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DISCUSSION PAPER SERIES

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ABSTRACT

The Relationship Between Intergenerational Mobility and Equality of Opportunity*

Among economists, empirical analysis of social mobility and the role of parental background is largely carried out in two separate strands of research. The intergenerational mobility literature estimates parent-child persistence in a certain outcome of interest, such as income. In contrast, the equality of opportunity literature is rooted in a normative framework, and has only more recently started generating empirical evidence. Intergenerational mobility regressions are relatively straightforward to estimate, but their normative implications are less obvious. In contrast, measures of equality of opportunity have a policy-relelvant interpretation, but are demanding in terms of data, requiring the researcher to observe a large set of determinants of socioeconomic status for large samples. But maybe the two approaches capture similar underlying dynamics? We compare the two approaches by estimating both equality of opportunity and intergenerational mobility measures — as well as sibling correlations — across 16 birth cohorts within 126 Swedish local labor markets. Using these estimates, we test to what extent the different measures correlate, resulting in insights on the plausibility of interpreting intergenerational mobility measures as informative about equality of opportunity.

JEL Classification: D31, J62, D63

Keywords: equality of opportunity, intergenerational mobility, sibling correlations

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1 Introduction

The study of intergenerational mobility and the role of family background has garnered increasing attention in research and policy. By comparing intergenerational estimates across time or places, scholars often conjecture that a lack of mobility indicates inequality of opportunity. For example, Chetty et al. (2014) highlight areas with high income mobility in the United States as "lands of opportunity," linking mobility to equality of opportunity.[1] Similarly, sibling correlations provide an alternative perspective on the family background's importance, and strong sibling similarities in outcomes are often taken as an indicator of inequality of opportunity.[2]

Theoretically, neither mobility measures nor sibling correlations need to be good indicators of equality of opportunity — as Björklund and Jäntti (2020) put it, the intergenerational approach captures a "special case" of equality of opportunity. Another strand of the literature, building on formalizations by Roemer (1998), explicitly estimates equality of opportunity (EOp) by isolating the effect of circumstances beyond one's control such as race or parental education on outcomes (see Roemer and Trannoy 2016, for a survey). While this approach builds on formal definitions and axioms, empirically distinguishing personal effort and choices from circumstances is complicated, and unobserved factors lead to omitted-variable biases.[3] In short, the mobility approach is practically appealing but lacks in conceptual clarity, while the EOp approach builds on a rigorous framework but is harder to empirically operationalize. But maybe they largely capture the same dynamics?

Our objective is to connect these two literatures. Using population-wide data from Sweden, we examine the correlation between measures of intergenerational mobility, sibling correlations, and indices of equality of opportunity across regions. Our goal is to empirically test whether a lack of intergenerational mobility, or strong sibling similarities, indeed implies inequality of opportunity, and vice versa.

In our analysis, we divide Sweden into 126 local labor markets (similar to US commuting zones), and estimate *regional* measures of mobility and IOp in permanent income. We estimate two intergenerational measures: the intergenerational income elasticity (IGE) and rank persistence. We also consider the sibling correlation as an alternative measure of the role of family background. We loosely refer to these three indices collectively as measures of *social mobility*.[4] To estimate inequality of opportunity (IOp) indices, we follow the machine-learning approach of Brunori et al. (2023). This approach amounts to using conditional inference forests to estimate the extent to which existing income inequality is due to circumstances as opposed to "effort".[5] We compute regional measures of

[1] See also Alesina et al. (2018), Corak (2013), and Ward (2023).

[2] See, e.g., Björklund et al. (2002, 2010), Björklund and Jäntti (2012), Hällsten (2014), Raaum et al. (2006), and Solon et al. (1991).

[3] Kanbur and Wagstaff (2016) note that these challenges hinder cross-time or cross-space EOp comparisons, as estimates may rely on inconsistent data.

[4] We acknowledge that the intergenerational measures are conceptually different from the sibling correlation, and that *social* mobility is often (especially among sociologists) measured in terms of social class. Nevertheless, for our purposes it is useful to group them under one umbrella term.

[5] The estimation of IOp is essentially a prediction problem, which makes it eminently suited for applying machine learning techniques. Brunori et al. (2023) show that their approach

inequality, and merge several administrative data sources to form a large set of circumstance variables.

Our contributions are threefold. First, we link the different literatures by providing evidence on the correlations between measures of social mobility and IOp indices. This is of immediate value: a strong correlation would indicate that differences over time or place in social mobility reflect differences in equality of opportunity. Second, by distinguishing between different mobility measures, we are able to examine which measures are more or less correlated with IOp. Third, through a number of robustness tests, we provide evidence on which factors determine the extent of correlation and in what contexts the correlations become weaker and stronger. We also show that our main results hold when we use variation across cohorts or cohort-by-region.

We begin by documenting national-level estimates that are similar in magnitude to those in prior studies for Sweden, with the IGE and rank-slope of about 0.23–0.25, a sibling correlation of 0.27, and a (relative) inequality of opportunity (IOp) index of 0.39. But the core of our analysis concerns the regional variation in these measures. In particular, we find a strong positive relationship between intergenerational and IOp measures across regions. IGEs and rank-slopes are strongly associated with IOp indices, with cross-region correlations above 0.8. Among the top (bottom) 25 regions in terms of mobility a majority of them are also among the top (bottom) 25 in EOp.

The sibling correlation, however, tends to be more moderately correlated with other measures, suggesting that it captures partly different aspects of family background. However, we also find that sampling variation across smaller regions depresses our estimated correlations between the different measures, and especially so for the sibling correlation. When weighting the estimates by region size or excluding smaller regions, the pattern for the sibling correlation is more similar to that of the other measures.

Furthermore, the strength of the correlation between IOp and social mobility is not due to a mechanical role of parental income in the IOp indices. In fact, the different measures remain nearly as strongly correlated when parental income is excluded from the circumstances underlying the IOp index.

Finally, we emphasize that while the *variation* in the measures co-move strongly across regions and cohorts, their *levels* in terms of explained income variation differ. The sibling correlation or (relative) IOp indices attribute much more of the variance in income to family-background factors compared to the (squared) IGE or rank correlation (see, e.g., Björklund and Jäntti 2020; Solon 1999).

1.1 Related literature

The study of intergenerational mobility (or its inverse, persistence) originates from Galton's (1886) work on height correlations and subsequent sociological studies of class and occupational mobility. Economists' interest in income mobility gained momentum later, spurred by theoretical advances (Becker and Tomes 1979, 1986) and improved intergenerational data (e.g., Solon 1992; Zimmerman 1992). Subsequent research has produced mobility estimates across numerous countries, leveraging administrative data in many developed nations to address

substantially outperforms earlier methods.

new questions (for reviews, see Black and Devereux 2011; Jäntti and Jenkins 2015; Nybom 2024). Key findings include that mobility is much lower than previously thought (e.g., Adermon et al. 2021; Mazumder 2005) and correlates negatively with inequality (Chetty et al. 2014; Corak 2013). While low mobility in specific outcomes like income is often viewed with concern, there is no normative consensus on whether higher mobility is inherently desirable.

The equality of opportunity (EOp) approach is more recent and measures how much inequality is driven by *circumstances* such as parental income, family structure, race, etc. Early studies examined the role of tax-and-transfer systems in reducing inequality of opportunity (Page and Roemer 2001; Roemer et al. 2003), while later work focused on estimating inequality of opportunity (IOp) across countries.[6] For example, Lefranc et al. (2008) find that Nordic countries come close to achieving equality of opportunity, while the U.S. and Italy lag behind. In Sweden, Björklund et al. (2012) attribute 30% of long-run income inequality to circumstances beyond one's control. Ferreira and Gignoux (2011) report that 25-50% of consumption inequality in Latin America stems from circumstances, while Hufe et al. (2022) measure "unfair inequality" over time and across countries. However, most estimates provide only lower bounds of IOp, due to unobserved circumstance variables. For overviews of this literature, see Ferreira and Peragine (2016) and Roemer and Trannoy (2016).

