Putting the Grandparents to Rest: False Positives in Multigenerational Mobility Research

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Abstract. Multigenerational mobility research is experiencing a renaissance. Such studies typically regress a person's status (education, social class, income) on that of their parents and grandparents. If the latter enters positively, it is common to infer that grandparents help furthering their grandchildren's position. But these associations are likely to be upwardly biased unless we can observe everything about parents that matters. Here we use newly harmonized population data on Swedish lineages to shed light on this issue. Studying cohorts born in the 1960s, their parents, and grandparents, we find substantial persistence in economic status across three generations, but little evidence of a direct 'grandparent effect'. In addition, the inclusion of well-measured and detailed grandparental indicators do not affect the parent-to-child estimates, indicating the robustness of the two-generation model. Coarsening our data to the quality of typical survey datasets, we then show how an impressive range of spurious results can be found, with grandparent "effects" in the literature likely to be inflated by a factor of two to five. We discuss implications of these findings.

1 Introduction

Studies of the intergenerational transmission of economic status have a long history in the social sciences (e.g., Atkinson 1981; Sewell and Hauser 1975) and are of wide interest as they pertain to the broader societal ideal of equality of opportunity (e.g., Breen and Jonsson 2005; Erikson and Goldthorpe 1992). Such research confirms that in all known societies, the importance of having the right parents is substantial: despite quite widespread intergenerational mobility, access to higher social positions and incomes is strongly related to parental characteristics.

Recent scholarship has drawn attention to limitations of the unidimensional models that permeate much of this literature. The standard one-parent one-offspring approach – typically using fathers' social class, income, or socioeconomic status to predict the same outcome among their children – risks underestimating long-run inequality transmission for at least three reasons: it neglects the influence of mothers (Beller 2009); of extended family members (Pfeffer 2014); and multiple stratifying dimensions (Mood 2017).

Recently, criticism has been mounted particularly against the two-generation approach, where studies focus on the parent-to-child associations. Following Mare's (2011) plea for multigenerational research, a growing number of studies consider grandparents' role in status transmission. While results have been mixed, some of these studies report that incorporating prior generations into models of status attainment improves predictions (e.g., Chan and Boliver 2013; Hällsten and Pfeffer 2017; Jaeger 2012; Zeng and Xie 2014). This has been seen as evidence that social mobility deviates from a first-order Markov chain, where each generation impacts exclusively on its children, and influences later generations only through them. If so, not only does the two-generation model overestimate the true rate of long-run social mobility; it also misrepresents the process behind it. Thus, for scholars of social stratification, a lot is at stake. Yet, one largely overlooked aspect of existing three-generation studies is that they share much the same limitations as the earlier two-generation research. In particular, if mothers or multiple dimensions of status are ignored, the influence of grandparents will generally be overstated and may simply reflect omitted variables in the parental generation.

A comprehensive assessment of what grandparents bring therefore requires expanding the model along more than one dimension. That is our first goal here. To do so, we draw on Swedish register data which allow a uniquely comprehensive view on the persistence of advantage. We study the interlocking dimensions of income, education, social class, and wealth; consider both mothers and fathers; and include information on all four grandparents. In addition, our data-set is big enough (n≈730.000) to estimate grandparent associations with great precision. Our main focus is the long-run persistence in income, treating other dimensions mainly as control variables in the parent generation. This focus is for illustrative purposes, and we present alternative analyses throughout and in supplementary materials. Income is, however, conceptually attractive: as a relatively final marker of success, it subsumes the cumulative impact of both education and occupation (Mood 2017). It also lets us cover extended family influence – through social networks, for example – in the labor market (cf. Knigge 2016), and is more normally distributed than wealth (Killewald, Pfeffer, and Schachner 2017).

Our first analysis estimates just how much of the gross grandparental estimate that remains after control for a large set of well measured parental variables. We then address heterogeneity in the contribution of grandparents – especially, stronger persistence at the top and bottom, a question that our population data are particularly well suited to answer.

The second goal of our study is to relate our results to the growing number of studies on grandparent "effects", by introducing a novel analysis of the importance of model specification in estimating such associations. We test how conclusions might change when information is sparser or discretion is used in model selection. This exercise is important given that most previous studies use survey data, with a limited set of variables available, presumably measured with considerable error.

Throughout our analyses, we find substantial (unconditional) three-generation associations. However, these turn out to be almost entirely mediated by socioeconomic correlates in the parental generation, including income but also education (level and field of study), social class, occupation, and wealth. Searching exhaustively for heterogeneity in the contribution of grandparents does little to alter this picture: it is as insubstantial when parents have high incomes (or education, or class position) as when they have low. However, once we allow for less stringent controls, typical for most studies, we are able to generate an impressive range of spurious results – a conservative estimate is that common grandparent "effects" in the literature are inflated by a factor of two to five.

Ultimately, our data pertain to a particular time and place and cannot tell us whether grandparents matter(ed) over and above what is channeled through their children in contexts other than that studied here. Nevertheless, our results cast doubt on the potential of standard designs to distinguish such influence from model artefacts. One conclusion that arises from the wide variety of results under different models, is that the common practice of controlling for a limited number of observed parental characteristics – often measured coarsely and with error – cannot tell us whether grandparents exert a direct influence. Given the wealth of data becoming available for multigenerational research, work in this vein will continue to flourish and there are several promising avenues of research. However, progress in understanding the mechanisms by which one generation's resources and characteristics trickle down to subsequent generations is conditional on greater methodological care than has been taken so far. In our view, greater attention to theoretically relevant and well identified two-generation models is more likely to resolve these issues than continuing the search for 'grandparent effects' along the lines of much recent research.

2 Background and previous literature

Multigenerational status transmission has been a concern for stratification scholars throughout the field's past (Mukherjee 1954; Pohl and Soleilhavoup 1982; Svalastoga 1959; Warren and Hauser 1997). But with increasing data availability, the number of studies has grown almost exponentially in recent years (Anderson et al. 2018). Most of this research is couched in terms of the "Markovian" question (Hodge 1966): do grandparents affect their grandchildren's educational, occupational, or income attainment directly or is all influence mediated through parents? A wealth of social, economic and psychological theoretical mechanisms make this a question of substantive interest. The fact that we live longer and healthier means that grandparents are around to provide cultural, social, and economic resources until old age (Mare 2011; Pilkauskas and Cross 2018). In some cultures, grandparents also co-reside with grandchildren, which may reinforce socialisation effects (Zeng and Xie 2014). Many social institutions furthermore distribute positions and rewards based on family connections that can outlast a single life. Legacy admissions to U.S. universities are one such example, as is the putative value of having a good name for access to jobs in sectors like finance, law, or politics (Knigge 2016; Mare 2011). Tax on inheritance may, as used to be the case in Sweden, make it profitable to give money to grandchildren rather than letting the children inherit it.

Despite this variety of theoretical reasons for expecting a 'grandparent effect', the extant literature shows a mixture of results (Anderson et al. 2017). Furthermore, partly because of different analytical strategies, authors even come to different conclusions based on similar results or sometimes the same data. For example, Warren and Hauser's (1997: 561) analysis of the Wisconsin Longitudinal Study found that "schooling, occupational status, and income of grandparents have few significant effects on the educational attainment or occupational status of their grandchildren when parents' characteristics are controlled". Using the same data, Jaeger (2012: 903) concluded that "the total effect of family background on educational success originates in the immediate family, the extended family, and in interactions between these two family environments". Similarly, Erola and Moisio (2006: 169) reported that grandparents add "very little explanatory power to the analysis of social mobility", only to be rebuked by Chan and Boliver (2014: 13) who read the same estimates as "not only statistically significant, but … also of substantive importance".

The evidence from Swedish data is no more consistent. Some previous studies report "grandparent effects", although not necessarily for all dimensions (e.g., Lindahl et al. 2015; Modin, Erikson and Vågerö 2013; Modin and Fritzell 2009; Dribe and Helgertz 2016), whereas others find no or very small such effects (Stuhler 2014; Adermon, Lindahl, and Palme 2016; Hällsten and Pfeffer 2017). Methods, data sets, and model specifications differ among these studies, so different findings are to some extent to be expected, but it is noticeable that studies reporting the weakest associations are based on recent register data.

