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The impact of a primary school reform on educational stratification: A Norwegian study of neighbour and school mate correlations

Oddbjørn Raaum, Kjell G. Salvanes and Erik Ø. Sørensen*

Summary

■ School quality is hard to define and measure. It is influenced by not only school expenditures, but also characteristics that are hard to measure like norms and peer effects among teachers and pupils. Furthermore, family background and community characteristics are important in explaining educational outcomes. In this paper we study the composite effect of primary schools and neighbourhoods on adult educational attainment controlling for family characteristics. Instead of identifying the effect of specific neighbourhood and school characteristics on educational attainment, we focus on correlations in final years of schooling among neighbouring children and school mates. We find a clear trend of declining influence of childhood location over the 24 year period (birth cohorts 1947-1970). Then we ask whether a change in the compulsory school law extending the mandatory years of education, can explain this pattern. We find some effect of the primary school reform on the change in the neighbourhood effect. Motivated by the fact that neighbouring children typically go to the same school, we estimate school mate correlations for children born in the 1960s. The overall impact of factors shared by children who graduated from the same school at the age of 15/16 is negligible. The variation in "school quality" and the impact of peers on final educational attainment seem to have been very limited in Norway.■

JEL classification: I21, J13, R23.
Keywords: Families, neighbours, schools, educational reforms, peer-effects.

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The impact of a primary school reform on educational stratification: A Norwegian study of neighbour and school mate correlations

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There is a controversy both among researchers and among the public on whether school quality matters, how (much) it matters, why it matters, and for which outcomes it matters. Empirical results from many countries have shown that school resources only have a modest impact on student achievement, but a relatively stronger impact on adult educational attainment.[1] School quality is hard to define and measure. It is not only influenced by school expenditures, but also characteristics that are hard to measure like norms, attitudes and peer effects among teachers and pupils (Hoxby, 2000). Furthermore, family background is important, and it has become clear that community characteristics—peer effects and neighbourhood institutions—are important in explaining educational attainment and adult earnings (Solon, et al. 2000; Page and Solon, 2003; Raaum, Salvanes and Sørensen, 2001).

However, few studies focus on the fact the primary/lower secondary school constitutes an important factor shared by children growing up in the same neighbourhood. The three factors, family, neighbourhoods (both as peer influence and institutions), and schools, probably also interact strongly as inputs in the human capital production function. Therefore, the causal effects of the three factors

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[1] School inputs measured by e.g. expenditures and teacher-pupil ratios appear to have modest, if any, effect on student achievement such as marks and test scores (see Hanushek, 2003, and Krueger, 2003, for US results, and Bonesrønning, 2003, for Norway). However, the same type of school resources seem to have a stronger impact on post-school outcomes like final educational attainment and earnings, although these findings are controversial (Betts, 1996; Dearden, Ferri and Meghir, 2002; Dustmann, Rajan and Van Soest, 2003).

are hard to disentangle, partly because of family sorting into neighbourhoods and schools. In the present paper, we try to analyse the composite effect of primary schools and neighbourhoods, and attempt to assess their impact on the variance in adult educational attainment. Instead of identifying the effect of specific neighbourhood and school characteristics on educational attainment, we focus on correlations in the final years of schooling among neighbouring children and school mates.

The starting point for our analysis is twofold. First, in a previous study we found that the importance of family background was stable while the effect of neighbourhoods on educational attainment and earnings is significantly lower for the 1955-65 birth cohort as compared to individuals born 1945-55 (Raaum, Salvanes and Sørensen, 2001). Second, in the 1960s—which is the childhood period for which the neighbourhood effect was found to be weakened—a primary school reform took place in Norway, extending the mandatory level of schooling from seven to nine years. Pre-reform, the Norwegian school system required children to attend school from the age of seven to the age of fourteen. After the reform, this was extended to the age of sixteen by adding two more years of mandatory education. The reform took place over a twelve-year period with different municipalities adopting the new school system at different times, allowing for time variation as well as regional variation. Evidence in Aakvik, Salvanes and Vaage (2003) suggests that this reform increased the participation in the above mandatory education as well as the returns to education.[2] They also found that the importance of family income was slightly weakened for post-reform students as compared to pre-reform students.

Our approach in this paper is to use a unique data set for Norway on neighbourhoods, schools and parental background to analyse whether the school reform also had an impact on equalizing the opportunity across neighbourhoods.[3] In addition to aims such as increasing the minimum level of education, and smoothing the transition to higher education, an important aim was also to increase equal-

[2] See Meghir and Palme (2003) for an analysis of the similar Swedish reform that took place in the 1950s. They also find that the reform had an impact on participation rates in higher education as well as reducing the impact of family background.
[3] See Oreopoulos (2003), Lochner and Morietti (2001) and Pischke (2003) for other examples of analysing social returns, as opposed to private returns of educational reforms.

ity of opportunity along socio-economic and geographic dimensions. It is this latter aspect that we analyse in this paper. The question is whether the school reform reduced the importance of the local neighbourhood. This type of primary school reform took place at about the same time in many other European countries and we think that Norway is a good case for analysing social returns of primary educational reforms, since the potential impact is expected to be stronger and thus easier to measure in the case of Norway. It has been pointed out that the Norwegian reform along with the Swedish reform went further both in the unification of the comprehensive school system as well as in promoting equality of opportunities (Leschinsky and Mayer, 1990). We then analyse the effect of schools as a part of neighbourhood effects by estimating school mate correlations over time, both as unadjusted correlations and controlled for family sorting. A school mate correlation is an overall measure of neighbourhood and different types of school effects, including school resource and peer effect. Again the question is whether school mate correlations have been reduced over time.

