Do Grandparents Still Matter?
Multigenerational Mobility in the US from 1940-2013

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January 8, 2016

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Abstract

Most studies of intergenerational mobility focus exclusively on the transmission of (dis)advantage from parents to their children. Detecting any influence of grandparents or even earlier generations on children's outcomes is seldom possible even in long-running longitudinal studies such as the Panel Study of Income Dynamics . We link family lines from the 1940 U.S. Census to the Current Population Survey of the 1970s and 1980s and to the 2000-13 censuses and American Community Surveys. This allows us to examine the impact of both parents and grandparents on children's adult income and educational attainment in a large, nationally-representative sample. After accounting for measurement error, we find that for both outcomes grandparents matter, and they matter more than a simple AR(1) characterization of the transmission process predicts. Our results suggest that two-generation studies overstate how quickly (dis)advantage dissipates over time within family lines.

This report is released to inform interested parties of ongoing research and to encourage discussion of work in progress. Any error or omissions are the sole responsibility of the authors. Any views expressed on statistical, methodological, technical, or operational issues are those of the author and not necessarily those of the U.S. Census Bureau. All data used in this paper are confidential. The estimates in this paper are based on responses from a sample of the population. As with all surveys, estimates may vary from the actual values because of sampling variation and other factors. All comparisons made in this paper have undergone statistical testing and are significant at the 95 percent confidence level unless otherwise noted. Estimates are calculated from the portion of each survey sample matched to administrative records, and so are not fully representative of the national population.

1 Introduction

Recent economic research on rising inequality in the U.S. since the mid-1970s (Piketty and Saez 2013) has been accompanied by increased research by economists on a related topic of long-standing interest among sociologists – correlation across generations within family lines in economic and social status. Though research on intergenerational mobility has advanced dramatically over the past two decades, with a proliferation of methodological improvements (Solon 1992; Mazumder 2005), data sources (Hendren et al. 2014; Feigenbaum 2014; Feigenbaum 2015; Hilger 2015; Clark et al. 2015; Grusky et al. 2015), and international comparisons (Björklund and Jäntti 1997; Solon 2002; Bourdieu, Ferrie, and Kesztenbaum 2009; Long and Ferrie 2013), this work has focused almost exclusively on the transmission of (dis)advantage from parents to their children. The influence of grandparents and earlier generations, if it is addressed at all, is inferred through iteration of the parental influence, a procedure that is unsatisfying in a number of respects. (Stuhler 2012)

The focus on just parents and children is unfortunate but dictated by practical considerations: few longitudinal studies have been running long enough to have captured the experiences of three generations through adulthood in the same family lines. Yet there are several reasons why we would expect that grandparents would matter. Mare (2011) describes several channels through which generations prior to the parents could have an impact on children's outcomes, including inheritance of financial assets or of social networks, or the direct effects that personal contact between grandparents and their grandchildren could have in a society where their increased longevity means their lives will increasingly overlap.

The inability to assess a “grandparent effect” also shapes how we view, in a purely statistical sense, the parent-child transmission. As Solon (1992) notes, an intergenerational income correlation will be biased downward by measurement error which will usually be most pronounced in the parents’ income data. But information on grandparents can be used to reduce the impact of this downward bias, even if we are interested in only parent-child transmission (Solon 2014). At the same time, if we possess three generations of data, we also possess data on two two-generation transitions, making it possible to assess how parent-child mobility may have changed across adjacent cohorts. Whether mobility has declined (Long and Ferrie 2013) or risen (Lee and Solon 2009; Hilger 2015) over time in the U.S. remains an open question that three generation data can address.

Finally, the presence of a grandparent effect has been detected in many of the places where it has been possible to look for it: rural China (Zeng and Xie 2014), Sweden (Lindahl et al. 2015), Britain (Chan and Boliver 2013), Germany (Hertel and Groh-Samberg 2014), Chile (Celhay and Gallegos 2015), Denmark (Boserup 2013) and even the pre-WWII U.S. (Olivetti et al. 2014, Long and Ferrie 2012). In fact, one of the few places where such an effect has not been consistently observed is the modern U.S. Warren and Hauser (1997), using the Wisconsin Longitudinal Study, found no independent grandparent effect, though Mare (2011, p. 16) warns that "mid-twentieth century Wisconsin families may be a population in which multigenerational effects are unusually weak." One study using the Panel Study of Income Dynamics (PSID) has found a grandparent effect (Hertel and Groh-Samberg 2014), but neither Hodge (1965), Behrman and Taubman (1985), nor Peters (1992) found one.

We contribute here to the research on three-generation mobility in several ways. We link family lines from the 1940 U.S. Census to the Current Population Survey (CPS) of the 1970s and 1980s and to the 2000-13 censuses and American Community Surveys (ACS). This allows us to examine the impact of both parents and grandparents on children's adult income and educational attainment in a large, nationally-representative sample. After accounting for measurement error, we find that for both outcomes grandparents matter, and they matter more than a simple AR(1) characterization of the transmission process predicts. Our results suggest that two-generation studies overstate how quickly dis(advantage) dissipates over time within family lines.

2 Data

To create three-generational links across the twentieth century, we use the complete count 1940 Census, the 1973, 1979, and 1981-1990 CPS ASEC, the 2000 Long Form Census, and the 2001-2013 ACS. We employ Person Identification Keys (PIK), assigned by the Census Bureau, to link individuals across data sources. PIKs are assigned by the Person Identification Validation System (PVS), which uses a probabilistic matching algorithm to compare characteristics of records in census and survey data to characteristics of records in a reference file constructed from the Social Security Administration (SSA) Numerical Identification System (or NUMIDENT) as well as other administrative data. These characteristics may include Social Security Number (SSNs), full name, date of birth, address, place of birth, and parents' names depending

on the information available in the census or survey.[1] The PIK uniquely identifies a particular person and allows us to link individuals across data sources processed by PVS.

For the contemporary linkages, we begin with a sample of both male and female children assigned PIKs in the 1940 Census. We observe these children living with their parents in 1940, creating the first parent-child link between the first and second generation. We then search for the second generation's PIKs in the 1973, 1978, and 1981-1990 CPS ASEC. From this linkage, we observe the outcomes of the second generation as adults and create another parent-child link from the second to the third generation by observing the second generation residing with their own children in the CPS ASEC. We then locate the PIKs of the third generation in the 2000 Long Form Census or the 2003-2013 ACS.

We rely on reported relationship to household head to establish parent-child relationships and identify spouses. We omit subfamilies in the 1940 Census, but we include subfamilies observed in the CPS ASEC. We require adults fall between the ages of 25-55 when we observe their adult education attainment, occupation, or earnings. We also require that the age observed for a record across multiple data sources are within a five-year interval around what we would expect. For example, if a person is 3 years old in 1940, we would that person to be roughly 53 years old in 1990, and we drop the observation if they are younger than 48 or older than 58 years old.

We observe at least one parent in the 1940 Census for 41,862 sons and 35,747 daughters in the CPS ASEC. From the CPS ASEC to 2000-2013, we observe at least one parent in the CPS ASEC for 33,112 sons and 34,310 daughters found in 2000-2013. Across all three files, we identify at least one grandparent in the 1940 census for 8,267 grandsons and 7,660 granddaughters. Once we remove observations missing education attainment, we observe education attainment across three-generations for 6,713 sons and 6,312 daughters. We observe earnings for 3,935 sons and 3,727 daughters, and we observe income for 4,244 sons and 3,985 daughters with a multigenerational link between the 1940 Census, CPS ASEC, and 2000-2013.

Tables 1-3 report descriptive statistics of the multigenerational education, earnings, and income samples. Across all three samples, grandparents are roughly 10 years younger than the parents. Fathers are roughly 7 years older than their children and mothers are roughly 4 years older than their children. Average education attainment

[1] See the Appendix A for more information on the record linkage technique.

increases across the 3 generations, with grandparents obtaining an average of 8 years of schooling, parents an average of 10 years, and children an average of 14 years.

