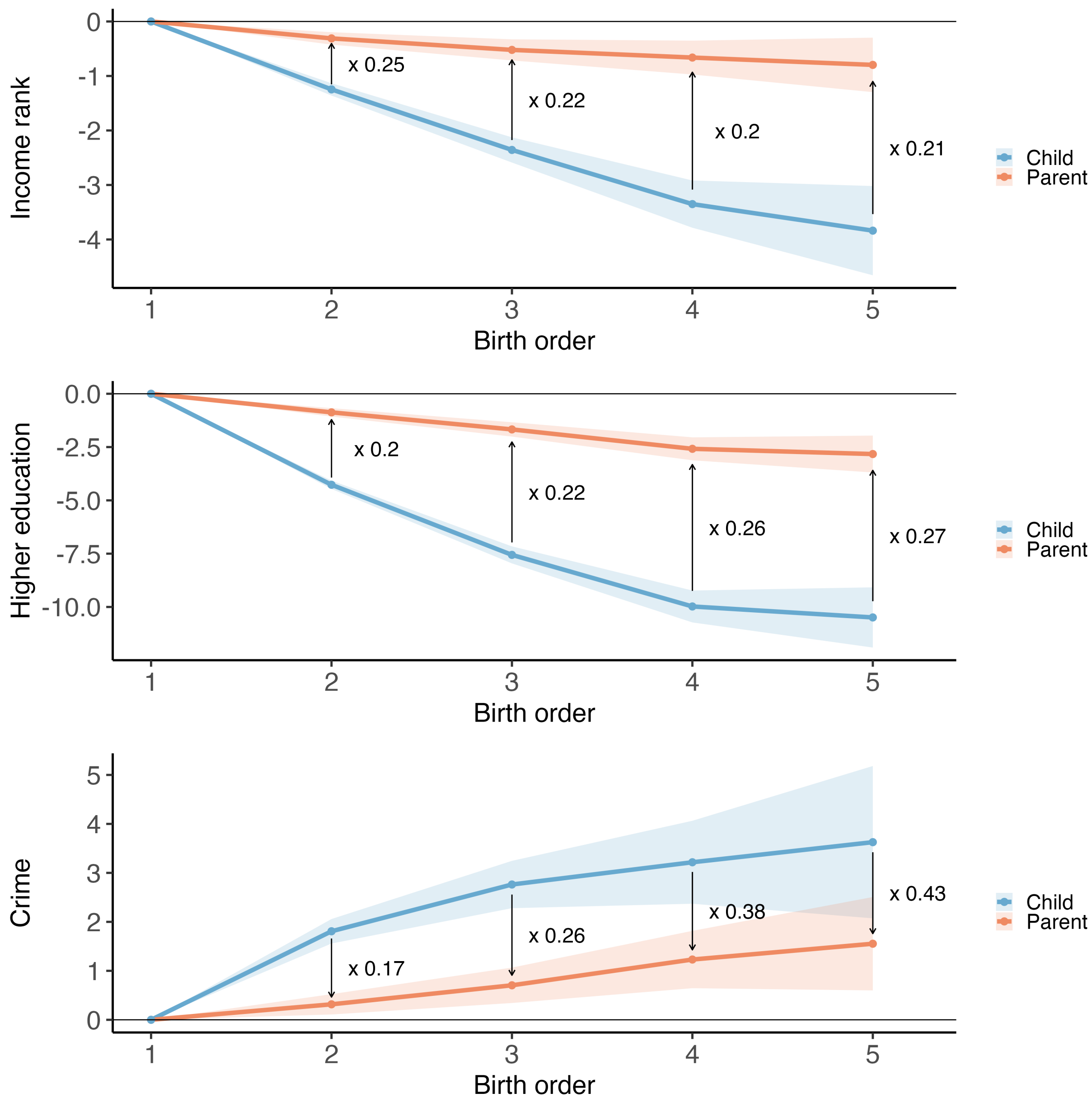
Notes: The sample in Panel A includes all individuals born between 1945 and 1970 who meet the sample selection criteria outlined in Section 2. This panel also includes individuals without children. Panel B includes all children of individuals from Panel A who were born before 1991. Outcomes are categorized by the birth order of the parent. Some children are in the sample twice: once for their father's birth order and once for their mother's birth order. All cells represent sample averages.

