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Intergenerational mobility in a recession:

Evidence from Sweden

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Intergenerational mobility in a recession: Evidence from Sweden

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Abstract

We use complete-count register data to describe various features of intergenerational mobility in Sweden. First, we document the extent of regional variation in educational and income mobility across Swedish municipalities, and describe its spatial pattern. Second, we study the stability of such regional rankings to the choice of mobility statistic. Third, we show that income inequality and mobility are negatively related, across all mobility measures. Fourth, we exploit variation in local exposure to show that the 1990s economic crisis and the 2007-2008 financial crisis had a negative effect on income mobility.

JEL Codes: J62, R00
Keywords: The geography of intergenerational mobility, multigenerational mobility, income inequality, recession

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1 Introduction

An increasingly popular approach to study intergenerational mobility is to compare mobility rates across *regions*. In early examples, Holmlund (2008) and Pekkarinen, Uusitalo and Kerr (2009), exploited the availability of large administrative registers to assess the effect of a *specific* policy or economic event on intergenerational mobility. A more recent strand of the literature inverts this research design; rather than studying whether a particular event affected mobility, it studies which local factors correlate with the observed variation in intergenerational mobility across regions. Following the influential study by Chetty et al. (2014), this approach has been applied in an increasing number of countries.[1]

In this study, we apply both flavors of this "*regional approach*" in Swedish register data to characterize some central aspects of the intergenerational transmission process. We make four related contributions: First, we describe the extent to which educational and income mobility vary across Swedish municipalities. Second, we study whether regional rankings are stable to the choice of mobility measure. This is important, because researchers' choices of which statistic to report are often restricted by data availability. Third, we study whether income inequality and mobility are related. And finally, we apply the regional approach to how economic downturns such as the 1990s economic crisis and the 2007-2008 financial crisis affected income mobility in Sweden.

We first report regional estimates of educational and income mobility for cohorts born in the 1980s (income) and early 1990s (education) across 290 *municipalities*. Swedish municipalities are key geographical entities in our context, as many public goods and schooling are provided at this level. In most cases, municipalities correspond to the relevant labor market of their inhabitants. Our evidence is complementary to evidence provided by Heidrich (2017) and Brandén (2019), who estimate income mobility for earlier birth cohorts, for a broader definition of *local labor markets* or *commuting zones*. While Heidrich only reports region-specific estimates that are significantly different from the mean, we report all correlations irrespectively of precision, as we aim to use those estimates in subsequent steps of the analysis. In addition to income, we also estimate intergenerational (parent-child) and multigenerational (grandparent-child) correlations in educational attainment.

[1] Examples include Connolly, Corak and Haeck (2019), Connolly, Haeck and Lapierre (2019) and Corak (2020) for Canada, Deutscher and Mazumder (2019) for Australia, Heidrich (2017) and Brandén (2019) for Sweden, Risa (2019) and Bütikofer, Dalla-Zuanna and Salvanes (2018) for Norway, Eriksen and Munk (2020) for Denmark, Acciari, Polo and Violante (2016) for Italy, or Bell, Blundell and Machin (2018) for the UK.

We then study whether regional comparisons are sensitive to the choice of mobility statistic on which they are based. Specifically, we study whether regional differences vary with the choice of outcome (education vs. income), summary statistic (e.g., regression vs. correlation coefficients), the time frame (inter- vs. multigenerational), and the choice of lineage (paternal vs. maternal line). In particular, we test whether summary statistics based on education – which can be estimated in most settings – can serve as a substitute for more data-demanding measures based on income. Our analysis is complementary to a detailed recent analysis by Mazumder and Deutscher (2021), who compare the ranking of Australian regions across different measures of income mobility.

We next use those regional estimates to answer two substantive questions, which have seen much interest recently. The first question is whether *intergenerational* mobility and *cross-sectional* inequality are systematically related. A robust pattern in international comparisons is that income mobility and inequality are negatively associated across countries (Blanden, 2011, Corak, 2013). We show that the same pattern holds across regions within Sweden. In line with recent evidence by Brandén (2019), who explore this relationship for Swedish men in earlier cohorts, we find that municipalities that are characterized by high income inequality in the parent's generation are also characterized by low (upward) mobility. We show that this finding is very robust to the choice of mobility measure. Moreover, we find the same pattern when considering educational mobility. Interestingly, the relationship to income inequality is much stronger when considering multigenerational (grandparent-child) rather than traditional intergenerational (parent-child) correlations.

Finally, we apply the regional approach to address a question on which only sparse evidence exists, namely if *economic shocks* affect intergenerational mobility. We estimate the mobility impact of two distinct recessions, the Swedish economic crisis in the early 1990s and the 2007-2008 financial crisis. For causal identification we exploit variation in the severity of the economic downturn across regions (i.e., variation in local crisis exposure) in a difference-in-differences design with continuous treatment. Focusing on the short-term impact, we show that both recessions – but in particular the financial crisis – had a more adverse impact on the expected income rank of children from disadvantaged background, in particular in younger age groups. As a consequence, absolute upward and relative income mobility decline in those municipalities that are more heavily exposed to the crises. Our findings therefore suggest that recessions – in addition to their distributionary impacts in the cross-section – tend to also increase inequality in the intergenerational dimension.

2 Data

Our main data sources are the *Longitudinal Integration Database for Health Insurance and Labor Market Studies* (LISA) and the Multigenerational Register, both provided by Statistics Sweden. The data contain employment information (employment status and employer identity), tax records (including annual labor earnings) and demographic information (such as age, education, and family composition) for all Swedish residents in the age 16 to 65 for the years 1985 to 2000 and age 16 to 74 in the years 2001 to 2017. Using the Multigenerational Register, we match individuals to their biological parents and grandparents.

2.1 Samples

We create four samples (see Table 1). For our descriptive analysis of educational mobility we consider birth cohorts 1986-92, as these cohorts had sufficient time to complete their education and we are able to merge educational information for their grandparents in the majority of cases (multigenerational analysis).[2] For our descriptive analysis of income mobility we consider slightly earlier cohorts born in 1981-87, and measure child earnings as a three-year average between age 29 to 31 and parental earnings as a five-year average when the child was aged 12 to 16. In both samples, we allocate parent-child pairs based on the child's municipality of residence when the child was aged 16. To assess the impact of the 1990s and financial crises, we construct two further samples, each split into a pre-and post-crisis period. The "pre"-periods are defined as the years 1986-89 for the 1990s crisis and 2003-06 for the financial crisis. The "post-"periods are defined as the periods 1992-95 and 2008-11, respectively. These samples are described further in Section 6.

2.2 Outcomes

Our primary outcome measures are years of schooling and labor earnings. For the descriptive analysis, we allocate parent-child pairs to the municipality in which the child lived

[2] We are able to identify parents and grandparents if they were at most 65 years in 1985. As our earliest child cohort is born in 1986, this restriction is not binding for the matching of parents but reduces the match rate of grandparents to approximately 55%. We note below that our results are similar when restricting to a balanced grandfather-father-child sample.

Table 1: Samples and Descriptive Statistics

Sample	**Descriptive**	**Descriptive**	**1990s Crisis**	**Financial Crisis**
Outcome	Education (years schooling)	Earnings (log earnings)	Earnings	Earnings
Analysis period	Birth cohorts 1986-92	Birth cohorts 1981-87	Years 1986-89 and 1992-95 Age 25-35	Years 2003-06 and 2008-11 Age 25-45
Observations	844,792	697,356	2,800,318	5,211,219
Means				
Child	13.53	7.63	7.20	7.52
Father	12.57	7.64	7.51	7.61
Mother	13.10	7.29	7.11	7.17
Grandfathers	9.53	-	-	-
Grandmothers	9.47	-	-	-

Notes: Sample size and means for each sample. The number of observations correspond to the number of distinct individuals in the child generation, except for the 1990s and financial crisis samples, in which individuals might be contained twice (in the pre- and post-period).

at age 16. To construct years of schooling, we distinguish five different degrees.[3] Labor earnings include the sum of all annual earnings according to employer-reported tax and earnings statements to the national tax agency, which includes all types of labor compensation subject to payroll taxes. The measure is not censored or top-coded, and includes bonus payments. We deflate earnings to 2018 prices and, to include zeros and reduce the influence of very low earnings, bottom-code earnings to 50,000 SEK (corresponding approximately to the 15th percentile).

For our descriptive analysis of income mobility (child cohorts 1981-87), we measure child earnings as a three-year average between age 29 to 31 and paternal earnings as a five-year average when the child was aged 12 to 16.[4] For estimating the impact of the 1990s and financial crises on intergenerational mobility (Section 6), we consider mean earnings over the respective pre- and post-period for different sets of child cohorts, while paternal earnings are measured as a five-year average between the years 1985 and 1989 (1990s crisis and financial crisis, earlier birth cohorts) or when the child was aged 10 to 14 (financial crisis, later cohorts).

[3] Specifically, we distinguish 1 = Primary and lower secondary education with less than 9 years (coded as 6 years of schooling); 2 = Primary and lower secondary education 9 years (9 years); 3 = Upper secondary education (12 years); 4 = Post-secondary education (16 years); and 5 = Postgraduate education (20 years).

[4] We focus on father's earnings rather than household earnings to avoid the need to account for differences in household structure.

2.3 Mobility statistics

We consider a range of standard mobility statistics, such as the intergenerational regression coefficient,

$$y_{ir} = \alpha_r + \beta_r y_i^p + \varepsilon_{ir}, \tag{1}$$

where y_{ir} is one of the outcomes described above for individual i in municipality (region) r and y_i^p is the corresponding outcome for a parent or grandparent. The slope β_r measures "relative" mobility, comparing socioeconomic status relative to the status of others in the same generation. We also consider the corresponding Pearson correlation coefficient, which abstracts from shifts in the variance between the two generations.

For our analysis of income mobility, we consider either log income (in which case the slope coefficient β_r can be interpreted as the *intergenerational elasticity*) or income ranks (in which case β_r can be referred to as the *rank slope*). Ranks are defined on the national level, separately within each child cohort. Based on the estimated intercept $\hat{\alpha}_r$ and the slope $\hat{\beta}_r$ we further construct the expected "absolute upward mobility" of children born to parents at the 25th percentile (i.e., $y_i^f = 0.25$), as defined in Chetty et al. (2014).

