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DOES FAMILY INCOME MATTER FOR SCHOOLING OUTCOMES? USING ADOPTEES AS A NATURAL EXPERIMENT*

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One would expect that family income is an important positive factor in children's school attainment. However, evidence 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 correct for biases arising from unobserved parenting qualities and from parents' differentiation between their own birth and adopted children. Family income still has a significant effect. It implies that high ability children in low income families face binding credit constraints that society may wish to relieve.

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; Duncan and Brooks-Gunn, 1997). 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 have become 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 relevant 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

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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 programmes 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 article we aim to mimic this experiment. The idea is fairly simple. If adopted children are children that are not genetically descended from the family that rears them, 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 randomisation 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 article unfolds as follows. Section 1 provides a brief background on adoptees in the economics literature. Section 2 describes our data. Section 3 presents and discusses the empirical findings, which pertain to both adoptee and own birth children. Section 4 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 5 summarises our conclusions.

1. Adoptees and Schooling Investments

In economics the idea of using 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

[1] According to evidence in Case *et al.* (2000, 2001), parents treat their own children better.

available that attempt to document (indirectly) the association between parent's income and the educational attainment of adopted children. We will review 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 (2000) 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 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 article we explore what happens to our income estimates when we deal with these potential biases directly.

In a similar fashion, Plug and Vijverberg (2003), we analyse a much larger sample of adoptees (see also Section 3). Our primary aim was 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 entered our analysis merely to see whether the IQ transmission mechanism was robust. We estimated that family income effects are positive and identical for adopted and biological children from which we concluded that parental income exerts a positive influence on the educational attainment of all children. We further found that parents do not differentiate between their biological and adopted children. However, it is not *a priori* clear whether our income results are due to differences in genetics or differences in upbringing.[3] The procedure we develop in this article 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 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

[2] Sacerdote (2000, 2002) 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. But Sacerdote uses 170 adoptees who come from the National Longitudinal Survey of Youth to examine the relationship between family income and educational attainment.

[3] This observation does not invalidate the general outcome that about 55 to 60% of the parental ability is genetically transmitted. Our study only required that the relevant part of family income used was orthogonal to parental IQ.

relationship between family income and educational attainment which is the focus of our article.

With similar motives to ours, Björklund and Richardson (2001) analyse 8,000 foreign adoptees in Sweden and find that parental education and income no longer seem statistically relevant to the educational attainment of adoptees. 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 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 *et al.* (2001) analyse 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 not do as 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 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 *et al.* (2000) provide some indirect evidence showing that parents raising adopted, step or foster children spend significantly less money on food. However, lower spending 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.

2. Data

This article employs the Wisconsin Longitudinal Survey, which is a unique data set with information on people who were born around 1939. The collection of these data started in 1957 with a questionnaire administered to the complete cohort of

[4] This is also what Biblarz and Raftery, (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.

students who graduated from a high school in Wisconsin in that year. The information in that first wave relates to the students' social background (parents' education and occupation, numbers of older and younger sibling), intelligence (measured as a standardised 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 labour market careers. In 1992, the same sample of persons was contacted once more in order to collect new information about their labour market experiences between their late 30s and early 50s. 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. For each child respondents were asked to list 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 two dependent variables, 'years of schooling' and 'college graduate'. 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; this is the case for about 40% of our adoption sample. Our empirical analysis takes account of this censoring in the standard way.[5] Note that we certainly would not want to delete censored observations from the sample: dropping children who are still in school would leave us with a sample in which young children are by implication all low achievers, and any estimated age effect in particular would be biased. As for our second dependent variable, 'college graduate' is a dichotomous variable indicating whether the child completed 16 or more years of education. When this variable is the dependent variable, the sample is further restricted to those children aged 23 or older, since younger children have not yet had a chance to complete college. As this dependent variable is dichotomous, the college graduate model is estimated with the probit technique.

There are two groups of explanatory variables. The first group is child-specific. Each model includes the age and gender of the child. As the respondents in the sample often have more than one child, we also 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.

[5] Let Y_i^* be the desired level of schooling for child i, modelled as a function of the set of explanatory variables in a usual way as $Y_i^* = \mathbf{X}_i\boldsymbol{\beta} + \epsilon_i$. Let Y_i be the observed amount of schooling that child i has completed at the time of the survey. If the child has completed her education, Y_i^* is equal to Y_i, but if she is still in school and therefore represents a censored observation, Y_i is less than or at most equal to Y_i^* because eventually this student will attain a schooling level past (or at least equal to) her current grade. This implies that the schooling model must be estimated by the Maximum Likelihood method, rather than by Ordinary Least Squares.

The second group consists of five other explanatory variables that are common to all children of a family. First, parental ability is the Henman-Nelson IQ score of the respondent parent measured during his/her junior year in high school, that is, in 1956. Note that the respondent parent is one of the children's parents. Second, parental education is measured as years of schooling of both parents. Third, the models include family income, measured both in 1975 and in 1992. Fourth, grandparents' education refers to the years of schooling of the parents of the respondent parent of the child. Fifth, grandparents' income measures the income of that respondent parent's family in 1957.

The number of original observations in 1957 was 10,317 but we work with a subsample of 4,778 families with 15,126 children, of whom 574 were adopted. Non-response is a threat to the validity of any study. In our case, using 4,778 of the original 10,317 respondents gives the appearance that non-response is serious but in fact it is merely a consequence of simple data requirements. Of the 5,539 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. Next, given the nature of the research problem, we drop 736 respondents who were childless in 1992. Then, since we do not want to get involved in this article in complications that arise if children are brought up in incomplete families, we eliminate 1,683 respondents who did have children but were single in either 1975 or 1992. Finally, the relevant variables must have been measured. In this regard, the main problems exist with the income values: about 872 families had missing income values in 1975 and 1992 and income was an unrealistically small amount for 71 families (families with less than $100 per month in either 1975 or 1992). At this point we have 5,175 families. Then, we exclude all children younger than 6 who could not have started their schooling yet, as well as children who left school before they were legally old enough to do so and children with missing information on their schooling. This dropped 400 families from the database. Our empirical strategy requires further that we use a sample of adopting families, which will be a subsample of the full sample of 15,126 children. The adoption sample comprises 574 children. Summary statistics on adoptees and own birth children appear in Table 1.

3. 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. A more detailed exposition of this bias is relegated to the Appendix. To tackle this bias, we will use additional ability measures, take advantage of the information available on grandparents, exploit the periodic measurement of family income and use spline functions to examine nonlinearities in the income effects. Results are reported in Tables 2 and 3.

