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Biases in standard measures of intergenerational income dependence

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Biases in standard measures of intergenerational income dependence

by

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Abstract

Estimates of the most common mobility measure, the intergenerational elasticity, can be severely biased if snapshots are used to approximate lifetime income. However, little is known about biases in other popular dependence measures. We use long Swedish income series to provide such evidence for linear and rank correlations, and rank-based transition probabilities. Attenuation bias is considerably weaker in rank-based measures. Life-cycle bias is strongest in the elasticity; moderate in the linear correlation; and small in rank-based measures. However, with important exceptions: persistence in the tails of the distribution is considerably higher, and long-distance downward mobility considerably lower, than estimates from short-run income suggest.

Keywords: Intergenerational mobility, rank correlation, measurement error
JEL-codes: J62

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1 Introduction

A growing literature studies to what degree differences in long-run economic status persist across generations. Researchers typically aim to estimate the dependence between *lifetime* incomes or earnings of parents and offspring, with strong associations implying a low degree of intergenerational mobility (and vice versa). Descriptive measures of that dependence provide not only a useful account of the dynamic dimension of inequality across generations, but are also key starting points for the analysis of the underlying causal mechanisms of transmission.

However, the most common dependence measure, the intergenerational elasticity, can be severely mismeasured if snapshots of income are used to approximate lifetime values. Elasticity estimates are sensitive to both attenuation (Solon, 1992; Mazumder, 2005) and life-cycle bias from heterogeneous age-income profiles (Grawe, 2006; Haider & Solon, 2006; Nybom & Stuhler, forthcoming). An improved understanding of these approximation biases have led to large upward corrections of elasticity estimates, and a substantially revised picture of how mobile many developed countries are (Solon, 1999; Black & Devereux, 2011; Jäntti & Jenkins, 2014).

Partly in response, researchers have turned to other dependence measures: *linear* or *Pearson correlations*, which abstract from changes in cross-sectional inequality; *rank* or *Spearman correlations*, which capture the association between the relative position of parents and children in the marginal distributions; and *transition matrices*, often with particular focus on the poorest or richest in the population. Rank-based measures are the basis for much of the recent evidence on mobility differentials across countries (Corak et al., 2014; Bratberg et al., 2015), time (Chetty et al., 2014b; Pekkarinen et al., 2015), and regions within countries (Chetty et al., 2014a).

However, little is known about the robustness of these alternative measures. We found no comprehensive evidence on approximation bias in linear correlation estimates, even though such estimates are frequently reported.[1] Work on rank-based measures includes O'Neill et al. (2007), who simulate the effect of observational errors on transition matrices; Dahl & DeLeire (2008), who note that estimates of the rank correlation are comparatively insensitive to specification choices; and Chetty et al. (2014a), who observe that such estimates appear stable when incomes are measured

[1] In particular, they are often used for cross-country and other comparative studies (Björklund & Jäntti, 2009).

beyond age 30. These are promising insights for the literature, which often has to rely on only a handful or even single income observation per individual.[2] However, the robustness of these alternative measures has not yet been assessed systematically using data on actual lifetime incomes.

In this paper we provide evidence on approximation biases across all four dependence measures. Our empirical analysis takes advantage of unusually long administrative series of income data from Sweden. The observation of nearly career-long income histories of parents and offspring allows us to derive benchmark estimates and to directly expose the bias that arises when using shorter income spans. Our analysis shows that the consequences of measurement error differ strongly across dependence measures. However, the observed patterns appear systematic and generalizable. A key finding is that rank-based estimates are both the least attenuated and the most stable over age. Consideration of such measures may thus mitigate much of the measurement issues that plague the literature.

The paper proceeds as follows. We describe our data in the next section, and summarize our basic results in Section 3. In Section 4 we provide theoretical explanations for our findings, present additional evidence, and discuss various correction procedures. We further study the copula of parental and offspring incomes, to test if the robustness of the positional measures holds along the joint distribution of ranks. Section 5 concludes.

2 Data

We use data from two administrative registers, put together by Statistics Sweden: the multi-generational register, from which we have access to a random sample of the Swedish population, including their biological parents; and the income register, from which we use total market income originating from tax assessments. We include sons born 1953-1957 with fathers born 1927-1941. Income data are available for the years 1960-2007 and we restrict the sample to fathers and sons with positive income in at least 10 years.

[2] Data availability is a particular problem in recent research on mobility across multiple generations or trends across time.

Our main sample (used in Sections 3 and 4) consists of 6,525 father-son pairs, with sons' (fathers') income measured from age 22 to age 50 (age 33 to age 60), irrespective of birth years. We divide the sum of real annual incomes by the number of non-missing incomes and take the logs of those values. Mean log lifetime income of sons (fathers) is 12.25 (12.22) with a standard deviation of 0.44 (0.43). For our analyses of Pearson correlations, we standardize log annual and lifetime incomes by birth year. Income ranks are computed from absolute levels, thus also including those who report zero income.[3] For our nonlinear analyses in Section 5 we use an extended sample for which we restrict the income streams to 1968-2007. The main difference in this sample is that incomes of fathers now are observed from age 41 to 60. All other sample criteria and variable definitions are held constant. The extended sample consists of 63,441 father-son pairs, and the mean of log lifetime income of sons (fathers) is 12.26 (12.25) with a standard error of 0.43 (0.45). Overall, these data offer a unique possibility to examine different measures of intergenerational mobility using nearly career-long income histories for two linkable generations.

3 Empirical strategy and basic results

Let log lifetime incomes of parents and children, x^* and y^*, be expressed as deviations from generational means. The unconditional population relationship between y^* and x^* can be summarized using different measures, such as the intergenerational elasticity (the slope coefficient in the regression of y^* on x^*), Pearson or Spearman correlations, or rank-based transition probabilities. But in applications we typically only observe short-run incomes

