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The effect of schooling on earnings: The role of family background studied by a large sample of Norwegian twins

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July 1999

The effect of schooling on earnings: The role of family background studied by a large sample of Norwegian twins*

by

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Abstract

A large representative sample of twins is used to estimate the causal effect of schooling on earnings in Norway, for both men and women. The within-twin-pair estimates reveal that standard cross section (OLS) estimates of the effect on *male hourly earnings* are biased upwards. For women, no such bias is found. However, family background seems to affect both educational attainment and post-schooling labour supply of women, implying that the standard estimate of the effect of schooling on *female annual earnings* is upward biased.

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1. Introduction

Numerous studies confirm that better-educated people are more successful in the labour market. This paper is about the interpretation of this positive correlation between individual labour market success and educational attainment. Is labour market success *caused* by schooling or do innately successful individuals acquire more education? Family background is one of the most important determinants of the labour market success. It is also widely accepted that family background variables like parental schooling and earnings are important determinants of educational attainment, see e.g. Nordli Hansen (1997) for recent Norwegian evidence.

The aim of this paper is *not to describe how* various family background characteristics affect schooling and earnings. Focus is rather to explore the extent to which the cross sectional relationship between schooling and earnings in the population is due to (unobserved) family characteristics that influence both educational attainment and post-schooling earnings. Our study of this "ability bias" is based on a large, representative sample of Norwegian twins. While twin samples usually come from surveys based on Medical Birth registers, or even twin assemblies, our data are established by creative matching of several official Norwegian registers. These registers cover the whole Norwegian population aged 16-69, including day of birth and a link to their parents. Twins are simply defined as individuals born at the same, or the next/previous, day by the same mother. Our twin pairs are therefore biological twins, not necessarily reared together, and we have no information about zygosity ("identical" or "fraternal" twins) at the individual level. We are able to compare twins with non-twins when data are generated *by the same process for both groups*. The large sample allows us to estimate returns to schooling for women and men separately.

The twin approach is based on the "Pure Family Effects" (PFE) assumption which claims that the (often limited) variation in educational attainment within equally sexed twin pairs are caused by factors without direct influence on post-schooling labour market performance. Given the PFE assumption, the causal effect of schooling can be estimated by means of within-family estimators. Scholars disagree on whether the PFE assumption is likely to hold or not. However, if the ability bias can be considered non-negative *a priori*, a twin-based estimate of the return to schooling which is lower than the standard OLS estimate provides a tighter upper bound on the causal effect of schooling on earnings.[1]

[1] This argument is valid, however, only when schooling is measured without error, see discussion in Bound and Solon (1999).

The focus on the potential ability bias is motivated by an interest in the interpretation of the correlation between educational attainment and earnings. It is due to sorting of individuals with respect to (pre-schooling) unobservable characteristics, or does it reflect productivity improvement caused by educational investments? The magnitude of the ability bias is linked to the existence of "sorting", see Weiss (1995), but it may also arise for other reasons. Of course, the social return to educational investments may differ from the causal effect on individual earnings for other reasons.

The paper is organised as follows. Section 2 and 3 discuss the earnings-schooling relationship and the assumptions under which the twin approach produces an unbiased estimate of the causal effect of schooling on earnings. The main findings of previous studies are briefly referred in section 4. Section 5 presents the Norwegian data on twins. The estimated returns to schooling, both from linear and non-linear models, are presented in section 6 and conclusions are drawn in section 7.

2. The effect of schooling on earnings

We focus on the *average marginal effect of schooling on earnings* (*b*) in a standard earnings equation,

(1) $$\ln(y_j) = a + bS_j + X_j g + u_j$$

where y_j is earnings of individual j, S_j is years of schooling, a is the common intercept, X_j is a vector of observables like experience and u_j is unobserved earnings determinants. It is well known that an unbiased estimate of the average marginal effect of schooling on earnings can be obtained by standard OLS procedures, if u_j is independent of S_j and X_j.

However, individual heterogeneity implies that educational attainment can be correlated with earnings capacity as well as return to schooling. Following Card (1998), (1) can be generalised to[2]

(2) $$\ln(y_j) = a + bS_j + X_j g + \{a_j + (b_j - b)S_j\} + e_j$$

where a_j is an individual "earnings capacity" component (defined as difference from the average, a) with a zero mean, b_j is the individual return to schooling and e_j measures unobserved earnings determinants uncorrelated with schooling. This formulation highlights two kinds of heterogeneity, with respect to earnings capacity (a_j) as well as return to

[2] A simplified linear version of Cards model.

schooling *(b_j)*.[3] Average capacity and return are *a* and *b*, respectively. Our interest is *b*, i.e. *the average marginal return to schooling,* and focus will be on whether the OLS-estimate of *b* is (asymptotically) biased. [4]

A comparison of (1) and (2) offers an interpretation of the "error term", u_j, in the standard earnings equation. First, variation in expected return to schooling give rise to a "return heterogeneity bias" in the case of correlation between b_j and S_j , i.e. the slope. Optimal schooling models of the Becker-type predict that this bias is positive when people have knowledge about their return (b_j) at the time when schooling decisions are made. In this case, individuals with higher return stay longer in school. Secondly, there may exist an "earnings capacity bias" due to correlation between a_j and S_j, i.e the intercept of the earnings equation. The direction of the earnings capacity bias is, however, ambiguous. Most researchers seem to believe that this bias is positive because children from "high ability" families have lower marginal cost of, or more taste for, schooling. If we go beyond the Becker-approach, we would immediately consider limited capacity as well as quality differences across schools and universities. In practice, the choice set differs across individuals. Although earnings capacity is different from cognitive ability, or performance in school, a positive correlation seems likely. Then, if access to colleges and universities are rationed on the basis of previous school performance, a positive relationship between schooling and earnings capacity will appear. Moreover, colleges of high quality may only be available to those with performance above a certain level during their first years in school. Consequently, ability is likely to be positively correlated with educational attainment. Griliches (1977) on the other hand, points out that the marginal cost of schooling is higher for the more able individuals since foregone income is higher for them. This opportunity cost argument suggests a negative correlation between earnings capacity and the level of schooling.

3. The Twin Approach: An application of the within-family estimator

The departure of the twin approach is that twins of equal sex have very similar personal characteristics. If the (often limited) variation in educational attainment within twin pairs are

[3] b is typically called "the return to schooling", but does only coincide with the internal rate of return to schooling investment under very strict assumptions, see Willis (1986) and a critical discussion in Heckman et al (1999).

[4] Note that this is different from the "effect of treatment (schooling) on the treated (schooled)", $E(b_j | S_j=s)$, see Heckman and Vytlacil (1998) for a discussion of this distinction in the case of return to schooling.

caused by factors without direct influence on post-schooling labour market performance, an unbiased estimate of the causal effect of schooling can be obtained by comparing earnings differences and differences in schooling within twin-pairs.

To fix ideas, rewrite (2) for twin 1 and 2 of family i;

(3) $$\ln(y_{i1}) = a_0 + bS_{i1} + X_{i1}\, g + a_{i1} + (b_{i1} - b)S_{i1} + e_{i1}$$

(4) $$\ln(y_{i2}) = a_0 + bS_{i2} + X_{i2}\, g + a_{i2} + (b_{i2} - b)S_{i2} + e_{i2}$$

The important question is how earnings ability and return vary with the level of schooling. Twin studies typically apply the "Pure Family Effects"(PFE)-assumption saying that earnings capacity and return to schooling are the same within families, here twin-pairs. The PFE-assumption can be formalised as

(5) $$a_{i1} = a_{i2} = a_i = \lambda(S_{i1} + S_{i2} - 2\overline{S_1})$$

(6) $$b_{i1} - b = b_{i2} - b = b_i - b = \psi(S_{i1} + S_{i2} - 2\overline{S_1})$$

where $\overline{S}_1$ is the average schooling level of twin number one (equal to twin two as numbering is random). However, for the population as the whole, i.e. across families, earnings capacity and return may vary systematically with the average schooling level of the siblings (twins). By inserting (5)-(6) in (3)-(4) we get under certain symmetry conditions, see Card (1998)[5],

(7) $$\ln(y_{ij}) = a_0 + bS_{ij} + c(S_{i1} + S_{i2}) + X_{ij}\, g + e_{ij}$$

where

$c = \lambda + \psi$.

From (7) we see that an unbiased estimate of *b* can be obtained by including the twins' total years of schooling, if the PFE-assumption holds. The model is labelled the SURE specification (seemingly unrelated regression) below as we take into account the correlated error terms of twins. An alternative estimator is the first difference ("fixed effect") where family components affecting earnings capacity and return drop out;

(8) $$\ln(y_i) = \ln(y_{i1}) - \ln(y_{i2}) = b(S_{i1} - S_{i2}) + (X_{i1} - X_{i2})\, g + e_{i1} - e_{i2}$$

Equation (7) also illustrates the reasons why the standard OLS estimate is potentially biased. The bias depends on the sign and magnitude of the coefficient on the sum of the twins' schooling years, c, which is related to how earnings capacity and return are correlated with schooling *across* families. Note that the OLS-estimate is likely to be upward-biased even if earnings capacity is unrelated to educational attainment. Optimal schooling models predict that return heterogeneity bias is always positive, as those with higher return take more schooling (y is positive).