The rest of the paper is organized as follows. In Section 1, we describe our approach. In Section 2 the data set, variable definitions and the educational reform are described. Section 3 provides the empirical results and the last section gives some concluding remarks.

1. Neighbour and school mate correlations

In order to study the impact of schools on adult educational attainment as measured by years of education, and to disentangle the family effects and neighbourhood effects, we use a variance decomposition approach. The idea is simple. If childhood neighbourhood have long-lasting effects on welfare, a resemblance in adult outcomes will appear among persons who grew up in the same local community. The same line of reasoning applies to schools and children who graduated from the same institution. Our empirical neighbour (school mate) correlation is an estimate of the proportion of the variance in years of schooling explained by factors shared by neighbouring children (school mates).

In order to illustrate the variance decomposition approach, we use a simple framework suggested by Solon et al. (2000). Let y_{ifi} be the years of education, for sibling i in family f in neighbourhood c.

$$y_{cfi} = \beta' Z_c + \alpha' X_{fc} + \varepsilon_{cfi}, \tag{1}$$

where X_{fc} is a vector of all family characteristics that influence years of education, Z_c contains all the neighbourhood characteristics, and ε_{cfi} represents unrelated individual factors orthogonal to both family and neighbourhood effects.[4] The total variance in years of schooling can be decomposed as:

$$\begin{aligned} \mathrm{var}\left(y_{cfi}\right) = {} & \mathrm{var}\left(\beta' Z_c\right) + \mathrm{var}\left(\alpha' X_{fc}\right) + 2\,\mathrm{cov}\left(\beta' Z_c, \alpha' X_{fc}\right) \\ & + \mathrm{var}\left(\varepsilon_{cfi}\right). \end{aligned} \tag{2}$$

We are looking for the relative influence of neighbourhoods on schooling, i.e. var$(\beta' Z_c)$/var (y_{cfi}). Empirically, we use the observed covariance in educational attainment among neighbouring children from different families. This covariance, using (1), is given by

$$\begin{aligned} \mathrm{cov}\left(y_{cfi}, y_{cf'i'}\right) = {} & \mathrm{var}\left(\beta' Z_c\right) + \mathrm{cov}\left(\alpha' X_{fc}, \alpha' X_{f'c}\right) \\ & + 2\,\mathrm{cov}\left(\beta' Z_c, \alpha' X_{fc}\right). \end{aligned} \tag{3}$$

As illustrated in (3), the neighbour covariance contains more than the variance of neighbourhood effects. The second term represents clustering of similar families in neighbourhoods. As families typically sort themselves into neighbourhoods, resemblance in outcomes of children growing up in the same local community (or school) will also reflect similar family backgrounds. The third term reflects the extent to which families are non-randomly distributed across neighbourhoods. We expect that advantaged families sort into advantaged neighbourhoods, reinforcing the impact of a non-random distribution of families on observed neighbour correlation. In the case of school mate correlations, compensating resource allocation across schools will tend to reduce—and possibly even reverse—the positive association between family and school effects.

[4] Since X and Z are latent vectors that include all relevant variables, the residual is orthogonal to both.

Empirically, we can estimate the part of $\alpha' X_{fc}$ related to *observed* family characteristics, and adjust the correlation, $\text{cov}(y_{cfi}, y_{cf'i'})/\text{var}(y_{cfi})$, by subtracting the covariance in predicted family effects (divided by the variance). However, since we control only for observed family characteristics, our estimated neighbour correlation represents an upper bound on the neighbourhood effects (see Altonji, 1988; Solon et al., 2000; and Page and Solon, 2000).[5] Obviously, the correlation in adult outcomes among persons who spent their childhood in the same local community cannot tell why neighbourhoods matter. It includes the joint effects of the distribution of characteristics (Z's) and their causal effects (β's).

"Neighbourhood effects" is a label for a variety of different mechanisms. The attitudes and behaviour of peers, the existence and enforcement of social norms as well as local institutions vary across neighbourhoods. Our focus is on the role of the primary school[6] as a potentially important factor shared by neighbouring children. Disentangling the impact of schools from other neighbourhood characteristics is hard as we do not have any reliable information (or assumptions) on the sorting of neighbours across schools, e.g. why neighbouring children go to different schools.

Our approach is less ambitious. First, we estimate the trend in neighbour correlations over a 25-year period, i.e. birth cohorts 1947-1970, with and without family background adjustment. As a by-product, we report estimates of the trend in intergenerational educational mobility. Second, we focus on specific birth cohorts, 1947-1956, that were affected by the primary school reform during the 1960s. We exploit this by estimating neighbour correlations in adult educational attainment by birth cohort for individuals as a function whether they lived in reform or non-reform municipalities. The idea is to assess whether the declining impact of neighbourhoods on educational attainment can be attributed to the introduction of the new school system. Finally, we look at school effects by means of school mate correlations. Resemblance in educational attainment among

[5] Variance decomposition to obtain the upper bound of effect of observed and unobserved effects may be preferred to regression analysis where studies often report unstable and small effects of community characteristics when these are directly included in the estimation equations of adult earnings or educational attainment (for an overview, see Ginther, Haveman and Wolfe, 2000).

[6] By primary school, we mean institutions responsible for compulsory schooling. It includes what is frequently called (lower) secondary levels.

children graduating from the same primary school will reflect the total contribution of school characteristics, including resources and composition of pupils. As similar families tend to cluster in schools, parental background adjustment is needed to tighten the upper bound on school effects. On the other hand, it is not so obvious that disadvantaged families sort into disadvantaged schools, as the allocation of school resources tends to favour schools with children in need of special treatment. Unfortunately, data on primary school attendance are not available for children born before 1960.

