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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.

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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 greater data availability, there is a small but growing literature that studies the intergenerational consequences of childhood experiences. The earlier studies mostly focused on education, but 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)). Overall, the literature studying causal intergenerational effects has produced mixed results, with some studies finding very large intergenerational spillovers and others finding none.

While these are thorough studies, they often focus on specific groups, such as disadvantaged children or those complying with particular reforms. Moreover, the identification of intergenerational effects requires assumptions that may be harder to justify over generations. We overcome these limitations by studying the intergenerational consequences of birth order: a childhood experience that has large effects on the parental generation, applies to roughly 80 percent of the population (everyone with at least one sibling) and that can be identified using relatively mild assumptions. The fact that almost everyone is exposed to birth order effects and the use of administrative data also enables us to zoom in on scarcely researched gender differences in intergenerational transmission.

Birth order is a unique 'treatment' in that it broadly affects multiple important dimensions of human capital. Children with a higher birth order have considerably less education, a lower IQ, unfavorable personality traits and leadership qualities, higher crime rates, lower subsequent earnings, and find lower educated partners (Black et al. (2005b); Kantarevic and Mechoulan (2006); Black et al. (2011); Black et al. (2018); Breining et al. (2020), Abdellaoui et al. (2022), Houmark (2023)).[1] These effects come from sibling comparisons and can thus not be attributed to differences in schools, neighborhoods, or genetic factors. 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)).[2] Consequently, our findings may offer particularly important insights into the potential for parental investments to make long-lasting impacts.

We follow the literature and estimate birth order effects on the first generation by using sibling comparisons. 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. 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 polygenetic score for education. 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

[1]To put these effects into perspective, we note that 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)).

[2]Two alternative theories suggest that birth order effects may be driven by (i) older siblings learning from the responsibility they have as role models or (ii) younger siblings being exposed longer to changes in family structure such as parental divorce. Contrary to (i), multiple papers (see Section 4) 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.

replace the outcomes of siblings with those of their children.[3]

Parental birth order effects have widespread applicability, and can - with administrative data - be estimated using a vast part of the population. We use rich administrative microdata from Statistics Netherlands, covering over 2 million children born between 1960 and 1990.[4] Using the child-parent register, we link these children to their relatives, allowing us to analyze within-family variation while maintaining a high level of statistical power. An additional contribution from this paper comes from observing the lifetime household income of both children and their parents.[5] This lets us contrast the impact of parental birth order with the birth order effects on parents' own incomes. From the ratio of these two effects, we discern the degree of intergenerational transmission. We also consider children's higher education completion as a measure of cognitive skills and crime as a measure of non-cognitive skills.[6]

Our main finding is that around 20 percent of the income disadvantages due to a higher birth order are transmitted to the next generation. For example, the income of third-born parents is 3 percentiles lower than that of first-born parents, whereas their children's income is 0.6 percentiles 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.

Next, we find that a higher parental birth order decreases education and increases boys' crime rates. The effects on crime are predominantly driven by increases in violent crime. The effects are economically meaningful, with increases up to 0.7 percentage points (17 percent) in the likelihood of sons of third-born parents being suspected of a violent crime compared to sons born to first-born parents. These findings highlight the importance of parents' childhood experiences in shaping boys' non-cognitive development and criminal behavior.

[3]Some additional complexities arise when estimating intergenerational effects, such as selective fertility and assortative mating. These are discussed in more detail in Section 4.

[4]This forms the core sample used for our income and education analyses. Since the crime outcome is only available for later cohorts, we restrict the crime analysis to children born between 1986 and 2001.

[5]As outlined in Section 2, previous studies have predominantly focused on the outcomes of the second generation during relatively early stages of life.

[6]In line with earlier research, we interpret the effects on crime as manifestations of lower-level acquisition of non-cognitive skills (Breining et al. (2020)).

An important question in the intergenerational mobility literature is whether inequality is transmitted to the next generation through fathers or mothers, sons or daughters, or their interaction. We find slightly larger (but not significantly different) paternal birth order effects on children's income and education, 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. For crime, the paternal birth order effects are significantly stronger. We find no differential effects for sons or daughters or their interaction with the gender of the parent.

We provide multiple extensions. First, we show that although a higher parental birth order affects children's year of birth and birth order, these potentially selective fertility patterns can not explain our findings.[7] Second, we rule out differences in neighborhoods as a mediating factor because parental birth order has no meaningful effect on the quality of the neighborhood where children grow up. Third, we show that individuals with a higher birth order also find lower-quality partners, suggesting that assortative mating amplifies the transmission of birth order effects within families. Fourth, we observe that birth order effects do not vary by parental birth order, suggesting that later-born parents do not compensate their later-born children based on their own experiences. Finally, we examine the role of parents' sibling sex composition, and find that birth order effects for men are stronger if they have more brothers, and these effects persist into the next generation.

Although manipulating birth order itself is challenging, this paper still provides meaningful insights for policy. Generally, our findings underscore that childhood experiences can have sizeable and multidimensional intergenerational effects. Because birth order effects are shown to arise from factors *within* the family, our results are particularly promising for childhood interventions targeted at the family. The benefits from such interventions may be larger than previously thought if their intergenerational and multidimensional spillovers are

[7] An empirical challenge here is the endogeneity of the second generation's year of birth and birth order. In Appendix B, we propose a simple two-step estimator that enables us to estimate intergenerational effects *net of* children's year of birth and birth order. This estimator is generally applicable in contexts where treatment in the first generation affects the timing of birth or the number of children.

taken into account.

2 Literature

We first briefly review the small but growing literature on causal intergenerational effects of childhood experiences.[8] The first contributions explore what happens when parents get more education due to college openings, compulsory schooling law reforms, or discontinuities in age-at-entry rules and includes research by Currie and Moretti (2003), Black et al. (2005a) and Oreopoulos et al. (2006).[9] Generally, some studies find no or small intergenerational effects, and others find quite sizeable effects on the next generation. However, all studies consistently find IV estimates that are smaller than OLS estimates. For example, Black et al. (2005a) and Holmlund et al. (2011) both find IV estimates for mothers that are around 50% of the OLS estimates in Norway and Sweden, whereas the estimates for men are insignificant.

More recently, researchers have broadened their focus to include other childhood experiences, such as early childhood education. Rossin-Slater and Wüst (2020) study the intergenerational effects of maternal preschool enrollment on children's education. By comparing second-generation coefficients with first-generation coefficients, they derive a transmission coefficient of 0.28 for years of education. Barr and Gibbs (2022) study the effects of maternal Head Start enrollment and - while they acknowledge the difficulty of comparing the effects across generations - their results indicate that the effects on the second generation are at least as large as the effects on the first generation. Both studies only consider mothers. García et al. (2023) study the effects of paternal Perry Preschool Program participation, and find sizeable spillovers on a range of outcomes, which are larger for sons than for daughters.

Going beyond (early childhood) education, four studies delve into the intergenerational

[8] We focus 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)).

[9] 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).

effects of health shocks and parental disability benefits receipt. Black et al. (2019) study the intergenerational effects of in-utero exposure to nuclear fallout. They find a decline in the IQ of the children of exposed individuals of about 60 percent of the effect in the first generation. These effects are mostly driven by exposed fathers. Bütikofer and Salvanes (2020) study the effects of a tuberculosis control program launched in Norway in 1948, and find sizeable effect not only on the first generation's education, health, and labor market outcomes but also on multiple outcomes of the second generation. Relating the effect of the program on the first and second generation's education, their estimates imply a degree of transmission of about 30 percent. The effects are relatively similar for fathers and mothers. East et al. (2023) analyze the effect of maternal in-utero exposure to Medicaid on birth outcomes and find a decline in low birth weight experienced by the second generation of about 48 percent of the effect in the first generation. Finally, Dahl and Gielen (2024) find that children whose parents' eligibility for disability benefits is reduced have better educational and labor market outcomes and also their children are born healthier. They do not consider gender differences for neither parents nor children.

The research above shows that certain childhood experiences can have larger intergenerational consequences than others, even if their effects on the first generation's income or education are similar. There are good reasons to expect that the intergenerational effects of childhood experiences may also differ by gender, but evidence from the papers above is limited. This highlights the need for further research into the transmission of different types of childhood experiences and how this differs by gender of the parent and the child. Our study addresses this gap by examining the intergenerational impacts of birth order and by investigating gender differences.

