Family background and school performance during a turbulent era of school reforms

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Summary: In the 1990s, Swedish education policy took several steps towards more decentralization and more room for parental school choice. The decade was also a turbulent one in other respects, with high unemployment and major cuts in school budgets. We study the relationship between pupils' school performance and their family background during this period of time. We use large register-based data sets and employ the grade average at age 16 as our measure of school performance. We also use register-based information to construct two alternative measures of family background. The first measure – the grade correlation between siblings born within three years of time – is a broad one and captures family as well as community factors shared by siblings. The second one – the association between grades and parental earnings – is a more narrow one. Surprisingly, we find that both relationships were remarkably stable over this turbulent period of time.

Equality of opportunity has been a major goal for Swedish education policy for a long time. Although the concept of equality of opportunity is a complicated one and may deserve a deep philosophical discussion, our pragmatic interpretation of Swedish public-policy discussion is that the equality of opportunity norm is violated if citizens' life chances depend on factors that they cannot influence themselves. One way to illustrate the public's notion of equality of opportunity is to adhere to the popular expression: "Children's life chances should not depend on the size of their parents' wallets". This expression has been used by politicians from the left to the right wing of the political spectrum and appeals to many people's notion of equity. Thus if we are interested in education policy, we could argue that the stronger the relationship between a measure of parental income and educational achievement, the less equality of opportunity there is.

In our view, one can also trace an even broader view of the equality of opportunity norm in Swedish education policy. Irrespective of parents' financial resources, an important goal has been to equalize the educational quality among students in different parts of country and among students who go to different schools in the same municipality.

The goal of equality of opportunity motivated many education reforms during last century.[1] The comprehensive school reform (*grundskolereformen*), which was implemented in the 1950s and 1960s and kept all pupils in the same school system with a common nationally decided curriculum until age 16, was motivated this way. Free college studies and universal financial support to college students are other examples of educational policies with the same motivation.

Maybe even stronger, this goal was reflected in Swedish education governance. For a long time, governance of primary and upper-secondary schools was very centralized and parental school choice strongly restricted. Although the municipalities ran these schools, the central government financed them and did so with detailed instructions how schools should use available resources. Further, the policy was restrictive towards private schools. So the compulsory school system through the 1980s could be described as a pure public one with placement of pupils in the most nearby school.

With this historical background in mind, the subsequent changes in Swedish education policy during the 1990s must be considered as quite substantial ones. One step was taken in 1990 when the municipalities got the full financial responsibility for primary and upper-secondary education. The earmarked money from the central government to the municipalities disappeared, so the room for differences in municipalities spending on education to affect school quality and thus outcomes increased.

Two changes in 1992 gave parents more choice. A voucher system required municipalities to satisfy parents' school choice subject to space limitations. But residing close to a school (the residence principle, *närhetsprincipen*) remained the

[1] See Erikson & Jonsson (1993, ch. 1), who looked into the political motivations for school reforms over a long period of time.

main principle for allocating students to primary schools. So if pupils residing close to a particular school would fill all slots, the other parents' choices would not be given any weight. The second change in 1992 required municipalities to fund private schools. In 2001, four percent of primary-school pupils attended private schools, up from less than one percent in 1990. The 2001 numbers were particularly high in some municipalities in the Stockholm area; 17.6 percent in Lidingö and 17.0 percent in Sollentuna. In Gothenburg it was 11.7 percent. See Skolverket (2001).

The turbulence of the 1990s was magnified by the severe macroeconomic downturn in the first part of the decade. Tax revenue fell sharply due to this downturn, with budget cuts in municipalities' school budgets as a consequence. The average pupil-teacher ratio increased from close to 11 to around 13. Some recent research suggests that pupils from poor family background could suffer more from bigger school classes than pupils from more well-to-do families, cf. Krueger (1999). The high unemployment also implied economic stress for the parents.

What happened to the association between family background and educational attainment during this turbulent period? That is the issue we would like to address in this study. However, to examine this issue as early as 2003 is not easy. Those primary-school pupils, who were affected by the changes in the 1990s, have not yet completed their education. And even more, they have not yet entered the labor market so it is too early to examine how they ultimately will be affected in terms of labor market achievement.

To be able to conduct a study this early, we instead focus on school performance. More specifically, we use grade averages for each cohort of 16-year old pupils, who completed primary school (*grundskolan*) during the period 1988-2000. Our use of grade averages is governed by data availability; we would have preferred to use

standardized test scores but such are not available. However, grades at the end of Swedish primary school are also illuminating for our purposes. First of all, these grades are used for entrance to upper-secondary school (*gymnasiet*). The competition to get into the most attractive upper-secondary schools increased during the decade so grades are important *per se* for these pupils. Second, Sweden has a national grading system with quite clear criteria for grading, so skills and grades should be quite strongly correlated. We describe the grading system in more detail below, where we also deal with the problem caused by a change of the grading system in the period of our study.

