THE ORIGINS OF INTERGENERATIONAL ASSOCIATIONS: LESSONS FROM SWEDISH ADOPTION DATA*

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Abstract: We use unique Swedish data with information on adopted children's biological and adoptive parents to estimate intergenerational mobility associations in earnings and education. We argue that the impact from biological parents captures broad pre-birth factors, including genes and prenatal environment, and the impact from adoptive parents represents broad post-birth factors, such as childhood environment. We find that both pre- and post-birth factors contribute to intergenerational earnings and education transmissions, and that pre-birth factors are more important for mother's education and less important for father's income. We also find some evidence for a positive interaction effect between post-birth environment and pre-birth factors.

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I. INTRODUCTION

A large body of evidence suggests substantial intergenerational associations in earnings and education. Recent research has documented some cross-national differences with weaker intergenerational associations in Scandinavian countries and stronger in the United States.[1] The standard approach in this literature relates children's outcomes to those of their rearing parents. But this approach does not allow one to sort out the underlying contributions of pre-birth factors (genetics and prenatal environment) from post-birth environmental factors. Understanding the origins of intergenerational persistence in economic status is necessary for developing relevant theories and designing social policies.

Adoption creates a potential "natural experiment" that generates some (partially) independent variation in pre-birth and post-birth factors. If adopted children are (conditionally) randomly assigned to adoptive parents and adoption takes place close to birth, then regressions of child outcomes on adoptive parents' characteristics and biological parents' characteristics can provide information on the relative importance of of post-birth and pre-birth factors. Existing studies of adopted children find lower intergenerational associations for adoptive families than for biological families suggesting some role for pre-birth factors; recent studies include Sacerdote [2000, 2002, 2004], Plug and Vijverberg [2003, 2004], Plug [2004], Björklund, Lindahl and Plug [2004]. [2] But lack of data on the characteristics of the biological parents of adoptees has limited the inferences that can be made from such samples.

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1 See Solon [1999] and a symposium in the *Journal of Economic Perspectives* (Summer 2002) for surveys.

2 See also Scarr and Weinberg [1978], and Das and Sjogren [2002] for work along these lines.

This paper uses a unique administrative data set on a large sample of adoptees born in Sweden (from 1962-66) that contains information on the children's educational and economic outcomes and on the characteristics of both their adoptive and biological parents. The intergenerational associations for this sample of adoptees are compared to a representative sample of all (nonadopted) children born in Sweden in this period and to sub-samples with similar characteristics of biological parents and similar post-birth environments as the sample of adoptees.

The basic finding is that both pre- and post-birth factors contribute to intergenerational earnings and education transmissions, and that pre-birth factors are more important for mother's education and less important for father's income. We also find some evidence for a positive interaction effect between post-birth environment and pre-birth factors.

The paper is organized as follows: Section II introduces the extended intergenerational models that capture transmissions from both biological and adoptive parents. Section III briefly discusses the institutional setting of adoptions in Sweden in 1962-1966 and the administrative data used in the analysis. Section IV reports our basic results. Sections V and VI present a number of robustness tests and results using non-linear models. Section VII discusses our main findings and concludes.

II. INTERGENERATIONAL TRANSMISSION MODELS

II. A. LINEAR MODELS

In recent intergenerational research, the prototypical model used by economists can be expressed as

$$Y_j^{bc} = \beta_0 + \beta_1 Y_j^{bp} + v_j^{bc}, \tag{1}$$

where Y denotes the logarithm of long-run income or a measure of educational attainment at adult age; subscript j indexes the family in which the child is born and raised; superscripts *bc* and *bp* denote the biological child and parent;[3] and v_j^{bc} is a child-specific characteristic uncorrelated with Y_j^{bp}. The estimated intergenerational coefficient β_1 measures the strength of the intergenerational association and represents the combined effect of many different mechanisms, including the effects that come from genetic inheritance, prenatal environment and the environment in which the child grew up.

Our data on adoptees and their biological and adoptive parents allow us to decompose the estimated intergenerational coefficient β_1 into two components – one component that measures the contribution of genetics and prenatal environment (which we label pre-birth factors) and the other one that measures the contribution of post-birth environment on the outcomes of children. We model the transmission of an outcome from the biological parent *bp* and the adoptive parent *ap* to the adopted child *ac* born in family j and adopted and reared in family i as

(2) $$Y_i^{ac} = \alpha_0 + \alpha_1 Y_j^{bp} + \alpha_2 Y_i^{ap} + v_i^{ac},$$

where v_i^{ac} represents unobserved child-specific characteristics uncorrelated with Y_j^{bp} and Y_i^{ap}. It is important to note that a parental Y does not only capture this parental characteristic but also everything else that is correlated with it. This means that we estimate parameters that capture broad pre-birth factors (Y_j^{bp}) and broad post-birth environment (Y_i^{ap}), and that we do not consider α_1 and α_2 as the causal impact of the parents' Y. Pre-birth factors include genetic and prenatal environmental factors. Post-birth

[3] Most studies – e.g. those that rely on U.S. PSID and NLSY data – have used rearing parents, including biological, step and adoptive parents. We use the notation *bp* for expositional convenience.

factors include, for example, the impact of Y on the quality and quantity of time, goods and money that parents devote to their children.

We can further separate pre-birth factors into impacts of genetic and prenatal environmental factors by estimating the effects of the biological mother and father in separate regressions. Assuming that genetic transfers that come from the biological mother and father are equally important, and that the father's behavior does not affect the child's prenatal conditions in the womb, we can interpret the impact of the father's characteristics as an estimate of genetic factors and the difference in impacts between the biological mother's and father's characteristics as an estimate of the impact of prenatal environmental factors.

The procedure to link the prototypical mobility model in (1) to the adoption model in (2) is relatively simple. For children who are not adopted but born and raised in the same family j, we know that Y_i^{ap} and Y_j^{bp} are identical. If we replace Y_i^{ap} with Y_j^{bp}, model (2) collapses to model (1) where the intergenerational transmission coefficients β_1 and $\alpha_1+\alpha_2$ are identical. Two considerations are of interest here. The first one relates to whether or not we can use adoptees to separate pre- from post-birth contributions. The second one relates to whether or not we can infer the relative contributions of pre- and post-birth factors for a representative Swedish child from a sample of adoptees. These considerations are explored below.

II.B. IDENTIFICATION ISSUES

Let us begin with the identification of pre- and post-birth contributions using adoptees. To estimate and interpret α_1 and α_2 as pre- and post-birth contributions we need to make two assumptions. First, adoptees are randomly assigned to adoptive families, or that the assignment is related to variables that we – as researchers – observe and control for. And second, children move to their adoptive parents immediately at birth. Both assumptions are easily violated.

For one, the assignment process is not always random. In fact, if adoption authorities have information on the children's biological parents, they may use it to match children to adoptive families. If matching occurs and the characteristics of the adoptees' birth and adoptive parents become correlated, adoption estimates will be biased. This is a serious problem, one that is shared with almost all adoption studies. If non-random assignment is present, it is possible that part of the estimated pre-birth effects are picking up the effect of those post-birth factors that are either unobserved *and* correlated with Y_j^{bp}, or observed *and* measured with error. And vice versa for the estimated post-birth effects. In Section V we formalize non-random assignment within an omitted variables and measurement error framework and examine whether non-random assignment itself is empirically important.[4]

Also, not all adoptees are adopted as babies. It is possible that some adoptees spend time with their birth parents, or receive institutional care between birth and adoption. In such cases, the estimated pre-birth effects are too high, and estimated post-birth effects too low. We assess the sensitivity of our results using a sample that is arguably more limited to children adopted as babies.