Table 1

Means and Standard Deviations for Adoptees and Own-Birth Children

	Adoptees	Own-Birth Children
Child's characteristics		
Years of education	12.675 (2.704)*	13.596 (2.516)
College education†	0.269 (0.444)	0.380 (0.485)
Still in school (censored)	0.383 (0.486)	0.241 (0.428)
Gender (daughter)	0.487 (0.500)	0.490 (0.499)
Age	23.956 (5.251)	26.601 (4.851)
Parent's characteristics		
Parents who raise both adopted and own birth children	0.562 (0.496)	0.032 (0.177)
Number of siblings	2.224 (1.695)	2.824 (1.650)
Age father	54.217 (2.289)	54.639 (2.672)
Age mother	52.486 (1.954)	52.287 (2.930)
Log parental income 1975	9.787 (0.443)	9.693 (0.489)
Log parental income 1992	11.129 (0.605)	10.958 (0.694)
IQ of parent (divided by 10)	10.402 (1.453)	10.121 (1.400)
Education of father in years	14.226 (2.817)	13.496 (2.612)
Education of mother in years	13.275 (1.954)	12.801 (1.674)
Log grandparent income 1957	8.625 (0.770)	8.605 (0.780)
Education of grandfather in years	10.462 (3.154)	10.199 (2.999)
Education of grandmother in years	10.758 (2.915)	10.506 (2.847)
Number of observations	574	14,552

*Standard deviations in parenthesis.
†Mean and standard deviations are calculated for respectively 363 and 11,623 observations. For this variable children younger than 23 with less than 16 years of education are not included.

3.1. *The Effect of Income and Additional Ability Measures*

Table 2 reports a set of baseline models of the determinants of the educational achievement of children using the WLS sample that consists of adopted children. The structure of this Table is as follows. The top half of the Table examines determinants of years of schooling using censored regression models; the bottom half reports the marginal effect of the explanatory variables in probit models where the dependent variable is college graduation. Robust standard errors are computed in order to account for within-family correlation of the disturbance terms.[6]

Apart from a measure of family income, all columns include groups of primary explanatory variables, namely the child's gender, age, and number of siblings. The second and third column add parental quality controls, which are parental IQ and years of schooling of both parents, respectively, and columns four and five extend the model to three generations by including the 1957 income and education of grandparents. Table 2 aims to highlight the effects of family income in the presence of control variables; in the next subsections, Table 3 examines the role of parental income when measured at different points in time and further focusses on nonlinearities in the income effect.

Across the columns of Table 2 we find that the impact of 1975 family income on the educational attainment of adopted children is positive and almost always

[6] 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.

Table 2

Schooling of Adopted Children: Baseline Specifications

	(1)	(2)	(3)	(4)	(5)
Years of schooling					
Log income 1975	1.018[†](0.270)***	0.975 (0.284)***	0.618 (0.255)**	0.619 (0.258)**	0.621 (0.258)**
Intercept	6.289 (2.672)***	6.178 (2.665)***	6.582 (2.316)***	5.558 (2.455)**	5.607 (2.467)**
Daughter	0.169 (0.204)	0.169 (0.204)	0.122 (0.194)	0.126 (0.194)	0.122 (0.193)
Age	−0.079 (0.023)***	−0.077 (0.023)***	−0.051 (0.022)**	−0.050 (0.022)**	−0.048 (0.021)**
Number of siblings	−0.180 (0.065)***	−0.187 (0.065)***	−0.152 (0.062)**	−0.149 (0.062)**	−0.146 (0.061)**
IQ of parent		0.047 (0.075)	−0.152 (0.074)**	−0.158 (0.074)**	−0.153 (0.073)**
Years of education father			0.249 (0.044)***	0.243 (0.044)***	0.244 (0.045)***
Years of education mother			0.067 (0.077)	0.066 (0.077)	0.078 (0.078)
Log income 1957				0.131 (0.121)	0.139 (0.122)
Years of education grandfather					0.001 (0.036)
Years of education grandmother					−0.036 (0.035)
Mean loglikelihood	−1.534	−1.534	−1.496	−1.495	−1.494
Sample size *N*	574	574	574	574	574
College education					
Log income 1975	0.186 (0.059)***	0.173 (0.062)***	0.112 (0.057)*	0.115 (0.056)**	0.113 (0.055)**
Daughter	0.010 (0.045)	0.013 (0.045)	0.009 (0.046)	0.012 (0.045)	0.010 (0.045)
Age	−0.011 (0.007)	−0.010 (0.007)	−0.004 (0.007)	−0.004 (0.007)	−0.004 (0.007)
Number of siblings	−0.038 (0.018)**	−0.041 (0.018)**	−0.036 (0.018)*	−0.036 (0.018)*	−0.036 (0.018)*
IQ of parent		0.015 (0.016)	−0.019 (0.017)	−0.022 (0.017)	−0.022 (0.017)
College education father			0.041 (0.010)***	0.039 (0.010)***	0.038 (0.010)***
College education mother			0.015 (0.014)	0.015 (0.014)	0.016 (0.014)
Log income 1957				0.059 (0.033)*	0.058 (0.035)*
Years of education grandfather					0.002 (0.007)
Years of schooling grandmother					−0.002 (0.008)
Mean loglikelihood	−0.553	−0.552	−0.517	−0.511	−0.511
Sample size *N*	363	363	363	363	363

[†]Robust standard errors are in parenthesis; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

statistically significant.[7] Moreover, the impact of income is nearly the same when IQ is entered, drops substantially when parental education is added, and is unchanged when grandparent variables are included.

What can we learn from these estimates? 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 may indicate that family income matters. Consider that a child's educational achievement is the result of parental time inputs and financial resources. If parental time is lacking, parents may substitute for their own time by purchasing market inputs (e.g., purchasing self-help study aids, hiring tutors, finding good after-school daycare, letting the child attend a private school where he receives more attention, sending the child to a boarding school). If credit markets were perfect and parents were able to invest in their child's education merely on the basis of future earning gains, the lack of parental time brought on by a larger family size should not hinder the child's educational achievement: parents will simply purchase more market inputs. Thus, unless parental time and financial resources are not fully substitutable and parental time is an essential input – i.e., when a given educational achievement level becomes unattainable when parental time drops below a certain minimum level – family size should not influence any child's schooling. But the estimates in Table 2 show that the number of siblings does have a negative impact on each child's educational attainment. It must be the case that

(*i*) parents are not utilising enough substitute (market) inputs to offset the decrease in their own time input and/or
(*ii*) parental time is an essential input. The range of market options make it less plausible that the latter condition holds, at least past the preschool age level.[8]

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. The absence of a significant impact of parental IQ might seem surprising at first glance, but recall that the sample pertains to adopted children. The effect of IQ cannot be genetic and therefore only captures unobserved parental quality and child environment. Father's education matters, but the effect of mother's education is small and

[7] As mentioned, family income is measured in logarithmic form. This specification implies that a proportional increase in income has a constant effect on educational attainment, and thus that the marginal impact of income is declining with income, in line with other research such as Duncan and Brooks-Gunn (1997), and Duncan *et al.* (1998).