We have two separate approaches to studying the relationship between pupils' grades and their socio-economic background. The first approach captures a broad notion of equality of opportunity. We use sibling correlations instead of parent-child relationships. The virtue of this approach is that siblings (who grew up together) get similar outcomes not only because they share the same family background (both "nature and nurture"), but also because they shared the same neighborhood, including the peers and the schools that were available where they grew up. A sibling correlation is therefore a broader measure of the impact of family and neighborhood conditions on child outcomes than is the relationship between child outcome and parental socio-economic status only. For example, if municipality-specific factors got a stronger impact on school performance during the 1990s, it will show up in a sibling correlation but not necessarily in the relationship between children's grades and their parents' income. The same applies to parents' ability (and willingness) to choose school for their children. If this ability became more important over time, it will also show up in the sibling correlation. We estimate sibling correlations in grade point averages for closely spaced siblings. We do this analysis for siblings belonging to

cohorts born within three calendar years; we use partly overlapping cohorts born 1972-74, 1973-75 and so on until 1982-84.

The second approach captures a narrower notion of family background by estimating the association between grade averages and parental earnings. ("parents' wallets"). We do such an analysis for 13 cohorts of pupils born 1972-1984 who graduated from compulsory school at age 16 between 1988 and 2000. We do separate analyses for father's and family earnings, as well as separate analyses for boys and girls.

The paper proceeds as follows. We describe the data in section 1. Then we present the sibling-correlations results in section 2. We continue with the analysis of the association between grade averages and parental earnings in section 3. Finally in section 4, we focus on the big-city areas where privatization went further than in the rest of the country. Section 5 summarizes and discusses our main findings.

1. Data

All our data stem from registers held by Statistics Sweden. The basis for the specific data set that we use is a 20 percent random sample of each cohort born in Sweden 1972-1984. By eliminating foreign born from our working data set, we deliberately abstain from focusing on the school-performance differentials between Sweden-born and foreign-born children. The immigration issue has been a very hot one in Sweden during the period of our study and the influx of refugee immigrants was high in the early 1990s.[2] As a consequence, a rising number of pupils were born abroad over the period. If we would find a changing relationship between family background and school performance among all pupils in Swedish schools, the change may not be

[2] See, e.g., the special issue of Swedish Economic Policy Review, 2000:2.

attributable to the school reforms. Due to our focus on the school reforms, we find it more pertinent to investigate whether the relationship between family background and school performance among Sweden-born pupils changed over the period.

In order to study family relationships, the random sample described in the previous section has been merged with other family members from Statistics Sweden's register data sets. First, we use a population register (*flergenerationsregistret*) to identify biological parents and siblings. Second, we use census data from 1975, 1980, 1985 and 1990 to identify the resident parents and siblings. In most, but not all, cases the resident parents and siblings are also the biological ones. For all children defined in this way, we used another register to obtain the grades when graduating from compulsory school at age 16 in ninth grade. For all parents, we used other registers to obtain background variables like parental age, death, earnings, and some other variables.

1.1. Grades

Starting with the graduates in 1988, Statistics Sweden collects information for all pupils in Swedish primary schools in a special data set (*Årskurs 9 registret*). The normal graduation age is 16 years, so the 1972 cohort was the first one to be covered by this data source.[3] This register contains data on grades in specific subjects and our task is to define a useful grade-point-average to be used as basic outcome measure in our study. In so doing, we had to take some restrictions into account. First, some subjects, like Math and English, are offered at different levels of study and the same grades are used at each level. Because there is no straightforward way to compare grades received at different levels of study, we decided not to use these grades.

[3] In our data, about 97.3 percent of each cohort graduated at age 16 (i.e., during the calendar year when they became 16 years old), about 2.3 percent at age 17 and about 0.4 percent at age 15.

Instead we used the grades in Swedish, Science, and Social science, which are three subjects studied at the same level for all students. Together, they represent quite broad skills and a significant part of the curriculum. Indeed, 48 percent of all study time in primary school from grade 1-9 is allocated to these three subjects.

Although we would have preferred to use results from coherent national tests, which are comparable over time, we would argue that a grade average based on these three study subjects is a relevant outcome that is of interest *per se* from the perspective of equality of opportunity. First, as already mentioned in the introduction, the entrance to upper-secondary studies has been based on the overall grade average during the whole period of our study; and our three chosen subjects represent a significant part of this overall grade average. The entrance requirements have varied among fields of study at the upper-secondary level, so the choice set is higher for those with good grades. Second, because the competition among specific upper-secondary schools increased during the 1990s and choice among them was based on the grade average, it became even more important during the period of our study to have good grades. Anecdotal evidence and newspaper reports suggests that pupils in the last years of primary school became increasingly eager to get good grades during the 1990s in order to have good options for their choice of upper-secondary school. Thus parental resources, reflected by e.g. earnings, might have become more important as well.

It is natural to consider, however, how accurately grades measure basic skills in the Swedish system. During most of the period of our study, Sweden had a national relative grading system. The grades ranged from 1 to 5, and the goal was that the national average should be 3.0 with standard deviation 1. Thus, the national fraction at each specific grade level should be predetermined. To guide teachers and school

leaders in their grading, national achievement tests were undertaken. These tests were constructed by the National Board of Education (*Skolöverstyrelsen*). They were given in Math and Swedish in 9th grade and in English in 8th grade.

