Dynastic Human Capital, Inequality, and Intergenerational Mobility[†]

By Adrian Adermon, Mikael Lindahl, and Mårten Palme*

We estimate long-run intergenerational persistence in human capital using information on outcomes for the extended family: the dynasty. A dataset including the entire Swedish population, linking four generations, allows us to identify parents' siblings and cousins, their spouses, and spouses' siblings. Using various human capital measures, we show that traditional parent-child estimates underestimate long-run intergenerational persistence by at least one-third. By adding outcomes for more distant ancestors, we show that almost all of the persistence is captured by the parental generation. Data on adoptees show that at least one-third of long-term persistence is attributed to environmental factors. (JEL I24, I26, J12, J24, J62)

The transmission of advantage across generations has far-reaching implications for how we view current levels of inequality and the degree of equality of opportunity in a society. In recent years, research on intergenerational mobility has benefited from availability of data spanning multiple generations, including either observable

family links or links through surnames.[1] Most studies based on such data suggest that the traditional child-parent regression model underestimates the long-term persistence. There is, however, still no consensus on how the difference between the two sets of results should be interpreted, implying very different views on what level of social mobility, ranging from fairly high levels of mobility to almost perfect persistence, that a society is facing.[2]

In this study, we propose a new estimator of long-run intergenerational persistence utilizing information from extended family members. Our modeling framework nests two alternative models. In the first, the latent variable child-parent model,[3] persistence is measured in relation to the position of the parents and the information obtained from the extended family members is merely used to solve a measurement problem. The second model, the extended family model, allows for direct influences from members of the extended family. A key finding of this paper is that a *lower bound* of the long-run intergenerational persistence implied by both of these models is estimated by the coefficient sum from a linear regression of the human capital outcomes of the children on the human capital outcomes of the extended family members. We also show that by cumulatively adding more and more extended family members, it is possible to obtain a progressively tighter lower bound estimate of long-run intergenerational persistence.

We use Swedish administrative data enabling us to construct family trees spanning four generations. We observe several individual human capital outcomes and measures of social background in the data. GPA in the last year of compulsory schooling is used to measure educational outcomes for up to 541,000 individuals in the child generation. Data from several administrative registers and censuses, including information on educational attainment, labor earnings, and occupation, for the period 1968–2009 are used to construct outcomes for everyone in the parental generation. The fact that the entire Swedish population is included in the data, and that we observe all intergenerational links up to great-grandparents, allows us to link dynasties in the parental generation including parents' siblings and cousins; siblings' and cousins' spouses; and siblings of spouses of aunts and uncles.

Our results unambiguously suggest that long-run persistence in human capital is much stronger than what we get from the traditional child-parent regression model used in most empirical studies on intergenerational mobility in human capital outcomes. By restricting the analysis to child-parent regressions, one misses at least one-third of overall persistence. Using years of schooling as the outcome for the extended family members in the parental generation, we estimate long-run persistence to be 0.52. As a sensitivity analysis we show that these estimates are unaffected by omitted group effects, such as those stemming from schools or neighborhoods. We also show evidence supporting the external validity of our findings, so

---

[1] See, e.g., Chan and Boliver (2013), Lindahl et al. (2015), Braun and Stuhler (2018), and Long and Ferrie (2018), which all use data where families have been linked through multiple generations, and, e.g., Clark (2014), Clark and Cummins (2015), and Barone and Mocetti (2016), who use data where generations have been linked through surnames.

[2] Some are skeptical of the new findings, arguing that the influence of ancestors might be spurious (Solon 2018) and that results based on surnames estimate a different parameter than the one obtained from traditional child-parent regressions (Chetty et al. 2014, Solon 2018).

[3] Used in the previous literature by Stuhler (2012), Clark (2014), and Braun and Stuhler (2018).

that estimates from child-parent regressions on other datasets are expected to underestimate long-run persistence to the same extent as in our study.

This paper contains four extensions to the main analysis described above. First, we combine information from three different measures for human capital outcomes in the parental generation (years of schooling, lifetime family income, and an index of occupational-based social stratification) using the proxy variable method proposed by Lubotsky and Wittenberg (2006).[4] The results from this analysis suggest an even stronger intergenerational persistence in human capital than the one obtained in the main analysis. Second, we show that estimates from an IV model, using extended family members' human capital outcomes as instrumental variables for parents' human capital outcomes, can be interpreted as an upper bound estimate of long-run intergenerational persistence. For years of schooling, the resulting estimated bounds of long-run intergenerational persistence are 0.52 and 0.61, respectively, rising to 0.60 and 0.66 when we combine the three proxies of human capital. Third, we estimate multigenerational models including outcomes for grandparents as well as great-grandparents. From these estimates we conclude that most of the persistence across generations is accounted for by the extended family in the parental generation, and that a prediction model with only vertical ancestors underestimates long-run intergenerational persistence. Fourth, we use outcomes from adopted children, rather than from those raised by their biological parents, to estimate extended family regression models.

The main contribution of this paper is that it provides a framework for estimating long-run intergenerational persistence using direct measures based on *observed* extended family relations. Several previous studies that have used surname groupings as indicators of social class (e.g., Clark 2014, Clark and Cummins 2015) have found evidence of very strong intergenerational persistence.[5] A limitation of this approach is that families with the same surname, but that are otherwise unrelated, can share factors determined outside the extended family, such as residential location or ethnicity, implying that the persistence across generations attributed to family ties may be overestimated. A related critique applies to papers based on the sibling correlation approach, since siblings typically share more than the social or economic position of the family, such as schools and neighborhoods.[6] Furthermore, any idiosyncratic shock affecting one sibling will be captured by this measure if

---

[4] Vosters and Nybom (2017) and Vosters (2018) apply this approach to estimate intergenerational income persistence using information on children and parents.

[5] Olivetti and Paserman (2015) use a similar strategy but instead of surnames they use first names to create pseudo links between fathers and sons and daughters. Güell, Rodríguez-Mora, and Telmer (2015) use the information contained in rare surnames, in combination with cross-sectional data, to estimate the intergenerational persistence in educational attainment. Santavirta and Stuhler (2019) review name-based approaches to intergenerational mobility research and list 14 such studies either published in journals or as working papers over the period 2012–2018.

[6] A major problem in this case is that exogenous school or neighborhood effects, not attributed to the family, but shared by siblings, will bias the estimates of intergenerational family persistence upward. For this reason, researchers have attempted to estimate the importance of the neighborhoods and the school, by using correlations in outcomes between children in the same neighborhoods (see Solon 1999 and Solon, Page, and Duncan 2000). For recent surveys of studies using the sibling correlation approach for economic outcomes, see Björklund and Salvanes (2011) and Björklund and Jäntti (2012). In Collado, Ortuño-Ortín, and Stuhler (2019), the authors estimate intergenerational and sibling correlations using the same dataset and conclude that these two approaches estimate different underlying parameters.

siblings influence each other directly.[7] In contrast to these studies, our detailed data on extended family links allow us to separate out the contribution of the family from other determinants. This is further validated by the robustness of our results to using an extended regression model controlling for neighborhood or school-by-year fixed effects.

In addition to being related to previous studies on individual intergenerational mobility, this paper is also akin to the literature repeatedly showing that group-level effects is a key mechanism behind persistence in socioeconomic positions across generations. Following the social capital theory (see Coleman 1988) and the strain theory (see Merton 1938), there is abundant evidence of the importance of social class. There is also a large empirical literature on the importance of race and ethnicity (Borjas 1992, Hertz 2008, Torche and Corvalan 2018). However, perhaps surprisingly, there are to our knowledge no previous studies measuring the intergenerational transmission using outcomes for the overall extended family, the group most closely connected to the parents' socioeconomic position, and therefore most closely related to the traditional parent-child measure of social mobility.[8]

Our rich data allow us to incorporate the approaches of several recent papers attempting to estimate long-run intergenerational persistence into our analysis. Our main source of variation is in the parental generation, which has the advantage of including extended family members for whom it is possible to measure outcomes at a similar point in time, and with better data quality than for more distant generations. However, we are also able to add the vertical dimension by adding the outcomes of ancestors (as in the recent multigenerational literature, e.g., Lindahl et al. 2015, Braun and Stuhler 2018, and Long and Ferrie 2018) as well as to add several proxies for human capital (as in Vosters and Nybom 2017 and Vosters 2018).

The paper proceeds as follows. Section I presents and discusses the underlying latent human capital models as well as the empirical specifications. Section II introduces the dataset, discusses construction of variables and presents descriptive statistics. Section III presents the main results, which are for estimates of long-run intergenerational persistence of human capital, emanating from regression models with years of schooling for extended family members included as explanatory variables. We also check the sensitivity of our results with respect to school and neighborhood effects, and discuss external validity of our findings. Section IV shows the results from four extensions of the main analysis described above, i.e., models using additional proxy variables for human capital; models for obtaining upper bound estimates of long-run intergenerational persistence; multigenerational models; and models for adoptees. Finally, Section V concludes.

---

[7] Evidence of positive spillover effects in educational outcomes are found in Landersø, Nielsen, and Simonsen (forthcoming) and Nicoletti and Rabe (2019).

[8] Several previous studies have added outcomes for selected extended family members when estimating intergenerational associations (e.g., Warren and Hauser 1997, Jæger 2012, Hällsten 2014). However, while they have added outcomes for these relatives to a regression model they have not focused on combining the information to produce a measure of long-run intergenerational persistence. The studies by Jæger (2012) and Hällsten (2014) instead takes the extended family into account by estimating cousin-correlations in outcomes.