The crucial question is whether the "Pure Family Effects" assumption holds for twins. Monozygotic ("identical") twins are genetically equivalent, and if they reared together, they also experienced the same environment during childhood. Dizygotic (fraternal) twins of the same sex are genetically as sisters/brothers, but, unlike many sisters and brothers, fraternal twins reared together enjoyed the same environment. Brother and sisters have frequently experienced different environment because the social and the physical environment change; Families move house, parents age and the size of the family size change as new children are born. Economic resources of the family change over the life cycle of the parents, through savings or heritage. Thus, siblings of the same age (twins) are likely to have enjoyed a more similar environment than sisters and brother in general.

The criticism of the PFE model emphasises that schooling effects are identified by twin-pairs where the two brothers or sisters end up with different educational attainment. Then, the crucial question arises immediately; If twins were exposed to very similar environment when they grew up, and for monozygotic twins, are genetically identical, why is there any difference in schooling between twins? Several studies argue that even monozygotic twins are different, from the "start", by differential treatment or desire to be different, see Bound and Solon (1999) for further references. Blanchflower and Elias (1996) find differences in schooling ability within pairs of both MZ and DZ twins, comparing a relatively small number of twins from the British Child Development Study.[6] Moreover, if parents recognise that twins are different, they may take steps to compensate and encourage schooling for one of them. Different treatment from parents may bias the return from schooling estimates in either direction.

In our context, these (often small) differences in character, attitude, cognitive ability or events during childhood and adolescence may be important factors behind the (often small) differences in schooling. Moreover, these characteristics or experiences *may* also affect earnings capacity and expected return to schooling, see discussion in Bound and Solon (1999).[7] As pointed out by Griliches (1979) twenty years ago, the twin-based estimate is

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[5] Under the assumption that b_i, S_{i1} and S_{i2} have a jointly symmetric distribution, see Card (1998), Appendix.

[6] These differences correlate across the 9 year period. Unfortunately, the limited number of twins and the strong correlation in schooling within twin pairs in the BCDS, make it impossible to assess the implications for estimates of the return to schooling.

[7] In the words of Bound and Solon (1999) " Any parent of monozygotic twins will tell you that their kids do differ in temperament and abilities. Often these differences are subtle, but presumably it is these differences, rather than coin flips, that account for the twin's divergent choices about schooling. And, if the same differences

closer to the true average marginal return to schooling, if and only if, the endogenous variation in educational attainment comprises *a smaller share* of the between-twin variation in schooling than it does of the between-families variation. Unfortunately, it is not sufficient than twins are more similar than a random pair of observably equivalent persons!

However, if we were confident that the correlation between earnings capacity and schooling is *non-negative* and that individuals with high return tend to invest more in educational attainment, the twin-based estimates are useful even if the PFE-assumption is violated. As the OLS and the within-family estimate both will be biased upwards in this case, we can argue that a twin-based estimate which turns out to be lower than the OLS-estimate has tightened the upper bound on the (average) return to schooling. This argument is only valid if schooling is measured correctly, since measurement error typically creates a downward bias which is larger for the within-family than the OLS-estimator, see discussion in Bound and Solon (1999).

The crucial PFE assumption is sometimes backed by indirect evidence. One strategy is to compare within-pair and across-pair correlations between schooling and observed characteristics (Z) which are supposed to correlate with earnings ability. Twin approach supporters would like to find, first, a significant across-pair correlation between S and Z and, secondly, no within-pair correlation based on regressing schooling differences on differences in Z's like schooling of spouse or physical /psychological characteristics. Ashenfelter and Rouse (1998) perform such tests and conclude that the PFE assumption is likely to hold in their data.[8] In light of the difference between Scandinavian countries and the US in wage structure, educational institutions and intergenerational earnings mobility, see Björklund and Jännti (1997), one might find US-based studies to be of limited interest for Norway. The results by Iscasson (1999a) on Swedish twin data are more relevant since Sweden and Norway are fairly similar, both in terms of labour market characteristics and educational institutions. Isacsson (1999a) tests the equal within-pair ability assumption by comparing across-pair and within-pair correlations between years of schooling and two physiological characteristics (birth weight and height) of the individual and between schooling and two

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in temperament and abilities also exert other influences on wages, the empirical association of the between-twins wage difference with the between-twin schooling difference reflects more than just the causal effect of the latter on the former".

[8] "Under some assumptions, this model (of schooling investment) implies that the schooling investment of genetically equivalent individuals should be the same, apart from random deviations that are not related to the determinants of schooling choices. Using data we have collected for identical twins, we find that a variety of direct and indirect tests provides little evidence inconsistent with this hypothesis", p.281.

psychological measures of the individuals personality. The general findings are that correlations were stronger in the across-pair than in the within-pair estimations, reasonably interpreted as supportive evidence for the assumption of randomly determined differences in schooling between twins of the same family.

Lastly, estimates on twin samples may not be representative. Twins can be different from non-twins. Blanchflower and Elias (1996) "presents evidence which suggest that there are significant differences between twins and non-twins in terms of measured ability (i.e test scores at the age of 7,11 and 16), schooling and economic gain from years of schooling". Ashenfelter and Rouse (1998) show that the twins of the Princeton Twinsburg study and their parents are better educated, have higher wages, is more likely to be covered by a union and, most important, have a higher OLS-estimate of return to schooling.[9] The practical difficulties associated with the collection of twin data can render representativity. One might argue that twin study estimates of returns to schooling can be generalised only if twins are representative with respect to observable individual and family characteristics *and* if simple schooling-earnings correlations (i.e. OLS estimates) are the same. Our study meets both these requirements as twins and non-twins are practically identical, in both respects.

The literature reveals that researchers have different opinions about the extent to which twin studies are useful to disclose, and correct for, a potential ability bias. While researchers who have invested heavily in the construction of twin data believe in the PFE assumption, others are sceptical.[10]

4. Previous studies

The average hourly wage premium associated with an extra year of schooling in Norway is typically estimated to be around by 4.0-5.5 per cent, see representative estimates in e.g. Asplund et al. (1996) and a recent summary in Barth and Røed (1999).[11] The education premiums in Norway are definitively smaller than what we find in most other countries. OECD (1997) finds that Norway has the lowest internal rate of return from completing a university degree.

[9] The 680 twins are compared with a similar Current Population Survey (CPS) sample and the General Social Survey in 1990-1994.

[10] A speculative interpretation is that personal interests and experience colour researchers' attitude. According to Card (1998), Becker (1964) suggests that scholars are biased in their opinion since they appreciate the idea that ability is a major cause of the high earnings received by college graduates (like themselves). On the other hand, as a parent of twins you are likely to object to the idea that your children have identical capacities and that "random" events explain why one of them stayed longer in school.

[11] Studies on annual earnings typically estimate somewhat higher premiums, 6-7 per cent.

The low returns to schooling in Norway, relative to most other countries, can be given different explanations. First, educational attainment has increased remarkably in Norway during the last 20-30 years and these supply shifts are likely to reduce relative wages of better-educated employees. Secondly, the high degree of centralisation and the focus on solidaristic wage policies, are commonly seen as important explanations for the low and stable wage dispersion in Norway, see Kahn (1998), Moene and Wallerstein (1997), Freeman (1996). These institutions tend to raise wages of the less educated and also keep earnings of university graduates down. Thirdly, the large public sector employs a high fraction of those with long schooling. Lastly, the low earnings premiums associated with more education *may* reflect that the ability bias is smaller in Norway than in other countries. As described in section 2, ability bias is closely related to the existence of heterogeneity with respect to earnings capacity and returns to schooling. A speculative conjecture is that a society with fairly equal distribution of earnings, wealth and opportunities due to a very low school/university fees combined with universal access to student loans and grants, will generate a weaker correlation between innate ability to succeed in the labour and educational attainment.

Few previous Norwegian studies have used test-scores, instrumental variable approach or sibling data to address to the potential ability bias in estimates of returns to schooling.[12] Hægeland, Klette and Salvanes (1998) is one exception, utilising the region in which the person grew up and parental educational attainment as determinants of schooling. The inclusion of generalised residuals from an ordered probit equation of educational choice into the estimated wage (earnings) equation seems to a have a modest effect the returns to schooling estimates. They find that OLS estimates are downward biased, but the upward adjustment following from the IV-estimator is minor. The similarity of Norway and Sweden justifies references to recent Swedish studies controlling for ability measured by intelligence scores, achievement tests and school marks at the age of 12-13. Kjellström (1997) find that the estimated wage premium of one extra year of schooling falls by 20 per cent. Meghir and Palme (1999) also show that the inclusion of ability measures reduces the annual return to schooling by a similar magnitude.

