The Shadow of Peasant Past: Seven Generations of Inequality Persistence in Northern Sweden[1]

Martin Hällsten and Martin Kolk
Stockholm University

The authors use administrative data linked to parish records from northern Sweden to study how persistent inequality is across multiple generations in education, occupation, and wealth, going from historical to contemporary time. The data cover seven generations and allow the authors to follow ancestors of individuals living in Sweden around the new millennium back more than 200 years, covering the mid-18th century to the 21st century. In a sample of around 75,000 traceable descendants, they analyze (*a*) up to fifth cousin correlations and (*b*) dynastic correlations over seven generations based on aggregations of ancestors' social class/status. With both approaches, the authors find that past generations structure life chances many generations later, even though the results align with traditional stratification research in that mobility across multiple generations is high. The results imply that today's inequality regime may have been formed many generations back.

INTRODUCTION

Life chances and life outcomes are correlated across generations, but over how many generations does inequality persist? This question is central for social theory, as it touches on the degree to which individuals form their own life outcomes and to what degree this is imposed on them by social structures. Early studies of income mobility found the persistence—the tendency

[1] We thank participants at presentations given at Nuffield College in Oxford (February 2019), Nordic Demographic Symposium in Reykjavik (June 2019), the European Consortium for Sociological Research, conference in Lausanne (September 2019), the SUDA Demographic Colloquium in Stockholm (February 2020), the Centre for Demographic

© 2023 The University of Chicago. All rights reserved. Published by The University of Chicago Press. https://doi.org/10.1086/724835

that descendants resemble their ancestor's status, be it at the top or bottom—from parents to children to be so low that persistence across multiple generations was considered unlikely. Becker and Tomes (1986) coined the now classic phrase "from shirtsleeves to shirtsleeves in three generations," meaning that "almost all earnings advantages and disadvantages of ancestors are wiped out in three generations. Poverty would not [persist] for several generations" (pp. S28, S32). Since the time of Becker and Tomes, the increased focus on measurement errors (Solon 1992) has revised this conclusion for two-generation studies, finding persistence to be rather high and thus lasting several generations, even though mobility dominates over persistence in explaining life chances in virtually all existing studies.[2] Some early studies of class mobility examined three-generational correlations (e.g., in the United States [Hodge 1966] and Britain [Ridge 1974]) but found persistence to be limited to two generations. Later, Warren and Hauser (1997) found no direct association between grandfathers' and grandchildren's social status in Wisconsin. In recent years, these conclusions have been challenged with new data and methods. We have witnessed a virtual explosion of studies documenting substantial associations across three generations between, for example, grandfathers' and grandchildren's outcomes (Anderson, Sheppard, and Monden 2018). The social panel surveys of the 1950s and 1960s now contain enough data to link up to three generations, and the expanding use of register data in the Nordic countries provides similar opportunities. However, empirical analyses over more than three generations are scarce, as even these data sources encounter a limitation: linking four or more generations is rarely possible with this data infrastructure. A collection of approaches have been used in earlier multigenerational stratification, including historical data sets (Mare and Song 2014; Song, Campbell, and Lee 2015; Song and Campbell 2017) and surnames (Clark 2014). However, studies following true kinship from historical into modern times are scant and are also largely limited to four generations (Dribe, Helgertz, and Van de Putte 2015; Lindahl et al.

and Ageing Research seminar in Umeå (February 2020), the Centre for Economic Demography seminar in Lund (September 2020), the Population and Health Research Group seminar in St. Andrews (January 2021), the CPop seminar in Odense (October 2021), and the RC28 in London (2022) and Jan Stuhler for valuable comments on our manuscript. All remaining errors are our own. We also want to thank the staff at the Demographic Database at Umeå University for digitizing the data and preparing our historical data. Funding was provided by Riksbankens jubileumsfond grant no. P17-0330:1 and the Swedish Research Council grant no. 2019-02552. Direct correspondence to Martin Hällsten, Universitetsvägen 10 B, Stockholm University, S-106 91 Stockholm, Sweden. Email: martin.hallsten@sociology.su.se

[2] We follow the convention in the literature of referring to the number of generations (nodes) studied, rather than across how many generations the inequality has traveled (ties). Transmissions from parents to children are commonly referred to as being two generations even though the difference between generations is only one. "Seven generations" refers to nodes, which is the same as six ties.

2015; Dribe and Helgertz 2016; Modalsli 2023; Kolk and Hällsten 2017), although Modalsli (2023) analyzed five generations.

We aim to expand the literature on the role of extended family background by analyzing the persistence of inequality across seven generations. Analyzing deep kinship patterns is a pioneering way to study the effect of social and kin background itself, as a direct influence from ancestors living six generations back is likely minimal. We argue that this approach answers novel questions on how individuals' social background is not just a factor of their parents but related to deeper traits engaged in families. When studying long-run mobility, we examine lineages including members born in the late 18th century as well as in the late 20th century. The socioeconomic context, occupational structure, and mechanisms of mobility are changing over the time period we examined, as society transforms from an agricultural economy where most goods are produced outside the market to an advanced service economy. Any examination of kinship's persistence across such generational depth will have to critically engage with finding measures that can trace intergenerational and multigenerational persistence across such a dynamic time period. We argue that the methodology we use provides a promising solution for measuring and thinking about the long-term influence kinship has on life chances in contemporary society.

Our approach is to merge modern Swedish administrative registers with historical registers based on church books. Sweden is an interesting case since the intergenerational class and income (but not education) mobility is among the highest in Western countries (Blanden 2013). Breen (2010) compares Sweden to Germany and the United Kingdom over cohorts born from the 1910s to 1970s and finds that Sweden begins with the highest level of mobility and that this also increases over successive birth cohorts. In Sweden, the church was responsible for national registration and kept administrative parish records of the entire population. They were used to keep track of the size of the local population but also contained events such as migrations and births recorded monthly. Church books from the northern Swedish regions of Skellefteå and Umeå from the 18th century to the middle of the 20th century (Westberg, Engberg, and Edvinsson 2016) have been digitalized.[3] Personal identity numbers were introduced in Sweden in 1947 and are recorded both in church books and modern administrative registers. They were used to merge the two sources. This means that merging historical and modern data is both straightforward and accurate. We trace ancestors in the Skellefteå and Umeå church books and analyze the social positions of their descendants in modern register data. Our target population is the kin network of descendants with ancestors living in the Skellefteå and Umeå regions. Our basic

[3] Similar data exist for several parishes in southern Sweden (Bengtsson, Dribe, and Svensson 2012).

research question is how much variation in a living person's outcomes is shared with horizontal kin (cousins) and how much that can be explained by ancestors (grandparents), both proximate (grandparents and cousins) and distant (e.g., fourth cousins and great-great-great grandparents). We address the research question using two approaches: cousin correlations and dynastic correlations, to be explained in detail below. With these analytical tools, we find persistence in education, occupations, and wealth across several generations. The persistence from distant ancestors is low but long lasting.

THEORETICAL BACKGROUND

Mechanisms of Multigenerational Persistence

Several theories can explain multigenerational kinship effects on socioeconomic outcomes (e.g., Cox and Fafchamps 2007; Mare 2011; Jaeger 2012; Anderson et al. 2018). However, when discussing potential influences over several generations, we can rule out any explanation based on direct social interactions, such as those between children and grandparents. Ruling out social interactions, we are left with different structural influences. The first explanation is genetic inheritance. However, genetic relatedness attenuates quickly for distant kinships.[4] If higher-order cousins are more similar in socioeconomic status (SES) outcomes than this, the reason is unlikely to be genetic.

In a population that is geographically limited or where cousin marriages are common, the average relatedness will be higher than these estimates. Similarly, assortative mating increases overall genetic similarity (Robinson et al. 2017). In northern Sweden, the frequency of cousin marriage was nontrivial and the marriage pool geographically constrained, meaning that overall inbreeding coefficients would be higher than those based on random mating for the values above (Egerbladh and Bittles 2011). Nevertheless, empirical assessments of the area's mean coefficient of inbreeding are too small to have substantial effects on higher-order cousin correlations (Bittles and Egerbladh 2005).

[4] Comparing kin born in the same generation, although siblings on average share 50% of genes, first cousins share 12.5% and second cousins share 3.125%. The relatedness of cousins thus shrinks by three-fourths for each generation of distance: third cousins share 0.78% and fourth cousins 0.19% of their genes; i.e., they are virtually unrelated. These numbers would increase under assortative mating, but estimates of genetic assortative mating suggest these to be too low to have any substantial impact, i.e., a correlation in latent genetic scores between spouses of around .02 to .04 (Domingue et al. 2014; Yengo et al. 2018; Collado, Ortuño-Ortín, and Stuhler 2022). From a vertical perspective, the relatedness to each ancestor shrinks by half for each generation of distance (father, 50%; grandfather, 25%; great-grandfather, 12.5%; great-great-grandfather, 6.125%, and so on).

Although direct social interaction between an ancestor and descendants can be ruled out, kinship background may be an important factor in maintaining family norms, culture, and ideas of social belonging. Quasi-direct inheritance mechanisms can be thought of as influences of ancestors that do not arise in parent-child interactions but have arisen long before that and are carried more or less unchanged from one generation to the next. Statistically, we would ascribe this to parent-child transmission, while the mechanism is the sequential transfers that exist over many parent-child pairs. One quasi-direct mechanism may be normative. For example, suppose descendants of a successful ancestor continue to celebrate this success to the extent that it becomes a stable norm. In that case, this could explain how past generations have a long-lasting influence. Such influence will operate via parents even though the origin is elsewhere in the kinship structure. A critical driver here is social closure. Kinship relations often involve solidarity and prioritization over other social relations (some suggest this has an evolutionary origin; Euler 2011). When kinships are closely integrated with other aspects of the social structure, such effects of social closure can be very substantial. In contemporary individualized high-income societies, however, a number of domains compete with kinship in terms of providing social relations between individuals, including schools, workplaces, friendship circles, and voluntary organizations, in contrast to past societies in which more domains of life were integrated with kinship.

A related plausible mechanism of quasi-direct inheritance is via symbolic assets. In many past societies, more aspects of social status were directly prescribed from the family's background. The most obvious examples are inherited positions and titles, such as in aristocratic and royal families, but land was also often passed down with little change across generations, and occupations were passed across generations in guilds. The key point is that land passes from parents to children, but the parents are not the original source of the land. In a society in which family backgrounds prescribe social status, even if individuals are less successful than their parents for occupation, income, or education, if essential assets that prescribe social status are heritable, they will be transmitted directly from grandparent to grandchild, even if occasional observed individual outcomes in an intermediate generation are low. Kin are often united by a common name or other symbols, which may provide a form of reputational, social capital that is valid outside the family but may also exert normative pressure within the family. Multigenerational transfers through symbolic means may sometimes be less dependent on social interaction, such as grandparents influencing later generations in their absence as role models or through inscribed social statuses. For example, Laband and Lentz (1985) argued that a rationale for politicians' children to become politicians is name recognition and voter loyalty. Second-generation politicians thus often beat first-generation politicians in elections (Feinstein

2010). Symbols are not necessarily linked to elites only but apply widely. An extreme form of symbolic mechanisms in the lower end of the social spectrum would be the stigma or labeling of being kin to a perpetrator (e.g., criminality is transmitted across two and three generations; Frisell, Lichtenstein, and Långström 2011; Eriksson et al. 2016). To the extent that social status is linked to racial and ethnic categories, such categories will be inherited across generations with high, if not perfect, fidelity (Torche and Corvalan 2016). Occasional outlier individuals may not transmit their status above/below that of their group. We note that our population was unusually ethnically homogenous (Alm Stenflo 1994), unlike many other parts of the world at the time.

The above mechanisms may be the more important explanation of outcomes like education or occupational careers, while outcomes such as wealth may operate differently. Here, the transfer of physical assets is a potential quasi-direct mechanism, and bequests are a direct way family advantage is transmitted across generations. Sweden has a bilateral kinship system where all sons and daughters inherit. Until the mid-19th century, sons inherited two-thirds and daughters one-third of their parent's estate, and family land was protected by law and could not be easily sold or transferred out of the family (Dackling 2013). After the mid-19th century, sons and daughters inherit equally by law, and children could force the sale of family land. However, in practice it was very common throughout the 19th and 20th centuries for agricultural families to concentrate the inheritance. Hence, often a single child (usually a son) maintained ownership of the family farm. Unlike some Western countries, in contemporary Sweden children by default have the right to half of their parent's estate, and sons and daughters cannot be fully disinherited. While the inheritance system would, in practice, typically create more substantial continuity along patrilineal lines, a tendency for stronger socialization and parent-child resemblance for mother-daughter ties, as well as the role of women as "kin-keepers" (Young and Wilmott 1957; Rosenthal 1985), may result in stronger continuity among maternal lines. In the two-generational literature, sister correlations are weaker than brother correlations (Björklund and Jäntti 2020), and this is also true for intergenerational correlations (Chadwick and Solon 2002; Jäntti et al. 2006). For three-generational studies, the review by Anderson et al. (2018) examined whether associations differ by grandparental gender and maternal or paternal lineage but found no clear pattern in the results, even if there were marginal differences for grandparental gender. In contemporary Western societies, some families organize generation-skipping family trust funds to facilitate such outcomes by transferring resources directly across three generations or more (Mare 2011). Ownership of firms is another case in point: a large share of all large private companies are still run by family firms using various corporate strategies to maintain family control over vast enterprises in Sweden and other Western countries (Masulis, Pham, and Zein 2011). On the extreme ends of the spectrum, kinship may be a carrier of wealth (or poverty) that is strongly transmitted across generations (Kotlikoff and Summers 1981; Adermon, Lindahl, and Waldenström 2018), and that also may give rise to strong normative behaviors to secure kinship advantages in dimensions related to career chances, such as educational investments (Hällsten and Pfeffer 2017). One explanation for such normative elite behavior is that generational transfers of wealth are often seen as a loan from the family rather than as personal assets (Schaeffer 2014; Kuusela 2018). Such dynastic thinking indicates that, in some cases, the family's survival is of greater importance than the will of the individual and would trigger investment in several outcome dimensions, not least education.

Formal Approaches to Multigenerational Persistence

A central tool to causally represent social mobility across multiple generations is a Markov model. A first-order Markov model (also referred to as an autoregressive AR(1) model) postulates that inequality is only transmitted sequentially across generations from parents to children so that there is no association between grandparents' and grandchildren's outcomes once parents' resources are considered. Formally, a first-order Markov process is memoryless: knowing the present state of a (statistical) system creates predictions about future events that are as accurate as knowing the system's full history. That is, any multigenerational causal influence can be reduced to the product of subsequent intergenerational (parent-child) associations.