I. Econometric Model

A. *Latent Variable Models and Long-Run Intergenerational Persistence*

The previous literature on intergenerational mobility emphasizes two reasons as to why the traditional child-parent regression model might underestimate the true level of persistence across generations. First, observed human capital offers a poor measure of latent human capital. The underlying reasons could range from simple measurement problems, to the educational attainment variable (even if measured correctly) being a poor indicator of an individual's latent human capital.[9] Second, it precludes influence from members of the extended family other than the parents. Such influences can for instance go through direct monetary or nonmonetary investments (e.g., quality time spent with grandchildren or nieces/nephews) as well as through passing on norms or acting as role models in a way that affects the human capital of the child.[10]

The following linear model, which we label the *Extended family model*, accommodates both these points of criticism:[11]

$$(1) \qquad y_c^* = \beta_0 + \beta_1 y_p^* + \beta_2 y_{sp}^* + \beta_3 y_{cp}^* + \cdots + \beta_K y_K^* + \varepsilon_c,$$

where y_c^* and y_k^*, $k \in \{p, sp, cp, \ldots, K\}$, are latent variables for the human capital outcome under study for the child (c) and each part of the dynasty in relation to the child, such that p stands for parent, sp for sibling of parent, cp for cousin of parent, etc., up to the Kth member of the dynasty.[12] In principle, a model for external family influences should also include ancestor generations other than the parental generation. For now, however, we focus on influences from extended family members in the *parental* generation and discuss the multigenerational case in Section IVC. We also assume that ε_c is independent of the y_k^* variables.[13]

In this linear model, long-run intergenerational persistence can be conveniently obtained by the sum of the marginal effects from increasing y_k^* by one unit for all K members of the extended family, conditional on the other $K - 1$

members, (i.e., as $\sum_{k=1}^{K} \beta_k$), as this can be used to calculate an individual's expected dependence on ancestors n generations back (i.e., as $(\sum_{k=1}^{K} \beta_k)^n$).[14]

An interesting special case of (1) is the latent variable AR(1) human capital model,

$$(2) \qquad\qquad y_c^* = \beta_0 + \beta y_p^* + \varepsilon_c,$$

which is obtained from equation (1) by imposing the restriction $\beta_k = 0$ for $k \in \{2, \ldots, K\}$. This model has been used as an analytical framework in several previous empirical studies focusing on estimating long-term persistence in human capital (see, e.g., Stuhler 2012, Clark 2014, Clark and Cummins 2014, and Braun and Stuhler 2018).[15] The advantage that persists relative to the mean child from one unit higher y_k^*, n generations ahead, is then simply defined as β^n.

B. Empirical Models

In the empirical setting, the latent variables y_c^* and y_k^* are not observed. For each child and dynasty member k, we observe the variables $y_q = y_q^* + u_q$, $q \in \{c, k\}$, consisting of the latent variable and a stochastic component u_q, which, in our basic framework is $\sim iid(0, \sigma_q^2)$. This means we assume that the stochastic components u_q are uncorrelated across extended family members within and between generations, as well as with the latent variables. We discuss a relaxation of the first part of this assumption ($\sigma_{u_k u_{k'}} = 0$) below. Each y_q is standardized to have mean zero and unity standard deviation.

We can estimate different versions of the following model using observable data:

$$(3) \qquad\qquad y_c = b_0 + b_1 y_p + b_2 y_{sp} + b_3 y_{cp} + \cdots + b_M y_M + e_c,$$

where y_c and y_k, $k \in \{p, sp, cp, \ldots, M\}$, are the human capital outcomes that we observe, and where we allow the number of observed proxies M to potentially differ from the number of latent variables K included in equation (1). It is well known that when at least some of the y_k terms are imperfect proxies for the y_k^* terms, the OLS estimates of the K parameters $\beta_1, \beta_2, \ldots, \beta_K$ will be biased in unknown directions (see Proposition 1 in the online Appendix). On the other hand, the focus in this paper is not on particular elements of the β vector, but on long-run intergenerational persistence. As shown above, this parameter is the sum of $\beta_1, \beta_2, \ldots, \beta_K$ in the extended family model (1), or β in the latent variable AR(1) model (2). The key question is, therefore, to what extent the estimates of $b_1, b_2, \ldots, b_M$ from equation (3) are informative about long-run persistence in the two models, respectively.

We propose $\sum_{k=1}^{M} b_k$ as an estimator of long-run intergenerational persistence in human capital. Online Appendix Section A shows, analytically and through simulations, some properties of this sum of coefficients estimator under various

---

[14] This calculation assumes stationarity of the intergenerational mobility process.

[15] This model was also the concern of earlier work estimating the intergenerational persistence parameter, where the main focus was on correcting for measurement error bias: e.g., Solon (1992), Björklund and Jäntti (1997), and Mazumder (2005).

conditions. First, the sum of the coefficient estimates for the M extended family members from (3) always provides a lower bound of long-run intergenerational persistence (i.e., $b_1 + b_2 + \cdots + b_M \leq \beta_1 + \beta_2 + \cdots \beta_K$), regardless of whether the true model is (1) or (2), and for all M and K (see Proposition 2 in the online Appendix).[16] This is an important result, since K is unknown. Second, for a given K, the downward bias in the sum of coefficients is always reduced by adding additional proxy variables, i.e., the bias is decreasing in M (see online Appendix Section A.2, Proposition 3). Hence, estimating the long-run persistence from the sum of $b_1, b_2, b_3, \ldots, b_M$ will always decrease the bias as the number of included members of the extended family grows. We can therefore exploit the advantage of our data structure and include outcomes for as many extended family members as possible, without the risk of overstating the true level of long-run persistence.

If we allow measurement errors to be positively correlated, the sum of the coefficient estimates remains a lower bound estimate of long-run intergenerational persistence (see online Appendix Section A.2, pp. 5–6). Online Appendix Section A also shows, through Monte Carlo simulations, that in most cases the bias is decreasing in the number of added proxies, although the bias will decrease at a slower rate than in the case with uncorrelated measurement errors (see online Appendix Section A.2, pp. 6–10). Generally, since we expect the marginal contribution of each family member to decrease with the distance in the family relation, i.e., $\beta_1 \geq \beta_2 \geq \cdots \geq \beta_K \geq 0$ and because we are more likely to observe those with closer family relations, we expect the remaining bias to be quite small for the number of M we are able to use in this study.[17]

To sum up, the two results summarized above imply that the estimates from the regression model in equation (3) are informative about long-run intergenerational persistence whether or not the true model for intergenerational transmission of human capital is some version of the extended family model (1), or the latent variable AR(1) model (2). This allows us to be agnostic about which of these models best represents the true causal structure of intergenerational transmission. Irrespectively of whether the extended family provides additional inputs to the outcome of the child, or if they act as proxies for the latent transmission from the parents, the estimates from the extended family regression model (3) can be used to reduce the downward bias in OLS estimates from child-parent regression models used to estimate long-run intergenerational persistence.

[16] A corollary of this proposition is that this result does not depend on the chosen categorization of extended family members, i.e., whether they are divided into parents' siblings, parents' cousins, etc., or into smaller subgroup categories. We use this natural categorization so that it is sufficient to have at least one relative in each of these categories in order to be included in our sample. A finer categorization (e.g., separate categories for relatives on the father's and mother's side) would result in fewer observations.

[17] The R^2 from an extended family regression provides an alternative estimate of long-run intergenerational persistence. There are, however, two reasons to why we choose to focus on the coefficient sum from equation (3) as our main measure of long-term intergenerational persistence. First, and most important, it has the economic interpretation of measuring how human capital advantages in the parental generation are expected to transform into the child generation. As such it connects very closely to the recent literature on estimating long-run intergenerational persistence parameters (e.g., see Nybom and Stuhler 2016; Braun and Stuhler 2018; Long and Ferrie 2018; and Collado, Ortuño-Ortín, and Stuhler 2019), as well as the classical references such as Becker and Tomes (1979, 1986), which define long-run intergenerational persistence with a latent variable model in mind. Second, as shown in online Appendix Section A.3, the R^2 from a regression model like the one in equation (3) has a larger downward bias compared to the coefficient sum from the same regression. This implies that the coefficient sum is a tighter lower bound of its population parameter compared to the R^2.

II. Data and Descriptive Statistics

A. *Data and Key Variables*

Our dataset is compiled from different Swedish registers and linked using the individual identification number. The Swedish Multi-generation Register, which covers the entire Swedish population, enables us to link biological (and adoptive) parents to all children born 1932 or later, provided that the child and the parents have been registered as living in Sweden at some point after January 1, 1961.[18] For our main analysis, we require (i) that we can identify cousins in the parental generation, i.e., that we are able to identify families through four generations; and (ii) that we are able to measure human capital outcomes in the parent and child generations.

We restrict the individuals in the child generation to be born after 1971, since the 1972 birth cohort is the earliest birth cohort where the grade point average (GPA), obtained at the end of compulsory school, normally at age 16, is available in Swedish registry data. The variable is constructed from the national registers, using grades in all compulsory subjects. The reason for using GPA as human capital outcome for the child generation in our main analyses is to maximize the number of observations available over four generations. To capture later education for the child generation we also use years of schooling as an outcome variable, constructed using information from national educational registers. The last year for which we observe data on GPA and educational attainment is in 2009. Hence, the child generation birth cohorts are 1972–1993 for GPA and 1972–1983 for years of schooling (to give the individuals enough time to finish tertiary education). Because grandparents of these child cohorts must be born 1932 at the earliest to be present in our data, the years-of-schooling sample is less representative and much smaller.

