Intergenerational mobility, intergenerational effects, sibling correlations, and equality of opportunity: a comparison of four approaches

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Intergenerational mobility, intergenerational effects, sibling correlations, and equality of opportunity: a comparison of four approaches

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Abstract

This paper presents and discusses four different approaches to the study of how individuals' income and education during adulthood are related to their family background. The most well-known approach, intergenerational mobility, describes how parents' and offspring's income or education are related to each other. The intergenerational-effect literature addresses the question how an intervention that changes parental income or education causally affects their children's outcome. The sibling-correlation approach estimates the share of total inequality that is attributed to factors shared by siblings. This share is generally substantially higher than what is revealed by intergenerational mobility estimates. Finally, the equality-of-opportunity approach is looking for a set of factors, in the family background and otherwise, that are important for children's outcomes and that children cannot be held accountable for. We argue that all four approaches are most informative and that recent research has provided insightful results. However, by comparing results from the different approaches, it is possible to paint a more nuanced picture of the role of family background. Thus, we recommend that scholars working in the four subfields pay more attention to each other's research.

Keywords: Intergenerational mobility, intergenerational effects, sibling correlations, inequality of opportunity

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1. Introduction

There are probably many reasons behind economists' recent great interest in research on intergenerational economic mobility and the role of family background. A major one, however, is that factors in the family background -- in a broad sense incorporating also the neighborhood that parents have chosen for themselves and for their children -- are generally regarded as sources of inequality that violate equality-of-opportunity norms. In this essay, we discuss four separate approaches that economists have pursued to study how income and schooling depend on family background. We emphasize that they constitute four separate approaches because they tend to inhabit their own separate spheres of literature. In our view, however, they are closely related to each other, and provide insights that complement each other to provide a more comprehensive picture of the role of family background. Even more important, the four approaches have quite different answers to the question of *how* important family background is.

The first approach is *intergenerational mobility*. Economists have been particularly active in estimating parameters that describe intergenerational income or earnings mobility, but some efforts have also been devoted to mobility in education.[3] Intergenerational mobility is basically a descriptive pattern that shows the relationship between parents' income, earnings or education and the same outcome of their offspring. The concept of mobility emphasizes the deviations between parents' and offspring's status, whereas the concept of persistence (the inverse of mobility) emphasizes the similarity in status.[4] The results from recent research on intergenerational economic mobility have received much public attention so this approach is the most well-known of the four.

The second approach addresses another question, namely: what is the *causal effect* of parental resources such as income or education on their children's performance during adulthood?[5] The purpose here is to estimate the causal effect of thought interventions in such parental resources. This issue is different from the intergenerational mobility one, but it is an empirical question how much the causal effects deviate from the descriptive mobility patterns.

The third approach learns about the role of family (and neighborhood) background from *sibling correlations*. A sibling correlation can be interpreted as the fraction of total inequality (measured as the variance) that can be attributed to factors shared by siblings. Obviously, siblings do not only share parental resources of the types that are considered in the intergenerational mobility and intergenerational effects literatures. They also share several important factors that are not available in typical household surveys and are hard to observe even with very ambitious research efforts. We will stress that the sibling correlation is a broader

3 See Solon (1999) for a survey of the intergenerational income mobility literature as well as the relationship between the intergenerational mobility and the sibling correlation approach. Björklund & Jäntti (2009), Black & Devereux (2011), Corak (2013) and Jäntti & Jenkins (2014) offer alternative and more recent surveys of the field. See Hertz et al. (2007) and Björklund & Salvanes (2011) for overviews of educational mobility.

4 We prefer the label persistence rather than mobility since most of the measures that are used capture persistence rather than mobility. Because the concept of intergenerational mobility is so will established, we talk about the intergenerational-mobility approach in this paper. But in the text we use the words mobility and persistence interchangeably depending on context.

5 See Holmlund et al. (2011) for a comprehensive treatment.

measure of the role of family background than the intergenerational correlation is. Yet, it is a lower bound on the importance of such factors since siblings do not share all background factors.

The fourth approach is called the *(in)equality-of-opportunity approach*, and is much inspired by Roemer (1998). Recent surveys of the empirical approach and results include Ramos & Van de gaer (2016), Ferreira & Peragine (2016) and Roemer & Trannoy (2016). The idea in this literature is to estimate the degree to which inequality in income (or other outcomes) can be attributed to circumstances, on the one hand, and individual effort, on the other. Circumstances are such factors that the individual cannot be held accountable for. Factors in the family background are natural candidates as such circumstances. Because there are several factors of this type, the approach is a multivariate one to the study of background factors in contrast to the univariate mobility approach.

In this essay we explain the approaches and summarize the main results, reporting also some new illustrations based on Swedish registry data. We emphasize that the approaches are closely related to each other, but yet give very different answers to the question of how important family background is. In particular, we argue that the relationships estimated in the intergenerational mobility literature are not very strong and imply that mobility is high. Further, the intergenerational-effects literature suggests that at most one half of these associations represent causal effects of parental resources. On the other hand, the results from the sibling correlation approach show that factors shared by siblings account for a much larger part of inequality than is revealed by the intergenerational mobility approach. And yet the sibling correlation is a lower bound of the contribution of family background to inequality. Here, the equality-of-opportunity approach has the potential to achieve two goals. First, it can help fill the gap between the intergenerational mobility and sibling correlation results by applying a richer set of family background variables than the ones used in mobility studies. Second, by also using family background variables that are not shared by siblings, it can help raise the lower bound provided by the sibling correlation. However, we conclude that empirical applications of the equality-of-opportunity approach so far have not yet been powerful enough to do so. More research is needed to find out what the missing factors are and to evaluate whether these missing factors violate equality-of-opportunity norms.

Our essay is, however, not as encompassing as it might look when we cover four subfields of literature. We do not cover the structural approach to parental investments in children.[6] However, the empirical patterns that we present can be regarded as useful background information for considerations about how to understand the family in a more structural sense. Neither do we discuss the sociological literature on occupational and class mobility, a literature that addresses what we prefer to call social mobility.[7] We focus on economic mobility, examining long-run income (or earnings) and years of education as outcomes.

6 See Francesconi & Heckman (2016) for a recent introduction to this literature.

7 See Breen et al. (2016), Torche (2015), Erikson & Goldthorpe (2010), Blanden (2013) and Björklund & Jäntti (2000) for comparisons of the mobility approaches in sociology and economics. Sibling correlations have also been used extensively in sociology with Corcoran et al. (1976) as an early contribution.

Another limitation of our essay is that we primarily focus on conceptual issues. The estimation problems that arise when a researcher tries to draw inferences about long-run outcomes from snapshots of income during parts of the lifetime are not discussed.

The paper has a simple structure. We discuss intergenerational mobility in section 2, intergenerational effects in section 3, sibling correlations in section 4, and the equality-of-opportunity approach in section 5. In each section, we discuss how the approach is related to the previous ones. In section 6 we use Swedish data to illustrate the relationship between the approaches. We start out to show the relationship between the intergenerational-mobility approach and the equality-of-opportunity approach. Then we add variables that we consider as circumstances and investigate how much is neglected by the mobility approach. We add variables that are shared by siblings and thus can fill the gap between mobility estimates and sibling correlations. We also add variables that are not shared by siblings and thus can raise the lower bound of the sibling correlation. Finally, in section 7, we report our conclusions about the four approaches and how they complement each other for a better understanding of the role of family background. We also give suggestions for future research.

2. Intergenerational mobility

The intergenerational-mobility approach describes the relationship between a specific socio-economic variable of parents with the same variable of the offspring. Economists have primarily focused on income or earnings and have strived for using lifetime or long-run income in order to eliminate the influence of transitory variation in income on the estimated intergenerational association.[8] The prototypical model is

$$(1) \quad y_i^{\text{offspring}} = \alpha + \beta y_i^{\text{parent}} + \varepsilon_i,$$

where y_i is the logarithm of long-run income of the offspring and the parent in family *i*. Thus, the regression coefficient can be interpreted as the elasticity of offspring's income with respect to the parent, the intergenerational elasticity (*IGE*). If income inequality is different in the two generations, the intergenerational correlation (*IGC*) will differ from the IGE according to

$$(2) \quad IGC = \beta(\sigma_p/\sigma_o),$$

where σ_p and σ_o denote the standard deviation of income for parents and offspring respectively.