With the implementation of the new curriculum in the school year of 1995/96, the relative grading system was replaced in favour of a new criterion-referenced system (*målrelaterat betygsystem*). [4] The grades in this new system have four levels: *IG* (not pass), *G* (pass), *VG* (pass with distinction) and *MVG* (pass with special distinction). The skills required to get a certain grade were pre-specified, and all teachers and schools were supposed to grade according to these pre-specified skills. National tests have been done since the introduction of the new grading system as well. They are done in Math, English and Swedish in fifth and ninth grade.

Despite the national tests, there is some room for teachers and schools to deviate from the nationally determined standards in the grading of their pupils. A clear indication is that the national average in the previous relative grading system in general was 0.2-0.3 units above the intended average 3.0. However, such grade inflation does not necessarily imply that the correlation between basic skills and grades is lower than it would have been without such grade inflation.

Another concern for us is that the new competition among schools – private as well as public – introduced by the school reforms in the 1990s could have made schools more eager to raise grades to look attractive to prospective pupils and their parents. Although it is likely that schools' grading behaviour have been affected by the reforms, it does not necessarily follow that the correlation between grades and basic skills has been reduced.

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[4] The implementation of the new curriculum for grades 1-7 took place in the school year 1995-96, 1996-97 for the 8th grade, and 1997-98 for the 9th grade.

With these concerns in mind, we have to do the best we can with the data at hand. In order to get comparable estimates for the whole period, we have to transform the grades to a common unit that is comparable over time. To do so, we started out with the 20 percent random sample of each cohort born in Sweden. We conditioned on having grades reported in the register. We then ranked all pupils in each subject and attached a percentile rank to each specific grade level. For example, in 1972 those with a 4 in Swedish ranked from percentile 63.6 to 93.4 so we attached the percentile (63.6+93.4)/2 = 78.5 to this grade. Each pupil in our analysis was then assigned a percentile value in each subject according to his or her grade in this subject. Then we computed an average of the pupil's three percentiles. In so doing, we attach the same weight to Swedish, Science and Social science.

The grade information in Science and Social science is more detailed than so. For some 80 percent of all students, both Science and Social science consist of four sub-fields with separate grades.[5] For these students, we used this additional information by first ranking the pupils in each of the four sub-fields, then computing the average rank in the four sub-fields and finally computing the average of the three main subjects.[6] This additional information about sub-field grades is useful since it generates more variation in the final grade average.

1.2 Parental earnings

We use annual earnings from work (*arbetsinkomst).* Statistics Sweden has constructed this earnings variable from employers' compulsory reports to tax authorities. Self-employment earnings are included, so there are no missing values for persons running

[5] The sub-fields of Science are Biology, Chemistry, Physics and Technology. Grades in Technology are not included for all years. The sub-fields in Social science are: Geography, History, Religion and Civics.

their own business. Further, some earnings-related social-insurance benefits are included, namely sickness pay (both own sickness and child's sickness) and parental-leave benefits. Because mothers more than fathers use parental-leave benefits and sickness pay to take care of a sick child, we do not get missing values on mother's earnings only because of such absenteeism. During the period that we study, sickness and parental leave benefits replaced 80-90 percent of foregone earnings. Unemployment benefits and training stipends for unemployed program participants are not included in the annual earnings measure.

Although it appears to be a straightforward task to estimate the relationship between pupils' grade average and their parents' earnings, there are some tricky choices involved in defining the samples and the variables. First, we must take a stand on the definition of the parents. In the first place, we use information about the biological father. But if there is no information about him, we use the (earliest) resident father in the census household if there is earnings information about such a person. We apply the same principle for mothers, even though it should be noted that it is much less common to live with a non-biological mother than with a non-biological father.

We follow the approach in the new economics literature on intergenerational income mobility and focus on long-run earnings (see Solon 1999). In our main analysis, we measure parental earnings as the average of two earnings observations around age 5 and 10 of the child.[7] We first take the log of real annual earnings and

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[6] Technically, we had to rank all pupils in the sub-fields. Thus, for pupils with "block-grades" only we attached their block-grade to each sub-field and then ranked all pupils in the sub-field.

[7] In some respects it would have been more natural to measure earnings at a later age, but then some fathers would have been old enough to be retired with missing earnings observations as a consequence.

then we compute the average of these two logged values.[8] In case one of these earnings observations is missing, we only use one earnings observation.

Swedish register information offers no information on working hours to construct full-time equivalent earnings, so our annual earnings measure also reflects variation in hours of work. The lower limit on annual earnings in the data source is SEK100 (≈12$). Because we use the logarithm of annual earnings, the results might be very sensitive to such low observations. We have settled for using a lower earnings limit at SEK10,000 (year 2000 price level) and treat observations below this level as missing ones.

We use two alternative parental earnings measures. First, we use father's earnings. Second, we use family earnings defined simply as the sum of father's and mother's earnings. The latter is a quite crude measure of the household consumption standard, and in subsequent work we will try to improve the measure by considering household size.