Table 2: Birth Order Effects on the First and Second Generation's Income Ranks

Birth Order	All (1)	Family size 2 (2)	Family size 3 (3)	Family size 4 (4)	Family size 5 (5)
	A. Intergenerational Birth Order Effects (β^{SG})				
2	-0.310***	-0.420**	-0.181	-0.406***	-0.485***
	(0.071)	(0.173)	(0.125)	(0.133)	(0.165)
3	-0.521***		-0.656***	-0.415**	-0.508**
	(0.122)		(0.238)	(0.207)	(0.226)
4	-0.660***			-0.547*	-0.702**
	(0.195)			(0.326)	(0.322)
5	-0.795**				-0.823*
	(0.313)				(0.461)
Mean	54.968	54.796	55.089	55.079	54.892
SD	27.201	27.426	27.233	27.089	26.88
N	2,881,765	858,952	883,600	680,984	458,229
	B. Birth Order Effects (β^{FG})				
2	-1.589***	-1.816***	-1.590***	-1.539***	-1.298***
	(0.049)	(0.101)	(0.081)	(0.102)	(0.145)
3	-2.968***		-3.066***	-2.762***	-2.837***
	(0.084)		(0.149)	(0.147)	(0.181)
4	-4.122***			-3.941***	-3.796***
	(0.137)			(0.227)	(0.246)
5	-5.224***				-4.849***
	(0.225)				(0.347)
Mean	53.748	54.588	54.287	52.818	51.162
SD	27.9	28.056	27.924	27.72	27.432
N	3,247,333	1,196,651	1,033,346	644,817	372,519
	C. Degree of Intergenerational Transmission (β^{SG}/β^{FG})				
2	0.195***	0.231**	0.114	0.264***	0.374***
	(0.045)	(0.097)	(0.079)	(0.089)	(0.133)
3	0.175***		0.214***	0.15**	0.179**
	(0.042)		(0.079)	(0.076)	(0.081)
4	0.16***			0.139*	0.185**
	(0.048)			(0.083)	(0.086)
5	0.152**				0.17*
	(0.061)				(0.096)

Notes: Panel A reports parental birth order effects, estimated according to equation 2. Panel B reports birth order effects on the first generation, estimated according to equation 1. Panel C reports the ratio of the estimates in panel A to the estimates in panel B. These ratios are computed using two-sample 2SLS. Standard errors are in parentheses. Standard errors in panel A (B) are clustered on the (extended) family level. Standard errors in panel C are based on the two-sample 2SLS correction from Inoue and Solon (2010). (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Figure 1: Parental Birth Order Effects, Birth Order Effects, and their Ratio



Notes: The blue coefficients represent birth order effects, estimated using equation 1, while the orange coefficients capture parental birth order effects, estimated using equation 2. The first panel presents results for household income ranks, based on 2,875,254 observations. The second panel presents results for higher education completion using the same sample. The third panel examines whether a son is suspected of a crime between ages 18 and 20, with a sample size of 1,516,009. The estimates of parental birth order effects for education and crime can also be found in Table A5 and A6, respectively. The numbers between the graphs represent the ratio of parental birth order effects to individual birth order effects. Shaded areas denote 95 percent confidence intervals, with standard errors clustered at the (extended) family level.

Table 3: Gender differences and intergenerational complementarity

	(1)	(2)	(3)	(4)
A. Birth order effects on the first generation	Personal income	Income partner	Household income	Household income rank (β^{FG})
Birth order × female	-1,691***	-1,617***	-3,308***	-1.400***
	(69)	(91)	(107)	(0.046)
Birth order × male	-3,203***	-575***	-3,779***	-1.545***
	(81)	(72)	(105)	(0.044)
N	3,247,333	3,247,333	3,247,333	3,247,333
B. Intergenerational birth order effects	Household income rank child (β^{SG})		Degree of transmission (β^{SG}/β^{FG})	
Birth order × female × daughter	-0.201***		0.144***	
	(0.070)		(0.05)	
Birth order × female × son	-0.207***		0.148***	
	(0.069)		(0.05)	
Birth order × male × daughter	-0.357***		0.231***	
	(0.072)		(0.048)	
Birth order × male × son	-0.286***		0.185***	
	(0.071)		(0.047)	
N	2,881,765		2,881,765	
C. Intergenerational complementarity	Household income rank child			
Birth order	-1.274***			
	(0.124)			
Parental birth order	-0.262***			
	(0.060)			
Birth order × parental birth order	-0.035			
	(0.052)			
N	2,881,765			

