Estimating the effect of mother's schooling on children's schooling using a sample of adoptees

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In a recent paper in this *Review* Jere Behrman and Mark Rosenzweig (2002) present empirical evidence that contradicts the conventional wisdom that maternal schooling has a bigger effect on the child's schooling than that of her husband.[1] They consider the impact of parental schooling on child schooling in the presence of unmeasured ability and assortative mating. Using twin data, they come to the surprising conclusion that the mother's schooling has little if any impact on the schooling of her child, holding everything else (including unobserved ability factors of either mother or father) constant.

Their findings –I reason– must be bolstered with further support. This paper considers the effects of unobserved inherited abilities on the child's schooling, but, instead of twinning, obtains identification from adopted children. If adopted children share only their parents' environment and not their parents' genes, any relation between the schooling of adoptees and their adoptive parents is driven by the influence parents have on their children's environment, and not by parents passing on their genes. Controlling for inherited abilities

and assortative mating I find that the association between mother's (but not father's) and child schooling disappears. These findings are consistent with the twin results of Behrman and Rosenzweig (2002).

This paper continues as follows. Section I sets out the identification strategy with adopted children. Section II provides a brief description of the Wisconsin Longitudinal Survey. Section III contains some background information on adoptions as practiced in the United States. In Section IV the parameter estimates are presented and compared to estimates reported in two recent adoption studies. Section V explores to what extent my findings are subject to contradictory interpretations. Section VI concludes.

I A simple mobility model

This paper uses a reduced form intergenerational mobility model, consistent with models of household resource allocations, where both parents have an effect on their child's schooling

$$S_i^c = \delta_1 S_i^m + \delta_2 S_i^f + \Gamma_1 h_i^m + f_i^m + \Gamma_2 h_i^f + f_i^f + \epsilon_i^c. \tag{1}$$

Subscript i indexes the family in which the child is raised, S_i^c indicates the child's schooling, S_i^m is the schooling of the mother, S_i^f is the schooling of the father, the h's are the unobserved heritable endowments of both parents, the f's are also unobserved endowments that express the child-rearing talents of both parents, and ϵ_i^c is a child-specific characteristic. The heritable endowments are passed on genetically and common to the parents' own birth children. The child-rearing endowments, however, are common to all children in the family. Attention is focused on parameter δ_1 that measures the effect of the mother's schooling on that of her child. This maternal effect should ideally capture the impact that her schooling has on the quality and quantity of time, goods and money she devotes to her child net of the effects that are genetically driven.

With conventional samples of parents and their own birth children direct estimation of (1) yields an upward biased estimate of δ_1. The problem is that heritable abilities, child-rearing endowments and assortative mating –when ignored– push δ_1 upwards.

With monozygotic twin mothers, identical in their inborn endowments but different in their amounts of schooling, Behrman and Rosenzweig (2002) obtain identification by taking the difference between the twin

mothers' children

$$\Delta S^c = \delta_1 \Delta S^m + \delta_2 \Delta S^f + \Gamma_2 \Delta h^f + \Delta f^f + \Delta \epsilon^c \tag{2}$$

It is easy to see that the bias caused by the mothers' heritable and inborn child-rearing endowments is eliminated. Differences between the fathers' heritable and child-rearing endowments, however, are still there and need to be taken into account in order to achieve identification. If not, the effect of the mother's schooling remains biased upwards if more schooled mothers marry partners with more favorable endowments and better fathering skills.[2]

This paper offers an adoption strategy to identify δ_1. The advantage of using adoptees is that these children do not share their parents' genes. If I think of adoption as a natural experiment where children given up for adoption are randomly placed in their adoptive families, I may either assume that unobserved heritable endowments of the adoptees' biological and adoptive parents are uncorrelated, or that the Γ coefficients are zero. Then for adoptees the schooling function in (1) is written down as

$$S_i^c = \delta_1 S_i^m + \delta_2 S_i^f + f_i^m + f_i^f + \epsilon_i^c. \tag{3}$$

By assumption the bias caused by the parents' heritable endowments is eliminated. The inborn child-rearing talents of both adoptive parents, however, still remain. There are two reasons for these unobserved child-rearing talents to be correlated with the mother's schooling. If better educated mothers have better mothering skills to begin with, and if better educated mothers choose their marriage partner for his parenting skills, the effect of the mother's schooling on that of her child is overestimated.

