The Transmission of Inequality Across Multiple Generations: Testing Recent Theories with Evidence from Germany

Short Title: "Inequality Across Multiple Generations"

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March 2016

Abstract

This paper shows that across multiple generations, the persistence of occupational and educational attainment in Germany is larger than estimates from two generations suggest. We consider two recent interpretations. First, we assess Gregory Clark's hypotheses that the true rate of intergenerational persistence is higher than the observed rate, as high as 0.75, and time-invariant. Our evidence supports the first but not the other two hypotheses. Second, we test for independent effects of grandparents. We show that the coefficient on grandparent status is positive in a wide class of Markovian models, and present evidence against its causal interpretation.

JEL classification: J62.

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Economists and social scientists have long been interested in the persistence of social status across generations. However, most studies focus on just two consecutive generations, parents and their children (see Solon, 1999; Black and Devereux, 2011, for literature reviews). Much less is known about the persistence of status across multiple generations.[1] Existing studies typically find that inequality is more persistent than estimates from parent-child correlations suggest, but attribute this additional persistence to very different underlying mechanisms. In this paper, we present direct evidence on the persistence of social status across up to four generations in 19th and 20th century Germany, and use our evidence to test recent theories of multigenerational persistence.

Two distinct theories have gained particular attention. Clark (2014) and Clark and Cummins (2014) argue that wealth, education or occupational status is transmitted via an underlying and unobserved *latent factor*. They suggest that the persistence of this underlying factor is not only very high–much higher than the persistence in observed outcomes between parents and children–but also steady across social systems and time. Mare (2011) points to a very different interpretation of multigenerational correlations. He argues that the previous literature suffers from a fundamental conceptual limitation in that it considers only the transmission between parents and children. Following his call to overcome this *"two-generation paradigm*", a fast-growing literature examines the existence of independent causal effects from other family members, in particular grandparents.[2]

Both theories can potentially explain why inequality is more persistent than parent-child correlations suggest. However, they point towards different underlying mechanisms. While Clark offers a provocative interpretation of the traditional parent-child perspective, Mare and others want to move beyond it. The two theories also have different policy implications: In Clark's perspective, the rate of social mobility is unaffected by the environment and, thus, resistant to social policies. Cross-country variation in parent-child correlations, as discussed

[1] See Warren and Hauser (1997) for a short review of earlier studies on the persistence of inequality across multiple generations, such as Hodge (1966). Among recent studies, Lindahl *et al.* (2015) exploit data from a survey of all pupils attending third grade in the Swedish city of Malmö in 1938. The survey follows the index generation until retirement and also provides information on parents, spouses, children, and grandchildren. The authors show that extrapolated estimates from two-generation studies considerably underestimate the persistence in labour earnings and educational attainment across multiple generations. They also find that even after controlling for parents' educational attainment, grandparents' education have an independent effect on the outcomes of grandchildren. Turning to occupational mobility, Long and Ferrie (2013b) study British and US census data for 1850 to 1910. The data provides information on the occupations of grandfathers, fathers, and sons. The authors find that in both Britain and the US, the occupation of grandfathers has an independent effect on the occupation of their grandsons, and that the actual rate of social mobility is significantly lower than estimates based on two-generation estimates suggest. Clark and Cummins (2014) analyse the transmission of wealth over five generations for people dying between 1858 and 2012 in England or Wales. Using rare surnames to track families, the authors find that the transmission of wealth is much more persistent than standard estimates would suggest.

[2] Chan and Boliver (2013), for instance, draw on data from three British birth cohort studies to analyse the association between the social class positions of grandparents and grandchildren in contemporary Britain. The authors find that even after controlling for parents' social position, grandparents' have a substantial effect on the social class that their grandchildren reach. Modin *et al.* (2013) show that ninth graders in contemporary Sweden are more likely to achieve top grades in Mathematics and Swedish if their grandparents also did well in these subjects. The authors include controls for the education level of both parents and grandparents, and interpret their results as evidence for a direct influence of grandparents on grandchildren. Hertel and Groh-Samberg (2013) use longitudinal survey data to analyse and compare class mobility across three generations in Germany and the US. They find that in both countries, the social class of grandfathers is directly associated with the social position of their grandchildren.

by Corak (2013) and others, is then without long-run significance. In contrast, Mare highlights the importance of context, arguing that the "correct" model of mobility may vary with historical and institutional factors.

We start our analysis by presenting novel evidence for Germany on the long-run persistence of occupational status and educational attainment, using data from three retrospective surveys, the German Life History Study, the Berlin Aging Study, and the National Educational Panel Study. The data sets contain measures of occupational status for three and of educational attainment for up to four generations, and, compared to previous studies, offer several advantages. First, four of our five samples are nationally representative. Second, we observe direct, non-imputed information on family links, education, and occupations for each generation. Finally, we observe five distinct cohort groups, which were differently affected by events in the first half of the 20th century, and in particular by World War I and II. The time dimension is especially interesting given Clark's and Mare's contrasting arguments on the importance of environmental and institutional factors.

Our finding suggests that the comparatively high intergenerational dependency of educational attainment in Germany (see, e.g., Shavit and Blossfeld, 1993, and Heineck and Riphahn, 2009) extends beyond two generations: our average estimate across three generations is 0.35 for regression and 0.25 for correlation coefficients, between 20% and 40% higher than comparable estimates for Sweden (Lindahl *et al.*, 2015). The correlation in occupational prestige is slightly lower than in education across two, but of similar magnitude across three generations.

We test if the iteration of parent-child measures provides a good approximation for status inequality across multiple generations. This question is important, because such iterations have been used, and because they imply that status differences tend to disappear quickly–leading to strong hypotheses about the persistence of inequality. For instance, Becker and Tomes (1986) conclude in their influential work on the economics of the family that "*almost all earnings advantages and disadvantages of ancestors are wiped out in three generations. Poverty would not [persist] for several generations.*"[3] However, we find that the persistence of inequality is substantially higher than the iteration procedure suggests. The actual three-generation estimates in schooling are about 40%, those in occupational prestige up to 70% higher than the predicted coefficients.

We then use our reduced-form evidence on multigenerational correlations to identify the parameters of the latent factor model underlying Clark's arguments, for each of our samples and outcomes. In contrast to Clark (2014), who identifies the parameters by averaging outcomes within surname groups, our identification strategy does not rely on the assumption that group members share no characteristics, other than the latent factor, that affect their status. The heritability of the latent factor averages around 0.60 across our samples, and is sub-

[3]The quote relates to their interpretation of parent-child coefficients from the empirical literature, but their model can explain departures from such pattern.

stantially larger than the observed parent-child correlations in status. This finding supports Clark's hypothesis that the transmission process is characterised by a higher degree of persistence than standard intergenerational estimates suggest. However, persistence is not as high as his estimates from surname groups, which cluster around 0.75, imply, and we do find statistically significant differences in its level across time. This finding suggests that the long-run potential of families does respond to the economic and institutional environment.

Next, we test whether the hypothesis that grandparents have an independent causal effect on their grandchildren can explain the observed pattern of multigenerational persistence. We first note an important link between the strand of literature that studies long-run inequality, and the strand that assesses the role of grandparents: *any* causal process that generates persistence over and above the rate implied by extrapolating two-generation measures also generates a positive grandparent coefficient in a regression of offspring status on parent and grandparent status, and vice versa. As many theoretical mechanisms can explain the former, the observation of a positive grandparent coefficient does not provide evidence against the traditional Markov (parent-child) perspective of intergenerational transmission. Indeed, statistical associations with grandparents vanish in four of our five samples when we control also for the social status of the mother, which is often not observed in multigenerational data. The large and robust grandparent effect that we find in the fifth sample seems to operate through indirect mechanisms, which do not require direct contacts between grandparents and grandchildren, as we show by exploiting quasi-exogenous variation in the time of grandparents' death generated by World War II.

Finally, we compare the two theories' performance in predicting multigenerational persistence. In particular, we identify the model parameters from three-generation data and use the estimated models to predict the persistence in educational attainment across four generations. We then compare the models' prediction to the actual persistence across four generations. We find that the latent factor model provides a good approximation, outperforming also the grandparental effects model. Overall, the literature's traditional focus on parent-child transmission, and its neglect of earlier ancestors, appears not a significant obstacle for understanding the persistence of economic status across multiple generations.

The rest of the paper is structured as follows. Section 1 discusses recent theories of multigenerational persistence and develops ways to test them. Section 2 describes our data and reports descriptive statistics. Section 3 presents our evidence on the persistence of educational attainment and occupational prestige across multiple generations in Germany. Section 4 presents our evidence on the latent factor and grandparental effect models, and Section 5 compares their success in predicting multigenerational persistence. Section 6 concludes.

1 Theory and Measurement

To summarise the degree to which a child's status depend on her parents' status, economists typically estimate the slope coefficient β_{-1} in a linear regression of outcome $y_{i,t}$ in offspring generation t of family i on parental outcome $y_{i,t-1}$,

$$y_{i,t} = \alpha + \beta_{-1} y_{i,t-1} + \varepsilon_{i,t}. \tag{1}$$

The coefficient β_{-1} captures the degree to which status differences among parents are, on average, transmitted to their offspring. Persistence across multiple generations can be similarly summarised by regressing $y_{i,t}$ on outcomes of grandparents $y_{i,t-2}$, great-grandparents $y_{i,t-3}$, and so on. The sequence of coefficients

$$\{\beta_{-1}, \beta_{-2}, \beta_{-3} ..., \beta_{-m}\}$$

or the corresponding correlation coefficients, which abstract from changes in the variance of the outcome across generations, then summarise the longevity of status inequality across generations. We now discuss several hypotheses on the relationship between two- and multigenerational persistence and propose ways to test them.

1.1 The Iterated Regression Procedure

Most of the existing literature observes data from two generations to estimate β_{-1}, but cannot provide direct estimates on the persistence of inequality over three or more generations. Instead, researchers have at times iterated estimates of β_{-1} to predict multigenerational persistence, assuming that $\beta_{-m} \approx (\beta_{-1})^m \; \forall m > 1$. This iterated regression procedure implies that status differences will disappear quickly even for high values of β_{-1} (see Stuhler, 2012, for a discussion).

Recently, researchers have begun to provide comprehensive evidence on multigenerational persistence. However, only few studies are based on direct observations of family links (see Dribe and Helgertz, 2013, and Lindahl *et al.*, 2015), and these data are typically from small geographic areas. Other researchers thus rely on novel methods to exploit repeated cross-sections instead.[4] These studies typically find that $\beta_{-m} > (\beta_{-1})^m$, an observation to which we refer to as "excess persistence". Several models of intergenerational mobility can explain such excess persistence (Solon, 2014; Stuhler, 2012; Zylberberg, 2013). We turn to two interpretations that have gained particular attention, and show how they can be tested in the data.

[4] Long and Ferrie (2013a) link individuals in British and U.S. censuses; Collado *et al.* (2013) exploit socioeconomic bias in the distribution of surnames in two Spanish regions; Clark (2013, 2014), Clark and Cummins (2014) and Güell *et al.* (2015) rely on the informative content in rare surnames; and Olivetti *et al.* (2014) on information in first names.

1.2 The Latent Factor Model and Clark's Hypotheses

Multigenerational persistence in socio-economic status may be higher than standard parent-child estimates suggest, because parents transmit their status indirectly through the inheritance of an underlying latent factor representing abilities, preferences, or other relevant characteristics (Clark and Cummins, 2014, and earlier working papers; Stuhler, 2012). To capture this idea in a simple way, suppose that the intergenerational transmission of observable outcome $y_{i,t}$ and unobservable endowment $e_{i,t}$ in a one-parent one-offspring family is governed by

$$y_{i,t} = \rho e_{i,t} + u_{i,t} \tag{2}$$

$$e_{i,t} = \lambda e_{i,t-1} + v_{i,t}, \tag{3}$$

where $u_{i,t}$ and $v_{i,t}$ are noise terms that are uncorrelated with other variables and past values. For simplicity, we normalise the variances of $y_{i,t}$ and $e_{i,t}$ to one, so that slope coefficients can be interpreted as correlations.

In this *"latent factor model"*, the offspring inherits her unobserved endowment from the parent (according to the *"heritability"* coefficient λ), and the endowment then translates into the observed outcome (according to the *"transferability"* coefficient ρ).[5] The observed correlation in outcome y between generation t and generation $t-m$ equals then

$$\begin{aligned}\beta_{-m} &= Cov(y_{i,t}, y_{i,t-m}) \\ &= \rho^2 Cov(e_{i,t}, e_{i,t-m}) \\ &= \rho^2 \lambda^m. \end{aligned} \tag{4}$$

The persistence of socio-economic status over generations thus decreases with both the persistence of the unobserved endowment, as captured by $\lambda = Cov(e_{i,t}, e_{i,t-m})$, and the transferability of the unobserved endowment into the observed outcome, as captured by ρ. Across multiple generations, however, persistence is predominantly governed by λ rather than ρ. This is because the latent factor $e_{i,t}$ is inherited m times across generations but only twice transformed into outcome $y_{i,t}$.

The iterated regression procedure implicitly assumes that the link between outcomes and latent factor is perfect ($\rho = 1$ and thus $Var(u_{i,t}) = 0$). In this case, estimates of β_{-1} could indeed be iterated to predict multi-

[5]This formulation can also capture earlier arguments on the dynamics of multigenerational mobility from the sociological literature. For example, Fuchs and Sixt (2007) compare educational attainment of children from educational climbers to children from similarly educated parents whose own parents had already high education, and find that children of educational climbers tend to do less well. In the interpretation of the latent factor model, children of educational climbers (high y_t, low y_{t-1}) tend to do less well because on average they have lower endowments e_t. However, sociological studies argue that high educational status may eventually feed back into its assumed determinants, such as *cultural* or *social capital* (see, for instance, Fuchs and Sixt, 2007, and the reply by Becker, 2007).

generational persistence, as $\beta_{-m} = (\beta_{-1})^m$. If the link between outcomes and underlying latent factor is instead imperfect ($\rho < 1$), we have $\beta_{-m} > (\beta_{-1})^m \; \forall m > 1$: status inequality is more persistent than the extrapolation from parent-child measures suggests.

Clark's Hypotheses. Clark (2014) and Clark and Cummins (2014) interpret their comprehensive empirical evidence on status persistence of rare surname groups through the lenses of this model. They formulate three major hypotheses on the intergenerational persistence of the underlying unobserved endowment, $\lambda = Cov(e_{i,t}, e_{i,t-1})$. First, they suggest that λ is larger than the reduced-form correlation β_{-1}, which is typically estimated in the literature. Second, they suggest that the difference is substantial. Their estimates of λ are around 0.75, implying that inequality persists across multiple centuries.[6] Third, Clark (2014) suggests that λ is close to a "universal constant" across social systems and time, unaffected by the institutional and economic environment.[7] This hypothesis implies that social policy can affect individuals' current positions, but not the long-run prospects of their families. Moreover, it suggests that differences in parent-child mobility across countries and time, as for instance documented in Long and Ferrie (2013b), are due to differences in ρ and thus without long-run implications.

