Does family income matter for schooling outcomes? Using adoption as a natural experiment

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One would expect that family income is an important positive factor in the school attainment of children. However, evidence on this relationship is often tainted by the lack of control for parental ability, since at least a portion of ability is transferred genetically to children. This paper uses a sample of adopted children and offers genetically unbiased estimates. We further aim to correct for biases arising from unobserved parenting qualities, parents' emotional and material differentiation between their own birth and adopted children, and the non-randomness by which adopted children are placed in adopting families. We find that family income still has a significant effect, which is consistent with a causal interpretation. It implies that high-ability children in low-income families face binding credit constraints that society may wish to relieve.

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Abstract

One would expect that family income is an important positive factor in the school attainment of children. However, evidence on this relationship is often tainted by the lack of control for parental ability, since at least a portion of ability is transferred genetically to children. This paper uses a sample of adopted children and offers genetically unbiased estimates. We further aim to correct for biases arising from unobserved parenting qualities, parents' emotional and material differentiation between their own birth and adopted children, and the non-randomness by which adopted children are placed in adopting families. We find that family income still has a significant effect, which is consistent with a causal interpretation. It implies that high-ability children in low-income families face binding credit constraints that society may wish to relieve.

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1 Introduction

Many empirical studies find family income to be an important factor in explaining school success of children (Becker and Tomes, 1985; Taubman, 1989; Haveman and Wolfe, 1995). The mechanism economists offer to explain this family relation is that children from poor families are restricted in their pursuit of more and higher quality education merely because their parents face credit constraints when financing their children's education.

More recently, economists are aware that such conclusions are as yet unwarranted because of flaws in the underlying empirical models (Blau, 1999; Cameron and Heckman, 1998, 2001; Cameron and Taber, 2001; Mayer, 1997; Shea, 2000). The problem is that most studies ignore the strong correlation of both family income and educational attainment with (mostly unobserved) ability. Any correlation between family income and children's school success may therefore not be indicative of causality at all: parents with high earnings are on average better endowed with ability than parents with low earnings, and they also tend to produce children who do well in school by virtue of superior genes. The fact that these children are successful and come from high income families fails to prove causality.

The relative importance of family income is clearly of relevance for both understanding the dynamics of the distribution of educational attainment and designing educational policies. If the income of parents has no impact on the production of human capital among their offspring, the distribution of income at any point in time is merely a reflection of the distribution of ability among the then-existing population. If family income does matter, the present distribution of income also depends on the income distribution among the previous generation and on the existence of credit constraints. A better understanding of the role of family income is therefore also required when designing educational policies. If public resources are spent on educational programs that are believed to alleviate the financial constraints of students from poor families, it is important to know whether family income is the actual mechanism at work. Similarly, income tax policies may, or may not, have a long term impact on the distribution of income of the next generation.

An experimental design where family income and children are randomly connected would break the genetic link and prove causality. In this paper we will mimic this experiment and look at children that are not genetically descended

from the family that rears them. Adopted children fulfill this requirement. Therefore, a relationship between family income and educational outcomes estimated on a sample of adopted children should be interpreted as causal.

There are, however, two pitfalls that are typical to adoption experiments which prevent us from conclusively establishing the existence of a causal relationship. First, genetically unbiased income estimates may still suffer from ability bias and thus do not necessarily prove that income matters. If parents who make more money are better parents to begin with, the estimated family income effects on the educational attainment of adopted children are too high because (unobserved) parenting qualities and (observed) parental income are positively correlated. Second, adoption results do not prove that family income matters for all children. Two reasons apply. First, parents may have heterogeneous preferences about children and treat their own birth children differently from their adopted ones.[1] Second, samples of adopted children produce insightful information for all children only if children are randomly given up for adoption and if these children are then randomly assigned to the new family of rearing. In practice, however, these randomization requirements are rarely met. We assemble additional evidence that make it plausible that these two pitfalls are not too deep and that the relationship is almost surely causal.

The plan of the paper unfolds as follows. Section 2 provides a brief background on adoptees in the economics literature. Section 3 describes our data. Section 4 presents and discusses the empirical findings. Section 5 further explores the problems that usually relate to adoption studies and discusses to what extent our findings are subject to contradictory interpretations due to underinvestment arguments and selectivity bias. Section 6 summarizes our conclusions.

2 Adoptees and schooling investments

In economics the idea to use adopted children is not entirely new, but the extent to which adoption results can be used for the relationship between family income and educational attainment is not readily known. There are only a few studies available that attempt to document (indirectly) the association between parent's income and the educational attainment of adopted children. We will go through these studies briefly and discuss our contributions relative to their work.

An important motivation for examining adoptees is to disentangle the relative contribution of nature and nurture in economic outcomes. In this context, Sacerdote (2002) explores a small sample of 170 adoptees.[2] He uses family income as the only explanatory variable and finds a positive effect of the parental

[1] According to evidence in Case, Lin and McLanahan (2000, 2001), parents treat their own children better.

[2] Sacerdote uses three separate samples of adopted children to examine the relative importance of nature and nurture on various outcomes like childhood test scores, educational attainment, marital status and earnings. Only one of these samples is used to examine the relationship between family income and educational attainment.

income on the adoptees' years of schooling. He also estimates the effect of parental income for own birth children and finds that both income estimates are statistically identical. He interprets these results to mean that the positive influence of family income on the educational attainment is rather driven by the family environment than by the family genes. But because his estimates do not correct for the biases arising from unobserved parenting qualities, from the non-randomness by which adopted children are placed in adopting families, and from parents' emotional and material differentiation between their own offspring and their adopted children, a causal interpretation is not that persuasive. In this paper we explore what happens to our income estimates when we deal with these potential biases directly.