We then link these children to their parents and other relatives using pseudo-anonymized personal identification numbers. We construct vertical family links up to great-grandparents, and are thus able to identify cousins in the parent generation and second cousins in the child generation.[19] Marriage and cohabiting registers further extend horizontal links. This allows us to link dynasties up to siblings and cousins of parents, the siblings' and cousins' spouses, and the siblings of the siblings' spouses.

We further compile data from registers (and censuses for earlier years) that contain information on education, income, and occupation for the parental and other ancestor generations. The education information is available in the 1970 census and in yearly registers between 1985 and 2009. Income data are drawn from tax registers and are available for the years 1968, 1971, 1973, 1976, 1979, 1982, and every year between 1985 and 2009. Occupation information is available from censuses every fifth year between 1970 and 1990. To be included in the dataset, we therefore also require that at least one of each *category* of relatives (i.e., siblings of

---

[18] For the non-adopted children, we always use the biological ancestors of the child, regardless of whether these are the parents who raise the children.

[19] We are not the first to link four generations using the Swedish Multigenerational registry (see, e.g., Hällsten 2014 and Persson and Rossin-Slater 2018).

parents, cousins of parents, etc.) in the parental generation must have survived and still be working in 1970.

In the main analysis we use years of schooling of the extended family members in the parental generation as proxy variables for human capital. For each family member, years of schooling is constructed using information on the level of completed education from the educational registers. In an extension of the analysis based on using several proxy variables for human capital for each extended family member, we use two additional outcomes: log income, and a social stratification index, based on the so-called CAMSIS index (Lambert and Bihagen 2012) for occupation-based social stratification (see Section IVA). Online Appendix Section B provides additional information regarding definitions and describes sources for all the variables.

The main independent variables are constructed by taking averages of non-missing observations within each dynasty category of relatives.[20] For example, if for one child we observe years of schooling for three of their four aunts/uncles, the category "aunts/uncles" years of schooling variable will be the average of those three, excluding the fourth.[21] In the dataset used for the analyses we always standardize these dynasty category averages to have mean 0 and standard deviation 1. Our final analysis sample consists of 541,459 observations.

B. *Descriptive Statistics*

Online Appendix Table D.1 shows descriptive statistics for the dataset, focusing on the sample where we use GPA as the child outcome.[22] The first three columns show averages of the number of years of schooling, average residualized log income, and the social stratification index (from 0 to 100) by dynasty category. Note that this table shows means and standard deviations for the original variables, whereas the results tables below always show estimates using standardized variables. The fourth column shows the average number of observations used for calculating the averages corresponding to each category in the dynasty. In effect, we only require one non-missing observation for each category of relatives for a child to be included in the main regressions.

The mean and standard deviation for GPA for the child generation is shown in the top row of online Appendix Table D.1, column 1. The original scores were percentile ranked by birth cohort, in order to take changes in the grading over time into account. The correlation between GPA and years of schooling in the subsample where both measures are available for the same individuals is 0.62.

The means and the standard deviations of years of schooling is similar within generations, but differs a lot across generations. The reason is that the schooling systems have largely remained constant within, but not between, generations. Income

---

[20] Crucially, we include family members of both genders. Using, e.g., only male relatives would bias our estimates toward zero if assortative mating is less than perfect.

[21] We also require that each dynasty category in the parental generation has at least one non-missing observations for years of schooling, income, and occupation (but allow the non-missing observations to come from different individuals). The results are virtually identical without imposing this restriction, but it has the advantage that the main results using the three different proxies can be compared for the same sample.

[22] We show descriptive statistics for the dataset where we use years of schooling as the outcome variable for the child generation in online Appendix Table D.2.

and social stratification are already normalized within birth cohorts, and hence show no trends across generations.

Summary statistics for all categories (except the children) are based on averages over various numbers of individuals, with more observations used for more distant dynasty categories. Hence, the standard deviations of these averaged variables will be lower. This shows why it is important to standardize the averages calculated for each dynasty category, if we want to make the estimates comparable.

Online Appendix Table D.3 shows correlations between the three main variables *Years of schooling*, *Log income*, and the *Social stratification* index, for the different dynasty categories in the parental generation. The highest correlation is observed between *Years of schooling* and the *Social stratification* index, whereas the two correlations with *Log income* are smaller. Although these three variables contain common information, they certainly also capture different dimensions of human capital since the correlations are quite modest, ranging between 0.2 and 0.5 (see the diagonal blocks). As expected, the correlations decrease with the distance between the parental dynasty categories, although the correlations between distant dynasty categories are not negligible.[23] For example, the correlations between parents' outcomes and the outcomes of the most distant category, siblings of spouses of parents' siblings (i.e., of the aunts and uncles), are still between 0.10 and 0.25.

III. Main Results

Table 1 shows the main results. All estimates shown in the table are obtained using GPA in the final year of compulsory schooling as outcome variable. The first part of panel A reports the results where we use OLS regression models and years of schooling of the extended family members in the parental generation as independent variables. The underlying regression models are various versions of equation (3). Column 1 starts by showing the results for the traditional child-parent regression model. Then, we sequentially add parents' siblings, spouses of aunts/uncles, parents' cousins, spouses of parents' cousins, and siblings of spouses of aunts/uncles to the specification. The explanatory variables, each representing the averages of years of schooling for the individuals in the respective extended family category, have been normalized to have standard deviations equal to one. We report the sum of the coefficients in the second row from the bottom of panel A of Table 1.

The results show very precise estimates for all parts of the extended family. The coefficient estimate from the bivariate regression model in column 1 is 0.36. Hence, on average, a child with parents with one standard deviation higher years of schooling, compared to the mean, has slightly more than one-third of a standard deviation higher GPA in the last year of compulsory school. As explained in Section I, estimates from such models are likely to underestimate long-run intergenerational persistence. In columns 2 through 6 we sequentially add years of schooling outcomes for more distant extended family members to the model. However, interpreting these

---

[23] We acknowledge that we use the term "distance" loosely. Since our dynasty definition includes both those that have the same ancestors as the parents (the parents' siblings and cousins) and those with in-law family ties (spouses of parents' siblings and cousins), the distance between the parents and extended family members is not always unambiguous a priori.

TABLE 1—HORIZONTAL GPA-SCHOOLING REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Main results						
Parents	0.361	0.301	0.295	0.286	0.285	0.284
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Aunts and uncles		0.138	0.118	0.109	0.109	0.105
		(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Spouses of aunts/uncles			0.046	0.043	0.042	0.034
			(0.001)	(0.001)	(0.001)	(0.002)
Parents' cousins				0.062	0.053	0.052
				(0.001)	(0.002)	(0.002)
Spouses of parents' cousins					0.018	0.018
					(0.002)	(0.002)
Siblings of spouses of aunts/uncles						0.024
						(0.001)
Sum of coefficients	0.361	0.439	0.460	0.500	0.507	0.518
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.151	0.166	0.168	0.171	0.171	0.172
Panel B. Lubotsky-Wittenberg estimates						
Sum of coefficients	0.465	0.534	0.550	0.585	0.590	0.597
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.186	0.198	0.199	0.202	0.202	0.202

Notes: Each column shows results from a separate regression of child's grade point average on parental generation outcomes. $N = 541,459$ observations. Parental generation variable is years of schooling in panel A, and the LW index of years of schooling, log income, and social stratification in panel B. Each parental generation outcome is the average across all members of the given category of relatives. All variables have been normalized to have mean 0 and standard deviation 1. Robust standard errors in parentheses.

estimates as showing separate dynasty category contributions can be misleading, since estimates of separate coefficients may be biased in unknown directions in the latent variable extended family model.

Focusing, instead, on the results for the sum of the coefficients for the extended family members, it is apparent that the estimates, as expected, increase as the dynasty definition becomes broader. These estimates should be interpreted as the change in the dependent variable associated with a standard deviation unit higher years of schooling of all included dynasty categories. For instance, the estimate of 0.460 in column 3 should be interpreted as the average change from a standard deviation unit higher years of schooling for parents, parents' siblings, and parents' siblings' spouses, where each of these three dynasty groups are weighted equally.

A key result is that the long-term intergenerational persistence of human capital, including the entire extended family in the regression, is estimated to 0.518. This is 43 percent larger than the traditional child-parent estimate of 0.361. As we show in Section I, this is a *lower bound* estimate of the long-run persistence across genera-tions.[24] As evident from the sum of coefficient estimates from the various versions

---

[24] To check whether nonlinearities play an important role in the dynastic intergenerational transmission, we estimate a lasso regression (Tibshirani 1996) where, in addition to the same birth year controls as in the main regressions, we also add all quadratic terms and pairwise interactions between the horizontal relatives' mean years of schooling. We use 10-fold cross-validation to find the optimal value of the tuning parameter, and the lasso selects 68 of the 77 included variables. We then estimate an OLS (post-lasso) regression using these variables. Since direct comparison of coefficients is not straightforward when nonlinear terms are involved, we instead focus

of regression model (3) shown in Table 1, it is mainly parents' siblings and cousins that contribute to tightening the lower bound.[25]

We also estimate models using (standardized) years of schooling as the child outcome, in the subsample where this measure is available. These point estimates are smaller, but qualitatively very similar to the ones shown in Table 1. Using GPA as an outcome, for this smaller sample, we find that the resulting regression estimates are remarkably similar to the estimates using years of schooling as outcome.[26] We can thus conclude that the results are robust with respect to the choice of outcome variable for the child generation, and we will therefore use GPA as dependent variable in the main analysis, since it allows a larger sample.[27]

Figure 1 illustrates and summarizes the main results. The horizontal axis measures years of schooling, residualized using a full set of year of birth and gender controls and averaged over the extended family members used in the regression. The data points represent mean child GPA in 20 quantile groups based on family schooling. The 45-degree diagonal line shows the outcome of perfect persistence across generations.[28] The solid lines show the relation between the outcomes in the parental generation and child GPA using either only parents (black line with circles); parents and their siblings (yellow line with triangles); parents, their siblings, the spouses of the siblings, and their cousins (blue line with squares); or all the extended family members (green line with crosses). Regressions of child GPA on the relevant extended family categories using the 20 quantiles give results very similar to the corresponding estimates reported in Table 1.