Moreover, the studies mentioned above often focus on the second generation's outcomes at relatively young ages. To our knowledge, this is the first study to examine the intergenerational transmission of long-run household income (dis)advantages stemming from childhood experiences. Household income provides a more comprehensive measure of economic status, particularly for females, for whom education and earnings often serve as imperfect proxies (Chadwick and Solon (2002)). Additionally, using household income allows us to directly compare our results to widely reported intergenerational mobility estimates.

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. Havari et al. use multi-country data from the Survey of Health, Aging, and Retirement in Europe (SHARE) and control for a range of factors, including parent's family size and year of birth. Barclay et al. use administrative data from Sweden, which allows them to use cousin comparisons as well. Both studies find positive effects of higher parental birth order on children's completed education.

We complement their results by (i) extending the results to the realms of income and crime, (ii) analyzing the degree of intergenerational transmission, (iii) zooming in on gender differences, and (iv) considering the roles of neighborhoods, fertility, assortative mating, and the sibling-sex composition. There are also important differences in their design, resulting in different estimates.[10]

Finally, we are related to papers that compare the children of monozygotic twins to study the intergenerational transmission of schooling (Behrman and Rosenzweig (2002), Antonovics and Goldberger (2005), Holmlund et al. (2011), Pronzato (2012)). As monozygotic twins are genetically identical, their children share on average half of their genes. Our paper is similar in the sense that we also consider the intergenerational transmission of within-family differences, and, as birth order is unrelated to children's genetic makeup, the effects can also not be explained by the treated parents' genes.[11] An advantage of our approach is

[10]The main difference with Havari and Savegnago (2022) is that we use much larger administrative data and focus on one country only, allowing us to use within-family comparisons with administratively registered outcomes. Barclay et al. (2021) also use within-family variation, but group the parents' year of birth into bins of 5 years, making it difficult to differentiate parental birth order effects from time trends. Moreover, their preferred specification includes children's outcomes and both parents' characteristics, both of which are after-treatment variables and can induce a bias.

[11]Note that, just like with twin-designs, the genes of the partner can still play a role. Generally, a common criticism of the within-family comparisons is that schooling differences between twins can be correlated with unobserved differences and can exacerbate measurement error problems (Bound and Solon (1999)). These issues are less likely problematic in this paper because differences in birth order are measured with great

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 in the parents' schooling are unobserved.

3 Data

We use administrative data on the entire population of the Netherlands from Statistics Netherlands. Using individual identifiers we can join records associated with an individual across a range of government services, such as the personal register, tax statements, enrollments in education, and crime incidents.

Sample. The personal register contains all individuals who have been registered in the Netherlands since 1995. From this, we select all individuals born in the Netherlands between 1945 and 1970. We drop all individuals from families of migrants (5.5%), where at least one birth date is missing (8%), and families with twins (3.7%).[12] We also drop single-child families (10%) because they are not used for identification and families with six or more children (13%) for conciseness of our results. We call the remaining individuals the first generation. In total, our first-generation sample includes 64 percent of all individuals who were born between 1945 and 1975 and have been registered in the Netherlands. We establish birth order by ranking all individuals who are linked to the same mother and father by their birth dates. Thus, our analysis focuses solely on birth order among full siblings.

Next, we link the first generation with their children.[13] We refer to the children of the first generation as the second generation. In the core analysis, we focus on children born before 1991. This sample includes 64 percent of all individuals registered in the Netherlands who were born before 1991 and whose parents were born between 1945 and 1970.

accuracy and assigned before children's outcomes are realized.

[12] We drop migrants for two reasons. First, family links are poorly observed for migrants. Second, families of migrants often arrive simultaneously, creating a correlation between birth order and age at arrival of siblings.

[13] To establish the parent-child links, we rely on legal relationships. Consequently, the identified parents are not necessarily the biological parents, but rather the ones who are most likely to have raised the children. Moreover, parents do not need to be together for the full period in which they raise their children.

Income. The income register records the gross household income extracted from (joint) tax statements spanning the period between 2003 and 2022. Household income encompasses all income from employment, entrepreneurship and capital as well as income insurance payments, social security benefits, conditional transfers, receiving income transfers, and employers' and employees' contributions to social insurance premiums.[14] We measure income in 2024 euros, adjusting for inflation using the consumer price index. We focus on household income because it provides a reliable measure of economic resources even in the case of non-participation in the labor market and it is commonly used in other intergenerational mobility studies (Chadwick and Solon (2002)). We have household income records for 97.3% of the second generation sample and 97.5% of the first generation sample.[15]

We use these data to obtain a proxy of an individual's lifetime household income. A well-known challenge here is that snapshots of an individual's income suffer from two types of measurement error. First, transitory shocks in income lead to classical measurement error and may attenuate estimates (Mazumder (2005)). Second, heterogeneous age-income profiles may cause life-cycle bias (Haider and Solon (2006)). We aim to mitigate such bias by calculating the average income over the five years that are nearest to the age of 35, and within the age range of 29 to 60. Measuring income over multiple years decreases bias due to measurement error, whereas taking income close to age 35 reduces life-cycle bias (Nybom and Stuhler (2017)). To measure income at suitable ages for all children in the sample, we restrict the income analysis to children born before 1991.[16]

[14] Income insurance payments concern benefits from social insurance, national insurance and private insurance related to unemployment, illness, disability or retirement. Social security benefits concern government-sponsored transfers such as welfare benefits or veteran pension payments. Conditional transfers are transfers tied to specific payments, such as rental or study allowances. Receiving income transfers consist of transfers between households such as alimony received from the ex-spouse.

[15] Income may be missing for various reasons, such as emigration, death, or individuals living in an institutional household for whom income is not measured. In Table A6 we report estimates of whether missing income records are related to parents' birth order. We find no relationship between parents' birth order and missing income for children. For the first generation, we see that the income of later-born children is slightly more likely to be missing. However, given the overall high coverage in the sample, we believe this is unlikely to significantly affect our estimates.

[16] For the children born in 1990 (7%) we observe only 4 years of income. Excluding these children does not affect the results. Moreover, for another 1.7 percent of the observations, we observe less than 5 years with income because of incomplete records. In those cases, we take the average income over all available

In our main analysis, we use the income percentile for each individual within the same cohort. There are two advantages to using percentiles, rather than commonly used log income. First, rank-based measures turn out to be least attenuated and most stable over age (Chetty et al. (2014), Nybom and Stuhler (2017)). Second, as the distribution of percentiles is equal across generations, the rank measure allows us to abstract away from changes in inequality over time. To evaluate whether our estimates change significantly when we measure income at different ages or in a different way, we also present results for various alternative measures.

Education. The education registers contain higher education degrees since 1986 and until 2022. We use these data to construct an indicator of whether an individual has obtained a higher tertiary education degree.[17]

Crime. The crime register data contains all offenses reported to the police between 2004 and 2022. The data contain the reporting date, the offense type, and the individual identifier of the suspected offender(s) whenever there is a known suspect. 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.[18] We distinguish between property crime, violent crime, and other types of crime based on the reported offense types. Since girls commit much fewer crimes on average, we 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 all individuals from the first generation who meet the sample selection criteria discussed above. For panel B we use all their children born before 1991, which are the children we will use for the income and education analyses.[19]

records.

[17]Obtaining a higher education degree before the age of 20 is extremely uncommon. Only 0.5% of the core sample is aged 20 or older in 1986 *and* does not have an available education record. This implies that missing records due to individuals graduating early is no concern. Moreover, less than 500 individuals (0.005%) are still enrolled in higher education in 2022, suggesting that unfinished education is also no concern.

[18]Nonetheless, we report the results for different ages in Appendix A.

[19]For the crime analysis we rely on a different sample of children born between 1986 and 2001. Summary statistics for those outcomes can be found in the tables with the results for crime.