Primarily due to data limitations, most studies have estimated (1) on samples of fathers and sons. Comparable results for a number of countries have been summarized in "The Great Gatsby Curve", showing a positive association between countries' cross-sectional disposable

[8] See Nybom & Stuhler (2016, 2017) for recent treatments of so-called life-cycle bias that arises if the ages at which parents' and offsprings' incomes are measured do not mimic lifetime income. See Mazumder (2005) for a comprehensive treatment of the attenuation bias that arises from transitory variation in income.

income inequality during sons' childhood and intergenerational persistence as measured by the intergenerational earnings elasticity.[9] Corak's (2013) version of this curve, using observations for 13 countries and with 1985 as the year of cross-sectional inequality, shows that the US, the UK and Italy stand out as the countries with high inequality and high persistence, whereas the Nordic countries form the opposite group of countries. The range of the estimated IGEs is from around 0.15 (Denmark) to around 0.50 (Italy and the US).

Due to data constraints, fewer studies have estimated IGCs. An exception, however, is Corak et al. (2014), who report both IGE- and IGC-estimates for Canada, Sweden and the US, using earnings as the outcome measure. Strikingly, they find that the discrepancy between Canada and Sweden, on the one hand, and the US on the other hand, becomes much smaller when comparing IGCs rather than IGEs. Indeed, their IGE-estimate for the US is 0.40 compared to an IGC-estimate of 0.26. The IGC-estimates for Canada and Sweden are only marginally lower than the one for the US.[10]

These results suggest that the high IGEs reported for the US reflect the fact that they are estimated for generations for which the variance of long-run income for offspring exceeded the corresponding variance for parents. Thus, it may be argued that a more realistic range of IGCs across countries is only 0.15 to 0.40 (or an even lower upper bound). This in turn implies R-squares for an intergenerational mobility equation in the range 0.025 to 0.16, which implies quite high mobility.

Recent research has also estimated income mobility with other metrics. Jäntti et al. (2006) report country comparisons of mobility estimates based on transition matrices. Further, Chetty et al. (2014) and Corak et al. (2014) have estimated the correlation in percentile rankings in earnings, which is simply the father-son Spearman (or rank) correlation. Such a correlation is an alternative to using the IGC to standardize the two distributions.[11] Using family income, Chetty et al. (2014) report rank correlations of 0.17 for Canada, 0.18 for Denmark, and 0.34 for the US. Using earnings, Corak et al. (2014) report father-son rank correlations of 0.24 for Canada, 0.26 for Sweden and 0.30 for the US. Although, these estimates are strongly significantly different from zero, they are far below 1.0 that would imply a deterministic relationship.

In order to further illustrate the strength of the descriptive intergenerational relationship, we present an empirical illustration based on Swedish data. We use the log of multi-year averages of sons' and both parents' gross total market income, including income of capital. Sample and variable definitions and descriptive statistics of the data set are presented in the Appendix.

Figure 1 shows the bivariate density of the rank of sons' and parents' income. The figure has 40x40 squares, i.e., we split both generations into groups of 2.5 percentiles. A deterministic relationship would imply that all observations are in the 40 diagonal squares with densities of 0.025. However, the figure reveals a considerable amount of mobility with observations quite

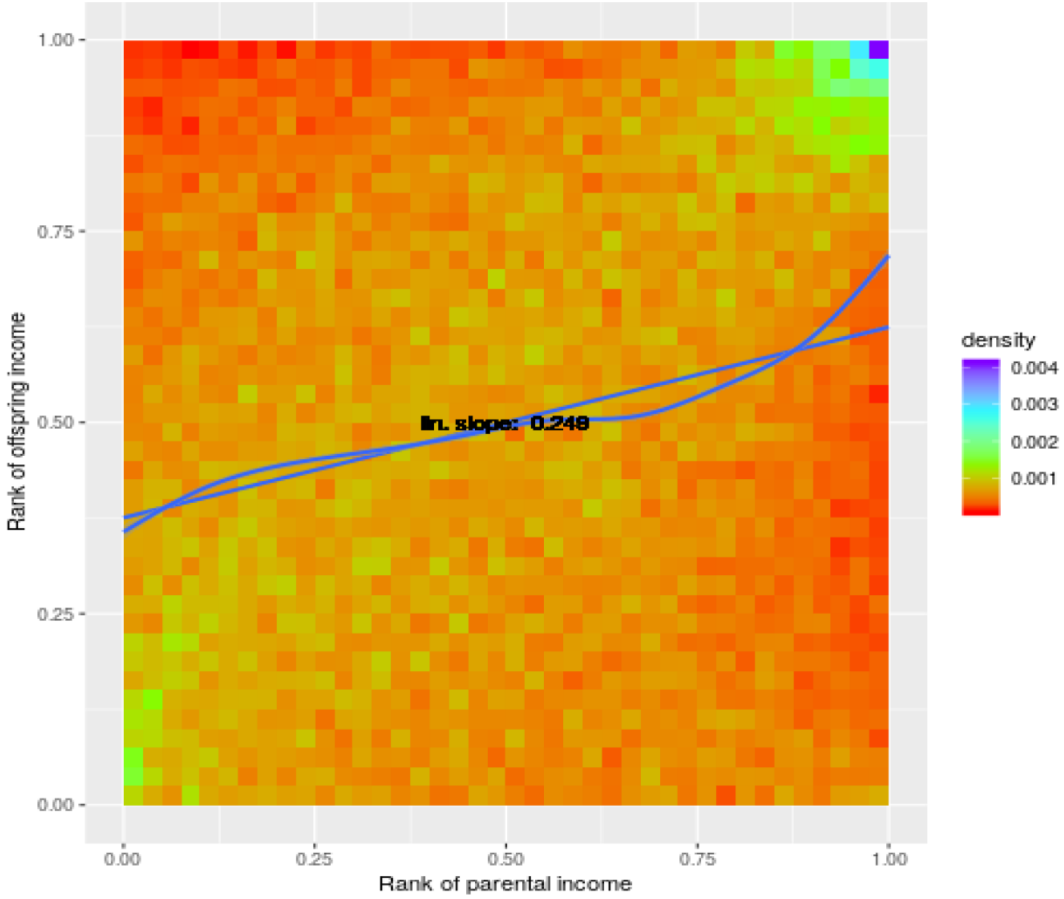
[9] Alan Krueger (2012), in his capacity as the chairman of the US Council of Economic Advisors, sparked the public interest in these results by, somewhat provocatively, calling the observed relationship "The Great Gatsby Curve".

[10] Here, Corak et al. (2014) caveat that the country-comparison might be flawed for lack of comparability of data. When using similar data definitions they do get an IGC-estimate of 0.26 for the US and 0.16 for Sweden.

[11] Corak et al. (2014, section 4.3) and Jäntti & Jenkins (2015) discuss the differences between the rank correlation and the IGC.

well spread out. The square with highest density is the one in the north-east corner, which has a density of 0.004. The linear rank-rank slope is 0.248 (std error: 0.003). The non-parametrically estimated curve has a steeper slope in the very top of the distribution. We also report (in the label of the Figure) that we have, using the same data, estimated an IGE of 0.287 (std error: 0.005) and an IGC of 0.175 (std error: 0.003).

Figure 1. Bivariate density of parent and offspring income (IGE: 0287, IGC: 0.175)



We conclude that the results from the previous literature and our display of Swedish data reveal a considerable amount of intergenerational mobility and thus not very strong family associations. For Sweden, the exception seems to be the very top of the income distribution.

Broadly, the same conclusion follows from research on educational mobility. Hertz et al. (2007) study educational persistence in a large number of countries. Due to the long-run expansion of education in most countries, it is not surprising that the difference between the estimated regression coefficients (the counterpart to the IGEs for income) and the IGCs are larger than for income. Hertz et al. (2007) report IGCs between 0.30 and 0.40 for the Nordic countries, Belgium, the Netherlands and the UK, whereas the estimate for the US is 0.46. Translated into R-squares, these numbers are only marginally higher than for income and earnings.

3. Intergenerational effects

While the intergenerational-mobility literature aims at describing the association between parental income or education and the corresponding variable for offspring, the intergenerational-effects literature addresses a causal question: what would happen to the offspring outcome if parental income or education were changed by means of some intervention, be it by policy or some other exogenous force. Obviously, the former descriptive association incorporates the impact of inherited abilities that are not changed by such thought interventions.