2. Sibling correlations in grades

2.1. Definitions

We start with the broader measure of socio-economic background, the sibling correlation. The familiar (Pearson) correlation coefficient measures the strength of the linear relationship between two variables, which in our application are grade averages for two siblings. Intuitively, it is easy to imagine that the more important that factors that siblings share are, the higher the correlation will be. And these factors could come from the family as well as from the neighborhood where they grew up. With some technical apparatus, one can also show that the sibling correlation measures the

[8] Because we use earnings from two different years, we use the consumer price index to deflate nominal earnings into the value of earnings in single year, in our case in year 2000 real earnings.

fraction of variation in the variable of interest (in our case the student's grade) that can be explained by the factors that siblings share (see e.g. Solon 1999). Note that in this context it is the sibling correlation that has this interpretation, not the squared correlation (the familiar R^2 - statistic.) For example, previous studies have estimated brother correlations in long-run earnings to around 0.20 for Sweden, so 20 percent of the variation in long-run earnings for men is due to factors shared by brothers.[9]

The broad interpretation of a sibling correlation makes this statistic like an omnibus measure of the importance of childhood conditions. Although this is an appealing property of the measure, two other properties must be kept in mind when it is used in a specific study. First, siblings can be defined in different ways and some sibling types share more common background than others do. For example, identical twins share the same genes and most likely also more common environment than most other sibling types. We use full biological siblings, who were born within three calendar years of time, so the results should be interpreted in light of this definition. For our purposes, it is crucial to apply the same definition for the whole period that we cover. Second, those who are the only child of their biological parents can not be taken into account in a sibling correlation study. We do not believe that this is a serious limitation though. As a sensitivity analysis, we have done the parent-child analysis of the subsequent section separately only for those who have siblings and the results for this group were qualitatively the same when we used the whole sample.

To sum up, in the sibling correlation estimations we use the following definitions and sample restrictions:

a) We use biological full siblings only.

[9] See Björklund et al. (2002).

b) We use siblings born within three calendar years. The first group was born 1972-74, and then we use partly overlapping groups born 1973-75, and so on until 1982-84.

c) We require that the siblings lived together in a census at age around five years. The reason we cannot have a stricter requirement in this respect is that we get the residential information from census data, and the last Swedish census was done in 1990 when our youngest cohort was six years old.

d) Some families have more than two siblings, which raises some technical estimation issues. From the point of view of statistical estimation efficiency, it is appealing to use all information to get as precise estimates as possible. It is not straightforward, though, to determine the weight that should be attached to the additional information provided by such families.[10] In our case there were only 20-30 families with three children or more born during the three-year time period, so we decided to treat each pair in such families as individual observations.

2.2. Descriptive statistics and results

We report both descriptive statistics for the samples that we use and the results in Table 1. The mean grade is defined as the average percentile rank for those in the sibling sample. They are ranked according to the random sample of the population of Sweden-born, so the numbers tell us whether those who have closely aged siblings are different than the rest of the population. It turns out that their average percentile grade rank is very close to 50.0. More important, there is not any drift in the mean and standard deviation of the grade. So it is hard to see that the siblings groups have

[10] Compare the discussion in Solon et al. (2000).

become different during the period. The data also show the well-known pattern that girls have higher grades than boys. Note that the sample sizes for brothers and sisters do not add up to those for all siblings; the reason is that all siblings also contain families with either a boy or a girl.

Our main interest is in the estimated correlations. For all siblings, they are very close to 0.50. The estimated standard errors are around 0.01, so a 95 percent confidence interval has a width around .04. This magnitude is informative in its own right. By the property of the sibling correlation, it follows that around half of the variation in the mean percentile rank of grades is attributed to factors shared by full siblings who are born within three years of time. The magnitude of the brother and sister correlations is somewhat higher, generally in the range .55-.60. It is not surprising that same-sexed siblings share more of family and neighborhood factors than all siblings do.

Finally, and most important, the sibling correlations have been remarkably stable over time, the range is from .537 for those born 1972-74 to .494 for those born 1981-83. These two estimates are significantly different from each other at conventional levels. So if there is any change at all over the period, there is a decline rather than an increase in the importance of background. It turns out, however, that it is the first estimate that deviates by being higher than the subsequent ones, and if one neglects the first observation the stability is quite striking. One could argue that most reforms (and the macro-economic shock) appeared later, so any effects from these events should appear later in the period that we cover.

We have examined how robust these findings are by experimenting with other spacing intervals for siblings, namely two and four years. The general impression of stability remains, although the estimates for the more closely spaced siblings are

somewhat higher, and the estimates for the more widely spaced siblings are somewhat lower. Further, we asked ourselves if the results could have been affected by twin births becoming more (or less) common over time. The reason such a pattern could be a concern is, of course, that twins share more genetic endowment and more environment than other full siblings. We could not find any such trend in the prevalence of twins. Further, the results did not change when we eliminated twins from the estimations.

