Great Equalizer or Great Selector?

Reconsidering Education as a Moderator of Intergenerational Transmissions

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Abstract:
A longstanding consensus among sociologists has held that educational attainment has an equalizing effect that increases mobility by moderating other avenues of intergenerational status transmission. This study argues that the evidence supporting this consensus may be distorted by two problems: measurement error in parents' socioeconomic standing, and the educational system's tendency to select people predisposed for mobility rather than to actually affect mobility. Analyses of family income mobility that address both of these problems in three longitudinal surveys converge on new findings. Intergenerational mobility is significantly lower among high school dropouts than others, but there are no significant differences in mobility across higher education levels. This is consistent with compensatory advantage processes among the least educated, in which individuals from advantaged backgrounds use family-based resources to compensate for their lack of human capital.

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Education plays a dual role as a mediator in intergenerational stratification processes. Because those from more advantaged social origins tend to attain more schooling than their less advantaged counterparts, education is an avenue of status transmission; but because many from disadvantaged origins do achieve educational success and some from advantaged origins do not, education is also an avenue of mobility (Blau and Duncan 1967). Education's role as a *moderator* of intergenerational processes is less clear. The issue is whether education alters direct intergenerational transmissions that operate through means other than schooling—e.g., the transfer of wealth, genes, or cultural traits (Bowles, Gintis, and Groves 2005; Goldthorpe 2014).

Decades of descriptive analyses have shown that as educational attainment increases, intergenerational associations—the similarity of children to parents—in class and socioeconomic status decrease (Bernardi and Ballarino 2016; Breen and Jonsson 2007; Hout 1988; Pfeffer and Hertel 2015). This supported a longstanding consensus among sociologists that education has an "equalizing effect" that increases mobility by weakening the role of direct intergenerational transmissions. Yet several recent studies have challenged this consensus by revealing surprisingly strong intergenerational associations among college graduates (Chetty et al. 2017; Witteveen and Attewell 2017; Zhou 2019) and graduate degree-holders (Torche 2011).

This study revisits education's role as an intergenerational moderator. I follow recent work and focus on family income mobility (Torche 2011; Western, Bloome, and Percheski 2008; Zhou 2019). Family income captures economic differences within the occupations often used in the past to measure class or status, and it better incorporates family-level processes that affect economic status and mobility. I begin by distinguishing causal and spurious reasons that family income mobility might differ by education level. Causal explanations cast schooling and background-related resources as *complements* (as in cumulative advantage) or *substitutes* (as in compensatory advantage) in the attainment process (e.g., Bernardi and Ballarino 2016). Spurious explanations involve bias from *selection* or *measurement error*. In the first case, instead of education actually causing economic mobility, educational systems may "select" people who are predisposed for upward or downward mobility for other reasons (Zhou 2019). In the second case, transitory income fluctuations cause long-term income to be measured unreliably, especially when data provide few observations (Solon 1992). We know that both problems can introduce bias into intergenerational associations, but we do not know how this bias might vary across education levels and thus distort evidence of education's presumed role as an equalizer.

This study is unique in examining education as an intergenerational moderator while addressing both measurement error and selection bias across the entire educational distribution. I use a Bayesian hierarchical modeling approach that provides reliable parent income estimates, adjusts for error in these estimates, and controls for observed individual attributes and unobserved family effects relevant to educational selection. I do so with data from three longitudinal surveys: the National Longitudinal Survey of Young Women (NLS-W), the National Longitudinal Survey of Youth 1979 (NLSY), and the Panel Study of Income Dynamics (PSID). Each has its own strengths and weaknesses, but together they converge to provide new insights on the relationship between schooling and economic mobility.

Education as an Intergenerational Moderator: Theory and Evidence

Most mobility studies aim to reveal descriptive patterns of intergenerational similarity in class or socioeconomic status that reflect many stratification processes. Studies that explore educational differences in mobility yield a few typical patterns: direct intergenerational associations decline as schooling increases, and they are often lowest among bachelor's degree-holders (Bernardi and Ballarino 2016; Breen and Jonsson 2007; Hauser and Logan 1992; Hout

1984, 1988, 2018; Pfeffer and Hertel 2015; Torche 2011). Intergenerational associations appear relatively strong, however, among those with graduate and professional degrees (Torche 2011), with the possible exception of doctoral degree-holders (Torche 2018).[1]

Does it matter whether these educational differences reflect education's causal role as an intergenerational moderator? I assume that it does, if for no other reason than we cannot help ourselves from inferring causation. We do so when we talk about education's "equalizing effect" (Zhou 2019), which we should distinguish from a descriptive equalization pattern. We also do so when evaluating theories, as when Breen and Jonsson (2007:1798) claim that European educational expansion increased intergenerational mobility "because labor markets were more meritocratic the higher the level of educational qualifications." And we do so when considering the real-world implications of empirical findings, as when Hout (1988:1391) says, "This finding provides a new answer to the old question about education's overcoming disadvantaged origins. A college degree can do it." Although this study is observational and has its own causal shortcomings, it will advance our understanding of how education might causally alter intergenerational transmissions with rigorous efforts to address alternative explanations.

The educational moderation of interest manifests statistically as interactions between the effects of social origins (parent attainment) and education on destinations (child attainment). The online supplement provides a conceptual diagram that adapts the familiar origin-education-destination triangle (e.g., Goldthorpe 2014) to illustrate potential sources of these interactions. Intergenerational associations could vary by education for two causal reasons: social background may alter education's effects on attainment, or education may alter direct background effects on attainment. Such interactions have a symmetry that permits both interpretations. This study, like those before it, cannot disentangle the two. Doing so will require testing specific mechanisms underlying these interactions. I aim to isolate either type from spurious alternatives.

Spurious reasons for intergenerational associations to vary with education concern education's role as a selector—that is, how individuals select or are sorted into different levels of education with respect to qualities that precede high school completion and thus might confound subsequent schooling's effects on socioeconomic attainment. Likely confounders are related to skills (Farkas 2003; Cunha et al. 2006), motivation (Sewell et al. 1969), and opportunity (Coleman et al. 1966). One possibility is differential selection, which occurs when confounders' effects on schooling depend on (interact with) one's social background, or when background effects on schooling depend on confounders. Another is that confounders' long-term effects on socioeconomic attainment depend on social background, or that background effects on attainment depend on confounders.

Background and Schooling as Substitutes or Complements

Causal interpretations of parent attainment-by-schooling interactions correspond to the idea that education is either a substitute for or a complement to background-related resources or opportunities. The equalizer hypothesis implies a *substitution* or *compensatory* relationship. A common theoretical proposition along these lines is the notion that high-skill labor market sectors are more meritocratic than low-skill sectors (Breen and Jonsson 2007; Hout 1988). High levels of education supposedly funnel workers into high-skill sectors, where their social origins are relatively unimportant. This could be because schooling actually enhances important skills, or merely because educational credentials signal that an individual has important skills for other reasons (Goldthorpe 2014). Compensatory advantage theory attends more to those with little education, who may instead end up in less meritocratic sectors where their origins matter more, perhaps because family-based resources can compensate for a lack of schooling (Bernardi and Ballarino 2016). Brand and Xie's (2010) negative educational selection hypothesis fits here as well; it suggests that those who are unlikely to complete college reap the highest economic returns to a degree because they lack other substitutable resources.

Other theories counter that education and social background may be *complements* in the attainment process. Cumulative advantage (Bernardi 2014; DiPrete and Eirich 2006) and human capital theories (Becker 1964) share this perspective. Being raised in a socioeconomically advantaged family can promote early skill development that enhances learning in school (Sørenson and Hallinan 1977), thus incentivizing additional schooling by heightening its returns (Cunha et al. 2006). Note the assumption of positive selection, which is characteristic of rational choice theories: those who will benefit most from schooling are most prone to attend (Carneiro, Heckman, and Vytlacil 2011; Willis and Rosen 1979). There could also be interactions between the quality and quantity of schooling. If children from advantaged backgrounds attend higher quality schools, they might learn more than their less advantaged peers with the same amount of schooling, yielding a larger payoff (Heckman, Layne-Farrar, and Todd 1996).

There are also grounds to question the idea that high-skill labor market sectors are more meritocratic than lower-skill sectors (Goldthorpe 2014). It has the flavor of functionalist theories that have not fared well in the past. Most notably, the notion that industrialization increases mobility (Treiman 1970) found little empirical support in comparative studies of social fluidity (Erikson and Goldthorpe 1992; Featherman, Jones, and Hauser 1975); any equalizing effects of industrialization appear modest at best (Breen 2004; Breen and Jonsson 2007). Rivera's (2012) research on elite professional service firms further challenges the notion of meritocracy in the high-skill labor market: impressive educational credentials could get a candidate's foot in the door, but getting hired depended heavily on familiarity with elite status cultures.

Recent empirical findings add weight to any doubts these theories raise about education's equalizing effects. As mentioned, Torche (2011) found stronger intergenerational associations for many socioeconomic outcomes among adults with graduate or professional degrees than among those with bachelor's degrees. Moreover, several studies find sizeable intergenerational associations among college graduates (Chetty et al. 2017; Witteveen and Attewell 2017), and Zhou (2019) finds these associations to be similar to those among people without college degrees. A few earlier studies even find positive interactions between education and parent attainment when predicting wages (Altonji and Dunn 1996; Ashenfelter and Rouse 1998).

Bias from Educational Selection

Whether or not education causally moderates intergenerational processes, empirical patterns also depend on spurious sources of moderation. One is *differential selection* into schooling in ways related to social background, which is tied to the confounding of education effects. Consider separate regressions of child attainment (*Y*) on parent attainment (*P*) at two different levels (N for no college, C for college): differences in the intergenerational associations confound a portion truly moderated by education, $\beta_C - \beta_N$, with a spurious portion driven by differential selection bias, $\frac{Cov_C(p_i,u_i)}{Var_C(p_i)} - \frac{Cov_N(p_i,u_i)}{Var_N(p_i)}$. This bias depends on differences across education levels in the association between parent attainment and confounders (*U*).

The implications for the equalizer hypothesis depend on differential selection across the entire educational distribution. Torche (2011) made important strides considering this problem. She suggested that at higher schooling levels, individuals from disadvantaged backgrounds are increasingly positively selected on things like ability or motivation relative to their more advantaged peers. If so, direct intergenerational effect estimates would become more negatively biased as education increased (see also Hout 2018). This could explain the common descriptive equalization pattern, but not the heightened intergenerational effects among those with graduate degrees. As Torche (2018) later noted, however, the latter difference could be spurious if selection bias were less pronounced at the post-baccalaureate level than at the baccalaureate level. Suppose bachelor's degrees are normative in socioeconomically advantaged families, so degree-holders from advantaged backgrounds are less selected than their disadvantaged peers on things like ability and motivation. Advantaged youths would become more positively selected at the transition to postgraduate education, possibly even more so than their less advantaged peers who had already survived more stringent selection at prior transitions. Moreover, though usually overlooked, selection processes could also downwardly bias intergenerational associations at the lowest schooling levels, leading us to understate education's equalizing effects. This would occur if high school dropouts from advantaged origins were especially negatively selected on ability or motivation compared to their more disadvantaged peers, which seems plausible.