In a similar fashion, Plug and Vijverberg (2000) analyze a much larger sample of adoptees (see also Section 3). Their primary aim is to disentangle the effect of parental IQ into a nature and nurture component on the basis of differences in educational attainment between adoptees and children who are their parents' own offspring. Family income enters their analysis merely to see whether the IQ transmission mechanism is robust. They estimate that family income effects are positive and identical for adopted and biological children from which they conclude that parental income exerts a positive influence on the educational attainment of all children. They further find that parents do not differentiate between their biological and adopted children. However, it is not a priori clear whether their income results are due to differences in genetics or differences in upbringing.[3] The procedure we develop in this paper separately identifies the effect of family income on the educational attainment of adopted children and the role of potential differences in upbringing.

Another study that uses adoptees is one by Das and Sjogren (2002) who examine the intergenerational mobility of income. Their objective is merely to obtain estimates that are not contaminated by genetic ability transfers that exist between two generations. From a sample of 126 adoptees they find that given the educational level of adoptees the intergenerational link in income between parent and child is largely driven by inherited abilities. Their study suffers from the caveats we previously mentioned, but more importantly, their study is not informative about the relationship between family income and educational attainment which is the focus of our paper.

With similar motives as ours, Björklund and Richardson (2001) analyze 8000 adoptees in Sweden and find that, with respect to the educational attainment of adoptees, parental eduction and income seem no longer statistically relevant. Their results further indicate that parents treat biological and adopted children similarly. In view of the sparse literature, it is certainly useful to have more than one study using comparable methodologies with different data. But our study also complements the work of Björklund and Richardson. We explicitly focus on

[3]This observation does not invalidate their general outcome that about 55 to 60 percent of the parental ability is genetically transmitted. Their study only requires that the relevant part of family income they use is orthogonal to parental IQ.

the role of family income and aim to correct for biases arising from unobserved parenting qualities that adoptive parents might have. In their analysis family income merely enters as one additional explanatory variable.

An alternative but related motivation for studying adoptees is to learn more about the role of blended family structures on the educational attainment of children. With alternative parent/child combinations including biological, step, foster and adoptive relationships, Case, Lin and McLanahan (2001) analyze whether the absence of a child's birth parent puts the child's educational success at risk. They find that when children in two parent households are raised by at least one parent who is not their own birth parent (especially mothers) they do less well in school. Because adopted, step, and foster children do equally worse, they conclude that parental investments are child specific and consistent with the evolutionary idea that parents want to protect their own genetic material.[4] As they themselves already point out, their conclusions are a bit flawed because they have no information on family income and therefore cannot rule out that their results are not selection driven. That is, observed differences in educational outcomes are generated by differences in unobserved characteristics among the birth, step, foster, and adopted children. In another study, Case, Fin and McLanahan (2000) provide some indirect evidence showing that parents spend significantly less money on food in the presence of adopted, step or foster children. However, lower spendings on food does not necessarily imply that parents devote relatively less of their income to the education of their adopted, step and foster children. With information on family income available we are able to shed some light on the question whether adopted children actually receive less educational funding. And with information on the educational attainment of other children raised in families with adopted children available, we obtain a better insight on the different motives parents may have in treating their children differently.

3 Data

This paper employs the Wisconsin Longitudinal Survey which is a unique U.S. data set with information on people who were born around 1939.[5] The collection of these data started in 1957 with a questionnaire administered to the complete cohort of students who graduated from a high school in Wisconsin in that year. The information in that first wave relates to the students' social

[4]This is also what Biblarz and Raferty (1999) conclude. Using data from four different samples they find that it is rather the absence of the birth mother than the absence of the father that is detrimental to the children's educational attainment. They further show that their findings are most consistent with an evolutionary view of parental investment.

[5]See also Plug and Vijverberg (2000). Support for collection and dissemination of data from the Wisconsin Longitudinal Study has been provided by the National Institute on Aging (AG-9775), the National Science Foundation (SBR-9320660), the Spencer Foundation, and the Center for Demography and Ecology and the Vilas Estate Trust at the University of Wisconsin-Madison.

background (parents' education and occupation, numbers of older and younger sibling), intelligence (measured as a standardized IQ test score), and aspirations. Subsequently, research was continued on a randomly selected one third of the original cohort. In 1964 and 1975, the respondents was approached again to obtain information about, among others, their schooling and labor market careers. In 1992, the same sample of persons was contacted once more in order to collect new information about their labor market experiences between their late 30s and early 50's. As well, this latest round contained questions about many facets of life events and attitudes. For more information on the WLS data, see, among others, Sewell and Hauser (1992) and Hauser et al. (1996).

Of particular interest for the present study, a set of questions targeted the educational attainment of the respondents' children. Respondents were asked to list for each child the highest grade or year of regular school that child ever attended, whether (s)he completed this grade or year, and whether (s)he attended a regular school in the last 12 months. From the information on educational attainment we create the variable "years of schooling." For those children who completed the highest level attended, "years of schooling" equals the number of years nominally required for that. Children who were still in school constitute censored observations and will be treated accordingly in our empirical analysis; this is the case for about 40 percent of our adoption sample. Note that deleting these observations from the analysis would cause the results to be biased. This holds true especially for the age variable because in that case only low achieving young children would be included in the sample. As the respondents in the sample often have more than one child, we construct sibling information variables for each child. Finally, we use information on the relationship of the child to the respondent to distinguish adopted children from children living with their biological parents.