[8] Clearly, at young ages, it is nearly indefensible to assume that parental time inputs are not essential. At older ages, supervision is still essential but it is debatable whether the parent must be the supervisor: many families function well under good nannies. One might also argue that adequate market inputs are prohibitively expensive but they are only so relative to current family income: the notion of affordability is irrelevant if credit markets are perfect and marginal benefits in terms of future earnings are adequate. Nevertheless, ultimately, any conclusion that income matters does of course not hinge on this parameter estimate.

statistically insignificant.[9] Again, this might be surprising since the mother's education is commonly believed to have a bigger effect on the child's schooling than that of her husband. However, this finding corresponds with research by Behrman and Rosenzweig (2002) and Plug (2004), who found that, controlling for inherited abilities and assortative mating, the association between mother's (but not father's) and child schooling disappears.[10]

Fourth, the income and education of grandparents are introduced as control variables in order to test notions that they are instrumental in setting the family's culture and attitudes towards educational investments and the ability to finance such investments (Hill and O'Neill, 1994). In our various models, only 1957 income is sometimes significant at the 10% level, and then only when the dependent variable is college graduation. It might be noted that Becker and Tomes (1986) derived an empirical model where grandparents' income enters with a hypothesised sign that is negative, as a control for unobserved parental ability. Our positive signs stand in contrast; however, as elaborated in the Appendix, it is easy to show that

(*i*) the magnitude of the income and education effects of grandparents should be smaller in a sample with adopted children than biological children, and
(*ii*) for both adopted and biological children the effect of grandparents' income may turn positive if grandparents have a direct impact on the schooling of grandchildren (Hill and O'Neill, 1994).

Fifth, all regression models include the child's gender and age. *Ceteris paribus*, the estimates in Table 2 show no statistically significant difference between sons and daughters, even if the daughter effect is positive in every specification of the education model. The age (or cohort) effect indicates that a child that is 10 years younger on average completes a half year more of schooling and is about 4% more likely to complete a college education, though that effect is imprecisely estimated since the age range is quite small.[11]

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

[9] Since all family background variables are correlated, the lack of statistical significance of mother's education and the drop in 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.

[10] Korupp *et al.* (2002) offer the hypothesis that it is the highest education level attained by either parent that sets more definite expectations for the child's educational achievement. Indeed, for the generation of WLS parents, the average mother is less educated than the father (Table 1). Replacing the parental schooling variables by the highest and lowest years attained widens the gap between the two parameters, compared to Table 2, when the dependent variable is years of schooling but not when it is college graduation. This points out that college graduation is a hurdle for which the child needs the support from both parents – and indeed this is already evident in Table 2 as well, where the parameter gap is smaller in the bottom half than in the top half.

[11] For the baseline and the full models (columns 1 and 5), we examined the idea that age enters nonlinearly. Squared age terms received a t-statistic of at most 0.87, and the income effects varied by at most 0.003 from the estimates reported in Table 2. This result carries over to models reported in the next tables. The cohort effect is essentially linear; any nonlinearity is not detectable with the short age span of the children in the sample.

relationship between family income and the educational attainment of adopted children.

3.2. *The Effect of Income Using Periodic Measurements*

We also get mileage from exploiting the periodic measurement of the income variable that is reported in the WLS data. We do so in Table 3. These periodic measures may have their own interpretation. First, consider the impact of family income measured in 1975. At this time the respondent is about 36 years 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 preparation for and success in school, which in turn invites further schooling investment when the child has become a young adult (Duncan and Brooks-Gunn, 1997, 2000; Duncan *et al.*, 1998; Blau, 1999).

In any case, the first row of each half of Table 3 (which copies the income parameters from Table 2) 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. The second row of each half of Table 3, which uses family income of 1992, shows that the estimated income effects are somewhat smaller but not substantially different from the 1975 estimates in the first row.

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 have 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, 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.[12] But there still is a

[12] 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.

Table 3

Schooling of Adopted Children: Family Income Coefficients Using Periodic Income Measures and Spline Regressions

	Years of schooling		College education	
	(1)	(2)	(3)	(4)
Entered separately				
Log income 1975	1.018† (0.270)***	0.621 (0.258)**	0.186 (0.059)***	0.113 (0.055)**
Log income 1992	0.849 (0.192)***	0.445 (0.195)**	0.168 (0.040)***	0.091 (0.044)**
Entered jointly				
Log income 1975	0.606 (0.301)**	0.468 (0.278)*	0.101 (0.069)	0.077 (0.062)
Log income 1992	0.634 (0.219)***	0.306 (0.210)	0.131 (0.047)***	0.068 (0.049)
Likelihood ratio test‡	24.27***	7.84**	18.57***	5.92*
Splines log income 1975				
0–20% ($N = 100, 54$)	0.868 (0.735)	1.002 (0.783)	0.097 (0.197)	0.165 (0.170)
20–40% ($N = 107, 74$)	−0.930 (1.634)	−2.694 (1.591)*	0.313 (0.471)	0.027 (0.486)
40–60% ($N = 103, 68$)	6.272 (2.877)**	6.943 (2.766)**	1.325 (0.585)**	1.528 (0.547)***
60–80% ($N = 113, 67$)	0.830 (2.437)	−1.323 (2.383)	−0.387 (0.059)	−0.790 (0.418)**
80–100% ($N = 151, 100$)	−0.502 (0.802)	−0.510 (0.638)	−0.006 (0.157)	−0.039 (0.140)
Splines log income 1992				
0–20% ($N = 64, 38$)	1.184 (0.586)**	1.340 (0.609)**	0.494 (0.211)**	0.672 (0.299)**
20–40% ($N = 116, 77$)	0.686 (1.403)	0.453 (1.311)	−0.172 (0.417)	−0.340 (0.386)
40–60% ($N = 117, 81$)	−0.642 (1.696)	−1.911 (1.619)	0.103 (0.418)	−0.061 (0.386)
60–80% ($N = 128, 71$)	2.416 (1.234)**	1.353 (1.226)	0.341 (0.258)	0.141 (0.255)
80–100% ($N = 149, 96$)	0.472 (0.581)	0.443 (1.311)	0.127 (0.094)	0.126 (0.098)
Control variables:				
Gender, age, siblings	Yes	Yes	Yes	Yes
Parent's IQ and education, grandparent's income and education	No	Yes	No	Yes

†Robust standard errors are in parenthesis; * significant at 10% level,** significant at 5% level, *** significant at 1% level.

‡Likelihood ratio tests test the joint significance of both income coefficients. Low values indicate statistical insignificance.

problem with 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 *et al.* (1996), Duncan *et al.* (1998) and Blau (1999) 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, Table 3 also includes a specification where both income measures are included simultaneously. Compared to the other two specifications, both estimates fall but remain 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% children's schooling increases by more than if one rises by 10% 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 on children's educational attainment. At the same time, there is support 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. In the more fully specified models, the income effects are imprecisely estimated and, in the college education equation, they are jointly significant only at a 10% 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 the simultaneous specification would have been roughly equal to those in the separate specifications, the permanent income hypothesis would have become more plausible. If each of the income parameters in the simultaneous specification would have been roughly equal to those in the separate specifications, 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 article, there is no harm: income matters.

