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ABSTRACT

On the Family Origins of Human Capital Formation: Evidence from Donor Children*

We introduce a novel strategy to study the intergenerational transmission of human capital, net of genetic skill transfers. For this purpose, we use unique data on children conceived through sperm and egg donation in IVF treatments in Denmark. Because the assignment of donors is not selective, the intergenerational human capital estimates allow for a causal nurture interpretation. Once we take account of genes, we find that only the education of mothers matters: the association between mother's education and child test scores is significant and large, whereas the association between father's education and child test scores is insignificant and practically zero.

JEL Classification: I24, J62

Keywords: intergenerational mobility, human capital, donor children

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1 Introduction

Why do better educated parents have better educated children? This question has attracted continuous attention from social scientists for well over a century. Their polar explanations are one of nurture, and one of nature. It is nurture if better educated parents provide a more advantageous environment for their children to do well in school. It is nature if better educated parents have certain genetic advantages that they pass on to their children. Of course, any intermediate explanation is possible too, and arguably more likely.

Previous adoption and twin studies seem to settle on nurture being (somewhat) less important than nature for the formation and intergenerational transmission of human capital (Taubman 1976, Plug and Vijverberg 2003, Plug 2004, Björklund, Lindahl, and Plug 2006, Björklund, Jännti, and Solon 2005, Sacerdote 2002 2007, Cesarini 2010, Cesarini and Visser 2017). There is, however, much uncertainty about the accuracy of these nurture and nature estimates; as noted in recent literature surveys, the adoption and twin strategies used to isolate nurture from nature influences often suffer from identification problems that bias results against the nurture explanation (Björklund and Salvanes 2011, Black and Devereux 2011, Holmlund, Lindahl, and Plug 2011, and Sacerdote 2011).

In this article, we introduce a novel identification strategy to more credibly identify the nurture effect in the intergenerational transmission of education. In particular, we exploit that some children are genetically unrelated to one of their rearing parents because they are conceived through sperm or egg donors in in vitro fertilization (IVF) treatments. Sperm donation refers to fertilization of the mother's egg with the sperm of an anonymous donor man. Resulting children are genetically related to the mother but not the father. Egg donation is like sperm donation in that the children are genetically related to the father but not the mother. In this IVF context, we identify the nurture effect by estimating how the educational outcomes of donor children relate to the educational outcomes of their genetically unrelated parents. Because the assignment of donors is not selective, we can give the corresponding intergenerational mobility estimates a causal nurture effect interpretation, one that captures the nurturing effect of both prenatal and postnatal environments.[1]

[1] In case nurture and nature interact, the nurture effects we estimate represent the average of different nurture effects, which then vary with the innate skills of donor children. When we explore nature-nurture interactions, however, we find little evidence that these interactions matter much for child test score outcomes.

We use several administrative registers to compile our primary sample of IVF children born in Denmark (between 1994-2007) with information on their donor status (conceived through sperm or egg donation), on various test score outcomes from nationwide standardized tests taken throughout their primary and lower secondary school years, and on the educational and labor market characteristics of their parents. This sample allows us to estimate intergenerational associations for donor and nondonor children. In addition, we use the same registers to compile two validity samples of children: one of adopted children, and one of all other children. These samples together allow us to compare intergenerational associations between donor and adopted children (to better assess the role of prenatal and postnatal environments) and between nondonor and all other children born in Denmark in this period (to better assess the generalizability of our findings).

To preview our main results, we find no evidence that the education of fathers matters for their children's test scores, once we take account of their genes. In donor families where children are genetically unrelated to the father, the association between paternal education and child test scores is statistically insignificant and close to zero. By contrast, we find strong evidence that the education of mothers matters, also net of their genes. In donor families where children are genetically unrelated to the mother, we find that children with better educated mothers perform much better on standardized achievement tests. The association between maternal education and child test scores is significant and large, and almost as large as the association we find for mothers and genetically related children.

It is important to know whether these nurture effects, or absence thereof, gathered in donor families generalize to more representative families. We identify two external validity risks: family heterogeneity in intergenerational skill effects and differences in parenting styles. Exploring the empirical relevance of these risks in a setting where children in donor families are genetically related to only one of their parents, we find no evidence that intergenerational skill transmission between parents and genetically related children differs in donor and representative families, nor that parents spend different amounts of time with their genetically unrelated children (in infancy and early childhood). We therefore believe that the nurture effects taken from donor families carry over to more representative families.

The paper unfolds as follows. Section 2 provides the literature background, and lists the main contributions of our study. Section 3 presents the institutional context of IVF treatments in Denmark and the administrative data. Section 4 describes how donors are assigned to IVF treated families. Section 5 introduces the novel strategy to identify the nurture influences of parental education in intergenerational mobility models using donor and nondonor IVF children. Section 6 presents our main set of results. Section 7 compares our results to those obtained using representative and adoption samples. Section 8 concludes.

2 Previous Literature

There is an active literature concerned with estimating the nurturing effect of family background on the skills and educational outcomes of children. See Björklund and Salvanes (2011), Black and Devereux (2011), Holmlund, Lindahl, and Plug (2011), and Sacerdote (2011) for recent literature reviews on the topic. In isolating nurture from nature influences, most studies estimate education associations of either sibling pairs or parent-child pairs using genetically informative samples of twins and adoptees. In this section, we will first summarize the nurture findings from previous sibling and intergenerational studies, before we turn to the main contributions of our study.

Sibling Studies

One line of studies relies on sibling associations. With different sibling pairs sharing different combinations of common genes and environment, researchers have used behavioral genetic models to decompose the overall educational outcome variation into nature and nurture components. The nurture component represents the impact of some latent family background component that captures all the environmental factors shared by siblings.

Studies that use twins identify the nature component from sibling association differences between identical and fraternal twins, which is then used to recover the nurture component. Studies that use adoptees identify the nurture component from sibling associations of either adopted siblings or adopted and non-adopted siblings. These studies generally find that nurture matters, explaining about 10 to 45 percent of the overall variation in the educational attainment.[2] These nurture estimates, however, rely on rather controversial

[2] We have taken sibling associations in education reported elsewhere and constructed a comprehensive set of nurture estimates. With twins, the nurture estimates range from 10 percent (Miller, Mulvey, and Martin 1995), 25-35 percent (Taubman 1976, Jencks and Brown 1977, Lykken et al. 1990, Isacsson 1999, Cesarini 2010), to 45 percent (Ashenfelter and

model assumptions regarding the representativeness of twins and adoptees, siblings (not) affecting one another, gene-environment independency, similarity in (parental) treatments, random partner choice (in case of twins), and random assignment to families (in case of adoptees). Any conclusion based on such nurture estimates should therefore be treated cautiously.[3]

Intergenerational Studies

Another line of studies relies on intergenerational associations. With different parents providing different combinations of genes and environment to their children, researchers have used regression models to link the educational characteristics of parents and children, after taking account of genetic skill transfers. The corresponding estimate expresses the nurturing impact of parental education, which in our case is the causal effect of parental education and any other environmental factor that is correlated with it.

Studies that use identical twin parents identify the nurturing effect from within-twin regressions linking the educational differences of twin parents to the educational differences of their children.[4] Studies that use adoptees identify the nurturing effect from simple cross-sectional regressions linking the educational outcomes of adoptive parents to the educational outcomes of their adopted children. Like the sibling association studies, these studies also find that nurture matters, explaining about 30 percent of the overall intergenerational association in education for mothers, and about 60 percent for fathers.[5]

Krueger 1994). With adoptees, the nurture estimates equally vary and range from 10 percent (Scarr and Weinberg 1994), 20 percent (Lichtenstein, Pedersen, and McClearn 1992, Cesarini 2010), to 45 percent (Teasdale and Owen 1984). Recent adoption studies that account for random assignment of adoptees to families find that nurture explains about 15 percent of the overall education variability (Sacerdote 2007, Fagereng, Mogstad, and Rønning 2021).

[3]Recent genome-wide association studies (GWAS), which estimate the nature component directly from genetic information, find much smaller nature estimates than those reported in twin and adoption studies. The study of Lee et al. (2018), for instance, attribute about 10 to 15 percent of all the variance in education (measured in years of schooling) to nature based on the polygenic scores of more than a million white/caucasian individuals. GWA designs, however, fail to capture all the relevant genetic variation, so the corresponding nature estimates likely underestimate the role of nature (Young 2019).

[4]This nurturing interpretation of within-twin estimates relies on an older argument that within-twin strategies used to identify the causal impact of parental education fail to account for all the relevant non-heritable ability differences between twins. These strategies therefore produce within-twin estimates that capture the impact of parental education and all the unshared non-heritable abilities that are correlated with it (Griliches 1979).

[5]Analogous to the sibling associations we summarized earlier, we have collected intergenerational associations in education from twin and adoption studies and expressed the corresponding nurture effect estimates as fractions of the overall (cross-sectional) intergenerational associations in education. With twin parents, we find that most fraction estimates range from

While these regression models provide nurture estimates that are easier to interpret than those provided by behavioral genetic decomposition models, there are limitations that may bias these nurture effect estimates downwards. The main limitation in twin studies relates to the non-heritable traits that twins share. If the differencing of twins not only differences out all the heritable traits but also some of the non-heritable traits that twins share, as Griliches (1979) and others have intermittently argued, the within-twin nurture effect estimates likely understate the nurturing influence of parental education. And similarly, the main limitation in adoption studies relates to the early childhood environment that adoptees may miss. If there is an important role for prenatal and early childhood conditions in explaining child outcomes, as Almond, Currie, and Duque (2018) and others have repeatedly found, the nurture effect estimates taken from adoptees will also understate the true impact of nurture on those outcomes.[6]

Our Contributions

Our study on children conceived through sperm and egg donations, when viewed as embryo adoptions, most closely relates to the recent adoption studies, which account for non-random assignment and investigate how educational and wealth outcomes of adopted children relate to the educational outcomes of their rearing parents (Björklund, Lindahl, and Plug 2006, Sacerdote 2007, Hægeland et al. 2010, Holmlund, Lindahl and Plug 2011, Black et al. 2019, Fagereng, Mogstad, and Rønning 2021). Our study is complementary to these adoption studies in two important ways. First, these adoption studies estimate nurture effects on samples of children who are adopted during early childhood (up to six to eighteen months). Our study estimates nurture effects on samples of donor children who are transferred into the womb 3 to 5 days after a successful fertilization. This means that, unlike the adoption studies, our nurture effect estimates capture

25 to 40 percent for mothers, and from 30 to 75 percent for fathers (Behrman and Rosenzweig 2002, Bingley, Christensen, and Jensen 2009, Holmlund, Lindahl, and Plug 2011, Pronzato 2012, Amin, Lundborg, and Rooth 2015). With adoptees, these fraction estimates are somewhat higher, ranging from 50 to 65 percent for mothers, and from 65 to 85 percent for fathers (Dearden, Machin, and Reed 1997, Sacerdote 2000, Plug 2004, Holmlund, Lindahl, and Plug 2011). Recent adoption studies that more carefully account for the random assignment of adoptees to families report smaller fraction estimates of about 30 percent for mothers, and about 45 percent for fathers (Björklund, Lindahl, and Plug 2006, Sacerdote 2007, Black et al. 2019).