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[4] Adoption authorities collect information on a wide array of factors. An omitted variables approach applies to those factors that are exclusively observed by adoption authorities (interviews and assessments of likely parenting skills and seriousness). A measurement error approach applies to those factors that are observed by the researcher, and possibly the authority, but that are imperfectly measured in our data (long run economic status).

To compare β_1 and $\alpha_1+\alpha_2$ we need to assume that adopted and own-birth children[5] and their parents are randomly drawn from one particular distribution of children and parents, and that parents do not differentiate between own-birth and adopted children.[6] Again, these assumptions may prove to be difficult to maintain. For the adoptees' parents, for example, Y_i^{ap} and Y_j^{bp} are distinctively different. Then how do the estimates for own-birth children compare to those obtained for adoptees? Investigating this is easier said than done. Whereas the estimates for own-birth children come from a representative sample, the adoption results come from a sample of children with, on average, disadvantaged pre-birth but favorable post-birth backgrounds. These opposite sources of selection make it difficult – if not impossible – to come up with a sample of own-birth children that is comparable to our sample of adoptees. We deal with this by comparing own-birth children to the adoptees in two different ways. First, we compare adoptees with own-birth children who start their lives under very similar conditions as adoptees. Second, we compare adoptees with another sample of own-birth children who are reared in similar environments as the adoptees. Different samples are made comparable through subsampling and propensity score matching.

Another issue is that children, who are given up for adoption, may be different from other children because of the adoption itself. If, for example, indications that adopted children reveal more emotional problems than their class mates – see Bohman [1970] for some Swedish evidence – reflect causal effects of adoption, outcomes like educational

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5 We use the concept of own-birth children whenever children are raised by their biological parents.

6 Previous adoption studies (such as Plug [2004] and Sacerdote [2004]) lacked data on biological parents of adoptees, and could therefore only estimate the impact of post-birth factors. They could only indirectly infer the impact of pre-birth factors by taking the difference between the intergenerational estimate for own-birth children and the estimate of the impact of post-birth factors. This means that they had to assume that $\alpha_1 = \beta_1 - \alpha_2$. However, since adoptees are different compared to own-birth children on a number of dimensions, it is not a priori clear that this assumption holds. In this paper we estimate both α_1 and α_2 directly.

attainment and earnings might also be affected. In the empirical analysis, however, we allow for adoptees and own-birth children to be different by running mobility models on separate samples. As long as these differences are unrelated to the parental schooling and earnings characteristics, any real adoption effect is captured by the difference between intercepts α_0 and β_0.

Note that if results are different, the implication is not necessarily that adoption itself has real effects. Functional form assumptions may be responsible as well. In particular, we focus our attention to genes-environment interactions.

II.C. NON-LINEAR MODELS

Interaction between genes and environment is an issue that has been given much attention in the recent literature.[7] Empirical tests, however, are only indirect and there is hardly any consensus about the presence of such interactions.[8] Our data set is especially suitable to test for such interactions. We have information on the adoptive and biological parents for a very large number of adoptees making it possible to estimate interaction terms with reasonable precision. We can modify model (2) to account for interaction effects of pre- and post-birth factors to

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7 For discussions, see Cunha et al. [2005] and Ridley [2003]. Turkheimer et al. [2003], an often cited study, use a little over 100 monozygotic and 200 dizygotic twin pairs and variance decomposition techniques to estimate how much genes and environment can explain the variation in IQ among 7 year old children, at different levels of socioeconomic background for their parents. They find genetic (shared environmental) effects to be more (less) important the higher the childrens' socioeconomic backgrounds.

8 Plomin, DeFries and Fuller [1988] survey genes-environment interaction studies in behavioral genetics. They find few statistically significant interactions and state that interactions "are easily posed but rarely documented." Björklund, Jäntti and Solon [2005] use sibling correlations for nine sibling types to decompose earnings variation into genetic and environmental components. When they extend a conventional model with a parameter that reflects interaction between genes and environment, they get a positive but insignificant estimate. Note that, while they also are using Swedish data, their approach is different from ours. First, they use sibling data, and we use intergenerational data. Second, they apply the variance decomposition approach, whereas we estimate regression coefficients telling what a unit of parental education implies for offspring's education and how a log point of paternal income (earnings) is related to offspring's log income (earnings). Third, they do not analyze education.

(3) $$Y_i^{ac} = \alpha_0 + \alpha_1 Y_j^{bp} + \alpha_2 Y_i^{ap} + \alpha_3 Y_j^{bp} Y_i^{ap} + u_i^{ac}\ .$$

The interaction coefficient α_3 is positive if children with beneficial pre-birth background benefit relatively more from a good post-birth environment, which would indicate that genetic and environmental factors are complements in the production of life success for the child. The corresponding model for parent and child in a non-adoption family, where Y_i^{ap} and Y_j^{bp} are identical, is written as

(4) $$Y_j^{bc} = \beta_0 + \beta_1 Y_j^{bp} + \beta_2 (Y_j^{bp})^2 + u_j^{bc}\ .$$

We do not want to infer interactions from a positive β_2 since functional-form assumptions dictate that the interaction in model (3) automatically leads to a non-linear mobility model in (4). If, for example, quadratic effects are present but not modeled in (3), β_2 would capture both interaction and quadratic effects. Thus, we also test for interaction effects in models where quadratic main effects are added to (3). Note also that if the interaction in (3) would look like $\sqrt{Y_i^{ap}} \cdot \sqrt{Y_j^{bp}}$, we return to the original linear model in (1).

III. INSTITUTIONS AND DATA[9]

We use administrative register data from Statistics Sweden on all legal adoptions, i.e., adoptions decided by public court and notified in the Swedish population register. In particular, our data set contains all persons who were born in Sweden between 1962 and 1966 and adopted by both parents. To show the usefulness of this data set, we start this

[9] The main sources for this section have been handbooks for social workers dealing with adoptions, the last one being Kungl. Socialstyrelsen [1968].

section by describing the Swedish adoption institutions during this period of time. Then we describe the data set and the variables in some detail.

III. A. ADOPTIONS IN SWEDEN 1962 – 1966

The basic principle of Swedish adoption law has always been that an adoption should be “in the best interests of the child”. This means, for example, that the decision whether an adoption should take place and the choice of adoptive parents should be motivated by concern of the child and not of the couple that wants to adopt. Economic compensation between the adoptive and the biological parents was not allowed. Because the period we consider was characterized as one with “excess demand” from prospective adoptive parents, payments to biological parents would probably have existed if allowed. An adopted child got the same legal status, e.g., with respect to inheritance, as a biological child. Further, all formal connections with the biological parents were broken.

A social authority within the local government was responsible for the process. Thus, mothers (and fathers) who wanted to adopt away a child as well as families who wanted to adopt contacted this authority. The legal adoption decision was taken by public court after being advised by the social authority.

The vast majority of adoptions took place at an early age of the child. In a typical case, an unmarried pregnant woman considered adoption and therefore contacted the social authority. But the mother could not decide to adopt away a child until she had recovered from the delivery. The new-born child was therefore initially placed at a special nursery home. An unmarried biological father had no formal say in the adoption decision, but ought to be contacted on the issue and allowed to give his opinion. Quite often, however, the father was unknown. The population register we use identifies 92 percent of the biological mothers and 58 percent of the biological fathers.