3 Measuring Mobility

Data on Education Attainment

The 1940 Census was the first census to collect information on schooling. Respondents reported both highest grade of schooling completed and whether they were attending school on March 1, 1940. Unlike future decennial censuses that limited education questions to the long form sample, the 1940 census requested schooling information from all respondents. However, the 1940 census did not collect years in various types of grades (e.g., years of college) and the convention of "common" schools (e.g., grammar school instead of graded schools) resulted in inflated completion of secondary school grades (Goldin and Katz, 1999). The 1973-1990 CPS March supplements also collected highest grade completed. The 2000 Census and the 2001-2013 ACS, however, did not collect highest grade completed. Instead, the 2000-2013 data collected educational categories that we use to construct a consistent years of schooling variable across all data sources.[2]

Earnings and Income Data

The 1940 Census requested wages and salaries for all respondents 14 years and older. This allows us to observe individual and household earnings of the first generation. The 1940 census did not collect wages and salaries self-employed respondents, which includes nearly all of those employed in farming and a high percentage of individuals with high levels of education attainment (Goldin and Katz, 1999). Consequently, our earnings and income samples include fewer observations than our education attainment samples. The CPS, ACS, and 2000 long form data report earnings, in addition to income.

4 Model and Raw Results

To measure mobility between two generations, we regress the child's outcome (Y_{it}) on the parent's outcome (Y_{it-1})

$$Y_{it} = \alpha + \beta_1 Y_{i,t-1} + \varepsilon_{it}. \quad (4.1)$$

[2] See Appendix A for more information on the years of schooling recode.

We also include fixed effects for the year we observe the child's adult outcome and fixed effects for the year we observe the parent's outcome as well as age and age-squared terms. We consider several outcomes including log income, log earnings, and educational attainment. We use the family income, earnings, and education in each generation averaged across all observed families.[3] These regressions produce a simple summary statistic that measures mobility across two generations.

To measure mobility across three generations, we regress the child's outcome on the parent's outcome while controlling for the grandparent's outcome ($Y_{i,t-2}$),

$$Y_{it} = \alpha + \beta_1 Y_{i,t-1} + \beta_2 Y_{i,t-2} + \varepsilon_{it}. \quad (4.2)$$

We control for age and age squared of the child, parent, and grandparent, and we include year fixed effects. We include regression results for three samples: 1) the three-generation sample where we successfully link grandparents, parents, and children, 2) the two generation grandparent-parent sample, and 3) the two generation parent-child sample. The three-generation sample is a subset of the two-generation samples as both intergenerational links are necessary to be present in the three-generation sample.

Educational Mobility

We analyze mobility of education first, as it is the least likely to be affected by measurement error (discussed in the next section). Our measure of educational attainment is the years of schooling of the most educated parent. The regression results for education attainment are shown in Table 5 Panel A. In both the three generation and two generation samples, we find an increase in the persistence of parent education on their children. For the three-generation sample, one year of grandparent education is associated with an additional 0.320 years of parent education and 1 year of parent education is associated win an additional 0.411 years of child education, shown in Columns 1 and 2 respectively. These estimates are slightly larger than the correlation between parent's and grandparent's education attainment (Column 5) and the correlation between the parent's and child's education attainment (Column 6). This may imply the estimated mobility difference between the two sets of generations is partially driven by changes in the educational variance over time as the

[3] This includes up to four sets of observed grandparents, two sets of observed parents and the one observed child family.

estimated correlation coefficients are 0.295 for grandparents to parents compared to 0.351 for parents to children.[4]

Our estimate for the intergenerational mobility of education is broadly consistent with the estimates in the literature. For example, Hertz et al (2007) estimate an average coefficient over a series of birth cohorts of 0.46. Although their average is greater than our estimate, the range of estimates in their data is relatively wide, from below 0.4 to above 0.5.[5] Similarly, Hilger (2015) estimates the intergenerational education correlation of 0.398 in 1940 and 0.278 in 2000 for the white population.

In column 3, we regress the child's education on the education of both earlier generations simultaneously. The parent coefficient is 0.385 and the grandparent coefficient is 0.042. Therefore, an additional year of parent education is associated with same increase in child education as nine additional years of grandparent education conditional on parent education, or for education, grandparents matter slightly more than 10 percent as much as parents matter.

Income and Earnings Elasticity

We also estimate the intergenerational elasticity of income and earnings, shown in Panels B and C of Table 5. We are restricted to using earnings in both sets of regressions for grandparents because income data is not available in the 1940 Census.

For the intergenerational elasticity of income, we estimate a coefficient of 0.172 for grandparents-parents and 0.275 for parents-children. The parent-child estimate is consistent with Solon (1992) estimates from single survey-year income for a slightly later period. For the full three-generation regression, the parent coefficient is 0.250 and the grandparent coefficient is 0.093.

For the intergenerational elasticity of earnings, we estimate a grandparent-parent coefficient of 0.155, which is not statistically significantly different from the 0.172 elasticity estimated for income. This is not surprising as grandparent earnings are used in both regressions. For the parent-child regression, the coefficient is only 0.177, much lower than the 0.275 for income. In the full three-generation regression, the parent

[4] The magnitude of the persistence is smaller in the two-generation samples. For grandparents to parents, the two-generation sample estimate is 0.295 (vs. 0.320). For parents to children, the two-generation sample estimate is 0.351 (vs. 0.411). This could be due to selection into the three-generation sample or due to averaging education across all families in a given generation. If we just follow male lines (shown in Appendix Table 1) or female lines (shown in Appendix Table 2) the three-generation parent-child coefficients are greater than the two-generation ones, but there is no statistical difference for the grandparent-parent coefficients.

[5] Their sample includes 3.351 individuals and parent education level is reported by children.

coefficient is 0.160 and the grandparent coefficient is 0.093. The grandparent coefficient is the same in the income and earnings regressions, although it nearly half the size of the parent-child earnings coefficient and closer to a third the size of the parent-child income coefficient.

Taken at face value, these results imply that grandparent earnings matter a lot in terms of child earnings or income, much more so than in terms of education. However, as Solon (2015) notes, if there is classical measurement error in incomes, the model we fit will result in upwardly biased (or spuriously positive) estimates for the association between grandparent and child status conditional on parent status. As we believe years of education are measured with much less error than income or earnings, it is therefore possible that the large grandparent coefficients in the income and earnings regressions relative to the education regressions are a result of measurement error. In the next section, we focus on this issue.

5 Measurement Error

Addressing measurement error is one of the most challenging aspects of studying intergenerational mobility. There are many potential sources of error, which are summarized in detail in Black and Devereux (2011) and Jäntti and Jenkins (2013). In this section, we discuss some aspects of this literature, how it is related to our data, and how we propose to address it.

Background

Ideally, intergenerational mobility estimates measure the relationship between the lifetime income and earnings of each generation. However, it is rarely the case that complete lifetime income histories are available for one, let alone multiple generations. For example, in our data, we observe only single-year earnings for all three generations and single-year income for the parent and child generations. Early mobility literature moved forward with single-year estimates but a number of studies have shown that using single year income or earnings causes a downward bias in the estimated elasticity.

