

Intergenerational effects in Sweden:
What can we learn from adoption data?

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Abstract:

We estimate the causal effects of parental schooling and earnings on that of their children using information on adopted children and their biological and adoptive parents living in Sweden. With adoptees, the idea is to eliminate effects that are genetically driven and thus mimic an experimental design where parents and children are randomly connected. Of course, real life samples of adoptees do not have the nice randomization characteristics of typical laboratory experiments. In this paper, we outline the potential dangers that usually affect the accuracy of adoption estimates, we discuss the assumptions needed to identify the intergenerational effects properly, and due to the richness of the data, we test how much these assumptions actually affect the outcomes.

Researchers from many academic disciplines have for long found adopted children an interesting group to study. Such children, especially when they have lived most of their lives with their adoptive parents, share their adoptive parents' and siblings' environmental influences but not their genetic ones. Thus, it has been argued, the study of adoptees can help researchers to disentangle genetic and environmental influences on various outcomes.

Data on adoptees (and/or twins) have been used in at least two different ways to learn about genetic ("nature") and environmental ("nurture") influences. One approach has been to allocate the total variance of an outcome into genetic and environmental components. In disciplines such as medicine, behavioral genetics and psychology, outcomes like diseases, personality traits and IQ have been investigated this way. Labor economists have used this approach to study influences of nature and nurture on education and earnings. See, e.g., Taubman (1976), Behrman et al. (1977), and Björklund, Jäntti & Solon (2003). Goldberger (1979) and Kamin & Goldberger (2002) have forcefully criticized much of the research following this approach. They have emphasized the random adoption assumption, but they also question whether the allocation of variance to genetic and environmental components provides meaningful information.¹

Another approach can be typified with the recent studies by Sacerdote (2000, 2002), Plug & Wijverberg (2003) and Plug (2003).² These studies use a regression framework to estimate the causal impact of a parental characteristic like schooling on a similar outcome

¹ In some applications, adoptees have been used in combination with twin data. A very rare, but interesting, group are monozygotic (so-called "identical") twins reared apart due to being adopted at birth by different adoptive parents. If the adoption process is random so that the environmental influences for such twin pairs are independent, all similarity among them would be due to their identical genetic setup. Thus such twin pairs are sometimes considered particularly useful for research purposes.

² See also Dearden et al. (1997), Björklund & Richardson (2001) and Das & Sjogren (2002) for more analysis along these lines.

for their offspring. Whereas a regression coefficient on parental schooling in a equation estimated on a random sample of parents and children is likely to capture causal schooling effects as well as genetic effects, the same coefficient estimated on a sample of adoptees is purged of the genetic effects. Thus it might better reflect the causal schooling effect of interest.

Both these research approaches using adoptees impose the same assumption about the adoption process, namely that the adopted children and their adoptive parents are connected (more or less) randomly. Two additional assumptions are that children are adopted as babies and that the rather small and potentially quite special group of adoptees can be used to make inference about the whole population. These assumptions are questionable and deserve to be scrutinized.

In this study we examine the usefulness of adoption data for research purposes such as those described above. We use a large data set of adoptees collected from Swedish population register data. These data are unique in a number of ways. Most important, we have information about biological and adoptive parents as well as biological and adoptive siblings. In this way we can examine whether the adoption process was random or not with respect to individual characteristics like schooling and earnings. Further, we can determine quite accurately from census data when then children moved to their adoptive parents. Finally, the children are adopted by both parents so we can rule out cases when one parent (usually the father) adopted a child that the spouse had from a previous relationship. We illustrate our analysis with an application employing the second regression-based research approach described above.

The paper proceeds as follows. We describe the institutional setting of adoptions in Sweden during our period of study as well as our data in section 1. In section 2, we report estimates of parental education and earnings effects using models previously used in the literature. We get results quite similar to those in previous studies, namely much smaller effects of adoptive parents' education and earnings than of biological parents'. Nonetheless, our estimates of the impact of adoptive parents' characteristics are reasonably precise and statistically different from zero. In section 3, we test whether the random adoption assumption. We find that it does not! Therefore we estimate models that controls for birth parents' characteristics. To our surprise, the results are not affected by this control. Section 4 concludes.