Our data span cohorts born in the 1960s and early 1970s, their parents, and grandparents. Our index generations, the children, grew up at a time when Sweden was an exceptionally equal society with a comprehensive welfare state, which may reduce the role both of parents and grandparents. On the other hand, income inequality was much greater in the grandparental generation than in the parental, so it is also possible that more privileged grandparents had greater opportunity to make a difference for grandchildren.

3 Problem and analytical approach

The standard practice in intergenerational research has been to study associations in single parental resource indicators (class, income, etc.) across two generations and estimate a stylized model such as:

$$y_C = \bar{y} + \beta y_P + e, \qquad \text{Equation 1}$$

where y_C is the child's observed outcome, y_P is a summary measure of social origin measured in the same way as y_C, and e is a stochastic error term encompassing idiosyncratic characteristics, omitted variables, and measurement error. Here and throughout, we use P and C to index parent and child generations – and later, GP to refer to the grandparent generation. The main interest in this model is in β as an omnibus measure of the persistence of advantage across generations; the larger β, the less intergenerational mobility. This parameter cannot be interpreted as causal, in the sense that experimentally manipulating y_P – for example, by raising parents' education, or redistributing income and wealth – would be expected to change y_C by any particular amount. Instead, β reflects the effect of a large number of parental characteristics, including genes and a combination of cultural, social, economic and other resources (and to some extent similarities across generations in structural locations, such as area of residence or ethnic group). Nevertheless, we care about β because how strongly status is transmitted is an important question independently of the mechanisms by which transmission comes about. Now consider the model that incorporates grandparents, and which is the typical one in the research tradition we engage with:

$$y_C = \bar{y} + \beta_P y_P + \beta_{GP} y_{GP} + u. \qquad \text{Equation 2}$$

Here, the reports of a positive value for β_{GP} is taken as evidence of a "grandparent effect". However, unlike β in Eq 1, β_{GP} is a conditional effect and its identification depends heavily on y_P, that is, how we define parental characteristics and how well we measure them. In general, the less precisely measured the parental model, the more factors are left in the residual to be picked up by the *GP* coefficient in the model. To interpret β_{GP} as a measure of *GP* influence therefore requires a leap of faith. Indeed, given how much parents supply their children with – all of their genetic material, for example, and nearly all primary socialization – it would seem safer to assume that y_{GP} proxies for unobserved characteristics of parents rather than those of grandparents.

While the potential of an inflated positive β_{GP} due to unmeasured parental characteristics is rather intuitive, there is a less intuitive mechanism that can, at least in theory, operate in the other direction and suppress the positive $\beta_{GP.}$ The logic of this mechanism is a case of the so-called Berkson's paradox, or collider bias (Pearl 2001, Breen 2018), which results from the creation of a 'spurious' correlation between two variables when conditioning on a third to which both are related. Solon (2014) gave an intuitive interpretation of this issue in multigenerational models: Observations with a given y_P but different y_{GP} are not fully comparable, because those who have arrived at y_P in a process of downward mobility may differ in unobservables from those who arrived at y_P in a process of upward mobility, or a process of intergenerational persistence. Parents who have been downwardly mobile may be so because they have some personal characteristic that is negative for their socioeconomic success (and vice versa for upwardly mobile parents), and if this disadvantageous characteristic is transmitted to children the negative effect pertaining to parents will be picked up by β_{GP}. However, it is also possible that parents' downward mobility may reflect unobserved negative characteristics at the *grandparental* level, in which case β_{GP} is not biased as an estimate of effects of grandparental resources, broadly conceived.

Caught between bias due to confounding and the risk of collider bias it appears that the former is, on the whole, more important to guard against (Greenland 2003; Ding and Miratrix 2014). Our own calculations, shown in Appendix C [not in this version], also suggest that this type of bias is likely to be of little importance for our estimates. We are more concerned about underestimating β_{GP} due to limited data on their resources, so we have taken pains to assure as good coverage as possible in these respects. We have also been able to include very similar, extensive indicators as for parents (income, wealth, education, and occupation).

In this study, we ask what happens to (our well measured) β_{GP} when we include an extensive set of parental characteristics, y_P, in our model, and we subsequently vary the quality of this parental data. This allows us to shed new light on what β_{GP} may reflect. To build intuition, we make use of nonparametric visualizations throughout. We first show that any direct *GP* influence in our data is small. Thereafter we go on to coarsen the quality of our data to mimic common survey datasets. We vary not only the parent attributes observed but also the level of detail (e.g., interval, categorical, or binary measurements) and simulate errors of observation. While the received wisdom about mismeasurement is that it biases associations downwards, in the multivariate case this is not true (Bohrnstedt and Carter 1971; Shear and Zumbo 2013). Indeed, in each alternative scenario we consider, the β_{GP} we find is larger than our best estimate – sometimes substantially so.

4 Data and definitions

Our data are compiled from Swedish population registers and Censuses covering the entire Swedish population aged up to 75. Register data have many well-known advantages, such as almost no non-response, very large n:s and thereby high precision in estimates, as well as high reliability in measures – education, for example, is almost entirely constructed from school records with very high coverage, and incomes are gathered from tax records. For intergenerational models, we can add another specific advantage over survey data, namely that reports on children, parents, and grandparents are collected (or, rather, assembled) for each generation independently. This means that we can rule out measurement errors (that may even be of a systematic kind) stemming from one generation reporting on the other(s),

We focus on Swedish-born men and women (the third generation, *C*) born in 1965-1972. Each individual is linked to biological and any adoptive parents and grandparents using a multigenerational identifier. The matching, done by Statistics Sweden, is entirely accurate, but age limits and mortality in the data means that we cannot match all grandparents. In the 1965 cohort we cover 46% of paternal grandfathers and 73% of maternal grandmothers, but coverage increases over cohorts. In the 1972 cohort we cover 69% of paternal grandfathers and 89% of maternal grandmothers. In both generations, adoptive parents are given priority over biological parents if both exist in the data, so a child can have only one set of parents. In the case of adoptive parents, their parents are also defined as grandparents. As is the case in all three-generation models, we must exclude index persons whose parents were not born in Sweden, or cases where all grandparents were born abroad. We retain all persons who can be linked to at least one parent and at least one grandparent.

The outcome variable in all analyses is child earnings at ages 35-40, defined as the average yearly income from employment and self-employment, and earnings-related benefits (e.g., sickness or parenting benefits). Earnings that are more than four standard deviations higher than the average in a given year (around 0.3 percent) are top coded. The variable is missing-coded if earnings are missing in more than two years in the 35-40 age span, and the final variable is z-standardized within each cohort and for men and women separately. We focus on individual earnings because these clearly pertain to the person in question rather than to any partner, which makes the results transparent. A focus on disposable income on the child side would require consideration of the role of partnership formation, something which is beyond the scope of this article. However, in appendix we report regressions using disposable income in the child generation. Likewise, we report results using children's education and occupational prestige (SIOPS) as outcomes [Not in this version].

The main predictor in the *GP* generation is *income*, constructed as follows: For each year in 1968 to 1972, we define the disposable family income of each of the four grandparents as all grandparental and partner incomes (from work and benefits), but not incomes of any children living in the household, and subtract taxes. Note that although the variable measures family income, it is constructed for each individual, so that if grandparents cohabit they have the same value, but if they do not the variable refers to different families (the correlation between the grandmother and grandfather disposable income is 0.87). Zero and negative incomes (0.6 percent of the sample) are missing-coded, and incomes above four standard deviations are top-coded. We then standardize this variable (mean 0, standard deviation 1) within groups based on grandparent's birth cohort and sex. We take the average over the years 1968 to 1972 for each grandparent, and repeat this procedure across all grandparents. If any grandparent is missing, the non-missing ones are used. By observing all grandparent incomes during the same time period we eliminate differences due to changes in taxation and available benefits, and by standardizing within cohort we neutralize the impact of grandparental age differences.