3.3. *The Effect of Income in Low and High Income Families*

In the models estimated so far, income enters in a logarithmic form. Thus, in effect, we implicitly assumed that family income has the same proportional effect for adopted children raised in both low and high income families, and we estimated the average family income effect. However, if credit markets are imperfect, income should matter more for children in lower income families.

We address this by employing a linear spline function in log-income.[13] Defining the distribution of income on the basis of the full WLS database, cutpoints of the spline function are placed at the 20th, 40th, 60th and 80th percentile of the income distribution, thus allowing income to exert varying strengths in the different income quintiles.[14]

Table 3 presents the income parameters of these models. The 1975 log-income spline shows a sharply positive parameter in the spline's midsection, flanked by flat or declining line segments of which the slopes are all statistically insignificantly different from 0. For the 1992 spline, only the slope of the first segment is statistically significant; the other slopes are imprecisely estimated. Income matters more for lower income households.

In interpreting the coefficients, recall that they measure the effect of proportional increases in income, which, as income rises, is equivalent to larger and larger nominal increases. For the model specification that uses a full set of controls, Panel A of Figure 1 visualises the estimated relationship for the adoptee sample, in

[13] Adding a quadratic or cubic log-income term to the model is another strategy, but it did not generate a statistically significant difference in the estimates – and as noted earlier, the log income specification itself already yields a greater income effect for low income families.

[14] In 1975, these cutpoints amount to annual household incomes of $12,100, $15,100, $18,100 and $22,400, respectively, whereas the 1992 cutoff points were $36,700, $50,348, $66,000 and $94,500, all measured in nominal prices. To place these values in context, the median household income in 1975 was $13,889 in 1975 and $35,954 in 1992 (US Executive Office, 1988, 2002). It should also be recognised that the WLS households belong to a particular birth cohort that was reaching its peak earnings potential in the 1980s and 1990s, was fairly well educated, and originated from a midwestern state that until the 1980s enjoyed above-average household incomes, even as it scattered across every state of the union (Park, 1999) – whereas the national statistics reflect an average across all cohorts from all states. When our discussion refers to low-income and high-income households, it does so in the context of the WLS income distribution.

comparison with the log-income specification discussed earlier. Note that the curves are shifted vertically so as to match at the midpoint of the third interval. Given that there are only about 100 observations per interval (and even less in the probit specification), it is possible that the fluctuation from the second to the fourth interval results from data outliers. In any case, the log-income version is a pretty accurate approximation of the overall relationship and certainly is preferred over a linear income specification.

For the early income specification, in essence, 1975 income does not contribute to greater schooling achievement of children in the lowest two income quintiles, whereas for 1992 income the largest gain is found exactly among the lowest income quintile. There is a straightforward explanation why income effects among low income households in 1992 are more substantial than income effects in 1975: when they are young, low income parents are less able to smooth their consumption profile. Suppose parents are forward-looking and optimise 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. But since they cannot use their future earnings as a collateral to borrow, they will, as a matter of optimising lifecycle behaviour, 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, consumption is more strongly impacted by momentary income variations as the time horizon is shorter.[15]

Furthermore, consider that in 1975, when parents were roughly 36 years of age, most children were in compulsory education, and some were not even in school yet, or were not even born. 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. This correlates also with Duncan *et al.* (1998), who found that family income received when the child is aged 0 to 5 years has more impact on schooling than the income during the ages of 6 to 15: low-income families cannot spend as much on their home environment as high-income families do. In 1992, on the other hand, children who are still in school (about 40%) are mostly in college or university and undergo schooling voluntarily. Because at this stage schooling is most costly, a given income increment is more important for a student coming from a low-income family than for one from a wealthy family, which in any case is more able to borrow from the capital market and spread payments out over a longer period of time.[16]

[15] 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.

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

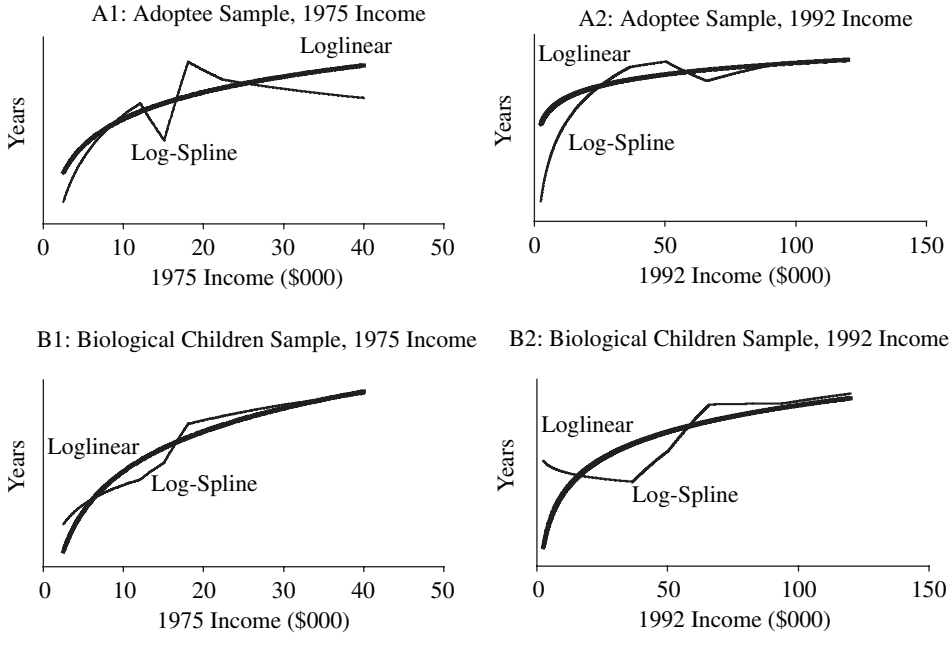

Fig. 1. *Comparing Income Effects from the Logarithmic and the Splined Specifications*
Note. Estimates pertain to the model specification with a full set of controls. Curves are shifted such that the ordinates at the midpoint of the third line segment are equal.

3.4. *Results for Own Birth Children*

The income estimates reported thus far are genetically unbiased and indicate quite convincingly that income matters for the schooling outcomes of adoptees. The natural question arises whether these estimates apply to other children as well. Tables 4 and 5 present the estimated schooling attainment models for own birth children in the same format as before.

Some results are very similar to those found before for adoptees. The age and sibling effects are quite the same; the effect of father's years of schooling is nearly identical; and grandparent variables do not matter. The effect of the child's gender is the same as for adoptees but now statistically significant: daughters stay in school longer. Other effects differ. Not surprisingly, parental IQ is highly important, and mother's education shows to be as important as father's.

The income effect is once again positive and statistically significant, for income from both years. The log-income spline function is once again suggestive of a stronger income effect around the middle income quintile, and with much larger quintile samples, the slope parameters are more precisely estimated. Nevertheless, as Panel B of Figure 1 illustrates, the log-income specification is an adequate parsimonious approximation to the overall income-education relationship.