Patterns of differential selection across education levels are difficult to predict, and so are the empirical consequences. The aforementioned study by Zhou (2019) made laudable efforts to account for confounders of bachelor's degree completion using covariates available in the NLSY data. Zhou revealed similar intergenerational family income associations among those with and without bachelor's degrees, but his study only focused on that particular distinction, obscuring patterns elsewhere in the educational distribution. Moreover, Zhou and others have noted the limits of addressing selection with observed variables alone: people unlikely to complete college based on observed variables, but who nonetheless do, may be exceptional on unobserved attributes that affect their attainment (Brand and Xie 2010; Breen, Choi, and Home 2015).

Two economics studies examining background-by-schooling interaction effects on wages show how and why we might go further to address selection on unobserved confounders. Altonji and Dunn (1996) eliminated unobserved family-level confounders using family fixed effects (sibling comparisons), and Ashenfelter and Rouse (1998) eliminated even more confounders by comparing identical twins. Both found positive interactions between social background and education, consistent with *disequalizing* educational effects. Yet it is difficult to reconcile these findings with the sociological mobility literature, partly because they use parent education as the background measure and treat education as a continuous variable (years of school).

A less obvious source of spurious background-by-education interactions is the possibility that background-related resources moderate the effects of confounders, not the effects of education. Another is that confounders, not education, moderate direct background effects. The story here is similar to the prior causal explanations, except that things like skills or parental support rather than education would be the true substitutes for or complements to background-based resources and opportunities. Additionally, resources derived from a socioeconomically advantaged upbringing may moderate barriers faced by minorities, in which case racial or ethnic disparities in schooling would create spurious interactions between parent attainment and education. Put simply, anything that influences schooling and is associated with socioeconomic background could be the true source of background-by-education interactions.

Measuring Attainment

Measuring attainment raises additional issues. Social origins and destinations are broad constructs operationalized in many ways. Sociologists often use occupational measures, which fit class schemas, are reliably reported, and are fairly stable among prime-age workers (Blau and Duncan 1967; Hauser and Warren 1997; Hout 1988, 2018). A weakness is that these measures are difficult to aggregate at the family level and are often based on fathers' occupations; this obscures variation in resources due to mothers' attainment, and it presents problems when examining daughters' mobility, which is affected by occupational segregation (Beller 2009). Occupational measures also obscure pay disparities within occupations, which are greater than between-occupation differences and fluctuate over time (Mouw and Kalleberg 2010).

Family income is an increasingly popular alternative because it accounts for inequality within occupational categories, and it accounts for processes that affect all family members' well-being—e.g., family formation, resource pooling, household division of labor—which helps mitigate the challenges posed by gender differences in labor force participation and occupational segregation (Torche 2011; Western et al. 2008; Zhou 2019). The downside is that income is volatile over time, and transitory fluctuation introduces error into measures of long-term or "permanent" financial resources. Such error in the parent generation attenuates intergenerational associations.[2] The typical solution is to average as many measures of parent income as possible. As analysts have incorporated more measures, intergenerational elasticity estimates have climbed from around 0.2 to around 0.4 or as high as 0.6 (Mazumder 2005; Solon 1992).

Differences in parent income measurement error across schooling levels might distort inferences about the equalization hypothesis. This is easiest to show in a bivariate regression with classical measurement error, where errors are uncorrelated with true permanent income: the difference in intergenerational associations across schooling levels N and C is $\beta_C \left(\frac{\sigma_{rC}^2}{\sigma_{rC}^2+\sigma_{eC}^2}\right) - \beta_N \left(\frac{\sigma_{rN}^2}{\sigma_{rN}^2+\sigma_{eN}^2}\right)$. The β's are the true associations, the σ_r^2's are the true conditional variances of parent attainment, and the σ_e^2's are the random error variances of the parent attainment measures. Attenuation bias will be worse at education levels with more error relative to true variance in parent attainment. It is unclear which levels these would be.

Ideally, we could harness the conceptual advantages of family income while minimizing measurement error. A common approach restricts analyses to cases with a certain number of parent income measures. This is most effective in data with many parent income measures; it is problematic in data with fewer measures (e.g., NLSY), requiring a tradeoff of either using few parent income observations and risking more measurement error, or requiring more measures but discarding cases at the risk of nonrandom sample selection bias. My modeling approach better addresses these measurement issues without dropping many cases. It not only eliminates transitory fluctuations, but also provides more reliable permanent parent income estimates and incorporates error in these estimates into intergenerational analyses.

Data and Measures

Data sources include the National Longitudinal Survey of Young Women (NLS-W), the National Longitudinal Survey of Youth 1979 (NLSY), and the Panel Study of Income Dynamics (PSID). NLS-W includes cohorts born in the 1950s, followed 1968-2003. I only include the subset of NLS-W respondents with parents in the NLS Older Men or Mature Women surveys, which provide useful parent income information. Prior analyses have included their male counterparts (NLS-M) (Altonji and Dunn 1996; Torche 2011); I exclude these men from the intergenerational analyses because they were not followed into their prime-earning years. NLSY includes cohorts born in the 1960s, followed 1979-2014. The PSID began with a sample of households in 1968 and has followed members and subsequent generations through 2015 as they formed new households. My PSID sample differs from that of Torche (2011), who only included cohorts born in the 1950s and kept a low-income oversample elsewhere deemed problematic; I keep subsequent cohorts born through the early 1980s but drop the low-income oversample.[3] Supplemental analyses explore the implications of differences in sample construction.

As described subsequently, the analyses involve two stages: the first uses models of parent income during childhood to improve its measurement, and the second incorporates parent income measurement error into intergenerational elasticity models.[4] The parent income analysis includes all person-year observations with parent income data from ages 1-18.[5] The intergenerational analysis includes all person-year observations with adult income data at age 30 or older, restricted to individuals with at least one parent income measure and with valid educational attainment data collected at age 25 or older.

Parent and child attainment are measured as log-transformed total family income, adjusted to 2010 dollars using the Personal Consumption Expenditures deflator. The PSID provides complete parent income data throughout childhood in some cases, NLS-W provides up to five measures during adolescence, and NLSY provides up to three during adolescence. I distinguish five categories of educational attainment: less than high school, high school (diploma or equivalent), some college, bachelor's degree, and graduate or professional degree.[6]

The time at which income is measured is an issue in both generations. Parent income may be more impactful at certain childhood stages than others (Carneiro and Heckman 2002; Duncan et al. 1998), and such differences could be problematic if the age of measurement is associated with socioeconomic background. Moreover, intergenerational associations tend to be strongest for outcomes in the prime earning years (mid-30s to mid-40s), so differences in the timing of adult income measures could be problematic as well (Haider and Solon 2006). I retain the year and age at each income measure to make appropriate adjustments in both stages of analysis.

I incorporate additional control variables to reduce selection bias. Ideally these would capture differences in skills, motivation, and opportunity. All surveys provide information on gender, race (white, black, other), number of siblings, and parent education (five categories). At best, these are proxies for likely confounders and do little to account for within-family differences. Unfortunately, this exhausts the controls in the PSID, which trades off its limited measures of confounders with superior parent income measurement. From the NLS-W, I also include adolescent cognitive skills (standardized achievement test scores) and occupational aspirations (Duncan's SEI), and both adolescents' and their parents' educational aspirations (highest grade). The main limitations of the NLS-W are the lack of noncognitive skill measures and the fact that achievement test scores are often missing (about 50%) and are based on different tests for different children. From the NLSY, I include adolescent educational aspirations and expectations, cognitive skills (standardized AFQT scores), and a delinquency scale (from a factor analysis of 17 self-reported delinquency/drug use items).[7] For missing data on these controls, I incorporate imputation models into the analyses, described next.

Methods

My analyses use Bayesian hierarchical models, broken into two stages to speed computation. The first estimates permanent parent family income, and the second uses these estimates (including their error) in intergenerational models predicting adult income. I use the Stan platform (RStan 2.15.1), which uses Hamiltonian Monte Carlo sampling, an efficient simulation method for complex Bayesian models (Carpenter et al. 2017; Gelman et al. 2013). I use four sampling chains per model, each chain providing 1,000 post-warmup samples. Trace plots and scale reduction factors suggest convergence for all models (Gelman et al. 2013).

In the first stage, I fit the hierarchical model shown in Equations 1a-1f. Time-specific log-parent income (P) is a function of an intercept and second-order polynomials for age and year ($\boldsymbol{X}$; centered at age 14 and year 1960); random effects for individuals (i), families (f), and PSID multigenerational clusters (k); and residual (transitory) variance (Eqs. 1a-1b).[8] Each random effect has a variance (Eqs. 1c-1e). Bayesian analyses require prior distributions for all parameters; I use weak priors (not shown), leaving enough uncertainty to ensure that the likelihood function (the data) dominates the priors.[9] I estimate "true" parent income (P^*) by summing the intercept and the individual, family, and cluster effects (Eq. 1f); this adjusts for differences in the age and year of measurement and discards transitory error variance.

$$P \sim N(\eta, \sigma_{rt}) \tag{1a}$$

$$\eta = \gamma_0 + \boldsymbol{X\gamma} + r_k + r_f + r_i \tag{1b}$$

$$r_k \sim N(0, \sigma_{rk}) \tag{1c}$$

$$r_f \sim N\left(0, \sigma_{rf}\right) \tag{1d}$$

$$r_i \sim N(0, \sigma_{ri}) \tag{1e}$$

$$P^* = \gamma_0 + r_k + r_f + r_i \tag{1f}$$

This approach should yield more reliable estimates than simply averaging available parent income measures, and it does so without dropping cases with few measures. The hierarchical model balances individual-specific information with information from siblings, multigenerational relatives, and the sample as a whole, all according to the precision at each level. More concretely, the fewer parent income measures for an individual, the more data from other family members is used to predict permanent parent income; the fewer measures from the family, the more data from the overall sample is used (and the more uncertainty there is).

