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How important is family background for labor-economic outcomes?☆

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ABSTRACT

This paper uses Swedish register data to examine four classical outcomes in empirical labor economics: IQ, non-cognitive skills, years of schooling and long-run earnings. We estimate sibling correlations – and the variance components that define the sibling correlation – in these outcomes. We also estimate correlations for MZ-twins, who share all genes. We also extend the variance-component decomposition by accounting for birth order. We find that conventional intergenerational approaches severely underestimate the role of family background, and that future research should follow a more multidimensional approach to the study of family background.

1. Introduction

When assessing the degree of equality of opportunity in a society, we want to know the importance of factors that individuals cannot be held accountable for.[1] This is generally the reason why we turn to the individual's family background – defined in a broad sense and including also the community and neighborhood – for finding such factors.

The traditional, and most frequent, approach in the study of the role of family background is to use some metric of the intergenerational association in a central socio-economic status variable such as occupational status, earnings or educational attainment. The metric used can generally be interpreted as a measure of mobility (or its inverse transmission). There is a long tradition in sociology of studying intergenerational class mobility based on class definitions from occupational data. In recent decades, economists have been quite active in estimating intergenerational income (or earnings) mobility, and both sociologists and economists have investigated educational mobility.[2]

The concept of mobility is of course an appealing and useful one. Yet, from the perspective of measuring inequality of opportunity, it has a major limitation. It is based on *one* single characteristic of the family (and, in addition, quite often only of the father). But obviously, family background has an impact on children in many ways that cannot be picked up by one single variable. This limitation appears quite clearly from recent studies that have estimated intergenerational correlations in outcomes of continuous variables such as years of schooling and earnings. Hertz et al. (2008) report intergenerational correlations in years of schooling around 0.4 for a large number of countries, which implies an R-square of 0.16. Surveys of intergenerational earnings correlations (see e.g. Björklund and Jäntti, 2009 and Blanden, forthcoming) show that for many countries, the best estimates are below 0.3, implying explanatory power below 10%.[3] However, measures of sibling similarity – most notably the sibling correlation in these socio-economic outcomes – tell a different story. A sibling correlation can be interpreted as the fraction of the total variation in an outcome which can be attributed to variation in the family component that is shared by all siblings in a family. These fractions are markedly higher than the R-squares implied by the intergenerational estimates.[4] For earnings, these correlations are in the range 25–50%, and for years of schooling, in the range 40–60%. See Björklund and Jäntti (2009) for estimates using earnings, and Björklund and Salvanes (2010) for estimates using years of schooling. The intuition behind the different pictures provided by intergenerational correlations and measures of sibling similarity is that the latter take into account not only the influence of the observed

☆ This paper is based on Björklund's key-note speech at the annual meetings of EALE in Cyprus on 2011. Björklund and Jäntti are jointly and equally responsible for the underlying research and the views presented in the paper. They thank Paul Devereux, Karin Hederos Eriksson, Daniel Schnitzlein and Ian Walker for useful comments that improved the paper and the Swedish Council for Working Life and Social Research (FAS) for financial support.

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[1] Even within the boundaries of economics, the literature on equality of opportunity is too large to cite properly. Roemer (1998) is a classic in the field.

[2] We consider Solon (1999) as the seminal survey of intergenerational income mobility. Björklund and Jäntti (2009) and Black and Devereux (2010) cover the subsequent literature with different focuses, and Corak (2006) emphasizes the cross-national comparisons. Blanden (forthcoming) offers an interesting comparison between the approaches in economics and sociology.

[3] A caveat is in order here. Partly due to data limitations and partly due to ignorance by researchers in the field (including ourselves), most available estimates are regression coefficients in log–log equations and are thus intergenerational elasticities. In order to obtain intergenerational correlations, one would need to adjust these estimates with the ratio of the standard deviations of parental long-run (log) earnings and offspring's long-run (log) earnings. Such corrections might change the cross-national picture as well as trend results a bit, but we do not think the arguments in this text would be affected.

[4] This insight goes back at least to Corcoran et al. (1976). See also Erikson (1987) and Sieben et al. (2001) for studies in sociology using occupational and educational variables.

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parental resource used in the intergenerational mobility analysis, but also all other unobserved factors that are shared by siblings and uncorrelated with the parental resource.

In this paper, we use Swedish register data to examine four classical outcomes in empirical labor economics: IQ, noncognitive skills, years of schooling and long-run earnings. We compare with a presumably more biological outcome, namely height, an outcome that also has received some attention in labor-economics circles in recent years, (see Case and Paxson, 2008).

We start by estimating sibling correlations – and the variance components that define the sibling correlation – in these outcomes. We stress that these estimates are lower bounds on the importance of family background. Obviously, each individual might get much more from her family background than what is shared with siblings. We then offer two extensions of the typical sibling correlation approach. First, we estimate correlations for MZ-twins, who share all genes, in contrast to full biological siblings, who on average share only about half of their genes. Second, we extend the variance-component decomposition by also accounting for the birth order of the siblings, a trait that obviously is not chosen by the individual herself. Our overall conclusion is that conventional intergenerational approaches severely underestimate the role of family background, and that future research should follow a more multidimensional approach to the study of family background.

The paper proceeds as follows. Section 2 presents the conceptual framework of our analysis and explains how we estimate the variance components that define the sibling correlation. We describe our data in Section 3. Section 4 reports the basic sibling correlations for Sweden and a comparison with previous estimates for other countries. In Section 5, we offer our two extensions of the sibling correlation. Section 6 contains a comparison between intergenerational correlations and sibling correlations. In Section 7, we discuss what mechanisms may explain why a sibling correlation is a broader measure of family background. Section 8 concludes.

2. Conceptual framework and estimation

Consider the following decomposition of an outcome, y, for individual j in family i:

$$y_{ij} = \alpha_i + b_{ij}, \tag{1}$$

where α_i is a component common to all siblings in family i, and b_{ij} is a component unique to individual j in family i. The latter component captures individual deviations from the family component. The two components are therefore orthogonal by construction. Thus, the variance of y_{ij} is the sum of the variances of the family and individual components:

$$\sigma_y^2 = \sigma_\alpha^2 + \sigma_b^2. \tag{2}$$

The share of the variance in the outcome variable, y_{ij}, which can be attributed to family background effects is:

$$\rho = \frac{\sigma_\alpha^2}{\sigma_\alpha^2 + \sigma_b^2}. \tag{3}$$

This share coincides with the correlation in the outcome variable between randomly drawn pairs of siblings, which is why ρ is called a sibling correlation.