Three other explanatory variables are common to all children of a family. First, parental ability is the Henman-Nelson IQ score measured during their junior year in high school, that is, in 1956. Note that the respondent is one of the children's parents. Second, parental education is measured as years of schooling of both parents. Third, the income variable is family income, measured both in 1975 and in 1992.

The number of original observations in 1957 equals 10317, but we work with a subsample of 4884 families with 15531 children of whom 590 were adopted. Non-response is a threat to the validity of any study. In our case, using 4884 of the original 10317 respondents gives the appearance that non-response is serious, but in fact it is merely a consequence of simple data requirements. Of the 5433 respondents who fell outside our sample, about 570 had died by 1992, around 300 could not be located, and some 900 did not cooperate with the 1992 survey. Given that 35 years had elapsed since the initial round in 1957, this response rate is in fact very high. In this paper we do not want to get involved in complications that arise if children are brought up in incomplete families. This eliminates roughly 1800 respondents who did not have a partner

and/or children. Finally, the relevant variables must have been measured. In this regard, the main problems exist with the income values: about 1500 families had missing income values in 1975 and 1992 and for about 100 families income was an unrealistically small amount (families with less than $100 per month in either 1975 or 1992). At this point we have 5383 families. Then about 500 families dropped out because their children were too young, because their children were neither adopted nor the biological offspring of both parents, and because information on their children's educational attainment was lacking. Our empirical strategy requires further that we use a sample of adopting families, which will be a subsample of the full sample of 15531 children. The adoption sample comprises 590 children. Summary statistics on adoptees and own birth children appear in Table 1.

4 Estimation results for adoptees

Focusing on the question how family income truly affects the educational attainment of children, we look at children that are genetically unrelated to the family they are raised in and estimate the usual relationship between family income and the educational outcomes on a sample of adopted children. We already discussed that when better parents make more money, income effects may suffer from ability bias. To tackle this bias, we will use additional ability measures, periodic measurement of family income and low income families. Results are reported in Tables 2 and 3.

4.1 Additional ability measures

Table 2 reports estimates of the relation between family income (and other family and individual characteristics) and the educational achievement of children using the WLS sample that consists of adopted children. The structure of the table is as follows. All columns present estimates of censored regressions of years of education on individual and family characteristics. The family level variables are log family income measured in 1975 and 1992, years of education of both parents, parental IQ test scores (of either mother or father, measured when teenager) and the number of siblings in the family. The individual control variables are the child's sex and age. In the first column we report regressions of years of education on only family income and the individual characteristics. In the second column we bring the number of children into the schooling equation to see whether it captures a portion of an indirect income effect. In columns three and four we add parental quality controls, which are parental IQ and years of schooling of both parents, respectively. The estimates reported in panel A are based on income measures in 1975 when most adopted children just started their primary education and parents are in their mid thirties. Panel B uses income measured in 1992 when parents are in their early fifties and most adoptees

had already left school (about 60 percent). In Panel C we include both income measures simultaneously to see whether it is income in 1975 or income in 1992 that is most important. The sample used for our analysis consists of all adopted children in the WLS dataset, which therefore means that some of these children are raised in the same family. The computation of the standard errors takes the family correlations in the error term into account.

With only the child's age and gender as individual controls, the first column of panel A in Table 2 reports a strong positive parental income effect. In the second column the number of siblings shows up significantly negative, but income estimates hardly change. In the third column we include parental IQ to control for unobserved parenting qualities directly. The impact of parental IQ is negligible and that income effects remain the same. The fourth column adds parental education as additional parenting quality controls. Income effects fall but remain significantly positive. We also find that the father's years of the education turns up significantly positive but that the impact of the years of education of the mother is not there.[6] Results in panels B and C are very similar to results in panel A.

What can we conclude from this? First, all income estimates suggest that parental income has a beneficial impact on the educational attainment of adopted and thus genetically unrelated children. Second, all of the estimated sibling effects are strongly negative. This result by itself suggests that family income matters: an increase in the size of the family creates financial obligations from which parents cannot escape. But apparently, parents do spend a little less on each child – which they would not do when credit markets were perfect and parents were able to invest in their children's education merely on the basis of future earning gains. Third, the absence of significant impacts of both parental IQ and years of education of the mother, both believed to be closely related to (unobserved) parenting quality, suggests that it is family income itself that is generating the environment in which adopted children do better in school. Thus, a careful consideration of the meaning of each estimate leads to the conclusion that our estimates are consistent with the idea that there is a causal relationship between family income and the educational attainment of adopted children.

4.2 Periodic measurement

We also get mileage from exploiting the periodic measurement of the income variable that is reported in the WLS data. First, consider the impact of family income measured in 1975. At this time the respondent is about 34 to 35 years

[6]Since all family background variables are correlated, the drop of the IQ estimate might be a typical case of multicollinearity. However, in this case we find no effect if we look at the impact of IQ and education of the mother together. Tests for joint significance are rejected. In specifications without parental IQ we find that the impact of the mother's years of education is even smaller and not statistically significant.

old and, on average, his or her children will be in primary school. 1975 family income may have three effects on the amount of schooling that children eventually complete: (i) according to the lifecycle theory of consumption, schooling later on is paid for by savings from income received earlier; (ii) according to the permanent income hypothesis, variations in 1975 income are indicative of, though imperfectly correlated with, permanent income on which parents base their consumption; (iii) according to the theory of household production, early income creates a family environment that is conducive to the child's success in school, which in turn invites further schooling investment when the child has become a young adult. In any case, panel A reports a positive parental income effect.

