

# Spillover bias in multigenerational income regressions\*

Jørgen Modalsli, Statistics Norway  
Kelly Vosters, UNC Charlotte

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## Abstract

Intergenerational persistence estimates are susceptible to several well-documented biases arising from income measurement, and it has become standard practice to construct income measures in a way to mitigate these. We show in this paper that even when these strategies are used, remaining bias can cause a spurious grandparent coefficient estimate in multigenerational income regressions. Our simulation results show that the remaining bias in OLS estimates even after using a 30-year average leads to an upward bias in the grandfather coefficient estimate. We also use an IV approach that is not susceptible to this remaining bias, but is more sensitive to lifecycle bias, though this is easily tested and can provide bounds on the estimates. With administrative data from Norway, we empirically reveal a positive spillover bias in the grandfather coefficient, and the combined evidence from our OLS and IV approaches suggest the positive OLS estimate could be spurious.

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# 1 Introduction

Societies throughout the world are concerned with the extent to which poverty (or privilege) persists across generations. Studies have established general levels of intergenerational mobility, and now are beginning to look at multigenerational mobility. Although researchers have acknowledged potential influences of measurement error in this setting, we formalize the econometric implications in this paper, and show with simulations and empirical evidence the notable result that grandparent coefficients are often biased upward.

There is a large descriptive literature examining the extent to which socioeconomic status is passed on from one generation to the next.<sup>1</sup> Estimating a basic model of the form

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

where  $y_{i0}$  is an outcome for a child in family  $i$  and  $x_{i1}$  the same outcome for the parent, gives an estimate of the summary statistic,  $\beta_1$ , describing associations across generations.<sup>2</sup> Although this provides a useful description of mobility, researchers are now attempting to explore whether there is more to the process—i.e., additional generations—that we should add to our general depiction of mobility. To paint a more complete picture one can add another generation to equation (1), estimating:

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

In this case,  $\gamma_1$  still describes transmission from parents (though now conditional on grandparents) and  $\gamma_2$  describes the persistence from grandparents to their grandchildren, conditional on parents. Even a small positive  $\gamma_2$  can have important implications for mobility, indicating slower mobility than implied by equation (1). Recent advances in both methods and data allow such extensions to additional generations, with studies generally focusing on outcomes such as income, education and occupation.

The income mobility literature has a rich history of understanding biases from income mea-

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<sup>1</sup>See Solon (1999) and Black & Devereux (2011) for thorough reviews of the literature on two-generation mobility.

<sup>2</sup>Intercepts are omitted to simplify presentation; the variables should be considered to be in deviation-from-mean form.

surement issues, and although we know our estimates are not perfect, we have a reasonable understanding of the remaining bias. It has become standard practice to minimize bias by averaging long timespans of annual incomes during midlife, but this is not yet feasible for three generations in most datasets. Furthermore, the nature of remaining bias arising from such limitations is more complicated in the multigenerational setting, and has distinct consequences for the grandparent coefficient estimate, which is typically the focus of multigenerational mobility studies. As noted in Solon (2018), measurement error could explain a small positive coefficient estimates on grandparents' income in equation (2).

In this paper, we explicitly show with theory and simulations how the biases from well known income measurement issues play out in the multigenerational income mobility estimates, in particular highlighting how bias spillover can produce such a false grandparent effect. Our simulations show that even using long-term averages of income during midlife for all three generations does not eliminate the possibility of estimating a spurious grandparent coefficient. We also propose an IV approach that has the advantage of requiring a shorter timespan of incomes to obtain consistent estimates. In addition to our simulation evidence, we also use administrative tax data from Norway to provide an empirical illustration of the bias spillover in the OLS and IV estimates, showing how it inflates the grandparent coefficient in the multigenerational regression. Our empirical results are consistent with the patterns in our simulation results, and our preferred estimates of the grandparent coefficient using methods to mitigate bias are not statistically significantly different from zero. So although we find small positive coefficient estimates, we cannot rule out the possibility these are spurious.

Why is a small positive grandparent coefficient so important? Because it implies that mobility across generations is slower than previously believed. If the model in (1) represents the true underlying transmission process, then we could use our estimates of  $\beta_1$  to approximate the association for further generations. For example, under simplifying assumptions, the persistence between the outcomes for children and their grandparents could be approximated by  $\beta_1^2$ . This approximation implies that persistence declines geometrically, so we would observe fairly rapid mobility across generations.<sup>3</sup> However, several recent multigenerational mobility studies find a positive grandparental coefficient in (2) (e.g., Clark, 2014; Clark & Cummins, 2015; Long & Ferrie, 2018; Lindahl *et al.*,

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<sup>3</sup>See Stuhler (2014) for further discussion of this approximation.

2015; Zeng & Xie, 2014; Hällsten, 2014; Olivetti *et al.*, 2014; Modalsli, 2016), which implies a *slower* than geometric rate of decline in persistence, or lower mobility.<sup>4</sup> For example, for Norway, the true  $\beta_1$  may be around 0.4,<sup>5</sup> which in a regression where log income is the outcome (so  $\beta_1$  is an inter-generational 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  $\gamma_2$  is about 0.1 (assuming  $\gamma_1$  is 0.4), would imply the child’s income would be about 27.5% above the mean.

Many recent multigenerational studies focus on outcomes such as education, occupation, or wealth, though a few have used data on individual’s income.<sup>6</sup> Lindahl *et al.* (2014, 2015) estimate unconditional and conditional (on parents) effects of grandparents for income and education in Malmö, Sweden, finding positive effects of grandparents for both outcomes. Modalsli (2016) uses administrative data on occupations and incomes for Norway, finding that grandparents do matter conditional on parents. Lucas & Kerr (2013) use a small subset of national data on income for three generations in Finland, but their results for grandparents are mixed.

There are a number of mechanisms that could explain a positive grandparent coefficient, ranging from biological to social influences or through resources.<sup>7</sup> Grandparents may have frequent interactions with grandchildren due to close geographic proximity, or have labor market connections from which the child may benefit, or they may make direct financial investments on behalf of the child (in a manner distinctive to how the parents would choose). Of course, identifying mechanisms is

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<sup>4</sup>Early studies did not find strong evidence of a conditional grandparent effect, but these datasets were often for a peculiar or non-representative sample (e.g., Warren & Hauser (1997), Hodge (1966) ).

<sup>5</sup>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  $\beta_1 = 0.42$ .

<sup>6</sup>Hertel & Groh-Samberg (2014) use the Panel Study of Income Dynamics (PSID) to study persistence in occupational class in the U.S.; Long & Ferrie (2018) use wealth-based occupational status measures constructed from U.S. Census data; Boserup *et al.* (2014) estimate multigenerational wealth elasticities 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. All of these studies find evidence of a small positive grandparent effect, and Ferrie *et al.* (2016) consider the possibility that their estimate could be a consequence of measurement error.

<sup>7</sup>The seminal theoretical work by Becker & Tomes (1979) arrives at the perhaps counter intuitive prediction of a negative effect of grandparents conditional on parent income, 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 child. 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.

always challenging, whether considering the intergenerational or multigenerational settings. And, while we by no means wish to decry the potential for these mechanisms to cause a positive grandfather coefficient, it is important to recognize the limitations of our empirical estimates given the data available to us.

Measurement issues play an important role in the descriptive mobility literature, and have received particular attention in the context of income mobility (e.g., Solon, 1992; Mazumder, 2005; Haider & Solon, 2006; Nybom & Stuhler, 2014). Whether using administrative or survey data, we do not observe the long-term (or lifetime) component of income ( $x_{ig}$  in the equations above). Rather we observe annual income measures that contain a transitory component in addition to the long-term component. Studies have long recognized the attenuation bias in estimates of  $\beta_1$  arising from transitory fluctuations in annual income of the parent generation. Considering the transitory fluctuations to be a source of (approximately) classical measurement error means using even a five-year average can lead to dramatic increases in estimates of intergenerational persistence, or substantially lower rates of mobility (Solon, 1992; Zimmerman, 1992). Also accounting for the persistence in these transitory components over time implies longer-term averages are needed to mitigate the attenuation bias, again leading to lower estimates of mobility (Mazumder, 2005). Timing during the lifecycle at which income is measured is also an important factor. Considering the lifecycle patterns in the importance of the transitory components as well as in the association between the annual incomes and the permanent component motivates the importance of measuring the long timespans of income during mid-life (Mazumder, 2005; Haider & Solon, 2006; Nybom & Stuhler, 2014).

In this paper, we extend the literature on these income measurement issues—which are well documented for the child-parent regression of equation (1)—to the multigenerational regression in (2). The implications are more complicated because correlation between the regressors  $x_{i1}$  and  $x_{i2}$  leads to spillover of bias from measurement error. For example, in the case where the true  $\gamma_2$  is zero, such spillover arising from measurement error in  $x_{i1}$  would lead to a positive estimate of  $\gamma_2$ . Further, we show that this potential for measurement error to lead to a spurious grandparent effect is still plausible even when the income measures are constructed as multi-year averages during mid-life, as has become standard practice. This paper also adds to the empirical multigenerational literature for Norway, providing empirical estimates of three-generation income elasticities.

In the next section, we summarize the existing results on measurement issues and associated biases in the intergenerational (parent-child) literature, and then discuss the consequences of these same issues for the multigenerational regression. We also conduct a simulation to illustrate the biases in the multigenerational estimates and present these results in Section 3. Section 4 describes our data and approach, followed by the empirical results. We provide conclusions in Section 5.

## 2 Biases from Measurement Issues

Measurement of income has long been known to introduce several different biases in intergenerational regressions. The measurement issues stem from the fact that, although we would like to estimate the intergenerational persistence in “permanent” or “lifetime” 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.<sup>8</sup> With these issues, the timing and duration of the lifespan for which we observe annual incomes are crucial to mitigating potential biases.

We begin this section by reviewing results from the existing literature on resulting biases in OLS and IV estimation of the intergenerational regression in equation (1). In Section 3.2, we then briefly note how these biases might affect extrapolations of the intergenerational coefficients to make inferences regarding multigenerational mobility. We turn to multigenerational regressions in Section 3.3, showing how the income measurement issues play out in OLS and IV estimation of equation (2).

### 2.1 Biases in the intergenerational regression

Measurement error (or transitory fluctuations) in annual income along with the life-cycle profile in income are two well documented sources of bias in intergenerational mobility studies, both of

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<sup>8</sup>For studies relying on retrospective questions in surveys (about own income in previous periods or about parents’ economic status a generation back) 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.

which can be mitigated with how income is measured. Measuring income during mid-life minimizes bias from the latter (Haider & Solon, 2006; Nybom & Stuhler, 2014). When income is measured in this timespan, averaging over several years of income has been shown to substantially reduce attenuation bias from measurement error or transitory fluctuations (Solon, 1992; Mazumder, 2005).

We begin our summary with the simple case of classical measurement error and no lifecycle effects, where parental log annual income in year  $t$ ,  $x_{1t}$ , is decomposed into a permanent component  $x_1$  and a white noise error or transitory component,  $v_{1t}$ :

$$x_{1t} = x_1 + v_{1t} \tag{3}$$

In this case, we know that the OLS estimate of  $\beta_1$  is attenuated:

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma_{x_1}^2}{\sigma_{x_1}^2 + \sigma_{v_1}^2} \tag{4}$$

where  $\sigma_{x_1}^2 = var(x_{i1})$  and  $\sigma_{v_1}^2 = var(v_{i1t})$ . Taking the average over  $T$  years of log income reduces the attenuation bias because  $\sigma_{v_1}^2$  is then replaced by  $\sigma_{v_1}^2 / T$  in (4). Note that in this simple setting, taking averages over several years for offspring income (the dependent variable  $y_{i0}$ ) reduces the error variance.

Under the strong assumptions of classical measurement error, instrumental variables estimation (IV) (with a valid instrument) provides consistent estimates of  $\beta_1$ . Early intergenerational studies use father's education to instrument for father's 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 their IV estimates are consistent with some persistence in the transitory component of income.

Mazumder (2005) subsequently showed that such persistence implies worse attenuation bias even when time-averaging with OLS estimation. Suppose the transitory component,  $v_{1t}$ , follows an AR(1) process:

$$v_{i1t} = \delta v_{i1t-1} + e_{i1t} \quad (5)$$

Then the OLS estimate converges to:

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma_{x1}^2}{\sigma_{x1}^2 + \frac{\sigma_{v1}^2}{T} \phi} \quad (6)$$

where

$$\phi = 1 + 2\delta \frac{T - \frac{1-\delta^T}{1-\delta}}{T(1-\delta)} \quad (7)$$

In this case, the attenuation bias is not reduced to the same extent by taking multi-year averages (since  $0 > \delta > -1$ ), and an IV approach 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, though the bias shrinks as  $s$  gets further from  $t$ . Defining  $T = s - t$ , the probability limit of the IV estimator is:

$$plim(\hat{\beta}_{1,IV}) = \beta_1 \frac{\sigma_{x1}^2}{\sigma_{x1}^2 + \delta^T \frac{\sigma_{e1}^2}{1-\delta^2}} \quad (8)$$

Further complicating things is the lifecycle variation in the size of  $\sigma_{v1}^2$ , which has been found to be U-shaped with the smallest level being in the early 40's (e.g., Mazumder, 2001, 2005).<sup>9</sup> When taking longer term averages of annual income,  $\sigma_v^2/T$  can potentially get larger if  $\sigma_{v1t}^2$  grows fast enough, thus exacerbating attenuation bias rather than reducing it.

Other studies have pointed out that 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  $x_{i1t} = \lambda_{1t} x_{i1} + v_{i1t}$ .  $\lambda_{1t}$  tends 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  $\lambda_{1t}$  leads to

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\lambda_{1t} \sigma_{x1}^2}{\lambda_{1t}^2 \sigma_{x1}^2 + \sigma_{v1}^2} \quad (9)$$

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<sup>9</sup>For Norway, Nilsen *et al.* (2012) do not find the full U-shape pattern found for other countries, rather they find the typical incline beginning in the early 40's, but with a stable level at younger ages. We discuss the implications of this further with our empirical results.



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 (9) is multiplied by  $\lambda_{0\tau}$  (the analogous parameter relating annual income in year  $\tau$  to permanent income for offspring). When a T-year average of income is used, again  $\sigma_{v1}^2$  is replaced by  $\sigma_{v1}^2/T$  and  $\lambda_{1t}$  is replaced by the average over the included years,  $\bar{\lambda}_{1T}$ . And, in the case of IV using an annual income to instrument for another,  $plim(\hat{\beta}_1)$  simplifies to  $\beta_1 \frac{\lambda_{0\tau}}{\lambda_{1t}}$ .

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

Many of the aforementioned intergenerational results have been documented in the literature (e.g., Zimmerman, 1992; Mazumder, 2001, 2005; Haider & Solon, 2006; Nilsen *et al.*, 2012; Nybom & Stuhler, 2014). And some of these methods for mitigating bias, such as measuring income at midlife and averaging over several years, have become standard practice. However, even when these practices are implemented, some bias still remains. In the two-generation setting, this may not be very problematic because it is generally believed that we know the direction of bias and often it is fairly small in magnitude. Still, we note in the next section that using these estimates to make inferences about multigenerational mobility could be misleading if we ignore the leftover bias.

## 2.2 Comparing estimates from two-generation regressions

As previously mentioned, studies sometimes extrapolate intergenerational regression estimates to approximate multigenerational mobility, and the above noted biases could lead to false conclusions of a grandparent effect. For instance, some studies compare estimates of the offspring-grandparent association ( $\beta_3$ ) with  $(\hat{\beta}_1)^2$ . If  $\hat{\beta}_3 > (\hat{\beta}_1)^2$ , this has been interpreted as evidence in favor of a grandparent effect (e.g., Lindahl *et al.*, 2015). If we consider the results above on attenuation bias, it is not clear that comparing  $\hat{\beta}_3$  and  $(\hat{\beta}_1)^2$  is strong enough evidence, even after properly accounting for estimation error, because of the attenuation bias that is present in the estimates. Define these attenuation factors  $\theta_1^*$  and  $\theta_3^*$  such that  $\hat{\beta}_1 = \theta_1^* \beta_1$  and  $\hat{\beta}_3 = \theta_3^* \beta_3$ . Then it is simple to show that 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? Using the preferred estimates of attenuation factors in Table 1 of

Mazumder (2005), if we use a 20-year average for parents' income ( $\theta_1^* = .88$  so  $(\theta_1^*)^2 = .77$ ), then a 9-year (or longer) average ( $\theta_3^* = .78$ ) for grandparents' income will give  $\theta_3^* > (\theta_1^*)^2$ , and thus  $\hat{\beta}_3 > (\hat{\beta}_1)^2$ .