(1) $$x = x^* + u$$

(2) $$y = y^* + v,$$

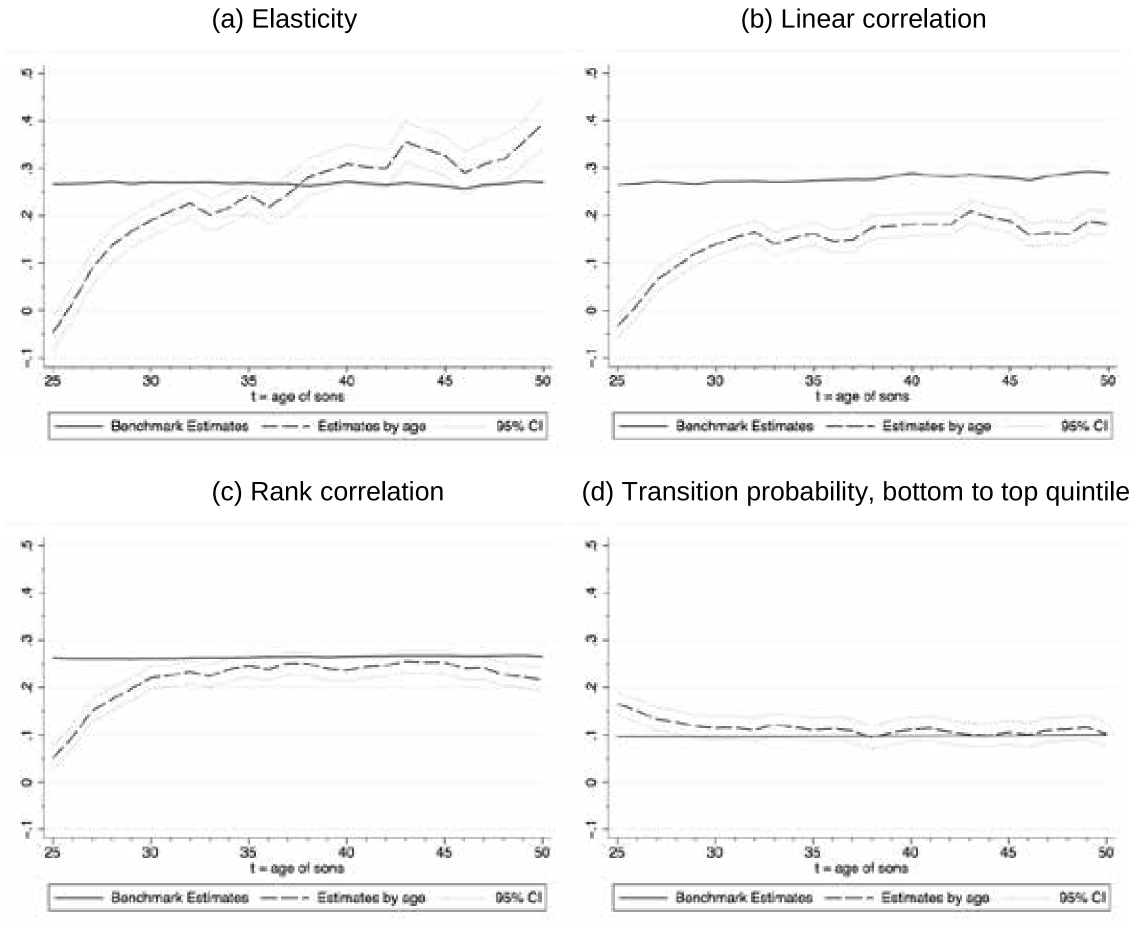
with u and v being approximation errors. Those proxies are often based on only a few annual observations.

Our empirical approach is straightforward. We compare benchmark estimates that are based on the observed long-run income with age-specific estimates based on annual

[3] Excluding zeros has little effect on our estimates of the rank correlation.

incomes for one or both generations. We focus first on “left-side” measurement error (in y^*), using annual income for sons but lifetime income for fathers.[4] Figure 1 presents our basic empirical findings. For each of the four measures it plots the benchmark estimate (solid line) against age-specific estimates based on annual incomes for offspring (dashed line).

Figure 1: Mobility measures, benchmark vs. annual (LHS ME)



Note: Each figure plots the benchmark estimate based on lifetime incomes for both generations against estimates based on annual income of sons (left-hand side measurement error). The sample and thus the benchmark estimates vary over age as we drop persons with missing annual observations also for estimation of the benchmark. For ease of exposition confidence intervals of the benchmark estimates are not plotted (they are similar to or slightly smaller in size than those of the age-specific estimates).

The main commonality across all measures is that they severely understate dependence at young ages. As individuals with high lifetime income tend to have low income at young age, for example due to accumulation of human capital, neither linear nor rank-

[4] For simplicity, we use the terminology “left-side” (for children) and “right-side” (for parents) measurement error not only for regression-based but for all measures.

based dependence measures are well approximated before the early 30s.[5] In contrast, the degree and direction of bias at later ages differs wildly.

Elasticity estimates suffer strongly from life-cycle effects: increasing almost monotonically over age, they substantially overstate dependence at later ages. Consistent with previous work, life-cycle bias is minimized when incomes are observed around mid-age (see Grawe, 2006; Haider and Solon, 2006). But the slope of annual estimates over age can be steep; between age 30 and 50, they double from about 0.2 to 0.4 in our data. The linear and rank correlations are much less sensitive to life-cycle effects. The linear correlation increases only slightly between age 30 and 50, while the rank correlation is remarkably stable.[6] Its gradual decline in the late 40s however indicates that rank correlations may become less reliable when income is observed at late age.

As Figure 1 concerns left-side measurement error, elasticity estimates are not systematically attenuated (see next section). Classical errors do affect the other three measures, such that dependence continues to be understated through mid-age and beyond. However, attenuation is much weaker in the rank than in the linear correlation. The former is thus not only stable over age, but also much closer to the benchmark from about age 30. The remaining bias is dwarfed by that at early age or that in the log-linear measures, but it is not necessarily negligible: in mid-age it amounts to 5-10 percent of the benchmark. In the last panel we consider the probability of moving from the parental bottom to the offspring top quintile ("rags to riches"), as in Chetty et al. (2014b, 2014a). The results mirror those for the rank correlation: mobility is strongly overstated before age 30, but rather accurately estimated and stable thereafter.

These findings suggest that positional measures may be preferable for the analysis of income mobility if measurement is a major concern – which is often the case. We proceed with a more detailed formal and empirical treatment of each of the four measures.

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[5] This finding matters since in intergenerational data the offspring tend to be observed at younger ages. For example, Chetty et al. (2014b) measure income of children at age 29-30 or younger, while Pekkarinen et al. (2015) measure at age 35.

[6] This finding supports arguments in Chetty et al. (2014b, 2014a), whose main analyses rely on short spans of income measured at around age 30 for children. Chetty et al. use an auxiliary data source to test for life-cycle bias, finding little trend in estimates between age 30 and 40.