The child was first placed in a prospective adoptive family on a trial basis as a foster child. Placements were recommended to occur before the age of 6 months. If the trial period – lasting some 3-6 months – had turned out well, the next step for the prospective adoptive parents was to apply to the court for a legal adoption decision. The formal process by the court could take several months so the adopted child could have spent quite a long time with its new parents when the adoption was finally formalized.

In general, the biological mother was young, unmarried and poor. Our Table I (see below), reporting descriptive statistics for all cases when both biological parents to adopted children in Sweden born 1962-1966 were known, shows that both biological mothers and fathers were younger than non-adopted children's parents. Nonetheless, there is a substantial age variation among biological parents and only 32 percent of the mothers and 12 percent of the fathers were teenagers when the child was born.

Low income was a common reason to leave a child for adoption and lower social classes were clearly overrepresented among biological mothers. It is notable, though, that socio-economic status before age 30 does not strongly correlate with long-run status. Further, "shame" was also a common reason for adoption. Thus, although we have reason to believe that most mothers had low socio-economic status also in a long-run perspective, they were not necessarily a very homogenous group in this respect.

The responsible social worker, assigned by the social authority, was required to undertake a careful investigation of prospective adoptive parents. The guidelines for adoptions emphasized that, given a reasonable "stable" situation, economic resources and social status were not most important. Although not a formal requirement, it was expected that the mother could stay home to care for the child. The guidelines said that good adoptive parents should be tolerant, since an adopted child could get into problems and maybe not meet the expectations of its parents. "Normal people" were considered the best adoptive parents. Nonetheless, due to these considerations, one would expect adoptive parents to be under-represented among families with low socio-economic status.

The law required that adoptive parents were 25 years of age. There was no upper age limit but the adoptive parents were supposed to be young enough to be able to be the biological parents of the child. There was no requirement about the duration of the marriage. But other requirements made it unlikely that newly married couples would be able to adopt a child. For example, the social worker was supposed to find out that the prospective adoptive parents would not be able to have own biological children.

A crucial issue in adoption research is whether there likely is selective placement so that there is a positive correlation in important traits between biological and adoptive parents. Our data – see Section V below – confirm such a positive correlation in education and income. Bohman [1970], who studied adoptions in Sweden, showed that the actual behaviour of social workers generated such patterns.

Although the adoption case described above was by far the most common one, there were also other cases. One case was when a foster child had "grown into" the foster family so that the foster parents wanted to adopt the child. Step-parent adoptions were yet another kind of adoption, namely such when the spouse of one biological parent adopted the latter's child. Our data allow us to identify such cases; we found 6 cases and eliminated them. Adoptions could also take place within families, e.g., the parents of a young mother could adopt a child that would have been their biological grandchild. Such cases would create severe problems for our study. However, they were very rare during our study period, see Nordlöf [2001].

III.B. THE DATA SET AND DESCRIPTIVE STATISTICS

Because of the adoption process described above, the Swedish population register contains information about both adoptive and biological parents. From the population register, we also get access to the adopted children's siblings, both on their biological parents' side (our data distinguish between full and half siblings) and on their adoptive parents' side. On the latter side, there could be both siblings who also are adopted and siblings who are biological children of the adoptive parents.[10]

Further, we use education and tax registers to get information about parental characteristics and child outcomes at adult age. The 1970 census and the 1990 version of Statistics Sweden's special education register provide information about biological and adoptive parents' education. The 1970 Census, upon which the education register is built, contains detailed education information, which is available in terms of detailed education classifications. We infer years of schooling and a university dummy from this information.[11] We use tax-register data to get fathers' earnings and income for 1970, 1975, 1980, 1985 and 1990. Earnings include income from work including self-

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[10] Our data tell us in what census the adoptee first lived with its adoptive parents, although the parents were not necessarily formal adoptive parents at this point of time due to the trial period and delay with the court's decision.

[11] We use the education register dated 1990 to obtain parental education measures. If not available, we use the 1970 Census instead. The reason for doing so is that some parents were quite young in 1970. The 1970 Census, upon which the education register is built, and the 1990 register contain identical educational level classifications. We assign the following years of schooling to seven educational levels: 7 for (old) primary school, 9 for (new) compulsory schooling, 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university, and 19 for Ph.D. University education is a dichotomous variable that indicates whether someone completed 15 or more years of schooling.

employment and sickness benefits. Income includes earnings, but also some taxable benefits like unemployment insurance and pensions as well as capital income and realized capital gains. The tax registers in turn are based on compulsory reports from employers, and the taxable benefits are similarly reported by the authorities responsible for the schemes. We use father's income averaged over a 20-year period running from 1970 to 1990.[12] Because (due to the adoption guidelines in the early 1960s) adoptive mothers were expected not to work, we focus only on the intergenerational mobility effects of father's earnings and income. Our analysis thus follows the empirical literature on income mobility that focuses on the impact of long-run income of fathers.

We measure children's outcome in 1999 when children are 33-37 years of age. At this age, children should have completed their school and their annual income is likely to measure long-run (or lifetime) income quite well with only a classical measurement error. This means that our intergenerational estimates will not suffer from so called life-cycle bias.[13] Education information comes from the education register.[14] Children's income and earnings data stem from the same administrative registers as the ones for parents.

The number of adopted children born in Sweden in 1962-1964 was about 1100 each year, and fell to 1000 in 1965 and 900 in 1966.[15] The number of adoptees born in Sweden between 1962-1966 equals 5292. We work with a subsample of 2125 adopted children.

[12] More specifically, we first exclude those observations in which annual income (or earnings) is missing, below 1000 dollars, or obtained when parents were younger than 30 or older than 60. With annual income and earnings measures measured in logarithms, we then take averages.

[13] Using U.S. data, Haider and Solon [2006] examine how intergenerational income estimates are affected by the age at which offspring's income is observed. They find that the bias is small when annual income is measured around age 35. Böhlmark and Lindquist [2005], who replicate and extend Haider and Solon on Swedish data, arrive at the same conclusion.

[14] We use the maximum level achieved in the 1990, 1993, 1996 or 1999 education registers. For children and their parents we use the same transformation from levels to years.

[15] With a falling number of children born in Sweden given up for adoption, the number of foreign born adoptees started to increase. For example, between 1962 and 1966 the number of international adoptions rose from 100 to 350.

The reduction of 3067 observations gives the impression that nonresponse is serious. Of the 3067 adoptees who fell outside the sample, about 500 observations were lost because of a set of age restrictions pertaining to the adopted child and adoptive parents,[16] 135 adoptees had no records on their own or adoptive parents educational classifications, and about 570 adoptees were eliminated because information on their biological mother was missing. The main problem exists with the adoptees' biological fathers. Almost 2000 adoptees have biological fathers that are unknown.[17] This leaves us with 2125 adoptees. The income analysis is based on fewer observations than the education analysis: about 150 (335) adoptees had annual income (earnings) values that were either missing or below the Swedish equivalent of 1000 dollars.

Table I reports the means and standard deviations of the main variables in our analysis. We report these statistics for our adoptee sample and for a representative sample of all own-birth children required to have lived with both their biological parents in the fall 1970 census; the latter are drawn from a 20 percent random sample of all non-adopted children born in Sweden in 1962-1966.[18] The table's first panel shows that adopted children are quite similar to the random sample of same-aged own-birth children when it comes to the outcome variables. There are some small differences, however, and they consistently show that adopted children did slightly worse: years of schooling is

[16] We exclude those who (i) did not live with adoptive parents in the November 1970 census, (ii) died at age < 26 (45 adoptees), (iii) had too young adoptive parents (< 25) or a too old adoptive parent (mother > 47 or father > 66).