[6] Other limitations relate to the larger impact of measurement error on within-twin estimates, the non-random assignment of adopted children into adoptive families, and the lack of representativeness of samples of twins and adoptees.

prenatal and very early childhood influences. Second, the adoption studies rely on parents and children that bear little resemblance to any other sample of (representative) parents and children.[7] Our study uses samples of donor children born and raised by IVF parents treated with either eggs from other IVF treated mothers or sperm from Danish sperm donors (with arguably comparable traits).[8] This means that our study has a somewhat greater representation than the adoption studies.

Compared to these adoption studies, however, our study also comes with three limitations. First, IVF treatments based on donors are quite rare which means that we work with relatively small samples.[9] Second, IVF treatments based on donors are quite recent interventions which means that most donor children are too young to measure their performance in terms of realized educational attainment and labor market outcomes. Instead, we work with intermediate school outcomes and measure the children's performance in terms of test score outcomes from national tests taken in primary and lower secondary education. Comparable test scores taken in primary education are often found to be strong predictors for outcomes that are realized later in life such as final exam scores, educational attainment and labor market earnings (Beuchert and Nandrup 2018, Woessmann 2018). Third, IVF treatments in Denmark are based on either egg or sperm donors, but never both, which means that the empirical strategy we propose only takes account of the genes of one parent. While this is a disadvantage whenever parents match nonrandomly, later in the paper we show that the estimated nurture effect estimates do not suffer much

[7]It is difficult to compare adoption and non-adoption families for a number of reasons. Adoptees are less comparable in that they are separated from their birth parents, possibly with traumatizing effects (Brodzinski 1987). Adoptees are also less comparable in that they are assigned to rearing parents that are very different from themselves. International adoptees (including Korean-born adoptees) look distinctively different from their rearing parents. National adoptees may look more similar to their parents, but often have distinctively different backgrounds. Björklund, Lindahl, and Plug (2006), for instance, document that Swedish-born adoptees are mostly born in less-advantaged families but placed in more-advantaged families.

[8]Nonexperimental evidence suggests that donor-treated parents under less restrictive donor assignment rules tend to choose donors who resemble the (infertile) partner (Nielsen, Pedersen, and Lauritsen 1995).

[9]This small sample limitation is shared with many twin and adoption studies on the topic. Examples of the earlier intergenerational twin and adoption studies are Behrman and Rosenzweig (2002), Sacerdote (2002), and Plug (2004). Behrman and Rosenzweig (2002) identify the intergenerational transmission of human capital in the Minnesota Twin Registry (MTR) by taking differences between identical twin parents with different levels of education; this is the case for 66 pairs of twin fathers and 87 pairs of twin mothers. Sacerdote (2002) identifies the nurturing impact of father education on child test scores and college attendance using 81 adoptees taken from the British National Child Development Survey (NCDS). Plug (2004) relies on 610 adoptees in the Wisconsin Longitudinal Survey (WLS) to estimate how parental education impacts child education through the family environment

from assortative mating bias (see table 4 panels A and B). Instead, we take advantage of this disadvantage; that is, we can test the external validity of this natural donor experiment by comparing the intergenerational skill associations obtained with children in donor families and their genetically related parents to those obtained with more representative children and parents.

In terms of empirical approach, we are aware of only one other nurture study that make use of children conceived through sperm and egg donations. Frances Rice et al. (2009) examine how prenatal smoking affects child outcomes using a survey sample of IVF-treated mothers with and without donor children treated in several UK and US fertility clinics. Their main finding is that prenatal smoking reduces birth weight in genetically unrelated and related children. Their approach has two potential problems: the assignment of donors to recipient mothers may not be random, and survey response may be selective. Our study differs from theirs in focus and approach. We are not only asking a different question, but we are also answering it with a more convincing empirical approach and better data. We exploit the quasi-random assignment of egg donors to more credibly identify the mother-child skill relationship, net of genetic skill transfers. We use an administrative sample with information on all donor treated mothers in Denmark.

3 IVF in Denmark: Institutions and Data

In this section, we describe the institutional setting for IVF treatments in Denmark, with emphasis on the use of donor eggs and sperm, and discuss how we construct our data from several administrative registers.

IVF Institutions

Danish couples who experience fertility problems typically visit their general practitioner for medical advice and fertility testing. When childless couples are medically diagnosed as infertile, their general practitioner can refer them to a fertility clinic or hospital. In case the women in infertile couples are below the age of 40, they are entitled to three IVF treatments at no cost.[10] Each

[10]While Danish law has set 46 as the maximum treatment age, public clinics generally use 40 as the threshold. The annual costs for IVF-related medication is born by the couple and amounts to about 4,000 DKK annually (which corresponds to US$640 in 2016). Free IVF treatments applies to first-born children only. In our study, single women were not allowed to undergo IVF treatments. A law change in 2007, however, made it possible for single women to undergo IVF treatment with donor sperm.

year, about 2,500 couples start an IVF-treatment and the average success rate per IVF treatment is 25-30 percent. Most couples undergo 3 to 4 treatments, leading to an overall success rate of 70-75 percent.

The standard IVF procedure works by collecting eggs, fertilizing eggs with sperm in a laboratory environment, and implanting the most promising embryo(s) back into the womb. Most IVF treatments involve the couples' own eggs and sperm. Some IVF treatments, however, involve either donor eggs or donor sperm. Danish law prohibits the fertilization of donor eggs with donor sperm, the argument being that the child should be genetically related to at least one of the parents. This implies that children conceived with donor eggs are genetically unrelated to their mother but genetically related to their father, and vice versa.

Infertile couples may need donors for a number of reasons: some women experience premature menopause, are born without ovaries, or had their ovaries removed; some men experience low sperm counts, or tube blockages; and some women and men have damaged reproductive organs (due to, for instance, previous cancer treatments) or carry possible dangerous genetic diseases. For those couples unable to produce viable eggs or sperm on their own, IVF treatments with donors may be the best alternative to get pregnant and have children.

The process of using donors in Denmark is highly regulated. Over the period we consider, egg and sperm donations were anonymous. Donor recipients (as well as the children born with donated eggs or sperm) were not informed about the identity of their donor, and vice versa, donors were not informed about the identity of their donor recipient. Donors were neither informed about the outcome of the treatment (that is, whether their donated eggs or sperm resulted in a pregnancy) nor could they claim legal parenthood over the children born with their donated eggs or sperm. Parents of donor children are encouraged to tell their children about their donor status around the age of five but are not legally obliged to do so.

Danish law also regulates who can become an egg donor. Over the study period we consider, only women who underwent IVF treatment themselves were allowed to donate eggs. Donor eggs were thus *surplus eggs* from IVF treated women who produced more eggs than needed for their own IVF treatment. To ensure donor egg quality, egg donor candidates were medically screened before they could donate eggs and, once approved, egg donors had to be younger than 35 when they donated eggs. Monetary compensation was not allowed. Only few women volunteer to donate eggs; over the 1995-2007 period, there was a donor egg shortage (donor egg demand exceeded donor egg supply).

Danish law is less restrictive for sperm donors. While the law dictated that, in IVF treatments, only clinics could buy donor sperm from anonymous donors, most men were allowed to donate sperm. Like egg donor candidates, sperm donor candidates were medically screened. Sperm donor candidates with a family history of serious hereditary mental and physical disorders were rejected. Once approved, candidates were repeatedly tested for infectious diseases for the full duration of the donation period. For sperm donations, a small monetary compensation was allowed. Many men donate sperm; over the 1995-2007 period, sperm banks held enough sperm to treat all infertile couples in need of donor sperm (donor sperm supply exceeded donor sperm demand).

For infertile couples treated with donor eggs, IVF clinics follow a three-stage IVF procedure. The first stage involves a hormone medication treatment to prepare the uterus for egg reception. The second stage involves the fertilization of donor eggs with sperm of the recipient's husband or partner. The third and final stage involves the fertilized donor-egg implantation into the recipient's uterus. Given the shortage of donor eggs, couples had to wait several years before they received treatment. In 2008, for instance, the average waiting time was 3.2 years (Larsen et al. 2009). For infertile couples treated with donor sperm, however, IVF clinics buy their donor sperm from sperm banks and just follow the standard IVF procedure. Given the excess supply of donor sperm, there were no waiting lists for IVF treatments with donor sperm.

IVF Register

In our empirical analyses, we exploit data from the Danish IVF register, currently held by the Danish Health Data Authority (Sundhedsdatastyrelsen).[11] The register covers information on IVF treatments taking place in public and private fertility clinics and hospitals in Denmark. The register is complete for the period 1994-2005 when reporting of IVF treatments was mandatory. We focus on couples who underwent IVF treatment in the period 1994 to 2007. It covers information on the main reason for infertility, the mode of treatment, the use of donor eggs and donor sperm, the number of eggs retrieved from the womb, the number of fertilized eggs transferred back, the date of treatment and clinic identifiers. It also records whether treated women agreed to donate eggs and, if

[11] Lundborg, Plug, and Rasmussen (2017) analyze the IVF register in another context: they exploit IVF treatment success at the first IVF treatment as a natural experiment to estimate the causal effect of having children on the career of women.

so, how many. We have merged the IVF register to other administrative registers to get longitudinal information on standard demographic variables (including birth year, gender, Danish citizenship, marital status, number of children, and education) running from 1991 to 2016 and standard labor market variables (including labor force status and annual earnings) running from 1991 to 2012.

To study intergenerational mobility patterns in education, we require educational outcomes of IVF-treated parents and their children. We use data from the Danish Education Register, which holds records on educational achievement in primary, lower and upper secondary, and tertiary education from the early 1970s onwards. For parents, we observe realized educational outcomes and take years of schooling as our main parental outcome. For their children, we observe test scores taken from multiple nationwide tests (including 4 tests in reading and 2 tests in math) that were introduced in Danish primary and lower secondary schools in 2010. Appendix Table A gives an overview over the years in which the various tests were taken. Most children in our sample window have taken 3 to 4 tests. Our main child outcome is the average of all available standardized test scores in reading and math.[12]

Our main analysis sample is restricted to those IVF children for whom we observe the score of at least one nationwide test. We select 19,509 children in the IVF register, who were not conceived through either donor sperm or donor eggs, 820 sperm donor children, and 157 egg donor children. For the egg donor sample, we are able to match (with some certainty) a sizable fraction of children to their egg donors; that is, we have information on egg donors for 97 children. See Section 4 for details.