Recent studies of twins from various countries include Behrman et al (1994), Behrman and Rosenzweig (1999) on the Minnesota Twins Registry, Ashenfelter and Krueger (1994)

[12] Schooling careers has been studied extensively in Norway and mainly by sociologists. A recent study by Nordli Hansen (1997) shows that parental income at the age 10-14, conditional on class background measured by the occupation of father, has a fairly strong effect on educational attainment at all levels.

and Ashenfelter and Rouse (1998) on the Princeton Twinsburg Study (PTS), Miller, Mulvey and Martin (1995),(1997) on the Australian Twin Registry and Isacsson (1999a,b) on the Swedish Twin Registry. The overall result of these studies is that OLS estimates are (slightly) upward biased, depending on adjustments for measurement error in the schooling variable.[13] In some cases, the ability bias disappears when measurement errors are taken into account.[14] The famous exception is Ashenfelter and Krueger (1994) who found a strong negative ability bias. This conclusion turned out to be wrong and due to sampling error, see Ashenfelter and Rouse (1998) and Rouse (1999). Miller at al (1997) is the only study reporting gender-specific results. They find a considerably greater family background effect for males than for females, leading to the conclusion that “The pure returns to schooling are greater for females than for males”, p. 133.

5. The Norwegian data of twins

While twin samples usually come from surveys based on Medical Birth registers, or even twin assemblies like the PTS, our data are established by “creative” matching of several official registers in Statistics Norway. These registers cover the whole Norwegian population aged 16-69, including day of birth and a link to their parents. Twins are simply defined as individuals born at the same, or the next/previous, day by the same mother. Our twin pairs are therefore biological twins, not necessarily reared together. Moreover, we have no information about zygosity, i.e. whether the individual twins are monozygotic or dizygotic. Based on the assumption that half of the non-identical twin pairs have the same sex, we can calculate the total number of identical twin pairs by deducting the actual number of pairs of different sex from the number of equally sexed pairs. This application of the so-called Weinbergs rule reveals that approximately half of our twin pairs are monozygotic. The register matching procedure seems to identify about 80 per cent of Norwegian twins born 1946-1965. This coverage falls from more than 90 per cent in the younger cohorts to about 50 per cent in the oldest, see details in appendix Table A1.

Our schooling variable is the *highest* level of educational attainment by October 1993, according to the official Norwegian education register. This register is updated on the basis of information from schools/universities and covers completed degrees and exams from courses

[13] Summaries with more details are given in Card (1998), Bound and Solon (1999) as well as in Ashenfelter and Rouse (1999).
[14] It should be noted, however, the measurement error corrections rely on very strong assumptions, see Kane, Rouse and Staiger (1997). The popular strategy to instrument schooling by means of reports from the other twin has recently been questioned by Neumark (1999) and Bronars and Oettinger (1999).

of at least 300 hours, given by officially approved schools and universities. The "years of schooling" variable is defined by the register as the standard number of years associated with a specific type of education.[15] Our schooling variable is then a measure of formal qualifications, rather than the actual years of schooling of the individual. Person may have spent less or more time in schools or universities than the standard period stipulated by the institutions. Schooling which do not raise the highest level of qualifications is not taken into account. Thus, the possible measurement error is not classical, but different from a simple random noise between "true" and reported educational attainment.

Parallel analyses are conducted on annual as well as hourly earnings. Annual earnings is the total sum of wages, salaries and sick-leave plus maternity-leave payments. The source is a tax-registry. Note that unemployment benefits are not included. Hourly earnings are based on wages and salaries, including taxable fringe benefits, in a "matched" employee-employer relationship. Details on the construction of hourly earnings are given in the appendix.

The samples are restricted as follows. First, those who completed schooling in 1992 or 1993 are excluded. Secondly, the sample is restricted to those who earned at least 36167 NOK in 1992 and 37033 NOK in 1993. This minimum amount is equal to the threshold qualifying for pension rights in the public age pension system ("Folketrygden"). Thirdly, to concentrate on wage earners, individuals are excluded if self-employment income amounts to more than 10 per cent of annual earnings. Earnings are measured as the average of 1992 and 1993, both for annual and hourly earnings. The person-specific control variables (X) include actual work experience, region, marital status, child born in 1991,1992 or 1993, and the number of children aged 2-6 and 7-16 by end of 1992. The appendix contains more information on the controls.

The total number of twin-pairs of the same sex we use in the estimations are 3431 (annual earnings sample) and 2325 (hourly earnings sample). We restrict ourselves to twins aged 28-47. The exclusion of younger twins secures that the vast majority of the cohorts have completed schooling, while the upper bound is due to low coverage of twin pairs in the data born before 1946. The sample reductions due to the various restrictions are shown in the appendix.

The representativity of the twin sample is studied in Table 1. Non-twins means are based on a random sample of individuals drawn from the same data source as the twins and

[15] Some institutional changes have taken place across cohorts. Compulsory schooling was raised gradually from 7 to 9 during the 1960's and 7 or 8 years are set to 9 in the analysis.

stratified by age and gender, one hundred for each pair of twins.[16] There are minor differences between twins and non-twins. Mean earnings are very close. While female twins have slightly more schooling than non-twins, the opposite holds for males. As far as individual characteristics are concerned, means of twins and non-twins are very similar.

Table 1. Individual characteristics of twins and non-twins. 28-47 years. Hourly earnings sample. Standard deviations in parentheses.

	Male Twins	Non-Twins	Female Twins	Non-Twins
Number of individuals	3112	168836	1538	105124
Age 31.12.93	37.5405	37.3108	37.1508	37.1131
	(5.5811)	(5.6251)	(5.6656)	(5.6402)
Married 1.1.93 (yes=1)	0.6160	0.6232	0.5618	0.6059
Women who gave birth 1993	-----	-----	0.0585	0.0621
Child born in 1992 (yes=1)	0.0749	0.0739	0.0540	0.0540
Children 1-6 years 31.12.92 (yes=1)	0.3554	0.3771	0.2633	0.2947
Children 7-16 years 31.12.92 (yes=1)	0.4441	0.4462	0.3901	0.4418
Years of schooling (mean, 9-20 years)	11.8570	12.0985	12.0923	12.0029
	(2.3496)	(2.4744)	(2.4403)	(2.4252)
Completed education 1990-91 (yes=1)	0.0537	0.0496	0.0618	0.0638
Ln (mean annual earnings 92-93)	12.4080	12.4247	12.1135	12.0706
	(0.3155)	(0.3252)	(0.2999)	(0.3121)
Ln (mean hourly earnings 92-93)	4.8401	4.8611	4.6736	4.6531
	(0.3037)	(0.3223)	(0.2886)	(0.3101)
Years of work experience 1967-91	16.2625	15.8666	13.3433	13.0234
	(5.6101)	(5.6125)	(5.0248)	(5.1391)
Region: County of				
Oslo and Akershus	0.2034	0.2196	0.3212	0.2679
Østfold, Hedmark and Oppland	0.1379	0.1318	0.1333	0.1274
Buskerud, Vestfold, Telemark and Agder	0.2040	0.1911	0.1619	0.1688
Rogaland and Hordaland	0.2134	0.1942	0.1580	0.1696
Sogn og Fjordane, Møre og Romsdal and Trøndelag	0.1526	0.1651	0.1190	0.1572
Nordland, Troms and Finnmark	0.0887	0.0982	0.1066	0.1091

Table 2 shows how educational attainment varies within twin pairs. Approximately 40 per cent of the twin pairs have the same years of schooling. The mean difference is 1.4 years for men and about 1.3 for women. The within-pair correlation in years of schooling is 0.58 for men and 0.67 for women. These correlations are very similar to the numbers reported in other twin studies, taking into account that our sample is an equal mix of both MZ and DZ twins.

[16] The non-twins are drawn from the annual earnings sample.

The earnings correlation is higher for annual than hourly earnings, presumably reflecting a labour supply effect of family background.

Table 2. Within-pair correlations in schooling and earnings. Hourly earnings sample.

	Male	Female
Number of pairs	1556	769
Fraction with same years of schooling	0.3811	0.4161
Differences in years of schooling:		
1 year	0.2423	0.2588
2 years	0.1716	0.1170
3 years	0.1015	0.1092
4 years	0.0456	0.0533
5 years	0.0263	0.0260
6+ years (6-9 years)	0.0315	0.0195
Mean (incl. zero)	1.4094	1.2848
Standard deviation (incl. zero)	(1.6123)	(1.5235)
Within-pair correlations in:		
Years of schooling	0.5848	0.6669
Ln (mean annual earnings)	0.4042	0.3199
Ln (mean hourly earnings)	0.3554	0.2748

Compared to other twin studies, we find lower earnings correlation, which is open to different explanations. First, as twin studies typically do not adjust for a common age or work experience effect, the age span will affect the estimated earnings correleation, see Solon et al (1991), Since the age span in our sample is tighter than in other studies, the unadjusted earnings correlation is lower. Secondly, a recent comparison of intergenerational earnings mobility in Sweden and the U.S. by Björklund and Jännti (1997), suggests that family background explains a smaller part of the earnings variation in Scandinavia than in the U.S. The higher earnings correlation reported for the Australia Twin Registry can partly be due to their rough earnings measures based on mean earnings of the twin's occupation. Even Isacsson (1999a) finds higher annual earnings correlation in Sweden than we do, but his sample is more restricted to full-time workers (both twins above higher earnings threshold in all four years).