Adoptees offer the unique opportunity to control for genetic effects, given there is random assignment of adoptees to their adoptive parents. One difficulty with this approach is that for adoptees the assignment process is not always random. Without random assignment, the relation between the mother's schooling and that of her child is no longer independent of inherited abilities. Non-random matches involve both related and unrelated adoptions. In case of related adoptions (including adoptions of relatives and stepparents) genetically related matches are obvious. Mothers who raise and adopt their relatives' children share genes with their adoptees' own birth parents because they are family. Mothers who adopt children

whom their partner brings into the marriage share common genes because there is assortative mating on the marriage market on heritable endowments that correlate with schooling (Behrman and Rosenzweig 2002). In case of unrelated adoptions, non-random matches occur less frequent but are still possible when better educated parents manage to adopt children with more fortunate backgrounds or when adoption agencies use corresponding qualities of both natural and adoptive mothers as a matching strategy (Judith Modell 1994; Barbara Melosh 2002).

What happens to δ_1 if the assumption of random assignment is relaxed, and Γ is allowed to be nonzero and positive?[3] Non-random assignment probably raises δ_1, but I expect that the upward bias is smaller for adoptees than for own birth children. The argument is as follows. Biased estimates are produced to the extent that heritable endowments of mothers who adopt and mothers who give up their children for adoption are (positively) correlated. Since any apparent difference between the heritable endowments of these mothers will lower the correlation between their genetic endowments, the same difference will reduce the impact of inherited abilities on the effect of maternal schooling as well. Unless adoptive mothers and mothers who register their children for adoption are identical, the transmission coefficient Γ and corresponding bias will thus always be smaller in adoption than in conventional mobility studies.

It thus seems that the adoption approach fails in finding the perfect identification for the effect of changing the mother's schooling on that of her adopted child. This does not mean, however, that the estimation of (3) on a sample of adoptees is without value. Compared to conventional estimates, this approach can be credited for reducing the bias. Compared to twin estimates, it is interesting to have a complementary approach that describes the same process of intergenerational mobility of schooling in the presence of inherited ability and assortative mating.

II Data

The data used in this paper is taken from the Wisconsin Longitudinal Survey, that contains detailed multi-generational information about families. The collection of these data started in 1957 when information was

gathered from a complete cohort of students who graduated from high schools in Wisconsin in that year. In 1992 the same students were contacted again and information was collected about their school careers, labor market status, family conditions, and the school careers of their children. For these children it is recorded whether they are their parents' own offspring or whether they are adopted.[4]

This study exploits a set of questions that targeted the educational attainment of these students' children. The former high-school students 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 two dependent variables are created: "years of education" and "college education". For those children who completed the highest level attended, "years of education" equals the number of years nominally required for that. Children who were still in school constitute censored observations and will be treated accordingly in the empirical analysis; this is the case for about 40 percent of the adoption sample. "College education" is a dichotomous variable indicating whether the child completed 16 or more years of education. When this variable is the dependent variable, children younger than 23 are excluded.

The number of original observations in 1957 equals 10317. I work with a subsample of 5582 respondents with 16481 children of whom 610 were adopted. Of the 4735 respondents who fell outside the sample, 570 had died by 1992, 300 could not be located, 900 did not cooperate. In 1992 about 1800 respondents were either partnerless or childless in 1992, about 370 respondents had no records on their own or partner's educational attainment, and for 620 respondents income values were either missing or too low (40 respondents reported less than $100 per month in 1992). These respondents were excluded. Finally, about 150 respondents drop out because their children were either too young, because their children were neither adopted nor the biological offspring of both parents, or because information on their children's educational attainment was lacking. Summary statistics on own birth children and adoptees appear in Table 1.

III Adoption Trends between 1960-1980

This Section presents some fragmented evidence on adoptions as practiced in the United States between the years 1960 and 1980, and focuses its attention on the non-random process that links adoptees to their adoptive families.[5] Two adoption trends relevant to this study are distinguished.

- In the United States the number of both related and unrelated adoptions increased up to year 1970. In the period 1960-1970 the shares of related and unrelated adoptions did not change much and were about the same. In 1960 about 54 percent of all adoptions were unrelated adoptions. In 1965 and 1970 these percentages amounted to 53 and 51 percent. In 1975 and 1982 these percentages fell to 37 and 36 percent. Responsible for this decline was the decline in the number of unrelated adoptions during the period 1970-1975. Between 1970 and 1980 the number of related adoptions remained roughly constant (Penelope Maza 1984; Kathy Stolley 1993).