For comparison purposes, we construct two additional samples. The first sample is the representative sample, which is a 30 percent random sample of families with nonadopted children born in similar years as the IVF children. The second sample is our adoption sample, which is a 30 percent random sample of families with children adopted from abroad. We observe 650,930 nonadopted and non-IVF children and 2,674 adopted children. Of these adoptees, 232 were adopted from South-Korea.[13]

[12]While we can separately examine test score performance in reading and math, we prefer the overall average of multiple test scores as the main child outcome for two reasons. First, and most importantly, it raises precision and reduces the influence of outliers. Second, it accounts for parental influences that possibly spill over across the different tests. We report separate results for math and reading in the sensitivity section, however.

[13]Previous adoption studies with Korean-born adoptees can approximate quasi-random assignment of children to families by exploiting a first-come first-serve principle in the adoption application process (Sacerdote 2007, Hægeland et al. 2010, and Fagereng, Mogstadt, and

Table 1
Summary statistics

	non donor children	sperm donor children	egg donor children	all other children	all adopted children	Korean adopted children
	(1)	(2)	(3)	(4)	(5)	(6)
	Child characteristics and outcomes:					
standardized test score	0.12	0.26	0.17	0.01	–0.03	0.21
male	0.51	0.53	0.56	0.51	0.44	0.66
birth year	2001.86	2001.41	2000.18	2001.05	2001.75	2001.65
number of children	19,509	820	157	650,930	2,674	232
	Parent characteristics and outcomes:					
Pretreatment characteristics						
years of schooling father	12.90	12.71	13.04	12.59	13.16	13.17
college education father (1/0)	0.30	0.29	0.36	0.25	0.37	0.36
Danish citizenship father (1/0)	0.95	0.97	0.95	0.89	0.97	0.97
birth year father	1966.43	1964.87	1962.82	1968.25	1965.99	1963.63
years of schooling mother	13.28	13.16	12.75	12.98	13.54	13.81
college education mother (1/0)	0.41	0.39	0.32	0.37	0.48	0.52
Danish citizenship mother (1/0)	0.94	0.95	0.95	0.89	0.97	0.99
birth year mother	1968.94	1968.79	1964.13	1970.83	1967.64	1964.65
years of schooling egg donor	–	–	12.88	–	–	–
college education egg donor	–	–	0.32	–	–	–
Danish citizenship egg donor (1/0)	–	–	0.97	–	–	–
birth year egg donor	–	–	1968.30	–	–	–
missing information egg donor	–	–	0.36	–	–	–
Posttreatment outcomes						
(first 5 years following child birth)						
change in annual earnings father	0.08	–0.00	0.10	–	–	–
parental leave days father	19.22	16.77	12.12	16.52	27.68	–
change in annual earnings mother	–0.10	–0.17	–0.09	–	–	–
parental leave days mother	333.78	298.46	297.65	272.33	254.22	–
divorce	0.17	0.21	0.11	0.27	0.34	–
number of children	19,509	820	157	650,930	2,674	232
number of mothers	14,200	617	127	406,109	2,328	196

Note–The table shows descriptive statistics for different intergenerational samples of children with test scores: (i) all nondonor IVF children; (ii) all sperm donor IVF children; (iii) all egg donor IVF children; (iv) a representative sample of all other children; (v) a representative sample of adopted children; and (vi) a representative sample of adopted children from South Korea. Appendix Table B contains the definition of all variables

Table 1 provides sample means for the intergenerational samples. We make three informative comparisons. First, IVF children, and donor children in particular, perform much better in nationwide tests than children in the representative sample. Compared to these representative children, for instance, we find that IVF children have 0.11-0.25 standard deviation higher test scores. Second, IVF treated parents and representative parents tend to be different. IVF treated parents are older than the representative parents. IVF treated parents are in more stable relationships than the representative parents. When we look at education, which is the parental characteristic at the center of this study, the differences are less pronounced. IVF treated fathers (not mothers) are better educated, but not by much, than the representative fathers. When compared to the education levels of parents in the adoption samples, however, IVF treated parents and representative parents appear much more similar than different. And third, we find that, among IVF treatments with egg donors, donor recipients are much older than egg donors, which is consistent with the age restriction of 35 imposed on egg donors. But when it comes to educational attainment, treated women and their donors are nearly identical.

4 Assignment of Donors to Families

In this section we document how the sperm and eggs of donors were assigned to Danish couples in IVF treatments. We are particularly interested in the extent to which donor assignment occurs randomly.

The Donor Assignment Process

As we mentioned above, donor assignment is bound by strict rules on donor anonymity. While donors are strictly anonymous, prospective parents can state their preferences for donors on five dimensions: skin color, hair color, eye color, weight and length. These donor preferences are expressed in a donor market with an excess supply of sperm donors and excess demand for egg donors, which implies that donor preferences are likely met for sperm donors, but not for egg donors.

With an excess supply of sperm donors, sperm donors are assigned based on the prospective parents' preferences, which makes donor assignment in principle

Rønning 2021). For comparison purposes, we also present results for Korean-born adoptees for whom the first-come first-serve principle applies. We need to assume random assignment, though, because we do not know the application position of prospective adoptive parents.

random conditional on these stated preferences. The IVF register does not record these preferences. In any case, our intergenerational results for fathers are not consistent with any strong selection based on donor preferences, as we will see later.

With an excess demand for egg donors, prospective parents in need of an egg donor are placed on a waiting list. Fertility clinics organize their own waiting lists. There are in total 21 fertility clinics (including hospitals that offer IVF treatments). Prospective parents choose one fertility clinic, which together with the shortage of eggs means that donor assignment depends on the position on the clinic-specific waiting list rather than donor preferences. To substantiate this claim, we quote from the guidelines of one of the largest IVF clinics in Denmark (Ciconia): "[B]ecause of the long waiting time, it is not possible to match physical characteristics. You are offered donor eggs in the same order as you have been put on the waiting list." (Ciconia 2015). The IVF register contains detailed records of the date and place of the IVF treatment. If we take the date of the first donor treatment (measured in calendar months) and a full set of clinic indicators to accurately proxy the clinic-specific waiting list order, the assignment of donor eggs to prospective parents should be as good as random conditional on the calendar month of first donor treatment and clinic fixed effects.

Is Donor Assignment Conditionally Random (for Women)?

With the IVF register at hand, we can identify (with some certainty) the recipients' egg donor and empirically assess whether egg donor assignment is conditionally random. In our IVF context where the nurturing influence of parental education is the treatment of interest, the natural verification test is to link the educational attainment of donor recipients to the educational attainment (or any other pre-assignment characteristic) of their donors. Conditional random assignment would predict zero associations, that is, after taking account of the assignment control variables.

The IVF register contains information on women who provide donor eggs and women who receive fertilized donor egg implants. For egg donors, the register records the number of donor eggs extracted and the extraction date. For donor egg recipients, the register records the number of donor eggs implanted and the treatment date, which may represent either the egg-implantation date (which occurs after the donors' extraction date) or the preparation-for-implantation date (which occurs before the donors' extraction date). With the treatment histories, we can link donor egg recipients to their egg donors based on the (correct) premise that fertility clinics take their egg donors from the women they treat and predominantly use fresh (fertilized) donor eggs as egg implants.[14] We define a match (with some certainty) if donor egg recipients and egg donors are treated at the same clinic and the recipients' treatment date occurs within one week after the donors' extraction date (when recipients receive the embryo implants) or within seven weeks before the donors' extraction date (when recipients receive medication to prepare their uterus for pregnancy and egg implants). The matched pairs constitute our verification sample. Over the 1994-2007 period, we are able to identify 533 matches treated at the same clinic at the same period. With matched-pair observations, egg donors and egg donor recipients may enter the sample several times. If egg donors produce enough viable donor eggs, they can serve multiple donor recipients. If previous IVF attempts with donor eggs failed, egg donor recipients may be treated with eggs from multiple donors (in multiple treatments). There are, in total, 364 different donor recipients and 419 different egg donors.[15]

Table 2 presents the test results. In particular, we estimate how the pre-assignment characteristics of egg donors (education, age, and Danish citizenship) relate to the educational attainment of egg donor recipients after controlling for the waiting list variables (month of first donor treatment and clinic fixed effects). In columns 1, 4 and 7, we regress the three donor outcomes on recipient years of education and waiting list controls. In columns 2, 5 and 8, we augment the regression with recipient birth year and citizenship. In columns 3, 6 and 9, we additionally control for the recipient's partner years of education, birth year and citizenship.We find that, conditional on the recipient's position on the waiting list, there is no relationship between the three donor characteristics and the recipient's education. All the education estimates are statistically insignificant

[14]Over the period we study, most treatments involve fresh embryo transfers. For the years 1994-1995, Westergaard et al. (2000) report that 90 percent of all embryo transfers were fresh embryo transfers. For the years 2006-2011, when we have information on fresh and frozen embryo transfers in IVF treatments, 86 percent of egg donor treatments made use of fresh (fertilized) eggs.

[15]The egg donor recipients in the verification sample only partially overlap with those in the intergenerational sample. The verification sample, for instance, contains more egg-donor recipients because we sampled all egg donor recipients including those whose donor egg implants did not result in children and those whose children had no available test scores. Of the 533 donor treatments in the verification sample, only 118 were successful and lead to children. We observe test scores for 97 children. The intergenerational sample, on the other hand, contains more egg-donor recipients with children because we could not match all egg-donor recipients to their donors.