4 Four common measures of intergenerational dependence

4.1 The elasticity

A useful way to summarize the intergenerational log-income relationship is the conditional expectation function (CEF). The coefficient from a linear regression of y^* on x^*, $\beta_{(x^*,y^*)} = Cov(x^*,y^*)/Var(x^*)$, provides the best (in an MMSE sense) linear approximation for its slope. Much of the literature concerns the estimation of this *intergenerational elasticity*, but usage of short-run instead of true lifetime incomes yields a consistent estimator of

$$(3) \qquad \beta_{(x,y)} = \frac{Cov(x,y)}{Var(x)} = \frac{\beta_{(x^*,y^*)}\,Var(x^*) + Cov(x^*,v) + Cov(y^*,u) + Cov(u,v)}{Var(x^*) + Var(u) + 2Cov(x^*,u)}.$$

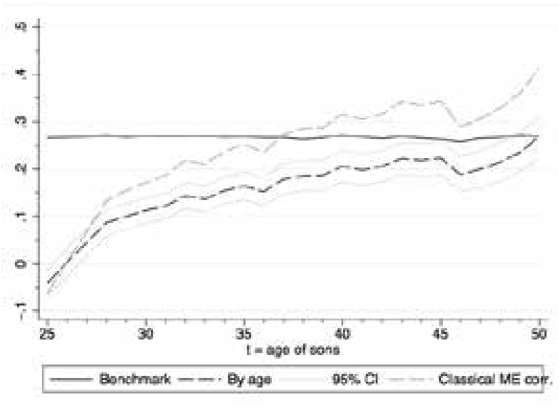
This coefficient may differ from the true elasticity for two reasons. First, it may be attenuated by classical measurement error. If u and v are assumed to be uncorrelated to true values and each other, equation (3) reduces to the textbook errors-in-variables formula $\beta_{(x,y)} = \beta_{(x^*,y^*)} rr_x$, where $rr_x = Var(x^*)/[Var(x^*) + Var(u)]$. Importantly, only classical error in parental income generates such *attenuation bias*. In addition, heterogeneity in income profiles may cause the covariance between error and true values to vary systematically over age, introducing *life-cycle bias* (Jenkins, 1987).

As we considered left-hand side measurement error, the bias shown in Figure 1 is exclusively due to life-cycle effects. Life-cycle bias is prone to rise with the age of offspring if those with higher lifetime incomes tend to have higher income growth rates. But the evolution of cross-sectional inequality over age may differ across populations, making comparative analyses difficult.[7] The attenuating effect of right-side measurement error is illustrated in Figure 2, which compares the benchmark elasticity against estimates based on annual incomes of sons at the specified age and fathers at age 45, with or without correction for attenuation bias.

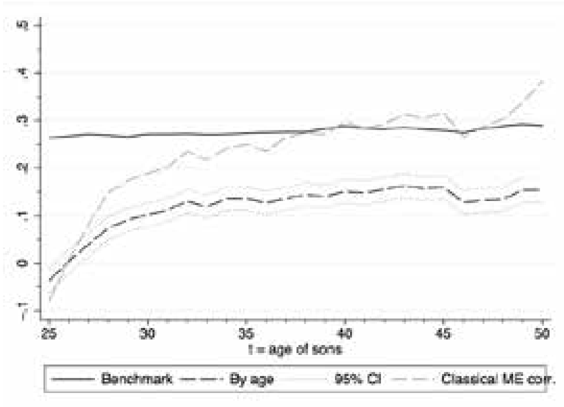
[7] Chetty et al. (2014a) report elasticity estimates that stabilize around age 30, while Grawe (2006), Nybom and Stuhler (forthcoming), and Gregg et al. (2014) find rather strong age dependency.

Figure 2: Mobility measures, benchmark vs. annual (BHS ME)

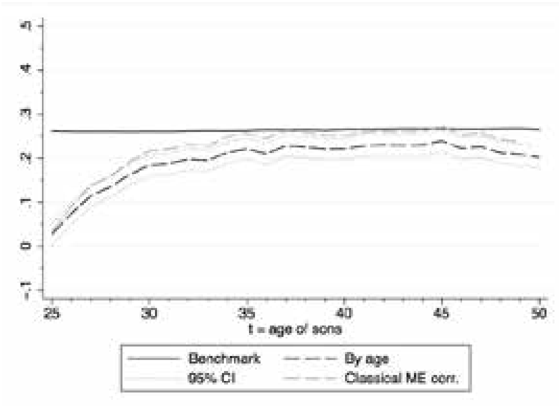
(a) Elasticity



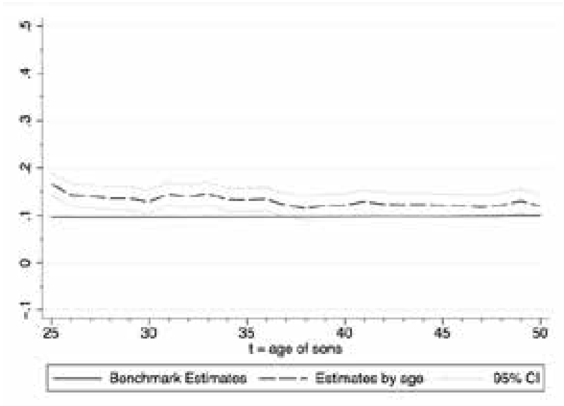
(b) Linear correlation



(c) Rank correlation



(d) Transition probability, bottom to top quintile



Note: Each figure plots the benchmark estimate based on lifetime incomes for both generations against estimates based on annual income of sons (varying by age) and fathers' income at age 45 (both-hand side measurement error). The sample and thus the benchmark estimates vary over age as we drop persons with missing annual observations also for estimation of the benchmark. Subfigures a-c additionally plot estimates by age that are adjusted using corrections for classical measurement error described in Section 4. For ease of exposition confidence intervals of the benchmark estimates are not plotted (they are similar to or slightly smaller in size than those of the age-specific estimates).

4.2 The linear correlation

The Pearson correlation coefficient, $\rho_{(x^*,y^*)} = Cov(x^*, y^*)/(\sqrt{Var(x^*)}\sqrt{Var(y^*)})$, abstracts from changes in the variance of income over generations. Under classical error, the usage of proxy income yields $\rho_{(x,y)} = \rho_{(x^*,y^*)}\sqrt{rr_x rr_y}$, . Correlation estimates are thus attenuated by classical errors in *both* parent and offspring income, while only the former affects the elasticity. This distinction is of practical importance; correlation estimates are much lower than elasticity estimates under left- (Figure 1) but more similarly attenuated under both-side measurement error (Figure 2). While the common practice of using multi-year averages for parents thus leads to large improvements for the elasticity, it is for the correlation equally important to also address transitory noise in offspring income.