If a first-order Markov model is true, and intergenerational influences are stable over time, it is possible to compute the projected correlations between an index generation and the kth generations as

$$\mathrm{Corr}(1, k) = \mathrm{Corr}(1, 2)^{k-1}. \tag{1}$$

This procedure of iterating intergenerational correlations with increasing exponents means that a grandparent-child correlation should equal the parent-child correlation squared. Although this logic was aimed at a vertical transmission model (where the resemblance between children and their parents and grandparents is examined), it also applies to horizontal models (where the resemblance between siblings and cousins is compared): first- and second-order cousin correlations are projected to be the squared and cubed sibling correlations (Knigge 2016; Lundberg 2018). Many recent works (Stuhler 2012; Lindahl et al. 2015; Knigge 2016; Braun and Stuhler 2018) have observed higher-order correlations at higher levels than those iterated, leading them to reject the (first order) Markov model. Some take this as evidence of direct grandparental effects, but drawing this conclusion is more complex as an estimated empirical function is not necessarily the true theoretical function; a wide range of theoretical models can predict such a pattern. For example, building on Clark and Cummins (2014), Braun and Stuhler (2018) developed a more realistic model where outcomes can take the indirect route via endowments that then predict the outcome (e.g., via skills rewarded in a labor market). Such a model gives higher multigenerational predictions than the simple iterations of equation (1) without any direct ancestor effects. A similar point has been made by Lundberg (2020), who shows that cousin correlations are not indicative in themselves of the underlying transmission processes but can be explained by a multitude of potential processes, not necessarily involving direct transmission from grandparents. Measurement errors can also distort any analyses of persistence across generations. In general, we would assume that precision is lower regarding distant historical times. In our case, we have to rely on priests' transcripts of occupational titles, where the underlying nomenclature is based on practices and not our own operationalization and measurement. Accordingly, even observing higher multigenerational correlations than what is predicted in the first-order Markov model is not necessarily a counterargument to the idea of sequential transmission between parents and children.

Higher-Order Markov Models

The Markov model can be extended by including more lags, such as direct influences from grandparents and ancestors (i.e., AR(2) and higher). Such direct grandparental associations do not operate via parents. The three-generation literature contains many studies that document a statistical association between grandparental associations once parental characteristics have been netted out (Anderson et al. 2018). Interestingly, many of these estimates of grandparental associations suggest that they have little dependence on actual contact, where contact is either physical proximity or generational overlap. Overall, this suggests that grandparents provide resources other than those that require direct social interaction. However, the question is whether the parental controls (or lower-order lags, when considering higher-order lags) are exhaustive in capturing the causal influence. Unmeasured (unobserved) heterogeneity or error-prone observations of parents' attributes may show up as "phantom" grandparental effects (Kelley 1973; Hällsten 2014), which makes higher-order Markov models sensitive to specification. Engzell, Mood, and Jonsson (2020) have shown that once parental characteristics are exhaustively measured, the direct three-generation association for income is small in Sweden. Still, some similar analyses, such as grandparental wealth and children's educational attainment, reach different conclusions (Hällsten and Pfeffer 2017).

Another problem with higher-order Markov models is that direct grandparental effects are also troubled by bad control (overcontrol) bias and collider bias (Elwert and Winship 2014). Parental characteristics lie on the causal path from grandparents and are essential mediators. The implicit assumption of any analysis, including mediators, is that they are exogenous; that is, mediators' residual factors are unrelated to the outcome's residual factors, which is highly questionable. There have been attempts to relax such assumptions via, for example, marginal structural models (Robins, Hernán, and Brumback 2000) for testing for multigenerational neighbor effects and wealth effects (Sharkey and Elwert 2011; Hällsten and Pfeffer 2017), but they come with new assumptions (e.g., no unmeasured confounding; Cole and Hernán 2008).

PREVIOUS STUDIES

Previous studies of inequality transmissions in contemporary times (the 20th century) have primarily been limited to three generations. Although the first modern multigenerational study (Warren and Hauser 1997) found no evidence of three-generational associations in Wisconsin, new literature, in most cases, finds three-generational associations in Western countries (Anderson et al. 2018), even for Wisconsin (Jaeger 2012). For Sweden, there are several three-generational studies due to good data availability. For example, for the town of Uppsala, Modin, Erikson, and Vågerö (2013) found associations in grade marks between grandparents and children when controlling for parents' education.

Of particular interest to us are studies that analyze persistence or mobility over four or more generations. However, this is still a very limited part of the literature, which is also concentrated on some specific regions. Several studies have come from Swedish data. Lindahl et al. (2015) analyzed data from Malmö in southern Sweden on individuals born in 1928, adding their parents, and their children and grandchildren (four generations). They found income correlations across three generations and education correlations across four, meaning that information on great-grandparents helped predict education attainment today. Lindahl et al. (2014) directly tested and rejected the (two-generation) Becker-Tomes model. Using population registers from Sweden, Hällsten (2014) estimated positive and substantial second cousin correlations in grade point average for the total Swedish population (under the condition of small intergenerational intervals). Adermon et al. (2018) analyzed wealth across three and four generations, finding persistence to be much higher than theoretical two-generation models suggest. However, while these studies include four generations, they are rather 3.5 generation studies as the current generation is still very young, and the data thus condition on small intergenerational distances.

Other studies are entirely focused on historical data. Knigge (2016) estimated second cousins having correlations in occupational status from the

19th century into the early 20th century in the Netherlands. Long and Ferrie (2018) linked historical censuses for the United States and Britain and found grandparental associations in the later 19th century. Mare and Song (2014) found persistence over many generations in China, from historical times to the early 20th century.

Another part of the literature uses indirect measures. Clark (2014) uses changes over time representation of surnames within high-status occupations to calculate mobility, and he finds intergenerational mobility to be very low, suggesting persistence to last over multiple generations (for a critique, see Chetty et al. 2014; Torche and Corvalan 2016). Similarly, Barone and Mocetti (2020) identify pseudodescendants via surnames and find persistence from historical to modern times in the now-Italian city of Florence. The most relevant literature related to our study links modern and historical individual-level microdata. In a pioneering study, Campbell and Lee (2011) found persistence over many generations in China from historical times to the present. In their study, SES is measured at the group level. Dribe and Helgertz (2016) used church books from southern Sweden linked to modern registers to study trends in mobility across three generations. They found a direct association between grandfathers' class or occupational status and grandsons' outcomes and that this three-generation association is stable over time. Modalsli (2023) linked historical census data to modern registers via surnames, drawing on Modalsli (2017). He analyzed five generations through a vertical design and found persistence over four generations. Although he found a substantive persistence coefficient for great-great-grandfathers, it was not significant.

A part of the literature extends the multigenerational approach to include demographic behaviors such as marriage, fertility, and mortality. Mare and Song (2014) use simulations to show how such behaviors play an essential role in moderating or amplifying SES effects. Song (2020) analyzes U.S. data from 1850 to 2015, with a similar approach. Song et al. (2015) show, in ancient China, that patrilineages founded by high-status males had a lower probability of extinction at each point in time, resulting in higher growth rates for the next 150 years. Kolk and Hällsten (2017) took a different prospective approach using data from northern Sweden with a prospective lump model that also incorporates demographic behavior, and they found a correlation between the proportion with tertiary education in great-grandchildren and great-grandparents' occupations.

In conclusion, the current literature documents persistence over four generations, with a few exceptions (Campbell and Lee 2011; Modalsli 2023). Yet, even these studies are limited by constraints in data, measurement, and sample sizes. With our current study, we overcome several of these limitations, even though we are limited to a local region within Sweden.

DATA

We use population data for Sweden that have been linked to church books in the Swedish regions of Skellefteå and Umeå. Figure 1 shows our study area in the north of Sweden. The church books in this area have been digitalized as part of a large research infrastructure project (Westberg et al. 2016). This region encompassed a population of predominantly agricultural workers before the 20th century. The farmers in the region mostly owned their own land, which was typical for northern Sweden. The end of the 19th century saw the rise of a sawmill industry, and the early 20th century saw the rise of mining and metallurgical industries after the discovery of an ore field near the town of Boliden. We know from prior research that inhabitants and descendants in this region have higher-than-average levels of education (Kolk and Hällsten 2017), despite the area being quite representative of Sweden socioeconomically in the middle of the 20th century (Kolk and Hällsten 2017). Local estimates of two-generation income mobility are among the highest in contemporary Sweden (Brandén 2018), even though the opposite was true for occupational mobility at the end of the 19th century (Berger et al. 2021). Our historical data have been digitized by the Demographic Database at Umeå University (Edvinsson 2000; Westberg et al. 2016) and include registers for 10 different parishes in northern Sweden. The parish records end at different time points between 1950 and 1970. We describe the data in further detail in appendix A.

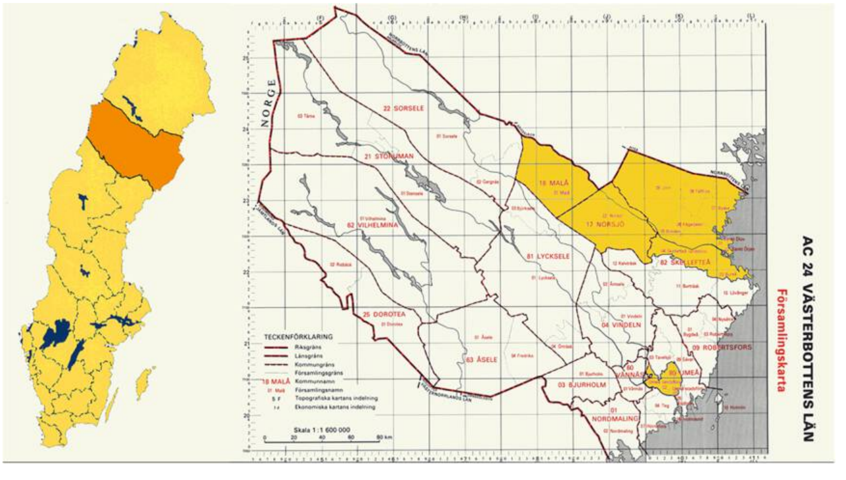


FIG. 1.—Map of Skellefteå and Umeå region. The darker areas on the right map mark the parishes in our historical data. (Source: Lantmäteriet.)

From 1960, we can link our church books with registers of Sweden's complete population, which allows us to follow our cohort as they disperse all over Sweden. A similar data set has been described elsewhere (Kolk and Hällsten 2017; Kolk and Skirbekk 2022). It should be noted that there is out-migration from our historical region before 1960 that we cannot follow outside the region, and there is also international migration after 1960. However, internal migration (and in particular migration to the United States) was lower in our region than in the rest of Sweden at the end of the 19th century (Berger et al. 2021).

Our sample is conditioned on residential stability across many generations from the late 18th to early 20th centuries. Thus, the selected sample from our local population has an overrepresentation of landowning farming families. However, an advantage of our data is that it allows us to follow kinship networks all over Sweden with national administrative registers after 1947. This allows us to capture the 20th- and 21st-century kinship networks of our lineages that had members living in Skellefteå in the middle of the 20th century regardless of where they live in Sweden, as well as to measure their socioeconomic information with high accuracy from 1960 onward.

Our population has two additional characteristics: there is a higher-than-average prevalence of cousin marriage in historical times, even though the mean coefficient of inbreeding in the area was not particularly high (Bittles and Egerbladh 2005), and the area is known for the incidence (1.5% of the population) of a genetically transmitted neurological disease: familial amyloid polyneuropathy (named *Skelleftesjukan* in Swedish).[5] We address these population characteristics in sensitivity analyses.

Constructing Kinship Networks

We define the index generation as cohorts born between 1940 and 1987 for whom we observe socioeconomic outcomes. We trace their ancestors back in time using repeated retrospective matching on links between children and their parents from church book records. This is similar to taking all adults in the last decade of the church books and trying to find them and their children (if they exist). This creates a kinship network that goes six generations back from the index generation (covering seven generations, including the index). We thus link each index to (up to) four grandparents in generation -2 (i.e., two generations back), up to eight great-grandparents in generation -3,

[5] The familial amyloid polyneuropathy disease has a late onset, in middle age, and even though it is genetically transmitted, only some 10% of those carrying the risk genes develop the disease. This means that it is unlikely that individuals are influenced by the disease in their early careers, or that they would adapt their career choices knowing that they carry risk genes.

and so on, until we reach (up to) 64 ancestors in generation −6 (assuming no endogamy). For example, fifth cousins only need to share one or two great-great-great-great-grandparents, a single couple can be the source of a large number of fifth cousins, and the size of fifth cousin groups is often large (although cousin group size varies considerably).

However, as the linkage is not perfectly complete, we cannot reach these theoretical maxima. We reach ancestors in the Skellefteå data by either the mother's or the father's lineage for approximately two-thirds of the index individuals; for one-third, we reach ancestors by both lineages. Table B1 shows the number of ancestors we can observe per generation. For the proximate generation, coverage is quite high but then declines. In the eldest generation, on average, we observe one-third of the 64 theoretical ancestors. To be able to compare estimates of persistence across generations, we delimit the sample to cases in which we can find at least one ancestor in generation −5, where our coverage is more complete. With this restriction, we have around 75,000 cases in our index generation. Our data quality is lower for individuals born before the late 18th century, and we therefore condition our population on having at least one fifth-generation ancestor (although we also analyze sixth-generation ancestors). In table B6, we show the share of individuals in our parishes that are a part of the kinship networks in our analysis population.

We code the relationships between ancestors, using the gender of each ancestor in sequences: m is mother, mm is mother's mother, f is father, fm is father's mother, and so on. All in all, we have four sequences for grandparents (mm, mf, fm, ff), eight sequences for great-grandparents (mmm, mmf, mfm, mfm, fmm, fmf, ffm, fff), and so on. This means that we have 64 sequences for generation −6. The sequence mmmmmm is thus the ancestor following the maternal line of kinship only. As an example, we display the birth years of all individuals in this kinship line in figure B1. There is substantial variation in birth years of past generations, but this is, of course, also driven by our wide span for the index cohort. This is motivated by power concerns but also because we know that there is great dispersion in birth years among those great-grandchildren of persons born in the mid-19th century in the church book data (Kolk and Hällsten 2017). We have done some sensitivity analyses of the effects of factors such as age differences between cousins, which have no fundamental impact on our results (see table F4).

