ISER Working Paper Series

Why Educated Mothers don't Make Educated Children?

A Statistical Study in the Intergenerational Transmission of Schooling

Chiara Pronzato
Institute for Social and Economic Research
University of Essex

No. 2008-11
March 2008

INSTITUTE FOR SOCIAL & ECONOMIC RESEARCH

E·S·R·C ECONOMIC & SOCIAL RESEARCH COUNCIL

www.iser.essex.ac.uk

Summary

More educated parents are observed to have better educated children. From a policy point of view, however, it is important to understand the reason. Are parents, because of more years of schooling, “better” in influencing children’s educational decisions? Or do children with better educated parents achieve better schooling results because of the inherited abilities?

When researchers have tried to answer this question, they have found conflicting results. In most of the cases, they have found a strong positive father’s effect with a negligible mother’s effect. In a few cases, a positive effect has been found for the mother and not for the father.

Samples used in this strand of research are usually small and not very representative of the population. The aim of this study is to explain to what extend the characteristics of the samples used can affect the results in this field of research.

Why Educated Mothers don't Make Educated Children?

A Statistical Study in the Intergenerational Transmission of Schooling

Chiara Pronzato

March 2008

ABSTRACT

More educated parents are observed to have better educated children. From a policy point of view, however, it is important to distinguish between causation and simple selection. Researchers trying to control for unobserved ability have found conflicting results: in most cases, they have found a strong positive paternal effect but a negligible maternal effect. In this paper, I evaluate the impact on the robustness of the estimates of the characteristics of the samples commonly used in this strand of research: samples of small size, with low variability in parental education, not randomly selected from the population.

JEL classification: I2
Keywords: intergenerational transmission, education, twin-estimator, sibling-estimator, power of the test

Acknowledgement:
Jean Monnet Action and the Norwegian Research Council have provided financial support for this project. I am grateful to Prof. John Ermisch for his supervision, to Prof. Ugo Colombino, to Prof. Rolf Aaberge, to Cheti Nicoletti and to Mark Taylor for their comments. Any error should be attributed to the author.

Contact:
Chiara Pronzato, Institute for Social and Economic Research, University of Essex, Colchester CO4 3SQ, UK. Tel 0044(0)1206873760. Email: chiara.pronzato@gmail.com

1. Introduction

Much research, in recent years, has focused on the link between parental education and children's education. The policy implications of a causal link between the two are huge, if the link between education and inequality remains strong over time. Increasing education today would lead to an increase in the schooling of the next generation and, in this way, a reduction in inequality among families.

It has been empirically shown that more educated parents have better educated children. Using simple regression analysis, the correlation between parent's and child's education is strong and robust to a number of controls, sample selections, and countries (Haveman and Wolfe, 1995). The coefficient of mother's education has been often found stronger than of the father's.

From a policy point of view, however, it is important to distinguish between causation and simple selection. Better educated parents have, on average, higher abilities, which partially transmit to their children. Better schooling performances of their children could, theoretically, reflect this higher average ability.

When researchers have tried to control for unobserved ability, they have found conflicting results. In most of the cases, they have found a strong positive father's effect with a negligible mother's effect. In a few cases, a positive effect has been found for the mother and not for the father. These results depend on different identification strategies, and on different sources of information. In part 2, I summarize techniques and results from previous studies, with particular attention to the work by Holmlund, Lindahl and Plug (2005). Their study uses different identification strategies with the same data source concluding that identification strategy matters.

The aim of this study is, indeed, to explain the difference between mother's and father's effects on children's education from a statistical point of view. Why is it that women's education does not appear to raise the education of their children? In this paper, I evaluate the impact of selecting samples of small size, with low variability in parental education, and not always representative of the population on the estimates of the intergenerational transmission of schooling.

In part 3, I apply one identification strategy, the twin estimator, using Norwegian register data, and reach the same conclusion as others: a strong positive effect of father's education and no effect of mother's education. Then I show through simulations (part 4) the extent to which small sample sizes and low variability in parental education is responsible for different results by gender, when using twins. In part 5, I examine the effect of using selected samples of parents (at the bottom and at the top of the educational distribution) when analysing the intergenerational transmission of schooling. Robustness checks (part 6) and conclusions follow (part 7).

2. The Intergenerational Transmission of Education in the Economic Literature

There are typically three strategies used in the literature to identify the effect of parental education on children's education: identical twins, adopted children, and using a reform of an educational system as an instrument for parental education.

Identical twins are the most similar individuals we can observe: they share the same family background, they experience lifetime events at the same time, and they share the same genes. When studying the intergenerational transmission of education, we usually compare the schooling of twins' children (i.e., cousins). These cousins share the ability transmitted by the twins. On the other hand, they are exposed to different treatments: they can be the children of the more educated twin, the other parent (twin's spouse) has different characteristics, and some of them can grow up in a single parent household. In such studies, we can identify the effect of parental education on children's education, looking whether the child of the more educated twin has higher educational attainment than the child of the less educated twin. The shortcoming of this strategy is the assumption of random education between twins: why do children with identical abilities end up with different levels of education? If there are some characteristics which make one twin gain more education than the other, and if these characteristics can be inherited by their children, then the resulting estimates are still biased. Despite this strong assumption, this method has been recognized as a good way to reduce ability bias. From the point of view of external validity, we may wonder whether twins are representative of the whole population: we know, for example, that they have lower weight at birth, they are more likely to have problems in language, and that they are brought up differently than other children (Schieve et al, 2002; Stewart, 2000; Mittler, 1971; Mowrer, 1954).

