

Why the Apple Doesn't Fall Far: Understanding Intergenerational Transmission of Human Capital

By SANDRA E. BLACK, PAUL J. DEVEREUX, AND KJELL G. SALVANES*

Parents with higher education levels have children with higher education levels. Why is this? There are a number of possible explanations. One is a pure selection story: the type of parent who has more education and earns a higher salary has the type of child who will do so as well, regardless. Another story is one of causation: obtaining more education makes one a different type of parent, and thus leads to the children having higher educational outcomes.

Distinguishing between these scenarios is important from a policy perspective. One of the key roles of publicly provided education in our society is to increase equality of opportunity, and many policies have been implemented to further that goal in recent years.¹ A possible benefit of this type of education policy is the spillover effect on later generations; having more educated citizens may have longer-run effects by improving the outcomes of their children. The research to date has been limited,

however, in its ability to distinguish between selection and causation.

This paper proposes to provide evidence on the causal link between parents' and children's education by using a unique dataset from Norway. During the 1960s, there was a drastic change in compulsory schooling laws in Norway. Pre-reform, children were required to attend school through the seventh grade; after the reform, this was extended to the ninth grade, adding two years of required schooling. Additionally, implementation of the reform occurred in different municipalities at different times, starting in 1960 and continuing through 1972, allowing for regional as well as time-series variation. Evidence in the literature suggests that these reforms had a large and significant impact on educational attainment which, in turn, led to a significant increase in earnings.² As a result, the reform provides variation in parental education that is exogenous to parental ability and enables us to determine the impact of increasing parental education on children's schooling. Although the instrument allows us to determine only the impact of increasing parental education from seven to nine years, this may be an important starting point for identifying the intergenerational transmission of education.

Using this reform as an instrument for parental education, we find little evidence of a causal relationship between father's education and children's education, despite significant and large OLS relationships. We find a significant causal relationship between a mother's education and her son's education but no causal relationship between a mother's education and her daughter's education. This suggests that high correlations between parental and children's education are due primarily to selection and not causation. It is important to note that we use

* Black: Department of Economics, University of California at Los Angeles, 8283 Bunche Hall, Los Angeles, CA 90095-1477, Institute for the Study of Labor (IZA), and National Bureau of Economic Research (e-mail: sblack@econ.ucla.edu); Devereux: Department of Economics, University of California at Los Angeles, 8283 Bunche Hall, Los Angeles, CA 90095-1477 and IZA (e-mail: devereux@econ.ucla.edu); Salvanes: Department of Economics, Norwegian School of Economics and Business Administration, Helleveien 30 N-5045, Bergen, Norway, Statistics Norway, and IZA (e-mail: kjell.salvanes@nhh.no). The authors would like to thank Marina Bassi for helpful research assistance. We also thank Maia Guell, Oskar Nordström Skans, and participants at the European Summer Symposium of Labour Economics (ESSLE), the Society of Labor Economists Annual Meeting, ITAM, the CEPR Uppsala Workshop, Columbia University, Brown University, Duke University, University of Florida, University of Arizona, Florida International University, Federal Reserve Bank of Cleveland, Centre de Recherche en Economie et Statistique (CREST), and University College London for helpful suggestions. Black and Devereux gratefully acknowledge financial support from the National Science Foundation and the California Center for Population Research.

¹ For example, the No Child Left Behind program supported by President George W. Bush.

² See Arild Aakvik et al. (2003). Results on the impact of similar reforms on educational attainment also exist for Sweden (see Meghir and Palme, 2003), and for England and Ireland (see Harmon and Walker, 1995; Oreopoulos, 2003).

Norwegian data. While Norway is similar to the United States and the United Kingdom in terms of educational attainment and educational institutions, its labor market institutions are more similar to other European countries.

The paper unfolds as follows: Sections I and II discuss relevant literature and describe the Norwegian education reform. Sections III and IV describe our empirical strategy and data. Section V discusses the effect of the Norwegian education reform on educational attainment and earnings and Section VI presents our results. Section VII presents some specification checks and Section XIII concludes.