Identification from Multigenerational Correlations. Our data is well suited to test Clark's hypotheses, for two reasons. First, individuals in our data are linkable across at least three generations. This allows us to directly identify the parameters of the latent model from multigenerational correlations. Under the latent model in equations (2) and (3), the parent-child coefficient in the standard intergenerational equation equals

$$\beta_{-1} = \frac{Cov(y_{i,t} y_{i,t-1})}{Var(y_{i,t-1})} = \rho^2 \lambda \tag{5}$$

while the grandparent-child coefficient equals

$$\beta_{-2} = \frac{Cov(y_{i,t} y_{i,t-2})}{Var(y_{i,t-2})} = \rho^2 \lambda^2. \tag{6}$$

The ratio β_{-2}/β_{-1} thus identifies λ, while $(\beta_{-1}^2/\beta_{-2})^{1/2}$ identifies ρ. Second, our data contains measures of three outcomes variables (formal schooling, schooling with university and vocational training, and occupational

[6] Most estimates for λ reported in Clark (2014) and previous working papers are in the range 0.7-0.85, rationalising the substantial persistence of status inequality across surname groups that he and his co-authors observe in several countries. Clark and Cummins (2014) find an intergenerational elasticity of wealth for surname cohorts in England and Wales in 1858-2012 of "close to 0.75 for all periods" (p. 2). Clark (2014) concludes that "*it takes hundreds of years for descendants to shake off the advantages and disadvantages of their ancestors*".

[7] Clark reads his empirical results as evidence for the dominance of nature over nurture in the intergenerational process. A large literature provides evidence on this question; for example, Björklund *et al.* (2006) study the relative importance of pre-birth (genetic and prenatal) factors using Swedish adoption data.

prestige) for five different samples. We can, therefore, not only test multiple times whether λ is indeed larger than β_{-1} (Clark's first hypothesis) and close to 0.75 (second hypothesis), but also assess whether it is stable over time (third hypothesis).

Assortative Mating. The argument that the inter-generational persistence of the underlying unobserved endowment, $Cov(e_{i,t}, e_{i,t-1})$, can be identified from multi-generational correlations carries over from the simplified one-parent to a more realistic two-parent setting. Persistence in the two-parent setting depends strongly on the degree of assortative mating in the population.

To see this, suppose that offspring' endowments are determined by the average of father's and mother's endowment according to

$$e_{i,t} = \tilde{\lambda}\bar{e}_{i,t-1} + v_{i,t}, \tag{7}$$

with $\bar{e}_{i,t-1} = (e^m_{i,t-1} + e^p_{i,t-1})/2$, and where m and p superscripts denote maternal and paternal variables, respectively. We continue to standardise the variance of $y_{i,t}$, $e^m_{i,t}$, and $e^p_{i,t}$ to one. The parent-child correlation in outcome y_t then equals

$$\begin{aligned} \beta_{-1} &= Cov(y_{i,t}, y^x_{i,t-1}) \\ &= \rho^2 Cov(e_{i,t}, e^x_{i,t-1}) \\ &= \rho^2\lambda \quad \forall x \in (m,p), \end{aligned} \tag{8}$$

where

$$\begin{aligned} \lambda &= Cov(e_{i,t}, e^x_{i,t-1}) \\ &= \tilde{\lambda}\left(1 + Cov(e^m_{i,t-1}, e^p_{i,t-1})\right)/2. \end{aligned} \tag{9}$$

In addition, the correlation between child outcome y_t and the outcome of any of her grandparents equals

$$\begin{aligned} \beta_{-2} &= Cov(y_{i,t}, y^{x,y}_{i,t-2}) \\ &= \rho^2\lambda^2 \quad \forall x,y \in (m,p), \end{aligned} \tag{10}$$

where x specifies whether we follow the maternal or paternal lineage, and y specifies whether we consider the grandfather ($y = p$) or grandmother ($y = m$) of that lineage.

It follows from equations (8) and (10) that also in the two-parent setting, the ratio β_{-2}/β_{-1} identifies the intergenerational persistence of the unobserved endowment between child and parent, $\lambda = Cov(e_{i,t}, e^{x}_{i,t-1})$. Furthermore, equation (9) illustrates that λ can interpreted as a reduced-form parameter that consists of two components: (i) the heritability of average parental endowment $\tilde{\lambda}$, and (ii) the degree of assortative mating in the population $Cov(e^{m}_{i,t-1}, e^{p}_{i,t-1})$. With perfect assortative mating, $Cov(e^{m}_{i,t-1}, e^{p}_{i,t-1}) = 1$ and the equations simplify to the one-parent model discussed in the previous section. But with imperfect assortative mating, we have that $\lambda < \tilde{\lambda}$. The persistence of the endowments between one parent and his or her child increases in the degree of assortative mating. Therefore, persistence in the two-parent setting is attenuated by the fact that parents are unlikely to have exactly the same endowment.

Equation (9) has two important implications for Clark's hypotheses. First, and as Clark acknowledges, the degree of assortative mating has to be high to be consistent with the hypothesis that λ is as large as 0.75. In particular, if average parental endowments are not perfectly transmitted ($\tilde{\lambda} < 1$), spouse correlations in underlying endowment have to be substantially larger than the values typically estimated for spouse correlations in observed status, such as educational attainment.[8] Second, the degree of assortative mating should also vary little across time and space to be consistent with Clark's hypothesis that the persistence in the unobserved endowment is close to a universal constant.

Measurement Error. As we describe in Section 2, our data is likely to reliably measure outcomes such as education and occupation. We nevertheless study the consequences of measurement error in Appendix A, and summarise our findings here. First, while classical measurement error leads to attenuation in the estimated autocorrelations β_{-1} and β_{-2} (see Solon, 2014), and thus also in $(\beta^2_{-1}/\beta_{-2})^{1/2} = \rho$, the attenuation bias cancels out in the ratio $\beta_{-2}/\beta_{-1} = \lambda$ if the signal-to-noise ratio remains stable across generations. However, one may expect the signal-to-noise ratio in our data to vary across generations, as information on the grandparent, parent, and offspring generation all come from respondents in the parent generation (see Section 2 for details). As respondents directly observe their own educational careers and that of their children, but not the educational careers of their parents, they might give less precise information on the latter. Appendix A shows that even in this case, we can obtain consistent estimates of λ as long as respondents know their own education equally well as that of their children.

[8] For example, Ermisch *et al.* (2006) estimate a spouse correlation in educational attainment of around 0.5 for a German sample. The correlation is similar in our data.

Time-varying Coefficients. Following the exposition of the latent factor model in Clark (2014) and Clark and Cummins (2014), we have so far assumed that ρ is time-constant. However, estimates of the persistence in the unobserved endowment can be affected by changes in ρ across generations. In Appendix B we, therefore, consider a latent factor model with time-varying coefficients to illustrate the problem, and to show that comparisons across our various samples and outcomes support the robustness of our findings. We also show that even with time-varying ρ, we can identify the persistence in the unobserved endowment if we observe four generations of individuals. Moreover, we estimate all parameters from correlation instead of regression coefficients, so as to abstract from secular trends in the variance of our outcome variables over time.

Comparison to Clark's Identification Strategy. Clark and co-authors identify the parameters of the latent factor model by averaging outcomes within surname groups. To understand the intuition behind their approach, note that equations (2) and (3) resemble an errors-in-variables model, so the usual strategies to address measurement error can be applied. In particular, the influence of errors can be reduced by averaging over repeated measurements of a variable, or within groups of individuals who share a similar level of endowment.

Such groups are readily available also in our data, as we observe siblings who share the same *parental* endowment $e_{i,t-1}$. To see how this may enable identification, consider the sibling correlation β_{sib}, defined as the share of status variance explained by family identifiers. In the latent model, this sibling correlation equals $\beta_{sib} = \rho^2\lambda^2$ and the ratio β_{sib}/β_{-1} therefore identifies λ. Clark's strategy to average across individuals in rare surname groups extends this logic to more distant family members: as individuals who share a rare surname are likely to share common ancestors, the average level of endowment differs systematically across surname groups. The principal advantage of this strategy is that parent-child links need not be directly observed.

This example illustrates that, in principle, quite different strategies may lead to identification of λ.[9] However, these strategies are not equally robust to plausible deviations from the latent factor model in its simplest form. For example, siblings share not only the same parents, but also other environmental factors – the components $u_{i,t}$ and $v_{i,t}$ are thus likely to be correlated within families.[10] Likewise, Clark's assumption that they are uncorrelated within rare surname groups may be violated if surnames themselves are associated with characteristics that are not captured by the latent model.[11] A second potential caveat is that regression to the mean can be

[9]Related, Vosters (2015) and Vosters and Nybom (2015) show that the aggregation of multiple status measures into a single "least-attenuated" estimate yields rates of persistence that are only modestly higher than estimates based on parental income only.

[10]Capturing shared environmental factors by $z_{i,t}$ and denoting its variance by σ_z^2, the ratio β_{sib}/β_{-1} then identifies $\lambda + \sigma_z^2/\rho^2\lambda$ – an upper bound for the heritability parameter λ. If environmental factors are important, σ_z^2 is large and the upper bound will be uninformative.

[11]Güell *et al.* (2015) note that averaging within surnames may "average away" intergenerational mobility, as group-average estimates capture only between-group mobility, which depends on the respective group variable. In particular, Chetty *et al.* (2014) argue that some of the surnames studied in Clark (2014) correlate with race or ethnicity, such that sustained inequality across surname groups may partly reflect inequality along ethnic lines. Finally, Solon (2015) notes that other types of group-average estimates from the previous literature do not support Clark's hypotheses.

only observed for surnames whose average status is sufficiently far from the population average. Accordingly, most but not all estimates in Clark (2014) are based on "elite" surnames, which may be less informative about the average degree of mobility in a population if intergenerational transmission is different in the tails of the distribution.[12] Our approach to identify λ via multigenerational correlations on the individual level requires direct information on family linkages and can be sensitive to measurement error (see Appendix A) but avoids the particular caveats that follow from the usage of grouped data.

1.3 The Grandparental Effects Model

Recently, the traditional assumption that status differences are only transmitted from parents to children has been forcefully challenged by Mare (2011). Instead, Mare argues that grandparents might have a *direct* influence on status differences among their grandchildren – that *grandparents matter*, at least in some populations or periods. Partly in response, a fast-growing strand of the literature aims to test and quantify "grandparental effects" (see Pfeffer, 2014, for a recent overview, and Solon (2014) for a theoretical treatment). These studies typically test in a first step if, conditional on parental status, a statistically significant association remains between offspring and grandparental status (see for example Chan and Boliver, 2013, and Hertel and Groh-Samberg, 2013).

Such independent associations have in turn important consequences for the longevity of status differences across generations. To see this formally, suppose that offspring's outcome depends positively on both her parent and her grandparent outcome

$$y_{i,t} = \gamma_{-1} y_{i,t-1} + \gamma_{-2} y_{i,t-2} + v_{i,t}, \tag{11}$$

with $\gamma_{-1} > 0$ and $\gamma_{-2} > 0$. Suppose further that $\gamma_{-1} + \gamma_{-2} < 1$, so that the AR(2) process in equation (11) is stationary. The two- and three-generation correlation coefficients in this model are given by

$$\beta_{-1} = \frac{Cov(y_{i,t}, y_{i,t-1})}{Var(y_{i,t-1})} = \frac{\gamma_{-1}}{1-\gamma_{-2}}$$
$$\beta_{-2} = \frac{Cov(y_{i,t}, y_{i,t-2})}{Var(y_{i,t-2})} = \frac{(\gamma_{-1})^2}{1-\gamma_{-2}} + \gamma_{-2}.$$

We then again have that $\beta_{-2} > (\beta_{-1})^2$, i.e., status inequality is more persistent than predicted by iterating parent-child elasticities.

[12] It is an empirical question if this selectivity matters. Clark (2014) and Clark and Cummins (2014) find a similar degree of persistence also when considering broader groups of the population. Björklund *et al.* (2012) find particularly high persistence among top incomes in Sweden.

Duality. As noted by Mare (2011), both strands of the literature, the strand on direct grandparental effects and that on multigenerational persistence, are thus closely related. In a regression context we can show how closely, as the relationship between the coefficient on grandparents and multigenerational associations can be derived precisely. The slope coefficients in a multivariate regression of child outcome y_t on parent outcome y_{t-1} and grandparent outcome y_{t-2}, β_p and β_{gp}, are given by

$$\beta_p = \frac{Cov(y_t, \tilde{y}_{t-1})}{Var(\tilde{y}_{t-1})} \quad \text{and} \quad \beta_{gp} = \frac{Cov(y_t, \tilde{y}_{t-2})}{Var(\tilde{y}_{t-2})}, \tag{12}$$

where $\tilde{y}_{t-1}$ is the residual from regressing y_{t-1} on y_{t-2}, and $\tilde{y}_{t-2}$ is the residual from the reverse regression (Frisch-Waugh-Lovell theorem). Under stationarity, both auxiliary regressions yield the intergenerational coefficient β_{-1}, so that we can rewrite the grandparent coefficient as

$$\beta_{gp} = \frac{Cov(y_t, y_{t-2} - \beta_{-1} y_{t-1})}{Var(y_{t-2})} \frac{Var(y_{t-2})}{Var(\tilde{y}_{t-2})} = \left(\beta_{-2} - \beta_{-1}^2\right) \frac{Var(y_{t-2})}{Var(\tilde{y}_{t-2})}. \tag{13}$$

In other words, *any* causal process that generates sustained excess persistence in the form of $\beta_{-2} > \beta_{-1}^2$ also generates a positive grandparent coefficient in multivariate three-generation regressions, and vice versa.[13] The assumption of stationarity simplifies the derivation but is not required for the result (see Online Appendix C.1).

The observation of a positive grandparent coefficient is thus simply the flip side of a less-than-geometric decay of multigenerational associations. As we have seen in the previous section, the latter observation can also be explained by the latent model, or various other models with a memory of just one generation (see Solon, 2014; Stuhler, 2012; and Zylberberg, 2013). Equation (13), therefore, illustrates that a positive grandparent coefficient in a child-parent-grandparent regression is no evidence for an important role of grandparents in the transmission process.

Test Procedures. We follow two strategies to test for a direct role of grandparents. Our first strategy is to test whether the positive grandparent coefficient declines–or even vanishes–if we control more fully for potentially relevant parent characteristics (as in Warren and Hauser 1997). This strategy is motivated by the observation that in a Markov model, a positive grandparent coefficient in a regression of child on parent and grandparent outcomes reflects correlation with other omitted parental characteristics. For example, the grandparent coefficient

[13] Clark and Cummins (2014) show that conditional on parental status, offspring and grandparental status will be positively correlated if the latent model correctly describes the true underlying mobility process. We show that this positive correlation extends to any data generating process that generates $\beta_{-2} > (\beta_{-1})^2$.

in the latent factor model equals (from eqs. (5), (6), and (13))

$$\beta_{gp} = \frac{\rho^2\lambda^2 - \rho^4\lambda^2}{1-\rho^4\lambda^2}, \tag{14}$$

which is positive for $0 < \rho < 1$ and $0 < \lambda < 1$. Under the latent model, the grandparent coefficient declines if we include multiple parental outcomes, each related to the latent factor by equation (2) (see Online Appendix C.2). In fact, the coefficient may eventually converge to zero even when the underlying latent variable is not observed, a hypothesis that we could test since our data include a large set of covariates for both parents in the index generation. In practice, however, it becomes increasingly difficult to judge if a variable contains further information on an individual's underlying endowment.