To conclude the data description, we sum up what we see as the attractive features, and also, some drawbacks of our data. First, sampling error or attrition is unlikely to bias our results. Secondly, on the basis of the same information set for twins and non-twins, we find that twins are representative. Thirdly, we use a two-year average of earnings, reducing the potential problem of transitory components. Fourthly, we have a fairly large sample, allowing for separate analyses of men and women. On the other hand, the data also have shortcomings. They offer no opportunity to distinguish between MZ and DZ twins. Moreover, we do not have a second source of information on educational attainment, which could help us to adjust for possible measurement error. However, we consider the quality of the schooling variable as a measure of formal educational qualifications in the registry to be better than in self-reported surveys.[17]

6. Estimated effects of schooling on earnings

In the model outlined above, an extra year of schooling is assumed to have the same effect on (log) earnings at all levels of educational attainment. Although linearity seems to be reasonable for some countries like the U.S., previous Norwegian studies indicate a flat region at around 13-15 years of schooling, see Asplund et al (1996), Hægeland et al (1998). Hence, we also estimate a more flexible model with separate dummies for years of schooling beyond the compulsory nine years.[18] A non-linear model also allows the ability bias to vary across different levels of educational attainment. We expect larger effects of schooling on annual than on hourly earnings, simply because working hours tend to be higher for more educated persons.[19]

The main results of the linear model are given in Table 3 and the standard cross section estimates (OLS) are presented in the two top rows. First, the twin samples are representative as the OLS-estimates of the linear model are indeed very similar for twins and non-twins, both by gender and earnings definition.

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[17] The standard measurement error model does not apply to our data since the register hardly over-report schooling at any level. The potential problem of our data is that some persons have additional schooling at the same level, which do not upgrade their highest level of educational attainment. We have not explored how this kind of error tends to bias our estimates.

[18] In fact, a school reform gradually introduced 9 years of compulsory in the 60'ies and early 70'ies. Seven or eight years of schooling is upgraded to nine in our analysis, but this corrections had negligible effects on the results.

[19] By construction, log (annual earnings) = log (hours worked during the year) + log (hourly earnings), although the exact decomposition is impossible in our data due to lack of detailed hours information. Thus, the annual earnings effect of schooling is the sum of the effect on hourly earnings and the effect on hours worked. Estimates from the mid-1990's in the U.S. indicate that one third of the annual earnings effect arises from hours and two thirds from hourly earnings, see Card (1998).

Table 3. Estimated average marginal return to schooling. Twins and Non-twins. Linear model.

	Annual earnings Men	Women	Hourly earnings Men	Women
Non-twins (OLS)	**0.0612**	**0.0713**	**0.0487**	**0.0435**
	(0.0003)	(0.0004)	(0.0003)	(0.0004)
Twins (OLS)	**0.0616**	**0.0700**	**0.0500**	**0.0435**
	(0.0024)	(0.0027)	(0.0022)	(0.0030)
First difference or fixed effect model (FE)	**0.0363**	**0.0579**	**0.0280**	**0.0400**
	(0.0048)	(0.0059)	(0.0046)	(0.0064)
Difference (OLS-FE)	*0.0253*	*0.0121*	*0.0220*	*0.0035*
SURE-model				
Own education	**0.0375**	**0.0551**	**0.0319**	**0.0395**
	(0.0043)	(0.0058)	(0.0041)	(0.0062)
Sum education	**0.0154**	**0.0093**	**0.0115**	0.0026
	(0.0025)	(0.0032)	(0.0024)	(0.0035)

Controls included: Years of work experience (actual), marital status, children, recently completed education, and region. Bold numbers indicate significance at 5 percent level.

The OLS estimates of twins and non-twins differ at most by 0.0013.[20] The OLS estimates of the average marginal return to schooling are also very close to what is found in previous Norwegian studies. Second, the estimates confirm that the effects on annual earnings exceed those on hourly earnings. The effect of schooling on annual earnings seems to be stronger for women than for men, while the effect on hourly earnings is somewhat larger for men. Taken together, this indicates a larger labour supply effect of schooling among women.

Control variables are all reasonably signed, shown in Table A4 in the appendix. Marriage is associated with higher earnings for males, but the marriage premium is not significant for hourly earnings. Married women earn less per year than unmarried, but no difference appears for hourly earnings. Children have no impact on male earnings. Women with children below 16, however, have considerably lower annual earnings than other female employees. The earnings premium per year associated with an extra year of work experience is about 1.1 for men and 2.1 per cent for women, measured at the average years of experience.

[20] In simple OLS regressions on the pooled samples of twins and non-twins, we find that the twin dummies and the interaction terms twin*schooling are far from significant (t-values well below one).

The marginal effect of experience on *hourly* earnings is more similar and around 0.9 per cent for both men and women, measured at the average years of experience.

As described in Section 3, we present two alternative within-family estimators. The estimates of the familiar first-difference model, or fixed model (FE), are shown in row 3, while the SURE model including the total years of schooling are shown in row 4 and 5. The gender difference motivates separate results men and women.

For men, there is clear evidence of a *positive ability bias*. The estimates of the FE and SURE models are both substantially lower than the corresponding OLS estimate. While the OLS annual earnings is about 0.062, the FE and SURE-estimates are similar and about 0.037. The estimates based on hourly earnings show the same pattern; the OLS at 0.05 is reduced to around 0.03 taking into account the heterogeneity of family background. The sign, magnitude and significance of the ability bias are measured by the coefficient for the twins' total years of schooling. It turns out (highly) significant in the SURE model. The ability bias is only slightly higher for annual than for hourly earnings, indicating that the reduction in the estimated casual effect of schooling on earnings male ability bias is due to unobservables with impact on hourly wages rather than factors related to unexplained variation in hours worked.

The within-family estimates reveal that ability bias is different and less important for women. First, no bias is found for hourly earnings, since the first-difference (FE) as well as the SURE estimates, are both very similar to the OLS. Female hourly earnings are not related to the total years of schooling for both twins. Looking at annual earnings, however, the standard cross section estimate seems to be upward biased since both the FE and the SURE estimates are lower than the OLS. The average marginal effect of schooling falls from 0.070 in the OLS to 0.058 in the first-difference model (FE). The estimate of the SURE model is 0.055 and the effect of the twins' total years of schooling is significantly positive. This, combined with the absence of ability bias for hourly earnings, suggests that some unobserved family characteristics affect female labour supply *and* schooling.[21]

One might suggest that the difference between the OLS and the within-family estimates is due to error in the measurement of educational attainment. As pointed out by Griliches (1979) and numerous recent studies, the downward bias induced by measurement error is exacerbated using within family estimators. We have no access to instruments or

[21] We should emphasise that heterogeneity of earnings capacity as well as differences in returns to schooling *across families* may explain that the OLS estimates are biased upwards, but we do not single out the importance of the two possible sources.

information about the reliability ratio to correct for possible measurement error.[22] However, we believe that measurement error is a minor problem in the educational register. Moreover, the difference in ability bias between men and women is hardly consistent with the downward measurement bias explanation. Measurement errors, if they are widespread, should be of similar magnitude for men and women. Thus, the lack of difference between OLS and within family estimates for *female hourly earnings* suggest that the *ability bias is negative if measurement errors are important.* If measurement error is widespread, then both estimates must biased downwards but for different reasons. Moreover, if measurement error bias is important, the within family estimates for women should be lower, relative to the OLS, compared to men because the within-pair correlation in schooling is higher for women, see Table 2. Since we are fairly confident that the ability bias is non-negative, we conclude that measurement error is unlikely to be the major explanation for why within-family estimates are lower than the OLS.

We also estimate a flexible non-linear model with dummies for years of schooling (9 to 18). The main motivation for this exercise is that previous Norwegian studies suggest very low marginal returns to schooling for medium-long higher education. We wonder if this non-linearity can be explain by different ability bias at various schooling levels and start with the results for men. Figure 1 contains estimates of four different models (linear/non-linear and OLS/FE model) and the distribution of educational attainment in the histogram, for the annual earnings sample.[23] The lines indicate *the marginal effects*, i.e. the earnings effect of one extra year of schooling at different levels of educational attainment. To facilitate a comparison with the linear model, the horizontal lines represent the estimates from Table 3. Consider first the OLS estimates where family (twin) relations are neglected. First, we note that the marginal effect is above this average at 11, 12 and 16 years of schooling.

[22] See Neumark (1999) for a critical discussion of IV-based within-twin estimates which claim to adjust for measurement errors. Neumark argues that "Although AK's (Ashenfelter and Krueger (1994)) IV estimator eliminates measurement error bias in the within-twin estimate, it amplifies the omitted variable bias from any differences within pairs, possibly substantially", p. 145.

[23] The pattern for hourly earnings is virtually identical, see Figure 2.