At this point, each individual has a posterior distribution of true parent income with a mean ($\bar{P}^*$) and standard deviation (σ_{P^*}). To incorporate measurement error into the second stage, I treat the posterior means as draws from sampling distributions centered on the unknown true values (P^*), with standard errors equal to the posterior standard deviations (Eq. 2a). I model adult income (Y) as a function of true parent income, education, parent income-by-education interactions, and a vector of controls ($\boldsymbol{Z}$), all within a hierarchical structure analogous to that in the first stage (Eqs. 2b-2f), with weak priors on all parameters. I also include imputation models for missing covariates in these analyses (not shown). This avoids sample selection incurred by dropping cases and accounts for uncertainty in the imputations (Little and Rubin 2002).

$$\bar{P}^{*} \sim N(P^{*}, \sigma_{P^{*}}) \tag{2a}$$

$$Y \sim N(\mu, \sigma_{ut}) \tag{2b}$$

$$\mu = \beta_0 + \beta_1 P^{*} + \beta_2 HS + \beta_3 Coll + \beta_4 BA + \beta_5 Grad + \beta_6 (HS \times P^{*}) + \beta_7 (Coll \times P^{*}) + \beta_8 (BA \times P^{*}) + \beta_9 (Grad \times P^{*}) + \boldsymbol{Z\beta} + u_k + u_f + u_i \tag{2c}$$

$$u_k \sim N(0, \sigma_{uk}) \tag{2d}$$

$$u_f \sim N\left(0, \sigma_{uf}\right) \tag{2e}$$

$$u_i \sim N(0, \sigma_{ui}) \tag{2f}$$

The baseline model controls for the main effects of race, gender, and quadratic functions of birth year and age. Age, education, and (log) parent income are fully interacted to account for the effects of education and social background on age-earnings profiles. To maintain comparability with prior work and capture the peak-earning years when intergenerational associations are highest, I report age-adjusted elasticities at age 40 (Haider and Solon 2006; Torche 2011; Zhou 2019). In addition to pooled analyses that assume no gender differences in covariate effects, I conduct separate gender-specific analyses.

I address the problems of educational confounding and selection as follows. After adding main effects of the available control variables, I include their interactions with parent income to account for differential selection into schooling and any complementarity or substitutability with social background. I include interactions between race and parent income for similar reasons, and interactions between race and education to ensure that race-based moderation of education effects is not misattributed to parent income.

For the gender-pooled PSID and NLSY analyses, I also fit intergenerational models that add family fixed effects (FE) to the controls.[10] These models replace the family and cluster random effects with fixed family-specific intercepts. Families include all children sharing either a mother or father. FE models use sibling comparisons to eliminate unobserved family-level confounders; they do not account for confounding related to sibling-specific attributes, though these models do so to the extent that the available individual-level controls allow. I do not conduct gender-specific FE analyses, which only allow same-sex sibling comparisons and lack the power for precise estimates.

Adding controls and family FE's absorbs mechanisms of direct intergenerational effects; in other words, these controls and family effects may be endogenous to family background and create overcontrol problems when estimating the main effects of parent income. Zhou (2019) used reweighting methods to avoid this problem, but those methods do not easily incorporate fixed effects. In any event, this seeming overcontrol issue is not a problem for examining *differences* in intergenerational effects across schooling levels. As long as education moderates direct intergenerational effects and the mechanisms explaining this educational moderation are not controlled, the interactions of interest remain identified. Given this study's goals, overcontrol problems would only arise from controlling for interactions between education and variables affected by parent attainment (e.g., cognitive skills, parental expectations).

Results

Descriptive Statistics and Parent Income Analyses

[Table 1. Summary Statistics]

Table 1 summarizes the data used in both stages of the analysis. It also includes key results from the first-stage parent income analysis. Note that parent income is measured an average of only 2-3 times per person in the NLS-W and NLSY, compared to over 11 times in the PSID. Hence, adjusted log-parent income (estimated in the first stage and used in the second stage) is estimated less precisely in the NLS-W and NLSY; the posterior standard deviations are 2-3 times as large in these data (0.29 and 0.24, respectively) as in the PSID (0.12). Table 1 also includes variance estimates from the parent income models: 30-50% of the variance is transitory, and most of the remainder is between families, with almost none between individuals in the same family (e.g., siblings with staggered childhoods).

The supplement compares the error-adjusted parent income estimates to the means of available measures in more detail. The correlations between the two are high (over .95), but the adjustments for measurement error entail some shrinkage toward the mean. This shrinkage is driven by imprecise estimates, especially individuals with very high or low averages based on only a few measures. The more uncertainty in the estimate, the more shrinkage. Not surprisingly, there is more uncertainty and more shrinkage in the NLS-W and NLSY than in the PSID.

Measurement Error

[Figure 1. Unconditional Elasticity Estimates]

Before delving into the intergenerational analyses, it is worth noting a few findings related to measurement error. Figure 1 shows intergenerational elasticity estimates without conditioning on education, adjusted for only race, age, and year. It compares the Bayesian estimates that adjust for measurement error to maximum likelihood estimates that do not (they specify parent income as the average of available measures). As expected, elasticity estimates are lower without adjusting for error, especially in the NLS-W (0.15) and NLSY (0.29). The

Bayesian estimates are much more in line with current knowledge (0.4-0.5, though slightly lower in the NLS-W), without restricting the samples based on the number of parent income reports.

[Figure 2. Sensitivity to Measurement Error, PSID]

Figure 2 better speaks to the implications for educational differences in mobility. In a sample of PSID cases with at least 4 parent income measures, I estimate the baseline intergenerational model via MLE after specifying parent income as the average of either 1, 2, 3, or 4 randomly selected parent income observations. As expected, using fewer measures attenuates the elasticities. More importantly, this attenuation bias differs across education levels. It is most pronounced among those who failed to complete high school, suggesting that failure to account for measurement error may overstate mobility among the least educated, which could obscure equalizing effects at the transition to high school completion.

Intergenerational Analyses

[Table 2. Intergenerational Income Elasticity Estimates: NLSY, Gender Pooled]

[Table 3. Intergenerational Income Elasticity Estimates: PSID, Gender Pooled]

[Table 4. Intergenerational Income Elasticity Estimates: NLS-W, Females]

Tables 2-4 summarize the results from the intergenerational analyses using the Bayesian hierarchical approach. All of these estimates adjust for measurement error; the focus here is on efforts to account for differential selection bias. The PSID and NLSY estimates are pooled across genders (gender-specific estimates appear in the supplement). These tables include education-specific elasticities (posterior means and standard deviations are comparable to point estimates and standard errors); all pairwise comparisons, reported as posterior mean differences between higher and lower education levels; and posterior probabilities that the latter differences are negative. Negative differences and high posterior probabilities are evidence that elasticities decrease as education increases (equalization); positive differences and low posterior probabilities are evidence that elasticities increase as schooling increases (disequalization). Figure 3 illustrates the results by dataset and gender, recentering estimates around the baseline elasticities for high school graduates to facilitate the comparison of trends.

[Figure 3. Intergenerational Elasticity Estimates, Adjusted for Measurement Error]

I focus on four questions. First, how do efforts to account for educational selection alter patterns of intergenerational elasticities across schooling levels? NLSY is likely superior in accounting for selection, as it has many useful individual-level controls and also accommodates family fixed effects. NLS-W has several useful individual-level controls but does not support family FE's; the opposite is true for PSID. All datasets have the same family-level controls. Second, is there a general trend of decreasing or increasing elasticities across schooling levels, consistent with general substitutability or complementarity between schooling and background-related resources? Third, are there particular schooling levels with relatively high or low elasticities? In particular, does a college degree has a unique equalizing effect? Fourth, is the elasticity higher among graduate degree holders than bachelor's degree holders?

Accounting for selection in the NLSY has two effects. First, looking at Figure 3, adding observed controls rotates the trend in elasticities across education levels counter-clockwise, such that elasticities decrease less or increase more with schooling than in the baseline model. Among females, this weakens a trend of declining elasticities across schooling levels; among males, it reveals a general increase in elasticities. In the gender-pooled analysis, the controls eliminate a trend of declining elasticities, which in the baseline model had indicated an equalization pattern beyond high school completion. Adding family FE's in the gender-pooled model eliminates most variation in elasticities across schooling levels, with one exception: it reveals a markedly higher elasticity among the least educated, and these differences are estimated with a high degree of confidence (see Table 2: $\Delta\beta = -0.17$ to -0.23, $P > 0.88$ for all comparisons to <HS).

Hence, the NLSY analyses suggest that much purported variation in mobility across schooling levels, particularly the general descriptive pattern of declining elasticities, is an artifact of educational selection. There is no evidence that a college degree has a unique equalizing effect relative to high school graduates or those with some college, and no evidence of any difference in intergenerational elasticities among graduate degree-holders relative to college graduates. There is evidence, however, of a different variety of equalization than previously noted: direct intergenerational effects are uniquely strong among high school dropouts, and this seems to be suppressed in descriptive analyses by bias from educational selection.

In the PSID, accounting for selection makes less difference, likely due to the lack of controls for individual-level confounders. Nonetheless, the preferred PSID estimates (controls and family FE's) reveal a relatively high elasticity among the least educated (<HS), with a high degree of confidence for most comparisons (Table 3; $\Delta\beta = -0.08$ to -0.21, $P > 0.74$). There is no evidence of declining elasticities beyond high school completion or of a bachelor's degree having an equalizing effect; if anything, elasticities increase gradually between high school completion and college completion. Elasticities are comparable at the baccalaureate and graduate degree levels in the pooled FE model.

The NLS-W estimates are only comparable to the female-specific estimates in the other datasets. The baseline model suggests declining elasticities moving from some college to bachelor's and graduate education. Mirroring the NLSY female estimates, adjusting for observed controls weakens this trend. Mirroring the gender-pooled FE analyses in other datasets, including observed controls also reveals a substantially higher elasticity among high school dropouts than among the more educated (Table 4: $\Delta\beta$ = -0.13 to -0.35, $P > .77$); these are the only differences estimated with much confidence. There is no unique equalizing effect of a bachelor's degree, and no increase in the elasticity at the post-baccalaureate level.

The gender-specific analyses in NLS-W and other data should be interpreted with ample caution, due to reduced statistical power and the inability to incorporate family fixed effects. Nonetheless, net of observable controls, across datasets there is more of a tendency for elasticities to increase beyond high school completion in the male-specific analyses, whereas the opposite is true in the female-specific analyses. This could be due to differences in educational selection or to gender differences in the attainment process.

To recap, the most rigorous efforts to address educational selection are in the NLS-W analysis with observed controls and in the gender-pooled NLSY and PSID analyses with controls and family fixed effects. None of these are perfectly comparable, but they all find a few things in common. First, the strongest intergenerational effects appear for high school dropouts, for whom elasticities are 0.1-0.4 points stronger than for those with more education; these are substantial differences and the only ones consistently estimated with a high degree of confidence. That this is only apparent in the NLS-W and NLSY after accounting for selection bias suggests that dropouts from advantaged backgrounds are negatively selected on traits that influence later earnings; this is not surprising, but it has been overlooked because prior discussions of selection focus on the highest schooling levels. Failing to account for selection thus obscures strong elasticities (low mobility) among dropouts, which suggests that something about an advantaged family background helps compensate for the lack of a high school education.