This research has applied alternative specifications. One is the same as equation (1) but with more information added in order to illustrate omitted variables and to come closer to the causal effect of parental resources. A second model has both parents' resources as independent variables:

$$(3) \quad y_i^{\text{offspring}} = \beta_0 + \beta_1 y_i^{\text{mother}} + \beta_2 y_i^{\text{father}} + \varepsilon_i.$$

These two specifications have different interpretations. In the first one, the coefficient on the single parental variable represents both the direct transfer from the given parent and the indirect transfer from the other parent via assortative mating. In the second one, the estimated transmission effects measure the effect of an increase in one parent's schooling on the schooling of the child, net of assortative mating effects. Holmlund, Lindahl & Plug (2011) discuss the merits of these two interpretations in more detail.[12]

The literature has predominantly used three empirical strategies in order to distinguish between the descriptive mobility association and the causal relationship. One strategy has been to use data on children of twin parents, and ideally children of monozygotic (MZ) twins. By regressing the difference across cousins in the outcome variable of interest on the difference in the same variable for their MZ-twin parents, the argument is that the equation is purged from omitted variables such as genes and shared environment. A second strategy has been to use data on adoptive children and their adoptive parents. The idea is that the regression of the former's income or schooling on the latter's will not include the impact of inherited abilities but only the causal nurturing effect of the rearing parents' resources. The third strategy has been to use an instrumental-variables (IV) technique to isolate the impact of changes in parental resources generated by an exogenous source such as a policy reform.

Holmlund, Lindahl & Plug (2011) examine research that has applied any of these strategies on schooling outcomes. In order to reconcile the results from a number of previous studies, they employ Swedish register data to estimate both the descriptive mobility associations and the presumed causal-effect parameters with the three strategies on the same sample and the same outcome variables, namely years of schooling. While they stress that the three strategies exploit different variation in the data and thus do not identify the same underlying parameters, their general conclusion is that more than half of the mobility estimates are driven by inherited abilities and therefore do not represent causal effects of additional parental schooling.

[12] They also discuss the interpretation of a third specification that uses the average of parents' resources.

Subsequent studies confirm this general conclusion although the relative importance of fathers and mothers for sons and daughters respectively seems to be an unsettled issue.[13]

A few studies have used one of the three strategies to estimate descriptive persistence and causal-effect estimates for long-run income. The results are qualitatively similar. Amin et al. (2011) use twins data from Sweden, whereas Björklund et al. (2006) use adoption data from Sweden. Both studies find that the causal-effect estimates are slightly below half of the descriptive persistence estimates. Cesarini et al. (2016) use lottery gains as an exogenous source of variation in parental income and find no significant impact on child outcomes.

We conclude that intergenerational-effect estimates are, in general, smaller than corresponding intergenerational-mobility estimates. A general pattern is that the causal-effect estimates are in the range 0-50% of the mobility estimates. This is not to say that estimates of such a lower magnitude – as long as they are not zero – are negligible and do not motivate policy concern. Further, the literature in this tradition recognizes that both the father and the mother may matter in a causal sense. Thus, the focus on primarily one parent in the intergenerational-mobility literature may obscure important dimensions of family background.

4. Sibling correlations

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

(4) $$y_{ij} = a_i + b_{ij},$$

where a_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. Therefore, the two components are by construction orthogonal and the variance of y_{ij} is the sum of the variances of the family and individual components:

(5) $$\sigma_y^2 = \sigma_a^2 + \sigma_b^2.$$

The share of the variance in the outcome variable attributable to family background effects is

(6) $$\rho = \frac{\sigma_a^2}{\sigma_a^2+\sigma_b^2}.$$

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

[13] Tsou et al. (2012) apply the adoption technique on data from Taiwan, and Amin et al. (2015) apply the twins technique on Swedish data.

as aspirations and cultural inheritance, as well as things not directly experienced in the home, such as school, church and neighborhood effects. Interactions among siblings will also 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 all captured by the individual component b_{ij}. Because such factors are also part of family and community background, the sibling correlation is a *lower bound* on the importance of such factors. [14]

Solon (1999) derives the following useful relationship between the sibling correlation and the intergenerational interesting relationship to the intergenerational correlation:[15]

(7) Sibling correlation = $(IGC)^2$ + other shared factors that are uncorrelated with the parental variable.

This result relies on the use of parental lifetime income to estimate the IGC. Obviously, parental lifetime income is shared by siblings and thus the same intergenerational equation applies to all siblings. Note here that the sibling correlation has the interpretation of an R-square from equation (5), whereas the squared IGC enters equation (6)

We now turn to a brief review of estimated sibling correlations in income (earnings) and years of education.[16] The estimates collected in Table 1 show that brother correlations in long-run earnings range from 0.32 (Norway) to 0.49 (USA). The estimates for years of schooling are somewhat higher with a range of 0.31 (sisters in Denmark), to .66 (brothers in Germany).

[14] One qualification is in order here. When outcomes are measured during adulthood, sibling interactions during adulthood will also affect the common family component. Thus, when we talk about "family background", it includes such interaction effects during adulthood so we give the concept of family background a broad interpretation.

[15] We are not aware of other derivations of this expression, but, in a more general way, it is also discussed in the early sociological literature on sibling correlations.

[16] The estimation of the variance components underlying the sibling correlation in income and earnings faces the same challenges in terms of life-cycle bias and attenuation bias as the estimation of intergenerational-mobility parameters. See Björklund, Jäntti & Lindquist (2009) for a discussion and further references.

Table 1. Selected estimates of sibling correlations in earnings and years of schooling.

Country	Study	Outcome	Sibling type	Estimate
Denmark	Schnitzlein (2014)	Long-run earnings	Brothers	.20
Denmark	Schnitzlein (2014)	Long-run earnings	Sisters	.19
Finland	Björklund et al. (2002)	Long-run earnings	Brothers	.26
Germany	Schnitzlein (2014)	Long-run earnings	Brothers	.43
Germany	Schnitzlein (2014)	Long-run earnings	Sisters	.39
Norway	Pekkarinen et al. (2017)	Long-run earnings	Brothers	.32
Sweden	Björklund et al. (2009)	Long-run earnings	Brothers	.35
USA	Mazumder (2008)	Long-run earnings	Brothers	.49
USA	Schnitzlein (2014)	Long-run earnings	Brothers	.45
USA	Schnitzlein (2014)	Long-run earnings	Sisters	.29
Denmark	Bredtmann & Smith (2018)	Years of schooling	Mixed sexes	.33
Denmark	Bredtmann & Smith (2018)	Years of schooling	Brothers	.31
Denmark	Bredtmann & Smith (2018)	Years of schooling	Sisters	.39
Germany	Schnitzlein (2014)	Years of schooling	Brothers	.66
Germany	Schnitzlein (2014)	Years of schooling	Sisters	.55
Netherlands	Sieben et al. (2001)	Years of schooling	Both sexes	.45
Norway	Björklund & Salvanes (2010)	Years of schooling	Mixed sexes	.41
Sweden	Björklund & Jäntti (2012)	Years of schooling	Brothers	.43
Sweden	Björklund & Jäntti (2012)	Years of schooling	Sisters	.40
USA	Mazumder (2008)	Years of schooling	Mixed sexes	.60

Notes: Finland: The estimates for long-run earnings stem for a comparative study that also includes Denmark, Norway and Sweden. All estimates in this study are produced by a different estimation technique than the one that is used in Björklund et al. (2002). In the latter study, the estimates are some .05–0.10 lower than in the more recent studies. Thus, the estimate reported for long-run earnings for Finland are probably also underestimated by a similar magnitude. Norway: The estimate .32 for long-run earnings is the average estimate for the birth cohorts 1953–59, 1956–1962, 1959–1965 and 1962–1968. The estimates for previous cohorts are higher and as high as 0.46 for the 1932–38, which is the first cohort in the study. Sweden: The estimate .35 for long-run earnings is the average estimate for the birth cohorts 1953–59, 1956–1962, 1959–1965 and 1962–1968. The estimates for the cohorts born in the 1930s are higher and as high as 0.49 for the cohort born 1932–38.

A key question is how these estimates compare to estimated IGCs. Can the IGCs account for most of the sibling correlation, or are “other shared factors uncorrelated with parental income (or education)” most important? A comparison of the R-squares implied by the IGCs reported and discussed in section 2 with the estimated sibling correlations reported in Table 1 clearly shows a considerable gap between the IGCs and the sibling correlation. Thus, “other factors” are the most important ones that siblings share. For example, our Swedish IGC of 0.175 implies an R-square of 0.031 to be compared with the sibling correlation around 0.30. The corresponding numbers for the US are a squared IGC of 0.068 (the estimate in Corak et al. 2014) and a sibling correlation of 0.49. The same general result follows when we look at years of schooling; a squared IGCs of 0.09 and a sibling correlation of 0.43 for Sweden and corresponding numbers of 0.21 and 0.60 for the US.