Changing the perspective from the index generation to the anchor ancestors who produced the descendants is also possible. Table B2 shows the birth years and the number of descendants for each ancestor each generation produced. On average, an ancestor in the eldest generation produced 160 descendants, but this value went as high as over 2,000. This also means that variations in the sizes of the kinship networks are considerable, which we address later (however, our findings suggest that this is not a very important factor).

Variable Coding

We analyze three outcomes in the index generation: education, occupation, and wealth. They represent different forms of stratification, that is, in skill levels, realized skill levels, and economic assets not necessarily generated in the labor market. Recent analyses from Sweden highlight how education and occupation are closer to each other as stratification dimensions and, in turn, different from wealth (Hällsten and Thaning 2022). However, since we are able to measure education with the least constraint, we focus the results on this outcome and comment on differences for occupation and wealth whenever they are substantial. We measure education by the highest level in the education registers, primarily drawing on school graduation records. We then convert this to years of education. We use data from 1990, 2001, 2007, 2012, and 2017, with 30 being the youngest age of sampled individuals (if we encounter diverging information, we take the highest value).

For occupations, we code International Socio-Economic Index of Occupational Status (ISEI) scores (Ganzeboom, De Graaf, and Treiman 1992) from the Swedish occupation register of 2001–17, which mainly contains employer-reported occupations. We prefer ISEI over the Cambridge Social Interaction and Stratification scale (CAMSIS, which we use for older generations; see below) because ISEI captures higher levels of intergenerational persistence (Hällsten 2019). We use conversion tools that translate ISCO-88 (COM), the European Union variant of the International Standard Classification of Occupations, into ISEI scores (Ganzeboom and Treiman 1996; Bihagen 2007). We take the averages of observed ISEI when there is information for several years to represent the individual's occupational outcome.[6]

For wealth, we rely on the Swedish wealth register from 1999 to 2007. This register is based on wealth tax data that are augmented with several additional sources, and it is of very high quality compared to most existing wealth data sources in the literature.[7] To reduce measurement errors, we compute the individual-level average of wealth from 1999 to 2007 and use this in our analyses. This measure correlates roughly .95 with annual wealth, allowing us to avoid some attenuation biases. The descriptive statistics for the index generation are shown in table B3.

[6] This departs from the current convention of taking the highest values (i.e., following the occupational maturity hypothesis). Compared to the results of such an approach, we observe slightly stronger correlations.

[7] Tax data information is essentially self-reported but subject to legal responsibility (and possible prosecution for tax fraud). The additional sources cover holdings in banks, insurance companies, and the like, as well as estate registers. This information is not censored. Importantly, all types of estates are rated at their market value (rather than some nominal tax value). What the wealth data do not cover are assets held outside Sweden that are thus not disclosed to the Swedish Tax Authority.

We use each of these variables in rank form. The rank provides a robust metric for intergenerational transmissions (Dahl and DeLeire 2008; Chetty et al. 2014), while reducing the impact of specification and measurement errors (Nybom and Stuhler 2017). For wealth, where skew is extreme, rank provides the most optimal scaling of alternatives (Killewald, Pfeffer, and Schachner 2017). The transformation method is inconsequential, as estimates using original scales for education and occupation (this makes little sense for wealth) show similar results (e.g., cf. tables 1 and 2 below).

The historical church book data contain information about an ancestor's occupation, as written down by the priests (but no other information on SES such as education). These occupational titles have been coded to a Swedish nomenclature and then translated into HISCO (historical ISCO; see van Leeuwen and Maas 2010). As with much historical data, the text strings that describe occupations are sometimes ambiguous because there was no standard format or nomenclature. There is also a male or husband bias in the text strings, where wives were given their husband's occupation (e.g., farmer's wife). We have prioritized unique information and only use what refers to a specific person and disregard information that is indirect (e.g., titles achieved by son/daughter or wife relation). By construction, we get a higher incidence of missing information for females than for males (cf. fig. B2). Since we can encounter conflicting information, we take the occupation associated with the highest achieved class position and the average status score (according to HISCLASS [historical international social class scheme] or HISCAM [historical CAMSIS]; see below).[8]

We have coded HISCO to the HISCAM status scale (Lambert et al. 2013; HISCAM version 1.3.1, November 2013).[9] The HISCAM is a historical version of the CAMSIS scale that ranks occupations based on marriage patterns under some idea of social closure (Prandy and Lambert 2003). However, this may not be the optimal scale to capture intergenerational processes (Hällsten 2019), but unfortunately historical versions of the ISEI that we use as an outcome are unavailable. Moreover, Song et al. (2020) discuss that some high-status occupations may become less prestigious over time. The HISCAM assumes a constant occupational score for a given occupation, which may bias multigenerational correlations. This may be the case for agricultural occupations particularly. Therefore we use an alternative approach. We have also coded HISCO to HISCLASS (Maas and Van Leeuwen 2016), a measure that

[8] We have also tested dropping female ancestors as a source of information, under the assumption that their occupational information may be less valid as an indicator of the family's status in historical times, but this does not change our results (not shown). The analyses use information from both males and females.

[9] For the HISCAM, we used the universal U2 scheme at http://www.camsis.stir.ac.uk/hiscam/. We take averages of different HISCAM scores when there is more than one observation per individual.

resembles EGP social class (Erikson and Goldthorpe 1992) with 12 categories. Table B4 shows examples of how HISCO occupations are linked to HISCLASS classes. We use a modified version HISCLASS to empirically construct an index of ancestors' class position using all observed discrete HISCLASS classes (see the second approach: dynastic correlations section below).

Figure B2 shows the proportion of missing occupation information by birth cohort (grouped in periods of 10 years) and gender. Although the information is limited before 1750, the amount of missing data is down to approximately 30% around 1840 for males. For females, it is never below 40%. Even with these high-quality data, we experience a limitation due to missing information that biases estimates, but we employ strategies to reduce this bias as explained below. The historical data cover birth cohorts up until 1930. As some ancestors are rather young (i.e., grandparents and great-grandparents of our youngest index generation cohorts), their occupations are not recorded in the historical data. We therefore used the 1960–90 census information for complementary occupational information. As these data do not contain HISCO, we used crosswalks from the native ISCO-58/NYK (Nordic Occupational Classification) codes to EGP and microclasses (Jonsson et al. 2009; Erikson, Goldthorpe, and Hällsten 2012) to generate close matches to HISCLASS. CAMSIS codes are available for the ISCO-58 in the censuses without crosswalks (Bihagen 2007), allowing us to substitute CAMSIS for HISCAM when the latter is missing (they have the same mean and variance). This may create less optimal measures for parents and grandparents, but as the information in historical data constrains us, we prioritize consistency. Table B5 shows the HISCLASS classes of ancestors by generation and their HISCAM scores. Unsurprisingly, the occupation structure is dominated by farmers and farm laborers. However, there is also great change over time following the industrial transformation in the region. Figure B3 describes the class structure in the historical church book data by birth cohort (grouped in periods of 10 years; we have grouped the 12 detailed HISCLASS categories into six broader categories; see table B4). The class structure changes considerably across our study period, where the share of agricultural occupations declines rapidly at the end of the period and where we see the rise of both manual and service-class occupations. This dynamic is not captured as well by CAMSIS, which has a mean close to 50 for all years, even though variance increases strongly over time. One important limitation in our data (and in HISCO) is that they do not differentiate between farmers of different land wealth. Our sample is dominated by farmers in historical times; we can distinguish between land tenure (whether they own their land) but not between land size, which varies considerably from larger land owners to close to subsistence farmers. This will cause a measurement error so that we underestimate multigenerational correlations.

METHODS

Our target estimand in this article is how much variance ancestors, distant and proximate, explain in their descendants' outcomes. Although some of the earlier multigenerational studies were motivated by a conceptual and methodological critique of the Becker and Tomes type of two-generation models (Mare 2011; Stuhler 2012; Clark 2014; Lindahl et al. 2014), new findings have spurred a critical debate regarding the interpretation of multigenerational effects (Solon 2014, 2018; Torche and Corvalan 2016; Braun and Stuhler 2018; Breen 2018; Lundberg 2018; Engzell et al. 2020). The majority of the literature relies on a vertical model that uses measured characteristics of all prior generations and then estimates intergenerational transfers. Here, single observations of an ancestor's SES are more often than not the rule, and they can be seen as more or less erroneous realizations of that ancestor's true SES. The standard method of minimizing these issues by averaging over many repeated observations (Solon 1992), assuming measurement errors to be random, is often not possible because of data limitations. The vertical models are also limited to assessing the variance in outcomes that can be linked to ancestors' observed characteristics.

A new addition to this field is dynastic correlations (Adermon, Lindahl, and Palme 2021), an extension of vertical models. The core idea is not only to view single observations of SES in an ancestor's life as a potentially erroneous realization (Solon 1992) but also to view individuals themselves as a potentially erroneous realization from their kinship lineage. Following the standard treatment of classical measurement errors, the latent SES of the kinship lineage can be captured by averaging the characteristics of entire clusters of kin. Adermon et al. (2021) incorporated horizontal parts of the extended family (parents' siblings and cousins, their spouses, and the spouses' siblings) as well as vertical parts (grandparents). They found the total dynastic persistence to be much higher than estimates from two-generation models, and the horizontal aspect largely drives this. In a similar vein, and closer to the cousin correlation approach, Collado et al. (2022) developed a more systemic approach that uses three generations of kin but also extends the kinship network horizontally to cousins and in-law relationships, to integrate intergenerational and assortative mating processes. They found that conventional measures based on observable characteristics greatly understated the importance of family background and the extent to which latent advantages correlate between kin. Even sibling correlations, known to be a more comprehensive measure of family background, underestimate latent advantages by about 50%. Collado et al. also found that a purely genetic transmission model cannot fit their system of kinship correlations.

A smaller portion of the literature instead bypasses many of the measurement problems by using a horizontal model where, for example, cousins within the same generation are compared at the same point in time (Jaeger 2012; Hällsten 2014). Measures of ancestors' statuses taken from potentially low-quality data sources never enter the model, which is identified with kinship links only. This is a great strength when working with historical data in which SES measurement is unavoidably less precise (see discussion below). An advantage compared to vertical models is that horizontal models include persistence due to both observable and unobservable characteristics of ancestors. The horizontal concept of family or kinship background is broad and contains everything shared, including local contexts. This means that, for example, effects of neighborhoods and schools are included in the measure (most likely only for siblings), but most mobility scholars would agree that segregation in neighborhoods and schools is an important way in which a family background reproduces advantage.

Although the cousin correlation framework is a rather new addition in the multigenerational mobility field, it has a stronger heritage in studies within psychology (e.g., Bouchard and McGue 1981) and epidemiology (e.g., Hsueh et al. 2000), and there are also examples of studies that have examined cousin correlations in criminal behavior (Frisell et al. 2011). A potential drawback of the cousin correlation method is that it can be estimated for one lineage only or averaged over several lineages as we do below. Still, the total influence of all lineages is not captured (Hällsten 2014). To some extent, this is a matter of perspective: seen from the ancestor, the cousin correlation captures his or her total influence, but seen from the descendant, there are many sets of cousins, each with their potential contribution. For example, the sibling correlation captures the influence of both parents (in nuclear families), but the first cousin correlation captures either the maternal or paternal grandparents (or their average), not their joint influence. Compared to our estimand that aims at the influence of all ancestors, the cousin correlation is therefore an underestimate of the influence, but likely a small one.[10] It should be noted that sibling correlations will include a component that stems from siblings mutually affecting each other, whereas this is not the case for the intergenerational correlation. A mutual component is still possible for cousin correlations, even though social interactions between distant cousins will decay strongly by generational distance.

[10] To scrutinize this issue would require some more advanced statistical analyses or a formal model. One could in principle estimate a multilevel model with cross-classified ancestor random effects, but in reality, such a model is not estimable. We have instead attempted to predict the cousin component (i.e., the predicted random effect—the best linear unbiased prediction, BLUP—for ancestors) from separate models for each lineage, assuming lineages are independent. The overlap of lineage effects is very large: combining the independently estimated BLUPs for several different ancestors (e.g., paternal and maternal grandparents or great-grandparents) in an outcome regression does not increase R^2 substantively compared to only including one of the BLUPs. We therefore believe the underestimation is rather modest.

Study Methodology

Our target population is the kinship network of descendants and their ancestors living in the Skellefteå and Umeå regions. We use two different methodological approaches: (1) cousin correlations and (2) dynastic correlations as estimators to assess multigenerational persistence, our estimand, in this population. Multigenerational persistence is the share of the variation of an index individual's outcome that is explained by ancestors, separately by ancestor generation. We explain these methods in more detail in the next two sections. Measures based on dynastic correlations, and designs based on surnames (Clark 2014; van Dongen, Eriksson, and Dribe 2018), are particularly advantageous if historical measurements of SES involve a large share of measurement errors. This is also the case if cross-sectional measurements of historical occupations poorly reflect social positions in a preindustrial society, for example, because social status is primarily prescribed by family background, social capital, and community networks or is related to the quantity and quality of land ownership. Measures based on large aggregations of occupations (dynastic correlations), or surnames themselves, are likely measured with relatively smaller measurement errors. Similarly, our horizontal approach (cousin correlations) is powerful if historical SES measurements have low precision, as they rely on highly accurate contemporary measurements of descendants' SES.

