Spillover bias in multigenerational income regressions*

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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 to mitigate these. However, remaining bias can lead to a spurious grandparent coefficient estimate in multigenerational regressions, a recent focus of the mobility literature. We show with theory and simulations that even using a 30-year income average can result in a small positive spurious grandfather coefficient estimate. We further propose an IV approach, showing that it is not susceptible to this spillover bias in simplified settings and that it can provide bounds on the parameters in a more general scenario. With administrative data from Norway, we reveal a positive spillover bias in the grandfather coefficient estimates, and the combined evidence from our OLS and IV approaches suggest the preferred small positive OLS estimate could still be upward biased.

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1 Introduction

Measurement error in a regressor is often acknowledged in empirical studies, but the focus tends to be only on potential error in the variable of interest and resulting biases in the corresponding coefficient. In reality, there is often measurement error in other regressors and this can cause bias in the coefficient of interest. Although the notion of bias in one coefficient arising from error in another regressor is a well-known econometric result, it is seldom addressed in practice with empirical studies.

The emerging multigenerational income mobility literature is a recent exception. The regression of interest uses offspring income as the dependent variable, with parent income and grandparent income as two regressors. In this case, the focus is generally on the coefficient on grandparent income, where a positive coefficient implies lower mobility levels; in a sense, the parental income measure is a "control" variable. Solon (2018) noted that due to the econometric result just described, measurement error in parental income could explain a small positive coefficient estimate on grandparents' income.

Our contribution to this literature is to formally show with theory, simulations, and administrative data the role that measurement error may play in the grandparent coefficient estimates. We consider well known income measurement issues and characterize their distinct implications for the multigenerational income mobility estimates, in particular highlighting how small positive grandparent coefficient estimates could be inflated, and may be a consequence of measurement error. First, we note that settings with lower intergenerational mobility (i.e., larger intergenerational persistence parameters) are more susceptible to this bias, due to two parameters underlying the spillover bias factor: the (parent-grandparent) correlation between the regressors, and the parent-child regression parameter. Second, our simulations show that even using long-term averages of income during midlife for all three generations will not eliminate the possibility of estimating a spurious grandparent coefficient. Third, we also show a counter-intuitive result that, for a given parental income measure (e.g., a 20-year average), improving the grandparent income measure actually inflates the spillover bias in the grandparent coefficient, which would otherwise incorrectly be interpreted as reducing attenuation bias. Additionally, we propose an IV approach that has the advantage of requiring a shorter timespan of incomes to minimize bias, and serves as a useful supplemental approach for gauging bias.

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. Further considering that we have very good administrative data, which is not susceptible to some important sources of error present in survey data, our empirical results can be considered an understatement of the potential biases.

More broadly, this paper contributes to the empirical literature as a cautionary note to remain cognizant of measurement error in regressors other than the variable of interest. Our explicit derivations and thus simulation use some assumptions specific to multigenerational mobility, but many of the results could apply also in other settings. For instance, the larger the correlation between the error-ridden regressor and the variable of interest, the larger the magnitude of the spillover bias. In fact, this correlation may be the actual motivation for including the control variable, if one believes the control is highly correlated with our variable of interest and would cause bias if omitted. Further, given that our measurement characterizations are based on income dynamics, a natural extension is to studies that control for a measure of individual or family income (e.g., the child health or early childhood schooling literatures).

The rest of the paper proceeds as follows. In the next section, we provide background on the intergenerational and multigenerational income mobility literatures. Then we formalize the biases from measurement issues in Section 3, both summarizing the existing results on biases in the intergenerational (parent-child) literature as well as extending these to the multigenerational setting. We use these theoretical results to run a simple simulation in Section 4, which illustrates the nature of these biases in the multigenerational estimates. Section 5 describes our administrative data and approach, followed by the empirical results. We provide conclusions in Section 6.

2 Background

Societies throughout the world are concerned with the persistence of poverty (or privilege) across generations, and there is a large descriptive literature examining the extent to which this intergenerational transmission of socioeconomic status occurs.[1] Estimating a basic model,

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

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, β_1, describing associations across generations.[2] 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, γ_1 still describes transmission from parents (though now conditional on grandparents) and γ_2 describes the persistence from grandparents to their grandchildren, conditional on parents. Even a small positive γ_2 can have important implications for mobility, indicating slower mobility than implied by equation (1). For example, Lindahl *et al.* (2015) find positive estimates of γ_2 using survey data on income and education in Malmö, Sweden, and conclude that "estimates obtained from data on two generations *severely underestimate* long-run intergenerational persistence in both labor earnings and educational attainments."[3]

To see this, note that if the model in (1) represents the true underlying transmission process,

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

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

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

then we could use our estimates of β_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 β_1^2. This approximation implies that persistence declines geometrically, so we would observe fairly rapid mobility across generations.[4] 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.*, 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.[5] For a numerical example, consider Norway, where the true β_1 may be around 0.4.[6] In a regression where log income is the outcome (so β_1 is an intergenerational income elasticity), a child whose parents have income 50% above the mean in their generation would be expected to have income around 20% above the mean in the child's generation. Conversely, if the grandparents had income, say, 75% above the mean in their generation, and γ_2 is about 0.1 (assuming γ_1 is 0.4), would imply the child's income would be about 27.5% above the mean.

Recent multigenerational studies use a variety of outcomes, such as education, occupation, or wealth, and a few have used data on individual's income.[7] 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. Long & Ferrie (2018) use income-based occupational status measures in historical censuses for Britain, also finding positive estimates of the grandparent coefficient.

A true small positive grandparent effect is certainly plausible, with a number of possible underlying mechanisms, ranging from biological to social influences or simply through resources.[8]

[4] See Stuhler (2014) for further discussion of this approximation.

[5] 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)).

[6] 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 $\beta_1 = 0.42$.

[7] 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.

[8] The seminal theoretical work by Becker & Tomes (1979) arrives at the perhaps counter intuitive prediction of

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 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 grandparent coefficient, it is important to recognize the limitations of our empirical estimates given the data available to us.

3 Biases from income measurement issues

Measurement issues have long played an important role in the descriptive mobility literature, and have received particular attention in the context of income mobility (e.g., Solon, 1992; Zimmerman, 1992; Mazumder, 2005; Haider & Solon, 2006; Nybom & Stuhler, 2014). The measurement issues stem from the fact that, although we would like to estimate the intergenerational persistence in a long-term (or lifetime) component of income, we do not observe this. Instead we rely on observed annual incomes, either from self-reported survey data or administrative records. The sources of bias that can arise from using such measures include transitory fluctuations in annual income (which we will consider to implicitly include any measurement error in annual reports) and lifecycle variation in both the relationship between permanent and annual incomes as well as in the share of annual income variation due to the transitory components.[9] 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

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.

[9] 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.

Section 3.3, showing how the income measurement issues play out in OLS and IV estimation of equation (2).

3.1 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 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}:

$$x_{i1t} = x_{i1} + v_{i1t} \tag{3}$$

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

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma^2_{x1}}{\sigma^2_{x1} + \sigma^2_{v1}}, \tag{4}$$

where $\sigma^2_{x1} = var(x_{i1})$ and $\sigma^2_{v1} = var(v_{i1t})$. Taking the average over T years of log income reduces the attenuation bias because σ^2_{v1} is then replaced by σ^2_{v1}/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 β_1. Early intergenerational studies use fathers' education to instrument for fathers' income (e.g., Solon, 1992) as well as annual income to instrument for multi-year averages (Altonji & Dunn, 1991), though both studies acknowledge the tenuousness of instrument exogeneity. In the latter approach, a valid instrument can only affect offspring income through the permanent component of the parental income average (so the transitory components cannot be correlated over time). Altonji & Dunn (1991) note that this may not hold because their IV estimates are consistent with some persistence in the transitory component of income.

Mazumder (2005) subsequently shows that such persistence implies worse attenuation bias even when time-averaging with OLS estimation. Suppose the transitory component, v_{i1t}, follows an AR(1) process with persistence parameter δ:

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

Then the OLS estimate converges to:[10]

$$plim(\hat{\beta}_{1,OLS}) = \beta_1 \frac{\sigma_{x1}^2}{\sigma_{x1}^2 + \frac{1}{T}\left(\frac{\sigma_{e1}^2}{1-\delta^2}\right)\phi}, \tag{6}$$

where

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

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:

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

Further complicating things is the lifecycle variation in the size of σ_{v1}^2, which has been found to be U-shaped with the smallest level being in the early 40s (e.g., Mazumder, 2001, 2005).[11] When taking longer term averages of annual income, σ_v^2/T can potentially get larger if σ_{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

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

[11]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.

income changes over the lifecycle, and this can lead to attenuation or amplification bias (e.g., Haider & Solon, 2006). To model this lifecycle variation, equation (3) becomes $x_{i1t} = \lambda_{1t} x_{i1} + v_{i1t}$. λ_{1t} tends to be less than one at younger ages, reaches one around the early 40s when annual income is a reasonable measure of average lifetime income, and then is greater than one at older ages. Incorporating λ_{1t} leads to

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

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 τ to permanent income for offspring). When a T-year average of income is used, again σ_{v1}^2 is replaced by σ_{v1}^2/T and λ_{1t} is replaced by the average over the included years, $\bar{\lambda}_{1T}$. 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 be attenuating or amplifying in nature, as shown by studies emphasizing the importance of measuring annual incomes during the age ranges for which λ_{1t} and $\lambda_{0\tau}$ (or $\bar{\lambda}_{1T}$) are approximately 1 (Haider & Solon, 2006; Nybom & Stuhler, 2014).

Many of the aforementioned intergenerational results have been documented in the literature (e.g., Solon, 1992; 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.

3.2 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 (β_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; Long & Ferrie, 2018). 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 θ_1^* and θ_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 10-year average for parents' income ($\theta_1^* = 0.79$ so $(\theta_1^*)^2 = 0.62$), then a 4-year (or longer) average ($\theta_3^* = 0.66$) for grandparents' income can give $\theta_3^* > (\theta_1^*)^2$, and thus $\hat{\beta}_3 > (\hat{\beta}_1)^2$.