The question of whether intergenerational measures are informative about EOp is contested. It has been argued that perfect equality of opportunity does not imply eliminating all resemblance between parents and children, because differences due to inherited ability and values will persist even in a perfectly fair society (Swift 2004). For this reason, Jencks and Tach (2006) argue that measures of intergenerational mobility are unreliable indicators of equality of opportunity. On the contrary, Torche (2015) argues that these sources of transmission are likely both small and similar in magnitude across space and time, so that (differences in) intergenerational mobility can be informative about EOp. We contribute to the discussion by testing this empirically.

Our paper also relates to recent empirical research. Brunori et al. (2013) analyze prior estimates for 16 countries in a meta study, finding correlations of 0.6 between IOp and IGEs. In a follow up, Brunori et al. (2023) provide similar cross-country correlations for 10 countries, finding moderate correlations (around 0.5-(-0.6) for traditional IOp estimates, while new machine-learning based estimates yield correlations of almost 0.9. In contrast, we use data from a single country, ensuring homogeneity in data quality and definitions across regions, as well as enabling us to use a richer set of circumstances.[7]

We also complement Deutscher and Mazumder (2023), who compare the ranking of Australian regions across many different measures of relative and absolute income mobility. While their focus is on providing a comprehensive framework for different mobility measures, they do include a measure of relative inequality of opportunity. However, they lack data on family characteristics other than income which limits their analysis of IOp. In contrast, we have access

[6]We will refer to the theoretical construct as *equality* of opportunity, or *EOp*, and the empirical estimates as *in*equality of opportunity, or *IOp*. IOp can be thought of as the inverse of EOp — when IOp is high, EOp is low, and vice versa.

[7]Blundell and Risa (2019) predict child incomes with family-background characteristics for 40 Norwegian regions and show that the model's explanatory power, measured by R^2, is highly correlated with intergenerational rank slopes in income.

to a very rich set of circumstance variables underlying our IOp indices and, consequently, their cross-region correlations are notably lower than ours. In addition, rather than using only a single measure of IOp, we provide estimates for both absolute and relative IOp and using different inequality indices. We further probe the sensitivity of our results to varying the set of circumstances, provide gender-specific estimates, use variation across cohorts as well as across regions, and address the impact of sampling variation on the various measures.

Naturally, we also build on prior studies measuring the role of family background for incomes in Sweden, using various measures and study populations.[8] In particular, Björklund and Jäntti (2020) discuss the various empirical approaches, concluding that all are informative about important questions, but that using only one of them in isolation could lead to mistaken conclusions. While they focus on national-level estimates, we instead study the joint variation in such estimates across regions and cohorts.

2 Measurement and Implementation

In this Section, we outline our set of measures of intergenerational mobility, sibling correlation, and inequality of opportunity. We briefly discuss their definition, measurement, and our practical implementations.

2.1 Intergenerational Mobility

Most empirical studies of intergenerational income persistence characterize the joint distribution of adult children's and their parents' lifetime incomes using linear summary measures.[9] The most established measure is the *intergenerational elasticity* (IGE), estimated as the slope coefficient in a regression of child on parent log income:

$$y_t = \beta y_{t-1} + \varepsilon_t, \tag{1}$$

where t indicates generation. The IGE, β, measures persistence, and the lower it is the higher is the expected rate at which incomes regress to the mean between generations. The widespread use of the IGE can be traced to its appealing regression-to-the-mean interpretation and its derivability from models of parental investments in children (Becker and Tomes 1979; Solon 2004).

However, there are practical challenges related to the measurement of lifetime income affecting the estimation of both the IGE and other related measures.[10] Partly for this reason, much recent work estimates rank-based mobility measures. The *rank slope* (or rank persistence) is estimated by regressing child's on parent's (within generation) percentile ranked incomes.[11] It measures the extent to

[8]See e.g. Björklund and Jäntti (2009), Björklund et al. (2012), Breen et al. (2016), and Hederos et al. (2017). For regional variation in intergenerational mobility, see Heidrich 2017.

[9]We do not address non-linear mobility measures in this paper. See further discussion in Deutscher and Mazumder (2023).

[10]The standard concerns are that attenuation (e.g., Mazumder 2005) and life-cycle biases (Haider and Solon 2006; Nybom and Stuhler 2016) arising from the approximation of lifetime income using short-run incomes make consistent estimation of the IGE more demanding.

[11]When the ranks are computed within the full population of interest, a regression of child on parent income rank gives an estimate of the (Spearman) rank correlation. In our application, it is more correct to talk about a rank slope, as in Chetty et al. (2014).

which child income tends to increase with parental income, abstracting from *any* distributional differences between generations. While the rank slope provides a robust measure of positional mobility, its scale-invariant interpretation can also be unappealing. For example, moving ten percentiles in the income distribution in a high-inequality country is much more meaningful in terms living standard than a similar shift in a low-inequality country.

The usage of rank-based measures is often motivated by practical features. For example, Chetty et al. (2014) argue that the approximate linearity of the conditional expectation of child on parent income rank makes them well suited for analyzing mobility differences across subgroups, and Nybom and Stuhler (2017) show that rank-based measures suffer less from measurement-error biases when lifetime incomes are unobserved. We estimate these two intergenerational measures separately by region and following conventional procedures, using father's income as the parental variable.

2.2 Sibling Correlations

An alternative measure of the importance of family background is the sibling correlation (see, e.g., Björklund and Jäntti 2009; Corcoran et al. 1990; Solon 1999). A common motivation for its usage is that it captures a broader scope of family influences than intergenerational measures. For example, Jäntti and Jenkins (2015) argue that if we would like to understand how important family background is for the distribution of economic status, a focus on parent-child associations captures only one specific dimension of the family. The sibling correlation instead captures the influence of all factors that siblings share in terms of some outcome, and not only parental income.

We can write log earnings for sibling j in family i as

$$y_{ij} = a_i + b_{ij}, \tag{2}$$

where a_i is a family component that is common between siblings, capturing parental characteristics, place of birth, and neighborhood, while b_{ij} is an individual component which is orthogonal to the shared component. Thus, the variance of log earnings can be decomposed as $\sigma_y^2 = \sigma_a^2 + \sigma_b^2$. The correlation between two siblings within family i is then

$$\text{Corr}(y_{ij}, y_{ij'}) = \frac{\text{Cov}(y_{ij}, y_{ij'})}{\sigma_y^2} = \frac{\sigma_a^2}{\sigma_a^2 + \sigma_b^2}, \tag{3}$$

and captures the share of the variance in log earnings due to shared factors.

Following Mazumder (2008), we estimate the multi-level model

$$y_{ij} = X'_{ij}\beta + a_i + b_{ij},$$

where X_{ij} is a set of cohort and gender dummies, using restricted maximum likelihood (REML). Estimates of $\hat{\sigma}_a^2$ and $\hat{\sigma}_b^2$ can then be plugged into Equation (3) to get an estimate of the sibling correlation. The sibling correlation equals the squared IGE plus a term capturing all shared factors orthogonal to parental earnings (Bingley and Cappellari 2018; Solon 1999). Note also that by necessity the sibling correlation is generally estimated for a slightly different population (siblings) than mobility or IOp measures (all children).

2.3 Inequality of Opportunity

Roemer (2004) points out that intergenerational associations are direct measures of inequality of opportunity only if two specific conditions apply: first, the advantages associated with parental background are entirely summarized by parental income (and its correlates); second, the concept of equality of opportunity (EOp) that is employed views as unacceptable any income differences in the child generation that are attributable to differences in innate talents.