Another strategy used to eliminate, or at least reduce, the ability bias in the estimation of the intergenerational transmission of education is to use a sample of adopted children. In fact, there is no transmission of ability between adopted children and their adoptive parents. In this case, a relationship between parental and children's education should reveal a causal link between the two, while the comparison with estimates obtained from own-born children can be suggestive of the importance of the ability bias. The most common criticism of this strategy is that children are not randomly assigned: if adoption authorities have information on children's natural parents, they can use them to match children to adoptive couples. Another criticism derives from the fact that adoptee parents are not representative: they are, on average, older and better educated than the overall population. This could threaten the generalization of the results.

Finally, other studies have used a reform of the educational system as an instrument for parental education. For example, increasing the age of compulsory schooling lengthens the years of education exogenously. Exploiting this exogenous variation, we can observe whether the children of parents whose education has increased because of the reform achieve more in the education system than children of parents not influenced by the reform. While there is no risk of ability bias, the ability of this approach to identify the causal effect depends on the strength of the instrument in generating enough variability. From an external validity point of view, results are difficult to generalize since the reform of compulsory schooling usually involves only the bottom part of the education distribution.

There are a number of papers which use different strategies, data, outcomes and control variables. I focus on those with "years of schooling" as the outcome variable and which take into account the level of education of both parents simultaneously (assortative mating). These studies are most comparable to the work carried out in this paper. Since the aim is to explain variation in the statistical significance of results, I indicate a parameter significant at 10% level with 1 star, at 5% with 2 stars, at 1% with 3 stars.

The first work making use of twins in the study of the intergenerational transmission of education is published by Behrman and Rosenzweig (2002). They use a sample of monozygotic twins drawn from the Minnesota Twin Register. Information was obtained through a mail survey. They have 424 twin-mothers and 244 twin-fathers. They find a positive effect of father's schooling on children's years of schooling (0.340**) and a weak negative effect of mother's education (-0.263*). They suggest that this pattern is consistent with the fact that women's time is an important determinant of children's outcomes: the potential positive effect of mother's education is offset by the fact that more educated women spend more time in the labour market and less time with their children. Antonovics and Goldberger (2005) cast doubt on the construction of the dataset used by Behrman and Rosenzweig. After what they consider to be appropriate cleaning of the dataset, they have a sample of 90 twin-mothers and 47 twin-fathers. But the striking difference between genders remains: the effect of father's education is positive (0.477 – standard error not available) and mother's effect very close to zero (-0.003 – standard error not available).

To estimate the effect of mother's schooling on children's schooling, Plug (2004) controls for the effect of unobserved inherited abilities obtaining identification by using adopted children instead of twinning. He uses information collected in 1992 from 610 students who graduated from high schools in Wisconsin in 1957 and also finds a strong and positive effect for father's education (0.209***) and little effect of mother's education (0.089). He proposes different explanations for this result: some are substantial (better educated women spend less time with the children, differences in upbringing between own children and adopted children, adopted children different from other children in ways related to maternal schooling effects); some are more related to the design of the analysis (measurement error, heterogeneity with respect to the age adopted children enter their adoptive families, selection of highly educated mothers and consequently little variance in their education). Using Swedish adoption data, Bjorklund, Lindahl and Plug (2005) are not only able to remove the "family fixed effect" from the effect of interest but also able to distinguish between pre-birth factors (genetics and prenatal environment) and post-birth environmental factors. They exploit the fact that Swedish register data contains information for both biological and adoptive parents. They have information on both couples of parents for 2125 adopted children. The effect of adoptive father's education is positive (0.094***) while that of adoptive mother's is small and insignificant (0.021).

Black, Devereux and Salvanes (2005a) use a reform of compulsory schooling in Norway in the years 1947-1958 as an instrument for parental education. This reform resulted in 2 years more of schooling (from 7 years to 9 years). They use administrative data linked with the municipalities which implemented the reform, year by year. They find a significant effect of parental schooling only when selecting low educated parents (the ones most likely influenced by the reform): a positive effect of mother's education (0.122***) but a marginal effect of father's education (0.041).

These conflicting results obtained using various identification strategies and different datasets raises the question of what drives the differences. Are they data specific or do they depend on identification strategy? Holmlund, Lindahl and Plug (2005) use different identification strategies with the same source of data reaching the conclusion that it is identification matters. They select from Swedish register data parents born between 1945 and 1955, whose experience of the reform of compulsory schooling depended on the municipality of residence. They find a positive result for mother's education (0.150*) when selecting those with low education, while they do not find any significant corresponding result for fathers' education (-0.030). Using information from 192 Swedish children adopted by parents of the 1945-1955 birth-cohorts, they find a positive effect of father's education (0.130*) and no effect of mother's education (-0.022). Finally, they have 3850 twin-mothers and 2306 twin-fathers (only half of them monozygotic), from which they estimate a mother's effect equal to 0.034 and a father's effect equal to 0.164***.

The aim of this paper is to show to what extent samples of small size, with low variability in parental education, not randomly selected from the population can affect our estimates.

3. Replicated Results

The first step of my empirical research is to replicate the analyses in previous work (Behrman and Rosenzweig, 2002; Antonovics and Goldberger, 2005; Holmlund et al,

2005) in order to outline similarities and differences in the results and in the definition of the variables. The intergenerational schooling effect is estimated by using twins. The twin-estimator indicates whether the child of the more educated twin obtains more schooling qualifications, keeping under control the ability transmitted by the parent.