A sibling correlation can thus be thought of as an omnibus measure of the importance of family background and community effects. It includes the variance of anything shared by siblings, such as (observed and unobserved) parental resources and parental influences such as aspirations and cultural inheritance, as well as things not directly experienced in the home, such as school, church and neighborhood effects. Interaction among the siblings is also likely to affect the common family component. However, genetic traits not shared by siblings, differential treatment of siblings, changes across time in the family, neighborhoods and schools are captured by the individual component b_{ij}. Because such factors are also part of family and community backgrounds, the sibling correlation is a *lower bound* on the importance of such factors. In Section 5, we discuss how to raise this lower bound and thus come closer to the total impact of family background.

We obtain estimates of the variance components by estimating a so-called mixed model:

$$y_{ij} = x_{ij}\beta + \alpha_i + b_{ij}. \tag{4}$$

We include cohort dummies as controls in the x-vector in Eq. (4), and when estimating models for both brothers and sisters we also include a gender dummy and interactions between gender and cohorts. We estimate Eq. (4) with restricted maximum likelihood (REML), assuming normality for the family and individual components. The standard error of the sibling correlation is obtained by using the boot-strapping technique with 1000 replications.

3. Data

Our data set is constructed by merging data from population registers held by Statistics Sweden and data from the compulsory military enlistment in Sweden. Our starting point is a 35% sample of people born in Sweden in 1951–67 randomly drawn from Statistics Sweden's so-called Multigenerational register. From this register we can identify siblings of those in the random sample, and we add all siblings also born on 1951–67 to the analysis sample. We define siblings as those who have the same biological mother and father.[5]

The military enlistment data include IQ test scores, a psychological profile, and various results on physical fitness tests. Lindquist and Vestman (2011) offer a careful description of these data. Military enlistment takes place at age 18 or 19, and enlistment was universal for all men at the time. The IQ test consists of four different parts (synonyms, inductions, metal folding and technical comprehension), each of which is graded on a scale from 1 to 9. The results from these subtests are transformed into a general measure of cognitive ability with values 1 to 9, following a normal (Stanine) distribution. The psychological profile is based on a 25-minute personal interview with a psychologist, who as a basis for the interview has information on the conscript's results from the IQ and physical fitness tests, school grades, and answers from a questionnaire on life outside the military (family, friends, etc.). The psychological profile has the purpose to capture the individual's ability to cope with the military service, so characteristics such as responsibility, independence, persistence, emotional stability and social skills are highly valued. This is why we (and others) call this variable as noncognitive skills. The psychological assessment is also graded on a Stanine scale from 1 to 9. We also obtain a measure of height from the medical examination at the military enlistment.

Statistics Sweden's Education register provides us with information on the individual's highest educational degree. We translate this degree into a continuous measure of years of schooling by assigning the years normally required to obtain the specific degrees.[6] We measure education around the age of 40.

[5] An alternative definition could be based on cohabitation during childhood. Studies based on survey data usually apply such a definition. See e.g. Page and Solon (2003a, 2003b) using the PSID and Schnitzlein (2011) using the German Socio-Economic Panel. Björklund et al. (2002) use Swedish data and compare the biological definition and the one based on cohabitation and find only small differences in estimated brother correlations in long-run earnings.

[6] We assign 7 years for the old primary school, 9 years for compulsory school, 11 years for short high school, 12 years for long high school, 14 years for short university, 15.5 years for long university and 19 years for a PhD degree. It is also feasible to apply the variance-component approach to econometric models with limited dependent variables and use the original levels of education instead of the transformation of levels to years. However, such an inquiry is beyond the scope of this paper.

Table 1
Sample descriptives. Means and standard deviations are within parentheses.

	# of families	# of individuals	IQ	Non-cognitive	Height, cm	Schooling, years	Ln earnings
Brothers							
All	369,622	481,644	5.14 (1.94)	5.07 (1.76)	179 (6.5)	12.0 (2.4)	12.17 (.69)
Twins, same sex	1015	2029	4.89 (1.94)	5.12 (1.76)	179 (6.4)	12.0 (2.6)	12.21 (.64)
Non-twins < 4 years apart	190,789	254,266	5.15 (1.96)	5.09 (1.77)	179 (6.5)	12.0 (2.4)	12.18 (0.69)
Non-twins ≥ 4 years apart	129,798	172,439	5.11 (1.94)	5.08 (1.74)	179 (6.5)	11.9 (2.4)	12.18 (.68)
Brothers + sisters							
All	595,582	1,015,301	n.a.	n.a.	n.a.	12.2 (2.4)	11.93 (.74)
Twins, different sexes	1355	2708	n.a.	n.a.	n.a.	12.1 (2.4)	11.97 (.68)
Non-twins < 4 years apart	279,890	533,320	n.a.	n.a.	n.a.	12.2 (2.4)	11.94 (.74)
Non-twins ≥ 4 years apart	197,054	363,429	n.a.	n.a.	n.a.	12.1 (2.3)	11.93 (.73)
Sisters							
All	398,887	533,657	n.a.	n.a.	n.a.	12.3 (2.3)	11.71 (.72)
Twins, same sex	1263	2525	n.a.	n.a.	n.a.	12.3 (2.3)	11.72 (.67)
Non-twins < 4 years apart	202,792	279,054	n.a.	n.a.	n.a.	12.4 (2.3)	11.72 (.72)
Non-twins ≥ 4 years apart	138,832	190,990	n.a.	n.a.	n.a.	12.3 (2.3)	11.71 (.71)

Note: we include singletons in the sibling samples, which explains why the number of observations is less than twice the number of families. Inclusion of singletons improves the precision of the family component. We use balanced samples for twins, but include a few triplets.