- When the number of unrelated adoptions dropped in the early 1970s, the number of international adoptions started to rise. Adoptions from abroad occurred prior to 1970, but their absolute numbers were small. In 1965 about 2 percent of all unrelated adoptions were international adoptions. In later years these percentages rose a little, but the actual incidence of foreign adoptions remained low. In the years 1970 and 1975 about 3 and 6 percent of all unrelated adoptions were adoptions from abroad (Richard Weil 1984; Stolley 1993). A similar pattern was observed for adoptions of children with special needs. Special needs adoptions involve older children, minority children, and children with physical, mental or emotional deficiencies. Before 1970 children with special needs were not considered for adoption. After 1970 special needs adoptions began to emerge, but like international adoptions, their numbers in absolute terms were very small between 1970 and 1980 (Dorcas Hardy 1984; Penelope Maza 1990; Rosenthal 1993).

These adoption patterns are consistent with the legalization of abortion in 1973. After adoption became legal, the number of children put up for adoption dropped dramatically (Stolley 1993; John Donohue and Steven Levitt 2003). With a falling supply of infants born in the United States given up for adoption,

adoption demand shifted towards foreign born children and children with special needs.

These adoption trends further suggest that the number of related adoptions in the United States is substantial. Since non-random assignment is most obvious among related adoptions, it is possible that adoptees in the WLS sample are not randomly assigned to their adoptive parents, and that the estimates in turn are tainted by selective placements.

IV Results

Table 2 presents the intergenerational mobility estimates of schooling. The structure of Table 2 is as follows. The first panel presents estimates of censored regressions on years of education. The second panel presents estimates of probit regressions on college education where children younger than 23 are excluded from the analysis.[6] Separate regressions are run on a sample of own birth children and on a sample of adoptees. All regressions include individual controls for the child's age and gender. These parameters are not reported. Instead, attention is focused on the effects of the schooling of parents have on the schooling of their children using different specifications.[7]

I begin with the schooling estimates that are obtained from a sample using children who are their parents' own biological offspring. In columns (1) and (2) the mother's and father's schooling are included as separate regressors. As expected, the estimated effects indicate that higher educated parents raise the number of years of schooling of their own birth children, and that the influence of the mother's schooling is somewhat more important than that of her husband. The effect of parental schooling as it is estimated represents both the direct transfer from the given parent and the indirect transfer from the other parent, which is due to assortative mating and the ensuing correlation of the parents' schooling, or on something that correlates with schooling. In the sample the correlation between the parents' schooling equals 0.544 which illustrates the non-randomness of matching. To quantify transmission effects due to assortative mating, mother's and father's schooling are included simultaneously to allow the schooling of both parents to have a separate effect on their child's schooling. Column (3) shows that the maternal schooling effect is most sensitive to the

inclusion of her partner's schooling. With her husband's schooling, the partial effect of mother's schooling is reduced by almost a half. The partial schooling effects of both parents are significantly positive and almost identical. Inclusion of family income as an additional control variable does not substantially change the parental schooling coefficients.

The fundamental problem with the interpretation of intergenerational mobility estimates thus far is that it ignores the strong correlation of parental schooling with unobserved ability. Since better educated parents are on average better endowed than less educated parents, they tend to produce children who do well in school by virtue of superior genes. As economists we are not so much interested in effects that are genetically driven. In essence genes are automatically passed on from parent to child without regards for incentives (Nathan Grawe and Casey Mulligan, 2002). It would be better to have information on that part of the mother's and father's schooling that is uncontaminated with family genes but still responsible for the school success of future generations.

In the next four columns the influence of the family genes is removed by estimating the previous intergenerational mobility specifications on a sample of adoptees. In columns (1) and (2) the estimated effects of parental schooling fall significantly, but the influences of the mother's and father's years of schooling are still statistically significant, positive and equally important.[8] These results show that part of the child's schooling is inherited, but that it is only the smaller part. Provided that the model is correctly specified, family genes are responsible for about 50 percent of the impact of mother's schooling and about 30 percent of the impact of father's schooling on that of their children. In column (3) the intergenerational effect of the marriage partner is taken into account. As before, the mother's schooling effect on that of her child's schooling is cut in half, which –I reason– is due to assortative matching among partners on schooling or on something that correlates with schooling.[9] But this time her schooling effect lacks statistical significance. With the additional effects of family income in column (4) the maternal coefficient falls somewhat further. The estimate of the effect of father's schooling on that of his child, however, is rather insensitive to the inclusion of mother's schooling (and family income). In contrast to maternal schooling effects, paternal schooling effects remain positive and statistically significant in all specifications.