Table 1 Sibling correlations, all siblings, brothers and sisters

	All siblings			**Brothers**			**Sisters**		
Cohorts	Correlation (std err)	Mean grade (std dev)	# of pairs	Correlation (std err)	Mean grade (std dev)	# of pairs	Correlation (std err)	Mean grade (std dev)	# of pairs
72-74	.537 (.010)	.50.4 (24.3)	7 469	.587 (.018)	45.7 (24.2)	2 139	.615 (.019)	54.9 (23.6)	1 801
73-75	.516 (.010)	50.6 (24.2)	6 884	.582 (.019)	45.9 (24.6)	1 861	.577 (.019)	55.2 (23.7)	1 778
74-76	.497 (.011)	51.0 (24.3)	6 813	.556 (.019)	46.6 (23.8)	1 903	.572 (.020)	55.5 (23.7)	1 747
75-77	.525 (.011)	50.7 (24.4)	6 221	.554 (.020)	45.4 (23.8)	1 703	.612 (.020)	55.7 (24.1)	1 613
76-78	.520 (.011)	50.4 (24.7)	6 013	.586 (.020)	45.5 (24.8)	1 647	.595 (.020)	55.8 (23.8)	1 573
77-79	.503 (.011)	50.4 (24.3)	6 340	.537 (.020)	46.0 (24.2)	1 759	.594 (.020)	56.0 (23.5)	1 669
78-80	.504 (.011)	50.6 (24.4)	6 651	.561 (.019)	45.7 (23.9)	1 821	.603 (.019)	55.8 (23.7)	1 688
79-81	.500 (.010)	50.5 (24.2)	7 201	.588 (.018)	46.5 (24.0)	1 933	.575 (.019)	54.9 (23.2)	1 837
80-82	.506 (.010)	50.8 (23.7)	6 928	.571 (.019)	46.6 (23.6)	1 966	.588 (.019)	55.8 (23.2)	1 730
81-83	.494 (.011)	51.1 (23.6)	6 869	.570 (.019)	46.3 (23.1)	1 935	.560 (.020)	56.4 (22.968)	1 736
82-84	.500 (.010)	51.3 (23.3)	7 339	.540 (.019)	46.2 (22.1)	2 010	.597 (.019)	56.5 (23.3)	1 847

3. Grades and parental earnings

3.1. Models and parameters of interest

Consider the following simple regression model:

$$GPA_i = \alpha + \beta logPE_i + \varepsilon_i, \qquad (1)$$

where *GPA* is the grade point average of pupil *i* as defined above and standardized to mean zero and standard deviation one, α is the intercept of the regression equation, $logPE_i$ is the logarithm of parental earnings with associated coefficient β, and ε_i is an error term. The regression coefficient β will then tell us by how much the standardized grade point average is expected to change due to a proportionate change in parental earnings, e.g., if $\beta = 0.4$ (as in some results below) a ten percent increase of parental income will increase the standardized grade point average by approximately .04. A .04 standard deviation change in the grade distribution, in turn, represents the move from the median in this distribution to the 52nd percentile.[11]

The regression coefficient in (1) is one of our measures of the association between school performance and parental earnings. It could, however, be argued that a complementary measure should be employed to study the evolution over time in the relationship if there is a trend in earnings inequality. Suppose (as is the case in our data for fathers) that earnings inequality increased over time. In that case a certain relative change, implies a shorter move in the earnings distribution than if the distribution would have remained constant. If the grade average is related to relative positions in the income distribution rather than to relative income differentials, one would expect the regression coefficient to fall over time as a response to rising

income inequality. One could therefore also examine the regression coefficient in a transformed version of model (1) that instead uses parental income standardized by its standard deviation:

$$GPA_i = \alpha + \beta^*((logPE)/STD)_i + \varepsilon_i \qquad (1^*)$$

where *STD* is the standard deviation of *logPE*, and β^* is the standardized regression coefficient.

A standardized regression coefficient of 0.2 (which is close to what we get) implies that a one standard deviation difference in the parental earnings distribution moves the pupil 0.2 standard deviations in the grade distribution. Such a change can be typified by a change from the median to the 84th percentile in the earnings distribution is associated with an expected change from the median to the 58th percentile in the grade distribution.

Both the standardized and unstandardized coefficients are informative, but address different questions. So we report both coefficients. Note also that the square of the standardized regression coefficient measures the fraction of the variation in grades that is explained by the variation in the parental income variable – it is equivalent to the familiar R^2-statistic. Finally, we stress that we do not claim that neither β nor $\beta*$ are purely causal parameters. Although parental earnings to some extent causally affect children's school attainment, our purpose is to descriptively relate pupils' grade average to a measure of family background that is easy to understand and easy to use.

3.2. Descriptive statistics and results

[11] These numbers rely on the assumption that GPA is normally distributed, a reasonable assumption in

We first report some descriptive statistics in Table 2. The sample size declines over time from around 20 000 pupils in 1972 to around 17 000 in 1984. This decline is consistent with the Swedish fertility patterns over this period of time. The gradual increase in father's and mother's age is also consistent with fertility patterns. Note also that fathers on average are 2.5-3.0 years older than mothers at the birth of the child. Regarding the earnings variables, it should be noted that the standard deviation of log earnings increases over time. Thus, the distinction between the standardized and unstandardized regression coefficients is important when we interpret the estimated coefficients. Finally, the sample observations reveal that there are some missing observations when we use family earnings and some more when we use father's earnings. Nonetheless, the non-response is quite low.[12]

The estimated coefficients are reported in Table 3a (both boys and girls) and separately for boys and girls in the identically organized Tables 3b and 3 c. There is a clear downward trend in the coefficients for unstandardized parental earnings. This is true for both genders and for the genders separately. The interpretation of this decline is that a given percentage parental earnings differential is associated with a shorter move in pupils' grade distribution.