Table 2
Relationship between egg donor characteristics and donor recipient education

	donor years of education			**donor age**			**donor citizenship**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
recipient years of education	–0.002	–0.004	–0.006	–0.012	0.014	0.003	–0.002	–0.002	–0.003
	0.031	*0.029*	*0.038*	*0.058*	*0.058*	*0.062*	*0.002*	*0.002*	*0.003*
recipient birth year		0.029	–0.016		–0.015	0.013		–0.0001	–0.001
		*0.013***	*0.022*		*0.032*	*0.029*		*0.001*	*0.001*
recipient citizenship		0.116	0.103		0.045	0.018		0.048	0.028
		0.264	*0.467*		*0.370*	*0.545*		*0.033*	*0.022*
partner years of education			0.020			–0.042			0.003
			0.049			*0.038*			*0.003*
partner birth year			–0.011			–0.029			–0.0003
			0.022			*0.028*			*0.002*
partner citizenship			0.072			0.288			0.018
			0.622			*0.592*			*0.021*
R-squared	0.008	0.020	0.025	0.005	0.007	0.008	0.003	0.011	0.019
F-test (joint significance)		1.93	1.96		0.10	1.00		0.93	2.05
number of observations	533	533	533	533	533	533	533	533	533
number of egg donors	419	419	419	419	419	419	419	419	419
number of donor recipients	364	364	364	364	364	364	364	364	364
clinic FE and treatment date controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Note–The dependent variables are egg donor years of education, age and Danish citizenship. In columns 1, 4 and 7, we regress donor characteristics on recipient years of education. In columns 2, 5 and 8, we add recipient birth year and Danish citizenship. In columns 3, 6 and 9, we add the recipient's partner years of schooling, birth year and citizenship (or partner averages of recipients treated in the same year and same clinic if partner information is missing). All regressions control for the first donor treatment date (in calendar months) and a full set of clinic indicators. The F-test reports whether the recipient characteristics are jointly significant (all values indicate statistically insignificance). Standard errors are clustered by recipients and shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level.

and close to zero. We also find that there is no relationship between the three donor characteristics and almost all other characteristics of the recipient and her partner. Test results are, in general, as one would expect with conditional random assignment of donor eggs to donor recipients.

5 Empirical Strategy

In our main analysis, we take the sample of IVF treated families and distinguish parents of nondonor children from parents of donor children. We begin by presenting a simple reduced-form intergenerational mobility model where both parents influence the educational achievement of their nondonor children

$$Y_i^c = \alpha^c + \alpha^m Y_i^m + \alpha^f Y_i^f + \delta' X_i + e_i^c. \tag{1}$$

In this regression model Y_i^c represents an intermediate educational achievement outcome (measured in nationwide achievement tests in primary and lower secondary education) of child c born and raised in family i with mother m and father f, Y_i^m and Y_i^f represent the educational achievement of the child's mother and father (measured in completed years of schooling), X_i represents a set of pre-determined child, family, and treatment variables (including the child's gender and birth year, the mother's and father's birth year and citizenship, the first treatment date and a full set of clinic indicators), and e_i^c represents exogenous child-specific characteristics. We measure birth year in continuous years and first treatment date in continuous calendar months (because of sample size considerations). The intergenerational coefficients α^f and α^m measure the intergenerational association between the educational achievement of genetically related children and parents and represent an unknown blend of nurture and nature influences. With samples of nondonor children, ordinary least squares (OLS) estimation of (1) yields estimates of α^f and α^m.

Our data on donor children allow us to isolate the nurturing component from the intergenerational coefficients α^m and α^f. To explain how we identify the nurturing effects, we introduce a simple hypothetical intergenerational transmission model, akin to the transmission model of Björklund, Lindahl, and Plug (2006), where all four parents can influence the child's education

$$Y_{ijk}^c = \beta^c + \beta^m Y_i^m + \gamma^m y_j^m + \beta^f Y_i^f + \gamma^f y_k^f + \theta' X_i + e_{ijk}^c, \tag{2}$$

where the subscripts c, i, j and k stand for child c raised in family i is conceived by egg donor j and sperm donor k, the superscripts m and f stand for the child's mother and father, Y_i^m and Y_i^f represent the observable measures of the educational achievement of the child's rearing (and genetically unrelated) mothers and fathers, y_j^m and y_k^f represent the unobservable measures of the educational achievement of the child's genetically related egg- and sperm-donor providers, X_i represents a set of pre-determined child, family, and treatment variables (as defined in (1)), and e_{ijk}^c represents unobserved child-specific characteristics.[16]

The intergenerational coefficients γ^f and γ^m measure the intergenerational associations between the educational achievement of the child and donors and represent the nature effects. The intergenerational coefficients β^f and β^m measure the intergenerational associations between the educational achievement of the child and genetically unrelated parents and represent the nurture effects. These nurture effects, which are the prime targets of estimation, must be interpreted broadly and capture the causal influence of parental education and any other unobserved parenting/nurturing skill that is correlated with it; that is, the nurture effects capture the influence of those parenting/nurturing skills that can be both the cause and consequence of parental education.

Before turning to the identification of nurture effects β^f and β^m using samples of donor children, we first reflect on the information we have on the parents. We have complete information on the rearing mothers and fathers. We have incomplete information on the egg donors (that is, we are able to match 64 percent of the children to their egg donors). We have no information on sperm donors. Our intergenerational analysis therefore centers around the rearing mothers and fathers and treats egg and sperm donors as omitted variables (unless indicated otherwise). This will cause no problem for identifying the nurture effect for mothers, but may complicate the identification of the nurture effect for fathers.

Identifying Nurture Effects (Internal Validity)

The identification of nurture effects β^f and β^m on samples of donor children without (using) information on donors must assume that donor assignment in IVF treatments is either random or related to variables that we observe and control for. The argument is simple: under (conditional) random donor assignment, we know that the characteristics of rearing parents and their donors are

[16]While this is clearly a very simplified description of the real world (it ignores, for instance, that the test scores of children can be affected by possible interactions between nature and nurture, which we discuss later in this section), Björklund, Lindahl, and Plug (2006) show that this very simple model characterizes the intergenerational transmission of education surprisingly well, at least for Swedes born in 1962-1966.

independent and that our nurture effect estimates are not affected once we exclude (or include) donor characteristics from (in) the intergenerational mobility regressions.

In case of egg donor children raised in family i with genetically unrelated mother i, egg donor j, and genetically related father i (with identical Y^f and y^f), donor assignment follows a clinic-specific waiting list principle. With controls for first treatment date (measured in calendar months) and a full set of clinic indicators to account for the position mothers take on the waiting list, we can ignore the influence of the donor's genes, as if γ^m is zero, and rewrite the intergenerational transmission model as

$$Y^c_{ij} = \beta^c + \beta^m Y^m_i + (\beta^f + \gamma^f) Y^f_i + \theta' X_i + e^c_{ij}. \tag{3}$$

With samples of egg donor children, direct estimation of (3) gives us an unbiased nurture estimate of β^m.[17]

In case of sperm donor children raised in family i with genetically related mother i (with identical Y^m and y^m), genetically unrelated father i, and sperm donor k (with unknown y^f_k), donor assignment is guided by stated donor preferences. Adding these variables to X in the intergenerational transmission model would analogously eliminate the genetic influences of the sperm donor

$$Y^c_{ik} = \beta^c + (\beta^m + \gamma^m) Y^m_i + \beta^f Y^f_i + \theta' X_i + e^c_{ik}. \tag{4}$$

The problem is that we do not observe these preference variables. With donor preferences excluded from (4), the estimate of β^f may be biased and capture not only the nurture effect of the rearing father but also part of the nature effect of the donor, that is, if the educational outcomes of rearing fathers and sperm donors are somehow related through the omitted preferences. There are, however, good a priori reasons to believe that these omitted preferences cause only little (upward) bias, given that donors are anonymous and parents can only choose out of five donor traits that are at best crude proxies for the donor's educational attainment.

Equations (3) and (4) relate the educational attainment of children to the educational attainment of *both* rearing parents. We purposefully do so to take account of confounding assortative mating effects. Would we exclude the genetically related father from (3), for example, the estimate of β^m captures not

[17]Our data on matched egg donors make is possible to estimate the same intergenerational models with the educational attainment of the matched egg donor as additional regressor. If assignment is conditionally random, the estimated nurture effect should not change when we include the matched egg donor's educational attainment. This is indeed what we document in the results section.

only the direct influence of the mother's education (representing the nurture effect) but also the indirect influence of the father's education (representing a mixture of nurture and nature effects) because better educated mothers tend to marry better educated fathers. With the educational attainment of both rearing parents in the same specification, we can better separate out the direct effects of the genetically unrelated parent from the indirect effects of the other genetically related parent. In our sensitivity analysis, we provide some evidence that confounding assortative mating effects are of little concern.

Extrapolating Nurture Effects (External Validity)

The extrapolation of nurture effects β^m and β^f obtained with donor children to a larger population of nondonor children must assume that nurture effects are similar in donor and nondonor families.

In our sample of IVF children, we can think of two reasons why the nurture similarity assumption gets violated. The first reason is that nurture effects are heterogeneous, that is, IVF parents with donor children are inherently different from IVF parents with nondonor children in how parents create environments for children to grow up, or in how children respond to their parents' upbringing. The second and related reason is that parents invest differently in donor and nondonor children. We propose two tests for such violations.

One test focuses on heterogenous nurture effects and compares intergenerational skill transfers of genetically related parents in donor and nondonor families. Because one parent in donor families needs to be the biological one, we can estimate the combined nature-nurture effect for at least one of the parents in donor families. If the combined nurture-nature effects are heterogeneous and different in donor and nondonor families, we should find different intergenerational transmission coefficients.[18]

The other test focuses on treatment differentials in terms of time parents spend with children and compares parental leave, labor supply, and divorce decisions of parents in donor and nondonor families. Parents may treat their genetically unrelated children differently for different reasons. If, on the one hand, genetically unrelated parents are parents that miss some evolutionary

[18] We exploit here that the intergenerational mobility models for nondonor and donor children can be connected in a relatively straightforward way. For nondonor children born and raised in family i, we know that Y_j^m and y_k^f are identical to Y_i^m and y_i^f. Only if nurture and nature effects are similar in families with nondonor and donor children, model (2) collapses to (1) where the intergenerational transmission coefficients $\beta^m + \gamma^m$ and $\beta^f + \gamma^f$ are identical to α^m and α^f.

drive (as suggested by Dawkins (1976, 2006) and partly supported by Case, Lin, and McLanahan (2000, 2001)), they may feel less attached to their donor children, and as a result take up less parental leave, work longer hours (assuming that child rearing is most time intensive when children are young), and face higher divorce risks. If, on the other hand, genetically unrelated parents are parents with a stronger demand for children, they may also feel more attached to their donor children and as a result treat their donor children more favorably, with reversed parental leave, labor supply, and divorce responses.