In the concept of EOp proposed by Roemer (1993, 1998), the population is partitioned into *types* comprising individuals with the same *circumstances*. The set of circumstances includes all factors beyond the child's control, which theoretically could include both typically observable (e.g., parental education) and unobservable (e.g., genetic makeup) factors. In empirical studies, this set is by necessity restricted to observable factors such as parental income and education, place of birth, race/ethnicity, etc. Each individual chooses their level of *effort*, which together with their circumstances results in a level of *advantage*. EOp obtains when individuals are rewarded for their effort, but not for their circumstances. Recognizing potential type-effort correlations, Roemer argues that EOp obtains only when the *distribution* of advantage is independent of type. In principle, this can be tested by forming types from individuals with similar circumstances and comparing the cumulative distribution functions of, e.g., earnings between types.

Let earnings Y be a function of circumstances C, efforts E, and unobserved random factors u:

$$Y = f(C, E, u). \tag{4}$$

Effort is partly influenced by circumstances, so we can rewrite this expression as $Y = f[C, E(C, w), u]$, where w captures efforts that are independent from circumstances. Since we are only interested in the total impact of circumstances on earnings, we can work with the reduced form $Y = g(C, \varepsilon)$.

Define the counterfactual earnings distribution $Y^C = \mathrm{E}(Y \mid C)$, which captures expected earnings conditional on circumstances C. A measure of *absolute* inequality of opportunity is then given by:

$$IOp_{abs} = I(Y^C), \tag{5}$$

where $I()$ is an inequality index.[12] Alternatively, *relative* IOp measures the share of overall inequality that is unfair:

$$IOp_{rel} = \frac{I(Y^C)}{I(Y)}. \tag{6}$$

The empirical challenge is to estimate the counterfactual distribution Y^C. The *parametric* approach (Bourguignon et al. 2007; Ferreira and Gignoux 2011) typically uses predicted values from a log-linear regression of earnings on circumstance variables to estimate Y^C, while the *nonparametric* approach (Checchi and Peragine 2010) partitions the sample into types based on observed circumstances and estimates Y^C as average incomes within types. Both approaches face challenges: if the models are too restrictive they risk underestimating IOp,

[12] We use the Gini coefficient as our inequality index in the main analysis, but also present robustness checks using the mean logarithmic deviation.

while if made too flexible they run the risk of overfitting.[13] We follow Brunori et al. (2023) and address this problem using machine learning methods.

Conditional inference trees (CIT) use recursive binary splitting to form predictions for an outcome variable. The sample is first split in two by selecting a variable and a cut-off value for that variable. Each subsample is then split similarly until a stopping rule is reached. For each variable and split, the algorithm tests whether the distribution of the outcome is independent of the variable. If the test fails to reject the null for each variable, the algorithm terminates and the tree is finished; if not, the variable with the lowest p-value is used for splitting. To find the cut-off value, a new test is performed for each potential value and the one with the lowest p-value is chosen.[14]

Conditional inference forests (CIF) apply the random forest approach of Breiman (2001) to CITs. To construct a CIF, we draw 200 bootstrap samples with a random subset of circumstances from the original sample, and estimate a CIT in each bootstrap sample.[15] We then form $\hat{Y}^C$ by averaging predictions across the bootstrap samples for each individual. An advantage of CIF is that we can use *surrogate splits* (Rieger et al. 2010), which enable us to retain individuals who have missing values on some circumstances.[16] We use the *party* R package (Hothorn et al. 2023) to estimate CIFs and calculate absolute and relative IOp for each local labor market. Because we are unable to observe all circumstances, our estimates should be viewed as *lower bounds* on the true IOp.

3 Data and descriptive statistics

We combine several administrative registers maintained by Statistics Sweden. Our source data cover the universe of the Swedish population aged 0–74 from 1965–2020 and their biological parents. All individuals are linked to population registers containing information on incomes, education, family relationships, and demographic events such as civil status, residency, and death. These include the national censuses (FoB) 1960–1990, the education register 1985–2020, and the income registers for the years 1968–2020.

3.1 Sample restrictions

We first select all 1,727,599 children born in Sweden between 1965 and 1980. We then restrict the sample to children whose mother and father were also born in Sweden, and were between 18 and 40 years old when the child was born, leaving 1,534,031 children in the sample. We further restrict the sample to children with at least three annual incomes above a minimum level in adulthood (as described

[13] The former could be due to a simple linear functional form in the parametric case, or a small and coarsened set of circumstances in the non-parametric case; the latter due to including interactions and polynomial terms in the parametric case, or dividing the sample into too fine-grained types in the non-parametric case.

[14] We use a size of 0.05 for the hypothesis tests, and adjust for multiple testing using the Bonferroni correction.

[15] Each bootstrap sample uses 60 percent of the sample, and is drawn without replacement. For each bootstrap draw, we use $\lceil \sqrt{k} \rceil$ circumstances, where k is the total number of circumstance variables in the sample.

[16] For observations with missing data on a selected circumstance, the algorithm instead uses a surrogate variable which is selected to best predict the split in the originally chosen variable. In our application, we allow for up to three surrogate splits.

in Section 3.2), reducing the sample by 190,844 observations, and to those who lived at least six consecutive years in the same local labor market during ages 2–12, further reducing the sample by 158,283 observations. These restrictions result in a core sample of 1,184,904 children.[17]

To maximize sample size and to retain comparability across the various measures, our main samples pool sons and daughters. For this reason, we adjust our income measures for mean differences by gender (see below). While gender has sometimes been regarded as a circumstance variable from an EOp perspective (Hederos et al. 2017), its role will play out very differently for intergenerational measures and the sibling correlation. We thus proceed with pooled samples and gender-adjusted income measures, but also present gender-specific estimates as robustness tests.

For the intergenerational and IOp analyses, we construct our *main analysis sample* by restricting the core sample to the 1,077,046 children whose fathers have non-missing incomes. For the sibling correlations, we construct a *sibling sample* of 767,005 children by dropping all singletons from the core sample.

3.2 Variable definitions

We create pre-tax income panels spanning 1968–2020. All incomes are deflated to 2020 SEK. We use two income measures (see Appendix A for details). First, *labor income* includes labor earnings and taxable work-related compensation, business income, and some labor-related benefits such as short-term sick pay and parental benefits. Capital income, pensions, and long-term sickness and parental leave benefits are not included. We observe labor incomes at least every third year between 1968 and 1985, and yearly thereafter.[18] Second, *disposable income* is calculated as the individual's (consumption-weighted) share of household disposable income, which includes after-tax labor earnings, business income, capital income, and transfers including unemployment, parental and sickness benefits, means-tested income support, pensions, study grants, and housing grants. Disposable incomes are available starting from 1990.

Our main analyses use labor income for the parental generation and disposable income for the child generation. The latter reduces the risk of underestimating consumption opportunities for those less attached to the labor market (e.g., some women). As we show below, the results are robust to using labor incomes for the child generation.[19]

To obtain a time-consistent permanent income measure we drop all annual incomes below a threshold corresponding to two "price base amounts", which in 2020 amounted to 44 percent of the lowest full-time entry wage in the collective agreements (Swedish National Mediation Office 2021).[20] We then approximate permanent incomes by averaging annual incomes between ages 30–40 for the child

[17] This latter restriction implies that we omit those from very mobile families who lack a stable region of residence in childhood.

[18] Income data (from the income and taxation register and censuses) up until 1985 is available for the years 1968, 1970, 1971, 1973, 1975, 1976, 1979, 1980 and 1982.