Table 1: Descriptive Statistics

A. First Generation	Birth Order				
	1	2	3	4	5
Year of Birth	1959	1959	1958.8	1958.8	1959.1
Male	51.4	51.2	51.1	51.2	51
Household income percentile	54.9	53.1	52.2	51.4	50.5
Has Child	78.3	77.5	77.7	77.6	77.1
Age at First Child	28.9	28.8	28.6	28.5	28.4
Number of Children	1.7	1.7	1.7	1.7	1.7
N	1,265,529	1,174,049	577,770	246,898	80,845

B. Second Generation	Parental Birth Order				
	1	2	3	4	5
Year of Birth	1981.6	1981.5	1981.6	1981.7	1982.0
Male	51.1	51.1	51.0	51.2	51.0
Household income percentile	55.4	55.2	55.1	55.1	54.8
Higher education completion	40.1	39.5	39.1	38.8	38.8
N	1,111,812	1,017,961	527,009	232,303	74,285

Notes: The sample in Panel A includes all individuals born between 1945 and 1970 who meet the sample selection criteria outlined in Section 3. This panel also includes individuals without children. The total sample size is 3,345,091 individuals, with household income percentiles calculated for the 3,261,683 individuals with non-missing income. 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. The total sample for Panel B includes 2,220,410 unique children, of whom 2,167,656 have non-missing income. The number of observations in Panel B is higher because some children are counted twice: once for their father's birth order and once for their mother's birth order. In total, there are 2,963,370 observations in Panel B, of which 2,894,481 have non-missing incomes. All cells represent sample averages.

Many children in the analysis sample occur once for each parent, and thus twice in the dataset.[20]

The first generation's outcomes show that individuals with a higher birth order on average have a lower income.[21] From the children's outcomes, we observe that children of higher-birth-order parents have lower income and are less often enrolled in higher education. However, these are just correlations. We next explain how we can identify causal (parental) birth order effects.

[20] We explain this in greater detail in Section 4.

[21] The average income percentile across the full sample is different from 50 because we take the income percentiles across the entire labor force in the Netherlands, which also includes dropped individuals such as migrants.

4 Identification

The goal is to estimate the effect of parental birth order on children's outcomes. Comparing children of parents with different birth orders is misleading because higher birth order parents come from larger, and thus likely different, families. To account for this, the previous literature on birth order effects on the first generation either adds a rich set of controls including family size, age-at-birth, and parent's age-at-birth, or, if data allows, uses sibling comparisons. Using within-family variation is particularly credible since it overcomes any confounding factors that arise from between-family comparisons and has the advantage of siblings being ex-ante genetically identical.

Our rich administrative data enables us to use within family variation. To estimate birth order effects on the first generation, we use 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 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. 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. However 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 polygenetic score for education. This suggests that, at least genetically, children of different birth orders are of similar 'quality'.[22] Moreover, in our main results, we 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, we believe such spillovers are unlikely to result in the observed patterns for two reasons. First, most studies find that, if there are spillovers, then 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.[23] 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)) 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 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,

[22]Generally, it can be hard for parents to infer their first children's 'quality' by the time they have another 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 is in contrast with the idea that parents stop having children after observing a problematic child.

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

and $\tau_{t(pf)}$ are parent's year of birth $\times$ month of birth $\times$ gender 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. Since the identifying variation comes from comparing cousins, this model is commonly referred to as a Cousin Fixed Effects (CFE) model.

While the cousin fixed effects framework is conceptually similar to the sibling fixed effects framework, some new considerations arise in the context of intergenerational effects.

Defining the treatment. A child is always the product of two (biological) parents. This means that a child has two parental birth orders: one for each parent. This raises the question of how to define the treatment of a child. Should both parents' birth orders be included in the regression? We emphasize that not the child, but the parent is the treated individual. In this view, a child does not have two treatment statuses, but it occurs twice in the sample: once as the outcome for each parent. In our analysis, we compare children once to cousins from their mother's side and once to cousins from their father's side.[24] These results are averaged into a single treatment effect that captures both father's and mother's birth order. We also explore heterogeneity in the effects by studying the effects for fathers and mothers separately. An important implication of including only one parent's birth order is that the estimated treatment effects include any mediating effects of assortative mating.

Selective fertility at the extensive margin. To understand how a parent's birth order affects children's outcomes, we want to compare children of the same type of parents, but whose birth order is varied exogenously. However, birth order itself can affect whether and how many children to have. When these fertility effects are heterogeneous across groups, they may lead to a non-random association between a parent's birth order and the parent's characteristics. In that case, differences between children of parents with different birth orders may be driven by their parent's characteristics rather than their parent's birth order.[25]

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

[25]A recent example of selective fertility in an intergenerational study is East et al. (2023). They find that white girls who are exposed to Medicaid in-utero tend to have more children themselves, whereas black

While this is a limitation that we share with all studies on intergenerational effects, our family fixed effects approach enables us to rule out many sources of differential fertility. The reason is that siblings (the cousins' parents) share many characteristics, such as their ethnicity and their home environment. Moreover, we show in Table 4 that birth order effects on fertility at the extensive margin are modest, suggesting that selective fertility is unlikely to play an important role.

Selective fertility at the intensive margin. In Table 4 we also show that a higher birth order marginally increases the number of children individuals have and that they are slightly younger when they have their first child. This means that their children have lower birth orders and birth years on average. 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. Therefore, we initially do not control for these outcomes and interpret them as mediators that could affect our estimates. Nevertheless, such indirect effects do not generalize well across settings. For instance, the larger the trend in education, the more significant the indirect effect of a child's year of birth will be. Therefore, in Section 6, we return to this issue and test whether these mediating variables significantly change our estimates. Overall, the results are very similar once children's birth order and year of birth are correctly taken into account.

5 Main Results

5.1 Intergenerational Birth Order Effects on Income

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

girls tend to have fewer children later. This also illustrates that even when total fertility among control and treatment groups is equal, there may still be (unobserved) differences in the characteristics of parents.

(parental) birth order effects estimated by the first generation's family size.

Panel A shows that children of parents with a higher birth order have lower incomes. For example, column 1 shows that children of a third-born parent have 0.6 percentile points lower incomes 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. We also observe that the effects of having a second or third-born parent are similar for all extended family sizes. This is inconsistent with optimal stopping models of fertility, which would suggest that only the last child has particularly bad outcomes.

While the effects in panel A are interesting on their own, they are not informative about *how much* of parents' disadvantages due to their birth order are transmitted to their children. To shed light on this, we present birth order effect estimates on the first generation's incomes in panel B. These estimates show that birth order has large effects on income.[26] Although there is limited prior evidence on birth order effects on income, these results are in line with results in Black et al. (2005b), who report birth order effects on earnings using specifications with rich control variables. 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.

Panel C displays the ratio of intergenerational birth order effects to the birth order effects on the first generation. The estimates in column 1 are centered around 0.2. The results in 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 0.21 (standard error 0.04).[27] We conclude that about 20 percent of the income disadvantages due to a higher birth order

[26] The sample in panel B includes all individuals from the first generation, including those without children. Applying the same regression to the sample of individuals with children yields very similar estimates. Results are available on request.

[27] See Table A5 column (1) for the 2SLS estimate.

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	-0.385***	-0.444***	-0.194	-0.495***	-0.495***
	(0.069)	(0.169)	(0.123)	(0.131)	(0.162)
3	-0.617***		-0.717***	-0.719***	-0.785***
	(0.116)		(0.233)	(0.204)	(0.221)
4	-0.759***			-1.012***	-1.013***
	(0.170)			(0.323)	(0.316)
5	-0.919***				-1.410***
	(0.248)				(0.452)
Mean	55.231	55.119	55.334	55.335	55.087
SD	26.899	27.111	26.931	26.786	26.604
N	2,894,481	863,037	887,515	683,872	460,057
	B. Birth Order Effects (β^{FG})				
2	-1.685***	-1.807***	-1.587***	-1.671***	-1.274***
	(0.048)	(0.102)	(0.082)	(0.103)	(0.147)
3	-2.938***		-3.133***	-2.943***	-2.938***
	(0.082)		(0.150)	(0.149)	(0.184)
4	-3.777***			-4.199***	-3.972***
	(0.121)			(0.230)	(0.250)
5	-4.618***				-5.187***
	(0.174)				(0.351)
Mean	53.451	53.922	53.903	52.872	51.687
SD	27.899	28.023	27.923	27.763	27.577
N	3,261,683	1,201,930	1,037,797	647,668	374,288
	C. Degree of Intergenerational Transmission (β^{SG}/β^{FG})				
2	0.228***	0.245**	0.122	0.296***	0.389***
	(0.042)	(0.096)	(0.078)	(0.081)	(0.134)
3	0.210***		0.229***	0.244***	0.267***
	(0.040)		(0.076)	(0.071)	(0.077)
4	0.201***			0.241***	0.255***
	(0.046)			(0.079)	(0.081)
5	0.199***				0.272***
	(0.055)				(0.089)

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). (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

are transmitted to the next generation.