2.4 Municipal shocks

To measure the severity of "local recessions" we import municipal shock measures as constructed by Engdahl and Nybom (2021). The local shock measures are based on employment data from the *Register-based Labor-Market Statistics* (RAMS), and defined as the municipal percentage-point drop in the employment rate between 1990-1993 (for the 1990s crisis) or 2008-2010 (for the financial crisis), which approximately corresponds to the change between the start and the trough of each crisis (on the national level).[5]

[5] The employment rate is defined as the share within the municipality that was employed in November each year. A complication is that the employment measure in RAMS changes in definition a few times during our time period, and one such important change takes place in 1994. As such, we compute the shock for the years 1990-1993 for the 1990s crisis, despite the fact that the crisis deepened even further in 1994. An alternative would have been to compute the shock measure using local unemployment changes (as in Yagan, 2019). Local unemployment data is however only available starting from 1992.

3 Geographical Variation in Intergenerational Mobility

We first document that intergenerational mobility varies across municipalities within Sweden.[6] To illustrate the extent of variation in educational mobility, Figure 1 plots the density of the intergenerational correlation in years of schooling across 290 municipalities, separately for father-child, mother-child, grandfather-child and grandmother-child pairs (pooling sons and daughters). The *within-municipality* parent-child correlations are clustered around a mean of 0.27, but vary from below 0.15 to above 0.35. We document below a similarly high variation when restricting our analysis to larger municipalities that are less affected by sampling error. The three-generation correlations are clustered around a mean of 0.10, again with large variation across municipalities.

Mother-child correlations tend to be slightly larger than the corresponding father-child correlations, which reflects the fact that daughters' educational attainment resembles more the attainment of their mother than their father (while the relations are more symmetric for sons). The inverse pattern applies for *grandparent* correlations, which are systematically larger for grandfather-child than for grandmother-child pairs.[7]

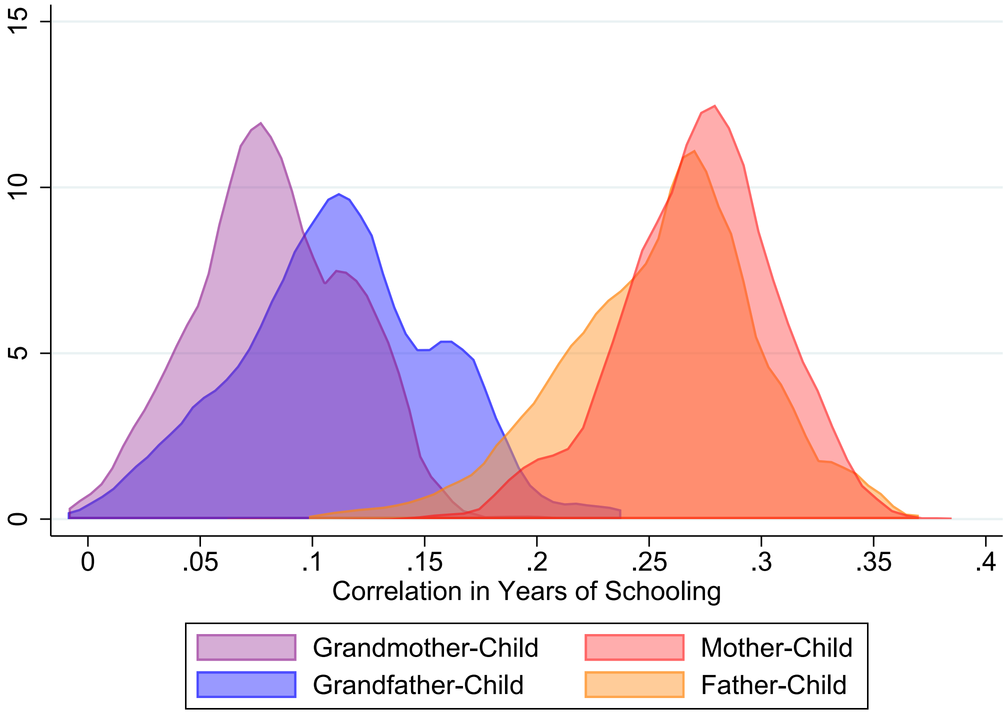
Parent-child correlations as plotted in Figure 1 may understate the extent to which socio-economic advantages are transmitted from one generation to the next, because educational attainment is just one aspect of a person's overall socio-economic status. Indeed, studies that combine multiple status proxies (Vosters and Nybom, 2017; Blundell and Risa, 2018), that integrate information from grandparents or the extended family (Braun and Stuhler, 2018; Colagrossi, d'Hombres and Schnepf, 2020; Adermon, Lindahl and Palme, 2019) or that consider long-run mobility across many generations (Clark, 2014; Barone and Mocetti, 2020) tend to find higher persistence.

To illustrate this understatement, we exploit the availability of three-generation data. Re-

[6] Swedish municipalities are key geographical entities in our context, as many public goods and schooling are provided at this level. Municipalities collect (labor) income taxes from their inhabitants and use the revenues to fund and organize day care centers and pre-school, compulsory school, and high schools. Before a decentralization reform in 1991, the national government had more power over the schooling system. The municipalities, however, were the responsible authority for their schools also before 1991. Virtually all children attend school in their home municipality. Municipalities have typically also been responsible for, among other things, adult education, social services and welfare support, and some aspects of labor market and activation policies.

[7] These opposing gender patterns illustrate that the intergenerational transmission process may vary across generations, which constitutes a challenge for attempts to estimate transmission models from multi-generational data (Nybom and Stuhler, 2019).

Figure 1: Intergenerational correlations in education in Sweden



Notes: Density of the estimated intergenerational correlations in years of schooling for parent-child and grandparent-child pairs across 290 Swedish municipalities (bandwidth: 0.01), based on birth cohorts 1986-92. Observations are weighted by the number of parent-child pairs.

cent studies on the national level find that multigenerational correlations tend to be larger than a simple iteration of parent-child correlations would suggest, i.e. intergenerational transmission cannot be approximated by a first-order Markov process. We find the same pattern on the *regional* level. In 79% of municipalities, the estimated grandfather-child correlation β_{-2} is larger than the square of the father-child correlation β_{-1},

$$\Delta = \beta_{-2} - \beta_{-1}^2 > 0.$$

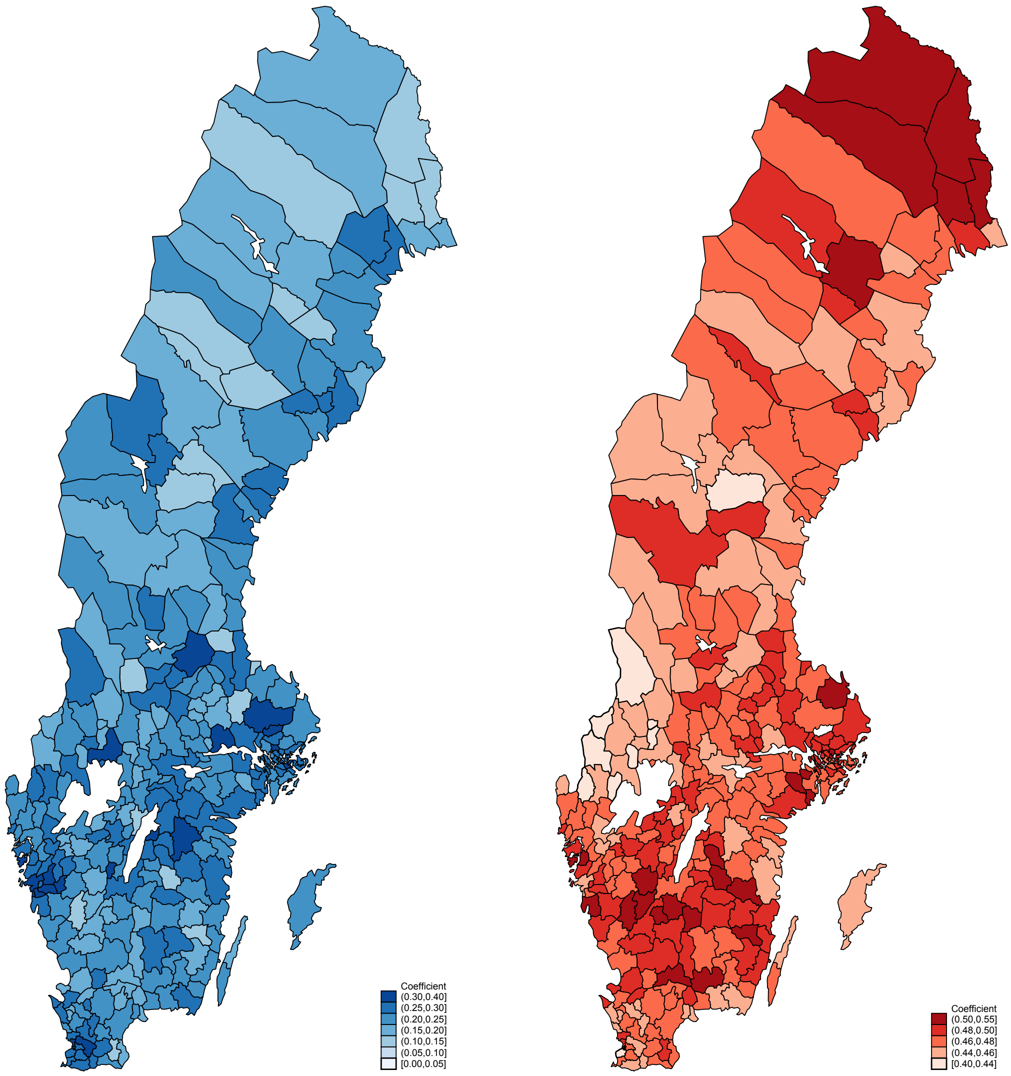
This share increases to 90% when weighting the correlations by the number of underlying observations, suggesting that some of the cases with estimated $\Delta < 0$ are due to sampling uncertainty. The gap Δ increases with the size of the municipality and the degree of socioeconomic inequality in the parent generation (see Section 5).

Parent-child correlations capture therefore only part of the intergenerational process, and tend to understate the extent of status transmission in the "long run" (across many generations). But while understating the *level* of transmission, parent-child correlations may

Figure 2: Intergenerational mobility across Swedish municipalities

(a) Years of schooling

(b) Upward mobility in earnings (p25)



Notes: The figures plot estimates of the father-child correlation in years of schooling (sub-figure a) and the absolute upward mobility in earnings, defined as the expected earnings rank of children at the 25th percentile of the parental distribution (sub-figure b), across 290 Swedish municipalities. The estimates are based on birth cohorts 1986-92 for schooling and 1981-87 for earnings.

still capture *differences* in long-run transmission across regions. We test and confirm this hypothesis in Section 4. Conventional parent-child correlations therefore remain useful for comparative purposes, irrespectively of whether our interest centers on two-generation or long-run transmission processes.