Notes: Panel A reports birth order effects on various outcomes, estimated according to 1, where birth order is interacted with gender. Panel B column (1) reports *parental* birth order effects, estimated according to 2, where parental birth order and the cohort fixed effects are interacted with the gender of the child. Panel B column (2) reports the ratio of the estimates in column (1) to the estimates in panel A column (4), with standard errors based on the two-sample 2SLS correction from Inoue and Solon (2010). Panel C reports results from a two-step procedure, where in step one, birth order is regressed on sibling fixed effects and year of birth × month of birth × gender × family size fixed effects. The reported estimates are the results from the cousin fixed effects model (2), where parental birth order is interacted with the residual from the first step. Standard errors are in parentheses. The standard errors in panel C are based on a clustered bootstrap with 200 replications. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Appendix A: Supplementary Results

Table A1: Birth order effects for all individuals and parents in our sample only

Birth Order	Income	
	(1)	(2)
2	-1.589***	-1.523***
	(0.049)	(0.081)
3	-2.968***	-2.827***
	(0.084)	(0.137)
4	-4.122***	-3.763***
	(0.137)	(0.219)
5	-5.224***	-4.966***
	(0.225)	(0.351)
Mean	53.748	53.58
SD	27.9	27.395
Sample	All individuals	Parents sample
N	3,247,333	1,470,196

Notes: This table presents birth order effects for the first generation for two samples. Column 1 estimates birth order effects for the entire first generation sample, replicating the main result in Table 2. Column 2 applies the same regression to the subsample of individuals with children born before 1991. These are all the parents of the children in our core analysis sample for the intergenerational effects. Standard errors are in parentheses. (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A2: Missing income

Birth Order	Missing income	
	(1)	(2)
2	-0.066	0.098***
	(0.044)	(0.032)
3	-0.114	0.284***
	(0.076)	(0.055)
4	-0.105	0.503***
	(0.121)	(0.089)
5	-0.148	0.898***
	(0.193)	(0.147)
Mean	2.754	2.922
SD	16.364	16.843
Generation	1	2
N	2,963,370	3,345,091

Notes: This table presents the effect of birth order and parental birth order on binary variables for missing income. Missing equals 100 when an individual does not have any records in the tax returns data, and it is zero otherwise. Column 1 is estimated according to equation 1. Column 2 is estimated according to equation 2. Standard errors are in parentheses. (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A3: (Parental) birth order effects with alternative income measures

(Parental)	Household Income Rank		Log Household Income		Personal Income Rank	
Birth Order	(1)	(2)	(3)	(4)	(5)	(6)
2	-1.603***	-0.309***	-0.033***	-0.007***	-1.140***	-0.257***
	(0.049)	(0.071)	(0.001)	(0.002)	(0.042)	(0.070)
3	-2.981***	-0.520***	-0.064***	-0.011***	-2.245***	-0.503***
	(0.084)	(0.122)	(0.002)	(0.003)	(0.072)	(0.120)
4	-4.142***	-0.658***	-0.089***	-0.015***	-3.140***	-0.677***
	(0.136)	(0.195)	(0.003)	(0.005)	(0.116)	(0.193)
5	-5.281***	-0.788**	-0.119***	-0.018**	-4.044***	-0.767**
	(0.224)	(0.313)	(0.005)	(0.008)	(0.190)	(0.310)
Mean	53.892	55.016	11.319	11.305	53.064	54.539
SD	27.798	27.164	0.67	0.655	28.501	27.277
Generation	1	2	1	2	1	2
N	3,238,606	2,879,255	3,238,606	2,879,255	3,238,606	2,879,255