First Approach: Cousin Correlations

Figure 2 describes the kinship structure we use to estimate sibling and higher-order cousin correlations. We use a methods-of-moments estimator (e.g., Solon, Page, and Duncan 2000) extended to cousins (Hällsten 2014). The methods-of-moments estimator uses the unique pairs of cousins (siblings) sharing an ancestor to estimate pairwise covariance terms in outcomes (omitting pairs sharing an ancestor at a lower level): $Y_{\text{cousin } i} \times Y_{\text{cousin } j}$ $(i \neq j)$, where Y is measured in deviation-from-mean form. This pairwise term is summarized, with different weighting schemes to handle linages of different sizes (see app. E) to generate the covariance and in turn the cousin correlation (ρ = covariance/variance).[11] Because we cover large spans of birth cohorts in

[11] For siblings, the number of pairs generated by n children is $n(n-1)/2$, but for cousins this is more complex (depending on how grandchildren are split among children, etc.). We simply generated all pairs of cousins sharing an ancestor. Most sibling correlation estimates now employ a parametric multilevel model estimated with REML (restricted maximum likelihood; following Mazumder 2008). However, the methods-of-moments estimator has some advantages in our case. First, as we prefer to analyze outcomes in a rank form that follows a uniform distribution, the method has the advantage, as Jäntti and Jenkins (2015) suggested, of making no formal distributional assumptions (as opposed to parametric multilevel models that assume that random effects are normal). Second, the methods-of-moments

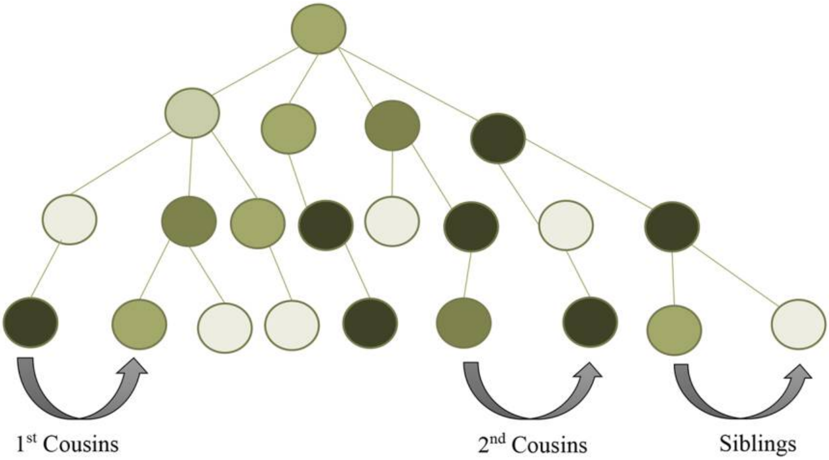


FIG. 2.—Data structure for cousin correlations.

our index generation, where there is a strong drift in education due to education expansion, and where this drift is unequal across genders because of women's liberalization (taking up education, entering the labor market), we control for this by birth year interaction dummies and gender in all models. We do this by residualizing our outcome variables on gender × birth year fixed effects (this also transforms Y to deviation-from-mean form). We compute bootstrap standard errors, which we cluster on the ancestor who generated the kinship relations. Since the number of pairs per ancestor becomes very large for higher-order cousins, we sample among pairs for fourth and fifth cousins to reduce computation burden. In our sampling, we stratify by ancestor and sample 50% or 15%, respectively, of pairs that exceed 5 in number

estimator makes it possible to stack cousins from different lineages and estimate a generic cousin correlation, whereas the multilevel method only allows lineage-specific correlations, effectively not making use of all available data (even though we only average and do not examine the joint impact of lineages). Third, the parametric model would require a complex structure of nested random effects (one for each cousin order). The methods-of-moments estimator only requires that we remove lower-order shared relations (e.g., sharing parents for first cousin correlations and sharing parents and grandparents for second cousin correlations). A final advantage is that because the level of observation is cousin pairs, we can take relational information into account in sensitivity analyses (e.g., age differences between cousins), which the parametric model does not allow for as easily. One disadvantage is that because we rely on bootstrapped standard errors, the methods-of-moments estimator has wider standard errors and thus has less statistical power compared to the true (but unknown) standard errors. However, as the unit of analysis is unique relations, this lower power is somewhat counteracted by a large number of relations for higher-order cousins.

per ancestor (to guarantee that all ancestors contribute) for fourth and fifth cousins respectively.

Measurement Errors, Cousin Correlations, and Markov Iterations

Lundberg (2018) raised concerns about measurement errors when projecting cousin correlation over generations and evaluating the first-order Markov assumption. Both siblings and cousins are exposed to the same measurement errors in outcomes, which will then depress both to the same extent if the measurement errors are classical. This will affect the likelihood that the process is a first-order Markov assumption, as the attenuation bias in sibling correlations will have a proportionally larger effect on the projected cousin correlation iterated from the sibling correlation than the attenuation bias in cousin correlations itself. Lundberg (2018) proposed a simple method to adjust for classical measurement errors by making ad hoc assumptions about the reliability of outcomes. A classical measurement error is a typical suspect when using survey data because test-retest reliability is imperfect, and the attention to measurement error has revolutionized the study of income mobility (e.g., Solon 1992).

However, the variables in our study are not from self-reports in surveys but are administratively generated. The chief data source for our first outcome measure, education, is administrative school records, with some 30% of the information in the oldest cohorts coming from censuses (see fig. B4). Even though census data are essentially self-reported survey data, the vast majority of the information we use does not change in any random way over time. It should also be noted that education itself, being cumulative, is rather time stable.[12]

Our second outcome measure, occupations, comes partly from self-reports in the censuses in 1985 and 1990 and partly from employer reports in the occupation register from 2001 to 2017. The measurement error properties of this variable have not been studied, but it should contain a greater number of classical errors due to self-reporting and occupations being truly time varying. It is important to note that occupational volatility is considered

[12] Statistics Sweden has extensively studied measurement errors in our education measure, as reported in Isacsson (2004). The conclusion is that measurement errors are not near classical but systematic (e.g., constant over- or underestimates, rather than fluctuations). Importantly, Isacsson found that this systematic measurement error's effect on estimates of income returns on education is zero in cross-sectional ordinary least squares regressions. This is an indication that our specific systematic measurement errors produce little bias in our case. More importantly, following Isacsson (2004), it is clear that applying corrections for classical measurement errors as in the errors-in-variables framework, which is often routinely done in the mobility literature, for processes where measurement is not classical can lead to overadjustment bias.

low compared to income, even though occupations are not as stable in younger birth cohorts as they were for older cohorts. We have nonetheless used averages of multiple measurements to net out any volatility or classical errors.

Our third outcome variable is wealth. This variable, coming from the wealth register, is also a blend of information: tax records, which are essentially self-reports (but under formal sanctions by the state), and administrative data from banks, insurance companies, and estate registers. The measurement error properties for this composite are unknown to us; however, as wealth holdings will vary over time, we have averaged over the eight years (1999–2007) to remove some of this volatility. In sum, our approach is to measure outcomes with the highest precision possible, using as much data as possible to net out potential measurement errors, but without making ad hoc adjustments that could do more harm than good if the true measurement error properties are unknown.

Second Approach: Dynastic Correlations

Figure 3 describes the main idea of dynastic correlations (Adermon et al. 2021): instead of estimating the correlations between single ancestors and the current generations, we measure the correlation between the entire kinship structure and the current generation. The idea is that we can tap the latent SES of all observed ancestors by averaging across their individual SES values. In our application of dynastic correlations, we take the average of all kin in each past generation (the average of all observed parents, all grandparents, etc.). As opposed to Adermon et al., we do not study any

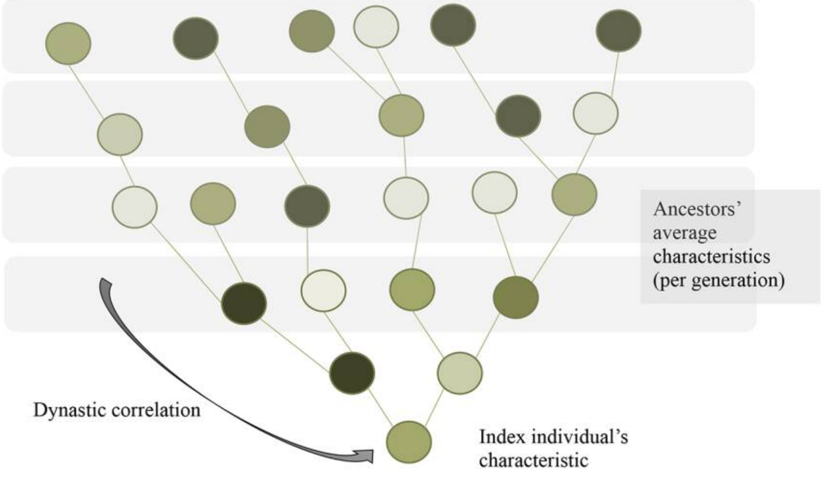


FIG. 3.—Data structure for dynastic correlations.

horizontal (in-law) relations, as we want our cousin correlations to generalize more directly to vertical transmission of status.

We construct the SES information of our ancestors through two different approaches (table B5 shows the data used). First, we use occupational status scores measured using the historical HISCAM version of the CAMSIS status scale. However, compared to other occupational scales, such as Treiman's Standard International Occupational Prestige Scale (SIOPS) or the status scale ISEI, intergenerational persistence between two generations is estimated to be lower with CAMSIS (Hällsten 2019); thus, we expect our results for the HISCAM to be a lower bound. Second, we empirically construct a kin class scale by using the ancestors' HISCLASS codes and inferring their relative SES by how well they predict the outcomes of offspring many generations later. The idea is to rescale the discrete class categories to the expected levels of the descendants' outcomes (Björklund and Sundström 2006; Hällsten and Pfeffer 2017). This means that we do not assume that status is constant over time (as in the HISCAM), only that the HISCLASS grouping is stable over time (compare the discussion of status decline in Song et al. [2020]).

For each ancestor we can identify in the data, we regress the outcome in our index generation on the ancestor's social class while controlling for the birth year. In this way, we make no assumptions about any structure of correlations between kin with certain relations to our index individuals; this is entirely decided by the data. We take the predicted margins for each class as the expected value that the descendant contributes. This is a continuous measure (although with lumpy distribution) and something that we can average across kin. We then summarize all the nonmissing predicted margins of various combinations of ancestors and produce what we denote a "kin class scale." We have used three different approaches to define this occupational scale: by ancestor relation, by ancestor generation, and by type of ancestor generation (we describe this in more detail in app. D). We choose the last of these schemes, which is conservative (and should be robust). We then take averages of these HISCAM or kin class scale scores separately for each ancestor generation, generating six different measures per approach (from generation -1 to -6). A major advantage of this approach is that we get robust measures of SES by generation across different lineages where we often do not observe all potential ancestors, and this varies systematically by lineage (e.g., some lineages are smaller than others, and others have primarily maternal rather than paternal kin). Our approach also minimizes measurement errors by using a large amount of information for each lineage.

Table D1 shows how these measures correlate across generations. These correlations are far from perfect, suggesting that each generation contributes independent information (also implying substantial mobility across generations in historical times). We also note that the correlations attenuate more strongly for the HISCAM than for the empirical kin class scale, suggesting that it may be more limited for multigenerational transmission. We normalize all the measures to mean 0 and standard deviation 1, but for interpretation, we will focus on standardized coefficients equivalent to intergenerational correlations (IGC) but for the multigenerational case (MGC).

Our final analytical step is to correlate the value of each ancestry generation with our youngest index generation, both separately for each ancestor's generation and in a joint model. We do this in a linear regression model, controlling for index generations with birth year interacted with gender. Following Adermon et al. (2021), we use robust standard errors. We expect our occupational class measure to be imperfect and, thus, for the estimates to be subject to (most likely) attenuation bias. As such, we believe these dynastic measures of ancestors' class will provide lower bound estimates of persistence.

RESULTS

We present our main results in two sections for our two approaches and then proceed with sensitivity analyses. The sensitivity analyses do not invalidate our findings.

Cousin Correlations

We begin our analyses with the horizontal perspective and estimate sibling and cousin correlations. Figure 4 shows cousin correlations in education, starting with sibling correlations. We find the first cousin correlation in education to be around .13, but we also estimate the sibling correlation to be around .32. Compared to previous estimates for Sweden, the sibling correlation (Björklund and Jäntti 2012) and first cousin correlation (Hällsten 2014) are slightly weaker in our data. This is in line with intergenerational estimates (Brandén 2018) showing weaker correlations in our area in modern times. Still, it may also reflect some homogeneity selectivity (what Solon [1989] refers to as homogeneity bias) as our sample is largely descendants of farmers from a specific region.

As we would expect, the size of the cousin correlation drops sharply across generations. For second cousins, it is estimated to around .05, and for third cousins it is .021. But it remains substantial for fourth cousins where the estimate is .011 and even for fifth cousins where it is .005. All these correlations are statistically significant (the smallest t-value is 12). It should be noted that cousin correlations are of the same dimension as explained variance (R^2). For example, our coefficient of .011 for fourth cousins is comparable to a multigenerational correlation r of .10 ($.011^{1/2}$), and our estimate of .005 for fifth cousins translates to an r of .07, which is certainly nonnegligible.

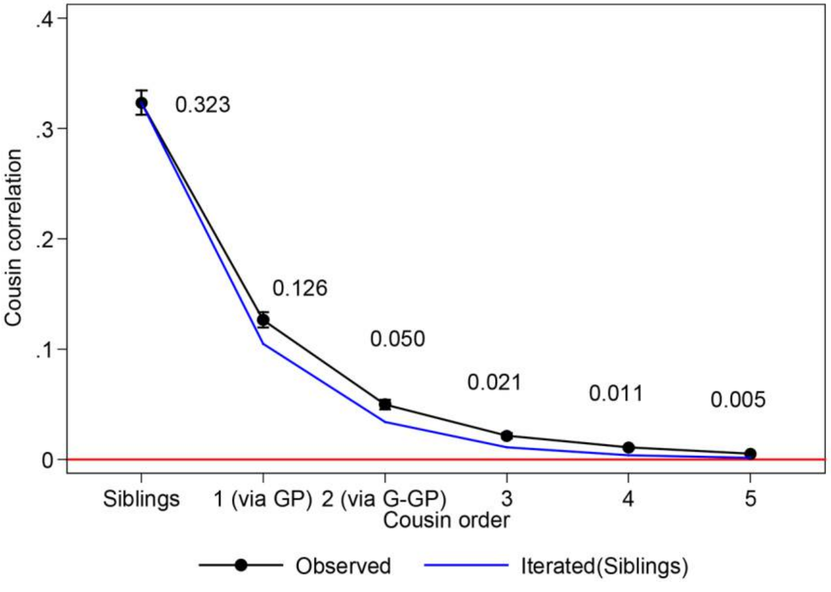


FIG. 4.—Cousin correlations in years of education with iteration based on sibling correlation. Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden.

We compare our estimated correlations to iterations of the sibling correlation, to test whether a first-order Markov process would fit the data, in which resemblance between distant kin is just a function of transmission of social status between parents and children repeatedly over many generations. We find that all our cousin correlations are substantially higher than this projection. For third, fourth, and fifth cousins, the estimated correlations are twice as high as the projection or higher. However, these projections may depend on the point of departure, and we have therefore computed what the sibling correlation would be under a Markov model based on the different cousin correlations. This is shown in figure C1. All cousin correlations imply stronger sibling correlations than what we observe, and this increases with higher cousin order, so the rejection of the Markov model is robust.