Our main interest is in the net association between *GP income* and *C earnings*, varying the range of parental attributes that the model conditions on. As control variables in the parent generation, we use the income, education (level and field of study), social class, occupation, and wealth of both parents as described in detail below.

Mother earnings and *father earnings* are defined as the average of annual earnings, including earnings-related benefits, at ages 44-55. Before taking the average, annual earnings are z-standardized by income year, cohort, and sex, and earnings above four standard deviations are top-coded. We construct a *parental earnings* variable by taking the average of mother and father earnings at ages 44-55.

Parental social class is coded from records about occupation in the Censuses 1960, 1970, 1975, 1980, 1985 and 1990. Priority is given to the occupations that the parents held when the child was aged 10-15. All censuses are however used in order to get as many non-missing records as possible. Class mobility among adults is low (Jonsson, 2001) so this procedure is unlikely to be problematic. Occupations are coded into 82 microclasses (ref; see table in appendix), and seven EGP classes: I=Upper middle class (professionals, higher administrative, executives), II=Middle class (semi-professionals [e.g., nurses], mid-level administrative, low-level managers), III=Routine non-manual (clerks, secretaries, office-workers), IV=self-employed, V=farmers, VI=skilled manual workers, VII=unskilled manual workers.

Parental education is included for each of the parents separately, measuring the highest completed level of education (ISCED, 3 digits) according to the education register. We use the most recent information available (later information is more reliable due to revisions of the educational register).

Parental wealth is measured for each parent as the average net worth of the five "best" years during 1968 and 1989, i.e., the years with the highest registered taxable wealth. The average of the mother's and father's wealth is then top-coded at four standard deviations above the mean within each cohort, and standardized per cohort. We also tested wealth defined as the average over the whole period or for two sub-periods with very different taxation limits (1968-1977; 1978-1989), and as categories, but the chosen definition had the strongest association to child earnings.

In additional analyses, we also include *grandparent social class* (EGP) and *wealth*. Both are measured identically to that of parents, class in the Censuses of 1960, 1970, and 1975, and *GP* wealth in 1968 through 1989. *Grandparent education* is measured with a slightly reduced, 5-category, variable due to the smaller variation in education in the older cohorts. In Appendix analyses, c*hild occupation* is measured as SIOPS, with priority given to the occupation held at ages 38-42, *child education* is measured in years as estimated from educational level and field, and *child family disposable* income is measured as the average across ages 35 to 40, excluding 0 incomes and topcoding at four standard deviations.

As control variables, all analyses also include child cohort dummies, and the age of parents and grandparents together with its square term. *Parents' age* is measured as the average over both parents at the child's year of birth (on average, fathers are three years older than mothers, the gap decreasing from 3.3 to 2.7 years from cohort 1965 to 1972). Extreme values (normally due to adoptions) are missing-coded. *GP age* is measured in 1970 as an average of the age of all grandparents recorded in the data.

The number of valid observations is displayed in Table 1 and ranges from 87,977 to 95,574 per cohort, resulting in an N of 733,913 in total (51% men). Appendix Table 1 assesses robustness of results to differential selection of grandparents by running the analyses in section 5 for each cohort separately.

--TABLE 1 ABOUT HERE--

5 Baseline results

A first look at intergenerational associations is provided in Figure 1, which plots expected child earnings (in standard deviations) by gender at each percentile of parent and *GP* income. For the *GP*C* association, we distinguish between the unconditional expectation and that after conditioning on parent attributes: earnings, occupation, and wealth. For comparison we superimpose the least-squares lines of best linear fit for each association. The main results from the corresponding regressions are provided in Table 2 (upper half). All income and earnings variables are standardized so their coefficients correspond to correlations.

--FIG 1 ABOUT HERE—

--TABLE 2 ABOUT HERE--

Bivariate associations

Our regression of child earnings on parent income (Table 2) yields a correlation of 0.28 (men, 0.29; women, 0.27), broadly in line with previous two-generation research for Sweden, and implying that between a fourth and a third of economic differences are transmitted from parent to child. While not shown in Figure 1, the *GP-P* association is similar at 0.35. If income transmission was a unidimensional Markov process – independent of class, education, or wealth – we would expect the bivariate association GP*C to be a mere 0.08 (0.28^2). What we actually observe is a much stronger GP-C association, which remains roughly half the size of the *P-C* association (men, 0.16; women, 0.13). When relating this ratio to previous studies on Swedish data, it is close to that found by Lindahl et al. (2015) who report a *GP-C* income elasticity of 0.18 compared to a two-generation estimate of 0.30–0.36 depending on whether the *P-C* or *GP-P* association is studied. A similar ratio is also reported by Adermon et al. (2016) and by Adermon et al. (2018) for wealth transmission across three generations.

Introducing parental controls

Our next question is what happens to the *GP-C* association once we condition on parent attributes, and the answer is that it shrinks dramatically. When controlling for parental earnings, the GP coefficient is reduced to 0.07 (men) and 0.05 (women). However, controlling only for parents' income is an insufficient representation of what parents bring to the dyadic relation with their children.Two-generation models typically acknowledge, beside economic factors, the importance of educational, social and cultural resources, as well as the strong inclination of children to follow in their parents footsteps in terms of occupations and social class attainment (e.g., Sewell and Hauser 1975, Erikson and Goldthorpe 1992; Jonsson et al. 2009). In line with this, the last set of estimates in Figure 1 show the *GP*C* association after controlling for parents' income, education, class, occupation, and wealth – predictors that all have net positive associations with child incomes (Mood 2017). Once we consider this multidimensional nature of the parent-child relation, the remaining *GP*C* associations all but disappear: they are reduced to 0.03 for men and 0.01 for women, estimates that because of the large sample size are estimated with very high precision. Keeping in mind that several relevant attributes of parents remain unobserved – genetic endowment, cultural values, parenting styles, etc. – we interpret the remaining *GP*C* association as consistent with a null effect. A potential collider bias is unlikely to be large enough to change this conclusion (see Appendix C – not in this version).

Another implication of our multidimensional perspective is that not only will parent and *GP* income independently predict child earnings, but so will parent and *GP* social class – or any other stratifying dimension. Figure 2 illustrates this, again using child earnings as the outcome but with class as the status indicator in prior generations. Like before, we see a sizeable association over three generations, but little evidence of a net *GP* "effect" once differences among parents are taken into account.

--FIG 2 ABOUT HERE--

Testing for heterogeneity

A common argument in the literature is that *GP* associations that are small on average may nevertheless hide pockets of inequality where a prominent grandparent matters more (Mare 2011; Pfeffer 2014). There is a strong theoretical argument for rich grandparents compensating their grandchildren when parents lack resources (e.g., Bengtson 2001) and some previous support for this contention (e,g., Braun and Stuhler 2017). On the other hand, in two-generation models economic advantages tend to be amplified at the higher end of parental resources (Bratsberg et al. 2007, Björklund, Roine and Waldenström 2012). Do grandparents' resources operate with greater force at the extremes? This hypothesis is difficult to test with only a sample of the population, as people with extreme incomes are unlikely to be observed, but our large-scale population data are ideal to study this question. Figure 3 shows the conditional *GP*C* income association at four points in the distribution of parent income: the top, bottom, and middle two deciles. If there were multigenerational persistence at the extremes, this would show up as a premium for high *GP* income in the bottom parent decile (the compensation hypothesis), or for high *GP* income in the top parent decile (the amplification hypothesis). However, looking at the evidence in our data, neither of these hypotheses find any support: the largely null *GP*C* association holds throughout the distribution. As a further test of heterogeneity, we also fitted models varying *GP*C* associations across microclasses in the parental generation. [Not in this version].

--FIG 3 ABOUT HERE--

Expanding the grandparent model

One objection to our approach is that by focusing on one *GP* characteristic at a time while including the full range of parent controls, we have unfairly tilted the playing field in favor of parental associations. To test a full(er) set of *GP* characteristics, we estimate a model that includes all four (when available) grandparents' income, education, social class, and wealth (before and after controlling for parental variables in their entire dimensionality). Table 3 shows the R-squared from these regressions.