[19] From the perspective of theories of parental investment in child human capital (e.g., Becker and Tomes 1979), one could argue that using disposable income among parents would be more appropriate. However, data restrictions prevent us from doing so, as data on disposable income is only available from 1990, while labor income is available from 1968.

[20] The price base amount is used across the Swedish social insurance system to price adjust transfers, pensions, and fines. In 2020, the amount was SEK 47 300 (around EUR 4,500).

and 35–55 for the parental generation. We exclude individuals with fewer than three annual income observations within the relevant age range, and demean child incomes by gender. We use these measures untransformed for the IOp estimates, take logs for the IGE and sibling correlations, and calculate national-level percentile ranks within cohort and gender for the rank regressions.

We use local labor markets as geographical units, following Chetty et al. (2014). We observe residency at birth and then annually from 1969. We recode the residency data to map into the 1985 municipality division before aggregating the municipalities into a total of 126 local labor markets. Each individual is assigned the region where they resided for the most years up until age 12.

In addition to parental income, we define a set of circumstance variables for the IOp analyses. Parental education is reported in levels which we convert into the corresponding years of schooling. We define one-digit parental occupation (ten categories) from the census closest in time to when the child was ten years old. We also include family size and both parents' age when the child was born, as well as measures of family stability during childhood. For the latter, we use indicators for whether the child lived in the same parish as both biological parents at age 14; whether either of the parents completed a divorce (not necessarily from each other) before the child turned 21; and whether either parent died before age 55 (also acting as a coarse measure of a poor health endowment). Finally, in a robustness test we include data on adolescent cognitive and non-cognitive skills from military enlistment tests (for men only).

3.3 Summary statistics and national-level estimates

We show summary statistics for the main sample in Table 1 and for the sibling sample in Table B.1. Panel E shows summary statistics of our various measures across regions, as well as their national-level counterparts (col. 5).[21] Reassuringly, our national-level estimates of the different measures are largely in line with prior evidence, despite some differences in either income concepts and/or cohorts studied.[22] The table further highlights two important patterns. First, the means across regions (Panel E, col. 1) are consistently lower than their national-level counterparts (col. 5). A possible reason for this is that regional-level income differences are suppressed in the former case but not the latter, which needs to be recognized throughout our discussion.[23]

Second, the national-level estimates highlight that the share of total inequality that is attributed to family-background factors is substantially higher for the sibling correlation and the IOp indices than what is implied by intergenerational estimates. Note that to get this inequality share for the IGE or rank correlation, we need to square those estimates (see also, e.g., Björklund and Jäntti 2020). However, while recognizing these differences in what the *levels* of the measures

[21] Table B.2 shows further IOp and inequality estimates at the national level (Panel A) and averaged across local labor markets (Panel B). In addition to what's shown in Table 1, we vary the included circumstances and show estimates using the mean logarithmic deviation (MLD) as inequality index. Estimates using the MLD are uniformly lower than our Gini-based estimates.

[22] See e.g. Björklund and Jäntti (2009, 2020), Björklund et al. (2012), Breen et al. (2016), and Nybom and Stuhler (2017).

[23] Also worth recognizing is that our regional-level analyses allow for region-specific coefficients when performing the predictions underlying the IOp indices. Further, the means in col. 1 are region-weighted while those in col. 5 are person-weighted (larger regions have more influence).

imply, the focus of our analysis is how differences in the various measures across regions (or cohorts) *correlate*.

Table 1: Summary statistics and national-level estimates

	Mean	Std. dev.	Min	Max	All
Panel A. Child					
Birth year	1972.5	4.5	1965.0	1980.0	
Income	199	146	94	77,074	
Share women	48%				
Panel B. Mother					
Birth year	1945.9	6.0	1925.0	1962.0	
Income	218	72	94	3,444	
Years of schooling	11.1	2.7	7.0	20.0	
Age at birth	26.5	4.5	18.0	40.0	
Panel C. Father					
Birth year	1943.5	6.3	1925.0	1962.0	
Income	322	150	97	20,642	
Years of schooling	10.8	3.0	7.0	20.0	
Age at birth	28.9	4.7	18.0	40.0	
Panel D. Family					
Family size	1.8	0.8	1.0	10.0	
Parents divorced	22%				
Parent died	11%				
Same parish	86%				
Panel E. Local labor markets					
IGE	0.18	0.04	0.04	0.34	0.23
Rank persistence	0.19	0.04	0.06	0.26	0.25
Sibling correlation	0.21	0.04	0.06	0.32	0.27
Absolute IOp	0.04	0.01	0.01	0.09	0.08
Relative IOp	0.24	0.06	0.05	0.38	0.39
Inequality (Gini)	0.18	0.01	0.15	0.24	0.20
N, main sample	8,548	19,419	110	182,857	1,077,046
N, sibling sample	6,087	13,712	80	129,044	767,005

Note: Panels A–D show summary statistics for the individual-level data, while Panel E shows summary statistics for the 126 local labor markets. The *All* column shows estimates and sample sizes for the full sample.

4 Results

Figure 1 plots the different persistence measures against absolute and relative IOp for each local labor market. There is a clear positive association between IOp and persistence in all panels. The figure also gives some indications of a nonlinear pattern, with stronger correlations for regions with higher intergenerational persistence — particularly for the absolute IOp measure in the left column.

This pattern is also reflected in the regression lines: the solid line shows the fit from a weighted regression, where larger regions (shown as larger circles) are given more weight than smaller ones, while the dashed line shows the unweighted regression. Larger regions tend to cluster in the upper right parts of the graphs (with higher intergenerational persistence as well as higher IOp), where the association is steeper. This results in steeper slopes (stronger correlations) for the weighted compared to the unweighted regressions. The sibling correlations are noisier, showing more dispersion at the upper end than the other measures.

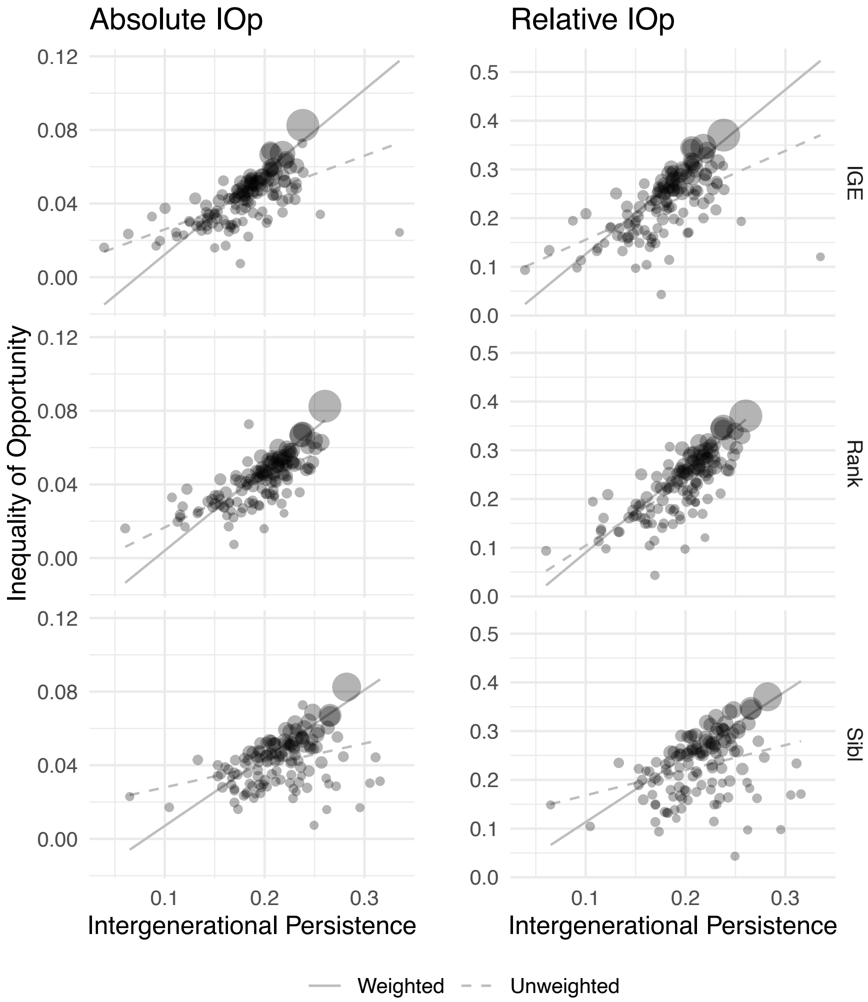

Figure 1: Relationship between inequality of opportunity and mobility measures

Notes: Each circle represents a local labor market. Circle size corresponds to the IGE sample size in each local labor market. Solid lines show OLS regressions weighted by the sample size, while dashed lines show unweighted regressions lines.