[17] The reduction of almost 2000 observations is substantial and may introduce sample selection bias. Later on in the paper we test whether the nonresponse is selective and muddling with our results. We find that this is not the case.

[18] We start off with 108550 but work with 94079 children. We loose almost 14500 observations because of missing school classifications (more than 5200) and because of children that are raised in single parent families (more than 9200).

about 0.4 years lower, the fraction with university education is 8 percentage points lower and earnings and income are 0.09 log points lower, compared to own-birth children.

The second panel compares the characteristics of own-birth and adopted children's birth parents. These differences are larger. Own-birth children's fathers have 0.73 more years of schooling than adopted children's fathers. The corresponding difference for mothers is 0.53. University education is also more frequent among own-birth children's birth parents than among adopted children's birth parents. The earnings and income differentials are 0.29 log points for fathers. Further, adopted children's birth parents are about 3.5 years younger than own-birth children's biological parents. The fraction of teenage parents is also considerably higher among adopted children.

The third panel reports characteristics of adoptive parents. Comparing with adopted children's birth parents, we find quite substantial differences, especially for father's characteristics. Average years of schooling are 1.30 higher, incidence of university education is 0.13 higher, earnings and income are around 0.40 log points higher. The age differentials are as large as 8.8 years for fathers and 9.6 years for mothers. These age differentials, in turn, probably account for some of the earnings and income differentials. In our subsequent analysis, we control for these age differentials.

These differences, which are all statistically significant, give the impression that children who are given up for adoption come from poorer families but are placed in well-to-do families. Despite mean differences between the parents, the standard deviations show that there is a considerable overlap between the three distributions. In some of the regressions using own-birth children and parents, we use samples of own-birth parents that are matched so as to mimic the adopted or biological parents. The matching procedure and the characteristics we match on are discussed in section V.D.

IV. BASIC RESULTS: LINEAR MODELS

Table II reports the intergenerational transmission estimates for education and income using a linear model. We run separate regressions on samples of own-birth and adopted children. In the first panel, we report the least square results of model (1) for education and income on the sample of own-birth children. All regressions include an intercept and individual controls for the child's gender, birth year and region of birth, and the respective parent’s birth year. These estimates are not reported. The regressions are typical of estimations in the previous intergenerational mobility literature.

The estimated effects of parental schooling show – as expected – that higher educated parents raise their children's schooling years and university graduation chances. We find that the schooling impacts of both parents are equally important. The magnitude of these estimates suggests that four more years of parental schooling - of either the father or mother - are associated with one more year of schooling for the child, and that a parent with a university degree is associated with a 34 percent higher likelihood that his or her children also will obtain such a degree. Another result, in columns 3 and 6, is that the coefficient for either parent's education falls quite substantially when their partner's education is brought into the equation. Assortative mating on education lies behind this pattern.

The estimated intergenerational elasticities with respect to father's earnings and income are virtually identical and equal to 0.24. They are similar to those obtained by Björklund and Jäntti [1997] and Björklund and Chadwick [2003] for Sweden.[19]

In the second panel, we report the results for specification (2) where our four outcomes for adoptees are run on the same variables for their adoptive and biological parents using the same format as before. The first three columns report the estimates for years of schooling. In column (1) we begin with the father-child link and find that the estimated effects of both the biological and adoptive father's years of schooling are statistically significant, positive and equally important. The estimate of 0.11 implies that one additional year of the biological or adoptive father's schooling raises the child's schooling by just over one-tenth of a year. In column (2), we consider the mother-child link. We observe positive and significant associations for both biological and adoptive mothers. Strikingly, however, the slope for biological mother's schooling of 0.13 is almost twice as high as the one for adoptive mothers.

Of course, the estimated transmission effects represent both the direct effect of the given parent's schooling and the indirect effect that comes from the other parent's schooling; the indirect effect is due to assortative mating on schooling, or on something else that correlates with schooling. In our sample, the years-of-schooling correlation between parents who adopt equals 0.49. Between adoptees' biological parents, however, this correlation is only 0.19. In column (3) we take the intergenerational effect of the partner into account by including both mothers' and fathers' schooling. For biological parents, the partial influences of both parents' years of schooling fall somewhat but remain statistically significant, positive and equally

[19] The previous mobility models are also estimated for sons and daughters separately. The intergenerational transmissions for schooling are found to be very similar. The intergenerational transmissions for earnings and income are, however, somewhat larger for sons. A similar pattern is observed when we estimate these models on our samples of female and male adoptees.

important. For adoptive parents, the partial schooling effects also fall, most notably for mothers. We find that the maternal schooling effect is no longer significant and close to zero while paternal schooling remains positive and significant. These results are in line with recent studies on intergenerational transmission of schooling that control for inherited ability and assortative mating and produce positive schooling effects for fathers but no effects for mothers; Behrman and Rosenzweig [2002, 2005], Antonovics and Goldberger [2005], Plug [2004].

In columns 4-6 of Table II, we switch the dependent and independent variable to a dummy for university degree. With this variable most of our findings are similar to those previously reported. The coefficients imply (*a*) that children with adoptive mothers and fathers with a university degree experience a significant higher chance of graduating from university themselves; (*b*) that for the intergenerational transmission of schooling, pre-birth factors also matter for university education; and (*c*) that the relative contribution of biological and adoptive parents are very similar to the ones obtained for years of schooling education, indicating that pre- and post-birth factors are equally important for father's schooling and that pre-birth circumstances are more important for mother's schooling. There is one notable difference. We find that the estimated university effect for adoptive mothers is positive and statistically significant with her partner's schooling included. Thus, it seems that for university education both adoptive parents' education contribute to the education of the next generation.

We now turn to earnings and income. In column (7), we begin with the father-child link in earnings and find that higher earnings of the adoptive and biological father are associated with higher earnings of the child. The estimated elasticities are 0.05 for the biological father's earnings and 0.10 for the adoptive father's earnings and only the latter is statistically significant.

The results for income, reported in column 8, are in line with what we observed for earnings. We find (*a*) positive and significant estimates for biological fathers; (*b*) positive and significant slopes for adoptive fathers; and (*c*) indications that adoptive fathers are more important than biological fathers in explaining the income link.[20]

In the bottom two rows of panel 2, we show the sum of the estimated coefficients for adoptive and biological parents of the adopted children. We find that, in most cases, the sum is only marginally different from the mobility estimate for a random sample of own-birth children (panel 1). Only for university education for mothers is the difference statistically significant.

We can learn four lessons from these adoption results. First, we find that biological parents matter. All our mobility specifications show positive and almost always significant slopes for biological fathers and mothers. The partial impacts we find for biological mothers' and fathers' education appear to be quite similar. This is exactly what we expected. As long as genes are automatically passed on from father to child and similarly from mother to child, the genetic effects should be identical. The small but positive differences between the effects of biological mothers and fathers further suggest that effects that run through the prenatal environment are relatively small. Such environmental effects are even smaller when we consider classification error as one of the alternative explanations. Suppose that the partial

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[20] We here note that the impact of the income of the adoptive father is much larger than for earnings. The difference in sample size is not responsible. In an analysis not reported in the paper, we find that income effects remain the same when estimated on the smaller earnings sample. The explanation for the different estimates is that post-birth factors are more important for the intergenerational transmission of non-labor income (mainly capital income for this sample) than for earnings, and that the reverse is true for post-birth factors. Anyway, when we test for equality of earnings and income coefficients, a t-test of 1.54 does not reject equality (p-value=0.123).

impacts of biological mothers and fathers are identical, but that fathers are more likely to be misclassified as biological fathers. We would then observe bigger effects for mothers. In adoption samples, where it might be more difficult to uncover the true identity of biological fathers, we would then expect classification errors to be higher for fathers than for mothers.