--TABLE 3 ABOUT HERE--

The contribution of the full set of *GP* indicators together in explaining adult grandchildren's income amounts to a fifth or a tenth of the percent of the total variance – that is, virtually nothing. In the extensive set of coefficients generated by the underlying models, a few *GP* estimates are significant (which would necessarily happen by chance, in combination with the efficiency created by the large n), but none are of any substantive size (and some estimates go in the 'wrong' direction, possibly because of collinearity). Thus, we conclude that even taking a multitude of grandparental resources into account, with appropriate controls at the parental level, the *GP→P→C* associations form an almost perfect Markov chain.

6 What do standard models miss?

As we have noted, results in the previous literature are mixed. Anderson et al. (2018) review 69 multigenerational analyses of educational attainment, of which slightly more than half report a statistically significant *GP* association net of observed parental characteristics. This ambiguity could reflect genuine contextual variation in grandparents' role across countries or times, or idiosyncratic differences between studies and datasets, including sample size, variable selection, reliability, and detail. The unique nature of our data means we cannot extend the approach taken here to other populations and datasets. However, we can turn the question on its head by asking: what would the more parsimonious models common in this literature make of our data? Keeping the population and institutional setting constant in this way, we learn precisely what influence data and model selection have on the results.

--TABLE 4 ABOUT HERE--

For some context, Table 4 reports the results of a literature search using the keywords “multigenerational”, “three-generation”, and “grandparents” to find relevant articles published since 2010 in three prominent sociology journals: *American Sociological Review, American Journal of Sociology,* and *Demography*. This is not an exhaustive survey of the field, but given that the journals are top tier we would expect these studies to represent the very research frontier. While Table 2 reveals quite wide variation in both data and approach, all studies attempt to estimate a direct grandparent effect by conditioning on parents in one way or another.[1]

Most studies include a range of variables to capture parent status, but quality and detail vary. To understand how limitations like these may shape results in standard analyses, we manipulate our data (1) by varying the detail (e.g., interval, rank, categorical, or binary variables), (2) by introducing measurement error, (3) by applying dominance coding to household status (typically, neglecting mothers), and (4) by including a subset of income, class, and education, or all three. To keep this analysis tractable, we have chosen to exclude wealth which adds the least explanatory power of all status dimensions.

(1) Level of detail. One important difference between our study and others is the detail of available data. Income is notoriously hard to measure, but we have access to full income histories from administrative records. Table 4 shows that even variables like education or occupation often appear in reduced (metric or coarse) form where much of the relevant variation may be lost. We identify three levels of detail for parental controls. First, we use the most detailed measures available to us: for income, this means a metric (untransformed) variable covering several years of income; for education a large set of unique combinations of level and field; for occupation a 83-category microclass scheme. We regress *C* income on each variable and use the fitted values as predictors in a multivariate analysis (cf. Hällsten and Pfeffer 2017).

[1] Some of them use nonstandard approaches like inverse probability weighting (IPW) to achieve that aim; we do not dwell on this distinction here because the fundamental assumption of selection on observables remains the same.

Secondly, we convert each measure to percentile ranks. This practice, used by Hällsten and Pfeffer (2017), was originally devised to deal with truncation and life-cycle bias in U.S. tax data (Chetty et al. 2014), but has also been shown to be less sensitive to income volatility and therefore an attractive alternative when having only one observation on the parental side (Gregg et al. 2017; Nybom and Stuhler 2017). These problems are less of a concern in the more comprehensive Swedish registers and it is unknown how this specification performs for (parental) control variables in a three-generation model as opposed to parental predictors in a two-generation model.[2] Third, we apply a categorical coding with 'medium granularity', at a detail used in many studies: quintiles for income, 5-category ISCED levels for education, and 7-category EGP class.

(2) Measurement error is ubiquitous in survey data. One reason is that respondents are fallible and may misreport. Another is the use of proxy variables like home ownership for wealth, or annual for lifetime income (e.g., Chan and Boliver 2013). Variables like these can be useful for understanding the direction of main effects, but cannot serve as control variables for the constructs they are meant to capture (Currie 2009). We generate measurement error as a weighted average of the true value (i.e., the observed value in our data) and a randomly chosen observation in the dataset. In choosing the error variance, we are guided by existing validation studies. Scandinavian register data have been used by Bingley and Martinello (2014, 2017) to assess the amount of misreporting in income and education in the Survey of Health, Ageing, and Retirement in Europe (SHARE), a dataset that Sheppard and Monden (2018) use for multigenerational analyses. Occupation variables are assessed in earlier generations of status attainment research (e.g., Bielby, Hauser, and Featherman 1977, Breen and Jonsson 1997). Taken together, these studies suggest that common survey variables may consist of up to xx% noise. Consequently, we assign the true value a weight of 0.xx in these analyses. For categorical variables, we select the correct value with probability equal to this weight, otherwise we assign that of a random match.[3]

(3) Dominance coding. In two-generation studies of the past it was common to assign household status based on a male breadwinner, to the neglect of mothers. In a refinement of this

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[2] Nybom and Stuhler (2017) report that rank correlations provide a worse fit in Swedish data than reported by Chetty et al. (2014). Mood (2017) finds that a linear model fits untransformed income well in Sweden once conditioning on social class. If so, the rank transformation may be a misspecification.

[3] One crucial type of error that we do not consider here is life-cycle bias, which occurs when outcomes are measured too early in life. This may be particularly important for studies attempting to uncover the influence of wealth; for example, Hällsten and Pfeffer (2017) include controls for parent wealth but the ratio of total *GP* to *P* financial wealth in their data is about 3.5, suggesting that the latter is unlikely to have reached maturity. Adermon et al. (2016), in their four-generation study, impute years of education for the youngest cohort that was still in school, using grades and choice of programme as main predictors.

'conventional approach', Erikson (1984) devised the 'dominance approach' whereby the spouse with the 'dominant' (normally 'higher') occupation/class got to represent the household's socioeconomic position. Later research has showed either approach that leaves out one spouse (often the woman) to be empirically untenable (Beller 2009; Mood 2017; Hout 2018). Nevertheless, this practice persists in multigenerational research (Song and Mare 2017). In most studies we surveyed, mothers were absent, partly or wholly. In some datasets, parents are the index generation and may include both mothers and fathers (Liu 2018), but spousal status remains unobserved. In multigenerational analyses, mothers may enter indirectly, as in Chan and Boliver's (2013) study where *GP* status is coded on the maternal side, while *P* variables pertain mostly to the father. Such designs are likely to confound multigenerational associations with the fact that both mothers and fathers contribute to their children's abilities and opportunities. We assess this issue by first entering (the ideal) separate terms for mothers and fathers, and, secondly, using a coarser (but common) alternative, namely taking the higher value of the two.

(4) Dimension reduction. Lastly, we ask what happens when one or more dimensions of parental status are excluded altogether. Although this is less common, there are studies that focus on only one status dimension such as education (Liu 2018, Song and Mare 2017) or occupation (Knigge 2016) to the exclusion of all others.

--FIG 4 ABOUT HERE--

Taking all above variations into account yields a total of 126 models. Figure 4 shows the distribution of the estimated *GP* coefficient across all these models, ranked from lower to higher. Our preferred estimate of 0.02 is easily tripled in data of unusually high quality, for example in a model without measurement error but with P variables education and income somewhat coarsely measured. Even more worryingly, a (not uncommon) dataset with measurement error and with only a categorical occupational variable returns a strong 'grandparent effect' of 0.15. Almost any deviation from the 'best' estimate contributes to lifting the '*GP* effect' to a level that is not possible to ignore.

While the 126 models in Figure 4 contain many combinations that can be identified in existing studies, it is perhaps more illuminating to study the distribution of estimates, conditioning on one operational choice at a time (and holding the others constant). This we do in Figures 5 to 8. The leftmost edge of the most high-quality alternative specification shows the location of our preferred estimate, where we use our data to their fullest potential, while the dotted line represents the average estimate across the distribution of the other three dimensions.