Let's call *Y* the child's years of schooling, *X* the parent's years of schooling, and *Z* other factors which may influence the child's education. For the child *i* in the family *j*, we have

$$y_{ji} = \beta x_{ji} + z'_{ji}\alpha + u_j + \varepsilon_{ji} \tag{1}$$

where β is the effect of parental education on the child's years of schooling, α the effect of other factors, *u* is the level of ability shared in the family *j*, and ε is the error term assumed to be white noise. A pooled regression of *Y* on *X* and *Z* (cross section) is not appropriate since it ignores the ability *u* shared in each family, which may be correlated with parental education. We can eliminate *u* from the equation, differencing the data in the following way

$$(y_{ji} - \bar{y}_{ji}) = \beta(x_{ji} - \bar{x}_{ji}) + (z_{ji} - \bar{z}_{ji})'\alpha + (\varepsilon_{ji} - \bar{\varepsilon}_{ji}) \qquad (2).$$

The above regression corresponds to the twin-estimator in this empirical application.

The informational basis for the empirical analysis is a register household panel data set covering the entire resident population of Norway for the years 1993-2001. In this dataset, I have information on the household size and composition as well as individual information such as place of residence, date of birth, educational level, etc.

Twins are defined as people of the same sex, born from the same parents, in the same calendar year and month. Around 50% are likely to be identical twins (monozygotic), while the other 50% are like other siblings (dizygotic), but unfortunately I cannot distinguish them[1]. In order to be included in the sample, both twins must have a partner and have at least one child aged over 22 in 2001[2].

Level of education	Duration	Years of schooling
1	3	3
2	6	9
3	2	11
4[3]	1	12
5	1	13
6	4	16
7	2	18
8	3	21

Table 1: level of education and duration in years in the Norwegian educational system (source: Norwegian Standard Classification of Education, Revised 2000)

Education variables are measured in 2001 for children and in 1993 for parents. The levels of education are transformed into years of schooling, according to the maximum level of education obtained, and following the indications in table 1.

	Mother-twins		Father-twins	
	Mean	Std dev	mean	standard deviation
Child's schooling	12.91	2.31	12.97	2.24
Twin's schooling	10.71	1.99	11.36	2.43
Partner's schooling	11.53	2.59	10.91	2.01
Partner's earnings	2.23	1.71	1.12	0.78
Woman	0.48		0.48	
First child	0.50		0.49	
Child's age	30.4	6.5	30.7	6.4
Twin's age	55.1	7.4	58.2	8.2
Observations	1983 (500 families)		1996 (503 families)	

Table 2A: descriptive statistics of the two samples of twins

[1] Holmlund, Lindahl and Plug (2005) have also the same limit in the Swedish data.

[2] Holmlund, Lindahl and Plug (2005) selected children aged over 22, Antonovics and Goldberger (2005) from 18 years old, Behrman and Rosenzweig (2002) include also children younger than 18.

[3] After the upper secondary school (level 4), students can decide to go to the University (level 6) or to complete their education with another year of study (level 5) which cannot be classified neither as secondary nor tertiary education. Obviously, after level 5 they can still have access to level 6 (Norwegian Standard Classification of Education, Revised 2000).

In table 2A I present the descriptives statistics and in tables 2B and 2C the parameter estimates. Among the covariates, I include the partner's schooling and earnings (measured in 1993) to control for assortative mating, gender and age of the child, and a dummy indicating the first child compared to the subsequent ones (Black, Devereux and Salvanes, 2005b)[4]. Including the variable age helps to control for performance of students too young to reach the highest levels of education[5].

The descriptives in table 2A show that twin-fathers are, on average, better educated than twin-mothers; twin-mothers' husbands are also better educated and earn more than twin-fathers' wives, as expected. The average age of children is between 30 and 31, while twin-mothers are younger than twin-fathers (55 compared to 58), due to the fact that women are on average younger than men at the birth of their children. We have around 2000 observations for both samples, which corresponds to 500 pairs of twins. On average, the twins have two children older than 22 years old.

	Cross section		Twin-estimator	
	Beta	St err	Beta	St err
Twins' schooling	0.193***	0.029	0.053	0.051
Partner's schooling	0.192***	0.023	0.072**	0.034
Partner's earnings	0.029	0.032	0.016	0.047
Woman	0.358***	0.097	0.370***	0.103
Age	0.003	0.008	-0.014	0.019
First child	0.222**	0.098	0.258**	0.115
Constant	8.201***	0.433	11.594***	0.858
Observations	1983 (500 families)			

Table 2B: effect of mother's education on children's years of schooling

In table 2B I present results for twin-mothers, while in table 2C for twin-fathers. I perform cross section analyses, and then I use the twin-estimator to control for transmitted parental ability. Despite differences in the selection of the samples, results are consistent with the rest of the literature. In the cross section estimations, all the

[4] Behrman and Rosenzweig (2002) include controls for partner's schooling and earnings. Holmlund, Lindahl and Plug (2005) also include gender of the child.

[5] De Hann and Plug (2007) propose and compare different methodologies to treat these censored observations, and conclude that using parental expectations if they were realizations seems to deal better with censoring problems. Register data do not provide this kind of information.

results are in the expected direction: we find a positive effect of parental education, being a woman, and being the first child in the family. In the twin-estimations, mothers' education loses its significance, while the impact of father's schooling is still significantly different from zero. Similar to the research cited above, the effect of mother's education surprisingly does not differ significantly from zero.