I. Background Information

The recent literature has taken three broad approaches to identify the intergenerational transmission of human capital: identical twins, adoptees, and instrumental variables.³ Jere R. Behrman and Mark R. Rosenzweig (2002) use data on pairs of identical twin parents to “difference out” any correlation attributable to genetics. Despite observing a positive correlation between a mother’s education and her child’s education, the authors find a negative and almost significant relationship between a mother’s schooling and her child’s schooling once one looks within female monozygotic twin pairs, thereby differencing out any genetic factors that influence children’s schooling. The analogous fixed-effects exercise using male monozygotic twin pairs gives coefficients for a father’s education that are about the same size as the OLS estimates. Recent work by Kate Antonovics and Arthur S. Goldberger (2003), however, calls into question these results and suggests that the findings are quite sensitive to the coding of the data. Also, it may be unrealistic to assume that twins differ in terms of education but not in terms of any other characteristic or experience that may influence the education of their offspring.⁴

Erik Plug (2004) uses data on adopted chil-

dren to investigate the causal relationship between parental education and child education. If children are randomly placed with adoptive parents, the relationship between parental education and child education cannot simply reflect genetic factors. Plug finds a positive effect of the father’s education on the child’s education, but no significant effect for a mother’s education. Unfortunately, there are a number of limitations of this approach: sample sizes are tiny, children are not randomly placed with adoptive parents, and the correlation between parents’ education and children’s education could be picking up the effects of any unobserved parental characteristic (patience, ability) that may influence child outcomes.⁵

Closest to our paper is work by Arnaud Chevalier (2004) and Philip Oreopoulos et al. (2003), who use changes in compulsory schooling laws to identify the effect of parental education on their children’s educational outcomes.⁶ Chevalier uses a change in the compulsory schooling laws in Britain in 1957 and finds a large positive effect of a mother’s education on her child’s education but no significant effect of father’s education. This paper suffers, however, from the fact that the legislation was implemented nationwide. As a result, the identifying variation in parental education arises both from secular trends in education and the once-off change in the law.⁷ Second, the sample includes only children who are still living at home with their parents and hence loses observations in a non-random fashion. Oreopoulos et al. use compulsory schooling legislation in the United States

⁵ Bruce Sacerdote (2002) also uses adoptees to distinguish the effect of family background on children’s outcomes from genetic factors. The focus of his paper, however, is the general impact of family socioeconomic status as opposed to the causal impact of parents’ education.

⁶ Katherine Magnuson (2003) uses random assignment into a “human capital development” program for welfare mothers as an instrument for mother’s educational attainment and finds evidence of an effect of mother’s education on children’s academic school readiness. Her work, however, examines the impact on young children and does not address the question of how it affects children’s ultimate education decisions.

⁷ Ignoring the existence of cohort effects may be a particular problem in this context, as less-educated individuals are more likely to have children while young, and so in a sample of individuals with children of a certain age, older individuals are likely to have more education. Thus, one would like to control for unrestricted age effects for parents.

³ More generally, there is a huge literature in both economics and sociology that studies intergenerational persistence of socioeconomic status. See, for example, Solon (1999) and Hauser and Logan (1992).

⁴ See Griliches (1979) and Bound and Solon (1999) for demonstrations that biases using sibling and twin fixed effects may be as big or bigger than OLS biases.

(which occurred in different states at different times) to identify the effect of parents' educational attainment on children's educational attainment. As a result, they are able to circumvent the problem encountered by Chevalier of coincident time effects. They find that increasing the education of either parent has a significant negative effect on the probability a child will repeat a grade. Because of limitations of the U.S. census data, however, they are able to look only at children's early behavior as their outcome measure. A unique aspect of our work is that we are able to follow children even after they have moved out of their parents' home and observe final educational attainment.

II. The Norwegian Primary School Reform

In 1959, the Norwegian Parliament legislated a mandatory school reform. Prior to the reform, children started school at the age of seven and finished compulsory education after seven years, at the age of 14. In the new system, the starting age was still seven years, but the time spent in compulsory education was now nine years.

In addition, the reform standardized the curriculum and increased access to schools, since the nine years of mandatory school was eventually made available in all municipalities. The goal of standardizing the curriculum was to improve the average level of quality of the schools; the increase in mandatory education was therefore likely accompanied by an improvement in school quality. As a result, our estimates will incorporate both the increase in years of education and the improvement in the quality. Given the positive correlation between the two, we will likely *overestimate* the effect of extra years of education on children's educational attainment.

Parliament mandated that all municipalities (the lowest level of local administration) implement the reform by 1973. As a result, although it was started in 1960, implementation was not completed until 1972.⁸ Thus, for more than a

decade, Norwegian schools were divided into two separate systems; the system you were in depended on the year you were born and the municipality in which you lived. The first cohort that could have been involved in the reform was the one born in 1947. These children started school in 1954, and either finished the pre-reform compulsory school in 1961 or went to primary school from 1954 to 1960, followed by the post-reform middle school from 1960 to 1963. The last cohort that could have gone through the old system was born in 1958.⁹

To receive funds from the government to implement the reform, municipalities had to present a plan to a committee under the Ministry of Education. Once approved, funding for teachers and buildings was provided by the national government. While the criteria determining selection are somewhat unclear, the committee did want to ensure that implementation was representative of the country, conditional on having an acceptable plan (Arne O. Telhaug, 1969; Olav A. Mediås, 2000).