Notes: This table presents birth order effects and intergenerational birth order effects on various income measures. The samples include all children and parents whose personal incomes are observed. Columns 1 and 2 replicate the main result for these subsamples. Columns 3 and 4 report results using the log of household income. Columns 5 and 6 report results using the personal income rank, which is computed relative to all individuals in the same cohort and of the same gender. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A4: (Parental) birth order effects with varying income observations

	A. Birth order effects				
Birth order	-1.479***	-1.496***	-1.487***	-1.474***	-1.470***
	(0.040)	(0.040)	(0.040)	(0.040)	(0.039)
# income observations	1	2	3	4	5
N	3,247,333	3,247,333	3,247,333	3,247,333	3,247,333
	B. Intergenerational birth order effects				
Parental birth order	-0.232***	-0.242***	-0.259***	-0.263***	-0.258***
	(0.058)	(0.058)	(0.058)	(0.058)	(0.058)
# income observations	1	2	3	4	5
N	2,857,825	2,857,825	2,857,825	2,857,825	2,857,825
	C. Intergenerational birth order effects				
Parental birth order	-0.263***	-0.282***			
	(0.058)	(0.058)			
Only income above age 32	No	Yes			
N	2,857,825	2,857,825			

Notes: Panel A (B) estimates (intergenerational) birth order effects on lifetime income according to equation 1 (2), where lifetime income is measured using a varying number of income observations. Panel C column (1) estimates intergenerational birth order effects using the same lifetime income variable as used in the main analysis, whereas column (2) replicates it using only incomes above age 32. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A5: Parental birth order effects on education: estimated by family size

Parental Birth Order	All	Family size 2	Family size 3	Family size 4	Family size 5
	(1)	(2)	(3)	(4)	(5)
2	-0.803***	-1.113***	-0.610***	-0.798***	-1.184***
	(0.122)	(0.292)	(0.213)	(0.229)	(0.287)
3	-1.571***		-1.909***	-1.024***	-1.768***
	(0.209)		(0.400)	(0.354)	(0.394)
4	-2.463***			-1.693***	-2.886***
	(0.334)			(0.555)	(0.559)
5	-2.616***				-3.123***
	(0.535)				(0.796)
Mean	39.583	39.31	39.955	39.782	39.08
SD	48.903	48.844	48.981	48.945	48.793
N	2,963,370	884,103	908,886	699,952	470,429

Notes: This table presents the effect of parental birth order on children's higher education attainment. The estimates are separated by family size. All models are estimated according to equation 2. Standard errors are in parentheses.
(*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A6: Parental birth order effects on boys' criminal behavior: estimated by family size

Parental Birth Order	All	Family size 2	Family size 3	Family size 4	Family size 5
	(1)	(2)	(3)	(4)	(5)
2	0.303**	0.678**	0.123	0.395	0.295
	(0.126)	(0.304)	(0.202)	(0.246)	(0.360)
3	0.682***		0.635	0.544	0.556
	(0.220)		(0.395)	(0.373)	(0.456)
4	1.173***			0.845	1.315**
	(0.357)			(0.591)	(0.639)
5	1.455**				1.564*
	(0.584)				(0.913)
Mean	9.549	9.479	9.364	9.773	9.933
SD	29.39	29.292	29.133	29.695	29.911
N	1,522,958	550,803	501,673	304,929	165,553

Notes: This table presents the effect of parental birth order on boys' criminal behavior. The estimates are separated by family size. All models are estimated according to equation 2. Standard errors are in parentheses.
(*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A7: Parental birth order effects on different types of crime

Birth Order Parent	Crime	Violent Crime	Property Crime	Other Crime
2	0.303**	0.184**	0.132*	0.039
	(0.126)	(0.084)	(0.075)	(0.094)
3	0.682***	0.446***	0.179	0.200
	(0.220)	(0.148)	(0.131)	(0.163)
4	1.173***	0.624***	0.330	0.409
	(0.357)	(0.241)	(0.214)	(0.265)
5	1.455**	0.802**	0.542	0.149
	(0.584)	(0.397)	(0.354)	(0.431)
Mean	9.549	3.998	3.123	4.942
SD	29.39	19.592	17.393	21.675
N	1,522,958	1,522,958	1,522,958	1,522,958