Another analogous comparison studies consider is whether $\hat{\beta}_3 > \hat{\beta}_1\hat{\beta}_2$, 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 $\hat{\beta}_3 > \hat{\beta}_1\hat{\beta}_2$ (despite the true relationship being $\beta_3 = \beta_1\beta_2$) if the attenuation factors satisfy $\theta_3^* > \theta_1^*\theta_2^*$. Since the same grandparent income measure is typically used in the offspring-grandparent and parent-grandparent regressions, $\theta_3^* = \theta_2^*$, meaning any $\theta_1^* < 1$ can lead us to mistakenly conclude that $\hat{\beta}_3 > \hat{\beta}_1\hat{\beta}_2$. 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).

3.3 Biases in the 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 $\rho = corr(x_{i1}, x_{i2})$, which is the intergenerational correlation in the permanent component of income between the parent and grandparent generations. For simplicity, consider the case of stationarity where $var(x_{i1t}) = var(x_{i2t}) = \sigma_x^2$ and $var(v_{i1t}) = var(v_{i2t}) = \sigma_v^2$. The probability limits of the OLS estimators from using annual income measures in the multigenerational equation (2) are:

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

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

The probability limit for each generation's coefficient is decomposed into a linear combination of the respective true parameter times an attenuation factor (θ), *plus* the other generation's true parameter times a spillover factor (ω). In a perfect world with no measurement error, and hence no bias, both attenuation factors would be equal to one, and both spillover factors would be equal to zero.

With measurement error, 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)—although the second element of the $plim(\hat{\gamma}_{2,OLS})$ sum will be zero, the first element ($\gamma_1\omega_2$) will still 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 σ_v^2 in equations (10a) and (10b) are replaced by (σ_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 $plim(\hat{\gamma}_2)$ is largely driven by the size of γ_1 and is also increasing in ρ, so we would expect it to be more substantial in countries with higher levels of intergenerational persistence. Conversely, since we expect the grandfather coefficient γ_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 when we incorporate key features of more realistic earnings processes.[12]

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 γ_1 and γ_2.[13] Although classical errors in variables scenario is 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 simple AR(1) process in equation (5) to capture persistence in the transitory component of earnings for both parents and grandparents, we replace σ_v^2 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 σ_v^2 is replaced with $\frac{1}{T}\left(\frac{\sigma_e^2}{1-\delta^2}\right)\phi$, where ϕ is from equation (7).

Studies have shown that the transitory components are correlated over time, but generally disappear after about 3 years.[14] This means that annual earnings measures 4 or 5 (or more) years apart can be used to instrument for each other, as it seems reasonable to assume that the measurement errors in these years are uncorrelated with each 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 γ_1 and γ_2 are identical to equations (10a) and (10b) except that each σ_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 as-

[12]The probability limits from the multigenerational regression without assuming stationarity are provided in the Appendix.

[13]A few 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.

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

sociation 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 λ_{gt} for each generation ($g = 1, 2$ for parents, grandparents):

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

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

When using T-year averages of income, λ_{gt} and σ_v^2 are replaced with $\bar{\lambda}_{gT}$ and σ_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 σ_v^2. If the increase in σ_v^2 is steep enough, then σ_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 σ_v^2 also leads to larger spillover and attenuation factors. And when considering lifecycle changes in λ_{gt}, the probability limits are slightly more complicated because we have to separately consider λ_{gt} for the income measure treated as endogenous and λ_{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 λ_{gs} appears in these equations, it is λ_{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.

Clearly all of these biases can have varying implications, none of which would be easy to see in isolation if all were incorporated in a probability limit at once. We used simple extensions to account for key features of the earnings process, reflecting the persistent nature of the transitory component and changes over the lifecycle, presenting them separately in sets of equations above. Still, even without incorporating the more complicated models used for realistic earnings processes, the probability limits do not readily exhibit implications of all measurement issues for the bias factors. Hence, we further discuss the implications of the measurement issues in the next section, where we perform simulations to better illustrate and quantify the consequences of these biases in different scenarios.

4 Simulation

To quantify the implications of these biases in multigenerational regressions, we conduct simple simulations based on equations (10a)-(12b). We vary the parameters ρ, δ, and λ_{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, ρ is the correlation in the permanent component of income, x_{ig}, across generations and hence reflects different levels of intergenerational persistence in different societies. The parameters δ and λ determine underlying earnings dynamics. δ is the autocorrelation coefficient in the transitory component of earnings (so a value of zero corresponds to classical errors in variables), and is an important factor determining the effectiveness of using time-averaging or IV estimation to reduce attenuation bias. λ_{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, σ^2_{xt}, so that we only need to make assumptions about the variance shares $\frac{\sigma^2_v}{\sigma^2_{xt}}$ and $\frac{\sigma^2_x}{\sigma^2_{xt}}$ to calculate the attenuation and spillover factors (θ and ω).[15]

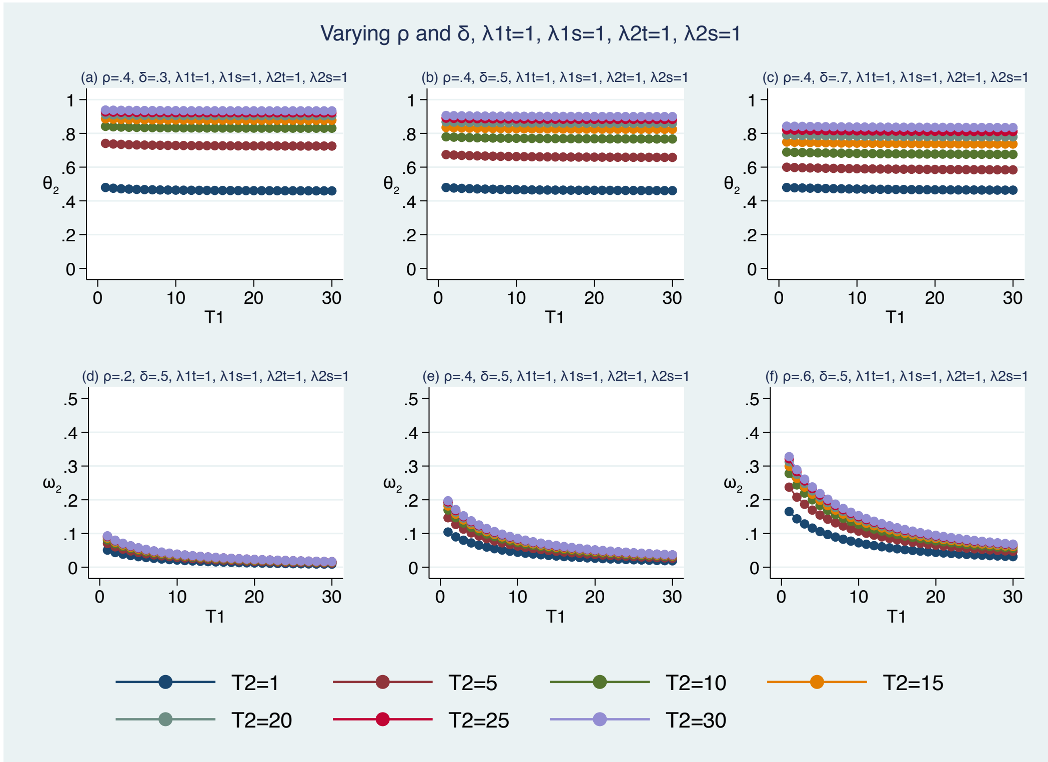
4.1 Illustrating attenuation and spillover bias

We consider several different scenarios, varying δ (0.3, 0.5, 0.7), ρ (0.2, 0.4, 0.6), and λ_{gt} (0.8, 1, 1.2). We set the variance shares at $\frac{\sigma^2_v}{\sigma^2_{xt}} = \frac{\sigma^2_x}{\sigma^2_{xt}} = 0.5$ for our base case, but also set $\frac{\sigma^2_v}{\sigma^2_{xt}} = 0.7$ for a robustness check. For a given set of these parameters, we vary the number of years over which income is averaged for parents (T_1) and grandparents (T_2) for OLS, or similarly, the number of years between the endogenous and instrument earnings measures for IV. We present results for a subset of these scenarios for pedagogical purposes, focusing on biases in the grandparent coefficient and considering a base case with $\rho = 0.4$, $\delta = 0.5$, and all $\lambda_{gt} = 1$. This base case is 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), and 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. If no bias were present, the attenuation factor (θ_2) would equal one and the spillover factor (ω_2) would equal zero. For our base case of $\rho = 0.4$, $\delta = 0.5$, time-averaging reduces attenuation bias from about 52% (θ_2=0.48) when using annual income ($T_2 = 1$) to about 10% (θ_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 (θ_2) for different values of δ. On the left, we can see that a smaller δ (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).

[15] Also following Mazumder (2005), we assume σ^2_e adjusts so that $\sigma^2_v = \frac{\sigma^2_e}{1-\delta^2}$ holds.

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



Similarly, the graph on the right shows that a larger δ means time averaging is less effective. In all cases, as we improve grandparents' income measure (moving from one dotted line to another), the attenuation factor is reduced. Improving the parental income measure by increasing T_1 (moving from left to right along each dotted line), does not help reduce attenuation bias in the grandparent coefficient. The intergenerational correlation, ρ, also has little impact on the attenuation bias, so we do not show the attenuation coefficients with different ρ here, though these results are available upon request.

The issue of spillover bias in the grandparent coefficient, however, is present because of ρ. For our base case of $\rho = 0.4$, $\delta = 0.5$, time-averaging reduces the spillover coefficient from about 10% (ω_2=0.1) when using annual incomes for both generations to about 4% (ω_2=0.04) with a 30-year average for each generation. We know $\rho > 0$ from the substantial body of evidence on parent-child mobility in many countries. The size of ρ, along with the parent coefficient γ_1, determine the size of the overall spillover bias. [16] As shown in the bottom row of Figure 1, when ρ is small (0.2), the

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

spillover coefficient ω_2 is also somewhat small. When we triple ρ to 0.6 the extent of spillover also approximately triples for shorter-term averages of parent income (i.e, small T_1). This combined with the fact that γ_1 is also likely larger for countries with large ρ implies that OLS estimates for such societies are more susceptible to spurious grandparent effects.