Of course, the objective of this subsection is a comparison of the effect of parental income between adopted and biological children. One would expect the impact of family income on educational achievement to be stronger for children raised by their own biological parents, since, in a biological child schooling model,

Table 4

Schooling of Own Birth Children: Baseline Specifications

	(1)	(2)	(3)	(4)	(5)
Years of schooling					
Log income 1975	1.017 (0.063)***	0.833 (0.060)***	0.369 (0.051)***	0.373 (0.051)***	0.370 (0.051)***
Intercept	8.466 (0.644)***	6.420 (0.606)***	5.034 (0.517)***	5.251 (0.549)***	5.323 (0.550)***
Daughter	0.148 (0.040)***	0.138 (0.040)***	0.112 (0.037)***	0.111 (0.037)***	0.111 (0.037)***
Age	−0.129 (0.006)***	−0.119 (0.005)***	−0.074 (0.005)***	−0.074 (0.005)***	−0.074 (0.005)***
Number of siblings	−0.176 (0.016)***	−0.173 (0.016)***	−0.116 (0.014)***	−0.116 (0.014)***	−0.115 (0.014)***
IQ of parent		0.352 (0.018)***	0.148 (0.017)***	0.149 (0.017)***	0.146 (0.017)***
Years of education father			0.252 (0.012)***	0.253 (0.012)***	0.250 (0.012)***
Years of education mother			0.248 (0.017)***	0.249 (0.017)***	0.246 (0.017)***
Log income 1957				−0.035 (0.030)	−0.043 (0.031)
Years of education grandfather					0.012 (0.009)
Years of education grandmother					0.002 (0.009)
Mean loglikelihood	−1.798	−1.778	−1.719	−1.719	−1.719
Sample size *N*	14,552	14,552	14,552	14,552	14,552
College education					
Log income 1975	0.204 (0.014)***	0.171 (0.013)***	0.088 (0.012)***	0.088 (0.012)***	0.088 (0.012)***
Daughter	0.032 (0.009)***	0.031 (0.009)***	0.030 (0.009)***	0.030 (0.009)***	0.030 (0.009)***
Age	−0.023 (0.001)***	−0.021 (0.001)***	−0.010 (0.001)***	−0.010 (0.001)***	−0.010 (0.001)***
Number of siblings	−0.032 (0.003)***	−0.032 (0.003)***	−0.024 (0.003)***	−0.024 (0.003)***	−0.023 (0.003)***
IQ of parent		0.066 (0.004)***	0.032 (0.004)***	0.032 (0.004)***	0.031 (0.004)***
College education father			0.048 (0.002)***	0.048 (0.003)***	0.048 (0.003)***
College education mother			0.044 (0.004)***	0.045 (0.004)***	0.044 (0.004)***
Log income 1957				−0.002 (0.007)	−0.003 (0.007)
Years of education grandfather					−0.001 (0.002)
Years of schooling grandmother					0.002 (0.002)
Mean loglikelihood	−0.625	−0.609	−0.566	−0.566	−0.566
Sample size *N*	11,623	11,623	11,623	11,623	11,623

Robust standard errors are in parenthesis; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

Table 5

Schooling of Own Birth Children: Family Income Coefficients Using Periodic Income Measures and Spline Regressions

	Years of schooling		College education	
	(1)	(2)	(3)	(4)
Log income 1975	1.017 (0.063)***	0.370 (0.051)***	0.204 (0.014)***	0.088 (0.012)***
Log income 1992	0.860 (0.045)***	0.330 (0.037)***	0.164 (0.011)***	0.069 (0.010)***
Log income 1975	0.623 (0.063)***	0.250 (0.052)***	0.130 (0.013)***	0.063 (0.013)***
Log income 1992	0.685 (0.047)***	0.273 (0.210)***	0.127 (0.011)***	0.055 (0.010)***
Splines log income 1975				
0–20% ($N = 3239,2508$)	0.143 (0.127)	0.182 (0.111)*	0.048 (0.030)	0.063 (0.029)**
20–40% ($N = 3056,2406$)	1.466 (0.457)***	0.500 (0.414)*	0.290 (0.106)***	0.118 (0.106)
40–60% ($N = 2741,2199$)	2.337 (0.645)***	1.365 (0.578)**	0.321 (0.139)**	0.160 (0.140)
60–80% ($N = 2642,2186$)	1.488 (0.554)***	0.233 (0.477)	0.345 (0.115)***	0.120 (0.115)
80–100% ($N = 2874,2324$)	1.091 (0.196)***	0.249 (0.158)	0.202 (0.038)***	0.073 (0.036)**
Splines log income 1992				
0–20% ($N = 2990,2392$)	−0.104 (0.090)	−0.066 (0.075)	−0.027 (0.023)	−0.016 (0.021)
20–40% ($N = 2890,2285$)	1.575 (0.323)***	0.838 (0.287)***	0.325 (0.077)***	0.190 (0.075)**
40–60% ($N = 2900,2352$)	2.124 (0.428)***	1.466 (0.382)***	0.325 (0.095)***	0.215 (0.094)**
60–80% ($N = 2901,2282$)	1.371 (0.332)***	0.028 (0.289)	0.252 (0.069)***	0.014 (0.069)
80–100% ($N = 2871,2312$)	0.722 (0.152)***	0.348 (0.121)***	0.150 (0.030)***	0.104 (0.028)***
Control variables:				
Gender, age, siblings	Yes	Yes	Yes	Yes
Parent's IQ and education, grandparent's income and education	No	Yes	No	Yes

Robust standard errors are in parenthesis; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.
Likelihood ratio tests test the joint significance of both income coefficients. Low values indicate statistical insignificance.

income contains genetic transfers from parents, whereas these genetic transfers do not exist for the adopted children within the family of rearing. However, rather surprisingly, we find that (*i*) the income coefficient estimates in the barest baseline model (column (1) of Table 4) are virtually identical to the income coefficients for adoptees; and (*ii*) the income estimates belonging to the full control model (column (5) of Table 4) are somewhat smaller than, but statistically indistinguishable from, those found for adoptees. The same is true for 1992 income; see Table 5.

What do these surprising results imply? Are they indeed suggesting that the positive influence of family income on the educational attainment reflects family environment rather family genes? Or do these results imply that there is something in the process of adoption that compensates for the lack of genetic transfer between biological and adopted children and leads to equal income effect estimates? In that case, the income effect among adoptees has a positive bias. In the next section, we examine alternative mechanisms that, among others, might create this type of an upward bias.

4. Sensitivity Analysis

Among adoptees and their parents, there are at least five ways in which attempts to estimate unbiased income effects may be confounded. Parents may treat adopted children differently from biological children. Parents who decide to adopt

children may have better parenting skills. Adoptees may differ in systematic ways from biological children. The placement of adoptees among adoptive families may not be random. Among birth parents and their children, it is possible that divorce and single parenthood complicate the identification of income effects. In this Section, we examine whether these mechanisms threaten a conclusion that family income matters, not only for adoptees but for other children as well.