To our knowledge, no direct evidence exists on life-cycle effects in intergenerational correlations, but arguments made with respect to other measures can be adapted. Grawe (2006) and Haider and Solon (2006) document how the tendency of early-career (late-career) income gaps to understate (overstate) lifetime differences generate life-cycle bias in elasticity estimates. Following Haider and Solon, capture this insight in the linear projection $y = \lambda y^* + w$ and assume that w is uncorrelated to x^*, such that

$$(4) \qquad \rho_{(x^*,y)} = \frac{Cov(x^*, y)}{\sqrt{Var(x^*)Var(y)}} = \rho_{(x^*,y^*)} \frac{\lambda\sqrt{Var(y^*)}}{\sqrt{\lambda^2 Var(y^*) + Var(w)}}.$$

We can illustrate some new insights by rewriting equation (4)

$$(5) \qquad \rho_{(x^*,y)} = \begin{cases} -\rho_{(x^*,y^*)} \dfrac{\sqrt{Var(y^*)}}{\sqrt{Var(y^*) + (\frac{1}{\lambda^2})Var(w)}} & \text{if } \lambda < 0 \\ \rho_{(x^*,y^*)} \dfrac{\sqrt{Var(y^*)}}{\sqrt{Var(y^*) + (\frac{1}{\lambda^2})Var(w)}} & \text{if } \lambda > 0 \end{cases}.$$

Correlation estimates should be sensitive to life-cycle bias at very early ages, where differences in current and lifetime income can be *negatively* correlated, such that $\lambda < 0$. We expect less sensitivity at mid- and late age: as correlations are invariant to positive monotone linear transformations of the variables, $\lambda > 0$ only enters the reliability ratio. The ratio goes to zero as $\lambda \to 0$ or one as $\lambda \to \infty$. While correlation estimates are thus expected to increase with λ over age, they should, in contrast to the elasticity, remain attenuated also at later ages.

Our empirical findings support these implications. Correlation estimates suffer from strong life-cycle bias at young age but remain comparatively stable beyond age 30, under both left- (Figure 1) and both-side (Figure 2) measurement error. Interestingly, correcting for attenuation bias, while indeed addressing attenuation, also escalates life-cycle effects. Equation (5) provides an explanation: the reliability ratio tends to rise with λ, such that the classical errors-in-variables model understates this ratio at late ages.

4.3 The rank correlation

Denote the ranks (normalized to the unit interval) of x^* and y^* in their respective distribution by $\tilde{x}^* = F_x(x^*)$ and $\tilde{y}^* = F_y(y^*)$. Denote observed ranks by

(6) $$\tilde{x} = \tilde{x}^* + \tilde{u}$$

(7) $$\tilde{y} = \tilde{y}^* + \tilde{v},$$

where $\tilde{u}$, $\tilde{v}$ are the errors in ranks. The Spearman rank correlation,

(8) $$\rho^S_{(x^*,y^*)} = \frac{Cov(\tilde{x}^*, \tilde{y}^*)}{\sqrt{Var(\tilde{x}^*)Var(\tilde{y}^*)}},$$

measures the extent to which one variable tends to increase with the other, without requiring that relationship to be linear.

Classical measurement error attenuates log-linear measures through its effect on the variance of observed incomes, but the variances of observed and true ranks are by definition equal. However, even errors that are random in the underlying values generate non-classical errors in ranks: as top (bottom) ranks cannot be overstated (understated), the correlation between errors in ranks and true ranks is negative.[8] Formally, we have $Var(\tilde{y}^*) = Var(\tilde{y}) = Var(\tilde{y}^*) + Var(\tilde{v}) + 2Cov(\tilde{y}^*, \tilde{v})$, and thus $Cov(\tilde{y}^*, \tilde{v}) = (-1/2)Var(\tilde{v})$.

A textbook errors-in-variables model is therefore not suitable for ranks. To capture the relationship between observed and true ranks we instead adapt the model proposed by Haider and Solon (2006) to formulate a generalized errors-in-variables model in ranks. Let

(9) $$\tilde{y} = \alpha + \lambda_y \tilde{y}^* + \tilde{w},$$

[8] For example, if y^* and v follow independent normal distributions with $y^* \sim N(\mu, \sigma^2)$ and $v \sim N(0, \sigma_v^2)$, then the rank of y^* is $\Phi\big((y^* - \mu)/\sigma\big)$ while its expected rank in the observed distribution equals $\Phi\left((y^* - \mu)/\sqrt{\sigma^2 + 2\sigma_v(\sigma_v + \sigma)}\right)$. The expected rank is below (above) the true rank if the latter is greater (smaller) than 0.5. This argument is related to the case with classification errors in binary variables (Aigner, 1973).

where λ_y is the slope coefficient in a linear projection of $\tilde{y}$ on $\tilde{y}^*$. By construction, $\widetilde{w}$ is uncorrelated to $\tilde{y}^*$, and we maintain the simplifying assumption that it is likewise uncorrelated to $\tilde{x}^*$. From the definition of ranks it follows that $\lambda_y \leq 1$. Let λ_{x} denote the analogous slope coefficient in a projection of $\tilde{x}$ on $\tilde{x}^*$. As noted above, Haider and Solon proposed this generalization of the textbook model to capture a particular form of non-classical measurement error. In ranks this generalization is more suitable than the textbook model even when the underlying errors in log values *are* classical.

Under errors on the left side, we have

$$(10) \qquad \rho^S_{(x^*,y)} = \frac{Cov(\tilde{x}^*, \tilde{y})}{\sqrt{Var(\tilde{x}^*)Var(\tilde{y})}} = \rho^S_{(x^*,y^*)}\left(1 + \frac{Cov(\tilde{y}^*, \tilde{v})}{1/12}\right) = \rho^S_{(x^*,y^*)}\lambda_y \,,$$

where we substituted equations (7) and (9).[9] As $Cov(\tilde{y}^*, \tilde{v}) < 0$, the rank correlation is understated. The case with right-side errors is analogous, and with errors on both sides we have

$$(11) \qquad \rho^S_{(x,y)} = \rho^S_{(x^*,y^*)}\left(1 + \frac{Cov(\tilde{y}^*, \tilde{v})}{1/12} + \frac{Cov(\tilde{x}^*, \tilde{u})}{1/12}\right) + \frac{Cov(\tilde{u}, \tilde{v})}{1/12} = \rho^S_{(x^*,y^*)}\lambda_y\lambda_x.$$

As the Pearson correlation, the Spearman correlation is thus subject to attenuation bias from errors in both parental and offspring income. But is it less or more attenuated? While rank correlations are less sensitive to outliers in the tails of the distribution, it does not necessarily follow that they are less sensitive to measurement error.[10] However, the probability density function of the errors in log income is negatively skewed in our data (cf. Guvenen et al., 2013), with frequent extreme (low-income) observations in the far left tail. Their influence is limited in ranks but large in logs.[11]

[9] Note that $Var(\tilde{y}^*) = Var(\tilde{y}) = Var(\tilde{x}^*) = Var(\tilde{x}) = 1/12$ by construction.
[10] For example, if y^* and x^* are normally distributed, and observations close to the mean suffer most from measurement error, then the rank correlation can be more attenuated than the log-linear measures.
[11] We have performed different types of regression diagnostics. First, we ran jackknife estimations for each measure. Indeed, the range of elasticity estimates, across replications when excluding one observation at a time, is more than fifteen times larger than the corresponding range for the rank correlation. Second, we find that the resulting observation-specific influences (dfbetas) are stable across the distribution of errors in ranks, but very large in the lower part, low in the middle, and large in the top of the distribution of errors in logs.