Figure 1 visualizes the additional persistence from incorporating information on outcomes from the extended family members in the analysis. Adding parents' siblings accounts for almost half of the increase in the slope of the sum of the estimates, whereas adding relatives up to cousins closes almost all of the remaining gap to the sum of coefficient estimates using the widest definition of the extended family. In addition, Figure 1 reveals that the relationships between children's GPA and the outcome in the parental generation are approximately linear, meaning that the linear regression model used in Table 1 provides a good approximation of intergenerational persistence.

---

on R^2, its drawbacks notwithstanding. The R^2 from this regression is 0.172, which is identical to the R^2 in column 6 of Table 1. We conclude that once we control for a large part of the dynasty, nonlinearities do not play an important role. This exercise is related to Blundell and Risa (2019), who use Norwegian data and machine learning methods to contrast the child-parent income rank-rank model with an extended model using additional measures of parents' characteristics. They find that the simple, rank-rank model explains about two-thirds of the more complete model.

[25] For some of the columns, the estimates shown in Table 1 also include those dynasty categories that are linked through marriages (e.g., spouses of parent's siblings and cousins). In online Appendix Table D.4, we show that our qualitative conclusions regarding the sum of coefficients remain if we only include parents, parents' siblings, and parents' cousins as dynasty categories. The estimate of the sum of coefficients is then 0.483 using the widest biological family definition, which is only about 7 percent lower than the 0.518 estimate reported in Table 1 for the widest family definition. Nevertheless, since, as we show in online Appendix Section A.2 (Proposition 3), adding proxies will always improve our long-run intergenerational persistence estimate, we choose to include all dynasty categories for our main results.

[26] See online Appendix Table D.5.

[27] This is further corroborated by noting that the child-parent estimate of 0.36 in column 1 of Table 1 is similar to child-parent years of schooling correlations estimated in other Swedish studies (see Björklund and Salvanes 2011 and Björklund and Jäntti 2012).

[28] Note that perfect persistence here means a unit increase in latent human capital for all extended family members in the dynasty, regardless of the number of members included in the true model, being associated with a unit increase in the human capital of the child. If the AR(1) model is the true model, the 45-degree line shows an intergenerational correlation in latent human capital across generations equal to one.
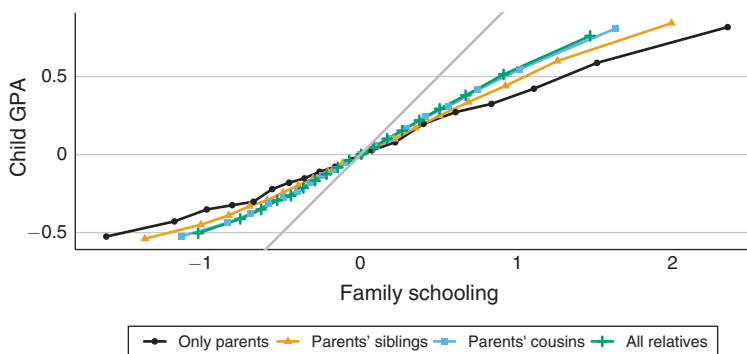
FIGURE 1. MAIN REGRESSIONS

Notes: Each point is calculated using the following steps. (i) Calculate dynasty average years of schooling for the indicated parental generation relatives (where each relative group is the standardized average of all relatives in that group, as in Table 1). (ii) Residualize the dynasty means on the full set of birth year and gender controls used in Table 1. (iii) Cut the dynasty means and child GPA into 20 quantile groups, and calculate the average within each group. The black circles include only parents; the orange triangles add aunts and uncles; the blue squares add spouses of aunts and uncles and parents' cousins; and the green crosses add spouses of parents' cousins and siblings of spouses of aunts and uncles.

A. *The Role of Potential Confounders: Sensitivity Analysis Including Residential Location and School Fixed Effects*

A potential concern with our estimates of intergenerational persistence attributed to the extended family is that they might also include influences from neighborhoods as well as race and ethnicity (see, e.g., the discussions in Solon 2018, and Chetty et al. 2014, on the results of Clark 2014). There are at least two reasons why we think that this critique is less relevant in our context. First, since we require great-grandparents to be identified in the data, our sample consists of children whose ancestors have been living in Sweden for at least four generations. During the period of the first generation, Sweden was ethnically very homogeneous and it is therefore unlikely that group effects based on race or ethnicity would affect our results. Second, Lindahl (2011) shows that neighborhoods are of limited importance in explaining variation in outcomes such as earnings, schooling, and student achievement in Sweden.

Nevertheless, to test if residential location can explain our large dynasty estimates, we have added various regional fixed effects to the baseline models. The results of this exercise are shown in online Appendix Table D.6, where we show the sum of the coefficients for models with more and more dynasty categories added. We control for mother's parish fixed effects (column 1); the child's residence (*SAMS*) fixed effects (column 2);[29] and, finally, school-by-year fixed effects based on the school the child attended in the last year of compulsory schooling, resulting in almost 27,000 fixed effects (column 3).[30] Comparing these results to Table 1, it

---

[29] Small Areas for Market Statistics (SAMS) is a definition of neighborhoods constructed by Statistics Sweden. Each of the about 9,000 SAMS areas of Sweden covers a homogeneous region with around 1,000 residents. See Statistics Sweden (2005).

[30] We acknowledge that including indicators for the child's residence and school can be seen as including "bad controls." However, information of the school attended by the parents is not available in our data.

is obvious that the baseline results are very robust, suggesting that the main results are not driven by dynasty members growing up in the same regions or attending the same schools.

B. *External Validity*

The results in this paper use rich administrative data that have made it possible to define broad extended family links, which is likely not feasible in datasets for many other countries. What can our results teach us about intergenerational mobility in other countries? One way to examine this important issue is to treat Swedish regions as separate entities, and estimate the same intergenerational models for each region. We then relate the intergenerational estimates across regions. We ask the following question: what is the relation between the traditional intergenerational persistence parameter and long-run intergenerational persistence?

The results from this exercise are shown in Figure 2.[31] The standard child-parent estimates are measured on the horizontal axis and the sum of the coefficient estimates on the vertical axis. Figure 2 reveals a strong positive relationship between the child-parent estimate and the sum of coefficients. The slope (standard error) is 0.869 (0.180) and we cannot reject a 1:1 relation between the two measures. This implies two things. First, it suggests that countries with a higher intergenerational association based on child-parent regressions are likely to have higher long-run intergenerational persistence; and second, that comparisons of long-run intergenerational persistence across time periods or across countries can probably be based on child-parent estimates, even though the latter are downward biased estimates of long-run intergenerational persistence in terms of levels.[32] These results are therefore reassuring from the point of view of ability to extrapolate the results in this paper to those for other countries, where only the child-parent regression model can be estimated.

IV. Additional Results

In this section we present results from three extensions of the main analysis. First, we extend our measure of human capital in the parental generation to the two additional measures log income and a social stratification index, using a proxy variable approach suggested by Lubotsky and Wittenberg (2006); second, we derive upper bound estimates of long-run intergenerational persistence; third, we include outcomes for ancestors beyond the parental generation in our model; and fourth, we use a sample of adoptees to assess to what extent our results are driven by genetic or environmental factors.

A. *Results from Models Using Additional Proxy Variables for Human Capital*

The previous literature on intergenerational mobility has used an approach suggested by Lubotsky and Wittenberg (2006)—henceforth, LW—to efficiently

---

[31] We define our regions using mother's county of residence from the 1985 census. Sweden had 24 counties in 1985.
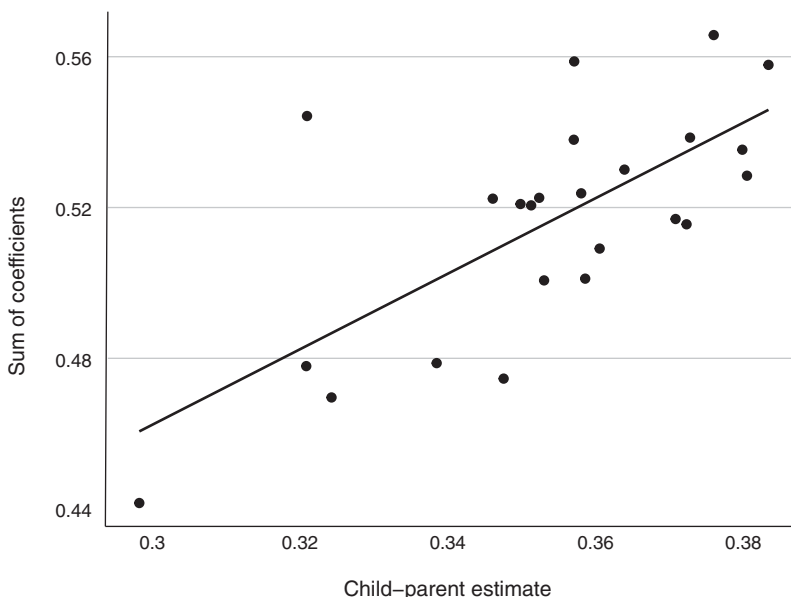[32] We thank an anonymous referee for providing us with the second point.