To further illustrate how status transmission varies across municipalities, Figure 2 maps estimates of the father-child correlation in educational attainment (sub-figure a) and the absolute upward mobility in earnings (sub-figure b), defined as the expected income rank of sons and daughters born to parents at the 25th percentile (see Section 2.3). The point here is not to focus on any particular municipality, as the municipality-specific estimates can be quite noisy. Instead, we aim to show that mobility varies *systematically* between municipalities. For educational attainment, the intergenerational correlation tends to be higher in the more densely populated southern parts of Sweden, and clusters of municipalities with high correlations (i.e. low mobility) are visible around Sweden's major cities.

The spatial pattern of upward intergenerational mobility in earnings (sub-figure b) is different; upward mobility in earnings tends to be greater in municipalities close to the larger cities and in central southern municipalities, but also in some rural areas in northern Sweden. We compare our estimates to Heidrich (2017), who provides detailed national and regional estimates of intergenerational income mobility for the Swedish population born between 1968 and 1976. We consider more recent cohorts born 1981-87, and our empirical specification differs in various aspects. Most importantly, we estimate mobility rates on the finer *municipality* level while Heidrich focuses on the broader definition of *local labor market regions*. Consistent with this and other differences in specification, we find slightly lower persistence.[8] We find an average rank-rank slope of 0.14 for father-child pairs (sons and daughters) or 0.17 for father-son pairs, compared to an average of 0.18 in Heidrich (2017).

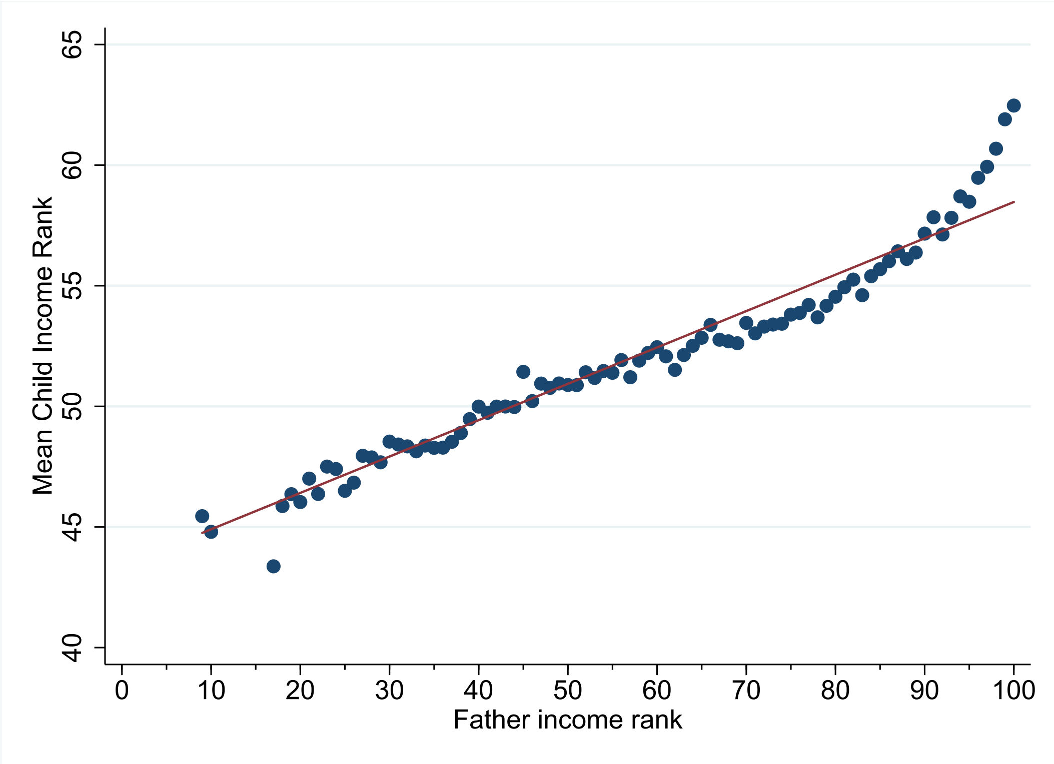
The spatial pattern appears similar, to the extent that they are comparable given the difference in area definitions. In particular, Heidrich (2017) notes that several local labor markets in the south of Sweden are doing particularly well in lifting children from lower income families, while absolute upward mobility is particularly low in municipalities in the Swedish inland located close to the Norwegian border. These patterns are also visible in Figure 2.[9] Our estimates of income mobility appear therefore broadly in line with her earlier estimates. In addition, we find comparatively high upward mobility in municipalities around Stockholm, and the northernmost municipalities in Sweden.[10]

[8] We consider individual-level rather than household income for parents, and rank-rank slopes tend to be steeper when based on household rather than individual income (Chetty et al., 2014 and Heidrich, 2017). Moreover, we measure incomes at a younger age for children (29-31 compared to age 32-34) and over a shorter age span for parents (at child age 12 to 16 compared to parental age 34 to 50) than Heidrich (2017), which will tend to attenuate our estimates (Nybom and Stuhler, 2017).

[9] A more specific comparison is difficult, due to the difference in regional classifications. However, Heidrich (2017) finds high upward mobility for the local labor market regions *Värnamo*, *Hylte*, *Ljungby*, *Hofors* and *Gnosjö,* and the corresponding municipalities have high upward mobility also according to our estimates.

[10] The spatial pattern we find is also fairly consistent with the results in Brandén (2019). He considers the same geographical units and approximately the same cohorts (1961-1980) as Heidrich (2017), but focuses on individual earnings of fathers and sons rather than household income. He finds that the father-son correlation in earnings rank is relatively high indicating low mobility in and around the three largest cities (Stockholm, Gothenburg, and Malmö), but also in large parts of southern Sweden.

Figure 3: The rank regression slope on the national level



Notes: Binned scatter plot of the expectation of child income conditional on father's income rank. Ranks are defined relative to the national distribution. Incomes below 500 SEK are assigned the mean rank of all bottom-coded observations (within cohort).

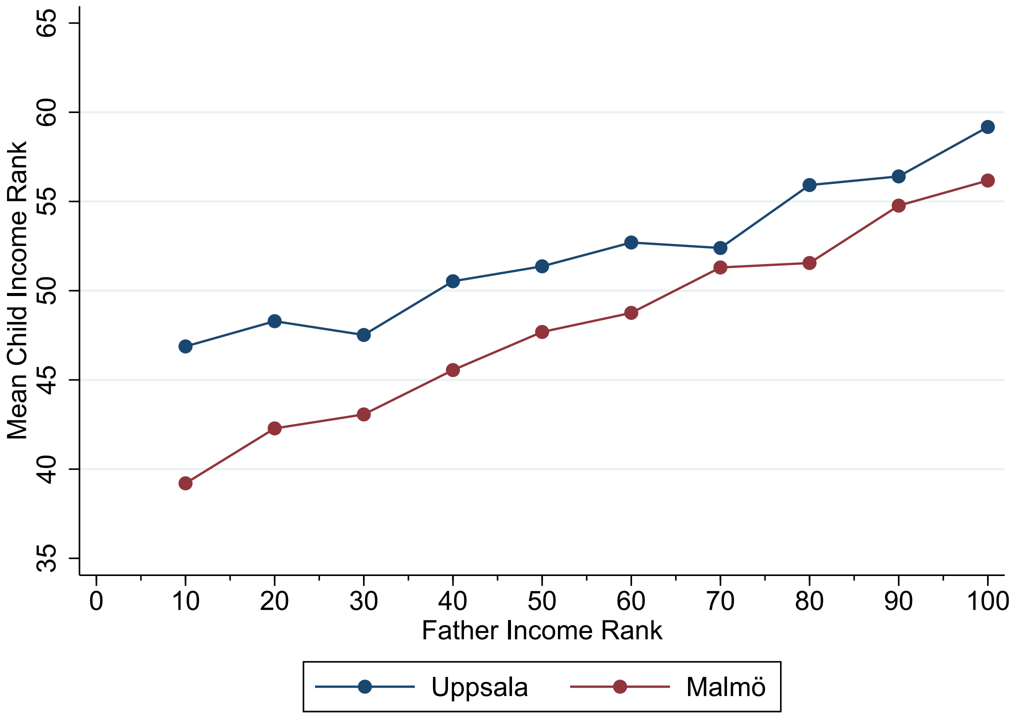
3.1 Methodological concerns

Regional estimates such as those reported in Figures 1 and 2 are subject to two methodological concerns. The first is whether intergenerational relations can in fact be sufficiently well summarized by simple summary measures, such as linear regression or correlation coefficients. For illustration, Figure 3 plots the mean child income rank against the parental income rank at the national level. The rank-rank relation is linear over most of the range, but much steeper at the very top of the distribution.[11] The conditional expectation function (CEF) is therefore not fully captured by a linear regression, in contrast to the pattern observed for the U.S. (Chetty et al., 2014). However, a linear fit provides a good approximation of the conditional expectation over most of the range. The expectation of child years of schooling conditional on father's years of schooling is approximately linear as well.

While the CEF appears sufficiently linear on the national level, it does not necessarily

[11] Nybom and Stuhler (2017) document a similarly steep relation at the very bottom of the parental income distribution. This pattern is not visible in Figure 3, as we bottom-coded earnings at a higher threshold (see Section 2).

Figure 4: Comparing the rank regression slope across cities



Notes: Binned scatter plot of the expectation of child income conditional on father's income rank (by decile).

follow that the conditional expectations are equally linear on the regional level. For illustration, Figure 4 plots the mean child income rank conditional on parent rank deciles for two Swedish cities, Malmö (red line) and Uppsala (blue line). Upward mobility is substantially lower and the rank-rank slope steeper in Malmö compared to Uppsala. However, the relationship is approximately linear in both cities.[12] We conclude that linear measures are well suited for cross-regional comparisons.