Another analogous comparison studies consider is whether  $\hat{\beta}_3 > \hat{\beta}_1 \cdot \hat{\beta}_2$ , where  $\hat{\beta}_2$  is an estimate of the parent-grandparent association (e.g., Lindahl *et al.*, 2015; Adermon *et al.*, 2018). In this case, if we again consider attenuation bias, we will mistakenly conclude that  $\hat{\beta}_3 > \hat{\beta}_1 \cdot \hat{\beta}_2$  (despite the true relationship being  $\beta_3 = \beta_1 \cdot \beta_2$ ) if the attenuation factors satisfy  $\theta_3^* > \theta_1^* \cdot \theta_2^*$ . Since the same grandparent income measure is typically used in the offspring-grandparent and parent-grandparent regressions,  $\theta_3^* = \theta_2^*$ , meaning any  $\theta_1^* < 1$  will lead us to mistakenly conclude that  $\hat{\beta}_3 > \hat{\beta}_1 \cdot \hat{\beta}_2$ . Although the biases can be complicated by lifecycle effects as discussed above, if income is measured during midlife so  $\bar{\lambda}_t \approx 1$ , then it is almost certain that  $\theta_1^* < 1$  for any long-term average of income; even using a 30-year average leaves an attenuation factor of 0.91 in the simulations in Mazumder (2005).

Although it is feasible that biases may affect the comparisons of intergenerational estimates, these comparisons were generally made due to data limitations. Now that it is possible to run the full multigenerational regression, we show in the next section that this presents unique challenges even with small amounts of bias remaining from parental income measures, as this bias spills over into—and has the opposite effect on—the grandparent coefficient in equation (2).

### 2.3 Multigenerational regression

We next turn to the multigenerational regression, showing the distinct implications of the measurement issues discussed above, including the consequences from bias that remains even after taking standard approaches to mitigate the measurement issues. The intergenerational correlation between parents' and grandparents' permanent components of income leads to spillover of these biases, a standard econometric result. Such spillover is often ignored because the affected coefficient is not for a variable of interest, but the opposite is true in this case—we are primarily interested in the grandparent coefficient. Notably, this spillover bias can produce a small positive coefficient estimate when the true parameter for grandparents is zero—or even negative—in the multigenerational equation in (2).

For 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.

To see the potential effects of bias spillover more precisely, we extend the simple scenario of classical measurement error to both generations. Consider annual income measures for both generations that follow equation (3), where now it also matters that  $v_{i1t}$  is orthogonal to  $v_{i2t}$ , so annual income is only related across generations through the permanent component of income. This is reflected below by  $\rho = \text{corr}(x_{i1}, x_{i2})$ . For simplicity, consider the case of stationarity where  $\text{var}(x_{i1t}) = \text{var}(x_{i2t}) = \sigma_x^2$  and  $\text{var}(v_{i1t}) = \text{var}(v_{i2t}) = \sigma_v^2$ . The probability limits of the OLS estimators from using annual income measures in the multigenerational equation (2) are:

$$\text{plim}(\hat{\gamma}_{1,OLS}) = \underbrace{\gamma_1 \frac{\sigma_x^2}{\sigma_x^2 + \sigma_v^2 \left( \frac{\sigma_x^2 + \sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}}_{\text{attenuation, } \theta_1} + \underbrace{\gamma_2 \frac{\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\sigma_x^2 + \sigma_v^2 \left( \frac{\sigma_x^2 + \sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}}_{\text{spillover, } \omega_1} \quad (10a)$$

$$\text{plim}(\hat{\gamma}_{2,OLS}) = \underbrace{\gamma_1 \frac{\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\sigma_x^2 + \sigma_v^2 \left( \frac{\sigma_x^2 + \sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}}_{\text{spillover, } \omega_2} + \underbrace{\gamma_2 \frac{\sigma_x^2}{\sigma_x^2 + \sigma_v^2 \left( \frac{\sigma_x^2 + \sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}}_{\text{attenuation, } \theta_2}. \quad (10b)$$

These equations show 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 the  $\text{plim}(\hat{\gamma}_{2,OLS})$  sum will be zero, but the first element ( $\gamma_1\omega_2$ ) will be positive. Hence, despite the true  $\gamma_2 = 0$ , one would still estimate a small positive coefficient. Even with the common practice of using multi-year averages of income, where then the  $\sigma_v^2$  in equations (10a) and (10b) are replaced by  $(\sigma_v^2/T)$ , some bias still remains—and will still cause upward bias in the other coefficient estimate—leaving open the possibility of estimating a spurious grandparent effect.

The size of the spillover bias in  $\text{plim}(\hat{\gamma}_2)$  is largely driven by the size of  $\gamma_1$  and is also increasing in  $\rho$ , so we would expect it to be more substantial in countries with higher levels of intergenerational persistence. Conversely, since we expect the grandfather coefficient  $\gamma_2$  to be small (if it is not zero),

we do not expect spillover to be a major contributor to bias in the parental coefficient estimate  $\hat{\gamma}_{1,OLS}$ . Rather, attenuation bias will still be the primary concern, and since  $\left(\frac{\sigma_x^2 + \sigma_v^2}{\sigma_x^2(1-\rho^2) + \sigma_v^2}\right) > 1$ , attenuation bias in the parental coefficient will be at least slightly worse in the multigenerational setting than it was in the intergenerational regression. In this case with stationarity, the attenuation factors and spillover factors are the same for parents and grandparents, so  $\omega_1 = \omega_2$  and  $\theta_1 = \theta_2$ . In theory, these could differ across generations without stationarity, and with some of the more realistic earnings processes we consider.<sup>10</sup>

Given that the equations above are based on the simple case of classical measurement error, IV using annual income in one year to instrument for another year would yield consistent estimates of  $\gamma_1$  and  $\gamma_2$ . In fact, multigenerational studies have used IV approaches to address measurement error, but have done so by using the outcome for grandparents to instrument for that for parents (Boserup *et al.*, 2014) or similarly have used great-grandparents to instrument for grandparents (Lindahl *et al.*, 2014). 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.

And, while the classical errors in variables scenario has been useful for exposition and for identifying methods to reduce bias in the intergenerational regression setting, studies recognize this is not realistic for the actual earnings process, especially to the extent that IV using consecutive annual incomes would provide consistent estimates. Considering the more realistic AR(1) process for the transitory component for parent and grandparent earnings, then  $\sigma_v^2$  is replaced with  $\frac{\sigma_e^2}{1-\delta^2}$  in the probability limits for the OLS estimators in (10a) and (10b). Or when we use T-year averages of annual income, each  $\sigma_v^2$  is replaced with  $(\sigma_v^2/T)\phi$ , where  $\phi$  is from equation (7).

Studies have shown that the transitory components are correlated over time, but generally disappear after about 3 years.<sup>11</sup> This means that annual earnings measures 4 or 5 (or more)

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<sup>10</sup>The probability limits from the multigenerational regression without assuming stationarity are provided in the Appendix.

<sup>11</sup>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.

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 other and are also uncorrelated with child's earnings. Hence, one approach we take is similar to Altonji & Dunn (1991), using parental annual earnings from one year to instrument for parents' earnings in a different year, and do the same for grandparents' earnings. Again using  $T = s - t$  to denote the number of years between the annual earnings measure used as an instrument (year  $s$ ) and treated as endogenous (year  $t$ ), the probability limits of the IV estimators for  $\gamma_1$  and  $\gamma_2$  are identical to equations (10a) and (10b) except that each  $\sigma_v^2$  is replaced with  $\delta^T \left( \frac{\sigma_e^2}{1-\delta^2} \right)$ . As with the intergenerational case, increasing  $T$  (years between the instrument and endogenous income measures) reduces attenuation bias.

We next turn to lifecycle related biases. The implications of age-related variation in the association between annual and permanent income for offspring is straightforward. Assuming we observe parents' and grandparents' permanent income, the multiplicative bias is the same as in the two generation regression,  $plim(\hat{\gamma}_1) = \lambda_{0\tau}\gamma_1$  and  $plim(\hat{\gamma}_2) = \lambda_{0\tau}\gamma_2$ , so to the extent that  $\lambda_{0\tau}$  is different from 1, both coefficient estimates are biased in the same direction by the same proportion. However, lifecycle bias arising from measurement of parent and grandparent income is more complicated, again leaving open the possibilities of attenuation or amplification bias. In this case, now assuming we observe permanent income for the offspring (and still maintaining stationarity), we distinguish between lifecycle effects with  $\lambda_{gt}$  for each generation ( $g = 1, 2$  for parents, grandparents):

$$plim(\hat{\gamma}_{1,OLS}) = \gamma_1 \frac{\lambda_{1t}\sigma_x^2}{\lambda_{1t}^2\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{2t}^2\sigma_x^2 + \sigma_v^2}{\lambda_{2t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} + \gamma_2 \frac{\lambda_{1t}\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\lambda_{2t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\lambda_{1t}^2\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{2t}^2\sigma_x^2 + \sigma_v^2}{\lambda_{2t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} \quad (11a)$$

$$plim(\hat{\gamma}_{2,OLS}) = \gamma_1 \frac{\lambda_{2t}\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\lambda_{1t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\lambda_{2t}^2\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{1t}^2\sigma_x^2 + \sigma_v^2}{\lambda_{1t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} + \gamma_2 \frac{\lambda_{2t}\sigma_x^2}{\lambda_{2t}^2\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{1t}^2\sigma_x^2 + \sigma_v^2}{\lambda_{1t}^2\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}. \quad (11b)$$

When using  $T$ -year averages of income,  $\lambda_{gt}$  and  $\sigma_v^2$  are replaced with  $\bar{\lambda}_{gT}$  and  $\sigma_v^2/T$ , respectively. So taking long-term averages during midlife helps to ensure that  $\bar{\lambda}_{gT} \approx 1$ . The other source of age-related bias is the U-shaped pattern in the size of  $\sigma_v^2$ . If the increase in  $\sigma_v^2$  is steep enough, then  $\sigma_v^2/T$  may 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  $T$ .

For IV, the noisier earnings measures with larger  $\sigma_v^2$  also leads to larger spillover and attenuation factors. And when considering lifecycle changes in  $\lambda_{gt}$ , the probability limits are slightly more complicated because we have to separately consider  $\lambda_{gt}$  for the income measure treated as endogenous and  $\lambda_{gs}$  for the income measure used as an instrument:

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1 \frac{\lambda_{1s}\sigma_x^2}{\lambda_{1s}\lambda_{1t}\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{2s}\lambda_{2t}\sigma_x^2 + \sigma_v^2}{\lambda_{2s}\lambda_{2t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} + \gamma_2 \frac{\lambda_{1s}\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\lambda_{2s}\lambda_{2t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\lambda_{1s}\lambda_{1t}\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{2s}\lambda_{2t}\sigma_x^2 + \sigma_v^2}{\lambda_{2s}\lambda_{2t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} \quad (12a)$$

$$plim(\hat{\gamma}_{2,IV}) = \gamma_1 \frac{\lambda_{2s}\sigma_x^2 \left( \frac{\rho\sigma_v^2}{\lambda_{1s}\lambda_{1t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}{\lambda_{2s}\lambda_{2t}\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{1s}\lambda_{1t}\sigma_x^2 + \sigma_v^2}{\lambda_{1s}\lambda_{1t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)} + \gamma_2 \frac{\lambda_{2s}\sigma_x^2}{\lambda_{2s}\lambda_{2t}\sigma_x^2 + \sigma_v^2 \left( \frac{\lambda_{1s}\lambda_{1t}\sigma_x^2 + \sigma_v^2}{\lambda_{1s}\lambda_{1t}\sigma_x^2(1-\rho^2) + \sigma_v^2} \right)}. \quad (12b)$$

Although  $\lambda_{gs}$  appears in these equations, it is  $\lambda_{gt}$  (for the endogenous measure) that matters more for lifecycle bias in IV estimates. So for both estimators it is important to measure income during the periods of life for which  $\lambda_{gt} \approx 1$  for each generation, which we do in our empirical approach. The implications of lifecycle bias are similar to what has been found for the intergenerational case; measuring income at too old of ages ( $\lambda_{gt} > 1$ ) leads to downward bias or at too young of ages ( $\lambda_{gt} < 1$ ) leads to amplification bias. We further discuss the implications of all of these measurement issues in the next section, where we perform simulations to illustrate the consequences of these biases in different scenarios.

### 3 Simulation

To quantify the implications of these biases in multigenerational regressions, we conduct simple simulations based on equations (10a)-(12b). We vary the parameters  $\rho$ ,  $\delta$ , and  $\lambda_{gt}$  to gauge the extent of these biases in a variety of plausible data generating scenarios, and assess the likelihood of estimating a spurious grandparent coefficient. Recall,  $\rho$  is the correlation between  $x_{ig}$  across generations and hence reflects different levels of intergenerational persistence in different societies. The parameters  $\delta$  and  $\lambda$  determine underlying earnings dynamics.  $\delta$  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 researchers' ability to reduce attenuation bias

with time-averaging or IV estimation.  $\lambda_{gt}$  reflects lifecycle variation in the association between lifetime and annual income in year  $t$  for generation  $g$ .

As above, we maintain stationarity. And similar to Mazumder (2005), we multiply through the above probability limits by the total variance of annual earnings,  $\sigma_{xt}^2$ , so that we only need to make assumptions about the variance shares  $\frac{\sigma_v^2}{\sigma_{xt}^2}$  and  $\frac{\sigma_x^2}{\sigma_{xt}^2}$  to calculate the attenuation and spillover factors ( $\theta$  and  $\omega$ ).<sup>12</sup>

### 3.1 Illustrating attenuation and spillover bias

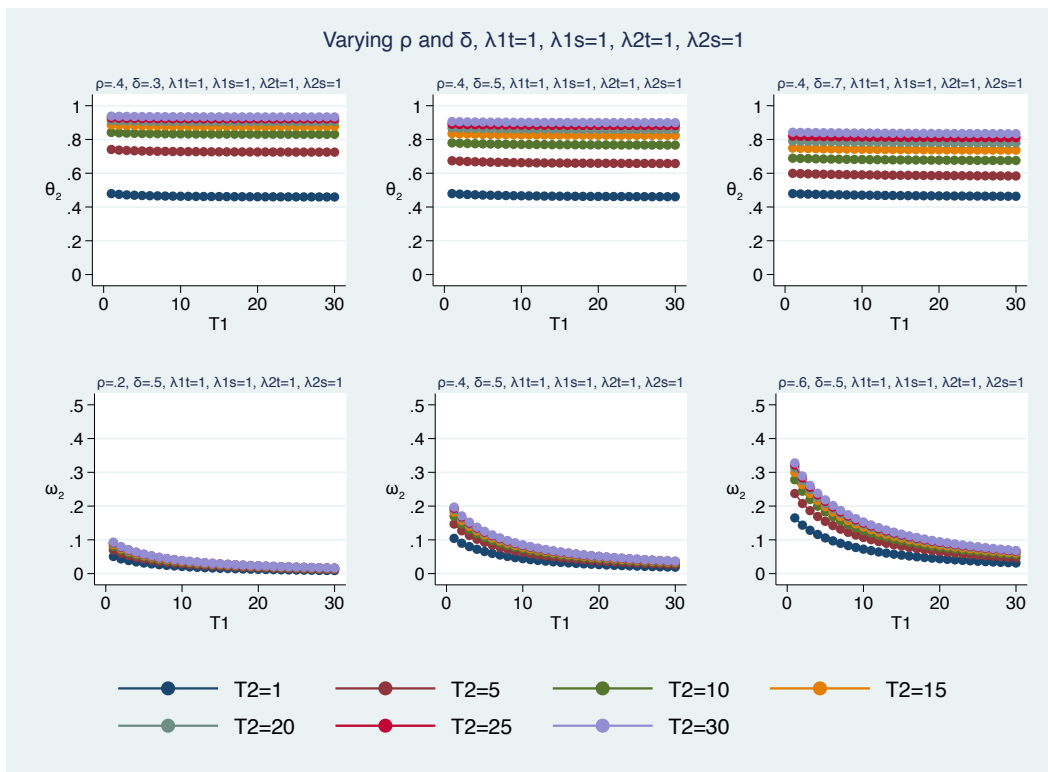
We consider several different scenarios, varying  $\delta$  (0.3, 0.5, 0.7),  $\rho$  (0.2, 0.4, 0.6), and  $\lambda_{gt}$  (0.8, 1, 1.2). We set the variance shares at  $\frac{\sigma_v^2}{\sigma_{xt}^2} = \frac{\sigma_x^2}{\sigma_{xt}^2} = 0.5$  for our base case, but also set  $\frac{\sigma_v^2}{\sigma_{xt}^2} = 0.7$  for a robustness check. For a given set of these parameters, we vary the number of years we average over 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 these scenarios for pedagogical purposes, focusing on biases in the grandparent coefficient using  $\rho = 0.4$ ,  $\delta = 0.5$ , and all  $\lambda_{gt} = 1$  as a base case. These scenarios are in the middle columns of Figures 1 (OLS) and Figure 2 (IV), where each dotted line corresponds to a different  $T_2$  (changing the grandparent income measure) whereas moving along one of these dotted lines from left to right corresponds to increasing  $T_1$  (improving the parental income measure).