Figure 1. Men. Annual earnings. Marginal effects of one year of schooling and schooling distribution.

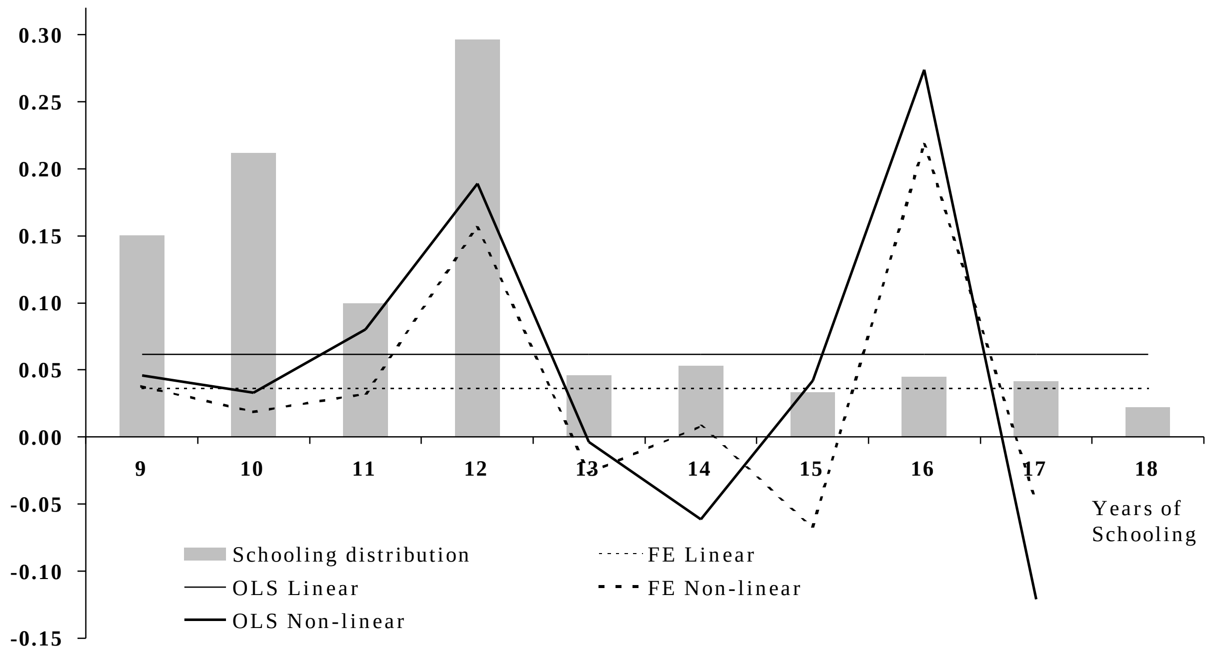
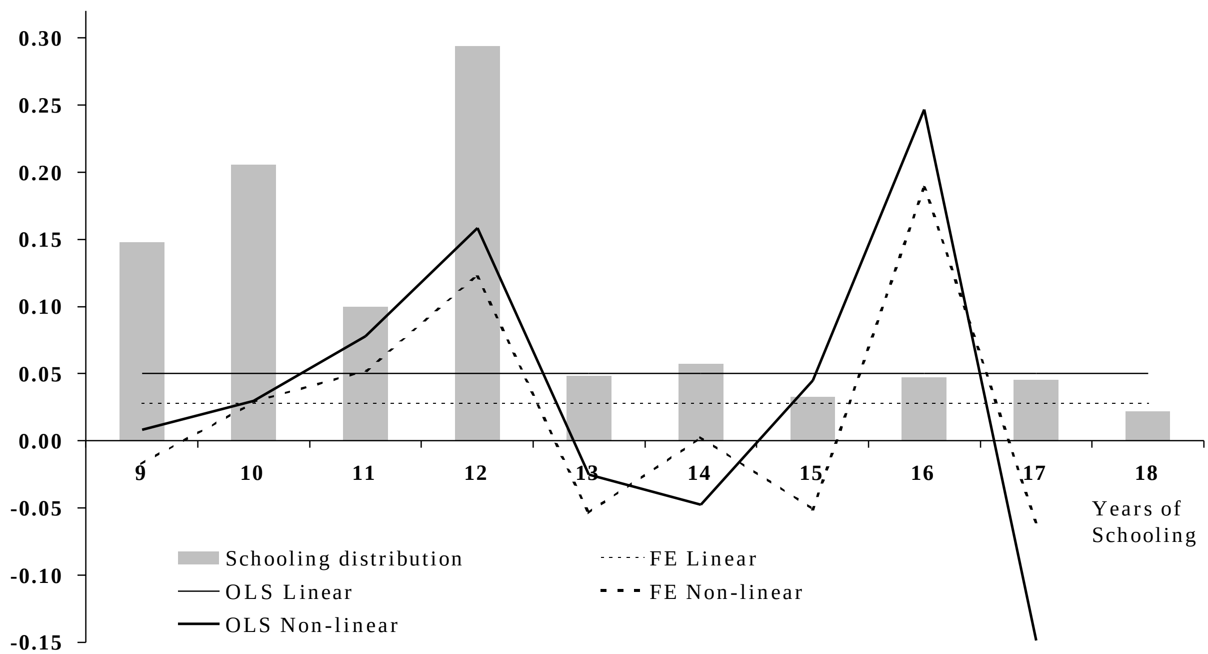


Figure 2. Men. Hourly earnings. Marginal effects of one year of schooling and schooling distribution.



One extra year at eleven years typically involves completing upper secondary schooling (high-school graduate) while 13 years involve some college or university training. Thus, men who take some higher education receive a substantial earnings premium, see details in appendix. Second, the marginal return of 2-4 years of college/university education is, however, low and even negative, but not significantly different from zero. On the other hand, the earnings gain from completing a 5-6 year university degree (at the Masters level) is above 20 per cent and highly significant.[24] The estimates of the fixed effect model confirm the positive ability bias found for men in the linear model. Except for 14 and 17 years, where the marginal effects of the non-linear OLS model are negative, the within-family estimates are lower. The marginal effects at 12 and 16 fall somewhat but remain high. In general, the low and even negative marginal effects of 2-4 years of higher education are still valid.

The results for women are mixed. We first look at *annual* earnings in Figure 3 for which the linear model in Table 3, indicated a moderately positive ability bias. The linear OLS model tracks the unrestricted model fairly well at the lower end of the schooling distribution, but it clearly underestimates the large earnings premium associated with completing a university degree at the Masters level. Like for men, marginal effects of shorter higher education are mixed. For women, extending higher education from one to two years has a high marginal return, while another year or two have minor effect on earnings. The marginal effects from the FE estimates are close to or below the OLS estimates, confirming the positive “hours-bias” suggested by the linear model. A standard parameter restriction test does not reject the linear model.

While the earnings measure is of minor importance for the male estimates, the linear model indicated a positive labour supply effect of schooling for women and this will raise the effect on annual earnings. Figure 4 shows the female marginal effects of schooling on *hourly* earnings. Like for annual earnings, the linear OLS underestimates the marginal return at 13 and 16 years. The low marginal return of extending a higher education to 3 or 4 years, is also disguised by the linear model. The direction of ability bias is, however, mixed. This is consistent with no (average) bias in the linear model. The very high marginal return of completing a full university degree (17 years) is reduced when we estimate within families. The marginal return at 13 years of schooling is even larger. The low marginal returns of medium long higher education remain. Finally, the estimates of the female non-linear model are not precisely determined and we are unable to reject the linear model.

[24] Linearity is rejected by means of standard parameter restriction tests, see details in appendix, even for within-family (FE) model.

Figure 3. Women. Annual earnings. Marginal effects of one year of schooling and schooling distribution.