It remains to consider the fact that a sibling correlation is a lower-bound estimate of the importance of family (and neighborhood) background. Factors that make siblings different are not captured by this statistic. We return to a discussion of such factors in section 6 where we compare the approaches. However, here we address the question of whether estimates of the correlation for monozygotic (MZ) twins provide a more reasonable estimate of the total impact of family background. The general argument for using such estimates is that fraternal siblings share on average only 50 percent of their genes, whereas MZ twins share the same initial set-up of genes. Because each person has all her genes from the parents, estimates for MZ twins might be more informative about the total impact of family background.

To evaluate the arguments that are involved, we consider the simplest version of the so-called ACE-model of genetic and environmental influence. Our outcome *Y* is determined by three factors, *A* (additive genes), *C* (common environment) and *E* (idiosyncratic environment), with factor-loading parameters *a*, *c* and *e*:

(8) $Y = aA+cC+eE.$

All variables are standardized to have mean zero and variance one. A further simplifying assumption is that the three factors are uncorrelated. It is natural to assume that perfectly idiosyncratic factors are uncorrelated with shared genetic and environmental influence, whereas it is very restrictive to assume that genes and shared environment are uncorrelated and that such factors do not interact.

The total variance of *Y* equals the sum of the squared factor loadings in equation (7), $a^2+c^2+e^2$. Without the restrictive assumption, the total variance would also contain components for covariances between genes and environment and these components would prevent an additive decomposition into genetic and environmental influences.[17] For our purpose of getting the total impact of family background, such covariance terms as part of the total variance are not problematic.

Because MZ twins share all their genes, the correlation between MZ twins provides an estimate of a^2+c^2. Non-twin full siblings, for whom our results above apply, share according to standard genetic models half of their genes.[18] Thus, we can write the correlation among such siblings as $0.5a^2+c'^2$, where c' denotes the fact that these siblings might share common environment to a different, and likely lower, degree than MZ twins.

For our underlying purpose to getting estimates of the total impact of family background, it is obviously an advantage that MZ-twin correlations capture the total genetic impact. The critical question is whether MZ twins or non-twin siblings provide the most reasonable estimate of common environment. To the extent that non-twin siblings are exposed to different environments due to parental income shocks and geographical mobility, it is an advantage to use information from MZ twins who have experienced the same environmental conditions.[19] If, however, a higher common-environment component for MZ twins reflects that such MZ twins influence each other more than siblings in general, these correlations will overestimate the importance of family background in the general population. Our conclusion is that the correlation among MZ twins possibly, but not necessarily, overestimates the total impact of family and community background.

[17] See Björklund, Jäntti & Solon (2005) who relax the restrictive assumption.
[18] See Benjamin et al. (2012).
[19] If the purpose would have been to determine the relative importance of genes and environment, the fact that MZ-twins share more environment than DZ-twins or other siblings would have been problematic.

In Table 2, we have collected a number of estimates of MZ- and DZ-twin correlations in earnings (income) and years of schooling from previous studies. The MZ estimates are much higher than ordinary sibling correlations. The DZ estimates, on the other hand, are in the same

Table 2. Selected estimates of MZ- and DZ-twin correlations in earnings (income) and years of schooling.

Country	Study	Outcome	MZ-female	DZ-female	MZ-male	DZ-male
Finland	Hyytinen et al. 2019	Earnings, 20 years	0.414	0.176	0.543	0.198
Finland	Hyytinen et al. 2019	Income, 20 years	0.405	0.197	0.533	0.209
Sweden	Björklund & Jäntti 2012	Earnings, 10 years	-	-	0.73	0.22
Sweden	Björklund & Jäntti 2012	Years of schooling	0.73	0.40	0.75	0.44
Sweden	Benjamin et al. 2012	Income, 5 years	0.297	0.198	0.512	0.201
Sweden	Benjamin et al. 2012	Income, 20 years	0.481	0.221	0.626	0.270
Australia	Miller et al.	Years of schooling	0.702	0.492	0.651	0.448
USA	Behrman & Taubman 1989	Years of schooling	-	-	0.75	0.55

Note: All studies do not report standard errors so we have omitted this information. However, most estimates stem from small samples so some caution is called for when interpreting the results.

range as the sibling correlations reported above, suggesting that spacing between sibling is not an important determinant of sibling similarity.

We conclude that estimates of sibling correlations show that family background is much more important than what is revealed by intergenerational correlations. Thus, much of the similarity among siblings is due to background factors that are orthogonal to parental income or education.

5. Equality of opportunity

Inspired by Roemer (1998), a burgeoning literature has aimed at disentangling the extent to which inequality can be attributed to, on the one hand, factors beyond the individual's control and which the individual as an adult cannot be held accountable for, called *circumstances*, and, on the other hand, factors for which the individual can and should be held accountable for, called *effort*. This approach has been implemented in several ways, but the following simple version is enough to clarify the relationship to the other three approaches. The outcome of interest for individual i, Y_i, is assumed to be determined by both circumstance variables (C_i) and effort variables (E_i) according to

$$(9) \quad Y_i = \alpha C_i + \beta E_i.$$

The starting point is that all determinants can be classified as circumstances or effort, so there is no need to add an error term to this equation.

Further, it is assumed that circumstances can have an impact of their own on effort so that

$$(10) \quad E_i = \delta C_i + v_i.$$

Equations (8) and (9) constitute the underlying structural model. Empirical estimates of the parameters of this full model would allow us to compute the share of overall inequality that can be attributed to circumstances and effort. By means of the structural parameters, it would also be possible to disentangle how changes in any specific circumstance via direct and indirect effects impact on this share of overall inequality.

Most empirical studies have instead focused on the model's reduced-form, namely

$$(11)\quad Y_i = (\alpha+\beta\delta)C_i + e_i$$

The first step in the implementation of the approach is then to estimate (10). Next, the inequality caused by circumstances is derived and computed using a suitable measure of inequality. Inequality due to circumstances as a fraction of total inequality is the final outcome of this procedure. When the variance is used as the inequality measure and parental income is the only circumstance, the R-square of equation (10) – and thus the squared IGC – answers the question of what fraction of inequality is due to this single circumstance. The intergenerational-mobility approach is thus a special case of the equality-of-opportunity approach. Indeed, it is a special case in two ways. First, it only uses a single circumstance, namely the parental counterpart to the outcome variable of interest. Second, it uses a specific index of inequality, namely the variance (of, in general, log income).

This version of the approach demonstrates the formal relationship with the mobility approach and we will use it below to discuss the pros and cons of the various approaches. However, it has been more common to apply a non-parametric version of the same procedure. The non-parametric version treats all circumstances as grouped variables to define a set of so-called types who have faced the same circumstances. Thus, all variation within a type is assumed to be generated by effort. By estimating the fraction of inequality that is due to between-type inequality, an estimate of inequality of opportunity is obtained. Because parental income – the key variable in the mobility approach – is a continuous variable, we stick to the parametric version.

Some empirical applications of the inequality-of-opportunity approach have also used effort variables, sometimes in combination with circumstance variables. The most frequently used effort variable has been hours of work. With such information it is also possible to explore within-type inequality and so-called ex post inequality among individuals with the same effort. We ignore these more elaborate versions of the inequality-of-opportunity approach.

The inequality-of-opportunity approach has several strengths when compared with the other approaches. First, this approach makes explicit the obvious point that inequality of opportunity is not caused by one single socioeconomic variable, be it long-run parental income, *or* parental occupation, *or* parental education. Indeed, many experiences during childhood may have long-run consequences, creating differences in outcomes and thus qualify as circumstances in the Roemer framework. Thus, this is a broader approach to examining equality of opportunity than the intergenerational mobility approach. This overall advantage in turn has several interesting implications.

First, this approach offers a straightforward way of using parental income, *and* occupation *and* education as circumstances rather than as in the mobility approach using only one of these variables. Clark (2013) argues forcefully that underlying “true” social status is a function of all these variables and the use of only one of them will underestimate the persistence of economic advantage over generations. Although the typical way of applying the equality-of-opportunity approach described above only allows us to combine these variables for parents, this is an advantage.