In Figure 5, we see how the estimate differs depending on the level of detail used. In Figure 6, we instead introduce errors of observation. Figure 7 reveals the impact of dominance coding on the size of the estimated *GP* association. In Figure 8, finally, we distinguish estimates by which each subset of *P* income, class, education is included as controls. These results can now be used to show which types of deviations from the ideal model produce the most overestimated GP effects. Of gravest concerns should be measurement error and using only one indicator of parental status as control. Using rank correlations is, as could be surmised, a good alternative to a metric form because it is robust to alternative levels of mismeasurement in particular, but the average estimate generated is still off the mark, as compared to the best metric estimate. Dominance coding does not appear to be a big problem, possibly because of high degrees of homogamy in marriages.

--FIG 5-8 ABOUT HERE--

7 Conclusion

The last decade has seen a sharp growth of interest in multigenerational processes in the transmission of advantage (Anderson, et al. 2018; Pfeffer 2014). While the literature is full of evocative statements about the influence of grandparents, these are generally based on less than adequate data. We have contributed to this literature taking an unusually comprehensive view: by studying the interlocking dimensions of income, education, social class, and wealth; by considering both mothers and fathers; and by including information from all four grandparents. Failure to incorporate mothers and multiple stratifying dimensions will, as a rule, lead researchers to overestimate the effects of grandparents. So, too, will errors of reporting, truncation, or categorization that are common in survey data. Without attempts to adjust for confounding – both observed and unobserved – conclusions of studies that claim to find a grandparent "effect" simply cannot be taken at face value.

One of few previous studies to adjust for such errors is Warren and Hauser's (1997) analysis of the Wisconsin Longitudinal Study. They conclude that "data are not consistent with the hypothesis" of a direct grandparent effect (p. 571). Later attempts have been limited by all or a combination of: incomplete data, poorly specified models, or crude estimation procedures. In this study, we used comprehensive data on Swedish lineages to put the three-generation model to a more rigorous test than has hitherto been possible. Our findings echo those of Warren and Hauser (1997): in our most comprehensive analyses, the additional (controlling for parents' characteristics) explanatory value of grandparent income, class, and wealth taken together amounts to *around one to two per mille* of the variance in adult grandchildren's income – that is, virtually nothing. This does not rule out a direct influence of grandparents in contexts other than that studied here, but it does cast doubt on the potential of standard designs to distinguish such influence from model artefacts.

In fact, we went further and asked what impact data limitations typical of the field would have on estimated associations in our setting. From an up-to-date literature survey, we identified four likely sources of bias: the detail of available data, the reliability with which it is measured, how separate statuses of parents are treated, and how comprehensive the concept of status used. The results are sobering: whatever we did to make our data cruder, we saw the estimated grandparent coefficient grow in size. Our conclusion is that grandparent "effects" in the literature are likely to be inflated by a factor of two to five, if not more. The greatest culprit appears to be measurement error, but all coarsening of measures contributes to inflating the 'grandparent effect'.

While our findings bring bad news for three-generation research, they come as good news for the conventional two-generation model: estimates of parental socioeconomic characteristics are only very slightly (or in most cases, not at all) affected by the inclusion or exclusion of grandparents' characteristics. The association between parental income and child's income, for example, is almost exactly the same in a two- and a three-generation model. Thus, the worry expressed by Mare (2011) about the limitations of the two-generational view, which has inspired so many recently in their search for 'grandparent effects', appears on the basis of our data to be entirely unfounded. The 'long arm of the family', while continually reproducing inequality across generations, at least takes on an obligingly simple form: It's all about the parents.

In light of this, it would seem that a more fruitful avenue to deepen our understanding of intergenerational persistence would be refining existing two-generation models, rather than extending overly simplified models across more than one generation. However, the wealth of data that is becoming available on family and kin is nevertheless promising because it enables the descriptive mapping of correlations between generations and more distant relatives, such as cousins (e.g., Hällsten 2014; Lundberg 2018). This may provide stratification research with further insights into patterns of inequality under different societal conditions without the problematic modelling of 'grandparent effects'.

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Table 1 Grandparent and parent coverage, sample size, and parent and grandparent age at child's birth

	1965	1966	1967	1968	1969	1970	1971	1972
N (children)	109,248	109,061	107,412	100,045	94,569	95,525	99,126	95,898
% with data on income for								
Father (earnings)	96	96	96	97	97	97	97	97
Mother (earnings)	99	99	99	99	99	99	99	99
Paternal grandfather	46	51	55	59	63	67	70	73
Paternal grandmother	54	60	65	69	73	76	79	82
Maternal grandfather	59	64	67	71	74	76	79	81
Maternal grandmother	69	74	77	80	83	85	87	89
Any parent	100	100	100	100	100	100	100	100
Any grandparent	81	85	88	91	93	95	97	98
Valid sample P+GP+C	87,977	92,544	94,775	90,937	87,985	90,664	95,574	93,457
Mean age at birth								
Father	30	30	30	29	29	29	29	29
Mother	27	26	26	26	27	27	26	27
Paternal grandfather	59	59	59	60	60	60	61	61
Paternal grandmother	55	55	55	56	56	56	57	57
Maternal grandfather	57	57	58	58	58	59	59	59
Maternal grandmother	53	53	54	54	54	55	55	55

Table 2 Regression of child earnings on grandparent income/EGP, parent earnings/EGP, and other parental controls

	Son's earnings (standardized) (N=377,436)				Daughter's earnings (standardized) (N=357,428)			
	Only GP income	Only P earnings	GP + P inc/earn	GP + full P controls	Only GP income	Only P earnings	GP + P inc/earn	GP + full P controls
Grandparent income	0,160		0,068	0,027	0,135		0,050	0,013
Parent earnings		0,291	0,268			0,266	0,248	
Father earnings				0,157				0,108
Mother earnings				0,083				0,114
All parent controls				yes				yes
R-squared	0,026	0,084	0,088	0,116	0,018	0,069	0,072	0,092
	Only GF EGP	Only F EGP	GF + F EGP	GF EGP + full controls	Only GF EGP	Only F EGP	GF + F EGP	GF EGP + full controls
Paternal grandfather EGP (ref. upper service class)								
Mid service class	-0,173		-0,113	-0,038	-0,145		-0,083	-0,009
Low non manual	-0,276		-0,142	-0,037	-0,245		-0,117	-0,013
Self-employed	-0,320		-0,145	-0,043	-0,275		-0,116	-0,017
Farmers	-0,376		-0,134	-0,030	-0,303		-0,107	-0,002
Skilled manual	-0,404		-0,202	-0,065	-0,344		-0,162	-0,030
Non-skilled manual	-0,446		-0,219	-0,067	-0,389		-0,185	-0,036
Missing occupation	-0,374		-0,191	-0,074	-0,291		-0,130	-0,020
Not in census	-0,420		-0,213	-0,077	-0,337		-0,157	-0,036
Father EGP (ref. upper service class)								
Mid service class		-0,250	-0,234			-0,242	-0,227	
Low non manual		-0,409	-0,388			-0,381	-0,363	
Self-employed		-0,518	-0,499			-0,463	-0,446	

Farmers		-0,655	-0,639			-0,428	-0,416	
Skilled manual		-0,599	-0,568			-0,529	-0,502	
Non-skilled manual		-0,677	-0,646			-0,597	-0,571	
Missing occupation		-0,827	-0,780			-0,677	-0,645	
R-squared	0,010	0,055	0,058	0,116	0,007	0,040	0,042	0,092

Note: All incomes and earnings are z-standardized

Table 3. R-squared from regressions of child earnings on grandparental variables with and without parental controls