An often omitted but likely important characteristic is the status of the second parent. Motivated by this observation, we test whether the grandparent coefficient remains robust to the addition of observed status of the initially omitted parent. This test only allows us to reject direct grandparent effects. If we continue to find a positive grandparent effect in regressions that condition on the status of both parents, we can still not rule out that other omitted parental characteristics are driving the result.[14] The two-parent version of the latent factor model provides an illustration. In this model, the grandparent coefficient in a regression of child outcome on parent and grandparent outcome from the same lineage (e.g. father and paternal grandfather) is given by equation (14), and thus positive. The coefficient is substantially smaller, but non-zero, when the observed status of *both* parents is included (see Online Appendix C.3).[15]

We can implement this test, as our data contain educational and occupational status measures of both father and mothers, and their respective parents. This opportunity is rare, because data that span three generations tend to capture only the socio-economic status of one parent, usually the father (as in the U.S. census data studied in Long and Ferrie, 2013a). An important exception is the study by Warren and Hauser (1997) who, using data from the Wisconsin Longitudinal Study, find no evidence for an independent influence of grandparents once they condition on the status of both parents. However, the influence of grandparents may be context-specific and vary with institutional circumstances. For example, Mare (2011) argues that "mid-twentieth century Wisconsin families may be a population in which multigenerational effects are unusually weak" (an argument adopted by others, such as Chan and Boliver, 2013), and calls for research on populations that underwent large transformations. Our data are interesting also from this perspective, as they comprise five distinct cohort groups

[14] Since omitted variables could, in principle, also bias the grandparent coefficient downwards (see Solon, 2014), even a non-positive grandparent coefficient is no definite evidence against direct grandparent effects.

[15] A two-parent version of the AR(2) model in (11) generates observationally similar implications. The interpretation of the remaining coefficient on grandparent status would differ – causal in the AR(2), spurious in the latent model – but is less relevant if this coefficient is small.

that were differently affected by events such as World War I and II.

Our second strategy to test for a direct role of grandparents uses this historical context to search for quasi-exogenous variation in children's exposure to their grandparents. Many of the channels through which grandparent effects may work require some level of proximity and interaction between grandparents and grandchildren. Highly-educated grandparents might, for instance, improve the educational success of their grandchildren by helping them with their homework or by serving as role models. However, grandparents can also influence their grandchildren without interacting directly with them, e.g., through wealth transmission, networks or reputation effects. To distinguish between the importance of direct and indirect effects, we test whether the size of any positive grandparent coefficient in (11) increases with grandchild's exposure to the grandparent–as it should if the coefficient indeed reflected the positive influence of grandparents and grandchildren spending time together.

This test boils down to re-estimating (11), adding interaction terms between the intergenerational coefficients and a measure of grandparent exposure. Following Adermon (2013) and Zeng and Xie (2014), we use the time of death of the grandparent as a measure of grandparent exposure. The idea is simple: Grandparents who die early cannot have effects on their later-born grandchildren that require personal contact. However, time of death might be correlated with unobserved factors that themselves influence the intergenerational transmission coefficient. To at least partly account for this potential source of bias, we exploit quasi-exogenous variation in the time of death generated by World War II. In particular, we estimate separate coefficients for grandfathers who were killed in World War II and those who were not, restricting the sample to grandfathers who served in the war. Conditional on war deployment, the probability of dying in the war was arguably less correlated with unobserved factors, in particular since a soldier's region of deployment did not depend on his region of origin (Overmans, 1999).

2 Data and Descriptive Statistics

Our empirical analysis uses life history data from three retrospective surveys, the German Life History Study (Deutsche Lebensverlaufsstudie, LVS), the Berlin Aging Study (Berliner Altersstudie, BASE), and the adult starting cohort of the National Educational Panel Study (Nationales Bildungspanel, NEPS). All three studies use standardised, face-to-face or telephone interviews to collect retrospective life histories of respondents.

The German Life History Study (Deutsche Lebensverlaufsstudie, LVS) is based on nationally representative samples of eight birth cohorts born in Germany between 1919 and 1971 (see Mayer, 2007 for an overview). We use data from two waves of the LVS. The first wave (LVS-1) surveys individuals in West Germany born in the years 1919-21, the second one (LVS-2) surveys individuals born in 1929-31.[16] Both samples are representative for German citizens who live in the Federal Republic of Germany or West Berlin (foreigners are excluded). The LVS-1 and LVS-2 consist of life histories from 1412 and 708 respondents, collected in 1985-88 and 1981-83, respectively. The LVS asks respondents in particular about their education, employment, and family history.

The Berlin Aging Study (Berliner Altersstudie, BASE) is a multidisciplinary survey of old people aged 70 to 105 years who live in former West Berlin (see Baltes and Mayer, 2001 for an overview). The main study was conducted between 1990 and 1993, and collected data on 516 respondents, randomly sampled from the city registry of Berlin. The sample was stratified by age and gender, so that in each of six age groups (70–74, 75–79, 80–84, 85–89, 90–94, and 95+ years), 43 men and 43 women were surveyed. BASE distinguishes between four research units. We mainly use information from the sociology unit, which focuses on the employment and family history of respondents, their family relationships and their economic situation.

The *adult starting cohort survey* of the National Educational Panel Study ("Nationales Bildungspanel", NEPS) is a repeated survey of individuals who are born between 1944 and 1986 and live in Germany (see Blossfeld and Maurice, 2011, for an overview). The survey provides detailed – partly retrospective – information on education, employment, and family histories, which have been collected between 2007 and 2013. We use data on individuals born 1944-49 (NEPS-1) and 1950-54 (NEPS-2). NEPS-1 and NEPS-2 contain data on 1943 and 2144 respondents, respectively.

Importantly, all five surveys (LVS-1, LVS-2, BASE, NEPS-1, NEPS-2) ask respondents not only about their own education and employment history but also about the educational attainment and occupation of their parents, spouses, siblings and children. In addition, persons interviewed for BASE were asked about the education of their grandchildren.[17] The data sets thus contain measures of occupational status for three consecutive generations and measures of educational attainment for up to four generations.

Across the four generations, the data sets span an historical episode of more than a century, and are thus a unique instrument for analysing intergenerational mobility in late 19th and 20th century Germany. Figure 1 gives an overview of the birth cohorts covered by the five samples. For each generation and sample, the figure plots the inner quartile range of the year of births (25th and 75th percentiles), along with the 10th, 50th and 90th percentiles indicated by additional vertical bars.

Along with their spouses, the actual respondents constitute the second or parent generation (G2) of our

[16] The labels LVS-1 and LVS-2 reflect the chronology of the cohorts' years of birth rather than the chronology of data collection. In fact, the LVS-2 data was collected before LVS-1. We do not use data for younger birth cohorts because their children have usually not completed their educational career at the time of data collection.

[17] The first part of the LVS-1, covering 407 respondents, also collected data on grandchildren. However, the question was dropped in the second part of the LVS-1 that covers 1005 respondents. We do not use the LVS-1 data on grandchildren because most of them had not finished school at the time of the interview.

analysis.[18] While the two LVS waves focus on cohorts born within narrow year bands, the oldest and youngest respondents in BASE are 35 years apart (see Figure 1). The parents of respondents, born on average in 1876 (BASE), 1889 (LVS-1), 1900 (LVS-2), 1916 (NEPS-1), and 1922 (NEPS-2) constitute the first or grandparent generation (G1), and the children of respondents, born on average in 1939 (BASE), 1950 (LVS-1), 1959 (LVS-2), 1975 (NEPS-1), and 1981 (NEPS-2) constitute the third or children generation (G3). The grandchildren of respondents, sampled only in BASE, are on average born in 1969. They constitute the fourth or grandchildren generation (G4).

Eliciting detailed life history data is less costly and time consuming if the data is collected retrospectively. However, retrospective data might suffer from recall bias, as respondents might not recall when an event happened or how exactly it took place. Furthermore, the reliability of retrospective data might decrease as respondents are asked to go further back in their family histories (Pfeffer, 2014). Measurement error should, however, only play a minor role in our analysis. First, our analysis focuses on the transmission of educational and occupational attainment. Retrospective surveys collect these dimensions of socio-economic status more reliably than other dimensions, such as income. Second, respondents were only asked to go back one generation in their family history, as they were asked about their parents but not their grandparents. Third, the quality of the retrospective data used in our study has been extensively evaluated, and its completeness and consistency has been improved by careful data editing (see Mayer, 2007 for a discussion). Finally, we note in Section 1.2 and Appendix A that plausible forms of measurement error, while leading to attenuation in the estimated autocorrelations β_1 and β_2, have little consequence for estimates of λ, our central parameter of interest.

2.1 Measures of Educational Attainment and Occupational Status

Our empirical analysis uses two different measures of educational attainment. The first measure counts only years of schooling. The second adds time spent in tertiary education or vocational training. The data sets generally record the highest school and vocational training degrees of an individual (LVS and NEPS also record the entire education history of index persons). We calculate years of education as the minimum time lengths required to obtain a particular degree.[19]

The BASE data set does not record educational attainment for grandmothers. Moreover, BASE only records school but not vocational training degrees for the grandfather, child, and grandchild generations. Consequently,

[18] While we do have detailed information on spouses, the data sets does not identify a specific spouse as the parent of an index person's child. Online Appendix D.3 describes the procedure that we use to link the spouses of index persons with their children.

[19] We take the minimum years of education required for a degree from Müller (1979). Online Appendix D.1 provides a detailed overview on how we mapped school, university, and vocational degrees into years of education. We keep this mapping constant over time, but our results remain robust to accounting for the introduction of a compulsory 9th grade after World War II.

we use years of schooling as our only measure for educational attainment in the analysis of the BASE data.

Some individuals of the younger generation did not yet complete schooling when the data were collected. This problem is relevant for the fourth generation in the BASE and the third generation in the LVS-2 sample, as the share of individuals still or not yet in school is 30.4% among the grandchildren of respondents in BASE, and 20.8% among the children of respondents in LVS-2. To avoid selectivity and to increase the sample size of our analysis, we generally use information on current school attendance to predict the final school degree of those individuals who are still in school and already attending secondary school (information on current school attendance is not available in LVS-1). At the age of ten, students in Germany are tracked into a high, medium, and low secondary school track. Changes between these different tracks are rather uncommon. The initial school track is, therefore, a strong predictor for the final school degree.

Our indicator for occupational status is the maximum occupational prestige score of an individual that we observe in the data. We base our analysis of occupational mobility on the LVS-1 and BASE samples only, as the LVS-2 and NEPS data do not contain information on the occupational status of the third generation. Moreover, our analysis is restricted to three generations, as the fourth generation was not old enough at the time of measurement for their occupational status to be informative about their long-run labour market success.

Both the LVS-1 and the BASE data record the occupation of the parents, spouses, and children of respondents at multiple points of their life cycles and document the entire occupational history of respondents themselves (see Online Appendix D.2 for details). The occupations are coded according to the three digit codes of the International Standard Classification of Occupations 1968 (ILO, 1969). Moreover, the data provide the occupational prestige score of each occupation, measured on the Magnitude-Prestige-Scale (MPS) (Wegener, 1985, 1988). The MPS is based on several prestige studies conducted in West Germany and ranges from 20 points (unskilled labourers) to 186.8 points (medical doctors). It is among the most commonly used prestige measures for Germany.

2.2 Descriptive Statistics

Table 1 reports, by generation, descriptive statistics for all five samples. Columns (2)-(5) report the mean birth year, educational attainment, and occupational prestige across generations and samples. The number in brackets is the share of non-missing observations. The final two columns report the total number of individuals in each group (counting also those with missing information), and the number of complete lineages for whom we observe educational attainment for at least one individual in the first three or all four generations.

The main reason for attrition of families is that individuals have no children. The LVS-1 (LVS-2) sample contains data on 1412 (708) respondents (see column (6)). Of those, 1218 (625) individuals have (biological) children. The share of respondents without children is slightly larger in the NEPS than in the LVS data. Childlessness is particularly pronounced in the BASE data, presumably reflecting the selective character of the sample (old individuals living in West Berlin). Of the 516 respondents in BASE, only 351 have children and 308 have grandchildren. In addition, information on the educational attainment of children is missing somewhat more frequently in the BASE data than in the other data sets.[20] The LVS-1 and LVS-2 samples contain data on 2515 and 1456 complete lineages across three generations, the BASE sample on 551 complete lineages, and the NEPS-1 and NEPS-2 samples on 2884 and 3263 complete lineages. The large number of observations allows us to be selective in our choice of sampling procedures, which we discuss in the next section.

Columns (3) and (4) of Table 1 show the mean and the share of non-missing observations of our two measures of educational attainment. For all five samples, we observe that time spent in education increases from one generation to the next. In the LVS-1, for instance, the first generation (born on average in 1889) spent on average 8.32 years in school (column (3)). Years of schooling increases to 8.77 years in the second generation (born on average in 1920) and to 9.80 years in the third generation (born on average in 1950). Along with education, occupational prestige also increases across generations.

However, the expansion of education came to an halt, and was even reversed, for the cohort born around 1930. This cohort (the second generation of the LVS-2 sample) was still in school during the final years of World War II and made the transition into the labour market in the immediate post-war period. The war severely reduced educational opportunities, as many schools were closed and apprenticeship position were lacking in the devastated economy (see, e.g., Müller and Pollak, 2004). As a consequence, the cohort born 1929-31 spent only 8.56 years in school and 9.95 years in school, university and vocational training, and thus considerably less than the cohort born ten years earlier (the second generation of the LVS-1 sample).

2.3 Lineages

The theoretical literature typically considers simplified one-parent one-offspring family structures, but in practice we face a varying number of lineages within each family. While of limited importance in two-generational studies, this issue becomes important in the multigenerational context. Two problems arise.

First, while we may follow both the matrilineal (all-female) or patrilineal (all-male) ancestors of an individual, most data sets do not cover all family members. For our analysis we could simply pool all observed lineages, or reduce the data to one observation for each pair of parents (e.g. their average or maximum status). But

[20] Almost 20% of all children born to the index persons surveyed by BASE died before their parent, many during World War II. For these children, information on their educational attainment is often missing.

the degree to which occupational or educational outcomes capture socio-economic status may differ between men and women, in particular for the earlier generations in our sample, in which female labour market participation was low. The correlation between occupational and educational measures is similar among men and women in the third generation, but substantially lower among women in the first two generations. Moreover, the observed parent-child correlations are lower for mothers than for fathers in our first generation for educational outcomes, and in the first two generations for occupational outcomes. For our analysis of educational outcomes, we therefore sample women in generations 2 and 3, but not in generation 1. For occupational outcomes, we sample women in generation 3 only and use male partners instead of female index persons in generation 2 (when observing their own parents is not required, i.e., for estimation of G2-G3 but not G1-G2 regressions). However, our results are similar when based on alternative sampling schemes, and we report a selection of estimates from patrilineal and matrilineal lineages in the Online Appendix (see Table 13).