Notes: This table present the effects of parental birth order on children's likelihood to be suspected of a crime between ages 18 to 21 in general, as well as for three (non-mutually exclusive) categories: property crime, violent crime, and 'other' crimes that do not fit those categories. All models are estimated according to equation 2. Standard errors are in parentheses.
(*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A8: Birth order effects on fertility

Birth Order	Has child	Number of children	Age at first child
	(1)	(2)	(3)
2	0.826***	-0.020***	-0.131***
	(0.078)	(0.002)	(0.010)
3	0.345**	-0.044***	-0.244***
	(0.135)	(0.003)	(0.018)
4	-0.527**	-0.067***	-0.332***
	(0.219)	(0.005)	(0.029)
5	-2.005***	-0.096***	-0.501***
	(0.360)	(0.009)	(0.047)
Mean	78.501	2.198	28.819
SD	41.081	0.868	5.277
N	3,247,333	2,549,205	2,549,205

Notes: This table presents the effect of birth order on fertility outcomes. The outcome in column 1 equals 100 if an individual has at least one child and zero otherwise, and the sample includes the core analysis sample of the first generation. The outcomes in columns 2 and 3 measure the number of children and the age when parents have their first child. The sample in columns 2 and 3 includes all individuals from the first generation who have a child. Standard errors are in parentheses (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A9: Controlling for children's year of birth and birth order

Parental Birth Order	Income		
	(1)	(2)	(3)
2	-0.310***	-0.259***	-0.266***
	(0.071)	(0.071)	(0.091)
3	-0.521***	-0.410***	-0.429**
	(0.122)	(0.122)	(0.191)
4	-0.660***	-0.476**	-0.506
	(0.195)	(0.195)	(0.33)
5	-0.795**	-0.539*	-0.589
	(0.313)	(0.314)	(0.445)
Mean	54.968	54.968	54.968
SD	27.201	27.201	27.201
Controls		x	
Two-step			x
N	2,881,765	2,881,765	2,881,765

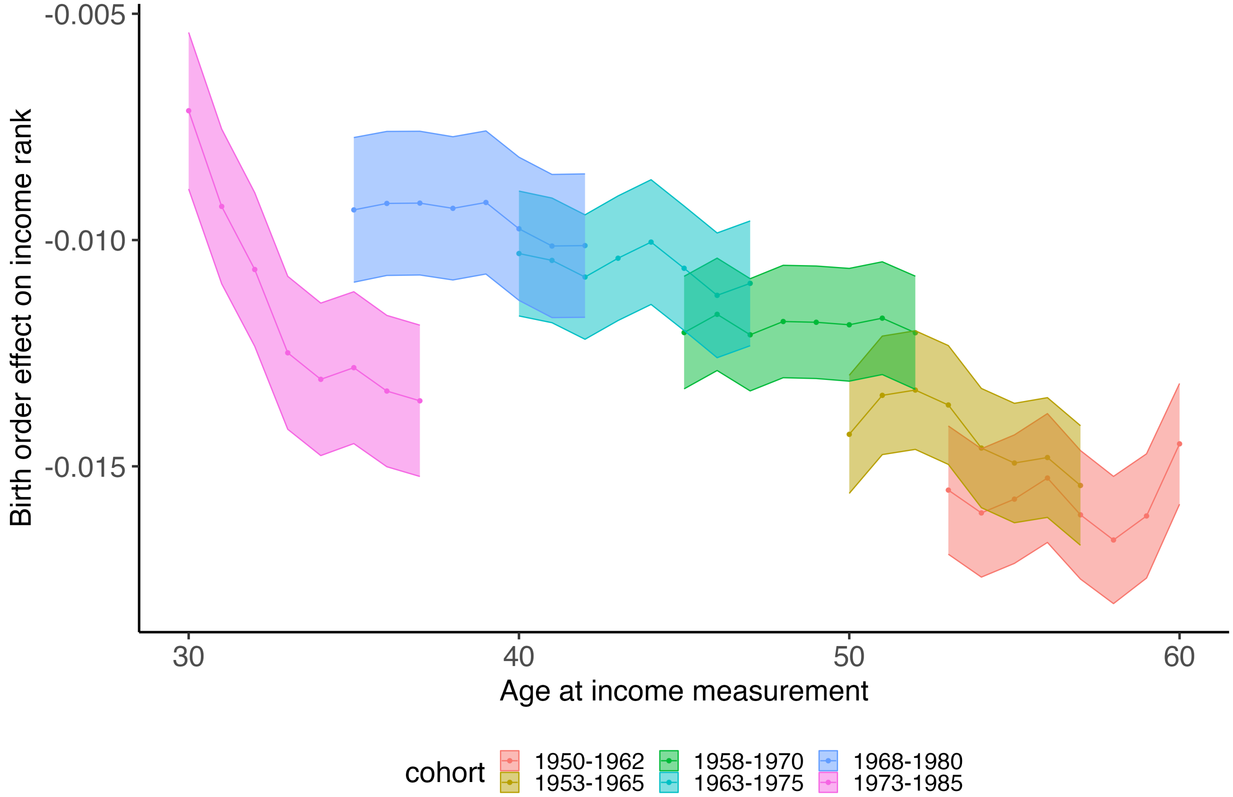
Notes: This table presents parental birth order effects while controlling for children's year of birth and birth order or by using the two-step estimator. Model 1 replicates the main result in Table 2. Model2 2 is estimated according to equation 2 and includes the children's year of birth and birth order as control variables. Model 3 is estimated using the two-step estimator from Appendix B. Standard errors are in parentheses. The standard errors in models with and without controls are clustered by extended family, and the two-step standard errors are computed using a block bootstrap to account for within extended-family correlation (200 repetitions). (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A10: The role of neighborhoods