4.1. *Are There Treatment Differentials?*

The first mechanism is that parents overinvest in their adopted children. To find out whether there are differences in upbringing between own birth and adopted children, one might wish to compare the educational outcomes of adopted and biological children, but then one must deal with the complication 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.[17] 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 urge to invest more heavily in their adopted children, relative differences between the parental income estimates relevant for educational outcomes of adoptees should be observed.

Results are presented in the top half of Table 6. With the specification presented in columns (1) and (5) of Table 2 as the baseline models, the first row 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. We then let the income parameters differ by including a 'having own birth siblings $\times$ log family income' interaction effect and thus examine whether parents allocate different amounts of money to the education of their adopted child when they have children of their own birth. With income measured in 1975, the interaction terms are mildly positive but statistically insignificant. With income measured in 1992, the interaction terms are mildly negative and only marginally significant when the dependent variable is years of schooling used in the stripped control model.

The bottom half of Table 6 inserts the variable 'having adopted siblings' in the educational attainment model of biological children and thus is the mirror image of the top half of the Table. Again, if the overinvestment argument is applicable, one would expect that own birth children would do worse in school in the presence of adopted siblings. Results in the first and third column of Table 6 show that own birth children raised with and without adopted siblings attain more or less the same amount of schooling. All coefficients attached to the variable 'having adop-

[17] For own birth children, income effects capture genetic transfers from parents. For adoptees, these transfers do not exist within the family of rearing. In Plug and Vijverberg (2003) 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 that approach is that the applied estimation procedure only isolates that component of income that is unrelated with family genes under a strict set of assumptions. In this article we relax these assumptions and introduce an alternative approach to identify treatment differentials.

Table 6

Schooling of Adopted and Own Birth Children: Examining Different Allocation Rules

	Years of schooling		College education	
	(1)	(2)	(3)	(4)
Adopted children				
Log income 1975	1.018 (0.236)***	0.620 (0.258)**	0.184 (0.059)***	0.113 (0.055)**
Having own birth siblings	0.070 (0.236)	−0.034 (0.217)	0.032 (0.054)	0.007 (0.054)
Log income 1975	0.991 (0.555)*	0.578 (0.479)	0.152 (0.118)	0.080 (0.109)
Having own birth siblings	−0.321 (6.029)	−0.627 (5.128)	−0.454 (1.259)	−0.478 (1.133)
Having own birth siblings × log income 1975	0.040 (0.619)	0.060 (0.524)	0.048 (0.134)	0.048 (0.121)
Log income 1992	0.849 (0.192)***	0.444 (0.194)**	0.167 (0.040)***	0.091 (0.044)**
Having own birth siblings	0.059 (0.233)	−0.030 (0.214)	0.033 (0.054)	0.012 (0.052)
Log income 1992	1.230 (0.265)***	0.718 (0.280)**	0.254 (0.079)***	0.161 (0.079)**
Having own birth siblings	6.911 (4.000)*	4.814 (3.799)	0.914 (0.215)	0.825 (0.357)
Having own birth siblings × log income 1992	−0.617 (0.363)*	−0.436 (0.344)	−0.134 (0.092)	−0.107 (0.090)
Own-birth children				
Log income 1975	1.017 (0.063)***	0.369 (0.051)***	0.205 (0.014)***	0.087 (0.012)***
Having adopted siblings	0.009 (0.133)	−0.276 (0.135)**	−0.026 (0.030)	−0.089 (0.029)***
Log income 1975	1.003 (0.063)***	0.360 (0.063)***	0.205 (0.014)***	0.088 (0.013)***
Having adopted siblings	−5.782 (4.064)	−4.026 (3.592)	0.181 (0.867)	0.305 (0.760)
Having adopted siblings × log income 1975	0.597 (0.415)	0.387 (0.367)	−0.020 (0.084)	−0.040 (0.080)
Log income 1992	0.860 (0.045)***	0.330 (0.037)***	0.164 (0.011)***	0.070 (0.010)***
Having adopted siblings	−0.058 (0.153)	−0.292 (0.136)**	−0.033 (0.030)	−0.091 (0.029)***
Log income 1992	0.858 (0.046)***	0.331 (0.038)***	0.165 (0.011)***	0.071 (0.010)***
Having adopted siblings	−1.035 (2.985)	0.159 (2.397)	0.342 (0.581)	0.516 (0.329)
Having adopted siblings × log income 1992	0.088 (0.267)	−0.040 (0.215)	−0.033 (0.056)	−0.059 (0.051)
Control variables:				
Gender, age, siblings	yes	yes	yes	yes
Parent's IQ and education, grandparent's income and education	no	yes	no	yes

Robust standard errors are in parenthesis; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

ted siblings' are close to 0 and statistically insignificant. When characteristics of parents and grandparents are added (columns (2) and (4)), however, we find that own birth children do end up with less schooling in the presence of adopted siblings. Even so, the income effect is never different in adoptive households.

To sum up, it is possible that equal income effects are observed for adopted and own birth children if adoptive parents would overinvest in their adopted children. This is not the case. In fact, we find no evidence that parents treat their adopted children differently from own birth children.[18]

4.2. *Are Adoptive Parents Better Parents?*

The second mechanism is that adoptive parents are different from other parents in ways that are related to income effects. From summary statistics in Table 1 we learn

[18] This also means that the Cinderella motive put forward by Case *et al.* (2000, 2001) is not observed in our data. Cinderella motives imply differences in upbringing: parents invest more in their own birth children because of some biological imperative.

that adoptive parents are better educated, have higher IQs, and make more money. Perhaps this also means that adoptive parents have better (but unobserved) parenting skills as well that push the income estimates upwards.

There is little in this regard that may be inferred from the sample of adopted children. Table 6 already reported that according to the full control model own birth children raised with adopted children do worse in school, not better as the superior parenting skills idea suggests. The interaction terms, however, indicate that these unobserved parenting skills are not correlated with the impact income has on the child's schooling.

We borrow from a related argument by Ginther and Pollak (2000) to explain why among parents with essentially the same IQ, schooling, income and family background, adoptive parents do significantly worse in raising their own birth children.[19] The presence of adopted children may have a disruptive effect on family life. Adoptive parents may have to deal with personalities that differ from their own offspring; genetic predispositions may express themselves over time; prenatal and, to the extent that adoption did not occur at birth, postnatal experiences may impact the adopted child later in life; and the adopted child's 'genealogical bewilderment' (the anthropological/psychological term describing the confusion about role models in regard to physical appearance and personality traits) may lead to frictions within the household (Humphrey and Humphrey, 1994; Modell, 1994). It may be that many adoptive households do not face any of these issues, but as the regression analysis records the average effect, the problems described in the psychology literature suggest 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.