	Cross section		Twin-estimator	
	Beta	St err	Beta	St err
Twins' schooling	0.244***	0.022	0.192***	0.039
Partner's schooling	0.110***	0.028	0.044	0.039
Partner's earnings	0.341***	0.066	0.257***	0.098
Woman	0.400***	0.093	0.488***	0.098
Age	0.016**	0.008	-0.039**	0.016
First child	0.243***	0.093	0.483***	0.109
Constant	7.801***	0.417	10.741***	0.752
Observations	1996 (503 families)			

Table 2C: effect of father's education on children's years of schooling

How much information can the twin-estimator exploit? In table 2D, I report the total variance, the within variance, and the percentage of twins with the same level of schooling. In the twin-fathers' sample around 50% of twins have the same level of education, while around 60% in the twin-mothers' sample. This implies that the twin-estimator only exploits information from less than half of the original samples. To what extent small sample size can affect the estimates? We then observe that the twin-mothers' sample shows a lower total and within variability. Can the lower level of female variability contribute to the explanation of different results between genders?

	Total variance	Within variance	Same number of years of schooling
Twin-mothers	3.97	0.74	59.8 %
Twin-fathers	5.92	1.18	49.5 %

Table 2D: variance of parental education in the samples of twins

4. Small Sample Size and Low Variability in Parental Education

The aim of this part is to understand how small sample sizes and low variability in parental education, typical of twins' samples, affect the power of the twin- estimator. The idea is to work through simulations. I would like to have a "large perfect population": "perfect" means without problems of ability bias, "large" means large enough to draw samples from it. Then I could estimate the "true" effect of parental education on children's education. Suppose that there were an effect. Then, I could draw small samples, estimate the effect, and count how many times I would reject the null hypothesis. This would result in the power of the test.

We do not know the true effect, and we do not have a population for estimating it. But we can pretend that the true effect is the one obtained using a sample of siblings. The sibling-estimator has been often used for the same purpose as the twin-estimator: to try to clean the effect of interest of the idiosyncratic characteristics of the family (Aaronson, 1998; Altonji and Dunn, 1996; Neumark and Korenman, 1994; Behrman and Wolfe, 1989; Ashenfelter and Zimmermann, 1997). Using siblings instead of twins has some advantages but also evident shortcomings. On the one hand, they have the same family background, experience similar lifetime events (but at different times), are more representative of the population, and can provide larger samples and more precise estimations. On the other hand, they do not share the same genes and the results obtained are then potentially biased. The estimated effect we get from siblings is likely to be overestimated, given that ability is not controlled for as efficiently as for twins. Even if I draw samples with the same number of observations as my original twins' sample, we are more likely to reject the null hypothesis. We need to consider the power of the test as an upper limit.

Before proceeding with simulations, I show the results using the sibling-estimator, and using siblings whose age difference is, at most, 24/36/48/60 months. I also show results of the sibling-estimator when I include in the sample all siblings (regardless the difference of age between them). Finally, I consider the results obtained from all children (cross-sectional analysis) that can be suggestive of the importance of the ability bias. The estimated effects are reported in tables 3A, 3B and 3C.

In table 3A, we can observe the estimated effects of mothers' schooling. In the first line, I report the result obtained with the twin-estimator. When I use the sibling-estimators, the effect is around 0.105, slightly larger when I consider all siblings, regardless of their age. The effect estimated for all children is substantially larger, showing the importance of the "family effect" in this context.

Relationship	Beta	Std error	Families	Observations
Twins	0.053	0.051	500	1 983
Siblings (24 m)	0.103***	0.014	5 294	21 749
Siblings (36 m)	0.110***	0.010	9 577	40 138
Siblings (48 m)	0.105***	0.008	13 059	55 159
Siblings (60 m)	0.103***	0.008	15 723	66 774
All siblings	0.115***	0.006	23 127	100 621
All children	0.222***	0.002	-	431 848

Table 3A: effect of mother's education on children's years of schooling, using different samples (controlling for partner's education and earnings; child's age, gender and first-born child). The numbers of months, in brackets, indicate the maximum distance between the births of the pair of siblings.

In table 3B, we can observe the effects estimated of father's schooling. Surprisingly, the effect obtained with the twin-estimator is larger in size than the one obtained with the sibling-estimator. The larger estimate in the case of twins may be the consequence of the small sample size. When I use the sibling-estimators, the effect is around 0.125. The cross sectional effect is still larger, as it was for mothers.

Relationship	Beta	Std error	Families	Observations
Twins	0.192***	0.039	503	1 996
Siblings (24 m)	0.130***	0.010	5 298	25 576
Siblings (36 m)	0.124***	0.008	10 729	46 819
Siblings (48 m)	0.125***	0.006	14 711	64 659
Siblings (60 m)	0.126***	0.006	17 785	78 517
All siblings	0.126***	0.005	26 910	121 512
All children	0.211***	0.001	-	476 463

Table 3B: effect of father's education on children's years of schooling, using different samples (controlling for partner's education and earnings; child's age, gender and first-born child). The numbers of months, in brackets, indicate the maximum distance between the births of the pair of siblings.

There are two interesting facts, when comparing results across mothers and fathers. First, the mothers' effect, on average, seems statistically smaller than fathers' effect. Second, in the mother's case, the bias seems to be more important. We go from 0.103-0.115 (sibling-estimators) to 0.222 (cross section) for mothers, while from 0.124-0.130 (sibling-estimators) to 0.211 (cross section) for fathers. The fact that ability is transmitted more between mothers and children than between fathers and children can be explained by the fact that mothers traditionally spend more time with their children[6].