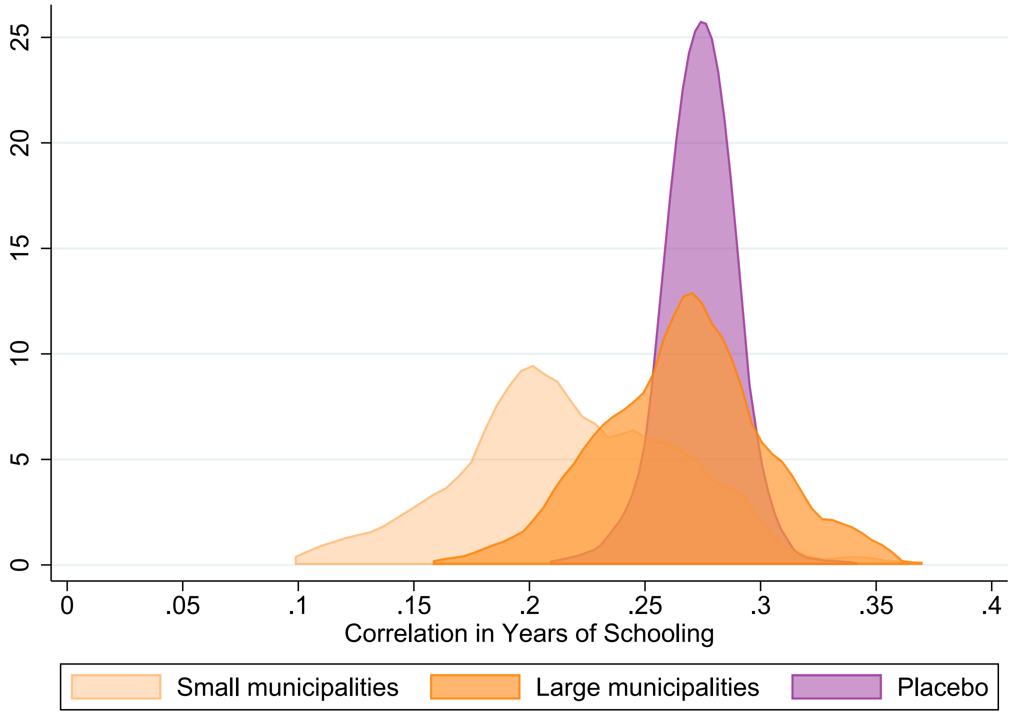
A second concern in fine-grained regional comparisons is sample size: Some of the variation across municipalities might not be "real" variation but reflect sampling error, in particular for smaller municipalities for which comparatively few parent-child pairs are observed.[13] While the mean number of father-child pairs per municipality is 2,809 for education (birth cohorts 1986-92) and 2,213 for income (birth cohorts 1981-87), in many municipalities we observe fewer than 1,000 pairs.[14] To probe the influence of sampling error, we implement

[12] Indeed, the increased steepness of the rank-rank correlation at the top of the paternal distribution appears less pronounced at the municipal level.

[13] As we observe the entire population, our estimates do not suffer from traditional sampling error. However, the observed mobility for a given set of parent-child pairs can be interpreted as a random draw from the "potential" distribution for that municipality.

[14] Moreover, inference based on income *ranks* is complicated complicated by the fact that each unit's rank depends on which other units have been sampled (Mogstad et al., 2019).

Figure 5: Intergenerational correlations by municipality size



Notes: Density plot of the estimated intergenerational correlations in years of schooling for father-child pairs across 173 "small" municipalities ($\leq$ 2,000 observations), 117 "large" municipalities ($>$ 2,000 observations), and 117 large "placebo" municipalities (random allocation of observations across municipalities).

two tests.

First, we split the sample into small municipalities ($\leq$ 2,000 observations) and larger municipalities ($>$ 2,000 observations), for which the influence of sampling error should be limited. Figure 5 plots the resulting densities of the father-child correlation in years of schooling. The correlations vary nearly as much for large as for small municipalities.[15] Second, we compute "placebo correlations" by reshuffling parent-child pairs randomly across all 290 municipalities, such that variation in the placebo correlations solely reflects chance. Figure 5 shows that for large municipalities, the resulting density is much narrower than the actual variation of intergenerational correlations across municipalities. This observation suggests that among larger municipalities, sampling error explains only a small share of the estimated variability in mobility rates.

[15] Figure 5 shows that the educational correlations are substantially lower for small municipalities, a pattern that is presumably related to the greater homogeneity in educational attainment in smaller municipalities. Educational outcomes vary more in larger municipalities, with a standard deviation of years of schooling of 1.27 in large compared to 1.03 in small municipalities. And while the estimated coefficient in an OLS regression of the municipal parent-child correlation on sample size is large and significant, it looses significance if we control for the standard deviation of years of schooling.

Still, some of the reported variation between municipalities will be noise, in particular for small municipalities, and sampling error may have a greater effect for income than education-based measures (Mazumder, 2005). Only few studies have addressed this issue systematically. Rather than estimating separate regressions for each region, Heidrich (2017) estimates a multilevel model that explicitly accounts for the influence of sampling error on the variation of regional mobility estimates. This approach effectively "shrinks" the region-specific estimates to their mean, in particular for small regions with imprecise estimates. Risa (2019) implements a similar shrinkage procedure for mobility estimates for Norway. In addition, Heidrich (2017) reports only those estimates that are significantly differently from the mean, and replaces all other region-specific estimates with that mean.

In this paper, we instead focus on the raw region-specific estimates, as our aim is to compare different mobility measures (Section 4) and to relate our findings to the existing literature reporting raw correlations. Moreover, we will relate those region-specific mobility estimates to other regional characteristics, such as inequality (Section 5) or economic shocks (Section 6). Even imprecise estimates are useful for that purpose, given that classical measurement error in a left-hand side variable does not affect consistency of the regression estimates. To reduce the influence of sampling error, we weight all regressions by the number of parent-child pairs by region, and restrict parts of the analysis to larger municipalities with more precisely estimated statistics.

4 Are Regional Rankings Stable?

Many different types of mobility statistics are in use, but researcher's choice of which statistic to report is restricted by data availability. Intergenerational linked income data is not available for many countries, and most data sets contain data on only two generations. It is therefore important to understand whether regional comparisons are sensitive to the choice of mobility statistic on which they are based. Specifically, we study whether regional differences vary with the choice of outcome (education vs. income), summary statistic (e.g., regression vs. correlation coefficients), the time frame (inter- vs. multigenerational), and the choice of lineage (paternal vs. maternal line).

Our main focus is on whether a simple summary statistic based on education – which can be estimated in most settings – can serve as a substitute for more data-demanding measures based on income. This analysis is complementary to detailed recent analysis by Mazumder and Deutscher (2021), who compare the ranking of Australian regions across different measures of income mobility, including measures of relative and absolute mobility, and more complex measures of "inequality of opportunity".

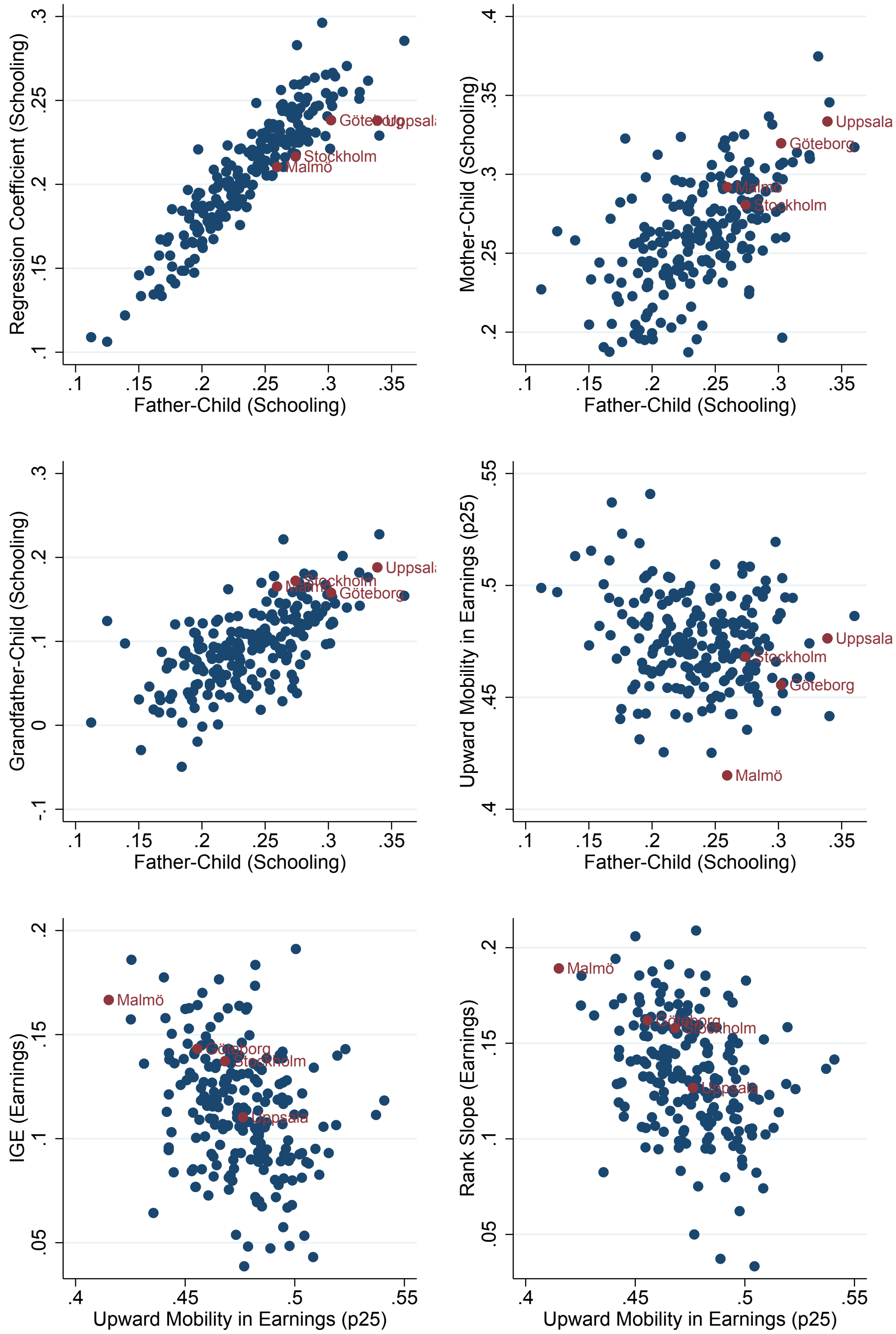
Figure 6 illustrates our findings. The top-left panel illustrates that regions rank similarly irrespectively of whether we consider the *correlation* in years of schooling (*x-axis*) or the corresponding *regression* coefficient (*y-axis*). The correlation between the two measures weighted by sample size is 0.88. This similarity of regression and correlation coefficients reflects the fact that Swedish municipalities follow broadly similar generational trends in inequality. Extrapolating from this result, we hypothesize that the distinction between regression and correlation coefficients – which is quite important for cross-country comparisons (Hertz et al., 2008) – will tend to be less critical for regional comparisons within countries, as trends in inequality will tend to differ less between regions within a given country than between countries.