Parental Birth order	Neighborhood income	Income	
	(1)	(2)	(3)
2	-0.130***	-0.311***	-0.295***
	(0.022)	(0.070)	(0.068)
3	-0.233***	-0.516***	-0.470***
	(0.037)	(0.119)	(0.118)
4	-0.343***	-0.654***	-0.592***
	(0.059)	(0.191)	(0.185)
5	-0.371***	-0.700**	-0.658**
	(0.094)	(0.305)	(0.301)
Mean	52.752	54.968	54.968
SD	7.356	27.201	27.201
Neighborhood FE			x
N	2,790,659	2,790,659	2,790,659

Notes: Column 1 presents the effect of parental birth order on the average income rank in the parents' neighborhood in 1995. Neighborhoods are based on Statistics Netherlands' most granular neighborhood classifier (in Dutch: 'buurt'), with average and median neighborhood sizes of 1160 and 560 residents, respectively. Average neighborhood income is measured by taking the average lifetime income ranks of all residents excluding the parents in 1995. The sample includes all children from the core analysis sample whose parents' neighborhoods are observed across all three columns. Column 2 estimates intergenerational birth order effects on income for this sample. Column 3 extends column 2 by including neighborhood fixed effects. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Figure A1: Birth order effects on income measured at different ages



Notes: This figure displays the estimated effect of birth order on income across cohorts and at different ages of income measurement. The Y-axis shows coefficients from Sibling Fixed Effects models (Equation 1) where birth order is included as a numerical variable. The X-axis indicates the age at which income is observed. Each line corresponds to a different birth cohort, as denoted by color. Within each cohort, estimates are based on identical samples. Shaded bands represent 95% confidence intervals.

Appendix B: a Two-Step Estimator for Intergenerational Causal Effects

In Table A8 we show that children of parents with a higher birth order tend to be born later and have a lower birth order. In the presence of birth order effects or differences in time trends, these differences in birth order and year of birth are mediating factors that affect the outcome. Ideally, we would like to compare children of similar birth order and birth year. However, directly controlling for children's year of birth or birth order leads to a bad control problem because these are 'after-treatment' variables. To deal with this, we propose an estimator that allows us to estimate the intergenerational effect of birth order net of a child's own year of birth or birth order. We discuss our estimator in the context of a general experiment so that it can be used by other researchers as well.