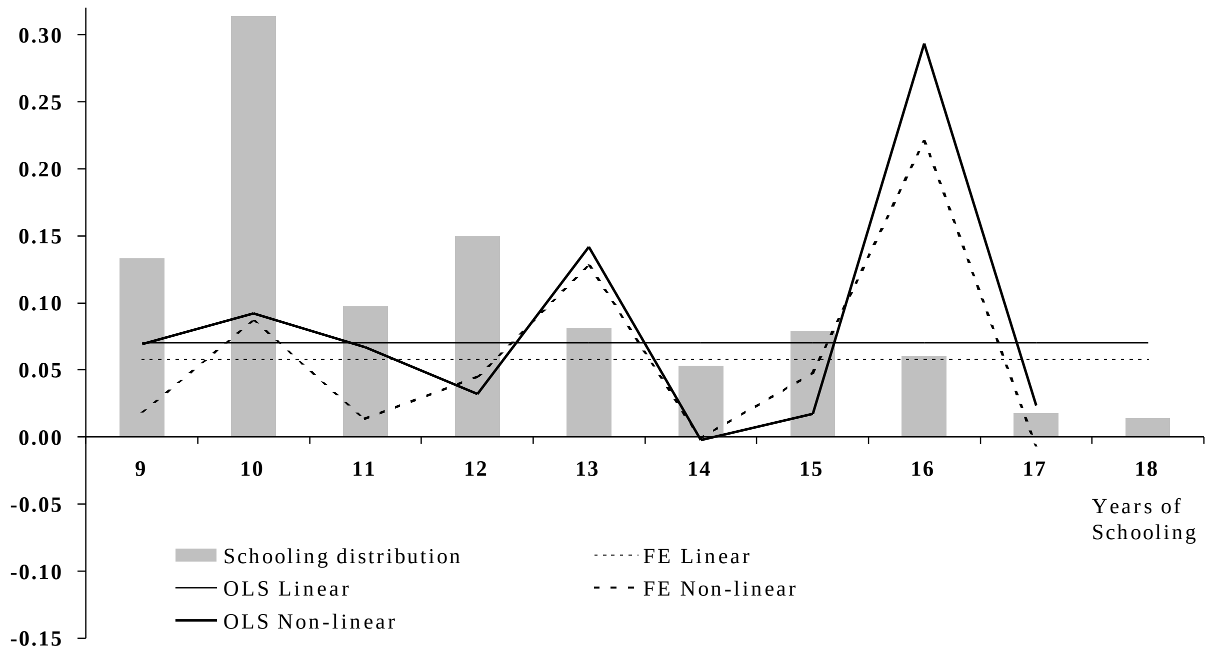
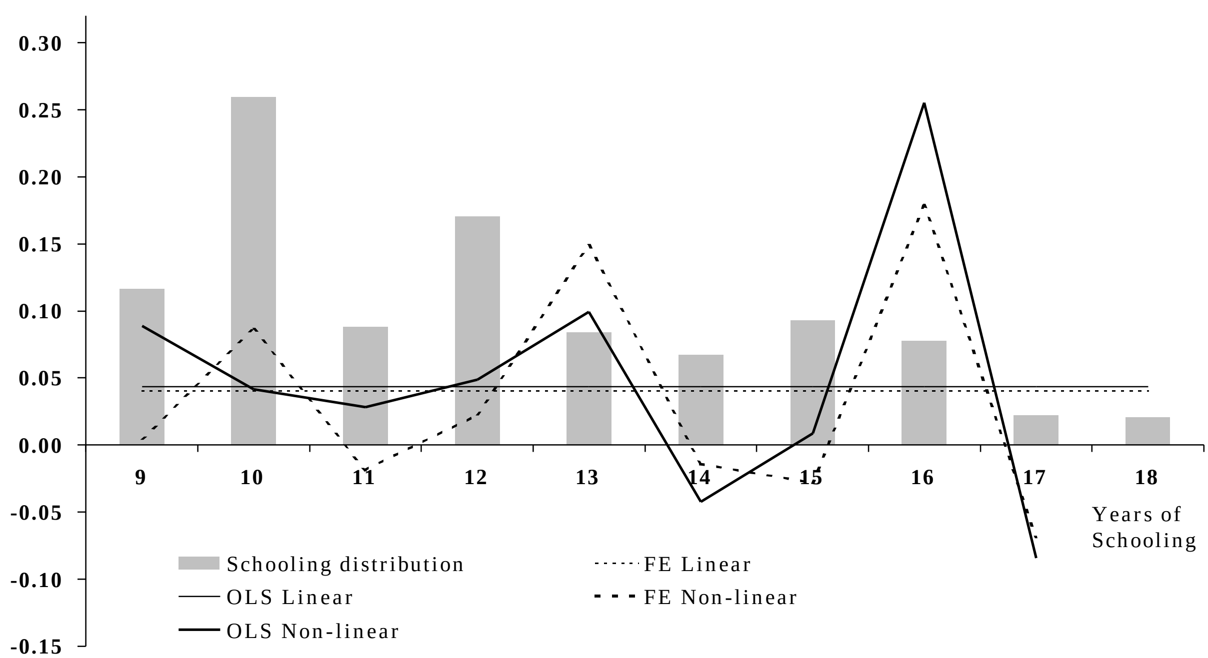


Figure 4. Women. Hourly earnings. Marginal effects of one year of schooling and schooling distribution.



7. Conclusions

The causal marginal effect of schooling on earnings is estimated by means of a large sample of Norwegian twins, aged 28-47. The main findings can be summarised as follows.

Twins are representative. Norwegian twins have the same individual characteristics as the overall population. There is no difference between twins and non-twins in cross-sectional OLS estimates of returns to schooling. Standard OLS-estimates show that an extra year of schooling is associated with an annual earnings premium of about 6 and 7 per cent, for men and women respectively. The return is about 4.5 per cent looking at hourly earnings. We interpret the higher premium for annual earnings as consequence of a positive correlation between schooling and hours worked. This labour supply effect is particularly strong for women.

Under the "Pure Family Effects" assumption the within-family estimator(s) provide an unbiased estimate of the causal effect of schooling on earnings. The linear model clearly indicates that OLS estimates based on *annual* earnings are biased upwards, but the explanation is different for men and women. Considering *male hourly* earnings, the within family estimates indicates that the casual effect of one extra year schooling is about 3 per cent, compared to the OLS estimate of 5 per cent. Labour supply of men, however, seems to be less affected by family background since the reduction in the schooling effect is about the same for hourly and annual earnings (for earnings per year, it falls from 0.062 to 0.037). The positive bias for male hourly earnings can be explained in several ways. First, it may reflect that individuals (from families) with high returns to schooling choose, or are allowed, to take more schooling. Second, the marginal costs of schooling, both pecuniary and non-pecuniary costs, may be lower for individuals with characteristics that are rewarded in the labour market. Examples of such characteristics are motivation, work effort capacity, ability to solve problems, work in teams, etc. These characteristics are likely to affect the sorting of employees into high-/low-paying firms, careers within firms and the content of individual wage contracts. Moreover, these characteristics may also affect educational choice.

Limited access to colleges and universities may also contribute to a correlation between (characteristics determining) earnings ability and the level of schooling. Studies from other countries, Sweden included, suggest that test scores and school performance are positively correlated with adult earnings. The rationing of slots in college, universities and even in the upper secondary school, has to a large extent been based on previous school performance of the applicants. Consequently, individuals who have been denied access to schools are on average those with weaker school performance and earnings ability.

The results are different for women since we find no indication of ability bias in the estimate of schooling effects on *hourly* earnings. For female annual earnings, however, the within-family estimate is lower than the OLS, 0.056 and 0.070 respectively. This suggests a positive “hours-bias” where family background influences educational attainment and post-schooling labour supply of women. The optimal schooling model of the Becker-type predicts that women who plan to work many hours, stay longer in school. For a given hourly wage premium, the economic return to schooling is positively related to the expected number of working hours after completing education. If family background is an important determinant of women’s position in the family (home production, child care etc.), we would expect to find a positive *annual* earnings ability bias, arising from heterogeneity of returns. As the variation in working hours is much lower for men, this effect is far less important for men.

The absence of ability bias in the OLS estimate of schooling effects *on female hourly pay* is striking. Several possible explanations can be given. Although women tend to have about the same length of schooling as men, the content of their education differ and they qualify for different jobs. Men are more likely to take jobs in the private sector and in firms with individual pay determination. With collectively determined wages, personal characteristics are likely to have less impact on individual wages. Our results are also consistent with the view that women are less guided by economic returns when they make choices about educational investments. Moreover, clever women may prefer other job characteristics, like flexibility, instead of wages. Lastly, non-random selection into the labour may reduce the correlation between schooling and ability for women. Imagine that innate ability and schooling both affect female wage opportunities, but not their value of non-market activities. Then, the self-selection will tend to give female labour force participants with low levels of schooling earnings higher innate ability than better-educated women. Even if schooling is positively related to earnings ability for *all* women of a given cohort, this correlation diminishes and may even disappear when we restrict ourselves to labour force participants.

The marginal effect of schooling on earnings is not the same for different levels of educational attainment, although the rejection of the linear model is less clear for women. The very low return associated with 3 or 4 years of higher education remain after having controlled for family background.

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Appendix

I. The Norwegian twins data

We match 10618 twin pairs, born 1946-1965, where both twins have the same information about their father and identical information on immigrant status (the variable "Landbakgrunn"). Foreign-born with two foreign-born parents are excluded.

I.1 Coverage

Coverage in Table A1 is defined by the fraction

$$\frac{\textit{Identified twin pairs}}{\textit{Number of twin pairs who survived their first three years}}$$

The denominator has to be estimated. Harris, Thambs and Magnus (1995) report actual number of twin pair (TP_3), of the cohorts 1967-1974, where both were alive at the age of 3, based on the Norwegian Medical Birth Registry. The actual number of twin pair births of each cohort (TP_0), also before 1967, is available in Historical Statistics, Statistics Norway. Thus, the number of twins pairs of cohort c, where both survived the first three years, is the defined as

$$\widehat{TP_3^c} = hTP_0^c,$$

where

$$h = \frac{TP_3^{1967-74}}{TP_0^{1967-74}} = 0.8561$$

Missing twin pairs are due to emigration or death after the age of 3, or lack of motherhood identification. Death and migration, by twins themselves or their biological parents, exclude twins from the sample.