Figure 1 shows the bias factors in the OLS estimate of the grandparent coefficient when we use time-averages of income. For our base case of  $\rho = 0.4$ ,  $\delta = 0.5$ , time-averaging reduces attenuation bias from about 52% ( $\theta_2=0.48$ ) when using annual income ( $T_2 = 1$ ) to about 10% ( $\theta_2=0.90$ ) with a 30-year average ( $T_2 = 30$ ). The set of graphs in the top row of Figure 1 shows the calculated attenuation coefficient for grandparents ( $\theta_2$ ) for different values of  $\delta$ . On the left, we can see that a smaller  $\delta$  (0.3) implies that time-averaging is more effective at reducing attenuation bias, a result that has already been shown for intergenerational regressions (Mazumder, 2005). The graph on the right shows that a larger  $\delta$  means time averaging is less effective. In all cases, as we improve grandparents' income measure (going from one line to another), attenuation bias is reduced. Improving the parental income measure (going from left to right along each line, increasing  $T_1$ ), does not help reduce attenuation bias in the grandparent coefficient. Similarly,  $\rho$  has little impact

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<sup>12</sup>Also following Mazumder (2005), we assume  $\sigma_e^2$  adjusts so that  $\sigma_v^2 = \frac{\sigma_e^2}{1-\delta^2}$  holds.

Figure 1: Attenuation ( $\theta_2$ ) and spillover ( $\omega_2$ ) bias in OLS coefficient for grandparent



on the attenuation bias, so we do not show the attenuation coefficients with different  $\rho$  here, though these results are available upon request.

The issue of spillover bias in the grandparent coefficient, however, is present because of  $\rho$ . For our base case of  $\rho = 0.4$ ,  $\delta = 0.5$ , time-averaging reduces the spillover coefficient from about 10% ( $\omega_2=0.1$ ) when using annual incomes for both generations to about 4% ( $\omega_2=0.04$ ) with a 30-year average for each generation. We know  $\rho > 0$  from the well documented results on parent-child mobility in many countries. The size of  $\rho$ , along with the parent coefficient  $\gamma_1$ , determine the size of the overall spillover bias. As shown in the bottom row of Figure 1, when  $\rho$  is small (0.2), the spillover coefficient  $\omega_2$  is also somewhat small. When we triple  $\rho$  to 0.6 the extent of spillover also approximately triples for shorter-term averages of parent's income (i.e, small  $T_1$ ). This combined with the fact that  $\gamma_1$  is also likely larger for countries with large  $\rho$  implies that OLS estimates for such societies are more susceptible to spurious grandparent effects.

There are other important patterns to note also. First, for a given parental income measure, attempting to reduce potential attenuation bias in the grandparent coefficient by including more



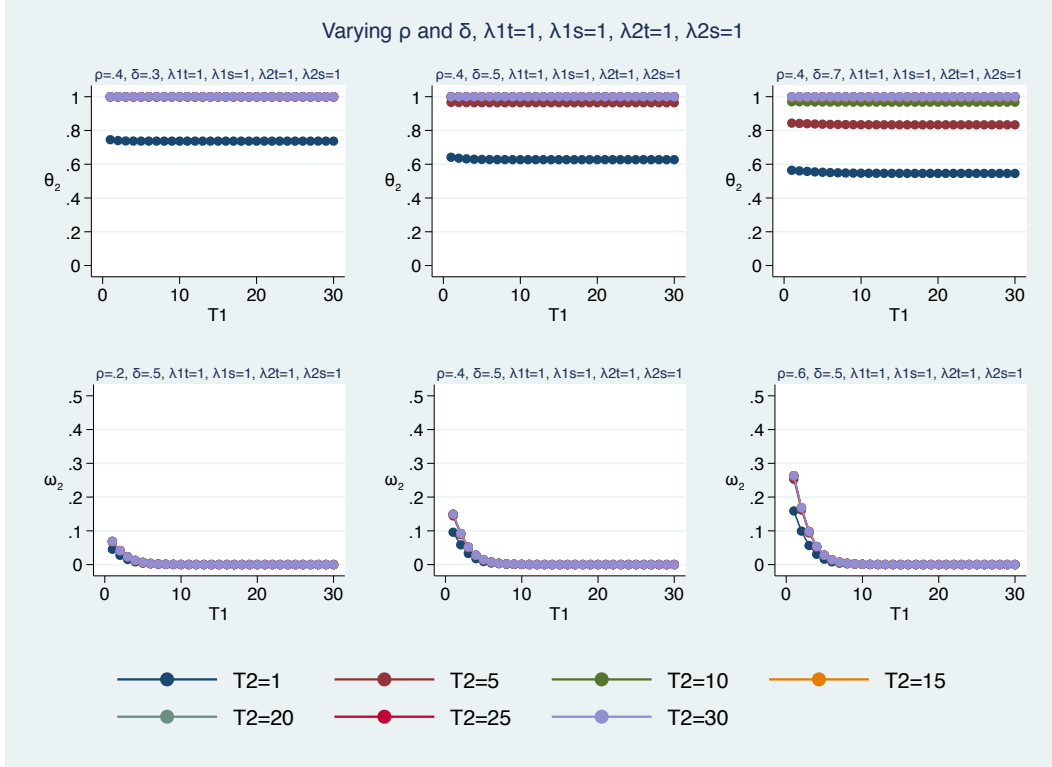
years in the average income measure for grandparents actually worsens the spillover bias. In  $\omega_2$ , time-averaging for the grandparent implies replacing the only  $\sigma_v^2$  outside of parenthesis by  $\sigma_v^2/T_2$ , which effectively shrinks the denominator thereby increasing  $\omega_2$ . (This can be seen explicitly in the more detailed probability limits in the Appendix.) So if the true  $\gamma_2 = 0$ , there is no attenuation to be concerned about, and the time-averaging for grandparents is actually creating a spurious small positive grandparent coefficient. This is illustrated below in Figure 3(b). Second, in countries with large  $\rho$  it takes far more years of observed income for parents 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.

Although this can be problematic for OLS estimation, there are more promising results for relatively small  $T$  with IV estimation. Figure 2 presents the computed attenuation ( $\theta_2$ ) and spillover ( $\omega_2$ ) factors for IV estimation using an annual income measure in year  $s$  to instrument for that in year  $t$ . In the figures,  $T_g$  indicates the difference in years ( $s-t$ ) between the instrument and endogenous measure for parents ( $g = 1$ ) and grandparents ( $g = 2$ ). First, although attenuation bias is again worse the larger  $\delta$  is, it can be nearly eliminated using income measures in a relatively short time period (up to about 10 years with high  $\delta$ ). When  $T_1 = T_2 = 1$ ,  $\theta_2$  is about 0.65 and reaches about 0.99 at  $T_1 = T_2 = 6$ . Second, the spillover bias is only slightly smaller than OLS with very small  $T_g$ , at about 0.1, but is nearly eliminated with only about a 6-year timespan of income for parents and grandparents, giving  $\omega_2 < 0.01$ . Although the spillover is again worse with larger  $\rho$ , it is still eliminated with relatively short timespans of income.

These simulation results are enlightening for multigenerational regressions, but have abstracted from the two age-related sources of bias: the lifecycle variation in  $\sigma_v^2$  and in the association between annual and lifetime income ( $\lambda_{gt}$ ). The implications of the former are fairly straightforward. A larger transitory variance share means a noisier income measure, so for each  $T_g$ , the attenuation factor is smaller (meaning worse attenuation bias). The spillover factor tends to be similar for small  $T$ , but then increase for large  $T$ . For example, if we set  $\frac{\sigma_v^2}{\sigma_x^2} = 0.7$  so  $\frac{\sigma_x^2}{\sigma_{xt}^2} = 0.3$ , then time-averaging over 30 years for OLS only reduces the attenuation bias to 20%. And the spillover coefficient is only reduced to 0.07 with a 30-year average income measure. For IV, the implications are less severe. At  $T_g = 6$ , the spillover coefficient is below 0.02 and the attenuation coefficient reaches 0.98.

The implications of lifecycle variation in the association between annual and lifetime income,

Figure 2: Attenuation ( $\theta_2$ ) and spillover ( $\omega_2$ ) bias in IV coefficient for grandparent



reflected by  $\lambda_{gt}$ , are more complicated. See Appendix Figures B.1-B.4 for the attenuation and spillover factors. The attenuation coefficient for OLS follows the same patterns found in previous studies for the intergenerational regression. When  $\lambda_{gt} > 1$ , as is the case for annual incomes measured at older ages, attenuation bias is worse ( $\theta_g$  is smaller). When income is measured at younger ages, so  $\lambda_{gt} < 1$ ,  $\theta_2$  can be larger than one which means there is amplification bias rather than attenuation.

The spillover factor ( $\omega_g$ ) is larger when  $\lambda_{gt} < 1$  and smaller when  $\lambda_{gt} > 1$ , reinforcing the attenuation or amplification bias from  $\theta_g$ . Considering the combined effect of the lifecycle effects on  $\theta_g$  and  $\omega_g$ , the OLS coefficient estimates of  $\gamma_g$  are possibly biased upward when  $\lambda_{gt} < 1$  and likely biased downward when  $\lambda_{gt} > 1$ . Standard practice is to measure income at ages when  $\lambda_{gt} \approx 1$ , but taking long-term averages of income can extend into age ranges where  $\lambda_{gt} \neq 1$ , although when extending ages symmetrically in both directions it is plausible that the  $\bar{\lambda}_{gT}$  remains around one.

With IV estimation, it is the age at which the endogenous earnings is measured that drives lifecycle bias. If  $\lambda_{gt} < 1$ , this can result in substantial amplification bias even after increasing  $T_g$ ,

while  $\lambda_{gt} > 1$  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.

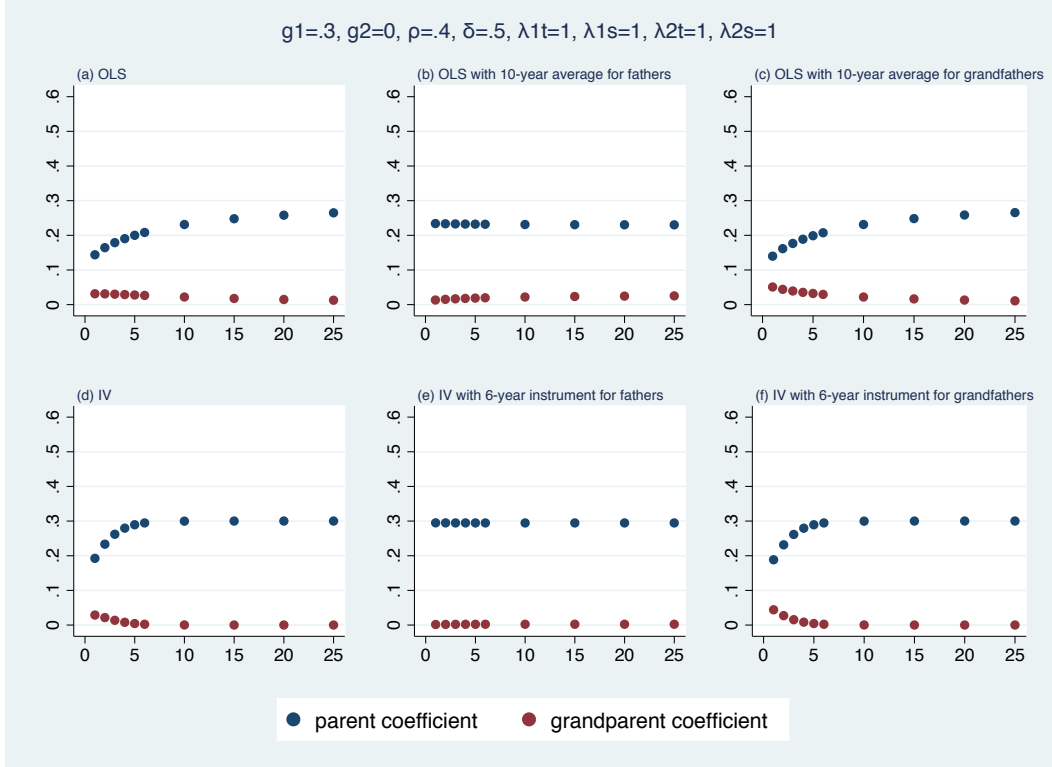
We focused most of our discussion here on the biases in the grandparent coefficient, which is of primary interest here. The analogous results for  $\theta_1$  and  $\omega_1$  for parents are the same by design (available upon request). However, the extent to which the spillover ( $\omega_1$ ) bias affects the magnitude of our coefficient estimate depends on the size of  $\gamma_2$ . Since this is presumably small relative to  $\gamma_1$ , spillover bias will not generally be extremely problematic for the parent coefficient so changing  $T_2$  does not have appreciable impacts on our estimate of  $\gamma_1$ . Rather, addressing the attenuation bias is the main issue, as is customary with intergenerational income regressions.

### 3.2 Illustrating a spurious grandfather coefficient

To illustrate the consequences of the biases in Figures 1 and 2 for the actual estimates researchers obtain, we next present figures with the corresponding coefficient estimates of  $\gamma_1$  and  $\gamma_2$  for our base case where  $\rho = 0.4$ ,  $\delta = 0.5$ . We choose  $\gamma_1 = 0.3$  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.

In Figure 3, the x-axis indicates the number of years used in the time-averages of income for OLS or the difference in years between the instrument and endogenous earnings measure for IV. For 3(a) and 3(d), we treat the measures for parents and grandparents symmetrically so  $T_1=T_2$ . The estimates in Figure 3(a) 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  $\gamma_2 = 0$ .) However, even with a 25-year average of income for parents, attenuation bias still remains. In 3(b), we isolate the effects of changing the grandparent measure by using what would be considered a reasonable measure for parent’s income—a 10-year average. This illustrates the fact that improving the grandparent income measure is causing an increase in  $\hat{\gamma}_2$ , 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  $\omega_2$ , 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  $T_1$

Figure 3: IV and OLS coefficients when  $\rho = 0.4$ ,  $\delta = 0.5$ ,  $\gamma_1 = 0.3$ ,  $\gamma_2 = 0$



for parents. The coefficient for parents increases as we reduce attenuation bias by averaging over more years, while the coefficient estimate for grandparents decreases as the spillover bias is reduced.

Turning to the IV estimates in 3(f), we see that instrumenting essentially eliminates the attenuation and spillover around  $T = 6$  years, and this holds across the other two treatments of solely changing the income measures for parents and grandparents in 3(d) and 3(f) as well. For IV, using a satisfactory instrument for parents (e.g.,  $T_1 = 6$ ) eliminates bias. In 3(f) we see that using a “good” measure for grandparents causes worse spillover when we do not use a good enough instrument for fathers ( $T_1$  is not large enough). 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 these implications of these biases in practice.

## 4 Data and empirical results

### 4.1 Data

For our empirical analysis, we use administrative data from Norway. This data has a uniquely long full-population coverage of tax data, 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 2014). This age range is selected to minimize lifecycle bias, while also allowing for averaging over multiple years of annual income to reduce error variance. Fathers and paternal grandfathers are then identified using the population register. We use a slightly higher age range (see below) for fathers and grandfathers because of data availability and the ages are consistent with attempting to avoid lifecycle bias ( $\lambda_{gt} \sim 1$ ) based on evidence in Nilsen *et al.* (2012). 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 (requiring 3 or more years of positive earnings, although 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.

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

	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
<i>N</i> (unique individuals)	5,064	9,831	171,939	335,155	171,939	335,155
<b><i>Fathers' generation</i></b> ( <i>Birth year range: 1950-1958</i> )						
Mean income	281,787	284,347	267,366		269,213	
Std. dev of income	136,513	137,667	195,184		199,443	
<i>N</i> (unique individuals)	4,673	8,451	292,288		292,288	
<b><i>Father's fathers' generation</i></b> ( <i>Birth year range: 1928-1935</i> )						
Mean income	201,850	202,197	194,142		203,323	
Std. dev of income	70,656	70,299	90,290		96,932	
<i>N</i> (unique individuals)	4,455	7,790	164,825		164,825	

Table 1: Descriptive statistics. Sample restriction: Birth cohorts 1974-1978, 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.

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 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 most appropriate to focus on sons, fathers and grandfathers in our analysis.<sup>13</sup>

## 4.2 Empirical approach

We estimate a series of intergenerational and multigenerational regressions to examine the influence of grandparents on their grandchildren’s earnings, and, in particular, look at the implications of the income measurement issues in the multigenerational model. In all models, the dependent variable is the 5-year average of log income for sons over ages 32-36. We also include dummy variables for the index generation’s year of birth. We begin by estimating two-generation models including son-father regressions, father-grandfather regressions, as well as son-grandfather regressions:

$$y_{i0} = \beta_1 x_{i1} + \epsilon_i \tag{13}$$

$$y_{i1} = \beta_2 x_{i2} + \epsilon_i \tag{14}$$

$$y_{i0} = \beta_3 x_{i2} + \epsilon_i \tag{15}$$

From these, we obtain several estimates of father-son associations, grandfather-father associations, as well as grandfather-son associations that are not conditional on fathers’ income. Since we are using log income (or averages of log income) as our income measures, these coefficients also have the convenient 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  $x_{i1}$  and  $x_{i2}$ . We first estimate these models using 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. Next, we turn to IV estimation using

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<sup>13</sup>The results for samples including daughters are similar, and are discussed below (Section 4.4).

annual log income measures 2 - 6 years apart as the instrument and endogenous regressors, again extending to 10, 15, 20, and 25 years for longer time distances between incomes. While many of the two-generation OLS results have been shown in prior studies, we use these regressions to show consistency of our results with these and to compare them to our IV estimates as well as to our estimates from the multigenerational regressions.