2. Data and school institutions

2.1. Families, neighbourhoods and school mates

The data set has been put together from sources provided by Statistics Norway (Møen, Salvanes, and Sørensen, 2003). The data include linked administrative data covering most of the Norwegian residents. We also have national censuses for 1960 and 1970 (Vassenden, 1987). Using a unique personal identifier given to all Norwegian residents by the national population register, we can link records from these data sets. We use a set of household and census tract identifiers in the census to identify families and place of residence during childhood. For the 1959-1970 birth cohorts, we have added which primary school they graduated from. The censuses also contain family background variables such as parents' education. The administrative register contains information on adult taxable income (excluding capital gains) and educational attainment. The linking of administrative to census data is not perfect, but for the subset of individuals we consider in this paper, more than 90 per cent can be linked across these datasets for the older cohorts, while the degree of linking is close to 100 per cent for younger non-immigrants. The main reason for non-linking is that the central register of residents based its first records on the census of 1960, and among those who left home before 1960, little was done to refine the information on parents. We have to drop some additional individuals with incomplete information on residence. Vassenden (1987) documents the construction and linking of the census files, while Statistics Norway (2001) documents the central register of education.

Neighbourhood is defined as census tract in 1960 or 1970. The total number of tracts increased from 7996 in 1960 to 8818 in 1970, with

most of the increase in urban areas. The average tract populations were 464 and 439 respectively, and these tracts were considerably smaller than those of most other country censuses of the time (Langen, 1975). With the single-year cohorts we use, the average number of individuals ("neighbours") per neighbourhood on which we have information varies from 6.1 to 8.4, with median neighbourhoods of 4 and 5 individuals.

School mates are individuals who graduated from the same school when leaving compulsory education (age 15/16 typically). The schools are larger than the neighbourhoods, with average cohorts of 62 students (median 49) in the 1959 cohort, with a trend toward smaller schools; in the latest cohort for which we have a full year, 1969, the mean graduating class has 55 students (median 41).[7]

We observe the neighbourhood in which children live at one point in time. Because families move, the neighbourhood at a single point in time may not accurately represent the environment children grew up in. On the other hand, people may move between very similar neighbourhoods. In a previous paper (Raaum, Salvanes and Sørensen, 2001), we examined the differential outcomes among those who stayed and those who changed location between the 1960 and 1970 census (using the list of comparable tract aggregations provided by Langen, 1975). We found that with respect to neighbour-correlations in adult educational achievement, this factor does not seem to cause major biases.

There are 451 municipalities in the 1970 census, and most of these have at least one school each, only a few have joint schools with neighbouring municipalities. In 1974, 247 municipalities had only one school, but there are 827 schools in all, which gives an overall average of 1.96 schools per municipality. Typically, a school district contains a number of census tracts and, by regulation, a census tract should not cross school district boundaries although this policy was more strictly enforced in rural than in urban areas (Byfuglien and Langen, 1983). Since some time passed between the census of 1970 and our observations of graduations, which appear from 1974 and onwards, internal migration makes it difficult to examine the map from census tracts to school districts in great detail. Noise induced by migration is correlated with the size of the school district, but the median school dis-

[7] The 1970 cohort is truncated since we have no information on people born after the date of the census (November 1, 1970).

trict had, as of 1974, graduates from 15 census tracts, whereas the 25th percentile school district had graduates from 11 tracts and the 75th percentile had 23 tracts represented.

Our measure of adult educational attainment in our main sample is taken from the register of the level of education maintained by Statistics Norway (Statistics Norway, 2001). This register provides a detailed code of the highest completed education, the completion date and to how many years of schooling the highest completed education corresponds. For individuals with no recent education, their level of education as of the 1970 census is recorded.

Information on the educational attainment of parents is different. The 1960 census data on parents contain only categorical coding of types of education. We have transformed the categorical education codes into years of education, using a two-step procedure. A first step maps 1960 census codes to 1970 census codes, using repeated observations of the same individuals in the two censuses. A second step maps 1970 codes into years of schooling, using the oldest observations in the central register of education. See Raaum, Salvanes and Sørensen (2001) for the details of this procedure.

2.2. The Norwegian mandatory school reform in the 1960s

In 1959, the Norwegian Parliament passed a law on mandatory schooling and the new compulsory 9 years of schooling were gradually implemented across the country over the years 1960 to 1972. This school reform extended the number of compulsory years of schooling from 7 to 9, keeping the school-starting age constant at 7. It also unified the education system beyond the age of 15/16. Before the reform, two years of junior high school preparing for senior high school were possible to obtain in some municipalities, but pupils in other areas had to move to another municipality to attend post-compulsory schools. The nine years in the new system were divided into two levels; first six years of primary school, then three years of lower secondary school which prepared for high school. Hence, for more than a decade, the Norwegian compulsory school was divided into two separate systems. The first cohort that was involved in the reform was the one born in 1947 and the last cohort that went through the old system was born in 1959.

The aims of the reform, explicitly stated in several governmental papers, were to increase the minimum level of educational attainment by extending the number of years of compulsory education, to

smooth the transition to higher education, and finally to enhance equality of opportunities, both along the socio-economic and the geographical dimension.