	Mothers			Fathers		
Relationship	Total variance	Within variance	Same number of years of schooling	Total variance	Within variance	Same number of years of schooling
Twins	3.97	0.74	59.8	5.95	1.18	49.5
Siblings(24 m)	4.15	1.04	50.9	6.11	1.55	39.4
Siblings (36 m)	4.26	1.09	49.5	6.20	1.58	38.1
Siblings (48 m)	4.24	1.11	48.9	6.29	1.65	37.2
Siblings (60 m)	4.19	1.11	48.5	6.26	1.66	37.1
All siblings	3.99	1.10	48.2	6.05	1.71	36.4
All children	4.34	-	-	6.39	-	-

Table 3C: variance of parental education, using different samples. The numbers of months, in brackets, indicate the maximum distance between the births of the pair of siblings.

Finally, I show the variability in the samples (table 3C). All variability measures are increased when considering siblings instead of twins, as expected. Mothers' education has less variability than fathers' education in all samples.

For the simulations, I choose as my "large perfect population" the sample of all siblings. Using this, the "true" effect of mother's schooling is 0.115 and the "true" effect of father's schooling is 0.126. We need to keep in mind that these estimates set upper limits, especially in the mothers' case since the ability bias seems to be more of an issue.

I first concentrate on the size of the twins' samples: I have 500 pairs of female twins, and 503 of male twins. I draw 1000 samples of these sizes from all female siblings and all male siblings, and I estimate the effect of parental education. Since the passage from 25000 observations (pairs of siblings) to 500 (pairs of twins) is very big, I also consider an intermediate step with a sample size of around 1000 pairs of siblings. In table 4A and 4B, I summarize the results, obtained with samples of around 1000 and 500 pairs of siblings, for mothers and fathers respectively.

[6] Bjorklund, Lindahl and Plug (2006) also find that that mother's pre-birth factors contribute more to the intergenerational transmission of education than father's pre-birth factors.

In the first lines I show the mean of the estimated effect and the mean of its standard error. Then the percentage of times the null hypothesis has been rejected when the error of the first type (or alpha) is set at 10%, 5%, and 1%[7]. In the last rows, some characteristics of the drawn samples are shown: number of pairs of siblings, number of siblings, total and within variance of parental education, and percentage of parents with the same years of schooling. If we look at the power of the tests, we can see the effect of small sample size on the probability of finding a significant effect of parental education. With 1000 couples of siblings (table 4A), we observe that the power of the test is approximately 1 when estimating the effect of father's education, while somewhat lower when estimating the effect of mother's education. With only 500 couples of siblings (table 4B), which corresponds to my actual twins' sample size, distances in the power of the test between father and mother's education effect widen: at any level of significance, we are about 20 percentage points more likely to reject the null hypothesis of zero effect for fathers than for mothers.

	Mothers		Fathers	
	Mean	Std dev	Mean	Std dev
Beta	0.111	0.033	0.126	0.027
Std error	0.030	0.001	0.024	0.001
Power of the test (alpha = 0.10)	0.966		0.999	
Power of the test (alpha = 0.05)	0.937		0.999	
Power of the test (alpha = 0.01)	0.851		0.993	
Total variance	3.99	0.2	6.07	0.27
Within variance	1.10	0.07	1.71	0.10
Same schooling	48.3		36.3	
Observations	4351	62	4542	62

Table 4A: mean of the effect of parental education on children's years of schooling (controlling for partner's education and earnings; child's age, gender and order of birth), power of test and variance of parental education, extracting 1000 families (1006 for fathers) from the sample of all siblings (1000 extractions)

[7] Alpha or error of the first type error is the probability of rejecting the null hypothesis when the null hypothesis is true.

	Mothers		Fathers	
	Mean	Std dev	Mean	Std dev
Beta	0.115	0.05	0.124	0.039
Std error	0.043	0.002	0.034	0.001
Power of the test (alpha = 0.10)	0.806		0.962	
Power of the test (alpha = 0.05)	0.740		0.935	
Power of the test (alpha = 0.01)	0.545		0.832	
Total variance	3.97	0.28	6.06	0.38
Within variance	1.10	0.10	1.71	0.14
Same schooling	48.3		36.2	
Observations	2178	43	2273	44

Table 4B: mean of the effect of parental education on children's years of schooling (controlling for partner's education and earnings; child's age, gender and order of birth), power of test and variance of parental education, extracting 500 families (503 for fathers) from the sample of all siblings (1000 extractions)

However, the samples used for the simulations, when are randomly selected, have more within variability than the actual twins' sample. Also the percentage of siblings with the same years of schooling is lower. The percentage of twin-mothers equally educated is 59.8 while the percentage of sibling-mothers equally educated is 48.3 (within variance from 0.74 to 1.11). The same for fathers: the percentage of equally educated falls from 49.5 (twins) to 36.2 (siblings) while the within variance from 1.18 (twins) to 1.71 (siblings). This is the obvious consequence of working with less similar individuals. But the within variability can be very important for my measure of the power of the estimator. In order to reduce the within variability, I first calculate the variance in parental education within each family of twins and siblings. Then, I match every twins' family with variance *v* with one siblings' family randomly selected among the ones with same variance *v*. Finally, I drop all information about twins and I end up with a sample of siblings' families with same number of families as in my original twins' sample and with the same within variability. Results for mothers and father are reported in the first two columns of table 5. The power of the test is generally lower, especially for mothers and for lower level of alpha. The distances between fathers and mother have also expanded. Given a positive effect of parental education, we are likely 8 times out of 10 to find a result significantly different from zero for fathers, and 6 times out of 10 for mothers (with alpha equal to 0.05).