The multigenerational regression we estimate for the conditional association between grandfathers' income and their grandchild's income is:

$$y_{i0} = \gamma_1 x_{i1} + \gamma_2 x_{i2} + \epsilon_i \tag{16}$$

We vary the income measures and estimation method used for this model in the same way described for the two-generation models. Then, to clearly illustrate the bias spillover implications in the multigenerational regression, 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 father’s income—in our case, the 10-year average of log income—and then vary how grandfather’s income is measured as described above, using OLS to estimate the models. Second, we do the same exercise using the long-term average of grandfather’s log income, but varying how father’s income is measured.

Next we use analogous approaches with IV estimation. We first vary the instruments from 2-6 years (and 10+ years when sufficient samples sizes remain) simultaneously for both fathers and grandfathers. Then we isolate the effects of changing the grandfather IV approach by using the 6-year instrument for fathers while varying that for grandfathers. Finally we illustrate the spillover bias in the grandfather coefficient by using the 6-year instrument for grandfathers while varying the instrument for fathers.

### 4.3 Main results

To examine income mobility across generations in Norway, we begin by showing results for two generation regressions to illustrate the results of various approaches in this well known setting. Then we turn to the multigenerational setting, which is of primary interest in this paper, to show how the biases and methods to alleviate them play out in these models. The three-generation



results exhibit similar consequences for fathers' coefficients in the multigenerational setting, but also show that bias spills over into the coefficient for grandfathers.

For both the two- and three-generation models, we begin with the naive approach of using OLS to estimate models with single annual income measures for fathers and grandfathers, and then proceed to use the now standard approach of using long-term averages of income, which has been shown to reduce attenuation bias from the transitory components. Finally, we use our IV approaches which allow for varying degrees of persistence in the transitory component of annual income. All results presented are based on a balanced sample of 5,064 sons matched to their fathers and paternal grandfathers, unless otherwise noted, and use sons' average log income over ages 32-36 as the dependent variable.

### 4.3.1 Two-generation regression results

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

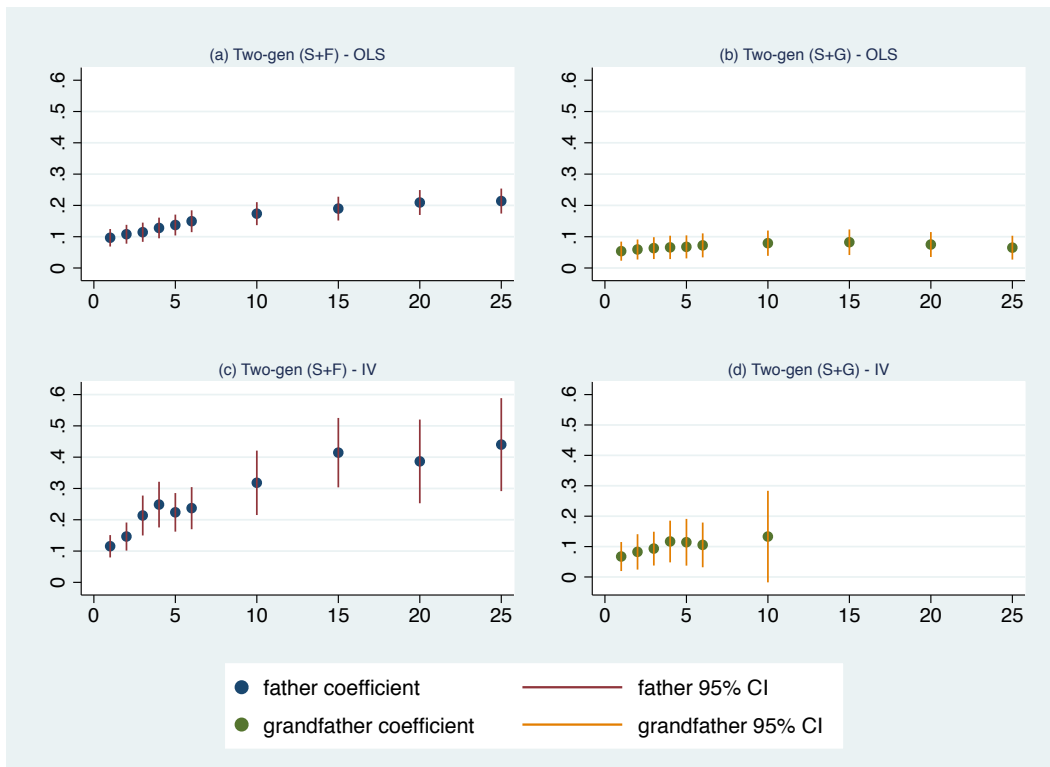


Figure 4 provides OLS estimates (top panel) and IV estimates (bottom panel), along with 95% confidence intervals, from the two-generation models in equations (13) and (15). Figure 4(a)

provides father-son intergenerational income elasticities, starting with the first estimate based on using annual log income measures for fathers, and then proceeding to the right with log income averaged over an increasing number of years. Each of the income measures is centered around age 43, expanding symmetrically as the number of years in the average increases. As we average over more years of log income for fathers, we see the expected pattern of IGEs increasing (ranging from about 0.10 to 0.21). This illustrates the established result that averaging mitigates the attenuation bias from using annual or short-term income measures as a proxy for permanent income.

One concern that remains even after taking long-term averages of log income is that this does not eliminate bias from the persistence in the transitory component of annual income. Therefore, we next use an IV approach using one log annual income measure to instrument for another. If there is no persistence in  $v_{itg}$ , this method produces consistent estimates regardless of the years serving as the instrument or endogenous variable (assuming they are mid-life to avoid lifecycle bias). To further allow for varying degrees of persistence, we vary how many years apart the instrument and endogenous measure are. Essentially, we are using the first annual log income measure in the corresponding multi-year average as our instrument, and the measure  $T$  years later as our instrument. The x-axis indexes the number of years after the “endogenous” income measure that we measure the “instrument” income, with all being centered around age 43 to minimize lifecycle bias.

To the extent that the transitory component is persistent over time, we expect the estimates to increase as we proceed left to right across Figure 4(c); increasing the years between the endogenous measure and instrument will reduce the attenuation bias as the correlation in the transitory component falls over time. 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 0.24 when using income measures 6 years apart, and 0.32 when using measures 10 years apart. Although these estimates are substantially higher than our OLS estimates, they are less precise as is characteristic of IV estimates, and they are comparable in magnitude to prior IGE estimates of about 0.34 found for Norway by Nilsen *et al.* (2012).<sup>14</sup>

The longer term estimates are based on subsets of our main sample which contributes to the im-

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<sup>14</sup>Modalsli (2016) found slightly lower persistence (0.14) in rank-rank intergenerational regressions, but these estimates were based on incomes at younger ages (28-32) so the smaller estimate is expected.

precision (97.3%, 94.7%, 92.5%, 87.3%, respectively, for the 10-, 15-, 20-, and 25-year estimates).<sup>15</sup> With these longer timespans possibly extending into ranges where  $\lambda_{gt} < 1$  (as the endogenous age decreases), the estimates around 0.4 may suffer from amplification bias. Our “reverse IV” results (using the younger age as the instrument) support this, as the estimates are substantially smaller around 0.3, and these may be attenuated. Taken together, our IV and reverse IV estimates could be considered lower and upper bounds on the true parameter, and this range of 0.3 to 0.4 is consistent with existing evidence for Norway.<sup>16</sup>

Figures 4(b) and 4(d) provide the analogous results for equation (15) relating sons’ income to 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, whether in the form of increasing  $\bar{\lambda}_{2T}$  or increasing  $\sigma_{vt}^2$  for these cohorts found by Nilsen *et al.* (2012).

The IV estimates in Figure 4(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 0.11. The estimate based on a 10-year distance between measures is similar at 0.13, but the sample is substantially smaller (N=3,535, 69.0%). The samples were even smaller for the 15-, 20-, and 25-year estimates so we omit these results out of concern regarding strong sample composition effects.

Naturally the grandfather-son associations should be smaller than the father-son associations. We can compare these results though, using the simple extrapolation discussed earlier, where  $\beta_1^2$  is used to approximate longer term persistence. If we use our largest OLS estimate, we get  $0.21^2 = 0.04$ , or with the largest IV estimate,  $0.40^2 = 0.16$ . Comparing these to our largest OLS and IV estimates from the son-grandfather regressions, which are 0.08 and 0.13, respectively, we cannot conclusively say whether the mobility predicted by the traditional intergenerational model understates persistence.

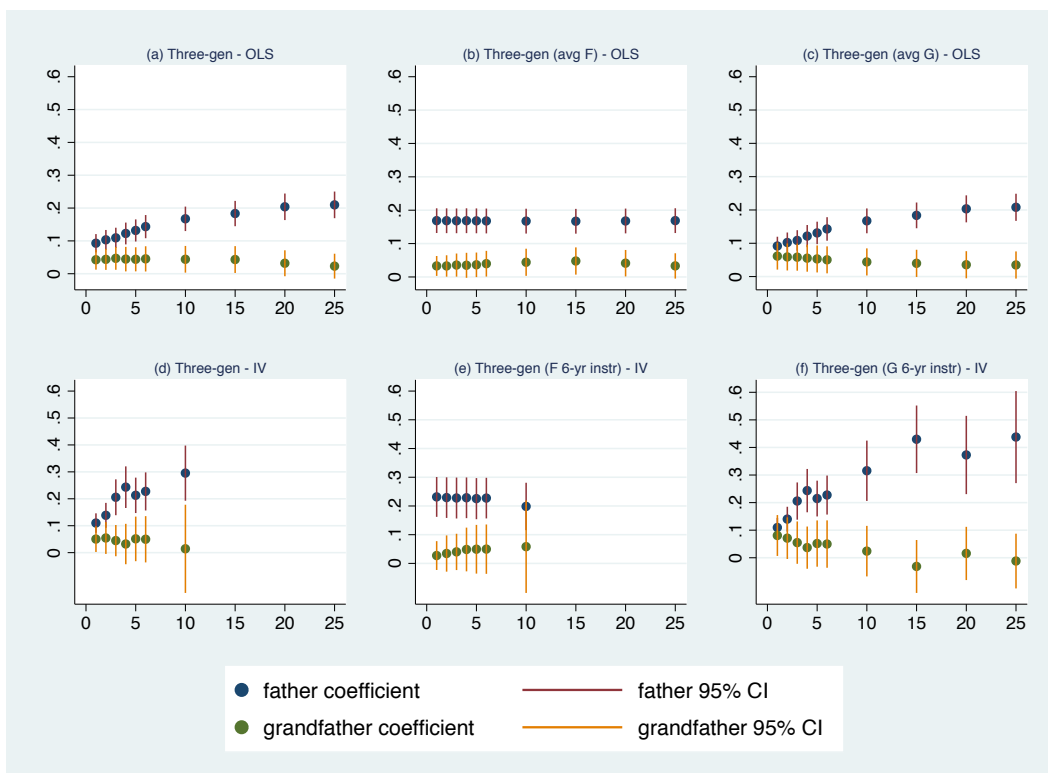
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<sup>15</sup>Sensitivity checks do not indicate that sample composition is driving these higher estimates.

<sup>16</sup>The results for fathers and grandfathers based on equation (14) are very similar to the father-son regressions (results available upon request). The OLS estimates range from 0.11 to 0.21 and the IV estimates range 0.15 to 0.25 for the 1 to 6 years between income measures, and rise to 0.46 with 10 years, though this is based on a smaller sample (N=3,262).

### 4.3.2 Three-generation regression results

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



We now turn to the multigenerational regression results. First, we conduct the same exercise as in Figure 4, where we take averages of log income, increasing the number of years that we average over symmetrically for fathers and grandfathers income. However, it is less clear what we expect to see in the grandfather coefficients because there are two competing biases. There is attenuation bias from measurement error in “own” income, yet there is an upward bias from measurement error in fathers’ income. In our simulation, we found that the reduction in the attenuation factor ( $\theta$ ) from improving one’s own income measure was more meaningful than the reduction in the spillover factor ( $\omega$ ) from improving the other generation’s income measure. Empirically, as shown in Figure 5, the attenuation bias decreases in the coefficient on fathers’ income, as the coefficient estimate increases from about 0.09 to 0.21 as we average over more years. These estimates are also similar to what we found for the father-son regression results. But the coefficient on grandfathers’ income fluctuates around 0.04-0.05, when going from annual measures to averaging over 15 years of income. The point estimates decrease slightly to 0.03 and 0.02 for the 20- and 25-year measures, and neither

of these is statistically significantly different from zero.

Given the strong similarity between the father coefficient estimates for the three generation models to the analogous estimates from the father-son models—for both the OLS and IV estimates—it appears that there is little or no spillover bias in the father coefficients from the measurement error in the grandfather income measures. This is also consistent with a very small (or zero) 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. We first use a “good” measure of father’s income (10-year average) throughout all models, while changing grandfathers’ income measure as before. This allows us to focus on how changing the grandfather measure affects the coefficient estimate for fathers. The OLS coefficient estimate for fathers remains essentially constant at 0.17 as we go from using annual income to longer-term averages for grandfathers income, indicating no spillover bias in the coefficient estimate for fathers, which again is consistent with the true  $\gamma_2$  being zero (or very small).

Further, comparing these OLS results in 5(b) to the results in 5(a) from simultaneously improving fathers’ income measures, we can confirm that spillover bias is present in the grandfather coefficient estimate. First, note that the estimates in 5(b) for using the 10-year average (so  $T_2 = 10$  also) are identical to the 10-year average results in 5(a) by construction. Focusing on  $T_g < 10$ , we see the grandfather coefficient estimates are larger when  $T_1$  also varied from 1 to 10 compared to when the 10-year average is used for fathers throughout. This implies the spillover bias from using the worse income measure for fathers ( $T_1 < 10$ ) led to a larger grandfather coefficient estimate in 5(a). The grandfather coefficient estimates for  $T_g > 10$  are consistent with this as well, as they are smaller than when the better income measures (i.e., longer term averages) were also used for fathers in 5(a). This is clear evidence of spillover bias, and also shows that even using a 10-year average to measure income for fathers is not sufficient to rule out spillover bias causing a spurious positive grandfather coefficient.

Next, we perform a similar exercise in 5(c) only now varying fathers’ income measures while we hold 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 5(a).

For grandfathers, the coefficient of 0.06 in 5(c) when fathers' income is measured using annual log income is larger than the 0.04 estimate in 5(a) when an annual measure was also used for grandfathers and then the estimates decrease as we improve the measure for fathers. These results again show evidence of spillover bias from measurement error in fathers' income, and the fact that for a given  $T_1$ , spillover is worsened by improving the measure for grandfathers (increasing  $T_2$ ). One might be tempted to interpret this as lower attenuation bias from using  $T_2 = 10$  in 5(c), but the fact that the grandfather coefficient estimates decrease as we increase  $T_1$  (holding  $T_2$  constant at 10) indicates that spillover bias is driving the underlying difference. This is also consistent with our simulation results where the true grandfather effect was zero and we improved father's measure to reduce the spillover. Based on our OLS results, we cannot decisively rule out that the grandfather effect in this sample is spurious and solely an artifact of measurement error in fathers' (average) income. If there is persistence in the transitory component of income, even our OLS estimates based on a 25-year average of log income are likely still biased.

Our IV approach will produce consistent estimates in this setting when  $T_g$  is large enough for the degree of persistence in  $v_{itg}$ . The bottom graphs in Figure 5 present the IV results analogous to those for OLS above. First, in 5(d) we 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 as indicated on the x-axis. The coefficient for fathers' income increases from 0.11 to 0.30 as we increase the number of years between the endogenous/instrument income measures, similar to the father-son IV results. The coefficient for grandfathers' income fluctuates around 0.03-0.05 for the 1-6 year measures, and is not statistically significant for the 2-6 year estimates. The 10-year estimate is smaller at 0.01 but is also based on a smaller sample (N=3,449, 68.1%). However, replicating all of Figure 5 for this smaller sample reveals similar patterns, so sample composition does not appear to be driving this small estimate. In general though, the pattern of increasing father coefficients and decreasing grandfather coefficients as we improve the income measures is also consistent with spillover bias from a poor income measure for fathers causing an upward bias in the grandfather coefficient.