Implementation process of the reform

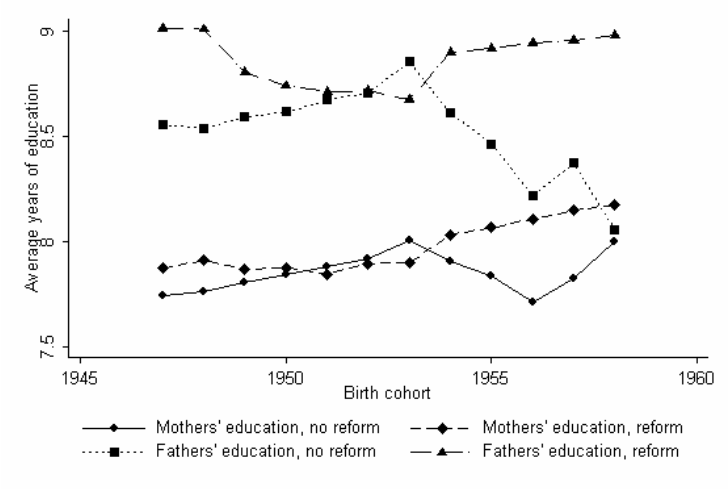
Under the law of 1959 for mandatory schooling, each municipality was invited to apply to a committee under the Ministry of Education to implement the reformed school system for the whole municipality. This application should include a plan for the new school in terms of buildings and funding, although the extra costs of teachers and buildings were provided by the state. The criteria for being selected among the applicants by the committee are not clear. However, the committee wanted to cover different types of communities, making the sample of implementing municipalities representative for the country and also the plans for buildings, teaching resources etc should be acceptable (Telhaug, 1969; Mediås, 2000).

We are assessing changes in neighbourhood effects (a relative measure) and not levels of education. Thus, we are less vulnerable to the problem of whether reform adoption was random in terms of school participation above the mandatory years of education. However, the question that is of course of interest also in our case is whether municipalities that have implemented the new system, at any given time (or for any given birth cohort), do not vary systematically from those who still kept the old school with 7 years of compulsory schooling. When comparing municipalities by reform status, systematic unobserved heterogeneity may bias our results. For instance, did the richest municipalities implement the reform first? Was it the cities? Or did municipals in poor rural areas implement the reform first since there were obvious economic incentives for implementing the reform? In the public debate from the 1950s and 1960s, it was claimed that the old educational system with more streaming, prepared better for high school and university studies than the new system, indicating that the rich and city areas perhaps implemented the reform late. It was also claimed in the public debate at the time that 9 years of mandatory schooling were not necessary in many rural communities, since fishing and farming were the main industries and those did not require 9 years or higher education.

We are not checking these hypotheses carefully in this paper, only presenting some indication of a possible relationship between the average years of parental schooling, by the birth cohort of their child

and the reform status displayed in Figure 1. The figure suggests that the unconditional transition (probability) was positively correlated with the educational attainment of the parents. In Aakvik, Salvanes and Vaage (2003), a detailed analysis of the process of allocating the reforms to municipalities is undertaken. As indicated from Figure 1, the case is not completely clear, but a more detailed analysis did not find support for a systematic allocation of the reform to municipalities.

Figure 1. Parental years of education (by primary school reform status and birth cohort)



Identification of reform status

Information on what type of primary school people attended is only available for those who never continued schooling above the mandatory years, so it is necessary to classify the type of primary education based on municipality of residence in the censuses of 1960 and 1970. It is, however, not an easy task to find municipality level information on reform implementation. The most authoritative list is Ness (1971), but this list is organized by municipalities in 1970. A series of municipality mergers and boundary adjustments in 1965-66 make it difficult to fix a point in time for the reform based on 1960 municipality for the later part of the 1960s. We want to concentrate on finding a date of implementation using the 1960 municipalities; since a 1970 mu-

nicipality can include several 1960 municipalities with different dates of implementation and thus, it is more difficult to fix a unique implementation year for the 1970 municipalities.

We use a classification scheme based on administrative data on adult educational achievement, focusing on those who left school with only primary education, let us call these people the "dropouts". For each 1960 municipality, we follow the cohorts of those who lived there at the time of the 1960 census. For each year, we calculate the share of dropouts from the old system and the share of dropouts from the new system. We use these dropout rates to calculate two candidate measures of reform date: The first when the dropouts from the old system stop appearing, and the second when the dropouts from the new system start showing up.

Since we must allow for some migration, we cannot simply use indicators of whether there are any dropouts at all as measures of school type. Such a scheme would be much too sensitive to internal migration of even a single individual who moved and dropped out in a municipality with a different implementation date than the one he left. This problem would be particularly important for dating the reform in the larger municipalities, since they receive the large number of migrants. In order to get around this, we need to measure the number of dropouts relative to the population of potential dropouts, and we need to set a positive cut-off rate to allow for some measurement error. We also want to avoid that this measurement error is systematically related to the schooling pattern in the municipalities, so we cannot use a uniform cut-off rate across all municipalities. Instead, we calculate municipality-specific "normal rates" of dropout based on the dropout rates of the 1946-1948 cohorts, which were not exposed to the reform. When the dropout rate from the old system falls below 50 per cent of this "normal" rate, we have the first candidate date of the implementation of the reform. Similarly, we calculate such normal dropout rates from the new system using the 1957-1959 cohorts which we know with certainty went through the new system. The year the rate of dropouts from the new system reaches 50 per cent of this second normal rate is our second candidate date of reform implementation.

When the two candidate measures agree on what year the reform was implemented, we use this as the year of implementation. Should there be a gap of one or two years between the two candidate measures, such that it would seem that the old system closed before the