1. Institutions and data

We use administrative register data from Statistics Sweden on all formal adoptions, i.e., adoptions confirmed by the court and notified in the Swedish population register. In particular, our data set contains all persons who were born between 1962 and 1973 and were adopted by both parents. To understand the usefulness of this data set, we start in this section by describing the institutional rules governing adoptions in Sweden during this period of time. Then we describe the data set and the variables in some detail.

1.1. Adoptions in Sweden 1962 - 1973

Sweden has had an adoption law since 1918. Although some changes of the law has taken place, its basic content has been the same during the whole period.³ We focus on the law

³ Another informative source from which we have benefitted much is Nordlöf (2001), who thoroughly describes how adoptions took place in Stockholm municipality during 1918-1973.

and the adoption administration as they were formulated and implemented during 1962-1973. Our main sources are two Handbooks for social workers and authorities involved in adoption issues, *Allmänna barnhuset* (1955, 1969). These Handbooks are authorized by the National Board of Social Affairs. The 1969 Handbook was an updated version of the older one. The Handbooks also contain the central parts of the adoption law. We focus on the law and the adoption administration as they were formulated and implemented during 1962-1973.

The fundamental principle of the law, and its first paragraph, has always been that an adoption should always be “to the best of the child”. This means that the decision whether an adoption should take place or not, and the choice of adoptive parents conditional upon adoption should serve the interest of the child. Another important principle was that any economic compensation from the adoptive parents to the biological parents, or from the biological parents (or their families) to the adoptive parents were not allowed.⁴ Because the period we consider was characterized as one with “excess demand” from prospective parents, payments to biological parents would probably have been more common if such payments had been allowed. Both the law and the Handbooks said explicitly that the adoption process should be cancelled if such payments were done.

Adoptions could take place in many different circumstances. In order to describe the formal process, we start by describing a typical case. Then we proceed with the less common cases.

The process in a typical case

⁴ There was only one exception to this rule. The biological parents (or their families) could give a once-and-for-all lump-sum payment for the child. This amount of money must be administered by the social authorities who gradually should forward the money to the adoptive parents.

Most adoptions took place at an early age of the child and the mothers were unmarried and quite young. In a typical case a young pregnant women considered adoption and therefore contacted the social authorities. One social worker became responsible for the case and thus the key person in the adoption process.

The decision to adopt could not be taken until the mother had recovered from the delivery. Thus the new-born child was initially placed at a special nursery home. The social authority could then start to work on the match between the child and prospective adoptive parents. When a candidate adoptive home had been found, the child was temporarily, and on trial, placed in this home. The 1969 Handbook recommends that this placement should take place before age 6 months, but age 7-8 months was common. A common reason for the delay was that the mother could not decide whether she wanted to adopt or not.

When the trial period had turned out well, the time was ready for a formal adoption decision. The mother had to sign a form saying that she accepted the adoption. This form did not name the adoptive parents. Finally, the decision was taken in court, after a formal application by the adoptive parents, and information was sent to the population register. For all these reasons, the adopted child could have spent quite long time with its new parents when the formal adoption decision was taken.

An unmarried biological father had no formal say in the adoption process, but should be contacted on the issue and allowed to give his opinion. But quite often the father was unknown. Of our 8309 observations, the biological mother's time of birth was identified for 93 percent of all cases, and 58 percent for the fathers.

Who were the typical mothers (and fathers)?

(Remains to be written)

Who were the typical children?

The health status of child was a major concern. It was an important task for the social worker to investigate the child's health status. The two Handbooks devote different space to the health issue. The 1955 Handbook only says that the social worker should have a doctor examine the child before the final decision is taken by the court. The 1969 Handbook, however, has a longer section about the child's mental and physical condition. The main focus is on genetic heredity of such conditions, and a main message of the text is to downplay the risk of inherited problems. In particular, the Handbook stresses that alcoholism is not genetically inherited. However, a few severe mental problems (like Schizophrenia and depression) and some named physical problems were considered likely to be inherited. In such cases adoptions are not advised to take place. But the Handbook also stresses that a mental or physical problem not necessarily rules out an adoption. In such cases, however, special care must be taken in choosing the best possible adoptive parents.