Exploring Nature-Nurture Interactions

In case nurture and nature interact, the nurture effects we estimate also turn heterogenous and must be interpreted as nurture effect averages, which vary with the innate skills of donor children. A number of theoretical models formalize these nature-nurture interactions. There are nurture-orientated interaction models where the skills produced in early childhood raise the productivity of parental environments and investments in later childhood years (Cunha and Heckman 2007). There are also nature-orientated interaction models where the genes exposed to different parental environments can lead to different gene expressions affecting the skill formation of children differently (Rutter 2006). A rising number of studies empirically test for these nature-nurture interactions. Those studies that estimate behavioral genetic models with interactions using twin siblings reared apart report only few statistically significant interactions (see Plomin, DeFries, and Fulker (1988) for a review on the topic). Those studies that estimate intergenerational associations using adoption data with information on the children's biological and adoptive parents generally find small and mostly insignificant interactions (Björklund, Lindahl, and Plug 2006, Brandén, Lindahl, and Öckert 2017, Black et al. 2019). While these nature-nurture interactions have been given much attention in the literature, valid interaction tests are scarce and often unsupportive.

In our setup, we can only implicitly test for nature-nurture interactions using information on donor children. With a sample of donor children who are genetically related to only one of their parents, we can modify the intergenerational models (3) and (4) and add interactions between educational attainment of the genetically unrelated and related parents to account for possible interactions between the nurture and nature effects

$$Y_{ij}^c = \beta^c + \beta^m Y_i^m + (\beta^f + \gamma^f) Y_i^f + \phi_3^{fm} Y_i^m Y_i^f + \theta' X_i + e_{ij}^c, \tag{5}$$

and

$$Y_{ik}^c = \beta^c + (\beta^m + \gamma^m)Y_i^m + \beta^f Y_i^f + \phi_3^{mf} Y_i^m Y_i^f + \theta' X_i + e_{ik}^c. \tag{6}$$

Here the coefficients ϕ^{fm} and ϕ^{mf} serve as implicit interaction tests. In case of positive interaction estimates, the ϕ^{fm} and ϕ^{mf} coefficients may capture various channels, including interaction effects, child-rearing complementarities between mothers and fathers, and other concavities in the child-parent relationship. In case of zero interaction estimates, however, we consider these channels, including nurture-nature interactions, unlikely.

6 Results

Table 3 presents the main intergenerational transmission estimates for education. We report estimates of regressions of standardized test scores on the years of education of both parents, with controls for the child's gender and birth year, the parents' citizenship and birth year, and where indicated the calendar month of first IVF treatment and clinic fixed effects. These control estimates are not reported. We run separate regressions on samples of IVF treated parents with nondonor, sperm donor, and egg donor children. For the sample of egg donor children, we always control for treatment date and clinic fixed effects to ensure that the assignment of donor eggs is conditionally random.

In columns 1 and 2 we begin with the intergenerational mobility associations obtained from the sample of nondonor children. For both parents, we find that the estimated associations between parental schooling and child test scores are large, positive, and statistically significant indicating that higher educated parents have on average children with higher test scores. The estimated associations do not change when we control for treatment date and clinic fixed effects. The overall magnitudes of these associations, which differ only a little for mothers and fathers, tell us that four more years of parental education of either parent are associated with children having about 0.26-0.30 standard deviation higher test scores. These estimates are comparable to those estimated in previous intergenerational mobility studies (Hægeland et al. 2010).

In columns 3 and 4 we run the same intergenerational mobility regressions on the sample of sperm donor children. Here the intergenerational associations are supposed to take account of the father's genes. For fathers of sperm donor children, we find that the estimates get much smaller. In fact, the estimated

Table 3
Regressions of child test scores on parent education using IVF children

	non donor children		**sperm donor children**		**egg donor children**	
	(1)	(2)	(3)	(4)	(5)	(6)
years of education mother	0.076	0.076	0.061	0.067	0.072	0.073
	*0.003******	*0.004******	*0.016******	*0.018******	*0.024******	*0.022******
years of education father	0.064	0.064	–0.010	–0.008	0.063	0.062
	*0.003******	*0.003******	*0.014*	*0.015*	*0.022******	*0.022******
years of education egg donor						0.008
						0.040
R-squared	0.129	0.132	0.047	0.085	0.258	0.260
number of observations	19,509	19,509	820	820	157	157
clinic FE and treatment date controls		✓		✓	✓	✓

Note–The dependent variable is the averaged standardized achievement test score. The independent variables are the parents' educational attainment measured in the nominal years spent in school. All specifications control for the gender and birth year of children, Danish citizenship and birth year of rearing mothers and fathers, and where indicated the first IVF treatment date (in calendar months) and a full set of clinic indicators. The specifications for mothers of egg donor children always control for IVF clinic fixed effects and IVF treatment date to ensure that donor assignment is conditionally random. The last specification (column 6) adds a control for educational attainment of the egg donor and an imputation indicator. In case the egg donor is not identified (60 out of 157 observations), we have imputed the educational attainment of the egg donor with the average educational attainment of the donors treated in the same year and same clinic. Standard errors are clustered by mothers and shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level.

associations, which we interpret as nurture effects, are practically zero, statistically insignificant, and (statistically) significantly smaller than those obtained for fathers of nondonor children.[19] For mothers of sperm donor children, however, we find that the intergenerational associations between the educational attainment of mothers and child test scores remain large, positive, and statistically significant, and statistically similar to those obtained for mothers of nondonor children. As before, the estimated associations do not change much when we control for treatment date and clinic fixed effects.

In columns 5 to 6 we switch to the sample of egg donor children and report intergenerational associations that take account of the mother's genes. We consider two specifications: without and with controlling for the egg donor's educational attainment. For the rearing mothers of egg donor children, we find that the intergenerational associations are large, positive, and statistically significant, and as large as those obtained for mothers of nondonor children. For fathers of egg donor children, we also find positive, sizable, and statistically significant associations that are practically identical to those obtained for fathers of nondonor children. When we control for the donor's educational attainment (measured in years of schooling), we expect the same intergenerational association for rearing mothers (because the assignment of donor eggs is essentially random) and a zero intergenerational association for the matched donors (because the estimated associations for mothers of nondonor children and rearing mothers of egg donor children are practically the same). This is indeed what we find. The intergenerational associations for the rearing parents hardly change. The intergenerational association for the matched donors is small, although imprecisely estimated.[20]

[19] A critical concern for our nurture effect interpretation is that the assumption of almost random sperm donations may not hold in regressions without donor preference controls. Would the assignment of sperm donors be selective, part of what we interpret as the nurture effect may in fact be genetic. With nurture estimates close to zero, however, we consider such a bias unlikely. Appendix A provides a more formal intergenerational transmission model with selective donor assignment and shows that the nurture effects we estimate are most consistent with a quasi random donor assignment procedure.

[20] In our intergenerational mobility sample of rearing mothers of egg donor children, we are able to identify the likely donor for 64 percent of all donor children. We have replaced missing education with the average education of the donors treated in the same year and clinic, and added a missing dummy for those donor children without donor information to the intergenerational mobility model. Excluding the missing observations does not affect our results. When we estimate the intergenerational model in column 6 using the subsample deleting the missing observations, the coefficients on the mother's education, father's education, and the matched donor's education are 0.059 (with a standard error of 0.041), 0.075 (with a standard error of 0.038), and 0.005 (with a standard error of 0.041), respectively. While less precise, these estimates do not differ in any material way from those reported for the full sample.

With small donor samples, there is the concern of imprecise nurture effect estimates. We can draw, nonetheless, three tentative conclusions from these findings. The first one is that the education of genetically unrelated fathers is of little help to their children's test scores; that is, the estimated zero nurturing effect of paternal education (identified on a sample of 820 sperm donor children) is precise enough to statistically rule out effect sizes larger than 0.021 (which is about one third of the overall intergenerational association we estimate for nondonor fathers). The second one is that the education of genetically unrelated mothers does matter; that is, the positive nurturing effect of maternal education (we estimate on a sample of 157 egg donor children) is large enough to be statistically significant but not precise enough to statistically rule out effect sizes as small as 0.025 (which is about one third of the overall intergenerational association for nondonor mothers). The third one is that the education of genetically unrelated mothers matters more than that of genetically unrelated fathers. When we test for differential impacts of the unrelated fathers and mothers (by pooling the donor samples and estimate columns 4 and 5 with a fully interacted regression model), we find that the nurture impact of mothers is significantly larger than that of fathers (the estimated difference is equal to 0.080 and comes with a standard error of 0.026).

Specification Issues

We consider several specification issues that are common to the analysis of intergenerational skill transmission. We focus here on those regression models that take account of first treatment date controls and clinic fixed effects (as in columns 2, 4 and 5 of table 3) and examine how assortative mating, nonlinearities in intergenerational skill transfers, and the use of alternative parental education and child achievement measures affect the nurture effect estimates.

First, we estimate the nurture effects in intergenerational transmission models that control for the educational attainment, birth year, and citizenship of both parents. Our motivation is that such a specification takes account of the nurture and nature effects of the genetically related parent that are due to assortative mating.[21] One concern is that we may inadequately control for as-

[21] When we estimate the assortative mating relationship between the educational attainment of partners (controlling for child gender and birth year, partner ethnicity and birth year, clinic fixed effects and waiting list controls), we get associations (with standard errors between parentheses) of 0.438 (0.018) for the non-donor sample, 0.451 (0.036) for the sperms donor sample, and 0.334 (0.080) for the egg donor sample.

Table 4
Alternative regressions of child test scores on their parents' education

	non donor children	sperm donor children	egg donor children
	(1)	(2)	(3)
A. Alternative specification: excluding spousal characteristics			
years of education mother	0.105	0.062	0.085
	*0.004******	*0.014******	*0.028******
years of education father	0.096	0.023	0.088
	*0.002******	*0.011******	*0.026******
B. Alternative specification: including additional spousal characteristics			
years of education mother			0.075
			*0.014******
years of education father		–0.011	
		0.014	
C. Alternative specification: including interactions			
years of education mother	0.081	0.035	0.073
	*0.013******	*0.066*	*0.175*
years of education father	0.070	–0.042	0.065
	*0.013******	*0.080*	*0.155*
years of education interaction	–0.0004	0.002	–0.0001
	0.001	*0.005*	*0.012*
D. Alternative independent variable: college education			
college education mother	0.295	0.238	0.165
	*0.017******	*0.051******	*0.137*
college education father	0.311	–0.012	0.444
	*0.018******	*0.050*	*0.133******
E. Alternative dependent variable: reading test scores			
years of education mother	0.075	0.062	0.097
	*0.006******	*0.015******	*0.036******
years of education father	0.054	–0.001	0.042
	*0.007******	*0.014*	*0.030*
F. Alternative dependent variable: math test scores			
years of education mother	0.065	0.072	0.009
	*0.016******	*0.027******	*0.026*
years of education father	0.064	–0.028	0.108
	*0.017******	*0.022*	*0.045******

Note–The table contains estimates from specifications that deviate from the baseline regression models (reported in columns 2, 4 and 5 of table 3). Panel A: the independent variables exclude all spousal characteristics. Panel B: the main independent variables include additional spousal labor market and health characteristics, including pretreatment annual income, annual fraction spent unemployed, and the annual number of sick leave days. Panel C: main independent variables include additional interaction between the educational attainment of the mother and father. Panel D: the main independent variables are the parents' college education indicators measuring whether parents completed 15 or more years of education. Panel E: the dependent variable is the averaged standardized child test score in reading. Panel F: the dependent variable is the averaged standardized child test score in math. Standard errors are clustered by mothers and shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level.

sortative mating spillovers and that the nurture effect is biased capturing not only the nurture effect from the genetically unrelated parent but also part of the nurture and nature effects from the other genetically related parent.

