Spillover bias in multigenerational income regressions*

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Abstract

A growing literature studies long-term income persistence across more than two generations. Despite a rich understanding of measurement-related biases for the parent-child model, far less is known for the multigenerational model that captures transmission from parents and grandparents. We show that even using a 25-year income average can result in a spurious grandparent coefficient. Importantly, for a given parental measure, averaging over more years for grandparents increases spillover bias. We propose an IV approach that can more effectively mitigate bias with shorter timespans of income. With Norwegian administrative data, we reveal a positive spillover bias in the grandfather coefficients.

Keywords: Multigenerational mobility; income mobility; measurement error; spillover bias

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Data Availability statement:

The data files of individual records from the tax registry and population registry collected can be obtained by authorized researchers according to the guidelines described http://www.ssb.no/en/omssb/tjenester-og-verktoy/data-til-forskning . There are legal limitations (the Norwegian Statistics Act) on making these data publicly available. However, summary tables of data as well as all programs and do-files used in writing the paper can be made publicly available.

1 Introduction

There has long been interest in the persistence of poverty (or privilege) across generations, leading to a large descriptive literature examining intergenerational transmission of socioeconomic status.[1] Until recently, the majority of these studies have focused on parent-child transmission. With newly available data, an emerging literature adds another generation to the parent-child model to learn more about the extent of long-term status persistence. Typically, a positive grandparent coefficient is estimated, suggesting that long-run mobility is lower than previously believed. Solon (2018) notes, however, that such a positive grandparent coefficient could arise from measurement error in parental income, a well-known econometric result. Some studies have attempted to address this using measurement strategies from the parent-child literature (e.g., averaging an outcome over more years). Despite a rich understanding of measurement-related biases in the intergenerational (parent-child) income mobility literature, far less is known about the implications for the multigenerational model that captures transmission from parents *and* grandparents. It turns out that even using income measures considered ideal for parent-child regressions may still lead to misleading interpretations of grandparent coefficients in multigenerational regressions, as we show in this paper.

Our contribution is to formally show with theory, simulations, and administrative data the role that measurement error in parent and grandparent income may play in the grandparent coefficient estimates. We highlight how small positive grandparent coefficient estimates could be inflated, and may be a consequence of measurement error. Our simulations show that even using long-term averages of income during midlife will not eliminate the possibility of estimating a spurious grandparent coefficient. Perhaps more importantly, we also show a counter-intuitive result that, for a given parental income measure (e.g., a 20-year average), improving the grandparent income measure actually inflates the spillover bias in the grandparent coefficient. This would otherwise be incorrectly interpreted as reducing attenuation

[1]See Solon (1999) and Black & Devereux (2011) for reviews of the literature on two-generation mobility.

bias. Finally, settings with lower intergenerational mobility (i.e., larger intergenerational persistence parameters) are more susceptible to this bias, which could have additional implications for cross-country comparisons. Additionally, we propose an IV approach that has the advantage of requiring a shorter timespan of incomes to minimize bias, and serves as a useful supplemental approach for gauging bias.

With administrative tax data from Norway, we provide an empirical illustration of the spillover bias in the OLS and IV estimates, showing how it inflates the grandparent coefficient in the multigenerational regression. The OLS estimate of the grandfather coefficient falls by 50% when we change the income measure for both generations from annual income to a 25-year average. When we use a 10-year average for grandfathers to isolate the spillover bias, changing only the measure for fathers still causes the grandfather coefficient to fall by 30%. The analogous results from our IV approach are similar, with the grandfather coefficients falling by 40% and 30% for the respective exercises. We also find similar results using rank correlations instead of intergenerational elasticities. Considering that we have very good administrative data, which is not susceptible to some important sources of error present in survey data, our empirical results can be considered an understatement of the potential biases.

These biases will become increasingly important as the availability of multigenerational data grows. Observing data on three generations is not trivial and has only recently become possible in some administrative datasets. The burgeoning of digitized historical records and advances in automated linking methods will surely lead to more multigenerational studies.[2]

The rest of the paper proceeds as follows. In the next section, we provide background on the existing multigenerational mobility literature.[3] In Section 3, we formalize the biases from measurement issues. We use these theoretical results to run a simulation in Section 4, which illustrates the nature of these biases in coefficient estimates from the multigenera-

[2]For example, Abramitzky *et al.* (2019), Bailey *et al.* (2017), and Price *et al.* (2019) are recent studies on advances in linking individuals across generations in historical data.

[3]Note that for both convenience and clarity we use "intergenerational" to refer to parent-child models and "multigenerational" to refer to models where grandparents are also included.

tional regression. Section 5 describes our administrative data and approach, followed by the empirical results. We provide conclusions in Section 6.

2 Background on multigenerational mobility

To see how accounting for the status of another generation may affect our estimates of persistence across generations, we begin with the basic parent-child model,

$$y_{i0} = \beta_1 x_{i1} + \epsilon_i. \tag{1}$$

y_{i0} is an outcome for a child in family i and x_{i1} is the same outcome for the parent.[4] The OLS estimate of β_1 is a summary statistic describing associations across generations. If this is the true transmission process, one could approximate the child-grandparent association with β_1^2 under some simplifying assumptions. This implies that persistence declines geometrically, so we would observe fairly rapid mobility across generations.

With the multigenerational regression equation,

$$y_{i0} = \gamma_1 x_{i1} + \gamma_2 x_{i2} + \epsilon_i, \tag{2}$$

γ_2 describes the persistence from grandparents to their grandchildren, conditional on parents, and γ_1 still describes transmission from parents (though now conditional on grandparents). A finding of γ_2=0 would confirm the model in (1) and the implied geometric decline in persistence across generations. Early studies tended to support this, as they did not find strong evidence of a conditional grandparent effect, but the datasets were often for a peculiar or non-representative sample (e.g., Hodge, 1966; Warren & Hauser, 1997).

Recent multigenerational mobility studies tend to estimate a positive grandparental coefficient, implying that parent-child estimates overstate mobility as $\gamma_2 > 0$ indicates a *slower*

[4] Intercepts are omitted to simplify presentation; the variables should be considered to be in deviation-from-mean form.

than geometric decline in persistence. While there is general agreement that a positive γ_2 can have important implications for our picture of long-term mobility relative to relying on the model in (1), there are two distinct theories on the interpretation. One interprets the positive coefficient as reflecting a "causal" grandparent effect while the other theory is that the grandparent outcome serves as an additional proxy for underlying parental latent status.

The latent status interpretation argues that the observed parental outcome measure (e.g., income) only partially captures the parent's unobserved socioeconomic status (e.g., Clark & Cummins, 2015), and the coefficient on the grandparent outcome actually reflects additional persistence in status from parents to children.[5] Braun & Stuhler (2018) estimate a multigenerational regression using education outcomes and occupational prestige, finding that adding other parental status measures substantially reduces the grandparent coefficient. The grandparent coefficient also remains largely unchanged in most subsamples with prematurely deceased grandparents. Both of these exercises support the notion that grandparents do not have a *direct* effect on grandchildren's education.[6]

Similarly, early theoretical work by Becker & Tomes (1979) arrived at the perhaps counter intuitive prediction of a negative γ_2, which implies persistence declines at a *faster* than geometric rate, or more rapid mobility. The intuition behind a negative coefficient is that if the increased income of grandparents did not raise the parents' income, this implies the parent got a poor draw on human capital endowment, and some of this is passed on to the

[5]There are a number of ways in which studies have attempted to test the theories surrounding parental latent status. Vosters & Nybom (2017) and Vosters (2018) estimate transmission of parent's latent status by aggregating information from other parental status measures to obtain a greatest lower bound on persistence and find marginally larger persistence estimates. Adermon *et al.* (2021) use a similar methodological approach but add relatives in the parent generation (e.g., aunts, uncles and their spouses), finding increased persistence but also showing that grandparents matter very little conditional on the inclusion of these relatives.

[6]Braun & Stuhler (2018) also identify an intergenerational inheritability parameter in their latent variable framework using a ratio of parent-child and grandparent-parent regression parameters to test other hypotheses of Clark (2014) and Clark & Cummins (2015). They explain that their estimates of this ratio are robust to measurement error biases as long as the error variances are constant across the appropriate generations (parent, child for one approach, and parent, grandparent in the other). This is related to our discussion in Section 3.4 on using two-generation estimates to assess mobility, though we focus on the more common practices of comparing estimates and products of estimates from child-parent, child-grandparent, and parent-grandparent regressions.

child.[7] Such negative coefficients are, however, rarely seen in empirical studies.[8]

On the other hand, there is also substantial interest in whether there is some "causal" process being captured, where grandparents matter for children's outcomes above and beyond transmission that occurs through the parents (Mare, 2011). In general, this literature is descriptive in nature, but studies have used variation in child-grandparent geographic proximity (Modalsli, 2021), timing of grandparent deaths (Braun & Stuhler, 2018; Lindahl *et al.*, 2014; Modalsli, 2021), and co-resident living arrangements (Zeng & Xie, 2014) to look for evidence of causal interpersonal mechanisms. Zeng & Xie (2014) show convincing evidence of a direct effect of co-resident grandparents on children's education in rural China. Other evidence is more mixed, consistent with a potential role for indirect influence of grandparents or with the latent status theory of mobility.

In one of the few studies able to estimate multigenerational *income* mobility regressions, Lindahl *et al.* (2015) use IV and OLS approaches to assess long-term mobility. They instrument for parent income using grandparent income, which identifies the parent-child persistence parameter under the latent status model. The large and statistically significant coefficient estimate lends support to the latent status model if the identifying assumption holds, but the authors point out that this may also reflect model misspecification of the parent-child regression. Subsequently finding a positive grandparent coefficient in the OLS estimation of the multigenerational model supports possible model misspecification and suggests that grandparents do affect children's outcomes conditional on parents.

Whether estimating a multigenerational regression out of interest in the direct effects or proxy interpretation, the resulting estimate of γ_2 is of primary interest and what we show

[7]Solon (2014) and Stuhler (2014) also adapt this theoretical framework, providing further discussion of how and why we might find a conditional grandparental effect, whether negative or positive.

[8]Lucas & Kerr (2013) use a small subset of national data on income for three generations in Finland, and find mixed results for grandparents with negative coefficient estimates though most are not statistically significant. With a unique dataset containing education outcomes for four generations of a sample of families in Malmö, Sweden, (Lindahl *et al.*, 2014) test the Becker-Tomes theory of a negative grandparent coefficient estimate by instrumenting for parental education with that of great-grandparents in a multigenerational regression and although the IV estimate for grandparents is positive in value, they cannot reject a negative alternative.

in this paper applies. If the true γ_2 is negative, for example, estimating a biased positive coefficient may actually lead us to incorrectly conclude that a model is wrong. A true positive γ_2, on the other hand, means mobility is slower than implied by the parent-child model in (1).[9] For a numerical example, consider Norway, where the true β_1 may be around 0.4.[10] In a regression where log income is the outcome (so β_1 is an intergenerational income elasticity), a child whose parents have income 50% above the mean in their generation would be expected to have income around 20% above the mean in the child's generation. Conversely, if the grandparents had income, say, 75% above the mean in their generation, and γ_2 is about 0.1 (assuming γ_1 is 0.4), would imply the child's income would be about 27.5% above the mean.

The analysis in this paper builds on the intergenerational elasticity framework that is typically used in the literature (Jantti & Jenkins, 2014). Some recent studies of intergenerational transmission in economics (e.g., Chetty *et al.*, 2014) have instead described persistence and mobility through the use of rank correlations. In such a framework, an individual's income rank (or the rank of some other characteristic) is modeled as a linear function of the parent's (or parents') rank in the parental distribution. Such an approach has been extended to multigenerational studies as well, adding the rank of grandparents to the setup (Adermon *et al.*, 2018). Translating between rank coefficients and intergenerational elasticities is not straightforward. Income inequality typically varies over time, and for this reason, the magnitude of movements in income level across generations and the magnitude of rank changes need not correspond across countries or time periods. Corak *et al.* (2014, p. 196) describe how the two measures differ, and how the rank correlation can be poorly suited to examine questions of equality of opportunity and comparisons of well-being across countries.

[9]Several other recent studies also find evidence of a positive grandparent effect. Hertel & Groh-Samberg (2014) use the Panel Study of Income Dynamics (PSID) to study persistence in occupational class in the U.S.; Modalsli (2021) uses administrative data on occupations and incomes for Norway; Long & Ferrie (2018) use wealth-based occupational status measures constructed from U.S. Census data; Boserup *et al.* (2014) estimate multigenerational persistence in wealth using Danish administrative records; Pfeffer (2014) uses the PSID to study educational mobility in the U.S.; Ferrie *et al.* (2016) further explore educational mobility in the U.S. using Census data and consider the possibility that their estimate could be a consequence of measurement error.

[10]Nilsen *et al.* (2012) find an estimate of 0.34 based on measuring income with a 15-year average, implying a potential attenuation factor of about 0.85 from Mazumder (2005); this implies $\beta_1 = 0.42$.

As we expect rank-rank regressions to become more prevalent also in the multigenerational literature, we have replicated parts of our analysis with rank correlations in addition to intergenerational elasticities. We find empirically, as well as in a simulation on synthetic data, that rank-rank coefficient estimates follow similar patterns to the IGEs. This suggests that the challenges of measurement error documented in this paper also apply when rank-rank regressions are used.

3 Biases from income measurement issues

Measurement issues have long played an important role in the descriptive mobility literature, and have received particular attention in the context of income mobility (e.g., Solon, 1992; Zimmerman, 1992; Mazumder, 2005; Haider & Solon, 2006; Nybom & Stuhler, 2014). The measurement issues stem from the fact that, although we would like to estimate the intergenerational persistence in a long-term (or lifetime) component of income, we do not observe this. Instead, we rely on observed annual incomes, either from self-reported survey data or administrative records. The sources of bias that can arise from using such measures include transitory fluctuations in annual income (which we will consider to implicitly include any measurement error in annual reports) and lifecycle variation in both the relationship between permanent and annual incomes as well as in the share of annual income variation due to the transitory components.[11] With these issues, the timing and duration of the lifespan for which we observe annual incomes are crucial to mitigating potential biases.

These measurement issues have distinct implications in the multigenerational regression, even after taking standard approaches to mitigate them. The intergenerational correlation between parents' and grandparents' permanent components of income leads to spillover of

[11] For studies relying on retrospective questions in surveys (about own income in previous periods or the economic status of parents or grandparents) the possibility of recall error introduces yet another bias. This will not be directly addressed here, as an increasing number of studies (including the present one) rely on administrative data that is collected during or shortly after the year the income is accrued. And although we focus primarily on income to be precise about the nature of measurement issues, our main results on the spillover bias generalize to other measures of socioeconomic status to some degree as well.

these biases, a standard econometric result. In many contexts, such spillover is ignored because the affected coefficient is not of interest, but the opposite is true in this case—we are primarily interested in the grandparent coefficient. This spillover bias can produce a positive coefficient estimate when the true parameter for grandparents is zero—or even negative—in the multigenerational equation in (2).

For basic intuition, first consider the simple setting where only parental income is measured with error and the measurement error is classical, but we perfectly observe grandparents' income (x_{i2}). Then the coefficient estimate on parents' income is attenuated, but the coefficient estimate on grandparents' income is actually biased upward because the underlying permanent component of parents' earnings is positively related to that of the grandparents. This intuition highlights the main point of the paper, but is obviously too simplistic a scenario as grandparents' income is also measured with error.

In this section, we show the equations for the OLS and IV estimates from the multigenerational regression, illustrating the sources of attenuation and spillover bias. We first consider the simple case of classical measurement error in section 3.1, then allow for persistence in the transitory component of income in section 3.2, and briefly discuss lifecycle effects in section 3.3. We point out the similarities to the intergenerational case and also highlight key aspects specific to the multigenerational setting.

3.1 Classical measurement error

In the simple case of classical measurement error—or classical errors-in-variables (CEV)—there are no lifecycle effects and log annual income in year t for generation g, x_{igt}, is the sum of a permanent component x_{ig} and a white noise error or transitory component v_{igt}:

$$x_{igt} = x_{ig} + v_{igt}. \tag{3}$$

Letting $g = 1$ for parents and $g = 2$ for grandparents, the CEV case also assumes that

v_{i1t} is orthogonal to v_{i2t}, so annual income is only related across generations through the permanent component of income. This relation is reflected below by $\rho \equiv corr(x_{i1}, x_{i2})$, which is the intergenerational correlation in the permanent component of income between the parent and grandparent generations. ρ thus reflects parent-grandparent persistence and is directly related to the parameter on grandparent income (call it β_2) in the respective bivariate model regressing grandparent income on parent income (specifically, $\beta_2 = \rho \frac{\sigma_{x1}}{\sigma_{x2}}$). For simplicity, we assume stationarity here such that $var(x_{i1t}) = var(x_{i2t}) = \sigma_x^2$ and $var(v_{i1t}) = var(v_{i2t}) = \sigma_v^2$, though this is relaxed in Appendix A. The probability limits of the OLS estimators from using annual income measures in the multigenerational equation (2) are:

$$plim(\hat{\gamma}_{1,OLS}) = \gamma_1 \underbrace{\frac{\sigma_x^2}{\sigma_x^2 + \sigma_v^2 \left(\frac{\sigma_x^2+\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}}_{\text{attenuation, } \theta_1} + \gamma_2 \underbrace{\frac{\sigma_x^2 \left(\frac{\rho\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}{\sigma_x^2 + \sigma_v^2 \left(\frac{\sigma_x^2+\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}}_{\text{spillover, } \omega_1} \tag{4a}$$

$$plim(\hat{\gamma}_{2,OLS}) = \gamma_1 \underbrace{\frac{\sigma_x^2 \left(\frac{\rho\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}{\sigma_x^2 + \sigma_v^2 \left(\frac{\sigma_x^2+\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}}_{\text{spillover, } \omega_2} + \gamma_2 \underbrace{\frac{\sigma_x^2}{\sigma_x^2 + \sigma_v^2 \left(\frac{\sigma_x^2+\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right)}}_{\text{attenuation, } \theta_2}. \tag{4b}$$

The probability limit for each generation's coefficient is decomposed into a linear combination of the respective true parameter times an attenuation factor (θ), *plus* the other generation's true parameter times a spillover factor (ω). For example, the form for grandparents is $plim(\hat{\gamma}_{2,OLS}) = \gamma_1\omega_2 + \gamma_2\theta_2$. In a perfect world with no measurement error ($\sigma_v^2 = 0$), both attenuation factors (θ) would be equal to one, and both spillover factors (ω) would be equal to zero, and there would be no bias.

With measurement error and a positive parent-grandparent income correlation, equation (4b) shows that even if grandparents do not have an effect on grandchildren's income conditional on parents—so $\gamma_2 = 0$ in equation (2)—the second element of $plim(\hat{\gamma}_{2,OLS})$ will be zero but the first element ($\gamma_1\omega_2$) will still be positive. In other words, despite the true $\gamma_2 = 0$, one would still estimate a positive coefficient. Further, the size of the spillover bias in $plim(\hat{\gamma}_{2,OLS})$ is largely driven by the size of γ_1 and is also increasing in ρ, so we expect it to be more substantial in countries with higher levels of intergenerational persistence.

Conversely, if the grandfather parameter γ_2 is not zero, it is likely small relative to the parent parameter γ_1, so we do not expect spillover to be a major contributor to bias in the parental coefficient estimate $\hat{\gamma}_{1,OLS}$. Rather, attenuation bias is the primary concern in the parental coefficient, and since $\left(\frac{\sigma_x^2+\sigma_v^2}{\sigma_x^2(1-\rho^2)+\sigma_v^2}\right) > 1$, the attenuation bias will be at least slightly worse than in a parent-child regression. Of course, researchers typically take averages over annual income measures to mitigate biases. This leads to replacing the σ_v^2 in equations (4a) and (4b) with σ_v^2/T, similar to what has been shown for parent-child regressions. This is also noted in Table 1, which we provide to summarize how equations (4a) and (4b) change under the different measurement scenarios and empirical approaches we consider in this section.

Table 1: Elements that take place of σ_v^2 in $plim(\hat{\gamma}_1)$ and $plim(\hat{\gamma}_2)$

Empirical approach	$v_{it} \sim$ CEV	$v_{it} \sim$AR(1)
OLS using annual income measures	σ_v^2	$\frac{\sigma_{e1}^2}{1-\delta^2}$
OLS using T-year averages of income	$\frac{\sigma_v^2}{T}$	$\frac{1}{T}\frac{\sigma_{e1}^2}{1-\delta^2}\left[1+2\delta\left(\frac{T-\frac{1-\delta^T}{1-\delta}}{T(1-\delta)}\right)\right]$
IV using annual incomes T years apart	0	$\delta^T\frac{\sigma_{e1}^2}{1-\delta^2}$

Notes: This table provides the elements that replace σ_v^2 in equations (4a) and (4b) under the empirical approaches and measurement error models we consider.

Note that in our simplified case with stationarity, the attenuation factors and spillover factors are symmetric for parents and grandparents, so $\omega_1 = \omega_2$ and $\theta_1 = \theta_2$. In theory, these could differ across generations without stationarity, and when we incorporate key features of more realistic earnings processes, as we show in Appendix A. One key result when we allow $\sigma_{x_g}^2$ and $\sigma_{v_g}^2$ to vary across generations is that averaging over more years for grandparents worsens the spillover bias in $plim(\hat{\gamma}_2)$. Algebraically, time-averaging for the grandparent implies replacing only the σ_v^2 outside of parenthesis in ω_2 with $\sigma_{v_2}^2/T_2$, which effectively shrinks the denominator thereby increasing ω_2. We revisit this result in Section 4.

What does time-averaging solve in the multigenerational regression then? Similar to the intergenerational case, it reduces the attenuation factor, but we know that some bias still remains. There is still a positive spillover factor that can cause upward bias in the other coefficient estimate—leaving open the possibility of estimating a spurious grandparent effect, even when taking long-term averages for parents and grandparents.

Under the strong assumptions of classical measurement error, instrumental variables estimation (IV) (with a valid instrument) using annual income in one year to instrument for another would yield consistent estimates. Early intergenerational studies use fathers' education to instrument for fathers' income (e.g., Solon, 1992) as well as annual income to instrument for multi-year averages (Altonji & Dunn, 1991), though both studies acknowledge the tenuousness of instrument exogeneity. In the latter approach, a valid instrument can only affect offspring income through the permanent component of the parental income average (so the transitory components cannot be correlated over time). Altonji & Dunn (1991) note that this may not hold because the IV estimates are consistent with some persistence in the transitory component of income.

More recently, studies using multigenerational data have applied IV approaches to multigenerational regressions to test both latent status and Becker-Tomes theories of causal pathways for intergenerational transmission. These approaches have used the outcome for grandparents to instrument for that for parents (Boserup *et al.*, 2014; Lindahl *et al.*, 2015) or similarly have used great-grandparents to instrument for grandparents (Lindahl *et al.*, 2014).[12]

[12] The instrument validity in these cases relies on the assumption that the grandparents' (great-grandparents') outcome does not affect the child's outcome except via the parents' (grandparents') outcome. Considering the theoretical mechanisms through which grandparents could exert a direct effect (after conditioning on parents), and the findings in recent research supporting such mechanisms (e.g., Zeng & Xie, 2014), it is unclear whether this assumption holds for the case of using a grandparent outcome to instrument for parents.

3.2 AR(1) persistence in the measurement error

The classical errors-in-variables scenario is useful for exposition and for identifying methods to reduce or eliminate bias. This is not realistic for the actual earnings process though, especially to the extent that IV using consecutive annual incomes would provide consistent estimates. One extension to the classical case is recognizing persistence in the transitory component v_{it}, assuming an AR(1) process with persistence parameter δ:

$$v_{i1t} = \delta v_{i1t-1} + e_{i1t}. \tag{5}$$

Now σ_v^2 is replaced with $\frac{\sigma_e^2}{1-\delta^2}$ in the probability limits for the OLS estimators in (4a) and (4b). Or when we use T-year averages of annual income, each σ_v^2 is replaced with $\frac{1}{T}\left(\frac{\sigma_e^2}{1-\delta^2}\right)\phi$, where ϕ is given by:[13]

$$\phi = 1 + 2\delta \frac{T - \frac{1-\delta^T}{1-\delta}}{T(1-\delta)}. \tag{6}$$

Persistence in v_{it} implies that time-averaging is less effective at mitigating attenuation bias. Put differently, it takes more years of income for averaging to achieve the same level of bias mitigation. The stronger the persistence (or the larger δ), the worse the bias is.

We propose an IV approach, on the other hand, that has the advantage of using fewer years of income to achieve a similar degree of bias reduction. The required number of years of income data—more precisely, the required time difference between two annual income measures—depends on the extent of persistence over time (δ). Again, larger δ implies more years are needed. Studies have shown that the transitory components are correlated over time, but generally disappear after about 3 years.[14] This means that annual earnings measures 4 or 5 (or more) years apart can be used to instrument for each other, as it seems reasonable to assume that the measurement errors in these years are uncorrelated with each

[13]Solon (1992) noted this more complicated scenario for the intergenerational case in footnote 17 of his paper and Mazumder (2005) subsequently examined the empirical implications.

[14]Moffitt & Gottschalk (1995) use the PSID data from 1969-87 and find that the transitory component is composed of serially correlated shocks that die out within 3 years. Using later years of the PSID, Haider (2001) notes that less than 15% of transitory shock remains after 3 years.

other and are also uncorrelated with child's earnings. Hence, our IV approach uses parental annual earnings from one year to instrument for parents' earnings in a different year, and does the same for grandparents' earnings. This approach is similar to that used by Altonji & Dunn (1991), though by not using time-averages we require fewer years of observed incomes.