Single-year earnings include transitory variations that potentially bias results. If the measurement error is classical, the downward bias can be substantial, as shown by Solon (1992). By averaging earnings over multiple years, Solon (1992) lessens this bias by reducing the “noise” associated with the measurement error relative to the signal. However, Mazumder (2005) shows that averaging over multiple years may not

be nearly as effective at reducing the attenuation if the transitory shocks are correlated over time. In a comparison of actual lifecycle income and income averaged over multiple years, Nybom and Stuhler (2011) find that this bias may still be substantial when using multiple years of income for the children. Another source of error arises from when the survey observes income. Haider and Solon (2006), Böhlmark and Lindquist (2006), and Grawe (2006) show that the measurement error in income is related to the age of the respondent in the survey. This lifecycle bias can attenuate measures of IGE.[6]

The literature on measurement error in income observed in surveys highlights another dimension of the problem, non-classical measurement error.[7] Piscke (1995) shows that the measurement error in annual wages is mean reverting. Using the PSID Validation Study, he compares reported earnings to the administrative earnings paid by the firm and shows that the error is negatively correlated with administrative earnings. Gibson and Kim (2011) also examine mean-reverting measurement error in survey data and show that if the error is in an independent variable, the direction of bias is uncertain, but if the error is in the dependent variable, the bias is downward.

To assess the degree of non-classical mean-reverting measurement error, we analyze CPS ASEC data linked to W-2 wage earnings reports to the Social Security Administration for CPS ASEC years 1991, 1994 and 1996-2009, hereafter called the CPS-DER.[8] The construction of this data is discussed in detail in Rothbaum (2015). We compare the family earnings reported on the survey to the average administrative family earnings when the older spouse is 35-50 years old. Controlling for CPS ASEC survey year, age, and age-squared of the older spouse in the survey, the correlation between the measurement error in earnings and the administrative earnings is -0.37, nearly twice as large as the largest value in Pischke's PSID Validation Study results. As a result, in this paper we consider a general error-in-variables model with mean reverting errors in each income year:

$$y_{it} = y_{it}^{*} + e_{it} \tag{0.1}$$

where y_{it} is the reported survey earnings for individual i in period t, y_{it}^{*} is that individual's true lifetime earnings, e_{it} is the reporting error. Because e_{it} is potentially

[6] Although, it is possible that lifecycle bias, especially with parent income measured at higher ages could inflate the estimated IGE as well.

[7] This literature is well summarized by Bound, Brown, and Mathiowetz (2001).

[8] As the W-2 data is contained in a file called the Detailed Earnings Record (DER). This data is first available in the 1991 CPS.

related to lifetime income, either indirectly through lifecycle bias or directly through other forms of non-classical measurement error, we separate it into two terms, an i.i.d. error term v_{it} and a term, δy_{it}^*, that is related to lifetime income so that $e_{it} = \delta y_{it}^* + v_{it}$ and:

$$y_{it} = y_{it}^* + \delta y_{it}^* + v_{it}. \quad (0.2)$$

We make no additional assumptions on the relationship between incomes in each generation.[9]

When we run the regression of child income on parent income, as in the previous section, the true coefficient we would like to estimate is:

$$\beta = \frac{\text{cov}(y_{t-1}^*, y_t^*)}{\text{var}(y_{t-1}^*)}. \quad (0.3)$$

However, as we do not observe y_t^*, we have estimated:

$$\hat{\beta} = \frac{\text{cov}(y_{t-1}, y_t)}{\text{var}(y_{t-1})}. \quad (0.4)$$

Both the numerator and the denominator can be expanded by replacing y_t and y_{t-1} with equation (0.2) so that:

$$\hat{\beta} = \frac{(1+\delta)^2 \text{cov}(y_{t-1}^*, y_t^*)}{(1+\delta)^2 \text{var}(y_{t-1}^*) + \text{var}(v_{t-1})}. \quad (0.5)$$

If $\delta = 0$, as with classical measurement error, the numerator does not contribute to the bias. However, if $\delta < 0$, as is the case in the CPS-DER linked data, then there is downward bias in $\hat{\beta}$ from the numerator. With $\delta < 0$, the direction of the bias in the denominator depends on the relative magnitudes of the δ, $\text{var}(y_{t-1}^*)$, and $\text{var}(v_{t-1})$.

However, we are also interested in the relationship between child and grandparent status conditional on parent status. With both parent and grandparent income as independent variables, we can express the estimated $\hat{\beta}_P$ and $\hat{\beta}_G$, where P = parent and G = grandparent, as functions of the measurement error. We solve for $\hat{\beta}_P$ and $\hat{\beta}_G$ in Appendix B. In each case, the estimated coefficient is a complicated function of the multigenerational income variance-covariance matrix and the measurement error structure in each generation. The coefficient on the parent term is:

[9] We have used the assumption that the non-classical measurement error is constant over time (δ is not indexed by t). This is to reduce the number of terms in our equations for clarity. It can be relaxed with little change to the discussion. We also could include a lifecycle bias term directly by indexing δ by respondent age. Again, for notational simplicity, we do not.

$$\hat{\beta}_P = \frac{(1+\delta)^4[\text{var}(y^*_{t-2})\text{cov}(y^*_t, y^*_{t-1}) - \text{cov}(y^*_{t-1}, y^*_{t-2})\text{cov}(y^*_t, y^*_{t-2})] + (1+\delta)^2\text{var}(v_{t-2})\text{cov}(y^*_t, y^*_{t-1})}{[(1+\delta)^2\text{var}(y^*_{t-1}) + \text{var}(v_{t-1})][(1+\delta)^2\text{var}(y^*_{t-2}) + \text{var}(v_{t-2})] - (1+\delta)^4\text{cov}(y^*_{t-1}, y^*_{t-2})^2} \quad (0.6)$$

The biased grandparent coefficient is shown in equation (B.5).

Solving for the True Coefficients Numerically

We would like to solve for the unknown parameters in equations (0.6) and (B.5) to be able to recover unbiased multigenerational mobility coefficients. However, even with known measurement error structure and income variances, these equations do not have a clear algebraic solution for the earnings covariances of interest. Given the estimated earnings variances, the three unknown parameters are 1) the correlation between parent and child earnings (ρ_{12}), 2) the correlation between parent and grandparent earnings (ρ_{01}), and 3) the correlation between grandparent and child earnings conditional on parent earnings (ρ_{02}).

We observe the biased coefficients ($\hat{\beta}_O$)[10] from the set of possible intergenerational regressions and can estimate the error structure CPS-DER data and can estimate the true lifetime income variances from the two data sets. With the observed data, we can solve for the true income relationships numerically using the insight that for any possible "true" multigenerational income relationship and measurement error structure, we can estimate the biased coefficients we would observe. We simulate y^*_{it} for $t = 0,1,2$ and $i = 1,2,\dots,n$ from a known income variance-covariance matrix. Then we impose a given measurement error on the observed incomes to get y_{it} for each individual in each generation. Finally, we estimate the biased coefficients ($\hat{\beta}_S$) by running the multigenerational mobility regression on the simulated incomes measured with error.

From a set of possible "true" income relationships, we compare $\hat{\beta}_O$ observed in our data to the simulated $\hat{\beta}_S$. Our estimate of the true β coefficients is the one from the set of possible relationships where $\hat{\beta}_S$ is closest to $\hat{\beta}_O$. We select the closest $\hat{\beta}_S$ by minimizing the sum of squared errors between the two sets of coefficients.

To test this method, we conduct a Monte Carlo simulation with 1,000 replications. In each replication, we randomly select parameters for the income variance-covariance matrix and the error structure from a uniform distribution of reasonable values. The income parameters (with ranges in parenthesis) are 1) ρ_{12}: the correlation between

[10] $\hat{\beta}_O$ contains the five IGE coefficients from four regressions of earnings between: 1) parents on grandparents, 2) children on parents, 3) children on grandparents, and 4) children on parents and grandparents simultaneously.

parent and child income (0.3-0.7), 2) ρ_{01}: the correlation between parent and grandparent income (0.3-0.7), and 3) ρ_{02}: the correlation between child and grandparent income conditional on parent income (0-0.2). The measurement error structure contains two parameters for each generation: 1) the variance of the measurement error term relative to the variance of true lifetime income (0-0.8) and 2) the correlation between the measurement error and true lifetime income (-0.8-0). Our simulations cover cases where grandparent income is uncorrelated with child income after accounting for parent income ($\rho_{02} = 0$), where there is no measurement error (measurement error variance equals 0), and where measurement error is classical (correlation between the measurement and true lifetime income equals 0).