The top-right panel illustrates that the ranking is a little more sensitive to whether we consider maternal rather than paternal education. The weighted correlation is 0.71. However, the correlation increases to 0.88 when restricting the sample to municipalities with at least 5,000 father-child pairs, suggesting that the difference in regional rankings based on father-child and mother-child correlations is primarily a finite sample phenomenon rather than a consequence of more fundamental differences in gender gaps between municipalities. This finding is intuitive, given the similarity of educational attainment in the Swedish context. However, it may not extrapolate to other settings or other outcomes in which gender differences are more pronounced.

The center-left panel shows that municipalities still rank similarly when considering *grandparent*-child correlations, even though these three-generation correlations are much smaller in levels. The weighted correlation between father- and grandfather-child correlations is 0.68.[16] This observation suggests that conventional parent-child correlations remain a useful summary measure for comparative purposes, even though they understate the extent of intergenerational transmission in the long run (Section 3). However, the correlation between the inter- and multigenerational measures does not increase further when restrict-

[16] The grandfather-child correlations are based on smaller samples, as grandfathers cannot be matched for all father-child pairs. However, the results are very similar when using a balanced grandfather-father-child sample.

Figure 6: Different measures of intergenerational mobility across regions



Notes: Scatter plots of different intergenerational dependence measures across Swedish municipalities. We compare the father-child correlation in years of schooling with the regression coefficient in schooling (top-left panel), the mother-child correlation in schooling (top right), the grandfather-child correlation in schooling (center left) and the upward mobility in earnings at the 25th percentile (center right). Moreover, we compare p25 upward mobility in earnings with the intergenerational elasticity of earnings (bottom left) and the rank-rank slope in earnings (bottom right). The plots are restricted to municipalities with at least 1,000 parent-child pairs with observed schooling.

ing the sample to larger cities. This points to systematic differences, and suggests that parent-child correlations do not fully capture regional variations in long-run transmission.

The center-right panel shows that *educational* and *income* mobility are not strongly related. Municipalities that have low educational mobility (here measured as the father-child correlation in years of schooling) tend to have low upward mobility in earnings (here measured as the expected rank for children born to fathers at the 25th percentile of the national distribution, see Section 2.3). But with a weighted correlation of -0.20, this relation is fairly weak. One likely reason is that regional measures of income mobility can be quite noisy (Heidrich, 2017). But the correlation remains weak when restricting our sample to larger cities, suggesting that it is not just a reflection of sampling error in fine-grained municipal estimates. This observation stands in contrast to cross-national comparisons, in which educational and income mobility are more strongly related (Blanden, 2011).

Finally, the two bottom panels illustrate that different measures of income mobility capture systematically different aspects of the transmission process. Municipalities characterized by high p25 upward mobility in earnings do tend to have a smaller intergenerational elasticity of earnings (slope coefficient in a regression of log child earnings on log earnings of the father, see Section 2.3) and a smaller rank-rank slope (slope coefficient in a regression of child income rank on paternal income rank).[17] However, with weighted correlations of -0.42 and -0.43, respectively, the relation is not very strong.[18] Mazumder and Deutscher (2021) report a similar finding in Australian Tax Data.

We conclude that mobility statistics based on education are quite stable to specification choices, but they are not a close substitute for more data-demanding summary statistics based on income. Moreover, different measures of income mobility capture different aspects of the intergenerational transmission process. Municipalities that do well in terms of absolute upward mobility tend to also be characterized by high relative mobility, but this relation is not very strong.

Consistent with this general conclusion, we observe that the largest Swedish cities exhibit opposing pattern in educational and income mobility. While Malmö is characterized by low income mobility, it ranks close to the Swedish average in terms of educational mobility. Uppsala instead is characterized by low educational mobility and moderate income mo-

[17] These relationships are similar when splitting the sample by child gender or when considering maternal rather than paternal earnings.

[18] Again, the correlations do not increase much when restricting our sample to larger municipalities with more precise mobility estimates.

bility. Malmö is a particular outlier in terms of absolute upward mobility. The expected income rank of children at the 25th percentile of the paternal distribution is 0.41, which places the city at the bottom across all 290 municipalities.[19] Still, this rate of upward mobility compares favorably to many regions in the US, in which the expected income rank at the 25th percentile can be as low as 0.36 (Chetty et al., 2014).

5 Regional Correlates

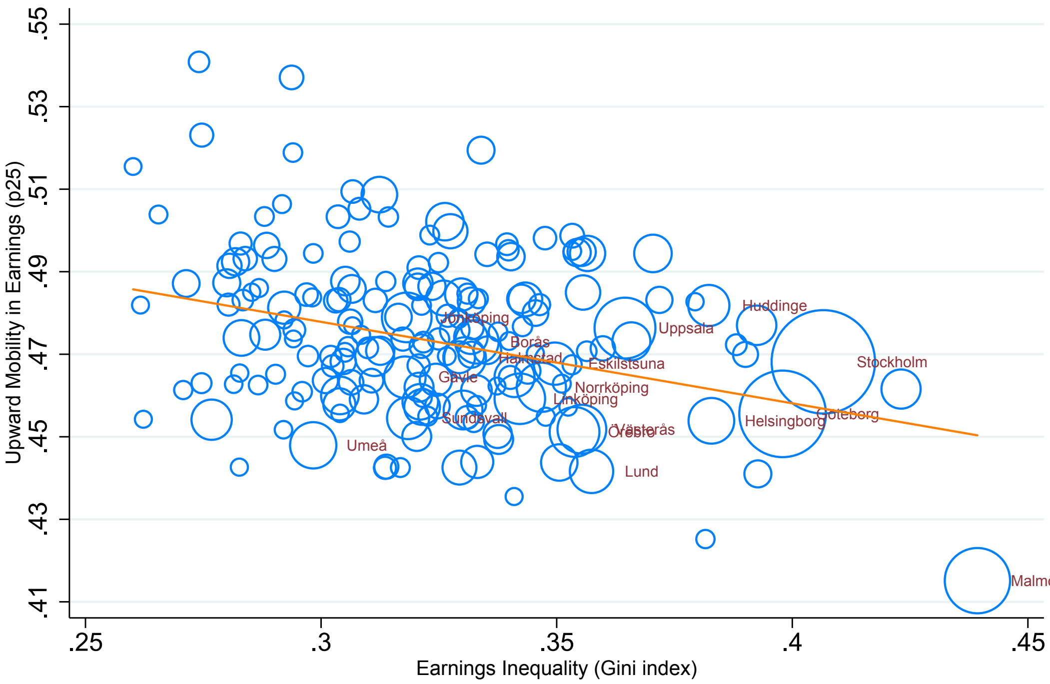
How intergenerational mobility varies across regions is interesting from a purely descriptive perspective. However, such regional variations might also point to specific causal determinants of mobility. As a simple first step, we relate the region-specific mobility statistics to other local characteristics (*"regional correlates"*) to test whether they are systematically associated with high or low mobility.

We focus here on one particular relation between *intergenerational* mobility and *cross-sectional* inequality. A robust pattern in international comparisons is that income mobility and inequality are negatively associated across countries (Blanden, 2011, Corak, 2013). However, it is unclear whether this statistical relation, informally labelled as the "Great Gatsby Curve", reflects a direct causal link, a third factor affecting both inequality and mobility, or whether the association is entirely spurious. Regional comparisons are a useful next step, as the number of observations can be much larger than in cross-country comparisons, as mobility statistics are from the same data source and therefore more comparable, and as many institutional and economic factors are held fixed when comparing regions within the same country.

Figure 7 plots the upward mobility in earnings (measured as the expected rank for children born to fathers at the 25th percentile of the national distribution) against a measure of earnings inequality in the parent generation. We measure inequality by the Gini index, but the results are similar when considering alternative measures, such as the standard deviation of earnings. We find a clear negative relation, with those municipalities characterized by high cross-sectional inequality often being characterized by low upward mobility as well. The relation also holds across Sweden's larger cities (*Stockholm*, *Malmo* and *Gothenburg*),

[19] An open question that we do not explore further here is whether observed earnings for residents in Malmö might be affected by cross-border commuting to Copenhagen.

Figure 7: The Great Gatsby Curve across Swedish municipalities



Notes: The figure plots the upward mobility in earnings (measured as the expected rank for children born to fathers at the 25th percentile of the national distribution) against a measure of income inequality (the Gini index in the parent generation). Scatter plot and linear fit restricted to municipalities for which at least 2,000 father-child pairs are observed. Weighted by the number of father-child pairs.

with *Malmo* being characterized by particularly high earnings inequality and low upward mobility.

How robust is this association between earnings inequality and mobility to the choice of mobility measure (see Section 4)? Table 2 extends on Figure 7 by reporting the correlation between different measures of intergenerational mobility and the Gini index. We report estimates separately for a sample of father-son pairs (Columns (1) and (2)) and a pooled sample comprising both sons and daughters (Columns (3) and (4)). For each sample, the first column reports raw correlations while the second column controls for municipality size, as income inequality tends to be more pronounced in the larger cities.[20] All correlations are weighted by the number of parent-child pairs observed for each municipality.

The first row of Table 2 correlates the rank slope in earnings with the Gini coefficient across all 290 Swedish municipalities. The correlations are around 0.3 when considering father-son pairs, and around 0.2 when pooling both sons and daughters. The correlations are similar or higher when considering the intergenerational elasticity of income (i.e., the log-

[20]Specifically, we regress the Gini index and each intergenerational statistic on the number of complete father-child pairs and report the pairwise correlations between the residuals from those regressions.