The probability limits of the IV estimators for γ_1 and γ_2 are identical to equations (4a) and (4b) except that each σ_v^2 is replaced with $\delta^T \left(\frac{\sigma_e^2}{1-\delta^2}\right)$ and $T = s-t$ now denotes the number of years between the annual earnings measure used as an instrument (year s) and treated as endogenous (year t). Increasing T (years between the instrument and endogenous income measures) reduces attenuation bias, and does so at a faster rate than taking T-year time averages of income. Thus, our IV approach provides further evidence on potential spillover bias that remains in OLS estimates, and does so with a shorter time span of income, which is particularly helpful with the limited time spans available for multigenerational datasets. Of course, there are often tradeoffs with relying on few incomes. For example, if there are many zero or missing incomes, one may want to adopt a more flexible approach with regard to the distance between incomes, the particular ages at which incomes are observed, or using time-averages instead of annual incomes. While these changes may increase the data requirements, such modifications are easily adopted. We discuss in the next section and in Appendix B the potential issues that arise when shifting the endogenous or instrument income measures to different points in the lifecycle.

3.3 Lifecycle Effects

We briefly discuss lifecycle related biases, but relegate most details to Appendix B, as the biases are algebraically more complicated and not immediately necessary for the main points of the paper. One source of age-related bias is the U-shaped pattern in the size of σ_v^2, meaning it gets very large at young and old ages, and is minimized around the early 40's. If the increase in σ_v^2 is steep enough, then taking long-term averages that expand into too young or old of ages may lead σ_v^2/T to grow as one averages over more years, worsening attenuation bias. In the multigenerational case, such a scenario would also lead to larger spillover bias for larger T.

The other source of lifecycle-related bias is the age-related variation in the association between annual and permanent income. To model this lifecycle variation, equation (3) becomes $x_{igt} = \lambda_{gt} x_{ig} + v_{igt}$, where $\lambda_{gt} < 1$ at too young of ages and $\lambda_{gt} > 1$ at too old of ages (e.g., Haider & Solon, 2006). For offspring ($g = 0$), the implications are straightforward, as λ_{0t} is simply a multiplicative bias factor, meaning both coefficient estimates are biased in the same direction by the same proportion to the extent that λ_{0t} is different from one. However, lifecycle bias arising from measurement of parent and grandparent income is more complicated, with attenuation or amplification bias possible depending on whether λ_{gt} is less than or greater than one. When using T-year averages of income, then it is the magnitude of the corresponding average $\bar{\lambda}_{gT}$ that is relevant. Taking long-term averages during midlife helps to ensure that $\bar{\lambda}_{gT} \approx 1$. The implications of lifecycle bias for parents and grandparents are similar to what has been found for the intergenerational case; measuring income at too old of ages leads to downward bias or at too young of ages leads to amplification bias. These results hold for both OLS and IV, though the one distinction with IV is that it is the age at which the endogenous income is measured that matters more for bias.

3.4 Comparing estimates from two-generation regressions

While we focus on results from multigenerational regressions, it continues to be common for studies of long-term mobility to also use estimates from bivariate regressions involving two generations. The implications of measurement error for such comparisons rely on the well-established results on bias in the parent-child regression. Still, it is important to keep in mind that the “ideal” income measures for particular generations may differ when using a multigenerational regression versus a set of two-generation estimates to approximate long-term mobility or make inferences about effects of grandparents.

Two such comparisons that are interpreted as evidence of further memory than the parent-child model are findings that $\hat{\beta}_3 > (\hat{\beta}_1)^2$ or that $\hat{\beta}_3 > \hat{\beta}_1\hat{\beta}_2$, where $\hat{\beta}_2$ is the OLS estimate from a grandparent-parent regression and $\hat{\beta}_3$ that from a grandparent-child regression. Denoting the corresponding attenuation factors θ_j^* to emphasize that these are for the bivariate regressions, we can see that comparing the OLS estimates may not be strong enough evidence, even after properly accounting for estimation error.

Consider first $(\hat{\beta}_1)^2$ versus $\hat{\beta}_3$. Even if $\beta_3 = (\beta_1)^2$, we would find that $\hat{\beta}_3 > (\hat{\beta}_1)^2$ when the attenuation factors satisfy $\theta_3^* > (\theta_1^*)^2$. How likely is this to occur? Based on the preferred estimates of attenuation factors in Table 1 of Mazumder (2005), using a 10-year average for parents' income ($\theta_1^* = 0.79$ so $(\theta_1^*)^2 = 0.62$) and a 4-year (or longer) average ($\theta_3^* = 0.66$) for grandparents' income can give $\theta_3^* > (\theta_1^*)^2$, and thus $\hat{\beta}_3 > (\hat{\beta}_1)^2$. Datasets that actually have income data for three generations are likely to have limitations in line with this example, where more years of income are available for fathers than grandfathers.

For the second comparison, we could find $\hat{\beta}_3 > \hat{\beta}_1\hat{\beta}_2$, despite the true relationship being $\beta_3 = \beta_1\beta_2$, if the attenuation factors satisfy $\theta_3^* > \theta_1^*\theta_2^*$. Since the same grandparent income measure is typically used in the offspring-grandparent and parent-grandparent regressions, $\theta_3^* = \theta_2^*$, meaning any $\theta_1^* < 1$ can lead us to mistakenly conclude that $\beta_3 > \beta_1\beta_2$. Even using a 30-year average leaves an attenuation factor of 0.91 according to Mazumder (2005), so rich administrative datasets with full lifetimes of incomes for parents could still be susceptible to this misinterpretation.

We note this because it is related to constructing income measures in a way to avoid error-related biases from driving conclusions about mobility. In particular, such comparisons are often used in conjunction with multigenerational regressions to study long-term mobility.[15]

[15] Another approach to measuring intergenerational (multigenerational) mobility that has often been used by sociologists is to compute sibling (cousin) correlations in outcomes. Solon (1999) and Björklund & Jäntti (2020) provide more detailed discussions of sibling correlations and Hällsten (2014) is a recent example using cousin correlations to study multigenerational mobility. Although these methods have less onerous data requirements, they are also susceptible to the sources of bias we consider. While the two approaches do not necessarily reflect identical concepts of mobility, the comparison of cousin correlations to the squared sibling correlations is loosely analogous to comparing the child-parent estimate with a child-grandparent estimate; the respective attenuation factors could similarly distort conclusions from such comparisons. Alternatively, comparing cousin correlations with the parental outcome "removed" is attempting to isolate a conditional grandparent effect akin to that obtained in multigenerational regressions. Naturally, the resulting estimate

4 Simulation

As we showed in Section 3, the grandparent probability limit can be written $plim(\hat{\gamma}_{2,OLS}) = \gamma_1\omega_2+\gamma_2\theta_1$. This illustrates three key points. First, even if there is no conditional grandparent effect, one can still estimate a positive grandparent coefficient due the additive spillover bias ($\gamma_1\omega_2$). Second, the magnitude of the spillover bias increases with the size of the parent parameter (γ_1). Third, we saw that the spillover factor (ω_2) is increasing in ρ, the parent-grandparent correlation. Taken together these last two points indicate that societies with higher levels of intergenerational persistence are more susceptible to the spillover bias. Even with gold standard approaches of taking long-term averages of income for all generations, some bias remains, and the grandparent coefficient could still be biased upward whether the true parameter is positive, zero, or even negative. In this section, we explore the potential magnitudes of these biases.

4.1 Illustrating sizes of the attenuation and spillover bias factors

To quantify the implications of these biases in multigenerational regressions, we conduct simple simulations based on equations (4a) and (4b). We first focus on the attenuation and spillover bias factors, θ and ω, respectively, and examine how these vary with the parameters ρ and δ for OLS and IV. Recall, ρ is the parent-grandparent correlation in the permanent component of income and hence reflects different levels of intergenerational persistence in different societies. The parameter δ is the autocorrelation coefficient in the transitory component of earnings (so a value of zero corresponds to classical errors in variables), and is an important factor determining the effectiveness of using time-averaging or IV estimation to reduce attenuation bias. In Appendix B, we also present some results where we vary λ_{gt}, which reflects lifecycle variation in the association between lifetime and annual income in

is vulnerable to an upward bias similar to what we call the spillover bias, as the noisiness of the observed parental outcome can lead to overestimation of the grandparent effect. To alleviate such biases, one would need to observe many years of income, which then undoes the lessened data requirements that often makes these approaches desirable.

year t for generation g.

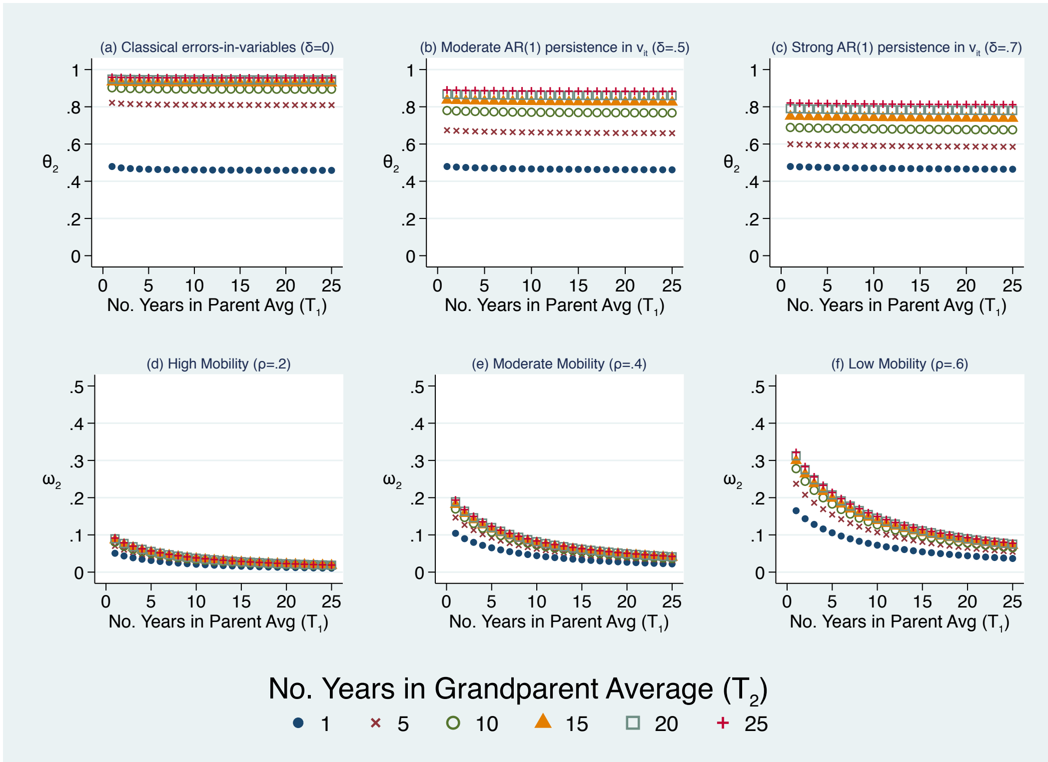
The bias factors alone allow one to gauge the extent of these biases in a variety of plausible data generating scenarios. These values also enable one to assess the likelihood of estimating a spurious grandparent coefficient in different scenarios, by also choosing potential values for the multigenerational parameters γ_1 and γ_2. We provide one such example, choosing plausible values for Norway to illustrate the pattern of estimates that may be observed in the event of a spurious grandparent effect.

We first use the results for equation (4b) to compute the bias factors for the grandparent coefficient, though with stationarity the bias factors are symmetric for the parent coefficient. We consider several different scenarios, varying δ (0, 0.5, 0.7) and ρ (0.2, 0.4, 0.6), with all $\lambda_{gt} = 1$. Multiplying the numerator and denominator of the probability limit by the total variance of annual earnings, σ^2_{xt}, allows us to simply make assumptions about the variance shares $\frac{\sigma^2_v}{\sigma^2_{xt}}$ and $\frac{\sigma^2_x}{\sigma^2_{xt}}$ to calculate the attenuation and spillover factors (θ and ω).[16] We set the variance shares at $\frac{\sigma^2_v}{\sigma^2_{xt}} = \frac{\sigma^2_x}{\sigma^2_{xt}} = 0.5$ for our base case, but also set $\frac{\sigma^2_v}{\sigma^2_{xt}} = 0.7$ for a robustness check. For a given set of these parameters, we vary the number of years over which income is averaged for parents (T_1) and grandparents (T_2) for OLS, or similarly, the number of years between the endogenous and instrument earnings measures for IV. We present results for a subset of the possible scenarios for pedagogical purposes, focusing on bias factors for the grandparent coefficient.

We consider a base case with moderate levels of both mobility ($\rho = 0.4$) and persistence in transitory shocks ($\delta = 0.5$), shown in graphs (b) and (e) of Figures 1 and 2. Each dotted line corresponds to a different T_2, so moving from one line to another corresponds to changing the grandparent income measure. Moving along a particular dotted line from left to right corresponds to increasing T_1, or improving the parental income measure.

[16] This is the same strategy taken by Mazumder (2005), and also following his strategy, we assume σ^2_e adjusts so that $\sigma^2_v = \frac{\sigma^2_e}{1-\delta^2}$ holds.

Figure 1: Attenuation factor (θ_2) and spillover factor (ω_2) in OLS coefficient for grandparent



Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the OLS probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,OLS}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

4.2 Size of bias in OLS estimates

Figure 1 shows the bias factors in the OLS estimate of the grandparent coefficient when we use time-averages of income. If no measurement error were present, the attenuation factor (θ_2) would equal one and the spillover factor (ω_2) would equal zero (so there would be no bias). With measurement error, attenuation bias worsens as θ_2 gets smaller than one and spillover bias worsens as ω_2 increases above zero.

Figures 1(a) - 1(c) provide the calculated attenuation coefficient for grandparents (θ_2) for different values of δ, with ρ fixed at 0.4. Across these graphs, we see a few important facts. First, it is the number of years averaged over for grandparents that is crucial for reducing the attenuation factor in the grandparent coefficient (and vice-versa for parents). Second, for a given grandparent income measure, taking a longer-term average for parents (going from left to right) has minimal impact, and the effect is actually in the opposite direction, very slightly worsening attenuation. Third, moving from classical measurement error to a moderate level of persistence ($\delta = 0.5$) in graph (b), we see that although time-averaging for grandparents still substantially increases the attenuation factor, it does so to a lesser extent relative to the classical case for a given T_2. In general, the degree of persistence in the transitory component of income, or δ, is key parameter in determining the size of the attenuation factor. The intergenerational correlation, ρ, has little impact on the attenuation bias, so we do not show the attenuation factors with different ρ here, though these results are available upon request.

The issue of spillover bias in the grandparent coefficient, on the other hand, is present because of ρ. In fact, the size of ρ, along with the parent coefficient γ_1, are primary determinants of the size of the overall spillover bias.[17] Graphs (d) - (f) in Figure 1 show how the spillover factor ω_2 changes with different values of ρ, now holding $\delta = 0.5$ fixed. When ρ is small (0.2), the spillover coefficient ω_2 is also somewhat small. When we triple ρ to 0.6 the extent of spillover also approximately triples. This combined with the fact that γ_1 is also likely larger for countries with larger ρ implies that OLS estimates for such societies are more susceptible to positive bias in grandparent coefficients.

There are also important patterns with regard to income measure construction. First, the key to reducing the spillover bias in the grandparent coefficient is time-averaging for the *parental* income measure. Even with a 25-year average for the grandparent income measure—which is not yet feasible in many datasets—the parental measure is still important for reducing spillover. For example, in the moderate case with $\rho = 0.4$ in 1(e), going from T_1=1 to a long-term average with T_1=25 reduces the spillover coefficient from about 20% to

[17] Although ρ and γ_1 are closely related and we expect them to generally follow similar patterns across countries, there are a few differences in what is captured in each. ρ reflects intergenerational transmission between the parent and grandparent generations and abstracts from changes in income inequality. γ_1, on the other hand, reflects transmission between the child and parent generations, conditional on grandparents. Changes in income inequality from the grandparent to parent generation would be reflected in γ_1.

about 4% when using a 25-year average for grandparents.

Second, for a given parental income measure, averaging over more years for grandparents—as we would do to reduce potential attenuation bias in the grandparent coefficient—actually worsens the spillover bias in the grandparent coefficient. Again, consider Figure 1(e), now looking at the scenario of using an annual income measure for parents (T_1=1), and going from using one year to a 25-year average for grandparents; the spillover factor doubles from $\omega_2 = 0.1$ (10%) to $\omega_2 = 0.2$ (20%). If the true $\gamma_2 = 0$, there is no attenuation to be concerned about, rather the time-averaging for grandparents is creating a spurious positive grandparent coefficient. This is illustrated later in Figure 3(b). The intuition here is that improving the grandparent measure is increasing the correlation between the two observed regressors, which results in a rise in the spillover.

Third, in countries with large ρ it takes far more years of observed parental incomes to eliminate/mitigate the spillover bias. When $\rho = 0.6$, for example, even using 30-year averages of income for parents and grandparents—which is not yet possible in any datasets we know of—the spillover bias is not eliminated.

4.3 Size of bias in IV estimates

While we see above that using time-averages with small T can leave substantial bias in OLS estimates, there are more promising results with IV estimation. Figure 2 presents the computed attenuation (θ_2) and spillover (ω_2) factors for IV estimation using an individual's annual income measure in year s to instrument for that individual's income in year t. In the figures, T_1 and T_2 now refer to the difference in years (s-t) between the instrument and endogenous measure for parents (going from left to right in each graph) and grandparents (going from one line to another), respectively. Of course, with classical measurement error in Figure 2(a), IV is consistent for any choice of income years. With AR(1) persistence in the transitory component, even though attenuation bias is again worse with larger δ, the attenuation can be nearly eliminated using income measures in a relatively short time period (up to about 10 years with high δ). As with OLS, the key to reducing the grandparent attenuation factor is increasing T_2, or improving the grandparent measure. For a moderate degree of persistence in the transitory component ($\delta = 0.5$) the attenuation factor θ_2 is about 0.65 (35% attenuation bias) when using consecutive annual income measures, and reaches about 0.99 (1%) when using a 6-year difference for grandparents.

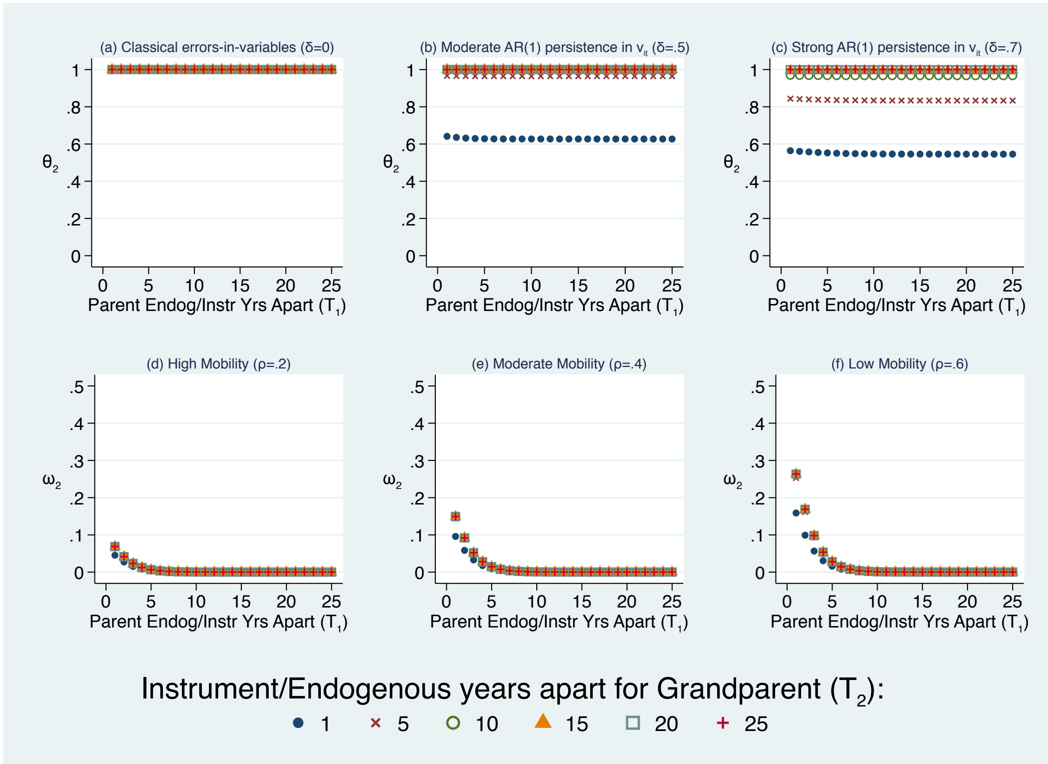
We see similarly promising results for the spillover factors in Figures 2(d) - 2(f). The spillover factor is similar to OLS when $T = 1$, with an ω_2 of approximately 0.1 (10%) with moderate mobility ($\rho = 0.4$). However, the spillover bias is nearly eliminated with only about a 6-year time span of income for parents and grandparents, giving $\omega_2 < 0.01$. Although the spillover is again worse with larger ρ, it is still eliminated with relatively short time spans of income. Also similar to OLS, it is the parental income measure that drives the spillover bias reduction, with the same result that improving the grandparental measure may worsen spillover, now only for very small T_1.

We focus most of our discussion here on the biases in the grandparent coefficient, which is often the focus of multigenerational studies. The analogous results hold for the parent attenuation and spillover factors as they are identical with the stationarity assumption, and are symmetric without stationarity (available upon request). Recall, the extent to which the spillover factor (ω_1) affects the magnitude of the parental coefficient estimate depends on the size of γ_2. Since this is presumably small relative to γ_1, spillover bias will not generally be problematic for the parent coefficient, so changing the grandparent income measure should not have appreciable impacts on our estimate of γ_1. Rather, addressing attenuation bias is the main issue for the parent coefficient, as is customary with intergenerational income regressions.

4.4 Life-cycle variation

These simulation results are enlightening for multigenerational regressions, but have abstracted from the two age-related sources of bias: the lifecycle variation in σ_v^2 and in the

Figure 2: Attenuation factor (θ_2) and spillover factor (ω_2) in IV coefficient for grandparent



Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the IV probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,IV}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

association between annual and lifetime income (λ_{gt}). The implications of the former are fairly straightforward. A larger transitory variance share means a noisier income measure, so for each number of years of income T_g, the attenuation factor is smaller (meaning worse attenuation bias). The spillover factor tends to be similar for small T, but then greater for large T. For example, if we make the transitory variation more important and set $\frac{\sigma_v^2}{\sigma_{xt}^2} = 0.7$ so $\frac{\sigma_x^2}{\sigma_{xt}^2} = 0.3$, then time-averaging over 25 years only reduces the attenuation bias to 23% (θ_2=0.77) for OLS. The spillover factor is only reduced to 0.07 (7%) with a 25-year average income measure. For IV, the implications are less extreme. At $T_g = 6$, the spillover coefficient is below 0.02 and the attenuation factor reaches 0.98.

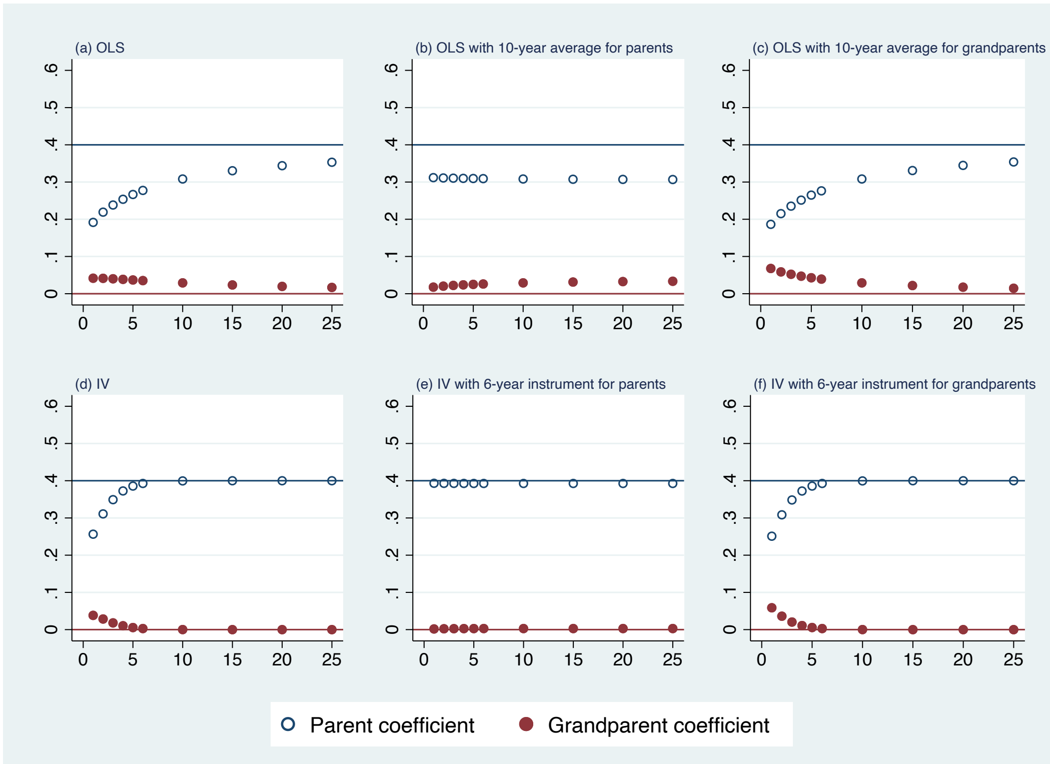
The implications of lifecycle variation in the association between annual and lifetime income for parents or grandparents, reflected by λ_{gt}, are more complicated, and we present analogous results for this in Appendix Figures B.1-B.4. The attenuation coefficient for OLS follows the same patterns found in previous studies for the intergenerational regression. Attenuation (θ_g) is worse when income is measured at older ages and potentially becomes amplification bias for younger ages. The spillover factor (ω_g) is larger for incomes at younger ages and smaller for incomes at older ages, reinforcing the attenuation or amplification bias from θ_g. Considering the combined implications of the lifecycle effects on θ_g and ω_g, the OLS coefficient estimates of γ_g are possibly biased upward when income is measured at too young of ages and likely biased downward when measured at older ages.