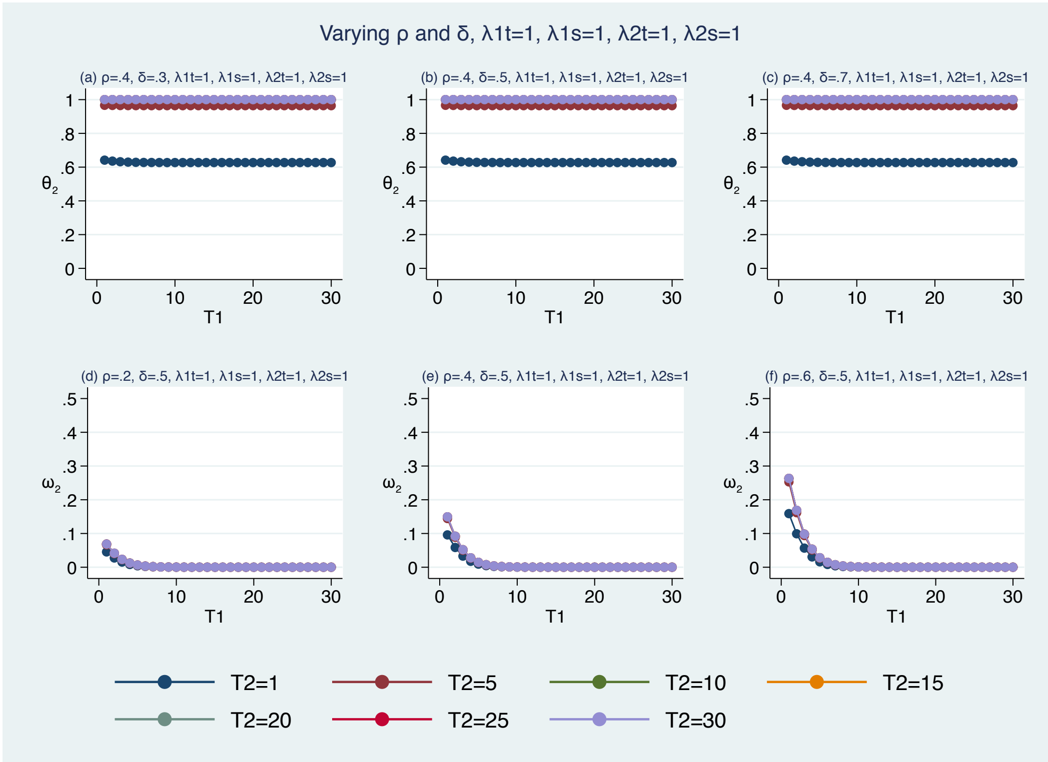
There are also other important patterns to note. First, for a given parental income measure, attempting to reduce potential attenuation bias in the grandparent coefficient by including more years in the average income measure for grandparents actually worsens the spillover bias. In ω_2, time-averaging for the grandparent implies replacing the only σ_v^2 outside of parenthesis by σ_v^2/T_2, which effectively shrinks the denominator thereby increasing ω_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 ρ 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 (θ_2) and spillover (ω_2) factors for IV estimation using an individual's annual income measure in year s to instrument for that individual's income in year t. In the figures, T_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 δ is, it can be nearly eliminated using income measures in a relatively short time period (up to about 10 years with high δ). When $T_1 = T_2 = 1$, the attenuation factor θ_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 ρ, it is still eliminated with relatively short timespans of income.

These simulation results are enlightening for multigenerational regressions, but have abstracted

between the child and parent generations, conditional on parents. And, changes in income inequality from the grandparent to parent generation would be reflected in γ_1.

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



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

The implications of lifecycle variation in the association between annual and lifetime income, reflected by λ_{gt}, are more complicated. See Appendix Figures B.1-B.4 for analogous graphs of 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 (θ_g is smaller). When income is measured at younger ages, so $\lambda_{gt} < 1$, θ_2 can be larger than one which means there is amplification bias rather than attenuation.

The spillover factor (ω_g) is larger when $\lambda_{gt} < 1$ and smaller when $\lambda_{gt} > 1$, reinforcing the attenuation or amplification bias from θ_g. Considering the combined implications of the lifecycle effects on θ_g and ω_g, the OLS coefficient estimates of γ_g are possibly biased upward when $\lambda_{gt} < 1$ 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 $\lambda_{gt} \neq 1$. Still, extending ages symmetrically in both directions leads to a greater likelihood that $\bar{\lambda}_{gT}$ 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.

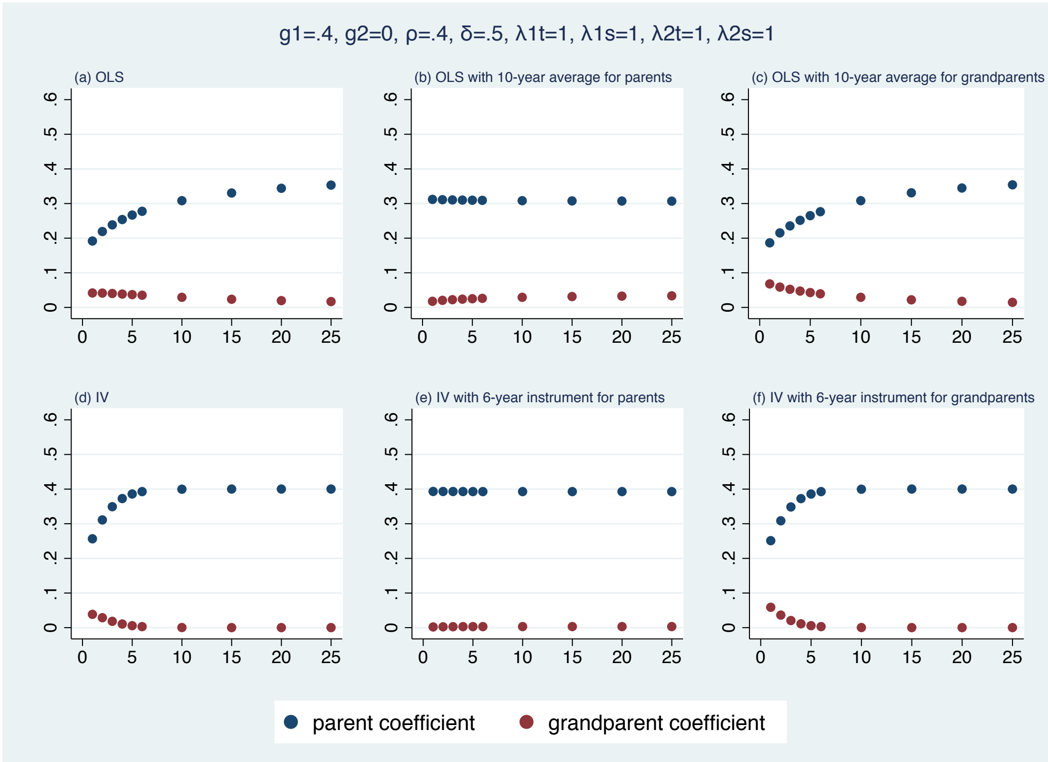
We focused most of our discussion here on the biases in the grandparent coefficient, which is of primary interest here. The analogous results for θ_1 and ω_1 for parents are the same by design (available upon request). However, the extent to which the spillover (ω_1) bias affects the magnitude of our coefficient estimate depends on the size of γ_2. Since this is presumably small relative to γ_1, spillover bias will not generally be problematic for the parent coefficient, so changing T_2 does not have appreciable impacts on our estimate of γ_1. Rather, addressing the attenuation bias is the main issue, as is customary with intergenerational income regressions.

4.2 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 γ_1 and γ_2 for our base case where $\rho = 0.4$, $\delta = 0.5$. We choose $\gamma_1 = 0.4$ and $\gamma_2 = 0$ to be the underlying population parameters as these are plausible population values for our sample from Norway and reveal the potential for a spurious grandparent coefficient in this setting.

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.

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



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 in $\hat{\gamma}_{1,OLS}$ 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 $\hat{\gamma}_{2,OLS}$, a result that would typically be interpreted as reducing attenuation bias. In our controlled setting here, we know that this is actually increasing the size of ω_2, hence increasing the size of the spurious grandparent coefficient. Figure 3(c) presents estimates from the opposite exercise, where we use a 10-year average for grandparents' income, but vary T_1 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. More generally, this illustrates how important the parental income measure is to our estimate of the grandparent coefficient.

Turning to the IV estimates in 3(d), 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 or grandparents in 3(d) and 3(f), respectively. 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 parents also (T_1 is not large enough). When we are able to use a 6-year distance in instrument and endogenous income for both generations though—which is feasible in some datasets now—IV does appear to nearly eliminate bias.

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.

5 Data and empirical results

5.1 Data

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

The offspring generation is comprised of men born 1974-1978, with incomes measured at ages 32-36 (until 2015). This age range is selected to minimize lifecycle bias, 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 based on evidence in Nilsen *et al.* (2012) for similar cohorts in Norway. To avoid sample composition differences across specifications and approaches, we present results based on a balanced sample where all three generations meet the following income requirements.

Sons must have positive income in at least three of five years from ages 32-36. The income measures are based on the log of annual labor income so we exclude observations with non-positive earnings. Included in our various constructions of earnings measures for fathers and grandfathers are averages over 2, 3, 4, 5, 6, 10, 15, 20, and 25 years (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.

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

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.