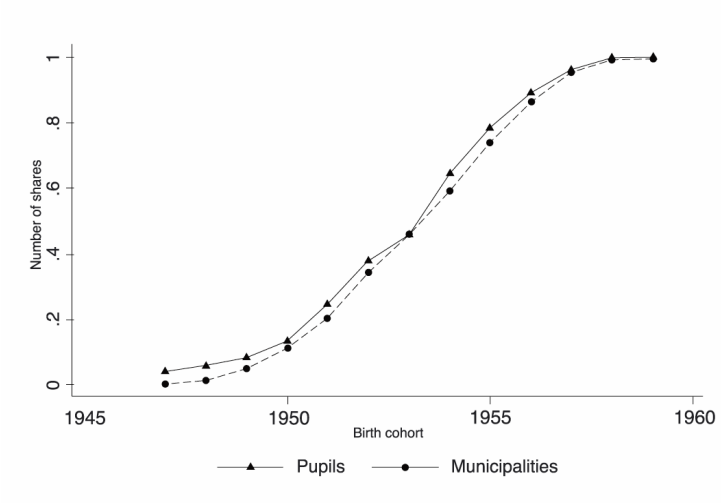
new one opened, we use the second candidate measure since this is most resistant to a secular decrease in the dropout rate. Should there instead be an overlap of one year between the two candidate measures, such that it seems that the old system and the new system coexisted for a year, we tried to check all larger municipalities (with more than 100 students) against the list in Ness (1971) and local informants. For smaller municipalities with one-year overlaps, we have randomly assigned one of the candidate years. The remaining municipalities, for which none of these methods worked, have been dropped from the sample. While there will certainly be some measurement error in our reform date taken as a flow indicator, we believe that the measurement error in the stock of reformed and non-reformed municipalities for a given year is small.

This method provides a year of implementation for 545 out of 728 municipalities. Table 1 displays the relative importance of the various rules in assigning an implementation date. The slow and gradual implementation of the reform is illustrated in Figure 2. Table A4 in the Appendix presents descriptive statistics for the included and non-included neighbourhoods. As we can see, there is very little difference. In our analysis below, we only consider birth cohorts where the smallest of the reformed and non-reformed group constitute at least 5 per cent of the students, and we therefore exclude the 1946-1947 and the 1957-1959 cohorts.

Table 1. Procedures of reform year identification

	Share of municipalities	Share of pupils
The two indicators coincide	.398	.555
One-year gap	.143	.125
Two-year gap	.059	.042
Manual inspection	.029	.078
Random assignment	.116	.071
Undecided, not used	.255	.129

Figure 2. Accumulated shares of after-reform municipalities and pupils



3. Results

Neighbour and school-mate correlations are estimated using the full list of all unique pairs within neighbourhoods or schools that are not also siblings; see Solon et al. (2000). Correlations are reported separately for each birth year, in order to distinguish between neighbourhoods located in pre- and post-reform municipalities. If we expanded the number of birth cohorts, each neighbourhood would consist of children who went to different school systems. One might argue that children are affected by the attitudes and behaviour among older peers and not only by those of equal age. However, those born in the same year would be exposed to the same environment, e.g. have the same older role models. Detailed results are reported in Appendix Tables A1-A3.

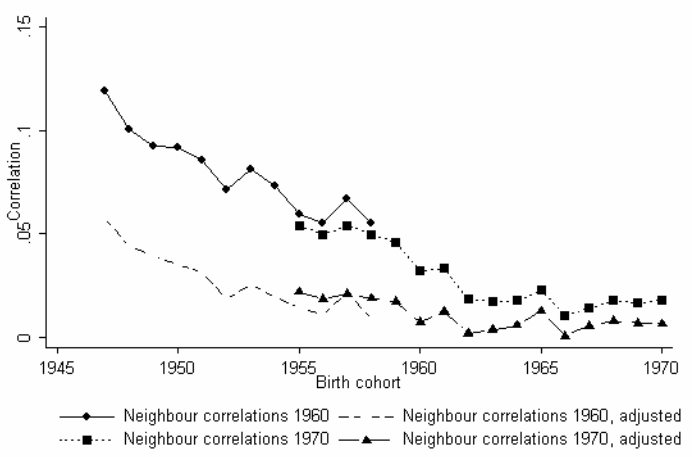
3.1. Trend in the effects of childhood neighbourhood and parental education

Figure 3 displays the correlations in educational attainment among neighbouring children by birth cohort.[8] The neighbourhoods of the

[8] The standard errors are not displayed, but they are very small and vary around .006, see the Appendix.

1947-1958 cohorts are defined by the 1960 census, while the 1970 census defines the neighbourhoods for the 1955-1970 cohorts. The figure also includes the family background adjusted correlations which subtract the covariance component arising from sorting on observed family characteristics (i.e. parental education). The correlations are substantial, around .1, for the cohorts born in the late 1940s and early 1950s. There is a clear trend of declining correlations until around the 1962 cohort, but from then onwards, the correlations are basically constant at a level of about .025. Since the estimates using the two alternative neighbourhood definitions for the "overlapping" birth cohorts, 1955-58, are basically the same, the lower correlations in the 1960 cohorts cannot simply reflect a change in the definition of neighbourhoods.

Figure 3. Neighbour correlations by birth cohort



Apparently, correlations in Figure 3 are small and some may find them negligible. However, if we convert a correlation estimate of .1 into "level effects" in years of schooling, we get a standard deviation of neighbourhood effects which amounts to about .95 years.[9] A correlation of .03 corresponds to a standard deviation of .5 years of schooling. Consequently, even seemingly negligible correlations are non-trivial. For comparison, a correlation of .4, which is the typical

[9] By rearranging (3) and using the observed standard deviation in schooling, which is about 3.

number for Norwegian siblings, corresponds to a standard deviation of effects of 1.9 years of schooling.

Figure 3 also reveals that family sorting matters. In order to adjust for parental education, we regress educational attainment on schooling years of the father and mother and neighbourhood dummies. Subtracting the covariance of predicted family effects from the total covariance and dividing by the total variance of educational attainment, we get the adjusted neighbour correlations. When correlations are adjusted for parental education, the estimates are reduced by more than fifty percent. While the neighbour correlations for the cohorts in the late 1940s and early 1950s remain significant, at around .04, they drop steadily over time and are close to zero from the 1960-cohorts onwards. As even the family adjusted correlations can be seen as upper bounds on the neighbourhood effects, we conclude that the impact of childhood community on adult educational attainment is negligible for Norwegians who are today in their thirties and early forties.