With IV estimation, it is the age at which the endogenous earnings is measured that drives lifecycle bias. If measured at younger ages when $\lambda_{gt} < 1$, this can result in substantial amplification bias even after increasing T_g, while older ages with $\lambda_{gt} > 1$ exacerbates attenuation bias. A simple way to test for this source of lifecycle bias in IV estimates—and potentially bound the true coefficient—is to do IV estimation twice, where the instrument and endogenous measures are reversed. For example, suppose there is an income measure from a younger age where $\lambda_{gt} < 1$ and another income measure from an older age where $\lambda_{gt} > 1$. Using the income from the younger age to instrument for the older age income can provide a lower bound (due to worsened attenuation). Exchanging these to use the older age income to instrument for the younger age income may produce an upper bound (due to possible amplification bias). Appendix B provides further discussion along with examples of this.

4.5 Illustrating a spurious grandfather coefficient

To illustrate the overall consequences of the bias factors in Figures 1-2 for the actual estimates researchers obtain, we next present figures with the corresponding coefficient estimates of γ_1 and γ_2 for our base case. The parent-grandparent intergenerational correlation (ρ) is 0.4 and there is moderate persistence in the transitory component of earnings ($\delta = 0.5$). We choose $\gamma_1 = 0.4$ and $\gamma_2 = 0$ to be the underlying population parameters as these are plausible population values for our sample from Norway and reveal the potential for a spurious grandparent coefficient in this setting.

Figure 3: OLS and IV coefficients when $\rho = 0.4$, $\delta = 0.5$, $\gamma_1 = 0.4$, $\gamma_2 = 0$



Note: This figure shows the values of OLS and IV regression coefficients using our equations for the respective probability limits. We set γ_1=0.4 and γ_2 =0, and use the attenuation and spillover factors from our simulation base case of ρ=0.4 and $\delta = 0.5$.

Figures 3(a)-3(c) provide OLS coefficient estimates from three different exercises in adjusting the income measures for parents and grandparents. The x-axis indicates the number of years used in the average income measure. To start, we treat the measures for parents and grandparents symmetrically in 3(a), increasing the number of years averaged over for both generations (so T_1=T_2 always). The coefficient estimates show that simultaneously averaging over more years for parents and grandparents both reduces attenuation bias in $\hat{\gamma}_{1,OLS}$ as well as the spillover bias in $\hat{\gamma}_{2,OLS}$. (We know there is no attenuation bias in $\hat{\gamma}_{2,OLS}$ because we set $\gamma_2 = 0$.)

Next, we isolate the effects of changing the grandparent measure in 3(b), by using what would be considered a reasonable measure for parent's income—a 10-year average. This illustrates the result that improving the grandparent income measure is causing an increase in $\hat{\gamma}_{2,OLS}$, a result that would typically be interpreted as reducing attenuation bias. In our controlled setting here, we know that this is actually increasing the size of ω_2, hence increasing the size of the spurious grandparent coefficient.

Figure 3(c) presents estimates from the opposite exercise, where we use a 10-year average for grandparents' income, but vary the number of years in the parental income average. The coefficient for parents increases as we reduce attenuation bias by averaging over more years, a standard result for parent-child regressions also. The coefficient estimate for grandparents decreases as the spillover bias is reduced, illustrating the importance of the *parental* income measure to our estimate of the *grandparent* coefficient.

Turning to the IV estimates in the three bottom graphs of Figure 3, the x-axis now indexes the difference in years between the instrument and endogenous earnings measure. First, in 3(d), we adjust the parental and grandparent income measures symmetrically, increasing the years between the instrument and endogenous measures. The rising time-distance in the instrumenting causes more dramatic reductions in attenuation bias and spillover bias in the first few years, essentially eliminating the attenuation and spillover bias around $T = 6$ years.

To gauge differential sensitivity to changing the parent *or* grandparent income instrument distance, we also do exercises analogous to those for OLS. The stable estimates in 3(e) are based on using a 6-year instrument for parents while adjusting the grandparent endogenous/instrument time distance. This shows that the parental instrument is key to mitigating biases.

Conversely, using a 6-year instrument for grandparents while increasing the time distance between the parental measures in Figure 3(f), the estimates follow a similar pattern to when both measures were changed symmetrically in 3(d). Comparing the first few grandparent estimates in 3(d) with those in 3(f) shows that using a "good" measure for grandparents

(6-year instrument) in 3(f) inflates the grandparent coefficient (worsens spillover) when we do not use a good enough instrument for parents also (i.e., for small T_1). When we are able to use a 6-year distance in instrument and endogenous income for both generations though—which is feasible in some datasets now—IV does appear to nearly eliminate bias.

In general, our simulation suggests that how parental income is measured is important for reducing biases. The parent coefficient estimates themselves follow familiar patterns from intergenerational studies, but less intuitive results arise for the grandparent coefficient. For a given *parental* income measure, improving the *grandparent* measure worsens spillover bias in the grandparent coefficient, which would otherwise be interpreted as reducing attenuation bias. Further, even using ideal long-term averages of income during midlife does not eliminate the spillover bias. For this reason, we also propose an IV approach as a supplementary exercise to gauge bias, because it has the advantage of dramatically reducing, and potentially eliminating, the biases with only about 7 years of income.

Our simulations also highlight the importance of ρ and γ_1 in the magnitude of spillover bias, which means extra caution should be taken with cross-country comparisons. Standard practice is to use identically constructed income measures in the respective countries to facilitate a valid comparison. Given our simulation results, this may not be enough to avoid misleading conclusions. A country with a larger ρ or γ_1 requires more years of parental incomes to avoid incorrectly estimating a positive grandparent coefficient when the true parameter is zero or negative. For example, if $\gamma_2 = -.05$ and $\gamma_1 = .4$ in our simulation (with $\delta = .5$ and $\rho = .4$), it would take about 10 years of parental income—for any grandparent measure—to estimate a negative value for $\hat{\gamma}_2$. If $\gamma_1 = .6$ in this scenario, then it takes 15-20 years of parent incomes to obtain a negative grandparent coefficient. If ρ is also .6, then even 30 years of parental income does not guarantee a negative $\hat{\gamma}_2$.

Finally, the results in this section are based on intergenerational elasticities (IGEs). We show in Appendix C.2 that running regressions on ranks, rather than log incomes, does not remove the potential for spillover bias. Even in rank-rank regressions where ranks are constructed based on 20-year income averages, there is potential for estimation of spurious grandfather coefficients.

While these simulation results are useful to show the nature of the biases under a known data generating process, we now turn to our administrative data to illustrate the implications of these biases in practice.

5 Data and empirical results

5.1 Data

For our empirical analysis we use administrative data from Norway. This data has a uniquely long full-population coverage of tax records, making it possible to follow individual incomes annually from 1967 onwards. We use data on labor income (*pensjonsgivende inntekt*, income that qualifies for the Norwegian public pension system). This includes wages and income from self-employment. The tax files include an individual identifying number that allows linkage to the Central Population Register, which has information on family links (fathers' and mothers' ID) for most individuals born in the 1940s or later.

The offspring generation is comprised of men born 1974-1978, with incomes measured at ages 32-36 (until 2015). This age range is selected to minimize lifecycle bias and allow for multi-year averages to reduce error variance, while also observing long time spans of their grandfathers' incomes at reasonable ages. Fathers and paternal grandfathers are identified using the population register. We use a slightly higher age range (see below) for fathers and grandfathers because of data availability and because the ages are consistent with attempting to avoid lifecycle bias based on evidence in Nilsen *et al.* (2012) for similar cohorts in Norway. To avoid sample composition differences across specifications and approaches, we present results based on a balanced sample where all three generations meet the following income requirements.

Sons must have positive income in at least three of five years from ages 32-36. The income measures are based on the log of annual labor income so we exclude observations with non-positive earnings. Included in our various constructions of earnings measures for fathers and grandfathers are averages over 2, 3, 4, 5, 6, 10, 15, 20, and 25 years. We require 3 or more years of positive earnings, but in practice there are at least 7 years of positive incomes for the longer-term averages. Our final analysis sample is comprised of 5,064 sons matched to their fathers and paternal grandfathers. Table 1 provides descriptive statistics for this sample, along with the general population weighted by the sample birth year distribution, as well as the unweighted population.

Table 2: Descriptive statistics

	Sample		Population (weighted)		Population (unweighted)	
	Men	M+W	Men	M+W	Men	M+W
Mean income	371,326	318,171	357,248	304,896	356,160	303,053
Std. dev of income	178,604	162,742	216,022	191,711	217,213	191,891
N (unique individuals)	5,064	9,831	171,939	335,155	171,939	335,155
Fathers' generation *(Birth year range: 1950-1958)*						
Mean income	281,787	284,347	267,366		269,213	
Std. dev of income	136,513	137,667	195,184		199,443	
N (unique individuals)	4,673	8,451	292,288		292,288	
Fathers' fathers' generation *(Birth year range: 1928-1935)*						
Mean income	201,850	202,197	194,142		203,323	
Std. dev of income	70,656	70,299	90,290		96,932	
N (unique individuals)	4,455	7,790	164,825		164,825	

Notes: The sample is comprised of individuals in the 1974-1978 birth cohorts with income in at least 3 years during ages 32-36, with fathers and grandfathers fulfilling the income requirements described in the text. Incomes shown are at age 34 for the index generation and at age 40 for the father and grandfather generations. Income is CPI-adjusted (1998 NOK; 1 NOK = 0.13 USD). Birth year ranges for fathers and grandfathers refer to the 5th and 95th percentile of the birth year distribution.

The average labor income of sons in our sample in the year they turn 34 (during 2004-2008) is 371,326 NOK (inflation adjusted to 1998). This is slightly higher than the population average shown in the second set of columns. One possible reason for the discrepancy is the role of immigrant background. Immigrants do in general have lower incomes than natives, and because of the strict requirement that both fathers' and grandfathers' identities are known in the registers, there are very few immigrants in our data set. The distribution of incomes (as measured by the standard deviation) is also somewhat lower in our sample than for the full population.

The fathers in our sample were born in the 1950s, so the corresponding "population" information is for all men born in the same period (weighted by the distribution of birth years in the sample), regardless of whether they have children. The slightly lower mean income in the general population is likely a reflection of the fact that lower-income men have a lower probability of starting a family. We see a similar difference in the distribution of grandfathers, born in the late 1920s and early 1930s. The birth year distribution of grandfathers is more skewed than that of fathers. Because grandfathers have to be born after 1928 in order to be young enough to have an observed income at age 39 (in 1967 when the income data start), we cut off a tail of older grandfathers while there is still a tail of younger grandfathers born in the 1930s. This also means that the average father-son age difference in our sample is likely to be lower than in the general population.

Although it would be nice to have a larger and unquestionably representative sample for Norway, it is not necessary for one of our primary purposes in this paper—to illustrate how bias from income measurement can inflate the grandparent coefficient or even produce a spurious grandparent effect. For this, it is most important to maintain a balanced sample across methods to avoid sample composition issues driving different patterns in our results. Additionally, we present results for males only. The tendency to omit females (especially mothers and grandmothers) from intergenerational income analyses arises in large part from female labor force participation patterns and the inability to observe outcomes. In our case, given that the rationale for our methodological choices is based on earnings processes for males, it is appropriate to focus on sons, fathers and grandfathers in our analysis.[18]

[18] The results for samples including daughters are similar, and are discussed below (Section 5.5).

5.2 Empirical approach

Focusing on the multigenerational model in equation (2), we estimate a series of regressions to examine the conditional association between grandfathers' income and their grandchildren's income and the underlying implications of income measurement issues. For all regressions, the dependent variable is the 5-year average of log income for offspring over ages 32-36, and we include dummy variables for the index generation's year of birth. Using log income (or averages of log income) as our income measures means the coefficients have the interpretation of intergenerational income elasticities (IGEs).

To examine the effects of income measurement choices on our estimates, we vary the estimation method as well as the measures we use for fathers and grandfathers. We first use OLS with annual log income measures, and then proceed to average over 2 - 6 years of annual log income, as well as 10-, 15-, 20-, and 25-year averages for longer-term measures, centering all of these averages around age 43 to minimize lifecycle bias. Next, we turn to IV estimation using annual log income measures 2 - 6 years apart as the instrument and endogenous regressors. Although the advantage of our IV approach is bias reduction in this short time span, we also extend to 10 years for a longer time distance between incomes. Since lifecycle bias in IV is driven by the age of the endogenous measure, we measure this at age 43, and measure the instrument income at age $43{+}T$.

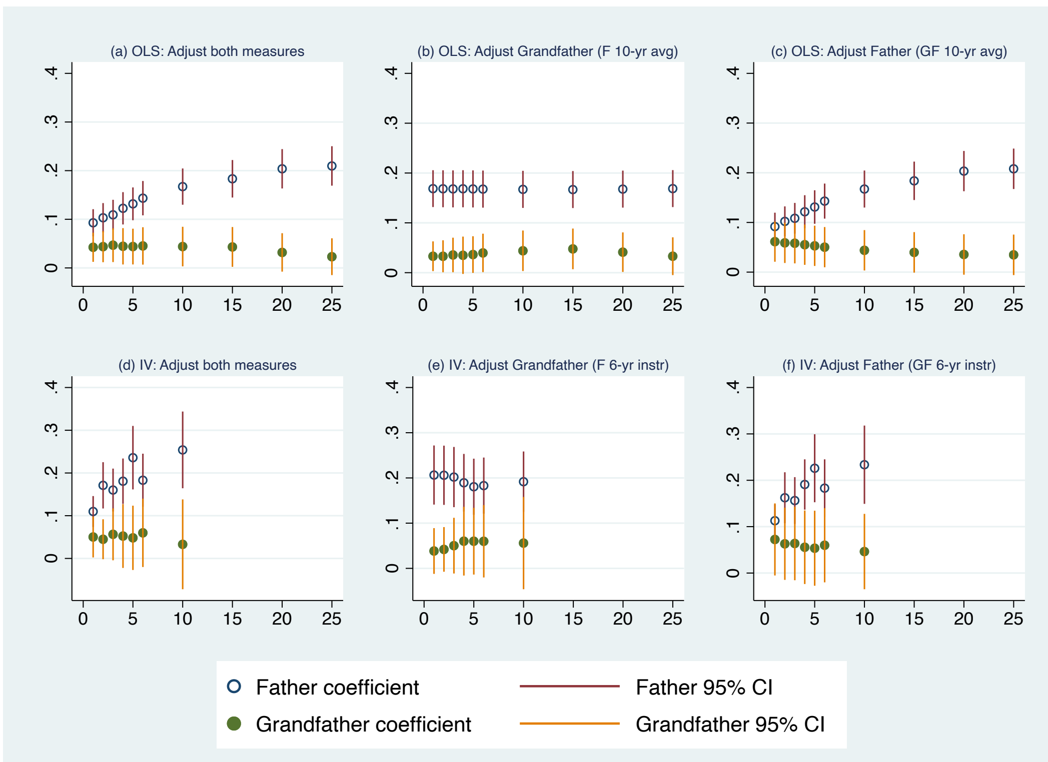
To clearly illustrate the bias spillover implications, we also vary the income measures separately for fathers and grandfathers as done in the simulations. First, we consider the case where we have a "good" measure of fathers' income—a 10-year average of log income—and then vary how grandfathers' income is measured as described above, using OLS to estimate the models. Second, we do the same exercise using the 10-year average of grandfathers' log income, but varying how fathers' income is measured.

Next, we use analogous approaches with IV estimation. We first isolate the effects of changing the grandfather IV approach by using the 6-year instrument for fathers while varying that for grandfathers. Then we illustrate the spillover bias in the grandfather coefficient by using the 6-year instrument for grandfathers while varying the instrument for fathers.

5.3 OLS results

The results from our multigenerational regressions are presented in Figure 4. All results are based on our balanced sample of 5,064 sons matched to their fathers and paternal grandfathers, unless otherwise noted.

Figure 4: OLS and IV estimates from three-generation regressions



Note: This figure shows the OLS and IV coefficient estimates and 95% confidence intervals from a series of multigenerational regressions. For OLS, the x-axis indexes the number of years used in the average income measure for the generation(s) for which the measure is being adjusted. For IV, this is instead the number of years between the instrument and endogenous incomes (measured at age 43).

5.3.1 Improving fathers' and grandfathers' income measures

First, we consider the effects of time-averaging by simultaneously increasing the number of years that we average over for fathers' and grandfathers' income. With two competing biases, it is less clear what we should expect for the grandfather coefficients in this setting. There is attenuation bias from measurement error in "own" income, yet there is an upward bias from measurement error in fathers' income. For fathers we expect the coefficient to increase, as reducing attenuation bias will outweigh any changes in spillover bias. Empirically this is what we find in Figure 4(a), as the coefficient estimate on fathers' income increases from about 0.09 to 0.21 as we average over more years. These estimates are very similar to father-son estimates for this sample (shown in Appendix E). The coefficient on grandfathers' income fluctuates around 0.04-0.05 for regressions with 1-15 year averages of income. The estimates decrease slightly to 0.03 and 0.02 for the 20- and 25-year measures, constituting a 50% reduction in the grandfather coefficient when going from a 1-year to 25-year average. However, these decreases in the 20- and 25-year estimates could also reflect lifecycle bias (e.g., growth in σ_v^2/T) as the average extends into older ages.

Given the stability of the father coefficient estimates it appears that there is little or no spillover bias in the father coefficients from the measurement error in the grandfather income. This is also consistent with a very small grandfather coefficient in the population. To disentangle the two sources of bias (attenuation from own income measure versus amplification from the other generation's income measure), we next present results where we change only one generation's income measure at a time.

5.3.2 Improving grandfathers' income measure

We now use a "good" measure of fathers' income (10-year average) throughout all models, while changing grandfathers' income measure as before. On one hand, this isolates the effect of changing the grandfather income measure on the coefficient estimate for fathers. The OLS coefficient estimate for fathers remains essentially constant at 0.17, the same value as the 10-year estimate in 4(a), as we go from using annual income to longer-term averages for grandfathers' income. This implies there is no detectable spillover bias in the coefficient estimate for fathers, which is consistent with the true γ_2 being zero (or very small).

On the other hand, this also illustrates the effect on the grandfather coefficient of improving the grandfather measure. The grandfather coefficient increases slightly as we average over more years. We would tend to interpret this as from reducing attenuation bias, but our simulation showed that improving the grandfather measure (for a given father measure) would also increase the spillover bias in the grandparent coefficient. However, here we cannot distinguish this from decreasing attenuation bias since we do not know the true population parameters.

5.3.3 Improving fathers' income measure

Figure 4(c) presents results from varying fathers' income measures while holding the measure for grandfathers constant at a 10-year average ($T_2 = 10$). By using a "good" measure of income for grandfathers, we can isolate the attenuation in the coefficient for fathers, and, more importantly for this setting, the spillover of bias into the coefficient for grandfathers. As expected, the coefficient for fathers increases from about 0.09 to about 0.21 as we average over more years, in line with our results from symmetrically improving income measures for fathers and grandfathers in 4(a).

For grandfathers, the coefficients are decreasing as we improve the income measure for fathers, showing the reduction in spillover bias. The grandfather coefficient estimate falls by 30%, from 0.06 to 0.04, when we change from using an annual to long-term average for fathers.

Taken together, Figures 4(a)-4(c) show how influential the parental income measure is in determining the results of a multigenerational regression. Having a long-term measure for grandfathers but an annual or short-term measure for fathers would lead to misleading results on long-term mobility. Fortunately, this is an unlikely scenario for many datasets, though is something to be mindful of as researchers use historical datasets that are limited in the number of incomes observed for an individual.

5.4 IV results

Our IV approach has the advantage, at least theoretically in our simple simulation, of nearly eliminating bias in this setting when T_g is large enough for the degree of persistence in v_{itg} (e.g., after 6-10 years in our simulation). The bottom graphs in Figure 4 present the IV results from exercises analogous to those for OLS above.

5.4.1 Improving fathers' and grandfathers' income measures

First, in 4(d) we increase the distance between years measured for endogenous and instrument income measures as indicated on the x-axis, simultaneously for fathers and grandfathers. The coefficient for fathers' income increases from 0.11 to 0.25 as we increase the number of years between the endogenous and instrument income measures, similar to the father-son IV results.[19] The coefficient for grandfathers' income fluctuates around 0.04-0.06 for the 1-6 year measures, falling by 40% from 0.05 at 1 year to 0.03 at 10 years, and is not statistically significant for the 2+ year estimates.[20] In general, the pattern of increasing father coefficients clearly indicates mitigating attenuation bias. The pattern for grandparents is not quite as clear, though the smaller coefficient from the 10-year estimate does suggest that spillover bias from poor income measures for fathers led to an upward bias in the grandfather coefficient.

5.4.2 Improving grandfathers' income measure

We vary fathers' and grandfathers' income measures separately to more carefully examine spillover bias. We first use the 6-year instrument as a "good" measure for fathers' income while changing the instrument for grandfathers' income. The pattern of results is similar to the analogous OLS results, with the coefficient on fathers' income remaining steady around

[19] For all IV estimations, the Kleibergen-Paap F-statistics confirm that our first stage is sufficiently strong ($F \geq 32$ in all regressions, and $F = 112$ on average for regressions with income at age 43 as the endogenous measure).

[20] The samples were slightly reduced again as T_2 increased, with the following sample sizes for T_2=3,4,5,6, and 10 year estimates; N=4,859 (96%), N=4,686 (93%), N=4,617 (91%), N=4,548 (90%), and N=4,221 (83%), respectively. However, replicating all of Figure 4 for this smaller sample reveals similar patterns, so sample composition does not appear to be driving this small estimate.

0.18-0.20. The coefficient for grandfathers is never statistically significantly different from zero, but does increase from 0.03 to 0.05 as we increase T_2, improving the grandparent income measure. This increase could be the consequence of reducing the attenuation factor or increasing the spillover factor, as shown in our simulation.

5.4.3 Improving fathers' income measure

We isolate the effects of measurement issues arising from fathers' income measures. Using a "good" measure for grandfathers' (the 6-year instrument) in all estimations, we vary the instrument for fathers' income. In Figure 4(f) the coefficient on fathers' income rises from 0.11 to 0.24 as we increase the years between the endogenous and instrument income measures from 1 to 10 years, which is nearly identical to the IV results in 4(d). Although the coefficient on grandfathers' income fluctuates, on average we do see it decrease as we improve the measure for fathers' income, falling by about 30%, from 0.07 to 0.05.

5.5 Robustness checks

5.5.1 Including women

The main analysis is conducted with only men in all three generations. This allows us to focus on a single lineage and avoid measurement issues related to the relatively low labor force participation of women in the initial two generations. However, for the offspring generation, there are fewer differences between men and women. Figure 4 is replicated in the Appendix for the full sample with both men and women in the youngest generation (Figure D.7). In general, the coefficients are slightly lower than for men only, and slightly more precise. The reduced level reflects generally lower intergenerational persistence typically found for samples including women, while the improved precision follows from the increased sample size. The patterns in the coefficients are nearly identical to our results based on sons only.