Second, the number of children, and thus the number of observations per generation, varies across families. Figure 3 in the Online Appendix depicts a typical family tree over four generations to illustrate the problem. The family provides three observations for the estimation of mobility across four generations (e.g. GC1-P1, GC2-P1, GC3-P1), but these lineages are not equally distributed across family members in the third generation: two lineages pass through child 1 (C1), one through child 2 (C2), and none through child 3 (C3). If our objective is to predict mobility across four generations based on observed mobility in the first three, should we include those lineages that did not reproduce to the fourth generation, or weight those with multiple children accordingly? The answers to these questions matter, because the joint distribution of parental and offspring status varies substantially with subsequent fertility of the latter. Table 12 in the Online Appendix reports, conditional on the number of children of interviewees in the LVS-1, the mean years of schooling of respondents and their parents, and estimates of the intergenerational coefficient between the two generations. Interviewees with multiple children have substantially lower educational attainment, and a higher intergenerational coefficient, than those with one or no child.

Two-generational estimates may thus fail to predict multigenerational correlations even when intergenerational transmission does follow a simple autoregressive process *within* each lineage, simply because we extrapolated from the wrong *set* of lineages. We aim to distinguish this source for failure of the iterated regression procedure, related to sampling issues and heterogeneous fertility patterns, from fundamentally different theories of status transmission within families, such as those that we discussed in Section 1. One potential solution is to use the same set of lineages for all regressions, keeping the number of observations that each family tree contributes constant across generations. For example, the lineages printed in bold in Figure 3 contribute three observations to the estimation of two-, three- and four-generation coefficients, while the other lineages are excluded. We follow this approach in those parts of our analysis where the sample sizes are sufficiently large.

3 Direct Evidence on Multigenerational Persistence

This section presents our results on the transmission of educational attainment and occupational status over multiple generations, and compares our direct estimates to predictions derived from two-generation data. We first analyse the persistence across three generations and then study the transmission of educational inequality across four generations.

Three Generation Evidence. Table 2 reports regression coefficients to summarise the transmission of inequality across two and three generations. Panel A describes intergenerational dependency in educational attainment, measured in years of schooling, for each of our five samples. Panel B considers a broader measure of educational attainment that includes tertiary and vocational education for LVS-1 and NEPS-1 samples, and Panel C considers a measure of occupational prestige for LVS-1 and BASE.[21] For each case, we report (i) the intergenerational coefficients across two generations, (ii) the *actual* coefficient across three generations, and (iii) the *predicted* coefficient across three generations, as derived from the iteration of the two intergenerational measures (see Section 1). Table 3 reports the corresponding correlation coefficients, which abstract from changes in the variance of the outcome variable across generations. The comparatively large sample sizes allow us to estimate coefficients in a balanced sample, which includes only complete lineages.

A number of findings emerge from our analysis. First, our estimates corroborate earlier findings (see for example Shavit and Blossfeld, 1993, and Heineck and Riphahn, 2009) that in comparison to other OECD countries, the persistence of educational attainment across two generations is particularly strong in Germany. The average across all coefficient estimates on years of schooling is 0.501 for regression and 0.422 for correlation coefficients (Tables 2 and 3, Panel A), between 20 and 30% higher than the corresponding averages in recent evidence for Sweden in Lindahl *et al.* (2015). The coefficients are similar if we include time spent in vocational training and tertiary education in our educational measure (Panel B), and slightly lower in occupational prestige (Panel C). While the regression coefficients differ substantially across generations, the correlation coefficients are comparatively stable (consistent with evidence from other countries reported in Hertz *et al.*, 2008). This pattern implies that while there are important non-stationarities in the intergenerational process, they are partly

[21] We do not report estimates based on the broader measure of educational attainment for LVS-2 and NEPS-2, as this measure is systematically missing for later born children. However, these estimates, which are available upon request, are in line with the evidence that we present here.

due to changes in the variance of the marginal distributions.

Second, the comparatively high intergenerational persistence of educational attainment in Germany extends beyond two generations: the average estimate across three generations is 0.354 for regression and 0.251 for correlation coefficients, between 20 and 40% higher than comparable estimates for Sweden in Lindahl *et al.* (2015). Due to differential trends in cross-sectional inequality, the gap is particularly large in regression coefficients. Remarkably, the average coefficient estimate across *three* generations in Germany is nearly as high as the corresponding average across *two* generations in Sweden.

Third, the iteration of intergenerational measures substantially underpredicts the persistence of economic status. The actual three-generation estimates in schooling (Panel A) are on average about 40% higher than the predicted coefficients. The difference is statistically significant on the 1% level in the LVS-2, NEPS-1, and NEPS-2, and on the 10% level in the BASE sample (based on repeated sampling on the family level with 500 repetitions). This pattern extends to our broader measure of educational attainment (Panel B) and to matrilineal or patrilineal lineages (see Table 13 in the Online Appendix). Under-prediction is even more severe in the occupational prestige variable, in which the actual coefficient estimate is up to 70% larger than the predicted value (Panel C).

Our evidence is thus consistent with findings from other countries in the recent literature: in both low- and high-mobility countries, iteration of intergenerational measures can lead to a substantial under-prediction of the long-run persistence in educational inequality. Since iteration implies that observed cross-country differences in mobility grow exponentially across generations, the method also overstates differences between countries.

Four Generation Evidence. The BASE sample allows us to consider the transmission of inequality in schooling across four generations. Table 4 reports the corresponding regression and correlation coefficients. In contrast to our previous analysis for three generations, we now report estimates from an unbalanced sample that includes incomplete lineages, which do not extend over four generations. The differences between the two- and three-generation estimates in Table 4 and the corresponding entries in Tables 2 and 3 reflect thus the importance of sampling choices. As expected (see Section 2.3), these choices do matter, but the broad magnitude of individual estimates and their difference across two or three generations remains the same.

The inclusion of an additional generation yields direct estimates of persistence across four generations, and additional estimates across two and three generations, allowing us to test the performance of the iteration procedure in two additional cases. The evidence supports our previous conclusion: the iteration of intergenerational coefficients understates actual persistence by between 35% (correlation coefficients across first three) and 95% (regression coefficient across four generations). Actual persistence across four generations is not negligible, with an estimated regression coefficient of about 0.2 and a correlation coefficient of 0.16.

4 Testing Models of Multigenerational Transmission

4.1 Evidence on the Latent Factor Model

This section presents our evidence on the stark interpretation of multigenerational correlations that Clark (2014) has recently offered. In particular, Table 5 reports parameter estimates of the latent factor model that is underlying his arguments for each of our outcomes and samples. These parameter estimates are based on the inter- and multigenerational correlations reported in Table 3, and use the fact that such direct evidence on the individual level is sufficient to identify the model parameters (see Section 1).

The first two columns of Table 5 report the average of the two parent-child estimates $\hat{\beta}_{-1}$ (i.e., the average of the intergenerational correlations between G1 and G2, and between G2 and G3) and the grandparent-child estimate $\hat{\beta}_{-2}$. Parameter estimates in columns (3) and (4) are then given by $\hat{\lambda} = \hat{\beta}_{-2}/\hat{\beta}_{-1}$ and $\hat{\rho} = (\hat{\beta}^2_{-1}/\hat{\beta}_{-2})^{1/2}$. We compute bootstrapped standard errors by repeated resampling from the respective estimation sample on the family level. We compute the parameter estimates also for the comparable evidence on multigenerational correlations in Sweden from Lindahl *et al.* (2015), and report them in Panel D.

A number of implications follow from the comparison of these estimates across outcomes, the two countries, and time. First, in each case the estimated persistence of the latent factor λ is larger than the estimated parent-child correlation in status.[22] The difference is often substantial, in particular for the occupational status measure. Our evidence is therefore consistent with Clark's first hypothesis – that the observed intergenerational correlations understate the strength of the actual underlying transmission process and thus the degree of status persistence across multiple generations. Second, our estimates of λ are lower, and in some cases substantially lower, than the estimates that Clark derives from his analysis of rare and elite surnames. Our estimates for Germany range between 0.494 and 0.699, and do not support Clark's second hypothesis that λ is around 0.75. The estimates of λ for Sweden implied by the correlations reported in Lindahl *et al.* (2015) are lower as well.[23]

Both findings are robust to alternative estimation procedures. Column (5) of Table 5 reports estimates for λ that are based only on intergenerational correlations between G1 and G2. This alternative estimator is based on fewer sample moments, and thus has larger standard errors, but is more robust to plausible forms

[22] This observation follows directly from $\lambda = \beta_{-2}/\beta_{-1}$ and the fact that multigenerational correlations in both the German and Swedish data are characterised by excess persistence.

[23] Clark (2012) acknowledges the difference between his estimates and the evidence reported in Lindahl *et al.* (2015) but argues that the difference is not statistically significant. In our sample, we can reject the null hypothesis $\lambda = 0.75$ on the 1% level for schooling outcomes in the LVS-1, NEPS-1, and NEPS-2, and on the 5% level for schooling in BASE (based on a bootstrap procedure that redraws samples on the family level).

of measurement error in the data (see Appendix A for details). In particular, while our baseline estimator in column (3) remains consistent as long as the signal-to-noise ratio is similar across generations, the estimator in column (5) is consistent even if there are larger errors in the status of respondent's parents. However, estimates in column (5) are similar (and on average slightly *lower*) than the corresponding baseline estimates, suggesting that response errors in the first generation are not a large concern. Moreover, we note in Appendix B that estimators based on four instead of three generations in the BASE sample, which are robust to changes in ρ across generations, yield similar estimates as well.

Finally, our findings are also not supportive of Clark's third hypothesis – that the true rate of persistence is constant across time and space. The verdict is not as unambiguous: while parent-child correlations are lower in Sweden than in Germany, estimates of λ are relatively close to each other.[24] However, the differences across time within Germany are substantial. For schooling, our estimate of λ in the LVS-2 is more than 40% higher than in the LVS-1 and more than 20% higher than in the BASE sample.[25] This finding suggests that the true rate of social mobility is not constant, but subject to the environment. However, we cannot conclusively rule out that changes in ρ_t drive the observed differences in λ (see Appendix B for details).

Overall, therefore, we find support only for Clark's first main hypothesis. Nevertheless, the latent model can rationalise the finding that the iteration of intergenerational correlations persistently understates the longevity of inequality across multiple generations.

Figure 2 compares the degree of longevity implied by our estimates to the longevity implied by Clark's second hypothesis and by the iterated regression procedure. We plot (1) the observed correlations in educational attainment across two and three generations; (2) the predicted correlations according to the iteration of the average parent-child correlations; (3) the predicted correlations according to the latent factor model, based on parameter estimates reported in Table 5; (4) the predicted correlations based on Clark's hypothesis that $\lambda = 0.75$. We focus on the broad measure of educational attainment in the LVS-1, in which our estimate $\hat{\lambda}$ is 0.616 and thus close to the average estimate across all cases.

The iteration procedure suggests that individuals regress quickly to the mean; inequality is not sustained across many generations. In contrast, the latent model, together with our estimate $\hat{\lambda} = 0.616$, suggests that multigenerational correlations remain non-negligible over much longer time intervals, falling below 0.1 only in the sixth generation (compared to the fourth generation for the iteration procedure). As differences in λ

[24] Instead, estimates of ρ are lower in Sweden. This finding suggests that Sweden's higher mobility rates may be less due to differences in the actual intergenerational transmission process, but instead due to differences in the degree to which individuals' underlying endowments determine socio-economic status. Such pattern would be consistent with Clark's "universal constant" hypothesis.

[25] As the sample sizes are large, we can reject the hypothesis of equal heritability, $\lambda_{LVS-1} = \lambda_{LVS-2}$, on the 5% level ($p = 0.016$). The sample size in the BASE data is substantially smaller, but the p-value for the hypothesis that $\lambda_{LVS-2} = \lambda_{BASE}$ is still $p = 0.152$.

accumulate across generations, even apparently modest differences lead to substantially different long-run persistence: Under Clark's second hypothesis, $\lambda = 0.75$, the multigenerational correlation after eight generations is four times higher than under our estimate $\hat{\lambda} = 0.616$. Our evidence thus implies substantially lower longevity of socio-economic inequality than the recent evidence from surname studies reported by Clark.

4.2 Evidence on Grandparental Effects

The latent factor model provides a simple rationalisation for the observed persistence of status inequality across generations, but many studies in the recent literature focus on an alternative hypothesis: that grandparents have an independent causal effect on their grandchildren.

Following these studies, we regress, for each outcome and sample, offspring status on both father and grandfather status. The coefficient estimates are reported in the first two columns of Table 6. The coefficient on grandparent status is positive in all and statistically significant (on the 5% level) in seven of our nine cases. Its size is non-negligible, and its sign is in contrast to predictions from the Becker and Tomes model, in which the grandparent coefficient should be negative (see Solon, 2014). Similar findings have recently received a great deal of attention in economics and other fields, in particular in sociological and demographic research.

However, we have shown in Section 1 that the coefficient on grandparents in such child-parent-grandparent regressions has little meaning, as it will be positive under any process that generates excess persistence – such as the latent factor model. To test if the coefficient is just an artefact of a Markovian transmission process, we add the status of the mother as a control variable. If the positive grandparent coefficient reflects bias from omitting relevant parental characteristics, then it should decrease substantially once we condition on both parents' status – or be zero when the grandparent coefficient *only* reflected correlation between the status of the grandfather and the mother.

Indeed, this is what we observe for four of our five samples (see columns (3) to (5) in Table 6). For schooling variables in the LVS-1, BASE, and the two NEPS samples, it suffices to add information on education of the mother to push the estimated coefficient on grandparents close to zero. The coefficients are either statistically insignificant, or turn insignificant if we add additional information on the occupational prestige of parents (NEPS-2). We observe the same pattern for our wider measure of educational attainment and the occupational prestige score, which remains robust also to the inclusion of interaction terms between the status of father and grandfather.[26] The association between grandparent and offspring outcomes appears therefore spurious in these

[26] We add educational instead of occupational attainment of the mother also in regressions using occupational prestige as the outcome variable, since the occupational prestige score is less informative for females in the parent generation (see Section 2.3).

four samples.[27] Similar evidence against direct grandparental effects, presented by Warren and Hauser (1997) using the Wisconsin Longitudinal Study, have been challenged with the argument that the influence of grandparents may vary with context (see Section 1.3). The fact that we do not find evidence for multigenerational causal effects in four samples covering different cohorts in Germany suggests that the finding from the Wisconsin sample is not an outlier.