Panel A of Table 2 confirms these results. Each table entry shows the Pearson correlation coefficient between the estimated IOp and one of the intergenerational persistence measures (or sibling correlation), as listed in the column headings. The first three columns show absolute IOp, while the last three show relative

IOp (i.e., the share of total inequality accounted for by observed circumstances). We present unweighted correlations in the first row, and correlations weighted by sample size in the second row.

Table 2: Main results

	Absolute IOp			Relative IOp		
	IGE	Rank	Sibl	IGE	Rank	Sibl
Panel A. All						
Unweighted	0.59	0.71	0.33	0.55	0.72	0.29
	(0.07)	(0.06)	(0.08)	(0.07)	(0.06)	(0.09)
Weighted	0.84	0.87	0.80	0.83	0.89	0.76
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.06)
Panel B. Men						
Unweighted	0.49	0.66	0.13	0.46	0.66	0.09
	(0.08)	(0.07)	(0.09)	(0.08)	(0.07)	(0.09)
Weighted	0.75	0.82	0.70	0.75	0.83	0.63
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)
Panel C. Women						
Unweighted	0.64	0.59	0.05	0.62	0.60	0.02
	(0.07)	(0.07)	(0.09)	(0.07)	(0.07)	(0.09)
Weighted	0.84	0.84	0.47	0.80	0.82	0.44
	(0.05)	(0.05)	(0.08)	(0.05)	(0.05)	(0.08)

Note: Each cell shows the correlation across 126 local labor markets between the measures of IOp and mobility indicated in the header, with standard errors in parentheses. "Weighted" rows show correlations weighted by the no. of observations of the local labor market. The IOp, IGE, and rank estimators use a larger sample than the sibling correlation: no. of observations is 1,077,046 and 767,005 respectively in Panel A.; 559,831 and 232,340 in Panel B.; and 517,215 and 201,137 in Panel C.

Overall, the two sets of measures are strongly correlated. The results are very similar for absolute and relative IOp. Further, the results are quite similar across the two intergenerational measures (IGE and rank-rank regression), while the sibling correlations are less correlated with IOp in the unweighted specifications.

The largest variation comes from whether we weight by region size or not. Weighed correlations, ranging from 0.8 to 0.9, are markedly higher than unweighted, ranging from 0.3 (for the sibling correlation) to 0.7. This is likely explained by the non-linear patterns observed in Figure 1, where correlations appear stronger for larger regions. However, we show in Section 4.1 that this pattern arises due to a larger influence of sampling variation in smaller regions, rather than true heterogeneity in the underlying processes.[24] For this reason, we focus most of our remaining analyses on the weighted estimates.

Panels B and C of Table 2 show results separately for men and women (i.e., sons and daughters), respectively. The IOp-IGE correlations are slightly larger

[24] A simple count of top vs bottom regions in terms of the different measures shows no marked differences (see Table B.6). Among both the top and bottom 25 regions in terms of mobility a majority of them are also among the top and bottom in EOp, and this overrepresentation is similarly large at both the top and the bottom.

for women, while the associations between IOp and the sibling correlation are larger among men.

4.1 Region size and sampling variation

We already noted that the correlations between IOp and intergenerational measures differ substantially depending on whether we use weights or not when estimating our correlations. This observation would be consistent with the correlations being stronger among larger compared to smaller local labor markets. In this section, we examine whether this pattern is a true reflection of heterogeneity in processes of mobility and opportunity across large and small regions, or whether it is an artifact of noisier estimates in smaller samples.

In Table B.4, we split the regions at the median size, and estimate separate correlations for larger and smaller regions. We find that *both* weighted and unweighted correlations are substantially lower for smaller regions. This large-vs-small difference is the largest for the sibling correlation, which is close to uncorrelated with IOp in small regions. Figure B.1 shows the relationship between local labor market size and our measures of intergenerational persistence and IOp. There is a pattern with larger regions having lower intergenerational mobility and less equality of opportunity. Furthermore, the intergenerational and sibling measures are much more dispersed for smaller regions. This elevated dispersion could reflect sampling variation which introduces measurement-error induced attenuation bias in the correlations, and is consistent with that the unweighted correlations are considerably lower in our main analysis.

To probe this hypothesis further, we perform a set of analyses where we enforce small samples sizes for all regions. We sample 100 observations (with replacement) from each region, and estimate the correlations for this sample. The procedure is repeated 500 times. Table B.5 shows means and bootstrap standard errors for the correlations. We show unweighted and weighted (using sample sizes from the full sample) correlations, as well as correlations separately by larger and smaller regions (again, split using the full sample). Strikingly, these correlations are all much lower than our main estimates, on the order of 0.4–0.6. While the bootstrap-based correlations for larger regions remain somewhat larger, the difference is small. Moreover, using our original weights based on labor-market size has virtually no impact on the bootstrap-based correlations.

We view these results as strong support for the hypothesis that the lower correlations for smaller regions are driven primarily by sampling variation, rather than reflecting a true feature of the structure of social mobility and opportunity across regions. The analysis also strengthens the case for focusing on weighted rather than unweighted correlations, as in the latter case the correlations will be more strongly attenuated by sampling variation among smaller regions.

4.2 Robustness to alternative specifications

Table 3 presents results from a set of alternative specifications. All panels show correlations weighted by sample size.

In our main specification, we use individualized *disposable income* as the outcome for the child generation. Panel A shows correlations when we instead use individual *labor income*.[25] The correlations fall somewhat across all measures,

[25]See Appendix A for precise definitions of these income measures.

Table 3: Alternative specifications

	Absolute IOp			Relative IOp		
	IGE	Rank	Sibl	IGE	Rank	Sibl
A. Individual incomes	0.69	0.79	0.60	0.71	0.81	0.59
	(0.07)	(0.05)	(0.07)	(0.06)	(0.05)	(0.07)
B. Mean Log Deviation	0.81	0.83	0.81	0.83	0.88	0.80
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
C. Spearman rank corr.	0.83	0.86	0.71	0.81	0.86	0.70
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
D. Excl. three largest cities	0.73	0.80	0.53	0.72	0.80	0.49
	(0.06)	(0.05)	(0.08)	(0.06)	(0.05)	(0.08)
E. Balanced sample	0.82	0.87	0.82	0.79	0.85	0.77
	(0.05)	(0.04)	(0.05)	(0.06)	(0.05)	(0.06)
F. No income restriction	0.79	0.82	0.53	0.72	0.83	0.48
	(0.06)	(0.05)	(0.08)	(0.06)	(0.05)	(0.08)
G. Income > one basic amt.	0.85	0.86	0.77	0.83	0.87	0.72
	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.06)
H. Cohorts, levels	0.64	0.79	0.70	0.58	0.76	0.69
	(0.21)	(0.16)	(0.19)	(0.22)	(0.17)	(0.19)
I. Cohorts, first differences	0.58	0.45	0.67	0.52	0.48	0.61
	(0.23)	(0.25)	(0.21)	(0.24)	(0.24)	(0.22)
J. County × cohort	0.73	0.72	0.69	0.72	0.73	0.65
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)

Note: Each cell shows the weighted correlation across local labor markets between the measures of IOp and mobility indicated in the header, with standard errors in parentheses. A. uses individual incomes as the outcome measure; B. uses the Mean Log Deviation instead of the Gini as the index of inequality; C. uses the Spearman rank correlation instead of the Pearson to estimate the correlations between IOp and mobility measures; D. excludes the three largest metropolitan areas: Stockholm, Gothenburg, and Malmö; E. uses a balanced sample with 706,589 obs. for each measure; F. removes the restriction on small incomes, while G. sets it at one basic amount; H. uses variation across 16 birth cohorts; I. uses first-differenced variation across birth cohorts; J. uses 384 county × cohort groups instead of local labor markets.

but remain high, around 0.6–0.8.