To check whether assortative mating spillovers are empirically relevant, we first run our intergenerational transmission models without all the controls of the genetically related parent to obtain nurture effect estimates with the largest assortative mating spillover bias possible. Large nurture effects would then indicate that the corresponding nurture effects are sensitive to assortative mating spillovers. Table 4 (panel A) reports the corresponding intergenerational transmission estimates. We note here that the reported estimates attached to either the mother's and father's education come from separate regressions. For mothers of donor children, we find that the intergenerational associations in columns 2 and 3 remain essentially unchanged. For fathers of donor children, we find that the intergenerational associations get somewhat larger, in particular, the biased nurture effect of the father's education on the child's test scores is now large enough to be statistically significant. The latter estimate is fully in line with a zero nurture effect for fathers, however. Once we recognize that the upward bias captures a combination of assortative mating effects (0.451 in footnote 22) and the impact of the partner's educational attainment (0.067 in table 3 column 4), it is easy to see that the estimated effect of the father's education of 0.023 in column 2 is entirely driven by assortative mating spillovers (0.451×0.067).

As an additional check on assortative mating spillovers, we run the same intergenerational transmission models but now with as many controls of the genetically related parent we consider relevant and have access to, including the parent's income, the fraction of days spent in unemployment, and the number of sick leave days (all measured the year before the child was born). Again, large changes in the estimated nurture effects would indicate severe spillover bias. Table 4 (panel B) reports the corresponding nurture effect estimates. We note here that it makes little sense to report the estimates attached to partner's education because of its strong correlation with the partner's labor market and health outcomes. With nurture effect estimates that are nearly identical to those reported in table 3, we do not believe that the nurture effects estimates in table 3 suffer much from assortative mating spillover bias.[22]

[22]Collado, Ortuño-Ortìn and Stuhler (2019) make a similar point when they model and quantify intergenerational transmission of education using extended family members (defined by common great grandparents) and their spouses: that is, partners hardly sort on genetic factors that drive their educational attainment.

Second, we estimate the nurture effects in a simple intergenerational transmission model where child skills depend linearly (and additively) on the educational attainment of both parents. The concern is that such a model may miss important nonlinearities in the intergenerational transmission of human capital skills that are induced by, for example, child-rearing complementarities between mothers and fathers or nature-nurture interactions. We test for nonlinearities in intergenerational skill transmission by adding interactions between the educational attainment measures of mothers and fathers. Table 4 (panel C) contains these intergenerational transmission estimates (based on intergenerational models 5 and 6). We find that all the interaction estimates are small and statistically insignificant. Given that we get precisely estimated zeros in the larger samples of nondonor children, we consider nonlinear transmission channels unlikely.

Third, we measure parental education by the number of years parents spent in school and estimate how child test scores respond to a one year increase in parental education (net of genetic skill transfers). The concern here, which is related to the nonlinearity concern mentioned above, is that the test score gains of children may depend on whether parents spend one more year in elementary school, high school, or college. If, for instance, child test scores respond stronger to parental school years spent in lower secondary education than to parental school years spend in university, we may find weaker nurture effects because we estimate our intergenerational mobility regression models on samples where particularly better educated fathers are overrepresented. This would be a valid concern had we observed positive nurture effects in models with parental college indicators as the main independent variables. This is not the case. Table 4 (panel D) reports additional intergenerational transmission estimates for parental college education. College education is a dummy variable and indicates whether the parent has spent at least two years in college or more. With college education, our findings are consistent to those previously reported. The intergenerational associations between the genetically unrelated father and child, which we interpret as nurture effects, are statistically insignificant and close to zero. The intergenerational associations between the genetically unrelated mother and children are less precisely estimated but continue to be large.

And lastly, we measure the educational achievement of children by overall test score performance averaging the test score performance on nationwide reading and math tests. One concern is that the test score gains may depend on test subject. Table 4 (panels E and F) reports intergenerational mobility estimates for standardized reading and math test scores separately. We find that all the intergenerational associations are less precisely estimated because we work with smaller samples.[23] We also find that the intergenerational associations (including those we interpret as the nurturing impacts of parental education) for reading and math test scores are very similar to the associations we estimate for the combined reading-math test scores. One exception is the relationship between the education of genetically unrelated mothers and child test scores in math, which is noticeably lower. But since the latter estimate for math is not precise enough to rule out an effect size as large as 0.060, it is difficult to draw firm conclusions about a nurturing effect of maternal education for child test scores in math, but not in reading.

7 Generalizability of Results

An important question is whether the nurture effect estimates taken from parents of donor children are (informative about and) generalizable to other parents of nondonor children. In an attempt to answer this external validity question, we compare intergenerational mobility patterns across different samples of parents and children.

IVF Families

We first compare IVF treated families with and without donor children. As we reported earlier, there are two external validity risks. The first one is that the nurture effects are heterogeneous: that is, nurture effects are different for different families. The second risk is one of treatment differentials: that is, parents treat donor children differently than nondonor (but otherwise similar) children.

To test for heterogeneous nurture effects, we focus on the rearing parents in donor and nondonor families and compare how the educational attainment of parents relates to the test scores of their genetically related children. For mothers of sperm donor and nondonor children, we find that increasing maternal education by a year is associated with standard deviation gains of 0.067 and 0.076 for child test scores (see table 3, columns 2 and 4). For fathers of egg

[23] We lose precision for two reasons. First, the test score performance is based on fewer tests. Second, the samples for children with available math test scores are considerably smaller, consisting of 11,532 nondonor children, 560 sperm donor children, and 123 egg donor children. There are fewer children with math test scores because children do fewer math tests and take their first math test at a later age.

donor and nondonor children, we find that one more year of education leads to comparable standard deviation gains of 0.063 and 0.064 for child test scores (see table 3, columns 2 and 5). With intergenerational associations being nearly identical, together with interaction associations being close to zero (see table 4, panel B), we find no clear evidence of heterogeneous nurture effects and conclude that families with donor children are comparable to families with nondonor children in terms of intergenerational skill transfers.

To test for treatment differentials, we focus on genetically related and unrelated parents in IVF families and compare their parental leave take up, labor supply, and divorce risk during the child's preschool years. Given that our nurture effect estimates are so different for fathers and mothers, we distill two possible external validity concerns. The concern with fathers of sperm donor children is that the zero nurture effects may arise because fathers feel less attached to their genetically unrelated children and take up less parental leave, work longer hours, and face higher divorce risks. And reversely, the concern with mothers of egg donor children is that the positive nurture effects may arise because they feel more attached to their children and take up more parental leave, work fewer hours, and face lower divorce risks.

Table 5 reports estimates from least-squares regressions on the sample of all IVF-treated families, with four different dependent variables: parental leave take up measured as the number of registered parental leave days taken by the parents during the first two and five years following child birth, labor supply response of parents measured as the percentage change between the average labor earnings in the four years before child birth and the average labor earnings in the first five years after child birth, and divorce measured as an indicator for whether married/cohabitating parents divorce/break up during the first five years following child birth. The independent variables of interest are indicators for egg and sperm donor families. As before, we include (but not report on) pre-determined controls for the child's gender and birth year, and the parents' citizenship and birth year in our regressions. Table 5 also reports summary statistics of parental leave take up, change in labor supply, and divorce rates. The sample sizes vary because of missing earnings averages and because of unavailable 5-year parental leave measures for the youngest cohorts of children.

Two things become clear from this table. First, the parental leave statistics clearly show that all fathers spend much less time with their children than mothers. Second, there is little evidence of the treatment differential concerns we expressed above. The estimated coefficients on sperm donor families are

Table 5
Relationship between parents' time spent with children and donor type in IVF treated families

	days parental leave^a (0-2 yrs)		**days parental leave^a (0-5 yrs)**		**labor supply change (0-5 yrs)**		**divorce (0-5 yrs)**
	mother (1)	father (2)	mother (3)	father (4)	mother (5)	father (6)	couple (7)
nondonor families (ref.)							
sperm donor families	0.506	1.240	–5.953	2.336	–0.043	–0.001	0.034
	6.216	*1.275*	*11.530*	*1.518*	*0.050*	*0.030*	*0.022*
egg donor families	8.235	–4.598	–11.560	–4.559	0.035	0.055	–0.056
	10.242	*1.160******	*14.087*	*1.286******	*0.059*	*0.067*	*0.022******
R-squared	0.173	0.031	0.145	0.031	0.026	0.028	0.012
number of observations	15,396	15,396	11,566	11,566	14,541	14,239	15,396
mean (nondonor families)	285.203	15.937	346.546	18.554	–0.047	0.138	0.165

Note–The dependent variables are the parents' parental leave take up measured as the number of registered parental leave days during the first two and five years following child birth (columns 1 to 4), labor supply response measured as the percentage change between the average labor earnings in the four years before child birth and the average labor earnings in the first five years after child birth (columns 5 and 6), and divorce measured as indicator for whether married/cohabitating parents divorce/break up during the first five years following child birth (column 7). The independent variables of interest are indicators for egg and sperm donor families. The nondonor families serve as the reference group. The samples treat each donor family as separate observation. All specifications control for the child's gender and birth year, the parents' citizenship and birth year, the first treatment date (in calendar months) and a full set of clinic indicators. Robust standard errors are shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level. (*a*) Before 2002, the paid parental leave policies (in our study period) allowed parents to take up 70 days maternity leave, 10 days paternity leave and 50 days shared leave. After 2002, these paid parental leave periods were extended to 90 days maternity leave, 10 days paternity leave and 160 days shared leave.

all statistically insignificant and mostly small, suggesting that fathers of sperm donor children spend as little time with their children as fathers of nondonor children. Also the estimated coefficients on egg donor families indicate that mothers of egg donor children spend not more but as much time with their children as mothers of nondonor children do. Only the divorce coefficients are sizable and have signs in the hypothesized directions; that is, compared to nondonor couples, we find that egg donor couples are 6 percentage points less likely to divorce, whereas sperm donor couples are 3 percentage points more likely to divorce. It is unlikely, though, that such divorce patterns alone can be held responsible for the nurturing impacts observed in this study. For mothers, the corresponding divorce estimate is large but not large enough to mask substantial nurture effects. And for fathers, the corresponding divorce estimate is not statistically significant and too small to explain away the zero nurture effects. As an additional check, we have run our intergenerational mobility regressions on a sample of arguably more attached fathers, deleting all the divorced couples from the sample. With a point estimate of -0.011 (0.016) with standard error in parentheses, we continue to find a zero nurture effect for nondivorced rearing fathers of sperm donor children.