Alternatively, these particular results may be driven by selection of a different kind. 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, and suppose that parents have imperfect information about the location or spread of this distribution. The firstborn child reveals information about this distribution 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 re-estimating the models in Table 6. The parameter estimates (not shown in the tables) were very similar but, as the sample size was a third smaller, none of the parameter estimates of the dummy was statistically significant any longer. It is therefore not clear that selection of this type is occurring either, but in any case, taken altogether, these findings are difficult to reconcile with the idea that adoptive parents are truly superior, and alleviate the fear that unobserved parenting skills might have biased the income parameters in the adoptee schooling models.

[19] 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.

4.3. *Are Adoptees Different from Other Children?*

Table 1 supports the notion that with respect to educational attainment adopted children are at a slight disadvantage.[20] Could this be because adoptees differ in some systematic way from other children, and could this difference impact the magnitude of the income effect?

While much of the previous work on differences between adopted and biological children has been done by psychologists, there is some economic literature available as well. Medoff (1993) shows that poor families and young single (and often socially disadvantaged) mothers face more difficulties to make ends meet, and are therefore more likely to put their children up for adoption; see also Melosh, 2002). If genes are somehow responsible for being poor or for ending up as a young single mother, adoptees will end up with inferior genes. With a similar prediction Becker (1991) puts forward an alternative mechanism: if parents could choose, they probably would put their inferior children up for adoption rather than their superior children. Furthermore, it is likely that compared to other kids adoptees are more often exposed to negative environmental experiences when they are young.

On their own, however, these differences cannot be held responsible for income effects that are too high. In the schooling model, the omitted (and unfavourable) variables that are typical to adoptees are all swept into the intercept, unless these variables correlate with income. Given that the WLS does not provide any information on the kids in our sample other than the characteristics we already use, it is impossible to test directly whether these omitted and unfavourable characteristics are muddling our income estimates.

Yet we are not so much concerned that this is actually happening. Estimated income effects are only blown up if parents try to compensate for their children's deficiencies. Table 6 provides no indication for believeing that parents apply different allocation rules.

4.4. *Is There Non-Random Assignment?*

The fourth mechanism is that children are not randomly assigned to their new family of rearing. 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 (Modell, 1994; Melosh, 2002), the underlying selection process would result in family income coefficients that tend to be too high. 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. Because the WLS does not provide this information, it is impossible to find a remedy to remove this bias.[21]

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

[21] Circumstantial evidence about adoption as practised in the US indicates that the number of non-random adoptions in the US is substantial (Plug, 2004), which indeed suggests the possibility of selection bias.

4.5. *Are Some Birth Children Raised in Disrupted Families?*

The sensitivity analysis thus far has concentrated on mechanisms that could possibly magnify the estimated income effects among adoptees. But, instead, perhaps it is the case that there are alternative mechanisms that attenuate the estimated income effects among own birth children. Notice that the two approaches are complementary: both intend to explain why the impact of family income on educational achievement is not stronger for children raised by their own biological parents.

We consider disrupted families as a possible cause. While we would prefer our baseline birth sample to be composed of undisrupted families with two biological parents present, we actually use a sample that consists of two-parent families where one of the parents is not necessarily the biological parent. We do not know how many, but there is a good chance that some of these families at one point or another were disrupted and temporarily single-parent families.[22] We omit all single-parent families observed in the years 1975 and 1992, but acknowledge that family disruptions are not fully absent from our sample, and that one of the parents may indeed be a stepparent, which brings frictions of its own (e.g., Wolfe *et al.*, 1996; Humphrey and Humphrey, 1994). If left untreated, these frictions could possibly pull the estimated income effects down and thus interfere with the relationship that we seek to understand in this article.

The problem is that the WLS only provides information on parent/child relationships for those parents who served as primary respondents in the WLS survey, but not for their husbands and wives. With information on family composition and marital histories that is available, it is still possible to get an idea whether family disruptions are affecting our income estimates. We perform three tests.

The first test is rather straightforward. We concentrate on children who experienced a family disruption and consider children of divorced parents, children of single parents and stepchildren as distinct groups. If we then limit our sample to these children, we would expect our estimated income effects to be smaller as long as family disruptions lead to reduced income effects that put the children's educational success at risk. In Table 7, rows (1), (2) and (3), the estimates attached to family income indeed indicate that the effects are almost always smaller for children with divorced parents, for children raised in single-parent families, and for stepchildren.[23] It is not entirely clear where these reductions come from, but for the purpose of this study we do not need to know.[24] What we

[22] In the US in 1998, only 68% of the children live with two parents (US Bureau of the Census, 1998). This percentage has trended downward, and is lower than among WLS parents who are predominantly Caucasian.

[23] The WLS not only records whether children are their parents' own offspring, or whether children are adopted but also whether children are stepchildren. In the survey these form three distinctive groups. On the basis of the latter classification, it is possible to construct a new sample of 947 children having stepparents. Note further that the divorce and single parenthood samples are not mutually exclusive.

[24] For example, single-parent families typically have less income; thus, does the lower educational attainment of children derive from the reduction of income itself, or from the absence of a second parent, from the stress that accompanies marital disruption, from the poorer neighborhood that lower income households move to etc. (McLanahan and Sandefur, 1994; Duncan and Brooks-Gunn, 2000)?

Table 7

Schooling Models and the Effects of Family Income Using Various (Sub)Samples

	Family income measured in 1975				Family income measured in 1992			
	Years of schooling		College education		Years of schooling		College education	
Baseline results								
Birth children ($N = 14552,11623$)	1.017 (0.063)***	0.370 (0.051)***	0.204 (0.014)***	0.088 (0.012)***	0.860 (0.045)***	0.330 (0.037)***	0.164 (0.011)***	0.069 (0.010)***
Children at risk								
(1) Birth children in single-parent families ($N = 1983,1652$)	0.700 (0.121)***	0.197 (0.107)*	0.121 (0.029)***	0.041 (0.026)*	0.687 (0.101)***	0.260 (0.092)***	0.136 (0.021)***	0.065 (0.021)***
(2) Birth children in divorced families ($N = 2293,1839$)	0.614 (0.103)***	0.194 (0.099)*	0.116 (0.023)***	0.048 (0.021)**	0.711 (0.095)***	0.243 (0.093)***	0.126 (0.024)***	0.047 (0.023)**
(3) Stepchildren ($N = 947,731$)	0.474 (0.146)***	0.240 (0.160)	0.102 (0.039)***	0.067 (0.039)*	0.671 (0.142)***	0.384 (0.149)**	0.098 (0.035)***	0.056 (0.036)
Pure sample								
(4) Birth children in first marriage families ($N = 12653,10094$)	1.080 (0.069)***	0.413 (0.055)***	0.217 (0.016)***	0.096 (0.014)***	0.886 (0.050)***	0.344 (0.040)***	0.170 (0.012)***	0.073 (0.010)***
Contaminated sample								
(5) All birth children ($N = 16929,13585$)	0.913 (0.052)***	0.317 (0.043)***	0.181 (0.012)***	0.076 (0.011)***	0.835 (0.040)***	0.317 (0.034)***	0.160 (0.009)***	0.069 (0.008)***
(6) All birth children and all stepchildren ($N = 17876,14316$)	0.897 (0.050)***	0.334 (0.043)***	0.178 (0.011)***	0.079 (0.010)***	0.788 (0.037)***	0.329 (0.031)***	0.151 (0.008)***	0.071 (0.008)***
Control variables								
Gender, age, siblings	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent's IQ and education, grandparent's income and education	No	Yes	No	Yes	No	Yes	No	Yes

Robust standard errors are in parenthesis; * significant at 10% level, ** significant at 5% level, *** significant at 1% level.
Coefficients come from a model using single parent indicators in 1975 and 1992 as additional control variables.

need to know is how strongly these selection effects may impact the estimated income effects in our baseline sample of own birth children. This is what we do in the next tests.