5.5.2 Income ranks

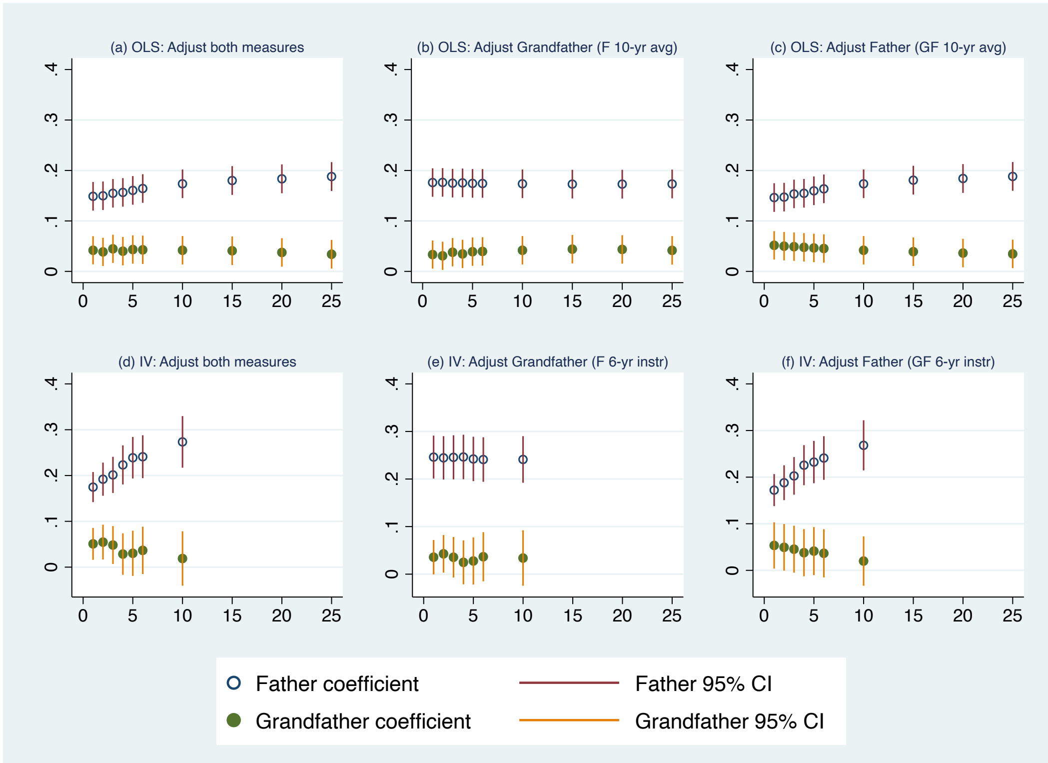
As rank correlations have been increasingly used in intergenerational income mobility studies, we replicate our analysis here using ranks of average income measures for all generations. Despite the inherent non-classical measurement error in income ranks, the sensitivities of the rank correlations tend to follow patterns very similar to the IGE, typically with less extreme degrees of bias (e.g., Nybom & Stuhler, 2017), which is what we find in our sample as well.[21]

We present estimates from regressions using income ranks in Figure 5. There is less spillover bias than found with the IGE, but it is still an issue as illustrated by the decreasing grandparent coefficients in 5(c). We see increasing grandparent coefficients in Figure 5(b) which again is a combination of reduced attenuation (if $\gamma_2 > 0$) and worsening spillover bias. When ranks of long-term averages are used, the coefficient estimates are very similar in magnitude to the corresponding IGEs, and suffer less attenuation bias with few years of income. The IV estimates also follow similar patterns, exhibiting more stability in that they fluctuate less than the IGE, but are generally higher in magnitude.

The robustness of rank correlations could be beneficial for multigenerational studies, especially those limited in years of income. If, for example, only one year of income is available for fathers, the rank-rank estimates are likely preferable as this is still the more important measure for bias. There appears to be less attenuation in the father coefficient and less spillover in the grandfather coefficient relative to the IGEs. If longer-term averages are possible, the rank-rank and IGEs are very similar, so the choice between the two measures remains a conceptual one.

[21] Studying biases in rank correlations algebraically is complicated because even purely random error in underlying incomes leads to non-classical measurement error in income ranks. Following the approach of Nybom & Stuhler (2017) and Haider & Solon (2006) to capture the non-classical error with a linear projection of the observed rank on true rank leads to OLS probability limits that reflect the same patterns we show for the IGE. Most notably, it is still the parental income measure that is most influential to biases, and improving the grandparent measure for a given parental measure worsens spillover in the grandparent coefficient (see Appendix C.1).

Figure 5: OLS and IV estimates from three-generation rank-rank regressions



Note: This figure shows the OLS and IV coefficient estimates and 95% confidence intervals from a series of multigenerational regressions. For OLS, the x-axis indexes the number of years used in the average income measure for the generation(s) for which the measure is being adjusted. For IV, this is instead the number of years between the instrument and endogenous incomes (measured at age 43).

6 Discussion

Overall, our OLS and IV results suggest that the true grandfather coefficient for our sample is very small, or possibly even zero. The OLS estimates based on longer-term income averages are around 0.02-0.03. The IV estimate based on the long-term instruments is about 0.03. None of the longer-term estimates are statistically significantly different from zero, although the IV estimates are quite imprecise. More importantly, the patterns in our empirical results mirror those found in the simulation, especially for OLS. Our IV estimates vary more widely, perhaps due to sensitivity to lifecycle effects, and also suggest persistence in the transitory component of income matters. Although the IV estimates are imprecise, they do provide a useful secondary empirical exercise for gauging bias. In general, our results show that empirically researchers must be aware of how sensitive the grandfather coefficient is to the construction of the *parental* income measure, and the tendency for the estimates to be positively biased.

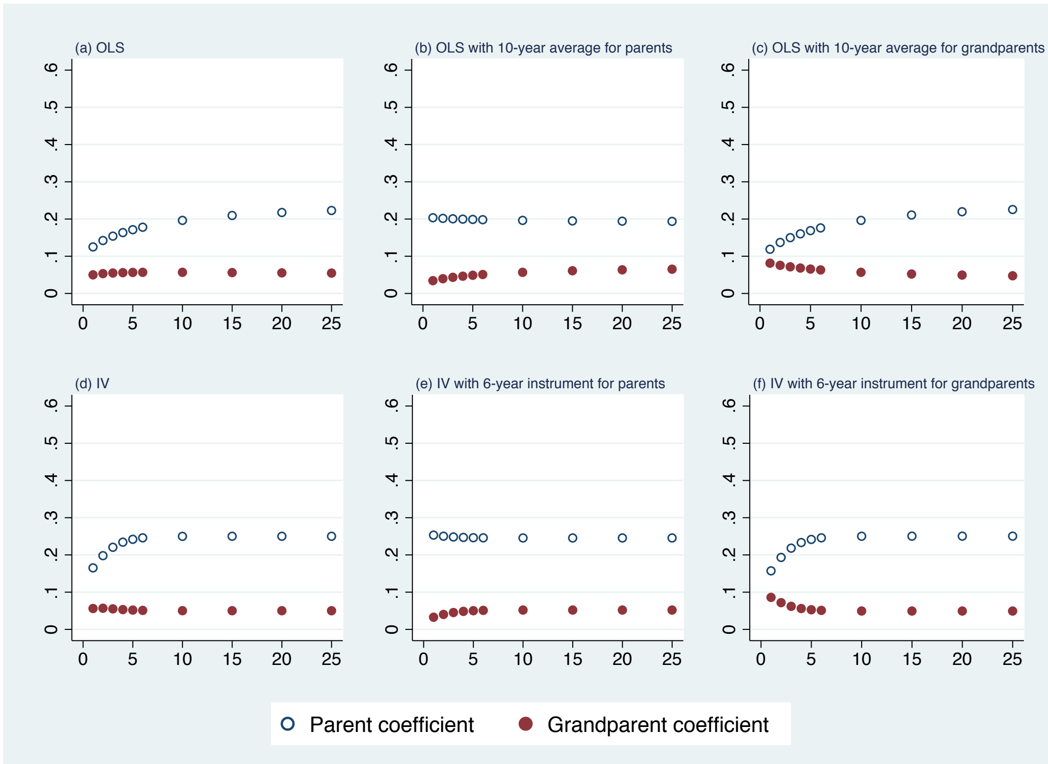
6.1 Relating the empirical results to the simulation

We observe several *patterns* in our empirical results that closely follow the hypothetical scenario presented with our simulation where we set $\gamma_1 = 0.4$ and $\gamma_2 = 0$. However, as far as *magnitude* is concerned, all of our OLS and IV coefficient estimates for fathers are well below 0.4, with the largest IV or OLS estimates ranging 0.21-0.25. As for the grandfather coefficient, the OLS estimates are potentially consistent with $\gamma_2 = 0$, as the smallest estimate from using the longest-term averages is 0.02. However, we noted this could be due to lifecycle bias, as a decline is also observed in the son-grandfather specifications for these longer-term averages. Fortunately, our IV approach exploits a shorter time span, avoiding this potential life cycle bias.

Considering our IV results, one slight divergence from our simulation results is that when we use the 6-year instrument for fathers across all specifications—which theoretically should nearly eliminate spillover bias—we see the grandfather coefficient fluctuating and slightly increasing from about 0.03 to about 0.05. In the other two sets of IV results the estimates also do not decline as dramatically as in our simulation example with $\gamma_2 = 0$.

In light of the IV results, it appears our empirical results could reflect a scenario with $\gamma_1 = 0.25$ and $\gamma_2 = 0.05$. To illustrate this, we recreate simulation Figure 3 with these revised population parameters in Figure 6. Now comparing these estimates with our empirical results, both the patterns and magnitudes are more closely aligned. This is not meant to be conclusive evidence that these are the population parameters for Norway, or even for our sample. Rather, we wish to point out that our IV approach is a useful supplementary exercise to the usual OLS estimates that researchers obtain.

Figure 6: OLS and IV coefficients from simulation when $\rho = 0.4$, $\delta = 0.5$, $\gamma_1 = 0.25$, $\gamma_2 = 0.05$



Note: This figure shows the values of OLS and IV regression coefficients using our equations for the respective probability limits. We set γ_1=0.25 and γ_2 =0.05, and use the attenuation and spillover factors from our simulation base case of ρ=0.4 and $\delta = 0.5$.

6.2 Concluding comments

This paper illustrates the implications of measurement error in the multigenerational setting. The spillover of bias from measurement error in the parents' income measures could lead to misleading conclusions regarding the effects of grandparents and our general understanding of long-term mobility. Our simulations show that even using a long-term average of income over 25 years during mid-life does not eliminate the potential for estimating a spurious grandfather coefficient. In addition, even when the true grandparent coefficient is zero, for a given measure of fathers' income, increasing the years we average over for grandfathers actually worsens the spillover bias. If we observe increasing coefficient estimates as a result, this could be misinterpreted as reducing attenuation bias in actual data settings. The IV approach we propose has the advantage of theoretically mitigating (or eliminating) these biases with relatively short timespans of income, depending on the degree of persistence in the transitory component of income.

With our administrative data, we show how the spillover of bias from measurement issues in fathers' income causes upward bias in the coefficient for grandfathers' income. With a 10-year average income measure for grandfathers, improving only the fathers' income measure from an annual measure to a 15-year average reduces the grandfather coefficient by 30%. We find a similar degree of bias reduction in our IV estimates, that require only two income measures in a 7- and 10-year timespan for grandfathers and fathers, respectively. In fact, our OLS results based on averaging over log incomes indicates that spillover bias may be causing a spurious grandfather coefficient estimate. Our IV approach is consistent with this, and although the estimates are imprecise, the approach has the advantage of avoiding some potential lifecycle bias in the OLS estimates.

Our empirical results are based on very good administrative data and in a setting with relatively low levels of intergenerational persistence. Our results should thus be considered a lower bound on the potential bias. Cross-country comparisons are further complicated by the fact that countries with higher levels of intergenerational persistence will be susceptible to substantially larger spillover biases. This could distort comparative conclusions even when income measures are constructed uniformly across countries. Although we focused on measurement issues with income in this paper, it is well known that measurement issues arise with all other status measures used as well. Some of the theoretical results presented here are based on models specific to earnings dynamics, but the approximately classical measurement error case is more broadly applicable. The issue of spillover bias from measurement issues is not unique to income and should be taken into consideration in any multigenerational regression setting.

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Appendix (for online publication)

A Derivations

The following provides derivations of the probability limits shown in the main text of the paper, though here we do not assume stationarity as done in the paper. This means that $\sigma^2_{x_g}$ and $\sigma^2_{v_g}$ are allowed to vary across generations ($g = 1, 2$).

In the population, the true multigenerational process is:

$$y_{i0} = \gamma_1 x_{i1} + \gamma_2 x_{i2} + \epsilon_i. \tag{7}$$

We observe annual earnings measures, x^*_{it1} for fathers and x^*_{it2} for grandfathers:

$$x^*_{it1} = x_{i1} + v_{it1}, \tag{8a}$$

$$x^*_{it2} = x_{i2} + v_{it2}. \tag{8b}$$

So the equation we estimate with our data is:

$$y_{i0} = \gamma_1 x^*_{it1} + \gamma_2 x^*_{it2} + \epsilon^*_{it}. \tag{9}$$

A.1 OLS estimation

We can derive the OLS estimator of γ_1 using the Frisch-Waugh-Lovell theorem and some algebra:

$$\begin{aligned}
\hat{\gamma}_{1,OLS} &= (x_1^{*'} M_2 x_1^*)^{-1} x_1^{*'} M_2 y && \text{(10a)}\\
&= [x_1^{*'}(I - x_2^*(x_2^{*'} x_2^*)^{-1} x_2^{*'}) x_1^*]^{-1} x_1^{*'}(I - x_2^*(x_2^{*'} x_2^*)^{-1} x_2^{*'}) y && \text{(10b)}\\
&= [x_1^{*'} x_1^* - x_1^{*'} x_2^*(x_2^{*'} x_2)^{-1} x_2^{*'} x_1^*]^{-1} [x_1^{*'} y - x_1^{*'} x_2^*(x_2^{*'} x_2^*)^{-1} x_2^{*'} y] && \text{(10c)}\\
&= \left[\sum_{i=1}^{N} x_{i1}^{*2} - \sum_{i=1}^{N} x_{i1}^* x_{i2}^* \sum_{i=1}^{N} x_{i2}^{*2} \sum_{i=1}^{N} x_{i2}^* x_{i1}^*\right]^{-1} \left[\sum_{i=1}^{N} x_{i1}^* y_i - \sum_{i=1}^{N} x_{i1}^* x_{i2}^* \sum_{i=1}^{N} x_{i2}^{*2} \sum_{i=1}^{N} x_{i2}^* y_i\right] && \text{(10d)}\\
&\vdots\\
\hat{\gamma}_{1,OLS} &= \frac{\sum_{i=1}^{N} x_{i1}^* y_i \sum_{i=1}^{N} x_{i2}^{*2} - \sum_{i=1}^{N} x_{i1}^* x_{i2}^* \sum_{i=1}^{N} x_{i2}^* y_i}{\sum_{i=1}^{N} x_{i1}^{*2} \sum_{i=1}^{N} x_{i2}^{*2} - \left(\sum_{i=1}^{N} x_{i1}^* x_{i2}^*\right)^2} && \text{(10e)}
\end{aligned}$$

Similarly, for γ_2, we get:

$$\hat{\gamma}_{2,OLS} = \frac{\sum_{i=1}^{N} x_{i2}^* y_i \sum_{i=1}^{N} x_{i1}^{*2} - \sum_{i=1}^{N} x_{i1}^* x_{i2}^* \sum_{i=1}^{N} x_{i1}^* y_i}{\sum_{i=1}^{N} x_{i1}^{*2} \sum_{i=1}^{N} x_{i2}^{*2} - \left(\sum_{i=1}^{N} x_{i1}^* x_{i2}^*\right)^2} \tag{11}$$

Taking the probability limits gives us:

$$plim(\hat{\gamma}_{1,OLS}) = \frac{cov(y, x_1^*) var(x_2^*) - cov(y, x_2^*) cov(x_1^*, x_2^*)}{var(x_1^*) var(x_2^*) - cov(x_1^*, x_2^*)^2} \tag{12a}$$

$$plim(\hat{\gamma}_{2,OLS}) = \frac{cov(y, x_2^*) var(x_1^*) - cov(y, x_1^*) cov(x_1^*, x_2^*)}{var(x_1^*) var(x_2^*) - cov(x_1^*, x_2^*)^2} \tag{12b}$$

Now we substitute equations (8a) and (8b) and use assumptions underlying classical errors-in-variables (CEV): x_1 and x_2 are orthogonal to v_1 and v_2 as well as orthogonality between v_1 and v_2. For notation, we define $\sigma_{x_g}^2 \equiv var(x_{ig})$ and $\sigma_{v_g}^2 \equiv var(v_{itg})$ for $g = 1, 2$

and $\rho \equiv corr(x_1, x_2)$. Then the elements of the probability limits are:

$$var(x_g^*) = \sigma_{x_g}^2 + \sigma_{v_g}^2 \tag{13a}$$

$$cov(x_1^*, x_2^*) = \rho\sigma_{x_1}\sigma_{x_2} \tag{13b}$$

$$cov(y, x_1^*) = \gamma_1\sigma_{x_1}^2 + \gamma_2\rho\sigma_{x_1}\sigma_{x_2} \tag{13c}$$

$$cov(y, x_2^*) = \gamma_2\sigma_{x_2}^2 + \gamma_1\rho\sigma_{x_1}\sigma_{x_2} \tag{13d}$$

Substituting these into (12a) and (12b) and rearranging gives us:

$$plim(\hat{\gamma}_{1,OLS}) = \gamma_1 \frac{\sigma_{x_1}^2}{\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\sigma_{x_2}^2+\sigma_{v_2}^2}{\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} + \gamma_2 \frac{\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_2}^2}{\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)}{\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\sigma_{x_2}^2+\sigma_{v_2}^2}{\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} \tag{14a}$$

$$plim(\hat{\gamma}_{2,OLS}) = \gamma_1 \frac{\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_1}^2}{\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)}{\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\sigma_{x_1}^2+\sigma_{v_1}^2}{\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)} + \gamma_2 \frac{\sigma_{x_2}^2}{\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\sigma_{x_1}^2+\sigma_{v_1}^2}{\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)} \tag{14b}$$

Although assuming that the transitory components are sources of classical measurement error does lend to the simplicity of these probability limits, it is generally believed that there is some persistence in the v_{itg} over time. So we can write the AR(1) process for the v_{it} where δ is the autocorrelation coefficient,

$$v_{itg} = \delta v_{it-1g} + e_{it}. \tag{15}$$

With this process for v_{itg}, each $\sigma_{v_g}^2$ is replaced with $\frac{\sigma_e^2}{1-\delta^2}$ in the probability limits above. Or when we use T-year averages of annual income, each $\sigma_{v_g}^2$ is replaced with:

$$\frac{1}{T_g}\frac{\sigma_e^2}{1-\delta^2}\left[1 + 2\delta\left(\frac{T_g - \frac{1-\delta^{T_g}}{1-\delta}}{T_g(1-\delta)}\right)\right]. \tag{16}$$

A.2 Instrumental variables (IV) estimation

Our IV approach uses log annual earnings in year s (z^*_{isg}) to instrument for log annual earnings in year t (x^*_{itg}) for that individual. So, in addition to equations (8a) and (8b) above, we have for our instruments:

$$z^*_{is1} = x_{i1} + v_{is1}, \tag{17a}$$

$$z^*_{is2} = x_{i2} + v_{is2}. \tag{17b}$$

We define $A_2 = I - x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2$, and again use the Frisch-Waugh-Lovell theorem and some algebra to derive the IV estimators:

$$\hat{\gamma}_{1,IV} = (z^{*'}_1 A_2 x^*_1)^{-1} z^{*'}_1 A_2 y \tag{18a}$$

$$= [z^{*'}_1 (I - x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2) x^*_1]^{-1} z^{*'}_1 (I - x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2) y \tag{18b}$$

$$= [z^{*'}_1 x^*_1 - z^{*'}_1 x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2 x^*_1]^{-1} [z^{*'}_1 y - z^{*'}_1 x^*_2 (z^{*'}_2 x^*_2)^{-1} z^{*'}_2 y] \tag{18c}$$

$$= \left[\sum_{i=1}^{N} z^*_{i1} x^*_{i1} - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \left(\sum_{i=1}^{N} z^*_{i2} x^*_{i2}\right)^{-1} \sum_{i=1}^{N} z^*_{i2} x^*_{i1}\right]^{-1} \left[\sum_{i=1}^{N} z^*_{i1} y_i - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \left(\sum_{i=1}^{N} z^*_{i2} x^*_{i2}\right)^{-1} \sum_{i=1}^{N} z^*_{i2} y_i \right] \tag{18d}$$

$$\vdots$$

$$\hat{\gamma}_{1,IV} = \frac{\sum_{i=1}^{N} z^*_{i1} y_i \sum_{i=1}^{N} z^*_{i2} x^*_{i2} - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \sum_{i=1}^{N} z^*_{i2} y_i}{\sum_{i=1}^{N} z^*_{i1} x^*_{i1} \sum_{i=1}^{N} z^*_{i2} x^*_{i2} - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \sum_{i=1}^{N} z^*_{i2} x^*_{i1}} \tag{18e}$$

Similarly, for γ_2, we get:

$$\hat{\gamma}_{2,IV} = \frac{\sum_{i=1}^{N} z^*_{i2} y_i \sum_{i=1}^{N} z^*_{i1} x^*_{i1} - \sum_{i=1}^{N} z^*_{i2} x^*_{i1} \sum_{i=1}^{N} z^*_{i1} y_i}{\sum_{i=1}^{N} z^*_{i2} x^*_{i2} \sum_{i=1}^{N} z^*_{i1} x^*_{i1} - \sum_{i=1}^{N} z^*_{i2} x^*_{i1} \sum_{i=1}^{N} z^*_{i1} x^*_{i2}} \tag{19}$$

Taking the probability limits we get:

$$plim(\hat{\gamma}_{1,IV}) = \frac{cov(z_1^*, y)cov(z_2^*, x_2^*) - cov(z_1^*, x_2^*)cov(z_2^*, y)}{cov(z_1^*, x_1^*)cov(z_2^*, x_2^*) - cov(z_1^*, x_2^*)cov(z_2^*, x_1^*)} \tag{20a}$$

$$plim(\hat{\gamma}_{2,IV}) = \frac{cov(z_2^*, y)cov(z_1^*, x_1^*) - cov(z_2^*, x_1^*)cov(z_1^*, y)}{cov(z_2^*, x_2^*)cov(z_1^*, x_1^*) - cov(z_2^*, x_1^*)cov(z_1^*, x_2^*)} \tag{20b}$$

Now we substitute equations (8a), (8b), (17a), and (17b) and use assumptions underlying classical errors-in-variables (CEV): x_1 and x_2 are orthogonal to v_1 and v_2; v_{it1} and v_{it2} are uncorrelated; v_{itg} and v_{isg} are uncorrelated. For notation, we define $\sigma_{x_g}^2 \equiv var(x_{ig})$ and $\sigma_{v_g}^2 \equiv var(v_{itg})$ for $g = 1, 2$ and $\rho \equiv corr(x_1, x_2)$, allowing us to write the elements of the probability limits as:

$$cov(z_g^*, x_g^*) = \sigma_{x_g}^2 + cov(v_{isg}, v_{itg}) = \sigma_{x_g}^2 \tag{21a}$$

$$cov(x_1^*, z_2^*) = cov(x_2^*, z_1^*) = \rho\sigma_{x_1}\sigma_{x_2} \tag{21b}$$

$$cov(y, z_1^*) = \gamma_1\sigma_{x_1}^2 + \gamma_2\rho\sigma_{x_1}\sigma_{x_2} \tag{21c}$$

$$cov(y, z_2^*) = \gamma_2\sigma_{x_2}^2 + \gamma_1\rho\sigma_{x_1}\sigma_{x_2} \tag{21d}$$

Substituting these into the probability limits in (20a) and (20b), and then doing some algebra shows that $plim(\hat{\gamma}_{1,IV}) = \gamma_1$ and $plim(\hat{\gamma}_{2,IV}) = \gamma_2$. However, if we consider the case of an AR(1) process for v_{itg}, then (21a) does not hold. Rather, $cov(v_{isg}, v_{itg}) = \delta^{T_g}\frac{\sigma_{e_g}^2}{1-\delta^2}$ where $T_g = t - s$ is the years between the earnings measures x_{itg} and z_{isg}. In this case, the probability limits of the IV estimators are the same as those for the OLS estimators in (14a) and (14b) except that $\sigma_{v_g}^2$ is replaced with $\delta^{T_g}\frac{\sigma_{e_g}^2}{1-\delta^2}$.

Table A.1 summarizes what takes the place of $\sigma_{v_g}^2$ for $g = 1, 2$ under the two different scenarios for the transitory component (CEV or AR(1)) for each of our estimation approaches.