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

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

Although it would be nice to have a larger and unquestionably representative sample for Norway, it is not necessary for one of our primary purposes in this paper—to illustrate how bias from income measurement can inflate the grandparent coefficient or even produce a spurious grandparent effect. For this, it is most important to maintain a balanced sample across methods to avoid sample composition issues driving different patterns in our results. Additionally, we present results for males only. The tendency to omit females (especially mothers and grandmothers) from intergenerational income analyses arises in large part from female labor force participation patterns and the inability to observe outcomes. In our case, given that the rationale for our methodological choices is based on earnings processes for males, it is appropriate to focus on sons, fathers and grandfathers in our analysis.[17]

5.2 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:

[17] The results for samples including daughters are similar, and are discussed below (Section 5.4).

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

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

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

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 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 are possible) 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.

5.3 Main results

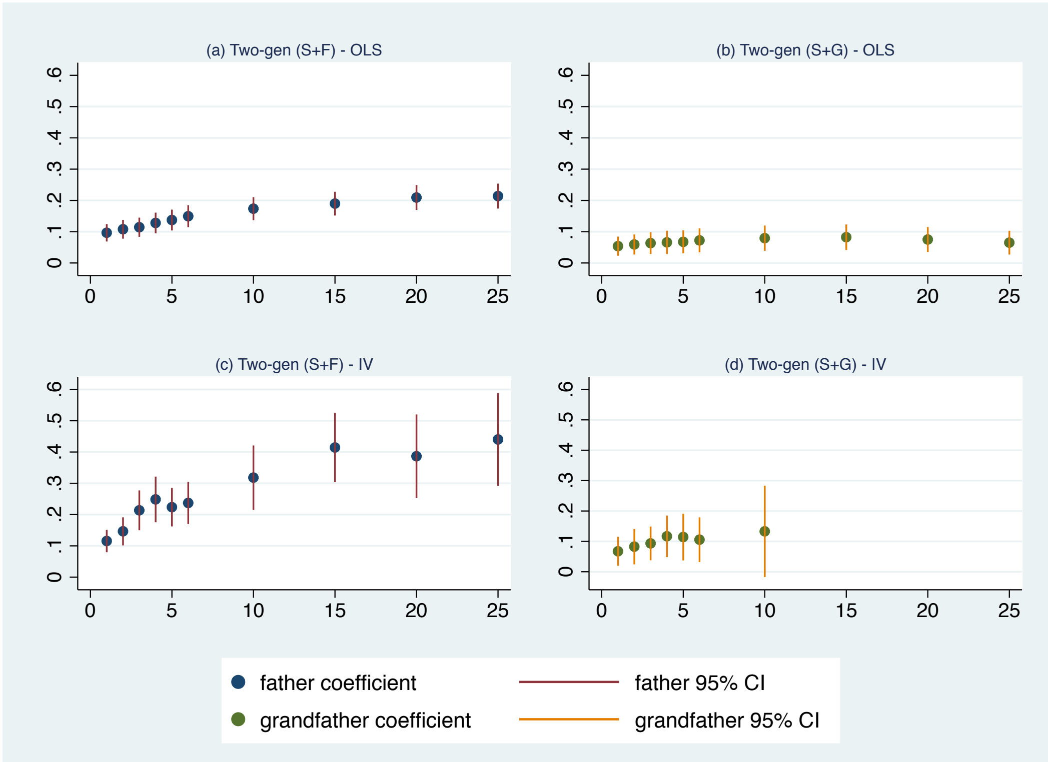
To examine income mobility across generations in Norway, we begin by showing results for two generation regressions to illustrate the results of various approaches in this well known setting. Then we turn to the multigenerational setting, which is of primary interest in this paper, to show how the biases 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.

5.3.1 Two-generation regression results

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, ex-

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



panding 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. Therefor, 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. We use 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).[18]

The longer term estimates are based on subsets of our main sample which contributes to the imprecision (97.3%, 94.7%, 92.5%, 87.3%, respectively, for the 10-, 15-, 20-, and 25-year estimates).[19] With these longer timespans possibly extending into ranges where $\lambda_{gt} < 1$ (as the endogenous age decreases), the estimates around 0.4 may contain 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 since it is possible $\lambda_{gt} > 1$ for these estimates, they may be attenuated. Taken together, our IV and reverse IV estimates (shown in Appendix) 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.[20]

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 either in the form of increasing $\bar{\lambda}_{2T}$ or increasing σ^2_{vt}.

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

[18]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.

[19]Sensitivity checks do not indicate that sample composition is driving these higher estimates.

[20]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).

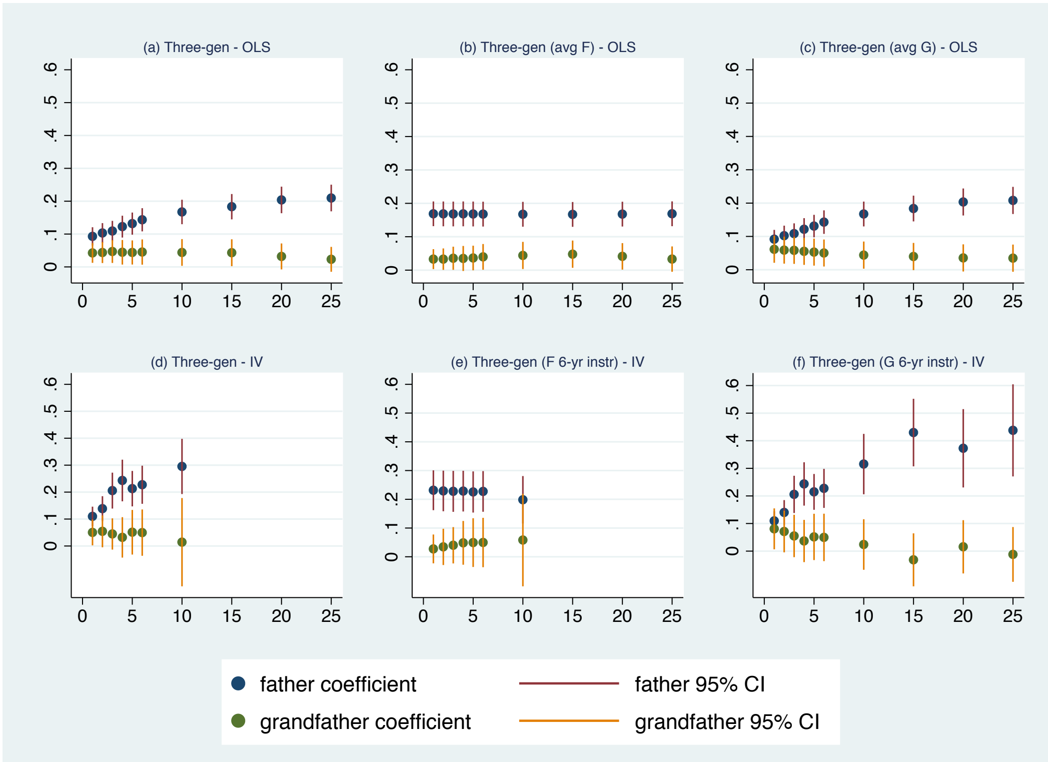
from 0.07 to 0.11. The estimate based on a 10-year distance between measures is similar at 0.13, but the sample is substantially smaller (N=3,535, 69.0%). The samples were even smaller for the 15-, 20-, and 25-year estimates so we omit these results out of concern regarding strong sample composition effects.

Overall, our results from regressions involving two generations follow the expected patterns established in prior studies. We can also consider comparisons of the son-father and son-grandfather estimates for implications regarding multigenerational mobility with the simple extrapolation discussed earlier, where β_1^2 is used to approximate longer term persistence. If $\hat{\beta}_3 > \hat{\beta}_1^2$, researchers take this to be evidence of slower multigenerational mobility than predicted by the simple intergenerational model in (13). Suppose we only had data that allowed us to use 5-year averages of income for parents and grandparents. In that case, $\hat{\beta}_{3,OLS} = 0.098$ is more than twice the size of $\hat{\beta}_{1,OLS}^2 = 0.039$. If we account for the attenuation bias in these estimates based on our simple earnings process with $\delta = 0.5$ and $\frac{\sigma_v^2}{\sigma_{xt}^2} = 0.5$, so $\theta = 0.69$), this implies the "true" parameters would be $\beta_{3,OLS} = 0.098$ and $\beta_{1,OLS}^2 = 0.039$, still exhibiting a large difference. If, however, we consider our "best" estimates (based on using 25-year average income measures), which are $\hat{\beta}_{1,OLS}^2 = 0.046$ and again accounting for the attenuation bias ($\theta = 0.90$), this implies $\beta_1^2 = 0.057$ and $\beta_3 = 0.072$. The much smaller difference of only 0.016 suggests mobility could be slightly slower than implied by the model in (13), but this is a very minor difference. And, if we consider a case with more persistence ($\delta = 0.8$) so the attenuation factor is 0.77, then the difference is only 0.007. In our results, comparing intergenerational estimates can be sensitive to the income measures used, and also the assumed earnings process if we attempt to account for attenuation bias underlying the estimates.

This is all the more reason we should also look at the multigenerational mobility estimates from regressions involving all three generations. The extrapolation of intergenerational persistence estimates to make inferences about long-term mobility stemmed from data limitations, and comparing our intergenerational estimates does not allow us to conclusively say whether the mobility predicted by the traditional intergenerational model understates persistence. We next turn to the actual multigenerational regressions that have become the focus of current studies, as they are becoming feasible with some datasets.

5.3.2 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 now what we should expect for 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 (θ) from improving one's own income measure was more meaningful than the reduction in the spillover factor (ω) 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 isolate the effect of changing the grandfather income measure on the coefficient estimate for fathers. The OLS coefficient estimate for fathers remains essentially constant though 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 γ_2 being zero (or very small).

Comparing these OLS results in 5(b) to the results in 5(a) where we simultaneously improve fathers' income measures, we can also 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. Then 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). Next we see 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.

Finally, this exercise of changing the grandfather measure while holding the father measure constant is also consistent with the counter-intuitive result we found with our simulation. The pattern of increasing grandfather coefficient estimates is similar to that we saw with our simulation results that showed improving the grandfather measure for a given father measure could worsen spillover. However, here we cannot distinguish this from decreasing attenuation bias since we do not know the true population parameters.