In the second panel of Table 2 the dependent variable switches to whether or not the child is graduated from college. Also the parental years of schooling measures are replaced for whether or not the mother or father completed their college education. With college education the findings are very similar to those previously reported. The coefficients which represent partial derivatives show (*a*) that children with adoptive mothers and fathers with a college degree experience a significant higher chance of graduating from college themselves; (*b*) that for the intergenerational mobility of schooling family genes still matter but that it is the larger part of the parents' schooling that contributes to a better family environment for children to graduate;[10] and (*c*) that when the schooling measures of both partners are included, her partial effect of having a college education almost disappears where his partial effect of having a college education remains positive and statistically significant.

Bruce Sacerdote (2000) and Anders Björklund and Katarina Richardson (2001) also investigate the mobility relation between the parents' schooling and that of their children using samples with adopted and own birth children.[11] With a rather small sample of 170 adoptees in the United Kingdom Sacerdote (2000) finds positive and statistically significant schooling effects of both parents when mother's and father's schooling are included as separate regressors. Similar to the findings presented in this paper is that the effect of mother's schooling on that of her adopted child is the most sensitive to the inclusion of her partner's schooling. He finds that the partial effect of maternal schooling is cut in half. The difference is, however, that her estimated schooling effect remains positive and statistically significant with her partner's schooling included.[12] With foreign born adoptees in Sweden Björklund and Richardson (2001) find the opposite and report insignificant schooling estimates that indicate that both mother's and father's schooling have no impact at all. The findings presented in this paper lie somewhere in between.

V Fact or fiction?

The results obtained thus far are in apparent contradiction with widely held wisdom that mother's schooling is important for her child's schooling, and that mother's schooling is more important than the schooling of

her husband. Is it possible that I misinterpret my findings?

There is no doubt that the adoption results presented in this paper do not perfectly identify the effect of changing the mother's schooling on that of her child. I already showed that the estimated effect of the adoptive mother's schooling is probably too high because of unobserved inborn parenting skills (of both mother and her marriage partner). Possible selection can even amplify this upward bias. If adoptions are related or if adoption agencies use information on the natural mother's schooling to place children in their adoptive families, and if this matching correlation is ignored, the mobility estimates are picking up selection effects that are now wrongfully credited to the mother's schooling.

To see whether related adoptions are seriously affecting the maternal schooling estimates, it is useful to consider the impact of the schooling of stepmothers on the schooling of her children. Because the majority of all related adoptions in the U.S. involve stepparents adopting a stepchild (Stolley 1993), stepmothers and their children form a reasonable control group to test the impact of selection. 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 are three distinctive groups. On the basis of the latter classification, it is possible to construct a new sample of 403 children having stepmothers. With this sample of stepchildren, I would then expect to find higher maternal schooling effects than for the adoption sample, assuming that most of the 610 adoptees are unrelated to their family of rearing. In Table 3 column (1) the estimates attached to the mother's years of schooling and college education indicate that the effects of mother's schooling are indeed bigger for stepchildren than for adoptees. But since resulting differences are statistically insignificant, it is difficult to draw firm conclusions about the degree of selection.

Regardless, whether it is better parenting or selection that leads to biased estimates, they cannot be held responsible for the maternal absence observed in this adoption study. Instead, it is better to look for mechanisms that can possibly create a downward bias, to which I turn next.

The first candidate is measurement error. It is well known that random measurement error biases any estimated effect to zero. To let this be consistent with my findings, the amount of measurement error for women's schooling has to be bigger than that for men's schooling. In samples where respondents are

predominantly male, and through which information of the marriage partner is gathered, this seems a reasonable explanation. However, in this sample where the number of mothers and fathers who serve as primary respondents is about the same, it is not. In Table 3 column (2) previous schooling models are estimated using only adoptive mothers who served as primary respondents in the WLS survey. Maternal schooling estimates are close to zero. Hence, measurement error is not my biggest concern.