	R-squared			
	Grandparent variables only	Parent variables only	Parents and grandparents	Grandparent contribution
Men				
GP incomes, EGP, wealth	0.039	0.115	0.117	0.002
GP incomes	0.026	0.115	0.116	0.001
GP EGP	0.020	0.115	0.116	0.001
GP wealth	0.014	0.115	0.116	0.001
GP education	0.009	0.115	0.116	0.001
N				377,436
Women				
GP incomes, EGP, wealth	0.029	0.092	0.093	0.001
GP incomes	0.018	0.092	0.092	0.000
GP EGP	0.015	0.092	0.092	0.000
GP wealth	0.010	0.092	0.092	0.000
GP education	0.008	0.092	0.092	0.000
N				357,426

Table 4. Overview of studies

Study	Source	C outcome	GP predictor	P education	P income	P occupation	P other	Both parents	Error correction	Method
Chan & Boliver (2013)	British cohort studies	Social class	Social class (mother's father)	Age at school leaving	Annual income (banded)	Social class (father only)	Home ownership	Class: father only; educa-tion, income: both.	No	Ordered logistic regression
Hällsten & Pfeffer (2017)	Swedish population registers	Educational achievement (GPA)	Wealth (percentiles)	Percentile rank	Percentile rank	Microclass	Cognitive and non-cognitive ability; wealth	Skills: father only; other variables: both.	No	Regression, cousin fixed-effects, mar-ginal structural models
Jaeger (2012)	Wisconsin Longitudinal Study	Years of schooling	Years of schooling	Years of schooling	Annual income (partly imputed)	Duncan SEI scale (father only)	Family size, cognitive ability, health	Occupation: father only; other variab-les: both.	No	Regression, cousin correlations
Knigge (2016)	Dutch Marriage Certificates	Occupational status	Occupational status	No	No	HISEI status scale (father only)	Family size	Father only	No	Regression, cousin correlations
Liu (2018)	Framingham Heart Study, Health and Retirement Study	Years of schooling	Years of schooling, polygenic education scores	Years of schooling	No	No	Polygenic education scores	No	Polygenic scores: yes; other variables: no.	Regression
Sharkey & Elwert (2011)	PSID Child Development Supplement	Cognitive ability	Parent neighborhood poverty in youth	Years of schooling	Annual income	Percentage college educated within occupation	Disability, welfare receipt, vocabulary score, hours worked, home ownership, ever married, attitudinal measures	Household head only	No	Regression, marginal structural models, sensitivity analysis
Song (2016)	Panel Study of Income Dynamics (PSID)	Years of schooling	Years of schooling	Years of schooling	Annual income	Duncan SEI scale	Family structure, disability status, homeownership	Occupation: average of both; education: highest only.	No	Mixed-effect models, inverse proba-bility treatment weighting
Song & Mare (2017)	Panel Study of Income Dynamics (PSID)	Education level, 3 categories	Education level, 3 categories	Education level, 3 categories	No	No	No	Yes	No	Logistic regression, demographic simulations
Zeng & Xie (2014)	Chinese Household Income Project	School dropout	Education level, percentile rank	Education level, percentile rank	No	Social class, 3 categories	Family size	Yes	No	Logistic regression

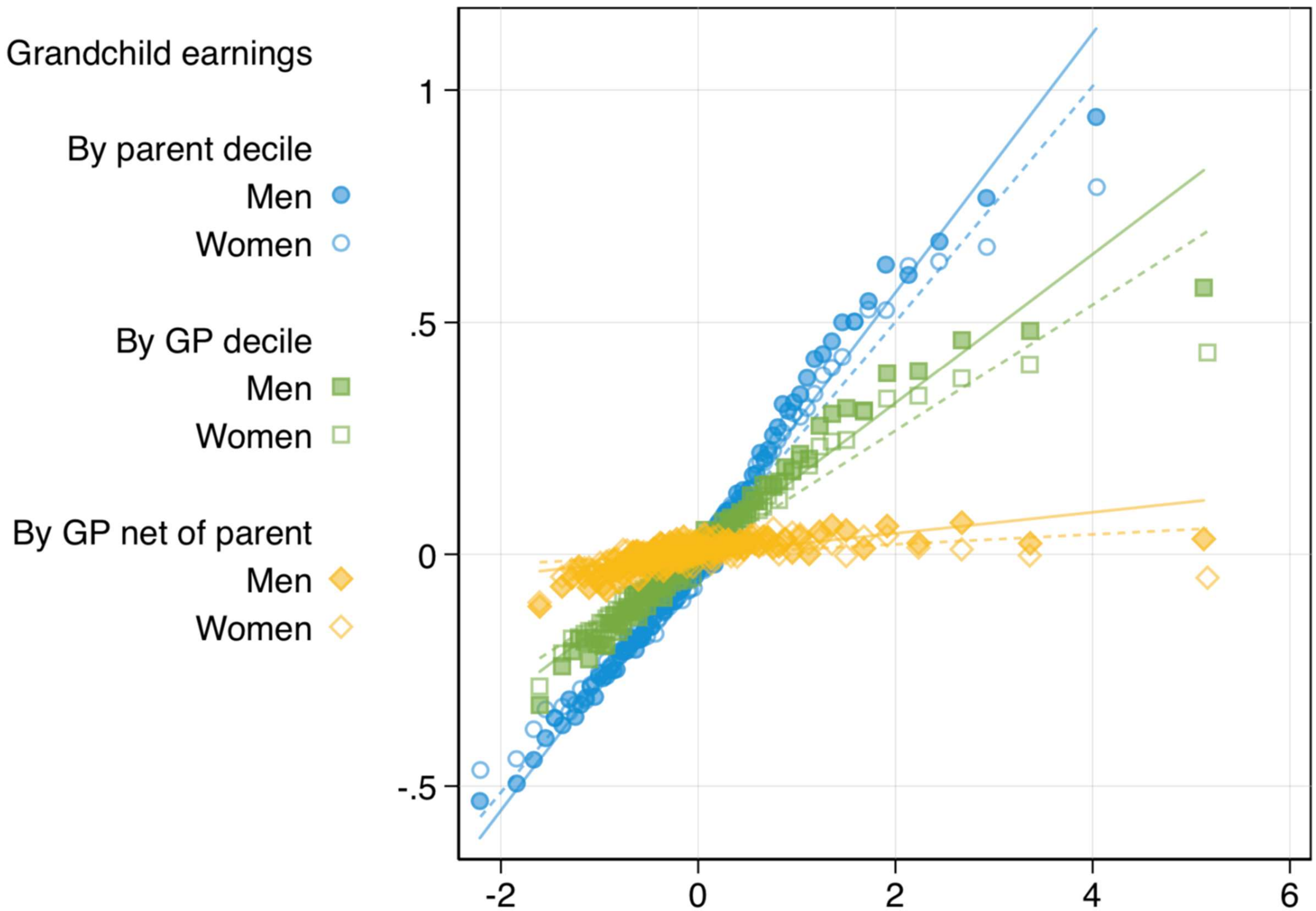

Grandchild earnings
By parent decile
Men
Women
By GP decile
Men
Women
By GP net of parent
Men
Women
1
.5
0
-.5
-2
0
2
4
6

Figure 1.