Because we control for municipality fixed effects, it is not necessary that the timing of the reform be unrelated to municipality characteristics. It is useful, however, to understand the determinants of the timing of the reform across municipalities. Previous research has found no relationship between such municipality characteristics as average earnings, taxable income, and educational levels, and the timing of implementation (see Suzanne S. Lie 1973, 1974). To examine this issue further, in Table 1A in the Appendix we regress the year of implementation on different background variables based on municipality averages, including parental income, the level of education, average age, the size of the municipality, and county dummies (there are 20 counties in Norway). Consistent with the existing literature, there appears to be no systematic relationship between the timing of implementation and average earnings, education levels, average age, the fraction of individuals with fewer than nine years of schooling, urban/rural status, industry or labor force composition, municipality unemployment rates in 1960, or the share of individuals who were

⁸ The reform had already started on a small and exploratory basis in the late 1950s, but it applied to a negligible number of students because only a few small municipalities, each with a small number of schools, were involved. See Lie (1974), Telhaug (1969), and Lindbekk (1992) for descriptions of the reform.

⁹ Similar school reforms were undertaken in many other European countries in the same period, notably Sweden, the United Kingdom, and, to some extent, France and Germany (Leschinsky and Mayer, 1990).

members of the Labor Party (the most pro-reform of the dominant political parties).

III. Identification Strategy

Our source of exogenous variation in parental education is the education reform in Norway that increased the number of years of compulsory schooling from seven to nine years and was implemented over a 12-year period from 1960 to 1971 in different municipalities at different times. We then observe the children of this generation in 2000.

Our empirical model is summarized by the following two equations:

$$(1) \quad ED = \beta_0 + \beta_1 ED^p + \beta_2 AGE \\ + \beta_3 FEMALE + \beta_4 AGE^p \\ + \beta_5 MUNICIPALITY^p + \varepsilon$$

$$(2) \quad ED^p = \alpha_0 + \alpha_1 REFORM^p + \alpha_2 AGE \\ + \alpha_3 FEMALE + \alpha_4 AGE^p \\ + \alpha_5 MUNICIPALITY^p + v.$$

In equations (1) and (2), *ED* is the number of years of education obtained by the child, *AGE* refers to a full set of years of age indicators, *MUNICIPALITY* refers to a full set of municipality indicators, and *REFORM* equals 1 if the individual was affected by the education reform and 0 otherwise. In all cases, the superscript *p* denotes parent, so, for example, *AGE^p* refers to a full set of indicator variables for the age of the parent. We estimate the model using Two Stage Least Squares (2SLS) so that equation (2) is the first stage and *REFORM^p* serves as an instrumental variable for *ED^p*.

There are two points to note about equations (1) and (2). First, both equations contain fixed cohort effects (to allow for secular changes in educational attainment over time) and municipality effects for parents. Even if the reform was implemented first in areas with certain unobserved characteristics, consistent estimation is still achieved so long as: (a) these characteristics are fixed over time during the 12-year period; (b) implementation of the reform is not correlated with changes in these characteristics;

or (c) these characteristics are not related to the schooling of the *children* of this generation.¹⁰

Second, we have included age indicators for the children to allow for the fact that not all children in our sample have finished schooling by 2000. The child's age may be endogenous because the parents choose the timing of births; therefore, in Section VII, we report estimates for specifications that exclude child age controls.

A. Restricting the Sample

Because the primary effect of the reform is at the bottom of the educational distribution, we conduct much of our analysis on the sample of mothers/fathers who have nine years or less of education. The additional assumptions we make in doing this are that individuals who get nine years of education after the reform would have received nine years or less of education if the reform had not been in effect, and that individuals who got nine years or less of education before the reform would have received nine years of education if the reform had been in effect.¹¹

In return for making these additional assumptions and restricting the sample, we are able to estimate a much stronger first stage and obtain more precisely estimated second-stage coefficients.¹² In Section VII, we describe features of the data that suggest that our assumptions are not unreasonable.

IV. Data

Our data come from linked administrative data that cover the entire population of Norwegians aged 16 to 74.¹³ We include cohorts of parents born between 1947 and 1958 in our sample. The sample of children includes the children of these parents who are age 25 to 35 in

¹⁰ We have also tried allowing for municipality-specific time trends as well as county-by-year fixed effects. Our results were insensitive to the inclusion of these extra variables.

¹¹ This second assumption rules out spillover effects of the reform of the sort that some signaling models imply.

¹² We have also tried using characteristics of the parents to split the sample based on predicted parental education rather than actual parental education. This approach gave us estimates that are consistent with the ones we report but were very imprecisely estimated.