Table A1. Coverage of identified twin-pairs, 28-47 years of age

Birth year	Age 1993	Twin-births	I. Both surviving 3 years*	II. Identified Twins**	Coverage (I/II)	Male	Female	Mixed
Fraction both surviving			0.8561					
1961-65	28-32	3501	2997	2761	0.9213	932	893	936
1956-60	33-37	3658	3132	2764	0.8825	868	879	1017
1951-55	38-42	3937	3371	2751	0.8161	884	877	990
1946-50	43-47	4394	3762	2342	0.6225	793	715	834
1946-65	28-47	15490	13262	10618	0.8006	3477	3364	3777

* Predicted from 1967-74 cohorts, on the basis of Harris, Tambs and Magnus (1995) and official statistics.
** Non-immigrants.

I.2 Sample restrictions

We focus on twins of the same sex, aged 28-47. The upper limit is due to low coverage for cohorts born before 1946. Other sample restrictions are for both twins. If a restriction is not met for one of the twins, the pair is excluded.

Table A2. Sample sizes and restrictions. Number of observations.

	Male	Female	Sum same sex	Mixed	Sum
All matched	3477	3364	6841	3777	10618
+ resident 1993	3444	3343	6787	3753	10540
+ complete information on schooling	3371	3279	6650	3685	10335
+ left school before 1992	3100	2982	6082	3344	9426
+ above annual earnings threshold in 1992 *and* 1993	1908	1523	3431	-----	-----
+ weekly hours 20+ in 1992 *and* 1993	1556	769	2325	-----	-----

First, 78 of the 10618 pairs are excluded because one was not Norwegian resident in 1993. Second, 205 pairs are removed due to incomplete information in the register of educational attainment. Third, 909 pairs are excluded because at least one of the twins stayed in school in 1992-3 (when we observe earnings). The major sample reductions are due to earnings restrictions. Concentrating on *equally sexed* pairs, 2651 pairs of 6082 (43.6 per cent) do not meet the earnings restriction, see section I.3 below for more details on the earnings sample restrictions. A larger fraction of females than males are excluded (48.9 versus 38.5 per cent). The annual earnings sample then consists of 3431 pairs. The sample size falls to 2325 pairs in the hourly earnings sample as we condition on minimum one identified job of 20 hours per week or more, with duration of at least one month, in both 1992 and 1993. As part-time is far more common among women, the female sample falls from 1523 to 769 and the number of male twin pairs falls from 1908 to 1556. The relatively large sample reductions are partly due to incomplete coverage in the employer-employee registry or part-time work. Note that *both twins* must meet the criteria in *both years*.

I.3 Earnings definitions

The annual earnings in 1992 and 1993 are collected from registers in Statistics Norway based on reports from employers, various public offices and tax-declarations. Earnings (called “pensjonsgivende inntekt”) include wages and salaries, earnings from self-employment and sick leave as well as maternity leave transfers. The samples are restricted to “wage earners”, as those with self-employment income which amounts to more than ten per cent of annual earnings are excluded. Our measure of annual earnings is net of unemployment benefits and self-employment income. Note also that annual earnings generally are sum of wages and salaries from several jobs.

I.3.1 Annual earnings sample

We exclude persons with low earnings, i.e those with annual earnings net of unemployment benefits below 1 G_t in each year, where G_t is the minimum level of earnings to get credits in the public pension system (G_t is “Folketrygdens grunnbeløp”). G_{1992}=36167 kroner and G_{1993}=37033 kroner. To secure consistency, we also require positive credits (“pensjonspoeng”) in the registry of annual pension credits. The annual earnings measure we use is the average of 1992 and 1993. Average earnings are in 1993-prices and the 1992-

earnings are multiplied by a factor of 1.034, reflecting an increase in nominal wage growth of 3.4 per cent from 1992 to 1993, Statistics Norway.

I.3.2 Hourly earnings sample

Hourly earnings are related to one specific job. The procedure goes as follows. Our point of departure is the registry of matched employee (Arbeidstakerregisteret, AREG) –salary (LTO, Lønns- og trekkoppgaveregisteret) records. This registry covers all jobs (AREG) expected to be more than four hours of work per week, matched with salary-records (LTO) including information on wages, salaries and fringe benefits which in sum is basis the pay-roll tax ("grunnlag for arbeidsgiveravgift"). The jobs in this registry include information on date of start and stop as well as a category of expected working hours per week; 4-19, 20-29 and 30 hours + . The same person may have many jobs, in different working hours categories during a calendar year.

We concentrate on job lasting at least 4 weeks and select the most important working hours category. The most important is defined as the category for which sum of all wages, salaries and fringe benefits over all jobs, are the highest. This sum is taken used as the basis for hourly earnings as long as it exceeds one G_t.

After having defined the most important working hours category, the dating of the job is checked with a registry of unemployment and active labour market programme participation. This unemployment-registry is called SOFA and is administered by the Directorate of Labour, who transfers individual records to Statistics Norway. In the cases of overlapping employment and unemployment status (periods), we adjust the duration of employment according to specific rules. Details are available on request. Note that these adjustments vary across type of unemployment (full-time unemployed, part-time unemployed, trainee, other programme participants etc. Some jobs are left out due to inconsistent dating.

For all (remaining) jobs, we calculate weekly earnings. For persons with one job, weekly earnings are the sum of wages and salaries (see above) divided by the number of weeks. Persons with more than one job are more complex, but we simply take the sum over job both for earnings and weeks worked. If there is overlapping job we deduct this overlapping period from the total duration, given an overlap of less than four weeks. With a longer overlapping period, we select the most important job.

Sick-leave periods and the corresponding transfers must be handled. Persons with "long-term sickness" (more than 14 consecutive days) may receive transfers rather than wage or salary paid by the employer. These transfers amount to 100 per cent of the wage, up to an annual wage sum of $6G_t$. Persons on sick-leave are likely to get an underestimates weekly pay as they frequently are being reported as still at work. We therefore add sick-leave transfers to the earnings before we calculate the weekly earnings. For those with more than one job, where some of them are not included in the calculation of weekly earnings, we add sick-leave transfers proportional to the duration of the jobs used.

To get a measured of hourly earnings we need information about hours worked. Number of hours worked within each working hours category (20-29, 30+ hours per week) are based on means from the 1992-3 Labour Force Surveys, stratified by gender and age groups. Details are as follows. Individual records from four quarterly surveys are merged (year-file). Note that no identifying link exists between LFS respondents and our data. Weekly hours are based on hours in the primary job of employees or persons temporarily off work. For employees with varying working hours, we use the average of the contracted hours over several weeks. If weekly hours are less than 4 or larger than 60, the observation is dropped. Remaining observations are used to estimate a weekly working hours, separately for men and women within each category (4-19, 20-29 and 30-60 hours per week). As our earnings study is based on persons 28-47 by 31 December 1993, we use persons aged 27-46 in the 1992 LFS (24811 interviews) 28-47 year old persons in the 1993 LFS (24772 interviews).

We restrict the hourly earnings sample to those with an estimated hourly earnings between 50 and 2000 kroner within the working hours categories 20-29 eller 30+. The hourly earnings measure we use is the average of 1992 and 1993. Average earnings are in 1993-prices and the 1992-earnings are multiplied by a factor of 1.034, reflecting an increase in nominal wage growth of 3.4 per cent from 1992 to 1993, Statistics Norway.

I.4 Control variables

Table A3. Control variables used in estimation

Control variables	Definition/coment
Married	By 1.1.93.
Child born in 1993	Only for women.
Child born in 1992	
Child born in 1991	
Children 2-6 years	By 31.12.92, born 1986-90.
Children 7-16 years	By 31.12.92, born 1976-85.
Years of work experience	Total number of years 1967-91 with earnings above the threshold qualifying for pension rights in the public age pension system ("Folketrygden"). The threshold ("G") is indexed annually and was 36167 kroner i 1992.
(Years of work experience)^2	
Completed education 1991	Highest level of educational attainment achieved in 1991
Completed education 1990	Highest level of educational attainment achieved in 1990
Regional dummies:	*Counties:*
Region 0	Oslo and Akershus
Region 1	Østfold, Hedmark and Oppland
Region 2	Buskerud, Vestfold, Telemark and Agder
Region 3	Rogaland and Hordaland
Region 4	Sogn og Fjordane, Møre og Romsdal and Trøndelag
Region 5	Nordland, Troms and Finnmark

II. Earnings equations and non-linear estimates of the returns to schooling

Table A4. Earnings equation estimates. OLS. Twins.