The main analyses use the Gini coefficient as inequality index underlying the IOp estimation. The results are basically unchanged if we instead use the mean logarithmic deviation (Panel B). Further, we use the Pearson correlation to measure the degree of association between the different measures of IOp and intergenerational persistence, but this estimator works best for linear associations. Given the non-linearities seen in Figure 1, a less restrictive estimator might better capture the relationship. To test this, we instead use the Spearman rank correlation in Panel C, with the results being largely unchanged.

A possible concern is that the high correlations (especially in the weighted case, see Figure 1) are driven by the three major metropolitan areas in Sweden (Stockholm, Gothenburg, and Malmö). The correlations do fall slightly when we exclude them from the analysis (particularly for the sibling correlation), although the results are similar overall (Panel D).

To estimate sibling correlations, we require families with at least two children, and thus have to exclude singletons. This results in an unbalanced sample, where the IOp and intergenerational measures are estimated on a different (and larger) sample than the sibling correlation. Panel E reports estimates using a balanced sample which imposes all sample restrictions from both the main and sibling samples. Again, the results are basically unchanged.

The main sample excludes individuals with annual incomes below two price base amounts (see Appendix A for details). As Panels F–G show, the results are generally robust to lowering or dropping this low-income cut-off. The exception is the association between IOp measures and the sibling correlation, which falls to around 0.5 when no restriction is used.

4.2.1 Different circumstance variables

To further gauge the robustness of our results, we study how different sets of circumstances impact the correlations. Table B.3 shows correlations between our mobility measures and IOp, estimated using different sets of circumstances. Col. (4) is our baseline specification, and thus reproduces Panel A of Table 2. The results are robust to varying the set of circumstances (cols. (1)–(4)), including adding gender as a circumstance (col. (5)).

Since parental income is included as a circumstance in the IOp calculations, one might worry that the correlations with measures of income mobility are mechanically driven by this factor alone. To test this, col. (6) removes parental income from the circumstances, yielding, perhaps surprisingly, very similar results to the baseline specification.

Cols. (7)–(8) show estimates using men with observed cognitive and non-cognitive skills measured at military enlistment tests. Col. (7) shows the baseline specification for this subsample, while col. (8) adds skills to the set of circumstances in the IOp estimation.[26] Adding these skill measures changes the correlations only marginally.

[26] It is debatable whether one should see these measures as circumstances, effort, or a combination of both. See for example Björklund et al. (2012), who provide arguments for their inclusion as circumstances.

4.2.2 Using variation over time

As an alternative to spatial variation, we explore variation over time in the associations between estimates of IOp and intergenerational persistence. Figure 2 shows time series plots of our measures, estimated at the national level. The measures appear to co-move over cohorts, with a non-trivial increase (less mobility, EOp, etc.) over the first five birth cohorts and a small subsequent reversion (more mobility, EOp, etc.) starting from the 1970 birth cohort.

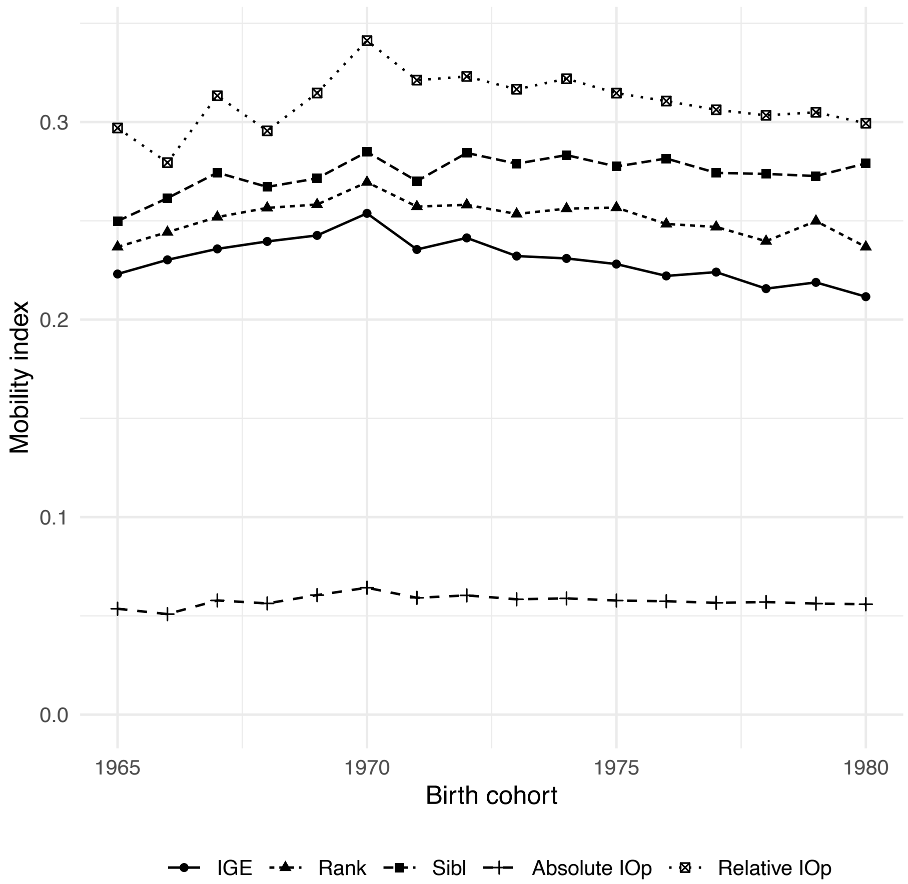

Figure 2: Relationship between inequality of opportunity and mobility measures

Notes: Each point shows the given measure estimated using the national cross-section for a given birth cohort.

In Panel H of Table 3, we show correlations between the measures using variation between cohorts. Since there is much less variation in cohort sizes than in region sizes, we only show unweighted estimates. Across cohorts, both absolute and relative IOp measures are highly correlated with the intergenerational measures, with correlations ranging from 0.6–0.8, although these estimates are naturally much less precise than those based on variation across regions.

These correlations might be driven by linear trends. A more stringent test is to estimate correlations in changes (by taking first differences of all variables).

We show such results in Panel I of Table 3. This reduces the correlations somewhat (and increases the standard errors further), but all correlations remain substantially positive, around 0.5–0.6.

Finally, in Panel J we replace the 126 local labor markets with 384 groups formed by interacting Sweden's 24 counties at the time with the 16 birth cohorts in our data, and perform our analyses across these groups. This results in a larger number of groups while reducing variation in sample sizes across groups.[27] Correlations drop slightly, but remain substantial at around 0.7.