Second, with this approach it is straightforward to account also for grandparents, the resources of which must be considered circumstances. However, this is not an advantage compared to the mobility approach since a more general mobility model (than eq. (1) above) also can incorporate grandparents.[20]

Third, when turning to other variables than those describing previous generations’ socio-economic status, this approach can incorporate both such variables that capture circumstances shared by siblings and such that are not shared. Shared variables may be family size, region, ethnicity, school quality etc. Such variables, in combination with a richer set of socio-economic status variables, may indeed help fill the gap between the intergenerational correlation squared and the sibling correlation as described in equation (5) above. Further, by incorporating variables not shared by siblings, this approach has the potential to go further than the sibling correlation approach and find that a larger part of inequality of outcomes can be attributed to family background than what is revealed by the sibling correlation. In other words, it is possible to raise the lower bound provided by the sibling correlation. One well-known variable that is relevant here is birth order. Any effect of birth order obviously originates from family background and can be considered a circumstance. But birth-order effects make siblings different and are not captured by the component that siblings share. Another candidate is month of birth. Research suggest that month of birth has an independent effect on income during adulthood. Such effects may to some extent be shared among siblings but most likely only partially.

Another strength of the EoP approach is that it is flexible with respect to the measure of inequality. It is possible to use measures that are sensitive to different parts of the income (or education) distribution. Considering that the whole approach is explicitly normative, this is an important advantage. Indeed, the variance, which is the inequality measure applied when using the R-square of an intergenerational mobility equation and the sibling correlation to evaluate inequality, does not satisfy one of the basic axioms of inequality measurement, namely the principle of transfers.[21] Note, however, that the analysis usually applies to the natural logarithm of income. Thus, the analysis is often conducted comparing the variance of logarithms, a measure that also violates the principle of transfers, but is invariant to scale, another basic axiom of relative inequality.[22]

From an empirical point, however, it is natural to compare the R-square of the regression equation used to estimate the contributions of circumstances with estimated sibling correlations

[20] See e.g. Lindahl et al. (2015), Solon (2018) and a special issue of the Research on Social Stratification and Mobility from 2014.

[21] See e.g. Jenkins & Van Kerm (2009).

[22] By contrast, the variance of income *levels* is translation invariant, meaning that the *addition* of a positive constant to all incomes leaves inequality unchanged, making it a measure of absolute inequality. Scale invariance means that *multiplication* of all incomes by a positive constant does not change inequality.

for the same outcome. An R-square that is much lower than the sibling correlation would signal that the equation potentially omits many important circumstances.

However, in light of the other approaches, the inequality-of-opportunity approach also has some major weaknesses. A fundamental weakness is that the approach, as it has been applied so far, implicitly assumes that the relationship between the outcome and the observed circumstances is causal. While there is no doubt that the true causal effect of, for example, parental income and education should be considered as circumstances beyond the control of the offspring, it is not obvious that the impact of the omitted variables that parental income and education brings with them in the regressions also are to be considered as circumstances. Since the studies of causal effects that we discussed above suggest that genetic effects are important omitted variables in descriptive mobility regressions, it may be argued that variables such as parental income and education primarily reflect circumstances. For many other variables, however, it is an open question whether similar arguments apply. The ideal empirical model to use to estimate the degree of inequality of opportunity would be a complete multivariate causal model and, needless to say, that is hard to achieve. Instead, it is reasonable to give the models that can be used a more descriptive interpretation.

A powerful application of the inequality-of-opportunity approach also requires very rich data. In addition to an ethically important outcome variable which reflects egalitarian goals, a rich set of circumstance variables is needed. In the literature, it is commonly argued that only a subset of the relevant circumstance variables are used and that the estimates of circumstances' contribution to inequality therefore shall be interpreted as a lower bound of inequality of opportunity. While our comparison with the sibling-correlation approach and our empirical results below strongly support this view, this argument also requires that the included circumstance variables capture the impact of circumstances only.

A burgeoning empirical literature has estimated inequality of opportunity of, in particular, income for both developed and developing countries. The recent surveys mentioned above in the introduction contain overviews of the results. For our purposes, it is of special interest to compare these results with those from the other three approaches, and especially the mobility approach. Because the inequality-of-opportunity studies have used a set of circumstances rather than parental income only, we would expect that these studies suggest that a higher fraction of income inequality can been attributed to circumstances than what the mobility studies suggest. This comparison is, however, complicated by the fact that most inequality-of-opportunity studies have used annual income as the outcome of interest, whereas the mobility studies have focused on long-run income. While long-run income is not necessarily the ultimate welfare-relevant income measure, it is most likely that the transitory income component (here we mean the deviation between annual and long-run income) is much harder to explain with the circumstance variables that have been used. This will drive the estimate of inequality of opportunity down.

Typical circumstance variables in the available studies have been parental education and occupation, nationality, region, and the number of siblings. For developed countries the estimates of circumstances' share of inequality range from 0.023 for Norway and 0.186 for the US. Although these estimates are based on the mean logarithmic deviation (also denoted

GE(0)) and not the variance of log income, they correspond to estimates of IGCs from 0.15 to 0.41, i.e., close to the estimates reported above, and far below the available sibling correlations.

Björklund, Jäntti & Roemer (2012) have used comparatively rich Swedish register data and added IQ at age 18 as a circumstance variable; note that 18 is the legal age in Sweden. Their outcome variable is long-run income. IQ turns out to be more important than all the conventional circumstance variables combined. Hederos, Jäntti & Lindahl (2017) extend this analysis by studying both men and women and using gender as a separate circumstance. Gender also turns out to be more important than the conventional circumstance variables.

6. Comparing the approaches: an empirical illustration

We now continue with an empirical illustration with our Swedish data set that we used above to estimate intergenerational mobility parameters. Our goal is to show how the approaches are related to each other and to illustrate the pros and cons of the equality-of-opportunity approach relative to the other approaches that we discussed in the previous section.

We have estimated a set of regression equations with a number of circumstance indicators as explanatory variables. Following the practice in the income-based intergenerational-mobility and sibling-correlation studies, we have estimated these equations with the log of long-run offspring income as the dependent variable. We have also used the log of long-run parental income on the right hand side as a basic circumstance variable. We report these estimates in the Appendix.

However, such equations cannot be used directly to calculate circumstances' share of total inequality with a rich set of inequality measures. In addition to the variance on which the R-square is based, we want to apply inequality measures that respect the principle of transfers. We use the Gini coefficient and three members of the generalized entropy (GE) class, namely GE(0) (also referred to as the mean logarithmic deviation), GE(1) and the squared coefficient of variation (CV^2), (which equals 2xGE(2)). We use these four measures because they are sensitive to (transfers in) different parts of the distribution and thus represent different values regarding income inequality. GE(0) and GE(1) are particularly sensitive to the bottom of the distribution, GE(2) to the top and the Gini to the middle of the distribution.[23] The GE(0) and GE (1) measures, however, cannot be computed from observations of log income. This fact is not a simple computational problem but reflects the more fundamental problem that a normative assessment of income inequality must have income – and not log of income – as the starting point. Taking the log of income implies a transformation of income that in itself has normative implications.

To illustrate also this point, the results in Table 3 are based on both income and its natural logarithm. For each set of circumstances we include, we test if they are statistically significant in the sense of whether the null hypothesis -- that the coefficients on the added circumstances

[23] See e.g. Jenkins (1991).

are zero -- can be rejected. The p-values, based on the asymptotic chi-squared statistic from a Wald test, in all cases indicate the null can be rejected (results reported in the appendix).[24]

The results in row A demonstrate the correspondence between the intergenerational-mobility and the equality-of-opportunity approaches; it is circumstances' share of total inequality with parental income as the only circumstance variable. The reported R-square in panel I is thus the squared IGC in Figure 1 above (0.175x0.175=0.031). The R-square is lower in panel II when we have recalculated explained log income into explained raw income.

We can already see an interesting persistent pattern in our results, namely that circumstances' share of inequality is very sensitive to the measure of inequality. In particular, when we use the Gini coefficient, circumstances' share is 0.215 when we use the log of income in panel I and 0.230 when we use the level of income in panel II. The GE(0) and GE(1) measures also reveal higher shares for circumstances than the R-square but far from the level when we apply the Gini. We return below to a discussion about the reasons for this result.

In row B, we apply a more flexible functional form for parental income by adding a quadratic term. As expected, all the measures of inequality now report a higher share for circumstances, but the differences are not substantial.

In the model in row C, we have added two frequently used circumstance variables, namely an indicator for intact family during childhood, as well as the number of siblings defined in three levels (no siblings, 1-2 siblings and 3+ siblings). As expected from previous research, these variables matter and raise the share of circumstances somewhat.

The model in row D adds parental occupation (10 levels) and parental education (3 levels) to the previous model with income and income squared. Thus, we here address Clark's (2014) claim that true social status is better measured by a combination of income, occupation and education.[25] Our results give some support to this claim. The R-square of the equation rises from 0.039 to 0.042 and circumstances' share of inequality rises from 0.252 to 0.263 according to the Gini.