Rank correlations are thus expected to be less attenuated than log-linear measures, as confirmed in Figure 1 and Figure 2.

While the Pearson correlation is invariant to a linear spread in income profiles over age, the rank correlation is invariant to any rank-preserving spread. We may thus expect that life-cycle bias is even less problematic for the Spearman than for the Pearson correlation. The results in Figure 1 and Figure 2 support this argument. As the other measures, the rank correlation is strongly underestimated at young ages, as those with high lifetime income tend to have low income (ranks) at the beginning of their career. However, the rank correlation is less affected by life-cycle effects at later ages.

Equations (10) and (11) also point towards a procedure to reduce attenuation bias in rank correlations. As errors in ranks are negatively correlated to true rank, standard methods do not apply.[12] However, if the errors in the underlying log values are classical, the resulting bias can be captured by the slope coefficient of the generalized errors-in-variables model in ranks from eq. (9), and can be estimated if two income observations are available for each person. The slope coefficient from a regression of an income rank observation $\tilde{y}_1$ on another observation $\tilde{y}_2$ equals

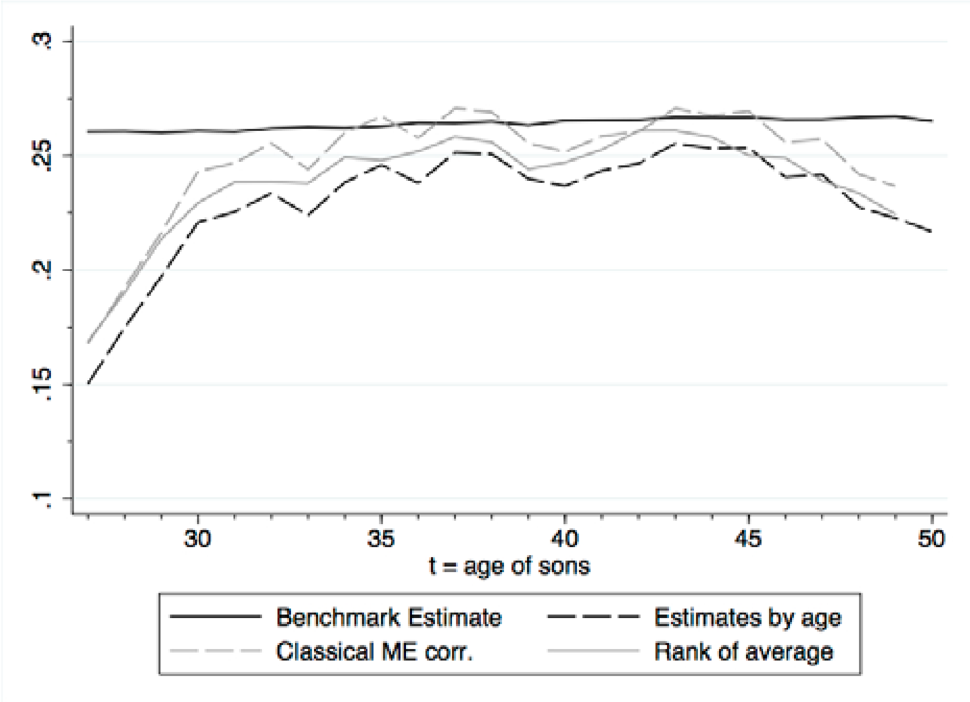
$$\frac{Cov(\tilde{y}_1, \tilde{y}_2)}{Var(\tilde{y}_2)} = \frac{Cov\left(\lambda_y \tilde{y}^* + \tilde{w}_1, \lambda_y \tilde{y}^* + \tilde{w}_2\right)}{Var(\tilde{y}_2)} = \lambda_y^2 + \frac{Cov(\tilde{w}_1, \tilde{w}_2)}{1/12}.$$

If the underlying errors in logs are classical then the errors $\tilde{w}_1$ and $\tilde{w}_2$ are uncorrelated, such that the square of the slope coefficient can be used to eliminate bias. Right-side or both-side measurement error can be treated accordingly. Of course, in practice the errors in logs are unlikely to be classical, and possibly serially correlated (see Mazumder, 2005), such that this correction will not eliminate attenuation in practice. However, as shown in Figure 3, we find that its application based on consecutive income observations still reduces the bias on average by more than 50 percent between age 30 and 50. A formal correction can thus produce better results than simple averages of the available observations, as is also the case for log-linear measures (see Solon, 1992). The correction procedure performs well also in the case of both-side measurement error

[12] For example, instrumenting the regressor in a regression of observed rank $\tilde{y}$ on $\tilde{x}_1$ by a second observation $\tilde{x}_2$ estimates $(\lambda_y/\lambda_x)\rho^S_{(x^*,y^*)}$, eliminating bias only if $\lambda_y = \lambda_x$.

(Figure 2), but does not address the bias at younger ages, where non-classical errors from life-cycle effects are the main concern.

Figure 3: Correction for classical errors in the rank correlation (LHS ME)



Note: The figure plots the benchmark estimate based on lifetime incomes for both generations against estimates based on annual income of sons (varying by age). Using two consecutive income observations, we compare estimates based on the rank of the two-year average with estimates that are based on the formal correction procedure that we describe in Section 4. The sample and thus the benchmark estimates vary slightly over age as we drop persons with missing annual observations also for estimation of the benchmark.

4.4 Transition matrices and the copula of parent and child incomes

To further characterize the joint distribution of parent and child income, researchers often estimate transition matrices that capture mobility across quantile-based income classes (Zimmerman, 1992; Jäntti et al., 2006). Interest often centers on specific elements, for example on the persistence in the very top or bottom (e.g., “poverty traps”), or on the probability to rise from the lowest to the highest class (“rags to riches”). With sufficient data we can distinguish more classes and thus approximate the copula, the joint distribution of parent and child percentile ranks $r = 1, \ldots, 100$. As the copula is a key determinant of any mobility measure (see Chetty et al. 2014b, 2014a), knowledge about its estimability in short income data will be valuable in a wide range of applications.