¹³ See Møen et al. (2003) for a description of the data set.

TABLE 1—SUMMARY STATISTICS

	Observations	Mean	Std. dev.
Children			
Age	172,671	27.78	2.34
Education	172,671	12.20	1.97
Earnings	172,671	202,370	138,908
Sex (female/male)	172,671	0.49	0.50
Mothers			
Age	143,579	49.55	2.63
Education	143,579	10.46	2.23
Earnings	140,839	171,417	119,193
Fathers			
Age	96,275	50.51	2.18
Education	96,275	11.01	2.54
Earnings	92,738	309,819	227,025

Notes: Sample includes children who were between the ages of 25 and 35 in 2000 who had at least one parent born during the reform period. Parental age and education are reported only for parents who are members of the reform cohorts.

2000. Note that a great advantage of our dataset over others in the literature is that we can link adult children in 2000 to characteristics of their parents, even in cases where the children do not live with their parents. Table 1 provides summary statistics for the individuals in our sample.¹⁴

To determine whether parents were affected by the reform, we need to link each parent to the municipality in which he or she grew up. We do this by matching the administrative data to the 1960 census. From the 1960 census, we know the municipality in which the parent's mother lived in 1960.¹⁵ At that time, the parents we are using in the estimation are between the ages of 2 and 13.¹⁶

Educational attainment is reported by the ed-

¹⁴ Note that it sometimes occurs that one parent is in our sample while the other is not because only one of them is born during the 1947–1958 period.

¹⁵ Since very few children live with their father in the cases where parents are not living together, we should have only minimal misclassification by applying this rule.

¹⁶ One concern is that there may be selective migration into or out of municipalities that implement the reform early. Since the reform implementation did not occur before 1960, however, this could be a problem for us only to the extent that families anticipated where the reform would be implemented first and made mobility decisions prior to the 1960 Census. Any reform-induced mobility subsequent to 1960 may affect the precision of our 2SLS estimates but not their consistency. Evidence from Meghir and Palme (2003) for Sweden and Telhaug (1969) for Norway suggest that reform-induced migration was not a significant consideration.

ucational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. The education register started in 1970; we use information from the 1970 Census for parents who completed their education before then. Thus, the register data are used for children and all but the earliest cohorts of parents who did not get any additional education after 1970. Census data are self-reported, but the information is considered to be very accurate. There are no spikes or changes in the education data from the early to the later cohorts.

Our primary data source on the timing of the reform in individual municipalities is the volume by Erik Ness (1971). To verify the dates provided by Ness, we examined the data to determine whether there appears to be a clear break in the fraction of students with fewer than nine years of education. In the rare instance when the data did not seem consistent with the timing stated in Ness, we checked these individual municipalities by contacting local sources. If the reform took more than one year to implement in a particular municipality, or if we were not able to verify the information given in Ness (1971), we could not assign a reform indicator to that municipality and the municipality was dropped from our sample. We are able to successfully calculate reform indicators for 545 out of the 728 municipalities in existence in 1960 (which constitutes 74 percent of the individuals in our sample).

V. The Effects of the Reform on Educational Attainment and Earnings

There is a significant literature demonstrating the effect of compulsory schooling laws on educational attainment.¹⁷ In the case of the Norwegian reform, the increase in compulsory schooling had a significant effect on educational attainment at the bottom of the distribution. Table 2 shows the distribution of education averaged over the two years prior to the reform and the two years immediately following the reform, including the year the reform was implemented. It is clear from this table that the

¹⁷ See, for example, Acemoglu and Angrist (2001) for work on the United States, and Oreopoulos (2003) for work on Europe.

TABLE 2—DISTRIBUTION OF EDUCATION TWO YEARS BEFORE AND AFTER THE REFORM

Years of education	Before	After
7	3.5%	1.2%
8	8.9%	1.6%
9	3.4%	12.9%
10	29.6%	26.6%
11	8.5%	8.8%
12	17.2%	19.1%
13	6.7%	6.7%
14	5.4%	5.8%
15	2.7%	3.4%
16+	14.2%	14.1%
<i>N</i>	89,320	92,227

Notes: Before indicates education distribution of cohorts in the two years prior to the reform, while After indicates the distribution of those two years post reform. Note that because the reform occurred in different municipalities at different times, the actual year of the reform varies by municipality.

primary effect of the reform was to reduce the proportion of people with fewer than nine years of education from 12 percent to 3 percent, with a new spike at nine years.¹⁸

There is also substantial evidence that the additional education induced by the reform has a positive and statistically significant effect on earnings. OLS results for the return to education in Norway are about 0.07; 2SLS estimates for our cohorts using the reform as an instrument give estimates of 0.040 (0.013) for men and 0.050 (0.016) for women. Thus, the return to