Next, in row E, we add information about grandparents' education. However, as in previous studies using such information, the sampling scheme does not allow us to identify all four grandparents.[26] We can identify paternal grandparents. Further, since these parents are quite old, there is only information about their education and there is not so much variation in education for these cohorts of Swedes. We measure grandparental education at three levels: both low, one high and one low, both high. In our background regressions with log income as the outcome variable, the coefficient for "both high" is +0.03 and strongly significantly different from zero. The coefficient "one high and one low", however, is not significantly

[24] The results for the hypothesis tests need to be taken with a grain of salt, as we estimate a series of regressions, always adding new circumstances, the coefficients on which are then tested for being statistically significantly different from zero. This is a situation in which some sequential testing procedure would be more appropriate. However, our purpose is merely here to demonstrate that all circumstances we include are "statistically significant" as conventionally examined and the "naive" procedure we use serves this purpose. As a more general remark, we note that such tests have often been ignored in previous studies. One reason for this may be that only few circumstances have been included in previous studies and it has been obvious that their coefficients are significantly different from zero.

[25] Note though that we stick to using long-run income as the outcome variable on the left hand side. Clark's claim is that both generation's true socio-economic status is a combination of income, occupation and education.

[26] Lindahl et al. (2015) use data from a Swedish survey and can also only identify two of four grandparents.

different from zero. Despite this fact, nothing happens to circumstances share of total inequality when we add these variables. This result may sound contradictory to the results in Lindahl et al. (2015). Using another data set, they find effects of grandparents and great-grandparents that suggest "much lower" long-run mobility than what is implied by the conventional two-generation model. However, they also report a small increase in the R-square when they add grandparental characteristics to a conventional two-generation model which suggests that the results are not so different in terms of the explanatory power of the models.[27]

Next, in row F, we add "first born" as a circumstance variable. As argued above, this is a variable that makes siblings different and thus a family background variable that is additional to what siblings share and what the sibling correlation accounts for. In the background regression equation for log income, this dummy variable is clearly significant with a coefficient estimate of 0.029. This point estimate is somewhat higher than in some previous studies on Swedish and Norwegian data (see Björklund & Jäntti 2012 and Black et al. 2005). Yet, this variable does not raise the contribution of circumstances to overall inequality in a visible way. Indeed, only the third decimal point of the Gini is changed when this additional circumstance is added.

Finally, we add two variables capturing the individual's skills at age 18, namely IQ (row G) and non-cognitive skills (row H). Although 18 is the age of becoming legal in Sweden and many other countries, it can be argued that skills at this age to a considerable extent is due to effort and that a teenager who works hard at school deserves the return to this hard work later in life. Against this view, it can be argued that the skills of an 18-years-old person to a large extent are determined by processes earlier in life which the individual cannot be held responsible for during adulthood. We do not have a strong opinion on this issue, but find it useful to investigate the importance of skills at age 18 in the framework of inequality of opportunity.[28]

The results in Table 3 show that these are really important variables. The R-square for the log of income rises from 0.042 to 0.064 (row G) and 0.081 (row H).[29] The other measures of inequality also rise considerably; for the Gini the share of inequality that is due to circumstances becomes around 40 percent. These results suggest that early skills are really important sources of inequality of opportunity. Indeed, when these variables are added other circumstance variables such as "grandparental education" and "first-born" are no longer significant in the background regression equation.

Although the final model, by including offspring's IQ and non-cognitive skills at age 18, only brings the R-square up to 0.081, this number is only about a third of the sibling correlations in long-run income reported above in section 4. This suggests that siblings share much that these circumstance variables do not pick up. It remains a challenge to learn what these factors

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[27] For log earnings their R-square rises from 0.064 (Table 7) to 0.067 (Table 9) and for years of schooling it rises from 0.152 (Table 6) to 0.164 (Table 8) when adding grandparental characteristics. Note here that their equations include polynomials in birth year for all generations, which raises the overall level of the R-square. We use the residual of income after control for age at income. Note also that we add grandparental characteristics after having added a set of other circumstances which helps explain that our R-square was not much affected by the marginal addition of grandparental education.

[28] Lindqvist & Vestman (2011) investigate these two variables as determinants of earnings in Sweden.

[29] Björklund, Jäntti & Roemer (2012) use IQ as a circumstance variable, whereas Hederos, Jäntti & Lindahl (2017) use both IQ and non-cognitive skills.

are. If they can be considered as circumstance variables, the degree of inequality of opportunity in society is severely underestimated in both the intergenerational-mobility and equality-of-opportunity approaches.

Interestingly, some studies in the sibling-correlation tradition have also addressed the question whether a richer set of family background variables can account for the gap between the fraction of the variance that can be explained by a single parental background variable and the fraction that can be attributed to the factors that siblings share. Applying the variance decomposition technique that is used to estimate the sibling correlation, Björklund, Lindahl & Lindquist (2010) explore determinants of long-run income on Swedish data.[30] They find that measures of family structure and social problems in the up-bringing family account for very little of the family component in addition to the standard variables income, education and occupation. However, by adding much more detailed information about the family such as parental involvement in schoolwork, parenting practices, and maternal attitudes, the explanatory power of the set of variables increases from about one quarter to nearly two thirds. These results suggest that quite detailed information about family background is needed to understand why family is as important as suggested by the sibling correlation.

Finally, we note again that a striking message of Table 3 is that the choice of inequality measure matters much for our interpretation of inequality. In particular, if we use the value judgements implied by the Gini coefficient, a much higher fraction of total inequality can be attributed to circumstances than for the other measures. Our interpretation of this result is that, as the Gini gives a high weight to income differences close to the middle of the income distribution, the circumstance variables do a good job of explaining such differences. The other measures give a higher weight to either low (GE(0) and GE(1)) or high (CV^2) values in the distribution and the circumstance variables are less effective in accounting for those outcomes. Relying only on standard econometric measures such as the R-squared for normative purposes can be quite misleading.

In a similar vein, as we discuss above, while the regression fit may be better after a taking the logarithm of income, as can be seen by comparing the R square across panels I and II, whether one examines the log or level of income has strong normative implications. The Gini coefficient, for instance, becomes remarkably low after the transformation and the two generalized entropy measures that place greater weight at the bottom of the distribution are not defined. When claiming to address the equality-of-opportunity issue, it is important to be very specific about all choices, including which type of transformations are applied to the data, as many such choices have normative implications.

[30] See Mazumder (2008) for a related exercise using US data with qualitatively similar results. Hederos Eriksson et al. (2016) perform a similar analysis of the importance of family and neighborhood background as determinants of criminal convictions and incarceration. They find that parental criminality and family structure account for more of the sibling correlation than parental income and education or neighborhood characteristics but that the lion's share of the sibling correlation is unaccounted for by these factors. Similar results are found by Bredtman & Smith (2018) for years of schooling in Denmark.

Table 3. Inequality of total income and share of inequality accounted for by circumstances with successively expanded circumstances – logs of offspring income in panel I, levels of offspring income in panel II.

I. Outcome: Log of offspring long-run income

	Gini	GE(0)	GE(1)	1/2CV^2	R^2
Overall inequality	0.023	n.a.	n.a.	0.003	
A: Log parental income	0.215	n.a.	n.a.	0.031	0.031
B: + log parental income squared	0.227	n.a.	n.a.	0.033	0.033
C: +family structure, number of siblings	0.252	n.a.	n.a.	0.039	0.039
D: +parental education and occupation	0.263	n.a.	n.a.	0.042	0.042
E: + grandparental education	0.263	n.a.	n.a.	0.042	0.042
F: +birth order	0.264	n.a.	n.a.	0.042	0.042
G: +own IQ	0.333	n.a.	n.a.	0.064	0.064
H: +own non-cognitive skills	0.376	n.a.	n.a.	0.081	0.081

II Outcome: Level of offspring long-run income

	Gini	GE(0)	GE(1)	1/2CV^2	R^2
Overall inequality	0.267	0.164	0.185	1.643	
A: Log parental income	0.230	0.041	0.036	0.008	0.006
B: + log parental income squared	0.249	0.046	0.043	0.010	0.007
C: +family structure, number of siblings	0.274	0.055	0.050	0.012	0.009
D: +parental education and occupation	0.289	0.059	0.053	0.013	0.009
E: + grandparental education	0.289	0.059	0.054	0.013	0.009
F: +birth order	0.290	0.059	0.054	0.013	0.009
G: +own IQ	0.364	0.089	0.081	0.019	0.014
H: +own non-cognitive skills	0.407	0.111	0.099	0.023	0.017

Note: The GE(0) and GE(1) in panel I are all not available (n.a.) as the log of long-run income generates some negative values.