In contrast, the estimated coefficient on grandparent status in the LVS-2 declines only modestly and remains statistically significant when we control for maternal education (see Table 6). However, education is an imperfect measure of parental status, and our regression might miss other important parental control variables. We therefore include parental wealth, residential property, occupational prestige, and income as additional controls in the LVS-2 regression.[28] These variables explain a significant share of the variability in child schooling. However, while decreasing, the grandparent coefficient remains sizeable and statistically significant (see Panel A of Table 7). This finding suggests that in the LVS-2 sample, the grandparent coefficient indeed represents an independent statistical association that cannot be (fully) explained by the observed socio-economic status of parents. This is in contrast to our other four samples, and provides support for Mare's hypothesis that their grandparent status matters in some populations and time periods. In fact, in additional regression, we find that even within the LVS-2, the grandparent coefficient changes markedly over time: it is large for offspring born shortly after World War II, and declines in later birth cohorts (Panel B of Table 7, first row). This decline is near monotonic over cohort deciles, and remains robust to the inclusion of cohort dummies for each generation.[29]

But via which pathways might grandparent status matter? Mare (2011) lists various plausible mechanisms, and Hertel and Groh-Samberg (2013) note that an independent causal link may occur also indirectly, for example when the status of grandparents influences the reference point and decision making of grandchildren. To distinguish between the importance of direct and indirect channels, we test whether the positive grandfather coefficient is smaller for grandchildren whose grandfather died early. This is what we would expect if the positive grandfather coefficient would (partly) reflect the positive influence of grandchildren spending time with or

[27] In unreported regressions, we also find that the grandparent coefficient is generally much smaller if we include the observed status of the biological child of the grandparent rather than its spouse–the child-in-law of the grandparent. We would expect to see this pattern if the grandparent coefficient reflects correlation with parental outcomes, and the status correlation is stronger between the grandfather and his child rather than between the grandfather and his child-in-law. This holds for example in the latent factor model with assortative mating, as we discuss in Online Appendix C.3.

[28] Specifically, we include the occupational prestige score for both parents, a dummy for the ownership of residential property, and seven dummies indicating the level of parental household wealth. We also include an income fixed effect, which we construct from the income history of the index person by regressing the log average of the starting and final salary in job spells from age 20 onwards on a quadratic in age, time fixed effects, and individual fixed effects. Alternative income definitions yield similar results.

[29] One possible explanation for this observation is that grandparent status mattered in those families in which financial and human capital was scarce. This should apply, in particular, to young parents right after the war, and thus to the parent generation in the LVS-2 (who was born around 1930). Importantly, higher education was still comparatively costly at that time: academic-track schools charged substantial fees until about 1958, and means-tested financial support for university attendance was introduced only from 1969 onwards. In line with that explanation, we also find the coefficient on grandfather status to be large only in LVS-2 families in which the educational, income, or occupational status of parents is low (see Panel B of Table 7).

receiving resources directly from their highly-educated grandparents.

Panel A of Table 8 reports results from LVS-2 regressions that add various measures of grandfather death and interaction terms between grandfather death and parental and grandparental status to our child-father-grandfather regression, controlling also for the birth year of the grandfather. As a measure of grandfather death, the regression in column (1) uses a dummy that indicates whether the grandfather was already dead when the grandchild was born (which is the case for 27.5% of all grandchildren in LVS-2). The interaction term between grandfather death and grandparental schooling enters with the expected negative sign but the point estimate is small and statistically insignificant. However, estimates in (1) will be biased if the time of death is correlated with unobserved factors that in turn influence the intergenerational transmission coefficient. This seems likely as early death is, in general, not random. In fact, we show in column (1) of Panel B of Table 8 that grandfathers who die before the birth of their grandchildren are (perhaps surprisingly) *more* educated than grandparents who die later–and are thus a selected group of individuals.

To at least partly account for such selectivity, regressions (2) to (4) use war-related measures of grandfather death. The idea is simple: Many members of LVS-2's grandfather generation, born on average around the turn of the century, were deployed in World War II, and dying in the war is arguably less correlated with unobserved factors than dying early in general. Consequently, regressions (2) and (3) use a dummy indicating whether the grandfather died between 1939 and 1945 as a measure of grandfather death, and regression (4) a dummy indicating whether the grandfather was killed in combat or was missing in action since World War II.[30] Furthermore, regressions (3) and (4) restrict the sample to grandchildren of grandfathers who were absent from home because of World War II or died in the war. Importantly, war death is not correlated with grandparental schooling (see Panel B of Table 8). This suggests that the use of war-related measures of grandfather death can at least partly alleviate the selection problem. The interaction term between war death and grandparental schooling is negative in all three regressions but statistically insignificant.

A problem with the regressions in (2) to (4) is the small sample size–and the ensuing lack of variation in the interaction term between war death and grandparental schooling. Overall, we have observations on 611 grandchildren whose grandfathers were absent or died in the war. Of those, only a little over a fifth lost their grandfather in the war. Moreover, the large majority of individuals in the grandparent generation only completed compulsory schooling. We are, therefore, left with little variation in our interaction term.

We address this problem in two ways. First, we use the fact that for the grandparent generation, there is

[30]The two indicators differ because a small number of grandfathers, for which we do not have information that they died in combat, still died between 1939 and 1945. The first indicator treats these cases as war deaths, the second indicator as missings (as we cannot conclusively decide whether they died of natural causes).

considerably more variation in vocational and tertiary than in secondary education. We thus re-run specification (4) using our broader measure of educational attainment to measure their status. The interaction term between war death and grandparental education is again negative but now close to zero (see column (5) of Table 8). Second, we re-estimate specifications (1) to (5) in an extended sample that, in addition to the LVS-2, also contains the LVS-1. Unfortunately, we cannot estimate specifications (3) to (5) in the LVS-1, as it does not contain information on war deployment of grandfathers. Therefore, we also add the third wave of the LVS to the extended sample.[31] Table 14 in the Online Appendix presents the results. They again show no evidence that the grandfather coefficient is smaller for grandchildren whose grandfather died early.

Overall, we find strong evidence against grandparental effects for four of our five samples (LVS-1, BASE, NEPS-1, and NEPS-2). We thus conclude that higher-order causal effects are generally not a key factor for explaining the less-than-geometric decay of socio-economic status across generations that we observe. However, the association between grandfather status and child outcomes appears robust for post-war cohorts in the LVS-2. Grandparent status in this sample matters even for grandchildren whose grandfather died early, providing evidence for indirect mechanisms that do not require direct contacts between grandparents and their grandchildren.

5 Predicting Multigenerational Persistence: A Horse Race

The observation of a fourth generation in the BASE sample allows us to test the two models further. In Table 9, we compare the actual correlation coefficient across four generations with predictions that we derive from (i) the iteration of parent-child measures, (ii) the latent factor model, and (iii) a second-order autoregressive model with "grandparental effects". As data on the first three generations alone are sufficient to identify the parameters of these models (see Section 1), the fourth generation offers an opportunity to test their ability to fit the data. We estimate each model on the same set of lineages and report bootstrapped standard errors.

The actual correlation across four generations in BASE is 0.164 (see row 1 of Table 9). The next two rows show that the iteration of parent-child correlations substantially understates the longevity of inequality. It makes little difference if we iterate parent-child correlations across the first three generations only (row 2) or across all four generations (row 3), suggesting that the procedure's failure to fit the data is not caused by abnormal patterns in the last observed generation.

In contrast, the latent factor model (row 4) performs comparatively well. Its predicted correlation across four generations, according to Section 1 computed as $\hat{\lambda}^3\hat{\rho}^2 = 0.144$, is within 15% of the actual correlation.

[31] The LVS-3 surveys respondents who were born in 1939-1941, and contains exactly the same information as the LVS-2. We do not use the LVS-3 in our main analysis, since the educational outcomes of the children generation are heavily censored. However, the LVS-3 still seems useful for our analysis of grandparental causal effects, for which the overall degree of status persistence is less relevant.

Row 5 illustrates that the grandparental effects model does less well. We estimate the standardised coefficients in a regression of offspring on parent and grandparent education. The autocorrelation across four generations in a second-order autoregressive process with coefficients β_p and β_{gp} equals $(\beta_p^3 + 2\beta_p\beta_{gp} - \beta_p\beta_{gp}^2)/(1-\beta_{gp})$. With $\hat{\beta}_p = 0.374$ and $\hat{\beta}_{gp} = 0.073$, we obtain an autocorrelation of 0.112, underestimating the degree of long-run persistence in our sample by about 30%. The simple latent factor model thus fits the data well and outperforms the grandparental effect model. Of course, other models (see for example Solon, 2014) not tested here may provide an even better characterisation of intergenerational processes.

6 Conclusions

This paper has presented direct evidence on the persistence of occupational status and educational attainment across up to four generations in 19th and 20th century Germany. Consistent with recent evidence for Sweden, we find that social mobility in Germany is substantially lower than estimates from two generations suggest.

We use our data to shed light on two theories of multigenerational transmission that have recently gained a lot of attention. First, we address Gregory Clark's hypotheses that the true rate of social mobility is low and constant across countries and time, unaffected by the environment or policy. We show that multigenerational data offer a direct path for identification of the latent factor model that is underlying these arguments, a path that avoids some of the pitfalls that affect estimates from averaging outcomes within surname groups. Our evidence suggests that the persistence in the latent factor is substantially higher than the parent-child correlation in observed outcomes, but also that its rate varies over cohorts and is not as high as Clark suggests.

Second, we ask if an independent causal effect of grandparents may contribute to the observed longevity of status inequality across generations. We show that the coefficient on grandparent status in a regression of child status on parent and grandparent status has little meaning, as it will be positive for *any* process that generates persistence in excess of the rate implied by iterating two-generation measures. We find strong evidence against "grandparental effects" for four of our five cohort groups, but also a robust positive association between grandparent and children status for the fifth cohort. The positive association seems to operate through indirect mechanisms, as we show by exploiting quasi-exogenous variation in the time of grandparents' death.

Overall, therefore, we argue that the literature's traditional focus on parent-child transmission, and neglect of earlier ancestors, is not a significant obstacle for understanding the slow decline in multigenerational correlations that we document in the data. In fact, the latent factor model, despite having a memory of just one generation, can also account for the added persistence, and does a better job in predicting our data on the persistence in educational attainment across four generations than the grandparental effects model. However, our evidence speaks against a deterministic view of social mobility. The degree of inter- and multigenerational persistence in socio-economic status is surprisingly similar across our five samples, but still sufficiently different to suggest that the parameters of the latent factor model are not constant over time and space.

At a more general level, our paper illustrates how the increased availability of multigenerational data provides an opportunity to assess theoretical hypotheses on the transmission of inequality across generations. Such data cannot only be used to identify models of inter- and multigenerational mobility, but also to test their ability to explain the persistence of socio-economic inequality over long time horizons.

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Tables and Figures

Table 1: *Sample Statistics*

	birth year	schooling in years		occupational	# individuals	# lineages
		secondary	w/ vocational	prestige		3/4 generations
LVS-1						
Grandparents	1889	8.32 (0.88)	9.57 (0.78)	55.86 (0.71)	2824	2515 / 555
Parents	1920	8.77 (1.00)	10.33 (1.00)	66.67 (0.99)	1412	
Children	1950	9.80 (0.94)	12.41 (0.91)	69.04 (0.85)	2871	
LVS-2						
Grandparents	1900	8.34 (0.95)	9.55 (0.90)	-	1416	1456 / -
Parents	1930	8.56 (1.00)	9.95 (1.00)	-	708	
Children	1959	9.84 (0.94)	11.92 (0.74)	-	1577	
BASE						
Grandparents	1876	8.70 (0.41)	-	54.66 (0.60)	1032	551 / 463
Parents	1906	8.73 (1.00)	-	70.64 (0.98)	516	
Children	1939	9.96 (0.88)	-	72.72 (0.84)	741	
Grandchildren	1969	10.82 (0.68)	-	-	898	
NEPS-1						
Grandparents	1916	8.73 (0.89)	10.71 (0.94)	46.88 (0.85)	3886	2884 / -
Parents	1947	9.92 (0.93)	13.03 (1.00)	97.08 (1.00)	1943	
Children	1975	11.18 (0.98)	14.31 (0.92)	-	3090	
NEPS-2						
Grandparents	1922	8.73 (0.91)	10.78 (0.95)	50.29 (0.86)	4288	3263 / -
Parents	1952	10.13 (0.96)	13.21 (1.00)	96.51 (1.00)	2144	
Children	1981	11.17 (0.98)	13.99 (0.81)	-	3497	

Note: The table reports sample means of birth year, educational attainment with and without vocational training, and occupational prestige, along with the number of observations across samples. The number in brackets is the share of non-missing observations in the respective outcome. The last column reports the number of lineages for whom education data are available in each of three or four consecutive generations.

Table 2: *Regression Coefficient over Three Generations*

	Actual			*Predicted*	
	G1-G2	G2-G3	G1-G3	G1-G3	N
Panel A: Schooling					
LVS-1	0.709	0.563	0.434	0.399	2383
	(0.048)	(0.032)	(0.050)	(0.036)	
LVS-2	0.460	0.629	0.483	0.290	1389
	(0.066)	(0.039)	(0.056)	(0.044)	
BASE	0.468	0.547	0.342	0.256	547
	(0.101)	(0.062)	(0.074)	(0.061)	
NEPS-1	0.416	0.366	0.242	0.152	2508
	(0.033)	(0.022)	(0.023)	(0.016)	
NEPS-2	0.468	0.381	0.268	0.178	2934
	(0.027)	(0.021)	(0.022)	(0.015)	
Panel B: Schooling w/ vocational					
LVS-1	0.550	0.518	0.401	0.285	1869
	(0.039)	(0.033)	(0.046)	(0.028)	
NEPS-1	0.398	0.342	0.195	0.136	2574
	(0.029)	(0.023)	(0.025)	(0.014)	
Panel C: Occupational Prestige					
LVS-1	0.533	0.414	0.340	0.221	2261[a]
	(0.079)	(0.028)	(0.041)	(0.037)	
BASE	0.670	0.378	0.315	0.254	542[b]
	(0.120)	(0.052)	(0.060)	(0.060)	

Note: Balanced sample, using complete lineages in which the respective outcome is observed for individuals in all three generations. Standard errors clustered on family level in parentheses. [a]Only 929 observations for G2-G1 regression. [b]Only 313 observations for G2-G1 regression.