In each iteration of the simulation, we select possible true income relationships to minimize $SSE_\beta = \sum(\beta_{kO} - \beta_{kS})^2$ for $k = 1, \ldots, 5$ by doing a grid search over a plausible range of intergenerational income correlations $\rho_{12} = [0.2, 0.8]$, $\rho_{01} = [0.2, 0.8]$, and $\rho_{02} = [-0.2, 0.2]$ with 0.05 as the step size in each dimension. After finding the correlations with the minimum SSE_β in this coarse grid, we find the local minimum near that point using the Nelder-Mead simplex direct search algorithm.[11]

In the 1,000 simulations, the mean absolute deviation between the known and estimated correlations was 0.003 for ρ_{12}, 0.005 for ρ_{01}, and 0.007 for ρ_{02} given the mean values for each of 0.5, 0.5, and 0.1 respectively (as expected given the sampling distributions). The maximum deviation over all simulations for all correlations was 0.034. In other words, given the error structure and the biased observed coefficients, in every one of the 1,000 simulations, the estimates for all three variables were very close to the known true values.

6 Error Adjustment and Results

Our simulation results give us confidence to proceed using this method to correct for measurement error in our biased estimates from Section 4. The first step is to estimate the measurement error parameters from the 1991-2009 CPS ASEC data linked to W-2 wage earnings subsample of parents with children present in the household. We proxy for lifetime income by averaging administrative family wage earnings when the older parent is 35-50. We drop all families with imputed wage earnings for either parent from

[11] Using MATLAB's fminsearch function.

the analysis. These data cover the period directly after our parent observations and overlaps with much of our child observations.

The raw measurement error is the difference between the log of observed survey earnings and the log of lifetime earnings observed in the W2 data. We regress this difference on survey year and age and age squared of the older parent when surveyed to control for year-specific unobserved factors and the nonlinear relationship between age and earnings. In order to run the simulation, we need estimates of three parameters in each generation: 1) the variance of lifetime earnings, 2) the variance of the measurement error, and 3) the correlation between the measurement error and lifetime earnings. We estimate this separately for each generation by reweighting the CPS-DER sample to match the adult age distribution of that generation. This allows us to account for variation in lifecycle bias due to the different age profiles of each cohort.

First, we calculate the ratio of the variance in lifetime earnings to survey earnings in the CPS DER and multiply this ratio by the survey earnings variance to get the lifetime earnings variance. The observed survey earnings variance in our grandparent sample is 0.83 compared to 0.72 and 0.87 in the parent and child sample respectively.[12] Second, we calculate the ratio of measurement error variance to lifetime earnings variance. We multiply this by the estimated lifetime earnings variance to get the variance of the measurement error. Third, we directly estimate the correlation between the measurement error and lifetime earnings in the CPS-DER. The results are shown in Table 6.

With these estimated parameters, we can solve for the unknown multigenerational income covariances and recover the unbiased regression coefficients, shown in Table 7. From the regression of child earnings on parent and grandparent earnings, the biased coefficients were 0.160 for parents and 0.093 for grandparents. However, with our measurement error adjustment, the bias-corrected IGE coefficients are 0.326 for parents and 0.127 for grandparents. Measurement error biases the results down for parents and grandparents by 51% and 27% respectively. The other regressions with one independent variable (children on parents, children on grandparents, and parents on grandparents) are biased downward by over 50% as well.

For grandparents, equation (B.5) does not clearly indicate a direction of bias. However, our numerical simulation estimates indicate that the regression results bias downward the extent to which grandparents matter statistically and that grandparent

[12] This is consistent with the decline in inequality from 1940-1970 and its subsequent increase through 2013 (Piketty and Saez 2014).

earnings are more strongly associated with child earnings. Encouragingly, our bias-corrected two-generation earnings elasticity estimates are also consistent with larger literature on earnings mobility (Chetty et al. 2014, Lindahl et al. 2015).

However, both sets of input parameters to the numerical solution model (β_O and the measurement error structure) are estimated with uncertainty. To account for this, we calculate standard errors for our error-corrected estimates using a multi-step process. First, we estimate the full variance-covariance matrix for the five parameters in β_O (from four regressions) using a bootstrap with 1,000 replications. We do the same to estimate the variance-covariance matrix of the three measurement error input parameters with 100 bootstrap replications from the CPS-DER sample. These variance-covariance matrices are then used as the bootstrap sampling distributions to incorporate the uncertainty in our numerical solution input parameters. We then use another bootstrap to calculate the standard errors in the numerical solution model, where in each iteration, we take a random draw from the β_O and measurement error parameter distributions. The standard errors from 100 replications of this bootstrap are shown in parenthesis in Table 7.

To this point, we have assumed that the measurement error structure can be estimated from the 1991-2009 CPS-DER file. This assumption is most plausible for the child generation, which overlaps with much of the CPS-DER sample period. Even the parent generation is relatively close in time and from the same survey as the CPS-DER sample. However, for the grandparent generation, we have no contemporaneous validation data to assess the nature of the measurement error. Therefore, we also conduct two sensitivity analyses.

In the first, we focus on the grandparent generation. We hold the measurement error parameters fixed for the parent and child generations and allow the grandparent parameters to vary widely around the values used in our baseline estimate. In the sensitivity analysis, we varied measurement error to true lifetime earnings from -0.6 to 0 and the variance of the measurement error from 0.4 to 1 relative to the variance in lifetime earnings. Taking the observed regression coefficients from Table 7, Panel C, we numerically solve for the true mobility under each measurement error structure in a matrix containing the aforementioned ranges and steps of 0.05. Given the ranges and step sizes, this involves solving for true mobility parameters under 169 measurement error scenarios ([0.4,-0.6], [0.4,-0.55], …, [0.4,0], [0.45,-0.6], [0.45,0.55], …, [1,0]]). In each case, we also calculated the minimum sum of squared errors (SSE) between the

our observed coefficients from Table 7, Panel C and the simulated observed parameters under that error structure and estimated true mobility relationships.

In this sensitivity analysis, we are primarily interested in how varying the measurement error structure affects our estimates of the relationship between grandparent and child earnings, conditional on parent earnings. We are testing whether our conclusion that grandparents matter is sensitive to the choice of measurement error in the grandparent generation. Across the 169 scenarios, the estimated correlation conditional on parent earnings varies from a minimum of 0.071 to a maximum of 0.161. While this range around our baseline estimate of 0.127 is relatively wide, in all cases, the relationship between grandparent and child earnings conditional on parent earnings is large.

In addition, we can use SSEs to assess which error structure is most consistent with the data, although the results should be interpreted with caution given the stochastic nature of the simulation. If we assign Akaike Weights to each error structure tested based on the SSEs, one error structure gets nearly all of the weight. In that model, the measurement error variance relative to true earnings variance is 0.8 and the correlation between the measurement error and true earnings is -0.25.[13] Also, correlation between parent and child earnings is 0.344, the correlation between grandparent and parent earnings is 0.355, and the grandparent effect conditional on parent earnings is 0.119. These coefficients are not statistically significantly different from and almost identical in magnitude to our baseline estimates in all three cases.

In the second sensitivity analysis, we allow each measurement error parameter to vary in each generation. In the analysis, we use three values for each parameter (p) in each generation, where p is equal to the value in the baseline model. For a given parameter and a perturbation term ϵ, the three values in the sensitivity analysis are $p - 0.1$, p, and $p + 0.1$. We calculate the true mobility parameters given the observed regression coefficients under $3^6 = 729$ scenarios of parameter combinations. In only three of the 729 cases (0.4 percent), is the estimated true coefficient less than 0.05 and the average estimate is 0.12. From these two sensitivity analyses, we conclude that the positive coefficient of grandparent earnings conditional on parent earnings is robust to alternative measurement error specifications.