The preferred analyses provide no compelling evidence of differences in mobility beyond high school completion. The PSID suggests slightly increasing elasticities, the NLS-W reveals fluctuating elasticities, and the NLSY—best suited to address differential selection—reveals no apparent differences; in no case are any differences estimated with confidence. There is also no evidence that a bachelor's degree has a unique equalizing effect; it is never the education level with the lowest elasticity. And there is no evidence of increasing elasticities between the baccalaureate and post-baccalaureate degree levels.

The online supplement provides coefficient estimates for covariates relevant to selection bias, based on the NLS-W model with controls and the gender-pooled NLSY and PSID models with controls and family FE's. The most striking findings concern cognitive skills. In both the NLS-W and NLSY, cognitive skills have an independent association with adult family income that varies inversely with parent income (negative interaction, t-ratio < -1.96). This suggests substitution or compensation between cognitive skills and background-related resources. Net of education, cognitive skills (or the lack thereof) appear more important to the attainment of those from disadvantaged economic backgrounds than to that of their more advantaged peers. From another perspective, direct background effects wane as cognitive skills increase. Given the positive association between cognitive skills and schooling, failing to account for this interaction likely misattributes a potential equalizing effect of cognitive skills to schooling, contributing to spurious patterns of educational equalization. Cognitive skills are both important sources of educational selection and potential moderators of direct intergenerational transmissions.

Additional Analyses

I conducted several additional analyses to assess the importance of measurement error and probe some differences with prior findings. I briefly summarize them here and describe them in more detail in the supplement. First, when I replicate my analyses without adjusting for measurement error in parent income, I find less overall variation in elasticities across education levels, lower elasticities among high school dropouts, and more evidence of an uptick in elasticities between the bachelor's and graduate degree levels. Hence, measurement error may both suppress equalization at the transition to high school completion and overstate the rise in elasticities beyond college completion.

Second, when I replicate my analyses using parent education or occupational status rather than income as the background measure, educational differences in background effects are more muted overall, and those that do exist diminish after accounting for selection. An absence of strong background effects among high school dropouts is noteworthy. These alternate background measures may fail to fully capture families' economic standing, including income disparities within occupations and education levels. Direct intergenerational transmissions among the least educated may depend on parental economic resources that are not captured by these cruder measures of parent socioeconomic status.

Finally, I explored differences between my findings and those of Torche (2011), who also examined family income elasticities in these datasets but used different samples and methods. Torche generally found a U-shaped pattern, with the lowest elasticities in the middle of the educational distribution. I attempted to replicate Torche's NLSY and PSID samples (with modest success) and subject those samples to my own analytic approach. I find that accounting for measurement error and educational selection accentuates the higher elasticities Torche found among the least educated, but reduces differences beyond high school completion. In other words, my analysis of the attempted replication sample reveals more of an L-shaped pattern consistent with my main findings. Our different findings appear to be due to the treatment of the data (adjusting for measurement error and educational selection) rather than case selection.

Discussion

Recent research casts doubt on the long-held belief that education is a moderating, equalizing force that fuels intergenerational mobility by breaking the link between one's social origins and destinations (e.g., Torche 2011; Witteveen and Attewell 2017; Zhou 2019). This study revisits this issue from a causal perspective, using methods designed to address multiple sources of bias that may plague prior research. Findings from three longitudinal surveys converge on new findings. Most notably, intergenerational economic mobility is uniquely low among high school dropouts, for whom childhood family income is a relatively strong predictor of adult family income. Economic mobility is significantly higher among high school graduates and remains similarly high among those with higher levels of education.

More specifically, contradicting prior descriptive mobility research and echoing Zhou's (2019) recent findings, I find no evidence that a bachelor's degree has a unique equalizing effect. Unlike Torche's (2011) descriptive analysis, but consistent with her more recent analysis of doctoral degree-holders (Torche 2018), I also find no compelling evidence that a graduate or professional degree has a disequalizing effect. The most unique contribution here is the finding that failing to complete high school seems to leave a strong direct link between family income in childhood and adulthood. Such direct links are substantially weaker among those who earn at least a high school degree. In other words, there is evidence that education has an equalizing effect, but it is concentrated at the threshold of high school completion.

This is the first study of this problem to address bias due to both measurement error in parent income and nonrandom selection into different education levels. Both are consequential. Parent income measurement error seems to attenuate intergenerational income elasticities and thus overstate mobility among high school dropouts, and it may also contribute to the spurious appearance of reduced mobility at the post-baccalaureate level. Selection bias further conceals the relatively low mobility among high school dropouts, and it contributes to spurious patterns of increasing mobility beyond high school completion. Analyses further suggest that the most salient sources of selection bias are related to unmeasured family characteristics and adolescent cognitive skills, the latter of which may have its own equalizing effect.

One implication of these findings is that the presumed equalizing effect of educational expansion in the twentieth century, posited to have increased the openness of American society (Hout 1988; Pfeffer and Hertel 2015), is unlikely to have actually been caused by gains in post-secondary attainment. Instead, it may have been due to increases in high school completion or to improved skills and family circumstances that also helped fuel educational expansion (Altonji, Bharadwaj, and Lange 2012). Furthermore, stagnation in the high school dropout rate since the 1970s (Heckman and LaFontaine 2010) may have contributed to the recent stagnation in economic mobility (Hout 2018; Lee and Solon 2009). Overall, my findings suggest that efforts to build cognitive skills and promote high school completion among disadvantaged youth may be crucial steps to increasing economic mobility. Such efforts could boost upward mobility and have the added benefit of preparing more students for success in higher education.

Turning to theoretical implications, my findings do not reveal any general substitutability or complementarity between schooling and social background. Hence, if the story is about meritocracy in the labor market, in must involve especially unmeritocratic processes in very low-skill sectors that hire high school dropouts. But the findings seem most consistent with theories of compensatory advantage in intergenerational processes (Bernardi 2014; Bernardi and Ballarino 2016). In combination, the low mobility (strong parental income effects) among high school dropouts and the weaker effects of cognitive skills among those from higher-income backgrounds suggest that economically advantaged families provide material, social, or cultural resources that help compensate for their children's lack of human capital.

This study emphasizes the causal status of education's role as an intergenerational moderator, but it has its own shortcomings in this respect. It is unclear how well I account for educational selection, and my inability to address the problem to the same extent in all analyses leaves questions about the different findings across datasets and genders. It is also unclear how well the model-based adjustments for measurement error work. Administrative data on parent income might be more accurate; the tradeoff is that such data often lack measures of skills and traits related to educational selection. Finally, to further understand these processes, we need to test specific mechanisms, not only of direct family background effects, but also of educational moderation. Likely candidates include family-based employment, intergenerational bequests, and family formation processes that could differ according to educational attainment. There is clearly much left to learn about education's role as a moderator of intergenerational stratification processes, and hopefully this study provides some footing going forward.

Endnotes

[1] Note that the prior literature and this study examine the effects of secondary and post-secondary educational attainment rather than the effects of education more broadly.

[2] Transitory fluctuations or other random errors in the child generation (the outcome) do not cause bias. Bias could also result from reporting errors systematically related to parent income or education in either generation, but this cannot be assessed without alternative income measures.

[3] Prior research fails to detect changes in mobility over these more recent cohorts (Lee and Solon 2009; Hout 2018), and my own additional analyses find no significant differences either. I exclude the Survey of Economic Opportunity (SEO) oversample of low-income families, which has been criticized for questionable sampling and generalizability (see Note 7 in Lee and Solon 2009). Like Torche (2011), I also exclude the immigrant refresher samples added later.

[4] I do not adjust for sampling weights. Weighting is only consequential if there are differential effects across groups with different weights (Solon, Haider, and Wooldridge 2015). I incorporate potentially problematic interactions into the analyses as confounders. Findings from baseline models with and without weights were very similar.

[5] To improve parent income estimates, I supplement NLS-W women's reports with those of their brothers in NLS-M and their parents in the Mature Women and Older Men surveys. In the PSID, Torche (2011) only included parent income during adolescence and dropped cases with fewer than 3 measures; I include measures from ages 1-18 and retain all cases with at least one measure. In the NLSY, prior analyses have used as few as one or two parent income measures (e.g., Torche 2011; Zhou 2019); I do as well, but I adjust for measurement error.

[6] In constructing these variables, I first use items pertaining specifically to college attendance and degree attainment; I then use the highest grade completed to code cases unresolved by these

items. PSID lacks comprehensive data distinguishing high school diplomas and GED's; following prior work on education-specific mobility, I do not differentiate the two.

[7] A concern with NLS-W and NLSY is that some respondents were old enough to have completed high school or started college, in which case these controls could be endogenous to schooling. This does not appear to influence the results; additional analyses provide almost identical patterns when limited to those assessed at ages 14-17 (see the supplement).

[8] This assumes classical measurement error. An alternative model with serial correlation of the transitory errors (e.g., Mazumder 2005) made no apparent difference; the true parent income predictions from both models were correlated above .99 at all education levels.

[9] For variance components, the standard deviations have half-normal (all-positive) priors $\sigma \sim N^{+}(0,2)$. Intercept priors are $\gamma \sim N(0,5)$ and priors on other coefficients are $\gamma \sim N(0,1)$.

[10] FE models interact within-family education differences with (mostly) between-family parent income differences to identify education's moderation effect.