7. Discussion and conclusions

We have discussed four approaches to the study of the relationship between family background and socio-economic status typified by education and long-run income during adulthood. We have argued that all four approaches are informative about important questions. In particular, we have stressed that the four approaches complement each other and in combination provide a richer picture of the role of family background than one of them in isolation.

The established study of intergenerational mobility in education or income (or for that matter social class that we have not discussed here) provides an easy-to-understand picture of the transmission of inequality from one generation to the next, a picture that the public-policy debate seems to appreciate. Such studies have often been motivated by the claim that they address the inequality-of-opportunity question. But as demonstrated by the equality-of-opportunity and sibling correlation estimates, there is much more to the importance of family background than is revealed by one single socio-economic status variable. It is, however, possible that a reasonable degree of comparability over time and space can be achieved by the one-dimensional mobility-approach to the study of family background.

The intergenerational-effects studies address well-defined questions of high scientific and public-policy relevance. Indeed, they answer the question of how equality of opportunity may change, or historically has changed, in a response to interventions in the income process or educational reforms. Although several studies suggest that such intergenerational causal effects may be quite small (especially compared to the intergenerational-mobility estimates), they may very well motivate policy interventions of various sorts. Further, the effects may be large in some social contexts. More generally, it is important to learn about potential overall and distributional effects of specific interventions that change the parental generation's income or education.

We have also stressed that it is important to learn about the overall impact of family background and other background factors that the individual cannot be held responsible for. Is the bulk of inequality in education and income due to individual effort and related factors from which the individual deserves the returns? Or do "circumstances" beyond the individual's control dominate among the determinants of observed inequality? Available estimates of intergenerational mobility suggest that it is the former question that should be given an affirmative answer.

Estimates of sibling correlations show that there is much more in family and community background than what intergenerational-mobility estimates reveal. The sibling correlation has a clear statistical interpretation as the fraction of observed variance that is due to factors shared by siblings. Yet, it does not provide the direct answer to any well-defined scientific or public-policy question. In our view, however, this correlation serves as an important benchmark against which intergenerational-mobility and equality-of-opportunity estimates more often should be compared. To learn what it is that siblings share but that is not accounted for by parental income, education, and occupation must have high research priority. If these factors primarily can be considered "circumstances", there is much inequality of opportunity in modern societies. In particular, the estimates for MZ twins, which likely account for all shared genetic influence, suggest that well above 50 percent of the variance may be due to circumstances.

The equality-of-opportunity approach addresses the underlying normative questions in a clear way. It has the obvious starting point that an assessment of inequality of opportunity should be based on a multivariate framework. Unlike the sibling-correlation approach, it is not restricted to factors that siblings share. Further, by allowing for alternative measures of inequality, this approach also offers a set of results that evaluate inequality through the lens of different value judgements. Nevertheless, most empirical approaches have not yet come that far in accounting for observed inequality of education and income. Although this approach should ideally be based on a multivariate causal model, we argue that there is much room for extending the currently used quite mechanical empirical model. One way is to add more and better circumstance variables. Examples of underexplored information in studies of income and education inequality of opportunity are early health and skill variables as well as school quality indicators. In the future it might also be possible to add explicit genetic information to the set of circumstances. Another way is to introduce more heterogeneity in the model and allow the impact of conventional circumstance variables to vary depending on the context of the individual. One example is parental income. In the mobility approach, this variable is assumed to have the same impact on all individuals and even on the siblings in the same family despite the fact that siblings may have grown up with quite different parental resources.[31]

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[31] See Bingley & Cappellari (2019) for a recent approach.

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Appendix

In this appendix, we describe the structure of our data, how we define the research population and sample, and what variable manipulations we apply. The appendix also reports all of our empirical estimates, including descriptive statistics for our sample.

Data

Structure of the dataset

We exploit a combination of Swedish administrative register data sets. A first and basic source is Statistics Sweden's so-called Multi-generational register. This is a register of all persons who were born 1932 and onward, and who have ever received a unique national registration number from 1961 and onward.[32] For the Swedish population defined in this way, the register contains information about biological (and adoptive) parents and their national registration number. From this information, one can also infer which individuals are related as siblings; full siblings are those who have the same father and mother, half siblings are those who only have one parent in common.

Our analysis sample is based on a 35 percent random sample of the Swedish population born 1932- 67 defined in this register; these individuals are called index persons. We also use the Multi-generational register to identify parents and siblings of index persons.

The second source is the set of bidecennial censuses conducted from 1960 to 1980. We can identify our main sample of sons in the households of these censuses as well as other persons in the household. Thus we can determine whether our offspring generation lived with their biological parents or not in the fall of these census years.

The third source is Statistics Sweden's income register, which in turn come from the Swedish tax assessment procedure. A limitation is that such data are available only from 1968 onwards. From that year the income register provides data on total income from all sources of income, from work, self employment, capital, real estate as well as some transfers (from 1974 onward). We use such income data for both parents and sons. The earlier data for parents stem from their own compulsory tax assessments. In later years, when we measure sons' incomes, the source of the data is compulsory reports by employers to the tax authorities.

The fourth source is the Swedish Military Enlistment Battery, which provides a measure of intellectual capacity. The purpose of these tests is to classify Swedish men to different military positions with different demands on general intellectual capacity. For the cohorts who now are adults, military service was compulsory in Sweden with only few exceptions. Generally, the tests were done during the year when men turned 18 years of age. The Enlistment Battery contained four cognitive tests: instructions, synonyms, metal folding and technical comprehension. The subtests were designed to measure the primary IQ factors Induction, Verbal Comprehension, Spatial Ability and Technical Comprehension respectively. We use a summary measure of intellectual ability based on the four tests provided by the military

[32] The requirement that the persons must have been registered in Sweden from 1961 and onward implies that persons who died between 1932 and 1960 are not included. For our purposes, however, this is not a problem since we want to observe outcomes in the 1990s and 2000s.

organization that runs the tests.[33] The Enlistment Battery also provides measures of height and weight, which we use to calculate the body mass index, BMI.

To construct our analysis sample, we make use of the fact that all four data sources contain the unique Swedish national registration number, by means of which we can merge the information from the four sources.

We need information on the offspring (in our case of sons, esp. income and their military conscription data) and their childhood circumstances, especially their parents' and their family characteristics, as well as grandparents. This means that we need to rely on the subset of our data in which both off- spring and at least one of the parents are index persons. We require both parents to be present in our data but only one of the grandparents (who can be either maternal or paternal). We have a number of cases in which both sets of grandparents are present; in those cases, we include an offspring as two distinct observations (excluding the second set of grandparents does not affect any of our results).

Income variables

As our outcome variable for sons, we use a measure of total market income before taxes provided by Statistics Sweden. It includes income from all sources, that is, labor, business, capital, realized capital gains as well as some taxable social transfers such as unemployment insurance, sickness pay, parental leave payment, and pensions. We use the average of real total income over the years when sons were 32-38 years of age. At these ages, we are likely to get a good estimate of long-run income (see Böhlmark and Lindquist, 2006). Further, averaging over as long a period as seven years is likely to eliminate most transitory income variation that is not relevant for our purposes.

Family background variables (circumstances)

Parents and family For parental income we apply the same income concept as for sons. We use a multi-year average of the sum of the two biological parents' incomes during the years when the son was 13-17 years old. We treat an income observation of SEK 100 or lower (in 2007 prices) as missing, so the over-time average is only taken for non-zero income. This measure we divide into four quartile groups of equal size.

To measure parental education, we make use of the fact that the 1970 census made special effort to collect information about education. We use the educational level of the biological parent who has the highest educational level according to the information in the census. This level in turn, we split into three groups: only compulsory school, more than compulsory school but no college, and at least some college.

Our measure of parental occupation is the occupation of the father as registered in the Census around the time the son was between 13 top 17 years old, measured using the top-level code.

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[33] Mårdberg & Carlstedt (1998) and Carlstedt (2000) provide more information on the cognitive tests. See also Björklund, Eriksson & Jäntti (2010).

We also use the censuses to construct a family type indicator. This is equal to one if the son lived with both biological parents during his first three censuses in life. For example, for the cohort born in 1955 this implies that we require that the son lived with both biological parents in the 1960, 1965, and 1970 censuses. If this condition is not fulfilled, the indicator takes on the value zero.