Table 3: *Correlation Coefficient over Three Generations*

	Actual			*Predicted*	
	G1-G2	G2-G3	G1-G3	G1-G3	N
Panel A: Schooling					
LVS-1	0.549	0.387	0.231	0.213	2383
	(0.042)	(0.026)	(0.028)	(0.022)	
LVS-2	0.432	0.406	0.293	0.175	1389
	(0.062)	(0.026)	(0.034)	(0.028)	
BASE	0.467	0.400	0.249	0.187	547
	(0.079)	(0.050)	(0.050)	(0.039)	
NEPS-1	0.372	0.384	0.226	0.143	2508
	(0.0302)	(0.0215)	(0.0206)	(0.0140)	
NEPS-2	0.425	0.399	0.255	0.170	2934
	(0.0235)	(0.0209)	(0.0193)	(0.013)	
Panel B: Schooling w/ vocational					
LVS-1	0.483	0.400	0.272	0.193	1869
	(0.036)	(0.028)	(0.031)	(0.019)	
NEPS-1	0.392	0.349	0.196	0.137	2574
	(0.027)	(0.022)	(0.026)	(0.013)	
Panel C: Occupational Prestige					
LVS-1	0.368	0.396	0.250	0.146	2261[a]
	(0.037)	(0.024)	(0.030)	(0.017)	
BASE	0.456	0.394	0.257	0.180	542[b]
	(0.088)	(0.045)	(0.049)	(0.041)	

Note: Estimates of the Pearson correlation coefficient. Balanced sample, using complete lineages in which the respective outcome is observed for individuals in all three generations. Bootstrapped standard errors clustered on family level in parentheses. [a]Only 929 observations for G2-G1 regression. [b]Only 313 observations for G2-G1 regression.

Table 4: *Regression and Correlation Coefficient over Four Generations*

2 Generations			3 Generations				4 Generations	
G1-G2	G2-G3	G3-G4	G1-G3		G2-G4		G1-G4	
			Actual	*Predicted*	*Actual*	*Predicted*	*Actual*	*Predicted*
Regression Coefficients:								
0.446	0.501	0.479	0.344	0.223	0.361	0.240	0.207	0.107
(0.057)	(0.050)	(0.049)	(0.070)	(0.037)	(0.048)	(0.037)	(0.067)	(0.022)
Correlation Coefficients:								
0.486	0.403	0.463	0.257	0.192	0.288	0.181	0.164	0.0871
(0.054)	(0.041)	(0.049)	(0.049)	(0.028)	(0.039)	(0.028)	(0.048)	(0.016)
N=413	N=1262	N=516	N=553		N=1025		N=470	

Note: Unbalanced sample from BASE, using all available observations. Bootstrapped standard errors clustered on family level in parentheses.

Table 5: *Parameter Estimates of the Latent Factor Model*

	(1) β_{-1}	(2) β_{-2}	(3) λ	(4) ρ	(5) λ_A
Panel A: Schooling					
LVS-1	0.468	0.231	0.494	0.974	0.421
	(0.026)	(0.027)	(0.044)	(0.045)	(0.043)
LVS-2	0.419	0.293	0.699	0.774	0.677
	(0.033)	(0.032)	(0.072)	(0.057)	(0.097)
BASE	0.434	0.249	0.574	0.869	0.534
	(0.047)	(0.078)	(0.095)	(0.085)	(0.108)
NEPS-1	0.378	0.226	0.598	0.795	0.609
	(0.020)	(0.022)	(0.054)	(0.044)	(0.065)
NEPS-2	0.412	0.255	0.619	0.816	0.600
	(0.017)	(0.021)	(0.042)	(0.032)	(0.045)
Panel B: Schooling w/ vocational					
LVS-1	0.442	0.272	0.616	0.847	0.563
	(0.023)	(0.032)	(0.058)	(0.043)	(0.060)
NEPS-1	0.370	0.196	0.530	0.836	0.501
	(0.019)	(0.024)	(0.058)	(0.049)	(0.059)
Panel C: Occupational Prestige					
LVS-1	0.382	0.250	0.654	0.764	0.681
	(0.033)	(0.027)	(0.072)	(0.062)	(0.125)
BASE	0.425	0.257	0.605	0.838	0.564
	(0.058)	(0.051)	(0.126)	(0.115)	(0.187)
Panel D: Evidence from Sweden (Lindahl et al., 2015)					
Schooling	0.353	0.216	0.611	0.760	
Earnings	0.288	0.141	0.490	0.767	

Notes: β-1 and β-2 are correlation coefficients. Estimates for Germany are from Table 3. Column (3) reports estimates for λ based on average intergenerational correlations, whereas column (5) reports estimates based on the intergenerational correlation between G1 and G2 only. The values for Sweden are taken from Tables 2 and 4 of Lindahl et al. (2015). Bootstrapped standard errors clustered on family level in parentheses.

Table 6: *The Grandparent Coefficient*

	(1)	(2)	(3)	(4)	(5)	
	w/o Mother			*w/ Mother*		
	Grandfather	Father	Grandfather	Father	Mother	N
Panel A: Schooling						
LVS-1	0.128**	0.459***	-0.020	0.319***	0.412***	2096
	(0.046)	(0.034)	(0.046)	(0.036)	(0.042)	
LVS-2	0.247***	0.516***	0.184**	0.422***	0.255***	1349
	(0.057)	(0.041)	(0.060)	(0.049)	(0.066)	
BASE	0.095	0.422***	0.024	0.299***	0.329***	528
	(0.075)	(0.071)	(0.071)	(0.081)	(0.097)	
NEPS-1	0.073**	0.326***	0.043	0.235***	0.198***	2192
	(0.026)	(0.023)	(0.024)	(0.025)	(0.025)	
NEPS-2	0.120***	0.319***	0.0477*	0.204***	0.278***	2669
	(0.023)	(0.022)	(0.022)	(0.023)	(0.024)	
Panel B: Schooling w/ vocational						
LVS-1	0.099*	0.500***	0.001	0.401***	0.306***	1446
	(0.050)	(0.036)	(0.049)	(0.040)	(0.044)	
NEPS-1	0.032	0.349***	0.005	0.270***	0.168***	2258
	(0.025)	(0.023)	(0.025)	(0.026)	(0.029)	
Panel C: Occupational Prestige						
LVS-1	0.187***	0.381***	0.074	0.266***	3.150***[a]	2007
	(0.044)	(0.032)	(0.047)	(0.035)	(0.877)	
BASE	0.130*	0.323***	0.028	0.165**	1.429[a]	512
	(0.062)	(0.058)	(0.059)	(0.057)	(1.462)	

Note: Balanced sample, using complete lineages in which all control variables are observed. Columns (1) and (2) report estimates from a regression of offspring status on father and grandfather status. Columns (3)-(5) add the respective maternal status (Panel A and B) or paternal and maternal schooling (Panel C). Standard errors are clustered on family level. * p<0.05, ** p<0.01, *** p<0.001. [a] Coefficient on maternal schooling (without vocational training).

Table 7: *Additional Evidence on the Grandparent Coefficient, LVS-2*

Panel A: Coefficient Robustness			
Grandfather coef.	0.247***	0.184**	0.130*
	(0.057)	(0.060)	(0.058)
maternal education	-	x	x
income, occ. prestige, wealth	-	-	x
Panel B: Coefficient Heterogeneity			
	yes	*no*	
early birth cohort	0.282***	0.081	
	(0.079)	(0.069)	
low education (father)	0.334*	0.017	
	(0.135)	(0.051)	
low income (respondent)	0.453***	0.071	
	(0.114)	(0.057)	
low occ. prestige (father)	0.258	0.126	
	(0.163)	(0.066)	

Note: Panel A reports the robustness of the grandfather coefficient in the LVS-2 as reported in Table 6 to the inclusion of further control variables. Panel B studies its heterogeneity over birth cohorts and with respect to the socio-economic status of parents. We distinguish G3 birth cohorts below the 50th percentile, fathers with only minimum (8 years) schooling, respondents with income fixed effect below the 25th percentile, and fathers with occupational prestige score below the 25th percentile. Standard errors clustered on family level in parentheses, * p<0.05, ** p<0.01, *** p<0.001.

Table 8: *Variation in the Grandparent Coefficient by Grandparent Survival, LVS-2*

Panel A	**Schooling Child (G3)**				
	(1)	(2)	(3)	(4)	(5)
Indicator grandfather death	*At birth*	*B/w 1939-45*	*B/w 1939-45*	*War death*	*War death*
Conditional on absence/death during WWII?	*No*	*No*	*Yes*	*Yes*	*Yes*
Schooling					
Grandfather	0.270***	0.252***	0.225***	0.223***	0.152***
	(0.062)	(0.055)	(0.075)	(0.076)	(0.048)
× Grandfather death	-0.048	-0.075	-0.046	-0.114	-0.015
	(0.117)	(0.224)	(0.231)	(0.272)	(0.166)
Father	0.506***	0.499***	0.423***	0.404***	0.379***
	(0.046)	(0.044)	(0.074)	(0.076)	(0.077)
× Grandfather death	-0.001	0.066	0.143	0.711	0.670
	(0.090)	(0.118)	(0.133)	(0.608)	(0.523)
Grandfather death	0.180	-0.184	-1.184	-4.993	-5.542
	(0.826)	(1.593)	(1.668)	(3.810)	(3.472)
Panel B	**Schooling Grandfather (G1)**				
	(1)	(2)	(3)	(4)	(5)
Indicator grandfather death					
At birth	0.254*				
	(0.140)				
B/w 1939-45		0.075	-0.197		
		(0.204)	(0.240)		
War death				0.140	-0.025
				(0.409)	(0.747)
# obs.	1317	1317	611	532	515

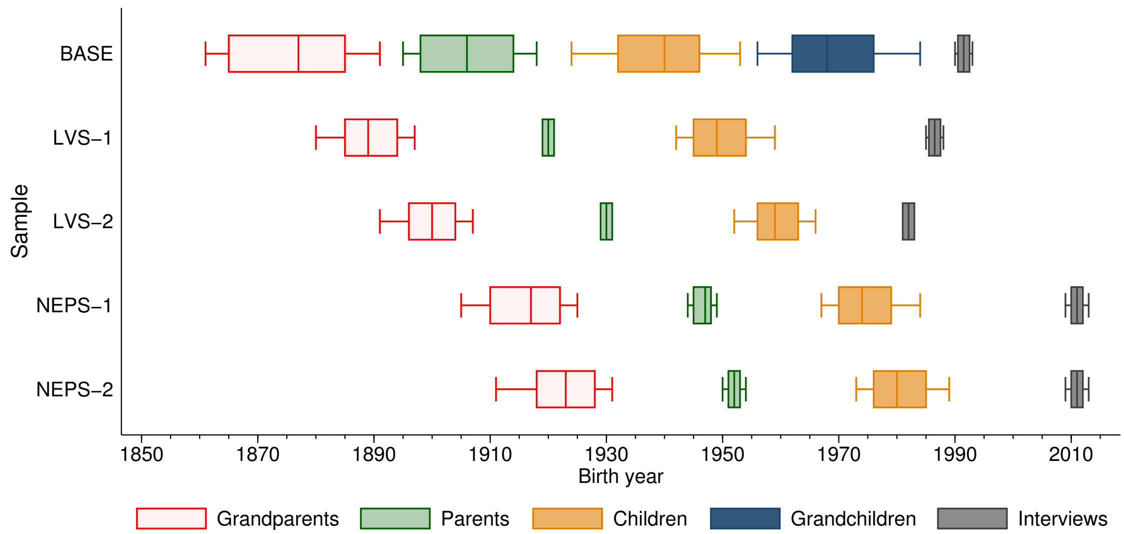
Notes: Panel A reports estimates from a regression of child schooling on father and grandfather schooling. All regressions in Panel A include a dummy for grandfather death, interaction terms between grandfather death and father/grandfather schooling, and a quadratic polynomial in the (hypothetical) age of the grandfather in 1988. Panel B reports estimates from a regression of grandfather schooling on an indicator of grandfather death and a quadratic polynomial in the (hypothetical) age of the grandfather in 1988. As an indicator for grandfather death, regression (1) uses a dummy indicating whether the grandfather was already dead when his grandchild was born, regressions (2) and (3) a dummy indicating whether the grandfather died between 1939 and 1945, and regressions (4) and (5) a dummy indicating whether the grandfather was killed during World War II or was missing in action since then. Regressions (3) to (5) restrict the sample to observations from G3 whose grandfather was absent because of World War II or died in the war. Regression (5) uses schooling with vocational training instead of just schooling as the education variable of the grandfather. Standard errors clustered on family level in parentheses, * p<0.10, *** p<0.01.

Table 9: *Predictions of the Correlation Coefficient across Four Generations*

		schooling (BASE)	
		coefficient	deviation
Actual	Four Generations	0.164	
		(0.053)	
Predictions	Iterative[a]	0.081	-50.7%
		(0.030)	
	Iterative, Four Generations[b]	0.085	-48.4%
		(0.023)	
	Latent Factor Model[a]	0.144	-12.7%
		(0.049)	
	Grandparent Effects[a]	0.112	-31.6%
		(0.041)	
# obs (G1-G3)		547	

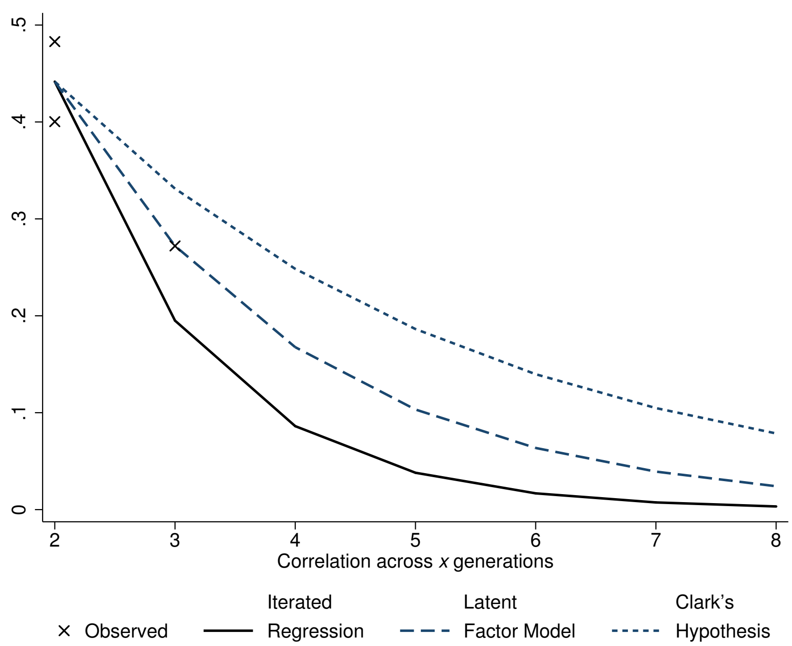
Note: Estimates of the Pearson correlation coefficient in the BASE sample. [a]Prediction based on complete lineages across the first three generations. [b]Prediction based on complete lineages across first three plus unbalanced fourth generation. Standard errors are bootstrapped on family level.

Figure 1: *Samples and Generations*



Note: For each generation and sample, the Figure plots the inner quartile range (25th and 75th percentiles), with the 10th, 50th and 90th percentiles indicated by additional vertical bars. Spouses of index persons not included.

Figure 2: *Predictions from the Iterated Regression vs. Latent Factor Model*



Note: The Figure plots (i) the observed correlation in educational attainment (with vocational training) across two and three generation and the predicted correlations according to (ii) the iteration of the average two-generation correlation (solid line); (iii) the latent factor model, identified from individual-level data (dashed line, $\hat{\lambda} = 0.616$); and (iv) Clark's hypothesis (short-dashed line, $\lambda = 0.75$).