Second, we provide evidence that adoptive parents matter as well. In our schooling regressions, the positive and significant associations found for both parents do indicate that better educated parents provide a better environment for their children to do better in school.

Third, on the basis of a comparison of biological and adoptive parents, we find that most of the mother's influence on children takes place through pre-birth factors. For fathers we find pre- and post-birth factors to be equally important for education, whereas post-birth factors are more important for earnings and income.

Fourth, the total impact of the adoptive and biological parents' resources on the outcomes of adoptive children is remarkably similar to the impact of the biological parent's outcomes for that of biological children. This indicates that adoption per se (the break from the biological mother, the time at the nursery) has almost no effect on the strength of the intergenerational schooling association among parents and children.[21]

V. ROBUSTNESS OF BASIC RESULTS

While our estimates suggest that for adoptees both their adoptive and birth parents matter, we should treat these estimates with care. Several problems involved in using adoption data could lead to misinterpretations. In this section we concentrate on four of these, being (*a*) non-random placement of adoptees to their adoptive families; (*b*) not all adoptees are adopted as babies; (*c*) many adoptees have unknown birth fathers; and (*d*) adoptees and adoptive

[21] Note that this does not hold for the estimated effects found for mothers with university degrees.

parents are different from other children and their parents. To examine the impact of each of these four problems, we focus our attention to specifications that use years of schooling and earnings for reasons of brevity.[22]

V.A. NON-RANDOM ASSIGNMENT

In almost all adoption studies random assignment is assumed. But in the presence of selective placement this assumption is violated and adoption estimates are biased. Without information on the adoptees' birth parents – which applies to most adoption studies – the random assignment assumption is practically untestable. With our data on the adoptees' biological background, however, we can test whether our adoption results are sensitive to selective placement.

We first investigate whether there is evidence of non-random assignment of adoptees in Sweden and estimate the relationship between the education and earnings characteristics of the adoptive and biological parents of adoptees. Random assignment would give us zero correlations. We, on the other hand, find correlations that range from 0.091 for fathers' earnings to 0.144 (0.140) for fathers' (mothers') years of schooling. These numbers are quite high and suggest that non-random assignment among adoptees and their adoptive parents is substantial.[23]

If we think of this particular assignment problem as an omitted-variable bias problem, non-random assignment affects our adoption estimates if Y_i^{ap} is correlated with unobserved

[22] We have also compared the results with those obtained for university education and income. We found no systematic differences. Sensitivity results for university education and income are available upon request.

[23] These correlations are not driven by age and region effects. When we regress out age and region effects, the correlations remain virtually identical. Also, note that the magnitude of the correlations is very similar to the correlations in IQ between the biological and adoptive mother/father found in the well-known Texas Adoption Study [Brody 1992].

pre-birth factors and/or Y_j^{bp} is correlated with unobservable post-birth factors. To assess the robustness of our results against omitted variables we check how the estimates attached to the adoptive parent's schooling and earnings change when we (*a*) exclude the biological parent's controls for schooling and earnings, and (*b*) include as many background characteristics of the biological parents available and measured around the time of adoption.[24] Small changes indicate robustness. In rows 2 and 3 in Table III, we show that the coefficients that correspond to the adoptive parent's outcomes rise (fall) when we exclude (include) the biological parents' schooling or earnings, but not by much. The only exception appears to be the estimate for the adoptive father's earnings in row 3, which is much smaller than the one we observe in our baseline. Yet the difference is not significantly different. By the same logic, it is useful to see what happens to the coefficients of the adoptees' birth parents when we exclude and include information of the adoptive parents. The estimates attached to the adoptees' birth parents in rows 4 and 5 appear to be even less sensitive to the inclusion (exclusion) of the adoptive parents' characteristics. The coefficients change, but they change only in the margin.

If we instead think of this assignment problem as a measurement error problem, adoption estimates are biased as well. In addition to the standard argument that measurement error in Y_j^{bp} and Y_i^{ap} will bias the estimated pre- and post-birth effects to zero, there is also the combination of non-random assignment and measurement error that is pushing the estimated pre- and post-birth effects upwards. That is, the estimated influence of post-birth factors will increase the greater the errors in measuring pre-birth factors, and vice versa.

[24] Biological parents' age, marital status, education, earnings, income, occupation and regional controls measured at 1965 or 1970.

When we formalize these measurement error processes, we are able to assess the magnitude of the bias. Suppose that the true mobility model is given by

(5) $$y_i^{ac} = \alpha_0 + \alpha_1 y_j^{bp} + \alpha_2 y_i^{ap} + v_i^{ac}.$$

Instead of y_j^{bp} and y_i^{ap} we observe Y_j^{bp} and Y_i^{ap} .We assume classical measurement error and define $Y^k = y^k + \varepsilon^k$, where $\varepsilon^k (k = bp, ap)$ represent the measurement errors that are uncorrelated with y_j^{bp}, y_i^{ap} and with each other. If $Cov(Y_j^{bp}, Y_i^{ap}) = C_Y$, and $Var(Y_j^{bp}) = Var(Y_i^{ap}) = V_Y$, the least-squares estimators in our estimated model will have the following properties:

(6) $$p\lim \hat{\alpha}_1 = \alpha_1 + (1-h)\frac{m\alpha_2 - \alpha_1}{1-m^2},$$

(7) $$p\lim \hat{\alpha}_2 = \alpha_2 + (1-h)\frac{m\alpha_1 - \alpha_2}{1-m^2},$$

where $m = C_Y / V_Y$ and $h = V_y / V_Y$ represent non-random assignment and reliability ratio.[25]

If we further assume reliability ratios *h* of 0.88 for our years of schooling and log earnings measures and a correlation between biological and adoptive parents schooling and earnings *m* of 0.15 – in the upper end of what we find in our data – it is possible to predict the magnitude of the bias.[26] The estimates in Table III row 1 suggest that $\hat{\alpha}_1$ and $\hat{\alpha}_2$ are equal to 0.11, 0.13 and 0.05, and 0.11, 0.07 and 0.10, for fathers' and mothers' schooling and fathers' earnings, respectively. Equations (6) and (7) then predict that true

[25] Note that the regression of Y_j^{bp} on Y_i^{ap} gives us an estimate of *m*.

[26] Our reliability ratios are based on those found in previous studies. Isacsson [1999], using the same education data as we, estimates a reliability ratio for years of schooling of 0.88. Reliability ratios in annual earnings measures are smaller: about 0.64, see Björklund [1993]. Reliability ratios, however, increase with the number of measures. Would the reliability ratio be obtained with 5, instead of 1, earnings measures, like we do, the ratio would rise to .89 in the absence of serial correlation; see Solon [1992, footnote 17] for formulas. Serial correlation is likely to be low when income is measured five years apart.

values α_1 and α_2 should be 0.13, 0.15 and 0.05, and 0.13, 0.08 and 0.11. It turns out that the bias introduced by the combination of selective placement and measurement error accounts for no more than 13 percent of the estimated impact of the parents' characteristics. Note that the bias in the estimated pre-birth (post-birth) effects introduced by those observed post-birth (pre-birth) factors that are measured with error is much smaller and accounts for at most 5 percent of the estimated impact of the adoptive (biological) parents' characteristics.[27]

In sum, we do not believe that selective placement is affecting our results in a substantial way as results remain qualitatively very similar whether we exclude or include variables (as in the case of omitted variables) or when we assume reasonable magnitudes of measurement errors.