Our measure of earnings includes income from work as an employee and as self-employed.[7] We want to study long-run earnings and follow the recent literature by using the average of log earnings during ages 31–40 years.[8]

For our sibling correlation estimations, we use a balanced sample with valid observations on all five outcome variables for brothers and valid observations on schooling and earnings for sisters and for mixed sibships. Table 1 reports the descriptive statistics of these samples. Our samples are large, which guarantees good precision for the estimates. The number of families for our brother sample is 369,622, and for sisters and brothers + sisters the samples are even larger. Because we follow the tradition from this field of including singletons, the number of individuals is less than twice the number of families.[9] Singletons improve the precision of the estimates since they help estimate the family component. The samples for same-sexed twins are, of course, much smaller. Yet, they exceed 1000 families for both brothers and sisters separately.

In the table we also report statistics for four sibling types, namely all siblings, twins of the same sex, and non-twins born < 4 years apart and ≥ 4 years apart. We use these subgroups to make inference about mechanisms behind sibling correlations. It helps our interpretation of this inference that the four sibling types have similar means and standard deviations for the outcome variables. The differences for years of schooling and the log of long-run earnings are negligible. The largest difference that we observe is the one for IQ, for which twins have 0.25 units lower outcome compared to all. Considering that the standard deviation is around 2.0, this discrepancy is only around 0.12 standard deviation.

We also estimate intergenerational associations. The fact that data from the military enlistment are available only for cohorts born from 1951 to 1979 places some restrictions on the sample we can use for these estimations. For IQ, noncognitive skills and height, we use fathers born on 1951–55 and their sons born through 1979. This means that we get young sons, but we have reason to believe that that is not very important.[10]

For our intergenerational estimations of schooling and earnings, we use offspring born on 1963–67. We use parental education in the census 1970 and offspring education in 2000. For earnings, we use parental earnings on 1974–83 and offspring's earnings on 1996–2007.

4. Sibling correlations in Sweden compared to other countries

We report our basic estimates of sibling correlations in Fig. 1.[11] Starting with the five outcomes for brothers, we find the highest correlations for height (.53) and IQ (.47). The schooling correlation is also quite high (.44), whereas the correlation for noncognitive skills is lower (.32) and the earnings correlation is the lowest one (.22). For schooling and earnings, we can also make a comparison between brothers, sisters and mixed sexes. The correlations are lower for sisters and for mixed sexes, especially for earnings. Previous studies have also found lower earnings estimates for sisters than for brothers (see Björklund et al., 2004 and Schnitzlein, 2011).

It is natural to ask the question whether Sweden in these dimensions is very different from other countries for which there also exist estimates. We have collected estimates for other countries in Table 2 and compare them with our Swedish results.

[7] Earnings (*arbetsinkomst*) is created by Statistics Sweden by combining wages and salaries and business income. It includes earnings-related short-term sickness benefits and parental-leave benefits but not unemployment and (early) retirement benefits.

[8] As observations in our estimations, we have used all individuals with at least one valid earnings observation in the age interval of 31–40. As a valid annual earnings observation, we treat observations with at least SEK100 in 2007 prices.

[9] Note, however, that singletons included in our samples might have siblings born outside of our observation window; in our samples we only include siblings born on 1951–1967.

[10] See Björklund et al. (2010a) and Black et al. (2009) for results that support this statement.

[11] A table with estimates of all variance components and their estimated standard errors is available on request from the corresponding author.

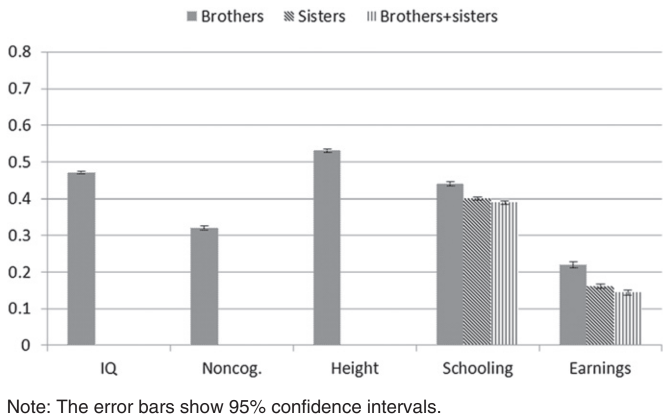


Fig. 1. Estimated sibling correlations for IQ, noncognitive skills, height, years of schooling, and long-run earnings. Separate estimates for brothers, sisters and brothers + sisters.

Table 2
Comparison of estimated brother (and mixed sexes) correlations for Sweden and other countries.

Outcome	Our estimate for Sweden	Other country	Other estimate	Study
IQ	.47 (.01)	Meta-study	.46 (n.a.)	Devlin et al. (1997)
Grades at end of compulsory school	.51[e] (.01)	England	.53 (.00)	Nicoletti and Rabe (forthcoming)
Height	.53 (.01)	US	.49 (.03)	Mazumder (2008, 2011)[a]
Schooling, years (mixed sexes)	.39 (.00)	US	.60 (.00)	Mazumder (2008)
		Former West Germany	.42 (n.a.)	Sieben et al. (2001)[b]
		Former East Germany	.27 (n.a.)	Sieben et al. (2001)[c]
		Netherlands	.47 (n.a.)	Sieben et al. (2001)[d]
		Norway	.41 (.01)	Björklund and Salvanes (2010)
Log of long-run earnings	.22	US	.49 (.02)	Mazumder (2008)
		Denmark	.23 (.01)	Björklund et al. (2002)
		Finland	.26 (.03)	Björklund et al. (2002)
		Norway	.14 (.02)	Björklund et al. (2002)
		Sweden	.25 (.01)	Björklund et al. (2002)
		Germany	.43 (.08)	Schnitzlein (2011)

[a] Mazumder (2008) measures height for adults, whereas Mazumder (2011) measures height during childhood.
[b] Estimate is an average of estimates for cohorts born on 1919–61.
[c] Estimate is an average of estimates for cohorts born on 1929–61.
[d] Estimate is an average of estimates for cohorts born on 1925–74.
[e] Source: Björklund et al. (2003).

Our estimate of .47 for IQ is almost identical to the estimate of .46 that Devlin et al. (1997) obtained in their meta-study of estimates from several countries. A related outcome is grades – measured as grade-point averages – from compulsory school so that whole cohorts are included in the sample. Using data from schools in England, Nicoletti and Rabe (forthcoming) estimated such sibling correlations to .53, which is quite close to an estimate of .51 found in a previous Swedish study. We have not found estimates of sibling correlations in noncognitive skills for any other country.