Assume that only a short-run income measure is observed for each offspring. Using the law of total probability, we can rewrite the probability that its rank is equal to i conditional on the true parent rank being j as

$$P(\tilde{y} = i|\tilde{x}^* = j) = P(\tilde{y} = i|\tilde{x}^* = j, \tilde{y}^* = i)P(\tilde{y}^* = i|\tilde{x}^* = j) + \sum_{r \neq i} P(\tilde{y} = i|\tilde{x}^* = j, \tilde{y}^* = r)P(\tilde{y}^* = r|\tilde{x}^* = j).$$

Assuming that the error in log offspring income is classical, we have $P(\tilde{y} = i|\tilde{x}^*, \tilde{y}^*) = P(\tilde{y} = i|\tilde{y}^*)$. Bayes theorem implies that $P(\tilde{y} = i|\tilde{y}^* = i) = 1 - \sum_{r \neq i} P(\tilde{y} = i|\tilde{y}^* = r)$, and thus

(12) $$P(\tilde{y} = i|\tilde{x}^* = j) = P(\tilde{y}^* = i|\tilde{x}^* = j) + \sum_{r \neq i} P(\tilde{y} = i|\tilde{y}^* = r)[P(\tilde{y}^* = r|\tilde{x}^* = j) - P(\tilde{y}^* = i|\tilde{x}^* = j)],$$

where the first right-hand term is the true probability and the second term is the bias.

For illustration, consider a transition probability on the main diagonal ($i = j$), such that

(13) $$P(\tilde{y} = i|\tilde{x}^* = i) - P(\tilde{y}^* = i|\tilde{x}^* = i) = \sum_{r \neq i} P(\tilde{y} = i|\tilde{y}^* = r)[P(\tilde{y}^* = r|\tilde{x}^* = i) - P(\tilde{y}^* = i|\tilde{x}^* = i)],$$

The terms in square brackets are always negative if $P(\tilde{y}^* = i|\tilde{x}^* = i) > P(\tilde{y}^* = r|\tilde{x}^* = i)\ \forall r \neq i$, that is, if the probability for offspring to have a certain rank is largest when their parent has the same rank. This assumption appears reasonable and does indeed hold in our data, as shown below. We thus find $P(\tilde{y} = i|\tilde{x}^* = j) < P(\tilde{y}^* = i|\tilde{x}^* = j)\ \forall i = j$: the diagonal elements of a transition matrix are understated under classical errors. It follows that off-diagonal elements are on average overstated, as was the case for the bottom-to-top transition probability considered in Figure 1 and Figure 2. Moreover, we may expect considerable differences within each category. The bias will tend to be larger in the corners of the transition matrix if $P(\tilde{y}^* = i|\tilde{x}^* = i)$ is particularly large for $i = 1$ (where ranks are bounded from below) or $i = 100$ (ranks bounded from above), and $P(\tilde{y}^* = r|\tilde{x}^* = i)$ decreases in the difference between r and i. And given the frequent occurrence of low-income observations in annual incomes we expect that *long-distance* transitions such as top to bottom are overestimated to a larger degree.

These theoretical predictions suggest that the observed robustness of rank correlations may not necessarily extend to other rank-based measures. We can test each of them by estimating the copula of parent and offspring incomes. Analysis of the full distribution is data intensive. Our sample size can be increased ten-fold if we measure parental incomes from 1968 instead of 1960, such that father's incomes are observed only from age 41 onwards (see Section 2). For the non-linear analysis this trade-off is worthwhile, in particular since income ranks are robust to monotone spreads of income profiles over the life-cycle.[13]

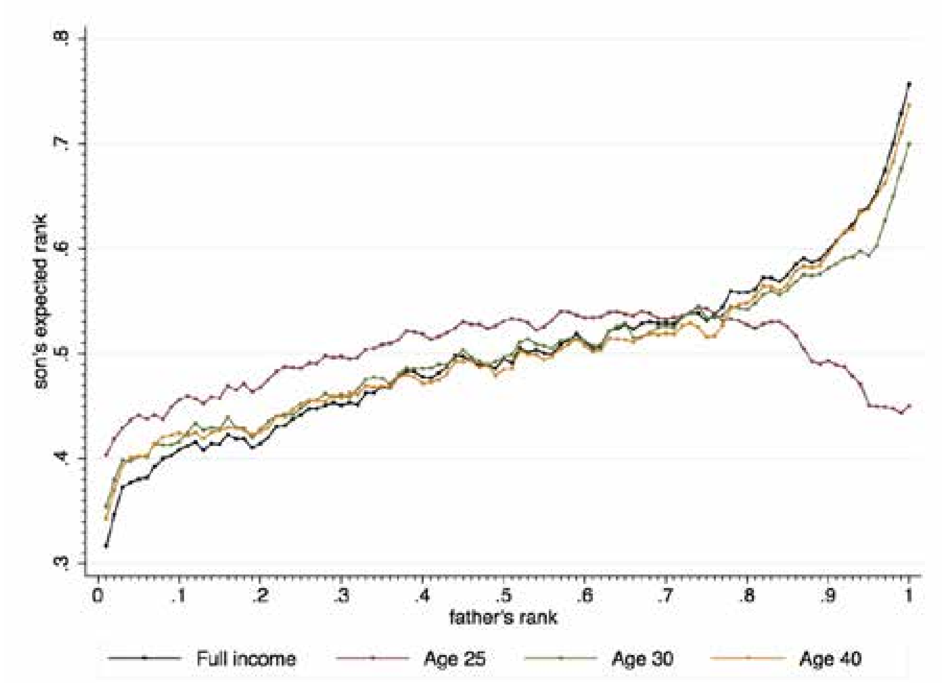
Consider first if usage of annual income observations misrepresents the conditional expectation of offspring rank. Figure 4 plots the benchmark estimate of the conditional expectation (black line) against estimates based on annual income for sons at ages 25, 30 and 40 (panel A) and annual incomes for both fathers and sons (panel B). In contrast to the pattern reported in Chetty et al. (2014a), the CEF is not linear in our data. Their argument that rank-rank are advantageous to log-log relationships because only the former are linear may therefore not extend to other populations or income definitions. Parent ranks are very consequential at the very top and bottom of the distribution. For example, average offspring rank increases from the 71st to above the 75th percentile when moving from the second-highest to the top percentile of parents; the increase is similarly steep when moving from the lowest to the second-lowest percentile. The CEF has a much lower slope in between the tails, pointing to particular mechanisms of income transmission among the very poorest and richest.[14]

[13] In our data, the correlation of "complete" lifetime measures of income rank over age 25-65 with shorter measures over age 33-60 is about 0.96, and about 0.91 over age 41-60.