V.B. ADOPTION AGE

Our second problem is that some children are adopted at a later age. If a significant number of adoptees are not adopted as babies, we end up overestimating pre-birth effects and underestimating post-birth effects. So far we have ignored adoption age and implicitly assumed that adoption took place at birth. With respect to the obtained effects for the adoptees' birth parents this seems a reasonable assumption. In Sweden possible post-birth effects that come from the adoptees' birth parents do not exist for most adoptees since most children that are registered for adoption are placed in special nursery houses the moment they are born. With respect to the obtained effects estimated for the adoptive parents, we are not so sure whether the adoption-at-birth assumption is likely to hold. The problem is that we do

[27] An example of this type of analysis in a different setting is contained in Borjas [1992].

not have a clean measure of adoption age. We can infer the age at adoption only crudely from the time we observe whether children are adopted or not in the censuses of 1965 or 1970. If we look at those adoptees born exactly one year prior to the census date in 1965 – meaning those who are born between October and December 1964 – we find that about 80 percent of all adoptees (94 out of 117) are adopted within a year. If this is typical for adoptees during this time, it implies that over 80 percent of all our adoptees are adopted within a year of their birth date. We therefore do not worry too much about this issue. We still perform some sensitivity analysis by restricting our adoption sample to all children between 0 and 2 who are adopted at the time we observe them in the census.[28] With this subsample, we can estimate the effects that come from the adoptive parents more accurately looking at adoptees who are more likely to be adopted as babies and receive the full parental treatment. Results are reported in row 6 of Table III. Other than the reduction in sample size, we find that the estimates attached to the parental schooling and earnings variables are very similar to the ones we observe for the full sample These results give no compelling reason to believe that timing of adoption seriously affects our estimates.

V.C. UNKNOWN FATHERS

A third problem could arise because we have restricted our sample of adoptees to those for which we have information on both biological parents. But for almost half of the original sample the father is unknown. It is possible that children with unknown birth fathers are different from other children in ways related to their parental resources. To test whether absent information on the father affects the estimates, we extend the current sample of

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[28] These children are born between November 1963 and November 1965, and live with their adoptive parents – or parents who subsequently will become their adoptive parents – at the time we observe them in the census in (November) 1965.

adoptees with adoptees for which we have only information on birth mothers. In row 7 of Table III we report schooling estimates for birth and adoptive mothers that are almost identical to the ones observed for the restricted sample (but with higher precision). We therefore rule out this source of bias.

V.D. COMPARABLE SAMPLES

Finally, we address the problem that adoptees and adoptive parents are different from other children and their parents. As discussed in Section II.B, we do this in two different ways. We consider own-birth children who are reared in a childhood environment that is comparable with the post-childhood environment of adoptees. We then consider own-birth children whose parental characteristics are comparable with those of adoptees' birth parents. We look at comparable samples in two different ways, first by using sub-sampling and then using propensity score matching.

We begin with limiting our own-birth sample to those families who also have adopted children. We know that the adopted and own-birth children in these families share the same childhood environment, but not the pre-birth experience. If estimates for adoptees are to be informative about intergenerational associations between own-birth children and their parents, we expect estimates for own-birth children with adopted siblings to be similar to the estimates for all own-birth children. This is not the case. In rows 9 and 10 we see that the estimates for own-birth children in adoptive families are somewhat bigger than the estimates for the representative sample of own-birth children.[29] We then limit our birth sample to those

[29] We do not think that treatment differentials are driving these results. Of course, these findings are consistent with the idea that parents may favor their own offspring over their adopted children. Case, Lin and McLanahan (2000) propose selfish genes as one of the responsible mechanisms. Selfish genes, however, would also predict that intergenerational transmission for adoptees with own-birth siblings are weaker than for other

children born in families in which at least one child is given up for adoption. We know that these own-birth children share similar genes and pre-childhood experiences with adoptees, and that they therefore start their lives under very similar conditions as adoptees do. When we estimate previous intergenerational relationships using these particular own-birth children, we find schooling and earnings estimates that are smaller than those observed for all own-birth children.[30]

The disadvantage of looking at particular sub-samples is that we need to work with very small samples. We therefore also present results for comparable samples using propensity score matching in rows 12 and 13,[31] even though we acknowledge that this may not eliminate all the differences between adoptees and own-birth children. When we match the characteristics of the own-birth children's parents against the characteristics of the adoptees' rearing parents, we get slopes that are very similar and in most cases statistically identical to those obtained for all own-birth children (row 12). When we make the samples more comparable by matching the characteristics of own-birth children's parents against the characteristics of adoptees' biological parents, the results are in line with what we find earlier: all estimates are smaller than those observed for all own-birth children (row 13). In

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adoptees. Rows 1 and 8 indicate that this is not the case. In fact, most of the estimates for adoptive children in these families are quite similar to estimates for the sample of all adopted children.

[30] But if parents (mostly mothers) could choose and rather put their problematic child up for adoption, it is possible that we find lower correlations because these children are less sensitive to parental treatments.

[31] The matching is done by regressing an indicator of adoption status (1 if adopted, 0 if non-adopted) on the following variables: 4 birth year dummies for the child, child's gender, 5 educational level dummies of the father and mother in 1970, father's and mother's income in 1970, dummies for father and mother having positive income in 1970, father's and mother's earnings in 1970, dummies for father and mother having positive earnings in 1970, about 50 birth year dummies for the father and mother, mother's marriage status measured in 5 categories in 1970, 57 two-digit occupation dummies for both parent's occupation in 1970, and 25 region dummies of the mother in 1965. When we match on adoptees' family environment (panel 3) we use the characteristics of the adoptive mother and father and when we match on adoptees' biological background (panel 2) we use the characteristics of the biological mother and father. All of the estimates were obtained using Leuven and Sianesi's [2003] psmatch2 program for Stata.

most cases the reduction is statistically significant. Overall, we find that subsampling and matching generate similar patterns.

We conclude that our results using more comparable samples suggest that intergenerational associations of schooling, earnings and income for own-birth children are often stronger for own-birth children who share post-birth backgrounds with adoptees than those own-birth children who share pre-birth backgrounds with adoptees. If these findings also imply that intergenerational associations are much stronger in families that are better educated and generate more income, we might question whether the linear specifications we estimate in equations (1) and (2) are in fact linear. Our results indicate they are not.

VI. NON-LINEAR MODELS

We test for non-linear intergenerational transmission by including the square of parental schooling and the square of the father's earnings and income. In the first panel of table IV, where we report the new mobility estimates obtained from our representative sample of own-birth children, we find that the estimates attached to the quadratic terms are always positive and statistically significant. This clearly suggests that the intergenerational associations are stronger in families with higher education and income. Our results correspond to the non-linear intergenerational effects found in earlier mobility studies; Behrman and Taubman [1990], Solon [1992], Björklund and Chadwick [2003].

The question why intergenerational transmissions are so much stronger at the top than at the bottom of the schooling, earnings and income distribution is an important one. Some authors have argued that interactions between nature and nurture are very important (Dickens and Flynn 2001; Ridley 2003). And indeed, if smart children would benefit relatively more from having smart parents, the intergenerational transmission would be greatest among high-educated and high-income families. Finding credible evidence, however, is difficult. Our adoption data offer a great opportunity to test whether this is the mechanism at work.