A Measurement error and recall bias

To assess the consequences of response errors, let reported status $\tilde{y}_{i,t}$ relate to actual status $y_{i,t}$ according to

$$\tilde{y}_{i,t} = y_{i,t} + \varepsilon_{i,t}. \tag{15}$$

We allow for the variance of the response error $\varepsilon_{i,t}$ to vary across generations, but assume that errors are uncorrelated with true status ("classical" measurement error). As our analysis of Clark's latent factor model is based on correlation coefficients, we standardise the variance of $y_{i,t}$ to one.

The correlation between the observed (with error) status of any two generations then identifies

$$Cor(\tilde{y}_t, \tilde{y}_{t-m}) = \frac{Cov(\tilde{y}_t, \tilde{y}_{t-m})}{\sqrt{Var(\tilde{y}_t)}\sqrt{Var(\tilde{y}_{t-m})}} = \lambda^m \rho^2 rr_{t,t-m}, \tag{16}$$

where

$$rr_{t,t-m} = \frac{1}{\sqrt{(1+Var(\varepsilon_t))}} \frac{1}{\sqrt{(1+Var(\varepsilon_{t-m}))}}$$

depends on the response errors in the respective generations. We are underestimating the correlation in education or occupations across generations in the presence of measurement error (if $rr_{t,t-m} < 1$).

Our baseline estimate of λ, as reported in column (3) of Table 5, is based on both two-generation correlations, $Cor(\tilde{y}_t, \tilde{y}_{t-1})$ and $Cor(\tilde{y}_{t-1}, \tilde{y}_{t-2})$, that we observe in our three generation data:

$$\frac{Cor(\tilde{y}_t, \tilde{y}_{t-2})}{\frac{1}{2}Cor(\tilde{y}_t, \tilde{y}_{t-1}) + \frac{1}{2}Cor(\tilde{y}_{t-1}, \tilde{y}_{t-2})} = \lambda \frac{rr_{t,t-2}}{\frac{1}{2}(rr_{t,t-1} + rr_{t-1,t-2})}. \tag{17}$$

The estimate is thus consistent if the variance of the response error is stable across generations (so that $rr_{t,t-1} = rr_{t-1,t-2} = rr_{t,t-2}$). However, respondents in the parent generation $t-1$ may know their own and their child's education, $y_{i,t-1}$ and $y_{i,t}$, but have less precise knowledge of their parent's education $y_{i,t-2}$ (as they do not directly observe the educational careers of their parents). In this case, $\frac{1}{2}(rr_{t,t-1} + rr_{t-1,t-2}) > rr_{t,t-2}$ and estimates based on equation (17) are downward biased. An alternative estimator, reported in column (5) of Table 5, is given by

$$\lambda_A = \frac{Cor(\tilde{y}_t, \tilde{y}_{t-2})}{Cor(\tilde{y}_{t-1}, \tilde{y}_{t-2})} = \lambda \frac{rr_{t,t-2}}{rr_{t-1,t-2}}. \tag{18}$$

It is based on fewer moments but consistently estimates λ as long as the response errors in the parent generation $t-1$ and child generation $t-2$ have equal variance. This assumption is plausible, since respondents can directly observe attainment in these generations, such that the errors should be small in both cases. Note that we could abstract entirely from measurement error by estimating λ from regression instead of correlation coefficients (i.e., from the regression of y_t on y_{t-1}, instrumented by y_{t-2}). However, correlation coefficients have the important advantage of being robust to proportional shifts in the variance of status across generations (see Appendix B).

B The latent factor model with time-varying coefficients

Consider a generalisation of the latent factor model with time-varying coefficients, assuming that

$$y_{i,t} = \delta_t \left(\rho_t e_{i,t} + u_{i,t}\right) \tag{19}$$

$$e_{i,t} = \gamma_t \left(\lambda_t e_{i,t-1} + v_{i,t}\right), \tag{20}$$

where $Var(u_{i,t}) = (1-\rho_t^2)Var(e_{i,t})$ and $Var(v_{i,t}) = (1-\lambda_t^2)Var(e_{i,t-1})$. The parameters δ_t and γ_t allow for changes in the variances of $y_{i,t}$ and $e_{i,t}$, while ρ_t and λ_t reflect the relative importance of their deterministic and stochastic components. The coefficient in a regression of child status in generation t on parent status in generation $t-1$ equals then

$$\frac{Cov(y_t, y_{t-1})}{Var(y_{t-1})} = \frac{\delta_t}{\delta_{t-1}} \gamma_t \rho_t \rho_{t-1} \lambda_t, \tag{21}$$

while the correlation coefficient equals

$$Cor(y_t, y_{t-1}) = \frac{Cov(y_t, y_{t-1})}{\sqrt{Var(y_t)}\sqrt{Var(y_{t-1})}} = \rho_t \rho_{t-1} \lambda_t. \tag{22}$$

Consistent with Hertz *et al.* (2008), we find substantial variation in the regression coefficient while the correlation is comparatively stable across samples and generations. We can thus abstract from an important source of time variation by considering the latter.

In this more general model, the ratios between three- and each of the two-generational correlations identify

$$\lambda_A = \frac{Cor(y_1, y_3)}{Cor(y_2, y_3)} = \frac{\rho_1}{\rho_2} \lambda_2 \tag{23}$$

$$\lambda_B = \frac{Cor(y_1, y_3)}{Cor(y_1, y_2)} = \frac{\rho_3}{\rho_2} \lambda_3, \tag{24}$$

and the ratio between the three- and the *average* two-generational correlations identifies

$$\bar{\lambda} = \frac{Cor(y_1, y_3)}{\frac{1}{2}Cor(y_1, y_2) + \frac{1}{2}Cor(y_2, y_3)} \approx \frac{\frac{1}{2}(\rho_1 + \rho_3)}{\rho_2} \frac{1}{2}(\lambda_2 + \lambda_3), \tag{25}$$

where we now use, for simplicity, subscripts 1, 2, 3 to refer to generation G1, G2, and G3, respectively. We report estimates of $\bar{\lambda}$ as our baseline in Section 4. Estimates of λ_A, which are more robust to plausible forms of measurement error (see Appendix A), are reported in column (5) of Table 5.

Equations (23) to (25) illustrate that our estimates of the heritability parameter λ can be down- or upward biased if the correlation between the latent factor and observed status ρ changes across generations. In particular, we may underestimate λ if ρ_t is exceptionally *high* in our index generation G2. A number of observations address this concern. First, our arguments are based on five distinct samples, comprising cohorts born in different times, and multiple status measures. It seems unlikely that ρ_2 is substantially larger than $\frac{1}{2}(\rho_1 + \rho_3)$ in each case. In fact, ρ_2 may have been comparatively *low* in the LVS-2, since educational and vocational careers of cohorts born 1929-31 were directly interrupted by World War II and the post-war displacement of ethnic Germans from the former eastern territories of Germany. Second, even with time-varying coefficients we can point identify one of the heritability parameters if four generations are observed, as

$$\lambda_C = \sqrt{\frac{Cor(y_1, y_3)\, Cor(y_2, y_4)}{Cor(y_1, y_2)\, Cor(y_3, y_4)}} = \sqrt{\frac{(\rho_1 \rho_3 \lambda_2 \lambda_3)\,(\rho_2 \rho_4 \lambda_3 \lambda_4)}{(\rho_1 \rho_2 \lambda_2)\,(\rho_3 \rho_4 \lambda_4)}} = \lambda_3 \tag{26}$$

$$\lambda_D = \sqrt{\frac{Cor(y_1, y_4)\, Cor(y_2, y_3)}{Cor(y_1, y_2)\, Cor(y_3, y_4)}} = \sqrt{\frac{(\rho_1 \rho_4 \lambda_2 \lambda_3 \lambda_4)\,(\rho_2 \rho_3 \lambda_3)}{(\rho_1 \rho_2 \lambda_2)\,(\rho_3 \rho_4 \lambda_4)}} = \lambda_3. \tag{27}$$

Estimating these expressions using four generations of educational attainment in the BASE sample, we find $\hat{\lambda}_C = 0.617$ (bootstrapped s.e. 0.088) and $\hat{\lambda}_D = 0.546$ (s.e. 0.106). These estimates are of similar magnitude to those reported in Table 5, and the null hypothesis $\lambda = 0.75$ can still be rejected on the 10% level.

Clark's second hypothesis, that the heritability of the latent factor is constant across time ($\lambda_t = \lambda \; \forall t$) and space is more difficult to assess. A latent factor model with constant coefficients ($\lambda_t = \lambda$ *and* $\rho_t = \rho \; \forall t$), as posited in Clark and Cummins (2014), can be rejected from the evidence summarised in Table 5. However, equations (23) to (25) illustrate that differences in $\hat{\lambda}$ can also be due to differential trends of ρ_t across generations. Two observations suggest that variation *only* in ρ_t is unlikely to explain our results. First, the observed differences in $\hat{\lambda}$ across samples are large, and thus consistent with the hypothesis $\lambda_t = \lambda$ only if ρ_t varies strongly across generations. Second, the variation in ρ_t would need to be of peculiar form to explain the contrast in the estimated autocorrelations in schooling between the LVS-1 and LVS-2. The three-generation estimate $\hat{\beta}_{-2}$ is larger but the two-generation estimates $\hat{\beta}_{-1}$ are smaller in the LVS-2. Without variation in λ_t, this contrast can be rationalised only if ρ_1 and ρ_3 are large, but ρ_2 particularly small in the LVS-2. While possible, we deem such pattern less likely than the alternative explanation, that λ_t is not constant over time.

ONLINE APPENDIX–NOT FOR PUBLICATION

C Theory

C.1 The grandparent coefficient under non-stationarity

Proposition: In a multivariate regression of child outcome y_t on parent outcome y_{t-1} and grandparent outcome y_{t-2}, the coefficient on the latter is positive if and only if the iteration of parent-child coefficients understates the observed persistence across three generations.

Without assuming stationarity, the grandparent coefficient equals (Frisch-Waugh-Lovell theorem)

$$\beta_{gp} = \frac{Cov(y_t, \tilde{y}_{t-2})}{Var(\tilde{y}_{t-2})}, \tag{28}$$

where $\tilde{y}_{t-2}$ is the residual from regressing y_{t-2} on y_{t-1}. As such we have

$$\tilde{y}_{t-2} = y_{t-2} - \frac{Cov(y_{t-1}, y_{t-2})}{Var(y_{t-1})} y_{t-1}$$

and we can write

$$\beta_{gp} = \left(\frac{Cov(y_t, y_{t-2})}{Var(y_{t-2})} - \frac{Cov(y_{t-1}, y_{t-2})}{Var(y_{t-1})} \frac{Cov(y_t, y_{t-1})}{Var(y_{t-2})} \right) \frac{Var(y_{t-2})}{Var(\tilde{y}_{t-2})} = \left(\beta_{-2} - \beta_{-1}^{gp \to p} \beta_{-1}^{p \to c} \right) \frac{Var(y_{t-2})}{Var(\tilde{y}_{t-2})}, \tag{29}$$

where $\beta_{-1}^{gp \to p}$ and $\beta_{-1}^{p \to c}$ are the two-generational slope coefficient in a regression of parent on grandparent, or child on parent outcome, respectively. We have $\beta_{gp} > 0$ if and only if $\beta_{-2} > \beta_{-1}^{gp \to p} \beta_{-1}^{p \to c}$.

C.2 The grandparent coefficient in a latent factor model with multiple status measures

Assume that multiple distinct outcomes $\{y_{i1,t-1}, y_{i2,t-1}, ...\}$ of offspring in family i in generation t are determined by

$$y_{ij,t} = \rho e_{i,t} + u_{ij,t} \; \forall j \tag{30}$$

$$e_{i,t} = \lambda e_{i,t-1} + v_{i,t}, \tag{31}$$

where the noise terms are uncorrelated with each other and past values. The variances of the outcomes and the latent variable $e_{i,t}$ are normalised to one. Suppressing the i subscript, the grandparent coefficient β_{gp} in the multivariate child-parent-grandparent regression

$$y_t = \beta_p y_{1,t-1} + \beta_{gp} y_{t-2} + \varepsilon_t$$

equals $\beta_{gp} = Cov(y_t, \tilde{y}_{t-2})/Var(\tilde{y}_{t-2})$, where $\tilde{y}_{t-2}$ is the residual from regressing y_{t-2} on $y_{1,t-1}$. The slope coefficient in this auxiliary regression equals $\beta = \rho^2\lambda$, such that substituting for $\tilde{y}_{t-2} = y_{t-2} - \beta y_{1,t-1}$ yields

$$\beta_{gp} = \frac{Cov(y_t, y_{t-2}) - \beta Cov(y_t, y_{1,t-1})}{Var(y_{t-2} - \beta y_{1,t-1})} = \frac{\rho^2\lambda^2 - \rho^4\lambda^2}{1 - \rho^4\lambda^2}.$$

Similarly, the grandparent coefficient β_{gp} in the regression

$$y_t = \beta_{1,p} y_{1,t-1} + \beta_{2,p} y_{2,t-1} + \beta'_{gp} y_{1,t-2} + \varepsilon_t$$

equals $\beta'_{gp} = Cov(y_t, \tilde{y}'_{t-2})/Var(\tilde{y}'_{t-2})$, where $\tilde{y}'_{t-2}$ is the residual from regressing y_{t-2} on $y_{1,t-1}$ and $y_{2,t-1}$. From equation (30), the two slope coefficients in this auxiliary regression are identical and given by $\tilde{\beta} = \rho^2\lambda/(1+\rho^2)$. Substituting for $\tilde{y}'_{t-2} = y_{t-2} - \tilde{\beta}(y_{1,t-1} + y_{2,t-1})$, we thus have

$$\beta'_{gp} = \frac{Cov(y_t, y_{t-2}) - \tilde{\beta} Cov(y_t, y_{1,t-1} + y_{2,t-1})}{Var(y_{t-2}) + \tilde{\beta}^2 Var(y_{1,t-1} + y_{2,t-1}) - 2\tilde{\beta} Cov(y_{t-2}, y_{1,t-1} + y_{2,t-1})} = \frac{\rho^2\lambda^2 - \rho^4\lambda^2}{1 - \rho^4\lambda^2 + \rho^2(1 - \rho^2\lambda^2)},$$

and $\beta'_{gp} < \beta_{gp}$ if $0 < \rho < 1$, i.e., if observed status is an imperfect measure of the underlying latent factor.

C.3 The grandparent coefficient in a latent factor model with assortative mating

Assume that endowments are determined by the average of father's and mother's endowment

$$y_{i,t} = \rho e_{i,t} + u_{i,t} \tag{32}$$

$$e_{i,t} = \tilde{\lambda} \bar{e}_{i,t-1} + v_{i,t}, \tag{33}$$

with $\bar{e}_{i,t-1} = (e^m_{i,t-1} + e^p_{i,t-1})/2$, and where the m and p supercripts denote maternal and paternal variables. Moreover, assume that parents match based on their latent factors,

$$e^m_{i,t-1} = m e^p_{i,t-1} + w_{i,t-1} \tag{34}$$

with $m = Cov(e^m_{i,t-1}, e^p_{i,t-1})$.