Decomposing a total treatment effect into indirect effects from children's year of birth or birth order and a remaining direct effect is not trivial. To illustrate this, consider the following Data Generating Process (DGP) where a child's year of birth is the only mediating factor:

$$\begin{aligned} Y_{cp} &= \beta_1 x_p + \beta_2 \tau_{cp} + \beta_3 I_p + \epsilon_{cp}, \\ \tau_{cp} &= \delta_1 x_p + \delta_2 I_p + \eta_{cp}. \end{aligned} \tag{3}$$

In this model, Y_{cp} is a measure of education of child c of parent p, x_p is the parent's treatment status, τ_{cp} is a child's year of birth, and I_p is parental income. All parameters are positive, meaning that parental treatment and income increase education and a child's year of birth. A child's year of birth also increases education due to a positive trend in education.

A regression of Y_{cp} on x_p gives the total effect of treatment, denoted β. The total treatment effect is made up of two parts: a direct effect (β_1) and an indirect effect ($\beta_2\delta_1$). The indirect effect occurs because the treatment also affects the child's year of birth, which in turn affects the child's education. This second effect may not always be relevant, as it depends on the specific context. For instance, the larger the trend in education, the more significant the indirect effect of a child's year of birth will be.

Isolating the direct effect (β_1) is challenging. Simply adding the child's year of birth as a control variable, for example, may not provide a consistent estimate for β_1. To see this, suppose that parental income I_p is unobserved and substitute τ_{cp} into the outcome model:

$$Y_{cp} = \beta_1 x_p + \beta_2 \underbrace{(\delta_1 x_p + \delta_2 I_p + \eta_{cp})}_{\tau_{cp}} + \nu_{cp},$$

where $\nu_{cp} = \beta_3 I_p + \epsilon_{cp}$. Since τ_{cp} is correlated with ν_{cp}, a regression of education on a parent's treatment status and a child's year of birth yields a biased estimate for β_2. Intuitively, the estimate not only captures birth year effects but also income effects that are correlated with year of birth. As $\beta_1 = \beta - \delta_1\beta_2$, a bias in β_2 also contaminates the estimate for β_1.

More generally, isolating the part of the treatment effect that is not related to a child's birth order or year of birth is complex because families who have children earlier or who have more children tend to differ in other aspects such as income and education. These unobserved confounding factors can bias the birth order and year of birth effects when they are included as control variables in the regression, and ultimately contaminate the estimate of the direct treatment effect.[25]

To address these issues, we propose a simple two-step estimator that allows us to consistently estimate an intergenerational treatment effect where mediating birth order and year of birth effects are partialled out.

The approach works as follows: in the first step, we use sibling comparisons from the second generation to estimate the effects of year of birth and birth order. Because siblings are exposed to the same parental treatment, these estimates are unrelated to the parents' treatment status. Additionally, by using sibling comparisons, we can ensure that these estimates are not biased by confounding factors such as differences in parents' income or education. In the second step, we correct the children's outcomes for birth order and year of birth using the estimates from the first step. Because this correction is unrelated to the treatment, we can consistently estimate the treatment effects on the corrected outcomes. Furthermore, since the outcomes of the children are corrected for birth order and year of birth, any variation in the corrected outcomes that is explained by the treatment must be the direct effect.

This two-step estimator is useful for two reasons. First, it can be used to determine whether time trends significantly affect the results. Although in our application the differences when using the two-step estimator are relatively small, they could be particularly important in situations where researchers find small intention-to-treat (ITT) effects and low take-up of the treatment. By inflating the ITT estimates by the take-up, any small differences in the year of birth will also be inflated, leading to potentially large differences in the total treatment effect.[26] Second, by normalizing all outcomes to the same birth order, the estimator allows researchers to use children of all birth orders, even in cases where treatment affects the number of children that parents have or when some children are censored. As discussed in Section 3, using children of all birth orders maximizes the power and external validity of the estimates.

The formal set-up. Suppose that there are n children from $P < n$ parents. We index the c^{th} child of a parent p by cp. A child cp has birth order $c \in \{1, ..., B\}$, is born in year $t_{cp} \in \{1, ..., T\}$ and has outcome Y_{cp}. Treatment x_p is randomly assigned to parents, such that the regression

$$Y_{cp} = \beta x_p + u_{cp} \tag{4}$$

consistently estimates the total treatment effect β, which includes the mediating effects of

[25] Another unintended consequence of adding a child's year of birth to a regression is that, in combination with a parent's year of birth, it also captures parents' age-at-birth effects. Whether a parent's age at birth is a mediator that should be netted out depends on the research question.