To estimate that part of the transmission that comes from the interaction between the post-birth environment (adoptive parents) and genetic factors (biological parents) we include the interacted effect between the adoptive and biological parents. This is done in the second panel of Table IV. We find evidence of a positive interaction for mother's education and father's earnings and income, but not for father's education. To test whether these interacted effects are not picking up other non-linear effects that possibly exist between parents and their children, we also include the square of parental education, earnings and income. Our results indicate that this is not the case. The interacted estimates are not sensitive to the inclusion of higher order terms.

VII. CONCLUSIONS

In this paper we investigate the origins of intergenerational education and income associations using data on Sweden-born adoptees and their biological and adoptive parents. Our empirical strategy is to decompose the intergenerational association into pre-birth and post-birth components, or combinations thereof. We use the biological parent's characteristics as an indicator of genetic background and prenatal environment, and the adoptive parent's characteristics as indicator of the child's post-birth environment. Our conclusions follow from regressions where we simultaneously include the biological and adoptive parent's characteristics in intergenerational mobility equations.

We find that both pre- and post-birth factors are important for the child's educational and economic outcomes. That is, for none of the outcomes studied these factors can be said to be negligible. The relative contributions fluctuate a bit. For mother's education, for example, we find that pre-birth factors are more important than post-birth environment, whereas for father's long-run earnings and income, the post-birth environment is more important than pre-birth factors. We also find evidence of slightly larger intergenerational transmission coefficients for biological mothers than for biological fathers. Because the impact of the biological mother reflects both genetic and prenatal environmental factors, whereas the impact of the biological father reflects only genetic factors, we believe that prenatal environmental factors are small in magnitude. This in turn allows us to discuss our estimated pre- and post-birth effects using the classical nature and nurture labels. For example, we can interpret our estimated interaction effects between biological and adoptive parents' status as nature-nurture interactions. Interestingly, such interactions are positive and significantly different from zero, for mother's education and father's income. These interactions further corroborate our conclusion that both pre- and post-birth factors are important, but also suggest that the importance of nurture varies across the nature distribution.

What are the implications of these findings? First, we provide evidence that both adoptive and biological parents matter, which suggests that both nature and nurture components are important. This implies that any comprehensive explanatory theory of intergenerational mobility must incorporate both factors like genetic heredity and factors in the rearing family. Any theory that only focuses on one of these will be incomplete. These results also help us to understand why specific policies have an impact on intergenerational mobility. For instance, welfare policies can increase mobility by improving the environment in which children are raised. Further, anti-discrimination policies can increase mobility by reducing the impact of physical characteristics that are genetically determined. Second, our findings with respect to the positive nature and nurture interactions also raise some interesting issues. Both nature and nurture remain important, but if the two operate together, it becomes very difficult to separate one from another. If improving equality of opportunity is a policy objective, then our results suggest that policies should be designed to improve the conditions particularly for children raised in low-educated and poor families. After all, in the presence of interactions these children suffer from being less able, being raised under poorer conditions as well as the interactions between the two. But if, at the same time, these policies do not recognize that genetically disadvantaged children benefit much less from an improved environment than more able children do, our results also suggest why policies like these have not been that effective in the past.

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TABLE I
MEANS AND STANDARD DEVIATIONS OF VARIABLES FOR CHILDREN AND PARENTS

	Own-birth children		*Adopted children*	
		Children		
Years of schooling	12.07	*2.07*	11.67	*1.89*
University education	0.32	*0.47*	0.24	*0.43*
Log earnings in 1999	7.54	*0.67*	7.43	*0.72*
Log income in 1999	7.62	*0.56*	7.53	*0.58*
Male	0.51	*0.50*	0.52	*0.50*
Age in 1999	35.29	*1.42*	35.49	*1.42*
		Birth parents		
Years of schooling, father	9.63	*3.12*	8.90	*2.51*
Years of schooling, mother	9.65	*2.83*	9.12	*2.43*
University education, father	0.16	*0.36*	0.07	*0.26*
University education, mother	0.16	*0.37*	0.08	*0.28*
Average log earnings 1970-90, father	7.67	*0.44*	7.38	*0.51*
Average log income 1970-90, father	7.69	*0.43*	7.40	*0.46*
Age when child is born, father	30.37	*6.58*	26.88	*6.96*
Age when child is born, mother	27.09	*5.73*	23.35	*5.80*
Teenage mother	0.09	*0.29*	0.32	*0.47*
Teenage father	0.02	*0.14*	0.12	*0.33*
		Adoptive parents		
Years of schooling, father			10.20	*3.31*
Years of schooling, mother			9.67	*2.99*
University education, father			0.20	*0.40*
University education, mother			0.18	*0.39*
Average log earnings 1970-90, father			7.77	*0.47*
Average log income 1970-90, father			7.81	*0.44*
Age when child is born, father			35.66	*5.36*
Age when child is born, mother			32.96	*4.93*
Number of observations	94,079		2,125	

Notes: Standard deviations are shown in italics. The exceptions to the stated number of observations are: for log earnings in 1999: 87,490 for own-birth children and 1,827 for adopted children, for log income in 1999: 92,168 for own-birth children and 1,998 for adopted children. For average log earnings 1970-1999: 93,627 for birth fathers of own-birth children, 2,078 for birth fathers of adopted children and 1,981 for adoptive fathers of adopted children. For average log income 1970-1999: 93,831 (101,027) for birth fathers of own-birth children, 2,107 for birth fathers of adopted children and 2,120 for adoptive fathers of adopted children.

TABLE II
ESTIMATED TRANSMISSION COEFFICIENTS IN LINEAR MODELS

	Years of Schooling			University			Earnings	Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own-birth children								
Bio father	.240**		.170**	.339**		.237**	.235**	.241**
	(.002)		(.002)	(.004)		(.004)	(.005)	(.004)
Bio mother		.243**	.158**		.337**	.246**		
		(.002)	(.002)		(.004)	(.004)		
Adopted children								
Bio father	.113**		.094**	.184**		.148**	.047	.059*
	(.016)		(.016)	(.036)		(.036)	(.034)	(.028)
Bio mother		.132**	.101**		.261**	.229**		
		(.017)	(.017)		(.034)	(.034)		
Adoptive father	.114**		.094**	.165**		.102**	.098**	.172**
	(.013)		(.014)	(.024)		(.026)	(.038)	(.031)
Adoptive mother		.074**	.021		.145**	.097**		
		(.014)	(.015)		(.024)	(.026)		
Sum of estimates for bio and adoptive fathers	.227**		.188**	.349**		.249**	.145**	.231**
	(.019)		(.029)	(.040)		(.059)	(.049)	(.040)
Sum of estimates for bio and adoptive mothers		.207**	.122**		.406**	.326**		
		(.021)	(.016)		(.039)	(.029)		

Notes: Standard errors are shown in parentheses; * indicates significance at 5% level, and ** at 1% level. All specifications include controls for the child's gender, 4 birth cohort dummies for the child, 8 birth cohort dummies for biological/adoptive father/mother and 25 region dummies of where the biological/adoptive family lived in 1965. The numbers of observations in the second panel for own-birth and adopted children are: 94,079/2,125 in columns 1-6, 87,079/1,780 in columns 7 and 91,932/1,976 in column 8.