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Table 1. Summary Statistics

	NLS-W			NLSY			PSID		
Parent Income Analysis									
Variables	Mean	SD	% Miss	Mean	SD	% Miss	Mean	SD	% Miss
Parent Income (log)	10.46	0.89	--	9.59	0.85	--	10.90	0.84	--
# measures	2.05	0.94	--	2.68	1.37	--	11.68	5.85	--
Age	17.05	1.57	--	17.03	1.42	--	10.89	5.30	--
Year of birth	1951	1.42	--	1962	1.31	--	1967	11.00	--
Observations and variances[a]	N	Var	% Share	N	Var	% Share	N	Var	% Share
Person-year (transitory)	6,155	0.34	43.88	16,953	0.22	30.69	60,332	0.35	50.95
Individuals	3,006	0.00	0.26	6,327	0.00	0.00	5,393	0.00	0.00
Families	2,036	0.44	55.87	4,571	0.50	69.31	1,965	0.20	29.37
Clusters	--	--	--	--	--	--	1,259	0.13	19.68
Intergenerational Analysis									
Variables	Mean	SD	% Miss	Mean	SD	% Miss	Mean	SD	% Miss
log-Income	10.88	0.94	--	10.56	1.06	--	11.06	0.92	--
Age	41.98	6.40	--	39.96	6.99	--	39.22	7.51	--
Year of birth	1952	1.08	--	1962	1.32	--	1964	9.85	--
Education									
< HS	0.11	0.32	--	0.13	0.34	--	0.19	0.39	--
HS	0.36	0.48	--	0.39	0.49	--	0.11	0.31	--
College	0.26	0.44	--	0.25	0.44	--	0.33	0.47	--
Bachelors	0.18	0.38	--	0.15	0.36	--	0.24	0.43	--
Graduate	0.09	0.28	--	0.07	0.26	--	0.13	0.33	--
Parent income (log)[b]									
Posterior mean	10.58	0.60	--	9.14	0.65	--	10.92	0.56	--
Posterior std dev	0.29	0.07	--	0.24	0.07	--	0.12	0.06	--
Parent education									
< HS	0.43	0.50	--	0.35	0.48	3.23	0.21	0.41	0.45
HS	0.32	0.47	--	0.39	0.49	3.23	0.23	0.42	0.45
College	0.12	0.33	--	0.12	0.32	3.23	0.26	0.44	0.45
Bachelors	0.08	0.27	--	0.08	0.28	3.23	0.17	0.38	0.45
Graduate	0.05	0.21	--	0.06	0.23	3.23	0.13	0.33	0.45

Siblings	4.00	2.67	0.66	3.80	2.61	0.13	3.54	1.74	3.06
Black	0.33	0.47	--	0.31	0.46	--	0.09	0.29	--
Other	0.01	0.11	--	0.21	0.41	--	0.02	0.14	--
Female	1.00	0.00	--	0.48	0.50	--	0.51	0.50	--
Cognitive skills	0.21	0.99	46.74	-0.49	0.93	3.73	--	--	--
Delinquency	--	--	--	0.03	0.81	7.72	--	--	--
Ed aspirations	14.05	1.89	--	14.29	2.19	0.42	--	--	--
Ed expectations	--	--	--	13.74	2.21	0.93	--	--	--
Parent ed aspirations	13.90	2.28	2.46	--	--	--	--	--	--
Occupational aspirations	53.30	18.50	29.71	--	--	--	--	--	--
Observations	N			N			N		
Person-year	5,244			46,287			36,115		
Individuals	1,057			5,260			3,982		
Families	914			3,869			1,943		
Clusters	--			--			1,252		

[a]Parent income variances are estimates (posterior means) from Bayesian hierarchical models in the first stage of analysis.
[b]Parent income estimates used in the intergenerational analysis are from the first-stage (parent income) analysis.

Table 2. Intergenerational Income Elasticity Estimates: NLSY, Gender Pooled

	Baseline		+ Controls		+ Family FE	
Elasticities	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)
< HS	0.252	0.050	-0.029	0.138	0.307	0.298
HS	0.336	0.029	0.071	0.136	0.091	0.291
College	0.222	0.035	0.025	0.145	0.075	0.305
Bachelors	0.219	0.044	0.057	0.153	0.132	0.312
Graduate	0.153	0.066	0.034	0.161	0.090	0.329
Differences	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)
HS - <HS	0.084	0.065	0.100	0.053	-0.216	0.976*
College - <HS	-0.030	0.701	0.054	0.217	-0.232	0.971*
Bachelors - <HS	-0.033	0.688	0.086	0.149	-0.175	0.888
Graduate - <HS	-0.099	0.883	0.063	0.264	-0.217	0.889
College - HS	-0.114	0.996*	-0.045	0.823	-0.016	0.576
Bachelors - HS	-0.117	0.987*	-0.014	0.584	0.041	0.359
Graduate - HS	-0.183	0.995*	-0.037	0.674	-0.001	0.508
Bachelors - College	-0.003	0.523	0.031	0.300	0.057	0.298
Graduate - College	-0.068	0.816	0.008	0.455	0.015	0.459
Graduate - Bachelors	-0.065	0.799	-0.023	0.612	-0.042	0.612

Estimates are from posterior distributions of elasticity parameters. *For educational differences, posterior probabilities above .95 are taken as significant evidence of declining elasticities (increasing mobility); posterior probabilities below .05 are taken as significant evidence of increasing elasticities (decreasing mobility).

Table 3. Intergenerational Income Elasticity Estimates: PSID, Gender Pooled

	Baseline		+ Controls		+ Family FE	
Elasticities	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)
< HS	0.459	0.050	0.498	0.090	0.541	0.154
HS	0.236	0.069	0.214	0.116	0.335	0.163
College	0.244	0.041	0.264	0.093	0.372	0.152
Bachelors	0.268	0.049	0.258	0.086	0.460	0.162
Graduate	0.309	0.065	0.283	0.108	0.464	0.174
Differences	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)
HS - <HS	-0.223	0.998*	-0.283	0.999*	-0.205	0.988*
College - <HS	-0.214	1.000*	-0.234	1.000*	-0.169	0.984*
Bachelors - <HS	-0.190	0.999*	-0.239	0.999*	-0.081	0.809
Graduate - <HS	-0.150	0.968*	-0.215	0.989*	-0.077	0.741
College - HS	0.008	0.463	0.049	0.272	0.036	0.360
Bachelors - HS	0.033	0.342	0.044	0.338	0.124	0.135
Graduate - HS	0.073	0.211	0.068	0.259	0.128	0.173
Bachelors - College	0.024	0.329	-0.005	0.536	0.088	0.135
Graduate - College	0.065	0.186	0.019	0.410	0.092	0.185
Graduate - Bachelors	0.040	0.305	0.024	0.382	0.004	0.492

Estimates are from posterior distributions of elasticity parameters. *For educational differences, posterior probabilities above .95 are taken as significant evidence of declining elasticities (increasing mobility); posterior probabilities below .05 are taken as significant evidence of increasing elasticities (decreasing mobility).

Table 4. Intergenerational Income Elasticity Estimates: NLS-W, Females

	Baseline		+ Controls	
Elasticities	Mean (β)	SD (β)	Mean (β)	SD (β)
< HS	0.198	0.090	0.385	0.177
HS	0.183	0.071	0.078	0.131
College	0.237	0.080	0.254	0.136
Bachelors	0.090	0.102	0.104	0.172
Graduate	-0.017	0.177	0.034	0.220
Differences	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)
HS - <HS	-0.015	0.566	-0.308	0.970*
College - <HS	0.039	0.367	-0.132	0.773
Bachelors - <HS	-0.108	0.789	-0.281	0.924
Graduate - <HS	-0.216	0.868	-0.351	0.920
College - HS	0.054	0.294	0.176	0.090
Bachelors - HS	-0.093	0.774	0.026	0.432
Graduate - HS	-0.200	0.860	-0.043	0.582
Bachelors - College	-0.147	0.876	-0.150	0.845
Graduate - College	-0.254	0.904	-0.219	0.852
Graduate - Bachelors	-0.107	0.702	-0.070	0.636

Estimates are from posterior distributions of elasticity parameters. *For educational differences, posterior probabilities above .95 are taken as significant evidence of declining elasticities (increasing mobility); posterior probabilities below .05 are taken as significant evidence of increasing elasticities (decreasing mobility).

Figure 1. Unconditional Elasticity Estimates. Intergenerational family income elasticity estimates without conditioning on education. Includes controls for race, age and year. The maximum likelihood analyses (MLE) measure parent income based on the average of available measures; the Bayesian analyses (Bayes) adjust for measurement error as described in the text.

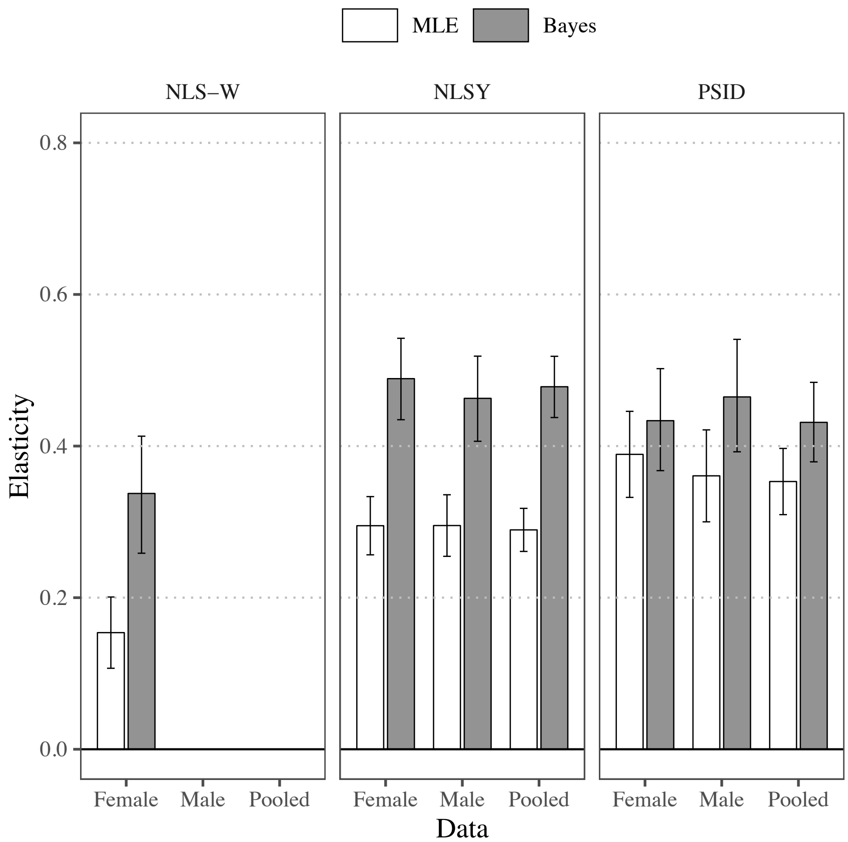


Figure 2. Sensitivity to Measurement Error, PSID. Education-specific family income elasticities from baseline intergenerational models (estimated via MLE), using average parent income based on 1, 2, 3, and 4 randomly selected parent income measures. Sample includes PSID cases with at least 4 parent income measures.