[26] For example, Rossin-Slater and Wüst (2020) find that women with access to preschool are 0.11 years older at their first birth. They also find that a mother's access to preschool at age 3 increases the likelihood that her child obtains more than a compulsory education by 0.9 percentage points. When inflated by the average take-up of 10 percent, their average treatment effect corresponds to roughly 10 percentage points. The 0.11 years difference in year of birth is also inflated by a factor of ten, which implies that the exposed children are born more than a year later on average. In the presence of strong positive trends in education, this could potentially explain a sizeable fraction of the total treatment effect.

birth order and year of birth. To decompose the effects into direct and indirect effects we consider

$$
\begin{aligned}
Y_{cp} &= \beta_1 x_p + \underbrace{\sum_{b=1}^{B} \gamma_k I[c=b]}_{\gamma_{cp}} + \underbrace{\sum_{t=1}^{T} \tau_t I[t_{cp}=t]}_{\tau_{cp}} + \epsilon_{cp} \\
&= \beta_1 x_p + \gamma_{cp} + \tau_{cp} + \epsilon_{cp}, \qquad (5)
\end{aligned}
$$

where γ_{cp} and τ_{cp} are birth-order and year-of-birth fixed effects. By including dummies for each birth order and year of birth, the specification above allows for non-linearity in their effects. β_1 represents the direct effect of treatment net of a child's year of birth and birth order. When treatment affects children's year of birth or birth order, $\beta_1 \neq \beta$ in general.

To estimate β_1, we assume that birth-order and year-of-birth effects are consistently estimated in a sibling fixed effects model. Using this assumption, the two-step procedure works as follows:

1. First, note that

$$
Y_{cp} = \alpha_p + \sum_{b=1}^{B} \gamma_k I[c=b] + \sum_{t=1}^{T} \tau_t I[t_{cp}=t] + \epsilon_{cp}, \qquad (6)
$$

   where $\alpha_p = \beta x_p$. Equation 6 corresponds to a sibling fixed effects model. By assumption, the corresponding regression estimates $\hat{\gamma}_k$ and $\hat{\tau}_t$ are consistent for γ_k and τ_t, respectively.

2. Use the estimates from step 1 to construct fitted values $\hat{\gamma}_{cp} = \sum_{b=1}^{B} \hat{\gamma}_k I[c=b]$ and $\hat{\tau}_{cp} = \sum_{t=1}^{T} \hat{\tau}_t I[t_{cp}=t]$. Deduct $\hat{\gamma}_{cp}$ and $\hat{\tau}_{cp}$ from both sides of equation 5 such that

$$
Y_{cp} - \hat{\tau}_{cp} - \hat{\gamma}_{cp} = \beta_1 x_p + \nu_{cp}, \qquad (7)
$$

   where $\nu_{cp} = \epsilon_{cp} + \tau_{cp} - \hat{\tau}_{cp} + \gamma_{cp} - \hat{\gamma}_{cp}$. Since x_p is randomly assigned to the parents and is not used in the estimation of $\hat{\gamma}_k$ and $\hat{\tau}_t$, $cov(x_p, \nu_{cp}) = 0$. As a result, a regression of $Y_{cp} - \hat{\tau}_{cp} - \hat{\gamma}_{cp}$ on x_p yields a consistent estimate for β_1.

Regular clustering methods do not yield proper standard errors for the two-step estimator because (i) the number of observations (children) in the sample depends on the treatment assignment and (ii) the first step adds additional uncertainty, and ignoring this will lead to underestimation of the standard errors. Instead, we use a simple clustered bootstrap procedure. Specifically, if there are P families in the sample, then we randomly draw P families with replacement. Next, we apply the two-step estimator to this sample to obtain $\hat{\beta}_1^1$. We repeat this process $R = 200$ times and store the resulting estimates in a vector $\hat{\beta}_{\mathbf{1}} = \{\hat{\beta}_1^1, \hat{\beta}_1^2, ..., \hat{\beta}_1^R\}$. The bootstrapped 95 percent confidence interval for $\hat{\beta}_1$ is then given by the interval between the 2.5^{th} and 9.75^{th} percentile of vector $\hat{\beta}_{\mathbf{1}}$.