¹⁸ The presence of some individuals with fewer than nine years of schooling when the reform is in place reflects the fact that there was not 100-percent compliance with the law and some individuals dropped out before completing compulsory schooling. It may also reflect the fact that, in some municipalities, the reform was implemented over several years, or possible error in the dating of reform implementation. These factors will tend to reduce the precision of our estimates without affecting consistency. Pre-reform, students could choose between a three-year or five-year high school track after completing the seven years of compulsory schooling. After the reform, this choice no longer existed and the standard high school track involved three years after the compulsory nine years of schooling. As a result, the educational distribution appears to have a “hole” at ten years of education after the reform (see Table 2). Individuals who would have done the three-year high school track before the reform would now ultimately achieve 10, 11, or 12 years of schooling. This is consistent with the fact that the proportion of individuals with 10 to 12 years of education is similar before and after the reform.

TABLE 3—RELATIONSHIP BETWEEN PARENTS’ AND CHILDREN’S EDUCATION

	Dependent variable: Children’s education			
	Full sample		Parent’s education <10	
	OLS	IV	OLS	IV
Mother—all	0.237* (0.003)	0.076 (0.139)	0.211* (0.017)	0.122* (0.043)
	<i>N</i> = 143,579		<i>N</i> = 39,605	
Mother—son	0.212* (0.004)	0.199 (0.185)	0.197* (0.021)	0.176* (0.054)
	<i>N</i> = 73,663		<i>N</i> = 20,135	
Mother—daughter	0.264* (0.004)	−0.029 (0.186)	0.225* (0.023)	0.066 (0.063)
	<i>N</i> = 69,916		<i>N</i> = 19,470	
Father—all	0.217* (0.003)	0.030 (0.132)	0.200* (0.021)	0.041 (0.062)
	<i>N</i> = 96,275		<i>N</i> = 22,148	
Father—son	0.209* (0.004)	0.029 (0.171)	0.151* (0.027)	0.008 (0.071)
	<i>N</i> = 49,492		<i>N</i> = 11,235	
Father—daughter	0.226* (0.004)	0.022 (0.186)	0.244* (0.033)	0.081 (0.094)
	<i>N</i> = 46,783		<i>N</i> = 10,913	

Notes: Sample includes children aged 25–35. Robust standard errors in parentheses. Each estimate represents the coefficient from a different regression. All specifications include dummies for parent’s age, parent’s municipality and child’s age.

* Significant at 5-percent level.

reform-induced education is both positive and statistically significant. Arild Aakvik et al. (2003) examine this issue in more detail and find heterogeneity in the returns, with returns as high as 0.10 for some groups.

VI. Results

A. Results for the Full Sample

The OLS results for equation (1) are presented in Table 3, column 1. As expected, we find a positive relationship between the years of education of the parents and their child’s education.¹⁹ This is true, regardless of whether we

¹⁹ In both the OLS and 2SLS analysis we report robust standard errors that allow for clustering at the parent’s municipality–parent’s cohort level. To deal with possible concerns about the effects of serial correlation on the standard errors, we have also experimented with clustering by parent’s municipality and found the 2SLS standard errors to be almost identical.

TABLE 3A—FIRST-STAGE RESULTS

	Full sample of parents		Parents' education <10 years	
	Mother's education	Father's education	Mother's education	Father's education
All	0.142* (.029)	0.192* (.042)	0.749* (.017)	0.795* (.024)
Sons	0.127* (.035)	0.196* (.051)	0.742* (.019)	0.814* (.029)
Daughters	0.161* (.036)	0.197* (.050)	0.755* (.019)	0.779* (.027)

Notes: Each estimate represents the coefficient from a different regression. Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age.

* significant at 5-percent level.

match mothers to sons, mothers to daughters, fathers to sons, or fathers to daughters. Our estimates suggest that increasing a parent's education by a year increases the child's education by about 0.20 to 0.25 of a year.²⁰ While the sample size varies, particularly between the father and mother regressions (due to the fact that many fathers are too old to be affected by the reform and our inability to match fathers who were not living with the family at the time of the 1960 census), our estimates are quite similar across samples.

Column 2 presents 2SLS results, where the instrument is the indicator for whether or not the father/mother was affected by the school reform in Norway. The 2SLS results are imprecisely estimated and are all statistically insignificant. The main reason for the lack of precision is the relatively weak first-stage relationship between the reform and years of education of the father/mother: the t-statistics for the reform indicator in the first stage are about five. (See Table 3a for the first stage estimates.) These relatively small t-statistics result from the fact that the reform is affecting only the relatively small fraction of the population with nine or fewer years of education. It is clear that to use the reform effectively as a source of exogenous variation, one needs to focus on the very bottom tail of the education distribution, where the reform has bite.