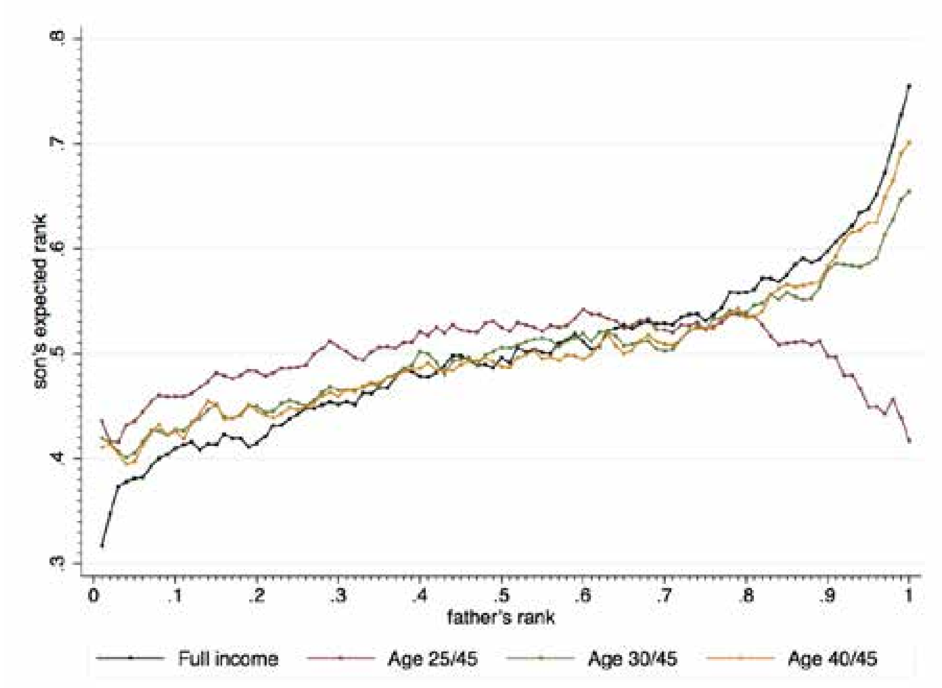
[14] One particular example is Corak and Piraino (2010), who illustrate that the transmission of employers is remarkably high in the very top in Canada. Björklund et al. (2012) also find that the intergenerational elasticity is very high among the top 0.1 percent in Sweden.

Figure 4: Son's expected rank by father's rank, benchmark vs. annual income

(a) Left-side measurement error



(b) Both-side measurement error



Note: Figures plot son's expected rank in the income distribution conditional on father's rank. The series are based on sons' and fathers' lifetime income (black line), sons' annual and fathers' lifetime income (left-side measurement error, panel A) or fathers' income at age 45 (both-side measurement error, panel B). We distinguish between estimates based on son's income at age 25 (red lines), age 30 (green lines), and age 40 (yellow lines). For ease of exposition we use three-year averages of annual ranks centered around these specific ages for the age-specific estimates.

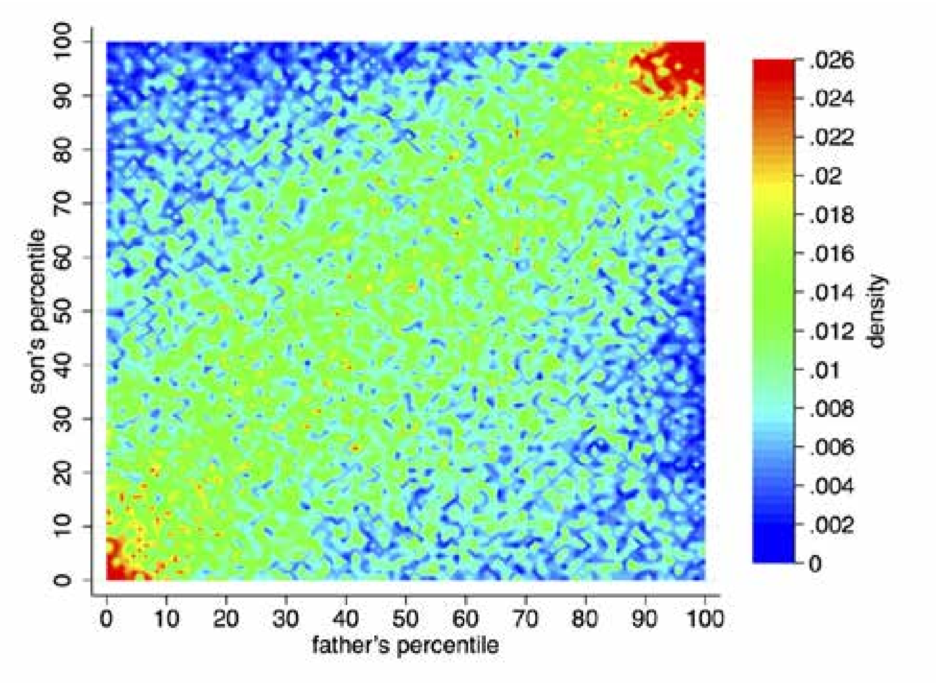
Panel A of Figure 4 shows that the CEF is poorly approximated by annual incomes at age 25, while over a wide range of percentiles it is well approximated already at age 30. However, not in the tails: average offspring rank is understated (overstated) by up to ten percent in the top (bottom) five percentiles of the parent distribution. The underestimation in the top becomes negligible when incomes around age 40 are used. In contrast, the prospects of the very poorest are consistently overestimated across all ages in our data. These patterns are amplified when annual incomes are observed for both generations. Panel B of Figure 4 plots estimates based on annual income of sons and fathers at age 45. Average offspring rank is now overstated by about 30 percent in the bottom percentiles of the parent distribution. Since even parents with high lifetime income can occasionally have very low annual incomes, it is particularly hard to identify those fathers with the lowest lifetime incomes from short income spans; as such, the CEF is particularly biased in this range.[15]

Figure 5 plots the copula of parent and offspring long-run incomes. The density is highest along the diagonal: as assumed in our derivation above, the likeliest position in the income distribution of children is the position of their parents. Since ranks are bounded, this probability is particularly high for the offspring with the very poorest and (especially) richest parents. The probability to reach the top percentile in the income distribution if the father was in the top percentile is 12.5 percent in our sample; their probability to reach the top five percentiles is above 38 percent. To yield a sensible scaling for the rest of the copula we thus censored the top percentile in Figure 5.

Figure 6 illustrates the bias that results from using annual income for sons at age 40 (Panel A) or annual incomes for both sons (at age 40) and fathers at age 45 (Panel B). To illustrate its size, the difference between the annual-based and the benchmark copula is expressed as a fraction of the cell probability that is expected under statistical independence.