We use data from the Multi-generational register to compute the number of full biological siblings. We split the observations into three groups: 0, 1-2 or 3+ siblings.

Grandparents We use grandparent's education in a similar way as we do parent's education. We take the grandparent with the higher level of education, and recode it into low, middle or high. There are three differences to the coding of parental education, however. First, while in the sample, we require both parents to be observed, we are content with observing only one of the grandparents. Second, as the grandparent's education stems from the 1970 census which recorded education only for those who were 59 years old or younger (i.e., born in 1911 or later), we have many missing observations. Thus, we include a "no info" category. And third, as the education levels were substantially lower in the grandparent's cohorts, we have defined "high" to include also some non-university education which for parents was coded as "middle".

Own characteristics For *IQ*, we split the summary measure of intellectual ability from the Military Enlistment Battery into four quartile groups The use of own IQ as a circumstance, as opposed to effort, is potentially controversial, for several reasons. First, cognitive test scores at age 18 are very likely affected by educational choices up to that age. Moreover, to some extent such choices, and performance within the chosen educational path, reflect effort on the part of the young individual making them. However, the key here is the following. We define as a circumstance factors that affect socio-economic outcomes, but for which we do not hold the individual responsible. Your actions and effort prior to the age of 18, even if they in part reflect your ambitions and motivations, are not something we would hold you responsible for.

Non-cognitive ability is measured based on structured a interview during military enlistment, which was mandatory for Swedish men aged 18-20. The approximately 25-minute interviews are conducted by a psychologist charged with rating an individual's suitability for military service. The conscript's overall suitability for military service is given a score from 1 to 9. The psychologist determines the score based on a number of specific characteristics such as level of responsibility, independence, outgoing character, persistence, emotional stability, power of initiation and social skills (Lindqvist and Vestman, 2011). We split the measure of non-cognitive ability into four quartile groups.

Descriptive statistics and regression results

We report descriptive statistics in Table A1, We report chi-square-statistics for whether or not the added terms are "statistically significant" (as measured by the Wald test whether the added terms have a coefficient vector that is jointly zero in Table A2. Finally, in Table A3, we report coefficient estimates of the regression equation underlying Table 3 in the main text.

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Table A1. Variable descriptives
A. Continuous variables

Variable	$\overline{x}$	$s(x)$	$p25$	$p50$	$p75$	n
Offspring Income (000)	280.37	359.33	201.48	251.38	314.12	107685
Parental Income (000)	359.80	160.91	269.73	339.50	415.66	107685
Offspring log income	12.38	0.67	12.21	12.43	12.66	107685
Parental log income	12.71	0.41	12.51	12.74	12.94	107685

B. Discrete variables

Variable	Category	Proportion
Family structure	Not intact family	0.291
	Intact family	0.709
Sibling (no of)	No sibs	0.139
	1-2 sibs	0.727
	3+ sibs	0.134
ParentEduc	Missing info	0.009
	Both low	0.354
	One low, One high	0.517
	Both high	0.120
Parental Occupation	0 Military	0.189
	1 Management	0.037
	2 Req adv univ educ	0.031
	3 Req some univ educ	0.094
	4 Admin and custom ser	0.059
	5 Service, care and sales	0.005
	6 Agricult, forestry and fish	0.094
	7 Construct and manuf	0.292
	8 Mach manuf and transport	0.133
	9 Req short educ	0.065
Grandparental Education	Missing info	0.517
	Both low	0.374
	One low, one high	0.085
	Both high	0.023
First Born	First born	0.432
	Not first born	0.568

Table A2. Regression test-statistics – p-values based on multivariate chi-squared tests for each set of included circumstances

Parental log income	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Parental log income ^{2}/100		0.000	0.000	0.000	0.000	0.000	0.000	0.000
Family			0.000	0.000	0.000	0.000	0.000	0.000
Sib			0.000	0.000	0.000	0.000	0.000	0.000
Parental Education				0.000	0.000	0.000	0.000	0.000
Parental Occupation				0.000	0.000	0.000	0.000	0.000
Grandparental Education					0.007	0.007	0.007	0.006
First Born						0.000	0.000	0.000
IQ							0.000	0.000
Non-cognitive skills								0.000

Note: Table shows the p-values for each regression of the multivariate test of setting all coefficients associated with a backgroud characteristic to zero. See Table A3 for corresponding coefficient estimates and standard errors.

Table A3. Regression results – taxable income. N=107685

Intercept		8.736 (0.062)	18.112 (0.622)	18.808 (0.621)	16.333 (0.641)	16.277 (0.642)	16.324 (0.642)	15.425 (0.635)	15.027 (0.629)
Parental log income		0.287 (0.005)	−1.211 (0.099)	−1.321 (0.099)	−0.880 (0.103)	−0.871 (0.103)	−0.885 (0.103)	−0.740 (0.102)	−0.670 (0.101)
Parental log income ^{2}/100			5.974 (0.394)	6.353 (0.393)	4.396 (0.413)	4.355 (0.413)	4.426 (0.413)	3.764 (0.409)	3.416 (0.405)
Family struc-ture (omitted: family sep.)	Intact family			0.108 (0.005)	0.107 (0.005)	0.107 (0.005)	0.106 (0.005)	0.092 (0.005)	0.079 (0.005)
Siblings (omit-ted: No sibs)	1-2 sibs			0.027 (0.006)	0.024 (0.006)	0.024 (0.006)	0.034 (0.006)	0.027 (0.006)	0.020 (0.006)
	3+ sibs			−0.023 (0.008)	−0.022 (0.008)	−0.023 (0.008)	−0.006 (0.008)	−0.006 (0.008)	−0.004 (0.008)
Parental Educa--tion (omitted: n.a.)	Both low				0.030 (0.021)	0.032 (0.021)	0.031 (0.021)	0.051 (0.021)	0.055 (0.021)
	One low, One high				0.064 (0.021)	0.065 (0.021)	0.063 (0.021)	0.055 (0.021)	0.051 (0.021)
	Both high				0.117 (0.022)	0.116 (0.022)	0.111 (0.022)	0.056 (0.022)	0.049 (0.021)
Parental Occu-pation (omitted: 0 Military)	1 Manageme nt				0.047 (0.011)	0.048 (0.011)	0.048 (0.011)	0.052 (0.011)	0.050 (0.011)
	2 Req adv univ educ				−0.005 (0.012)	−0.004 (0.012)	−0.005 (0.012)	0.007 (0.012)	0.010 (0.012)
	3 Req some univ educ				0.011 (0.008)	0.011 (0.008)	0.011 (0.008)	0.023 (0.008)	0.017 (0.008)
	4 Admin and custom ser				−0.040 (0.010)	−0.040 (0.010)	−0.039 (0.010)	−0.019 (0.010)	−0.021 (0.010)
	5 Service, care and sales				−0.047 (0.028)	−0.045 (0.028)	−0.047 (0.028)	−0.001 (0.028)	0.003 (0.027)
	6 Agricult, for-estry and fish				−0.027 (0.008)	−0.027 (0.009)	−0.026 (0.009)	0.008 (0.008)	0.011 (0.008)
	7 Construct and manuf				−0.035 (0.007)	−0.034 (0.007)	−0.034 (0.007)	0.000 (0.007)	0.005 (0.007)
	8 Mach manuf and transport				−0.050 (0.008)	−0.048 (0.008)	−0.048 (0.008)	−0.006 (0.008)	0.002 (0.008)
	9 Req short educ				−0.038 (0.009)	−0.037 (0.009)	−0.036 (0.009)	−0.008 (0.009)	−0.012 (0.009)
Grandparental Education (omitted: n.a.)	Both low					−0.009 (0.004)	−0.011 (0.004)	−0.000 (0.004)	−0.001 (0.004)
	One low, one high					0.005 (0.007)	0.002 (0.007)	0.004 (0.007)	0.000 (0.007)

	Both high					0.030 (0.013)	0.026 (0.013)	0.026 (0.013)	0.018 (0.013)
First Born (omi-tted: First born)	Not first born						0.029 (0.004)	0.012 (0.004)	0.006 (0.004)
IQ (omitted: 1st qrt)	2nd qrt							0.106 (0.005)	0.070 (0.005)
	3rd qrt							0.165 (0.006)	0.115 (0.006)
	4th qrt							0.278 (0.006)	0.214 (0.006)
NC (omitted: 1st qrt)	2nd qrt								0.138 (0.005)
	3rd qrt								0.177 (0.006)
	4th qrt								0.238 (0.006)
Adj R^2		0.030 7	0.0327	0.039	0.0418	0.0419	0.0423	0.064	0.0803