The declining neighbourhood effects may reflect that sorting on unobserved family characteristics has become less severe over time. This explanation can be checked by looking at how adult education is distributed within and between neighbourhoods over time, since we expect the sorting on the basis of parental education to be the same as that on unobserved characteristics. Table 2 is taken from Raaum, Salvanes and Sørensen (2001) and shows that the between-neighbourhood component has become more important over time, indicating that sorting has been more, rather than less, severe.

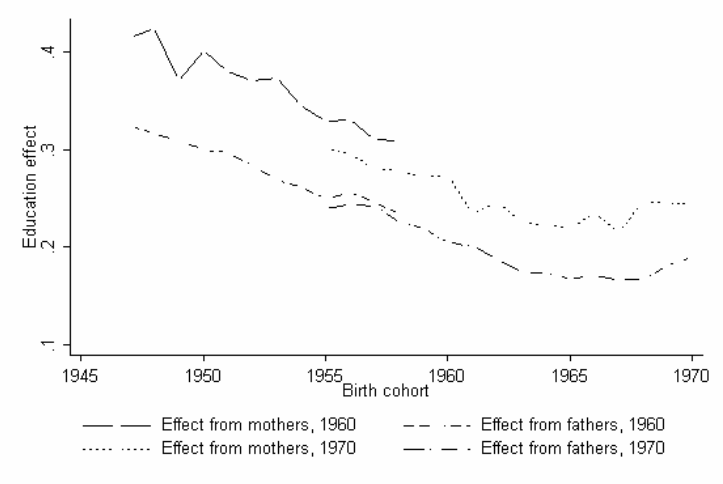
Table 2. Degree of neighbourhood sorting

	Mother's education		Father's education	
	1945-55	**1955-65**	**1945-55**	**1955-65**
mean	8.005	8.678	8.771	9.503
$\hat{\sigma}_u$	.611	.846	.780	1.314
$\hat{\sigma}_e$	1.578	1.814	1.873	2.505
$\hat{\rho} = \hat{\sigma}_u^2 / \left(\hat{\sigma}_u^2 + \hat{\sigma}_\varepsilon^2\right)$	.130	.179	.171	.216

Note: The decomposition of the variance of parental schooling. Estimates from the fixed effect regression $E_{ic} = \overline{E} + u_c + \varepsilon_{ic}$ (neighbourhood fixed effects). Sample is restricted to parents aged 30-50 at the time of the censuses. This table is taken from Raaum, Salvanes and Sørensen (2001).

The family adjustment is based on cohort-specific estimates of the relation between the schooling years of parents' and children. Figure 4 displays the estimated regression coefficient of the schooling years of the mother and father. An interaction term turns out negative and the coefficients are evaluated at the mean for fathers and mothers. Two striking results appear. First, there is a clear trend of declining relation between educational attainment of parents and child, suggesting that intergenerational educational mobility has increased, in accordance with Bratberg, Nilsen and Vaage (2002). Second, the "effect" of the mother's education seems to be the stronger.

Figure 4. Effects of parental education on children's schooling (by birth cohort)



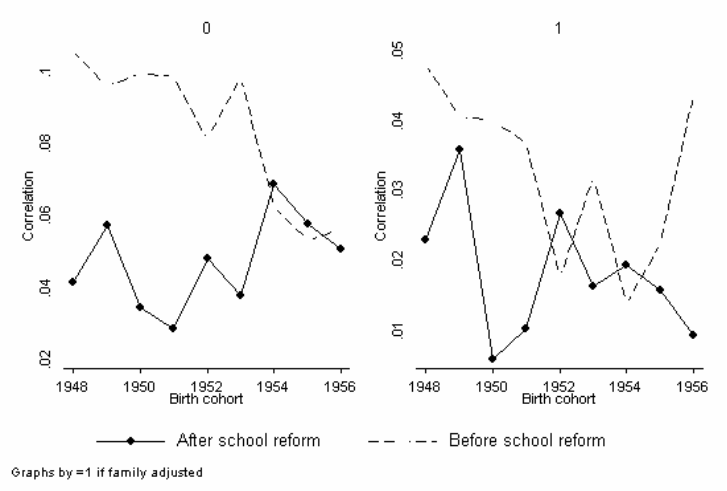
3.2. Neighbour correlations by primary school reform status

For each of the 1948-1956 birth cohorts, we classify individuals as "before- or after-reform" according to the reform-status of the municipality in which their neighbourhood is located. Neighbour correlations are then estimated separately by cohort and reform status. This exercise is motivated by the pattern of declining neighbour correlations; if the primary school reform lowered the impact of childhood location, we expect to find a lower correlation among neighbouring children who went to the new school system. Consequently, as more children were entering the new school, the overall neighbour correlation would drop as a result of the reform.

The neighbour correlations are displayed in the left panel in Figure 5, while the family adjusted estimates are shown in the right panel. First, looking at the left panel, we see that the after-reform correlations are all lower than the before-reform correlations during the first seven years (incl. the 1953 cohort). By 1953, about 50 per cent of the cohort lived in municipalities which had implemented the new school system. Thereafter, the correlations of two groups are basically the same. We also see that the trend of declining correlations, with the exception of the 1953-cohort, remains when we consider the before-reform neighbourhoods. No such trend is found for the after-reform individuals.

Second, the right panel shows that the difference according to reform status drops significantly when we adjust for parental education. Although the estimated neighbour correlations are higher in the before-reform municipalities in seven of nine cases, there is no clear pattern. There is a tendency to lower post-reform correlations in municipalities with an early implementation. This is restricted to the 1947-1951 cohorts and the fraction of pupils in the new school is less than 25 per cent in these cohorts. At most, the primary school reform implemented throughout the 1960s had a modest impact on the overall trend of declining neighbourhood effects.