TABLE III

SENSITIVITY ANALYSES: ALTERNATIVE SAMPLES AND SPECIFICATIONS

	Years of schooling				Earnings	
	Fathers		Mothers		Fathers	
	Bio	Adopt	Bio	Adopt	Bio	Adopt
Adopted children						
(1) **Baseline results:**	.113**	.114**	.132**	.074**	.047**	.098
(N=2125, 1780)	(.016)	(.013)	(.017)	(.014)	(.034)	(.038)
Other samples:						
Non-random assignment						
(2) exclude info on birth		.126**		.093**		.095*
parents (N=2125, 1780)		(.013)		(.014)		(.037)
(3) include info on birth		.097**		.055*		.027
parents (N=2125, 1780)		(.013)		(.015)		(.039)
(4) exclude info on adoptive	.132**		.150**		.045	
parents (N=2125, 1780)	(.016)		(.017)		(.033)	
(5) include info on adoptive	.105**		.117**		.038	
parents (N=2125, 1780)	(.017)		(.018)		(.034)	
Age of adoption						
(6) adopted between age 0	.109**	.120**	.124**	.062*	.079	.124
and 2 (N=638, 573)	(.030)	(.024)	(.033)	(.026)	(.067)	(.074)
Missing birth fathers						
(7) including those without			.128**	.083**		
info on birth fathers			(.012)	(.010)		
(N=4123)						
Sub samples						
(8) raised with own-birth	.059	.129**	.114**	.052	.123	.035
siblings (N=526, 435)	(.034)	(.024)	(.035)	(.030)	(.064)	(.077)
Own-birth children						
(9) **Baseline results:**	.240**		.243**		.235**	
(N=94079, 87079)	(.002)		(.002)		(.005)	
Other samples:						
(10) raised with adopted	.285**		.251**		.280**	
siblings (N=412, 381)	(.031)		(.035)		(.080)	
(11) with bio siblings adopted	.180**		.106		.216	
out (N=193, 160)	(.056)		(.067)		(.113)	
Matched samples:						
(12) on adoptees, rearing	.248**		.254**		.217**	
parents (N=84358, 78229)	(.003)		(.004)		(.008)	
(13) on adoptees' bio	.199**		.196**		.182**	
background (N=93655, 86703)	(.008)		(.009)		(.021)	

Notes: Standard errors are shown in parentheses; * indicates significance at 5% level, and ** at 1% level. All specifications include controls for the child's gender, 4 birth cohort dummies for the child, 8 birth cohort dummies for biological/adoptive father/mother and 25 region dummies of where the biological/adoptive family lived in 1965. Also sample sizes are shown in parentheses. For instance, in row 1 (N=2125, 1780) means that number of observations is 2125 in columns 1-4 (for years of schooling) and 1780 in columns 5-6 (for earnings).

TABLE IV

ESTIMATED TRANSMISSION COEFFICIENTS IN NON-LINEAR MODELS WITH INTERACTIONS

	Years of schooling				University		Earnings		Income	
	Fathers		Mothers		Fathers	Mothers	Fathers		Fathers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Own-birth children										
Bio parent		-.009		-.058**				-.807**		-.938**
		(.015)		(.017)				(.075)		(.064)
Bio parent squared		.011**		.014**				.069**		.077**
		(.001)		(.001)				(.005)		(.004)
Adopted children										
Bio parent	.050	-.222	-.055	-.472**	.199**	.166**	-.187	-.403	-1.164*	-1.342*
	(.051)	(.127)	(.055)	(.139)	(.045)	(.041)	(.108)	(.502)	(.525)	(.670)
Bio parent squared		.015*		.023**				.017		.015
		(.006)		(.006)				(.037)		(.034)
Adoptive parent	.061	-.003	-.097	-.310**	.170**	.108**	-.293*	-.076	-.995*	-.998
	(.043)	(.090)	(.050)	(.121)	(.025)	(.026)	(.125)	(.648)	(.501)	(.710)
Adoptive parent squared		.004		.012*				-.003		.003
		(.004)		(.005)				(.043)		(.035)
Bio parent*Adoptive parent	.006	.003	.018**	.013*	-.041	.286**	.043**	.034**	.156*	.151*
	(.004)	(.005)	(.005)	(.005)	(.074)	(.071)	(.015)	(.010)	(.067)	(.068)

Notes: Standard errors are shown in parentheses; * indicates significance at 5% level, and ** at 1% level. All specifications include controls for the child's gender, 4 birth cohort dummies for the child, 8 birth cohort dummies for biological/adoptive father/mother and 25 region dummies of where the biological/adoptive family lived in 1965. The numbers of observations in the second panel for own-birth and adopted children are: 94,079/2,125 in columns 1-6, 87,079/1,780 in columns 7 and 91,932/1,976 in column 8.

APPENDIX TABLE (NOT INTENDED FOR PUBLICATION)
SENSITIVITY ANALYSES: ALTERNATIVE SAMPLES AND SPECIFICATIONS

	University				Income	
	Fathers		Mothers		Fathers	
	Bio	Adopt	Bio	Adopt	Bio	Adopt
Adopted children						
(1) **Baseline results:**	.184**	.165**	.261**	.145**	.059*	.172**
(N=2125, 1976)	(.036)	(.024)	(.034)	(.024)	(.028)	(.031)
Other samples:						
Non-random assignment						
(2) exclude info on birth		.177**		.174**		.169**
parents (N=2125, 1976)		(.024)		(.024)		(.030)
(3) include info on birth		.121**		.110**		.140**
parents (N=2125, 1976)		(.025)		(.025)		(.032)
(4) exclude info on adoptive	.209**		.298**		.058*	
parents (N=2125, 1976)	(.036)		(.033)		(.028)	
(5) include info on adoptive	.154**		.234**		.060	
parents (N=2125, 1976)	(.037)		(.034)		(.029)	
Age of adoption						
(6) adopted between age 0	.314**	.224**	.228**	.100*	.040	.178**
and 2 (N=638, 602)	(.068)	(.042)	(.062)	(.045)	(.055)	(.057)
Missing birth fathers						
(7) including those without			.211**	.142**		
info on birth fathers			(.023)	(.018)		
(N=4123)						
Sub samples						
(8) raised with own-birth	.171**	.172**	.256**	.131*	.082	.156*
siblings (N=526, 435)	(.074)	(.050)	(.070)	(.052)	(.053)	(.064)
Own-birth children						
(9) **Baseline results:**	.338**		.337**		.241**	
(N=940792, 91932)	(.004)		(.004)		(.004)	
Other samples:						
(10) raised with adopted	.392**		.276**		.297**	
siblings (N=412, 405)	(.053)		(.054)		(.072)	
(11) with bio siblings adopted	.497**		.001		.147	
out (N=193, 180)	(.116)		(.181)		(.101)	
Matched samples:						
(12) on adoptees, rearing	.248**		.254**		.217**	
parents (N=84358, 78229)	(.003)		(.004)		(.008)	
(13) on adoptees' bio	.199**		.196**		.182**	
background (N=82536,	(.008)		(.009)		(.021)	
91532)						

Notes: Standard errors are shown in parentheses; * indicates significance at 5% level, and ** at 1% level. All specifications include controls for the child's gender, 4 birth cohort dummies for the child, 8 birth cohort dummies for biological/adoptive father/mother and 25 region dummies of where the biological/adoptive family lived in 1965. Also sample sizes are shown in parentheses. For instance, in row 1 (N=2125, 1976) means that number of observations is 2125 in columns 1-4 (for university education) and 1976 in columns 5-6 (for income).