	Mothers		Fathers		Mothers with fathers' variability	
	Mean	Std dev	Mean	Std dev	Mean	Std dev
Beta	0.113	0.062	0.12	0.048	0.109	0.052
Std error	0.052	0.001	0.041	0.001	0.044	0.001
Power of the test (alpha = 0.10)	0.672		0.870		0.773	
Power of the test (alpha = 0.05)	0.571		0.802		0.674	
Power of the test (alpha = 0.01)	0.367		0.596		0.481	
Total variance	3.65	0.25	5.51	0.37	4.17	0.23
Within variance	0.73	0.02	1.12	0.03	1.09	0.03
Same schooling	59.8		49.5		49.6	
Observations	2125	42	2203	40	2115	40

Table 5: mean of the effect of parental education on children's years of schooling (controlling for partner's education and earnings; child's age, gender and order of birth), power of test and variance of parental education, extracting "not randomly" 500 families (503 for fathers) from the sample of all siblings (1000 extractions)

Finally, in the final group of simulations (third column of table 5), I look at the power of the test using the sibling-mothers but with the twin-fathers' variability. I match every twin-father's family with variance *v* with one sibling-mother's family randomly selected among the ones with same variance *v*. The idea is to measure the power of the test when we estimate the mother's effect, if we could have the same "quality" of information we have for fathers. The probability of rejecting the null hypothesis has increased, but not to the same level as fathers: for example, with alpha equal to 0.05, the power would go from 0.571 to 0.674 for mothers, while it is 0.802 for fathers.

For all these simulations, we need to remember that results are to be considered as upper limits, since ability is not taken into account in the same way as for twins. To what extent can these results be generalized? It is difficult to say. Previous studies have in common with my study a lower variation in maternal than in paternal education but, in general, they seem to have larger within variability than my Norwegian sample[8]. My empirical exercises can provide hints for investigation with other sources of data. And it can suggest caution when comparing results for different sub-groups of the population, which can imply strong policy recommendations.

From a statistical point of view, we have observed that small sample size and low within variability have an impact on the probability of rejecting the hypothesis that the

[8] For example, Holmlund, Lindahl and Plug (2005) have a sample of twins where 61% of twin-pairs have different level of education.

parameter of interest is zero. This is particularly true for mothers: within variability is particularly low for women, and seems to be the consequence of schooling decisions of previous generations. Sisters seem more likely to study the same number of years, and also more likely to study less than brothers, which reduces variability too.

From a substantial point of view, however, we have evidence of a smaller effect of mother's education. In the existing literature, a positive and significant effect of mother's schooling has been found when using the reform of compulsory schooling as an instrument, and selecting less educated women (Black, Devereux and Salvanes, 2005a; Holmlund, Lindahl and Plug, 2005). To explore this issue in more detail, in the next paragraph, I examine the impact of one additive year of education at the bottom and at the top of the educational distribution.

5. Use of Not Random Samples of Parents

In order to study heterogeneous effects of an increase in schooling along the educational distribution, I divide the samples of siblings in two parts: one where both siblings have fewer than 11 years of education (bottom part) and another where they both have 12 years or more (top part). Table 6A summarizes the results.

	Both siblings <= 11 years of schooling		Both siblings >= 12 years of schooling	
	Mothers	Fathers	Mothers	Fathers
Beta	0.163***	0.153***	0.053***	0.114***
Std error	0.012	0.013	0.019	0.011
Families	16 608	13 609	2 364	5 465
Observations	75 424	65 636	8 490	20 481

Table 6A: heterogeneous effects of parental education on children's years of schooling in the bottom and top part of the distribution (controlling for partner's education and earnings; child's age, gender and first-born child)

The effect of an increase in education for low educated parents on children's schooling is strong and positive, for both fathers and mothers. Instead, when we look at the increase of education of highly educated parents, we observe a smaller effect, especially for mothers.

Using the same simulation setting, we measure the power of the test in the case we have a small sample (500 families, around 2000 children).

	Both siblings <= 11 years of schooling			
	Mothers		Fathers	
	Mean	Std dev	Mean	Std dev
Beta	0.164	0.083	0.151	0.079
Std error	0.072	0.003	0.067	0.003
Power of the test (alpha = 0.10)	0.702		0.701	
Power of the test (alpha = 0.05)	0.604		0.600	
Power of the test (alpha = 0.01)	0.404		0.384	
Total variance	1.00	0.01	0.99	0.01
Within variance	0.34	0.02	0.35	0.02
Same schooling	63.5		62.3	
Observations	2269	43	2426	48

Table 6B: mean of the effect of parental education on children's years of schooling (controlling for partner's education and earnings; child's age, gender and first-born child), power of test and variance of parental education, extracting 500 families (503 for fathers) from the sample of all siblings (1000 extractions)

The bottom part of the education distribution has the same small amount of variation for mothers and fathers (table 6B), which may explain the low power of the estimator in both cases. The power of the estimator is extremely low for the effect of an increase in education among highly educated mothers, assuming that there is any effect (table 6C). On the other hand, we are very likely to get a significant result when looking at highly educated fathers (table 6C).