[13] The estimated ratio of measurement error to true earnings variance is almost identical to the parameter used in our baseline estimates (0.79 vs. 0.80). The correlation between measurement error and lifetime earnings is lower however at -0.25 compared to -0.36 in the baseline model.

7 Comparability with Clark's Estimates of Family Status Transmission

Even after correcting for measurement error, our mobility estimates suggest far greater social mobility than those calculated by Clark (2014), which range from 0.7 to 0.8. In Clark's framework, the following function describes the underlying persistent of family socio-economic status

$$y_t^* = \beta y_{t-1}^* + e_t \tag{7.1}$$

where y_{t-1}^* is a measure of the previous generation's status, y_t^* is a measure of the next generation's status, and y^* is normally distributed, mean zero, and has a constant population variance σ^2 (Clark 2014, pg. 293-294). Typically, y_t^* and y_{t-1}^* are not directly observed and economists use earnings or occupational status as proxy for measures of family status. Then the observed status measure, y, is a function of the true status measure, y^*, and an error term, v,

$$y_t = y_t^* + v_t. \tag{7.2}$$

Clark's work addresses the issue of unobserved socioeconomic status using information on family status that persists by surname to estimate the underlying true β. In our framework, we can algebraically back out β by making strong assumptions about the error terms and assuming the transmission of status across generations follows a first-order Markov, AR(1), process. Let $\hat{b}_1$ be the estimated coefficient from the following regression of the child's observed outcome on the parents' observed outcome

$$y_t = b_1 y_{t-1} + u_t \tag{7.3}$$

and $\hat{b}_2$ be the estimated coefficient from the regression of the child's obeserved outcome on the grandparents' observed outcome

$$y_t = b_2 y_{t-2} + u_t. \tag{7.4}$$

If we assume the transmission of status is an AR(1) process, then we can express y_t as

$$y_t = b y_{t-1} + u_t = b^2 y_{t-2} + b u_{t-1} + u_t = b^2 y_{t-2} + u^*$$

where $u^* = b u_{t-1} + u_t$. Then the expected values of $\hat{b}_1$ and $\hat{b}_2$ can be expressed as

$$E(\hat{b}_1) = \lambda\beta \tag{7.5}$$

and

$$E(\hat{b}_2) = \lambda\beta^2, \tag{7.6}$$

where v and u are serially and mutually uncorrelated and λ is equal to

$$\lambda = \frac{\sigma_v^2}{\sigma_v^2 + \sigma_u^2}. \tag{7.7}$$

Here, σ_v^2 is the variance of underlying social competence and σ_u^2 is the variance of the random component. Altogether, this implies $\hat{b}_1 \div \hat{b}_2 \approx \beta$. Using the earnings estimates from Table 5, we can estimate the "true" persistence of status across generations as $\beta \approx 0.116 \div 0.177 = 0.655$.

An estimated intergenerational elasticity of 0.655 is well within range of Clark's estimates. However, despite Clark's assumption that mobility across generations follows an AR(1) process in all societies, none of our empirical results using direct intergenerational links suggest that the transmission of status follows an AR(1) process. Clark would argue the positive, statistically significant coefficient on grandparents suggests the presence of significant results are due to classical measurement error. However, our error-corrected estimates on the grandfather actually *increase*, suggesting the measured autocorrelations do not decline at a geometric rate in spite of measurement error.

8 Conclusion

In this paper, we study multigenerational mobility using linked survey data from the 1940 Census, the CPS ASECs spanning 1973-1990, and the 2000 Census and 2001-2013 ACS. We find strong evidence that grandparents' status is associated with child status even after controlling for parent status. This is true for both education and earnings.

We also show that measurement error is present in our income and earnings data by studying validation data with linked survey and administrative earnings. This measurement error is non-classical and mean reverting, which is consistent with prior research on a variety of surveys. This leaves the direction of bias in our coefficients due to measurement error unclear. We use numerical simulations to find unbiased estimators of multigenerational mobility of earnings and find that grandparents matter in a way that is not driven by measurement error. On the contrary, there is a downward bias in the relationship between grandparent and child earnings conditional on parent earnings.

We show that conditional on parent status, nine years of grandparent education is associated with the same increase in child education as an additional year of parent

education. An additional 3% of grandparent earnings is associated with the same increase in child earnings as an additional 1% of parent earnings.

This contributes to a growing literature documenting that studying intergenerational mobility from parents to children understates the extent to which advantage persists across generations. However, this still leaves open the question of why. Do grandparents matter directly or because they are proxying for an excluded group characteristic? This is an important topic for future research.

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Table 1: Descriptive Statistics of Three-Generation Education Sample

	(1) Paternal Grandfathers mean/sd	(2) Paternal Grandmothers mean/sd	(3) Maternal Grandfathers mean/sd	(4) Maternal Grandmothers mean/sd	(5) Fathers mean/sd	(6) Mothers mean/sd	(7) Sons mean/sd	(8) Daughters mean/sd
Age	39.86	35.81	38.45	34.45	47.28	44.06	40.23	39.73
	(9.05)	(8.34)	(8.84)	(8.19)	(6.74)	(6.39)	(6.97)	(6.95)
Highest Grade	8.16	8.54	8.20	8.68	9.87	9.72	13.77	14.01
	(3.64)	(3.34)	(3.69)	(3.37)	(3.24)	(2.65)	(2.27)	(2.18)
Observations	9159	9367	7980	8239	11967	12852	6713	6312

Table 2: Descriptive Statistics of Three-Generation Earnings Sample

	(1) Paternal Grandfathers mean/sd	(2) Paternal Grandmothers mean/sd	(3) Maternal Grandfathers mean/sd	(4) Maternal Grandmothers mean/sd	(5) Fathers mean/sd	(6) Mothers mean/sd	(7) Sons mean/sd	(8) Daughters mean/sd
Age	39.25	35.28	37.97	34.13	47.01	43.99	40.14	39.80
	(8.94)	(8.14)	(8.73)	(8.11)	(6.65)	(6.34)	(6.92)	(6.91)
Highest Grade	8.46	8.73	8.51	8.91	10.22	9.91	13.99	14.19
	(3.58)	(3.27)	(3.64)	(3.29)	(3.08)	(2.53)	(2.12)	(2.05)
Total Personal earnings	1141.15	55.11	1150.22	48.89	23430.90	5877.01	57293.99	31010.34
	(946.53)	(236.43)	(965.89)	(229.84)	(18664.32)	(8520.53)	(74101.29)	(33043.77)
Observations	5659	5764	4974	5056	7174	7575	3935	3727

Table 3: Descriptive Statistics of Three-Generation Income Sample

	(1) Paternal Grandfathers mean/sd	(2) Paternal Grandmothers mean/sd	(3) Maternal Grandfathers mean/sd	(4) Maternal Grandmothers mean/sd	(5) Fathers mean/sd	(6) Mothers mean/sd	(7) Sons mean/sd	(8) Daughters mean/sd
Age	39.33	35.38	38.00	34.11	47.16	44.10	40.22	39.88
	(8.95)	(8.18)	(8.70)	(8.03)	(6.74)	(6.35)	(6.93)	(6.95)
Highest Grade	8.41	8.68	8.42	8.82	10.11	9.80	13.88	14.08
	(3.58)	(3.28)	(3.64)	(3.31)	(3.14)	(2.60)	(2.24)	(2.18)
Total Personal Income	1130.23	55.01	1132.45	48.59	22959.63	5771.85	57479.51	31910.07
	(943.34)	(233.83)	(954.58)	(226.36)	(18668.32)	(8481.25)	(82776.45)	(35356.87)
Observations	5971	6078	5376	5461	7596	8129	4244	3985