Table A.1: Elements that take place of $\sigma^2_{v_g}$ in $plim(\hat{\gamma_1})$ and $plim(\hat{\gamma_2})$

Estimation method	$v_{itg} \sim$ CEV	$v_{itg} \sim$AR(1)
OLS using annual income measures	$\sigma^2_{v_g}$	$\frac{\sigma^2_{e1}}{1-\delta^2}$
OLS using T_g-year averages of income	$\frac{\sigma^2_{v_g}}{T_g}$	$\frac{1}{T_g}\frac{\sigma^2_{e1}}{1-\delta^2}\left[1+2\delta\left(\frac{T_g-\frac{1-\delta_g^T}{1-\delta}}{T_g(1-\delta)}\right)\right]$
IV using annual incomes T_g years apart	$n.a.$	$\delta^{T_g}\frac{\sigma^2_{e1}}{1-\delta^2}$

B Lifecycle Effects

B.1 Derivations: lifecycle effects in multigenerational regression

For the multigenerational regression, we consider lifecycle profiles in income for fathers and grandfathers separately, where the relationship between annual and lifetime or permanent income is written

$$x^*_{it1} = \lambda_{1t}x_{i1} + v_{it1}, \tag{22a}$$

$$x^*_{it2} = \lambda_{2t}x_{i2} + v_{it2}. \tag{22b}$$

Considering again the probability limits in equations (14a) and (14b) in Appendix A with the same orthogonality conditions, we can use the equations in (22a) and (22b) to write the elements of the probability limits as:

$$var(x^*_g) = \lambda_{gt}\sigma^2_{x_g} + \sigma^2_{v_g} \tag{23a}$$

$$cov(x^*_1, x^*_2) = \lambda_{1t}\lambda_{2t}\rho\sigma_{x_1}\sigma_{x_2} \tag{23b}$$

$$cov(y, x^*_1) = \lambda_{1t}\gamma_1\sigma^2_{x_1} + \lambda_{1t}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \tag{23c}$$

$$cov(y, x^*_2) = \lambda_{2t}\gamma_2\sigma^2_{x_2} + \lambda_{2t}\gamma_1\rho\sigma_{x_1}\sigma_{x_2} \tag{23d}$$

Then the OLS probability limits in equations (14a) and (14b) are now:

$$plim(\hat{\gamma}_{1,OLS}) = \gamma_1 \frac{\lambda_{1t}\sigma_{x_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\lambda_{2t}^2\sigma_{x_2}^2+\sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} + \gamma_2 \frac{\lambda_{1t}\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)}{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\lambda_{2t}^2\sigma_{x_2}^2+\sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} \quad (24a)$$

$$plim(\hat{\gamma}_{2,OLS}) = \gamma_1 \frac{\lambda_{2t}\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)}{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\lambda_{1t}^2\sigma_{x_1}^2+\sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)} + \gamma_2 \frac{\lambda_{2t}\sigma_{x_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\lambda_{1t}^2\sigma_{x_1}^2+\sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)}. \quad (24b)$$

The equations for our instruments can now be written:

$$z_{is1}^* = \lambda_{1s}x_{i1} + v_{is1}, \quad (25a)$$

$$z_{is2}^* = \lambda_{2s}x_{i2} + v_{is2}. \quad (25b)$$

With IV estimation, if we assume the v_{itg} are essentially white noise error, then the elements of the probability limits are:

$$cov(z_g^*, x_g^*) = \lambda_{gt}\lambda_{gs}\sigma_{x_g}^2 + cov(v_{isg}, v_{itg}) = \lambda_{gt}\lambda_{gs}\sigma_{x_g}^2 \quad (26a)$$

$$cov(x_1^*, z_2^*) = \lambda_{1t}\lambda_{2s}\rho\sigma_{x_1}\sigma_{x_2} \quad (26b)$$

$$cov(x_2^*, z_1^*) = \lambda_{2t}\lambda_{1s}\rho\sigma_{x_1}\sigma_{x_2} \quad (26c)$$

$$cov(y, z_1^*) = \lambda_{1s}\gamma_1\sigma_{x_1}^2 + \lambda_{1s}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (26d)$$

$$cov(y, z_2^*) = \lambda_{2s}\gamma_2\sigma_{x_2}^2 + \lambda_{2s}\gamma_1\rho\sigma_{x_1}\sigma_{x_2}. \quad (26e)$$

The probability limits of the estimators are:

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1\frac{1}{\lambda_{1t}} \quad (27a)$$

$$plim(\hat{\gamma}_{2,IV}) = \gamma_2\frac{1}{\lambda_{2t}}. \quad (27b)$$

This illustrates the fact that it is the age at which the endogenous income measure is observed that drives the direction and magnitude of the bias in the IV estimates.

With an AR(1) process for v_{itg}, the elements of the IV probability limits can be written:

$$cov(z_g^*, x_g^*) = \sigma_{x_g}^2 + cov(v_{isg}, v_{itg}) = \lambda_{gt}\lambda_{gs}\sigma_{x_g}^2 + \delta_g^{T_g}\left(\frac{\sigma_e^2}{1-\delta_g}\right) \tag{28a}$$

$$cov(x_1^*, z_2^*) = \lambda_{1t}\lambda_{2s}\rho\sigma_{x_1}\sigma_{x_2} \tag{28b}$$

$$cov(x_2^*, z_1^*) = \lambda_{2t}\lambda_{1s}\rho\sigma_{x_1}\sigma_{x_2} \tag{28c}$$

$$cov(y, z_1^*) = \lambda_{1s}\gamma_1\sigma_{x_1}^2 + \lambda_{1s}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \tag{28d}$$

$$cov(y, z_2^*) = \lambda_{2s}\gamma_2\sigma_{x_2}^2 + \lambda_{2s}\gamma_1\rho\sigma_{x_1}\sigma_{x_2}. \tag{28e}$$

The probability limits of the IV estimators are below, except that $\sigma_{v_g}^2$ is replaced by $\delta_g^{T_g}\left(\frac{\sigma_e^2}{1-\delta_g}\right)$:

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1\frac{\lambda_{1s}\sigma_{x_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2+\sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} + \gamma_2\frac{\lambda_{1s}\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2\left(\frac{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2+\sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2)+\sigma_{v_2}^2}\right)} \tag{29a}$$

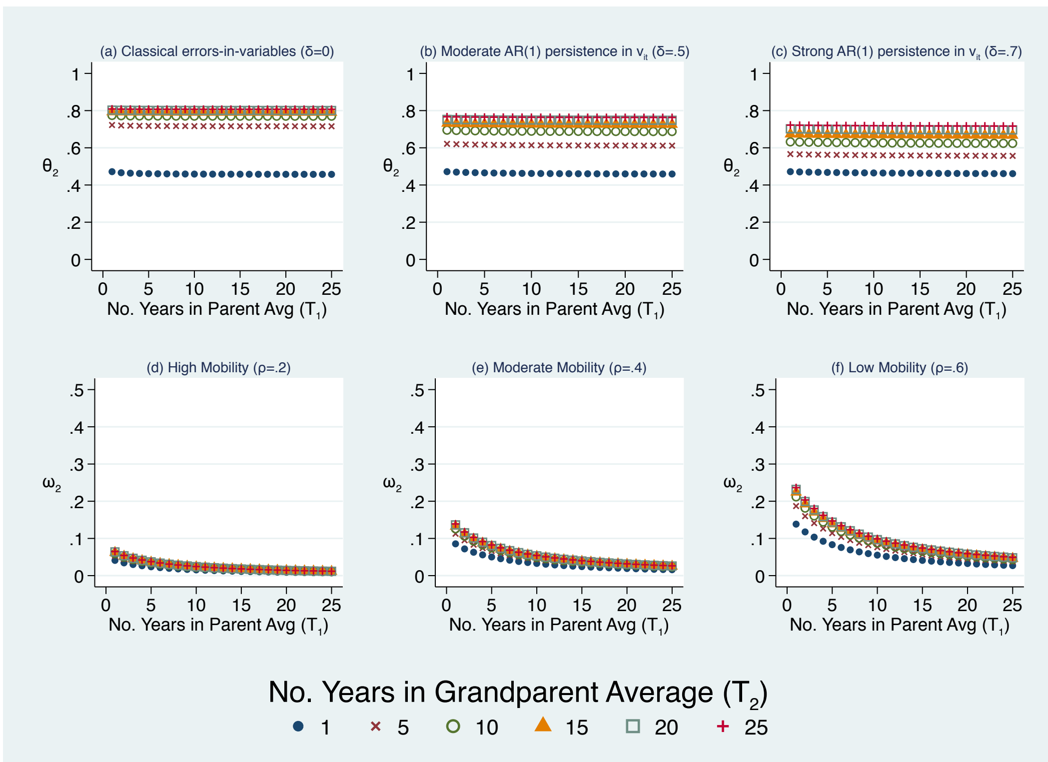
$$plim(\hat{\gamma}_{2,IV}) = \gamma_1\frac{\lambda_{2s}\sigma_{x_1}\sigma_{x_2}\left(\frac{\rho\sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2+\sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)} + \gamma_2\frac{\lambda_{2s}\sigma_{x_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2\left(\frac{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2+\sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2)+\sigma_{v_1}^2}\right)}. \tag{29b}$$

Although an AR(1) process for v_{itg} complicates the probability limits, it still holds that the lifecycle bias is primarily driven by the age at which the endogenous income measure is observed.

B.2 Simulation results with lifecycle effects

This section provides figures showing how the attenuation and spillover biases change with the point in the lifecycle at which income is measured for fathers and grandfathers (varying λ_{1t} and λ_{2t}). Figures B.1 and B.2 show that the attenuation factor for OLS estimates is larger when $\lambda_{gt} > 1$ and is an amplification factor when $\lambda_{gt} < 1$, respectively. This is the same pattern shown previously for two-generation (e.g., father-son) regressions. The changes in the magnitude of the spillover factor reinforce the attenuation or amplification bias.

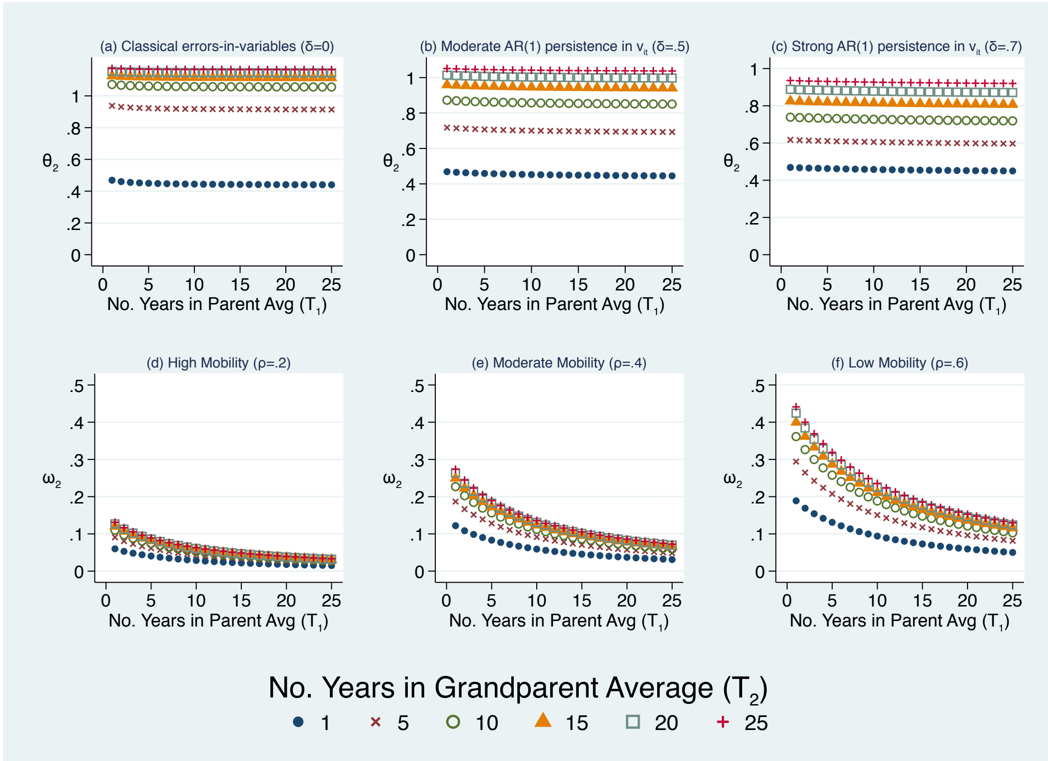
Figure B.1: Attenuation and spillover in OLS estimates when $\lambda_{1t} = \lambda_{2t} = 1.2$



Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the OLS probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,OLS}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

For IV estimates, lifecycle bias is driven by the age at which the endogenous measure is observed. Figures B.3 and B.4 show that the attenuation and spillover factors are affected similarly to those for OLS. The attenuation factor for IV estimates is larger when $\lambda_{gt} > 1$

Figure B.2: Attenuation and spillover in OLS estimates when $\lambda_{1t} = \lambda_{2t} = 0.8$



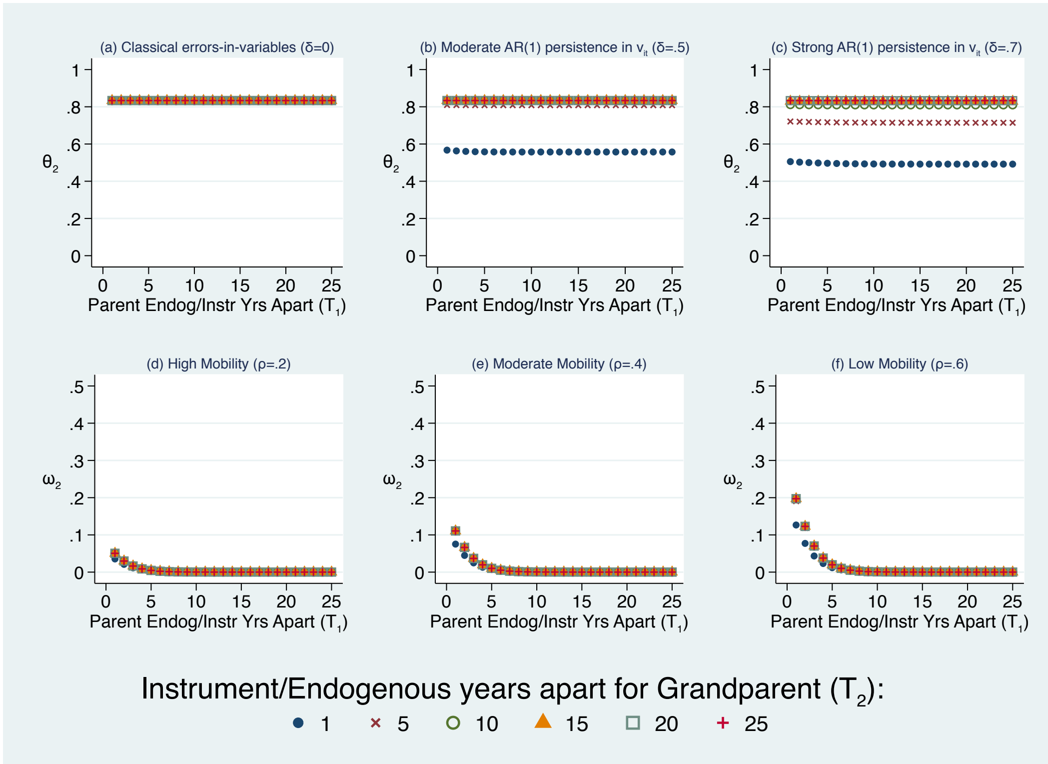
Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the OLS probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,OLS}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

(Figure B.3) and is an amplification factor when $\lambda_{gt} < 1$ (Figure B.4). The changes in the magnitude of the spillover factor reinforce the attenuation or amplification bias.

B.3 Bounding with IV: empirical illustration of lifecycle effects

In our main results, we use income at age 43 as the endogenous income measure to abstract from lifecycle bias. Ideally, λ_{gt} is approximately one at this age, but even if not, we still know that λ_{gt} is constant as we change our instrument income measure. Our instrument is thus taken from subsequent later ages as we increased the years between the income measures to reduce correlation in the transitory component, which was our primary focus. However, we can also use the fact that the direction and magnitude of the lifecycle bias in

Figure B.3: Attenuation and spillover in IV estimates when $\lambda_{1t} = \lambda_{2t} = 1.2$, $\lambda_{1s} = \lambda_{2s} = 1$

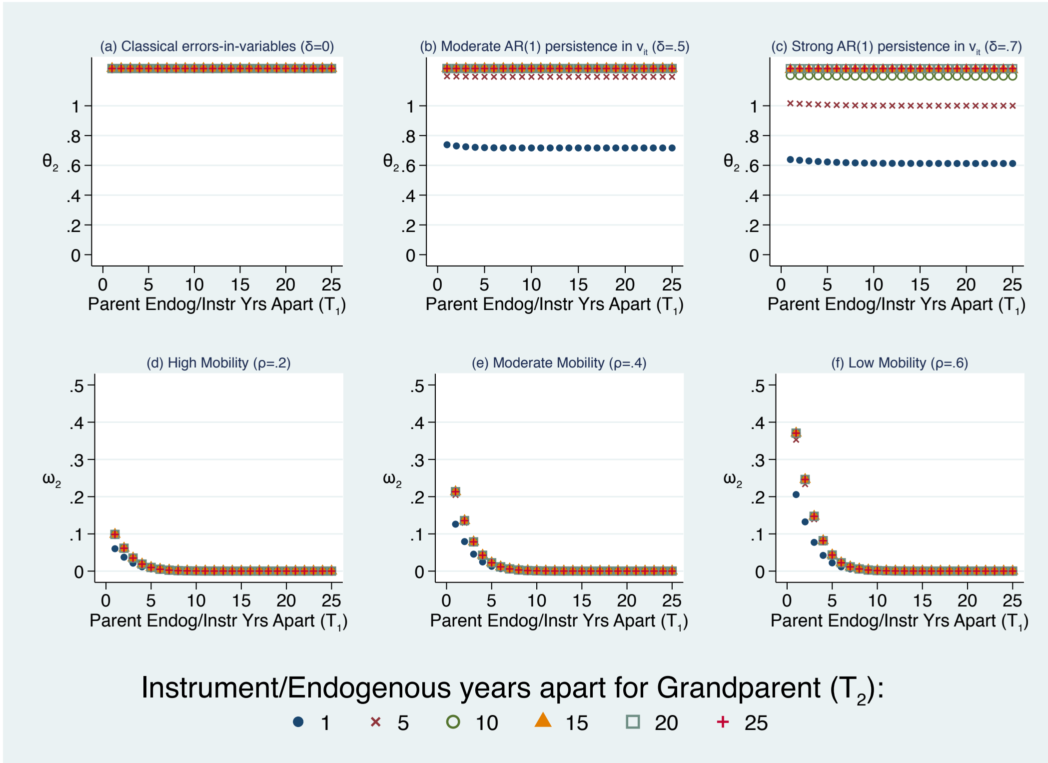


Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the IV probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,IV}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

IV estimates are driven by the age of the endogenous income measure to bound the true parameter. As discussed above, measuring the endogenous income measure when $\lambda_{gt} < 1$ (measuring income too young) causes amplification bias while $\lambda_{gt} > 1$ (too old of ages) causes attenuation bias. This means we can perform two sets of IV estimations to bound the true population parameters: one set where we treat the younger age income as endogenous (thus potentially causing amplification bias), and another set where we treat the older age income as endogenous (potentially causing attenuation bias).

We first illustrate the bounding in Figure B.5 with estimates from two-generation regressions, where the left is for son-father regressions and the right for son-grandfather regressions. As in our main approach, the endogenous income measure is that observed at age 43, and

Figure B.4: Attenuation and spillover in IV estimates when $\lambda_{1t} = \lambda_{2t} = 0.8$, $\lambda_{1s} = \lambda_{2s} = 1$



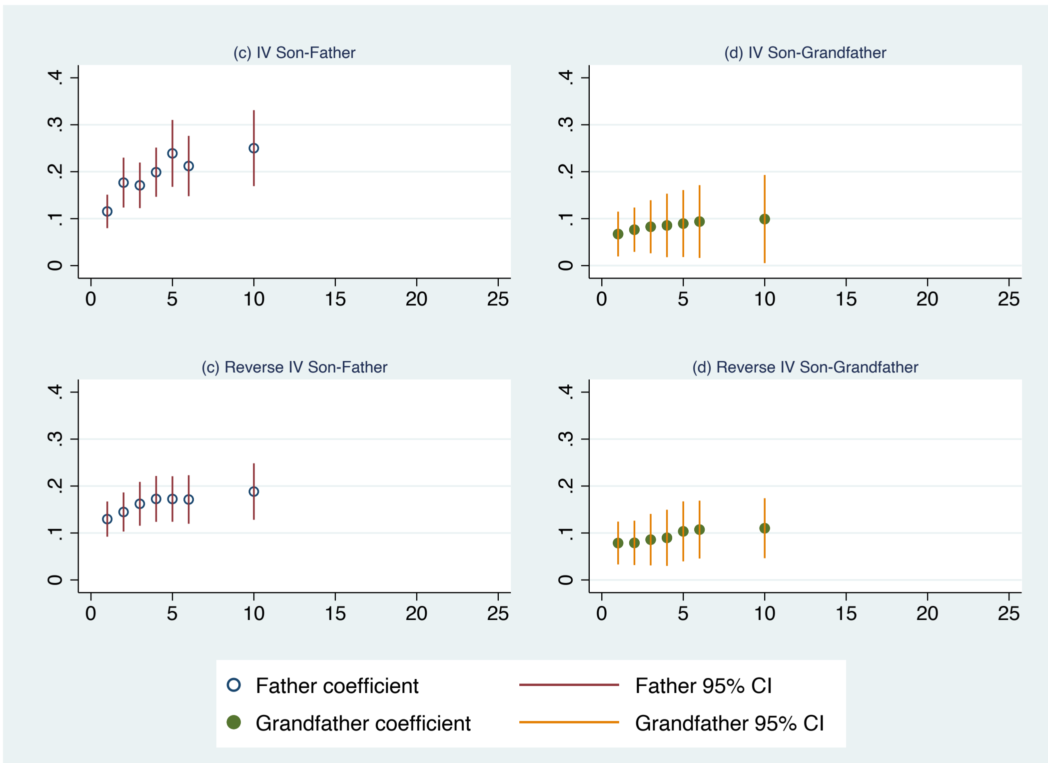
Note: This figure shows the values of the attenuation factor (θ_2) and spillover factor (ω_2) in the IV probability limit for the grandparent coefficient, $plim(\hat{\gamma}_{2,IV}) = \gamma_2\theta_2 + \gamma_1\omega_2$. In graphs (a) - (c) , δ is set to 0, 0.5, 0.7, respectively, while $\rho = 0.4$ is constant. In graphs (d) - (f), ρ is set to 0.2, 0.4, and 0.6, respectively, while $\delta = 0.5$ does not change. Within a graph, moving along a dotted line corresponds to improving the parental income measure, and going from one line to another reflects changes in the grandparent measure.

the instrument is T years later. We see the father-son estimates rising as we increase T in the first graph, consistent with the correlation in the transitory component of income declining over time. Our second set of estimates below this ("Reverse IV") are from swapping the instrument and endogenous measures, so that λ_{gt} may be greater than one as we are using the older ages as the endogenous measure. In this case, our estimates tend to be smaller than the main IV results, consistent with the algebraic result that the estimates are further attenuated due to measuring endogenous income at older ages.

The graphs on the right side of Figure B.5 are for regressions relating sons' income to grandfathers' income. The IV estimates grow somewhat as we increase T, which is consistent with the decreasing correlation in the transitory component, since we are holding λ_{2t} fixed.

To explore the role of lifecycle effects, we turn to estimates from our reverse IV approach treating the older age income as endogenous, provided in the bottom (right) graph of Figure B.5. These estimates are very similar, suggesting the lifecycle profile in λ_{2t} may not vary substantially during this age span for grandfathers.

Figure B.5: Two-generation IV estimates when income at younger versus older age is used as the endogenous measure



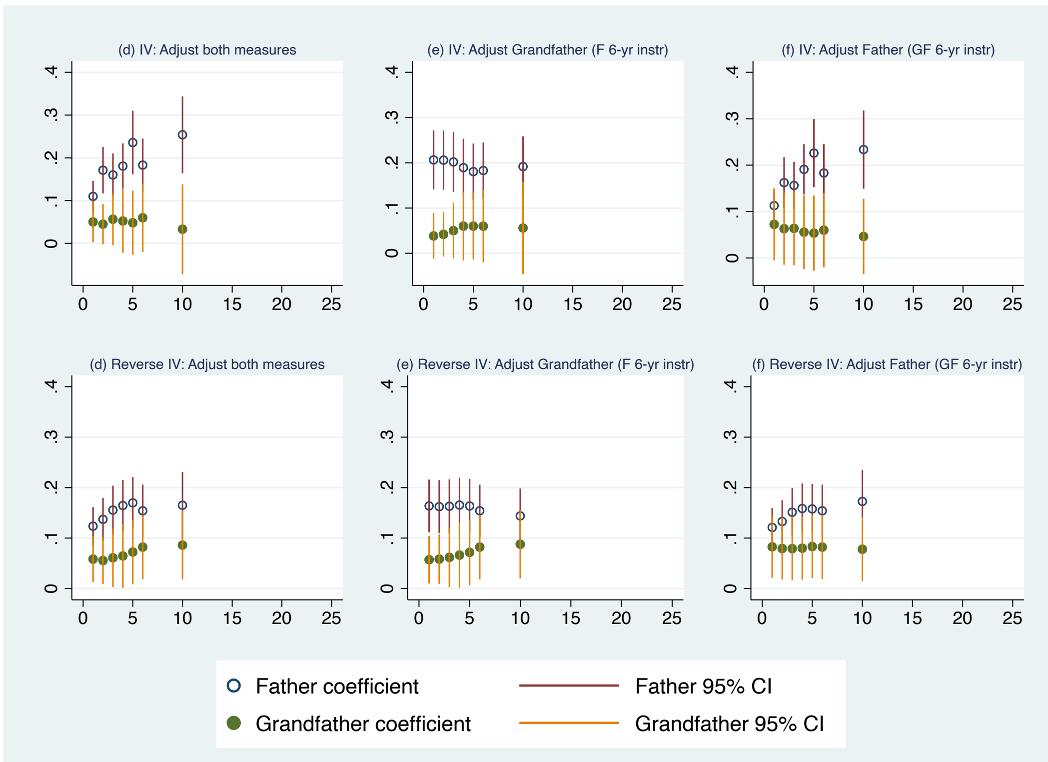
Note: This figure shows the IV and "Reverse IV" coefficient estimates and 95% confidence intervals from son-father regressions and son-grandfather regressions. The x-axis indexes the number of years between the instrument income and endogenous income. For IV, the endogenous measure is at age 43 and instrument at age 43+T, while these are swapped for Reverse IV.

The graphs in Figure B.6 present the IV results from our main results for the multigenerational regression as well as analogous results from our reverse IV approach. The difference is the same as discussed with the intergenerational case—we are now varying the age at which we are measuring the income measure treated as endogenous, to assess lifecycle effects. Our set of estimates in the top graphs use age 43 income as the endogenous measure, and our set of estimates in the bottom graphs use the older age (43+T) income as the endogenous measure.