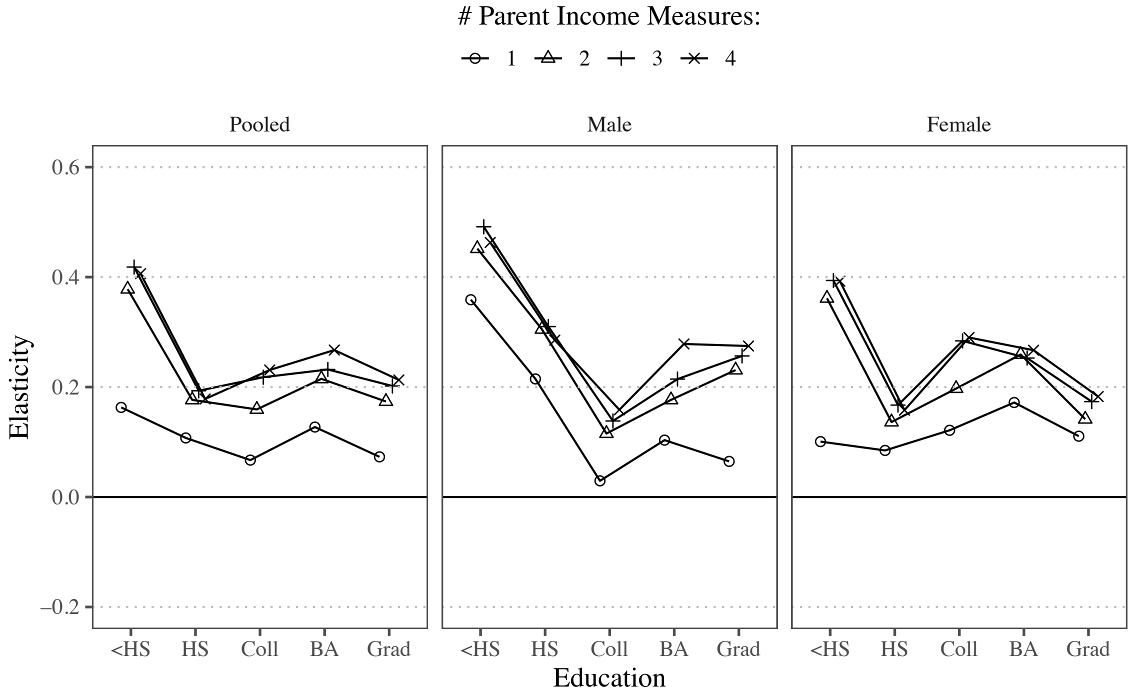
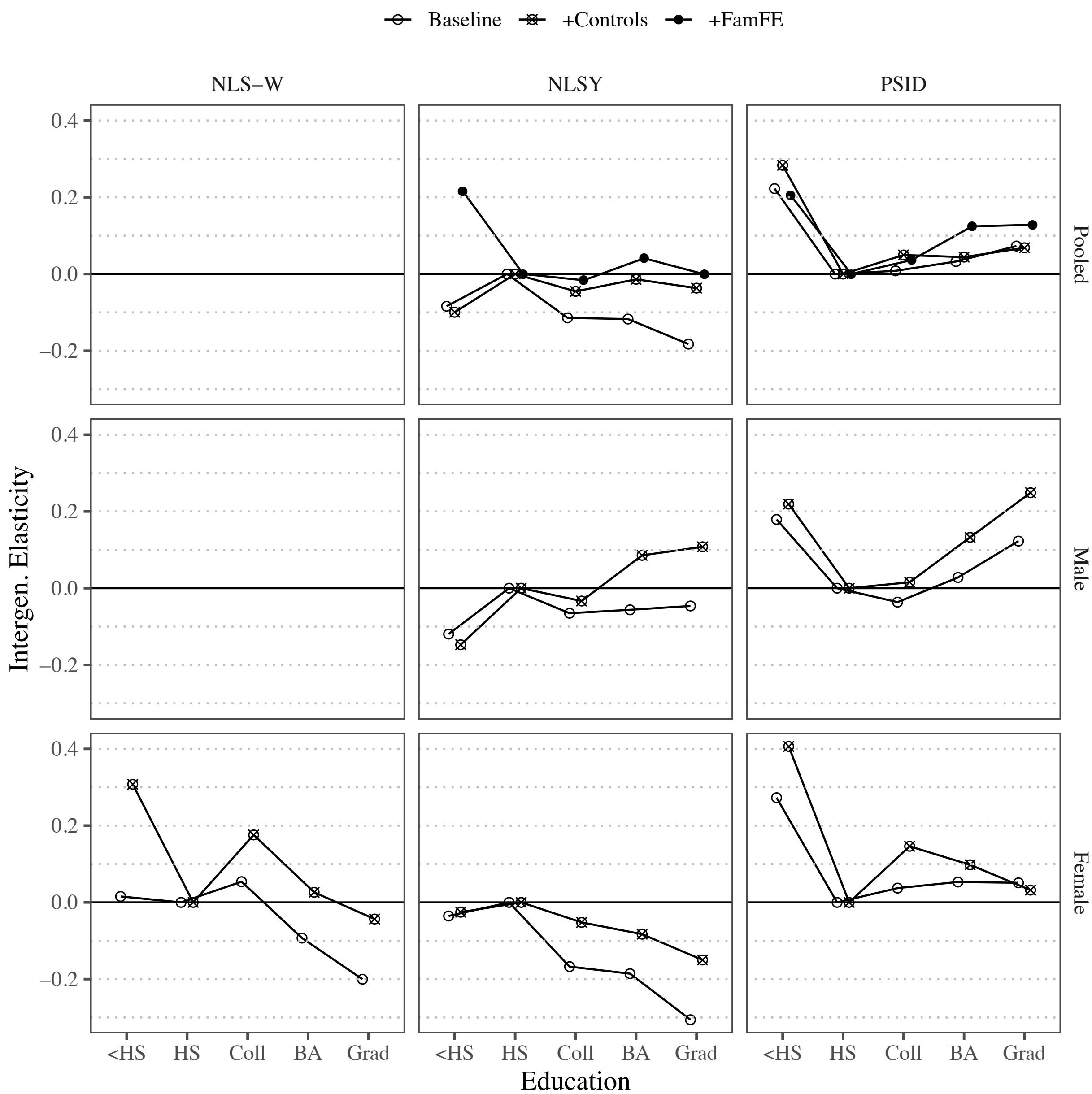


Figure 3. Intergenerational Elasticity Estimates, Adjusted for Measurement Error. Results based on three Bayesian hierarchical models: the baseline specification, one with additional controls for confounding, and one with controls and family fixed effects (gender pooled analyses only). Elasticities are recentered around the baseline estimates for high school completion to facilitate comparison.



Supplementary Materials

Conceptual Model

The educational moderation of interest manifests statistically as interactions between the effects of social origins (parent attainment) and education on destinations (child attainment). Figure S1 uses the OED (origin-education-destination) triangle (e.g., Goldthorpe 2014) to illustrate potential sources of these interactions. The solid black arrows decompose the total intergenerational effect into a direct effect and an indirect effect mediated by education. *U* represents confounders of schooling effects—things that affect schooling and child attainment, captured by solid gray arrows from *U* to *E* and *D*. When ignored, *U* will bias estimates of education effects and the corresponding indirect intergenerational effects; it will also bias direct effects by inducing a spurious association between *O* and *U* when conditioning on education, known as endogenous selection or collider variable bias (Elwert and Winship 2014; Zhou 2019).

My modification of the OED triangle is the addition of arrows to clarify sources of origin-by-education interaction. The black dotted arrows represent two causal reasons why direct intergenerational associations may vary with education: socioeconomic background may alter schooling's effects on attainment (path 1a), or schooling may alter direct background effects on attainment (path 1b). The gray dotted arrows represent spurious reasons for intergenerational associations to vary with education, all related to education's role as a selector; that is, individuals select or are selected into education systematically with respect to qualities that represent confounders here. One possibility is differential selection, which occurs when confounders' effects on schooling depend on one's social background (path 2a), or when background effects on schooling depend on confounders (path 2b). Another is that these confounders' long-term effects on socioeconomic attainment depend on social background (path

3a), or that the confounders alter direct background effects on attainment (path 3b). Theory and evidence regarding these various paths are discussed in the manuscript.

Gender-Specific Results and Control Variables

Tables S1-S2 summarize the PSID and NLSY gender-specific elasticity estimates that are presented graphically in Figure 4 of the manuscript. Table S3 summarizes estimates for selected control variables, based on the NLS-W model with controls and the gender-pooled NLSY and PSID models with controls and family FE's. These results are discussed in the manuscript.

Parent Income Estimates

Figure S2 plots each individual's adjusted parent log-income estimate (posterior mean of true parent income predicted from the first stage of the Bayesian analysis) against the mean of available observations, with a solid diagonal line showing a perfect linear relationship and a dashed line showing a linear fit to the data. The size of the points is proportional to the uncertainty (posterior standard deviation) of the adjusted estimates. The correlations between these estimates are high (over .95 in all cases), but the linear relations are flatter than the diagonals, indicating shrinkage toward the means in the adjusted estimates. This shrinkage is driven by imprecise estimates, especially by individuals with high or low averages based on only a few measures; the more uncertainty in the estimate, the more shrinkage. Not surprisingly, there is more uncertainty and more shrinkage in the NLS-W and NLSY than the PSID.

Influence of Measurement Error

Figure S3 summarizes results from intergenerational family income analyses intended to replicate the findings in Figure 4 but without adjusting for measurement error in parent family income, which is measured using the average of available observations. The baseline and control specifications are fit as random effects models via maximum likelihood; the fixed effects specification is fit via OLS; both use multiple imputation for missing covariates. Compared to the findings from the preferred analyses (Figure 4), these estimates tend to reveal less variation in elasticities across education levels, slightly lower elasticities among high school dropouts, and more evidence of an uptick in elasticities between the bachelor's and graduate degree levels. This suggests that differential attenuation bias due to measurement error may distort evidence of educational moderation; specifically, it may suppress the equalizing effects of high school completion and contribute to a spurious pattern of reduced mobility at post-baccalaureate levels.

Parental Education and Occupational Status

Additional analyses summarized graphically in figures S4-S5 replicate the intergenerational models predicting total family log-income, but they replace parent income with parent education or occupational status. For parent education, I use the highest level completed by either parent and assign the same five categories listed for children, but I treat parent education as continuous for simplicity. For occupational status, I use the Duncan SEI score (rescaled in 20-point units) for the highest-status occupation of either parent during adolescence; this is unavailable in the PSID.

These intergenerational effects are weaker than the parent income effects. This is not surprising given that these parent attainment measures do not account for the combined effects of both parents' status or for income differences within education and occupation groups. The most rigorous efforts to address selection suggest very minor differences in intergenerational effects across schooling levels, with no evidence of equalization. The most notable difference from the family income elasticity analyses is the absence of relatively strong parent education or occupational status effects among high school dropouts. This suggests that the direct transmissions among the least educated may depend on familial income-based resources not captured by cruder socioeconomic measures.

Age Restrictions

Figure S6 compares findings from the NLS-W and gender-pooled NLSY samples to a replication when limited to those surveyed at ages 14-17. This ensures that the controls are not endogenous to educational attainment. There are insufficient observations to replicate the family fixed effects analysis. The findings are very similar regardless of age restrictions, indicating that the findings reported in the manuscript are robust to concerns about the endogeneity of controls.