²⁰ This is consistent with the general findings in this literature; results from the United States and United Kingdom suggest intergenerational education elasticities between 0.20 and 0.45 (Dearden et al., 1997; Mulligan, 1999).

B. Results for the Restricted Sample

The results for the sample of parents with nine or fewer years of education are in columns 3 and 4 of Table 3, and the first stage estimates are in Table 3a. The OLS estimates are quite similar to those obtained from the full sample. However, consistent with the evidence presented earlier, the first stage for the low education sample is much stronger than that for the full sample.

As expected, the 2SLS estimates (column 4) are quite similar to the results for the full sample, but much more precisely estimated. For fathers, the estimates are all close to zero, statistically insignificant, and the father-all and father-son estimates are statistically different from the OLS estimates. For mothers, there is a positive effect of maternal education on the education of sons but no such relationship for daughters (the mother-daughter coefficient is also statistically different from the OLS coefficient). Taken as a whole, the results indicate that the positive correlation between parents' education and children's education largely represents positive relationships between other factors that are correlated with education. These could be ability, family background, income, or other factors. The true causal effect of parental education on child education appears to be weak.²¹

Figure 1 provides a visual representation of our results for the restricted sample, presenting the effects of the reform on parents' education (the first stage), as well as the effects of the reform on the children (the reduced form) after taking out municipality and cohort effects.²² Time zero represents the year of implementation of the reform. We see that the reform did have a large impact on parents' educational attainment. It is also clear, however, that the effect of the reform on children's educational attainment is small, with only the mother/son pair demonstrating any real relationship.

²¹ We also estimated equations with the education of both parents included. In this case, IV estimates are identified off of the fact that many individuals are of a different age or grew up in a different municipality than their spouse. Results are similar in that we find a positive effect of maternal education but no effect of father's education.

²² Note that individual points should be interpreted with caution, as there is substantial sampling error.

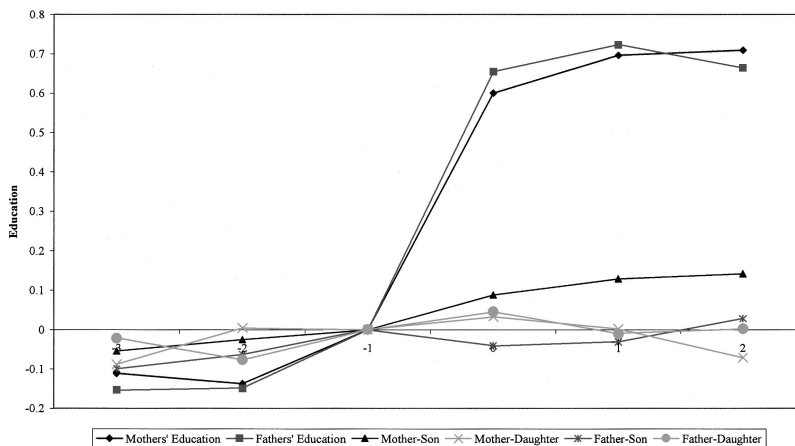


FIGURE 1. EFFECT OF NORWEGIAN EDUCATION REFORM ON EDUCATION FIRST STAGE (EFFECT ON PARENTS) REDUCED FORM (EFFECT ON CHILDREN)

Notes: Estimated on the restricted sample. Lines represent average education for each group with cohort and municipality effects taken out; time zero represents the year of the reform.

Our finding that the IV estimates are smaller than the OLS estimates is intuitive in that we expect education choice to be positively correlated with unobserved ability. This finding is not, however, in keeping with much of the returns to education literature. Typically, IV estimates are found to be larger than OLS estimates. We suspect a few reasons for this divergence. First, our education data are of very high quality and probably have little or no measurement error. Thus, unlike in other studies, our OLS estimates may not be subject to downward biases due to measurement error. Second, our use of an education reform and our ability to control for both cohort and municipality effects leads to greater confidence that the instrument is not correlated with unobserved ability, and therefore our IV estimates are not upward biased. Finally, high IV estimates in the endogenous education literature are often rationalized by heterogeneous returns to education with particularly high returns for the group of people whose behavior is affected by the instrument being used. Because credit constraints are unlikely to have been a major determinant of educational choice in the lower tail of the Norwegian distribution at this time, it is plausible that the returns to education for individuals affected by the reform are no higher than average.