We have no evidence from our sources suggesting that children of one specific sex were more available for adoption than children of the other sex. This is not to be expected either, as in the typical case the biological child was considered for adoption before the parents knew the sex of the child. One of our sources, however, reports that the demand

for girls for adoption was higher than for boys. The explanation given for this phenomenon was that girls were considered easier to adopt successfully than boys. Nonetheless, the 1969 Handbook cites a study from 1951 – Curman (1951) – that suggests that adoptions of girls were more frequently considered failures than adoptions of boys. The authors of the Handbook interpret this paradoxical finding as suggesting that those who could consider adopting boys were more tolerant persons than other adoptive parents. We have no ability to evaluate this information, but it motivates us to do separate analysis by sex.

Who were the typical adoptive parents?

Those who wanted to adopt should contact the social authorities in their municipality. One social worker was responsible for a very careful investigation of these parents. A series of interviews should take place and the social worker should summarize her experience in a special report that later was used to match parents and children. We have looked for information whether the same social worker was involved in the evaluations of both the child and the prospective parents. So far we do not have a clear picture in this respect.

Both the 1955 and the 1969 Handbook stress that economic resources and social status were not most important, although a reasonably “stable” economic situation was important. Further, the apartment should not inconveniently small for the family and the child, since otherwise a stressful situation could occur. The 1969 Handbook very stressed that good adoptive parents should be tolerant. The reason that tolerance was considered important was that an adopted child could get into problems and maybe not meet the

expectations of its parents. In that case, a tolerant attitude of the parents would be crucial. The Handbook provides several examples how the social worker could ask questions that would reveal whether parents are tolerant or not. Further, it was important to discover cases when prospective parents saw an adoption as a way of solving a problem of their own rather getting a child that they really wanted to care for.

“Normal people” are the best adoptive parents according to the 1969 Handbook. Nonetheless, due to the considerations, one would not expect to find adoptive parents evenly distributed in the income and education distribution. In particular, one would expect them to be under-represented in the lower parts of these distributions.

The law required that the adoptive parents should be 25 years of age. This rule did not apply to some step-parent adoptions however. There were no formal rules about an upper age limit. Both the 1955 and the 1969 Handbook recommends quite strongly, however, that adoptive parents should not be older than they could have been the biological parents of the child.

In contrast to some other countries, 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 get own biological children.

Single parents were according to the law allowed to adopt. Our sources convincingly demonstrate that such were extremely rare.

We have looked for information about the attitude to working mothers in the guidelines for adoptions. The 1955 Handbook is quite explicit in this respect. Illustrated by a drawing of a women lovingly holding a young child, the Handbook says that

families where the mother can stay home to care for the child, at least during the first years, should be given priority. The 1969 Handbook, however, is completely silent about working mothers.

The matching of the child and the parents

From our methodological perspective, the central issue is how the matching of children (and thus biological parents) and adoptive parents took place. In particular, we want to know whether matching could be related to any characteristics that are correlated with our outcome variables of interest, namely earnings and education. Because the social worker, who was responsible for the adoption process of a specific child, had access to quite much information about the biological parents (at least the mother) and prospective adoptive parents, there was room for such systematic matching. In many cases, however, biological parents did not know anything about the adoptive parents, and adoptive parents only wanted to have limited information about the biological parents.

The fact that there was room for systematic matching does not imply that such matching took place to a significant extent. The main focus in the two Handbooks is on guiding the social worker to find suitable adoptive parents in general. Indeed, the 1969 Handbook says explicitly: “The social worker’s ambition to find an adoptive home that fits a specific child particularly well is often unrealistic. The important task is to find good adoptive parents who can be expected to give children in general good conditions.” (Own translation from Barnsjukhuset (1969, page 31).

Although the 1955 Handbook also focuses on what are generally good adoptive parents rather than on good matches, it has a few sections that explicitly talks about

matching. After having stressed that adoptive parents should have a good and stable income that generally exceeds that of the biological family, the Handbook also says that the standard of the adoptive family should not be so high that the child does not fit in, (Barnsjukhuset 1955, page 38). Then follows a section where the same statement is done about intellectual standards. The Handbook says that “...although a stimulating environment can help develop any child, a too large gap between adoptive parents’ talent and that of the adopted child can cause problems for the child”. The motivation for this statement is that adoptive parents might become disappointed if the child does not meet the expectations of the adoptive parents.