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 pattern of results is similar to the analogous OLS results, with the coefficient on fathers' income remaining steady, though at a slightly higher level of 0.23. The coefficient for grandfathers is never statistically significantly different from zero, but does increase slightly as we increase  $T_2$ . To check for lifecycle effects, we turn to our reverse IV results (Appendix Figure C.6). With the income measure at older ages treated as endogenous (so  $\lambda_t > 1$ ), the pattern is reversed and the grandfather coefficient is declining as we increase  $T_2$  (and is closer to zero).

We next 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. In Figure 5(f) the coefficient on fathers' income rises from 0.11 to 0.32 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 5(d). Although the coefficient on grandfathers' income fluctuates, on average we do see it decreasing as we improve the measure for fathers' income, ranging from 0.08 to -0.03. Notably the coefficient is negative in sign for a couple of the longer-term scenarios, but the grandfather coefficient is not statistically significantly different from zero in any of these regressions. The reverse IV results are similar though the father coefficient fluctuates around 0.3 for the longer-term averages, similar to the father-son reverse IV results.

Overall, our OLS and IV results do not decisively reject that that true grandfather coefficient is zero or very small. The OLS estimates based on longer-term averages are around 0.02-0.03. The IV estimates based on longer-term instruments are also near zero, with some being negative in sign. None of the longer-term estimates are statistically significantly different from zero, and 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 fluctuate more, likely due to sensitivity to lifecycle effects, and also suggest persistence in the transitory component of income matters. Regardless, our results show that empirically researchers must be aware of the potential for a misleadingly large grandfather coefficient estimate.

## 4.4 Robustness checks

The above 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. To examine whether our results are sensitive to only including men, we have also conducted our analyses on the sample with both men and women in the final generation, as well as on a grandfather-father-daughter sample.

Figures 4 and 5 are replicated in the Appendix for the full sample (Figures C.7 and C.8). In general, the coefficients for the full sample (men and women) are slightly lower than for men only, and more precise. The reduced level reflects generally lower intergenerational persistence typically found for samples including women, while the higher precision follows from the increased sample size. The patterns in the coefficients are nearly identical to our results based on sons only. The only divergence is the 10-year IV estimates, and based on the reverse IV results for this sample, this appears to be an artifact of lifecycle bias.

## 5 Conclusion

The role of measurement error in estimating intergenerational income regressions has been addressed extensively in the literature, and the resulting biases are fairly well understood—it has become standard to take steps to mitigate these biases. Even though some bias remains, we often have a sense of the size or at least direction of the remaining bias. However, the implications of measurement error in the multigenerational regression are more complicated and are becoming increasingly important as studies focus on estimating the conditional effect of grandparents to better understand the rate of mobility over generations. This paper illustrates the implications of measurement error in this setting, showing that the spillover of bias from measurement error in the parents' income measures could lead to misleading conclusions regarding the effects of grandparents. Our simulations show that even using a long-term average of income over 10 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 leading to increasing coeffi-



cient estimates; this could be misinterpreted as reducing attenuation bias in actual data settings where we do not know the true grandparent coefficient is zero. The IV approach we propose has the advantage of mitigating (or eliminating) these biases with relatively short timespans of income, depending on the degree of persistence in the transitory component of income. And, although the IV estimator is more susceptible to lifecycle bias, once can easily test for this using the “reverse IV” approach, which combined with the original IV estimates also provides bounds on the coefficients.

With our administrative data, we see the expected result that time-averaging reduces attenuation bias in OLS estimates, and we also show similar results for IV approaches that allow for persistence in the transitory component of annual income measures. Our sample is a subset from Norway that had data sufficient for our empirical approach, but our intergenerational results are in line with prior studies using larger samples from Norway. In the multigenerational setting, we showed how the spillover of bias from measurement issues in fathers’ income causes upward bias in the coefficient for grandfathers’ income. 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 also consistent with this, and, although the estimates are imprecise, they leave open the possibility of a zero grandfather coefficient, or even a negative one, as predicted by Becker & Tomes (1979).

Exploring transmission of socioeconomic status beyond two generations is an important direction in the literature, but researchers need to be even more cautious about biases from measurement error than in the intergenerational setting. We focused on measurement issues with income in this paper, but measurement issues arise with all other status measures used as well. So, although the theoretical results presented here are based on models specific to earnings dynamics, 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 below  $\sigma_{x_g}^2$  and  $\sigma_{v_g}^2$  are allowed to vary across generations ( $g = 1, 2$ ).

In the population, the true multigenerational process is:

$$y_{i0} = \gamma_1 x_{i1} + \gamma_2 x_{i2} + \epsilon_i. \quad (17)$$

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

$$x_{it1}^* = x_{i1} + v_{it1}, \quad (18a)$$

$$x_{it2}^* = x_{i2} + v_{it2}. \quad (18b)$$

So the equation we estimate with our data is:

$$y_{i0} = \gamma_1 x_{it1}^* + \gamma_2 x_{it2}^* + \epsilon_{it}^*. \quad (19)$$

## A.1 OLS estimation

We can derive the OLS estimator of  $\gamma_1$  using the Frisch-Waugh-Lovell theorem and some algebra:

$$\hat{\gamma}_{1,OLS} = (x_1^{*'} M_2 x_1^*)^{-1} x_1^{*'} M_2 y \quad (20a)$$

$$= [x_1^{*'} (I - x_2^* (x_2^{*'} x_2^*)^{-1} x_2^{*'}) x_1^*]^{-1} x_1^{*'} (I - x_2^* (x_2^{*'} x_2^*)^{-1} x_2^{*'}) y \quad (20b)$$

$$= [x_1^{*'} x_1^* - x_1^{*'} x_2^* (x_2^{*'} x_2^*)^{-1} x_2^{*'} x_1^*]^{-1} [x_1^{*'} y - x_1^{*'} x_2^* (x_2^{*'} x_2^*)^{-1} x_2^{*'} y] \quad (20c)$$

$$= \left[ \sum_{i=1}^N x_{i1}^{*2} - \sum_{i=1}^N x_{i1}^* x_{i2}^* \sum_{i=1}^N x_{i2}^{*2} \sum_{i=1}^N x_{i2}^* x_{i1}^* \right]^{-1} \left[ \sum_{i=1}^N x_{i1}^* y_i - \sum_{i=1}^N x_{i1}^* x_{i2}^* \sum_{i=1}^N x_{i2}^{*2} \sum_{i=1}^N x_{i2}^* y_i \right] \quad (20d)$$

⋮

$$\hat{\gamma}_{1,OLS} = \frac{\sum_{i=1}^N x_{i1}^* y_i \sum_{i=1}^N x_{i2}^{*2} - \sum_{i=1}^N x_{i1}^* x_{i2}^* \sum_{i=1}^N x_{i2}^* y_i}{\sum_{i=1}^N x_{i1}^{*2} \sum_{i=1}^N x_{i2}^{*2} - \left( \sum_{i=1}^N x_{i1}^* x_{i2}^* \right)^2} \quad (20e)$$

Similarly, for  $\gamma_2$ , we get:

$$\hat{\gamma}_{2,OLS} = \frac{\sum_{i=1}^N x_{i2}^* y_i \sum_{i=1}^N x_{i1}^{*2} - \sum_{i=1}^N x_{i1}^* x_{i2}^* \sum_{i=1}^N x_{i1}^* y_i}{\sum_{i=1}^N x_{i1}^{*2} \sum_{i=1}^N x_{i2}^{*2} - \left( \sum_{i=1}^N x_{i1}^* x_{i2}^* \right)^2} \quad (21)$$

Taking the probability limits gives us:

$$plim(\hat{\gamma}_{1,OLS}) = \frac{cov(y, x_1^*) var(x_2^*) - cov(y, x_2^*) cov(x_1^*, x_2^*)}{var(x_1^*) var(x_2^*) - cov(x_1^*, x_2^*)^2} \quad (22a)$$

$$plim(\hat{\gamma}_{2,OLS}) = \frac{cov(y, x_2^*) var(x_1^*) - cov(y, x_1^*) cov(x_1^*, x_2^*)}{var(x_1^*) var(x_2^*) - cov(x_1^*, x_2^*)^2} \quad (22b)$$

Now we substitute equations (18a) and (18b) 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:

$$\text{var}(x_g^*) = \sigma_{x_g}^2 + \sigma_{v_g}^2 \quad (23a)$$

$$\text{cov}(x_1^*, x_2^*) = \rho\sigma_{x_1}\sigma_{x_2} \quad (23b)$$

$$\text{cov}(y, x_1^*) = \gamma_1\sigma_{x_1}^2 + \gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (23c)$$

$$\text{cov}(y, x_2^*) = \gamma_2\sigma_{x_2}^2 + \gamma_1\rho\sigma_{x_1}\sigma_{x_2} \quad (23d)$$

Substituting these into (22a) and (22b) and rearranging gives us:

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

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

Although assuming that the transitory components are sources of classical measurement error does lead 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  $\delta$  is the autocorrelation coefficient,

$$v_{itg} = \delta v_{it-1g} + e_{it}. \quad (25)$$

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:

$$\frac{1}{T_g} \frac{\sigma_e^2}{1-\delta^2} \left[ 1 + 2\delta \left( \frac{T_g - \frac{1-\delta^{T_g}}{1-\delta}}{T_g(1-\delta)} \right) \right]. \quad (26)$$

## 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 (18a) and (18b) above, we have for

our instruments:

$$z_{is1}^* = x_{i1} + v_{is1}, \quad (27a)$$

$$z_{is2}^* = x_{i2} + v_{is2}. \quad (27b)$$

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:

$$\hat{\gamma}_{1,IV} = (z_1^{*'} A_2 x_1^*)^{-1} z_1^{*'} A_2 y \quad (28a)$$

$$= [z_1^{*'} (I - x_2^*(z_2^{*'} x_2^*)^{-1} z_2^{*'}) x_1^*]^{-1} z_1^{*'} (I - x_2^*(z_2^{*'} x_2^*)^{-1} z_2^{*'}) y \quad (28b)$$

$$= [z_1^{*'} x_1^* - z_1^{*'} x_2^*(z_2^{*'} x_2^*)^{-1} z_2^{*'} x_1^*]^{-1} [z_1^{*'} y - z_1^{*'} x_2^*(z_2^{*'} x_2^*)^{-1} z_2^{*'} y] \quad (28c)$$

$$= \left[ \sum_{i=1}^N z_{i1}^* x_{i1}^* - \sum_{i=1}^N z_{i1}^* x_{i2}^* \left( \sum_{i=1}^N z_{i2}^* x_{i2}^* \right)^{-1} \sum_{i=1}^N z_{i2}^* x_{i1}^* \right]^{-1} \left[ \sum_{i=1}^N z_{i1}^* y_i - \sum_{i=1}^N z_{i1}^* x_{i2}^* \left( \sum_{i=1}^N z_{i2}^* x_{i2}^* \right)^{-1} \sum_{i=1}^N z_{i2}^* y_i \right] \quad (28d)$$

⋮

$$\hat{\gamma}_{1,IV} = \frac{\sum_{i=1}^N z_{i1}^* y_i \sum_{i=1}^N z_{i2}^* x_{i2}^* - \sum_{i=1}^N z_{i1}^* x_{i2}^* \sum_{i=1}^N z_{i2}^* y_i}{\sum_{i=1}^N z_{i1}^* x_{i1}^* \sum_{i=1}^N z_{i2}^* x_{i2}^* - \sum_{i=1}^N z_{i1}^* x_{i2}^* \sum_{i=1}^N z_{i2}^* x_{i1}^*} \quad (28e)$$

Similarly, for  $\gamma_2$ , we get:

$$\hat{\gamma}_{2,IV} = \frac{\sum_{i=1}^N z_{i2}^* y_i \sum_{i=1}^N z_{i1}^* x_{i1}^* - \sum_{i=1}^N z_{i2}^* x_{i1}^* \sum_{i=1}^N z_{i1}^* y_i}{\sum_{i=1}^N z_{i2}^* x_{i2}^* \sum_{i=1}^N z_{i1}^* x_{i1}^* - \sum_{i=1}^N z_{i2}^* x_{i1}^* \sum_{i=1}^N z_{i1}^* x_{i2}^*} \quad (29)$$

Taking the probability limits we get:

$$plim(\hat{\gamma}_{1,IV}) = \frac{cov(z_1^*, y) cov(z_2^*, x_2^*) - cov(z_1^*, x_2^*) cov(z_2^*, y)}{cov(z_1^*, x_1^*) cov(z_2^*, x_2^*) - cov(z_1^*, x_2^*) cov(z_2^*, x_1^*)} \quad (30a)$$

$$plim(\hat{\gamma}_{2,IV}) = \frac{cov(z_2^*, y) cov(z_1^*, x_1^*) - cov(z_2^*, x_1^*) cov(z_1^*, y)}{cov(z_2^*, x_2^*) cov(z_1^*, x_1^*) - cov(z_2^*, x_1^*) cov(z_1^*, x_2^*)} \quad (30b)$$

Now we substitute equations (18a), (18b), (27a), and (27b) 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  $\rho \equiv \text{corr}(x_1, x_2)$ , allowing us to write the elements of the probability limits as:

$$\text{cov}(z_g^*, x_g^*) = \sigma_{x_g}^2 + \text{cov}(v_{isg}, v_{itg}) = \sigma_{x_g}^2 + \sigma_{v_g}^2 \quad (31a)$$

$$\text{cov}(x_1^*, z_2^*) = \text{cov}(x_2^*, z_1^*) = \rho\sigma_{x_1}\sigma_{x_2} \quad (31b)$$

$$\text{cov}(y, z_1^*) = \gamma_1\sigma_{x_1}^2 + \gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (31c)$$

$$\text{cov}(y, z_2^*) = \gamma_2\sigma_{x_2}^2 + \gamma_1\rho\sigma_{x_1}\sigma_{x_2} \quad (31d)$$

Substituting these into the probability limits in (30a) and (30b), and then doing some algebra shows that  $\text{plim}(\hat{\gamma}_{1,IV}) = \gamma_1$  and  $\text{plim}(\hat{\gamma}_{2,IV}) = \gamma_2$ . However, if we consider the case of an AR(1) process for  $v_{itg}$ , then (31a) does not hold. Rather,  $\text{cov}(v_{isg}, v_{itg}) = \delta^{T_g} \frac{\sigma_{e_g}^2}{1-\delta^2}$  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 (24a) and (24b) 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  $\sigma_{v_g}^2$  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  $\sigma_{v_g}^2$  in  $\text{plim}(\hat{\gamma}_1)$  and  $\text{plim}(\hat{\gamma}_2)$

Estimation method	$v_{itg} \sim \text{CEV}$	$v_{itg} \sim \text{AR}(1)$
OLS using annual income measures	$\sigma_{v_g}^2$	$\frac{\sigma_{e_1}^2}{1-\delta^2}$
OLS using $T_g$ -year averages of income	$\frac{\sigma_{v_g}^2}{T_g}$	$\frac{1}{T_g} \frac{\sigma_{e_1}^2}{1-\delta^2} \left[ 1 + 2\delta \left( \frac{T_g - \frac{1-\delta^{T_g}}{1-\delta}}{T_g(1-\delta)} \right) \right]$
IV using annual incomes $T_g$ years apart	<i>n.a.</i>	$\delta^{T_g} \frac{\sigma_{e_1}^2}{1-\delta^2}$

### A.3 Lifecycle Effects

We can also consider lifecycle profiles in income for fathers and grandfathers, where the relationship between annual and lifetime or permanent income is written

$$x_{it1}^* = \lambda_{t1}x_{i1} + v_{it1}, \quad (32a)$$

$$x_{it2}^* = \lambda_{2t}x_{i2} + v_{it2}. \quad (32b)$$

Considering again the probability limits in equations (24a) and (24b), we can use the equations in (32a) and (32b) to write the elements of the probability limits as:

$$\text{var}(x_g^*) = \lambda_{tg}\sigma_{x_g}^2 + \sigma_{v_g}^2 \quad (33a)$$

$$\text{cov}(x_1^*, x_2^*) = \lambda_{t1}\lambda_{t2}\rho\sigma_{x_1}\sigma_{x_2} \quad (33b)$$

$$\text{cov}(y, x_1^*) = \lambda_{t1}\gamma_1\sigma_{x_1}^2 + \lambda_{t1}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (33c)$$

$$\text{cov}(y, x_2^*) = \lambda_{t2}\gamma_2\sigma_{x_2}^2 + \lambda_{t2}\gamma_1\rho\sigma_{x_1}\sigma_{x_2} \quad (33d)$$