The degree of transmission is somewhat smaller than the rank-rank correlation between parental income and children's incomes in our sample, which is 0.27. Whether the causal transmission of 0.2 is considered high or low depends on one's priors.

On the one hand, although intergenerational causal effects are often smaller than sample associations, this need not be true for the spillovers of specific childhood experiences. Given that birth order substantially affects cognitive and noncognitive skills, partner selection, and financial resources, initial disadvantages might compound over time, resulting in larger effects for subsequent generations. 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.

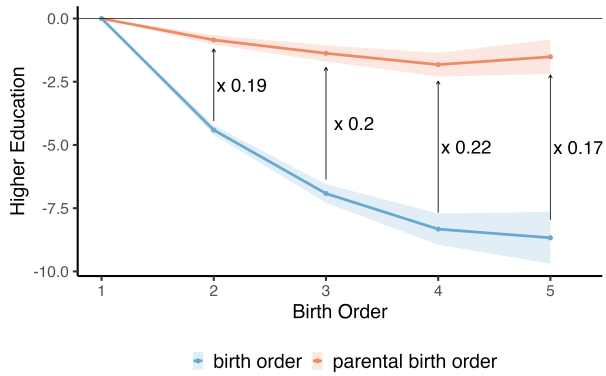
At the same time, the fact that the intergenerational spillovers of birth order are nonzero is also not evident. Multiple papers show that treatments that affect parents do not always result in intergenerational spillovers, or result in considerably smaller spillovers than conventional OLS estimates suggest (Holmlund et al. (2011), Page (Forthcoming)). In that sense, the intergenerational spillovers of birth order are relatively large.

5.2 Intergenerational Birth Order Effects on Education and Crime

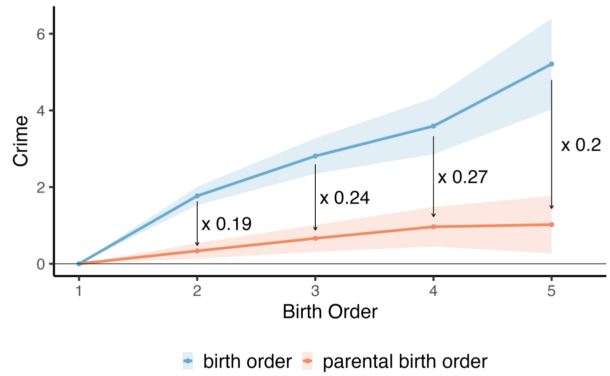
We next present estimates of parental birth order effects on education and crime. In contrast to the findings in the previous section, we do not have the data to examine parents' university enrollment or criminal activity at the same ages as their children, thus precluding a direct calculation of intergenerational transmission. Instead, we compare the parental birth order effects to the second generation's birth order effects.

Figure 1 shows parental birth order effects, birth order effects, and its ratio for both outcomes. In panel (a) we see that parental birth order significantly decreases children's higher education attainment. For example, children of third-born parents are 1.4 percentage points less likely to have a higher education degree than children of first-born parents. In

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



(a) Education



(b) Crime

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. Panel (a) presents results for higher education completion, based on 2,963,370 observations. Panel (b) examines whether a son is suspected of a crime between ages 18 and 20, with a sample size of 1,522,958. The estimates of parental birth order effects for both outcomes can also be found in Table A2 and A3, 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. Although birth order is a discrete variable, the lines have been extrapolated for visual clarity.

panel (b) we see that a higher parental birth order also increases children's likelihood to be suspected of a crime.

The ratios of parental birth order effects to birth order effects are close to 20 percent for both outcomes. Even though the estimates 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 A1 we show that the rise in crime is primarily driven by violent offenses. In this category, the effect sizes can reach as high as 20 percent. We interpret these findings as significant evidence that parental birth order influences children's human capital beyond cognitive abilities.

In Table A2 and A3 we report the results separately by family size. Like in the previous section, the estimates are similar across different family sizes. Additionally, in Table A10 and Table A11, we provide the results for crime in various age groups. While the effects are most prominent around age 18, which is consistent with this being a prime age for boys to engage in criminal activities, the effects are also evident at other ages.

5.3 Gender heterogeneity

Table 3 presents estimates of how birth order effects differ by gender of the parent or the child. We estimate the effects by interacting parental birth order with the gender of the corresponding parent, child, or both. In columns 3 and 4 we also interact the parent's birth-year $\times$ birth-month $\times$ gender with the gender of the child to flexibly control for the main effect of the child's gender and different time trends for boys or girls. We have converted birth order into a numeric variable from 1 to 5. Although the estimates in Table 2 show that birth order effects are not entirely linear, the results do not differ much if we impose this restriction and it makes the estimates more precise and readable.

In panels A and B we see consistent results for children's incomes and education. Paternal birth order effects are somewhat larger than maternal birth order effects, but the differences are not statistically significant. There are only small and insignificant differences between sons or daughters, or their interaction with the gender of the parents. For boys' criminal behavior in panel C, we find a substantially larger effect of paternal birth order than maternal birth order.

As before, it is interesting to relate the parental birth order effects on income to the effects seen in the first generation to calculate the gender-specific degree of intergenerational transmission. To do so, we perform a similar analysis as in Table 2, but now with gender interactions.

First, in Table A4 we show that birth order effects on the first generation's income is larger for men. Although evidence on gender heterogeneity in birth order effects is surprisingly scarce, these results differ from previous estimates by Black et al. (2005b) and Houmark (2023). Black et al. (2005b) use earnings instead of household income, which may be measured with more error, especially for women, and Houmark (2023) consider outcomes during primary school, which may explain these differences. To test whether the effects are genuinely larger for men, we also provide results using completed education in the same table.[28]

[28]Data on education is considerably less complete for older cohorts because for these cohorts the database

Table 3: Intergenerational Birth Order Effects by Gender

	(1)	(2)	(3)	(4)
	A. Income			
Parental Birth Order	-0.273***	-0.243***	-0.261***	-0.211***
	(0.054)	(0.059)	(0.057)	(0.064)
Parental Birth Order × Father		-0.064		-0.108*
		(0.051)		(0.063)
Parental Birth Order × Son			-0.024	-0.064
			(0.036)	(0.048)
Parental Birth Order × Father × Son				0.088
				(0.071)
Mean	55.231	55.231	55.231	55.231
SD	26.899	26.899	26.899	26.899
N	2,894,481	2,894,481	2,894,481	2,894,481
	B. Education			
Parental Birth Order	-0.598***	-0.556***	-0.558***	-0.513***
	(0.093)	(0.101)	(0.098)	(0.110)
Parental Birth Order × Father		-0.091		-0.097
		(0.089)		(0.109)
Parental Birth Order × Son			-0.066	-0.070
			(0.061)	(0.082)
Parental Birth Order × Father × Son				0.009
				(0.123)
Mean	39.583	39.583	39.583	39.583
SD	48.903	48.903	48.903	48.903
N	2,963,370	2,963,370	2,963,370	2,963,370
	C. Crime			
Parental Birth Order	0.298***	0.199*		
	(0.100)	(0.109)		
Parental Birth Order × Father		0.196**		
		(0.087)		
Mean	9.549	9.549		
SD	29.39	29.39		
N	1,522,958	1,522,958		

Notes: Panels A, B, and C report parental birth order effects interacted by gender of the parent, child, or both, on income, education, and crime, respectively all estimated according to equation 2. Regressions in columns (1) and (2) include extended family and parental birth-year × birth-month × gender fixed effects. Regressions in columns (3) and (4) include extended family and parental birth-year × parental birth-month × parental gender × child gender fixed effects. Standard errors are clustered on the extended family and are reported in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Consistent with the results above, we find larger birth order effects on males' completed education.