We can also translate these numbers into substantive differences in education. We use the back-of-the-envelope method of Solon et al. (2000, p. 390) that translates the sibling correlation, as an effect of a latent variable, into shifts in the standard deviation of the outcome. Using estimates on (untransformed) years of education displayed in table 1 (that are very similar to those using ranks), we have a sibling correlation of around .32; that is, the variation in education is more than 3 times as large as the latent kinship factor (1/.32), and the standard deviation is then 1.75 times larger ($[1/.32]^{1/2}$). A

TABLE 1
COUSIN AND SIBLING CORRELATIONS IN ABSOLUTE YEARS OF EDUCATION AND OCCUPATION (Absolute ISEI Scores)

Cousin Order	ρ	SE	t	95% CI		Iterated (Sibling)	Difference	Unique Ancestors	No. Ties
Education:									
0 (sibs)	.327	.005	62.8	.317	.337	.327		25,030	52,123
1	.127	.004	28.8	.118	.135	.107	.02	12,573	251,396
2	.050	.003	19.8	.045	.054	.035	.015	9,481	1,274,838
3	.021	.001	17.8	.019	.024	.011	.01	12,447	10,789,168
4	.011	.001	18.2	.010	.012	.004	.007	8,503	21,536,050
5	.005	.001	10.2	.004	.006	.001	.004	5,841	35,416,306
Occupation:									
0 (sibs)	.311	.006	47.8	.298	.323	.311		24,111	49,135
1	.122	.003	35.9	.115	.129	.096	.026	12,436	238,446
2	.050	.003	18.5	.045	.055	.03	.02	9,430	1,209,023
3	.022	.001	16.8	.019	.024	.009	.012	12,403	10,235,505
4	.010	.001	13.9	.008	.011	.003	.007	8,474	20,379,704
5	.005	.001	9.6	.004	.006	.001	.004	5,822	33,528,817

NOTE.—Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden. All estimates are based on untransformed outcomes (i.e., not using rank transform) and weighting scheme 2. CI = confidence interval.

1-SD change in the kinship factor is associated with a $1/1.75 = 0.57$ SD change in years of education. We can translate this into mean differences using the standard deviation of education displayed in table B3 (SD = 2.47, with a mean of 12.66). A 1-SD (out of approximately four possible SDs) change in the latent kinship factor is associated with 1.41 more years of education (2.47×0.57). Using this calculation, we find that 1 SD of the latent first cousin factor increases education by 0.88 years, the second cousin factor gives 0.55 more years, and the third and fourth cousin factors give 0.36 and 0.26 more years respectively. The fifth cousin correlation translates to .17 more years of education. If we were to use the translation offered by Duncan and Raudenbush (1999, p. 33), the effect sizes corresponding to our ICCs (interclass correlations) would be even larger.[13] Even though these calculations are only indicative at best, they suggest that kinship has a substantial bearing on inequality.

How do these results apply to occupation and wealth? Table 2 displays the results for education and adds occupation and wealth. For occupation, the correlations are marginally smaller, for example, the first cousin correlation is now .114, and the third is .020. However, the same conclusion that the first-order Markov process does not fit the data still holds, as observed correlations

[13] Duncan and Raudenbush (1999) specify that for two equal-sized experimental groups, ICC $= R^2 = d^2(d^2 + 4)$, where d is the SD difference in group means (effect). Solving for d yields $d = [-4R^2/(R^2 - 1)]^{1/2}$, and with a sibling correlation of .32, we get $d = 1.38$ and a difference of 3.42 years of education (1.38×2.47) instead of 1.41.

TABLE 2
Cousin and Sibling Correlations in Years of Education, Occupation (ISEI Scores), and Wealth

Cousin Order	ρ	SE	t	95% CI		Iterated (Sibling)	Difference	Unique Ancestors	No. Ties
Education:									
0 (sibs)	.323	.006	57.8	.312	.334	.323		25,030	52,123
1	.126	.004	35.1	.119	.133	.105	.022	12,573	251,396
2	.050	.002	22.6	.045	.054	.034	.016	9,481	1,274,838
3	.021	.001	17.7	.019	.024	.011	.010	12,447	10,789,168
4	.011	.001	17.7	.009	.012	.004	.007	8,503	21,536,050*
5	.005	.000	12.3	.004	.006	.001	.004	5,841	35,416,306*
Occupation:									
0 (sibs)	.289	.006	47.4	.277	.301	.289		24,111	49,135
1	.114	.003	36.7	.108	.120	.084	.030	12,436	238,446
2	.046	.002	19.0	.041	.050	.024	.022	9,430	1,209,023
3	.020	.001	16.6	.018	.022	.007	.013	12,403	10,235,505
4	.009	.001	14.8	.008	.010	.002	.007	8,474	20,379,704*
5	.004	.001	8.8	.003	.005	.001	.004	5,822	33,528,817*
Wealth:									
0 (sibs)	.304	.005	60.8	.294	.314	.304		23,916	48,426
1	.076	.004	21.7	.069	.083	.092	−.016	12,415	234,382
2	.030	.002	14.8	.026	.034	.028	.002	9,419	1,190,794
3	.013	.001	12.6	.011	.015	.009	.004	12,399	10,068,614
4	.008	.001	13.2	.007	.009	.003	.005	8,470	20,031,320*
5	.005	.001	9.8	.004	.006	.001	.004	5,819	32,965,923*

Note.—Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden. All estimates are based on ranks and weighting scheme 2. CI = confidence interval.

* Sample of pairs (see text for details).

are higher than the iterated sibling correlation. Hence, both outcomes that have a strong relation to skills in the labor market come out similarly. For wealth, we see different results. While the sibling correlation is similar in magnitude, there is a strong difference between the sibling correlation, on the one hand, and all the cousin correlations, on the other. The cousin correlations follow the same decay pattern as before but are smaller in magnitude compared to the sibling correlation.

This means that the multigenerational part is weaker, but kin close to the index kin matter as much as for other outcomes. Still, the fifth cousin correlation in wealth is very similar to education and occupation. Iterating from the sibling correlations, the projection from a first-order Markov model now provides better fit for first cousins, but then the observed correlations are larger than the projection. Taking into consideration that wealth stems from a different process and is generated more by direct inheritance and less from labor market activities, the results are remarkably similar rather than different.

Still, we find statistically significant fifth cousin correlations of very similar magnitude across all outcomes (.004 to .005, with t-values between 9 and 12). All in all, these results suggest that within our local sample, inequality tends to persist across five generations with a more complex transmission mechanism than simple parent-child transfer.

Heterogeneity by Ancestor Position

In figure 5 and table 3, we estimate cousin correlation separately by ancestors' class positions. We use the class position of the cousin base, that is, the ancestor who is the shared ancestor of the cousins. We give priority to information about the husband in a dyad, but use information on the female spouse if male information is missing (e.g., using information on a maternal grandfather and if missing a maternal grandmother). We code HISCLASS classes into five groups—farm, service, skilled, lower/unskilled, and upper—using the collapsed scheme of table B4 (we group lower/unskilled together ignoring the farm distinction). Because we are most interested in the long-term impact, we zoom in on second to fifth cousins in figure 5 (table F1 provides information also for siblings and first cousins).

For all cousin orders, we find strikingly larger correlations for those descending from upper-class positions (the correlation is larger because of larger covariance, and the variance is rather stable across categories; see table F5).

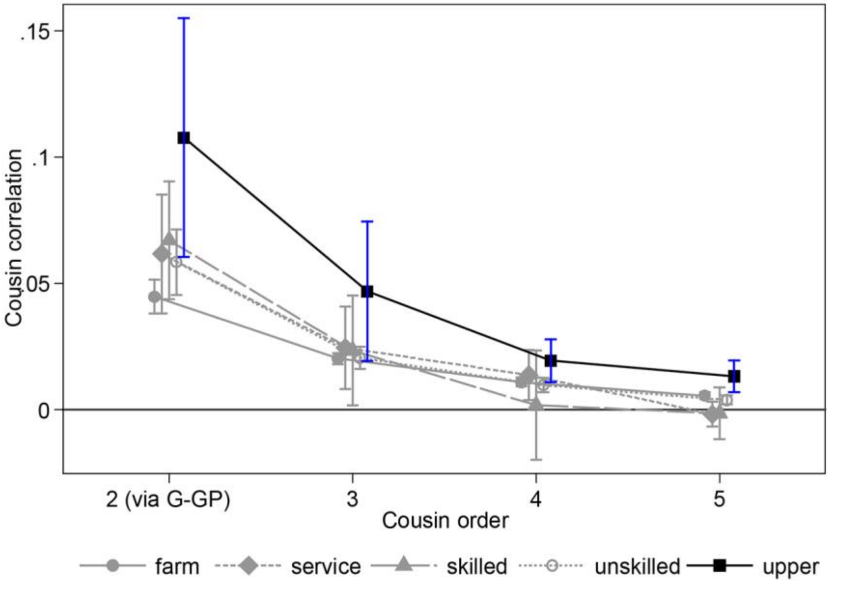


FIG. 5.—Higher-order cousin correlations in years of education by ancestor's SES. Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden.

TABLE 3
Cousin Correlations in Years of Education by Class Position of Ancestor

Cousin Order/ Class of Ancestor Defining Cousins	ρ	SE	*t*	95% CI		Iterated (Sibling)	Difference	Unique Ancestors	No. Ties
0:									
Farm	.284	.021	13.7	.244	.325	.284		1,647	4,811
Service	.277	.011	26.1	.256	.298	.277		7,903	15,013
Skilled	.304	.009	32.0	.285	.323	.304		7,805	16,208
Lower/ unskilled	.348	.015	23.2	.319	.377	.348		3,251	6,945
Upper	.433	.013	32.4	.407	.460	.433		4,353	8,969
1:									
Farm	.112	.006	19.0	.100	.124	.081	.031	5,000	144,414
Service	.170	.011	16.2	.149	.190	.077	.093	1,535	17,511
Skilled	.126	.012	10.6	.103	.150	.092	.034	2,381	30,566
Lower/ unskilled	.128	.008	16.7	.113	.143	.121	.007	2,958	51,460
Upper	.212	.028	7.5	.157	.267	.188	.024	555	6,020
2:									
Farm	.045	.003	13.1	.038	.051	.023	.022	6,207	1,006,645
Service	.062	.012	5.1	.038	.085	.021	.04	561	39,394
Skilled	.067	.012	5.6	.044	.090	.028	.039	741	54,561
Lower/ unskilled	.058	.007	8.8	.045	.071	.042	.016	1,785	160,379
Upper	.108	.024	4.5	.060	.155	.081	.026	126	12,367
3:									
Farm	.020	.001	18.4	.018	.022	.007	.014	7,724	7,483,641
Service	.025	.008	3.0	.008	.041	.006	.019	299	114,520
Skilled	.023	.011	2.1	.002	.045	.009	.015	379	107,789
Lower/ unskilled	.021	.002	9.3	.016	.025	.015	.006	3,789	2,847,852
Upper	.047	.014	3.3	.019	.074	.035	.011	178	92,976
4:									
Farm	.011	.001	12.1	.009	.013	.002	.009	4,655	12,337,519
Service	.014	.005	2.7	.004	.024	.002	.012	196	357,497
Skilled	.002	.011	.2	−.020	.023	.003	−.001	135	147,234
Lower/ unskilled	.010	.001	6.9	.007	.012	.005	.005	2,849	7,419,814
Upper	.019	.004	4.5	.011	.028	.015	.004	171	588,769
5:									
Farm	.005	.001	7.9	.004	.007	.001	.005	2,524	14,270,205
Service	−.002	.003	−.7	−.007	.003	.000	−.002	149	878,716
Skilled	−.002	.005	−.3	−.012	.009	.001	−.002	63	210,817
Lower/ unskilled	.004	.001	4.2	.002	.006	.002	.002	1,909	7,835,121
Upper	.013	.003	4.1	.007	.019	.007	.007	147	2,069,653

Note.—All estimates are based on weighting scheme 2. CI = confidence interval.

The size of the higher correlation in the upper category almost compares to going up one generation; for example, the third cousin correlation of upper is similar to the second cousin correlation for the rest of the ancestor classes. For fifth cousins, the upper category stands out very much in relative terms.

Moreover, for second and third cousins, farm stands out for having lower correlations, but this is not the case for higher-order cousins. In sum, it seems that farm descendants, if anything, have lower cousin correlations. This is important because it suggests that we can then rule out the whole persistence process being merely about a farm versus nonfarm divide.

Dynastic Correlations

We now shift to a vertical perspective and estimate dynastic correlations. First, we estimate models between our index generation and each ancestor generation separately to quantify gross persistence over many generations. Second, we include all generations simultaneously, in order to estimate the net contributions of each generation. Figure 6 shows dynastic correlations for education estimated from both the average HISCAM and our constructed kin class scale (see the second approach: dynastic correlations section above). The full regression output results are displayed in table D2, and appendix D contains additional discussion regarding different specifications of our dynastic correlation approach.

In figure 6, the first estimate (-1 generations back, for parents) comes from a (separate) regression of the index generation's education rank on parents' average occupational characteristics, the second estimate (-2 generations back) comes from a regression on grandparents' occupational characteristics, and so on. We focus on the standardized coefficient (i.e., IGC; $b \times [SD(X)/SD(Y)]$) but apply this for the MGC. The results for parents reveal that the estimates are lower than what has been found in prior literature, especially for the HISCAM (even though the current estimates are a hybrid across SES dimensions, descendants' education on ancestors' occupations, this also holds for HISCAM-on-HISCAM analysis).[14] Thus, it seems that the HISCAM fails to fully capture the persistence across generations. Even the better fitting constructed kin class scale may not be fully optimal, but this is a trade-off to have one consistent measure for all generations. Nonetheless, for both scales, the results strongly reject the first-order Markov model. We generally find that the kin class scale achieves stronger dynastic correlation than the HISCAM, but otherwise, the patterns are remarkably similar, with only weak decay in the more distant generations. With the HISCAM, we find that the point estimate is more stable for the last generation of ancestors than the preceding one. This lack of decay is likely an effect of more effectively reducing measurement (or realization) errors by averaging across a fast-growing number of ancestor kin for each prior generation.