Turning to height, we also find a striking similarity to what has been found in other countries. Our estimate for Sweden is 0.53 compared to two independent estimates of 0.49 for the US (Mazumder, 2008, 2011).

The country differences are more striking when it comes to schooling and long-run earnings. For schooling, Mazumder's (2008) estimate of .60 for the US is substantially higher than the estimates around .40 for Sweden, West Germany, the Netherlands and Norway. For long-run earnings, Mazumder's estimate for the US is .49 compared to estimates around .25 or lower (as low as .14 for Norway). According to Schnitzlein's (2011) recent study, Germany with an estimate of .43 comes closer to the US than to the Nordic countries. Here, we want to remind the reader that the sibling correlation is a relative measure of the contribution of family background to overall inequality, and that the degree of overall inequality must also be considered when comparing the polar cases for the US and the Nordic countries. In the US, almost 50% of a larger level of inequality can be attributed to family and community backgrounds, whereas in the Nordic countries only a quarter of a lower level of inequality can be attributed to the same factors.[12]

In our view, the sibling correlations reported in this section are of a considerable magnitude, especially when interpreted as the fraction of total inequality that can be attributed to family and community factors shared by siblings. Furthermore, we have stressed that these numbers are lower bounds of the overall importance of such factors, the reason being that all influences from family and community background are not shared by siblings. We now turn to two attempts to raising this lower bound provided by the sibling correlation.

5. Family factors not shared by siblings: raising the lower bound

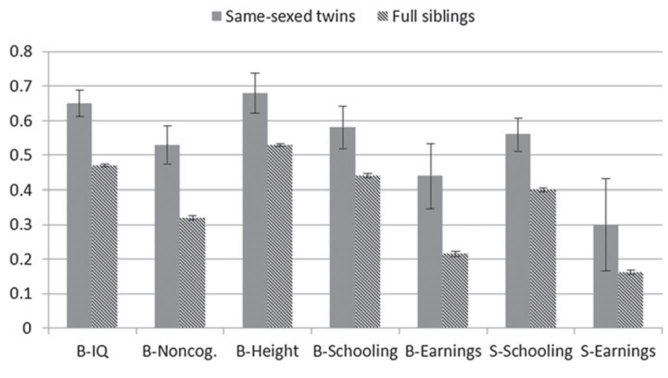
Broadly speaking, there are at least three types of factors that are not shared by siblings but yet are parts of the overall influence of family and community background on inequality among individuals. First, it is obvious that each person has all her (initial[13]) genes from her parents, but that full biological siblings share only about half of their genes. Second, not all family and environmental influences are shared by siblings. In particular, it is reasonable to believe that siblings can be affected differently by changes in family and neighborhood conditions that happen in different phases of the their lives. Third, there is likely some differential treatment by parents that is part of family background but is not captured by a common family component. It is a challenging task to come closer to a more complete measure of the role of family background. In the following, we make attempts to remedy the first and third of these problems.

5.1. Accounting for genes

An appealing approach to solving the first problem is to estimate sibling correlations for monozygotic (MZ-) twins (also called identical twins). Such siblings share all their (initial) genes and since each individual gets all her (initial) genes from her parents, this is an argument for relying on the results for MZ-twins. Another argument in favor of using MZ-twins is that they share more environmental influences, such as going to the same schools and living in the same neighborhood, as well as being exposed to the same shocks in the lives of their parents. (Note, however, that this is a major disadvantage when MZ- and dizygotic (DZ-) twins are used to distinguish between nature and nurture effects, but that is a different issue than the one

[12] Here, another caveat is in order. The data for the Nordic countries stem from administrative registers, which in turn are based on tax returns data from employers, whereas the German and US data stem from interviews. While there are no a priori reasons to believe that this difference in the data sources affects the comparison in any specific direction, it remains an important task for future research to examine whether sibling correlation and intergenerational estimates in general are influenced by the type of data used in the analysis.

[13] The qualification "initial" is motivated by the insight from epigenetics that genes may change in their interaction with the environment. See Lundborg and Stenberg (2010) for a recent discussion.



Note: The error bars show 95% confidence intervals.

Fig. 2. Estimated sibling correlations for years of schooling and long-run earnings. Separate estimates for same-sexed twins and all full siblings, and for brothers (B-) and sisters (S-).

Note: The error bars show 95% confidence intervals.

Fig. 3. Estimated sibling correlations for years of schooling and long-run earnings. Separate estimates for MZ-twins and all full siblings, and for brothers (B-) and sisters (S-).

we address here.) Nevertheless, there is one potential disadvantage in using MZ-twins to make inferences about the general population, namely that such pairs of siblings may interact more intensively with each other than most other siblings do. Obviously, interaction among siblings is one aspect of family background that we want to capture in a summary measure of the role of family background, but we do not want to include potentially excessive interactions among MZ-twins.

In order to assess this potential disadvantage of using MZ-twins, we address the question on how important interaction among siblings is in general. If such interaction is important, we would expect large differences in sibling similarity among siblings of different ages. The closer in age siblings are, the more likely they are to interact.[14] We thus estimate separate correlations for DZ-twins, non-twins with less than or equal to four years of age difference and siblings born more than four years apart. However, this is not a clean test of the importance of interactions because more widely spaced siblings are also likely exposed to different environmental influences of different types. Further, birth spacing is chosen by parents and thus potentially correlated with unobserved family characteristics. Nevertheless, we argue that small differences between these three sibling types can be considered as suggestive evidence that permanent family factors are most important and that interactions and/or time-dependent influences are less important.