The gradual increase in earnings inequality, in particular father's earnings, reported in Table 2 implies that the standardized coefficients could have evolved differently from the unstandardized ones. The results reveal that this is indeed the case. We find the stability of the estimated standardized coefficients striking. True, a very close look at the numbers shows that the estimates for father's standardized earnings are somewhat higher in 1972-73 than in the last two years. But the

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our data.

[12] Indeed, we are quite surprised by the small discrepancy between the number of observations with valid grade observations and valid earnings observations and will look close at the data to see if we have made any errors.

magnitudes are only in the order of .02, which due to the good precision of the estimates is statistically different from zero at conventional levels. If, instead, the first two cohorts are neglected, it is the stability that is most striking. The interpretation is that a given move in the parental earnings distribution is associated with about the same move in the grade distribution during the whole period of our study.

We note also that we cannot see much of a gender difference in these intergenerational associations; the estimated coefficients are about the same for boys and girls. On the other hand, the coefficients for family earnings are somewhat higher than those for father's earnings, suggesting a separate role for maternal earnings.

Finally, we interpret the results in terms of the explanatory power of parental earnings. As shown within brackets in the tables, the equations' R-squares are in the range .04 to .05 for father's earnings and .06 to .08 for family earnings. These numbers are low compared to the sibling correlations around .50, suggesting that parental earnings is not the major factor that generates similar outcomes for (closely spaced) siblings. Now, it could be argued that the log-linear functional form used in our estimations tends to underestimate the explanatory power of parental earnings. So we estimated also more flexible functional forms. Indeed, the R-squares increased somewhat when we added a squared term for earnings, but they did not exceed .10 for any of the earnings measures.[13] In an additional analysis, we also added six dummy variables for parental education, using data from Statistic Sweden's special education register. The R-squares almost doubled suggesting that education per se could be as important as earnings for children's outcomes. Nonetheless, the R-squares did not exceed 0.18, which is far from the estimated sibling correlations around 0.5.

[13] For both earnings measures, the linear and quadratic terms had positive coefficients, suggesting that the grades become increasingly sensitivity to earnings differentials at the top of the distribution.

These findings leave us with an interesting challenge for future research: what is it that (a) is shared by siblings but (b) is uncorrelated with parental long-run earnings and education that make siblings get quite similar grades?

Table 2. Descriptive statistics. Means and standard deviations in parentheses.

Year of birth	Father's age at birth of child	Mother's age at birth of child	Log father's earnings	Log family earnings	# of obs. with valid grade information[a]	# of obs. with valid father's earnings[b]	# of obs. with valid family earnings[c]
1972	29.1	26.4	12.186	12.483	20 064	19 808	20 010
	(5.8)	(4.8)	(.417)	(.404)			
1973	29.3	26.5	12.172	12.539	20 072	19 620	19 987
	(5.7)	(4.8)	(.470)	(.441)			
1974	29.3	26.6	12.162	12.526	20 338	19 890	20 247
	(5.7)	(4.9)	(.468)	(.448)			
1975	29.5	26.7	12.155	12.515	19 053	18 678	18 962
	(5.6)	(4.8)	(.456)	(.435)			
1976	29.7	27.0	12.143	12.507	18 128	17 796	18 045
	(5.5)	(4.8)	(.461)	(.441)			
1977	29.9	27.2	12.132	12.500	17 931	17 648	17 856
	(5.6)	(4.8)	(.464)	(.431)			
1978	30.2	27.5	12.205	12.617	17 076	16 576	16 986
	(5.6)	(4.9)	(.503)	(.467)			
1979	30.5	27.7	12.197	12.607	17 717	17 235	17 634
	(5.7)	(4.9)	(.502)	(.469)			
1980	30.6	27.9	12.191	12.603	17 985	17 556	17 903
	(5.6)	(5.0)	(.494)	(.462)			
1981	30.8	28.0	12.133	12.554	17 514	17 143	17 429
	(5.7)	(5.1)	(.511)	(.466)			
1982	31.0	28.2	12.109	12.526	17 302	16 947	17 221
	(5.7)	(5.0)	(.514)	(.481)			
1983	31.2	28.3	12.073	12.488	17 011	16 605	16 924
	(5.7)	(5.1)	(.538)	(.501)			
1984	31.3	28.5	12.191	12.617	17 029	16 482	16 906
	(5.7)	(5.0)	(.550)	(.508)			

Notes: a) The subset of the cohort with valid grade data in the register. b) This is the subset of those with valid grade information who also have valid paternal earnings observations. It is the sample sizes in the regressions reported in Table 3a. c) This is the subset of those with valid grade information who also have valid family earnings data. It is the sample sizes in the regressions reported in Table 3a.

Table 3a. Regression coefficients, regressions of grade and parental earnings. Standard errors within parentheses, R^2 within brackets. Both siblings.