We first adjust the instrument for both fathers' and grandfathers' income at the same age, using, respectively, fathers' and grandfathers' annual log income from a later year, increasing the distance between years measured for endogenous and instrument income measures as indicated on the x-axis. Similar to the intergenerational case, the coefficient on fathers' income does appear somewhat sensitive to lifecycle bias, showing the same pattern but at slightly lower levels than the intergenerational regression. The grandfather coefficient estimates are similar to the main results, with the exception of the 10-year estimate, though these estimates are based on a smaller sample as noted in the main text.

Next we vary father and grandfather income measures separately to more carefully examine spillover bias. We first use the 6-year instrument for fathers' income while changing the instrument for grandfathers' income. The coefficient on fathers' income remains steady, and the reverse IV results are very similar to the main results. We then isolate the effects of measurement issues arising from fathers' income measures by using a "good" measure for grandfathers' (the 6-year instrument) in all estimations, while varying the instrument for fathers' income. These results are similar to the results in (d) where both fathers' and grandfathers' income measures are varied simultaneously.

Figure B.6: Three-generation IV estimates when income at younger versus older age is used as the endogenous measure



Note: This figure shows the IV and "Reverse IV" coefficient estimates and 95% confidence intervals from a series of multigenerational regressions. The x-axis indexes the number of years between the instrument income and endogenous income for the generation(s) for which the measure is being adjusted. For IV, the endogenous measure is at age 43 and instrument at age 43+T, while these are swapped for Reverse IV.

C Using income ranks in the multigenerational regression

C.1 Derivations for multigenerational regression with income ranks

The following provides derivations of probability limits analogous to those shown in the main text of the paper, though here we are using (normalized) ranks of error-ridden incomes as the status measure for each generation in the multigenerational model.

With classical measurement error, the attenuation bias in the IGE is driven by the increased variance of observed incomes (through the variance of the transitory component). With ranked incomes, by definition the variance of the (normalized) observed rank and true rank are both 1/12.

When income ranks are used instead of (log) incomes, the nature of the measurement error is complicated by the fact that there is necessarily (negative) correlation between the true rank and the measurement error. This negative correlation arises from the fact that ranks at the top (bottom) of the distribution cannot be misreported to be higher (lower) ranks.

Given this, a classical measurement error framework is not appropriate. We follow the approach of Nybom & Stuhler (2017) and Haider & Solon (2006) for modeling non-classical measurement error with a linear projection of the observed outcome on the true value.

We now consider x^*_{itg}, x_{ig}, and v_{itg} to be the observed (annual) income *rank*, true (lifetime) income *rank*, and the (annual) error *in ranks*, respectively (for $g = 1, 2$ for parents, grandparents). ρ denotes the parent-grandparent correlation in x_{ig}. Approximating the non-classical measurement error with linear projections gives us the measurement equations:

$$x^*_{it1} = \lambda_{1t} x_{i1} + v_{it1}, \tag{30a}$$

$$x^*_{it2} = \lambda_{2t} x_{i2} + v_{it2}, \tag{30b}$$

where λ_{gt} now reflects the quality of the observed income rank (i.e., how well it reflects the true rank). The idea is that this reflects the non-classical nature of the measurement error since λ_1 and λ_2 are less than (or equal to) one, and by definition of a linear projection, the true values are orthogonal to the errors.

We can use the measurement equations above to write the elements of the probability limits as:

$$var(x_g^*) = 1/12 \text{ by definition} \tag{31a}$$

$$cov(x_1^*, x_2^*) = \lambda_{1t}\lambda_{2t}\rho(1/12) \tag{31b}$$

$$cov(y, x_1^*) = \lambda_{1t}\gamma_1(1/12) + \lambda_{1t}\gamma_2\rho(1/12) \tag{31c}$$

$$cov(y, x_2^*) = \lambda_{2t}\gamma_2(1/12) + \lambda_{2t}\gamma_1\rho(1/12). \tag{31d}$$

Substituting these into the probability limits in (12a) and (12b) and rearranging gives us:

$$plim(\hat{\gamma}_{1,OLS}) = \gamma_1 \; \lambda_{1t} \left(\frac{1 - \lambda_{2t}^2\rho^2}{1 - \lambda_{1t}^2\lambda_{2t}^2\rho^2} \right) + \gamma_2 \; \lambda_{1t} \left(\frac{\rho(1 - \lambda_{2t}^2)}{1 - \lambda_{1t}^2\lambda_{2t}^2\rho^2} \right) \tag{32a}$$

$$plim(\hat{\gamma}_{2,OLS}) = \gamma_2 \; \lambda_{2t} \left(\frac{1 - \lambda_{1t}^2\rho^2}{1 - \lambda_{1t}^2\lambda_{2t}^2\rho^2} \right) + \gamma_1 \; \lambda_{2t} \left(\frac{\rho(1 - \lambda_{1t}^2)}{1 - \lambda_{1t}^2\lambda_{2t}^2\rho^2} \right). \tag{32b}$$

The determinants of the size of bias follow the case with log incomes. Focusing on the grandparent coefficient, attenuation is alleviated by using a better grandparent income measure, and to a lesser degree by using a worse parent income measure and lower intergenerational persistence levels. The spillover bias is primarily alleviated by using a better parental income measure, and to a lesser degree by using a worse grandparent income measure (the counterintuitive result found with log incomes), and again countries with higher persistence levels are susceptible to larger spillover bias.

For the IV approach, using rank income in year s to instrument for rank income in year

t, we write the analogous measurement equations for our instruments:

$$z^*_{is1} = \lambda_{1s} x_{i1} + v_{is1}, \tag{33a}$$

$$z^*_{is2} = \lambda_{2s} x_{i2} + v_{is2}. \tag{33b}$$

Assuming v_{isg} and v_{itg} are uncorrelated (as in CEV) leads to consistency of IV in the log income case but does not lead to consistent estimates when using income ranks.[22] The probability limits of the estimators are:

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1 \frac{1}{\lambda_{1t}} (1 - \rho^2) \tag{34a}$$

$$plim(\hat{\gamma}_{2,IV}) = \gamma_2 \frac{1}{\lambda_{2t}} (1 - \rho^2) \tag{34b}$$

Attenuation depends on how strong the income measure is for one's own generation (λ_{gt}) as well as the level of mobility (ρ). With no correlation between v_{isg} and v_{itg}, there is no spillover bias. Yet in our empirical results, we find evidence of spillover bias because the grandfather coefficients decline as we improve the income measure for fathers (holding the grandfather measure constant).

Kitagawa *et al.* (2018, p. 4) note, regarding the use of the linear projection approximation and the resulting proposed correction in Nybom & Stuhler (2017), *"While this property is shown to hold approximately in their data, it is not generally known what assumptions on the underlying distributions that can guarantee it."* Thus, instead of using a simple "simulation" as we had done for the log income case in the main text, we use a simulation to generate a synthetic sample to further evaluate how measurement error in income plays out in coefficient

[22] This is also true of the parent-child regression, as noted in footnote 13 of Nybom & Stuhler (2017). With λ_{yt} as the slope coefficient in the linear projection for child income ranks, the parent-child regression yields $plim(\hat{\beta}_{1,IV}) = \beta_1 \frac{\lambda_{yt}}{\lambda_{1t}}$. We do not address measurement error in child income ranks, consistent with the rest of our paper where we do not vary child income measures, but λ_{yt} would similarly enter the probability limits as a multiplicative factor.

estimates from the multigenerational model with rank incomes used as status measures. This is discussed in the next section.

C.2 Simulating measurement error with income ranks

As described in Section 2, some studies of multigenerational mobility are based on linear regressions on income ranks rather than the conventional IGE setup. To verify that the biases we document in this article are not exclusive to the log-log (IGE) specification, we conduct a supplementary simulation exercise with rank-rank regressions.

We again consider a process (as in equation (2)),

$$y_{i0} = \gamma_1 x_{i1} + \gamma_2 x_{i2} + \epsilon_i. \tag{35}$$

In the benchmark analysis, y and x refers to log income. We here supplement this with an analysis where y and x refer to an individual's rank in the income distribution of their own generation. As the errors in income ranks are non-classical, we do not solve for measurement error in closed form (as in equations (4a) and (4b)). Rather, we construct a simulated (synthetic) sample with known parameters and examine the rank coefficients that emerge from an estimation on this synthetic sample.

The synthetic sample consists of 10,000 lineages. Each generation has a latent income x, and has 25 annual income observations with a given error structure (discussed below). We do not consider life cycle variation in this exercise. Each generation has exactly one descendant, who inherits the latent income with parameter γ_1 from the generation before and γ_2 from two generations before.

Initial incomes (the latent individual component) are drawn from a log-normal distribution where the true parental coefficient γ_1 is 0.4 and the true grandparental coefficient γ_2 is zero. In the following, we consider parameters on log incomes, though ranks are still constructed based on the underlying incomes (e.g., on averages of incomes, not averages of log incomes).

The error structure is either classical errors-in-variables (see equation (3)) or AR(1) (equation (5)). We set $\sigma_x^2 = \sigma_v^2 = 1$, $\delta = 0.5$. In the CEV case the error term is drawn from $N(0, \sqrt(\sigma_v^2))$. In the AR1 case the error term for the first period is drawn similarly to the error term for the AR1 case. Subsequent error terms are given by $\delta \log(v_{t-1}) + (1-\delta)e$ where e is drawn from $N\left(0, \sqrt{\frac{1-\delta^2}{(1-\delta)^2}}\right)$.

Each lineage is simulated for 10 generations. Within each generation, 25 time periods are simulated. We discard information from the five first generations, and verify that the calculated coefficients are stable for generations 5-10. From a given simulation, we then get six calculations for each parameter for the population of 10,000 lineages. We repeat the simulation ten times.[23]

Table C.2 reports the regression results for the synthetic sample. Each line reports regression coefficients on grandparents for regressions on six generations and 10,000 dynasties, where the "true" grandparental coefficient is zero. The first panel lists results where the error structure is classical errors-in-variables, while the second panel lists results where the error structure is AR(1).

We observe from Table C.2 that the simulation corresponds well to the analytical results from Section 3 and 4; the first two columns compare analytical and simulated results for OLS, while the third and fourth do the same for IV. We therefore turn to the fifth column, reporting rank-rank regression results, where an analytical calculation is not available.

We see from this rightmost column that the use of rank correlations does not remove the issues concerning spillover bias. With a true coefficient of zero and a CEV error structure, using ranks on one year of income gives a rank coefficient of 0.05. Increasing the number of years of incomes over which the ranks are constructed reduces the coefficient somewhat, but even with averages over 20 years a positive coefficient of around 0.016 is obtained. With AR(1) the problem is even larger, with an average estimated coefficient of 0.023 even with

[23] We verify that the reported averages of coefficients do not depend on the "age" of the synthetic individual at the time of measurement.

Table C.2: Simulation results

Error structure and number of years	True β_2	OLS (log-log) (Analytical)	OLS (log-log) (Simulated)	IV (log-log) (Analytical)	IV (log-log) (Simulated)	Rank-Rank (Simulated)
CEV, $T = 1$	0	0.0417	0.0418	0.0000	0.0009	0.0517
CEV, $T = 3$	0	0.0330	0.0340	0.0000	-0.0005	0.0400
CEV, $T = 5$	0	0.0250	0.0256	0.0000	-0.0020	0.0320
CEV, $T = 10$	0	0.0152	0.0159	0.0000	0.0028	0.0233
CEV, $T = 20$	0	0.0085	0.0090	0.0000	0.0006	0.0165
AR(1), $T = 1$	0	0.0404	0.0415	0.0407	0.0399	0.0476
AR(1), $T = 3$	0	0.0416	0.0402	0.0222	0.0181	0.0429
AR(1), $T = 5$	0	0.0399	0.0369	0.0072	0.0048	0.0387
AR(1), $T = 10$	0	0.0335	0.0302	0.0002	0.0005	0.0318
AR(1), $T = 20$	0	0.0239	0.0202	0.0000	0.0022	0.0230

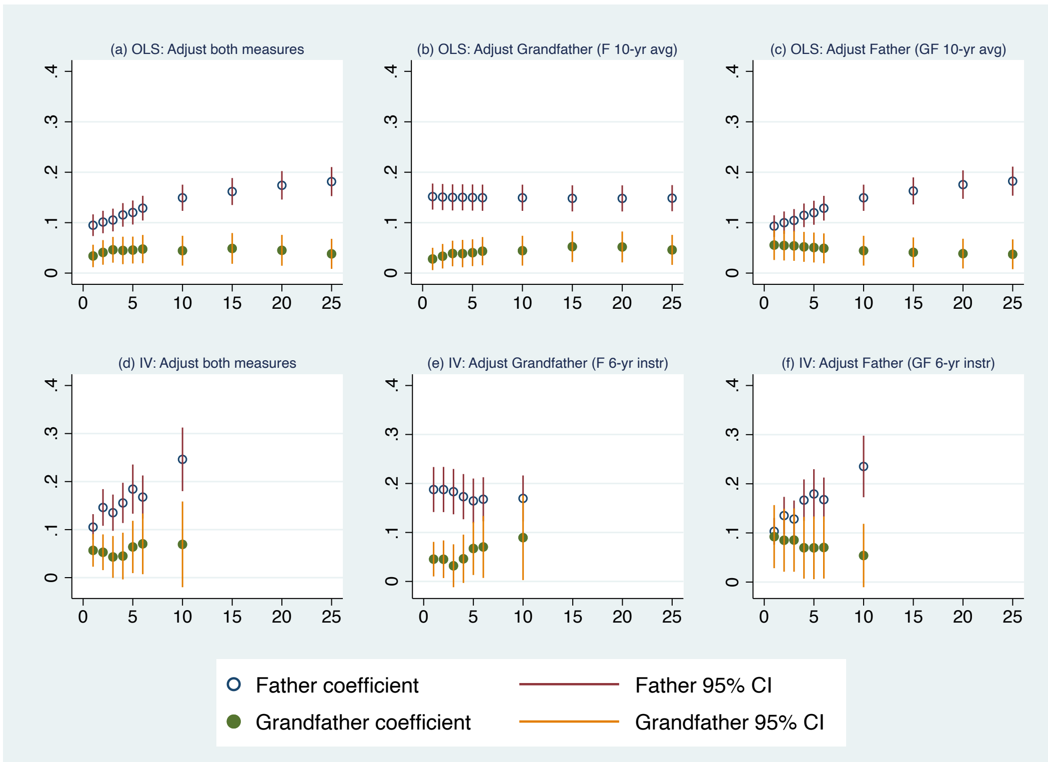
Notes: Error structure is either CEV (classical errors-in-variables) or AR(1). T refers to the number of years over which income is averaged (for OLS and for the construction of ranks) or the distance between instrument and instrumented for IV.

20 years of income averages. Hence, we conclude that the issues of spillover bias documented in this paper are not limited to IGE (log-log) measures of multigenerational persistence.

D Robustness check using men and women

Below are the figures (analogous to the main results) for the sample with men and women in the youngest generation.

Figure D.7: OLS and IV estimates from three-generation regressions. Men and women in final generation.



Note: This figure shows the OLS and IV coefficient estimates and 95% confidence intervals from a series of multigenerational regressions with men and women included in the offspring generation. For OLS, the x-axis indexes the number of years used in the average income measure for the generation(s) for which the measure is being adjusted. For IV, this is instead the number of years between the instrument and endogenous incomes (measured at age 43).

E Two generation regressions

E.1 Biases in the child-parent regression

Our results for the attenuation factors in the multigenerational regression closely follow what has been shown previously for income measurement related biases for the intergenerational regression in equation (1). We provide a brief review of such findings here.

In the simple case of classical measurement error—or classical errors-in-variables (CEV)—there are no lifecycle effects and parental log annual income in year t, x_{i1t}, is decomposed into a permanent component x_{i1} and a white noise error or transitory component, v_{i1t}:

$$x_{i1t} = x_{i1} + v_{i1t}. \tag{36}$$

In this case, we know that the OLS estimate of β_1 is attenuated:

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma^2_{x1}}{\sigma^2_{x1} + \sigma^2_{v1}}, \tag{37}$$

where $\sigma^2_{x1} = var(x_{i1})$ and $\sigma^2_{v1} = var(v_{i1t})$. Taking the average over T years of parental log income reduces the attenuation bias because σ^2_{v1} is then replaced by $\sigma^2_{v1} \,/\, T$ in (37) (e.g., Solon, 1992). Under the strong assumptions of classical measurement error, instrumental variables estimation (IV) (with a valid instrument) provides consistent estimates of β_1 (e.g., Solon, 1992; Altonji & Dunn, 1991).

Now suppose the transitory component, v_{i1t}, follows an AR(1) process with persistence parameter δ:

$$v_{i1t} = \delta v_{i1t-1} + e_{i1t}. \tag{38}$$

Then the OLS estimate converges to:[24]

[24] Solon (1992) originally noted this more complicated probability limit in footnote 17 of his paper, and Mazumder (2005) subsequently examined the empirical implications.

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma^2_{x1}}{\sigma^2_{x1} + \frac{1}{T}\left(\frac{\sigma^2_{e1}}{1-\delta^2}\right)\phi}, \tag{39}$$

where

$$\phi = 1 + 2\delta \frac{T - \frac{1-\delta^T}{1-\delta}}{T(1-\delta)}. \tag{40}$$

In this case, the attenuation bias is not reduced to the same extent by taking multi-year averages (since $0 > \delta > 1$). The implications for IV are also less promising, in that the correlation in the transitory components mean using an annual income measure in year s to instrument for income in year t (or an average ending in year t) no longer provides a consistent estimate. However, the bias shrinks as s gets further from t, as can be seen in (41). Defining $T = s - t$, the probability limit of the IV estimator is:

$$plim(\hat{\beta}_{1,IV}) = \beta_1 \frac{\sigma^2_{x1}}{\sigma^2_{x1} + \delta^T \frac{\sigma^2_{e1}}{1-\delta^2}}. \tag{41}$$

So for both OLS and IV estimation, we know that some bias remains, and use income measurement strategies to minimize the extant bias.[25]

Two features of the lifecycle patterns in income have been shown to bias estimates of intergenerational persistence. First, there is lifecycle variation in the size of σ^2_{v1}, which has been found to be U-shaped with the smallest level being in the early 40s (e.g., Mazumder, 2001, 2005). When taking longer term averages of annual income that may extend into too young or too old of ages, σ^2_v/T can get larger if σ^2_{v1t} grows fast enough, thus exacerbating attenuation bias rather than reducing it.

Second, the relationship between annual incomes and permanent income changes over the lifecycle, and this can lead to attenuation or amplification bias (e.g., Haider & Solon, 2006). To model this lifecycle variation, equation (3) becomes $x_{i1t} = \lambda_{1t} x_{i1} + v_{i1t}$. λ_{1t} tends

[25]For example, Mazumder (2005) shows that there may be about 10% attenuation bias remaining even when using a 30-year income average. More recently, Vosters & Nybom (2017) and Vosters (2018) look for evidence of more substantial attenuation bias from measurement error with respect to a latent construct of socioeconomic status, finding that the remaining bias is in line with earlier intergenerational studies.

to be less than one at younger ages, reaches one around the early 40s when annual income is a reasonable measure of average lifetime income, and then is greater than one at older ages. Incorporating λ_{1t} leads to

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\lambda_{1t}\sigma_{x1}^2}{\lambda_{1t}^2\sigma_{x1}^2 + \sigma_{v1}^2} \tag{42}$$

for OLS estimates from using an annual income measure for parents. If an annual measure is used for offspring as well, $plim(\hat{\beta}_1)$ in (42) is multiplied by $\lambda_{0\tau}$ (the analogous parameter relating annual income in year τ to permanent income for offspring). When a T-year average of income is used, again σ_{v1}^2 is replaced by σ_{v1}^2/T and λ_{1t} is replaced by the average over the included years, $\bar{\lambda}_{1T}$.

In our proposed IV approach using one annual income measure (x_{i1t}) to instrument for another (x_{i1s}), $plim(\hat{\beta}_1)$ simplifies to $\beta_1 \frac{\lambda_{0\tau}}{\lambda_{1t}}$, meaning it is the age at which the endogenous income measure is observed that drives the size of the lifecycle bias. Further, this means at too young of ages, $\lambda_{1t} < 1$ so the bias is actually an amplification bias, while at too old of ages, $\lambda_{1t} > 1$, resulting in attenuation bias. This means that two sets of IV estimates—one set with young ages as the endogenous measure and another set with older ages as the endogenous measure—can be used as a supplementary exercise to bound the true population parameter.

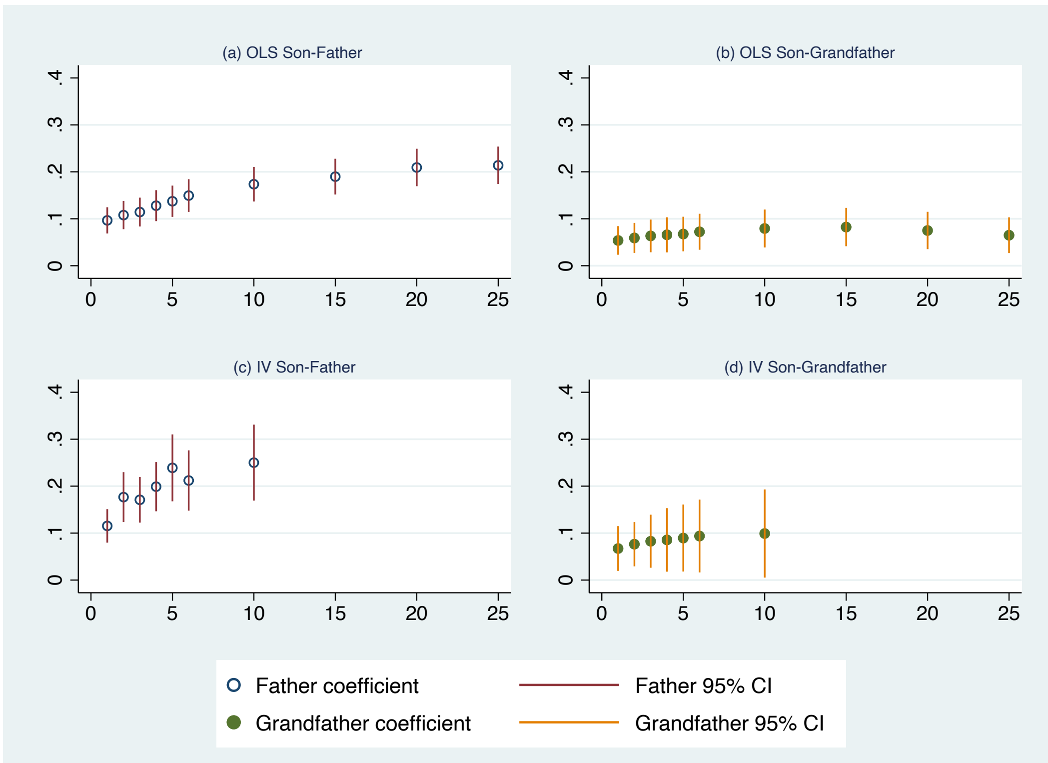
In summary, the lifecycle related bias in OLS or IV estimates can be attenuating or amplifying in nature, as shown by studies emphasizing the importance of measuring annual incomes during the age ranges for which λ_{1t} and $\lambda_{0\tau}$ (or $\bar{\lambda}_{1T}$) are approximately 1 (Haider & Solon, 2006; Nybom & Stuhler, 2014).

E.2 Empirical results

We also provide results for two-generation models using son-father pairs and son-grandfather pairs to both show that our data follows the results on well-known biases and to serve as a reference point for our multigenerational regressions including grandparents. Figure E.8 provides the OLS estimates (top panel) and IV estimates (bottom panel), along with 95% confidence intervals.

As expected, the father-son intergenerational income elasticities in Figure E.8(a) rise as we average over more years of income for fathers (ranging from about 0.10 to 0.21).

Figure E.8: OLS and IV estimates from two-generation regressions



Note: This figure shows the OLS and IV coefficient estimates and 95% confidence intervals from son-father regressions and son-grandfather regressions. For OLS, the x-axis indexes the number of years used in the average income measure for the oldest generation in each regression. For IV, this is instead the number of years between the instrument income and endogenous income (measured at age 43).

Figure E.8(c) shows the estimates from our IV approach, using one log annual income measure to instrument for another. To the extent that the transitory component is persistent over time, we expect the estimates to increase as we increase the years between the endogenous measure and instrument (proceeding left to right). In general, this is what we see for the father-son persistence estimates. The estimates range from 0.12 for the case using income only one year later as the instrument to 0.21 when using income measures 6 years apart, and 0.25 when using measures 10 years apart.[26]

Figures E.8(b) and E.8(d) provide the analogous results for a regression relating sons' income to only grandfathers' income. We see the expected pattern of OLS estimates increasing as we average over more annual log income measures, with the estimates ranging from 0.05 when using annual log income to about 0.08 when using longer term averages. There is a slight decline in the estimate based on the 25-year averages of log income to 0.07, which may arise from lifecycle effects in the form of either increasing $\bar{\lambda}_{2T}$ or increasing σ^2_{vt}.

The IV estimates in Figure E.8(d) exhibit a similar pattern, with estimates growing as the years between the endogenous and instrument income measures increases from one to six years, ranging from 0.07 to 0.09, and still similar at 0.10 for 10 years though this estimate is less precise.[27]

Figure E.9 provides results for the two-generation regressions with the sample including men and women in the youngest generation. The patterns are very similar to the results for the main sample.