5 Conclusions

The study of social mobility is often motivated with reference to the normative concept of *equality of opportunity* (EOp). However, it is not clear a priori how well EOp is actually captured by estimates of social mobility. The purpose of this paper is to provide a bridge between the different concepts. To this end, we estimate a set of intergenerational measures (IGEs, rank correlations) and sibling correlations, along with indices of inequality of opportunity, for each of 126 Swedish local labor markets as well as over cohorts. We then calculate how strongly these different measures correlate across regions and over time.

Our findings suggest that the *variation* in income-based measures of IOp and intergenerational persistence is intimately related. We first show that the intergenerational measures (elasticity and rank correlation) correlate very strongly with inequality of opportunity (IOp) indices across Swedish regions, while the sibling correlation is only slightly less strongly correlated with IOp measures.

Moreover, the strong associations between IOp and intergenerational persistence is not driven by a mechanical role of parental income in the IOp indices. As we show, the various measures remain strongly correlated also when parental income is excluded from the set of circumstances underlying the IOp, as well as in a number of different robustness analyses. Finally, we study correlations across birth cohorts, both nationally and within regions. While the correlations are somewhat smaller and less precisely estimated (for the national cohort variation), they are still substantial.

We want to emphasize, however, that the various measures we study provide quite different answers to the key question of what share of total inequality that can be attributed to family-background factors. This share is substantially higher for the sibling correlation and the IOp indices than what is implied by intergenerational estimates. But while the *levels* of the measures thus can have vastly different interpretations, our analysis emphasizes that *differences* in the various measures across regions (or cohorts) correlate strongly. Because the literatures in question are primarily comparative, studying variation across countries or over time, this is an important insight.

Taken together, our findings suggest that the measures of intergenerational mobility that are often used in the empirical literature can indeed be informative about variation in equality of opportunity, as the two concepts are strongly correlated across both space and time. However, more evidence on this topic

[27]Sample sizes for the county $\times$ cohort groups vary between 477 and 12,055, with a mean of 2,805. For the local labor markets, sample sizes vary between 110 and 182,857, with a mean of 8,548.

would be very valuable. The landscape of Swedish local labor markets constitute a rather specific context, and the patterns might differ across countries or over longer time periods.

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A Variable definition details

Individualized disposable incomes are calculated by multiplying household disposable income by an adjustment factor, which takes two slightly different forms. For the period 1990–2004, this factor is calculated by dividing the individual's consumption weight by the sum of each family member's consumption weights; and for the (partly overlapping) period 1998–2021, it is calculated by dividing one by the sum of family consumption weights. We use the *dispinkpersf* variable for the earlier period, and the *dispinkke* variable for the later, both from the LISA register. For the overlapping period 1998–2004, when both definitions are available, we take the average.

The consumption weights are defined as follows: 1.16 for one adult, 1.92 for two adults, and 0.96 for each additional adult; 0.56 for children aged 0–3, 0.66 for children aged 4–10, and 0.76 for children aged 11–17. To illustrate, in a family of two adults and two children aged 3 and 5, the sum of family consumption weights is $1.92 + 0.56 + 0.66 = 3.14$. The earlier definition then gives an adjustment factor $0.96/3.14 = 0.31$ (where the numerator comes from dividing 1.92 by two), while the newer definition gives $1/3.14 = 0.32$.

Households are defined through individuals with a family relationship (married, registered partners, cohabiting with common children, parents, and guardians) who are registered as residents of the same property. Cohabiting unmarried couples with no children cannot be linked, and so appear as single households in our data.

For individual labor income, we use the following registers and variables: from the income and taxation (IoT) register, we use the sum of *injo*, *inro*, *intj*, and *sjoin* for 1968; the sum of *ainjo*, *ainro*, *aintj*, and *sjoin* for 1971, 73, and 76; and the *arbink* variable for 1979 and 82. We also use *arbink* from the 1970, 75, and 80 censuses (FoB). For 1985–1989, we take the sum of *loneink*, *fink*, and *arbers* from the LOUISE register; and for 1990–2021, we use *forvers* from the LISA register.

Information on highest level of completed education comes from the LISA register for the years 1990–2021. We translate this into years of schooling as follows: old primary school = 7 years; new primary school = 9 years; short high school = 11 years; long high school = 12 years; short tertiary education = 14 years; long tertiary education = 16 years; and Ph.D. = 20 years.

We also use data on highest completed level of education from the 1960 and 70 censuses. The 1970 census has a clearly defined coding scheme that we translate directly to years of schooling. For the 1960 census there is a variable for level of education, but due to lacking documentation it cannot be directly translated into years of schooling. To circumvent this, we exploit the panel structure of our data to impute years of schooling as follows: for each level in the 1960 variable, we calculate the modal value from the 1970 years of schooling variable using all individuals who were observed in the given category in 1960.

We use local labor markets for 1985 as the geographic units of observation in the main analyses. These are defined by grouping municipalities according to observed commuting patterns. The local labor markets were created by Statistics Sweden in a conscious effort to form local labor market regions suitable for economic analysis (Statistics Sweden 2010).

Family size is measured as the mother's total number of biological children. We observe this in the 2022 multigenerational register, when all mothers in our sample are at least 60 years old.

B Additional results

Table B.1: Summary statistics, sibling sample

	Mean	Std. dev.	Min	Max
Panel A. Child				
Birth year	1972.6	4.2	1965.0	1980.0
Income	199	123	94	37,230
Share women	48%			
Panel B. Mother				
Birth year	1946.3	4.9	1925.0	1962.0
Income	217	70	94	2,040
Years of schooling	11.1	2.7	7.0	20.0
Age at birth	26.2	4.3	18.0	40.0
Panel C. Father				
Birth year	1943.9	5.2	1925.0	1962.0
Income	322	151	97	20,642
Years of schooling	10.8	3.0	7.0	20.0
Age at birth	28.6	4.5	18.0	40.0
Panel D. Family				
Family size	2.3	0.5	2.0	10.0
Parents divorced	22%			
Parent died	12%			
Same parish	88%			

Note: Table shows summary statistics for the individual-level data in the siblings sample.

Table B.2: IOp estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. National IOp								
Gini								
Absolute IOp	0.069	0.074	0.075	0.077	0.080	0.071	0.076	0.083
Relative IOp	0.353	0.376	0.383	0.393	0.409	0.362	0.385	0.421
Inequality	0.197	0.197	0.197	0.197	0.197	0.197	0.196	0.196
Mean log deviation								
Absolute IOp	0.008	0.009	0.009	0.009	0.010	0.008	0.009	0.011
Relative IOp	0.129	0.136	0.144	0.147	0.157	0.120	0.143	0.166
Inequality	0.065	0.065	0.065	0.065	0.065	0.065	0.065	0.065
Panel B. Mean IOp								
Gini								
Absolute IOp	0.048	0.050	0.050	0.042	0.043	0.036	0.036	0.042
Relative IOp	0.264	0.275	0.273	0.227	0.236	0.196	0.195	0.231
Inequality	0.182	0.182	0.182	0.182	0.182	0.182	0.181	0.181
Mean log deviation								
Absolute IOp	0.004	0.004	0.004	0.003	0.003	0.002	0.002	0.003
Relative IOp	0.078	0.079	0.077	0.055	0.058	0.042	0.044	0.057
Inequality	0.054	0.054	0.054	0.054	0.054	0.054	0.054	0.054
Circumstances								
Parental income	✓	✓	✓	✓	✓		✓	✓
Parental education		✓	✓	✓	✓	✓	✓	✓
Parental occupation			✓	✓	✓	✓	✓	✓
Family characteristics				✓	✓	✓	✓	✓
Gender					✓			
Skills								✓
No. of observations	1,077,046	1,077,046	1,077,046	1,077,046	1,077,046	1,077,046	501,591	501,591

Note: The table shows IOp and inequality estimates, using the *Gini* or *mean logarithmic deviation* as the inequality index. Panel A. shows estimates at the national level, while Panel B. shows averages across local labor markets. *Parental income*, *education*, and *occupation* includes the variable for both parents separately. *Family characteristics* includes family size, both parents' year of birth and age when the child was born, and indicators for early parental death, divorce during childhood, and living in the same parish as both parents during childhood.