Then the OLS probability limits in equations (24a) and (24b) are now:

$$\text{plim}(\hat{\gamma}_{1,OLS}) = \gamma_1 \frac{\lambda_{1t}\sigma_{x_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2 \left( \frac{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)} + \gamma_2 \frac{\lambda_{1t}\sigma_{x_1}\sigma_{x_2} \left( \frac{\rho\sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)}{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2 \left( \frac{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)} \quad (34a)$$

$$\text{plim}(\hat{\gamma}_{2,OLS}) = \gamma_1 \frac{\lambda_{2t}\sigma_{x_1}\sigma_{x_2} \left( \frac{\rho\sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)}{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2 \left( \frac{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)} + \gamma_2 \frac{\lambda_{2t}\sigma_{x_2}^2}{\lambda_{2t}^2\sigma_{x_2}^2 + \sigma_{v_2}^2 \left( \frac{\lambda_{1t}^2\sigma_{x_1}^2 + \sigma_{v_1}^2}{\lambda_{1t}^2\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)}. \quad (34b)$$

The equations for our instruments can now be written:

$$z_{is1}^* = \lambda_{1s}x_{i1} + v_{is1}, \quad (35a)$$

$$z_{is2}^* = \lambda_{2s}x_{i2} + v_{is2}. \quad (35b)$$

With IV estimation, if we assume the  $v_{itg}$  are essentially white noise error, then the elements

of the probability limits are:

$$cov(z_g^*, x_g^*) = \sigma_{x_g}^2 + cov(v_{isg}, v_{itg}) = \lambda_{gt}\lambda_{gs}\sigma_{x_g}^2 \quad (36a)$$

$$cov(x_1^*, z_2^*) = \lambda_{1t}\lambda_{2s}\rho\sigma_{x_1}\sigma_{x_2} \quad (36b)$$

$$cov(x_2^*, z_1^*) = \lambda_{2t}\lambda_{1s}\rho\sigma_{x_1}\sigma_{x_2} \quad (36c)$$

$$cov(y, z_1^*) = \lambda_{1s}\gamma_1\sigma_{x_1}^2 + \lambda_{1s}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (36d)$$

$$cov(y, z_2^*) = \lambda_{2s}\gamma_2\sigma_{x_2}^2 + \lambda_{2s}\gamma_1\rho\sigma_{x_1}\sigma_{x_2} \quad (36e)$$

And the probability limits of the estimators are:

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1 \frac{1}{\lambda_{1t}} \quad (37a)$$

$$plim(\hat{\gamma}_{2,IV}) = \gamma_2 \frac{1}{\lambda_{2t}} \quad (37b)$$

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

$$cov(z_g^*, x_g^*) = \sigma_{x_g}^2 + cov(v_{isg}, v_{itg}) = \lambda_{gt}\lambda_{gs}\sigma_{x_g}^2 + \delta_g^{Tg} \left( \frac{\sigma_e^2}{1 - \delta_g} \right) \quad (38a)$$

$$cov(x_1^*, z_2^*) = \lambda_{1t}\lambda_{2s}\rho\sigma_{x_1}\sigma_{x_2} \quad (38b)$$

$$cov(x_2^*, z_1^*) = \lambda_{2t}\lambda_{1s}\rho\sigma_{x_1}\sigma_{x_2} \quad (38c)$$

$$cov(y, z_1^*) = \lambda_{1s}\gamma_1\sigma_{x_1}^2 + \lambda_{1s}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \quad (38d)$$

$$cov(y, z_2^*) = \lambda_{2s}\gamma_2\sigma_{x_2}^2 + \lambda_{2s}\gamma_1\rho\sigma_{x_1}\sigma_{x_2} \quad (38e)$$

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

$$plim(\hat{\gamma}_{1,IV}) = \gamma_1 \frac{\lambda_{1s}\sigma_{x_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2 \left( \frac{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)} + \gamma_2 \frac{\lambda_{1s}\sigma_{x_1}\sigma_{x_2} \left( \frac{\rho\sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2 \left( \frac{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2(1-\rho^2) + \sigma_{v_2}^2} \right)} \quad (39a)$$

$$plim(\hat{\gamma}_{2,IV}) = \gamma_1 \frac{\lambda_{2s}\sigma_{x_1}\sigma_{x_2} \left( \frac{\rho\sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2 \left( \frac{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)} + \gamma_2 \frac{\lambda_{2s}\sigma_{x_2}^2}{\lambda_{2s}\lambda_{2t}\sigma_{x_2}^2 + \sigma_{v_2}^2 \left( \frac{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2 + \sigma_{v_1}^2}{\lambda_{1s}\lambda_{1t}\sigma_{x_1}^2(1-\rho^2) + \sigma_{v_1}^2} \right)}. \quad (39b)$$

## B Simulations

### B.1 Simulation results with lifecycle effects

Figure B.1: Attenuation and spillover in OLS estimates when  $\lambda_1 = \lambda_2 = 1.2$

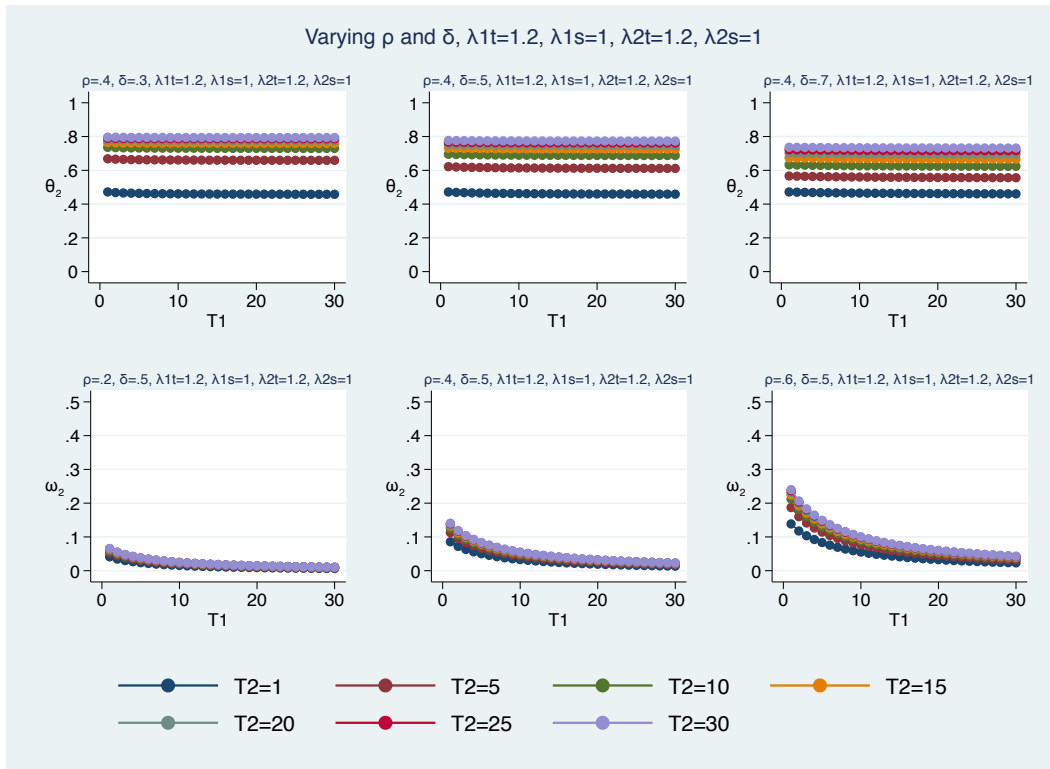


Figure B.2: Attenuation and spillover in OLS estimates when  $\lambda_1 = \lambda_2 = 0.8$

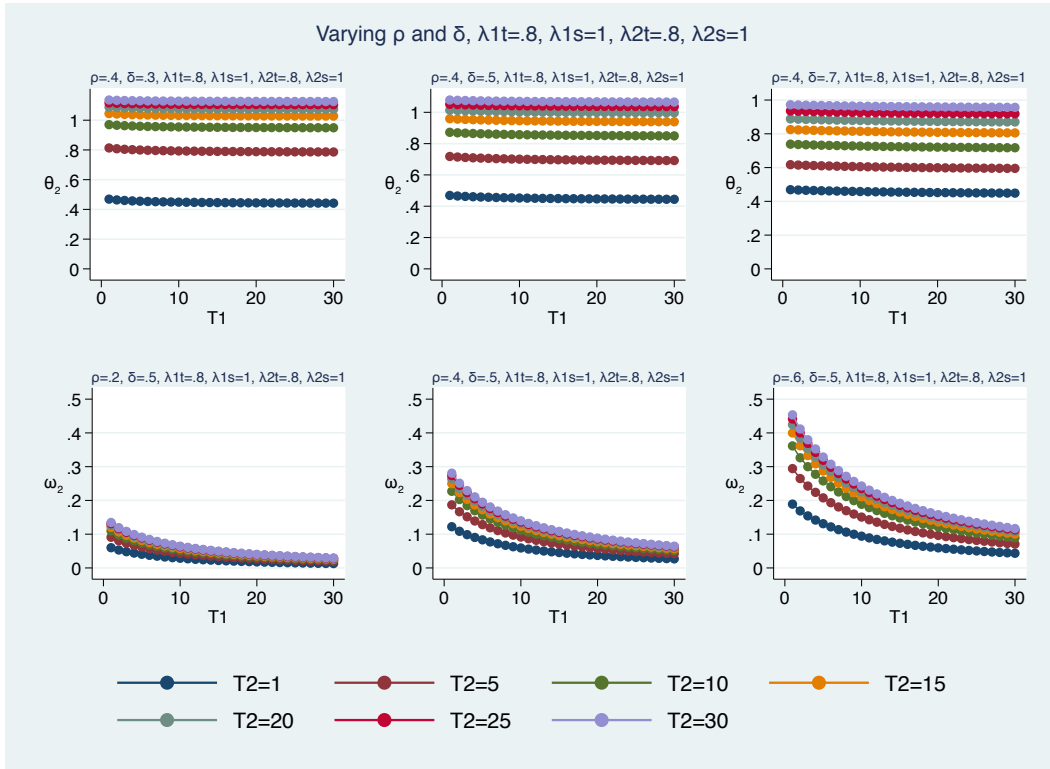


Figure B.3: Attenuation and spillover in IV estimates when  $\lambda_{1t} = \lambda_{2t} = 1.2$ ,  $\lambda_{1s} = \lambda_{2s} = 1$

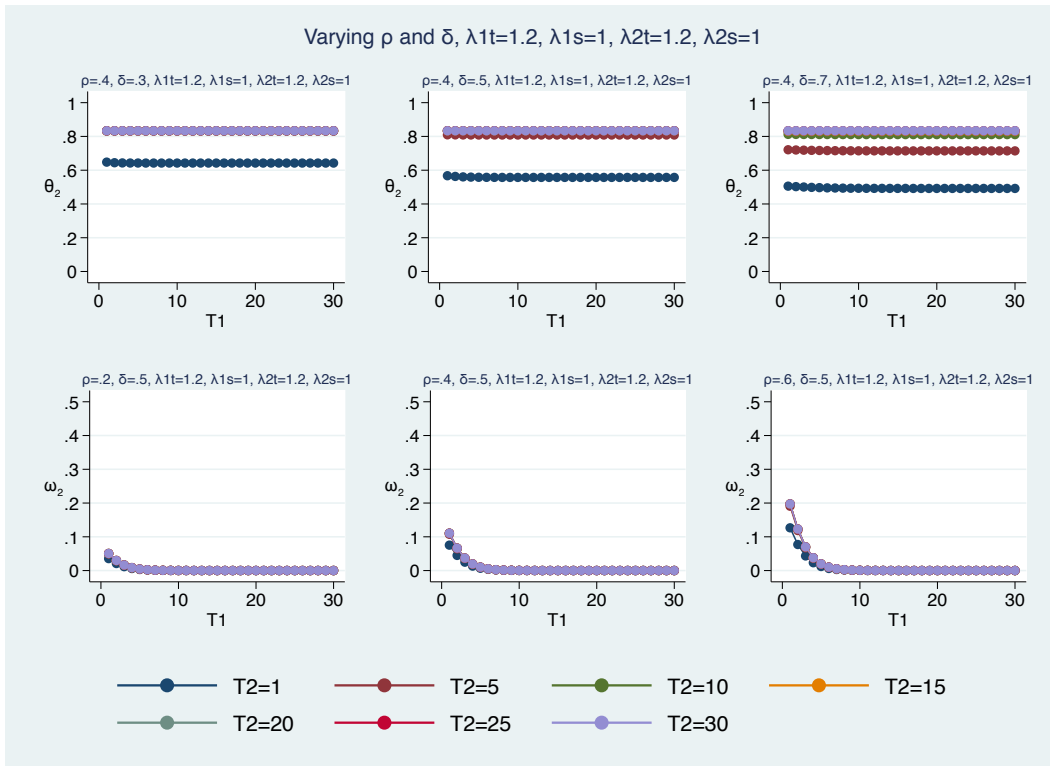
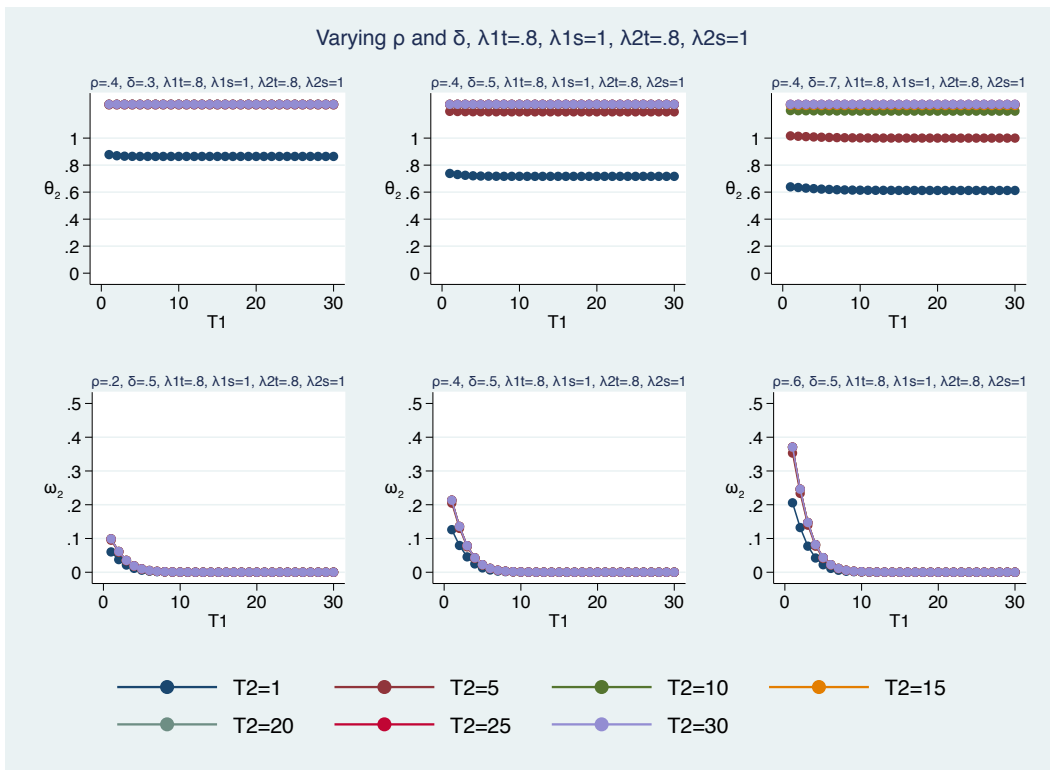


Figure B.4: Attenuation and spillover in IV estimates when  $\lambda_{1t} = \lambda_{2t} = 0.8$ ,  $\lambda_{1s} = \lambda_{2s} = 1$



## C Empirical results

### C.1 Reverse IV results for main sample

Figure C.5: Attenuation and spillover in 2 generation IV estimates when income at older age is used as the endogenous measure