VII. Robustness/Specification Checks

Having found little causal effect of parents' education on children's educational attainment, we next conduct a number of robustness checks to verify our findings (see Table 4).²³ First, because education may have an impact on the timing of childbearing, children's age may be endogenous. In columns 1 and 2, we reestimated the specifications excluding child's age from the regression. As one can see from the results, this does not affect our conclusions. A related issue is whether the reform affects the decision to have children; in this case, the parents in our sample who have children are a selected group and our 2SLS estimates may be biased. We checked this possibility by examining whether the reform affects the probability that a potential parent ends up in our sample (by having at least one child age 25 or more in 2000) and found no evidence of this.

A second concern is that inaccurate measurement of the exact timing of the reform, or lags in implementation (given the necessity to build new infrastructure), could bias the 2SLS estimates. To check this, we have tried dropping all

²³ Unless otherwise specified, we are focusing on the restricted sample in this section.

TABLE 4—ROBUSTNESS CHECKS

	Dropping child's age		Dropping reform year along with years before and after reform		Early cohorts (<1953)		Dropping teenage parents		Bottom 20% of parents' distribution	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Mother—all	0.217* (0.017) N = 39,605	0.127* (0.043)	0.232* (0.021) N = 30,847	0.123* (0.057)	0.245* (0.021) N = 29,374	0.128 (0.062)	0.221* (0.020) N = 27,900	0.076 (0.054)	0.301* (0.016) N = 26,362	0.079 (0.059)
Mother—son	0.201* (0.021) N = 20,135	0.182* (0.054)	0.200* (0.027) N = 15,609	0.179* (0.073)	0.221* (0.026) N = 14,944	0.243* (0.075)	0.210* (0.025) N = 14,156	0.139* (0.065)	0.274* (0.020) N = 13,466	0.141* (0.070)
Mother—daughter	0.233* (0.023) N = 19,470	0.072 (0.063)	0.269* (0.029) N = 15,238	0.056 (0.085)	0.262* (0.029) N = 14,430	0.014 (0.088)	0.230* (0.029) N = 13,744	-0.002 (0.079)	0.308* (0.023) N = 12,883	-0.003 (0.083)
Father—all	0.202* (0.022) N = 22,148	0.040 (0.062)	0.220* (0.027) N = 18,223	0.017 (0.102)	0.213* (0.024) N = 19,622	0.093 (0.072)	0.206* (0.022) N = 20,352	0.039 (0.065)	0.208* (0.015) N = 17,317	-0.016 (0.079)
Father—son	0.153* (0.027) N = 11,235	0.009 (0.071)	0.175* (0.034) N = 9,278	0.002 (0.131)	0.163* (0.031) N = 9,980	-0.004 (0.082)	0.154* (0.029) N = 10,316	0.020 (0.077)	0.190* (0.020) N = 8,752	0.011 (0.101)
Father—daughter	0.247* (0.033) N = 10,913	0.078 (0.094)	0.259* (0.041) N = 8,945	0.068 (0.139)	0.256* (0.037) N = 9,642	0.183 (0.111)	0.254* (0.034) N = 10,036	0.050 (0.099)	0.261* (0.023) N = 8,465	-0.084 (0.125)

Notes: Sample includes children aged 25–35. Robust standard errors in parentheses. Each estimate represents the coefficient from a different regression. All specifications include dummies for parent's age, parent's municipality, and child's age (unless otherwise specified). In columns (1) to (8), the sample used is the sample of parents with fewer than ten years of education.

* significant at 5-percent level.

observations from the reform year and the years immediately preceding and following it. As can be seen, however, from Table 4, columns 3 and 4, this change in sample had little effect on the results.

A third potential concern is that, because of the timing of the reform, we observe only those children of parents who had children relatively young. While there is little that we can do to remedy this, we can test the sensitivity of our results by using only the early cohorts of parents. If the results are similar to those from the full sample, it suggests they are unlikely to be biased by this constraint. Table 4, columns 5 and 6, present the results using only the first six of our 12 cohorts (parents born before 1953). While the mother-son estimate is slightly larger than before, the overall conclusions are the same.

As an additional check on this issue, we have also carried out the analysis after dropping all teenage parents from the sample. The OLS and IV estimates (presented in columns 7 and 8 of

Table 4) are little changed by this additional sample restriction.

Next, we address two potential censoring concerns. First, children of low-education parents may always get the minimum education mandated by law and, as a result, we would see all these children clustered at nine years of education. This would cause our estimates for this group to be close to zero even when the "true" effect of parental education on desired education is larger. There is, however, only a small density at nine years of education (around 9 percent), so this is unlikely to be a problem.²⁴

The second concern is that some individuals have not completed schooling by age 25 (approximately 7 percent in our restricted sample). We have tested the sensitivity of our results to estimating the relationship between parents'

²⁴ Estimates were also unaffected when we treated these observations as left censored and applied a Tobit IV approach.

education and children's education using instrumental variables in a Tobit framework; the results are very similar to those presented here, so censoring bias does not seem important.