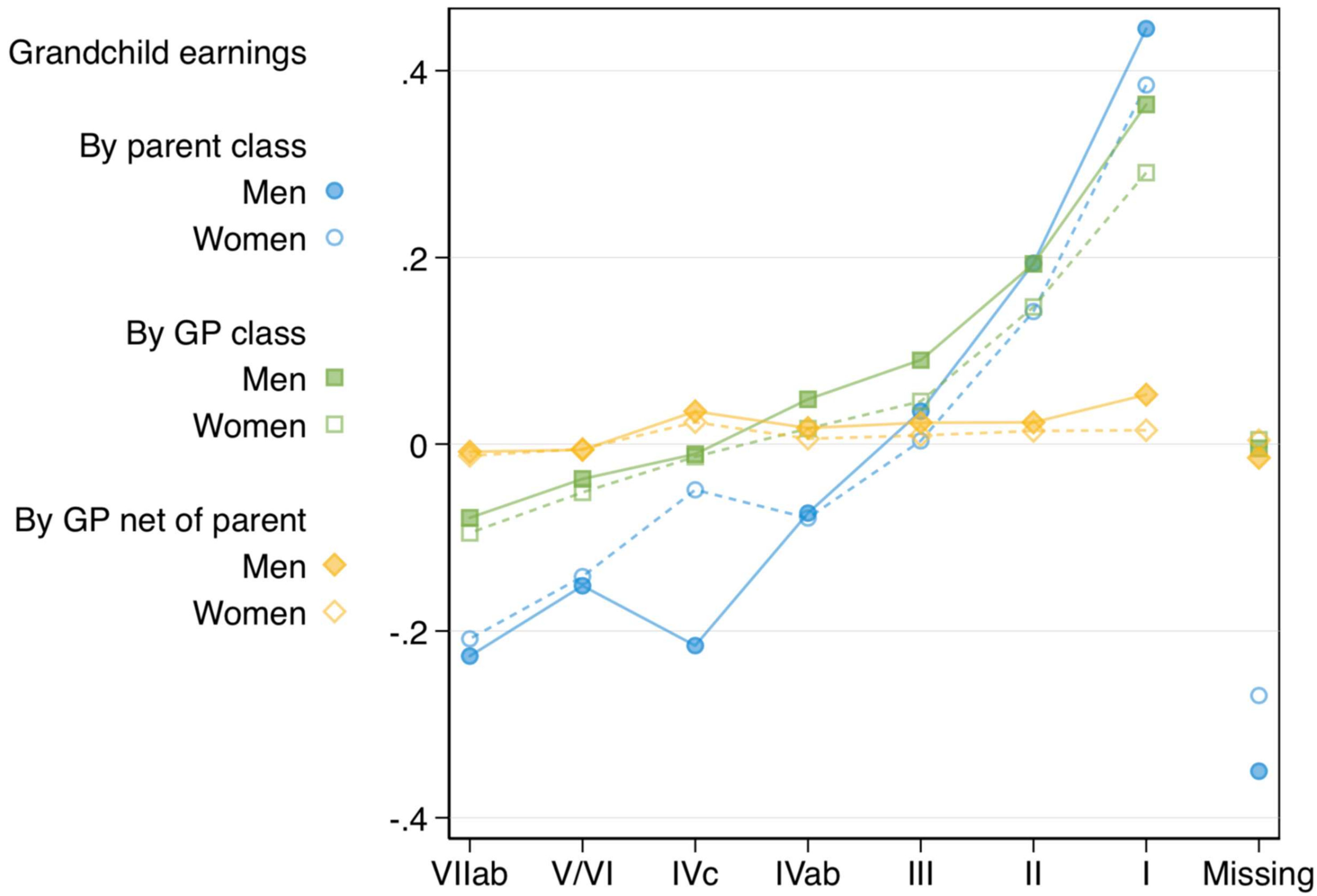
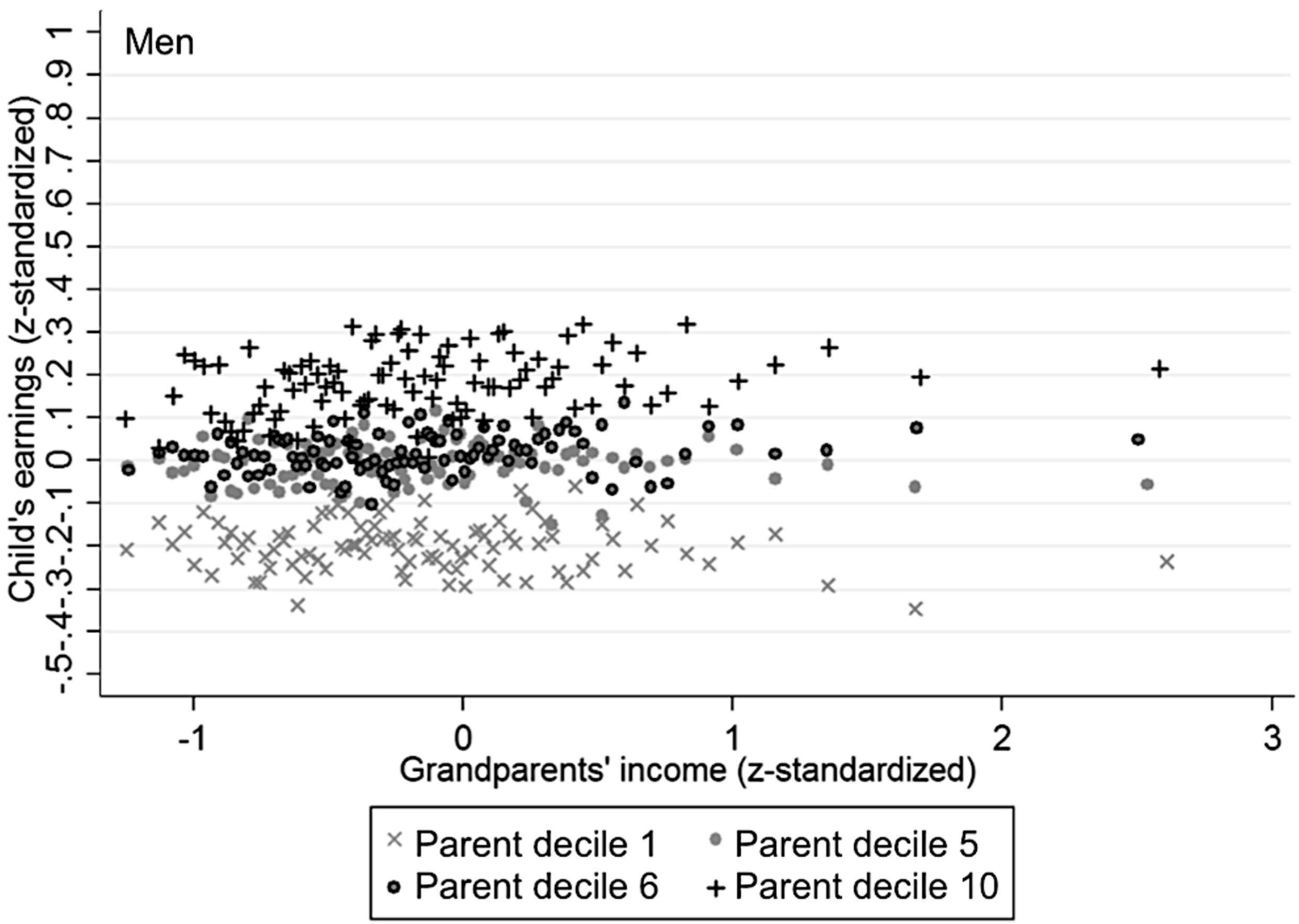

Grandchild earnings
By parent class
Men
Women
By GP class
Men
Women
By GP net of parent
Men
Women
.4
.2
0
-.2
-.4
VIIab
V/VI
IVc
IVab
III
II
I
Missing

Figure 2



Men
Child's earnings (z-standardized)
-.5 -.4 -.3 -.2 -.1 0 .1 .2 .3 .4 .5 .6 .7 .8 .9 1
-1
0
1
2
3
Grandparents' income (z-standardized)
Parent decile 1
Parent decile 5
Parent decile 6
Parent decile 10