The second candidate is that adoptive mothers are different from other mothers in ways related to maternal schooling effects. Summary statistics in Table 1 already illustrated that mothers who adopt are better educated than mothers who raise their own birth children, who themselves are mostly high school graduates. I work with a particular sample where highly educated mothers are overrepresented. Perhaps this means that the adoptive mother's schooling variables exhibit too little variation, and that the absence of variation reflects the absence of her schooling effects. Or perhaps this means that mothers spend relatively more time working than raising children –more education usually raises labor market attachment– and that therefore their adopted children cannot benefit from their mother's attention.[13] It is possible to deal with this issue in part by focusing on own birth children who are brought up with adopted siblings. If the sample is limited to those own birth children who are raised by adoptive mothers and maternal schooling effects are estimated from this subsample, I indirectly isolate the unobservables that are typical to adoptive mothers and thus for the bias that it entails. If adoptive mothers are indeed different from other mothers, then absent maternal schooling effects should be observed. This is not the case. Table 3 column (3) reports positive and significant maternal schooling effects, identical to the effects observed for all own birth children. The likelihood ratio tests reveal that none of the estimated coefficients varies across own birth children with and without adopted siblings.

The third candidate is heterogeneity with respect to the age adoptees meet their adoptive families. Because this age is not recorded, it is possible that the share of adoptees who were brought into their adoptive families at a later age forces the estimated impact of mother's schooling to zero. If the mother's time is an important determinant in raising her children during the preschool years, and the impact of her schooling is most effective when children are young, some adoptees in my sample fail to receive these

maternal benefits because they are placed in their families at a later age. There are two problems related to this argument. First, a similar argument fails to explain the positive and statistically significant schooling effects of adoptive fathers. After all, it is mostly the mother's and not the father's time that is the primary input when raising young children. Second, this argument coincides with possible selection effects that operate in opposite direction. Many adoptions of older age involve related adoptions, which –when ignored– pushes the maternal schooling effect upwards. The bias could go either way. To explore this issue further, I consider realized adoptions before abortion was legalized and estimate the schooling models using adoptees born before 1970. Since unrelated adoptions of older children began only to emerge in the early 1970s the impact of the mother's schooling should then be estimated more accurately using a sample that is arguably more limited to unrelated adoptees who are U.S born and adopted at birth. Results are reported in Table 3 column (4). Because the maternal schooling estimates turn out to be practically identical to results obtained using all adoptees, adoptees who are adopted at a later age do not appear to be driving my results.

A fourth and related candidate is that children who are given up for adoption are different from other children in ways related to maternal schooling effects. Perhaps it is the case that foreign born and special needs adoptions attenuate maternal schooling effects. For reasons previously mentioned, this possibility is dismissed because the adoption of children born abroad and children with special needs started when most adoptees in the sample had already met their adoptive parents. Surely, it is possible that there are other unobserved characteristics that make adoptees different from other children. But I have little indication of what these might be or how to remove their corresponding bias. I do know, however, that in the empirical analysis the intercept captures the effect that adoptees are on average different from own birth children. Unless these differences correlate with mother's schooling, there will be no downward bias in the maternal schooling estimate.

The fifth and final candidate is a potential difference in upbringing. Anne Case, I-Fin Lin and Sara McLanahan (2000, 2001) put forward the Cinderella motive where mothers favor their own offspring because of some evolutionary drive to protect their genetic material. If this is the case and mothers do as a result invest relatively less in their adopted child, the estimated effect of mother's schooling on a sample

of adoptees proves not to be insightful. To get an idea how seriously differences in upbringing affect my outcomes, the educational outcomes of their adoptees with and without own birth siblings are compared. Cinderella motives predict that adoptees with own birth siblings would do relatively worse in school. Table 3 column (5) indicates that there are virtually no differences between the maternal schooling estimates among adoptees with and without own birth siblings.[14] Similarly, own birth children with adopted siblings would then do relatively better than own birth children without adopted siblings. In Table 3 column (2) any such difference is not observed. Hence, there is no evidence that mothers more heavily invest in their own birth children.

This is as far as the data allow me to go, but I think it is far enough to be confident about the results.