A. *The Validity of Restricting the Sample to Parents with Fewer Than Ten Years of Education*

Finally, as discussed earlier, the estimates from the restricted sample may be biased if there are systematic changes in the composition of the group of parents with fewer than ten years of education. These could arise if the proportion of individuals with fewer than ten years changes after reform implementation. We see in Table 2, however, that the proportion of individuals with nine years or less of education stays constant when we compare two years before to two years after the reform.

To investigate this issue further, we test whether, conditional on cohort and municipality effects, the proportion of individuals with fewer than ten years of education in municipality-year cells is related to the reform. We find no statistically significant effect of the reform once we exclude observations from the reform year and the years immediately preceding and following reform implementation. This suggests that there may be no significant spillover effects of the reform; those who obtained nine or fewer years of education before the reform would have continued to do so after the reform.

Additionally, we have examined the family background characteristics of the individuals (parents) with nine or fewer years of education in the years before and after the reform to see if the composition of our sample appears to have changed. If, for example, there were positive spillover effects of the reform, we might expect to see the post-reform individuals with nine or fewer years of education looking observably "worse" than those prior to the reform. The variables we can look at include the log of family income (from the 1970 Census) and the educational attainment of the mothers and fathers (of our parents). When we regress each of these variables on the reform indicator along with cohort and municipality effects for the sample of individuals with nine or fewer years of education, we find no evidence of any compositional change after the reform.

As a more rigorous test for composition bias,

we have reestimated the specifications using a sample of the lowest 20 percent of the education distribution in each municipality in each year (breaking ties randomly so that we have exactly 20 percent of observations per municipality). This approach involves weaker assumptions than the sample split of fewer than ten years of education in that it does not require the proportion of fewer than ten to remain constant. What is required is that the implementation of the reform in a municipality has no systematic effect on the relative position in the educational distribution of individuals in that cohort in that municipality. The results, reported in columns 9 and 10 of Table 4, indicate that using the bottom 20 percent of the distribution gives quite similar results to using the fewer-than-ten sample split.

As a final check, we have also conducted our estimation on samples with higher educational cutoff points. When we look at the results obtained for the sample of parents with fewer than 12 years of education (or fewer than 13 years of education), they are as we would expect; the coefficient estimates are very similar to those from the sample of parents with fewer than ten years of education, but the standard errors are larger. These numerous checks suggest that our results are not being driven by the use of our restricted sample.

VIII. Conclusions

By using the increased educational attainment induced by the change in the compulsory schooling legislation in Norway, in combination with a unique dataset containing the entire population of the country, we are able to estimate the causal relationship between parents' education and that of their children. Despite strong OLS relationships, we find little causal relationship between parent education and child education. The one exception is among mothers and sons; when mothers increase their educational attainment, their sons get more education as well. These results are robust to a number of specification checks.

What explains these findings? In the working paper version of this paper (Black et al., 2003), we examined some of the possible mechanisms through which this relationship may be working, including whether the women who received more education due to the reform married better educated or wealthier men (they don't) and

whether these more highly educated women are making a quantity/quality tradeoff by having fewer children (they aren't). While we are able to rule out a few mechanisms, a number remain, including the most direct, which suggests that higher maternal education may reduce the cost (in terms of effort) of education for the child.²⁵

Our results provide limited support for inter-

generational spillovers as a compelling argument for compulsory schooling laws. It is important to remember, however, that we are studying an education reform that increased education at the bottom tail of the distribution. It is plausible that a policy change that increased enrollment in higher education would have been transmitted more successfully across generations. Also, our results from Norway may not generalize to countries that have more costly education and higher returns to skills. While these results are compelling, much more work needs to be done on this important topic.

²⁵ See Black et al. (2003) for a more complete discussion of the findings.

APPENDIX

TABLE 1A—TIMING OF THE IMPLEMENTATION OF THE REFORM
(*Dependent variable: Year of reform*)

	Coefficient	Standard error
Share of fathers with some college	5.05	5.59
Share of mothers with some college	21.98	11.32
Father's income (mean)	-0.004	0.005
Mother's income (mean)	-0.037	0.014
Father's age (mean)	-0.06	0.20
Mother's age (mean)	-0.19	0.25
Share of municipality with fewer than 9 years of education	0.18	1.23
Size of municipality/100	0.19	0.30
Unemployment rate 1960	-16.30	15.48
Share workers in manufacturing 1960	0.98	4.47
Share workers in private services 1960	5.74	8.18
Share labor vote 1961	1.57	2.80
Constant term	1980.51	9.69

Notes: Robust standard errors. All variables are municipality-level variables. Regression includes 19 county indicators.

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