Second, consider the impact of family income measured in 1992. At this stage of the parental lifecycle, more than half of all children have just ended their schooling career, and college expenses may still be taking a big bite out of the parents' budget. Again, one may offer a permanent income and a lifecycle theory argument. If parents anticipate on their future income (which is closely related to permanent income) while funding their children's education, 1992 income will still be important when the children have finished school. In panel B where we use family income of 1992, we find that our estimated income effects are not substantially different from the 1975 estimates in panel A.

If we accept these outcomes at face value, these results suggest that parental income is important, whether it is obtained when students are in their in early childhood, or when they already left school. However, if income measured in either 1975 or 1992 has an impact on educational attainment before schooling is commenced or after schooling is completed,[7] one would think that it is rather the parents' permanent income than the momentary parental income that generates a beneficial impact on the educational attainment of children. But there still is a problem in this reasoning: momentary parental income measures are window variables that fail to capture information relevant over other parts of the child's life. As Wolfe, Haveman, Ginther and An (1996) argue, one-year window measurements of income serve as weak proxies for the long-term financial circumstances of parents and may result in unreliable estimates. To deal with this window issue in part, both income measures are included simultaneously in Panel C. Compared to the first two panels, both estimates fall but remain significantly positive, with almost identical impacts of both 1975 and 1992 income. Moreover, the sum of the two estimates is larger than each of the separate ones, implying that if both rise by, say, 10 percent children's schooling increases by more than if one rises by 10 percent and the other is not controlled for. These findings are therefore consistent with the notion that momentary family income is indeed an imperfect measure of permanent income and that permanent income impacts children's educational attainment. At the same time, there is support

[7]For example, if parents borrow money to finance their offspring's education, future income affects current expenditures. And vice versa, if parents save income to finance their offspring's education, past income affects current educational spending.

for the lifecycle theory of consumption: the estimates show that family income during early childhood has a positive effect on the educational attainment of adopted children, and, controlling for childhood income, empty-nest income has an additional positive effect on children's schooling. Yet some caution is warranted. The final column reports income effects that are only jointly significant at a 10 percent level.

In all, the difference between the lifecycle theory and the permanent income hypothesis is subtle. If the sum of the income parameter estimates in panel C would have been roughly equal to those in panels A or B, the permanent income hypothesis would have become more plausible. If each of the income parameters in panel C would have been roughly equal to those in panels A and B, the lifecycle theory would have received more support. As it is, the estimates fall right in the middle of these alternatives. For the purpose of this paper, there is no harm: income matters.

4.3 Low income families

So far we have pooled all adoptees. Thus, in effect, we implicitly assumed that family income has the same effect for adopted children raised in both poor and rich families and we estimated the average family income effect. However, if credit markets are imperfect, family income should matter more for children in poor families. Let us therefore now restrict our attention to families at the bottom of the family income distribution.

Defining the distribution of income on the basis of the full WLS database, we create three subsamples of low income adoption families by placing thresholds at the 30th, 40th and 50th percentile of the income distribution. For each subsample, Table 3 reports the income parameters of the same series of specifications as above, omitting other parameters for reasons of brevity. In panel A parameter estimates are all smaller than the baseline results and are all statistically insignificant: income in 1975 does not matter for the eventual educational attainment of adoptees. The results in panel B, however, suggest that 1992 income does matter for low income families. Although the subsamples are very small, all 1992 estimates are statistically significant, and are all larger than the corresponding estimates for children in all families reported in Table 2.

There is a straightforward explanation why income effects in 1992 are more substantial than income effects in 1975: when they are young, poor parents are less able to smooth their consumption profile. Suppose parents are forward-looking and optimize over a long horizon, and suppose also that earnings profiles rise toward middle age and decline in old age. Under such conditions, young parents want to consume more than their resources allow for, especially when they live under poor conditions. But since they cannot use their future earnings as a collateral to borrow, they will, as a matter of optimizing lifecycle behavior, consume all of their (momentary) income when they are young. If this is the case, variations in income early in the lifecycle cannot impact consumption later

in life. On the other hand, middle age income will become more important for the post-secondary education of children: when the credit constraint lifts as income and family assets rise, the time horizon is shorter and consumption is more strongly impacted by momentary income variations.[8]

Furthermore, consider that in 1975, when parents were roughly 36 years of age, almost all children were in compulsory education. At the beginning of the school career education is compulsory, educational costs are low, and family income can have at most a muted effect on the educational outcomes of their children. In 1992, on the other hand, children who are still in school (about 40 percent) are mostly in college or university and undergo schooling voluntarily. Because at this stage schooling is most costly, students coming from poor families are the most sensitive to failing capital markets.[9]

5 Extrapolation towards own birth children

We have produced income estimates that are genetically unbiased and indicate quite convincingly that income matters for the schooling outcomes of adoptees. It is tempting to conclude that therefore family income matters for other children as well. However, we believe that extrapolation is only meaningful if we first explore to what extent our findings are subject to contradictory interpretations due to underinvestment arguments and selectivity bias.

5.1 Treatment differentials

Our estimated income effects may be extrapolated under the condition that parents do not differentiate between their biological and adopted children. However, if there are differences in upbringing and these differences are captured by the income coefficient, such an extrapolation is not correct. When parents treat their adopted children differently, there are only two possible options. They either invest less in their adopted children and income effects will be too small, or they invest more and estimated income effects will be too large. Case, Lin and McLanahan (2000, 2001) argue that parents will probably invest more in their own birth children children because of some biological imperative.

To find out whether there are different allocation rules among adopted and biological children one might wish to compare the educational outcomes of adopted and biological children, but then one must deal with the complication

[8] Note that this line of reasoning also allows one to argue that, in the presence of an inverted-U shaped income profile, credit constraints placed on young low-income parents actually enhance their children's educational attainment by preventing these parents from consuming their middle age income while young. This leaves more financial resources for the time when educational expenditures are encountered.