Table 2: The Great Gatsby Curve across Swedish municipalities

	Sons		Sons and Daughters	
	(1)	(2)	(3)	(4)
Income				
Rank slope	0.29	0.26	0.19	0.19
	p=0.00	p=0.00	p=0.00	p=0.00
Elasticity (log)	0.30	0.23	0.18	0.16
	p=0.00	p=0.00	p=0.00	p=0.01
Correlation (log)	0.46	0.34	0.33	0.27
	p=0.00	p=0.00	p=0.00	p=0.00
p25 upward	-0.28	-0.31	-0.48	-0.42
rank mobility	p=0.00	p=0.00	p=0.00	p=0.00
Education				
Father-child	0.34	0.27	0.37	0.30
correlation	p=0.00	p=0.00	p=0.00	p=0.00
Grandfather-	0.57	0.42	0.55	0.40
child correlation	p=0.00	p=0.00	p=0.00	p=0.00
Municipality size		x		x

Notes: The table reports the pairwise correlations between the indicated intergenerational statistic and the Gini index (weighted by the number of father-child pairs). The rank slope is the slope coefficient from a regression of child on father's income rank. The elasticity (log) and correlation (log) are the slope coefficient from a regression of child log income on father's log income and the corresponding Pearson correlation, respectively. P25 upward mobility is defined as the expected child income rank at the 25th percentile of father's ranks. Educational correlations are based on years of schooling. Columns (1) and (3) report raw correlations while columns (2) and (4) report the correlation between the residuals from separate regressions of the Gini and intergenerational coefficient on sample size.

log regression slope) or the intergenerational correlation in income (i.e., the log-log Pearson correlation). In all cases, the relations are highly significant ($p \leq 0.01$). The fourth row reports the full-sample equivalent to Figure 7, confirming that the expected income rank of children at the 25th percentile of the parental distribution are significantly lower when income inequality is high. Intergenerational persistence is therefore systematically higher, and upward mobility significantly lower, when earnings inequality is high.

This observed negative relation between income inequality and intergenerational mobility also holds when measuring mobility based on educational outcomes. For example, the cross-regional correlation between the Gini index and the father-child correlation in years of schooling is around 0.3, depending on specification. Interestingly, the Gini index relates even more strongly to the multigenerational *grandfather*-child correlation, with correlations that are between 30-65% higher. The raw correlations are above 0.5. While the table reports only the parent- and grandparent-child correlations from the paternal side, the pattern is similar for all possible gender combinations.

This evidence adds to a small number of recent studies that document a similar "Great Gatsby Curve" across regions within countries.[21] Moreover, our findings suggest that these within-country "Great Gatsby Curves" are robust to the choice of mobility statistic. This robustness is notable, given that different mobility statistics are only imperfectly related (see Section 4). We further find that the link between intergenerational mobility and income inequality is at least as pronounced for educational outcomes as for income. This observation is useful, as intergenerational linked data containing income for two generations are not available for many countries or settings.

Overall, we find a strong negative relation between cross-sectional inequality and mobility, which is quite robust to the choice of mobility statistic and other specification choices. While our research design does not allow us to establish whether this relation is causal, the statistical pattern appears quite consistent with such interpretation.

6 Do Recessions Affect Mobility?

Economic downturns can have important distributionary effects, as they tend to affect some groups, such as young workers and those with lower education levels, more than others (Hoynes, Miller and Schaller, 2012; Yagan, 2019). However, it is not yet understood if recessions also affect intergenerational mobility, i.e. whether the impact of the recession varies systematically with family background.

We address this question by studying two distinct events, the Swedish economic crisis in the early 1990s and the financial crisis that affected Sweden between 2008 and 2010. For causal identification of the effects of recession on intergenerational mobility we exploit variation in the severity of the economic downturn across municipalities (i.e., variation in *local exposure*). We use local exposure measures as constructed by Engdahl and Nybom (2021) based on employment data from the *Register-based Labor-Market Statistics* (RAMS). The shocks are defined as the municipal percentage-point drop in the employment rate between 1990-1993 (for the 1990s crisis) or 2008-2010 (for the financial crisis), which approximately corresponds to the change between the start and the trough of each crisis (on the national level).

[21] Examples include Chetty et al. (2014) for the U.S. and Güell et al. (2018) and Acciari, Polo and Violante (2016) for Italy.

6.1 The 1990s Economic Crisis and the Financial Crisis

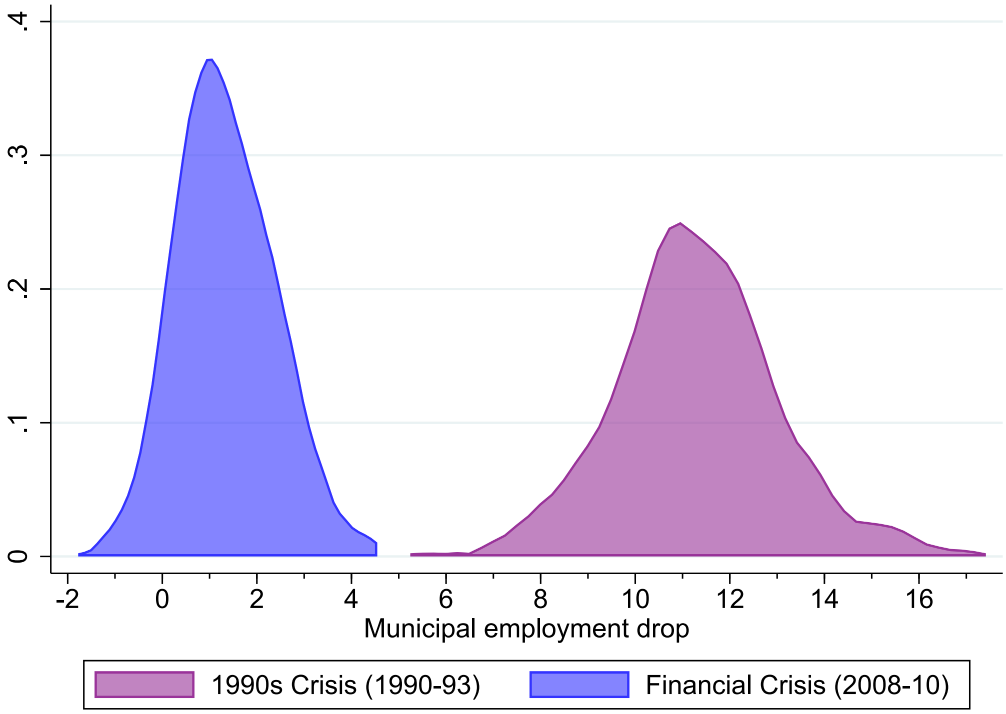
Over the last three decades, Sweden went through two serious economic downturns.[22] The first and the deepest was the the economic crisis of the 1990s. The extensive capital-market deregulations in the 1980s – which for example lifted credit restrictions on banks – are often regarded as one of the root causes to the crisis. These gave rise to a large increase in lending to and debt among households and firms, and quickly rising asset prices and consumption. Due to various factors around 1990, including the high interest rates due to the German reunification and the large Swedish tax reform 1990-1991, the overheated Swedish economy was punctured and a deep recession started (Jonung et al., 2009). This recession developed into a full-scale banking and financial crisis, which in turned required cuts in the public sector. These budget cuts were extensive, and made the crisis quite long lasting – it took until 1997 before a clear recovery was visible on the Swedish labor market. During the first three years of the 1990s crisis, the unemployment rate rose by almost 8 percentage points and then stayed at a high level for several years before it slowly started to decrease. Employment fell by over 10 percentage points and had only partially recovered 10-15 years after the advent of the crisis.

The global financial crisis originated from the US financial markets in the fall of 2007. When the scope of credit risks (e.g. housing credit) that US banks had taken over the years prior came to light, the US housing market crashed and several important credit institutes and investment banks went bankrupt. These events spread across the globe and a deep debt crisis hit the Euro zone, with the countries in Southern Europe most strongly affected. The Swedish labor market was also affected, in the form of rising unemployment and falling employment over the years 2008 to 2010. However, the recession affected a smaller set of industries (e.g. the automobile industry) and was comparatively mild, both compared to the US and compared to the Swedish crisis in the 1990s. The crisis caused a drop in employment of about 2 percentage points, but this loss was followed by a period of strong employment growth over many years. Unemployment rose by around 2-2.5 percentage points between 2008-2010 and then recovered.

Figures 8 plots the density and spatial distribution of employment losses across municipalities, separately for the two crises (see Section 2.4). The 1990s crisis had strong impact

[22] We do not analyze a third and milder downturn that started around 2001, right after the burst of the so-called IT bubble, and which was accentuated by global uncertainty after the terror attacks in New York in September 2001.

Figure 8: Employment loss during the 1990s crisis and the financial crisis



Notes: Density of municipal employment loss during the 1990s crisis and the financial crisis (bandwidth: 0.4), based on Engdahl and Nybom (2021).

on the labor market, with an average drop in employment of more than 11 percent. In contrast, the financial crisis had comparatively mild effects in Sweden, with employment dropping on average by less than 2 percent. However, in both cases the employment effects varied strongly across municipalities. Figure 9 illustrates that the shocks were widely distributed across municipalities. The figure is based on a similar figure in Engdahl and Nybom (2021), who also describe the spatial pattern of the respective shock in more detail. The 1990s shock had worse impacts in larger cities and municipalities, while the impact of the financial crisis was more evenly distributed.