We view these test results as supportive evidence of the greater external validity of the nurture effect estimates within our population of successfully treated IVF families.

Representative Families

We next compare IVF families and representative families. In particular, we compare intergenerational mobility patterns between IVF families with genetically related children and families drawn from the full population of families with children born in the same research window. In both types of families, the intergenerational transmission estimates represent an overall blend of nature and nurture influences. If we would get the same intergenerational transmission estimates, we conjecture that the process of skill transmission is comparable in the two types of families, and that the intergenerational transmission estimates obtained with IVF families have a wider generalizability.

Table 6 presents the intergenerational transmission estimates for education for the different samples. In column 1 we report estimates for the sample of representative families. In column 2 we reproduce our baseline estimates for the sample of nondonor IVF families (as reported in table 3) for ease of comparison.

We find that the estimated associations between parental schooling and child test scores are all large and positive. Although they are not identical, the intergenerational associations differ only a little across the two types of families.

Adoptive Families

And lastly, we compare intergenerational mobility patterns in families with donor children and families with adopted children. In both types of families, we can estimate nurture effects, that is, if children are genetically unrelated to their rearing parents. The adoptees are suitable for identifying (and comparing) nurture effects: adoptees are adopted at infancy (the vast majority of adoptees is adopted below the age of 2) to ensure that the nurture effect estimates capture most of the early childhood influences; and adoptees are foreign born to ensure that the assignment process is fairly random.[24]

In columns 3 and 4 we report the intergenerational transmission estimates for foreign-born adoptees as well as for Korean-Danish adoptees. In columns 5 and 6 we reproduce our baseline estimates for the sample of donor children (as reported in table 3) for ease of comparison. For the sample of foreign-born adoptees, we find small, positive, and statistically significant nurture effects indicating that higher educated parents provide a better nurturing environment for adopted children to perform well in school. The associations are similar for mothers and fathers and imply that four more years of parental education of either parent are associated with children having about 0.10 standard deviation higher test scores. For the sample of Korean-Danish adoptees, the estimates are also positive and small, but too imprecise to make informative comparisons.

How are we to interpret the difference between the nurture effect estimates taken from adopted children to those taken from donor children? If we think of the nurture effect estimates obtained with adoptive parents and foreign-born adoptees as representative for all other parents and children (including IVF parents and donor children), the difference should capture the nurturing influence of parental education on the prenatal and very early childhood environment. We find the largest difference for rearing mothers, that is, the intergenerational transmission estimate for mothers of egg donor children is three times as high

[24] As discussed in Holmlund, Lindahl, and Plug (2011), most adoptive parents know little, if anything, about the biological background of foreign-born adoptees. They know, like we do, the adoptees' gender, age, and country of origin. We therefore run our intergenerational mobility regressions with additional controls for the adoptees' country of birth (measured by indicator variables for the 20 most popular countries of birth) and assume that assignment of adoptees to families is conditionally random.

Table 6
External validity regressions of child test scores on their parents' education

	all other children	IVF nondonor children	all adopted children	Korean adopted children	sperm donor children	egg donor children
	(1)	(2)	(3)	(4)	(5)	(6)
years of education mother	0.085	0.076	0.019	0.023	0.067	0.072
	*0.001****	*0.003****	*0.008****	*0.025*	*0.018****	*0.024****
years of education father	0.072	0.064	0.026	0.001	–0.008	0.063
	*0.001****	*0.003****	*0.008****	*0.022*	*0.015*	*0.022****
R-squared	0.162	0.132	0.127	0.054	0.085	0.258
number of children	650,930	19,509	2,674	232	820	157
country-of-origin FE			✓			
clinic FE, calendar month		✓			✓	✓

Note–The dependent variables are averaged standardized test scores. The independent variables are the parents' educational attainment measured in the nominal years spent in school. All specifications control for the gender and birth year of children, and the birth year and Danish citizenship of mothers and fathers. The specification in column 3 further controls for country-of-origin fixed effect (for the 20 most popular countries of origin). The specifications in column 2, 5 and 6 further controls for clinic fixed effects and calendar month of first donor treatment. Standard errors are clustered by mothers and shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level.

as the estimate for mothers of adopted children. When we test for differential impacts (by pooling the egg donor and adoption samples and estimate columns 3 and 6 with a fully interacted regression model), we find that the nurture impact of egg donor mothers is significantly larger than that of adoptive mothers (the estimated difference is equal to 0.052 and comes with a standard error of 0.023). These nurture effect estimates provide some suggestive evidence that better educated mothers are better able in creating a prenatal and early childhood environment that improves child test scores.

We find the opposite, but smaller, difference for rearing fathers, that is, the intergenerational transmission estimate appears somewhat larger for fathers of adopted children than for fathers of sperm donor children. An interpretation is difficult because the estimated difference is not precise enough to reject that the two nurture effect estimates are different. Perhaps there is no difference. Or perhaps there is a marginally larger nurture effect for fathers of foreign-born adoptees, which may then be attributed to the selective screening of prospective adoptive parents. Over the period we consider, prospective parents were assessed on the basis of formal criteria (regarding background, age, marital stability, income et cetera) and in-depth interviews (regarding personal history and family relationships) before they were qualified to adopt. If selected prospective adoptive parents have somewhat stronger parenting skills than the average IVF parents in our sample, selective screening may explain the marginally stronger impact for adoptive fathers but at the same time may compromise (and underestimate) the prenatal and early childhood environment interpretation of the differential impact of donor and adoptive mothers.[25]

Interpreting the Results

Our findings clearly suggest that, once we take account of genes, better educated fathers are of little help to their children's test scores, while better educated mothers raise children with better test scores. In particular, when we take our

[25]In an analysis not shown in this paper, we have also estimated the intergenerational transmission models where the samples of nondonor children (raised in IVF and representative families) and adoptees are reweighted to match either the sample of egg donor children or the sample of sperm donor children. The weights are based on the estimated propensity scores taken from probit regressions on the likelihood of either being an egg donor or a sperm donor child on the child's gender and birth year, their parents' years of schooling, ethnicity, and birth years, and in case of nondonor children in IVF families, the waiting list controls and clinic fixed effects. The intergenerational mobility associations obtained with these reweighted samples (available upon request from the authors) are nearly identical to those reported in table 6.

estimates at face value, it seems that fathers impact children's test scores only through genes and mothers impact children's test scores only through environment. What is the explanation?

On the one hand, we are surprised to find that the genes of fathers are so much more important than those of mothers. If genes are passed on to children from fathers and mothers alike, we would expect that mothers impact their children's test scores also through genes, and that their environmental influences would augment their genetic contribution, not replace it. We have explored several possible explanations that may amplify the nurture effect of donor mothers, including donor mothers spending more time with their donor children (they do not, see table 5), donor children having a genetic advantage over nondonor children (they have not, see table 1), and the presence of nature-nurture interactions (there are none, see table 4).[26] While we can rule out a few explanations, some remain. Perhaps better educated donor mothers have better child-rearing skills, masking the nondonor mothers' genetic impact on child test scores. Perhaps donor fathers bond less well with donor children and exert little effort in raising them, masking the nondonor fathers' environmental impact. Or perhaps the donor samples are just too small to uncover that mothers impact child test scores through genes (and fathers through environment).

On the other hand, we are not surprised to find that mothers contribute more to the child's environment than fathers do. A large part of the explanation must be that prenatal (and very early childhood) conditions are crucial for the development of child skills. It explains why the nurture effects in donor families are much stronger for mothers than for fathers: unlike fathers, mothers are pregnant, carry children, give birth, and spend observably more time taking care of children in early childhood. And it explains why the nurture effects for rearing mothers are much stronger for donor children than for adopted children: unlike adopted children, donor children benefit from their mother's exposure in

[26] To briefly motivate these explanations, we focus on mothers and decompose their intergenerational coefficients α^m and β^m into a nature component (h), nurture component (e), and the interaction between the two ($h \times e$): $\alpha^m = h^m + e^m + h^m \times e^m$, and $\beta^m = e^{dm} + h^{ed} \times e^{dm}$, where superscripts m, dm, and ed stand for nondonor mothers, donor mothers (donor recipients), and egg donors (donor providers), respectively. Taking the estimated coefficients at face value, we set α^m and β^m equal to each other to arrive at the following nature expression $h^m = [e^{dm} - e^m] \times [1 + h^{ed}] + [h^{ed} - h^m] \times e^m$. It is easy to see that we underestimate the influence of nature (and overestimate the influence of nurture) in case (i) donor mothers have better child-rearing skills and/or provide a more advantageous environment for their children ($e^{dm} > e^m$); and (ii) donor mothers raise children with certain genetic advantages ($h^{ed} > h^m$) and exhibit nature-nurture interactions. We consider the latter channel unlikely, given that egg donors have no educational advantage over other mothers (see table 1) and interaction proxies are small and statistically insignificant (see table 4).

pregnancy and very early childhood.

We next take a closer look at the relationship between prenatal environment and the education of (prospective) mothers. In particular, we have prenatal information on whether IVF treated women smoke during their pregnancy, recorded in the IVF register for the period 2006 to 2011, which enables us to regress a prenatal smoking indicator on the years of education of both partners (in the treated couple) and the standard controls including the partners' citizenship and birth year, the calender month of treatment and clinic fixed effects. We consider all treated women including unsuccessfully treated women to account for possible prenatal smoking influences on the success of treatment (having a livebirth). If better educated women are better in avoiding prenatal risks, we should find that better educated women smoke less during pregnancy.

Table 7
Regressions of prenatal smoking on education using IVF treated women

	non donor treatment	**sperm donor treatment**	**egg donor treatment**
	(1)	(2)	(3)
years of education treated woman	–0.019	–0.025	–0.014
	*0.002******	*0.004****	*0.007**
R-squared	0.030	0.047	0.031
number of treated women	74,904	6,443	589
mean prenatal smoking	0.121	0.117	0.093

Note–The dependent variable is prenatal smoking (0/1). The main independent variable is the (prospective) mother's educational attainment measured in the nominal years spent in school. The samples treat each IVF attempt as separate observation. All specifications further control for the partner's educational attainment, Danish citizenship and birth year of both partners, first IVF treatment date controls (measured in calender months) and a full set of clinic indicators. Standard errors are clustered by prospective mothers and shown in italics; * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** at 1 percent level.