FIGURE 2. REGIONAL ESTIMATES OF CHILD-PARENT AND DYNASTIC INTERGENERATIONAL MOBILITY

Notes: The points represent estimates corresponding to column 1 (horizontal axis) and column 6 (vertical axis) in Table 1 for each of Sweden's 24 counties. Dependent variable is child's grade point average. Number of observations in each county range between 4,735 and 79,696, with mean 22,540 and standard deviation 15,084.

combine proxy variables for parental human capital (see Vosters and Nybom 2017 and Vosters 2018). LW show that their method of combining proxy variables provides estimates with a lower attenuation bias compared to models with only one proxy variable. These results suggest a possibility for improving on our lower bound estimates by using additional proxy variables for human capital outcomes for each extended family member, so that $y_{kj} = y_k^* + u_{kj}$, where k is an index for extended family member such that $k \in \{p, sp, cp, \ldots, K\}$, and j is an index for proxy variables, which in our case are *years of schooling*, *log income*, and an occupational-based *social stratification index*. This approach might be particularly useful if the measurement errors are correlated across extended family members, because, as discussed in Section IB, this will slow down the pace at which the bias decreases when more extended family members are added.

The LW approach, extended to several latent variables, proceeds by first regressing the outcome variable y_c on the full set of j proxy variables for each of the k extended family members y_{jk}, i.e., $y_c = \pi_0 + \sum_{jk} \pi_{jk} y_{jk} + \varepsilon_c$. The coefficients from this regression are then combined to give the coefficient on the latent variable for extended family member k as the (optimally weighted) linear combination $\hat{\pi}_k^* = \sum_j (\text{cov}(y_c, y_{jk})/\text{cov}(y_c, y_{1k})) \hat{\pi}_{jk}$.[33] This coefficient is scaled to be directly comparable to a coefficient for y_{1k}, which is always years of schooling in

[33] In practice, we estimate the LW coefficients using residuals from regressions of each variable on the full set of birth year controls.

our regressions. Note that we produce separate LW weighted indices for each of the dynasty categories in the extended family models.[34]

Panel B in Table 1 shows results from a specification where we, in addition to years of schooling, also include log income and the social stratification index as proxy variables for human capital,[35] and use the LW approach to obtain a broader human capital measure for the relatives.[36] As explained above, the LW estimates are scaled with respect to the years of schooling variable. This means that they are always interpretable in terms of a unit change in years of schooling for each relative. As before, we use GPA of the child as the outcome variable. Comparing the estimates for the sums of the coefficients with the corresponding ones in panel A, we see that they are always larger. The long-term intergenerational persistence of human capital using the entire extended family is estimated to 0.597. This is 28 percent larger than the LW weighted child-parent estimate of 0.465 reported in column 1, and 15 percent larger than the sum of the coefficient estimates using only years of schooling of 0.518 reported in column 6, panel A.[37]

The LW approach assumes that years of schooling, log income, and the social stratification index can all be viewed as proxy variables for a single latent variable for extended family member k. However, we might reasonably believe that these variables reflect the transmission of different factors than years of schooling, e.g., through social networks or access to investment capital. As a consequence, the LW approach might estimate a broader parameter than the one in our main analysis. When we use either log income or the social stratification index for the extended family members (each measure normalized to have standard deviation equal to 1 for each dynasty category) in separate regressions, we find that the child-parent estimates for these outcomes are lower than for years of schooling, but that the sum of coefficient estimates increases more in relative percentage terms (over 70 percent for these two measures, relative to 43 percent for years of schooling), resulting in sum of coefficient estimates that are somewhat lower for log income and the social stratification index (see online Appendix Tables D.8 and D.9).[38,39]

---

[34] The approach in the original LW paper assumes one common latent variable, allows the proxy errors to be correlated, but assumes that the proxy variables should be excludable in the main latent variable equation. In an extended framework with several latent variables (as equation (1)), we need to assume that all the proxy variables are excludable in the main equation, including all the latent variables for the extended family categories.

[35] For comprehensive reviews of the results from the intergenerational mobility literature using various measures, see Black and Devereux (2010) and Björklund and Salvanes (2011).

[36] The LW results for sequentially adding the full set of relatives (corresponding to panel A for the OLS estimates) are shown in online Appendix Table D.7.

[37] Online Appendix Figure D.1 shows the main results from the LW-based measures in a similar way as in Figure 1. Compared to Figure 1, the slopes of the lines are somewhat steeper, showing the increased estimated persistence from also including log income and the social stratification index. The differences between the lines are also smaller than in Figure 1, showing that the additional proxy variables matter relatively more, the fewer extended family members that we use in the model, although the results are qualitatively the same.

[38] The bottom rows of online Appendix Table D.7 show the sum of coefficients and the R^2 from a regression where we have included the three proxies for each of the six dynasty categories separately (each of the 18 variables are standardized). They also show the sum of all coefficient estimates, weighted equally, in contrast to the LW-weighted sum reported in panel B of Table 1. The coefficient sums are now bigger, but the proportional increase from column 1 to column 6 is very similar as for the LW weighted index. Hence, our main results using the LW approach is robust to alternative weighting schemes.

[39] As a final robustness check, we relax the linearity assumption to allow for the best possible fit using a post-lasso procedure similar to that described in footnote 24, but additionally allowing interactions between the three proxies, within and between dynasty categories. The results from this exercise show a very small improvement in terms of R^2, from 0.202 to 0.205.

B. *Upper-Bound Estimates of Long-Run Intergenerational Persistence*

The data structure used in this study allows us to estimate an *upper bound* of intergenerational persistence. This is achieved by using 2SLS, where the extended family members' human capital outcomes are used as instrumental variables for parents' years of schooling. It follows trivially (see Proposition 5 in online Appendix Section A.4) that this procedure provides a consistent estimator of the β parameter in equation (2) under the assumptions that this latent variable AR(1) model is the true model for the intergenerational persistence, and that $u_k \sim iid(0, \sigma_{u_k})$ for each $k \in \{p, sp, cp, \ldots, K\}$.[40]

In a second case, where we maintain the assumption of *iid* u_k terms, but where we assume that the true model is the extended family model (equation (1)) rather than the latent variable AR(1) model, online Appendix Section A.4 (Proposition 4) shows that the 2SLS model will estimate an upper bound of the sum of the coefficients as long as the extended family members' outcomes are non-negatively correlated with the error term in the main equation (see, e.g., Conley, Hansen, and Rossi 2012). Hence, regardless of whether model (1) or (2) is true, the 2SLS estimate of regressing child's outcome on the outcome of the parent, using the extended family members as instruments is, in our application, an upper bound estimate of long-run intergenerational persistence of human capital.[41]

However, if the u_k terms across extended family members in the parental generation are positively correlated, the interpretation of the 2SLS estimator as an *upper bound* does not necessarily apply. We therefore provide two additional extensions. First, we use the 2SLS method as above, but restrict the instrument set to outcomes for more distant relatives. This procedure only requires the weaker assumption of the u_k terms being uncorrelated between parents and more distant relatives used as instruments, but allows the u_k terms to be correlated between parents and closer relatives (e.g., between parents and their siblings), for which the *iid* assumption is most likely to be violated.

Second, for comparability with the LW weighted results provided in Table 1, we also provide 2SLS versions of the LW estimator. As we argued in Section IVA, the LW approach might be particularly useful if the measurement errors are correlated across extended family members, because this will slow down the pace at which the

[40] Note that this, in effect, is an extension of the approach in Behrman and Taubman (1985), who used father's twin sibling's educational attainment as an instrument for father's education in an intergenerational education regression. This approach is also related to more recent papers, such as Lindahl et al. (2015), who used grandparent's education as an instrument for parent's education, to obtain an upper bound of the intergenerational education association; and Grönqvist, Öckert, and Vlachos (2017), who used father's brother's cognitive score as an instrument for father's cognitive score to correct for measurement error bias in estimates of intergenerational associations in cognitive abilities. In a recent paper, Colagrossi, d'Hombres, and Schnepf (2019) estimate models using grandparents' educational attainment as instrument for parents' educational attainment using survey data for 28 EU countries. They find that, on average, the resulting IV estimates are about 20 percent larger than the corresponding OLS estimates.

[41] Our OLS-IV bounding approach is related to Solon (1992) where father's education is used as an instrument for father's own income, in a regression model of son's income and father's income. The argument is that when income is measured with error and education is not excludable in the main equation, the resulting 2SLS estimate will provide an upper bound estimate, whereas an OLS estimate, because of measurement error, will estimate a lower bound.

bias decreases when more extended family members are added.[42] The LW-2SLS approach requires that the proxy variables, for all extended family members other than the parents, are excludable in the main latent variable equation for consistency. If the true model includes several latent variables (as in equation (1)), so that the exclusion restrictions for the extended family members do not hold, the β parameter will be overestimated if the instruments are nonnegatively related to the error term in the main equation.[43]

The 2SLS estimates are shown in online Appendix Table D.10. The first stage estimates are all very precise, with nontrivial associations between parents' years of schooling and the years of schooling of all other extended family members. The baseline 2SLS estimates are, as expected, larger than the coefficient sums from the extended family models. The upper bound estimate, using the widest definition of the extended family, is 0.613, which is 18 percent larger than the sum of the estimated OLS coefficients using the same extended family definition. We also note that the 2SLS estimates are very robust with respect to which instrumental variables are included. The 2SLS estimates from models using more distant family members as instruments are somewhat larger (at most 8 percent), suggesting that although the assumption of uncorrelated measurement errors between siblings in the parental generation is too strong, the consequences for these upper bound estimates are not dramatic (see online Appendix Table D.11).