Figure 3

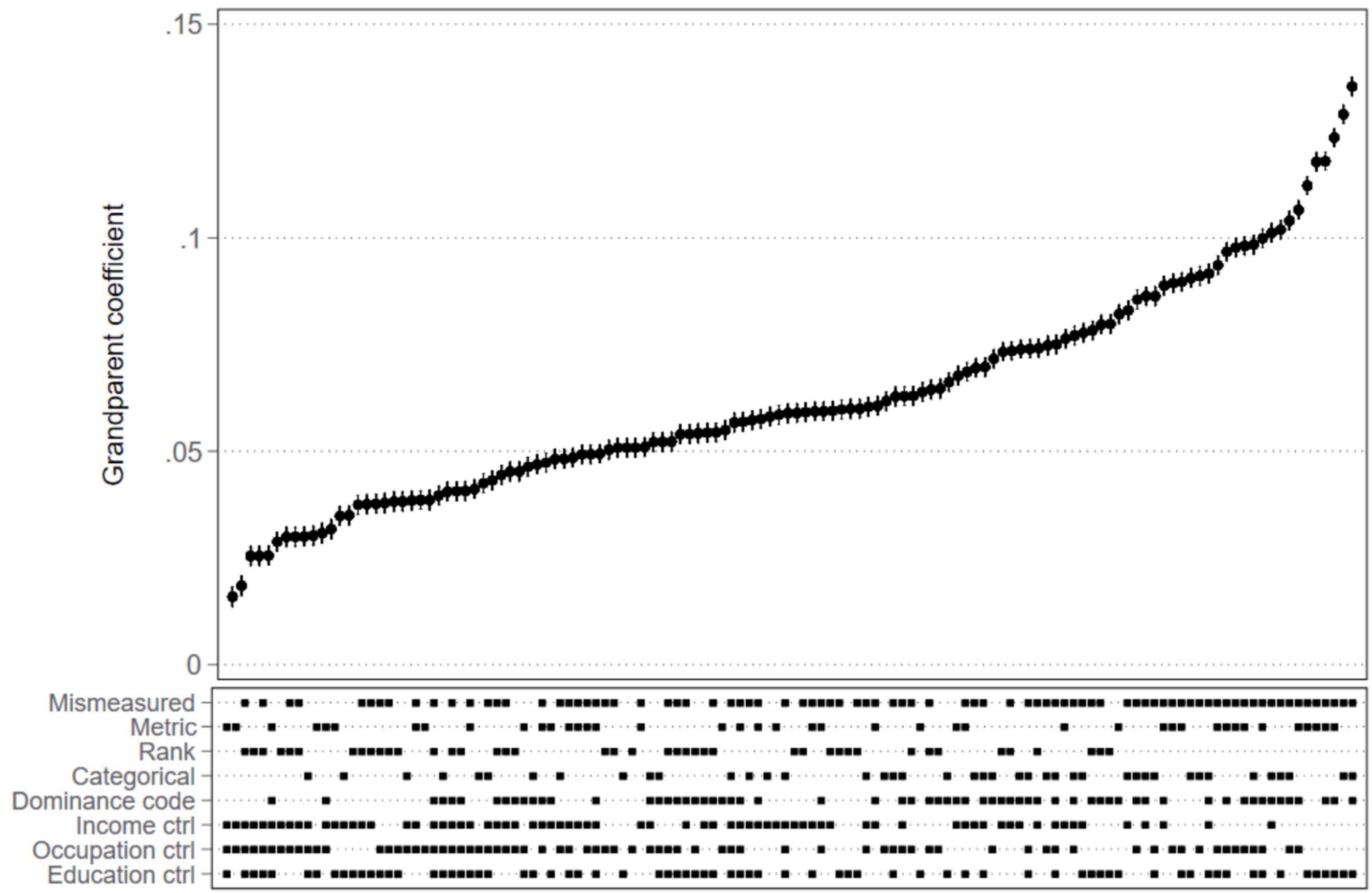

Grandparent coefficient
.15
.1
.05
0
Mismeasured
Metric
Rank
Categorical
Dominance code
Income ctrl
Occupation ctrl
Education ctrl

Figure 4

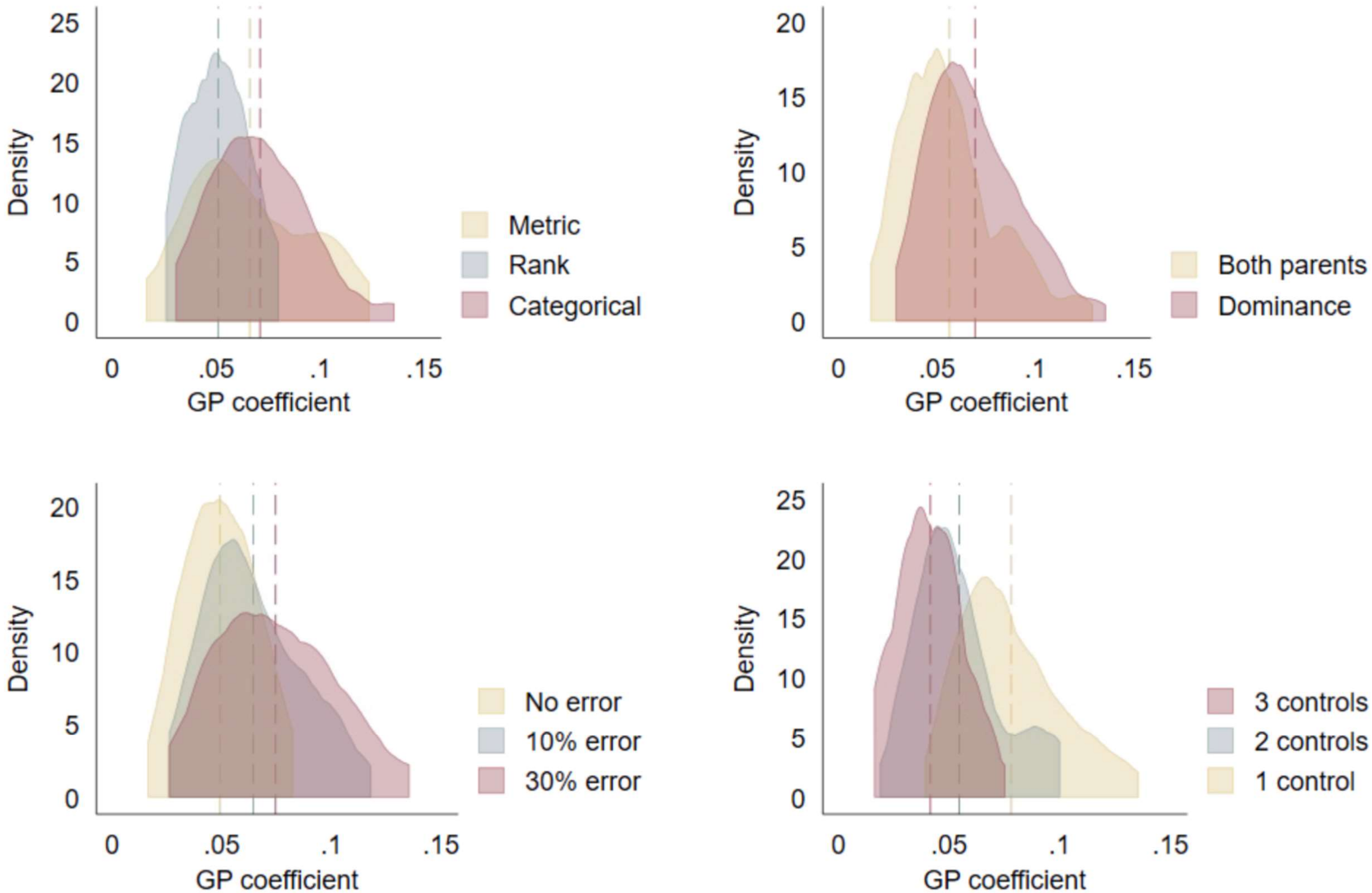

Density
GP coefficient
0
.05
.1
.15
0
5
10
15
20
25
Metric
Rank
Categorical
Both parents
Dominance
No error
10% error
30% error
3 controls
2 controls
1 control

Figure 5-8

Appendix table: Regression of child earnings (standardized) on grandparent income (standardized) and parent controls per cohort.

	Men		Women	
	GP only	GP - P control	GP only	GP - P control
1965	0.159	0.028	0.131	0.015
1966	0.160	0.024	0.133	0.010
1967	0.153	0.022	0.128	0.010
1968	0.156	0.020	0.138	0.017
1969	0.162	0.027	0.129	0.007
1970	0.172	0.035	0.143	0.017
1971	0.161	0.032	0.138	0.016
1972	0.159	0.030	0.142	0.008