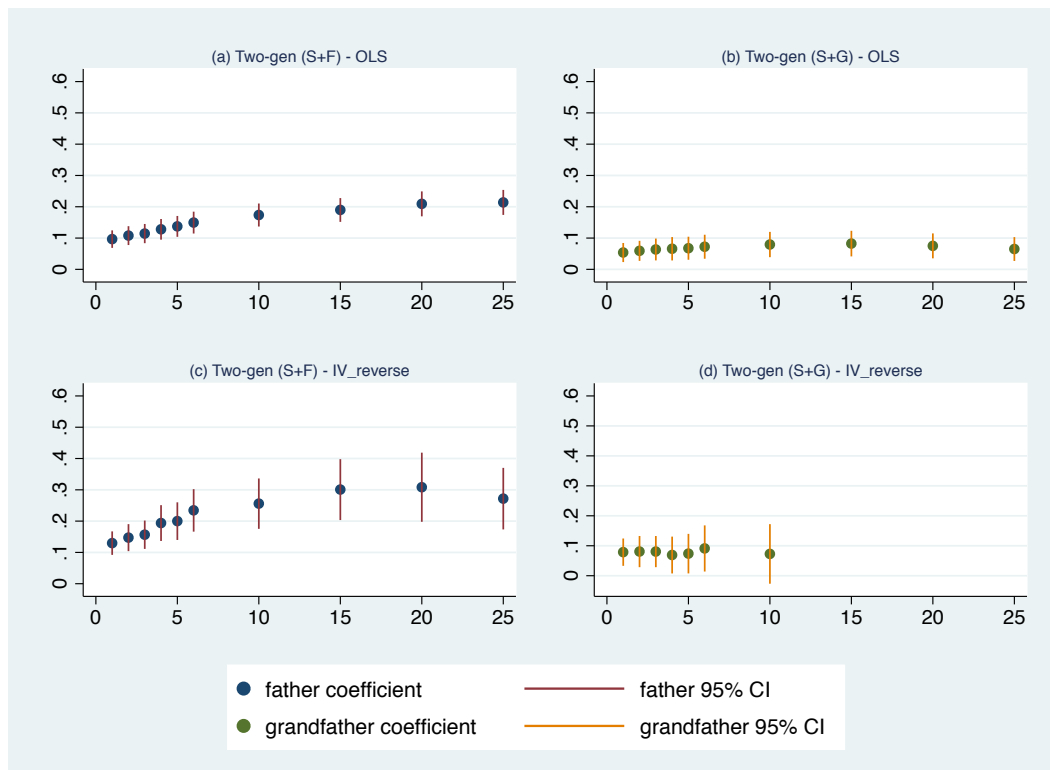
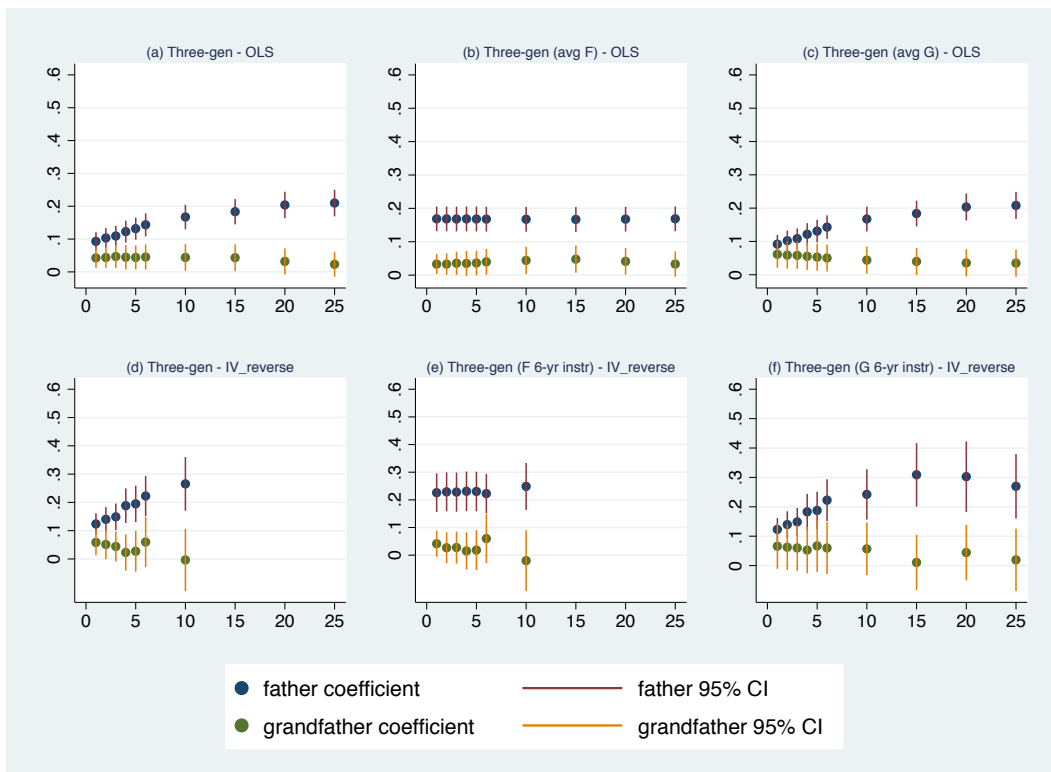




Figure C.6: Attenuation and spillover in 3 generation IV estimates when income at older age is used as the endogenous measure



## C.2 Figures using men+women sample

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

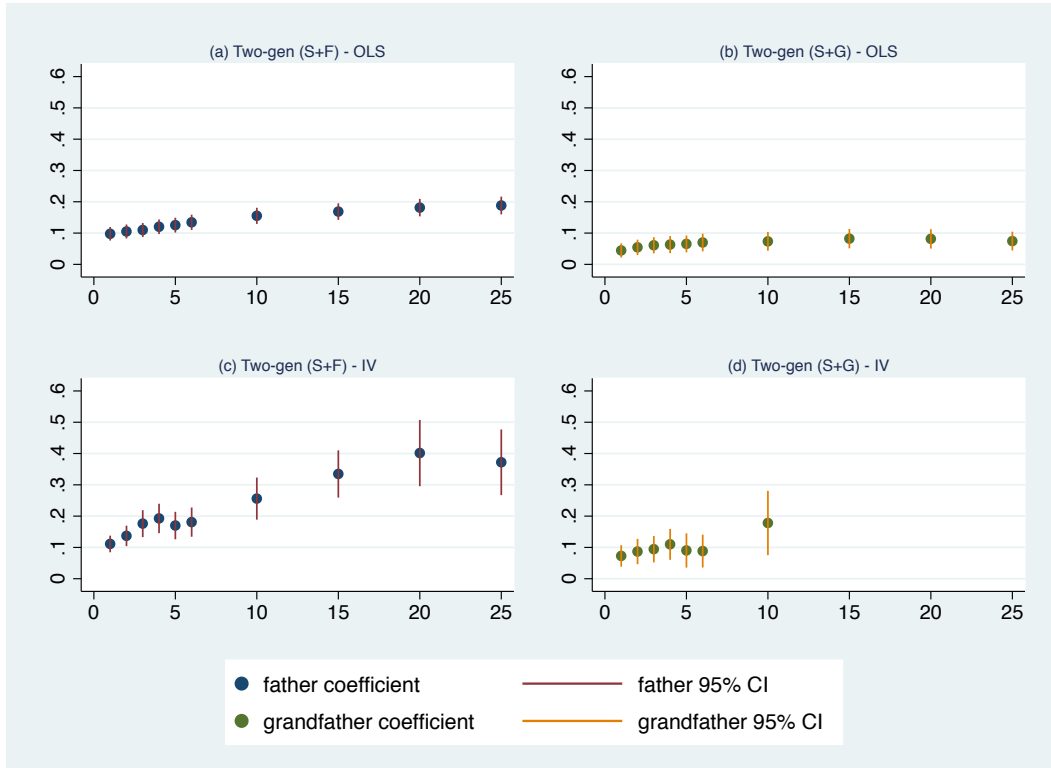
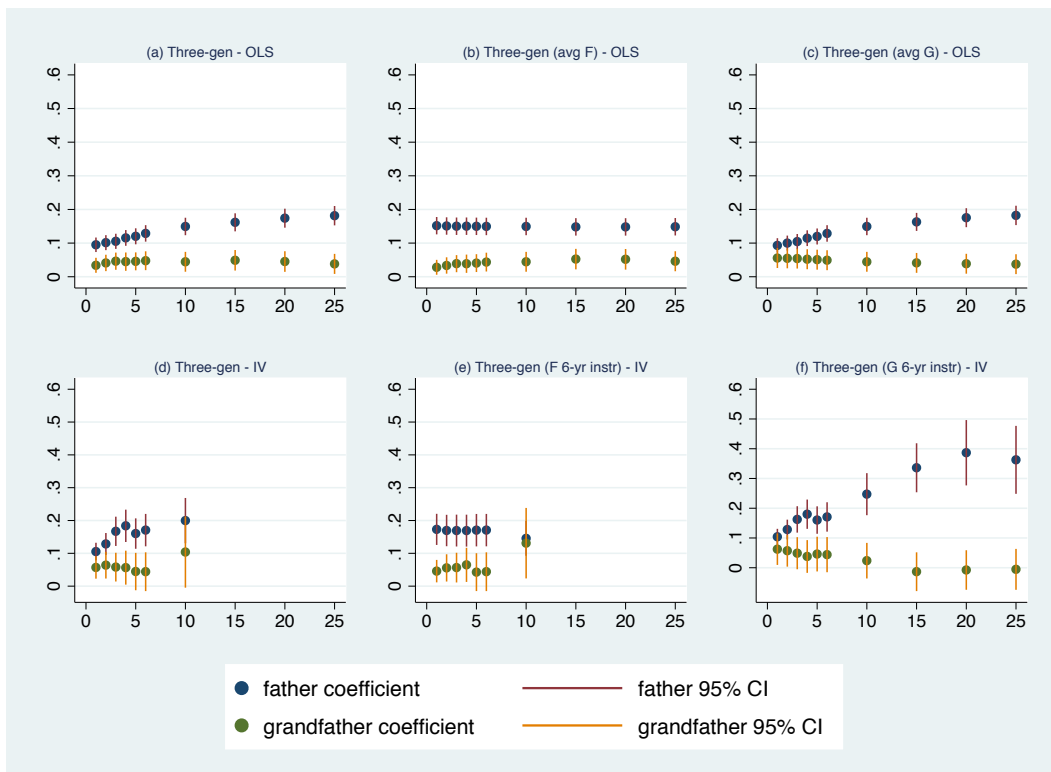


Figure C.8: OLS and IV estimates from three-generation regressions. Men and women in final generation.



### **C.3 Tables of regression coefficients**

Men only (for men and women, see below).

See Tables C.2-C.10

## (a) Sons and fathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.137 (0.020)	0.119 (0.017)	0.107 (0.016)	0.091 (0.015)	0.097 (0.014)	0.086 (0.015)	0.100 (0.015)
2 years	0.148 (0.019)	0.131 (0.018)	0.117 (0.017)	0.108 (0.015)	0.105 (0.015)	0.108 (0.015)	
3 years	0.153 (0.020)	0.136 (0.018)	0.124 (0.017)	0.114 (0.016)	0.119 (0.016)		
4 years	0.155 (0.019)	0.141 (0.018)	0.128 (0.017)	0.126 (0.016)			
5 years	0.158 (0.019)	0.142 (0.018)	0.137 (0.017)				
6 years	0.158 (0.019)	0.149 (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 over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.070 (0.019)	0.054 (0.020)	0.043 (0.019)	0.048 (0.015)	0.054 (0.016)	0.045 (0.016)	0.041 (0.013)
2 years	0.073 (0.020)	0.058 (0.021)	0.054 (0.018)	0.059 (0.016)	0.059 (0.017)	0.051 (0.016)	
3 years	0.071 (0.022)	0.063 (0.020)	0.062 (0.018)	0.063 (0.018)	0.060 (0.017)		
4 years	0.073 (0.021)	0.068 (0.019)	0.066 (0.019)	0.065 (0.018)			
5 years	0.077 (0.020)	0.071 (0.020)	0.067 (0.019)				
6 years	0.078 (0.021)	0.072 (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 C.2: OLS estimates from two-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36.

## (a) Sons and fathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.154 (0.022)	0.147 (0.024)	0.138 (0.021)	0.133 (0.020)	0.115 (0.018)	0.149 (0.023)	0.127 (0.018)
2 years	0.167 (0.026)	0.167 (0.025)	0.172 (0.025)	0.146 (0.023)	0.177 (0.027)	0.159 (0.023)	
3 years	0.180 (0.027)	0.199 (0.030)	0.174 (0.028)	0.214 (0.033)	0.171 (0.025)		
4 years	0.207 (0.031)	0.191 (0.031)	0.248 (0.037)	0.192 (0.028)			
5 years	0.202 (0.032)	0.261 (0.039)	0.224 (0.031)				
6 years	0.270 (0.041)	0.237 (0.034)					
10 years	0.318 (0.053)						
15 years	0.414 (0.057)						
20 years	0.386 (0.068)						
25 years	0.440 (0.076)						

## (b) Sons and grandfathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.086 (0.034)	0.059 (0.027)	0.077 (0.024)	0.077 (0.022)	0.067 (0.024)	0.067 (0.021)	0.059 (0.021)
2 years	0.072 (0.033)	0.088 (0.027)	0.103 (0.029)	0.082 (0.030)	0.076 (0.024)	0.072 (0.025)	
3 years	0.105 (0.034)	0.109 (0.031)	0.101 (0.036)	0.093 (0.028)	0.083 (0.029)		
4 years	0.128 (0.037)	0.103 (0.037)	0.117 (0.035)	0.092 (0.033)			
5 years	0.114 (0.040)	0.116 (0.035)	0.114 (0.039)				
6 years	0.135 (0.040)	0.105 (0.038)					
10 years	0.133 (0.077)						
15 years							
20 years							
25 years							

Table C.3: IV estimates from two-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36.

## (a) Sons and fathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.192 (0.026)	0.164 (0.024)	0.147 (0.022)	0.119 (0.018)	0.130 (0.019)	0.109 (0.017)	0.121 (0.017)
2 years	0.232 (0.032)	0.199 (0.029)	0.163 (0.025)	0.147 (0.022)	0.145 (0.021)	0.132 (0.021)	
3 years	0.265 (0.036)	0.212 (0.031)	0.186 (0.029)	0.157 (0.023)	0.162 (0.024)		
4 years	0.274 (0.037)	0.228 (0.034)	0.194 (0.029)	0.161 (0.025)			
5 years	0.300 (0.041)	0.227 (0.033)	0.200 (0.031)				
6 years	0.292 (0.040)	0.234 (0.035)					
10 years	0.256 (0.041)						
15 years	0.301 (0.050)						
20 years	0.308 (0.056)						
25 years	0.272 (0.050)						

## (b) Sons and grandfathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.092 (0.025)	0.083 (0.032)	0.053 (0.023)	0.064 (0.020)	0.079 (0.023)	0.059 (0.022)	0.064 (0.020)
2 years	0.109 (0.029)	0.085 (0.033)	0.059 (0.026)	0.081 (0.027)	0.079 (0.024)	0.071 (0.025)	
3 years	0.109 (0.029)	0.088 (0.034)	0.068 (0.031)	0.080 (0.027)	0.086 (0.028)		
4 years	0.111 (0.030)	0.098 (0.038)	0.069 (0.031)	0.083 (0.030)			
5 years	0.116 (0.031)	0.097 (0.038)	0.074 (0.034)				
6 years	0.120 (0.033)	0.091 (0.039)					
10 years	0.073 (0.051)						
15 years							
20 years							
25 years							

Table C.4: “Reverse IV” estimates from two-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36.

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	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)
2 years	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)
3 years	0.143	0.127	0.113	0.103	0.100	0.105	
	(0.020)	(0.018)	(0.017)	(0.015)	(0.015)	(0.016)	
4 years	0.047	0.037	0.034	0.044	0.046	0.038	
	(0.021)	(0.021)	(0.018)	(0.016)	(0.017)	(0.016)	
5 years	0.148	0.131	0.119	0.109	0.114		
	(0.020)	(0.018)	(0.017)	(0.016)	(0.016)		
6 years	0.043	0.039	0.041	0.047	0.044		
	(0.022)	(0.020)	(0.018)	(0.018)	(0.017)		
10 years	0.150	0.135	0.123	0.121			
	(0.020)	(0.018)	(0.017)	(0.016)			
15 years	0.043	0.044	0.045	0.046			
	(0.021)	(0.020)	(0.019)	(0.017)			
20 years	0.152	0.136	0.132				
	(0.019)	(0.018)	(0.017)				
25 years	0.046	0.046	0.044				
	(0.021)	(0.020)	(0.019)				
30 years	0.152	0.143					
	(0.019)	(0.018)					
35 years	0.047	0.045					
	(0.021)	(0.020)					
40 years	0.167						
	(0.019)						
45 years	0.044						
	(0.021)						
50 years	0.183						
	(0.020)						
55 years	0.043						
	(0.021)						
60 years	0.204						
	(0.021)						
65 years	0.032						
	(0.020)						
70 years	0.210						
	(0.021)						
75 years	0.023						
	(0.019)						

Table C.5: OLS estimates from three-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation



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

Table C.6: OLS estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

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

Table C.7: OLS estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.148 (0.023)	0.144 (0.024)	0.132 (0.021)	0.127 (0.020)	0.110 (0.018)	0.144 (0.023)	0.119 (0.018)
	0.051 (0.034)	0.028 (0.027)	0.048 (0.024)	0.054 (0.022)	0.050 (0.024)	0.043 (0.021)	0.045 (0.022)
2 years	0.164 (0.027)	0.161 (0.026)	0.162 (0.025)	0.138 (0.024)	0.171 (0.028)	0.149 (0.024)	
	0.025 (0.034)	0.046 (0.029)	0.064 (0.029)	0.054 (0.030)	0.045 (0.024)	0.050 (0.026)	
3 years	0.172 (0.028)	0.190 (0.031)	0.166 (0.029)	0.206 (0.034)	0.160 (0.026)		
	0.044 (0.036)	0.064 (0.032)	0.055 (0.037)	0.045 (0.030)	0.056 (0.031)		
4 years	0.197 (0.032)	0.183 (0.033)	0.243 (0.039)	0.178 (0.029)			
	0.065 (0.039)	0.053 (0.039)	0.032 (0.038)	0.061 (0.036)			
5 years	0.193 (0.034)	0.255 (0.041)	0.213 (0.034)				
	0.053 (0.043)	0.036 (0.038)	0.051 (0.042)				
6 years	0.263 (0.044)	0.227 (0.036)					
	0.036 (0.047)	0.050 (0.044)					
10 years	0.295 (0.052)						
	0.014 (0.084)						
15 years							
20 years							
25 years							