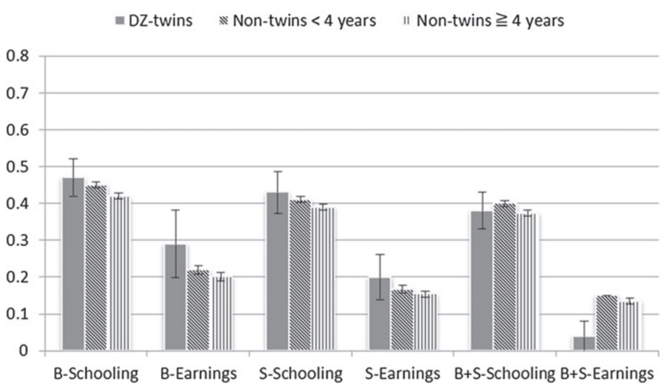
In our data set, we do not have direct access to information about zygosity. Therefore, we apply a technique by Nicoletti and Rabe (forthcoming) which provides approximations of MZ- and DZ-correlations. This technique uses the key underlying information that we can identify DZ-twins of mixed sexes. The crucial assumption is that each variance component of mixed-sexes twins deviates from the variance component of same-sexes DZ-twins in the same way as the variance components of same-sexes and different-sexes non-twins deviate from each other. A further assumption is that 50% of all DZ-twins have mixed sexes, 25% are DZ-twin sisters and 25% are DZ-twin brothers. See Appendix A for more details.[15]

Before examining estimates for MZ-twins, we take a look at the assumption-free estimates of correlations for same-sexed twins. With our data, we can estimate such correlations for all five outcomes for brothers and two outcomes for sisters. We report the estimates in Fig. 2 and compare them with our previously reported correlations for full siblings. In our judgment, the correlations for same-sexed twins are overall substantially higher than those for all full siblings. For example, for noncognitive skills the correlations are .53 and .32 respectively. The differences are smaller for sisters than for brothers, but yet are clearly higher for same-sexed twin sisters.

One can expect that these differences are driven by MZ-twins having markedly higher correlations than DZ-twins. Our application of Nicoletti and Rabe's approach confirms this expectation. The results reported in Fig. 3 suggest that the MZ-correlations are above .7 for schooling (both brothers and sisters) and for earnings (brothers). The sister estimates for earnings are .4.

Taken at face value, these estimates imply that more than 70% of the variation in outcomes such as schooling for men and women and long-run earnings for men can be accounted for by family background factors that the individual cannot be held fully accountable for. The main reservation is then that MZ-twins may have more similar outcomes because of interaction among themselves that has no counterpart in the population at large, about which we want to make inference.

As argued above, we examine this question by comparing the sibling correlations between DZ-twins, closely spaced (<4 years) and more widely spaced siblings (≥4 years). Fig. 4 reports these estimates for schooling and earnings separately for brothers, sisters and



Note: The error bars show 95% confidence intervals.

Fig. 4. Estimated sibling correlations for years of schooling and long-run earnings. Separate estimates for DZ-twins, non-twin siblings born within 4 years of time, non-twin siblings born at least 4 years apart, and separate estimates for brothers (B-), sisters (S-) and for brothers + sisters (B + S-).

[14] Sibling interaction might be a non-linear function of the age difference, for example a large age difference might coincide with much interaction if the older sibling acts as a role model for, or caretaker of, a younger one. It is beyond the scope of this paper to examine such interactions.

[15] We are not aware of any study that evaluates this technique, although such a study would be valuable. Technically, it would be feasible to perform such a study by combining data from various sources, but we do not have permissions required to merge such data.

brothers + sisters. The general result is that the differences are not huge, although the low precision for the DZ-correlations calls for some caution in our interpretations. The main exception pertains to earnings and families with brothers + sisters. Here, we have an estimate of .04 for DZ-twins and around .15 for both closely and widely spaced non-twins. We don't have a good explanation for these results for this outcome and family type. If they are to be interpreted in terms of interaction, the conclusion would be that there is a "negative" interaction among brothers and sisters who are twins, and that this negative interaction would create independent outcomes rather than similar ones. Such a conclusion is at best speculative.

Our overall conclusion from these results is that sibling similarity is not very much affected by age differences. We consider this as suggestive evidence that sibling similarity is driven by permanent factors shared by siblings. Nonetheless, we cannot rule out that there is some special interaction that makes MZ-twins so similar in our outcomes.

5.2. Differential treatment of first born

It is also appealing to extend the analysis and incorporate such family-background effects that are due to differential treatment of siblings. With administrative register data, this is a difficult task because the required information about parental behavior is typically not available in such registers. However, influential recent research has shown that one observed indicator of differential treatment is important, namely the birth order of the siblings. Black et al. (2005, 2011) find significant and, they argue, substantial effects of birth order on years of schooling and IQ, respectively. Their result is that early-born children do better than later-born siblings on both outcomes, but the magnitude of the effect is largest for the first-born compared to the second-born child. These effects have high credibility since they are estimated within the family, i.e., with family-specific fixed effects. Most likely, some kind of differential treatment lies behind these effects.

If there is systematic variation within a family across birth order, the conventional sibling correlation will not detect these effects but treat them as part of the individual components. However, from an equality-of-opportunity point of view, it must be considered a circumstance, which the individual has no control over. Thus, we argue that this part of the individual component above is one type of family-background effect.

We define first born in the same way as we have defined siblings above, namely the first-born sibling is the first-born child with the same biological mother and father. Note that the first-born child might be born outside of our sample window starting in 1951. We observe whether this is the case, and define first born accordingly. In order to estimate the variation created by being first born, we estimate within-family equations with a dummy for being first born as an explanatory variable. We consider the explanatory power of this variable as variation that is due to the family.

In Table 3, we report the results for our different outcomes. The second column reports the regression coefficient from the within-family equation. It turns out that these coefficients are always positive and significantly different from zero. The magnitude of the estimates implies that the first-born child has slightly above 0.2 years of schooling more than its siblings and around .25% higher long-run earnings; these results are quite similar for brothers and sisters separately, and for brothers + sisters in combination. For brothers, the coefficients for IQ and noncognitive skills imply effects of being first born of approximately .1 standard deviation. The effect on height is only .28 cm, or about .03 standard deviation. Nonetheless, the contribution of being first born to the total variance of the outcomes is low. In no case does it exceed 1% of the total variance. We conclude that birth order is not a major source of the family impact on labor-economic outcomes and thus not a major source of inequality of opportunity.

Table 3
The contribution of birth order, defined as being first born, to overall variance. Regression coefficient from an equation with the individual component as the dependent variable and a dummy for being first-born as the independent variable. Standard errors are within parentheses.