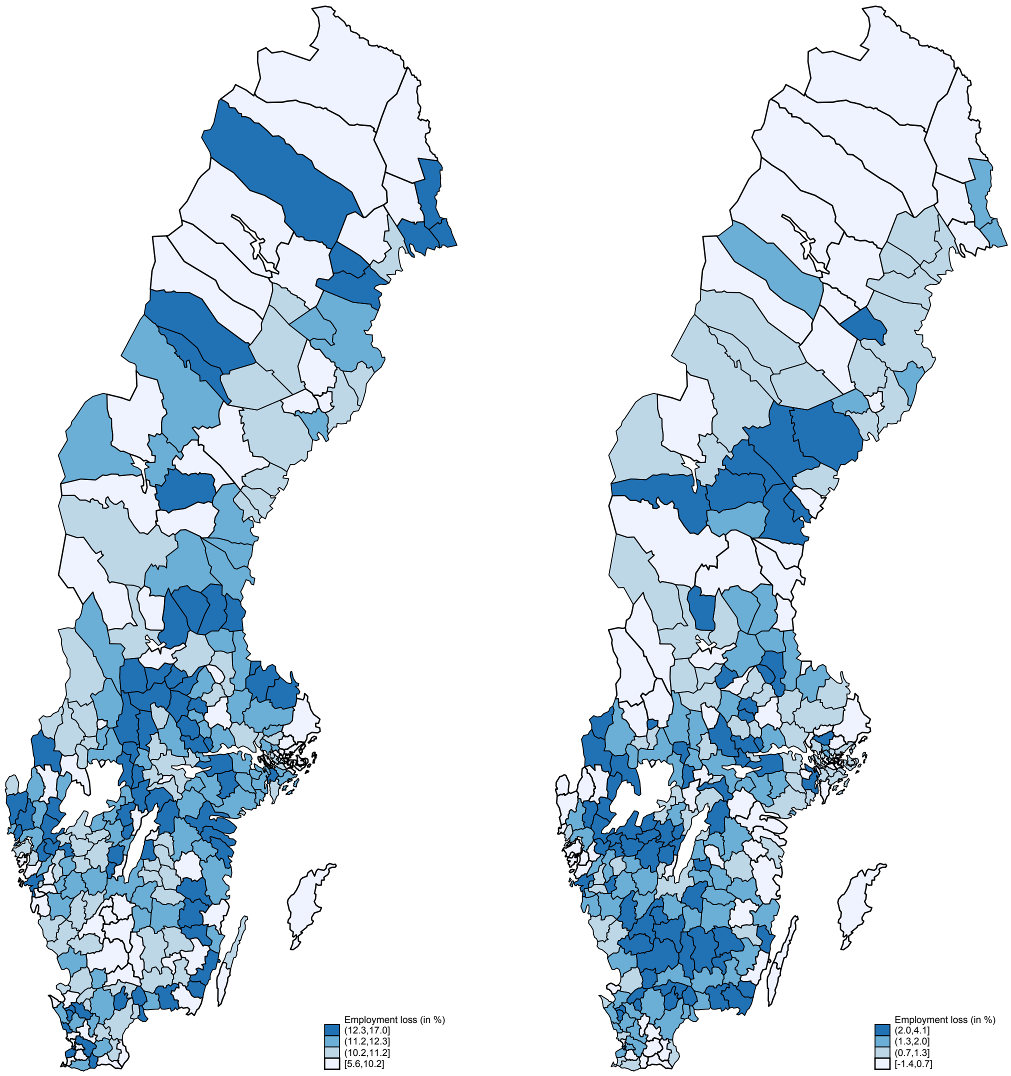
6.2 Empirical specification

How did these recessions affect intergenerational mobility? We estimate the impact based on a two-step process. In a first step, we estimate the intergenerational regression

$$y_{ir} = \alpha_{rt} + \beta_{rt} y_i^f + \varepsilon_{ir}, \tag{2}$$

Figure 9: Employment loss during the 1990s crisis and the financial crisis

(a) 1990s crisis (1990-93) (b) Financial crisis (2008-10)

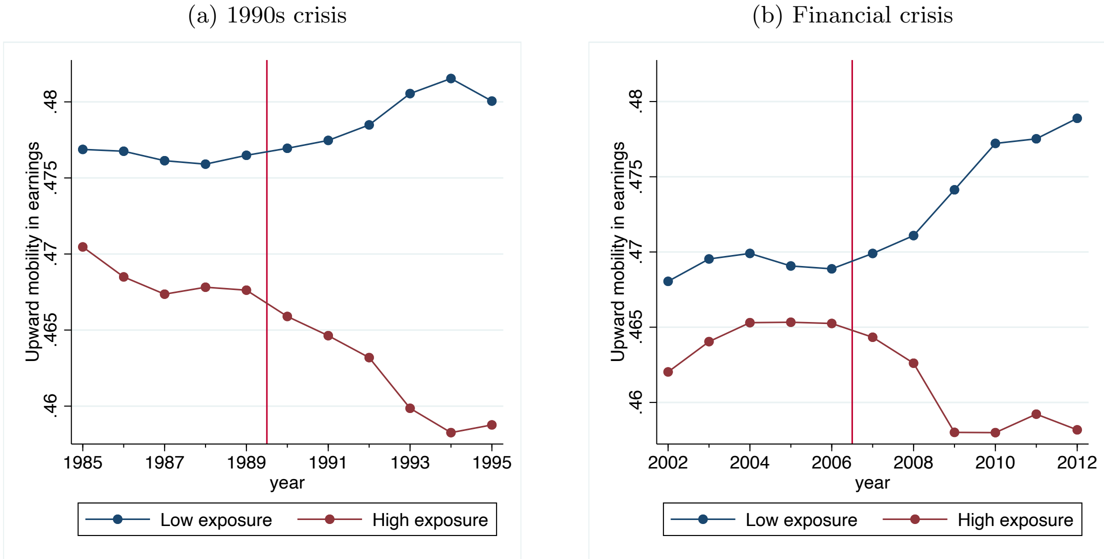


Notes: Municipal employment loss during the 1990s crisis (sub-figure a) and the financial crisis (sub-figure b), based on Engdahl and Nybom (2021).

where y_{ir} is the income rank (defined within the national distribution for the respective birth cohort and age) of individual i in municipality (region) r and y_i^f is the income rank of the father (defined within the child's birth cohort). We estimate this regression separately for each municipality r, and for the pre- and post-recession period ($t = \{pre, post\}$).[23] Based on the estimated intercept $\hat{\alpha}_{rt}$ and $\hat{\beta}_{rt}$ we can construct other mobility statistics $\hat{\theta}_{rt}$, such as the expected "absolute upward mobility" of children born to parents at the 25th percentile (i.e., $y_i^f = 0.25$), as defined in Chetty et al. (2014).

[23] The income ranks y_{ir} are based on the mean log income over the respective pre- and post-period, while the father's rank y_i^f is based on a five-year average income in 1985-89 (1990s crisis and financial crisis, earlier birth cohorts) or when the child was aged 10-14 (financial crisis, cohorts born 1975-1991).

Figure 10: Common Trends



Notes: Expected income rank of children from low-income background ($y_i^f < 0.5$) aged 25-35, separately for municipalities in highly exposed areas (employment loss greater than the median loss) and less exposed areas (employment loss below median).

In a second step, we regress the change in mobility between the pre- and post-recession period on the local severity of the recession,

$$\hat{\theta}_{r,post} - \hat{\theta}_{r,pre} = \mu + \gamma_\theta Shock_r + u_r \quad (3)$$

where γ_θ captures the causal effect of the recession on mobility statistic θ. The "pre"-periods are defined as 1986-89 for the 1990s crisis and 2003-06 for the financial crisis. Since our income data starts in 1985, we restrict our analysis of the 1990s crisis to younger individuals (age 25-35) for whom the parents are more likely to be still contained in the income data.[24] The "post-"periods (post *onset* of the crisis) are defined over the periods 1992-95 and 2008-11, respectively. To account for the varying size of municipalities, second-step regressions are weighted by the minimum number of observations in the pre- and post-period. We use the current area of residence of the "child" to allocate parent-child pairs to municipalities. We are therefore measuring the short-term effect of a local economic downturn on intergenerational mobility in a *region*, not the long-run effects of local economic downturns on *individuals*.

The second-step regression (3) can be interpreted as a generalized difference-in-differences

[24] The share of individuals for whom we cannot merge paternal income is high even in the age group 30-35. However, while this might affect the level of our estimates, we are here interested in the differential change over time.

Table 3: The impact of economic downturns on income mobility

	Sons			Sons and Daughters		
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ p25 mobility	Δ p75 mobility	Δ slope	Δ p25 mobility	Δ p75 mobility	Δ slope
Panel A: The 1990s Crisis (Municipal employment drop in 1990-93)						
Baseline	-0.658***	-0.610***	0.096	-0.496***	-0.414***	0.163
	(0.085)	(0.081)	(0.150)	(0.062)	(0.056)	(0.114)
Age 25-30	-0.777***	-0.723***	0.107	-0.567***	-0.548***	0.039
	(0.102)	(0.123)	(0.200)	(0.076)	(0.087)	(0.139)
Age 30-35	-0.399***	-0.278**	0.241	-0.352***	-0.126	0.451**
	(0.097)	(0.096)	(0.219)	(0.068)	(0.068)	(0.148)
N	284	284	284	284	284	284
Panel B: The Financial Crisis (Municipal employment drop in 2008-10)						
Baseline	-1.243***	-0.856***	0.774**	-0.916***	-0.726***	0.381*
	(0.180)	(0.220)	(0.234)	(0.145)	(0.162)	(0.155)
Age 25-30	-1.500***	-1.002**	0.997**	-1.160***	-0.856***	0.607*
	(0.228)	(0.303)	(0.359)	(0.197)	(0.230)	(0.246)
Age 30-35	-1.005***	-0.721***	0.568*	-0.685***	-0.598***	0.173
	(0.174)	(0.183)	(0.229)	(0.120)	(0.130)	(0.167)
Age 35-40	-0.935***	-0.957***	-0.044	-0.678***	-0.780***	-0.204
	(0.177)	(0.202)	(0.241)	(0.129)	(0.166)	(0.190)
Age 40-45	-0.739***	-0.518***	0.442	-0.588***	-0.397**	0.382
	(0.156)	(0.155)	(0.260)	(0.124)	(0.126)	(0.194)
N	290	290	290	290	290	290

Notes: The table reports difference-in-differences estimates of the impact of a 1% drop in local employment on expected income ranks (in percentiles) based on equation (3). Observations are weighted by the minimum of the number of observations in the pre- and post-periods, respectively. Robust standard errors in parentheses, * p<0.05, ** p<0.01, *** p<0.001.

design with a continuous treatment variable, based on a parallel trends identifying assumption. In support of this assumption, Figure 10 plots the expected income rank of children from low-income background ($y_i^f < 0.5$) for the 1990s crisis and financial crisis, separately for municipalities in highly exposed areas (employment loss greater than median) and less exposed areas (employment loss below median). The level and trends in upward mobility were relatively similar in low and high exposure municipalities before each crisis, but diverged strongly thereafter. We interpret this observation as indirect evidence in support of the identifying assumption that the trends would have remained parallel in the absence of these economic downturns.

6.3 Impact estimates

Table 3 reports the impact estimates resulting from regression (3), separately for the 1990s crisis (Panel A) and the financial crisis (Panel B), separately for sons (Columns (1)-(3)) and a pooled sample for sons and daughters (Columns (4)-(6)). We first show that economic downturns decrease "absolute upward mobility" of disadvantaged children born to parents at the 25th percentile, $\beta_{25} = E[y_{ir}|y_i^f = 0.25]$ (i.e., in the bottom half). For example, in our baseline sample comprising individuals aged 25-35, a 1% drop in employment during the 1990s crisis decreases the expected income rank by 0.7 percentiles. The implied total drop in upward mobility is large, given that some municipalities experienced employment losses of 10 percent or more. While the financial crisis had a more modest effect on employment, its spillover on absolute upward mobility was nearly twice as large, with a one percentage drop in employment decreasing β_{25} by 1.2 percentiles.