[9] Also for students who already left school, family income might still be of importance. Most children have just ended their schooling career, and parents might still be paying for their children's college expenses.

that differences between parental income effects relevant for educational outcomes of biological and adopted children are tainted by genetic transfers and thus do not necessarily identify treatment differentials.[10] Instead we compare the educational outcomes of adoptees who have been raised with and without siblings who are their parents' own offspring. The idea is that when parents feel the biological urge to more heavily invest in their own birth children, relative differences between the parental income estimates relevant for educational outcomes of adoptees should be observed.

Results are presented in Table 4. With the fullest specification presented in Table 2 as the baseline model, the first column of Table 4 adds the variable "having own birth siblings" to see whether adoptees raised by parents who also raise children of their own birth attain more or less schooling. There proves to be virtually no difference: adopted children with and without own birth siblings do equally well in school. In the second column, we allow parents to allocate different amounts of money to the education of their adopted child when they have children of their own birth: we let the income parameters differ and include an "having own birth siblings×log family income" interaction effect. Whether we use income measured in 1975 or in 1992, the interaction terms are statistically insignificant. These results suggest that there is no evidence that parents treat their adopted children differently from own birth children.

5.2 Problems in using adoption as a natural experiment

Samples of adopted children produce unbiased estimates that may be extrapolated to own birth children if children are randomly given up for adoption and if the same children are then randomly assigned to the new family of rearing. In practice, these randomization requirements are rarely met and create various types of ability and selection bias that may muddle our income estimates. We discuss three potential sources of bias.[11]

The first potential source of bias arises when we consider adoptees. Table 1 shows that with respect to educational attainment adopted children are at a slight disadvantage.[12] Medoff (1993) argues that children who are given up for adoption are more likely to come from poor and low ability families. If genes

[10]For own birth children income effects capture genetic transfers from parents. For adoptees these transfers do not exist, at least, not in the family of rearing. In Plug and Vijverberg (2000) we compared the educational outcomes of adoptees and own birth children but used income shocks purged of parental ability to test for treatment differentials. We found no clear evidence for differences in upbringing. The disadvantage of this approach is that this estimation procedure only isolates that component of income that is unrelated with family genes under a strict set of assumptions. In this paper we relax these assumptions and introduce an alternative approach to identify treatment differentials.

[11]For a related discussion, see also Plug and Vijverberg (2000) who focus on a nature/nurture decomposition of parental IQ effects.

[12]In an attempt to measure completed schooling only we also compare the means of years of education for children aged 23 or older. Differences in years of education reduces but compared to adoptees, own birth children stay in school somewhat longer (13.569 versus 14.045).

matter this means that adopted children probably are less endowed. With a similar prediction Becker (1991) puts forward another mechanism: if parents could choose, they probably would put their inferior children up for adoption rather than their superior children. In our educational attainment model, this relative but unobserved ability deficit of adoptees is swept into the intercept unless parents treat their children differently in response to differences in their children's individual ability.[13] According to the results reported in the previous section there is no reason to believe that parents apply different allocation rules.

The second potential source of bias arises when we consider adoptive parents. Summary statistics in Table 1 already illustrate that adoptive parents are not randomly drawn from the population at large but are on average better educated, have higher IQs, and make more money. The important question is what happens if adoptive parents tend to be better parents as well. The situation looks to be parallel with the first. The average effect of the superior parenting skills among adoptive parents is captured in the intercept unless parenting skills are correlated with earnings, in which case the income parameter estimate is biased.

There is little in this regard that may be inferred from the sample of adopted children, but we are actually able to say something about the unobserved parenting skills of adoptive parents if we exploit the potential differences between own birth children who live with and without adopted siblings. If adoptive parents tend to be better parents, own birth children raised with adopted children should do better in school. Thus, Table 5 displays estimates of the same schooling model as before (Table 2), now applied to own birth children and adding the dummy variable "having adopted siblings." The coefficient on this dummy is negative and statistically insignificant, except for in the last column where parental education is added as an explanatory factor, in which case adoptive parents do significantly worse in raising their own birth children. Together, these findings are difficult to reconcile with the idea that adoptive parents are truly better and alleviate the fear that unobserved parenting skills might have biased the income parameters in the adoptee schooling models.

Table 5 also reveals the estimated income coefficients for own birth children. It is quite surprising that these estimates are statistically identical to the income coefficients for adoptees in Table 2. One might have expected the impact of family income on the educational achievement to be stronger for children raised by their own biological parents, since in a biological child schooling model income captures genetic transfers from parents. For adopted children, however, these

[13]The existing empirical literature thus far has not been conclusive on family allocation rules. Some studies suggest that parents choose to invest more in their less talented children to compensate for their ability deficit (Ashenfelter and Rouse, 1998; Ermish and Francesconi, 2000) while other studies find that parents overinvest in more talented children and reinforce talent gaps (Behrman, Pollak and Taubman, 1982; Behrman, Rosenzweig and Taubman, 1994; Miller, Mulvey and Martin, 1995). The problem with these studies is that they all rely on differences between fraternal twins, identical twins and other biological siblings and that their results are difficult to extrapolate to the case of adoptees.

genetic transfers do not exist within the family of rearing.[14] Is there an explanation for this? Is there something in the process of adoption that might compensate for the lack of genetic transfer between biological and adopted children which would lead to equal income effect estimates? This would happen if parents would overinvest in their adopted children or if adoptive parents would indeed be better parents. However, our findings so far show clearly that these two mechanisms are essentially not there and may therefore be discarded.