We compute the relative degree of transmission interacted by gender in Table A5. Although the absolute effect of a father's birth order is somewhat higher, fathers also experience a stronger income effect themselves. The transmission estimates show that these effects cancel each other out so that the relative effects are similar for fathers and mothers. That is, fathers transmit equally much of their income disadvantage due to their birth order as mothers do. The relative degree of transmission is also equal for sons or daughters.

5.4 Sensitivity

We check whether the results differ for two alternative income measures. First, we consider the log of income. This allows us to estimate the birth-order equivalent of the Intergenerational Income Elasticity (IGE). Second, we consider individuals' personal income ranks. These results are reported in Table A7. The degree of intergenerational transmission for the log of income or personal income ranks is also close to 20 percent.

We next investigate sensitivity concerning the age at which income is measured. First, in Table A8 we select all parents for whom we observe incomes between 2003 and 2013 to see whether estimates for the first generation differ if we measure income in different periods. We find that the estimates do not change. Estimating intergenerational birth order effects at varying ages is more difficult because only for children born between 1975 and 1980 we observe all incomes between ages 29 and 41. This leaves too little variation within extended families to reliably estimate the effects. Instead, in Table A9 we use these cohorts to estimate the cross-sectional rank-rank correlation in income, where we measure children's income at varying ages. If there is no evidence of life-cycle bias using this specification, then we are

mostly relies on large-scale surveys instead of administrative records. As a result, we observe only 44 percent of the parents' education records. Nevertheless, the birth order effects we find for this generation are consistent with previous results in Black et al. (2005b) and De Haan (2010), as well as with birth order effects in the second generation. Moreover, as long as the bias due to missing values is unrelated to gender, then the estimates in Table A4 should still be informative of the gender differences of the true effects.

Table 4: Effects on Fertility

Birth Order	Has Child	Number Children	Age at First Child	Year of Birth	Birth Order
	(1)	(2)	(3)	(4)	(5)
2	0.637***	-0.020***	-0.143***	-0.133***	-0.013***
	(0.075)	(0.002)	(0.010)	(0.010)	(0.001)
3	0.608***	-0.043***	-0.225***	-0.242***	-0.027***
	(0.128)	(0.003)	(0.017)	(0.017)	(0.002)
4	0.613***	-0.063***	-0.262***	-0.313***	-0.043***
	(0.188)	(0.005)	(0.025)	(0.025)	(0.003)
5	0.528*	-0.088***	-0.351***	-0.420***	-0.062***
	(0.273)	(0.007)	(0.036)	(0.036)	(0.004)
Mean	77.814	2.195	28.78	1981.591	1.632
SD	41.55	0.869	5.282	6.081	0.802
Generation	1	1	1	2	2
N	3,345,091	2,602,946	2,602,946	2,963,370	2,963,370

Notes: This table presents the effect of birth order on fertility outcomes. The outcome in column 1 is an indicator that equals 1 if an individual has at least one child, 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. The outcomes in columns 4 and 5 measure children's year of birth and birth order. The sample in columns 4 and 5 is the core analysis sample for the second generation. All models are estimated according to equation 2. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

more confident that this is also not an issue for our main results. We find that the estimates do not change if we measure income between ages 29 to 34, 33 to 37, or 37 to 41.

6 Extensions

6.1 The role of fertility

To understand whether differential fertility patterns affect our results, we estimate birth order effects on an indicator of having any children at all, the number of children, and the age at which individuals have their first child.[29] The results are presented in Table 4.

Column 1 shows that birth order increases the likelihood of having at least one child. The magnitude of these effects is very small, however. A higher birth order increases the

[29] We have also estimated the intergenerational birth order effects on child gender, and find no differences.

likelihood of having children by less than one percent. Moreover, this difference is the same for all later-born compared to the first-born, regardless of their birth order. We conclude from this that selective fertility at the extensive margin is unlikely to be a driver of our results. Columns 2 and 3 show that, at the intensive margin, a higher birth order decreases the number of children and the age at which individuals have a first child. As a result, children born to higher birth order parents are born earlier and have a lower birth order themselves. This is shown in columns 4 and 5. We next explore the importance of these mediators in our results.

As there has been a positive trend in education and a negative trend in crime over recent decades, variations in children's birth years could affect our findings. Similarly, as birth order has sizeable effects on income, education, and crime, even minor differences in children's average birth order might alter our results. To determine whether these mediating factors influence our findings, we also estimate models that include year-of-birth and birth-order control variables. However, directly controlling for these after-treatment variables, which are impacted by the treatment, might introduce biases.

Some studies estimating intergenerational effects limit their analysis to first-born children, to ensure that birth-order effects do not influence the results (e.g., Currie and Moretti (2003), Rossin-Slater and Wüst (2020)). This approach has three drawbacks. First, dropping all children of higher birth order can lead to unnecessary losses in statistical power. Second, first-born children are not representative of the entire population, and their response to parental influences may differ from later-born children. For example, Muslimova et al. (2020) show that first-born children benefit disproportionately from inheriting a high polygenetic score. Third, excluding higher birth order children ensures that birth order effects do not impact results, but it does not address the issue of varying birth years.

We propose a simple two-step estimator that consistently estimates an intergenerational treatment effect, accounting for a child's birth year and birth order. In the first step, we use sibling comparisons from the second generation to estimate the year of birth and birth order effects. Because siblings are exposed to the same 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' earnings or education. In the second step, we residualize the children's outcomes using the estimated birth order and year of birth effects. We then estimate the treatment effects on the residuals. Any remaining variation in the residuals that is explained by the treatment must be unrelated to the children's birth order or year of birth.

This two-step estimator is broadly applicable for estimating intergenerational effects in contexts where a treatment affects the number or timing of children. In Appendix B, we explain the two-step estimator in greater detail and demonstrate its application in a Monte Carlo simulation.

In Table A12 we compare our main results with estimates from models that incorporate birth order and year of birth control variables and our two-step estimator. The estimates are very similar across all three specifications and for all outcomes. Consequently, we conclude that our main results are not driven by differences in children's birth order or birth year.

6.2 The role of neighborhoods

The influence of neighborhoods on intergenerational mobility has been the subject of extensive research (Mogstad and Torsvik (2023)). It is plausible that neighborhoods also contribute to our outcomes if parents with higher birth orders tend to settle in neighborhoods of lower quality than their older siblings. To assess this, we investigate whether the neighborhood a child resides in at age 16 is impacted by parental birth order. We use the average household income rank and education of all residents born between 1970 and 1990 as proxies for neighborhood quality.[30] Surprisingly, the findings displayed in Table 5 exhibit economically trivial effects. This suggests that neighborhood dynamics are unlikely to be a driver behind our observed results.

[30] The average and median neighborhood sizes in the year 2000 are 5030 and 2847 residents, respectively.

Table 5: Parental Birth Order Effects on Neighborhood Quality and Moving

Parental Birth Order	Average Income Percentile Neighborhood	Average Education Neighborhood
2	-0.031*	-0.003
	(0.016)	(0.002)
3	-0.043	-0.002
	(0.028)	(0.003)
4	-0.073*	-0.002
	(0.040)	(0.005)
5	-0.072	0.001
	(0.058)	(0.007)
Mean	51.357	14.283
SD	6.337	0.749
N	2,907,099	2,907,099

Notes: This table presents the effect of parental birth order on neighborhood characteristics. A child's neighborhood is based on the address where the child lives at age 16, or, if the child turns 16 before 1995, the address of the mother in 1995. Assignment of addresses to neighborhoods is based on neighborhood (in Dutch: "buurt") codes from Statistics Netherlands. The average income percentile is calculated by averaging the household income percentile of all residents born between 1970 and 1990. The average education is calculated by averaging the years of education of all residents born between 1970 and 1990. The sample includes all children from the second generation whose neighborhoods at age 16 are observed. All models are estimated according to equation 2. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

6.3 The role of assortative mating

We next consider the effect of birth order on partner choice. In particular, does a higher birth order also result in partners with lower income? When preferences for partners are entirely driven by the neighborhood or family individuals grow up in, then birth order will not affect partner choice because siblings share the same neighborhood or family. On the other hand, when individuals find their partners in secondary or subsequent education or during their work, then birth order likely has an effect through the types of schools individuals attend or jobs they have.