How can we interpret these results? First, they seem to confirm (or not contradict) those obtained using different strategies than twins. Studies which make use of compulsory schooling reform as an instrument (Black, Devereux and Salvanes, 2005a; Holmlund, Lindahl and Plug, 2005) have found a positive effect of education only for low educated mothers. Here, using the sibling-estimator, we observe a positive effect for mothers and fathers, but not likely to be significant[9]. The probability of observing any result significantly different from zero may depend on the variability of parental education, which relies on the strength of the reform in generating enough variability, and may

[9] Studies which make use of the compulsory schooling reform have larger samples (around 10/20 thousand observations).

differ between women and men[10]. Moreover, the reform not only increases the years of schooling exogenously, but also may change assortative mating within couples: men involved in the reform are more likely to marry younger (and better educated) women, while women involved in the reform are more likely to marry older (and less educated) men. If the interaction between the levels of education of the two spouses, which I am not taking into account, is important, we may think to be likely to observe a stronger effect of the mother than of the father[11].

	Both siblings >= 12 years of schooling			
	Mothers		Fathers	
	Mean	Std dev	Mean	Std dev
Beta	0.054	0.041	0.115	0.039
Std error	0.042	0.001	0.037	0.001
Power of the test (alpha = 0.10)	0.340		0.918	
Power of the test (alpha = 0.05)	0.243		0.867	
Power of the test (alpha = 0.01)	0.091		0.695	
Total variance	3.79	0.09	6.14	0.19
Within variance	1.54	0.08	1.85	0.12
Same schooling	45.9		42.9	
Observations	1796	26	1918	31

Table 6C: mean of the effect of parental education on children's years of schooling (controlling for partner's education and earnings; child's age, gender and first-born child), power of test and variance of parental education, extracting 500 families (503 for fathers) from the sample of all siblings (1000 extractions)

When we consider studies which make use of adoption, we are looking at the effect of an increase in education for highly educated parents. I find a small effect of mother's education (and very low likelihood to observe it significant) and a positive effect of father's education (very likely to be observed significant). Adoption studies (Holmlund,

[10] This intuition is not confirmed by the data: Black, Deveurex and Salvanes (2005a) show in the first stage results that the reform has affected more men's schooling (0.795***) than women's schooling (0.749***); Holmlund, Lindahl and Plug (2005) have similar qualitatively results (0.349*** for men and 0.277*** for women).

[11] In 80% of my sample of siblings, the man is older than the woman, and the average difference is 4 years. If we assume that the reform has not completely cancelled the difference in age between men and women, we can expect the sample of fathers involved in the reform married with women who went to school when the reform was fully implemented. If I consider low educated parents only, the effect of father's education when his spouse's education is 11 is equal to 0.105*** while the effect of father's education when his spouse's education is 9 is equal to 0.211***. On the other hand, the effect of mother's education when her spouse's education is 9 is equal to 0.148*** while the effect of mother's education when her spouse's education is 11 is equal to 0.125***.

Lindahl and Plug, 2005; Plug, 2004; Bjorklund, Lindahl and Plug, 2005) have found a positive effect of father's schooling, and no effect of mother's.

The part of the educational distribution involved in any identification strategy seems to be a very important aspect to take into account.

One question, at least, is still to be answered: why does increasing education of highly educated women not increase the schooling of the next generation? Are these women working more and spending less time with their children? Or does assortative mating play any role? This calls for further investigation.

	Low educated mothers		High educated mothers	
	no	yes	no	yes
Beta	0.189***	0.163***	0.096***	0.053***
Std error	0.012	0.012	0.019	0.019

Table 7: effects of mother's education on children's years of schooling in the bottom and top part of the distribution with and without controlling for partner's education (always controlling for partner's earnings; child's age, gender and order of birth)

In table 7, I concentrate on the mothers, both low and high educated, before and after the inclusion of the variable indicating their spouse's schooling. Using the sample of siblings, we find that the effect of an increase in schooling, for highly educated mothers, decreases dramatically when controlling for partner's years of schooling, while this seems not to be the case when considering the bottom part of the distribution.

6. Robustness Checks

In the data, in addition to the maximum level of education, there are two other pieces of information that could be used to define the years of schooling variables: the age at which education was completed and the level of education currently attended[12].

I now make use of these variables to examine the robustness of my previous results. Having attributed the years of schooling corresponding to the maximum level obtained, I

[12] They have not been used to have a definition of the variables closer to that used by Holmlund, Lindahl and Plug (2005).

allow for the possibility that a person stops her/his education between two levels. For example, a person states 4 as maximum level, which would imply s/he has studied until the age of 19. The correspondent years of schooling would be 12. If the age of schooling completion in the data is 21, I update her years of schooling to 14. See at table 8 to have a comprehensive definition of the variable. This procedure of adjustment has been applied to children with completed education before the first year available of the panel (1993). For children still enrolled in school along the panel (1993-2001), I can observe whether they attended any further year of schooling compared to the maximum qualification of education stated[13].

I report results obtained using the twin-estimator, and using the sibling-estimator for low and high educated parents; and the respective simulations to measure the power of the estimators. In parenthesis, I will show the corresponding result with the "old" definition of the variables, in order to make the comparison easier.