VI Concluding remarks

This paper examines the impact of parental schooling on the child's schooling and uses adoptees to get rid of persistency effects caused by the parents' genes. The results indicate that especially for mothers inherited abilities and assortative mating play an important role in the intergenerational transmission of schooling. In fact, for adoptees I find no treatment effect for the mother's schooling, conditional on her husband's schooling.

It should be noted, however, that the WLS data on adoptees and their parents do not posses the properties of a clean and well-defined experiment, and that obtained results require a careful interpretation. There are two potential dangers to an adoption experiment. First, adoptees and adoptive parents are different from other children and their parents. This argument suggests that my maternal schooling estimates may be biased and suffer from omitted variables, but I have little indication of what these might be. The sensitivity analysis ruled out a number of plausible candidates. Second, adoptees are not always randomly assigned to their adoptive parents. This argument suggests that a portion of what is interpreted as the impact of the parent's schooling may in fact be genetic. With respect to paternal schooling estimates there is some merit

to this view. However, with respect to the estimated maternal effect it is not. Non-random assignment and corresponding upward bias form no danger when interpreting the absence of maternal schooling effects.

In all, these results, in combination with the parallel findings of Behrman and Rosenzweig (2002) using twins, support the idea that the positive influence of mother's schooling on that of her child disappears when heritable abilities and assortative mating are taken into account.

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Notes

[1] "The human capital of the mother is usually more closely related to the attainment of the child than is that of the father." (Bob Haveman and Barbara Wolfe, 1995 p.1855)

[2]To remedy the impact of these unobservables Behrman and Rosenzweig (implicitly) assume inborn child-rearing endowments of fathers away and use different measures for the fathers' heritable endowments to subsequently test the sensitivity of their estimate of δ_1.

[3]In principle Γ can be negative if better educated mothers seek for more unfortunate children to adopt. There is some indirect evidence (Donald Unger, Penny Diener and Nancy Wilson 1988; James Rosenthal 1993) that mothers without fertility problems are more likely to adopt children with special needs (including older children, minority children, children with physical, mental or emotional deficiencies). In this paper's

sample the mechanism of special needs adoption by better educated mothers is essentially not observed and may therefore be discarded (see also Section III).

[4]For more information on the WLS data, I refer to William Sewell and Robert Hauser (1992) and Robert Hauser, Taissa Hauser, Deborah Carr and Jennifer Sheridan (1996).

[5]This particular period is considered because 95 percent of all adoptees in the sample are born between 1960 and 1980.

[6]The age limit of 23 years is selected in recognition of potential selectivity effects. If children who are too young to be graduated from their college education are not taken into account, young children with less education would be overrepresented in the sample.

[7]The estimations use all children, including all children raised in one family. With multiple observations of one family, standard errors are not independent within families and are biased downwards. I therefore estimate the model with clustered error terms to control for correlation within families and thus present robust standard errors in the tables.

[8]In an analysis not shown in this paper, the previous schooling models are estimated using all children (own birth children and adoptees) where all coefficients vary by adoption status. The interacted schooling effects were found to be statistically significant indicating that parental schooling estimates for own birth and adopted children are indeed statistically different. Bruce Sacerdote (2002) and Erik Plug and Wim Vijverberg (2003) focus on nature/nurture decompositions and interpret interacted adoption effects to measure the relative importance of inherited abilities.

[9]Also for parents who adopt this reduction closely corresponds to the correlation between their amounts of schooling (0.502).

[10]With college education, about 55 percent of the mother's schooling and 20 percent of the father's schooling is genetically transmitted.

[11]In a similar fashion Erik Plug and Wim Vijverberg (2002) analyze the WLS sample of adoptees to estimate the effect of family income on the child's schooling. In their analysis father's and mother's schooling are merely used to see whether family income effects are robust. This paper is different because of its explicit focus on the impact of the schooling of parents on that of their children.

[12]I am grateful to Bruce Sacerdote for running the latter specification –which was not included in his paper– especially for me.

[13]A similar point about more schooled mothers spending more time in the labor market is put forward by Behrman and Rosenzweig (2002) to explain their absent maternal schooling effects.

[14]For college education the likelihood ratio test points to a difference: it turns out that it is the schooling effect of fathers that differ statistically among adoptees with and without own birth siblings.