To investigate the effect of birth order on assortative mating we estimate the effect of birth order on the other parent's personal income rank.[31] We will focus solely on individuals

[31] Personal income includes all income from labor, entrepreneurship, and income transfers. This measure of income excludes certain types of income that are measured at the household level, such as income from capital and conditional income transfers. As before, we focus on income between ages 29 to 60 and take the average of personal income for the 5 years closest to age 35. We then take the rank of this average relative to the income of all other individuals in the same cohort and gender.

Table 6: Birth Order Effects on Partner's income

	Income Percentile Partner
Birth Order	-0.507***
	(0.050)
Mean	51.852
SD	28.171
N	2,383,591

Notes: This table presents the effect of birth order on a partner's income percentile. Birth order is converted to a numeric variable, ranging from 1 to 5. The model is estimated according to equation 1. The sample consists of all individuals from the first generation with at least one child and whose partner's income rank is available. Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

with children, to align with our focus on intergenerational effects. Table 6 shows that a higher birth order decreases the partner's income percentile. The magnitude of the effect is about a third of birth order effects on own income. We conclude from this that assortative mating slows down the rate at which birth order effects fade out over generations relative to a society in which individuals 'randomly' find partners.

6.4 Complementarities between parents' birth order and children's birth order

One might expect that birth order effects for children vary based on their parents' birth order for two reasons. First, birth order effects may diverge due to dynamic complementarities (Cunha and Heckman (2007)). Dynamic complementarities suggest that the returns on investments are greater for children with higher initial endowments. Muslimova et al. (2020) employs a lower birth order as a proxy for increased parental investments and demonstrates that first-born children disproportionately benefit from a high polygenetic score (the initial endowment). While we cannot observe children's genes, we can use the parents' birth order as an alternative proxy for their initial endowment.[32] The presence of dynamic complementarities would imply that children of first-born parents would gain disproportionately from

[32] Abdellaoui et al. (2022) show that firstborns find partners with higher polygenetic scores. This means that children of first-born parents are likely to have higher polygenetic scores themselves as well. More generally, children of first-born parents have higher educated and wealthier parents, creating higher initial endowments beyond their genetic makeup.

Table 7: Birth Order Effects Interacted with Parental Birth Order

	Income	Education	Crime
Birth Order	-1.204***	-3.646***	1.398***
	(0.115)	(0.192)	(0.250)
Birth Order × Birth Order Parent	0.042	-0.017	0.001
	(0.049)	(0.083)	(0.109)
Mean	55.231	39.583	9.549
SD	26.899	48.903	29.39
N	2,894,481	2,963,370	1,522,958

Notes: This table presents heterogeneity in birth order effects by parental birth order. Birth order and parental birth order are converted to numeric variables from 1 to 5. All models include family fixed effects and birth-year × birth-month × gender × parental birth order fixed effects. Standard errors are in parentheses.
(*** $p < 0.001$, ** $p < 0.01$, * $p < 0.5$)

being first-born themselves.

An alternative mechanism posits that parents with a higher birth order recognize the disadvantages they faced due to their birth order and consider this when raising their own children. They might devote special attention to their later-born children, ensuring they do not experience the disadvantages the parents endured. This mechanism would also suggest that birth order effects are more pronounced for children of first-born parents.

To test these hypotheses, we examine heterogeneity in children's birth order effects by their parents' birth order. For simplicity, we convert the birth order and parental birth order variables to numeric variables. We then fully interact the Sibling Fixed Effects regression (1) with parental birth order to allow for heterogeneity in the effects.[33] The results are presented in Table 7. In line with previous findings, we observe strong birth order effects. However, there appears to be little difference by parental birth order. Consequently, we do not find evidence in favor of dynamic complementarities or compensating behavior from the parents.

[33]Note that there is no coefficient for the effect of parental birth order because it is absorbed by the family fixed effects.

Table 8: (Parental) Birth Order Effects and Sibling Sex Composition

	Income (parent)	Income (child)	Education	Crime
	(1)	(2)	(3)	(4)
Birth Order Parent	-1.186***	-0.302***	-0.809***	0.128
	(0.062)	(0.084)	(0.147)	(0.155)
Birth Order Parent × Male	-0.123	0.116	0.390*	0.322
	(0.092)	(0.128)	(0.224)	(0.223)
Same-sex Birth Order Parent	-0.004	0.084	0.411**	0.117
	(0.076)	(0.101)	(0.176)	(0.182)
Same-sex Birth Order Parent	-0.293**	-0.289	-0.764**	-0.202
× Male	(0.132)	(0.187)	(0.327)	(0.327)
Generation	1	2	2	2
Mean	53.451	55.231	39.583	9.549
SD	27.899	26.899	48.903	29.39
N	3,261,683	2,894,481	2,963,370	1,522,958

Notes: This table presents heterogeneity in birth order effects depending on a child's gender and the gender composition of the older siblings. The birth order of the parent and the birth order of the parent among same-sex siblings are converted into numerical variables from 1 to 5. Column (1) is estimated using individuals from the first generation and includes family and birth-year × birth-month × gender fixed effects. Columns (2) to (4) are estimated using the children from the second generation and include extended family and parental birth-year × birth-month × gender fixed effects. For column (1), 'male' is an indicator that equals one if the individual is a male. For columns (2) to (4), 'male' is an indicator that equals one if the treated parent is a male. Standard errors are in parentheses. (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.5$)

6.5 The role of sibling sex composition

Finally, we ask whether birth order effects depend on the sex composition of the older siblings, and if so, whether this heterogeneity in the effects is also visible in the intergenerational effects. The only paper that studies this source of heterogeneity is Black et al. (2018). They study birth order effects on non-cognitive skills and employment amongst brothers and find larger negative effects for being born late when there are more boys among the older siblings. Because of data limitations, they do not report results for girls.

We first test for heterogeneity in the effects of the first generation. Similar to Black et al. (2018), we parameterize the gender composition of older siblings by using the birth order amongst same-sex siblings in the family, and we interact this with the gender of the child. We add these two variables to the Sibling Fixed Effects model (1) that includes the main birth order effect interacted by the gender of the child. This allows birth order effects to vary for boys and girls and to depend on whether they have more or less older same-sex siblings. Similar to Black et al. (2018) we see that birth order effects are larger for men if they have more older brothers (column 1, Table 8). Including the sibling sex composition does not change the birth order effects for women.

Columns 2 to 4 report heterogeneity in the intergenerational effects. The specification is the same as in column 1, but the outcomes are replaced by the outcomes of siblings' children. The results for income and education are less precise, but the point estimates suggest that intergenerational birth order effects are larger for fathers who have more older brothers. For education, the effects are also larger for mothers who have more brothers. Unfortunately, the estimates for crime are too imprecise to draw conclusions from.

7 Conclusion

In this paper, we explore the intergenerational impact of parental investments on their children's income, education, and crime. We proxy parental investments during childhood with birth order, a childhood experience that affects about 80% of the population. Using rich administrative microdata from Statistics Netherlands we find that around 20 percent of the income disadvantages due to a higher birth order are transmitted to the next generation. We also find decreases in children's education and sizeable increases in boys' criminal behavior, mainly driven by violent offenses.

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.

Although we can rule out the mediating roles of neighborhoods or fertility, there are still many channels through which parents' birth orders can affect their children's human capital. For example, parents with higher birth orders are not only higher educated and richer, but they are also known to have favorable non-cognitive skills, which could be an important aspect of their child-rearing skills. The exact mechanisms driving our results remain an open question and represent an interesting area for future research.