	Annual earnings Men	Women	Hourly earnings Men	Women
Intercept	**11.265**	**11.005**	**3.986**	**4.081**
	(0.0508)	(0.0500)	(0.0500)	(0.0588)
Years of schooling	**0.0616**	**0.0700**	**0.0500**	**0.0435**
	(0.0024)	(0.0027)	(0.0022)	(0.0030)
Married	**0.0714**	**-0.0802**	0.0119	-0.0157
	(0.0125)	(0.0135)	(0.0120)	(0.0153)
Child born in 1993	-----	0.0313	-----	**0.0790**
		(0.0247)		(0.0301)
Child born in 1992	-0.0043	**-0.0916**	-0.0084	**0.0755**
	(0.0200)	(0.0247)	(0.0191)	(0.0312)
Child born in 1991	0.0082	**-0.1808**	0.0281	0.0063
	(0.0202)	(0.0247)	(0.0197)	(0.0306)
Children 2-6 years	-0.0050	**-0.1101**	0.0048	-0.0269
	(0.0121)	(0.0140)	(0.0115)	(0.0171)
Children 7-16 years	0.0143	**-0.1118**	0.0194	0.0222
	(0.0124)	(0.0133)	(0.0117)	(0.0155)
Years of work experience	**0.0413**	**0.0366**	**0.0344**	0.0072
	(0.0053)	(0.0053)	(0.0053)	(0.0063)
(Years of work experience)^2	**-0.0009**	**-0.0006**	**-0.0008**	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Completed education 1991	**-0.1133**	-0.0217	**-0.0962**	0.0058
	(0.0309)	(0.0347)	(0.0300)	(0.0381)
Completed education 1990	**-0.1185**	0.0321	**-0.0665**	-0.0233
	(0.0308)	(0.0387)	(0.0314)	(0.0424)
Region 1	**-0.1458**	**-0.1426**	**-0.1632**	**-0.0693**
	(0.0179)	(0.0194)	(0.0172)	(0.0223)
Region 2	**-0.0808**	**-0.1333**	**-0.0927**	**-0.0781**
	(0.0162)	(0.0187)	(0.0154)	(0.0208)
Region 3	-0.0001	**-0.0944**	-0.0180	**-0.0712**
	(0.0163)	(0.0183)	(0.0153)	(0.0210)
Region 4	**-0.1266**	**-0.1456**	**-0.1242**	**-0.0644**
	(0.0172)	(0.0191)	(0.0166)	(0.0235)
Region 5	**-0.1284**	**-0.0699**	**-0.1265**	**-0.0921**
	(0.0203)	(0.0222)	(0.0197)	(0.0243)
Adj. R-sq.	0.2289	0.3214	0.1572	0.2030
Number of individuals	3816	3046	1538	3112

See table A3 for definition of control variables. Bold numbers indicate significance at 5 percent level.

Table A5. Estimated non-linear return to schooling. Male twins.

	Annual earnings OLS	Fixed effect	SURE Own education	SURE Sum education	Hourly earnings OLS	Fixed effect	SURE Own education	SURE Sum education
10 years	**0.0457*#**	0.0378*	0.0385*	-0.0010*	0.0081*	-0.0172*	-0.0150*	0.0139
	(0.0171)	(0.0260)	(0.0260)	(0.0175)	(0.0164)	(0.0249)	(0.0248)	(0.0168)
11 years	**0.0789***	0.0565	0.0579	0.0098*	0.0380*	0.0117*	0.0166*	0.0104
	(0.0209)	(0.0314)	(0.0312)	(0.0212)	(0.0199)	(0.0300)	(0.0299)	(0.0203)
12 years	**0.1592 #**	**0.0888**	**0.0906**	**0.0468 #**	**0.1161 #**	**0.0640 #**	**0.0699 #**	0.0300
	(0.0166)	(0.0263)	(0.0259)	(0.0169)	(0.0159)	(0.0254)	(0.0249)	(0.0162)
13 years	**0.3483*#**	**0.2452*#**	**0.2493*#**	**0.0616**	**0.2746*#**	**0.1864*#**	**0.1992*#**	0.0474
	(0.0273)	(0.0412)	(0.0404)	(0.0276)	(0.0256)	(0.0391)	(0.0382)	(0.0258)
14 years	**0.3448***	**0.2183***	**0.2227***	**0.0819**	**0.2494***	**0.1327***	**0.1515***	**0.0652**
	(0.0261)	(0.0398)	(0.0386)	(0.0265)	(0.0242)	(0.0375)	(0.0363)	(0.0247)
15 years	**0.2834***	**0.2266***	**0.2278***	0.0229	**0.2016***	**0.1354**	**0.1457**	0.0277
	(0.0314)	(0.0479)	(0.0465)	(0.0315)	(0.0302)	(0.0459)	(0.0445)	(0.0303)
16 years	**0.3254***	**0.1597**	**0.1660***	**0.1029*#**	**0.2465***	0.0844	**0.1048**	**0.0913***
	(0.0279)	(0.0458)	(0.0432)	(0.0285)	(0.0260)	(0.0438)	(0.0412)	(0.0267)
17 years	**0.5991*#**	**0.3781*#**	**0.3830*#**	**0.1411***	**0.4931*#**	**0.2732*#**	**0.2993*#**	**0.1276***
	(0.0291)	(0.0517)	(0.0477)	(0.0302)	(0.0268)	(0.0490)	(0.0448)	(0.0280)
18 years	**0.4781*#**	**0.3300***	**0.3319***	0.0747	**0.3444*#**	**0.2125***	**0.2475***	0.0450 #
	(0.0372)	(0.0634)	(0.0595)	(0.0387)	(0.0354)	(0.0613)	(0.0572)	(0.0370)
Linearity test (F-test for OLS and FE, Wald-test for SURE)								
Statistic	10.44	3.56	28.97	11.63	11.09	3.45	28.88	9.33
P-value	0.0001	0.0004	0.0003	0.1684	0.0001	0.0006	0.0003	0.3153

Bold numbers if significantly different from zero, * if significantly different from 12 years of schooling and # if x years of schooling is significantly different from x-1 years of schooling, all at 5 percent level. Controls in Table A3 are included.

Table A6. Estimated non-linear return to schooling. Female twins.

	Annual earnings OLS	Fixed effect	SURE Own education	SURE Sum education	Hourly earnings OLS	Fixed effect	SURE Own education	SURE Sum education
10 years	**0.0691*#**	0.0190*	0.0215*	0.0300*	**0.0887*#**	0.0047	0.0111	**0.0554 #**
	(0.0193)	(0.0323)	(0.0322)	(0.0204)	(0.0239)	(0.0396)	(0.0394)	(0.0253)
11 years	**0.1614*#**	**0.1053 #**	**0.1027 #**	0.0360	**0.1301**	0.0911 #	0.0957 #	0.0159
	(0.0254)	(0.0409)	(0.0406)	(0.0263)	(0.0308)	(0.0500)	(0.0497)	(0.0323)
12 years	**0.2283 #**	**0.1186**	**0.1150**	**0.0757**	**0.1583**	0.0723	0.0716	**0.0586**
	(0.0228)	(0.0412)	(0.0409)	(0.0250)	(0.0265)	(0.0495)	(0.0492)	(0.0299)
13 years	**0.2602**	**0.1639**	**0.1568**	**0.0624**	**0.2069**	0.0951	0.0941	**0.0823**
	(0.0265)	(0.0475)	(0.0472)	(0.0291)	(0.0310)	(0.0553)	(0.0549)	(0.0341)
14 years	**0.4018*#**	**0.2915*#**	**0.2840*#**	**0.0731**	**0.3061*#**	**0.2441*#**	**0.2417*#**	0.0390
	(0.0306)	(0.0525)	(0.0523)	(0.0332)	(0.0331)	(0.0580)	(0.0576)	(0.0363)
15 years	**0.3995***	**0.2902***	**0.2788***	**0.0754**	**0.2636***	**0.2300***	**0.2236***	0.0206
	(0.0275)	(0.0508)	(0.0501)	(0.0305)	(0.0309)	(0.0572)	(0.0565)	(0.0346)
16 years	**0.4167***	**0.3383***	**0.3244***	0.0480	**0.2723***	**0.2017***	**0.2065***	0.0384
	(0.0295)	(0.0549)	(0.0540)	(0.0327)	(0.0321)	(0.0618)	(0.0603)	(0.0361)
17 years	**0.7100*#**	**0.5590*#**	**0.5329*#**	**0.1157**	**0.5276*#**	**0.3809*#**	**0.3905*#**	0.0848
	(0.0479)	(0.0799)	(0.0788)	(0.0505)	(0.0509)	(0.0889)	(0.0871)	(0.0550)
18 years	**0.7337***	**0.5527***	**0.5305***	**0.1365**	**0.4434***	**0.3120***	**0.3033***	0.1043
	(0.0530)	(0.0865)	(0.0860)	(0.0558)	(0.0515)	(0.0836)	(0.0828)	(0.0550)
Linearity test (F-test for OLS and FE, Wald-test for SURE)								
Statistic	4.12	1.36	9.99	7.37	3.48	1.56	12.40	12.50
P-value	0.0001	0.2108	0.2655	0.4969	0.0005	0.1341	0.1343	0.1304

Bold numbers if significantly different from zero, * if significantly different from 12 years of schooling and # if x years of schooling is significantly different from x-1 years of schooling, all at 5 percent level. Controls in Table A3 are included.