Table 4: Descriptive Statistics of the Third Generation

	Women, Education	Men, Education	Women, Earnings	Men, Earnings	Women, Income	Men, Income
Age	39.73	40.23	39.80	40.14	39.88	40.22
	(6.95)	(6.97)	(6.91)	(6.92)	(6.95)	(6.93)
White	0.92	0.94	0.94	0.95	0.93	0.95
	(0.26)	(0.24)	(0.24)	(0.22)	(0.25)	(0.23)
Black	0.05	0.04	0.04	0.03	0.05	0.03
	(0.22)	(0.20)	(0.21)	(0.17)	(0.22)	(0.18)
Hispanic	0.04	0.04	0.04	0.04	0.04	0.04
	(0.20)	(0.19)	(0.19)	(0.19)	(0.19)	(0.19)
2000	0.55	0.56	0.53	0.55	0.53	0.55
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
2001	0.02	0.02	0.02	0.02	0.02	0.02
	(0.13)	(0.12)	(0.14)	(0.12)	(0.14)	(0.12)
2002	0.01	0.02	0.01	0.02	0.01	0.02
	(0.11)	(0.13)	(0.11)	(0.13)	(0.11)	(0.12)
2003	0.02	0.01	0.02	0.01	0.02	0.01
	(0.13)	(0.12)	(0.13)	(0.12)	(0.14)	(0.12)
2004	0.01	0.02	0.01	0.02	0.01	0.02
	(0.11)	(0.13)	(0.11)	(0.14)	(0.11)	(0.14)
2005	0.05	0.05	0.06	0.05	0.06	0.05
	(0.23)	(0.22)	(0.23)	(0.22)	(0.23)	(0.22)
2006	0.04	0.04	0.04	0.05	0.04	0.05
	(0.20)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)
2007	0.04	0.04	0.04	0.05	0.04	0.05
	(0.20)	(0.21)	(0.20)	(0.22)	(0.20)	(0.22)
2008	0.05	0.05	0.06	0.05	0.05	0.04
	(0.22)	(0.21)	(0.23)	(0.21)	(0.23)	(0.21)
2009	0.04	0.04	0.05	0.05	0.05	0.05
	(0.21)	(0.21)	(0.22)	(0.21)	(0.22)	(0.21)
2010	0.04	0.04	0.04	0.04	0.04	0.04
	(0.19)	(0.19)	(0.20)	(0.19)	(0.21)	(0.19)
2011	0.04	0.03	0.04	0.03	0.04	0.03
	(0.19)	(0.18)	(0.18)	(0.18)	(0.19)	(0.18)
2012	0.04	0.04	0.04	0.04	0.04	0.04
	(0.20)	(0.20)	(0.20)	(0.21)	(0.20)	(0.21)
2013	0.04	0.04	0.04	0.03	0.04	0.03
	(0.19)	(0.19)	(0.19)	(0.17)	(0.20)	(0.18)
Household Income			85742.12	88139.25	83459.25	85410.64
			(88140.03)	(98602.86)	(87986.66)	(97258.81)
Personal Earnings			31010.34	57293.99		
			(33043.77)	(74101.29)		
Personal Income					31910.07	57479.51
					(35356.87)	(82776.45)
Highest Grade	14.01	13.77	14.19	13.99	14.08	13.88
	(2.18)	(2.27)	(2.05)	(2.12)	(2.18)	(2.24)
Observations	6312	6713	3727	3935	3985	4244

Table 5: Intergenerational Mobility Regression Results
A. Years of Schooling

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.411	0.385			0.351
		(0.007)	(0.008)			(0.004)
Grandparent	0.320		0.042	0.164	0.295	
	(0.006)		(0.006)	(0.006)	(0.003)	
Observations	13,006				37,072	41,740

B. Income Elasticity (Earnings for Grandparents)

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.275	0.250			0.239
		(0.014)	(0.014)			(0.005)
Grandparent	0.172		0.093	0.135	0.164	
	(0.009)		(0.011)	(0.011)	(0.004)	
Observations	8,216				27,634	40,725

C. Earnings Elasticity

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.177	0.160			0.146
		(0.012)	(0.013)			(0.005)
Grandparent	0.155		0.093	0.116	0.158	
	(0.010)		(0.011)	(0.011)	(0.005)	
Observations	7,649				26,708	37,294

This table shows the results of four regressions for intergenerational mobility for several measures of status: A. years of schooling (for the most educated parent), B. log of family income (with earnings for grandparents as income is not available), and C. log of family earnings. In each case, the status measure is averaged across all observed families in each generation. This includes up to four sets of observed grandparents, two sets of observed parents and the one observed child family. The regression models include: 1) parents on grandparents, 2) children on parents, 3) children on parents and grandparents, and 4) children on grandparents on the sample where all three generations are observed and 5) parents on grandparents and 6) children on parents where the link to the third generation is not required. Each regression includes survey year fixed effects and controls for age and age-squared of the older partner in each generation.

Table 6: Numerical Solution Parameters

Parameter	Dataset	Grandparents	Parents	Children
Intermediate Parameters				
Survey Earnings Variance	Main	0.83	0.72	0.87
Ratio of Variances: Lifetime to Single-Year Earnings	CPS-DER	0.95	0.97	0.95
Ratio of Variances: Measurement Error and Lifetime Earnings	CPS-DER	0.79	0.77	0.78
Final Parameters used in Simulation				
Estimated Lifetime Earnings Variance	Both	0.79	0.70	0.82
Estimated Measurement Error Variance	Both	0.48	0.42	0.50
Correlation of Measurement Error and Lifetime Earnings	CPS-DER	-0.36	-0.37	-0.35

This table shows the parameters used to solve for the bias-corrected estimates of intergenerational earnings mobility. In the numerical solution model, we need estimates in each generation for the variance of earnings, the variance of the measurement error, and the correlation between the measurement error and lifetime income. We use the simulation to solve for the unknown covariance structure between incomes in each generation. The survey earnings variance is calculated for each generation from the observed earnings in our main datasets used in the regressions. The ratio of the variances of 1) lifetime and single-year earnings and 2) measurement error to lifetime earnings are calculated from the CPS-DER data. The CPS-DER sample is reweighted to match the age distribution of the each generation to account for variation in lifecycle bias due to the different age profiles of each cohort. Lifetime earnings is calculated as the average earnings for the family when the older spouse is 35-50 years old in the CPS-DER sample. The estimated lifetime earnings variance is calculated by multiplying the survey earnings variance and the ratio of variances of the lifetime to single-year earnings. The estimated measurement error variance is calculated by multiplying the lifetime earnings variance times the ratio of variances of the measurement error and lifetime earnings. The correlation between the measurement error and lifetime earnings is calculated from the age-weighted CPS-DER sample in each generation.

Table 7: Intergenerational Earnings Elasticity Estimates Corrected for Measurement Error (With Percent Downward Bias Italicized)

	Parents	Children		
	(1)	(2)	(3)	(4)
Parent		0.372	0.326	
		(0.038)	(0.039)	
		52	*51*	
Grandparent	0.318		0.127	0.231
	(0.038)		(0.016)	(0.023)
	51		*27*	*50*

This table shows the estimated impact of measurement error on the multigenerational earnings elasticity coefficients. We numerically solve for the true intergenerational income elasticity given the observed biased coefficients and the parameters shown in Table 6. Standard errors were calculated as follows. First, the variance-covariance matrix of β_O was calculated by bootstrap with 1,000 replications. Next, the variance-covariance matrix for the numerical solution parameters were calculated by bootstrap with 100 replications from the CPS-DER. Finally, a bootstrap of the numerical solution was conducted with random draws from the β_O and input parameter distributions using the estimates as the means and variance-covariance matrices as the multivariate normal error distributions. The downward bias (in percent) is the percent difference between the error-corrected estimates and the coefficients in Table 5 Panel C. The estimated mobility difference between the two sets of generations (0.318 for grandparents to parents vs. 0.372 for parents to children) are entirely driven by changes in the variances as the estimated error-corrected correlation coefficients are 0.342 for both grandparents to parents (0.3415) and parents to children (0.3418).