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Appendix A: Supplementary Results

Table A1: Parental Birth Order Effects on Different Types of Crime

Birth Order Parent	Crime	Violent Crime	Property Crime	Other Crime
2	0.321***	0.187**	0.128*	0.053
	(0.123)	(0.082)	(0.073)	(0.092)
3	0.640***	0.426***	0.200	0.170
	(0.213)	(0.143)	(0.127)	(0.158)
4	0.924***	0.571***	0.321*	0.212
	(0.315)	(0.211)	(0.188)	(0.235)
5	0.934**	0.718**	0.465*	-0.224
	(0.456)	(0.307)	(0.273)	(0.337)
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 A2: 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	-0.850***	-1.113***	-0.610***	-0.798***	-1.184***
	(0.120)	(0.292)	(0.213)	(0.229)	(0.287)
3	-1.381***		-1.909***	-1.024***	-1.768***
	(0.201)		(0.400)	(0.354)	(0.394)
4	-1.831***			-1.693***	-2.886***
	(0.296)			(0.555)	(0.559)
5	-1.520***				-3.123***
	(0.431)				(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 A3: 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	0.321***	0.678**	0.123	0.395	0.295
	(0.123)	(0.304)	(0.202)	(0.246)	(0.360)
3	0.640***		0.635	0.544	0.556
	(0.213)		(0.395)	(0.373)	(0.456)
4	0.924***			0.845	1.315**
	(0.315)			(0.591)	(0.639)
5	0.934**				1.564*
	(0.456)				(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 A4: Birth Order Effects on the First Generation by Gender

	Income		Education	
Birth Order	-1.326***	-1.165***	-0.239***	-0.223***
	(0.038)	(0.042)	(0.008)	(0.009)
Birth Order × Male		-0.315***		-0.032***
		(0.036)		(0.007)
Mean	53.451	53.451	13.668	13.668
SD	27.899	27.899	3.432	3.432
N	3,261,683	3,261,683	1,493,903	1,493,903

Notes: This table presents gender heterogeneity in birth order effects on the first generation. Birth order is converted to a numeric variable, ranging from 1 to 5. Income corresponds to the household income rank. The education variable measures the highest level of completed education converted into a years of education variable. All models are estimated according to equation 1, where in columns 2 and 4 the birth order variable is interacted with a gender dummy. Standard errors are in parentheses.
(*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A5: Degree of Intergenerational Transmission of Birth Order Effects by Gender

	Income Rank			
	(1)	(2)	(3)	(4)
Parental Income Rank	0.208***	0.208***	0.201***	0.181***
	(0.04)	(0.051)	(0.043)	(0.055)
Parental Income Rank × Father		-0.001		0.034
		(0.04)		(0.049)
Parental Income Rank × Son			0.014	0.055
			(0.027)	(0.041)
Parental Income Rank × Father × Son				-0.071
				(0.055)

Notes: this table reports the degree of intergenerational transmission in income due to the parent's birth order. The estimates are computed using two-sample 2SLS regressions. The steps are as follows. First, in the first generation sample, the household income rank is regressed on birth order × gender dummies and family and birth-year × birth-month × gender fixed effects. This is the first stage regression. Second, the estimates from the first stage regression are used to compute predicted income ranks of the parents of the children in the second generation sample. Third, in the second generation sample, the children's income rank is regressed on the predicted income rank of the parent and extended family and parental birth-year × birth-month × gender fixed effects. This is the second stage regression. In columns 2, 3, and 4, the instrumented parental income rank is interacted with the gender of the parent, child, and both. The parental birth-year × birth-month × gender fixed effects in columns 3 and 4 are interacted by the gender of the child to control for the main effect of a child's gender. The estimates above correspond to the coefficients of the predicted income ranks and their interactions in the second stage regression. The standard errors are based on the two-sample 2SLS correction in Inoue and Solon (2010). Standard errors are in parentheses. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A6: Effects on Missing Income Information

Parental Birth Order	Missing income (child)	Missing income (parent)
2	-0.042	0.063**
	(0.040)	(0.029)
3	-0.067	0.216***
	(0.067)	(0.050)
4	-0.106	0.302***
	(0.098)	(0.073)
5	-0.116	0.516***
	(0.144)	(0.104)
Mean	2.325	2.493
SD	15.069	15.593
Generation	2	1
N	2,963,370	3,345,091

Notes: This table presents the effect of birth order and parental birth order on missing income. Missing is an indicator of not having any records in the tax returns data. Column 1 is estimated according to equation 1. Column 2 is 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 with Alternative income Measures

(Parental)	Household Income Rank		Log Household Income		Personal Income Rank	
Birth Order	(1)	(2)	(3)	(4)	(5)	(6)
2	-0.385***	-1.685***	-0.008***	-0.033***	-0.334***	-1.595***
	(0.069)	(0.048)	(0.001)	(0.001)	(0.069)	(0.051)
3	-0.617***	-2.938***	-0.013***	-0.057***	-0.485***	-2.883***
	(0.116)	(0.082)	(0.002)	(0.002)	(0.116)	(0.087)
4	-0.759***	-3.777***	-0.016***	-0.074***	-0.608***	-3.747***
	(0.170)	(0.121)	(0.003)	(0.002)	(0.170)	(0.127)
5	-0.919***	-4.618***	-0.020***	-0.089***	-0.434*	-4.423***
	(0.248)	(0.174)	(0.005)	(0.004)	(0.249)	(0.183)
Generation	2	1	2	1	2	1
Mean	55.231	53.451	11.447	11.428	54.264	52.947
SD	26.899	27.899	0.537	0.569	27.148	28.35
N	2,894,481	3,261,683	2,894,481	3,261,683	2,879,399	3,139,552

Notes: this table presents birth order effects and intergenerational birth order effects on various income measures. Standard errors are in parentheses. Columns 1 and 2 are the same as in Column 1 in Table 2. 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. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A8: Birth Order Effects: Measuring income in different periods

Birth Order	(1)	(2)	(3)	(4)
2	-1.492***	-1.497***	-1.488***	-1.492***
	(0.058)	(0.059)	(0.059)	(0.059)
3	-2.646***	-2.664***	-2.678***	-2.657***
	(0.102)	(0.102)	(0.103)	(0.103)
4	-3.442***	-3.456***	-3.482***	-3.440***
	(0.151)	(0.152)	(0.152)	(0.153)
5	-4.167***	-4.218***	-4.209***	-4.049***
	(0.220)	(0.221)	(0.221)	(0.222)
Mean	54.061	54.014	53.873	53.753
SD	27.571	27.681	27.799	27.888
Period of income measurement	5 years closest to age 35	2003-2007	2006-2010	2009 - 2013
N	2,496,938	2,496,938	2,496,938	2,496,938

Notes: this table presents birth order effects on the household income rank. The sample corresponds to all individuals from the first generation who are less than 60 years old in 2013 and have no missing incomes for all years between 2003 and 2013. The outcome in column (1) is the household income rank used in the main results, which is based on the average income over the 5 years closest to age 35. See Section 3 for a detailed explanation. Columns (2) to (4) averages household income over 5 specific years, as indicated in the table. The household income ranks are taken relative to all other individuals of the same age in the Netherlands with a non-missing household income. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A9: Testing for life-cycle bias in child income

	(1)	(2)	(3)	(4)
Rank-rank mobility	0.263***	0.268***	0.255***	0.251***
	(0.001)	(0.001)	(0.001)	(0.001)
Mean	55.867	55.02	55.279	55.101
SD	26.493	26.793	26.797	26.939
Age of income measurement	Default	29-33	33-37	37-41
N	683,165	683,165	683,165	683,165

Notes: this table reports regression coefficients for regressions of the child household income rank on parent household income rank. The sample corresponds to all children from the second generation who were born between 1975 and 1980 and who have non-missing incomes between the ages of 29 to 41. The outcome in column (1) is the household income rank used in the main results, which is based on the average income over the 5 years closest to age 35. See Section 3 for a detailed explanation. Columns (2) to (4) averages household income over 5 specific ages, as indicated in the table. The household income ranks are taken relative to all other individuals of the same age in the Netherlands with a non-missing household income. (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$)

Table A10: Effect of Parental Birth Order on Crime at Other Ages

	Suspected of any crime between ages								
	15-17	16-16	17-19	18-20	19-21	20-22	21-23	22-24	23-25
	A. Excluding year-of-birth and birth-order fixed effects								
Parental	0.178	0.069	0.225*	0.354***	0.206*	0.136	0.031	0.014	0.106
Birth Order	(0.122)	(0.122)	(0.122)	(0.119)	(0.123)	(0.127)	(0.132)	(0.137)	(0.145)
	B. Including year-of-birth and birth-order fixed effects								
Parental	0.234*	0.125	0.302**	0.440***	0.295**	0.234*	0.127	0.107	0.187
Birth Order	(0.121)	(0.122)	(0.121)	(0.118)	(0.123)	(0.126)	(0.131)	(0.137)	(0.144)
Mean	10.531	11.256	11.325	10.885	10.284	9.412	8.534	7.636	6.864
SD	30.695	31.605	31.69	31.145	30.376	29.2	27.939	26.558	25.284
N	969,737	1,071,118	1,169,973	1,267,951	1,167,128	1,065,161	959,329	851,514	739,984