[14] Björklund and Jäntti estimated the father-daughter correlation in education to be around 0.33 ($0.11^{1/2}$) and the father-son correlation to be 0.39 ($0.15^{1/2}$) for individuals born from 1951 to 1967 (see table 4 in Björklund and Jäntti 2012). The parent-child correlation in CAMSIS for individuals born from 1956 to 1982 is estimated to be 0.31 (Hällsten 2019).

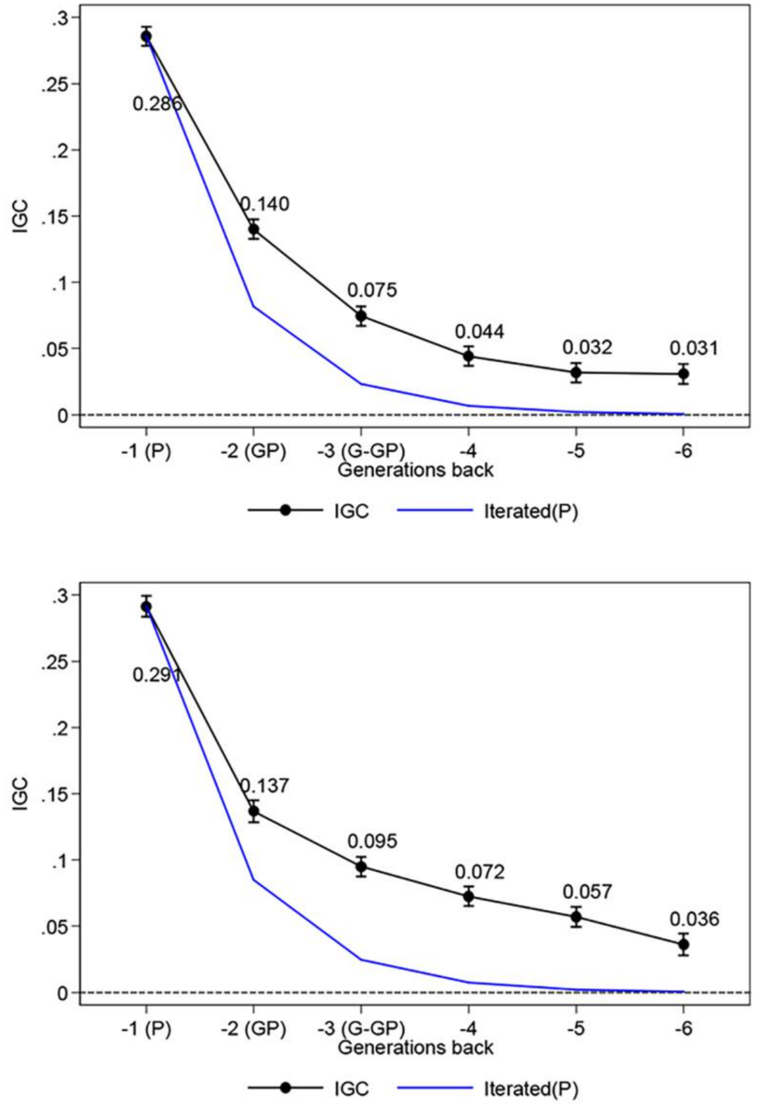


FIG. 6.—Dynastic correlations in years of education in separate models by generation using HISCAM (*top*) and kin class scale (*bottom*). Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden.

Table 4 summarizes our findings and includes estimates for occupation and wealth (essentially summarizing the model structure from table C2). For each outcome measure, its first column displays the MGC (and the second column its standard error). For reference, the third column presents the addition in R^2 compared to an empty model, which is very close to the square root of the MGC, and the fourth and fifth columns display R^2 and the number of observations for reference.[15] The first row shows an empty model for comparison, followed by separate regressions by generation on the following six rows. We find somewhat stronger correlations for occupation, which may not be surprising given that occupation is both a dependent and an independent variable. For wealth, the dynastic correlations are generally weaker, which is mainly because the grandparental correlation is weaker. However, for more distant kin, the correlations are very similar across the three outcome measures. When we compare the results to cousin correlations, the main difference is for wealth and for proximate kin. With cousin correlations, we found relatively strong first cousin correlation (i.e., reflecting grandparents), but with dynastic correlations, the grandparent correlations are relatively weaker. We speculate that this is because occupation and class are more generic stratification markers in historic times (and strongly linked to wealth), while they are more exclusively linked to labor market inequality in modern times and less to wealth (cf. Hällsten and Thaning 2022). For wealth, we also find a larger discrepancy between the HISCAM and the kin class scale optimized for wealth; the HISCAM captures a smaller part of the association from the empirical scale.

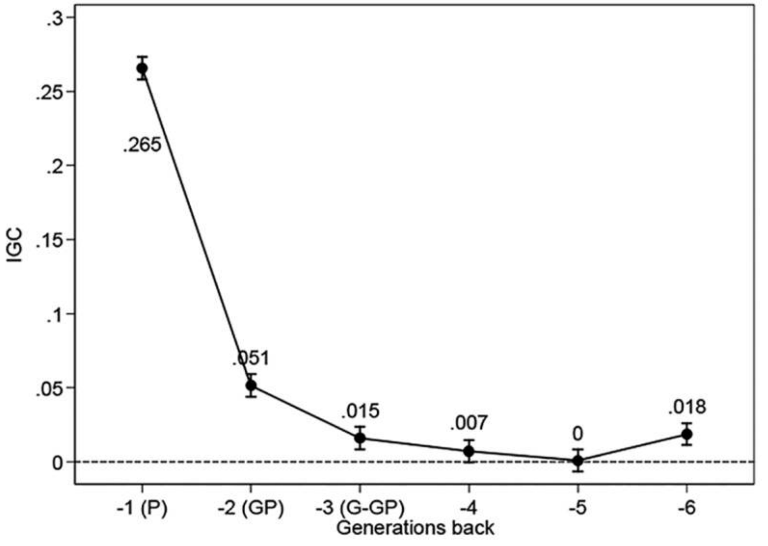
We estimate a higher-order Markov model for education by including all generations (-1 to -6) jointly in the same regression (i.e., an AR(6) model). The results for education are shown in figure 7 (model 8 in table D2). The dynastic correlations for each of the generations are now mutually controlled. All generations have substantially weak but significant contributions, except for generation -5 in the HISCAM specification, and generation -6 in the kin class scale, meaning that the model provides an acceptable fit to the data (in alternative specifications of the kin class scale, generation -6 turns out positive; see figs. D1 and D2). However, the difference between parents and later generations is substantial: the estimates for the middle generations are much lower, meaning that parents contain most of the information from these past generations, a conclusion similar to that of Björklund and Jäntti (2020), Engzell et al. (2020), and Lundberg (2020). Nonetheless, the sum of the MGC for each separate generation is the total dynastic correlation (Adermon et al. 2021). The final row for each panel of table 4 shows the

[15] We note that there are some differences between R^2 and ΔR^2 to the multigenerational correlations derived from b coefficients, which is an indication of homogeneity biases (Solon 1989). Adermon et al. (2021, n. 17) also notes that R^2 has more downward bias than sums of b-coefficients. We therefore do not focus much on R^2 and ΔR^2.

TABLE 4
DIFFERENT SPECIFICATIONS OF DYNASTIC CORRELATIONS

	Years of education					Occupation (ISES Scores)					Wealth				
	MGC	SE	ΔR^2	R^2	N	MGC	SE	ΔR^2	R^2	N	MGC	SE	ΔR^2	R^2	N
Average HISCAM:															
Empty (only controls)				.047	67,884				.007	66,072				.006	67,563
Generation −1, P	.286	.003	.079	.126	67,884	.306	.003	.091	.098	66,072	.111	.003	.012	.018	67,563
Generation −2, GP	.140	.003	.019	.066	67,884	.153	.003	.023	.03	66,072	.061	.003	.004	.01	67,563
Generation −3, G-GP	.075	.003	.005	.053	67,884	.082	.003	.007	.014	66,072	.033	.003	.001	.007	67,563
Generation −4	.044	.003	.002	.049	67,884	.052	.003	.003	.01	66,072	.032	.003	.001	.007	67,563
Generation −5	.032	.003	.001	.048	67,884	.028	.003	.001	.008	66,072	.035	.003	.001	.007	67,563
Generation −6	.031	.003	.001	.048	67,884	.026	.003	.001	.008	66,072	.032	.003	.001	.007	67,563
Generation −1 to −6 jointly*	.360		.083	.13	67,884	.382		.095	.102	66,072	.182		.014	.02	67,563
Kin class scale:															
Empty (only controls)				.047	67,826				.007	66,014				.006	67,505
Generation −1, P	.291	.004	.069	.116	67,826	.318	.004	.083	.09	66,014	.121	.003	.014	.020	67,505
Generation −2, GP	.137	.004	.015	.062	67,826	.179	.004	.023	.031	66,014	.080	.003	.006	.012	67,505
Generation −3, G-GP	.095	.003	.009	.056	67,826	.092	.003	.008	.015	66,014	.083	.003	.007	.013	67,505
Generation −4	.072	.003	.005	.052	67,826	.073	.003	.005	.013	66,014	.056	.003	.003	.009	67,505
Generation −5	.057	.003	.003	.050	67,826	.053	.003	.003	.01	66,014	.048	.003	.002	.008	67,505
Generation −6	.036	.004	.001	.048	67,826	.043	.003	.002	.009	66,014	.035	.003	.001	.007	67,505
Generation −1 to −6 jointly*	.411		.076	.123	67,826	.453		.091	.099	66,014	.247		.022	.028	67,505

NOTE.—All outcomes are in rank form. MGC = b × [SD(X)/SD(Y)]. P = parent; GP = grandparent; G-GP = great-grandparent.
* For the joint specification, the MGC is summed over all generations.



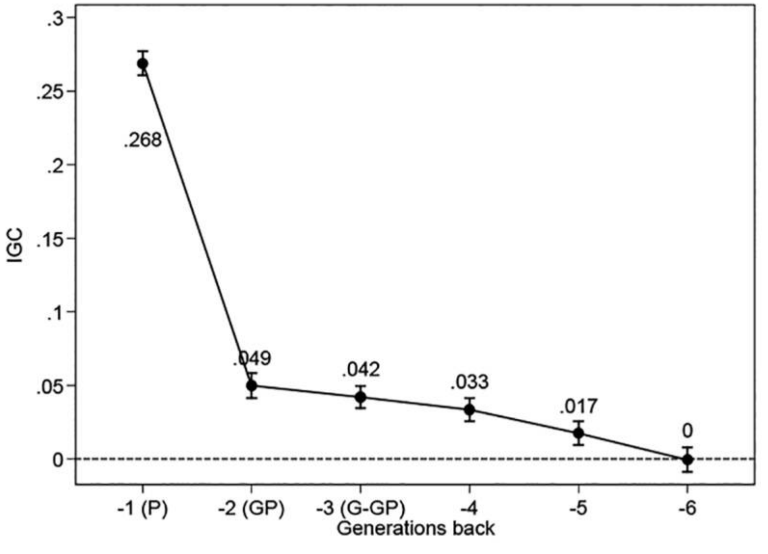


FIG. 7.—Dynastic correlations in years of education in models where generations are included together using HISCAM (*top*) and kin class scale (*bottom*). Men and women born 1940–87 in Sweden with a fifth-generation descendant in northern Sweden.

sum of the MGC coefficient in the joint specification. This figure is very similar for education and occupation, at above 0.40 with the kinship scale and 0.36–0.38 with the HISCAM. For wealth, the total dynastic correlation is lower (0.18–0.25). When we compare the total dynastic correlation with the parental correlation, we find that it is substantially larger, similar to Adermon et al. (2021). For wealth, where the parent association is rather weak, the total dynastic correlation is up to twice as high. To sum up our findings, even though the level of inequality transmitted over generations is generally low, it is persistent, and a two-generation parent-child model underestimates this persistence. Even though wealth is less connected to education and occupation, the similarity in results in all three outcomes for distant kinship are striking.

Sensitivity Analyses

We conducted a number of sensitivity analyses, as shown in appendixes D–H, to address the heterogeneity of our estimates (differences across subgroups), population selectivity (how different features of our population influence our estimates), and bias (how our sample estimates are representative of the population). Our study design spans an immense sociocultural transformation over the 19th and 20th centuries. In appendix D, we examine the assumption of the dynastic correlation approach (Adermon et al. 2021), namely, that averaging over ancestors will increase the signal-to-noise ratio and capture latent SES, which holds up empirically. We have variation in the number of ancestors we observe, and we find that the multigenerational correlations are stronger, the more ancestors we average over. Historical occupational measures provide social status measurement with relatively high imprecision, and the more ancestors you average over, the more measurement error is reduced and the signal-to-noise ratio improved. Using dynastic correlation methods is thus an advantage for analyzing multigenerational transmission for historical data. This also suggests that our main estimates presented above may be underestimating persistence.

Also in appendix D, we examine how the intergenerational parent-child correlation has changed across generations (as estimated by averages by generation, using the scales for our dynastic correlation approach). To understand long-range social mobility across five or more generations, it is helpful to understand how parent-child status transmissions have changed over time, as multigenerational resemblance is shaped by multiple correlations in this realm. We find the IGCs to be remarkably stable (typically around 0.3, ranging from 0.2 to 0.35) when using both the observed occupational class (CAMSIS) and a kin class scale approach. In appendix E, we examine the impact of the size of the kinship network and conclude that size is not a strong driver of the cousin correlations we observe. We examine heterogeneity in cousin correlations in appendix F. We find that cousin correlations are somewhat stronger along maternal lineages, but not stronger among strictly paternal lineages, suggesting that inheritance (surnames and to a lesser extent wealth) from father to son is not driving our results. We also find that cousin correlations are somewhat stronger among men and weaker among women (despite gender having been residualized) and mixed-gender cousins. In another analysis, we find that there is some substantial variation in sibling correlations by age for individuals in our index generation, yet for cousins of any order, there is not any clear pattern. In the appendix, we also provide a methodological discussion of our analyses of cousin correlations split by ancestor's class.

In appendix G, we examine the impact of kin marriage by a sensitivity analysis in which we discard the one-third of the kinship networks where any cousin marriage is prevalent, but the results are virtually unchanged. We therefore argue that cousin or kin marriages are unlikely to drive our main findings. In appendix H, we address selectivity by comparing our local northern population to a Swedish reference population. We find that our locale does not deviate much from the reference in terms of education, occupation, and wealth, and we generally find lower persistence in our local sample than in the population, indicating that our estimates are likely to be lower bound compared to the rest of Sweden.