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 coefficients are decreasing as we improve the income measure for fathers, showing the reduction in spillover bias. We can also evaluate the counter-intuitive simulation result by comparing the first coefficient of 0.06 in 5(c) when fathers' income is measured using annual log income (and a 10-year average is used for grandfathers) to the estimate of 0.04 in 5(a) when an annual measure was used for both generations. 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 has the advantage, at least theoretically in our simple simulation, of nearly eliminating bias in this setting when T_g is large enough for the degree of persistence in v_{itg} (e.g., after 6-10 years in our simulation). The bottom graphs in Figure 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 for endogenous and instrument income measures 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 and 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 suggest that the true grandfather coefficient for our sample is very small, or possibly even zero. The OLS estimates based on longer-term income averages are around 0.02-0.03. The IV estimates based on longer-term instruments fluctuate around zero, with some being negative in sign. None of the longer-term estimates are statistically significantly different from zero, although the IV estimates are quite imprecise. More importantly though, the patterns in our empirical results mirror those found in the simulation, especially for OLS. Our IV estimates vary more widely, likely due to sensitivity to lifecycle effects, and also suggest persistence in the transitory component of income matters. Although the IV estimates are imprecise, they do provide a useful secondary empirical exercise for gauging bias. In general, our results show that empirically researchers must be aware of how sensitive the grandfather coefficient is to the construction of the *parental* income measure, and the tendency for the estimates to be positively biased.

5.4 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 with both men and women in the youngest generation (Figures C.7 and C.8). In general, the coefficients 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 improved 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.

6 Conclusion

The role of measurement error in estimating intergenerational income regressions has been addressed extensively in the literature. The resulting biases are fairly well understood and it has become standard to construct income measures in a way that mitigates 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 rates of long-term mobility.

This paper illustrates the implications of measurement error in the multigenerational 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 20 years during mid-life does not eliminate the potential for estimating a spurious grandfather coefficient. In addition, even when the true grandparent coefficient is zero, for a given measure of fathers' income, increasing the years we average over for grandfathers actually worsens the spillover bias. If we observe increasing coefficient estimates as a result, this could be misinterpreted as reducing attenuation bias in actual data settings where we do not know the true grandparent coefficient is zero. The IV approach we propose has the advantage of theoretically mitigating (or eliminating) these biases with relatively short timespans of income, depending on the degree of persistence in the transitory component of income. And, although the IV estimator is more susceptible to lifecycle bias, one can easily test for this by obtaining two sets of IV estimates—the original IV estimates and the "reverse IV" estimates switching the endogenous and instrument income measures—which then provide bounds on the coefficients.

With our administrative data, we see the expected result that time-averaging reduces attenuation bias in OLS estimates in the intergenerational regression, and we also show similar results for IV approaches that allow for persistence in the transitory component of annual income measures. In the multigenerational setting, we show 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 $\sigma^2_{x_g}$ and $\sigma^2_{v_g}$ are allowed to vary across generations ($g = 1, 2$).

In the population, the true multigenerational process is:

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

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

$$x^*_{it1} = x_{i1} + v_{it1}, \tag{18a}$$

$$x^*_{it2} = x_{i2} + v_{it2}. \tag{18b}$$

So the equation we estimate with our data is:

$$y_{i0} = \gamma_1 x^*_{it1} + \gamma_2 x^*_{it2} + \epsilon^*_{it}. \tag{19}$$

A.1 OLS estimation

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

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

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

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

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

$$\vdots$$

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

Similarly, for γ_2, we get:

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

Taking the probability limits gives us:

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

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

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:

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

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

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

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

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

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

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

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

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

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:

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

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:

$$z^*_{is1} = x_{i1} + v_{is1}, \tag{27a}$$

$$z^*_{is2} = x_{i2} + v_{is2}. \tag{27b}$$

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:

$$\hat{\gamma}_{1,IV} = (z^{*'}_1 A_2 x^*_1)^{-1} z^{*'}_1 A_2 y \tag{28a}$$

$$= [z^{*'}_1 (I - x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2) x^*_1]^{-1} z^{*'}_1 (I - x^*_2(z^{*'}_2 x^*_2)^{-1} z^{*'}_2) y \tag{28b}$$

$$= [z^{*'}_1 x^*_1 - z^{*'}_1 x^*_2 (z^{*'}_2 x^*_2)^{-1} z^{*'}_2 x^*_1]^{-1} [z^{*'}_1 y - z^{*'}_1 x^*_2 (z^{*'}_2 x^*_2)^{-1} z^{*'}_2 y] \tag{28c}$$

$$= \left[\sum_{i=1}^{N} z^*_{i1} x^*_{i1} - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \left(\sum_{i=1}^{N} z^*_{i2} x^*_{i2}\right)^{-1} \sum_{i=1}^{N} z^*_{i2} x^*_{i1}\right]^{-1} \left[\sum_{i=1}^{N} z^*_{i1} y_i - \sum_{i=1}^{N} z^*_{i1} x^*_{i2} \left(\sum_{i=1}^{N} z^*_{i2} x^*_{i2}\right)^{-1} \sum_{i=1}^{N} z^*_{i2} y_i\right] \tag{28d}$$

$$\vdots$$

$$\hat{\gamma}_{1,IV} = \frac{\sum_{i=1}^N z^*_{i1} y_i \sum_{i=1}^N z^*_{i2} x^*_{i2} - \sum_{i=1}^N z^*_{i1} x^*_{i2} \sum_{i=1}^N z^*_{i2} y_i}{\sum_{i=1}^N z^*_{i1} x^*_{i1} \sum_{i=1}^N z^*_{i2} x^*_{i2} - \sum_{i=1}^N z^*_{i1} x^*_{i2} \sum_{i=1}^N z^*_{i2} x^*_{i1}} \tag{28e}$$

Similarly, for γ_2, we get:

$$\hat{\gamma}_{2,IV} = \frac{\sum_{i=1}^N z^*_{i2} y_i \sum_{i=1}^N z^*_{i1} x^*_{i1} - \sum_{i=1}^N z^*_{i2} x^*_{i1} \sum_{i=1}^N z^*_{i1} y_i}{\sum_{i=1}^N z^*_{i2} x^*_{i2} \sum_{i=1}^N z^*_{i1} x^*_{i1} - \sum_{i=1}^N z^*_{i2} x^*_{i1} \sum_{i=1}^N z^*_{i1} x^*_{i2}} \tag{29}$$

Taking the probability limits we get:

$$plim(\hat{\gamma}_{1,IV}) = \frac{cov(z^*_1, y)cov(z^*_2, x^*_2) - cov(z^*_1, x^*_2)cov(z^*_2, y)}{cov(z^*_1, x^*_1)cov(z^*_2, x^*_2) - cov(z^*_1, x^*_2)cov(z^*_2, x^*_1)} \tag{30a}$$

$$plim(\hat{\gamma}_{2,IV}) = \frac{cov(z^*_2, y)cov(z^*_1, x^*_1) - cov(z^*_2, x^*_1)cov(z^*_1, y)}{cov(z^*_2, x^*_2)cov(z^*_1, x^*_1) - cov(z^*_2, x^*_1)cov(z^*_1, x^*_2)} \tag{30b}$$

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 $\sigma^2_{x_g} \equiv var(x_{ig})$ and $\sigma^2_{v_g} \equiv var(v_{itg})$

for $g = 1, 2$ and $\rho \equiv corr(x_1, x_2)$, allowing us to write the elements of the probability limits as:

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

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

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

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

Substituting these into the probability limits in (30a) and (30b), and then doing some algebra shows that $plim(\hat{\gamma}_{1,IV}) = \gamma_1$ and $plim(\hat{\gamma}_{2,IV}) = \gamma_2$. However, if we consider the case of an AR(1) process for v_{itg}, then (31a) does not hold. Rather, $cov(v_{isg}, v_{itg}) = \delta^{T_g}\frac{\sigma_{e_g}^2}{1-\delta^2}$ where $T_g = t - s$ is the years between the earnings measures x_{itg} and z_{isg}. In this case, the probability limits of the IV estimators are the same as those for the OLS estimators in (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 $plim(\hat{\gamma}_1)$ and $plim(\hat{\gamma}_2)$

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

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

$$x^*_{it1} = \lambda_{t1} x_{i1} + v_{it1}, \tag{32a}$$

$$x^*_{it2} = \lambda_{2t} x_{i2} + v_{it2}. \tag{32b}$$

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:

$$var(x^*_g) = \lambda_{tg}\sigma^2_{x_g} + \sigma^2_{v_g} \tag{33a}$$

$$cov(x^*_1, x^*_2) = \lambda_{t1}\lambda_{t2}\rho\sigma_{x_1}\sigma_{x_2} \tag{33b}$$

$$cov(y, x^*_1) = \lambda_{t1}\gamma_1\sigma^2_{x_1} + \lambda_{t1}\gamma_2\rho\sigma_{x_1}\sigma_{x_2} \tag{33c}$$

$$cov(y, x^*_2) = \lambda_{t2}\gamma_2\sigma^2_{x_2} + \lambda_{t2}\gamma_1\rho\sigma_{x_1}\sigma_{x_2} \tag{33d}$$

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

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

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

The equations for our instruments can now be written:

$$z^*_{is1} = \lambda_{1s} x_{i1} + v_{is1}, \tag{35a}$$

$$z^*_{is2} = \lambda_{2s} x_{i2} + v_{is2}. \tag{35b}$$

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

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

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

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

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

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

And the probability limits of the estimators are:

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

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

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

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

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

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

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

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

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

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

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

B Simulations

B.1 Simulation results with lifecycle effects

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

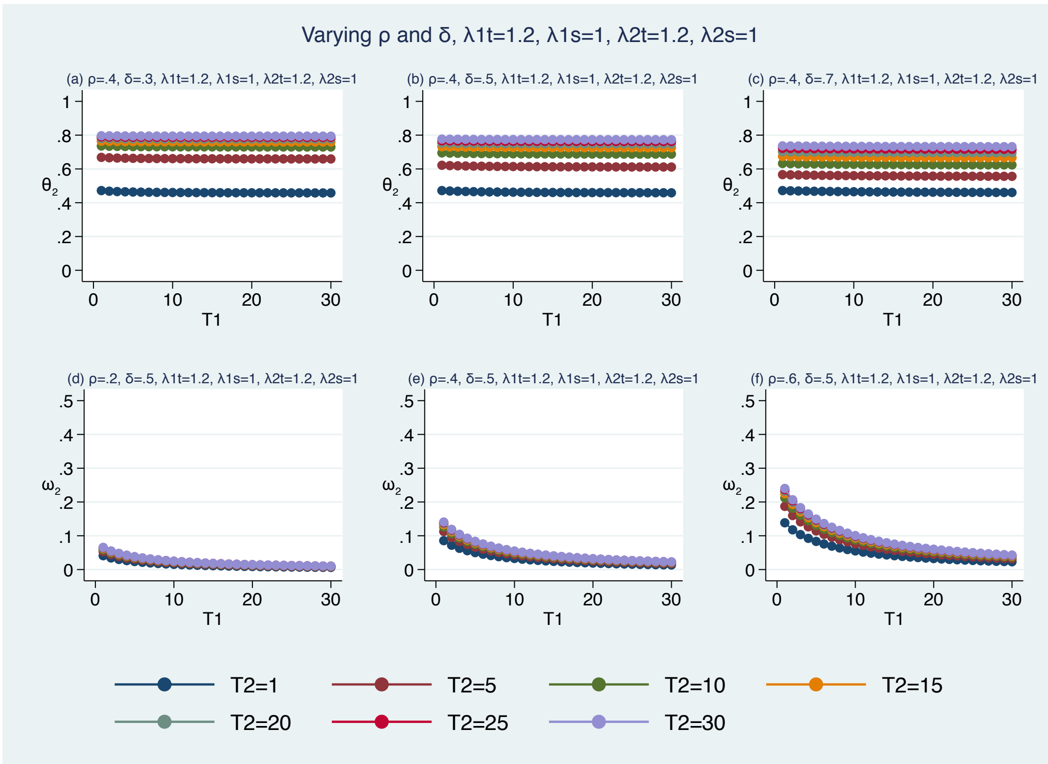


Figure B.2: Attenuation and spillover in OLS estimates when $\lambda_1 = \lambda_2 = 0.8$

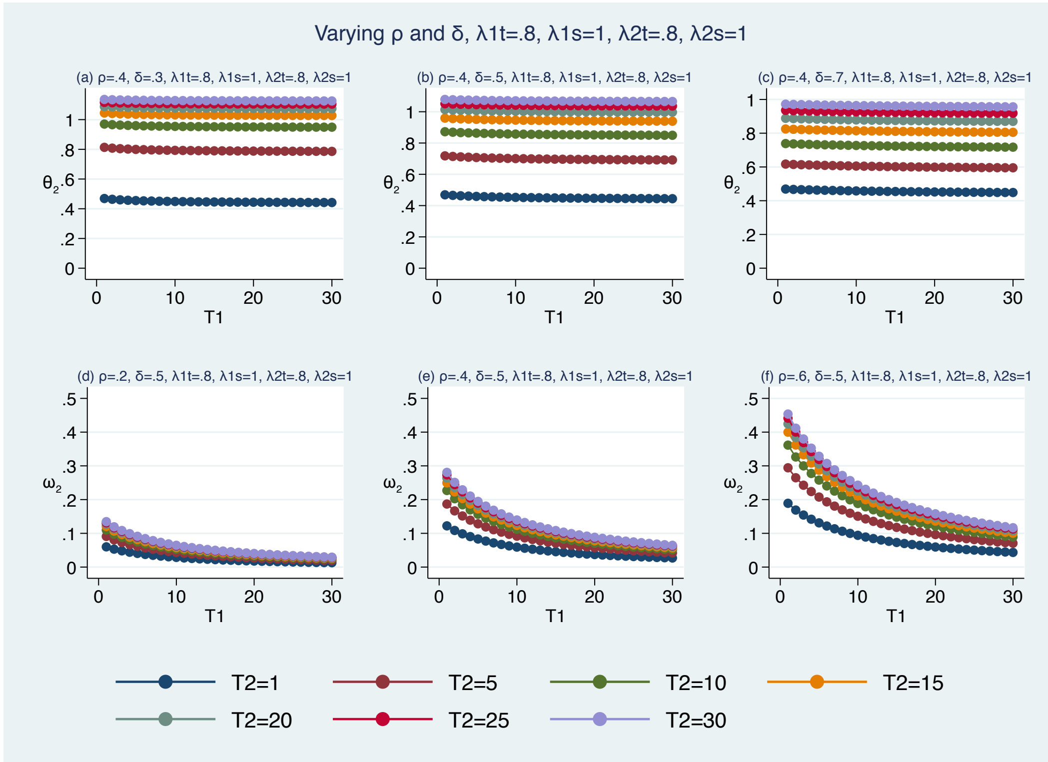


Figure B.3: Attenuation and spillover in IV estimates when $\lambda_{1t} = \lambda_{2t} = 1.2$, $\lambda_{1s} = \lambda_{2s} = 1$

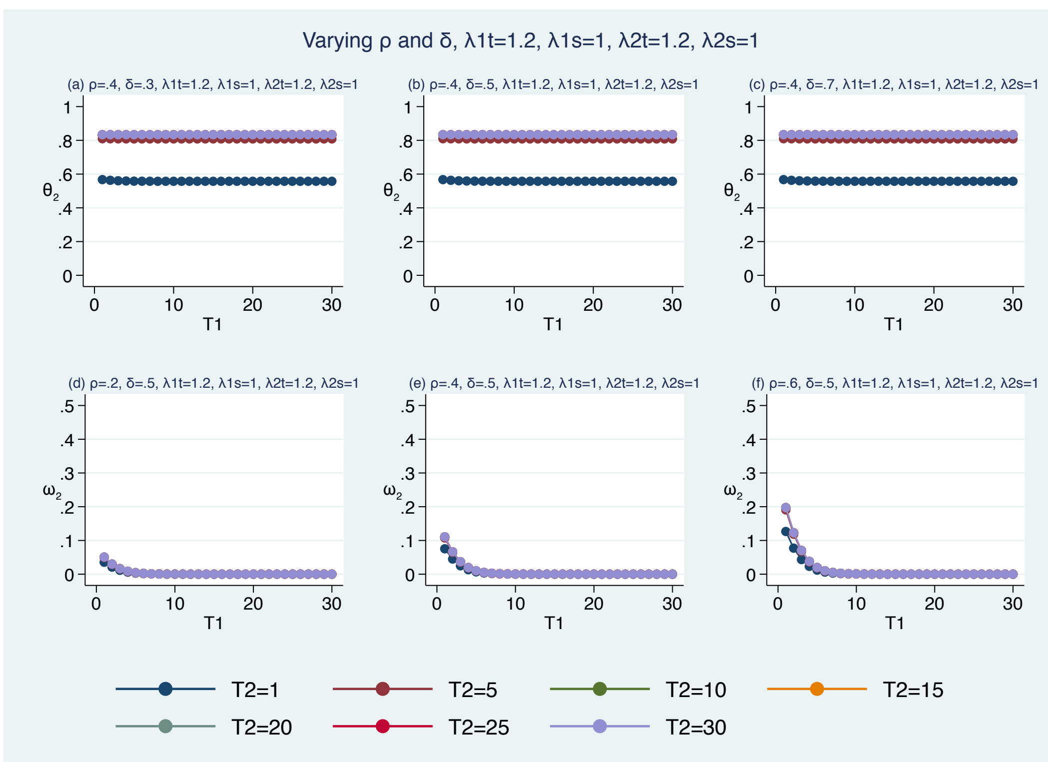
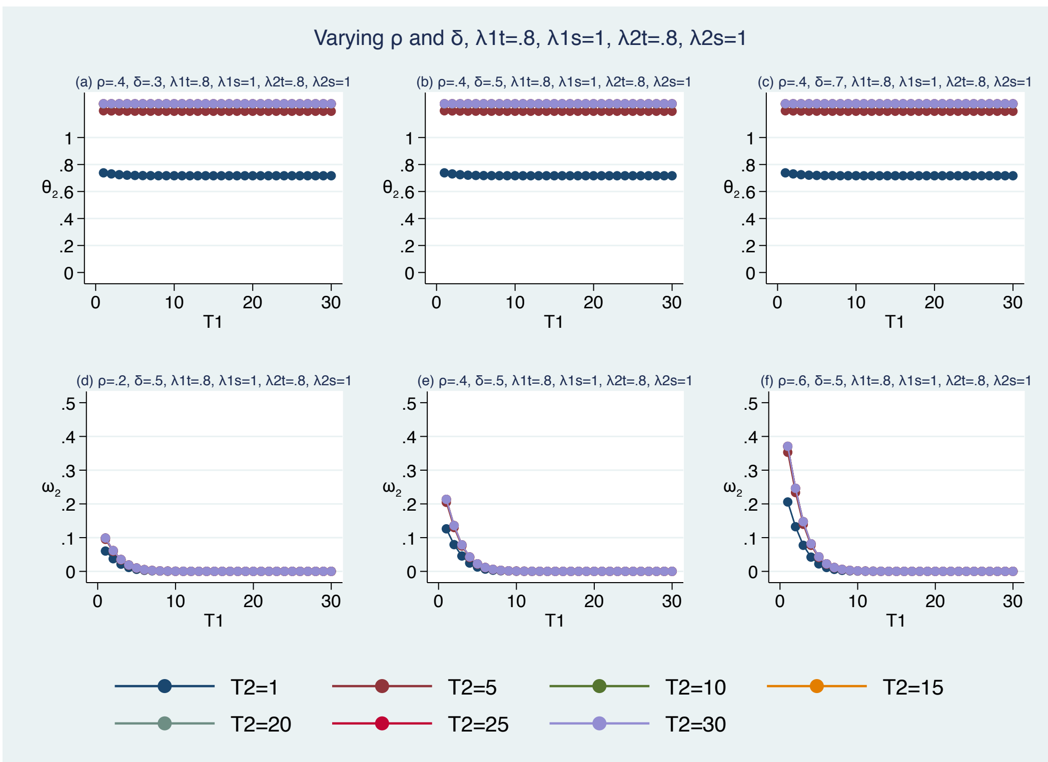