Table C.8: IV estimates from three-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.146	0.140	0.131	0.125	0.109	0.142	0.118
	(0.023)	(0.024)	(0.021)	(0.020)	(0.018)	(0.024)	(0.018)
	0.057	0.066	0.068	0.075	0.081	0.071	0.086
2 years	(0.040)	(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.041)
	0.159	0.161	0.164	0.140	0.168	0.148	
	(0.027)	(0.027)	(0.026)	(0.023)	(0.028)	(0.024)	
3 years	0.053	0.059	0.059	0.071	0.067	0.079	
	(0.041)	(0.039)	(0.038)	(0.038)	(0.038)	(0.042)	
	0.173	0.193	0.170	0.206	0.161		
4 years	(0.028)	(0.032)	(0.028)	(0.034)	(0.026)		
	0.048	0.050	0.057	0.055	0.077		
	(0.042)	(0.039)	(0.038)	(0.039)	(0.042)		
5 years	0.201	0.190	0.243	0.182			
	(0.033)	(0.032)	(0.040)	(0.029)			
	0.039	0.051	0.037	0.071			
6 years	(0.043)	(0.039)	(0.039)	(0.042)			
	0.199	0.260	0.215				
	(0.033)	(0.043)	(0.033)				
10 years	0.039	0.031	0.051				
	(0.043)	(0.040)	(0.043)				
	0.265	0.227					
15 years	(0.044)	(0.036)					
	0.018	0.050					
	(0.045)	(0.044)					
20 years	0.315						
	(0.056)						
	0.024						
25 years	(0.047)						
	0.430						
	(0.062)						
30 years	-0.032						
	(0.049)						
	0.373						
35 years	(0.072)						
	0.016						
	(0.049)						
40 years	0.438						
	(0.085)						
	-0.012						
45 years	(0.051)						

Table C.9: IV estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.229 (0.036)	0.236 (0.035)	0.232 (0.035)	0.227 (0.035)	0.231 (0.035)	0.231 (0.035)	0.229 (0.036)
	0.043 (0.036)	0.010 (0.029)	0.030 (0.026)	0.044 (0.023)	0.027 (0.026)	0.027 (0.022)	0.026 (0.023)
2 years	0.235 (0.036)	0.232 (0.035)	0.227 (0.035)	0.229 (0.036)	0.231 (0.036)	0.228 (0.036)	
	0.012 (0.036)	0.036 (0.031)	0.058 (0.031)	0.034 (0.032)	0.032 (0.026)	0.031 (0.027)	
3 years	0.229 (0.036)	0.228 (0.035)	0.230 (0.036)	0.228 (0.036)	0.227 (0.036)		
	0.042 (0.038)	0.063 (0.033)	0.041 (0.038)	0.040 (0.032)	0.037 (0.032)		
4 years	0.223 (0.036)	0.231 (0.035)	0.228 (0.036)	0.224 (0.037)			
	0.074 (0.040)	0.043 (0.040)	0.048 (0.039)	0.043 (0.038)			
5 years	0.228 (0.036)	0.230 (0.036)	0.225 (0.037)				
	0.048 (0.044)	0.050 (0.040)	0.049 (0.043)				
6 years	0.226 (0.037)	0.227 (0.036)					
	0.058 (0.046)	0.050 (0.044)					
10 years	0.198 (0.042)						
	0.058 (0.082)						
15 years							
20 years							
25 years							

Table C.10: IV estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.185 (0.027)	0.157 (0.025)	0.143 (0.023)	0.114 (0.018)	0.124 (0.019)	0.104 (0.018)	0.116 (0.017)
	0.053 (0.025)	0.053 (0.032)	0.025 (0.024)	0.044 (0.020)	0.058 (0.023)	0.044 (0.022)	0.050 (0.020)
2 years	0.222 (0.032)	0.189 (0.030)	0.159 (0.025)	0.140 (0.022)	0.137 (0.021)	0.129 (0.021)	
	0.061 (0.030)	0.049 (0.034)	0.025 (0.027)	0.051 (0.026)	0.056 (0.024)	0.050 (0.027)	
3 years	0.256 (0.037)	0.204 (0.032)	0.180 (0.030)	0.149 (0.024)	0.155 (0.025)		
	0.043 (0.032)	0.046 (0.034)	0.031 (0.032)	0.044 (0.027)	0.061 (0.030)		
4 years	0.264 (0.039)	0.218 (0.035)	0.188 (0.031)	0.156 (0.026)			
	0.045 (0.032)	0.051 (0.039)	0.023 (0.033)	0.049 (0.033)			
5 years	0.290 (0.043)	0.216 (0.035)	0.195 (0.033)				
	0.045 (0.034)	0.049 (0.040)	0.027 (0.037)				
6 years	0.283 (0.042)	0.222 (0.036)					
	0.038 (0.036)	0.060 (0.045)					
10 years	0.265 (0.048)						
	-0.004 (0.056)						
15 years							
20 years							
25 years							

Table C.11: “Reverse IV” estimates from three-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.182	0.154	0.136	0.111	0.123	0.103	0.113
	(0.028)	(0.026)	(0.024)	(0.019)	(0.020)	(0.017)	(0.018)
	0.049	0.058	0.064	0.067	0.066	0.070	0.081
2 years	(0.040)	(0.039)	(0.040)	(0.039)	(0.039)	(0.039)	(0.044)
	0.220	0.187	0.153	0.139	0.137	0.126	
	(0.033)	(0.030)	(0.026)	(0.023)	(0.022)	(0.021)	
3 years	0.045	0.053	0.058	0.062	0.062	0.079	
	(0.039)	(0.040)	(0.039)	(0.039)	(0.040)	(0.044)	
	0.252	0.200	0.176	0.148	0.154		
4 years	(0.038)	(0.033)	(0.031)	(0.024)	(0.025)		
	0.040	0.048	0.055	0.060	0.073		
	(0.040)	(0.039)	(0.040)	(0.040)	(0.044)		
5 years	0.262	0.220	0.183	0.153			
	(0.040)	(0.036)	(0.031)	(0.026)			
	0.034	0.046	0.053	0.074			
6 years	(0.039)	(0.040)	(0.040)	(0.044)			
	0.290	0.219	0.187				
	(0.045)	(0.035)	(0.033)				
10 years	0.031	0.045	0.067				
	(0.040)	(0.040)	(0.045)				
	0.280	0.222					
15 years	(0.042)	(0.036)					
	0.033	0.060					
	(0.040)	(0.045)					
20 years	0.242						
	(0.044)						
	0.057						
25 years	(0.046)						
	0.309						
	(0.055)						
30 years	0.011						
	(0.048)						
	0.302						
35 years	(0.061)						
	0.044						
	(0.048)						
40 years	0.270						
	(0.056)						
	0.019						
45 years	(0.054)						

Table C.12: “Reverse IV” estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.227 (0.036)	0.225 (0.035)	0.232 (0.036)	0.230 (0.035)	0.226 (0.036)	0.229 (0.035)	0.227 (0.036)
	0.045 (0.027)	0.050 (0.034)	0.011 (0.025)	0.021 (0.022)	0.041 (0.024)	0.024 (0.023)	0.034 (0.021)
2 years	0.225 (0.036)	0.223 (0.036)	0.232 (0.036)	0.229 (0.036)	0.225 (0.036)	0.228 (0.036)	
	0.052 (0.032)	0.054 (0.036)	0.013 (0.028)	0.027 (0.029)	0.042 (0.025)	0.031 (0.027)	
3 years	0.223 (0.036)	0.224 (0.036)	0.231 (0.036)	0.228 (0.036)	0.225 (0.036)		
	0.054 (0.033)	0.055 (0.037)	0.015 (0.033)	0.028 (0.030)	0.046 (0.030)		
4 years	0.224 (0.036)	0.222 (0.036)	0.231 (0.036)	0.228 (0.036)			
	0.055 (0.033)	0.061 (0.042)	0.015 (0.034)	0.028 (0.033)			
5 years	0.223 (0.036)	0.220 (0.036)	0.230 (0.036)				
	0.057 (0.035)	0.063 (0.043)	0.018 (0.037)				
6 years	0.220 (0.037)	0.222 (0.036)					
	0.061 (0.037)	0.060 (0.045)					
10 years	0.249 (0.043)						
	-0.020 (0.056)						
15 years							
20 years							
25 years							

Table C.13: “Reverse IV” estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men in final generation



## C.4 Tables for sample of men+women

See Tables C.14-C.21

## (a) Sons/daughters and fathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.126 (0.013)	0.100 (0.012)	0.094 (0.011)	0.086 (0.011)	0.098 (0.011)	0.080 (0.010)	0.084 (0.011)
2 years	0.130 (0.013)	0.113 (0.012)	0.105 (0.011)	0.105 (0.011)	0.102 (0.011)	0.094 (0.011)	
3 years	0.134 (0.013)	0.120 (0.012)	0.117 (0.012)	0.110 (0.011)	0.109 (0.011)		
4 years	0.137 (0.013)	0.128 (0.012)	0.120 (0.012)	0.116 (0.012)			
5 years	0.143 (0.013)	0.130 (0.012)	0.125 (0.012)				
6 years	0.144 (0.013)	0.134 (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 (0.017)	0.053 (0.015)	0.043 (0.014)	0.049 (0.012)	0.044 (0.012)	0.048 (0.012)	0.040 (0.010)
2 years	0.065 (0.016)	0.057 (0.015)	0.054 (0.014)	0.054 (0.013)	0.055 (0.013)	0.052 (0.011)	
3 years	0.065 (0.016)	0.062 (0.015)	0.058 (0.014)	0.061 (0.013)	0.058 (0.012)		
4 years	0.069 (0.016)	0.064 (0.015)	0.063 (0.014)	0.063 (0.013)			
5 years	0.070 (0.015)	0.068 (0.015)	0.065 (0.014)				
6 years	0.073 (0.015)	0.070 (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 C.14: OLS estimates from two-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36.

## (a) Sons/daughters and fathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.137 (0.015)	0.130 (0.016)	0.119 (0.014)	0.131 (0.015)	0.111 (0.014)	0.121 (0.016)	0.106 (0.014)
2 years	0.153 (0.017)	0.142 (0.017)	0.161 (0.018)	0.137 (0.017)	0.152 (0.019)	0.129 (0.017)	
3 years	0.158 (0.018)	0.180 (0.021)	0.153 (0.019)	0.176 (0.022)	0.143 (0.019)		
4 years	0.196 (0.022)	0.165 (0.021)	0.193 (0.024)	0.155 (0.021)			
5 years	0.181 (0.022)	0.202 (0.024)	0.170 (0.022)				
6 years	0.218 (0.027)	0.181 (0.024)					
10 years	0.256 (0.034)						
15 years	0.335 (0.039)						
20 years	0.402 (0.054)						
25 years	0.372 (0.054)						

## (b) Sons/daughters and grandfathers

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.083 (0.024)	0.060 (0.019)	0.076 (0.018)	0.064 (0.016)	0.073 (0.017)	0.066 (0.016)	0.047 (0.015)
2 years	0.072 (0.023)	0.087 (0.020)	0.081 (0.020)	0.087 (0.021)	0.080 (0.019)	0.058 (0.018)	
3 years	0.101 (0.025)	0.087 (0.022)	0.102 (0.024)	0.094 (0.021)	0.069 (0.021)		
4 years	0.100 (0.026)	0.108 (0.025)	0.110 (0.025)	0.076 (0.023)			
5 years	0.121 (0.028)	0.115 (0.026)	0.090 (0.028)				
6 years	0.133 (0.030)	0.088 (0.027)					
10 years	0.178 (0.052)						
15 years							
20 years							
25 years							

Table C.15: IV estimates from two-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36.

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

Table C.16: OLS estimates from three-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men and women in final generation

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

Table C.17: OLS estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men and women in final generation

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

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

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.132 (0.015)	0.127 (0.016)	0.113 (0.014)	0.126 (0.015)	0.105 (0.014)	0.117 (0.016)	0.101 (0.014)
	0.057 (0.024)	0.036 (0.019)	0.054 (0.018)	0.045 (0.016)	0.057 (0.017)	0.049 (0.016)	0.034 (0.015)
2 years	0.150 (0.018)	0.135 (0.017)	0.154 (0.019)	0.128 (0.017)	0.146 (0.019)	0.122 (0.018)	
	0.037 (0.023)	0.057 (0.021)	0.048 (0.021)	0.064 (0.021)	0.053 (0.019)	0.040 (0.019)	
3 years	0.150 (0.019)	0.172 (0.021)	0.144 (0.020)	0.167 (0.023)	0.135 (0.019)		
	0.059 (0.025)	0.056 (0.022)	0.070 (0.024)	0.058 (0.022)	0.043 (0.022)		
4 years	0.188 (0.023)	0.154 (0.021)	0.184 (0.025)	0.145 (0.022)			
	0.056 (0.027)	0.074 (0.026)	0.056 (0.026)	0.048 (0.025)			
5 years	0.169 (0.023)	0.192 (0.025)	0.160 (0.024)				
	0.078 (0.029)	0.061 (0.028)	0.045 (0.029)				
6 years	0.206 (0.029)	0.171 (0.025)					
	0.070 (0.032)	0.044 (0.030)					
10 years	0.200 (0.035)						
	0.104 (0.055)						
15 years							
20 years							
25 years							

Table C.19: IV estimates from three-generation models. Note: 10-year averages start at age 38 and 15-year averages start at age 36. Men and women in final generation

Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.125	0.122	0.110	0.122	0.104	0.111	0.098
	(0.015)	(0.016)	(0.014)	(0.015)	(0.014)	(0.016)	(0.014)
	0.054	0.054	0.057	0.059	0.062	0.061	0.067
2 years	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)	(0.029)
	0.143	0.133	0.153	0.128	0.140	0.119	
	(0.018)	(0.017)	(0.019)	(0.017)	(0.019)	(0.018)	
3 years	0.049	0.051	0.045	0.057	0.053	0.063	
	(0.028)	(0.027)	(0.028)	(0.027)	(0.027)	(0.029)	
	0.147	0.172	0.146	0.162	0.133		
4 years	(0.019)	(0.022)	(0.020)	(0.023)	(0.019)		
	0.048	0.040	0.047	0.049	0.057		
	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)		
5 years	0.187	0.158	0.180	0.145			
	(0.023)	(0.021)	(0.025)	(0.022)			
	0.037	0.044	0.038	0.057			
6 years	(0.028)	(0.028)	(0.028)	(0.029)			
	0.173	0.192	0.160				
	(0.023)	(0.026)	(0.024)				
10 years	0.041	0.035	0.046				
	(0.028)	(0.028)	(0.030)				
	0.204	0.171					
15 years	(0.028)	(0.025)					
	0.033	0.044					
	(0.029)	(0.030)					
20 years	0.247						
	(0.036)						
	0.024						
25 years	(0.031)						
	0.336						
	(0.042)						
30 years	-0.013						
	(0.033)						
	0.387						
35 years	(0.056)						
	-0.008						
	(0.034)						
40 years	0.363						
	(0.058)						
	-0.005						
	(0.035)						

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



Income averaged over ...	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.172	0.176	0.173	0.172	0.173	0.174	0.174
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)
	0.051	0.027	0.045	0.044	0.046	0.039	0.022
2 years	(0.025)	(0.020)	(0.019)	(0.017)	(0.017)	(0.016)	(0.016)
	0.175	0.172	0.171	0.170	0.173	0.173	
	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)	(0.025)	
3 years	0.033	0.053	0.055	0.056	0.047	0.027	
	(0.024)	(0.022)	(0.021)	(0.021)	(0.019)	(0.019)	
	0.170	0.171	0.169	0.169	0.172		
4 years	(0.025)	(0.024)	(0.025)	(0.025)	(0.025)		
	0.062	0.060	0.065	0.056	0.033		
	(0.026)	(0.023)	(0.025)	(0.023)	(0.023)		
5 years	0.169	0.169	0.169	0.171			
	(0.025)	(0.024)	(0.025)	(0.025)			
	0.069	0.070	0.065	0.037			
6 years	(0.027)	(0.026)	(0.027)	(0.026)			
	0.167	0.169	0.171				
	(0.025)	(0.025)	(0.025)				
10 years	0.077	0.070	0.043				
	(0.029)	(0.029)	(0.029)				
	0.167	0.171					
15 years	(0.025)	(0.025)					
	0.080	0.044					
	(0.033)	(0.030)					
20 years	0.146						
	(0.027)						
	0.131						
25 years	(0.055)						

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