This brings us to the third and probably the most serious source of bias. The important question is how adoptees are selected into adoptive households.[15] If high ability parents manage to adopt children from high ability natural parents, or if adoption agencies use corresponding qualities of both natural and adoptive parents as a matching strategy, the underlying selection process would result in family income coefficients that tend to be too high, even with unrelated adoptions. To test how serious this selection effect really is, we would need to know the family background of the natural parents of adopted children, and information on the timing of the adoption. Of course, the WLS does not provide this information. Hence, it is impossible to find a remedy to remove this bias.

5.3 Adoptees and blended family structures

Our results also prove to be insightful about the role of blended family structures on the educational attainment of children. That is, if the Cinderella motive put forward by Case, Lin and McLanahan (2000, 2001) would be applicable to adoptees, one should expect that (*i*) adoptees would do worse in school if they are raised together with own birth siblings, and (*ii*) own birth children would do better in school in the presence of adopted siblings. Our observations show that own birth children end up with lesser schooling in the presence of adopted

[14]We do observe this absence of genetic transmissions in the adoptee schooling model for years of education of the mother and for parental IQ, variables which are obviously tainted by unobserved parental abilities. In a recent twin study of Behrman and Rosenzweig (2002) it is found that once you control for heritable ability and assortative mating the positive influence of the mother's schooling disappears. In this paper we produce a parallel finding using adoptees instead.

[15]There is the possibility of related adoptions. These adoptions involve primarily parents who raise and adopt their relatives' children, or parents who raise and adopt their partner's children after a remarriage. Inclusion of related adoptees in the adoptee sample will lead to an overstatement of the role of family income due to selection. However, we do not think that related adoptions form a serious threat. The WLS provides information on parent child relationships and uses nine different classifications: biological, adopted, step, foster, grandchild, legal ward, niece/nephew, other non-relatives, and child of partner/lover. If we use this classification and treat all alternative parent/child pairs as related adoptions, we end up with 1085 children. Of all adoptions in our sample, this amounts to more than 60 percent which is very close to the percentages found by Stolley (1993), who reports that of all adoptions in the early 1070s more than 50 percent were related adoptions and that this percentage rose to almost 75 percent in the early 1980s. In our analysis we excluded all these alternative family related relationships and proceeded under the plausible assumption that all of our remaining 590 adoptees are unrelated to the family of rearing.

siblings which is inconsistent with the idea that parents treat their own offspring better because of some biological urge.

Given that the Cinderella motive no longer seems applicable, we borrow an alternative explanation from Ginther and Pollak (2000) that is indeed consistent with our findings.[16] The presence of adopted children may have a disruptive effect on family life and that it is perhaps the stress and strain of raising an adopted child that has a detrimental influence on parenting quality and thus also on the educational attainment of siblings who are the biological offspring of both parents. Note further that we only observe statistically significant effects when years of education of both parents are included as explanatory variables (Table 5, column 4). The estimates imply that the negative impact of having adopted siblings is in absolute terms about as large as the positive influence of 1.2 years of schooling of either father or mother.

It might be that these results are driven by selection. Think of the quality of a child as a random draw from a distribution, the location of which is determined by the genetic endowments of the parents. However, parents have imperfect information about the location or spread of this distribution. The firstborn child provides revealing information to the parents, and it is perhaps the case that parents who face a bad draw decide they would rather adopt than have another child on their own. If so, this would introduce a negative correlation between unobserved child endowments and the dummy variable of having adopted siblings among a portion of the biological child sample. We examined this idea by excluding all firstborn biological children from the sample and reestimating the models in Table 5. The parameter estimates were very similar but, as the sample size was a third smaller, none of the parameter estimate on the dummy was statistically significant any longer.

6 Discussion and concluding remarks

This paper measures the effect of family income on schooling. Typically, estimated income effects are potentially biased by the fact that high ability parents not only generate more income but produce high ability children as well. To circumvent this problem we examine, as a natural experiment, those adopted children who are genetically unrelated to the family of rearing. We regress the years of schooling of adopted children on the characteristics of adopting families and find that a better access to financial resources improves the educational achievement of adoptees.

Our estimates are genetically unbiased, but this does not necessarily prove that family income has a causal effect on schooling. There are two problems that must be solved first.

[16] Ginther and Pollak look at stepchildren, and while the argument applies quite well, it does not necessarily hold for adoptees. The difference between adoptees and stepchildren is that stepchildren are raised by one of their own birth parents and adopted children are not.

The first problem is that parents influence their kids through other things than genetics. If high ability parents make more money and are also better parents, research on adoptees does not help. To tackle this bias we use additional ability measures, periodic measurement of family income, and low income families to distill the family income effect. In isolation each test demonstrates that family income is a significant factor. Taken together, the results strongly suggest that there is a causal relation between family income and school success for adoptees.

The second problem is that adoption does not have the nice randomization characteristics of typical laboratory experiments. We contemplate how adoption might be associated with differences between adoptive and natural parents, between adopted and own birth children, and between the way these children are brought up, but we find none of these factors to be relevant for the educational attainment model. Thus, our causal interpretation of the estimate of family income remains unaffected. Only in the case that high ability parents manage to adopt children from high ability natural parents is the effect of family income overstated – but information on this might only be available from the most specialized kind of data source. This means that we can take our adoption results seriously as long as we assume that most adoptive parents do not know who the biological parents of their adopted children were.

We also estimate the effect of parental income for own birth children and find the income estimates for adopted and own birth children to be statistically identical. The parallel findings in Sacerdote (2002) give us a compelling reason to believe that we do have evidence of a causal effect, but again the case for such a conclusion must be bolstered with future confirming evidence.