Table B.3: Circumstances

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Absolute IOp								
IGE	0.86	0.84	0.84	0.84	0.83	0.80	0.73	0.72
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
Rank persistence	0.86	0.86	0.86	0.88	0.87	0.87	0.81	0.80
	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)
Sibling correlation	0.77	0.79	0.79	0.81	0.81	0.79	0.68	0.69
	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)
Panel B. Relative IOp								
IGE	0.89	0.84	0.84	0.82	0.81	0.76	0.73	0.70
	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
Rank persistence	0.90	0.89	0.89	0.88	0.87	0.85	0.81	0.80
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Sibling correlation	0.73	0.74	0.76	0.77	0.76	0.73	0.62	0.62
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
Circumstances								
Parental income	✓	✓	✓	✓	✓		✓	✓
Parental education		✓	✓	✓	✓	✓	✓	✓
Parental occupation			✓	✓	✓	✓	✓	✓
Family characteristics				✓	✓	✓	✓	✓
Gender					✓			
Skills								✓
No. of observations	1,077,046	1,077,046	1,077,046	1,077,046	1,077,046	1,077,046	501,591	501,591

Note: *Parental income*, *education*, and *occupation* includes the variable for both parents separately. *Family characteristics* includes family size, both parents' year of birth and age when the child was born, and indicators for early parental death, divorce during childhood, and living in the same parish as both parents during childhood.

Table B.4: By sample size

	Absolute IOp			Relative IOp		
	IGE	Rank	Sibl	IGE	Rank	Sibl
Panel A. N $\leq$ 3,396						
Unweighted	0.41	0.44	0.10	0.34	0.46	0.05
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)
Weighted	0.56	0.53	0.22	0.51	0.55	0.19
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
Panel B. N > 3,396						
Unweighted	0.71	0.77	0.59	0.70	0.79	0.50
	(0.09)	(0.08)	(0.12)	(0.09)	(0.08)	(0.12)
Weighted	0.86	0.88	0.85	0.85	0.89	0.83
	(0.07)	(0.06)	(0.08)	(0.07)	(0.06)	(0.08)

Note: Each cell shows the correlation across local labor markets (LLM) between the measures of IOp and mobility indicated in the header, with standard errors in parentheses. "Weighted" rows show correlations weighted by the no. of observations of the LLM. The sample has been split at the median LLM size in two parts, with 63 LLMs each. The smaller LLMs are shown in Panel A., and the larger in Panel B. The IOp, IGE, and rank estimators use a different sample than the sibling correlation: no. of observations is 99,826 and 109,031 respectively in Panel A.; and 977,220 and 657,974 in Panel B.

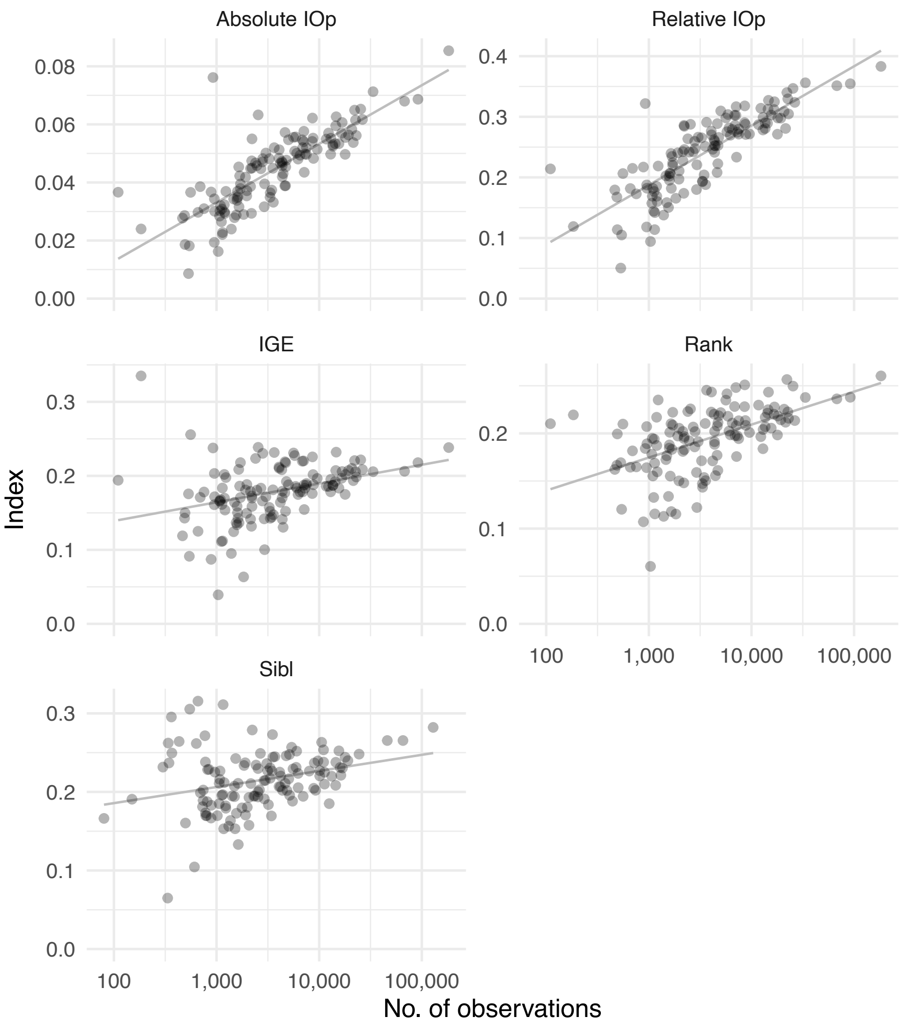

Figure B.1: Relationship between IOp and mobility indices and region size

Notes: Each circle represents a local labor market. Lines show OLS regressions of the indicated persistence or IOp measures on sample size.

Table B.5: Bootstrap simulation, 100 obs./LLM

	Absolute IOp		Relative IOp	
	IGE	Rank	IGE	Rank
A. Unweighted	0.54	0.42	0.52	0.45
	(0.08)	(0.09)	(0.07)	(0.07)
B. Weighted	0.56	0.45	0.53	0.45
	(0.14)	(0.17)	(0.14)	(0.16)
C. N $\leq$ 3,396	0.52	0.40	0.49	0.42
	(0.11)	(0.11)	(0.10)	(0.10)
D. N $>$ 3,396	0.56	0.45	0.53	0.47
	(0.10)	(0.10)	(0.10)	(0.09)

Note: The table shows versions of the main results where we enforce a uniform sample size of 100 observations from each local labor market by sampling with replacement. The procedure is repeated 500 times, and each cell shows mean correlations and standard errors from the bootstrap distributions. Panel A. shows unweighted correlations, while Panel B. shows correlations weighted using sample sizes from the full data. Panels C. and D. show correlations separately for smaller and larger local labor markets (in the original data), as in Table B.4.

Table B.6: Nonlinearities

	Bottom 25		Top 25	
	Absolute IOp	Relative IOp	Absolute IOp	Relative IOp
IGE	12	12	12	12
Rank	14	15	13	14
Sibl	10	9	9	10

Note: Each cell shows the number of regions that are among the bottom (top) 25 regions in terms of both IOp and mobility for the indicated pairs of measures.