Finally, the 1955 Handbook added that some attention should also be paid to the physical attributes, like height and colour, of the adoptive parents and the biological parents.⁵ Too large differences in this respect could make the child look strikingly different from its parents, a fact that can generate frequent annoying comments.

Our conclusion is that one cannot consider the matching of children and adoptive parents during the period 1962-1973 as the outcome of a random process. However, it is an open question whether the relevant correlation between the inherited traits from the biological parents and those receive from the adoptive parents is strong enough to severely an estimator that assumes random matching of adoptees.

Other cases of adoptions

Although the typical adoption case described above seems to have been the most common one, other cases are also discussed in the literature. Some of these other cases

⁵ This section of the Handbook is illustrated with a drawing of a black-haired child and two blond parents with the subtitle: “One should avoid placing a child with a typical South-European look among blond and tall parents”.

required more or less the same careful investigations by social authorities as the typical case described above.

The same procedure applied when the biological parents were married. The study of adoptees in Stockholm (Nordlöf, 2001) showed that some 8 percent of all adopted children had married biological parents. Our sources suggest that adoptions took place in such cases because the biological parents were not able to take care of the child.

It could also happen that older children were adopted. A frequent case was when a foster child “grew into” the foster family so that the foster parents wanted to adopt the child. In such cases the formal procedure was slightly different.

Step-parent adoptions were yet another kind of adoptions, namely such when the spouse of one biological parent adopted the latter’s child. There were two cases of such adoptions and they show up in the population register data in different ways. If one spouse had a child from a previous marriage that a new spouse wanted to adopt, such an adoption could take place. In such a case, the child is simply adopted by the new spouse. Because such children are adopted by one parent only, they are not part of our basic data set.

Another case of step-parent adoption took place when one spouse brought to a new marriage an own child that was born outside of marriage. In such cases, the child could not formally be adopted by one new parent only. Instead it had to be adopted by both the biological parent who brought the child to the new relationship and the new spouse. Thus, it could be that some children who are adopted by two parents have one formal adoptive parent who also is a biological parent. From the data point of view, such cases are problematic for us because those who are counted as an adoptive parent could also be a

real biological parent. In our data set, such cases can be detected as cases when the biological parent and the adoptive parent have identical values on all variables. As we explain below, we looked for such cases and found only very few.

Adoptions could also take place within families, e.g., the parents of a young mother could adopt a child that would be their biological grandchild. Such cases would create severe problems for our study. We are confident, though, that such cases are extremely rare during the period that we consider. According to Nordlöf (2001), who examine the basic information pertaining to all adoptions in Stockholm municipality, found that among 1186 adoptions of children born 1960-73 only four were done by relatives. Such adoptions were slightly less uncommon in the 1940s and before. Although Stockholm might not be representative in this respect, we do not believe that such within-family adoptions constitute a severe problem for our data set.

1.2 The data set

The court made the final formal decision about adoption after an application from the adoptive parents. When the decision had been taken, a report was sent to the population register office. Thus the population register has information about both the biological and adoptive parents.

There is a variable in the Swedish register data on date of adoption. But the variable has many missing values, and a civil servant at Statistics Sweden told us that this variable is not reliable. Thus we use the census information to figure out in what household the adopted children lived at a certain period of time.

Table 1 reports our total data set of all children adopted by both parents during the period 1962-1973. The total number was quite stable around 1000 children per year during the first three years, but then started to decline quite dramatically. A new abortion law, effective from 1963 onwards, and more efficient birth control are considered the most important explanations of the decline. Because there was an “excess demand” for children to adopt already before this decline, it is not surprising that the 1962-73 was the period when international adoptions became more frequent, see Björklund & Richardson (2001).

Our analysis samples are smaller than the 8309 observations in Table 1. One restriction that we impose is that the adoption took place – that is the child had moved to the adoptive parents – before age ten. Further, the education analysis requires that both child and parental education data are available. Similarly, the earnings/income analysis requires that child and parental earnings/income data are available.

The Swedish population-register system thus makes it possible to identify adopted children, their biological parents, and their adoptive parents. We have also access to the adoptive children’s siblings, both their adoptive siblings via their adoptive parents and their biological (full- or half) siblings via their biological parents.