Figure B.4: Attenuation and spillover in IV estimates when $\lambda_{1t} = \lambda_{2t} = 0.8$, $\lambda_{1s} = \lambda_{2s} = 1$



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

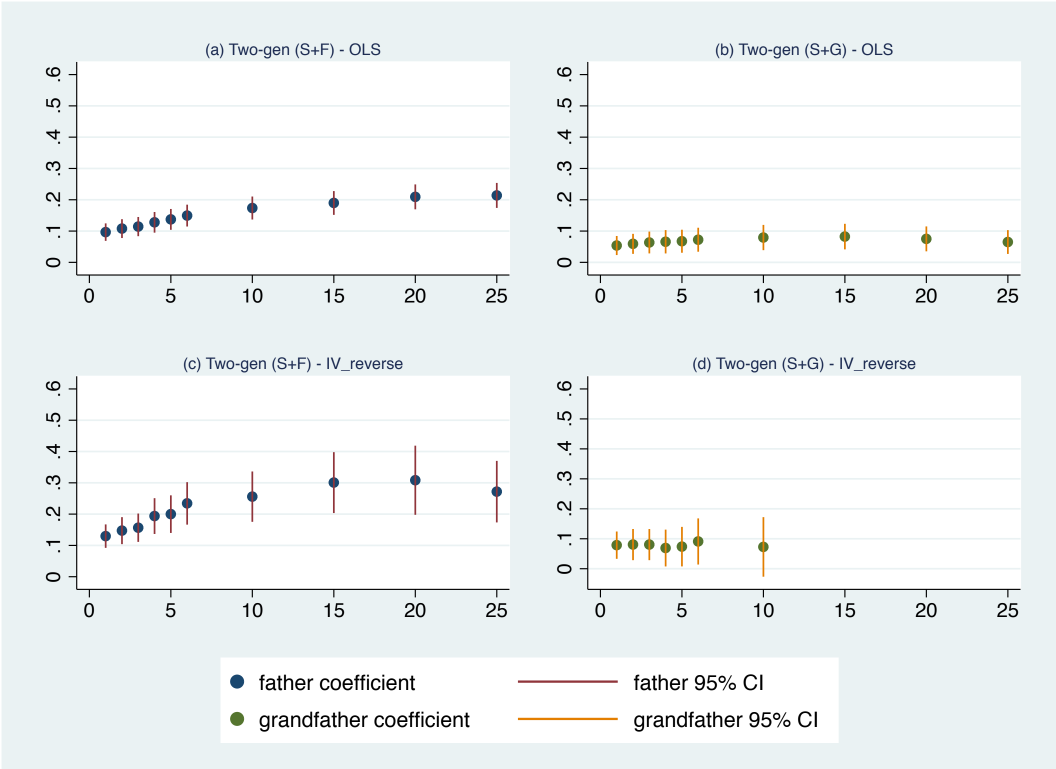
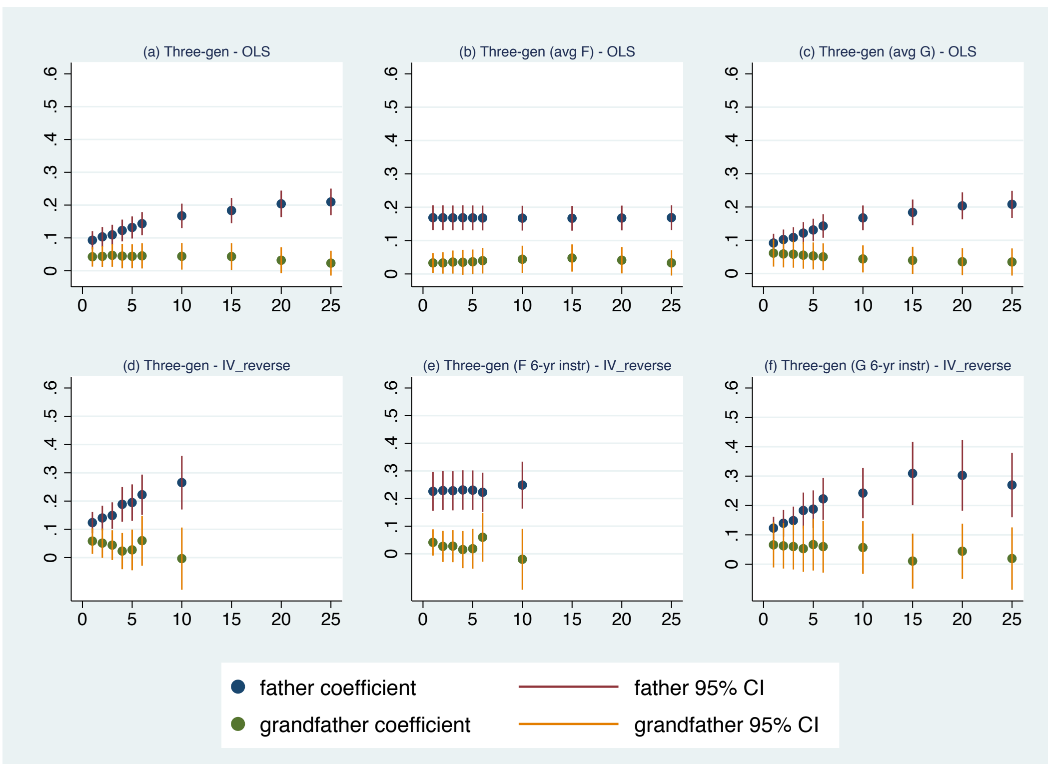


Figure C.6: Attenuation and spillover in 3 generation IV estimates when income at older age is used as the endogenous measure



C.2 Figures using men+women sample

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

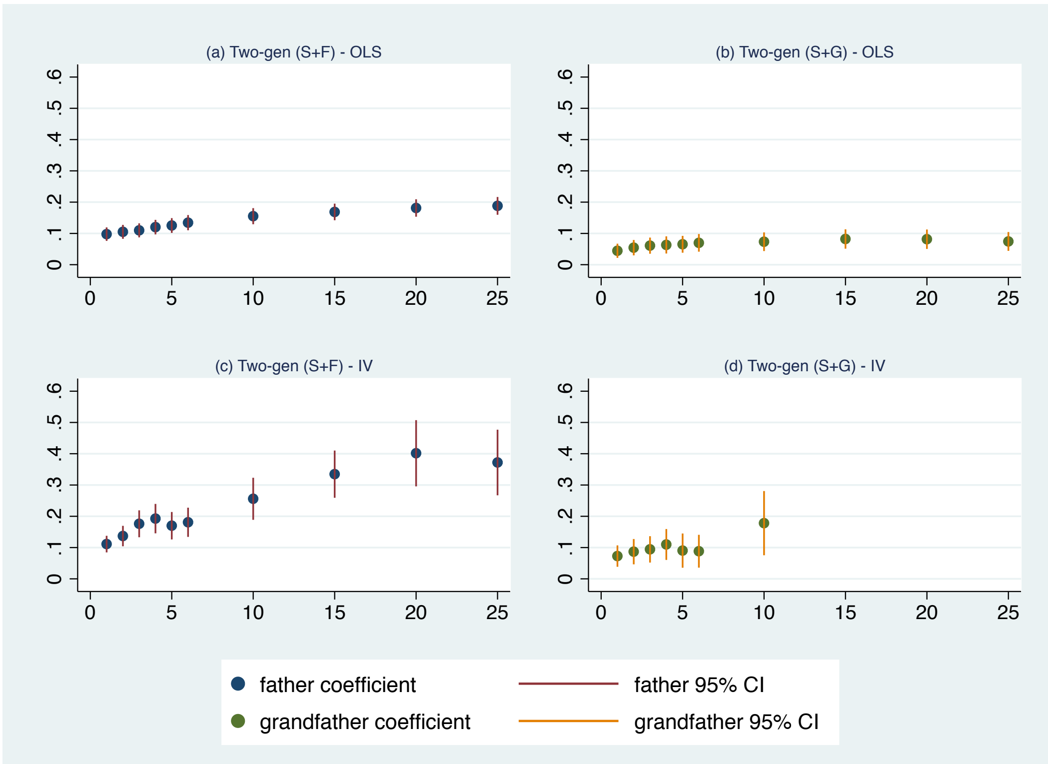
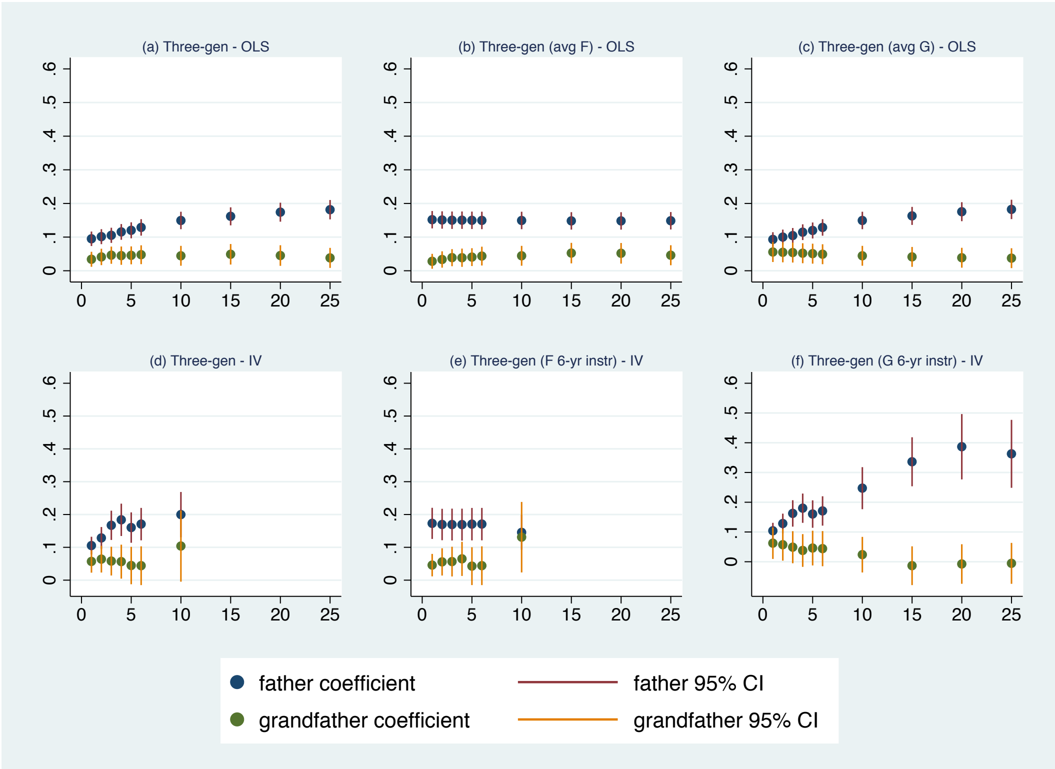