Comparisons to Torche (2011)

Here I revisit the differences between my findings and those of Torche (2011) in the NLSY and PSID. My preferred analytic samples use more recent data than Torche's, and thus incorporate more adult income data in both sources and add several additional cohorts in the PSID. Unlike, Torche, I exclude the PSID's SEO oversample of low-income families (see Endnote 3 in manuscript), and I include parent income data throughout childhood, whereas Torche only included adolescent measures. Torche also dropped PSID cases with less than three parent income measures; I incorporate them but adjust for measurement error. Finally, Torche measured parent and child income by averaging available measures during adolescence (14-22) and adulthood (38-42), respectively, whereas I include all time-specific parent (ages 1-18) and child (30+) measures in a multilevel model.

The advantages of my sample construction are that I include updated data, more child income observations during adulthood, more parent income measures throughout childhood in the PSID, and that I exclude fewer cases due to insufficient parent income measures (I only drop cases with no observations). This allows my analysis to yield more precise parent income estimates, to adjust for error in those estimates, and to adjust for educational selection, with less risk of sample selection bias. Nonetheless, to the extent that findings differ, it is useful to explore why. I first attempted to reconstruct Torche's sample and replicate her variable construction. I was unable to reconstruct the sample exactly. Torche no longer has the data or code from her study, but she graciously offered guidance that helped me come close. Table S4 shows the educational distributions in her original sample and my attempted replication.

Next I attempted to replicate Torche's family income elasticity patterns. Using my pseudo-replication sample, and treating the data as Torche did, I started with baseline models comparable to hers that do not address selection or measurement error (random effects models fit via MLE). The findings are illustrated in Figure S7 alongside Torche's estimates (obtained from Table 5 in Torche 2011). My baseline model closely replicates Torche's trends in elasticities among NLSY women and PSID men; there are modest discrepancies for the other samples. The main discrepancy in the NLSY male sample was Torche's higher elasticity among high school dropouts; the main discrepancy in the PSID female sample was Torche's lower elasticity among high school dropouts. Less importantly, but perhaps also of interest, I supplemented this analysis with controls for observed confounders and family fixed effects; this adjusts for selection but not measurement error. The adjustments for observed controls make little difference here, leaving patterns similar to the baseline analyses. Torche did not report gender-pooled analyses, but I incorporated family FE's into a pooled analysis. These analyses reveal a steady increase in elasticities across education levels in the PSID, and M-shaped fluctuation in the NLSY.

I then took the sample of observations (persons and years) from the attempted Torche replication and treated the data and analyses exactly as I did in the manuscript—a person-year dataset of all income measures, with a two-stage analysis using hierarchical Bayesian models to adjust for measurement error and educational selection. Figure S8 illustrates the results, again alongside Torche's. In both surveys, I obtain findings fairly similar to those reported in the manuscript (Figure 4). The main differences are slightly lower (but still high) elasticities among high school dropouts and slightly higher elasticities among bachelor's degree recipients. Relative to the baseline model—which in this case adjusts for measurement error—controls tend to mitigate any patterns of equalization beyond high school completion. The pooled FE analyses, which do the most to adjust for selection, reveal no evidence of a general trend of equalization, no evidence that elasticities are especially weak among bachelor's degree holders, and no evidence that elasticities are especially strong among graduate degree holders. Moreover, compared to the aforementioned analyses that did not adjust for measurement error, these FE models reveal relatively strong elasticities among high school dropouts, just as they did in the analyses with my preferred samples.

To summarize, I was unable to exactly replicate Torche's sample or elasticity estimates, but I came reasonably close. When subjecting the pseudo-replication sample to my full analysis, I find no evidence to support the equalization hypothesis, and I find patterns that are broadly consistent with those reported for my main analytic samples. Consequently, I am confident that the main differences in my findings and Torche's (2011) are due to the treatment of data rather than differences in case selection. In general terms, Torche found a U-shaped pattern, with the lowest elasticities in the middle of the educational distribution. Accounting for measurement error and educational selection accentuates the higher elasticities she found among the least educated, but weakens the differences in elasticities beyond high school completion.

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Goldthorpe, John H. 2014. “The Role of Education in Intergenerational Social Mobility: Problems from Empirical Research in Sociology and Some Theoretical Pointers from Economics.” *Rationality and Society* 26(3):265–89.

Torche, Florencia. 2011. “Is a College Degree Still the Great Equalizer? Intergenerational Mobility across Levels of Schooling in the United States.” *American Journal of Sociology* 117(3):763–807.

Zhou, Xiang. 2019. “Equalization or Selection? Reassessing the ‘Meritocratic Power’ of a College Degree in Intergenerational Income Mobility.” *American Sociological Review* 84(3):459–485.

Table S1. Intergenerational Income Elasticity Estimates: PSID, by Gender

	Males				Females			
	Baseline		+ Controls		Baseline		+ Controls	
Elasticities	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)
< HS	0.472	0.069	0.459	0.109	0.493	0.065	0.739	0.076
HS	0.293	0.103	0.240	0.139	0.220	0.094	0.333	0.130
College	0.257	0.062	0.255	0.120	0.257	0.051	0.479	0.071
Bachelors	0.321	0.069	0.372	0.130	0.273	0.069	0.431	0.090
Graduate	0.416	0.102	0.488	0.147	0.271	0.080	0.365	0.115
Differences	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)
HS - <HS	-0.179	0.943	-0.219	0.975*	-0.273	0.995*	-0.406	1.000*
College - <HS	-0.216	0.995*	-0.204	0.990*	-0.236	1.000*	-0.260	0.999*
Bachelors - <HS	-0.151	0.948	-0.087	0.787	-0.220	0.993*	-0.308	1.000*
Graduate - <HS	-0.056	0.673	0.030	0.443	-0.222	0.988*	-0.374	1.000*
College - HS	-0.036	0.622	0.015	0.444	0.037	0.356	0.146	0.095
Bachelors - HS	0.028	0.411	0.133	0.150	0.053	0.325	0.098	0.203
Graduate - HS	0.123	0.194	0.249	0.041*	0.051	0.338	0.032	0.376
Bachelors - College	0.064	0.233	0.117	0.109	0.016	0.425	-0.048	0.755
Graduate - College	0.159	0.086	0.233	0.026*	0.014	0.440	-0.114	0.863
Graduate - Bachelors	0.095	0.211	0.116	0.184	-0.002	0.504	-0.066	0.763

Estimates correspond to posterior distributions of elasticity parameters. *For educational differences, posterior probabilities above .95 are taken as evidence of declining elasticities; posterior probabilities below .05 are taken as evidence of increasing elasticities.

Table S2. Intergenerational Income Elasticity Estimates: NLSY, by Gender

	Males				Females			
	Baseline		+ Controls		Baseline		+ Controls	
Elasticities	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)
< HS	0.160	0.068	-0.363	0.204	0.362	0.072	0.252	0.188
HS	0.280	0.041	-0.215	0.203	0.397	0.042	0.277	0.180
College	0.215	0.055	-0.249	0.216	0.230	0.046	0.225	0.189
Bachelors	0.224	0.068	-0.130	0.227	0.211	0.058	0.194	0.198
Graduate	0.233	0.098	-0.108	0.246	0.091	0.086	0.127	0.205
Differences	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)	Mean ($\Delta\beta$)	P($\Delta\beta$<0)
HS - <HS	0.119	0.061	0.147	0.033*	0.035	0.332	0.026	0.393
College - <HS	0.054	0.268	0.114	0.117	-0.132	0.944	-0.027	0.610
Bachelors - <HS	0.063	0.253	0.233	0.023*	-0.150	0.955*	-0.057	0.694
Graduate - <HS	0.073	0.273	0.255	0.036*	-0.270	0.993*	-0.124	0.828
College - HS	-0.065	0.845	-0.033	0.675	-0.167	0.997*	-0.052	0.797
Bachelors - HS	-0.056	0.764	0.086	0.194	-0.186	0.998*	-0.083	0.841
Graduate - HS	-0.046	0.670	0.108	0.197	-0.306	1.000*	-0.150	0.925
Bachelors - College	0.009	0.458	0.119	0.106	-0.018	0.607	-0.031	0.645
Graduate - College	0.019	0.439	0.141	0.123	-0.138	0.925	-0.098	0.833
Graduate - Bachelors	0.010	0.468	0.022	0.419	-0.120	0.885	-0.067	0.732

Estimates correspond to posterior distributions of elasticity parameters. *For educational differences, posterior probabilities above .95 are taken as evidence of declining elasticities; posterior probabilities below .05 are taken as evidence of increasing elasticities.

Table S3. Selected Control Variable Coefficients

	NLS-W		NLSY		PSID	
	+ Controls		+ Family FE		+ Family FE	
	Mean (β)	SD (β)	Mean (β)	SD (β)	Mean (β)	SD (β)
Black	-0.090	0.140	--	--	--	--
× HS	-0.300*	0.150	-0.170	0.140	-0.280*	0.130
× College	-0.130	0.160	-0.010	0.150	-0.090	0.120
× Bachelor's	-0.230	0.170	0.090	0.180	0.020	0.160
× Graduate	0.150	0.220	0.260	0.240	0.160	0.230
× Parent Income	-0.190	0.120	0.110	0.190	0.110	0.190
Other	0.210	0.420	--	--	--	--
× HS	-0.270	0.680	-0.090	0.130	-0.740*	0.320
× College	-0.040	0.450	0.050	0.150	-0.380	0.260
× Bachelor's	-0.400	0.570	-0.150	0.190	-0.240	0.320
× Graduate	0.270	0.640	0.040	0.230	0.040	0.420
× Parent Income	-0.020	0.510	-0.080	0.200	-0.010	0.580
Parent educ.: HS	0.020	0.060	--	--	--	--
× Parent Income	0.110	0.110	-0.040	0.090	-0.190	0.180
Parent educ.: College	0.080	0.080	--	--	--	--
× Parent Income	0.040	0.160	0.120	0.160	-0.040	0.200
Parent educ.: Bachelor's	-0.170	0.130	--	--	--	--
× Parent Income	0.270	0.200	0.230	0.210	-0.020	0.250
Parent educ.: Graduate	-0.290	0.220	--	--	--	--
× Parent Income	0.520†	0.300	0.080	0.330	-0.050	0.270
Cognitive skills	0.100*	0.040	0.170*	0.040	--	--
× Parent Income	-0.220*	0.060	-0.120*	0.050	--	--
Delinquency	--	--	-0.040	0.030	--	--
× Parent Income	--	--	0.010	0.040	--	--
Siblings	-0.010	0.010	--	--	--	--
× Parent Income	-0.020	0.020	-0.020*	0.010	--	--
Educational aspirations	0.080*	0.020	0.000	0.020	--	--
× Parent Income	0.050†	0.030	0.000	0.020	--	--
Educational expectations	--	--	0.010	0.020	--	--
× Parent Income	--	--	0.010	0.020	--	--
Parent educ. aspirations	0.020*	0.010	--	--	--	--
× Parent Income	-0.040*	0.020	--	--	--	--
Occupational aspirations	-0.070*	0.030	--	--	--	--
× Parent Income	0.020	0.050	--	--	--	--

Estimates are from posterior distributions of regression coefficients. Controls involving gender, age and year of birth included but not shown. *|t-ratio|>1.96, †|t-ratio|>1.65.