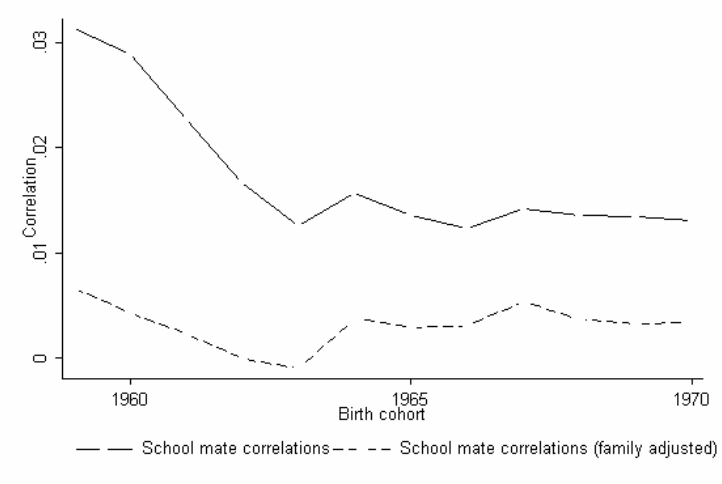
Figure 5. Neighbour correlations (by primary school reform status and birth cohort)



3.3. School mate correlations

A lower secondary school identifier is available from 1974 onwards, enabling us to construct school mates defined as children born between 1959 and 1970 who graduated from the same school around the age of 15/16. All went to the new system with nine years of compulsory schooling. Figure 6 displays correlations in years of schooling among school mates, by birth cohort. The upper line shows the unadjusted correlations and we recognize the pattern of declining correlations found among neighbouring children in the pre-1962 cohorts. We note, however, that the school mate correlations are significantly lower than the corresponding neighbour correlations.

Figure 6. School mate correlations (by birth cohort)



Again, we expect that the sorting of families into local communities and school areas will give a positive bias in the estimates of overall school effects. The family adjusted school mate correlations are significantly lower and even close to zero. Thus, we find a negligible impact of factors shared by children who graduated from the same school at the age of 15/16. In other words, the variation in "school quality" and the magnitude of peer-effects seem to be very small. This is consistent with the negligible neighbour correlations of the same cohorts, and also the low levels of "between-school" variance typically found in studies of student performance distributions (Coleman, 1966; OECD, 2003). One caveat needs to be emphasized. The inter-

pretation of a family adjusted school mate correlation as an upper bound on the school effects is based on the assumption that children of "advantaged" families go to "good schools", i.e. $\mathrm{cov}(\beta' Z_c, \alpha' X_{fc}) \geq 0$. Since school resources are partly distributed in a compensating way, which provides extra resources to schools teaching pupils with specific needs, this assumption may not hold. On the other hand, our family background adjustment is unlikely to account for the total impact of clustering of similar families in schools.

4. Conclusions

This paper has studied the composite effect of primary schools and neighbourhoods on adult educational attainment in Norway, with a particular emphasis on changes over time. We focus on correlations in the final years of schooling among neighbouring children as well as school mates. These correlations measure the proportion of the variance in years of schooling, explained by factors shared by individuals who grew up in the same local community or graduated from the same school at the age 15/16. We do not identify the effects of *specific* neighbourhood and school characteristics, but the correlations measure the relative importance of childhood neighbourhood and school. As such, the measures are closely linked to "inequality of opportunity", where the location of your parents' home affects your adult outcome.

The impact of neighbourhoods on educational attainment has diminished, in accordance with Raaum, Salvanes and Sørensen (2001). Estimating neighbourhood effects for all birth cohorts from the late 1947 to 1970, we find a clear trend of declining correlations until around the 1962 cohort. From then onwards, the correlations are basically constant and close to zero when we adjust for family sorting into local communities.

We single out the primary school reform gradually introduced during the 1960s as a potential explanation, because primary schools constitute an important part of the neighbourhoods. The reform extended compulsory schooling from 7 to 9 years, provided a common curriculum for all schools and was aimed at equalizing opportunities across socio-economic and geographical backgrounds. For each of the 1947-1956 birth cohorts, we classify individuals as "before- or after-reform", according to the reform-status of the neighbourhood. The estimated neighbour correlations tend to be higher in the before-

reform municipalities, but the difference is reduced when we adjust for parental education. The primary school reform implemented throughout the 1960s cannot fully explain the trend of declining neighbourhood effects in Norway.

Finally, we estimate school mate correlations for children born between 1959 and 1970, looking for the impact of factors shared by children who graduated from the same school at the age 15/16. Effects of school resources and organizational practices, peer effects within schools and local communities are included in this measure. Accounting for family sorting, the school mate correlations are close to zero. Thus, the variation in "school quality" and the impact of peers on final educational attainment seem to have been very limited in Norway, consistent with the negligible neighbour correlations found for the same cohorts.

Focusing on Norwegians presently in their thirties and early forties, their childhood neighbourhood and primary school have had a negligible impact on their educational attainment. Since significant neighbourhood effects are found for those who are ten years older, it seems likely that policy changes have been effective in levelling the playing field across local communities. Even if the effects of the primary school reform are found to be limited, we believe that redistributive policies equalizing spending across municipalities and other educational reforms are likely explanations.