Group/outcome	Regression coefficient	Variance component, percent of total variance			
	First-born	Family	Individual	First born	Total
Brothers					
IQ	.236 (.005)	47.2	52.1	0.7	100
Noncognitive	.165 (.005)	32.0	67.7	0.3	100
Height	.280 (.018)	52.6	46.5	0.9	100
Schooling	.248 (.007)	44.0	55.6	0.4	100
Earnings	.024 (.002)	21.8	78.2	0.0	100
Sisters					
Schooling	.230 (.006)	40.4	59.4	0.2	100
Earnings	.027 (.002)	16.1	83.9	0.0	100
Brothers + sisters					
Schooling	.229 (.005)	39.5	60.4	0.1	100
Earnings	.023 (.001)	15.5	84.5	0.0	100

Note: twins are excluded from this analysis. Cohort effects are controlled for in an initial step, so all estimates are done on data adjusted for such effects.

Our results are similar to those of Black et al. for Norway in the sense that we obtain the same sign of being first born. Their estimates are, however, larger in magnitude. For example, the interpretation of the estimates in Black et al. (2005) suggest that first born have an advantage in terms of years of schooling clearly above 0.3 years, and the results in Black et al. (2011) suggest an advantage in terms of IQ of around 0.15 standard deviation. Further, in Black et al. (2011) they report that their set of birth-order variables explain 3% of the within-family (individual) variance of IQ. Thus, considering that the individual variation is around half of all variation, the contribution from birth order in Table 3 would have been approximately 1.5% instead of 0.7%.

We want to stress that this approach has used only one dimension of differential treatment within the family. Needless to say, there might be many other forms of differential treatment that have considerable effects. The larger these are, the more a sibling correlation will underestimate the role of family background.

6. Sibling correlations versus intergenerational associations and effects

We have argued that the sibling correlation is a broader measure of the role of family background than conventional intergenerational measures. In order to illustrate this proposition with our Swedish data, we make use of the following relationship between the sibling correlation and a corresponding intergenerational correlation (IGC):

$$\text{Sibling correlation} = (\text{IGC})^2 + \text{other shared factors that are uncorrelated with the parental variable.} \quad (5)$$

This relationship is formally derived by Solon (1999), but it is also discussed in a more informal way in the early sociology literature. In Table 4, we use our estimates of sibling correlations and corresponding intergenerational estimates to implement the decomposition in Eq. (5). Panel A reports the results for brothers and

Table 4
Sibling and intergenerational correlations (IGC). Percent of the sibling correlation attributable to parent's trait and to other factors is within parentheses.

Variable	$\rho=\sigma_\alpha^2/(\sigma_\alpha^2+\sigma_y^2)$	$IGC^2=R^2$	Other factors
A. Brother and father–son correlations			
IQ	.49	.08	.41
		(18)	(82)
Noncognitive	.45	.02	.43
		(4)	(96)
Height	.44	.19	.25
		(43)	(57)
Schooling	.46	.15	.31
		(33)	(67)
Earnings	.24	.02	.22
		(8)	(92)
B. Sister and father–daughter correlations			
Schooling	.40	.11	.29
		(28)	(72)
Earnings	.21	.01	.20
		(5)	(95)

Table 5
A comparison of intergenerational models with one and two parents. Percent of the sibling correlation attributable to parents' traits and to other factors is within parentheses.

A. Brother correlations and intergenerational models with one and both parents					
Variable	IGC		$\rho=\sigma_\alpha^2/(\sigma_\alpha^2+\sigma_y^2)$	$IGC^2=R^2$	Other factors
Schooling	.38		.46	.15	.31
				(33)	(67)
	Partial IGC fathers	Partial IGC mothers	$\rho=\sigma_\alpha^2/(\sigma_\alpha^2+\sigma_y^2)$	R^2	Other factors
Schooling	.28	.19	.46	.17	.29
				(37)	(63)
B. Sister correlations and intergenerational models with one and both parents					
Variable	IGC		$\rho=\sigma_\alpha^2/(\sigma_\alpha^2+\sigma_y^2)$	$IGC^2=R^2$	Other factors
Schooling	.32		.40	.11	.29
				(28)	(72)
	Partial IGC fathers	Partial IGC mothers	$\rho=\sigma_\alpha^2/(\sigma_\alpha^2+\sigma_y^2)$	R^2	Other factors
Schooling	.22	.20	.40	.13	.27
				(33)	(67)

father–son correlations. Father's traits account for between 4% (noncognitive skills) and 43% (height) of the sibling correlation, so "other factors" clearly dominate.[16] It is beyond our goals in this paper to explain why the father is most important for height and least important for noncognitive skills.[17] In Panel B, we report the results for sisters and father–daughter correlations for schooling and earnings. The general pattern is very similar, although with an even lower contribution to the sibling correlation from father's traits.

Now, it can be argued that these findings are not so surprising since the intergenerational model only includes one parent, namely the father. Although mother's earnings was probably an unreliable indicator of both material resources and social status for cohorts born in the early 1950s, a reasonable causal model of the parental influence on IQ, noncognitive skills, height and schooling should include parental measures for both fathers and mothers. Indeed, such a model is also the typical point of departure in the studies that aim at estimating causal effects of parental resources, (see, e.g., Holmlund et al., 2011, and our discussion below in this section). Data availability prevents us from estimating models with both parents for the outcomes (IQ, noncognitive skills and height) that stem from the military enlistment. Further, we consider mother's earnings during offspring's childhood as an unreliable indicator of family resources for the cohorts that we analyze. It is, however, reasonable to estimate intergenerational schooling models with years of schooling of the father and mother as separate explanatory variables.

In Table 5, we extend the comparison of the explanatory power of intergenerational models and the level of the sibling correlation to the case of both parents' schooling. Here, we have standardized both father's and mother's years of schooling so our intergenerational coefficient estimates are partial correlations. The results show that adding mother's schooling raises the explanatory power of the intergenerational model but not that much. The "other factors" are still the dominating part of the sibling correlation. A major reason why the impact is not that large is that mother's and father's schoolings are positively correlated due to positive assortative mating on this trait.