We also use other options offered by Statistics Sweden’s data. We use information from the censuses in 1965, 1970, 1975, 1980, 1985 and 1990 to identify with whom the children lived. In this way we can with some, but not absolute, precision determine when the adopted children started to live with the adoptive parents. Further, we use other registers to get information about parental characteristics and child outcomes at adult age. We use the 1970 census to get information about biological and adoptive parents’

education. (The Swedish 1970 Census contains detailed education information.) Further, we use tax register data to get father's income for the census years 1970, 1975, 1980, 1985 and 1990.

We measure children's outcome in the 1990s, also using administrative register data. Education information comes from the special education register (*utbildningsregistret*) that Statistics Sweden has developed since the late 1980s and that has high quality and coverage. Annual earnings also stem from administrative registers, which in turn have been created by information from compulsory tax reports from employers.

Both the information on parental education from 1970 and children's education from the 1990s exist in terms very detailed classifications into level and field of education. We have transformed this information into years of education.

1.3 Descriptive statistics

See Table 2.

(This section remains to be written)

2. Baseline results

2.1 The effects of parents' education: baseline results

In our empirical analysis on education effects we use two alternative measures of education. The first is the traditional measure, years of education. For parents we infer years of education from the detailed information in the 1970 census.⁶ For children we use the information in Statistics Sweden's education register dated 1999 to infer years of schooling. Thus, the children born in 1973 are at least 26 years of age when we measure this outcome. The second measure is a dummy variable for a college degree.

We then regress the education variables of the child on the same variables of their parent(s). Table 3 reports these intergenerational estimates. The structure of Table 3 is as follows. The first three columns present the estimates based on years of education. The next three columns present the estimates based on college education. Within columns we distinguish between father's and mother's education. The first panel presents estimates of separate regressions using a samples on own birth children. The second panel presents estimates of the same regressions using a sample on adoptees. All regressions include individual controls for the child's age and gender, and the father's and mother's age. These parameters are not reported.

We begin with the estimates that come from a sample using children who are their parents' own biological offspring. As expected the coefficients are strongly significantly different from zero. The order of magnitude of the estimates for years of schooling suggest that one more year of parental education – father's or mother's – is associated with one quarter of a year more education for the child. Another result, in columns 3 and 6, is that the coefficient for either parent's education is reduced when the other parent's

⁶ In fact, we use the highest education reported in either the 1970 census or the education register dated 1990. The reason for doing so is that some parents were quite young in 1970.

education also is incorporated in the equation. Assortative mating on education lies behind this pattern.

As we already mentioned, we are not so much interested in effects that are genetically driven. We would prefer to have information on that part of education that is uncontaminated with family genes but still responsible for the success of future generations. In the next panel we get rid of the influence of family genes by estimating the previous intergenerational specifications on a sample of adoptees. We find that the estimated effects fall significantly, but that the impact of parental education on the education of adoptees remains statistically significant and positive.

2.2 The effect of parents' earnings and income: baseline results

In our empirical analysis on income effects we choose our outcome variables to be earnings and total income. Earnings include all income components that come from work. Total income includes these earnings, but also pensions, income from own business, capital income and realizations from capital gain.⁷ We then regress the income variables of the child on the same income variables of their parent(s). We define these income variables as follows:

- The dependent variable Y_i^c is the logarithm of the child's annual earnings and total income obtained in 1999. We restrict ourselves to children of 30 years and older who earn more than a 1000 dollars.⁸

⁷ In the descriptive statistics tables we use the consumer price index to make these income measures comparable over time.

⁸ Measurement error --associated with transitory income shocks-- in the dependent variable increase the estimated standard errors and may bias the constant term. Since slope coefficients are not affected, we are not so much concerned about measurement error in the child's income and earnings variable.