To sum up, our conclusion motivates the design of educational policies that benefit the poor. Provided that one has a reliable method to measure children's ability, society benefits from alleviating the financial constraints that keep able but low-income students from seeking a more advanced education. Such short-run assistance will also have long-term benefits: these program beneficiaries will not only earn higher incomes but also, given the intergenerational genetic transfer of ability, tend to have higher-ability children, who, because of their parents' income, will be able pursue their desired level of education on their own. It should be emphasized, though, that a general removal of financial constraints cannot be expected to yield equal educational outcomes for all children because more than half of the intergenerational transfer of ability is genetically determined (Plug and Vijverberg, 2000). It is the smaller half, not genetically determined, that we are concerned with in this paper.

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Table 1:
Descriptive statistics for adoptees and own birth children

	adoptees		**own birth children**	
children				
years of education	12.672	*2.708*	13.557	*2.562*
still in school (censored)	0.389	*0.488*	0.241	*0.427*
gender (daughter)	0.486	*0.500*	0.490	*0.499*
age	23.915	*5.316*	26.593	*5.171*
family				
families with both adopted and own birth children	0.559	*0.496*	0.032	*0.177*
number of siblings	2.198	*1.699*	2.841	*1.676*
log family income 1975	9.776	*0.447*	9.679	*0.505*
log family income 1992	11.127	*0.604*	10.957	*0.694*
IQ of parent (divided by 10)	10.418	*1.454*	10.119	*1.403*
education of father in years	14.279	*2.808*	13.482	*2.612*
education of mother in years	13.279	*1.948*	12.792	*1.669*
number of observations	590		14941	

Standard deviations in italics

Table 2:
Schooling of adopted children: baseline specifications

A: Using family income measured in 1975

intercept	5.777	*2.502***	6.937	*2.599****	6.772	*2.590****	7.174	*2.284****
daughter	0.152	*0.202*	0.146	*0.201*	0.145	*0.201*	0.103	*0.191*
age	-0.082	*0.023****	-0.071	*0.024****	-0.068	*0.024****	-0.044	*0.022***
number of siblings			-0.178	*0.065****	-0.187	*0.065****	-0.151	*0.061***
log income 1975	1.042	*0.256****	0.934	*0.265****	0.874	*0.279****	0.514	*0.249***
IQ of parent					0.066	*0.074*	-0.141	*0.073**
years of education father							0.249	*0.043****
years of education mother							0.080	*0.075*
Mean loglikelihood	-1.529		-1.523		-1.522		-1.484	
Sample size N	590		590		590		590	

B: Using family income measured in 1992

intercept	5.951	*2.502***	6.520	*2.237****	6.357	*2.254****	7.886	*2.175****
daughter	0.198	*0.201*	0.191	*0.199*	0.191	*0.199*	0.121	*0.192*
age	-0.064	*0.023****	-0.052	*0.023***	-0.051	*0.023***	-0.036	*0.021**
number of siblings			-0.201	*0.064****	-0.207	*0.064****	-0.168	*0.060****
log income 1992	0.857	*0.256****	0.818	*0.265****	0.783	*0.196****	0.377	*0.249**
IQ of parent					0.051	*0.073*	-0.133	*0.073**
years of education father							0.238	*0.045****
years of education mother							0.081	*0.073*
Mean loglikelihood	-1.526		-1.518		-1.518		-1.484	
Sample size N	590		590		590		590	

C: Using family income measured in 1975 and 1992

intercept	2.448	*2.642*	3.587	*2.728*	3.591	*2.730*	5.810	*2.559***
daughter	0.208	*0.201*	0.200	*0.200*	0.200	*0.200*	0.129	*0.197*
age	-0.070	*0.023****	-0.058	*0.024****	-0.057	*0.024***	-0.041	*0.022**
number of siblings			-0.180	*0.065****	-0.184	*0.066****	-0.152	*0.062***
log income 1975	0.642	*0.287***	0.526	*0.297**	0.508	*0.305**	0.382	*0.269*
log income 1992	0.620	*0.221****	0.628	*0.217****	0.614	*0.218****	0.258	*0.214*
IQ of parent					0.030	*0.074*	-0.146	*0.073***
years of education father							0.237	*0.041****
years of education mother							0.074	*0.071*
Mean loglikelihood	-1.521		-1.515		-1.515		-1.492	
Sample size N	590		590		590		590	

Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level. Each censored regression model uses years of education as the dependent variable. In the fourth column of Panel C where income measured in 1975 and 1992 are included simultaneously, the parameter estimates are individually statistically insignificant but jointly significant at a 10 percent level.

Table 3
Schooling of adopted children: family income coefficients in low income families

A: Using family income measured in 1975

baseline results	1.042	*0.256****	0.934	*0.265****	0.874	*0.279****	0.514	*0.249***
low income families in 1975								
lowest 30% (N=142)	0.817	*0.521*	0.612	*0.578*	0.491	*0.601*	0.786	*0.498*
lowest 40% (N=218)	0.700	*0.474*	0.515	*0.536*	0.250	*0.557*	0.228	*0.511*
lowest 50% (N=268)	0.542	*0.447*	0.375	*0.497*	0.201	*0.513*	0.190	*0.475*
controls								
gender, age	yes		yes		yes		yes	
number of siblings	no		yes		yes		yes	
parental IQ	no		no		yes		yes	
parental education	no		no		no		yes	

B: Using family income measured in 1992

baseline results	0.857	*0.256****	0.818	*0.265****	0.783	*0.196****	0.377	*0.249**
low income families in 1992								
lowest 30% (N=125)	1.363	*0.510****	1.363	*0.510****	1.368	*0.507****	1.427	*0.502****
lowest 40% (N=185)	0.963	*0.458***	0.976	*0.453***	0.970	*0.449***	1.002	*0.437***
lowest 50% (N=237)	0.886	*0.372***	0.911	*0.366***	0.887	*0.369***	0.851	*0.369***
controls								
gender, age	yes		yes		yes		yes	
number of siblings	no		yes		yes		yes	
parental IQ	no		no		yes		yes	
parental education	no		no		no		yes	

Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level. Each censored regression model uses years of education as the dependent variable.