Our major conclusion from this exercise is that siblings share more than the single parental trait typically included in an intergenerational mobility equation. Indeed, most of what siblings share is not even correlated with the single parental trait that is included in such equations. Considering that the sibling correlation is a lower bound on the overall importance of family and community background, it is even more obvious that the focus on intergenerational associations in the literature is a narrow one. It is like studying only "the tip of the iceberg".

Our conclusion that intergenerational correlations are poor indicators of the role of family background comes from conventional intergenerational models that *describe* the transmission of a trait, or an outcome such as earnings, from one generation to the next. It is important to distinguish these deliberately descriptive models from models of the "causal effect" of parental resources on children's outcomes. There is also a growing literature that aims at estimating the causal effect of parental traits on their children. Most of these studies examine parental resources, such as income or education, and the research question is whether it is likely that a policy-induced intervention in these resources will affect the outcome of the next generation. Needless to say, this is an important question, although we consider the descriptive question to be important as well. The latter descriptive question informs about the degree of intergenerational transmission in a society, a pattern of considerable academic and political interests.

The literature on causal effects is concerned about the importance of omitted (and plausibly unobservable) variables in the descriptive patterns. Three methods have been used to eliminate the bias from such variables, namely (i) identical twins, (ii) adoptees and (iii) instrumental variables based on policy reforms or other exogenous variation. A recent study by Holmlund et al. (2011) is probably the most comprehensive analysis of intergenerational causal schooling effects of this type. While the first set of studies applied the methods to different data sets and reached different conclusions, the authors of this study applied the three methods on the same (Swedish) register-based data set. Their intergenerational effect estimates were concentrated around 0.10, whereas the corresponding intergenerational schooling associations were in the range 0.20 to 0.30. Thus, less than half of the descriptive patterns can be considered as causal effects of parental schooling per se.[18]

A similar, but slightly smaller set of studies, has considered the extent to which intergenerational earnings associations are causal (in this specific sense). Our reading of these studies brings us to about the same conclusion as for schooling: less than half of the intergenerational association can be considered causal. Using Swedish data on twin fathers and their sons, Amin et al. (2011b) estimate an intergenerational elasticity of .276 and a causal effect of 0.10. Among other recent studies, we note that Løken (2010) exploits exogenous variation in parental income caused by oil shocks in Norway and finds no significant effects on

[16] Björklund and Jäntti (2009) report similar findings when comparing intergenerational earnings models and sibling correlation for several countries.

[17] Our estimate from an intergenerational model which regresses son's height on father's height without any standardization is .49. This estimate is close to what Galton (1886) reports in his famous study on data from the 19th century: .62. However, Galton used the average height of both parents, which probably raised the estimate a little.

[18] This conclusion is consistent with recent studies using twin parents and adoptees on Norwegian and Swedish data. See Hægeland et al. (2010) for Norway and Amin et al. (2011a) for Sweden.

children's school achievement. On the other hand, Akee et al. (2010) estimate substantial effects of parental income on child outcomes when using variation in parental income caused by the opening of casinos in some parts of the US. While these two studies suggest that the effect of parental monetary resources may depend on contextual circumstances, our overall interpretation of these studies is that only a fraction of the intergenerational income elasticity is likely to represent causal effects of this parental resource.

To summarize, this recent research has been shown that there might be some causal effects of parental education on the schooling level of the next generation. Such effects should be considered in the design of education policy and be included on the benefit side in cost-benefit analyses of schooling reforms. Yet, the effects are too small to help us understand what is important in family background. Whereas the intergenerational associations provide the "tip of the iceberg", we are tempted to say that these causal effects can be considered "the tip of the tip of iceberg".

7. What is missing in conventional intergenerational mobility models?

In our view, it is a major challenge for future research in this field to understand what it is that is so important in family background – and captured by the sibling correlation – but missing in conventional intergenerational mobility models. From an equality-of-opportunity perspective, it is crucial to understand what this is and to evaluate the degree to which it violates equality norms. Previous research only gives some hints about what is important.

One approach with valuable insights is to compare sibling and neighborhood correlations. The idea is that a sibling correlation captures both family and neighborhood factors which are important for the outcome, whereas the neighborhood correlation captures neighborhood factors and the family traits which are correlated within the neighborhood. Thus, a neighborhood correlation captures only some family effects and therefore provides an upper bound of the importance of neighborhood factors. If this upper bound is quite low compared to the sibling correlation, one can conclude that family factors are the most important ones behind the sibling correlation.

Solon et al. (2000) proposed this approach and applied it to US PSID data. They estimated the sibling correlation for years of schooling to be 0.5 and the corresponding neighborhood correlation to be 0.1, suggesting that the family is much more important than the neighborhood. Page and Solon (2003a, 2003b) apply the same approach to long-run earnings of brothers and sisters. Similar conclusions follow from these studies, although the upper bound for neighborhood factors for brothers' long-run earnings was higher, around one half of the sibling correlation. More recently, the approach has also been applied on data for Norway, Sweden and England. Raaum et al. (2006) use Norwegian census data and find that the neighborhood correlation in years of schooling and long-run earnings is less than a third of the sibling correlation. Lindahl (2011), using a Swedish data set for Stockholm, finds neighborhood correlations in schooling and income which are even smaller relative to sibling correlations than in the Norwegian study. She also finds neighborhood correlations in grade point averages and test scores in compulsory school which are very low compared to what other Swedish studies have found for sibling correlations. Nicoletti and Rabe (forthcoming) use English register data on school performance at the end of primary and compulsory schools. They find that the neighborhood explains at the most a fifth of the sibling correlations of 0.54 and 0.61 for primary and compulsory schools respectively.

Another clue about what factors may be particularly important comes from our finding that the sibling correlation is not very sensitive to the age difference between the siblings. This result suggests that the family background factors to look for should be permanent ones, and not represent time-specific shocks in the family's life.

Having stressed that the family is where to look for explanatory factors, we also note that one of our results implies that institutions have the potential to change the role of family background for labor-economic outcomes. The difference in sibling correlations in long-run earnings between the US on one hand, and the Nordic countries on the other, is striking; about 0.50 compared to about 0.25. This finding suggests a big role for labor-market institutions. Schnitzlein (2012) reports a result in the same spirit. Using Danish data, he finds a striking similarity in sibling correlations between natives and quite different groups of immigrants. He argues that this supports the view that institutions are more important than cultural differences across families.