Own lineage. The grandparent coefficient in a regression of offspring status on parent and grandparent status from the *same* lineage (i.e., father and paternal grandparent, or mother and maternal grandparent)

$$y_t = \beta_p y^x_{t-1} + \beta_{gp} y^{x,y}_{t-2} + \varepsilon_t \quad \text{for } x = \{m, p\},\, y = \{m, p\} \tag{35}$$

equals $\beta_{gp} = Cov(y_t, \tilde{y}_{t-2})/Var(\tilde{y}_{t-2})$, where $\tilde{y}_{t-2}$ is the residual from regressing $y^{x,y}_{t-2}$ on y^x_{t-1}. The slope coefficient in this auxiliary regression equals $\beta = \rho^2\lambda$ (see Section 1.2), where λ is given by equation (9), and therefore

$$\beta_{gp} = \frac{\rho^2\lambda^2 - \rho^4\lambda^2}{1 - \rho^4\lambda^2}. \tag{36}$$

Different lineages. The grandparent coefficient in a regression of offspring status on parent and grandparent status from *different* lineages (i.e., father and maternal grandparent, or mother and paternal grandparent),

$$y_t = \beta'_p y^x_{t-1} + \beta'_{gp} y^{y,z}_{t-2} + \varepsilon_t \quad \text{for } x = \{m, p\},\, y \neq x, \text{ and } z = \{m, p\} \tag{37}$$

equals $\beta'_{gp} = Cov(y_t, \tilde{y}^{y,z}_{t-2})/Var(\tilde{y}^{y,z}_{t-2})$, where $\tilde{y}^{y,z}_{t-2}$ is the residual from regressing $y^{y,z}_{t-2}$ on y^x_{t-1}. The slope coefficient in this auxiliary regression equals $\beta = m\rho^2\lambda$ (see Section 1.2), such that

$$\beta'_{gp} = \frac{\rho^2\lambda^2 - m\rho^4\lambda^2}{1 - m^2\rho^4\lambda^2}. \tag{38}$$

We have that $\beta'_{gp} > \beta_{gp}$ if status is imperfectly correlated with underlying endowments ($0 < \rho < 1$), assortative mating is imperfect ($0 \leq m < 1$), and intergenerational transmission is non-zero ($0 < \lambda \leq 1$).

Both parents. The grandparent coefficient in a regression on the status of grandparent and *both* parents,

$$y_t = \beta_x y^x_{t-1} + \beta_y y^y_{t-1} + \beta''_{gp} y^{x,z}_{t-2} + \varepsilon_t \quad \text{for } x = \{m, p\},\, y = \{m, p\},\, x \neq y \text{and } z = \{m, p\}, \tag{39}$$

equals $\beta''_{gp} = Cov(y_t, \tilde{y}^{x,z}_{t-2})/Var(\tilde{y}^{x,z}_{t-2})$, where $\tilde{y}^{x,z}_{t-2}$ is the residual from regressing $y^{x,z}_{t-2}$ on y^x_{t-1} and y^y_{t-1}. The slope coefficient in this auxiliary regression can be shown to equal $\left(\rho^2\lambda - m^2\rho^4\lambda\right)/\left(1 - m^2\rho^4\right)$ on y^x_{t-1} and $\left(m\rho^2\lambda - m\rho^4\lambda\right)/\left(1 - m^2\rho^4\right)$ on y^y_{t-1}. After simplification, we have

$$\beta''_{gp} = \frac{\rho^2\lambda^2(\rho^2 - 1)(m\rho^2 - 1)}{1 - m^2\rho^4 + \rho^4\lambda^2\left(m^2(2\rho^2 - 1) - 1\right)} \tag{40}$$

where $\beta''_{gp} < \beta'_{gp}$ if $0 < \rho < 1$, $0 \leq m \leq 1$, and $0 < \lambda \leq 1$.

D Data

D.1 Educational attainment

The data sets generally provide the highest school degree and the highest vocational training degrees that an individual has obtained (if any). From this information, we calculate years of schooling as the minimum lengths of time required to earn a given school degree. So as to obtain our measure of total years of education, we further add the minimum years required to complete a given vocational training degree to the years spent in school. Table 10 shows the minimum time lengths that we use to calculate our education measures (taken mainly from Müller, 1979). In our analysis of the NEPS, we directly use the variable on years of education provided in the data set. This variable is again based on an individual's highest school and vocational training degrees. We adjust the NEPS education variable to our mapping in Table 10 by recoding the minimum years of education from nine to eight years and, hence, abstract from the introduction of a compulsory 9th grade after World War II. Our results in Sections 3 and 4 remain nearly identical if we do not adjust the NEPS education variable.

Table 10: *Minimum lengths of time required to earn a given degree*

Degree	Minimum time length
School Degree	
No completed school degree	8 years
Sonderschulabschluss (special needs school)	8 years
Volks-/Hauptschulabschluss (low school track)	8 years
Mittlere Reife (medium school track)	10 years
Fachhochschulreife (high school track)	12 years
Abitur (high school track)	13 years
Vocational Training Degree	
No vocational degree	0 years
Agricultural or household apprenticeship	2 years
Industrial apprenticeship	2 years
Vocational school degree	2 years
Commercial apprenticeship	3 years
Master craftsman	4 years
University of applied sciences degree	4 years
University degree	5 years
Other vocational training degree	2 years

D.2 Occupational status

Our indicator for occupational status is the maximum occupational prestige score of an individual that we observe in the data. The data sets record the prestige score for the different groups of family members at multiple but different points of their life cycles, as shown in Table 11 for the LVS-1 and BASE data.

Table 11: *Points of the life cycle at which occupational status is recorded*

Generation	Relation to index person	LVS-1	BASE
First	Father	Occupation learned; occupation when index person was 15 years old; last occupation before retirement or death	Occupation when index person was 15 years old
	Mother	Occupation learned; main occupation until index person was 16 years old	Occupation learned
Second	Index person	Entire occupation history	Entire occupation history
	Spouse	Occupation learned; occupation before marriage; occupation during marriage; current occupation (entire occupation history)[a]	Occupation learned; occupation before marriage; occupation during marriage; current occupation (entire occupation history)
Third	Children	Main occupation	Main occupation

Notes: [a]The LVS-1 contains data on the entire occupation history of the spouses of those 407 index persons who were surveyed using face-to-face interviews.

D.3 Linking spouses and children

The data sets generally records information on the current spouse or partner of the index person and on all previous spouses (but not on previous partners with whom the index person was not married). Information include the birth year, educational attainment, occupational status and period of marriage or partnership.[32] However, the data sets do not identify a specific spouse as the parent of a specific child of the index person. We link spouses to children according to the following set of rules (which we apply one after the other):

1. If an index person has only one spouse, we identify this spouse as the parent of all children of the index person.

2. If an index has more than one spouse, we identify that spouse (partner) as the parent of a child with which the index person was married (in a partnership) at the time of birth.

3. If an index has two (three) spouses, we identify the first spouse as the parent of a child if the child was born before the first marriage. We identify the second (third) spouse as the parent of a child if the child was born after the index person broke up with the first (second) spouse but before she or he broke up with the second (third) spouse.

We cannot link spouses and children if a) the index person has more than one spouse and b) the birth year of a child is missing.

[32]The LVS-1 does not contain information on the educational attainment or occupational status of previous spouses.

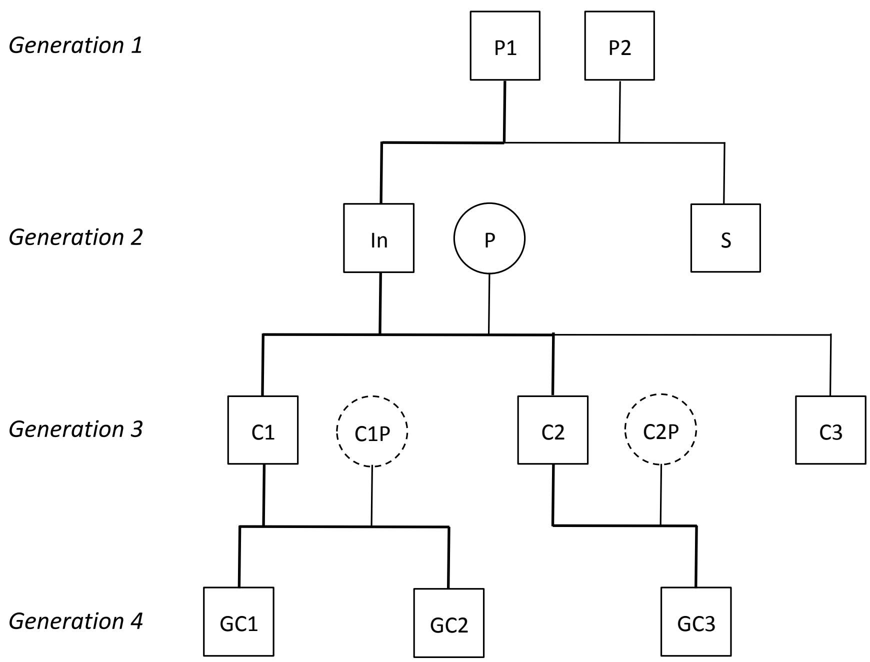
D.4 Lineages

Table 12: *Variation in Educational Attainment and Mobility with Family Size*

	# children		
	none	one	multiple
schooling /w vocational			
mean parents	8.66	8.47	8.36
	(0.125)	(0.069)	(0.040)
mean respondents	9.21	8.71	8.70
	(0.133)	(0.069)	(0.052)
intergenerational coef.	0.44	0.46	0.68
	(0.088)	(0.072)	(0.062)
# obs.	145	336	772

Note: The table reports, separately for respondents in the LVS-1 with no, one or multiple children, the mean years of schooling (with university and vocational training) of the respondents, the mean schooling of their parents, and the intergenerational coefficient from a regression of the former on the latter.

Figure 3: *A Hypothetical Family Tree Across Four Generations*



Note: The Figure depicts a hypothetical family tree across four generations: Parents ("Px"), Index ("I") and their partner ("P") and siblings ("S"), children ("Cx") and their partners ("CxP"), and grandchildren ("GCx"). Direct ancestors of the first generation are depicted by squares, their partners by circles. Educational status of members with dashed lines are unobserved in our samples. Complete four-generational lineages in bold.

E Matrilineal and Patrilineal Lineages

Table 13: *Status Correlation Between First and Third Generation*

	Matrilineal Lineages			*Patrilineal Lineages*		
	Actual	Predicted	N	Actual	Predicted	N
Panel A: Schooling						
LVS-1	0.155	0.124	2271	0.223	0.221	2182
	(0.032)	(0.019)		(0.030)	(0.021)	
LVS-2	0.225	0.236	1400	0.292	0.161	1365
	(0.035)	(0.030)		(0.033)	(0.029)	
BASE	-	-	-	0.258	0.196	541
				(0.047)	(0.052)	
NEPS-1	0.208	0.159	2600	0.223	0.153	2357
	(0.019)	(0.013)		(0.022)	(0.015)	
NEPS-2	0.202	0.154	3039	0.256	0.159	2792
	(0.017)	(0.013)		(0.020)	(0.013)	
Panel B: Schooling w/ vocational						
LVS-1	0.177	0.130	1841	0.272	0.221	1692
	(0.033)	(0.019)		(0.034)	(0.021)	
NEPS-1	0.207	0.130	2620	0.189	0.161	2376
	(0.022)	(0.013)		(0.026)	(0.015)	
Panel C: Occupational Prestige						
LVS-1	0.082	0.054	1088	0.250	0.146	2261
	(0.037)	(0.017)		(0.030)	(0.016)	
BASE	-	-	-	0.257	0.180	542
				(0.049)	(0.041)	

Note: Estimates of the Pearson correlation coefficient between G1 and G3. Balanced sample, using complete lineages in which the respective outcome is observed for individuals in all three generations. Bootstrapped standard errors clustered on family level in parentheses.

F Additional Evidence on the Grandparent Coefficient

Table 14: *Variation in the Grandparent Coefficient by Grandparent Survival, Extended Sample*

Panel A	**Schooling Child (G3)**				
	(1)	(2)	(3)	(4)	(5)
Indicator grandfather death	*At birth*	*B/w 1939-45*	*B/w 1939-45*	*War death*	*War death*
Conditional on absence/death during WWII?	*No*	*No*	*Yes*	*Yes*	*Yes*
Samples	*LVS-1/-2/-3*	*LVS-1/-2/-3*	*LVS-2/-3*	*LVS-2/-3*	*LVS-2/-3*
Schooling					
Grandfather	0.172***	0.166***	0.174***	0.171***	0.146***
	(0.037)	(0.032)	(0.057)	(0.057)	(0.036)
× Grandfather death	-0.012	0.029	-0.001	-0.038	0.071
	(0.063)	(0.098)	(0.139)	(0.156)	(0.122)
Father	0.479***	0.480***	0.438***	0.425***	0.389***
	(0.029)	(0.026)	(0.059)	(0.060)	(0.060)
× Grandfather death	-0.006	-0.019	0.093	0.080	0.057
	(0.048)	(0.072)	(0.103)	(0.133)	(0.163)
Grandfather death	0.251	-0.035	-0.999	-0.567	-1.406
	(0.492)	(0.728)	(1.052)	(1.427)	(1.258)
Panel B	**Schooling Grandfather (G1)**				
	(1)	(2)	(3)	(4)	(5)
Indicator grandfather death					
At birth	0.221***				
	(0.067)				
B/w 1939-45		0.128	-0.089		
		(0.087)	(0.139)		
War death				-0.041	-0.225
				(0.173)	(0.326)
# obs.	4269	4279	1113	989	956

Notes: Panel A reports estimates from a regression of child schooling on father and grandfather schooling. All regressions in Panel A include a dummy for grandfather death, interaction terms between grandfather death and father/grandfather schooling, a quadratic polynomial in the (hypothetical) age of the grandfather in 1988, and dummies for the index cohort considered (LVS-1, LVS-2, LVS-3). Panel B reports estimates from a regression of grandfather schooling on an indicator of grandfather death and a quadratic polynomial in the (hypothetical) age of the grandfather in 1988, and dummies for the index cohort considered (LVS-1, LVS-2, LVS-3). As an indicator for grandfather death, model (1) uses a dummy indicating whether the grandfather was already dead when his grandchild was born, regressions (2) and (3) a dummy indicating whether the grandfather died between 1939 and 1945, and regressions in (4) and (5) a dummy indicating whether the grandfather was killed during World War II or was missing in action since then. Regressions (3) to (5) restrict the sample to observations from G3 whose grandfather was absent because of World War II or died in the war. Regression (5) uses schooling with vocational training instead of just schooling as the education variable of the grandfather. Standard errors clustered on family level in parentheses, *** p<0.01.