Appendix Table 1: Intergenerational Mobility Regression Results for Male Lines
A. Years of Schooling

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.321	0.309			0.296
		(0.008)	(0.008)			(0.004)
Grandparent	0.346		0.026	0.132	0.351	
	(0.009)		(0.007)	(0.007)	(0.005)	
Observations	8,211				28,737	30,592

B. Income Elasticity (Earnings for Grandparents)

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.280	0.256			0.212
		(0.017)	(0.017)			(0.005)
Grandparent	0.177		0.072	0.117	0.167	
	(0.009)		(0.013)	(0.013)	(0.005)	
Observations	6,148				21,926	34,454

C. Earnings Elasticity

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.194	0.174			0.136
		(0.015)	(0.016)			(0.005)
Grandparent	0.175		0.076	0.116	0.162	
	(0.011)		(0.013)	(0.011)	(0.006)	
Observations	5,827				21,283	32,064

This table shows the results of four regressions for intergenerational mobility for several measures of status: A. years of schooling (for the most educated parent), B. log of family income (with earnings for grandparents as income is not available), and C. log of family earnings. In each case, the status measure is for families along the male lines in each generation. This includes the son's family, his father's family, and his paternal grandfather's family. The regression models include: 1) parents on grandparents, 2) children on parents, 3) children on parents and grandparents, and 4) children on grandparents on the sample where all three generations are observed and 5) parents on grandparents and 6) children on parents where the link to the third generation is not required. Each regression includes survey year fixed effects and controls for age and age-squared of the older partner in each generation.

Appendix Table 2: Intergenerational Mobility Regression Results for Female Lines
A. Years of Schooling

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.344	0.321			0.305
		(0.010)	(0.011)			(0.004)
Grandparent	0.280		0.035	0.125	0.274	
	(0.007)		(0.007)	(0.007)	(0.004)	
Observations	6,977				22,997	34,560

B. Income Elasticity (Earnings for Grandparents)

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.268	0.242			0.234
		(0.016)	(0.016)			(0.006)
Grandparent	0.180		0.099	0.42	0.168	
	(0.011)		(0.014)	(0.014)	(0.005)	
Observations	5,504				18,212	34,299

C. Earnings Elasticity

	Three Generation Sample				Two Generation Samples	
	Parents	Children			Parents	Children
	(1)	(2)	(3)	(4)	(5)	(6)
Parent		0.167	0.150			0.149
		(0.015)	(0.015)			(0.005)
Grandparent	0.160		0.094	0.117	0.161	
	(0.013)		(0.014)	(0.014)	(0.006)	
Observations	5,094				17,565	31,713

This table shows the results of four regressions for intergenerational mobility for several measures of status: A. years of schooling (for the most educated parent), B. log of family income (with earnings for grandparents as income is not available), and C. log of family earnings. In each case, the status measure is for families along the female lines in each generation. This includes the daughter's family, her mother's family, and her maternal grandmother's family. The regression models include: 1) parents on grandparents, 2) children on parents, 3) children on parents and grandparents, and 4) children on grandparents on the sample where all three generations are observed and 5) parents on grandparents and 6) children on parents where the link to the third generation is not required. Each regression includes survey year fixed effects and controls for age and age-squared of the older partner in each generation.

Appendix A

Appendix A: Data Appendix

Person Identification Validation System

The Census Bureau uses the Person Identification Validation System (PVS) to assign unique PIKs to person records to facilitate unduplication and record linkage. The PVS uses a probabilistic matching algorithm and processes data through modules, with each module blocking the data in different ways and comparing different fields to find matches (Felligi and Sunter, 1969). To assign the PIKs, PVS compare personally identifiable information of the Personal identifiers are compared to data in a reference file constructed from the Social Security Administration (SSA) Numident file and other federal-agency administrative data, using different combinations of Social Security Numbers (SSNs), full name, full date of birth, and address (Wagner and Layne, 2014). Records cascade through matching modules, and only those observations that did not receive a PIK pass from one module to the next. All reference file records are available for linkage in each module. Through this process, the PVS appends PIKs to census, survey, and administrative records data.

PVS follows the typical steps in record linkage: preprocessing, sorting into blocks, identifying potential matches, and resolving best matches. Person records are preprocessed to standardize the blocking and matching fields between the census file and the reference file, ensuring that similar variables align. Next, PVS sorts the input and reference files into "blocks" or "cuts" for comparison. Blocking creates reasonably sized search spaces to find candidate matches, which is important with large files (Michelson and Knoblock, 2006). The SSA Numident contains nearly 500 million SSNs and forms the bulk of the reference file used in the matching process. The reference file is enhanced using administrative records to obtain additional variables not in the Numident, such as place of residence. The reference file includes all transactions associated with each SSN.[14] Consequently, the reference file is large and it is technically infeasible to compare every Numident record to every census record simultaneously. PVS compares census and reference file records within several passes in each module. Each pass alters the comparison of characteristics specified by the data processing staff, using slight variations of the match fields, with subsequent passes permitting more fuzziness in the match. For instance, the first pass may require that first name, last name, and year of birth match exactly, but the next pass may permit more tolerance in year of birth.

Within each pass of a module, potential matches are assigned a total score depending on the similarity of the characteristics of the input records and reference file records. PVS

[14] The SSA records transactions as corrections or name changes made on a particular SSN's record. The average number of transactions per SSN is 2.1 (Harris, 2014).