However, we also want to understand what family background factors can account for the variation in the family component in a given population. In an exploratory study, Björklund et al. (2010b) look for such factors by adding variables shared by siblings to the x-vector in Eq. (4). They find that measures of family structure and social problems account for very little of sibling similarity (the family component) beyond that already accounted for by parental income, education and occupation. However, when they add indicators of parental involvement in schoolwork, parenting practices and maternal attitudes, the explanatory power increases from about one quarter (using only traditional socio-economic variables) to almost two-thirds. Thus, such factors seem to be promising candidates for incorporating into more causal frameworks of the role of family background. Such approaches may also benefit from paying more attention to health variables and to the role played by the extended family, for example grandparents. See Mare (2011) for an insightful plea for a multigenerational approach to the study of family background.

Exploratory approaches such as nature–nurture decompositions of outcomes discussed in this paper suggest that genetic inheritance cannot be neglected in a comprehensive model of the impact of family background. For example, Björklund et al. (2005) use nine sibling types of different genetic and environmental connectedness to estimate the fractions of inequality in long-run earnings that can be attributed to nature and shared environments. They find the two types of factors to be of roughly equal importance. Although such estimates (as suggested by Beauchamp et al., 2011) can possibly be interpreted as reduced-forms effects of genetic inheritance at birth, to be really useful they need to be complemented by studies of the mechanisms via which genetic inheritance impacts the final outcomes.

Our exposition has been primarily empirical. However, we want to acknowledge that the search for factors that explain more than the conventional intergenerational approach ideally should be guided by a comprehensive theoretical model about how family and community factors interact to form the labor-economics outcomes that we have discussed. The prototypical model in the literature, which also empirical researchers in the field frequently refer to, is the so-called Becker–Tomes model; see Becker and Tomes (1979, 1986). Solon (2004) derives the intergenerational equation that follows from this model. An advantage with the Becker–Tomes model is that it is encompassing, and Solon shows how the intergenerational income elasticity can be pinned down to parameters representing preferences, technology, prices and policy. Nonetheless, the model takes many family background effects as exogenous (or "mechanical") and only explains how offspring's human capital is determined by parental income via parental investments. Thus, the model focuses on a causal effect of parental income on offspring's outcomes, or what we in the previous section called "the tip of the tip of the iceberg". In our view, this research field would benefit from new theoretical perspectives.

8. Conclusions

We have found that factors shared by siblings account for some 40–60% (and maybe even more) of inequality in productive traits such as IQ, noncognitive skills, height, schooling as well as long-run earnings during adulthood. The literature also suggests that factors related to the family rather than to the neighborhood are the most important ones. We draw two general conclusions from these findings.

The first conclusion refers to positive analysis. In order to understand these outcomes, labor economists should pay much attention to the family and what is going on in the family. In order to understand what is so important, future research must take a much broader view than only focusing on typical intergenerational associations that are one-dimensional in nature.

The second conclusion is more normative. The numbers just mentioned imply that factors beyond an individual's own full control account for a considerable share of inequality of outcomes which are very important in life. This raises concerns about substantial inequality of opportunity in modern societies. In order to understand how, and how much, these factors violate norms of equality of opportunity, we need to learn more about them. This is yet another reason for not only focusing research on what we have called "the tip (of the tip) of the iceberg", namely conventional intergenerational mobility associations. Future research should also focus on the hard-to-measure attributes of the family which seem to be very important for typical labor-economic outcomes.

Appendix A. Identification of correlations for MZ- and DZ-twins

An important basic source of information comes from the fact that can directly observe all mixed-sex twins, who are DZ-twins. Thus we can directly estimate $V^F_{DZ,\ FM}$, that is, the variance of the family component for DZ-twins who are of mixed sexes (F = female and M = male).

In order to infer $V^F_{DZ,\ MM}$ and $V^F_{DZ,\ FF}$ we make the following assumptions:

$$V^F_{DZ,\ MM} = V^F_{DZ,\ FM} + \left(V^F_{NT,\ MM} - V^F_{NT,\ FM}\right) \tag{1}$$

$$V^F_{DZ,\ FF} = V^F_{DZ,\ FM} + \left(V^F_{NT,\ FF} - V^F_{NT,\ FF}\right) \tag{2}$$

where NT denotes non-twins. Here we use closely spaced non-twins (born less than years apart).

In order to identify the corresponding expressions for MZ-twins we make use of two weak assumptions. First, we use:

$$V^F_{T,\ MM} = V^F_{MZ,\ MM} \times P_{MZ,\ MM} + V^F_{DZ,\ MM} \times P_{DZ,\ MM} \tag{3}$$

$$V^F_{T,\ FF} = V^F_{MZ,\ FF} \times P_{MZ,\ FF} + V^F_{DZ,\ FF} \times P_{DZ,\ FF} \tag{4}$$

where P denotes the proportion of twin brothers (and sisters) who are MZ and DZ. The only assumption here is that the means of the family components are the same. Note also that $P_{MZ,\ MM} + P_{DZ,\ MM} = 1$.

Now we need to estimate these proportions in order to solve the MZ-variances from Eqs. (3) and (4). For this purpose we assume that 50% of all DZ-twins have mixed sexes, 25% are DZ-twin sisters and 25% are DZ-twin brothers. From this assumption follows that we have the following numbers (N) of different twin types:

$$N_{DZ,}\ MM = 0.5 \times N_{DZ,\ FM} \quad \left(\text{Here we have } N_{DZ,\ FM} \text{ in our data}\right)$$

$$N_{MZ,\ MM} = N_{T,\ MM} - N_{DZ,\ MM} \quad \left(\text{We also have } N_{T,\ MM} \text{ in our data}\right).$$

Similarly for females:

$$N_{DZ,\ FF} = 0.5 \times N_{DZ,\ FM} \quad \left(\text{Here we have } N_{DZ,\ FM} \text{ in our data}\right)$$

$$N_{MZ,\ FF} = N_{T,\ FF} - N_{DZ,\ FM} \quad \left(\text{We also have } N_{T,\ FF} \text{ in our data}\right).$$

From these numbers, we can compute the proportions needed in Eqs. (3) and (4).

In exactly the same way, we estimate the variance of the error component for the individual.

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