We also used the LW-proxy variable approach extended to a 2SLS framework.[44] The LW-2SLS estimates are larger than the coefficient sums from the LW-extended family models. Using the widest definition of the extended family, the upper bound estimate is 0.664, which is only 11 percent larger than the sums from the comparable LW-extended family model. As discussed in Section IVA, the LW approach might plausibly capture a broader parameter than our main analysis, so that direct comparisons should be interpreted with caution.

C. *Results from Multigeneration Models*

So far, we have focused on the horizontal dimension in the parental generation. Such models have the advantage of only requiring outcomes to be measured for those extended family members that are comparable in the sense that they have attended the same school system. Furthermore, it allows us to use data from a time period covered by high-quality administrative registers. However, a large and (mostly) recent literature has documented multigenerational associations from models including outcomes of grandparents and, in some cases, great-grandparents.[45]

---

[42] We replace y_{jk} by $\hat{y}_{jp}$, where $\hat{y}_{jp}$ is the predicted outcome variables from three first stage regressions of years of schooling, family income, and the social stratification index of the parents on the corresponding variables for the extended family members. The $\hat{y}_{jp}$ terms are then re-weighted, similarly to the standard LW approach. More specifically, we estimate $y_c = \pi_0 + \sum_j \pi_{jp} \hat{y}_{jp} + \varepsilon_c$, and then calculate the LW-2SLS estimates as $\hat{\pi}^*_{2SLS,p} = \sum_j (\text{cov}(y_c, \hat{y}_{jp})/\text{cov}(y_c, \hat{y}_{1p})) \hat{\pi}_{jp}$, where k has been replaced with p to indicate the parent category.

[43] The 2SLS-LW estimates will be different from the LW estimates if the three proxies reduce, but do not completely eliminate, any remaining attenuation bias from measurement errors.

[44] See panel C of online Appendix Table D.10.

[45] An advantage with multigenerational approaches for estimating long-run intergenerational persistence is that the assumption of stationarity of the intergenerational mobility process can be relaxed. We do not look deeper into this here. Nybom and Stuhler (2016) provide important theoretical and empirical evidence on how equilibriums

Table 2—Multigenerational GPA Regressions

	Vertical regressions				Horizontal coefficient sums			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Parental generation	0.361		0.326	0.330	0.518		0.480	0.489
	(0.001)		(0.001)	(0.002)	(0.002)		(0.002)	(0.003)
Grandparental generation		0.204	0.073	0.070		0.246	0.026	0.036
		(0.001)	(0.001)	(0.002)		(0.002)	(0.002)	(0.003)
Great-grandparents				0.005				−0.006
				(0.002)				(0.002)
Implied AR(1)		0.451	0.472	0.485		0.496	0.527	0.536
R^2	0.151	0.076	0.159	0.153	0.172	0.082	0.175	0.167
Observations	539,493	539,493	539,493	337,265	539,493	539,493	539,493	337,265

Notes: Each column shows results from a separate regression of child's grade point average on ancestor years of schooling. In columns 5–8, each table entry shows the sum of coefficients for all available types of relatives in each generation (for the parental generation, these are parents, aunts and uncles, spouses of aunts/uncles, parents' cousins, spouses of parents' cousins, and siblings of spouses of aunts/uncles; for the grandparental generation, they are grandparents and grandparents' siblings). Each parental generation outcome is the average across all members of the given category of relatives. All variables have been normalized to have mean 0 and standard deviation 1. Implied AR(1) shows the AR(1) coefficient that would produce the same intergenerational persistence as the estimated model after 10 generations. Robust standard errors in parentheses.

Our latent variable models (1) and (2) can easily be extended to allow for the potential influence of ancestors preceding the parental generation. The regression model we use is an extension of equation (3) where we also include years of schooling for grandparents, grandparents' siblings, and great-grandparents, which all are observable in our data.

The results for the multigenerational regression models are shown in Table 2. Columns 1–4 show the results from models restricted to the vertical dimension, i.e., including parents, grandparents, and great-grandparents; while columns 5–8 show results from regressions including all the dynasty members in the ancestor generations, i.e., incorporating both the vertical and horizontal dimensions.[46] To condense the presentation, columns 5–8 show the sum of the coefficient estimates for each generation. Below the main results we show an estimate which measures how large the parameter in an AR(1) model would have to be in order to produce the same long-run intergenerational persistence as implied by the estimates from the multigenerational models (*Implied AR(1)*; see Long and Ferrie 2018).[47]

The results in column 3 of Table 2 show that the estimates for grandparents still enter positively in the regressions after controlling for parental years of schooling.[48] However, comparing columns 3 and 7 reveals that most of the effect goes away when we include years of schooling for the extended family in the parental generation. These results thus suggest that the extended family in the parental generation picks up most of the variation from more distant ancestors, although the grandparent generation coefficient is still positive. The results in columns 4 and 8 further

---

can respond to structural changes (due to reforms, etc.), and show that they can cause nonmonotonic transitions between steady states.

[46] This includes all the extended family categories from Table 1 for the parental generation, parents' aunts and uncles for the grandparent generation, as well as great-grandparents.

[47] Corresponding estimates using the LW approach are shown in online Appendix Table D.12.

[48] Similar to many previous studies, see Anderson, Sheppard, and Monden (2018) for a review.

reinforce this interpretation. This implies that researchers do not necessarily need outcomes for ancestor generations to obtain accurate estimates of long-run intergenerational persistence.

Three additional results should be noted in Table 2. First, when we simulate the multigenerational models 10 generations forward and calculate the AR(1) coefficient that would produce the same long-run advantage (*Implied AR(1)*), we find that using only the vertical dimension gives smaller long-run intergenerational persistence estimates than the lower bound estimates we get using the extended family models in Table 1 (0.451–0.485, versus the coefficient sum of 0.518 in column 6 of Table 1). Second, the model that combines the vertical and horizontal dimensions gives an implied AR(1) coefficient very similar to the sum of coefficients reported in Table 1 (0.496–0.536 versus 0.518). Third, following the Braun and Stuhler (2018) method of estimating long-run intergenerational persistence by dividing a child-grandparent regression coefficient by a child-parent regression coefficient, under the assumption that the AR(1) model is the true model, we find an estimate of 0.57 (using the estimates in columns 1 and 2). This is somewhat higher than the 0.518 estimate for the coefficient sum of the extended family members. On the other hand, this estimate is also lower than the 2SLS estimates discussed in Section IVB, which are consistent under the AR(1) model.[49]

D. *Results from Models Estimated on a Sample of Adoptees*

To what extent can long-run intergenerational persistence be attributed to pre-birth (mostly genetic) or post-birth (environmental) factors? To study this question we apply the above framework to a dataset of adopted children and their adoptive parents, drawn from administrative Swedish registers. These data allow us to estimate the part of the long-run intergenerational persistence that remains after eliminating the genetic part of the family links. We expand the latent variable extended family model (1) in Section I to allow for separate transmission channels from genetic (G) and environmental (E) factors as follows:

$$(4) \qquad y_c^* = \theta_0 + \theta_1 y_{E_p}^* + \theta_2 y_{E_{sp}}^* + \cdots + \theta_K y_{E_K}^*$$
$$+ \delta_1 y_{G_p}^* + \delta_2 y_{G_{sp}}^* + \cdots + \delta_K y_{G_K}^* + \epsilon_c,$$

where the terms $y_{E_k}^*$ represent intergenerational transmission due to environmental factors, and the terms $y_{G_k}^*$ represent intergeneration transmission due to genetic factors, stemming from the $k \in \{p, sp, cp, \ldots, K\}$ extended family members.[50] If we

---

[49] The approach to estimate long-term intergenerational persistence suggested by Braun and Stuhler (2018) has been applied to many other countries (see Colagrossi, d'Hombres, and Schnepf 2019 and Neidhöfer and Stockhausen 2019), generally finding estimates of long-run intergenerational persistence in the range of 0.55–0.75.

[50] We have assumed additive $y_{E_k}^*$ and $y_{G_k}^*$ effects, hence ruling out gene-environment interactions. The additivity assumption in models (4) and (5) is unlikely to hold strictly. However, there is some recent evidence (Brandén, Lindahl, and Öckert 2018 and Black et al. 2020) suggesting that they are of minor empirical importance for estimates in adoption regression models.

impose the restrictions $\theta_k = 0$ and $\delta_k = 0$ for $k \in \{2, \ldots, K\}$, model (4) collapses to the child-parent latent variable model $y_c^* = \theta_0 + \theta y_{E_p}^* + \delta y_{G_p}^* + \epsilon_c$.[51]

Following the reasoning in Section I, we posit that long-run intergenerational persistence that is *not* due to genetic transfers is captured by $\theta_1 + \theta_2 + \cdots + \theta_K$ in the extended family model (4), and by θ in the latent AR(1) version of this model. Regardless of which version of these models that is the true one, we can use data on adopted children, their adoptive parents, and the adoptive parents' siblings, cousins and other extended family members to estimate a *lower bound* for the long-run intergenerational persistence that is due to family environment, from the estimated sum of the coefficients in the regression model:

$$(5) \qquad y_{ac} = a_0 + a_1 y_{ap} + a_2 y_{sap} + \cdots + a_M y_{Map} + w_c,$$

where *ac* denotes the adopted child; *ap* denotes an adoptive parent; and *sap* the adoptive parent's sibling, up to *Map* adoptive extended family members in the adoptive parent's generation.[52]

In addition to the assumptions discussed in Section I, the following five assumptions are needed for the sum of the estimates from equation (5) to provide a lower bound estimate of the long-run intergenerational persistence due to family environmental contributions:[53]

  (i) Adoptees are conditionally randomly assigned to adoptive extended families.