F Tables of regression coefficients for all empirical results

F.1 Tables for Men only (main sample)

[26]As with the main results, in all IV estimations, the Kleibergen-Paap F-statistics confirm that our first stage is sufficiently strong ($F \geq 32$ in all regressions, and $F = 112$ on average for regressions with income at age 43 as the endogenous measure).

[27]The samples were slightly reduced again as T_2 increased, with the following sample sizes for T_2=3, 4, 5, 6, and 10 year estimates; N=4,908 (97%), N=4,769 (94%), N=4,712 (93%), N=4,701 (93%), and N=4,470 (88%), respectively. However, estimating these regressions on the most restrictive sample produces similar results, so sample composition changes are not driving our results.

Table F.3: OLS estimates from two-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.Estimates from Figure E.8 (panels a and b) in bold.

(a) Sons and fathers

Income averaged	Age starting from...						
over...	39	40	41	42	43	44	45
1 years	0.137	0.119	0.107	0.091	**0.097**	0.086	0.100
	(0.020)	(0.017)	(0.016)	(0.015)	**(0.014)**	(0.015)	(0.015)
2 years	0.148	0.131	0.117	**0.108**	0.105	0.108	
	(0.019)	(0.018)	(0.017)	**(0.015)**	(0.015)	(0.015)	
3 years	0.153	0.136	0.124	**0.114**	0.119		
	(0.020)	(0.018)	(0.017)	**(0.016)**	(0.016)		
4 years	0.155	0.141	**0.128**	0.126			
	(0.019)	(0.018)	**(0.017)**	(0.016)			
5 years	0.158	0.142	**0.137**				
	(0.019)	(0.018)	**(0.017)**				
6 years	0.158	**0.149**					
	(0.019)	**(0.018)**					
10 years	**0.174**						
	(0.019)						
15 years	**0.190**						
	(0.019)						
20 years	**0.209**						
	(0.020)						
25 years	**0.214**						
	(0.020)						

(b) Sons and grandfathers

Income averaged	Age starting from...						
over...	39	40	41	42	43	44	45
1 years	0.070	0.054	0.043	0.048	**0.054**	0.045	0.041
	(0.019)	(0.020)	(0.019)	(0.015)	**(0.016)**	(0.016)	(0.013)
2 years	0.073	0.058	0.054	**0.059**	0.059	0.051	
	(0.020)	(0.021)	(0.018)	**(0.016)**	(0.017)	(0.016)	
3 years	0.071	0.063	0.062	**0.063**	0.060		
	(0.022)	(0.020)	(0.018)	**(0.018)**	(0.017)		
4 years	0.073	0.068	**0.066**	0.065			
	(0.021)	(0.019)	**(0.019)**	(0.018)			
5 years	0.077	0.071	**0.067**				
	(0.020)	(0.020)	**(0.019)**				
6 years	0.078	**0.072**					
	(0.021)	**(0.020)**					
10 years	**0.079**						
	(0.021)						
15 years	**0.082**						
	(0.021)						
20 years	**0.075**						
	(0.020)						
25 years	**0.065**						
	(0.019)						

Table F.4: IV estimates from two-generation models. Estimates from Figure E.8 (panels c and d) in bold.

(a) Sons and fathers

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.154	0.147	0.138	0.133	**0.115**	0.149	0.127
	(0.022)	(0.024)	(0.021)	(0.020)	**(0.018)**	(0.023)	(0.018)
2 years	0.167	0.167	0.172	0.146	**0.177**	0.159	0.160
	(0.026)	(0.025)	(0.025)	(0.023)	**(0.027)**	(0.023)	(0.022)
3 years	0.180	0.199	0.174	0.214	**0.171**	0.190	0.192
	(0.027)	(0.030)	(0.028)	(0.033)	**(0.025)**	(0.026)	(0.030)
4 years	0.207	0.191	0.248	0.192	**0.199**	0.234	0.171
	(0.031)	(0.031)	(0.037)	(0.028)	**(0.027)**	(0.036)	(0.026)
5 years	0.202	0.261	0.224	0.221	**0.239**	0.205	0.205
	(0.032)	(0.039)	(0.031)	(0.029)	**(0.036)**	(0.032)	(0.025)
6 years	0.270	0.237	0.257	0.247	**0.212**	0.247	0.234
	(0.041)	(0.034)	(0.033)	(0.037)	**(0.033)**	(0.031)	(0.031)
10 years	0.277	0.320	0.363	0.279	**0.250**	0.258	0.214
	(0.044)	(0.040)	(0.049)	(0.046)	**(0.041)**	(0.041)	(0.035)

(b) Sons and grandfathers

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.086	0.059	0.077	0.077	**0.067**	0.067	0.059
	(0.034)	(0.027)	(0.024)	(0.022)	**(0.024)**	(0.021)	(0.021)
2 years	0.072	0.088	0.103	0.082	**0.076**	0.072	0.065
	(0.033)	(0.027)	(0.029)	(0.030)	**(0.024)**	(0.025)	(0.027)
3 years	0.105	0.109	0.101	0.093	**0.083**	0.072	0.077
	(0.034)	(0.031)	(0.036)	(0.028)	**(0.029)**	(0.030)	(0.031)
4 years	0.128	0.103	0.117	0.092	**0.086**	0.079	0.081
	(0.037)	(0.037)	(0.035)	(0.033)	**(0.035)**	(0.032)	(0.035)
5 years	0.114	0.116	0.114	0.091	**0.090**	0.085	0.089
	(0.040)	(0.035)	(0.039)	(0.037)	**(0.036)**	(0.036)	(0.041)
6 years	0.135	0.105	0.114	0.104	**0.094**	0.099	0.012
	(0.040)	(0.038)	(0.045)	(0.042)	**(0.040)**	(0.046)	(0.036)
10 years	0.145	0.142	0.016	0.054	**0.099**	0.041	-0.004
	(0.061)	(0.065)	(0.048)	(0.053)	**(0.048)**	(0.058)	(0.053)

Table F.5: "Reverse IV" estimates from two-generation models.

(a) Sons and fathers

Time difference obs. - instr.	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.192	0.164	0.147	0.119	0.130	0.109	0.121
	(0.026)	(0.024)	(0.022)	(0.018)	(0.019)	(0.017)	(0.017)
2 years	0.232	0.199	0.163	0.147	0.145	0.132	0.141
	(0.032)	(0.029)	(0.025)	(0.022)	(0.021)	(0.021)	(0.020)
3 years	0.265	0.212	0.186	0.157	0.162	0.143	0.155
	(0.036)	(0.031)	(0.029)	(0.023)	(0.024)	(0.022)	(0.022)
4 years	0.274	0.228	0.194	0.161	0.173	0.150	0.169
	(0.037)	(0.034)	(0.029)	(0.025)	(0.025)	(0.023)	(0.025)
5 years	0.300	0.227	0.200	0.168	0.172	0.151	0.162
	(0.041)	(0.033)	(0.031)	(0.025)	(0.025)	(0.025)	(0.024)
6 years	0.292	0.234	0.206	0.157	0.171	0.148	0.160
	(0.040)	(0.035)	(0.031)	(0.023)	(0.026)	(0.024)	(0.025)
10 years	0.297	0.241	0.203	0.167	0.188	0.156	0.160
	(0.046)	(0.036)	(0.033)	(0.029)	(0.031)	(0.028)	(0.024)

(b) Sons and grandfathers

Time difference obs. - instr.	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.092	0.083	0.053	0.064	0.079	0.059	0.064
	(0.025)	(0.032)	(0.023)	(0.020)	(0.023)	(0.022)	(0.020)
2 years	0.109	0.085	0.059	0.081	0.079	0.071	0.066
	(0.029)	(0.033)	(0.026)	(0.027)	(0.024)	(0.025)	(0.022)
3 years	0.109	0.088	0.068	0.080	0.086	0.064	0.064
	(0.029)	(0.034)	(0.031)	(0.027)	(0.028)	(0.026)	(0.027)
4 years	0.111	0.098	0.069	0.083	0.090	0.063	0.075
	(0.030)	(0.038)	(0.031)	(0.030)	(0.031)	(0.030)	(0.027)
5 years	0.116	0.097	0.074	0.091	0.103	0.077	0.076
	(0.031)	(0.038)	(0.034)	(0.030)	(0.033)	(0.030)	(0.028)
6 years	0.120	0.091	0.068	0.112	0.107	0.069	0.066
	(0.033)	(0.039)	(0.035)	(0.034)	(0.031)	(0.032)	(0.027)
10 years	0.139	0.102	0.054	0.097	0.110	0.081	0.066
	(0.040)	(0.050)	(0.035)	(0.034)	(0.033)	(0.036)	(0.028)

Table F.6: OLS estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 4 (panel a) in bold.

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.133	0.116	0.105	0.088	**0.093**	0.083	0.097
		(0.020)	(0.017)	(0.016)	(0.015)	**(0.014)**	(0.015)	(0.015)
	G	0.048	0.039	0.027	0.036	**0.043**	0.037	0.030
		(0.019)	(0.020)	(0.019)	(0.015)	**(0.015)**	(0.016)	(0.013)
2 years	F	0.143	0.127	0.113	**0.103**	0.100	0.105	
		(0.020)	(0.018)	(0.017)	**(0.015)**	(0.015)	(0.016)	
	G	0.047	0.037	0.034	**0.044**	0.046	0.038	
		(0.021)	(0.021)	(0.018)	**(0.016)**	(0.017)	(0.016)	
3 years	F	0.148	0.131	0.119	**0.109**	0.114		
		(0.020)	(0.018)	(0.017)	**(0.016)**	(0.016)		
	G	0.043	0.039	0.041	**0.047**	0.044		
		(0.022)	(0.020)	(0.018)	**(0.018)**	(0.017)		
4 years	F	0.150	0.135	**0.123**	0.121			
		(0.020)	(0.018)	**(0.017)**	(0.016)			
	G	0.043	0.044	**0.045**	0.046			
		(0.021)	(0.020)	**(0.019)**	(0.017)			
5 years	F	0.152	0.136	**0.132**				
		(0.019)	(0.018)	**(0.017)**				
	G	0.046	0.046	**0.044**				
		(0.021)	(0.020)	**(0.019)**				
6 years	F	0.152	**0.143**					
		(0.019)	**(0.018)**					
	G	0.047	**0.045**					
		(0.021)	**(0.020)**					
10 years	F	**0.167**						
		(0.019)						
	G	**0.044**						
		(0.021)						
15 years	F	**0.183**						
		(0.020)						
	G	**0.043**						
		(0.021)						
20 years	F	**0.204**						
		(0.021)						
	G	**0.032**						
		(0.020)						
25 years	F	**0.210**						
		(0.021)						
	G	**0.023**						
		(0.019)						

Table F.7: OLS estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 4 (panel b) in bold.

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.168	0.170	0.171	0.170	**0.169**	0.170	0.170
		(0.019)	(0.019)	(0.019)	(0.019)	**(0.019)**	(0.019)	(0.019)
	G	0.043	0.032	0.016	0.023	**0.033**	0.025	0.023
		(0.019)	(0.020)	(0.019)	(0.015)	**(0.015)**	(0.016)	(0.013)
2 years	F	0.168	0.170	0.170	**0.168**	0.168	0.169	
		(0.019)	(0.019)	(0.019)	**(0.019)**	(0.019)	(0.019)	
	G	0.044	0.030	0.024	**0.033**	0.035	0.029	
		(0.021)	(0.021)	(0.018)	**(0.016)**	(0.017)	(0.015)	
3 years	F	0.169	0.169	0.169	**0.168**	0.168		
		(0.019)	(0.019)	(0.019)	**(0.019)**	(0.019)		
	G	0.039	0.032	0.032	**0.036**	0.035		
		(0.022)	(0.020)	(0.018)	**(0.018)**	(0.017)		
4 years	F	0.168	0.168	**0.168**	0.168			
		(0.019)	(0.019)	**(0.019)**	(0.019)			
	G	0.039	0.037	**0.035**	0.036			
		(0.021)	(0.020)	**(0.019)**	(0.017)			
5 years	F	0.168	0.168	**0.168**				
		(0.019)	(0.019)	**(0.019)**				
	G	0.042	0.039	**0.036**				
		(0.021)	(0.020)	**(0.019)**				
6 years	F	0.167	**0.168**					
		(0.019)	**(0.019)**					
	G	0.043	**0.040**					
		(0.021)	**(0.020)**					
10 years	F	**0.167**						
		(0.019)						
	G	**0.044**						
		(0.021)						
15 years	F	**0.167**						
		(0.019)						
	G	**0.048**						
		(0.021)						
20 years	F	**0.168**						
		(0.019)						
	G	**0.041**						
		(0.020)						
25 years	F	**0.169**						
		(0.019)						
	G	**0.033**						
		(0.019)						

Table F.8: OLS estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 4 (panel c) in bold.

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.132	0.114	0.101	0.086	**0.092**	0.081	0.096
		(0.020)	(0.017)	(0.016)	(0.015)	**(0.014)**	(0.015)	(0.015)
	G	0.048	0.055	0.058	0.062	**0.061**	0.064	0.059
		(0.021)	(0.021)	(0.021)	(0.021)	**(0.021)**	(0.021)	(0.021)
2 years	F	0.142	0.125	0.110	**0.102**	0.099	0.103	
		(0.020)	(0.018)	(0.017)	**(0.015)**	(0.015)	(0.016)	
	G	0.047	0.053	0.056	**0.059**	0.060	0.059	
		(0.021)	(0.021)	(0.021)	**(0.021)**	(0.021)	(0.021)	
3 years	F	0.147	0.129	0.118	**0.108**	0.113		
		(0.020)	(0.018)	(0.017)	**(0.016)**	(0.016)		
	G	0.047	0.052	0.055	**0.058**	0.057		
		(0.021)	(0.021)	(0.021)	**(0.021)**	(0.021)		
4 years	F	0.149	0.134	**0.122**	0.120			
		(0.020)	(0.018)	**(0.017)**	(0.016)			
	G	0.047	0.052	**0.055**	0.055			
		(0.021)	(0.021)	**(0.021)**	(0.021)			
5 years	F	0.151	0.136	**0.131**				
		(0.019)	(0.018)	**(0.017)**				
	G	0.047	0.052	**0.053**				
		(0.021)	(0.021)	**(0.021)**				
6 years	F	0.152	**0.143**					
		(0.019)	**(0.018)**					
	G	0.048	**0.050**					
		(0.021)	**(0.021)**					
10 years	F	**0.167**						
		(0.019)						
	G	**0.044**						
		(0.021)						
15 years	F	**0.184**						
		(0.020)						
	G	**0.040**						
		(0.021)						
20 years	F	**0.203**						
		(0.021)						
	G	**0.036**						
		(0.021)						
25 years	F	**0.208**						
		(0.021)						
	G	**0.035**						
		(0.021)						

Table F.9: IV estimates from three-generation models. Estimates from Figure 4 (panel d) in bold.

Time difference		Age starting from...						
obs. - instr.		39	40	41	42	43	44	45
1 years	F	0.148	0.144	0.132	0.127	**0.110**	0.144	0.119
		(0.023)	(0.024)	(0.021)	(0.020)	**(0.018)**	(0.023)	(0.018)
	G	0.051	0.028	0.048	0.054	**0.050**	0.043	0.045
		(0.034)	(0.027)	(0.024)	(0.022)	**(0.024)**	(0.021)	(0.022)
2 years	F	0.164	0.161	0.162	0.138	**0.171**	0.149	0.147
		(0.027)	(0.026)	(0.025)	(0.024)	**(0.028)**	(0.024)	(0.022)
	G	0.025	0.046	0.064	0.054	**0.045**	0.050	0.039
		(0.034)	(0.029)	(0.029)	(0.030)	**(0.024)**	(0.026)	(0.028)
3 years	F	0.172	0.190	0.166	0.206	**0.160**	0.174	0.192
		(0.028)	(0.031)	(0.029)	(0.034)	**(0.026)**	(0.026)	(0.031)
	G	0.044	0.064	0.055	0.045	**0.056**	0.045	0.048
		(0.036)	(0.032)	(0.037)	(0.030)	**(0.031)**	(0.032)	(0.032)
4 years	F	0.197	0.183	0.243	0.178	**0.181**	0.230	0.151
		(0.032)	(0.033)	(0.039)	(0.029)	**(0.027)**	(0.038)	(0.025)
	G	0.065	0.053	0.032	0.061	**0.052**	0.051	0.054
		(0.039)	(0.039)	(0.038)	(0.036)	**(0.038)**	(0.033)	(0.035)
5 years	F	0.193	0.255	0.213	0.198	**0.236**	0.176	0.192
		(0.034)	(0.041)	(0.034)	(0.031)	**(0.038)**	(0.031)	(0.026)
	G	0.053	0.036	0.051	0.054	**0.048**	0.061	0.054
		(0.043)	(0.038)	(0.042)	(0.041)	**(0.038)**	(0.036)	(0.041)
6 years	F	0.263	0.227	0.236	0.243	**0.183**	0.230	0.244
		(0.044)	(0.036)	(0.034)	(0.040)	**(0.032)**	(0.033)	(0.034)
	G	0.036	0.050	0.046	0.049	**0.060**	0.053	-0.045
		(0.047)	(0.044)	(0.048)	(0.045)	**(0.041)**	(0.046)	(0.039)
10 years	F	0.239	0.302	0.382	0.293	**0.254**	0.243	0.247
		(0.044)	(0.043)	(0.053)	(0.051)	**(0.046)**	(0.044)	(0.042)
	G	0.069	0.044	-0.107	-0.056	**0.033**	-0.040	-0.085
		(0.066)	(0.067)	(0.055)	(0.058)	**(0.054)**	(0.064)	(0.057)

Table F.10: IV estimates from three-generation models, six year time difference for fathers. Estimates from Figure 4 (panel e) in bold.

Time difference obs. - instr.		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.205	0.210	0.207	0.203	**0.206**	0.206	0.204
		(0.034)	(0.033)	(0.033)	(0.034)	**(0.033)**	(0.033)	(0.034)
	G	0.050	0.016	0.036	0.048	**0.038**	0.037	0.036
		(0.036)	(0.028)	(0.025)	(0.023)	**(0.026)**	(0.022)	(0.023)
2 years	F	0.209	0.207	0.203	0.204	**0.206**	0.202	0.191
		(0.034)	(0.033)	(0.034)	(0.034)	**(0.033)**	(0.034)	(0.032)
	G	0.020	0.043	0.064	0.048	**0.042**	0.043	0.046
		(0.035)	(0.030)	(0.030)	(0.032)	**(0.025)**	(0.027)	(0.030)
3 years	F	0.205	0.204	0.204	0.203	**0.202**	0.189	0.182
		(0.034)	(0.034)	(0.034)	(0.034)	**(0.034)**	(0.033)	(0.032)
	G	0.050	0.069	0.058	0.053	**0.050**	0.049	0.052
		(0.036)	(0.033)	(0.038)	(0.031)	**(0.031)**	(0.032)	(0.032)
4 years	F	0.200	0.205	0.203	0.199	**0.189**	0.180	0.184
		(0.034)	(0.034)	(0.034)	(0.034)	**(0.033)**	(0.032)	(0.032)
	G	0.081	0.061	0.064	0.058	**0.060**	0.054	0.051
		(0.039)	(0.041)	(0.037)	(0.036)	**(0.039)**	(0.034)	(0.035)
5 years	F	0.202	0.204	0.200	0.186	**0.181**	0.182	0.189
		(0.034)	(0.034)	(0.034)	(0.033)	**(0.032)**	(0.032)	(0.033)
	G	0.066	0.067	0.067	0.066	**0.060**	0.054	0.046
		(0.044)	(0.039)	(0.042)	(0.043)	**(0.038)**	(0.037)	(0.041)
6 years	F	0.201	0.201	0.188	0.177	**0.183**	0.186	0.199
		(0.034)	(0.034)	(0.033)	(0.032)	**(0.032)**	(0.033)	(0.033)
	G	0.076	0.068	0.077	0.071	**0.060**	0.052	-0.034
		(0.044)	(0.043)	(0.048)	(0.044)	**(0.041)**	(0.046)	(0.039)
10 years	F	0.178	0.186	0.200	0.203	**0.192**	0.209	0.223
		(0.033)	(0.033)	(0.034)	(0.035)	**(0.034)**	(0.036)	(0.037)
	G	0.094	0.075	-0.045	-0.003	**0.056**	-0.035	-0.070
		(0.064)	(0.066)	(0.051)	(0.057)	**(0.052)**	(0.066)	(0.057)

Table F.11: IV estimates from three-generation models, six year time difference for grandfathers. Estimates from Figure 4 (panel f) in bold.

Time difference		Age starting from...						
obs. - instr.		39	40	41	42	43	44	45
1 years	F	0.150	0.138	0.128	0.128	**0.113**	0.138	0.120
		(0.023)	(0.025)	(0.021)	(0.021)	**(0.018)**	(0.024)	(0.019)
	G	0.063	0.067	0.069	0.065	**0.072**	0.073	0.070
		(0.039)	(0.039)	(0.040)	(0.039)	**(0.040)**	(0.039)	(0.040)
2 years	F	0.160	0.154	0.166	0.145	**0.162**	0.147	0.154
		(0.028)	(0.026)	(0.026)	(0.023)	**(0.028)**	(0.024)	(0.023)
	G	0.061	0.064	0.062	0.061	**0.063**	0.071	0.063
		(0.040)	(0.040)	(0.040)	(0.040)	**(0.040)**	(0.040)	(0.040)
3 years	F	0.168	0.190	0.174	0.199	**0.156**	0.182	0.182
		(0.028)	(0.031)	(0.028)	(0.034)	**(0.026)**	(0.026)	(0.031)
	G	0.059	0.057	0.060	0.049	**0.064**	0.065	0.062
		(0.040)	(0.040)	(0.040)	(0.040)	**(0.040)**	(0.040)	(0.040)
4 years	F	0.202	0.192	0.233	0.179	**0.191**	0.220	0.150
		(0.034)	(0.032)	(0.039)	(0.029)	**(0.028)**	(0.037)	(0.025)
	G	0.053	0.057	0.049	0.053	**0.056**	0.064	0.063
		(0.041)	(0.040)	(0.040)	(0.040)	**(0.040)**	(0.041)	(0.040)
5 years	F	0.202	0.248	0.210	0.218	**0.226**	0.177	0.196
		(0.033)	(0.042)	(0.034)	(0.031)	**(0.037)**	(0.031)	(0.027)
	G	0.053	0.046	0.053	0.043	**0.054**	0.068	0.051
		(0.041)	(0.040)	(0.041)	(0.040)	**(0.041)**	(0.040)	(0.039)
6 years	F	0.254	0.221	0.251	0.237	**0.183**	0.232	0.224
		(0.043)	(0.036)	(0.035)	(0.038)	**(0.032)**	(0.033)	(0.032)
	G	0.042	0.051	0.044	0.043	**0.060**	0.056	0.034
		(0.041)	(0.041)	(0.041)	(0.041)	**(0.041)**	(0.040)	(0.040)
10 years	F	0.244	0.309	0.347	0.274	**0.234**	0.232	0.208
		(0.043)	(0.042)	(0.050)	(0.050)	**(0.043)**	(0.042)	(0.038)
	G	0.044	0.035	0.013	0.023	**0.046**	0.052	0.050
		(0.042)	(0.040)	(0.042)	(0.042)	**(0.041)**	(0.042)	(0.041)

Table F.12: "Reverse IV" estimates from three-generation models.