Family background, however, remains an important determinant of educational attainment. The evidence on how family effects have changed over time is mixed. Apparently, the declining relation between educational attainment of children and parents, as well the drop in neighbourhood effects, are both at odds with the stable sibling correlations found in Raaum, Salvanes and Sørensen (2001). As neighbourhood and parental education represent factors typically shared by siblings, we would expect sibling correlations to fall as well. However, alternative measures of intergenerational mobility do not necessarily change in the same direction. Sibling correlations are affected by intra-family resemblance as well as inter-family differences. Imagine that educational reforms induce all "talented" children from "disadvantaged" families (where "talent" is shared by siblings), to continue school and enter higher education. If parental resources only allowed one of the children to enter university in the earlier cohorts, the reforms would reduce intra-family differences which would contribute to a higher resemblance in educational attainment among sib-

lings. This example illustrates the possibility that intra-family resemblance is strengthened, while differences between families are reduced.

In the Nordic countries, access to rich administrative and census data has opened up during the last five to ten years. Matched data on individuals, families, schools and neighbourhoods facilitate new approaches in future studies trying to disentangle the effects of these factors. Good data help considerably, but the real challenge is to establish a framework which enables us to identify behaviour as well as responses to policy changes.

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Appendix

The correlation estimator is described in Solon et al. (2000). Each neighbourhood or school-mate group consists of many pairs of individuals. Earlier research has indicated that the weighting strategy is not critical, so all these pairs are weighted equally and an ordinary correlation is calculated on this expanded dataset. For calculation of the standard errors, we have used a bootstrap estimator, re-sampling with municipalities as the clustering unit and 300 replications.

Table A1. Trend in neighbour correlations

Birth Cohort	1960 neighbourhoods		1970 neighbourhoods	
	Unadjusted	Family adjusted	Unadjusted	Family adjusted
1947	.1191(.0289)	.0576(.0106)		
1948	.1004(.0179)	.0438(.0056)		
1949	.0924(.0144)	.0392(.0043)		
1950	.0918(.0170)	.0352(.0043)		
1951	.0853(.0153)	.0314(.0041)		
1952	.0713(.0109)	.0187(.0038)		
1953	.0813(.0159)	.0257(.0043)		
1954	.0732(.0128)	.0194(.0032)		
1955	.0596(.0084)	.0140(.0030)	.0534(.0066)	.0220(.0042)
1956	.0554(.0068)	.0112(.0038)	.0498(.0073)	.0187(.0033)
1957	.0673(.0096)	.0215(.0034)	.0542(.0069)	.0213(.0039)
1958	.0552(.0063)	.0089(.0055)	.0495(.0065)	.0189(.0037)
1959			.0460(.0069)	.0173(.0036)
1960			.0321(.0059)	.0074(.0034)
1961			.0332(.0037)	.0122(.0036)
1962			.0185(.0027)	.0018(.0038)
1963			.0175(.0030)	.0036(.0038)
1964			.0177(.0029)	.0058(.0035)
1965			.0229(.0029)	.0128(.0034)
1966			.0106(.0025)	.0008(.0026)
1967			.0141(.0025)	.0055(.0025)
1968			.0177(.0027)	.0080(.0025)
1969			.0166(.0029)	.0070(.0022)
1970			.0181(.0042)	.0066(.0037)

Table A2. Neighbour correlations by reform status

Birth cohort	Post-reform neighbour-hoods		Pre-reform neighbourhoods	
	Unadjusted	Family ad-justed	Unadjusted	Family ad-justed
1948	.0414(.0127)	.0229(.0101)	.1059(.0186)	.0480(.0066)
1949	.0574(.0145)	.0358(.0147)	.0959(.0160)	.0404(.0049)
1950	.0343(.0094)	.0058(.0098)	.0100(.0162)	.0400(.0041)
1951	.0285(.0054)	.0102(.0063)	.0985(.0164)	.0367(.0069)
1952	.0480(.0059)	.0267(.0055)	.0812(.0163)	.0175(.0041)
1953	.0379(.0054)	.0162(.0045)	.0983(.0169)	.0320(.0068)
1954	.0686(.0127)	.0192(.0031)	.0623(.0219)	.0139(.0069)
1955	.0577(.0092)	.0157(.0027)	.0531(.0146)	.0223 (.0077)
1956	.0507(.0072)	.0092(.0036)	.0562(.0101)	.0478(.0089)

Table A3. School mate correlations

Birth cohort	Unadjusted	Family adjusted
1959	.0313 (.0063)	.0067(.0029)
1960	.0289(.0044)	.0043(.0034)
1961	.0226(.0035)	.0023(.0032)
1962	.0167(.0020)	-.0000(.0036)
1963	.0126(.0020)	-.0009(.0038)
1964	.0157(.0015)	.0038(.0031)
1965	.0137(.0016)	.0029(.0029)
1966	.0124(.0015)	.0031(.0022)
1967	.0142(.0018)	.0054(.0024)
1968	.0136(.0017)	.0037(.0027)
1969	.0134(.0019)	.0032(.0033)
1970	.0131(.0018)	.0034(.0025)

Table A4. Descriptive statistics and neighbour correlations for neighbourhoods that were matched and not matched in the pre- and post-reform analysis

Co-hort	Average education Matched	Average education No match	Average earning Matched	Average earnings No match	Neighb. correla-tion Matched	Neighb. correla-tion No match	Share Matched pupils
1948	11.13	11.14	240133	249901	.04347	.09331	.868
1949	11.23	11.27	248255	256356	.05873	.09616	.865
1950	11.34	11.36	251938	262164	.04561	.09027	.865
1951	11.44	11.47	254454	263525	.06010	.08532	.863
1952	11.45	11.56	255013	270125	.05315	.07689	.862
1953	11.52	11.66	258094	270610	.03496	.07524	.866
1954	11.62	11.68	259919	269817	.04665	.07510	.866
1955	11.67	11.72	264785	271710	.03620	.05701	.865
1956	11.63	11.74	258100	268968	.04451	.05029	.869