- The explanatory variable Y_i^p is a parental income average taken over a 20 year period running from 1970 to 1990. We first exclude those observations in which annual earnings and total income are missing, less than a 1000 dollars, or obtained when parents were younger than 30 or older than 60.⁹ We then pool all these income measures measured in logarithms for the observed years, regress out age and time effects, and use the remaining residuals to construct a 20-year average. We do this separately for annual earnings and total income of fathers (Y_i^f), and for annual earnings and total income of the family including the annual earnings and total income of both fathers and mothers ($Y_i^f + Y_i^m$).¹⁰

Previous estimates of intergenerational mobility mostly report earnings and income relationships between fathers and their children. In this paper we also look at family earnings and income because we think it is the sum of all income components that is relevant in explaining the effects of expenditures on children.¹¹

Table 4 reports the intergenerational estimates of earnings and total income. The structure of Table 4 is as follows. The first two columns present the estimates based on earnings. The next two columns present the estimates based on total income. Within columns we distinguish between the earnings and total income obtained by the father and obtained by the whole family (including the earnings and total income of both father and

⁹ The age and income restrictions we impose on our sample are common in other intergenerational mobility studies.

¹⁰ Since measurement error in the explanatory variable biases any estimated mobility effect to zero, we are concerned about measurement error in the parent's income and earnings variable. To tackle the problem of measurement error, and the downward bias that it entails, we follow Solon's suggestion (1992) and just take the average over a 20 year period.

¹¹ We do not perform a separate exercise for mothers. We think it is too risky to attribute the influences of the mother's earnings to expenditures on children. Results could then just as easily come from labor supply effects where mothers spend relatively more time working than raising children.

mother). The first panel presents estimates of separate regressions using a sample on own birth children. The second panel presents estimates of the same regressions using a sample on adoptees. All regressions include individual controls for the child's age and gender, and the father's and mother's age. These parameters are not reported.

We begin with the earnings estimates that come from a sample using children who are their parents' own biological offspring. In column (1) we look at the child's earnings and include the father's earnings as a separate regressor. As expected, the estimated coefficient indicates that richer fathers have richer children. In column (2) we replace father's earnings with family earnings and observe that the mobility estimate using family earnings is somewhat higher. The elasticities we find range from 0.22 for father and child earnings to 0.28 for family and child earnings. These estimates are very similar to those obtained by Björklund and Jäntti (1997). When we switch the relevant income variables to total income in columns (3) and (4), results remain virtually identical.

As we already mentioned, we are not so much interested in effects that are genetically driven. We would prefer to have information on that part of the income that is uncontaminated with family genes but still responsible for the success of future generations. In the next panel we get rid of the influence of family genes by estimating the previous intergenerational specifications on a sample of adoptees. In columns (1) and (2) we find that the estimated effects using parental earnings fall significantly, but that the impact of parental earnings on the earnings of adoptees remains statistically significant and positive. Compared to previous estimates, we find that the earnings

elasticities are cut in half. In columns (3) and (4) we replace earnings for total income, and see that the elasticities drop to 0.15.¹²

At this stage, our estimates indicate that the earnings and income elasticities are much lower for adoptees than for own birth children. As we already mentioned, these estimates are probably subject to several caveats that are common to adoption experiments. To these caveats we turn in section 3.

Quantile regression results. See table 8.

(Text remains to be written)

3. Tests of the underlying assumptions

3.1 Are children randomly assigned to their adoptive parents?

The key assumption in most adoption experiments is that children given up for adoption are randomly placed in their adoptive families. But is the assignment of adoptees to their adoptive parents purely random? Probably not. If adoptions are somehow related and involve parents who raise and adopt the children of relatives, friends or neighbors, there is selectivity due to matching (i.e., genetic bias).¹³ Non-random assignment will push

¹² It is interesting to see that the earnings and income relationships between fathers and their adopted children are very similar to the earnings and income relationship between families and their adopted children. Is it possible that adoption agencies apply certain selection rules that are consistent with this finding? Perhaps, adoption agencies place children in families under the condition that mothers stay at home and raise their children.

¹³ Another reason for non-random matching could be that adoption agencies use information on the characteristics of the biological or social parents to place children in their adoptive families. This was discussed in section 1.

estimated effects upwards because the estimates then capture both treatment and selection.¹⁴

To see whether there is selective placement in Sweden, we begin to compare education and income characteristics of adoptive and own birth parents of adoptees. Random assignment would give us zero correlations. The first panel in Table 5A, however, reports correlations that range from 0.117 to 0.142. The first panel in Table 5B reports correlations that range from 0.109 to 0.140. These numbers are quite high and suggest that non-random assignment between adoptees and their adoptive parents is substantial. To get an idea of the magnitude of these correlations, note that the earnings correlations in Sweden for brothers range from 0.250 to 0.283 (Björklund, Eriksson, Jäntti, Raaum and Österbacka 2002).