Year	Stand. grade, father's earnings	Stand. grade, family earnings	Stand. grade, standardized father's earnings	Stand. grade, standardized family earnings
1972	.542 (.017)	.630 (.017)	.226 [.051] (.007)	.255 [.065] (.007)
1973	.494 (.015)	.575 (.016)	.232 [.054] (.007)	.253 [.064] (.007)
1974	.472 (.015)	.551 (.015)	.221 [.049] (.007)	.247 [.061] (.007)
1975	.470 (.016)	.572 (.016)	.214 [.046] (.007)	.249 [.062] (.007)
1976	.464 (.016)	.552 (.016)	.214 [.046] (.007)	.244 [.060] (.007)
1977	.480 (.016)	.605 (.017)	.223 [.050] (.007)	.261 [.068] (.007)
1978	.449 (.015)	.541 (.016)	.226 [.051] (.008)	.253 [.064] (.007)
1979	.428 (.015)	.519 (.016)	.215 [.046] (.007)	.243 [.059] (.007)
1980	.443 (.015)	.570 (.016)	.219 [.048] (.007)	.263 [.069] (.007)
1981	.424 (.015)	.569 (.016)	.217 [.047] (.007)	.265 [.070] (.007)
1982	.411 (.015)	.550 (.015)	.211 [.045] (.008)	.264 [.070] (.007)
1983	.391 (.014)	.542 (.015)	.210 [.044] (.008)	.271 [.073] (.007)
1984	.374 (.014)	.476 (.015)	.206 [.042] (.008)	.242 [.059] (.007)

Table 3b. Regression coefficients, regressions of grade on parental earnings. Standard errors within parentheses, R^2 within brackets. Boys.

Year	Stand. grade, father's earnings	Stand. grade, family earnings	Stand. grade, standardized father's earnings	Stand. grade, standardized family earnings
1972	.574 (.023)	.664 (.023)	.239 [.057] (.010)	.268 [.072] (.009)
1973	.509 (.020)	.597 (.021)	.239 [.057] (.010)	263 [.069] (.009)
1974	.523 (.020)	.603 (.021)	.245 [.060] (.010)	270 [.073] (.009)
1975	488 (.022)	.590 (.022)	.222 [.049] (.010)	.256 [.066] (.010)
1976	.486 (.022)	.567 (.023)	.224 [.050] (.010)	.250 [.063] (.010)
1977	.500 (.022)	628 (.023)	.232 [.054] (.010)	.271 [.073] (.010)
1978	464 (.021)	.564 (.022)	.233 [.054] (.010)	.263 [.069] (.010)
1979	453 (.021)	.532 (.022)	.228 [.052] (.010)	.249 [.062] (.010)
1980	.453 (.021)	.596 (.022)	224 [.050] (.010)	.275 [.076] (.010)
1981	.408 (.020)	.539 (.021)	.209 [.044] (.010)	.251 [.063] (.010)
1982	.412 (.020)	.548 (.021)	.212 [.045] (.010)	.263 [.069] (.010)
1983	.378 (.019)	.509 (.020)	.203 [.041] (.010)	.255 [.065] (.010)
1984	.364 (.019)	.460 (.020)	.200 [.040] (.010)	.234 [.055] (.010)

Table 3c. Regression coefficients, regressions of grade on parental earnings. Standard errors within parentheses, R^2 within brackets. Girls.

Year	Stand. grade, father's earnings.	Stand. grade, family earnings.	Stand. grade, standardized father's earnings	Stand. grade, standardized family earnings
1972	.514 (.023)	.600 (.024)	.214 [.046] (.010)	.243 [.059] (.010)
1973	.476 (.021)	.560 (.022)	.224 [.050] (.010)	.247 [.061] (.010)
1974	.414 (.021)	.496 (.021)	.194 [.038] (.020)	.223 [.050] (.009)
1975	.450 (.022)	.551 (.022)	.205 [.042] (.010)	.239 [.057] (.010)
1976	.447 (.022)	.546 (.022)	206 [.042] (.010)	.241 [.058] (.010)
1977	.457 (.022)	.567 (.023)	.212 [.045] (.010)	.244 [.059] (.010)
1978	.430 (.021)	.516 (.022)	.216 [.047] (.010)	.241 [.058] (.010)
1979	.397 (.020)	.510 (.021)	.200 [.040] (.010)	.239 [.057] (.010)
1980	.444 (.020)	.548 (.021)	.219 [.048] (.010)	.253 [.064] (.010)
1981	.422 (.020)	.586 (.022)	.220 [.048] (.010)	.273 [.075] (.010)
1982	.423 (.020)	.562 (.021)	.217 [.047] (.010)	.270 [.073] (.010)
1983	.407 (.020)	.580 (.021)	.219 [.048] (.010)	.291 [.085] (.010)
1984	.377 (.020)	.490 (.021)	.207 [.043] (.010)	.249 [.062] (.011)

4. What happened in the big-city areas?

It could be argued that we do not see much change in the relationships at the national level because a major policy change like the introduction of private schools only affected a few percent of all pupils during the period of time that we study. But, as mentioned in the introduction, such schools became particularly popular in the big-city areas, especially Stockholm and Gothenburg. Thus, it could be that family background became more important in these areas.

We estimated both sibling correlations and the intergenerational equations separately for the big-city area (defined as Stockholm county plus Gothenburg municipality) and the rest of the country. We report the sibling correlations in Table 4 and the intergenerational equations in Table 5. Our main conclusion is that the evolution over time is not different in these two parts of the country. The sibling correlations in Table 4 are less precisely estimated for the two regions, particularly for the big-city area. The standard error is around .025, so 95 percent confidence intervals for single years are quite wide. Nonetheless, the results do not suggest that the big-city areas had an evolution of this statistic that differs from the rest of the country. The same basic conclusion applies to the results from the intergenerational equations in Table 5.