Table 7 reports the least-squares regression results using the sample of IVF women who have been treated (and recorded in the register) between the years 2006-2011. We find that the estimates attached to the treated woman's years of schooling are all statistically significant, negative and sizable, suggesting indeed there is less prenatal smoking among all better educated pregnant women, including those who carry genetically unrelated babies.

8 Discussion and Conclusion

In this paper we investigate the intergenerational persistence in human capital skills, net of genetic skill transfers, using a novel strategy based on Danish children conceived through sperm and egg donations in IVF treatments. By considering donor children, we can eliminate the genetic connection between children and one of their parents, that is, children from sperm donors are genetically related to their mother but not their father, and children from egg donors are genetically related to their father but not their mother. We measure intergenerational persistence in human capital skills by estimating the relationship between parental education and child test performance on nationwide tests taken in primary and secondary education using samples of genetically related and genetically unrelated children.

We find, first, that there is a strong relationship between parental education and child test scores for donor-treated parents and their genetically related children. The intergenerational associations, which represent a blend of nurture and nature effects, are all large and positive, and similar to the ones we estimate using a conventional representative sample of parents and children. Second, we find an equally strong human capital relationship for donor-treated mothers and their genetically unrelated children, suggesting that most of the human capital skill transmission between mothers and children is nurture driven. The intergenerational associations are statistically significant and large, as large as the ones we find for mothers of genetically related children, and significantly larger than the ones we find for mothers of adopted children. And third, we find that there is no such relationship for donor-treated fathers and their genetically unrelated children, suggesting that the strong associations between fathers and their genetically related children are mostly nature driven. The intergenerational associations are practically zero and significantly smaller than the ones we find for fathers of genetically related children as well as the ones we find for donor-treated mothers of genetically unrelated children.

Of course, our empirical strategy only works if donor assignment is either random or related to variables that we observe and control for. In the Danish context, we know exactly how donors are assigned to prospective parents. For rearing mothers of egg donor children, the assignment of eggs is based on the position mothers take on the clinic's waiting list. With regression models that account for the clinic queue order, we show that donor assignment is as good as random. For rearing fathers of sperm donor children, the assignment of donor sperm is based on unobserved donor preferences. If the assignment of sperm donors is selective, our primary concern would be that part of what we interpret as the nurture effect may in fact be genetic. With nurture estimates for fathers close to zero, however, we consider such a selection bias unlikely. Because the assignment of donors to parents is not selective, we can interpret the nurture effect estimates (as presented for mothers and fathers) in a causal way and conclude that it is the nurturing impact of the education of mothers that matters most for the test scores of children.

What could be the mechanism behind these findings? While keeping in mind the standard caveats about interpreting reduced-form findings, our nurture effect estimates are consistent with the notion that prenatal and early childhood conditions are essential for the development of child skills. It explains why mothers matter more than fathers: unlike fathers, mothers are the ones who carry children, give birth, and observably spend much more time taking care of children during early childhood. It explains why donor mothers matter more than adoptive mothers: unlike mothers of adopted children, mothers of egg donor children are the ones who provide their children's prenatal and very early childhood environment. And it explains why high educated mothers matter more than low educated mothers: with more human capital skills, mothers are better in avoiding prenatal risks (in terms of reduced smoking in pregnancy) and spend more quality time with their children during their very early childhood years (as documented in the time use studies of Guryan, Hurst, and Kearney (2008) and Bonke (2009)).

To the eye, our finding that education of mothers matters more than that of fathers, once we take account of genetic skill transfers, appears contrary to those of recent adoption studies (Plug 2004, Björklund, Lindahl, and Plug 2006, Sacerdote 2007, Black et al. 2019, Fagereng, Mogstad, and Rønning 2021). This is not so surprising, however, given that we use a different strategy and study different outcomes that are realized in a different country. Our study relies on donor children. The other studies rely on adoptees. With rearing mothers of adoptees, for example, it is not only possible but (as we have shown) highly probable to get weaker nurture effect estimates because the adoption strategy misses those beneficial influences of maternal education on the child's prenatal and very early environment that our strategy with rearing mothers of donor children captures. Also our study examines test score outcomes in primary and lower secondary education when children are young and education is mandatory. The adoption studies examine skill outcomes of children realized in (early) adulthood including overall educational attainment, earnings, and wealth. With different outcomes, it is possible to get larger nurture effects for fathers if better educated fathers matter more for later-life outcomes that are more financially intensive and less time intensive, such as attending university, labor market outcomes, or portfolio holdings. In addition to this, our study examines intergenerational persistency in human capital skills in Denmark where education is heavily subsidized, skill returns are low, and parental leave arrangements are generous. Some adoption studies look at intergenerational persistency in the US (Plug 2004, Sacerdote 2007). With children growing up in countries with more costly education and higher skill returns, it is possible to get larger nurture effects for fathers if better educated fathers (who would otherwise see little return to their child investments) devote more time and money on their children. And with children growing up in countries with less generous parental leave arrangements, it is also possible to get weaker nurture effects for mothers if better educated mothers (who would otherwise spend more time with their children) take up less maternity leave. Clearly, much more work needs to be done to explore these possibilities; this is a priority for our future research.

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Appendix A - Donor assignment bias

To better understand how selective donor assignment complicates the identification of the nurturing effect of the rearing (and genetically unrelated) parent's education, we provide a more formal framework in which we allow donor assignment to impact the intergenerational education effects.

Consider the linear intergenerational mobility model for donor families relating donor child test score Y^c to the observable educational outcomes of the rearing (and genetically unrelated) parent Y^p, the rearing (and genetically related) spouse Y^{sp}, and the unobservable educational outcome of genetically related (and environmentally unrelated) donor y^d

$$Y_i^c = \beta^p Y_i^p + \gamma^p y_i^d + (\beta^{sp} + \gamma^{sp}) Y_i^{sp} + \theta X_i + e_i^c.$$

The idiosyncratic error term e^c represents an unobservable child characteristic that is uncorrelated with the observable and unobservable parent characteristics Y^p, Y^{sp} and y^d. Our nurture parameter of interest β^p is identified with least squares if the observable Y^p is uncorrelated with the unobservable genetic endowments y^d. If donor assignment is selective, however, this is unlikely.

We next extend the model and allow for a linear assignment process between the donor and both rearing parents. In particular, we relate the unobservable educational attainment of the donor to the observable educational attainment of both parents

$$y_i^d = r^p Y_i^p + r^{sp} Y_i^{sp} + \rho X_i + u_i^d.$$

The idiosyncratic error term u^d is uncorrelated with Y^p and Y^{sp}. In case donor assignment is guided by the donor preferences of parents, the idiosyncratic error term u^d reflects that parents can at most express a noisy proxy for the donor's educational attainment. In case donor assignment is selective, the donor's genetic endowments indirectly affect the test scores of donor children through both their parents, which is clearly seen when we insert the donor assignment equation into the intergenerational mobility equation and get

$$Y_i^c = (\beta^p + \gamma^p r^p) Y_i^p + (\beta^{sp} + \gamma^{sp} + \gamma^p r^{sp}) Y_i^{sp} + \phi X_i + \epsilon_i^c,$$

where the coefficients $\gamma^p r^p$ and $\gamma^p r^{sp}$ reflect donor assignment bias. The question we ask here is whether donor assignment bias is empirically ignorable or not?

For egg donors, we know that donor assignment is random conditional on the clinic-specific waiting list controls (which are captured by the calendar date of first treatment and clinic fixed effects in X). With the assignment coefficients r^p and r^{sp} that are zero in both theory and practice (see results in table 2), our donor design identifies the nurturing effect for mothers of egg donor children.

For sperm donors, we know that donor assignment is random conditional on the stated preferences for the five donor characteristics (which are not captured in X). With the assignment coefficients r^p and r^{sp} being possibly positive in theory (assuming that parents, if they can choose, choose their donors based on shared characteristics), we may overestimate the nurturing effect for fathers of sperm donor children. Our intergenerational estimates, however, are inconsistent with any strong selection based on donor preferences. First, we find the estimated coefficient for fathers of sperm donor children $\beta^p + \gamma^p r^p$ is close to zero, which is consistent with a zero assignment coefficient r^p. Second, the estimated coefficients for mothers of sperm donor children $\beta^{sp} + \gamma^{sp} + \gamma^p r^{sp}$ and for mothers of nondonor children $\beta^{sp} + \gamma^{sp}$ are similar to each other, which is again consistent with a zero assignment coefficient r^{sp} (assuming that the estimates obtained with donor families are representative for nondonor families). With the assignment coefficients r^p and r^{sp} that are arguably zero, any concern about selective donor assignment seems misplaced.

Appendix B - Tables

Appendix Table A
National test subjects by grade in primary and lower secondary education

grade:	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Danish, reading		✓		✓		✓		✓	
math			✓			✓			

Appendix Table B
Variable descriptions

Variable	Description
standardized test score	Test score from Danish National Tests. The score is standardized to mean 0 and standard deviation 1 across each cohort. The math tests cover numbers and algebra, geometry, and applied mathematics. The reading tests cover language comprehension, decoding, and reading comprehension.
male	Child gender is 1 for male, 0 for female.
child birth year	Year of birth of the child.
years of schooling mother	Years of schooling (highest completed) of the mother.
years of schooling father	Years of schooling (highest completed) of the father.
college education mother	College education is 1 if mother has more than 14 years of completed education, and 0 otherwise.
college education father	College education is 1 if father has more than 14 years of completed education, and 0 otherwise.
birth year mother	Year of birth of the mother.
birth year father	Year of birth of the father.
Danish citizenship mother	Mother is of Danish origin, i.e. at least one of her parents is of Danish origin.
Danish citizenship father	Father is of Danish origin, i.e. at least one of his parents is of Danish origin.
change in annual earnings mother	Difference in mother log average labor incomes 4 years before birth and 5 years after (missing income is counted as zero-income).
change in annual earnings father	Difference in father log average labor incomes 4 years before birth and 5 years after (missing income is counted as zero-income).
parental leave mother	Total days of maternal leave two/five years after child's year of birth (birth year is year 1). Last year included is 2007.
parental leave father	Total days of parental leave two/five years after child's year of birth (birth year is year 1). Last year included is 2007.
divorce	Divorce is 1 if the couple divorced within the first five years of the child's life, and 0 otherwise.