Notes: This table presents the effect of parental birth order on boys' criminal behavior at various ages. Each column shows the effect of parental birth order on the likelihood that a boy is suspected of any crime between the corresponding ages. Parental birth order is converted to a numerical variable ranging from 1 to 5. Since we keep the sample the same, but crime is only observed between 2003 and 2020, the number of observations differs for different age windows. The regressions in panel A are estimated according to equation 2. As criminal behavior is measured in different periods across models, we also include children's year of birth and birth order fixed effects in panel B to net out any differences in time trends. Standard errors are in parentheses (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A11: Effect of Parental Birth Order on Violent Crime at Other Ages

	Suspected of a violent crime between ages								
	15-17	16-18	17-19	18-20	19-21	20-22	21-23	22-24	23-25
	A. Excluding year-of-birth and birth-order fixed effects								
Parental Birth Order	0.113	0.098	0.102	0.211***	0.214***	0.115	-0.024	-0.039	-0.060
	(0.081)	(0.084)	(0.083)	(0.081)	(0.081)	(0.081)	(0.083)	(0.086)	(0.089)
	B. Including year-of-birth and birth-order fixed effects								
Parental Birth Order	0.141*	0.128	0.144*	0.255***	0.257***	0.158**	0.013	-0.004	-0.031
	(0.081)	(0.083)	(0.083)	(0.081)	(0.081)	(0.081)	(0.083)	(0.086)	(0.089)
Mean	3.915	4.536	4.735	4.627	4.133	3.626	3.151	2.762	2.43
SD	19.395	20.808	21.239	21.006	19.906	18.693	17.469	16.389	15.398
N	1,225,487	1,251,199	1,263,764	1,267,951	1,167,128	1,065,161	959,329	851,514	739,984

Notes: This table presents the effect of parental birth order on boys' violent criminal behavior at various ages. Each column shows the effect of parental birth order on the likelihood that a boy is suspected of any violent crime between the corresponding ages. Parental birth order is converted to a numerical variable ranging from 1 to 5. Since we keep the sample the same, but violent crime is only observed between 2003 and 2020, the number of observations differs for different age windows. The regressions in panel A are estimated according to equation 2. As criminal behavior is measured in different periods across models, we also include children's year of birth and birth order fixed effects in panel B to net out any differences in time trends. Standard errors are in parentheses (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

Table A12: Controlling For Children's Year of Birth and Birth Order

Parental Birth Order	Income			Education			Crime		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2	-0.385***	-0.335***	-0.347***	-0.850***	-0.674***	-0.717***	0.321***	0.365***	0.367***
	(0.069)	(0.069)	(0.076)	(0.120)	(0.119)	(0.115)	(0.123)	(0.122)	(0.094)
3	-0.617***	-0.527***	-0.549***	-1.381***	-1.072***	-1.143***	0.640***	0.756***	0.762***
	(0.116)	(0.116)	(0.138)	(0.201)	(0.200)	(0.203)	(0.213)	(0.212)	(0.160)
4	-0.759***	-0.636***	-0.661***	-1.831***	-1.444***	-1.518***	0.924***	1.083***	1.094***
	(0.170)	(0.170)	(0.208)	(0.296)	(0.293)	(0.304)	(0.315)	(0.313)	(0.245)
5	-0.919***	-0.776***	-0.813***	-1.520***	-1.037**	-1.136***	0.934**	1.196***	1.209***
	(0.248)	(0.248)	(0.230)	(0.431)	(0.427)	(0.373)	(0.456)	(0.453)	(0.342)
Mean	55.231	55.231	12.034	39.583	39.583	24.771	9.549	9.549	14.312
SD	26.899	26.899	26.896	48.903	48.903	48.539	29.39	29.39	29.093
Controls		x			x			x	
Two-step			x			x			x
N.	2,894,481	2,894,481	2,894,481	2,963,370	2,963,370	2,963,370	1,522,958	1,522,958	1,522,958

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. Models 1, 4, and 7 are the same as the main results. Models 2, 5, and 8 are estimated according to equation 2 and include the children's year of birth and birth order as control variables. Models 3, 6, and 9 are 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). (*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$)

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

In Table 4 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:

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

where 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. The child's year of birth is a mediator of the total treatment effect since it is affected by the treatment and it affects the outcome as well.

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.[34]

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.[35] 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 4, 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

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

[34] 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.

[35] 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.

consistently estimates the total treatment effect β, which includes the mediating effects of birth order and year of birth. To decompose the effects into direct and indirect effects we consider

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

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

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

   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

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

   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.

A Monte Carlo Simulation exercise. Next, we simulate the performance of the proposed two-step estimator. We model a DGP for the outcome y along the lines of example 3. We use this DGP to illustrate that adding birth order and year of birth as control variables lead to inconsistent estimates, whereas our two-step procedure performs well. We also show that a simple bootstrap procedure recovers the correct standard errors.

We consider the following DGP:

$$\begin{aligned} E[Y_{cp}|x_p, \tau_{cp}, b_{cp}, I_p] &= \beta_0 + \beta_1 x_p + \beta_2 \tau_{cp} + \beta_3 b_{cp} + \beta_4 I_p, \\ E[\tau_{cp}|x_p, I_p] &= \gamma_0 + \gamma_1 x_p + \gamma_2 I_p, \\ E[BO_{cp}|x_p, I_p] &= \delta_0 + \delta_1 x_p + \delta_2 I_p, \end{aligned}$$

where, for simplicity, we assume that treatment x_p, birth order b_{cp} and parental income I_p are dummy variables, τ_{cp} takes values from zero to eleven, and Y_{cp} is a continuous variable. The parameter values are $\{\beta_0, \beta_1, \beta_2, \beta_3, \beta_4\} = \{0, 3, 4, 2, 6\}$, $\{\gamma_0, \gamma_1, \gamma_2\} = \{5, 0.2, 0\}$, and $\{\delta_0, \delta_1, \delta_2\} = \{1.15, 0.1, 0.05\}$. The goal is to identify the *direct* treatment effect $\beta_1 = 3$. A regression of Y_{cp} on x_p only gives the *total* treatment effect $\beta = \beta_1 + \beta_2 \cdot \gamma_1 + \beta_3 \cdot \delta_1 = 4$. To estimate β_1 we apply the two-step estimator.

Regular clustering methods do not yield proper standard error for the two-step estimator because (i) the number of observations in the sample depends on the treatment assignment and (ii) the first step adds additional uncertainty. Instead, we propose the following simple bootstrap procedure. Suppose there are P families in the sample. First, 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 $R = 299$ times and store the resulting estimates in a vector $\hat{\boldsymbol{\beta}}_{\mathbf{1}} = \{\hat{\beta}_1^1, \hat{\beta}_1^2, ..., \hat{\beta}_1^R\}$. 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 $\hat{\boldsymbol{\beta}}_{\mathbf{1}}$. The bootstrap procedure is particularly suitable for the estimation of intergenerational effects because it takes into account that, although the number of parents stays the same, the number of children may differ across different draws.

We generate $r = 500$ samples, each comprising $P = 1000$ parents, according to the DGP above. We then execute three analyses: (i) a regression of the outcome Y_{cp} on the treatment x_p, (ii) the same regression but including controls for a child's year of birth and birth order, and (iii) our two-step estimator. The outcomes are outlined in Table A13. The first three columns show the average coefficients from regressions (i), (ii), and (iii), respectively. The fourth column reports the proportion of samples where the bootstrapped confidence interval contains the true value. Column 1 highlights the proximity of the total treatment effect estimate $\hat{\beta}$ to 4. Column 2 shows that a direct regression accounting for children's birth year and birth order produces a biased estimate $\hat{\beta}_1^c$ of $\beta_1 = 3$. Column 3 shows that, on average, the two-step estimator $\hat{\beta}_1^{TS}$ yields estimates close to the true value. Column 4 shows that the bootstrap procedure performs well.

Table A13: Monte Carlo Simulation Results

$\hat{\beta}$	$\hat{\beta}_1^C$	$\hat{\beta}_1^{TS}$	Fraction of False Rejections
3.950	2.868	2.978	0.054