  (ii) The adoption takes place shortly after birth.[54]

  (iii) There are no prenatal environmental effects.

  (iv) The biological parents and their extended family members have no contact with the adopted child post-adoption.

  (v) The estimates using adoptees are externally valid.

Since Assumption (iii) is unlikely to hold in a strict sense (see Currie and Almond 2011), equation (5) is likely to yield downward biased estimates of the sum of the θ_k

---

[51] Although a restriction like $\delta_k = 0$ for $k \in \{2, \ldots, K\}$, i.e., that genetic factors only transfer from parents to child, might be practically reasonable, we also know that the way genes are transmitted across generations is complicated enough to not rule out possible correlations between children and biologically related extended family members, other than the biological parent.

[52] Imposing the restriction $a_k = 0$ for $k \in \{2, \ldots, M\}$, regression model (5) collapses to the special case where an outcome of the adopted child is linearly related to the outcome of an adoptive parent, which has been estimated in many previous empirical studies using samples of adoptees. (See Sacerdote 2002, Plug and Vijverberg 2003, and Plug 2004, who applied the adoption approach to years of schooling, and Haegeland et al. 2020, which is one of very few studies that looked at GPA as an outcome. See also Holmlund, Lindahl, and Plug 2011 and Sacerdote 2011 for reviews.)

[53] These assumptions are discussed in, e.g., Holmlund, Lindahl, and Plug (2011) in the context of the child-parent model.

[54] If the adoptees are adopted at a later age, one needs to assume that the postnatal pre-adoption environment (e.g., the quality of the nursery home) is uncorrelated with the post-adoption family environment, or has no influence on the outcome of the adopted child.

terms. This is an additional reason why the sum of the coefficient estimates in equation (5) should be interpreted as a lower bound estimate.[55]

To satisfy Assumptions (i), (ii), and (iv), we have restricted the sample to international adoptees and to those adopted before their first birthday. In online Appendix Section C, we further investigate Assumption (i) and conclude that international adoptees in Sweden (adopted at infancy) during this time are in effect quasi-randomly assigned to their adopting parents (see online Appendix Table C.2). Below we also discuss the issue of external validity of our estimates.

Panel A in Table 3 shows the main results for the sample of adoptees corresponding to those in Table 1 for the non-adoptees.[56] It is immediately apparent that the statistical precision, because of the much smaller sample size, is substantially inferior compared to the main results. They reveal, nevertheless, some interesting findings. The magnitude of the coefficient estimates are, for the most part, smaller than the corresponding ones in Table 1. This is, however, less true for extended family members other than the parents, suggesting that the genetic link, as expected, works primarily between children and parents. Comparing the sum of the coefficient estimates for the adoption sample with the corresponding sums of the main estimates in Table 1, it turns out that the share of overall persistence in human capital attributed to environmental factors is about 30 percent. The corresponding share for the LW estimates, shown in panel B, are similar and never estimated to be above 40 percent.[57]

The adopted children are overrepresented among the later birth cohorts in the sample we use. Re-estimating our main model on a sample from the population of non-adoptees that have been matched to be similar to the sample of adoptees with regard to the distribution across birth cohorts for children and parents, shows that the estimates are only somewhat lower than our main ones in Table 1 (see online Appendix Tables C.3 and C.4).

V. Conclusions

This paper proposes a new measure of long-term intergenerational mobility based on the extended family, or the dynasty. It shows that the sum of the coefficients from a regression where the standardized human capital outcomes from the extended family are included provides a *lower bound* estimate of the level of intergenerational persistence, irrespective of whether the true underlying transmission follows a latent $AR(1)$ child-parent model, or an extended family model. The results show that conventional studies on intergenerational mobility, based on child-parent regression models, underestimate true long-run persistence by at least one-third.

Although the main message of this study is that child-parent models give downward-biased estimates of long-run intergenerational persistence, we also show how results from these simpler models are still useful in specific contexts. Section IIIB

---

[55] An alternative interpretation of adoption regression models (see Björklund, Lindahl, and Plug 2006) is to settle for parameters capturing pre- and post-birth transmission channels. Model (4) can then be reinterpreted so that $y_{E_k}^*$ represents intergenerational transmission due to post-natal environmental factors, while $y_{G_k}^*$ represents intergenerational transmission due to genetic and prenatal environmental factors.

[56] Descriptive statistics for the sample of adoptees are shown in online Appendix Table C.1.

[57] We disregard the LW estimates in the last four columns as they are extremely imprecisely estimated.

TABLE 3—ADOPTEES REGRESSIONS

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Main estimates						
Parents	0.101	0.077	0.068	0.068	0.068	0.072
	(0.027)	(0.030)	(0.031)	(0.032)	(0.032)	(0.032)
Aunts and uncles		0.063	0.039	0.042	0.043	0.041
		(0.035)	(0.038)	(0.038)	(0.038)	(0.038)
Spouses of aunts/uncles			0.054	0.054	0.055	0.058
			(0.034)	(0.034)	(0.034)	(0.038)
Parents' cousins				−0.017	−0.023	−0.021
				(0.028)	(0.033)	(0.033)
Spouses of parents' cousins					0.010	0.007
					(0.034)	(0.034)
Siblings of spouses of aunts/uncles						−0.010
						(0.035)
Sum of coefficients	0.101	0.140	0.161	0.148	0.153	0.147
	(0.027)	(0.035)	(0.038)	(0.042)	(0.045)	(0.045)
R^2	0.152	0.155	0.158	0.161	0.162	0.165
Panel B. Lubotsky-Wittenberg estimates						
LW estimates	0.121	0.179	0.217	0.204	0.519	0.549
	(0.036)	(0.123)	(0.909)	(2.323)	(22.635)	(14.829)
R^2	0.154	0.160	0.165	0.167	0.178	0.181

Notes: Each column shows results from a separate regression of adoptive child's grade point average on parental generation outcomes. $N = 903$ observations. Data are restricted to foreign-born adoptees, with an age at adoption of at most 12 months. Parental generation variable is years of schooling in panel A, and the LW index of years of schooling, log income, and social stratification in panel B. Robust standard errors in parentheses. Each parental generation outcome is the average across all members of the given category of relatives. All variables have been standardized using the means and standard deviations from the main sample. All regressions include controls for region-of-birth fixed effects and age of adoption in months. Panel B uses bootstrap standard errors from 5,000 bootstrap replications.

shows that the *relative* levels of intergenerational persistence in different geographical units were largely unaltered when comparing the results from the child-parent model with those from the extended family one. In addition, Section IVD shows that the relative importance of post-birth, or nurture, factors in determining intergenerational persistence using international adoptees, gives results quite similar to those reported in the previous literature using the child-parent model (see Holmlund, Lindahl, and Plug 2011).

Are data on outcomes for more than two generations needed for obtaining useful estimates of long-term intergenerational persistence? Our interpretation of the results comparing estimates from our extended family approach with those obtained using multigenerational regression models in Section IVC suggests that this is not the case. Most of the persistence is captured by the outcomes of the extended family in the parental generation. By including the extended family in multigeneration models, we show that at least 95 percent of the overall persistence is captured without including data from the grandparent or great-grandparent generations.

Availability of large sets of register data has proved to be very useful in the study of income inequality and intergenerational mobility. Previous research has shown the potential of such data in, e.g., analyzing different forms of heterogeneity in mobility patterns, the implications of residential mobility, and intergenerational effects of specific public policy initiatives. This study demonstrates the usefulness

of mapping out distant extended family links in such data, making it possible to identify and analyze whole dynasties. By doing that, it also points to the possibilities for future research to use rich administrative data to further explore the complex processes that underlie social mobility.

REFERENCES

Adermon, Adrian, Mikael Lindahl, and Mårten Palme. 2021. "Replication Data for: Dynastic Human Capital, Inequality, and Intergenerational Mobility." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/E123761V1.

Anderson, Lewis R., Paula Sheppard, and Christiaan W. S. Monden. 2018. "Grandparent Effects on Educational Outcomes: A Systematic Review." *Sociological Science* 5 (6): 114–42.

Barone, Guglielmo, and Sauro Mocetti. 2016. "Intergenerational Mobility in the Very Long Run: Florence 1427–2011." Bank of Italy Working Paper 1060.

Becker, Gary S., and Nigel Tomes. 1979. "An Equilibrium Theory of the Distribution of Income and Intergenerational Mobility." *Journal of Political Economy* 87 (6): 1153–89.

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

Behrman, Jere, and Paul Taubman. 1985. "Intergenerational Earnings Mobility in the United States: Some Estimates and a Test of Becker's Intergenerational Endowments Model." *Review of Economics and Statistics* 67 (1): 144–51.

Björklund, Anders, and Markus Jäntti. 1997. "Intergenerational Income Mobility in Sweden Compared to the United States." *American Economic Review* 87 (5): 1009–18.

Björklund, Anders, and Markus Jäntti. 2012. "How Important Is Family Background for Labor-Economic Outcomes?" *Labour Economics* 19 (4): 465–74.

Björklund, Anders, Mikael Lindahl, and Erik Plug. 2006. "The Origins of Intergenerational Associations: Lessons from Swedish Adoption Data." *Quarterly Journal of Economics* 121 (3): 999–1028.

Björklund, Anders, and Kjell G. Salvanes. 2011. "Education and Family Background: Mechanisms and Policies." In *Handbook of Economics of Education*, Vol. 3, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann, 201–47. Amsterdam: Elsevier.

Black, Sandra E., and Paul J. Devereux. 2010. "Recent Developments in Intergenerational Mobility." In *Handbook of Labor Economics*, Vol. 4B, edited by Orley Ashenfelter and David Card, 1487–541. Amsterdam: Elsevier.