This effect of local economic downturns on income mobility varies strongly with age. In both crises, the drop in upward mobility was most severe for the youngest considered group (age 25-30) and diminishes monotonically over age. For example, the effect of the 2007-2008 financial crisis on upward mobility of those aged 25-30 was twice as large ($\hat{\beta} = -1.50$) as compared to those in the middle of their career at age 40-45 ($\hat{\beta} = -0.74$). These impact estimates are precisely estimated and the differences between age groups statistically significant. The strong negative effect on the youngest age group is notable, given that income measured at such early age are noisier measures of economic status, which we would expect to attenuate the estimated effect. The qualitative finding of a drop in upward mobility is otherwise fairly mechanical, in that a local economic decline is of course associated with a corresponding drop of the area's inhabitants in the national income distribution.

More interesting is whether the effects of such local decline are experienced similarly for children from disadvantaged and advantaged family background. To answer this question, we report in Columns (2) and (5) how the two recessions affected the expected income rank of advantaged children born to high-income families at the 75th percentile of the parental distribution, $\beta_{75} = E[y_{ir}|y_i^f = 0.75]$. We find a similar age pattern as for the 25th percentile, with individuals more negatively affected if exposed to the recession at an earlier age. However, the impact estimates at the 75th percentile tend to be smaller than the corresponding estimates at the 25th percentile. For example, a 1% drop in local employment during the financial crisis decreases the expected income rank of individuals aged 30-35 at the 25th percentile by 1 percentiles, but the expected rank at the 75th percentile by only 0.7 percentile.

This comparison between upward mobility at the 25th percentile and the 75 percentile therefore implies that *relative* mobility has decreased in those municipalities that were more affected by the 1990s or financial crisis. To test this hypothesis formally, we report in Columns (3) and (6) how the two recessions shifted the slopes of the conditional expectation function (i.e., the rank-rank slope in equation (2)). We multiply these slopes $\times 100$, such that the reported second-step coefficients can be interpreted as the change in the expected rank difference between those born to the bottom ($y_i^f = 0$) or top of the parental income distribution ($y_i^f = 100$). With few exceptions, the coefficient estimates are positive, indicating that the slope became steeper – the expected rank decreased less at the top of the distribution than at the bottom, such that relative mobility declined.

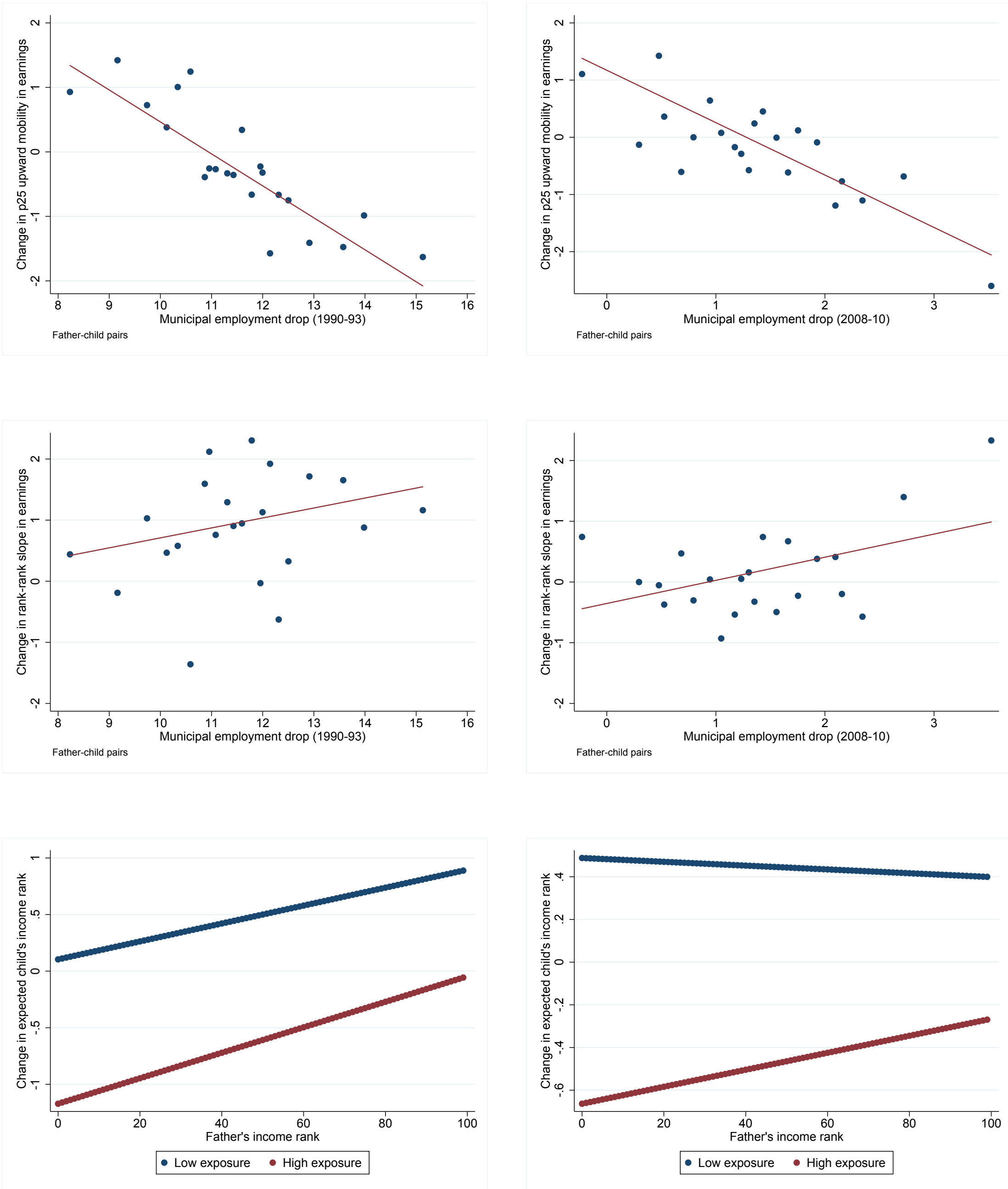
These estimates are statistically significant for our baseline samples in the financial crisis. The pattern is more noisy across age groups, but relative mobility appears to have declined particularly strongly for young workers. While the point coefficients are also positive for the 1990s crisis, the pattern is much less pronounced, and statistically significant only for one group (age 30-35, sons and daughters).

Figure 11 provides a non-parametric, graphical representation of the results for our pooled baseline sample of sons and daughters. The first panel in the respective sub-figure for the 1990s crisis (sub-figure a) and the financial crisis (sub-figure b) shows a binned scatter plot of the change in p25 upward mobility on our local exposure measure (municipal employment loss). Consistent with the coefficient estimates from Table 3, we see a stronger decline in upward mobility in those municipalities that were more heavily hit by the recession. For both recessions, the relationship is well captured by a linear fit, suggesting that the impact of local economic downturns on upward mobility tend to be proportional to its severity.

The second panel in each sub-figure shows that the rank-rank slope increases in those municipalities that were more heavily hit by the respective recession. However, this increase is fairly weak for the 1990s crisis, and driven by those municipalities most heavily exposed in the case of the 2007-2008 financial crisis. The final panel illustrates the implication from the previous two figures: in more exposed areas (employment loss greater

Figure 11: The impact of economic downturns on income mobility

(a) The 1990s Crisis (b) The Financial Crisis



Notes: The first two panels show binned scatterplots of the post- vs. pre-shock difference in p25 upward income mobility and the rank-rank slope against the municipal employment drop. The final panel plots the post- vs. pre-change in the conditional expectation of child income rank conditional on father's income rank, separately for municipalities exposed to a more severe (employment loss greater than median) and less severe economic downturn (employment loss below median).

than the median loss), the conditional expectation function shifts downward, but *more so* so for children from disadvantaged, low-income families. We see only a minor drop in the expected income rank for children from high-income families.

We therefore find that the economic downturns had a more damaging effect on the expected income of children from disadvantaged backgrounds. This pattern is particularly pronounced for the 2007-2008 financial crisis. While the financial crisis had comparatively mild employment effects in Sweden, it had a particularly asymmetric impact, leading to strong income losses for young workers from disadvantaged backgrounds.

Of course, it is unclear whether these findings extend to other countries or other types of economic downturns. Different factors underlying a recession might also lead to different distributional and therefore intergenerational impacts. However, it is interesting that the qualitative pattern are similar across the two crises considered here. We may therefore hypothesize that economic downturns tend to decrease intergenerational mobility, in particular upward mobility in income, and that these effects tend to be larger for younger age groups.

However, the estimates reported here capture only the *short-term* and *regional* impact of each recession on intergenerational mobility. An interesting question, which we do not pursue here, is how persistent these effects are over time: whether recessions affect relative mobility only temporarily, or whether they lead to persistent shifts in the income gap between children from advantaged and disadvantaged family backgrounds over their entire careers.

7 Conclusions

Intergenerational mobility varies substantially across regions in Sweden, and this variability may be informative about different aspects of the transmission process. With this objective in mind, we first described the extent of regional variation in educational and income mobility across Swedish municipalities, and its spatial pattern. Educational mobility tends to be lower in the more densely populated southern parts of Sweden, and in and around the major cities. The pattern of income mobility is more dispersed, but upward mobility tends to be comparatively high in many municipalities in Southern Sweden, for children born around Stockholm, and in the northernmost municipalities in Sweden.

We next considered whether the regional rankings are stable to the choice of mobility measures. We find that different measures of *educational* mobility are highly correlated, and similar for father-child as for mother-child correlations, for regression vs. correlation coefficients, and for inter- (parent-child) vs. multigenerational (grandparent-child) correlations. But regional rankings are more sensitive to the choice of measure for *income* mobility, and areas that are characterized by high upward mobility in income are not necessarily the same areas that are characterized by high relative mobility.

These regional variations are interesting from a descriptive perspective, but our main purpose was to exploit them to learn more about the intergenerational transmission process. In particular, we are interested in the relation between cross-sectional inequality and intergenerational mobility. We find that municipalities characterized by high income inequality in the parent generation also tend to be characterized by low intergenerational mobility. This relationship is pronounced for all mobility measures that we considered. As a novel observation we find that the inequality-mobility relation is particularly pronounced for *multi*generational correlations.

Finally, we studied whether macroeconomic conditions affect intergenerational mobility. While the effects of economic downturns on cross-sectional inequality are well studied, there exists little evidence on their effects on intergenerational mobility. Exploiting regional variations in the severity of the 1990s economic crisis and the 2007-2008 financial crisis in Sweden, we found that children from higher-income families suffered only a modest drop in their income ranks, while children from low-income families experienced a much steeper drop. The drop is particularly pronounced for younger workers. Our findings therefore suggest that recessions not only affect cross-sectional inequality, but also decrease intergenerational mobility.

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