In the second test we re-estimate the education attainment models using only own birth children, raised by currently married parents who are still in their first marriage. The idea is that income effects should then be estimated most accurately using a sample that is limited to children who are raised in families where frictions are absent. Results are reported in Table 7 row (4). The estimates suggest that income effects are indeed somewhat stronger for children who come from undisrupted families and thus receive undisturbed treatments. But these differences are only variations at the margin, and with these new estimates there are still virtually no observed differences between adopted and own-birth children.

In the third and final test we aggravate (and not eliminate) the bias due to family disruptions. The intuition is purposefully to contaminate our sample with children from disrupted families to get a meaningful lower bound on our income estimates. We extend the current sample of birth children with own birth children raised in single-parent families and step-children and re-estimate previous mobility models. Results are reported in Table 7 rows (5) and (6). Having already established that income effects are indeed smaller in disrupted families, it comes as no surprise that income effects fall. Yet the reduction is too small to overrule the observation that the positive influence of family income on the educational attainment of adoptees and own-birth children is identical.[25]

5. Discussion and Concluding Remarks

This article 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.

The first problem is that parents influence their children 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, take advantage of the information available on grandparents, exploit the periodic measurement of family income, and examine income effects in low, middle and high income families. 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.

[25] The results in rows (5) and (6) are not sensitive to the inclusion of single-parenthood, divorce and stepchild indicators as additional control variables.

The second problem is that adoption does not have the nice randomisation 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 specialised 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 (2000) again give us a compelling reason to believe that we do have evidence of a causal effect. Even so, as always, 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 low income households. 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 programme 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 emphasised, 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, 2003). It is the smaller half, not genetically determined, that we are concerned with in this article.

Appendix

Becker (1991) developed an intergenerational schooling model where the child's schooling is explained by family income and the child's endowments. Consider this model, writing the child's schooling function as:

$$S_c = a_1 Y_p + h_c + \epsilon_c. \tag{1}$$

S_c indicates the child's schooling, Y_p is the parent's income, h_c are unobserved heritable endowments of the child relevant to his or her schooling, and ϵ_c is a child-specific characteristic that is random.

The objective is to estimate the parameter a_1 that measures the effect of parent's income on the child's schooling net of the effects that are driven by the unobserved endowments of the child. With information on S_c and Y_p, direct estimation yields an upward biased estimate of a_1. The argument is as follows. If endowments of parents h_p are passed on to their child according to the relationship

$$h_c = b_1 h_p + v_c, \tag{2}$$

where b_1 measures the degree of mobility, the child's schooling is modelled as

$$S_c = a_1 Y_p + b_1 h_p + v_c + \epsilon_c. \tag{3}$$

Clearly, a_1 is overestimated since the unobserved h_p is positively correlated with Y_p, assuming that b_1 is positive.

Adoptees

This article uses an adoption strategy to obtain a better estimate of a_1. The advantage of using adoptees is that these children do not share their adoptive parents' genes. For parents and their own birth children, endowment transmission runs through both genetic and cultural channels. For adoptive parents and their adopted children, genetic transfers do not exist.

If we separate the mobility parameter b_1 into a genetic b_{1g} and a cultural component b_{1c} and recognise that the adoptive parents impart the cultural contribution, the endowment mobility relationship for adoptees is modified as follows

$$h_c = b_{1g} h_p^* + b_{1c} h_p + v_c. \tag{4}$$

The unobserved endowments of the adoptees' biological parents are represented with h_p^*. Inserting this into the schooling function of the adopted child yields

$$S_c = a_1 Y_p + b_{1g} h_p^* + b_{1c} h_p + v_c + \epsilon_c. \tag{5}$$

By assumption the bias caused by the parent's endowments that are passed on genetically is eliminated.

Nevertheless, this approach fails to identify a_1, since the bias caused by unobserved parental child-rearing talents is still present.[26] If left untreated, the effect of the parent's income on the schooling of his or her child remains overestimated. This does not mean, however, that the estimation of the schooling equation on a sample of adoptees is without value. This approach can be credited with reducing the bias.

Three Generations

Akin to Becker (1991) and Becker and Tomes (1986), information about grandparents may be introduced to remove the child-rearing bias in a_1 that is caused by unobserved parenting skills. As the analogue to the child's schooling function, assume that the parent's schooling can be modelled in the form of

$$S_p = a_1 Y_g + h_p + \epsilon_p. \tag{6}$$

where Y_g denotes grandparent income. We remove the endowments by subtracting $b_{1c} S_p$ from S_c in the child schooling model. In this equation

$$S_c - b_{1c} S_p = a_1 Y_p - a_1 b_{1c} Y_g + b_{1g} h_p^* + (b_{1c} h_p - b_{1c} h_p) + v_c + \epsilon_c - b_{1c} \epsilon_p \tag{7}$$

it is clear that the parent's endowments drop out. For three generations the new mobility relation is written down as

$$S_c = a_1 Y_p + b_{1c} S_p - a_1 b_{1c} Y_g + b_{1g} h_p^* + v_c + \epsilon_c - b_{1c} \epsilon_p. \tag{8}$$

[26] We see adoption as a natural experiment where children given up for adoption are randomly placed in their adoptive families, and assume that Y_p and h_p^* are unrelated.

By assumption the bias caused by the parent's child-rearing talents is eliminated.

Elimination of the child-rearing bias comes at a cost. We now risk the bias caused by ϵ_p. When the child is in school, ϵ_p is known to the parent since his or her schooling is already realised and does not disappear in the schooling equation. If S_p, Y_p, and Y_g are positively correlated, the OLS estimates of the coefficients on S_p and Y_p are biased downwards, and the coefficient on S_g is biased upward.

Identification becomes more troublesome if grandparents have a direct effect on the child's schooling (Hill and O'Neill, 1994). With direct grandparental effects the predicted effect of Y_g on S_c is no longer negative but rather becomes indeterminate. Since the model then also requires information on greatgrandparents, which is unavailable in any common data set, the three generation approach fails in identifying a_1 because of omitted variable bias.

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