Having established that children are not randomly assigned to their adoptive parents, it is most likely that our adoption estimates are tainted by selective placements.¹⁵ In order to get an accurate assessment of mobility estimates, these selection effects need to be taken into account. In Tables 5A and 5B we present two separate tests where these selection effects are quantified.

Our first test is rather straightforward. We limit our adoption sample to adoptees for which we have outcome information on both natural and adoptive parents, and run two separate mobility models on this restricted adoption sample. We first re-estimate previous mobility models and test whether the mobility effects correspond to those previously reported. These estimates are presented in the second panel of Tables 5A and 5B. We find positive and significant mobility effects, very close to the effects observed for all

¹⁴ Note that adoptions of relatives should only be a problem if within-home country adoptions are made.

¹⁵ It might be the case that assignment is random, conditional on some variables. For instance, we should add controls for the regions where the adoptees were born.

adoptees. The likelihood ratio tests –not reported in this paper- further reveal that none of the estimated coefficients varies across adoptees with and without information available on their birth parents.

We then run these mobility regressions once more and include the outcome characteristics of the adoptees' own birth parents as additional regressors (controls). This is the actual test. The underlying idea is fairly simple. If the adoptees' natural and adoptive parents have something in common, and that something correlates with earnings and income, then mobility effects as estimated in the previous panel also capture selection effects. Because part of the treatment effect as identified by adoption now comes from the adoptees' own birth parents, we should be able to control for it by adding the outcome measures of the adoptees' own birth parents as additional regressors. Panel 3 in Tables 5A and 5B presents these regression results. Much to our surprise, we find that the estimates that measure the impact of adoptive parents' characteristics on that of their children are insensitive to the inclusion of the birth parents' characteristics. The estimates we get for we find for adoptees vary between 0.049 and 0.107 for education and between 0.103 and 0.165 for earnings/income. Note again that education is measured in years whereas earnings/incomes are logged.

Results on selective placements -associated to the birth parents' coefficients- are somewhat mixed. The earnings and income effects of birth parents are only statistically significant and positive when we consider the earnings and income that is generated by both birth parents. The earnings and income effects of birth fathers are very small in size, and statistically insignificant. We are aware that these particular income effects not only

pick up that part of income that is due to selection, but also that part of income that is genetically driven. The results for education, however, are statistically significant.

The second test identifies the degree of selective placement of adoptees directly and involves children who are raised by their own birth parents in the presence of adopted siblings. We limit our sample of own birth children to children with adopted siblings for which we have education or earnings/income information available on their natural parents. We then run regressions on this restricted sample using characteristics of the adopted siblings' birth parents as explanatory variables. The idea is that under random assignment these own birth children have no relationship with these parents. In fact, what we do is we mimic a situation where we regress the child's outcomes on the same variables of complete strangers. Any estimated effect would then come from selection, and demonstrate that these children and parents are somehow connected.¹⁶ In the fourth panel of Table 5B, we observe that all these estimated earnings and income effects are close to zero. The corresponding estimates for education reported in Table 5A are, however, positive and significantly different from zero.

To conclude, selective placements appear to be common practice in Sweden in the early 60s, but they do not appear to affect our mobility results found for adoptees. This means that we can take our adoption results seriously, even in the presence of non-random assignment.

3.2 Are all adoptees adopted at birth?

¹⁶ We thank Alan Krueger who suggested this idea. To let this test be informative, we ignore sibling or peer effects.

So far we have ignored all timing effects, and treated our estimates as if all adoptees receive the same treatment. However, some adoptees fail to receive the life-long treatment effect because they are placed in adoption families at a later age. This type of heterogeneous treatment effects would be absent if all children are adopted as babies. But are all adoptees in our sample adopted at birth? Probably not. There is some evidence that in the early 60s most adoptees born in Sweden were 7 to 8 months old at the time of adoption. However, we cannot rule out at forehand that some adoptees are brought into their adoptive families at a later age and affect our estimates.

Registers do not record the age of adoption. Instead, we measure whether or not a child is adopted in 1965, 1970 or in 1975. This information, in combination with birth date, gives us a rough proxy for the time of adoption. For some children we know they are adopted within one month of birth, whereas for others we can only be sure that they are adopted within the first 5 years after birth. With this proxy it is possible to deal with this timing issue in part. We perform three separate tests.