Table 1–Means and Standard Deviations of Selected Variables in WLS Sample

Sample	Own Birth Children		Adopted Children	
Education in years	13.577	*2.526*[a]	12.686	*2.691*
College graduate[b]	0.365	*0.481*	0.249	*0.433*
Still in school (censored)	0.238	*0.426*	0.391	*0.488*
Gender (daughter)	0.491	*0.500*	0.490	*0.500*
Age	26.638	*4.861*	23.955	*5.282*
Families with own birth and adopted children	0.031	*0.173*	0.557	*0.497*
Education in years, father	13.463	*2.615*	14.245	*2.842*
Education in years, mother	12.802	*1.671*	13.265	*1.934*
College graduate, father	0.262	*0.440*	0.355	*0.479*
College graduate, mother	0.144	*0.351*	0.219	*0.414*
Log family income measured in 1992	10.939	*0.712*	11.121	*0.629*
Number of observations	15871		610	

[a] Standard deviations in italics.

[b] Mean and standard deviations are calculated for respectively 12438 and 369 observations. For this variable children younger than 23 are not included.

Table 2–Estimates of the Effects of Mother's and Father's Schooling on Children's Schooling.

	Own birth children				Adopted children			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Estimates using Years of Education as Dependent Variable:							
Mother's years of education	0.538		0.296	0.276	0.276		0.104	0.089
	0.016[***a]		*0.017*[***]	*0.017*[***]	*0.063*[***]		*0.075*	*0.076*
Father's years of education		0.389	0.296	0.268		0.267	0.233	0.209
		0.009[***]	*0.011*[***]	*0.011*[***]		*0.037*[***]	*0.043*[***]	*0.044*[***]
Log family income in 1992				0.340				0.330
				0.035[***]				*0.188*[*]
Number of observations	15871	15871	15871	15871	610	610	610	610
	Estimates using College Education as Dependent Variable:							
Mother with college education	0.385		0.222	0.201	0.178		0.046	0.032
	0.015[***]		*0.019*[***]	*0.019*[***]	*0.063*[***]		*0.063*	*0.061*
Father with college education		0.382	0.320	0.285		0.290	0.271	0.234
		0.012[***]	*0.014*[***]	*0.014*[***]		*0.054*[***]	*0.059*[***]	*0.061*[***]
Log family income in 1992				0.094				0.088
				0.009[***]				*0.039*[**]
Number of observations	12438	12438	12438	12438	369	369	369	369

[a] Robust standard errors are in italics; * significant at 10% level ,** significant at 5% level, *** significant at 1% level.
All regressions include additional controls for the child's age and gender.

TABLE 3–ESTIMATES OF THE EFFECTS OF MOTHER'S SCHOOLING ON CHILDREN'S SCHOOLING USING VARIOUS SUBSAMPLES.

	(1) Stepchildren with Stepmothers	(2) Adoptees with Mothers who are Primary WLS Respondents	(3) Own Birth Children with Adopted Siblings	(4) Adoptees born before 1970	(5) Adoptees with Own Birth Siblings
	Estimates using Years of Education as Dependent Variable:				
Mother's years of education	0.154	-0.002	0.290	0.113	0.112
	0.086[a]	*0.117*	*0.074***	*0.064**	*0.093*
Father's years of education	0.162	0.301	0.280	0.184	0.184
	*0.058****	*0.061****	*0.053****	*0.039****	*0.053****
Number of observations	403	242	494	369	340
Likelihood ratio tests: Differences across adoptive subsamples[b]	7.61	6.45	4.73[c]	2.95	6.07
	0.179	*0.264*	*0.449*	*0.707*	*0.299*
	Estimates using College Education as Dependent Variable:				
Mother with college education	0.078	-0.054	0.111		0.023
	0.077	*0.077*	*0.080*		*0.076*
Father with college education	0.167	0.496	0.326		0.157
	*0.084***	*0.092****	*0.061****		*0.077***
Number of observations	334	151	337		212
Likelihood ratio tests: Differences across adoptive subsamples	2.00	10.89	7.18[c]		12.61
	0.849	*0.057**	*0.207*		*0.027***

[a] Robust standard errors are in italics; * significant at 10% level ,** significant at 5% level, *** significant at 1% level. All regressions include controls for the child's age and gender.

[b] Likelihood ratio tests indicate whether estimated coefficients vary across the subsamples of adoptees. Insignificant test statistics, reported with corresponding p-values, suggest the absence of structural differences.

[c] This likelihood ratio test indicates whether estimated coefficients vary across the subsamples of own birth children.