Table 4:
Schooling of adopted children: examining allocation rules

A: Using family income measured in 1975

intercept	7.183	*2.264****	7.328	*2.264****
daughter	0.103	*0.191*	0.103	*0.191*
age	-0.044	*0.022***	-0.044	*0.022***
number of siblings	-0.148	*0.065***	-0.148	*0.065***
log income 1975	0.514	*0.249***	0.499	*0.456*
IQ of parent	-0.142	*0.074**	-0.142	*0.074**
years of education father	0.250	*0.043****	0.250	*0.043****
years of education mother	0.079	*0.074*	0.080	*0.076*
own birth siblings	-0.026	*0.215*	-0.024	*4.889*
own birth siblings×log income 1975			0.022	*0.500*
Mean loglikelihood	-1.484		-1.484	
Sample size N	590		590	

B: Using family income measured in 1992

intercept	7.894	*2.173****	4.695	*2.983*
daughter	0.121	*0.192*	0.122	*0.191*
age	-0.035	*0.021**	-0.035	*0.021*
number of siblings	-0.165	*0.063****	-0.168	*0.062****
log income 1992	0.376	*0.197**	0.672	*0.274***
IQ of parent	-0.134	*0.074**	-0.139	*0.075**
years of education father	0.239	*0.045****	0.236	*0.045****
years of education mother	0.081	*0.073*	0.081	*0.072*
own birth siblings	-0.026	*0.214*	5.153	*3.765*
own birth siblings×log income 1992			-0.466	*0.341*
Mean loglikelihood	-1.484		-1.484	
Sample size N	590		590	

Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level.
Each censored regression model uses years of education as the dependent variable.

Table 5
Schooling of own birth children: baseline specifications

A: Using family income measured in 1975

intercept	7.930	*0.600****	8.888	*0.603****	6.671	*0.568****	5.103	*0.492****
daughter	0.130	*0.040****	0.147	*0.040****	0.137	*0.039****	0.112	*0.037****
age	-0.130	*0.006****	-0.126	*0.005****	-0.117	*0.005****	-0.074	*0.005****
number of siblings			-0.171	*0.016****	-0.168	*0.015****	-0.112	*0.014****
log income 1975	1.025	*0.058****	0.965	*0.058****	0.793	*0.055****	0.351	*0.048****
IQ of parent					0.359	*0.018****	0.155	*0.017****
years of education father							0.251	*0.012****
years of education mother							0.249	*0.017****
having adopted siblings	-0.042	*0.148*	0.014	*0.150*	-0.135	*0.145*	-0.282	*0.133***
Mean loglikelihood	-1.806		-1.799		-1.779		-1.720	
Sample size N	14941		14941		14941		14941	

B: Using family income measured in 1992

intercept	7.923	*0.533****	8.717	*0.537****	7.059	*0.502****	5.249	*0.423****
daughter	0.130	*0.040****	0.147	*0.040****	0.139	*0.039****	0.113	*0.037****
age	-0.122	*0.005****	-0.119	*0.005****	-0.112	*0.005****	-0.072	*0.005****
number of siblings			-0.169	*0.017****	-0.168	*0.016****	-0.113	*0.014****
log income 1992	0.887	*0.044****	0.849	*0.058****	0.687	*0.055****	0.319	*0.037****
IQ of parent					0.323	*0.018****	0.143	*0.017****
years of education father							0.244	*0.012****
years of education mother							0.245	*0.017****
having adopted siblings	-0.102	*0.150*	-0.045	*0.151*	-0.168	*0.147*	-0.295	*0.134***
Mean loglikelihood	-1.796		-1.790		-1.773		-1.718	
Sample size N	14941		14941		14941		14941	

C: Using family income measured in 1975 and 1992

intercept	3.712	*0.594****	4.724	*0.599****	3.644	*0.577****	3.700	*0.521****
daughter	0.130	*0.039****	0.146	*0.039****	0.138	*0.039****	0.113	*0.037****
age	-0.122	*0.005****	-0.119	*0.005****	-0.113	*0.005****	-0.073	*0.005****
number of siblings			-0.156	*0.016****	-0.157	*0.015****	-0.109	*0.014****
log income 1975	0.640	*0.059****	0.598	*0.059****	0.523	*0.056****	0.243	*0.049****
log income 1992	0.704	*0.046****	0.681	*0.046****	0.547	*0.045****	0.263	*0.038****
IQ of parent					0.308	*0.018****	0.140	*0.017****
years of education father							0.237	*0.012****
years of education mother							0.240	*0.017****
having adopted siblings	-0.094	*0.148*	-0.041	*0.147*	-0.159	*0.143*	-0.287	*0.132***
Mean loglikelihood	-1.789		-1.783		-1.768		-1.717	
Sample size N	14941		14941		14941		14941	

Robust standard errors are in italics; * significant at 10% level,** significant at 5% level, *** significant at 1% level. Each censored regression model uses years of education as the dependent variable.