The main source of bias with reference to our target population is (selective) out-migration. In appendix I, we analyze migration patterns in historical times. We find that our local sample is influenced by selective migration biased toward ancestors being farmers and ancestors who were residentially stable for other reasons. In order to assess the impact of selective out-migration, we conduct a simple simulation of multigenerational correlations where sample truncation occurs selectively on the basis of intermediary generations' outcome levels (so that we remove an entire kinship if generations 2 or 3 have high SES) in appendix J. This reveals that multigenerational correlations attenuate with such positive selectivity. Our simulation is based on the premise that, as a strategy for upward mobility, high-status individuals are more likely to out-migrate. We underestimate rather than overestimate true dynastic or cousin correlations with such a scenario. In appendix K, we address whether our kinship effects may reflect geographical clustering. While this is an intriguing question of causality, and we lack data to properly evaluate this (since neither kinship nor location is exogenous), we find that our estimated kinship effects exist beyond geographical clustering, so our finding cannot be reduced to geography in disguise.

DISCUSSION

Our findings suggest that prior generations may structure life chances more than 200 years later. Distant family members—even if they are not necessarily aware of each other's existence—show much more resemblance than expected from a simple sequential transmission from parents to children. At the same time, the role of ancestors is small overall, and mobility far outweighs persistence. The highest long-term transmission of inequality we have estimated is for descendants' education, where the fourth cousin correlation (including unobserved factors) of 0.011 would amount to a multigenerational correlation of 0.10 ($0.011^{1/2}$), and the dynastic correlation (only including observed occupations) for generation −6 is 0.031 (HISCAM) or .036 (our kin class scale). This may still be seen as substantive transmission, given how far it has traveled, yet mobility strongly dominates over persistence. However, we also observe more substantial persistence at the top. For descendants of ancestors with advantaged class positions, long-term persistence is much stronger (the third cousin correlation of .047 in this group—see table 3—would amount to a multigenerational correlation of .22 over five generations). This suggests that social persistence is larger at the top of society, something that a few prior studies have found for two and three generations (Björklund, Roine, and Waldenström 2012; Hällsten 2014) but that we know can generalize to many more generations.

We find that the results for occupation closely resemble those for education, whereas wealth, not surprisingly, is slightly different. This is due mainly to mobility patterns in proximate generations. However, when we focus on the role of more distant kinship, the similarity in persistence is very striking. It is interesting to note that, similar to several recent studies, the persistence rate across generations beyond grandparents is rather stable and incompatible with a first-order (parent-child) Markov process. However, even though we can fit up to a sixth-order Markov model with the dynastic correlations approach (i.e., with generational lags under mutual control), the direct associations from more distant generations are small and do not contribute much more inequality than what already resides in parental and grandparental coefficients. Taken together, these findings suggest that (*a*) most transmissions in the earliest generations are sequential from generation to generation, but what is transferred remains mostly intact rather than decaying, and (*b*) what is transferred is only a partial explanation of life chances within that generation. This is close to the latent factor model of Clark and Cummins (2014) and Braun and Stuhler (2018), where some endowment is transferred across generations and then translated into inequality within that generation. In their model, high multigenerational correlations will occur without the existence of transfers that skip a generation. The important point here is that the model implies that the transfer of a trait from parents to children is strong and that the translation of the trait into outcomes is weak; this is what creates this persistent pattern.

Because of the lack of direct social interaction across distant generations, we can rule out any larger direct ancestor effects, but the scope for quasi-direct effects (transfers from parents not originating in parents) is much larger. We may only speculate about this, but physical assets (such as farmland or property), strong norms or skills, or symbolic assets may be potential explanations.

This scenario would also fit with an inheritance of racial or ethnic categories (Torche and Corvalan 2016), that is, something more precisely inherited (depending on closure) and that may structure life chances but not perfectly so (depending on exclusion and marginalization).[16] Nonetheless, shifting the perspective to kinship structures as social groups, where social reproduction works at the group level, appears to be a fruitful endeavor, and it should be a future aim to identify more kin group-level characteristics that explain persistence.

Our findings on the long-run persistence of family background agree with increasing attention to discussions of multigenerational effects and a broader conceptualization of family background beyond the influence of parents only (Mare 2011). While mobility in most Western countries is high as measured in traditional stratification research, we also observe clear social closure in elite societal positions. We argue that our empirical findings can be interpreted as support for the existence of long-lasting family background effects that may help explain such phenomena. Some aspects of status that may explain our findings are family symbols, cultural capital, and other cultural aspects of family identity that may not be measured in observed parental (or grandparental) characteristics.

Our results are from a society that is often characterized as both mobile and equal. However, if we find clear evidence of long-lasting effects of kinship in a society such as Sweden, this raises the issue of similar or larger effects being found in societies with higher social closure, such as the United States. Future research should examine the long shadow of family backgrounds in societies such as the United States with greater income inequality and continental European societies that are characterized by a more rigid class structure, where an entrenched gentry with a monopoly on economic and political power has played a more important historical societal role. Researchers may also want to explore the role to which explicitly kin-oriented social institutions such as family businesses, occupational inheritance (e.g., among medical doctors), the kind of dynastic identities reflected in naming practices such John D. Rockefeller III, or generation-skipping trusts may explain our findings (Mare 2011).

Given the long time period that our kinship networks evolve over, when ancestors experienced the industrial revolution and significant demographic transitions, our findings are also likely contingent on several period effects, given how kinship, the occupational structure, and social mobility have changed over the period (Lee 2003; Song et al. 2020). We want to stress, however, that these changing processes are simply a reflection of the research question we ask. When we examine the influence of great-great-great-grandparents

[16] Although northern Sweden has an indigenous population of Samis, their prevalence in our region is low (Alm-Stenflo 1994).

on current life chances, these ancestors will have lived in vastly different circumstances, and any long-run mobility is shaped just as much by social mobility in the late 19th century as by Sweden's contemporary education system. We think our approaches are attractive, as they can detect the extent of resemblances across family members living in a present society without extensive modeling on inter- and multigenerational mobility across dramatically different socioeconomic contexts. Looking for the influence of long-run multigenerational persistence in a society that is unchanging over hundreds of years may be an interesting theoretical exercise through which we may learn about social mobility. Still, it is not empirically reflective of extended family background structures or family chances in an early 21st-century industrialized country. Interestingly, to the extent that we can examine intergenerational mobility with our estimates, it appears to be largely stable (app. D).

Our data set is local in nature in two ways: it comes from Sweden, which is one of the most egalitarian countries in the world for social mobility and equality of life chances, but our region is also one of the most egalitarian parts of Sweden. A considerable share of agricultural workers owned their own land. Unlike large parts of Europe and Sweden, the extent of "proletarianization" of agriculture (where agricultural workers work without tenure on someone else's land) was low (Alm Stenflo 1994). The region has also been described as having a history of above-average social and cultural capital, reflected in, for example, high literacy (Sörlin 2014).[17] Our results must be seen in the light of coming from an egalitarian corner of one of the most egalitarian countries in Europe. Finally, measurement errors play an important role in mobility research. With historical data, these problems are even more acute. We have used cousin and dynastic correlations as methodological approaches that handle measurement errors well, but we do not think we have been able to cancel all biases. With more data covering many generations becoming available, it is necessary to use but also to further develop methods than can handle measurement errors in historical data. It may be that reliance on methods grounded in noisy measurements of historical occupations has led researchers of historical stratification to underestimate the role of family background. Our results from dynastic correlations may help reconcile popular and narrative conceptions of historical Europe as consisting of rigid and immobile societies where social status was primarily prescribed at birth with the sometimes surprisingly high measurements of social mobility from stratification research (e.g., van Leeuwen and Maas 2010).

[17] To underline this, we can add that a number of famous Swedish authors stem from this region: P. O. Enqvist, Stieg Larsson (known for the best-selling *Millennium* trilogy), Stig Larsson (a different author), Sara Lidman, Torgny Lindgren, and Nikanor Teratologen.

REFERENCES

Adermon, Adrian, Mikael Lindahl, and Mårten Palme. 2021. "Dynastic Human Capital, Inequality and Intergenerational Mobility." *American Economic Review* 111:1523–48.

Adermon, Adrian, Mikael Lindahl, and Daniel Waldenström. 2018. "Intergenerational Wealth Mobility and the Role of Inheritance: Evidence from Multiple Generations." *Economic Journal* 128: F482–F513.

Alm Stenflo, Gun. 1994. *Demographic Description of the Skellefteå and Sundsvall Regions during the 19th Century*. Umeå: Demografiska databasen.

Anderson, Lewis R., Paula Sheppard, and Christiaan W. S. Monden. 2018. "Grandparent Effects on Educational Outcomes: A Systematic Review." *Sociological Science* 5:114–42.

Barone, Guglielmo, and Sauro Mocetti. 2020. "Intergenerational Mobility in the Very Long Run: Florence, 1427–2011." *Review of Economic Studies* 88:1863–91.

Becker, Gary S., and Nigel Tomes. 1986. "Human Capital and the Rise and Fall of Families." *Journal of Labor Economics* 4 (3): S1–S39.

Bengtsson, Tommy, Martin Dribe, and Patrick Svensson. 2012. The Scanian Economic Demographic Database, version 3.0. Machine-readable database. Lund University, Centre for Economic Demography.

Berger, Thor, Per Engzell, Björn Eriksson, and Jakob Molinder. 2021. "Social Mobility in Sweden before the Welfare State." SocArxiv. https://osf.io/preprints/socarxiv/ebmva/.

Bihagen, Erik. 2007. "Nya möjligheter för stratifieringsforskning i Sverige: Internationella yrkesklassificeringar och stratifieringsmått över tid" [New opportunities for social stratification research in Sweden: International occupational classifications and stratification measures over time]. *Sociologisk Forskning* 44:52–67.

Bittles, Alan H., and Inez Egerbladh. 2005. "The Influence of Past Endogamy and Consanguinity on Genetic Disorders in Northern Sweden." *Annals of Human Genetics* 69:549–58.

Björklund, Anders, and Markus Jäntti. 2012. "How Important Is Family Background for Labor-Economic Outcomes?" *Labour Economics* 19:465–74.

Björklund, Anders, and Markus Jäntti. 2020. "Intergenerational Mobility, Intergenerational Effects, Sibling Correlations, and Equality of Opportunity: A Comparison of Four Approaches." *Research in Social Stratification and Mobility* 70:100455.

Björklund, Anders, Jesper Roine, and Daniel Waldenström. 2012. "Intergenerational Top Income Mobility in Sweden: Capitalist Dynasties in the Land of Equal Opportunity?" *Journal of Public Economics* 96:474–84.

Björklund, Anders, and Marianne Sundström. 2006. "Parental Separation and Children's Educational Attainment: A Siblings Analysis on Swedish Register Data." *Economica* 73:605–24.

Blanden, Jo. 2013. "Cross-Country Rankings in Intergenerational Mobility: A Comparison of Approaches from Economics and Sociology." *Journal of Economic Surveys* 27:38–73.

Bouchard, Thomas J., and Matt McGue. 1981. "Familial Studies of Intelligence: A Review." *Science* 212:1055–59.

Brandén, Gunnar. 2018. "Does Inequality Reduce Mobility? The Great Gatsby Curve and Its Mechanisms." Ph.D. dissertation. Uppsala University, Department of Economics.

Braun, Sebastian Till, and Jan Stuhler. 2018. "The Transmission of Inequality across Multiple Generations: Testing Recent Theories with Evidence from Germany." *Economic Journal* 128:576–611.

Breen, Richard. 2010. "Educational Expansion and Social Mobility in the 20th Century." *Social Forces* 89:365–88.

Breen, Richard. 2018. "Some Methodological Problems in the Study of Multigenerational Mobility." *European Sociological Review* 34:603–11.

Campbell, Cameron, and James Z. Lee. 2011. "Kinship and the Long-Term Persistence of Inequality in Liaoning, China, 1749–2005." *Chinese Sociological Review* 44:71–103.

Chadwick, Laura, and Gary Solon. 2002. "Intergenerational Income Mobility among Daughters." *American Economic Review* 92:335–44.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where Is the Land of Opportunity? The Geography of Intergenerational Mobility in the United States." *Quarterly Journal of Economics* 129:1553–623.

Clark, Gregory. 2014. *The Son Also Rises: Surnames and the History of Social Mobility*. Princeton, N.J.: Princeton University Press.

Clark, Gregory, and Neil Cummins. 2014. "Intergenerational Wealth Mobility in England, 1858–2012: Surnames and Social Mobility." *Economic Journal* 125:61–85.

Cole, Stephen R., and Miguel A. Hernán. 2008. "Constructing Inverse Probability Weights for Marginal Structural Models." *American Journal of Epidemiology* 168:656–64.

Collado, M. Dolores, Ignacio Ortuño-Ortín, and Jan Stuhler. 2022. "Estimating Intergenerational and Assortative Processes in Extended Family Data." Unpublished manuscript. Universidad Carlos III de Madrid. https://janstuhler.files.wordpress.com/2022/02/manucript_v44_paper.pdf.

Cox, Donald, and Marcel Fafchamps. 2007. "Extended Family and Kinship Networks: Economic Insights and Evolutionary Directions." Pp. 3711–84 in *Handbook of Development Economics*, edited by T. Paul Schultz and John A. Strauss. Amsterdam: North-Holland.

Dackling, Martin. 2013. *Släktgårdens uppkomst: Jord och marknad i Skaraborg, 1845–1945*. Göteborg: Institutionen för historiska studier, Göteborgs universitet.

Dahl, Molly W., and Thomas DeLeire. 2008. "The Association between Children's Earnings and Fathers' Lifetime Earnings: Estimates Using Administrative Data." Discussion paper no. 1342-08. University of Wisconsin—Madison, Institute for Research on Poverty.

Domingue, Benjamin W., Jason Fletcher, Dalton Conley, and Jason D. Boardman. 2014. "Genetic and Educational Assortative Mating among US Adults." *Proceedings of the National Academy of Sciences* 111:7996–8000.

Dribe, Martin, and Jonas Helgertz. 2016. "The Lasting Impact of Grandfathers: Class, Occupational Status, and Earnings over Three Generations in Sweden, 1815–2011." *Journal of Economic History* 76:969–1000.

Dribe, Martin, Jonas Helgertz, and Bart Van de Putte. 2015. "Did Social Mobility Increase during the Industrialization Process? A Micro-Level Study of a Transforming Community in Southern Sweden, 1828–1968." *Research in Social Stratification and Mobility* 41:25–39.