Black, Sandra E., Paul J. Devereux, Petter Lundborg, and Kaveh Majlesi. 2020. "Poor Little Rich Kids? The Role of Nature versus Nurture in Wealth and Other Economic Outcomes and Behaviors." *Review of Economic Studies* 87 (4): 1683–1725.

Blundell, Jack, and Erling Risa. 2019. "Intergenerational Mobility as a Prediction Problem." Unpublished.

Borjas, George. J. 1992. "Ethnic Capital and Intergenerational Mobility." *Quarterly Journal of Economics* 107 (1): 123–50.

Brandén, Gunnar, Mikael Lindahl, and Björn Öckert. 2018. "The Importance of Nature-Nurture Interactions for Socioeconomic Outcomes: Evidence from a Large Sample of Adoptees." Unpublished.

Braun, Sebastian Till, and Jan Stuhler. 2018. "The Transmission of Inequality across Multiple Generations: Testing Recent Theories with Evidence from Germany." *Economic Journal* 128 (609): 576–611.

Chan, Tak Wing, and Vikki Boliver. 2013. "The Grandparents Effect in Social Mobility: Evidence from British Birth Cohort Studies." *American Sociological Review* 78 (4): 662–78.

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 (4): 1553–623.

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

Clark, Gregory, and Neil Cummins. 2015. "Intergenerational Wealth Mobility in England, 1858–2012: Surnames and Social Mobility." *Economic Journal* 125 (582): 61–85.

Colagrossi, Marco, Beatrice d'Hombres, and Sylke V. Schnepf. 2019. "Like (Grand)Parent, like Child? Multigenerational Mobility across the EU." IZA Discussion Paper 12302.

Coleman, James S. 1988. "Social Capital in the Creation of Human Capital." *American Journal of Sociology* 94: S95–S120.

Collado, M. Dolores, Ignacio Ortuño-Ortín, and Jan Stuhler. 2019. "Kinship Correlations and Intergenerational Mobility." Unpublished.

Conley, Timothy G., Christian B. Hansen, and Peter E. Rossi. 2012. "Plausibly Exogenous." *Review of Economics and Statistics* 94 (1): 260-72.

Currie, Janet, and Douglas Almond. 2011. "Human Capital Development before Age Five." In *Handbook of Labor Economics*, Vol. 4B, edited by Orley Ashenfelter and David Card, 1315–1486. North-Holland: Amsterdam.

Grönqvist, Erik., Björn Öckert, and Jonas Vlachos. 2017. "The Intergenerational Transmission of Cognitive and Noncognitive Abilities." *Journal of Human Resources* 52 (4): 887–918.

Güell, Maia, José V. Rodríguez-Mora and Christopher I. Telmer. 2015. "The Informational Content of Surnames, the Evolution of Intergenerational Mobility, and Assortative Mating." *Review of Economic Studies* 82 (2): 693–735.

Hægeland, Torbjørn, Lars Johannessen Kirkebøen, Oddbjørn Raaum, and Kjell G. Salvanes. 2010. "Why Children of College Graduates Outperform Their Schoolmates: A Study of Cousins and Adoptees." IZA Discussion Paper 5369.

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.

Hertz, Tom. 2008. "A Group-Specific Measure of Intergenerational Persistence." *Economics Letters* 100 (3): 415–17.

Holmlund, Helena, Mikael Lindahl, and Erik Plug. 2011. "The Causal Effect of Parents' Schooling on Children's Schooling: A Comparison of Estimation Methods." *Journal of Economic Literature* 49 (3): 615–51.

Jæger, Mads Meier. 2012. "The Extended Family and Children's Educational Success." *American Sociological Review* 77 (6): 903–22.

Lambert, Paul S., and Erik Bihagen. 2012. "CAMSIS for Sweden, SSYK, version 1.0 [electronic file]." Released September 7, 2012. http://www.camsis.stir.ac.uk/Data/Sweden90.html.

Landersø, Rasmus, Helena Skyt Nielsen, and Marianne Simonsen. Forthcoming. "How Going to School Affects the Family." *Journal of Human Resources.*

Lawrence, Matthew, and Richard Breen. 2016. "And Their Children after Them? The Effect of College on Educational Reproduction." *American Journal of Sociology* 122 (2): 532–72.

Lindahl, Lena. 2011. "A Comparison of Family and Neighborhood Effects of Grades, Test Scores, Educational Attainment and Income: Evidence from Sweden." *Journal of Economic Inequality* 9 (2): 207–26.

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): 1–33.

Long, Jason, and Joseph Ferrie. 2018. "Grandfathers Matter(ed): Occupational Mobility across Three Generations in the US and Britain, 1850–1910." *Economic Journal* 128 (612): F422–45.

Lubotsky, Darren, and Martin Wittenberg. 2006. "Interpretation of Regressions with Multiple Proxies." *Review of Economics and Statistics* 88 (3): 549–62.

Mare, Robert D. 2011. "A Multigenerational View of Inequality." *Demography* 48 (1): 1–23.

Mare, Robert D., and Vida Maralani. 2006. "The Intergenerational Effects of Changes in Women's Educational Attainments." *American Sociological Review* 71 (4): 542–64.

Mazumder, Bhashkar. 2005. "Fortunate Sons: New Estimates of Intergenerational Economic Mobility in the United States Using Social Security Data." *Review of Economics and Statistics* 87 (2): 235–55.

Merton, Robert K. 1938. "Social Structure and Anomie." *American Sociological Review* 3 (5): 672–82.

Neidhöfer, Guido, and Maximilian Stockhausen. 2019. "Dynastic Inequality Compared: Multigenerational Mobility in the United States, the United Kingdom, and Germany." *Review of Income and Wealth* 65 (2): 383–414.

Nicoletti, Cheti, and Birgitta Rabe. 2019. "Sibling Spillover Effects in School Achievement." *Journal of Applied Econometrics* 34 (4): 482–501.

Nybom, Martin, and Jan Stuhler. 2016. "Interpreting Trends in Intergenerational Mobility." Unpublished.

Olivetti, Claudia, and M. Daniele Paserman. 2015. "In the Name of the Son (and the Daughter): Intergenerational Mobility in the United States, 1850–1940." *American Economic Review* 105 (8): 2695–724.

Persson, Petra, and Maya Rossin-Slater. 2018. "Family Ruptures, Stress, and the Mental Health of the Next Generation." *American Economic Review* 108 (4–5): 1214–52.

Plug, Erik. 2004. "Estimating the Effect of Mother's Schooling on Children's Schooling Using a Sample of Adoptees." *American Economic Review* 94 (1): 358–68.

Plug, Erik, and Wim Vijverberg. 2003. "Schooling, Family Background, and Adoption: Is It Nature or Is It Nurture?" *Journal of Political Economy* 111 (3): 611–41.

Sacerdote, Bruce. 2002. "The Nature and Nurture of Economic Outcomes." *American Economic Review* 92 (2): 344–48.

Sacerdote, Bruce. 2011. "Nature and Nurture Effects on Children's Outcomes: What Have We Learned from Studies of Twins and Adoptees?" In *Handbook of Social Economics*, Vol. 1, edited by Jess Benhabib, Alberto Bisin, and Matthew O. Jackson, 1–30. Amsterdam: Elsevier.

Santavirta, Torsten, and Jan Stuhler. 2019. "Name-Based Estimators of Intergenerational Mobility: Evidence from Finnish Veterans." Unpublished.

Solon, Gary. 1992. "Intergenerational Income Mobility in the United States." *American Economic Review* 82 (3): 393–408.

Solon, Gary. 1999. "Intergenerational Mobility in the Labor Market." In *Handbook of Labor Economics*, Vol. 3A, edited by Orley C. Ashenfelter and David Card, 1761–1800. Amsterdam: Elsevier.

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 (612): F340–52.

Solon, Gary, Marianne E. Page, and Greg Duncan. 2000. "Correlations between Neighboring Children in Their Subsequent Educational Attainment." *Review of Economics and Statistics* 82 (3): 383–92.

Song, Xi, and Robert D. Mare. 2015. "Prospective versus Retrospective Approaches to the Study of Intergenerational Social Mobility." *Sociological Methods & Research* 44 (4): 555–84.

Tibshirani, Robert. 1996. "Regression Shrinkage and Selection via the Lasso." *Journal of the Royal Statistical Society Series B* (*Methodological*) 58 (1): 267–88.

Statistics Sweden. 2005. "Geografi i Statistiken." Meddelanden i samordningsfrågor 2005:2. Statistics Sweden https://www.scb.se/contentassets/5f7f821206e24fc88501f4b6f5d1c559/mis-2005-2. pdf (accessed September 28, 2020).

Stuhler, Jan. 2012. "Mobility across Multiple Generations: The Iterated Regression Fallacy." IZA Discussion Paper 7072.

Torche, Florencia, and Alejandro Corvalan. 2018. "Estimating Intergenerational Mobility with Grouped Data: A Critique of Clark's Son Also Rises." *Sociological Methods & Research* 47 (4): 1–25.

Vosters, Kelly. 2018. "Is the Simple Law of Mobility Really a Law? Testing Clark's Hypothesis." *Economic Journal* 128 (612): F404–21.

Vosters, Kelly, and Martin Nybom. 2017. "Intergenerational Persistence in Latent Socioeconomic Status: Evidence from Sweden and the United States." *Journal of Labor Economics* 35 (3): 869–901.

Warren, John Robert, and Robert M. Hauser. 1997. "Social Stratification across Three Generations: New Evidence from the Wisconsin Longitudinal Study." *American Sociological Review* 62 (4): 561–72.