Time difference obs. - instr.		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.185	0.157	0.143	0.114	0.124	0.104	0.116
		(0.027)	(0.025)	(0.023)	(0.018)	(0.019)	(0.018)	(0.017)
	G	0.053	0.053	0.025	0.044	0.058	0.044	0.050
		(0.025)	(0.032)	(0.024)	(0.020)	(0.023)	(0.022)	(0.020)
2 years	F	0.222	0.189	0.159	0.140	0.137	0.129	0.135
		(0.032)	(0.030)	(0.025)	(0.022)	(0.021)	(0.021)	(0.021)
	G	0.061	0.049	0.025	0.051	0.056	0.050	0.042
		(0.030)	(0.034)	(0.027)	(0.026)	(0.024)	(0.027)	(0.024)
3 years	F	0.256	0.204	0.180	0.149	0.155	0.144	0.154
		(0.037)	(0.032)	(0.030)	(0.024)	(0.025)	(0.022)	(0.024)
	G	0.043	0.046	0.031	0.044	0.061	0.037	0.038
		(0.032)	(0.034)	(0.032)	(0.027)	(0.030)	(0.028)	(0.027)
4 years	F	0.264	0.218	0.188	0.156	0.165	0.157	0.159
		(0.039)	(0.035)	(0.031)	(0.026)	(0.026)	(0.024)	(0.026)
	G	0.045	0.051	0.023	0.049	0.065	0.033	0.047
		(0.032)	(0.039)	(0.033)	(0.033)	(0.032)	(0.031)	(0.027)
5 years	F	0.290	0.216	0.195	0.164	0.170	0.152	0.154
		(0.043)	(0.035)	(0.033)	(0.026)	(0.026)	(0.025)	(0.025)
	G	0.045	0.049	0.027	0.046	0.072	0.042	0.042
		(0.034)	(0.040)	(0.037)	(0.033)	(0.033)	(0.031)	(0.028)
6 years	F	0.283	0.222	0.203	0.152	0.154	0.154	0.160
		(0.042)	(0.036)	(0.033)	(0.024)	(0.026)	(0.025)	(0.027)
	G	0.038	0.060	0.018	0.070	0.082	0.023	0.027
		(0.036)	(0.045)	(0.038)	(0.035)	(0.032)	(0.033)	(0.028)
10 years	F	0.283	0.227	0.206	0.158	0.165	0.145	0.149
		(0.048)	(0.038)	(0.036)	(0.031)	(0.034)	(0.030)	(0.027)
	G	0.050	0.039	-0.011	0.049	0.086	0.038	0.032
		(0.046)	(0.053)	(0.039)	(0.037)	(0.035)	(0.038)	(0.030)

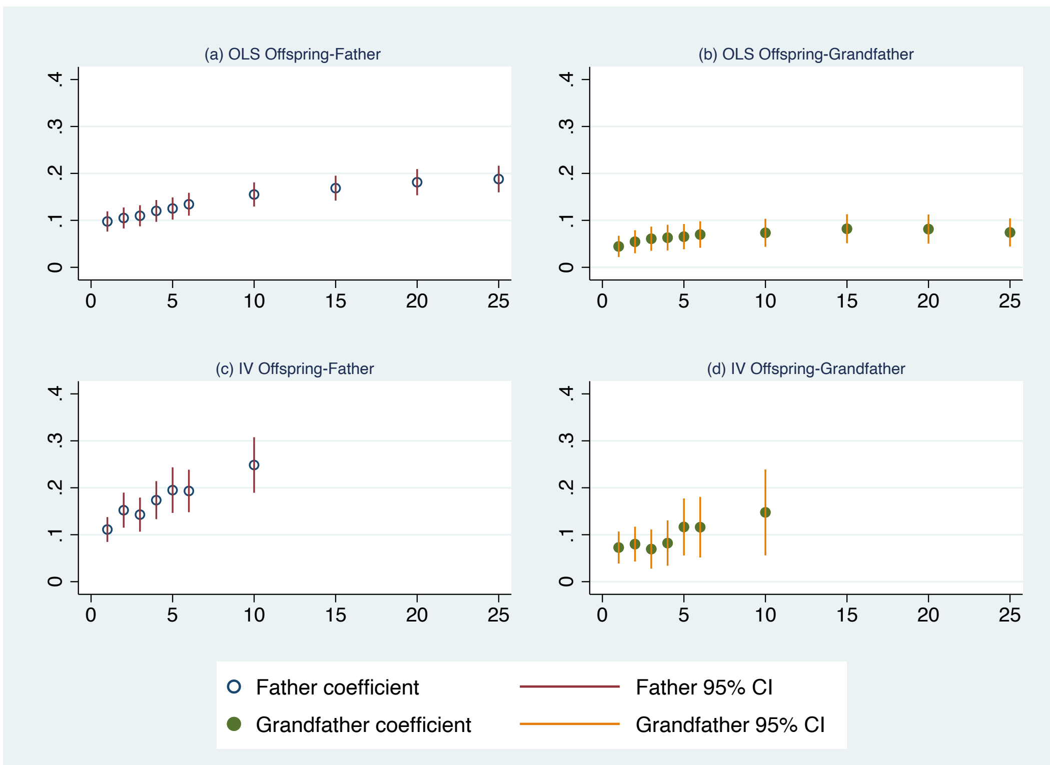
Table F.13: "Reverse IV" estimates from three-generation models, six year time difference for fathers.

Time difference obs. - instr.		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.164	0.164	0.168	0.166	0.164	0.166	0.165
		(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
	G	0.060	0.053	0.024	0.033	0.057	0.033	0.042
		(0.027)	(0.034)	(0.025)	(0.022)	(0.024)	(0.023)	(0.021)
2 years	F	0.161	0.163	0.167	0.166	0.162	0.166	0.168
		(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)
	G	0.070	0.057	0.028	0.041	0.058	0.039	0.044
		(0.032)	(0.037)	(0.028)	(0.028)	(0.025)	(0.028)	(0.024)
3 years	F	0.160	0.162	0.167	0.165	0.163	0.169	0.168
		(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)
	G	0.071	0.058	0.032	0.043	0.062	0.031	0.035
		(0.032)	(0.037)	(0.033)	(0.029)	(0.030)	(0.029)	(0.028)
4 years	F	0.160	0.163	0.166	0.165	0.165	0.170	0.159
		(0.027)	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.026)
	G	0.073	0.065	0.033	0.041	0.066	0.025	0.047
		(0.033)	(0.042)	(0.034)	(0.033)	(0.033)	(0.031)	(0.028)
5 years	F	0.161	0.161	0.166	0.167	0.164	0.159	0.160
		(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.026)	(0.027)
	G	0.075	0.067	0.033	0.052	0.072	0.040	0.049
		(0.034)	(0.043)	(0.036)	(0.034)	(0.033)	(0.031)	(0.029)
6 years	F	0.159	0.163	0.170	0.165	0.154	0.164	0.163
		(0.028)	(0.028)	(0.028)	(0.028)	(0.026)	(0.027)	(0.028)
	G	0.081	0.061	0.028	0.063	0.082	0.027	0.033
		(0.037)	(0.045)	(0.038)	(0.036)	(0.032)	(0.034)	(0.028)
10 years	F	0.153	0.158	0.166	0.162	0.144	0.151	0.152
		(0.027)	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)	(0.028)
	G	0.092	0.064	0.010	0.050	0.088	0.044	0.037
		(0.043)	(0.054)	(0.036)	(0.036)	(0.034)	(0.039)	(0.029)

Table F.14: "Reverse IV" estimates from three-generation models, six year time difference for grandfathers.

Time difference obs. - instr.		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.187	0.158	0.140	0.108	0.121	0.106	0.116
		(0.028)	(0.026)	(0.024)	(0.019)	(0.020)	(0.017)	(0.018)
	G	0.069	0.073	0.077	0.085	0.083	0.085	0.087
		(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.032)
2 years	F	0.232	0.193	0.155	0.132	0.133	0.132	0.127
		(0.034)	(0.030)	(0.026)	(0.022)	(0.021)	(0.021)	(0.021)
	G	0.057	0.066	0.075	0.081	0.079	0.083	0.086
		(0.032)	(0.032)	(0.031)	(0.032)	(0.031)	(0.032)	(0.032)
3 years	F	0.265	0.205	0.175	0.138	0.151	0.141	0.142
		(0.039)	(0.033)	(0.030)	(0.023)	(0.024)	(0.022)	(0.023)
	G	0.050	0.064	0.072	0.078	0.079	0.083	0.087
		(0.032)	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)	(0.032)
4 years	F	0.273	0.223	0.179	0.143	0.159	0.147	0.155
		(0.041)	(0.036)	(0.030)	(0.025)	(0.025)	(0.023)	(0.026)
	G	0.050	0.062	0.070	0.081	0.080	0.086	0.082
		(0.031)	(0.032)	(0.031)	(0.032)	(0.032)	(0.032)	(0.032)
5 years	F	0.294	0.218	0.188	0.149	0.158	0.148	0.155
		(0.044)	(0.034)	(0.032)	(0.025)	(0.025)	(0.024)	(0.026)
	G	0.048	0.061	0.071	0.082	0.083	0.083	0.066
		(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
6 years	F	0.282	0.226	0.189	0.137	0.154	0.149	0.153
		(0.041)	(0.036)	(0.032)	(0.023)	(0.026)	(0.024)	(0.027)
	G	0.048	0.063	0.074	0.087	0.082	0.067	0.068
		(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.031)	(0.032)
10 years	F	0.281	0.235	0.189	0.150	0.173	0.156	0.145
		(0.047)	(0.038)	(0.034)	(0.030)	(0.032)	(0.028)	(0.026)
	G	0.057	0.048	0.060	0.077	0.078	0.072	0.072
		(0.033)	(0.032)	(0.032)	(0.033)	(0.032)	(0.033)	(0.034)

Figure E.9: OLS and IV estimates from two-generation regressions. Men and women in final generation.



Note: This figure shows the OLS and IV coefficient estimates and 95% confidence intervals from offspring-father regressions and offspring-grandfather regressions. For OLS, the x-axis indexes the number of years used in the average income measure for the oldest generation in each regression. For IV, this is instead the number of years between the instrument income and endogenous income (measured at age 43).

F.2 Tables for sample of men and women

Table F.15: OLS estimates from two-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

(a) Sons/daughters and fathers

Income averaged over...	Age starting from... 39	40	41	42	43	44	45
1 years	0.126	0.100	0.094	0.086	0.098	0.080	0.084
	(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)
2 years	0.130	0.113	0.105	0.105	0.102	0.094	
	(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	
3 years	0.134	0.120	0.117	0.110	0.109		
	(0.013)	(0.012)	(0.012)	(0.011)	(0.011)		
4 years	0.137	0.128	0.120	0.116			
	(0.013)	(0.012)	(0.012)	(0.012)			
5 years	0.143	0.130	0.125				
	(0.013)	(0.012)	(0.012)				
6 years	0.144	0.134					
	(0.013)	(0.012)					
10 years	0.155						
	(0.013)						
15 years	0.169						
	(0.014)						
20 years	0.181						
	(0.014)						
25 years	0.188						
	(0.014)						

(b) Sons/daughters and grandfathers

Income averaged over...	Age starting from... 39	40	41	42	43	44	45
1 years	0.056	0.053	0.043	0.049	0.044	0.048	0.040
	(0.017)	(0.015)	(0.014)	(0.012)	(0.012)	(0.012)	(0.010)
2 years	0.065	0.057	0.054	0.054	0.055	0.052	
	(0.016)	(0.015)	(0.014)	(0.013)	(0.013)	(0.011)	
3 years	0.065	0.062	0.058	0.061	0.058		
	(0.016)	(0.015)	(0.014)	(0.013)	(0.012)		
4 years	0.069	0.064	0.063	0.063			
	(0.016)	(0.015)	(0.014)	(0.013)			
5 years	0.070	0.068	0.065				
	(0.015)	(0.015)	(0.014)				
6 years	0.073	0.070					
	(0.015)	(0.014)					
10 years	0.073						
	(0.015)						
15 years	0.082						
	(0.016)						
20 years	0.081						
	(0.016)						
25 years	0.074						
	(0.015)						

Table F.16: IV estimates from two-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

(a) Sons/daughters and fathers

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.137	0.130	0.119	0.131	0.111	0.121	0.106
	(0.015)	(0.016)	(0.014)	(0.015)	(0.014)	(0.016)	(0.014)
2 years	0.153	0.142	0.161	0.137	0.152	0.129	0.138
	(0.017)	(0.017)	(0.018)	(0.017)	(0.019)	(0.017)	(0.017)
3 years	0.158	0.180	0.153	0.176	0.143	0.161	0.157
	(0.018)	(0.021)	(0.019)	(0.022)	(0.019)	(0.019)	(0.020)
4 years	0.196	0.165	0.193	0.155	0.174	0.183	0.156
	(0.022)	(0.021)	(0.024)	(0.021)	(0.021)	(0.024)	(0.019)
5 years	0.181	0.202	0.170	0.189	0.195	0.181	0.186
	(0.022)	(0.024)	(0.022)	(0.023)	(0.025)	(0.022)	(0.019)
6 years	0.218	0.181	0.209	0.198	0.193	0.212	0.196
	(0.027)	(0.024)	(0.025)	(0.025)	(0.023)	(0.023)	(0.023)
10 years	0.236	0.267	0.276	0.226	0.248	0.239	0.197
	(0.029)	(0.027)	(0.031)	(0.031)	(0.030)	(0.031)	(0.024)

(b) Sons/daughters and grandfathers

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.083	0.060	0.076	0.064	0.073	0.066	0.047
	(0.024)	(0.019)	(0.018)	(0.016)	(0.017)	(0.016)	(0.015)
2 years	0.072	0.087	0.081	0.087	0.080	0.058	0.063
	(0.023)	(0.020)	(0.020)	(0.021)	(0.019)	(0.018)	(0.019)
3 years	0.101	0.087	0.102	0.094	0.069	0.071	0.098
	(0.025)	(0.022)	(0.024)	(0.021)	(0.021)	(0.022)	(0.026)
4 years	0.100	0.108	0.110	0.076	0.082	0.104	0.096
	(0.026)	(0.025)	(0.025)	(0.023)	(0.025)	(0.027)	(0.028)
5 years	0.121	0.115	0.090	0.089	0.117	0.102	0.109
	(0.028)	(0.026)	(0.028)	(0.026)	(0.031)	(0.029)	(0.031)
6 years	0.133	0.088	0.106	0.132	0.116	0.122	0.088
	(0.030)	(0.027)	(0.032)	(0.034)	(0.033)	(0.034)	(0.037)
10 years	0.171	0.174	0.121	0.110	0.148	0.084	0.035
	(0.048)	(0.049)	(0.051)	(0.048)	(0.047)	(0.041)	(0.040)

Table F.17: OLS estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.123	0.097	0.092	0.083	0.095	0.077	0.081
		(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)
	G	0.039	0.040	0.030	0.039	0.034	0.041	0.032
		(0.016)	(0.015)	(0.014)	(0.012)	(0.011)	(0.011)	(0.009)
2 years	F	0.126	0.110	0.102	0.101	0.098	0.091	
		(0.013)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	
	G	0.045	0.040	0.039	0.041	0.043	0.042	
		(0.016)	(0.015)	(0.014)	(0.012)	(0.012)	(0.011)	
3 years	F	0.130	0.115	0.113	0.105	0.105		
		(0.013)	(0.012)	(0.012)	(0.011)	(0.011)		
	G	0.043	0.043	0.040	0.046	0.044		
		(0.016)	(0.015)	(0.014)	(0.013)	(0.012)		
4 years	F	0.133	0.123	0.115	0.111			
		(0.013)	(0.012)	(0.012)	(0.012)			
	G	0.045	0.044	0.045	0.047			
		(0.016)	(0.015)	(0.014)	(0.013)			
5 years	F	0.138	0.125	0.120				
		(0.013)	(0.012)	(0.012)				
	G	0.045	0.047	0.046				
		(0.015)	(0.015)	(0.014)				
6 years	F	0.139	0.129					
		(0.013)	(0.012)					
	G	0.048	0.048					
		(0.015)	(0.014)					
10 years	F	0.149						
		(0.013)						
	G	0.044						
		(0.015)						
15 years	F	0.162						
		(0.014)						
	G	0.049						
		(0.016)						
20 years	F	0.174						
		(0.014)						
	G	0.045						
		(0.016)						
25 years	F	0.181						
		(0.015)						
	G	0.038						
		(0.015)						

Table F.18: OLS estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.151	0.151	0.153	0.151	0.152	0.151	0.151
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
	G	0.034	0.033	0.021	0.029	0.028	0.033	0.026
		(0.016)	(0.015)	(0.013)	(0.012)	(0.011)	(0.011)	(0.009)
2 years	F	0.150	0.151	0.151	0.151	0.151	0.150	
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	
	G	0.040	0.032	0.030	0.033	0.036	0.034	
		(0.016)	(0.015)	(0.014)	(0.012)	(0.012)	(0.011)	
3 years	F	0.150	0.150	0.151	0.150	0.150		
		(0.013)	(0.013)	(0.013)	(0.013)	(0.013)		
	G	0.038	0.036	0.034	0.039	0.038		
		(0.016)	(0.015)	(0.014)	(0.013)	(0.012)		
4 years	F	0.150	0.150	0.150	0.150			
		(0.013)	(0.013)	(0.013)	(0.013)			
	G	0.041	0.038	0.039	0.040			
		(0.016)	(0.015)	(0.014)	(0.013)			
5 years	F	0.150	0.150	0.150				
		(0.013)	(0.013)	(0.013)				
	G	0.042	0.042	0.040				
		(0.015)	(0.015)	(0.014)				
6 years	F	0.150	0.150					
		(0.013)	(0.013)					
	G	0.045	0.043					
		(0.015)	(0.014)					
10 years	F	0.149						
		(0.013)						
	G	0.044						
		(0.015)						
15 years	F	0.148						
		(0.013)						
	G	0.052						
		(0.016)						
20 years	F	0.148						
		(0.013)						
	G	0.052						
		(0.016)						
25 years	F	0.149						
		(0.013)						
	G	0.046						
		(0.015)						

Table F.19: OLS estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Income averaged over...		Age starting from... 39	40	41	42	43	44	45
1 years	F	0.122	0.096	0.090	0.082	0.093	0.075	0.079
		(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)
	G	0.050	0.055	0.056	0.059	0.056	0.060	0.058
		(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
2 years	F	0.125	0.108	0.100	0.100	0.097	0.089	
		(0.013)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	
	G	0.050	0.053	0.055	0.055	0.055	0.056	
		(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	
3 years	F	0.129	0.114	0.111	0.104	0.104		
		(0.013)	(0.012)	(0.012)	(0.012)	(0.011)		
	G	0.049	0.052	0.052	0.054	0.054		
		(0.015)	(0.015)	(0.015)	(0.015)	(0.015)		
4 years	F	0.132	0.123	0.115	0.111			
		(0.013)	(0.012)	(0.012)	(0.012)			
	G	0.049	0.050	0.052	0.052			
		(0.015)	(0.015)	(0.015)	(0.015)			
5 years	F	0.138	0.125	0.120				
		(0.013)	(0.012)	(0.012)				
	G	0.047	0.050	0.051				
		(0.015)	(0.015)	(0.015)				
6 years	F	0.139	0.129					
		(0.013)	(0.013)					
	G	0.047	0.049					
		(0.015)	(0.015)					
10 years	F	0.149						
		(0.013)						
	G	0.044						
		(0.015)						
15 years	F	0.163						
		(0.014)						
	G	0.041						
		(0.015)						
20 years	F	0.175						
		(0.014)						
	G	0.039						
		(0.015)						
25 years	F	0.182						
		(0.015)						
	G	0.037						
		(0.015)						

Table F.20: IV estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Time difference		Age starting from...						
obs. - instr.		39	40	41	42	43	44	45
1 years	F	0.132	0.127	0.113	0.126	0.105	0.117	0.101
		(0.015)	(0.016)	(0.014)	(0.015)	(0.014)	(0.016)	(0.014)
	G	0.057	0.036	0.054	0.045	0.057	0.049	0.034
		(0.024)	(0.019)	(0.018)	(0.016)	(0.017)	(0.016)	(0.015)
2 years	F	0.150	0.135	0.154	0.128	0.146	0.122	0.124
		(0.018)	(0.017)	(0.019)	(0.017)	(0.019)	(0.018)	(0.017)
	G	0.037	0.057	0.048	0.064	0.053	0.040	0.037
		(0.023)	(0.021)	(0.021)	(0.021)	(0.019)	(0.019)	(0.019)
3 years	F	0.150	0.172	0.144	0.167	0.135	0.144	0.149
		(0.019)	(0.021)	(0.020)	(0.023)	(0.019)	(0.020)	(0.022)
	G	0.059	0.056	0.070	0.058	0.043	0.041	0.061
		(0.025)	(0.022)	(0.024)	(0.022)	(0.022)	(0.021)	(0.023)
4 years	F	0.188	0.154	0.184	0.145	0.156	0.171	0.138
		(0.023)	(0.021)	(0.025)	(0.022)	(0.021)	(0.025)	(0.019)
	G	0.056	0.074	0.056	0.048	0.045	0.065	0.063
		(0.027)	(0.026)	(0.026)	(0.025)	(0.025)	(0.025)	(0.027)
5 years	F	0.169	0.192	0.160	0.167	0.184	0.157	0.174
		(0.023)	(0.025)	(0.024)	(0.024)	(0.026)	(0.022)	(0.020)
	G	0.078	0.061	0.045	0.051	0.064	0.068	0.069
		(0.029)	(0.028)	(0.029)	(0.027)	(0.028)	(0.028)	(0.030)
6 years	F	0.206	0.171	0.186	0.184	0.168	0.198	0.186
		(0.029)	(0.025)	(0.026)	(0.027)	(0.023)	(0.024)	(0.025)
	G	0.070	0.044	0.049	0.076	0.070	0.071	0.038
		(0.032)	(0.030)	(0.032)	(0.031)	(0.032)	(0.034)	(0.040)
10 years	F	0.201	0.250	0.261	0.217	0.246	0.226	0.202
		(0.030)	(0.029)	(0.035)	(0.034)	(0.034)	(0.033)	(0.028)
	G	0.094	0.084	0.028	0.007	0.069	0.018	-0.027
		(0.049)	(0.049)	(0.056)	(0.045)	(0.046)	(0.046)	(0.045)

Table F.21: IV estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Time difference		Age starting from...						
obs. - instr.		39	40	41	42	43	44	45
1 years	F	0.187	0.191	0.188	0.187	0.188	0.188	0.184
		(0.024)	(0.024)	(0.023)	(0.024)	(0.023)	(0.024)	(0.024)
	G	0.044	0.021	0.041	0.039	0.045	0.037	0.022
		(0.025)	(0.020)	(0.018)	(0.017)	(0.018)	(0.016)	(0.015)
2 years	F	0.190	0.187	0.187	0.184	0.188	0.183	0.173
		(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
	G	0.025	0.048	0.048	0.055	0.045	0.027	0.036
		(0.024)	(0.021)	(0.021)	(0.022)	(0.020)	(0.019)	(0.019)
3 years	F	0.185	0.187	0.185	0.184	0.183	0.173	0.164
		(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.023)
	G	0.056	0.053	0.064	0.055	0.032	0.039	0.057
		(0.025)	(0.023)	(0.025)	(0.023)	(0.022)	(0.022)	(0.023)
4 years	F	0.185	0.185	0.185	0.181	0.173	0.164	0.168
		(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)
	G	0.061	0.069	0.063	0.036	0.046	0.060	0.058
		(0.027)	(0.027)	(0.027)	(0.025)	(0.025)	(0.025)	(0.027)
5 years	F	0.183	0.185	0.182	0.170	0.164	0.167	0.172
		(0.024)	(0.024)	(0.024)	(0.024)	(0.023)	(0.023)	(0.024)
	G	0.077	0.068	0.041	0.051	0.067	0.062	0.062
		(0.030)	(0.029)	(0.029)	(0.028)	(0.028)	(0.028)	(0.031)
6 years	F	0.183	0.182	0.171	0.160	0.168	0.170	0.171
		(0.024)	(0.024)	(0.024)	(0.024)	(0.023)	(0.024)	(0.025)
	G	0.077	0.042	0.059	0.078	0.070	0.069	0.045
		(0.032)	(0.029)	(0.032)	(0.031)	(0.032)	(0.035)	(0.039)
10 years	F	0.162	0.169	0.169	0.178	0.169	0.186	0.194
		(0.024)	(0.025)	(0.025)	(0.025)	(0.024)	(0.026)	(0.027)
	G	0.105	0.101	0.063	0.031	0.089	0.025	-0.018
		(0.048)	(0.050)	(0.055)	(0.044)	(0.044)	(0.046)	(0.044)

Table F.22: IV estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

Time difference		Age starting from...						
obs. - instr.		39	40	41	42	43	44	45
1 years	F	0.126	0.121	0.109	0.124	0.103	0.109	0.097
		(0.015)	(0.017)	(0.015)	(0.016)	(0.014)	(0.016)	(0.015)
	G	0.089	0.090	0.092	0.085	0.092	0.095	0.093
		(0.032)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)
2 years	F	0.143	0.129	0.154	0.129	0.135	0.117	0.135
		(0.019)	(0.017)	(0.020)	(0.017)	(0.019)	(0.018)	(0.018)
	G	0.085	0.089	0.082	0.084	0.085	0.093	0.078
		(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.033)	(0.032)
3 years	F	0.145	0.170	0.145	0.159	0.128	0.156	0.146
		(0.020)	(0.022)	(0.020)	(0.023)	(0.019)	(0.020)	(0.022)
	G	0.085	0.080	0.084	0.077	0.085	0.078	0.077
		(0.033)	(0.032)	(0.033)	(0.033)	(0.033)	(0.032)	(0.032)
4 years	F	0.190	0.156	0.176	0.142	0.167	0.169	0.138
		(0.024)	(0.022)	(0.025)	(0.022)	(0.022)	(0.025)	(0.019)
	G	0.075	0.083	0.077	0.080	0.070	0.078	0.076
		(0.033)	(0.033)	(0.033)	(0.033)	(0.032)	(0.032)	(0.032)
5 years	F	0.172	0.185	0.158	0.187	0.179	0.158	0.173
		(0.024)	(0.026)	(0.024)	(0.024)	(0.026)	(0.022)	(0.020)
	G	0.079	0.077	0.081	0.062	0.070	0.078	0.067
		(0.033)	(0.033)	(0.033)	(0.032)	(0.033)	(0.032)	(0.032)
6 years	F	0.198	0.167	0.205	0.186	0.168	0.194	0.185
		(0.028)	(0.025)	(0.026)	(0.027)	(0.023)	(0.023)	(0.024)
	G	0.073	0.079	0.061	0.063	0.070	0.069	0.059
		(0.033)	(0.033)	(0.032)	(0.033)	(0.032)	(0.032)	(0.032)
10 years	F	0.208	0.252	0.266	0.222	0.235	0.219	0.184
		(0.029)	(0.029)	(0.034)	(0.032)	(0.032)	(0.032)	(0.026)
	G	0.063	0.054	0.043	0.046	0.054	0.064	0.076
		(0.032)	(0.032)	(0.033)	(0.033)	(0.033)	(0.033)	(0.035)