Table 4. Sibling correlations, all siblings. By region.

Or notCohorts	Correlation(std err) Stockholm + Gothenburg	# of pairs	Correlation(std err) Rest of the country	# of pairs
72-74	.557 (.021)	1 434	.532 (.011)	6 035
73-75	.529 (.023)	1 322	.512 (.012)	5 562
74-76	.507 (.024)	1 300	.494 (.012)	5 513
75-77	.513 (.026)	1 110	.527 (.012)	5 111
76-78	.530 (.024)	1 153	.518 (.012)	4 860
77-79	.561 (.023)	1 228	.489 (.012)	5 112
78-80	.497 (.023)	1 373	.506 (.012)	5 278
79-81	.510 (.023)	1 509	.498 (.011)	5 692
80-82	.527 (.023)	1 370	.500 (.012)	5 558
81-83	.491 (.024)	1 372	.494 (.012)	5 497
82-84	.515 (.023)	1 444	.496 (.011)	5 895

Note: Stockholm includes all municipalities in Stockholm county, whereas we only include Gothenburg municipality.

Table 5. Regression coefficients, regressions of grade on earnings. By region.

	Stockholm + Gothenburg		Rest of the country	
Year	Stand. grade, standardized father's earnings	Stand. grade, standardized family earnings	Stand. grade, standardized father's earnings	Stand. grade, standardized family earnings
1972	.222	.260	.227	.257
	(.014)	(.014)	(.008)	(.008)
1973	.239	.242	.230	.259
	(.014)	(.014)	(.008)	(.008)
1974	.233	.258	.217	.246
	(.014)	(.014)	(.008)	(.008)
1975	.218	.250	.212	.250
	(.014)	(.015)	(.008)	(.008)
1976	.231	.247	.209	.245
	(.015)	(.015)	(.008)	(.008)
1977	.236	.272	.217	.258
	(.015)	(.015)	(.009)	(.008)
1978	.228	.245	.223	.255
	(.016)	(.016)	(.009)	(.009)
1979	.229	.251	.208	.241
	(.015)	(.015)	(.009)	(.009)
1980	.213	.267	.219	.263
	(.015)	(.015)	(.009)	(.008)
1981	.233	.253	.208	.269
	(.015)	(.015)	(.009)	(.009)
1982	.204	.253	.212	.268
	(.015)	(.015)	(.009)	(.009)
1983	.225	.258	.200	.273
	(.015)	(.015)	(.009)	(.009)
1984	.209	.235	.199	.239
	(.015)	(.015)	(.009)	(.009)

Note: See Table 4.

5. Conclusions

We have studied how the relationship between Sweden-born pupils' grade average at age 16 and their family background evolved during the period 1988-2000. This was a period of a severe macro-economic crisis, cuts in school budgets and several school reforms that, among others, allowed private schools and more parental choice. We employed two summary measures of the relationship between pupils' grade average and their family background, namely (i) the "broad" sibling correlation measure that captures all factors – family as well neighborhood factors – shared by siblings, and (ii) the more narrow correlation between grade average and parental earnings. To our surprise, we found that both measures were strikingly constant over this turbulent period of time. It was also basically constant in the big-city areas, where more privatization took place and the room for parental choice increased more than in the rest of the country.

Although the results suggest that events like those that took place during the 1990s may not be as important for intergenerational associations as many (including ourselves) have believed, we stress that we have not studied the causal impact of neither the reforms nor the economic crisis on the associations. One cannot rule out that the counterfactual – no school-reforms and budget cuts and no economic crisis – would have implied a weakening of the associations. One candidate explanation for such a weakening effect is public daycare. If children from poor families benefit more from such daycare than other children do, the extension of this program during the period that we study could have contributed to weaker associations between family background and school achievement.

Further, we have deliberately focused on Sweden-born pupils only, and therefore abstained from the problems associated with ethnic segregation among Sweden-born

and the new waves of immigrants who came to Sweden during the period.[14] Also, we have only looked at one link between family background and educational attainment, namely school performance at the end of compulsory school. A complete analysis of the association between family background and educational attainment must also take into account the subsequent choices of upper-secondary and university studies.

Another caveat is the time dimension of our study. Those who are concerned about the policy reforms from the equality of opportunity point of view could argue that it takes longer time for deleterious effects to appear. In particular the impact of more private schools could take longer to materialize.

Finally, we note some quite obvious extensions of our work. The data contain information about school and municipality of the primary-school graduates. Although, the sibling correlation, which captures family and neighborhood factors shared by siblings, was quite constant over the turbulent period, it might be that the impact of the specific municipality, school, or even the class unit increased over time. There are alternative research strategies available to look at such effects in some more detail. Gustafsson et al. (2000) estimate intra-class correlations and find that they increased for cohorts 1972-1979. Another approach would be to follow Solon et al. (2000) and estimate school and municipality correlations and compare these with the broader sibling-correlation measure. It would be surprising if so big policy changes as those in Sweden during the 1990s can take place without making the municipality and school more important than they used to be.

[14] See Dryler (2001) for such an analysis.

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