Duncan, Greg J., and Stephen W. Raudenbush. 1999. "Assessing the Effects of Context in Studies of Child and Youth Development." *Educational Psychologist* 34:29–41.

Edvinsson, Sören. 2000. "The Demographic Data Base at Umeå University—a Resource for Historical Studies." Pp. 231–48 in *Handbook of International Historical Microdata for Population Research*, edited by Patricia Kelly Hall, Robert McCaa, and Gunnar Thorvaldsen. Minneapolis: Minnesota Population Center.

Egerbladh, Inez, and Alan H. Bittles. 2011. "Socioeconomic, Demographic and Legal Influences on Consanguinity and Kinship in Northern Coastal Sweden, 1780–1899." *Journal of Biosocial Science* 43:413–35.

Elwert, Felix, and Christopher Winship. 2014. "Endogenous Selection Bias: The Problem of Conditioning on a Collider Variable." *Annual Review of Sociology* 40:31–53.

Engzell, Per, Carina Mood, and Jan O. Jonsson. 2020. "It's All about the Parents: Inequality Transmission across Three Generations in Sweden." *Sociological Science* 7:242–67.

Erikson, Robert, and John H. Goldthorpe. 1992. *The Constant Flux: A Study of Class Mobility in Industrial Societies*. Oxford: Clarendon.

Erikson, Robert, John H. Goldthorpe, and Martin Hällsten. 2012. "No Way Back Up from Ratcheting Down? A Critique of the 'Microclass' Approach to the Analysis of Social Mobility." *Acta Sociologica* 55:211–29.
Eriksson, Karin Hederos, Randi Hjalmarsson, Matthew J. Lindquist, and Anna Sandberg. 2016. "The Importance of Family Background and Neighborhood Effects as Determinants of Crime." *Journal of Population Economics* 29:219–62.
Euler, Harald A. 2011. "Grandparents and Extended Kin." Pp. 181–210 in *The Oxford Handbook of Evolutionary Family Psychology*, edited by Catherine Salmon and Todd K. Shackelford. New York: Oxford University Press.
Feinstein, Brian D. 2010. "The Dynasty Advantage: Family Ties in Congressional Elections." *Legislative Studies Quarterly* 35:571–98.
Frisell, Thomas, Paul Lichtenstein, and Niklas Långström. 2011. "Violent Crime Runs in Families: A Total Population Study of 12.5 Million Individuals." *Psychological Medicine* 41:97–105.
Ganzeboom, Harry B. G., Paul M. De Graaf, and Donald J. Treiman. 1992. "A Standard International Socio-Economic Index of Occupational Status." *Social Science Research* 21:1–56.
Ganzeboom, Harry B. G., and Donald J. Treiman. 1996. "International Stratification and Mobility File: Conversion Tools." Unpublished manuscript. http://www.harryganzeboom.nl/ISMF/index.htm. Accessed June 4, 2020.
Hällsten, Martin. 2014. "Inequality across Three and Four Generations in Egalitarian Sweden: 1st and 2nd Cousin Correlations in Socio-Economic Outcomes." *Research in Social Stratification and Mobility* 35:19–33.
Hällsten, Martin. 2019. "Scaling Occupations in Intergenerational Research: An Evaluation with Swedish Register Data." SocArxiv. https://osf.io/preprints/socarxiv/uxy9q/.
Hällsten, Martin, and Fabian T. Pfeffer. 2017. "Grand Advantage: Family Wealth and Grandchildren's Educational Achievement in Sweden." *American Sociological Review* 82:328–60.
Hällsten, Martin, and Max Thaning. 2022. "Wealth as One of the 'Big Four' SES Dimensions in Intergenerational Transmissions." *Social Forces* 100 (4): 1533–60.
Hodge, Robert W. 1966. "Occupational Mobility as a Probability Process." *Demography* 3:19–34.
Hsueh, W. C., B. D. Mitchell, R. Aburomia, T. Pollin, H. Sakul, M. Gelder Ehm, B. K. Michelsen, M. J. Wagner, P. L. St. Jean, W. C. Knowler, D. K. Burns, C. J. Bell, and A. R. Shuldiner. 2000. "Diabetes in the Old Order Amish: Characterization and Heritability Analysis of the Amish Family Diabetes Study." *Diabetes Care* 23:595–601.
Isacsson, Gunnar. 2004. "Estimating the Economic Return to Educational Levels Using Data on Twins." *Journal of Applied Econometrics* 19:99–119.
Jaeger, Mads Meier. 2012. "The Extended Family and Children's Educational Success." *American Sociological Review* 77:903–22.
Jäntti, Markus, Bernt Bratsberg, Knut Røed, Oddbjørn Raaum, Robin Naylor, Eva Österbacka, Anders Björklund, and Tor Eriksson. 2006. "American Exceptionalism in a New Light: A Comparison of Intergenerational Earnings Mobility in the Nordic Countries, the United Kingdom and the United States." IZA Discussion paper no. 1938. Institute of Labor Economics, Bonn.
Jäntti, Markus, and Stephen P. Jenkins. 2015. "Income Mobility." Pp. 807–935 in *Handbook of Income Distribution*, edited by Anthony B. Atkinson and François Bourguignon. Amsterdam: Elsevier.
Jonsson, Jan O., David B. Grusky, Matthew Di Carlo, Reinhard Pollak, and Mary C. Brinton. 2009. "Micro-Class Mobility: Social Reproduction in Four Countries." *American Journal of Sociology* 114:977–1036.
Kelley, Jonathan. 1973. "Causal Chain Models for the Socioeconomic Career." *American Sociological Review* 38:481–93.

Killewald, Alexandra, Fabian T. Pfeffer, and Jared Schachner. 2017. "Wealth Inequality and Accumulation." *Annual Review of Sociology* 43:379–404.
Knigge, Antonie. 2016. "Beyond the Parental Generation: The Influence of Grandfathers and Great-Grandfathers on Status Attainment." *Demography* 53:1219–44.
Kolk, Martin, and Martin Hällsten. 2017. "Demographic and Educational Success of Lineages in Northern Sweden." *Population and Development Review* 43:491–512.
Kolk, Martin, and Vegard Skirbekk. 2022. "Fading Family Lines—Women and Men without Children, Grandchildren and Great-Grandchildren in 19th, 20th, and 21st Century Northern Sweden." *Advances in Life Course Research* 53:100481.
Kotlikoff, Laurence J., and Lawrence H. Summers. 1981. "The Role of Intergenerational Transfers in Aggregate Capital Accumulation." *Journal of Political Economy* 89:706–32.
Kuusela, Hanna. 2018. "Learning to Own: Cross-Generational Meanings of Wealth and Class-Making in Wealthy Finnish Families." *Sociological Review* 66:1161–76.
Laband, David N., and Bernard F. Lentz. 1985. "Favorite Sons: Intergenerational Wealth Transfers among Politicians." *Economic Inquiry* 23:395–414.
Lambert, Paul S., Richard L. Zijdeman, Marco H. D. Van Leeuwen, Ineke Maas, and Kenneth Prandy. 2013. "The Construction of HISCAM: A Stratification Scale Based on Social Interactions for Historical Comparative Research." *Historical Methods: A Journal of Quantitative and Interdisciplinary History* 46:77–89.
Lee, Ronald. 2003. "The Demographic Transition: Three Centuries of Fundamental Change." *Journal of Economic Perspectives* 17:167–90.
Lindahl, Mikael, Mårten Palme, Sofia Sandgren-Massih, and Anna Sjögren. 2014. "A Test of the Becker-Tomes Model of Human Capital Transmission Using Microdata on Four Generations." *Journal of Human Capital* 8:80–96.
Lindahl, Mikael, Mårten Palme, Sofia Sandgren Massih, and Anna Sjögren. 2015. "Long-Term Intergenerational Persistence of Human Capital: An Empirical Analysis of Four Generations." *Journal of Human Resources* 50:1–33.
Long, Jason, and Joseph Ferrie. 2018. "Grandfathers Matter(ed): Occupational Mobility across Three Generations in the US and Britain, 1850–1911." *Economic Journal* 128: F422–F445.
Lundberg, Ian. 2018. "Measuring Multigenerational Mobility: What Can We Learn from Cousin Correlations." Office of Population Research, Princeton, N.J.
Lundberg, Ian. 2020. "Does Opportunity Skip Generations? Reassessing Evidence from Sibling and Cousin Correlations." *Demography* 57:1193–213.
Maas, Ineke, and Marco Van Leeuwen. 2016. "HISCLASS." IISH Data Collection, V1. International Institute of Social History. https://hdl.handle.net/10622/HEFSW2.
Mare, Robert. 2011. "A Multigenerational View of Inequality." *Demography* 48:1–23.
Mare, Robert D., and Xi Song. 2014. "Social Mobility in Multiple Generations." Working paper series no. 14-2014. California Center for Population Research.
Masulis, Ronald W., Peter Kien Pham, and Jason Zein. 2011. "Family Business Groups around the World: Financing Advantages, Control Motivations, and Organizational Choices." *Review of Financial Studies* 24:3556–600.
Mazumder, Bhashkar. 2008. "Sibling Similarities and Economic Inequality in the US." *Journal of Population Economics* 21:685–701.
Modalsli, Jørgen. 2017. "Intergenerational Mobility in Norway, 1865–2011." *Scandinavian Journal of Economics* 119:34–71.
Modalsli, Jørgen. 2023 "Multigenerational Persistence: Evidence from 146 Years of Administrative Data." *Journal of Human Resources* 58 (3): 929–61.
Modin, Bitte, Robert Erikson, and Denny Vågerö. 2013. "Intergenerational Continuity in School Performance: Do Grandparents Matter?" *European Sociological Review* 29:858–70.
Nybom, Martin, and Jan Stuhler. 2017. "Biases in Standard Measures of Intergenerational Income Dependence." *Journal of Human Resources* 52:800–825.

Prandy, Kenneth, and Paul Lambert. 2003. "Marriage, Social Distance and the Social Space: An Alternative Derivation and Validation of the Cambridge Scale." *Sociology* 37:397–411.

Ridge, John M. 1974. *Mobility in Britain Reconsidered*. Oxford: Clarendon.

Robins, James M., Miguel A. Hernán, and Babette Brumback. 2000. "Marginal Structural Models and Causal Inference in Epidemiology." *Epidemiology* 11:550–60.

Robinson, Matthew R., et al. 2017. "Genetic Evidence of Assortative Mating in Humans." *Nature Human Behaviour* 1:0016.

Rosenthal, Carolyn J. 1985. "Kinkeeping in the Familial Division of Labor." *Journal of Marriage and Family* 47:965–74.

Schaeffer, Merlin. 2014. "The Social Meaning of Inherited Financial Assets: Moral Ambivalences of Intergenerational Transfers." *Historical Social Research/Historische Sozialforschung* 39:289–317.

Sharkey, Patrick, and Felix Elwert. 2011. "The Legacy of Disadvantage: Multigenerational Neighborhood Effects on Cognitive Ability." *American Journal of Sociology* 116:1934–81.

Solon, Gary. 1989. "Biases in the Estimation of Intergenerational Earnings Correlations." *Review of Economics and Statistics* 71:172–74.

Solon, Gary. 1992. "Intergenerational Income Mobility in the United States." *American Economic Review* 82:393–408.

Solon, Gary. 2014. "Theoretical Models of Inequality Transmission across Multiple Generations." *Research in Social Stratification and Mobility* 35:13–18.

Solon, Gary. 2018. "What Do We Know So Far about Multigenerational Mobility?" *Economic Journal* 128: F340–F352.

Solon, Gary, Marianne E. Page, and Greg J. Duncan. 2000. "Correlations between Neighboring Children in Their Subsequent Educational Attainment." *Review of Economics and Statistics* 82:383–92.

Song, Xi. 2020. "Multigenerational Social Mobility: A Demographic Approach." Working paper (PSC/PARC) 2020-52. University of Pennsylvania Population Center.

Song, Xi, and Cameron D. Campbell. 2017. "Genealogical Microdata and Their Significance for Social Science." *Annual Review of Sociology* 43:75–99.

Song, Xi, Cameron D. Campbell, and James Z. Lee. 2015. "Ancestry Matters: Patrilineage Growth and Extinction." *American Sociological Review* 80:574–602.

Song, Xi, Catherine G. Massey, Karen A. Rolf, Joseph P. Ferrie, Jonathan L. Rothbaum, and Yu Xie. 2020. "Long-Term Decline in Intergenerational Mobility in the United States since the 1850s." *Proceedings of the National Academy of Sciences* 117:251–58.

Sörlin, Sverker. 2014. *Pianona i Västerbotten: Det politiska och politikens platser*. Stockholm: Premiss.

Stuhler, Jan. 2012. "Mobility across Multiple Generations: The Iterated Regression Fallacy." IZA Working paper 7072. Institute of Labor Economics, Bonn.

Torche, Florencia, and Alejandro Corvalan. 2016. "Estimating Intergenerational Mobility with Grouped Data: A Critique of Clark's the Son Also Rises." *Sociological Methods and Research* 47:787–811.

van Dongen, Elien, Björn Eriksson, and Martin Dribe. 2018. "Does Anders' Son Also Rise? Class Heterogeneity in Intergenerational Occupational Persistence during Swedish Industrialization." Paper presented at the 2018 Population Association of America meeting, Denver, April 26–28.

van Leeuwen, Marco H. D., and Ineke Maas. 2010. "Historical Studies of Social Mobility and Stratification." *Annual Review of Sociology* 36:429–51.

Warren, John Robert, and Robert M. Hauser. 1997. "Social Stratification across Three Generations: New Evidence from the Wisconsin Longitudinal Study." *American Sociological Review* 62:561–72.

Westberg, Annika, Elisabeth Engberg, and Sören Edvinsson. 2016. "A Unique Source for Innovative Longitudinal Research: The POPLINK Database." *Historical Life Course Studies* 3:20–31.

Yengo, Loic, Matthew R. Robinson, Matthew C. Keller, Kathryn E. Kemper, Yuanhao Yang, Maciej Trzaskowski, Jacob Gratten, Patrick Turley, David Cesarini, Daniel J. Benjamin, Naomi R. Wray, Michael E. Goddard, Jian Yang, and Peter M. Visscher. 2018. "Imprint of Assortative Mating on the Human Genome." *Nature Human Behaviour* 2:948–54.
Young, Michael, and Peter Wilmott. 1957. *Family and Kinship in East London*. Glencoe, Ill.: Free Press.