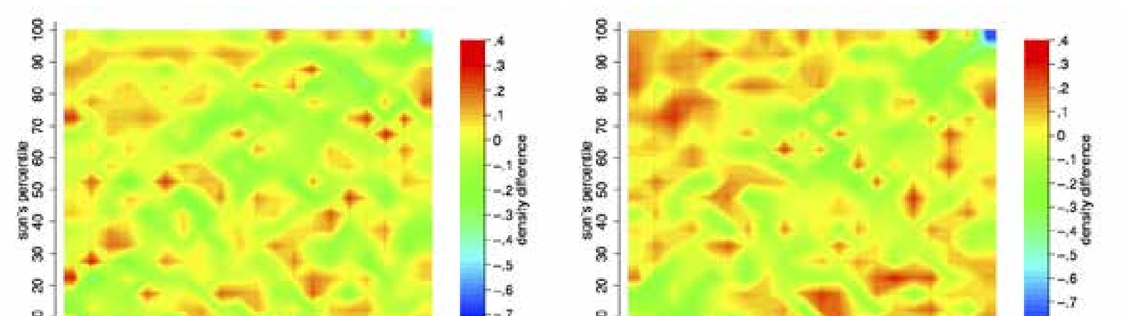
[15] In fact, the observed mean rank of sons from the lowest percentile of fathers is *higher* than the mean for the next few percentiles. The reason for such temporary low-income spans can be manifold; high income earners could choose leisure and live off their wealth, they could earn their income abroad or for other reasons avoid domestic taxation, and so on. Their frequency is thus also likely to vary across data sources (c.f. survey data and tax-based administrative data) and income definitions. In contrast, we do not occasionally observe very high annual incomes for those with low lifetime incomes, as expected.

Figure 5: Joint density of son's and father's rank (benchmark)



Note: The figure plots the copula, i.e. the joint density distribution of son's and father's income ranks (in percentiles), using lifetime incomes for both generations based on 100x100 data points, interpolated. Under statistical independence each cell has expected density 0.01 and color light green. Saturated green, yellow and red indicates excess densities, while light blue and blue indicates densities that are lower than what we would have under independence. Densities along the diagonal capture immobility, off-diagonal densities mobility.

Figure 6: Bias in joint density of son's and father's rank



Note: The figures plot the biases in the copula, i.e. the joint density distribution of son's and father's income ranks (in blocks of five percentiles each, interpolated). We use income at age 40 for sons and lifetime income (left-side measurement error, panel A) or income at age 45 (both-side measurement error, panel B) for fathers. The colored scale indicates the magnitude of the bias, i.e. the density difference compared to when lifetime incomes are used for both generations. For comparability across the copula, we scale the density difference in each 5x5 cell by the density that we would expect under statistical independence (1/400). For example, a density difference of 0.5 implies that the density was 1/800 higher in annual than in lifetime data.

A number of patterns emerge. First, usage of annual incomes leads to underestimation along and overestimation away from the diagonal – in line with our theoretical arguments. Second, the biases are largest in magnitude in the tails of the distribution.

While the inheritance of poverty and top incomes is often of particular interest, their estimates are also the most inaccurate in short data. Our findings are thus consistent with simulation results by O'Neill et al. (2007), who suggest that classical measurement error leads to overestimation of mobility in particular in the tails of the distribution. Considerably mismeasured is also the extent of long-distance downward mobility. As even sons from top-income parents have occasionally low annual incomes, the probability that offspring from top-income parents fall to the bottom is substantially overstated in annual data.[16] We thus find that the effect of measurement error on the copula has a quite logical structure. Researchers need to exercise particular caution when studying long-distance mobility, the inheritance of poverty, or the inheritance of top incomes.

5 Conclusions

We examine attenuation and life-cycle bias in four widely used measures of intergenerational dependence using nearly complete lifetime income histories of Swedish fathers and sons. As summarized in Table 1, we find that dependence (mobility) is severely understated (overstated) in all measures if snapshots of income are observed before age 30. In contrast, the degree and direction of bias differs strongly at later ages. Elasticity estimates suffer strongly from life-cycle bias. Linear correlations are more stable over age but also severely attenuated, and while corrections for classical measurement error can reduce attenuation they may escalate life-cycle effects.

Positional measures such as the rank correlation and transition probabilities fare much better. Particularly encouraging is the stability over age of rank-based estimates once incomes are observed beyond age 30. Although some attenuation bias remains, even single-year estimates understate persistence by less than 20 percent, which can be reduced further if multiple observations are available. However, as classical errors in underlying values turn non-classical once transformed into ranks, standard correction procedures do not yield unbiased estimates. We proposed an alternative correction method that reduces attenuation more efficiently than a simple averaging of income observations. Those with access to only few income observations per individual may

[16] This problem would be of even greater concern when incomes are recorded as shorter snapshots (e.g. weekly or monthly), as is the case in some survey-based data sources.

want to resort to rank-based measures, which compared to other measures are particularly advantageous in such cases. They are likewise attractive in analyses that put large requirements on the data. Such cases include the study of mobility trends and cross-country comparisons based on data sets that differ in sampling or quality.

Table 1: Attenuation and life-cycle bias in common mobility measures

	Attenuation bias			Life-cycle bias
	LHS	RHS	BHS	
Elasticity	*None*	*Large*	*Large*	*Large*
Correlation	*Moderate*	*Moderate*	*Large*	*Moderate*
Rank correlation	*Small*	*Small*	*Moderate*	*Small*
Transitions	*Small*	*Small (strong in tails)*	*Moderate*	*Small (moderate in tails)*

However, even rank-based measures can be quite inaccurate in the tails of the distribution, and those tails are often of special interest. Persistence in lifetime income is strikingly large in the very top and bottom of the distribution in our data, where a one percentile increase in father's rank can be associated with a more than four times larger rise in average offspring rank. Such nonlinearities are important; the top percentiles hold a disproportionally large share of total income in most countries, and we may care particularly about mobility among the poor, for whom small income changes can have large welfare implications. High persistence in the tails may also relate to certain mechanisms, such as the inheritance of firms, capital and employers or, on the other end, credit constraints, inheritance of long-term joblessness, poor skills, or health. Linear measures cannot capture such patterns. But our results suggest that even non-linear analyses may fail to capture them, if based on short income data.

Fortunately, the pattern of deviations across the joint distribution is quite systematic. Transition probabilities tend to be understated along the diagonal and overstated along the off-diagonal of the offspring-parent copula in annual estimates. Long-distance downward mobility can be highly overstated – sons of high-income parents do occasionally have low-income episodes, but those episodes are unlikely to extend over long spans. More generally, anyone may temporarily move into the bottom of the distribution, while the top is more persistently populated by a select few. It is thus not surprising that we find the largest bias in bottom-to-bottom transitions: the inheritance of poverty and the potential existence of “poverty traps” are likely understated in annual data.

Of course, robustness is only one factor in the choice of measures in applications. The conceptual relevance of different dependence measures also needs to be considered. The intergenerational elasticity is more “consequential” – it describes the rate at which (log) incomes regress to the mean over generations – and can be derived as a reduced-form relationship from standard models of parental investments in offspring (Becker & Tomes, 1979; Solon, 2004). Rank-based measures may instead have quite different implications depending on context and underlying income distribution. For example, moving ten percentiles in a high-inequality country like the U.S. has other welfare implications than a comparable move in Sweden. We hope our findings help researchers to evaluate the tradeoff between measurement and such conceptual considerations. At a minimum, our evidence provides a case for the use of rank-based measures as complements to or robustness tests for the more traditional measures.

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