The first test is rather straightforward. We re-estimate previous models using only adoptees that are adopted within the first 12 months of birth. The idea is that treatment effects should then be estimated more accurately using a sample that is limited to adoptees who receive the full parental treatment. Results are reported in the first panel of Table 6A and 6B. We find that the estimates attached to all the earnings and income variables indicate that effects are indeed somewhat bigger for adoptees who are adopted as babies. Yet it is rather difficult to draw firm conclusions about timing effects because of small sample size and large standard errors.

The second test measures the accuracy of our age of adoption proxy. We run regressions on two adoption samples. One sample consists of adoptees age 0-2 at time of adoption born 2 years prior the observation date in 1965. The other sample consists of adoptees age 0-5 at time of adoption born two years after the observation date in 1965. We report these results in Panels 2 and 3. The estimates are practically identical across subsamples. Our findings support the notion that in the 1960s most adoptees born in Sweden were 7 to 8 months old at the time of adoption, and that adoptees who are adopted at a later age are not affecting our mobility results.

To explore this issue further, we use the full adoption sample and allow treatment effects to be different for adoptees adopted at different ages through interaction effects. We re-run the regressions and add age of adoption and age of adoption interacted with the parental earnings and income variables as additional regressors.¹⁷ A negative interaction effect would then correspond with the idea that adoptees adopted at a later age receive only part of the treatment. In panel 4 we find that the interaction effects are close to zero and statistically insignificant. Timing effects do not seem to be important. However, when we replace the earnings with total income we find marginally significant negative estimates on interacted income. Now timing of adoption seems to affect the treatment children receive. The effect of age of placement on the treatment is relatively small. Provided that timing effects are correctly specified, these estimates can still predict the intergenerational estimate. Conditional upon adoption at birth, the elasticities we find range from 0.20 for father and child income to 0.22 for family and child income.

¹⁷ We need to report the estimate for the main effect of adoption age here as well. If we only add the main effect first, we also see if the characteristics of the parents are enough related to adoption age to affect the intergenerational estimate of interest.

These observations give no compelling reason to believe that timing of adoption seriously affects the treatment.

3.3 Are adoptees and their parents different?

We now turn to the question whether we can compare our adoption sample with more conventional samples on parents and their own birth children. Why might these groups not be comparable? Adoptees might be drawn from the pool of children with disadvantageous opportunities, either because of having inferior genes or because of negative early environmental experiences, emerging either in the womb or due time in institutional caretaking.¹⁸ The parents that adopt might be better than average parents, either because they met the requirement that is needed in order to adopt or because they have a clear preference for parenting. We therefore first compare descriptive statistics to see if adoptees and their adoptive parents are different from own birth children and their parents. This is done in Table 7A and 7B.

Still we are only faced with a serious problem if these differences are somehow related to the mobility effects we have estimated thus far. We present two separate tests. The first test involves own birth children who are brought up by their birth parents who also registered one of their own birth children for adoption. If the sample is limited to these particular own birth children and treatment effects are estimated from this subsample, we indirectly isolate the unobservables that are typical to adoptees and thus for the bias that it entails. If children who are given up for adoption are indeed different from other children, we should then observe treatment effects that are more comparable

¹⁸ These issues are much more important when using foreign adoptees. Undernourished mothers in developing countries or neglected institution for children waiting to be adopted.

to the effects found for adoptees. This is not the case. Table 7A and 7B panel 1 reports positive and significant mobility effects, much more similar to the effects observed for all own birth children.

The second test exploits a very similar idea. If adoptive parents are different from other parents, we might control for these omitted variables by limiting the sample of own birth children to those raised in adoptive families and estimate the treatment from this subsample. If parents that adopt are indeed different from other parents in ways related to the treatment, then smaller mobility effects should be observed.¹⁹ We are left undecided. Table 7A and 7B, panel 2, report positive and significant effects. But the elasticities we find are always smaller than those observed for all own birth children, and always higher than those observed for adoptees.

5. Discussion and conclusions

(Remains to be written)

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¹⁹ We ignore the possibility that parents emotionally and materially differentiate between their own birth and adopted children.

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