Figure C.8: OLS and IV estimates from three-generation regressions. Men and women in final generation.



C.3 Tables of regression coefficients

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

See Tables C.2-C.9

(a) Sons and fathers

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

(b) Sons and grandfathers

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

Table C.2: OLS estimates from two-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.Estimates from Figure 4 (panels a and b) in bold.

(a) Sons and fathers

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.154	0.147	0.138	0.133	**0.115**	0.149	0.127
	(0.022)	(0.024)	(0.021)	(0.020)	**(0.018)**	(0.023)	(0.018)
2 years	0.167	0.167	0.172	**0.146**	0.177	0.159	
	(0.026)	(0.025)	(0.025)	**(0.023)**	(0.027)	(0.023)	
3 years	0.180	0.199	0.174	**0.214**	0.171		
	(0.027)	(0.030)	(0.028)	**(0.033)**	(0.025)		
4 years	0.207	0.191	**0.248**	0.192			
	(0.031)	(0.031)	**(0.037)**	(0.028)			
5 years	0.202	0.261	**0.224**				
	(0.032)	(0.039)	**(0.031)**				
6 years	0.270	**0.237**					
	(0.041)	**(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

Time difference	Age starting from...						
obs. - instr.	39	40	41	42	43	44	45
1 years	0.086	0.059	0.077	0.077	**0.067**	0.067	0.059
	(0.034)	(0.027)	(0.024)	(0.022)	**(0.024)**	(0.021)	(0.021)
2 years	0.072	0.088	0.103	**0.082**	0.076	0.072	
	(0.033)	(0.027)	(0.029)	**(0.030)**	(0.024)	(0.025)	
3 years	0.105	0.109	0.101	**0.093**	0.083		
	(0.034)	(0.031)	(0.036)	**(0.028)**	(0.029)		
4 years	0.128	0.103	**0.117**	0.092			
	(0.037)	(0.037)	**(0.035)**	(0.033)			
5 years	0.114	0.116	**0.114**				
	(0.040)	(0.035)	**(0.039)**				
6 years	0.135	**0.105**					
	(0.040)	**(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; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.Estimates from Figure 4 (panels c and d) in bold.

(a) Sons and fathers

Time difference obs. - instr.	Age starting from... 39	40	41	42	43	44	45
1 years	0.192	0.164	0.147	0.119	0.130	0.109	0.121
	(0.026)	(0.024)	(0.022)	(0.018)	(0.019)	(0.017)	(0.017)
2 years	0.232	0.199	0.163	0.147	0.145	0.132	
	(0.032)	(0.029)	(0.025)	(0.022)	(0.021)	(0.021)	
3 years	0.265	0.212	0.186	0.157	0.162		
	(0.036)	(0.031)	(0.029)	(0.023)	(0.024)		
4 years	0.274	0.228	0.194	0.161			
	(0.037)	(0.034)	(0.029)	(0.025)			
5 years	0.300	0.227	0.200				
	(0.041)	(0.033)	(0.031)				
6 years	0.292	0.234					
	(0.040)	(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

Time difference obs. - instr.	Age starting from... 39	40	41	42	43	44	45
1 years	0.092	0.083	0.053	0.064	0.079	0.059	0.064
	(0.025)	(0.032)	(0.023)	(0.020)	(0.023)	(0.022)	(0.020)
2 years	0.109	0.085	0.059	0.081	0.079	0.071	
	(0.029)	(0.033)	(0.026)	(0.027)	(0.024)	(0.025)	
3 years	0.109	0.088	0.068	0.080	0.086		
	(0.029)	(0.034)	(0.031)	(0.027)	(0.028)		
4 years	0.111	0.098	0.069	0.083			
	(0.030)	(0.038)	(0.031)	(0.030)			
5 years	0.116	0.097	0.074				
	(0.031)	(0.038)	(0.034)				
6 years	0.120	0.091					
	(0.033)	(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; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

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

Table C.5: OLS estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 5 (panel a) in bold.

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

Table C.6: OLS estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 5 (panel b) in bold.

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

Table C.7: OLS estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 5 (panel c) in bold.

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

Table C.8: IV estimates from three-generation models. Note: 10-year averages start at age 38. Estimates from Figure 5 (panel d) in bold.

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

Table C.9: IV estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38. Estimates from Figure 5 (panel e) in bold.

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

Table C.10: IV estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Estimates from Figure 5 (panel f) in bold.

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

Table C.11: "Reverse IV" estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

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

Table C.12: "Reverse IV" estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

Time difference obs. - instr.		Age starting from...						
		39	40	41	42	43	44	45
1 years	F	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)
	G	0.049	0.058	0.064	0.067	0.066	0.070	0.081
		(0.040)	(0.039)	(0.040)	(0.039)	(0.039)	(0.039)	(0.044)
2 years	F	0.220	0.187	0.153	0.139	0.137	0.126	
		(0.033)	(0.030)	(0.026)	(0.023)	(0.022)	(0.021)	
	G	0.045	0.053	0.058	0.062	0.062	0.079	
		(0.039)	(0.040)	(0.039)	(0.039)	(0.040)	(0.044)	
3 years	F	0.252	0.200	0.176	0.148	0.154		
		(0.038)	(0.033)	(0.031)	(0.024)	(0.025)		
	G	0.040	0.048	0.055	0.060	0.073		
		(0.040)	(0.039)	(0.040)	(0.040)	(0.044)		
4 years	F	0.262	0.220	0.183	0.153			
		(0.040)	(0.036)	(0.031)	(0.026)			
	G	0.034	0.046	0.053	0.074			
		(0.039)	(0.040)	(0.040)	(0.044)			
5 years	F	0.290	0.219	0.187				
		(0.045)	(0.035)	(0.033)				
	G	0.031	0.045	0.067				
		(0.040)	(0.040)	(0.045)				
6 years	F	0.280	0.222					
		(0.042)	(0.036)					
	G	0.033	0.060					
		(0.040)	(0.045)					
10 years	F	0.242						
		(0.044)						
	G	0.057						
		(0.046)						
15 years	F	0.309						
		(0.055)						
	G	0.011						
		(0.048)						
20 years	F	0.302						
		(0.061)						
	G	0.044						
		(0.048)						
25 years	F	0.270						
		(0.056)						
	G	0.019						
		(0.054)						

Table C.13: "Reverse IV" estimates from three-generation models, long-term average for grandfathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

C.4 Tables for sample of men+women

See Tables C.14-C.20

(a) Sons/daughters and fathers

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

(b) Sons/daughters and grandfathers

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

Table C.14: OLS estimates from two-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

(a) Sons/daughters and fathers

Time difference obs. - instr.	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.137	0.130	0.119	0.131	0.111	0.121	0.106
	(0.015)	(0.016)	(0.014)	(0.015)	(0.014)	(0.016)	(0.014)
2 years	0.153	0.142	0.161	0.137	0.152	0.129	
	(0.017)	(0.017)	(0.018)	(0.017)	(0.019)	(0.017)	
3 years	0.158	0.180	0.153	0.176	0.143		
	(0.018)	(0.021)	(0.019)	(0.022)	(0.019)		
4 years	0.196	0.165	0.193	0.155			
	(0.022)	(0.021)	(0.024)	(0.021)			
5 years	0.181	0.202	0.170				
	(0.022)	(0.024)	(0.022)				
6 years	0.218	0.181					
	(0.027)	(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

Time difference obs. - instr.	Age starting from...						
	39	40	41	42	43	44	45
1 years	0.083	0.060	0.076	0.064	0.073	0.066	0.047
	(0.024)	(0.019)	(0.018)	(0.016)	(0.017)	(0.016)	(0.015)
2 years	0.072	0.087	0.081	0.087	0.080	0.058	
	(0.023)	(0.020)	(0.020)	(0.021)	(0.019)	(0.018)	
3 years	0.101	0.087	0.102	0.094	0.069		
	(0.025)	(0.022)	(0.024)	(0.021)	(0.021)		
4 years	0.100	0.108	0.110	0.076			
	(0.026)	(0.025)	(0.025)	(0.023)			
5 years	0.121	0.115	0.090				
	(0.028)	(0.026)	(0.028)				
6 years	0.133	0.088					
	(0.030)	(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; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31.

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

Table C.16: OLS estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

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

Table C.17: OLS estimates from three-generation models, long-term average for fathers. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

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

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

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

Table C.19: IV estimates from three-generation models. Note: 10-year averages start at age 38; 15-year averages start at age 36; 20-year averages start at age 33; and 25-year averages start at age 31. Men and women in final generation

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

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

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

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