Level of education	Correspondent years of schooling	Theoretical age -end	Actual ending age between	Years of schooling
1	3	10	11-15	4-9
2	9	16	17	10
3	11	18	-	-
4	12	19	20-22	13-15
5	13	20	21-23	14-16
6	16	23	24	17
7	18	25	26-27	19-20
8	21	28	-	-

Table 8: strategy to define the number of years of schooling in the middle of two consecutive levels

When estimating with samples of twins, I obtain 0.085 (0.053) for mothers and 0.209*** (0.192***) for fathers. The probability to reject the null hypothesis, with alpha set to 0.05, is 0.699 (0.571) for mothers and 0.893 (0.802) for fathers. If mothers had the same variability in their education as fathers, the power of the test would increase to 0.813 (0.674).

When analyzing low educated and high educated parents separately, I find that the effect is 0.187*** (0.163***) for low educated mothers and 0.174*** (0.153***) for low

[13] Suppose the maximum qualification stated in 2001is the fourth level (secondary school, 12 years of schooling). I can observe whether they attended University any year between 1993 and 2001, even without obtaining a degree.

educated fathers. The respective power of the test are 0.643 (0.604) and 0.661 (0.600) with alpha set to 0.05. The effect of high educated mothers' education is 0.087*** (0.053***) while the effect of high educated fathers' education is 0.116*** (0.114***). The respective power of the test are 0.540 (0.243) and 0.894 (0.867) with alpha set to 0.05.

The power of the estimators is larger than in previous analyses because of the increased variability in both parental and children's education. However, distances between mothers and fathers' effects and power of the tests have not changed.

7. Conclusions

The aim of the paper is to look at the effect of parental education on children's schooling, with particular attention to the difference between the mother's and the father's effects from a statistical point of view. In this paper, using the sibling-estimator, my results confirm, or at lest do not contradict, the previous ones which were obtained with different identification strategies and different sources of data. The part of the educational distribution involved in any identification strategy seems to be a very important aspect to take into account. I still find evidence of a smaller effect of the mother compared to the father, but only at the top of the educational distribution. This is important from a policy point of view: in contexts where the average level of schooling is low, increasing women's education may have beneficial effects for children's schooling. What remains unexplained is why mother's effect is smaller for highly educated mothers than for highly educated fathers. Are these women working more and spending less time with their children? Or does assortative mating play any role? Further research is needed

Another contribution of this paper is to assess to what extent small sample sizes and low variation in parental education affects the power of the implemented estimator. Assuming an effect for both mothers and fathers, I demonstrate that we are more likely to observe it for fathers than for mothers. This suggests caution when comparing results for different sub-groups of the population, which can imply strong policy recommendations.

Another involuntary contribution is to provide the opportunity to compare results obtained employing the twin-estimator and the sibling-estimator. Despite the shortcomings in using siblings instead of twins, estimates show more precision and stability.

References

Aaronson D. (1998), *Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes*, The Journal of Human Resources, 33: 915-946.

Altonji J. G., Dunn T. A. (1996), *Using Siblings to Estimate the Effect of School Quality on Wages*, The Review of Economics and Statistics, 78: 665-671.

Antonovics K. L., Goldberger A. S. (2005), *Does Increasing Women's Schooling Raise the Schooling of the Next Generation? Comment*, The American Economic Review, 95: 1738-1744.

Ashenfelter O., Zimmerman D. J. (1997), *Estimates of the Returns to Schooling from Sibling Data: Fathers, Sons, and Brothers*, The Review of Economics and Statistics, 79: 1-9.

Behrman J. R., Rosenzweig M. R. (2002), *Does Increasing Women's Schooling Raise the Schooling of the Next Generation?* , The American Economic Review, 92: 323-334.

Behrman J. R., Wolfe B. L. (1989), *Does More Schooling Make Women Better Nourished and Healthier?* , The Journal of Human Resources, 24: 644-663.

Bjorklund A., Lindahl M., Plug E. (2006), *The Origins of Intergenerational Associations: Lessons from Swedish Data*, The Quarterly Journal of Economics 121: 999-1028.

Black S. E., Devereux P. J., and Salvanes K. J. (2005 a), *Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital*, The American Economic Review, 95: 437-449.

Black S. E., Devereux P. J., Salvanes K. J. (2005 b), *The More The Merrier? The Effect of Family Size and Birth Order on Children's Education*, The Quarterly Journal of Economics 120: 669-700.

De Hann M., Plug E. (2006), *Estimates of the Effect of Parent's Schooling on Children's Schooling Using Censored and Uncensored Samples*, IZA Discussion Paper series.

Haveman R., Wolfe B. (1995), *The Determinants of Children's Attainments,* Journal of Economic Literature, 33: 1829-1878.

Holmlund H., Lindahl M., Plug E. (2005), *Estimating Intergenerational Schooling Effect: A Comparison of Methods*, mimeo.

Mittler P. (1972), *The Study of Twins*, Baltimore: Penguin Books.

Mowrer E. R. (1954), *Some Factors in the Affectional Adjustment of Twins*, American Sociological Review, 19: 468-471.

Neumark D., Korenman S. (1994), *Sources of Bias in Women's Wage Equations: Results Using Sibling Data*, The Journal of Human Resources, 29: 379-405.

Plug E. (2004), *Estimating the Effect of Mother's Schooling on Children's Education Using a Sample of Adoptees*, The American Economic Review, 94: 358-368.

Schieve LA et al. (2002), *Low and Very Low Birth Weight in Infants Conceived with Use of Assisted Reproductive Technology*, New England Journal of Medicine, 346: 731-737.

Stewart E. A. (2000), *Exploring Twins: Towards a Social Analysis of Twinship*, McMillan Press, UK.