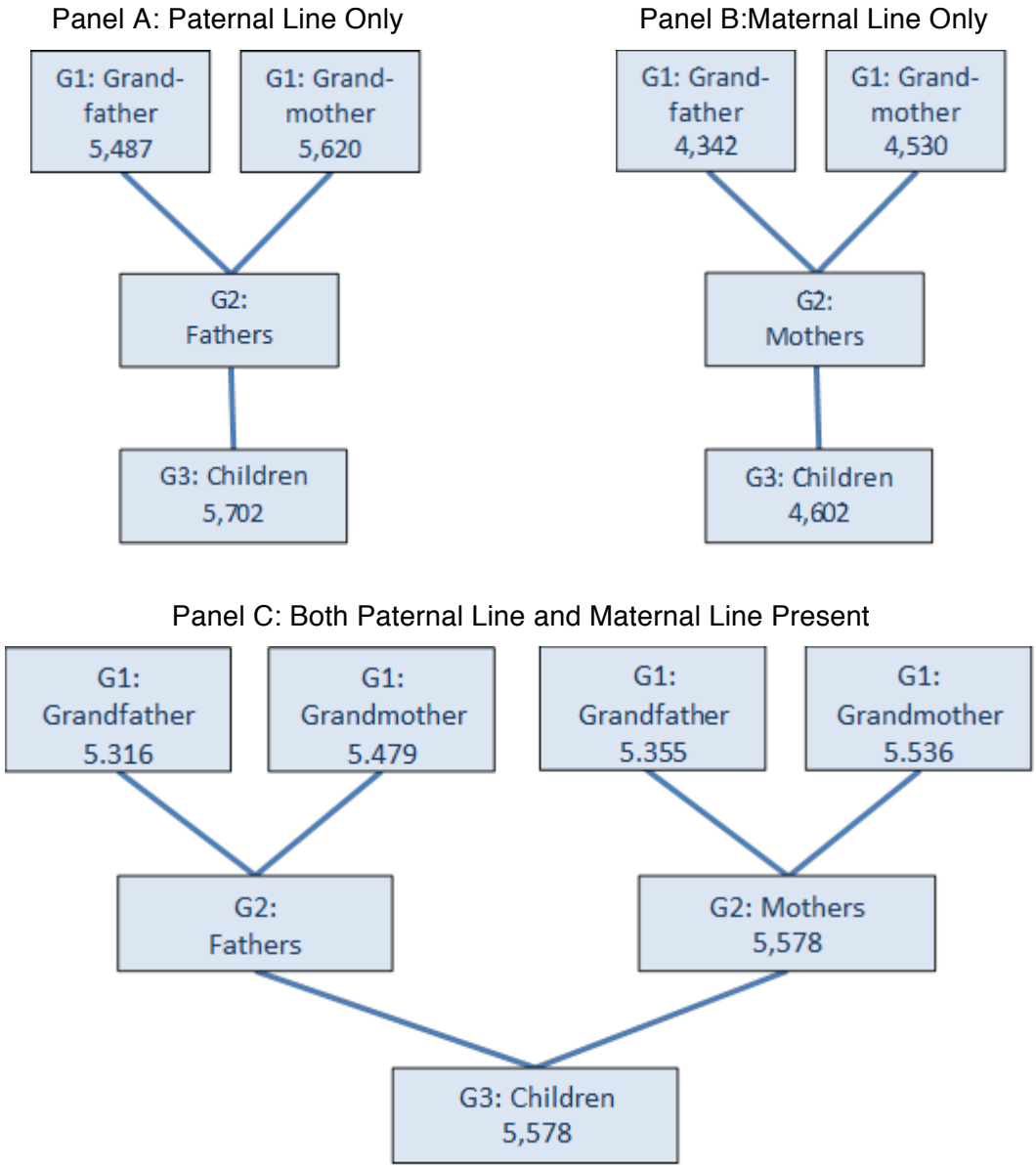
employs a string comparator program to measure Jaro-Winkler distances between first and last names in the input and reference files (Winkler, 1995). The PVS string comparator was developed by Winkler (1995) and measures the distance between two strings on a scale from 0 to 900, where a distance score of 0 is given if there is no similarity between two text strings and a score of 900 is given for an exact match. The cutoff value for the string distance is set to 750. These distances serve as a metric of how closely two names match, while allowing for some degree of misspelling. For numeric variables, such as year of birth, a maximum acceptable difference between the variable value in the input and reference record is programmable. This also allows for creation of an interval, or band, around year of birth to permit inexact matches. The total score is calculated as the sum of the agreement and disagreement weights attributed to each matching variable (Felligi and Sunter, 1969).

Potential matches are identified within each pass of a module, and only those with an overall score greater than a user-specified cutoff score are retained as potential matches. Input records that do not receive a match in the first pass move to the next pass. Once the input data has been processed through all passes of a module, potential matches are grouped into one file and sorted by person and by score.

The final step of a module evaluates the potential matches. The matches with the highest scores are processed using a decision rule to determine if a PIK will be assigned. If one potential match has a higher score than all the other potential matches for a particular input record, then the PIK associated with that reference record is assigned to that input observation. If there are multiple potential matches for a particular input observation with the same high score, then no PIK is assigned in that module. Records that fail to find a match in a module are passed along to the next module.

Building Multiple Generations

Appendix Figure 1: Three Generational Links



Education Recode

1940 'Higrade' IPUMS code	1940 Code Label	Years of Schooling	Recode to match 2000	Consistent Years of Schooling Across Three Generations
0-4	less than 1st grade	0	1	0
4	1st grade	1	2	2
5	2nd grade	2	2	2
6	3rd grade	3	2	2
7	4th grade	4	2	2
8	5th grade	5	3	5.5
9	6th grade	6	3	5.5
10	7th grade	7	4	7.5
11	8th grade	8	4	7.5
12	9th grade	9	5	9
13	10th grade	10	6	10
14	11th grade	11	7	11
15	12th grade	12	9	12
16	1 year college	13	11	13
17	2 year college	14	12	14
18	3 year college	15	12	14
19	4 year college	16	13	16
20	5+ year college	17	14	17

Appendix B

Derivation of Biased Coefficients from Regression of Child Status on Parent and Grandparent Status

We can solve for the biased coefficients from the regression of child status on parent and grandparent status in a *relatively* simple form. For the parent coefficient ($\hat{\beta}_P$), it is then equal to:

$$\hat{\beta}_P = \frac{E(y_{t-2}^2)E(y_t y_{t-1}) - E(y_{t-1}y_{t-2})E(y_t y_{t-2})}{E(y_{t-1}^2)E(y_{t-2}^2) - E(y_{t-1}y_{t-2})^2} \tag{B.1}$$

Substituting (0.2) to replace each y_t with $(1+\delta)y_t^* + v_t$, we get:

$$\hat{\beta}_P = \frac{E[(1+\delta)^2 y_{t-2}^* + v_{t-2}]E((1+\delta)^2 y_t^* y_{t-1}^*) - E((1+\delta)^2 y_{t-1}^* y_{t-2}^*)E((1+\delta)^2 y_t^* y_{t-2}^*)}{E[(1+\delta)^2 y_{t-1}^* + v_{t-1}]E[(1+\delta)^2 y_{t-2}^* + v_{t-2}] - E((1+\delta)^2 y_{t-1}^* y_{t-2}^*)^2} \tag{B.2}$$

If we normalize lifetime income in each period to have mean zero, and combine terms, we get:

$$\hat{\beta}_P = \frac{(1+\delta)^4[\mathrm{var}(y_{t-2}^*)\mathrm{cov}(y_t^*, y_{t-1}^*) - \mathrm{cov}(y_{t-1}^*, y_{t-2}^*)\mathrm{cov}(y_t^*, y_{t-2}^*)] + (1+\delta)^2\mathrm{var}(v_{t-2})\mathrm{cov}(y_t^*, y_{t-1}^*)}{[(1+\delta)^2\mathrm{var}(y_{t-1}^*) + \mathrm{var}(v_{t-1})][(1+\delta)^2\mathrm{var}(y_{t-2}^*) + \mathrm{var}(v_{t-2})] - (1+\delta)^4\mathrm{cov}(y_{t-1}^*, y_{t-2}^*)^2} \tag{B.3}$$

Without non-classical ($\delta = 0$) and classical ($\mathrm{var}(v_t) = 0$ in each generation) measurement error, this reduces to:

$$\hat{\beta}_P = \frac{\mathrm{var}(y_{t-2}^*)\mathrm{cov}(y_t^*, y_{t-1}^*) - \mathrm{cov}(y_{t-1}^*, y_{t-2}^*)\mathrm{cov}(y_t^*, y_{t-2}^*)}{\mathrm{var}(y_{t-1}^*)\mathrm{var}(y_{t-2}^*) - \mathrm{cov}(y_{t-1}^*, y_{t-2}^*)^2} \tag{B.4}$$

which is the unbiased parent coefficient. We could do the same for the grandparent coefficient ($\hat{\beta}_G$) to get:

$$\hat{\beta}_G = \frac{(1+\delta)^4[\mathrm{var}(y_{t-1}^*)\mathrm{cov}(y_t^*, y_{t-2}^*) - \mathrm{cov}(y_{t-1}^*, y_{t-2}^*)\mathrm{cov}(y_t^*, y_{t-1}^*)] + (1+\delta)^2\mathrm{var}(v_{t-1})\mathrm{cov}(y_t^*, y_{t-2}^*)}{[(1+\delta)^2\mathrm{var}(y_{t-1}^*) + \mathrm{var}(v_{t-1})][(1+\delta)^2\mathrm{var}(y_{t-2}^*) + \mathrm{var}(v_{t-2})] - (1+\delta)^4\mathrm{cov}(y_{t-1}^*, y_{t-2}^*)^2} \tag{B.5}$$

which also reduces to the unbiased coefficient if $\delta = 0$ and $\mathrm{var}(v_t) = 0$ in each generation.