Table S4. Educational Distributions in Torche (2011) and Attempted Replication

	NLSY		PSID	
	Torche	Replication	Torche	Replication
Males				
Total N	2,171	2,075	1,065	1,192
% < HS	24.9	16.0	11.1	11.1
% HS	36.1	42.6	40.8	33.3
% College	19.9	22.5	26.9	31.2
% Bachelors	13.7	13.5	18.3	20.2
% Graduate	5.4	5.3	2.9	4.2
Females				
Total N	2,093	1,994	1,476	1,418
% < HS	16.6	10.6	8.4	7.5
% HS	36.8	38.3	39.3	31.7
% College	26.6	30.4	33.1	38.4
% Bachelors	13.8	14.5	15.8	16.2
% Graduate	6.2	6.1	3.5	6.1

Figure S1. Conceptual Model. O refers to parent attainment (origins), D to socioeconomic outcomes (destinations), E to educational attainment, and U to confounders. Solid black arrows show direct and indirect intergenerational associations, and solid gray arrows show confounding of education effects. Dotted arrows show causal (black) and spurious (gray) sources of origin-by-education interactions.

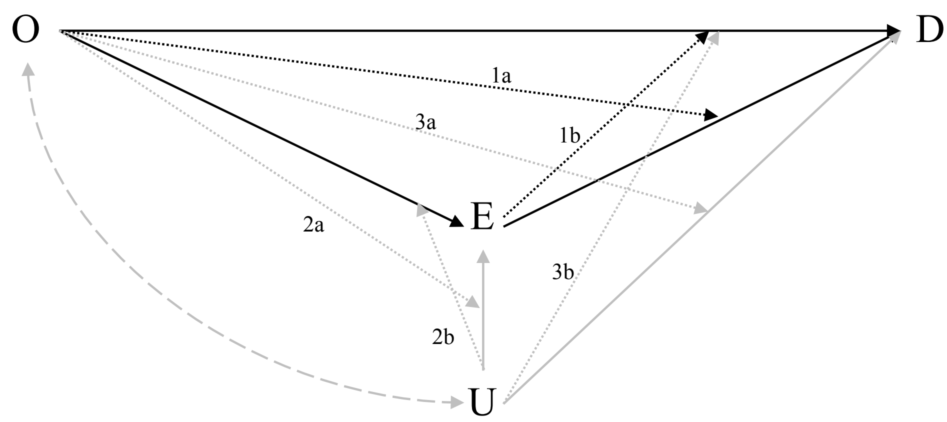

Figure S2. Comparison of Adjusted and Average Parent Income. Points represent the means of the posterior distributions of adjusted parent log-income (y-axis) and the average of available parent log-income measures (x-axis); both are centered around their sample means. Point sizes are proportional to the uncertainty in the adjusted estimates (the standard deviations of their posterior distributions). Gray diagonal corresponds to perfect relationship; dashed line corresponds to linear fit.

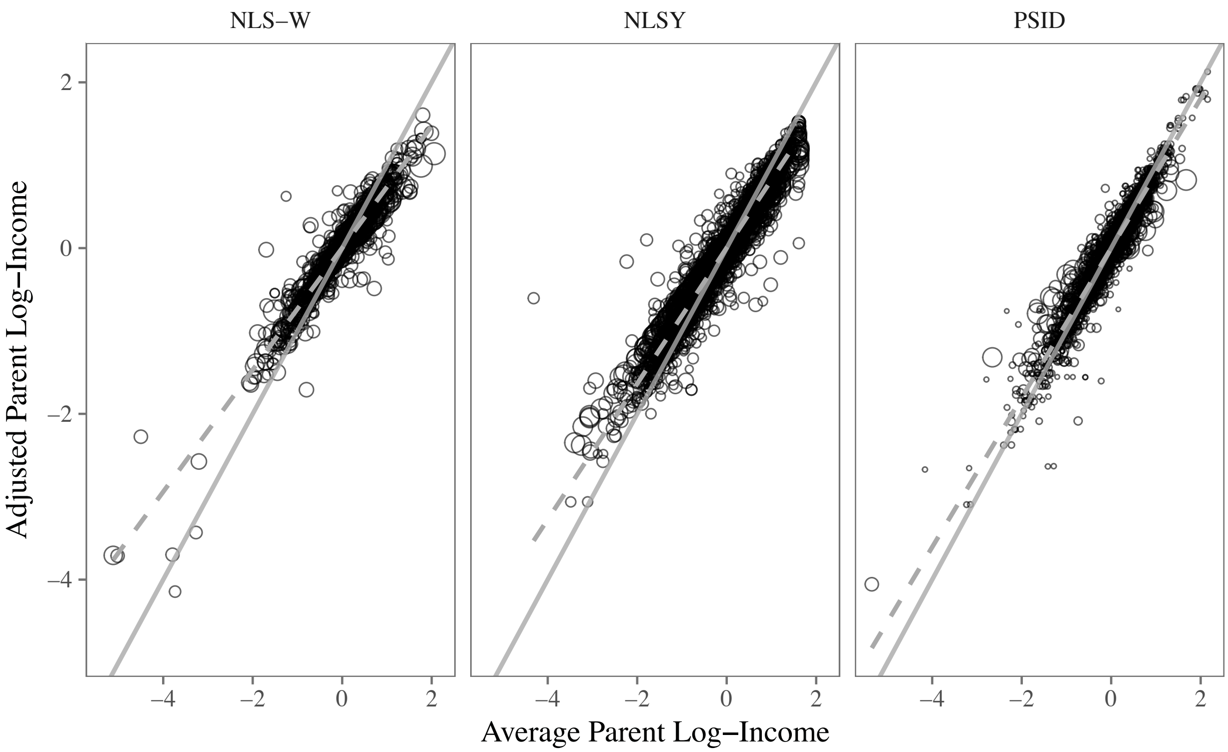

Figure S3. Intergenerational Elasticity Estimates without Measurement Error Adjustments. Based on specifications analogous to those summarized in Figure 4, but using the average of available parent income measures without further adjustments for measurement error. The random effects models are fit via MLE and the fixed effects models are fit via OLS.

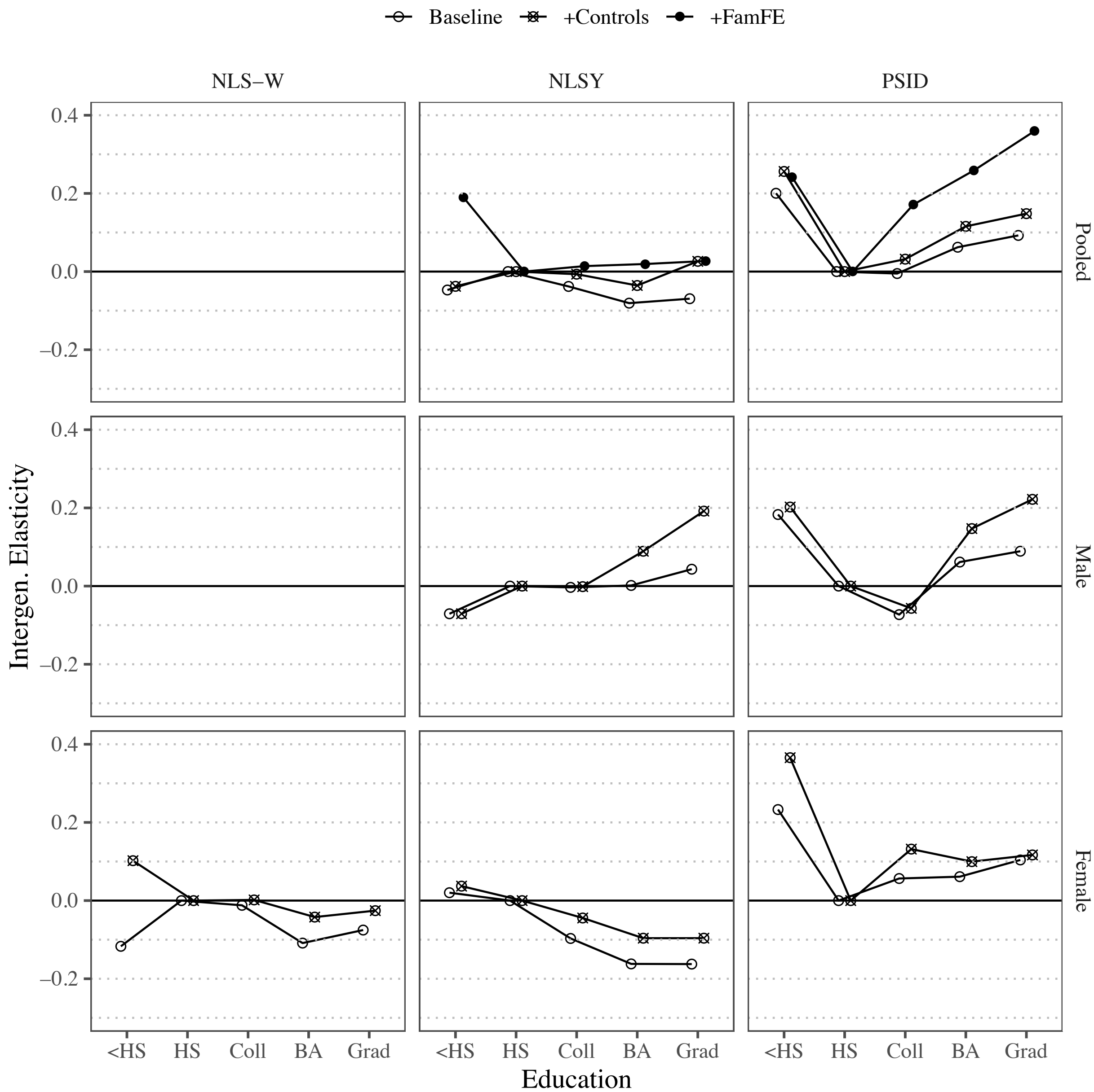

Figure S4. Parent Education Effects with Controls for Selection. Results based on three Bayesian hierarchical models: the baseline specification, one with additional controls for confounding, and one with family fixed effects (gender pooled analyses only). Elasticities are recentered around the baseline estimates for high school completion to facilitate comparison. Parent education (five categories) treated as continuous.

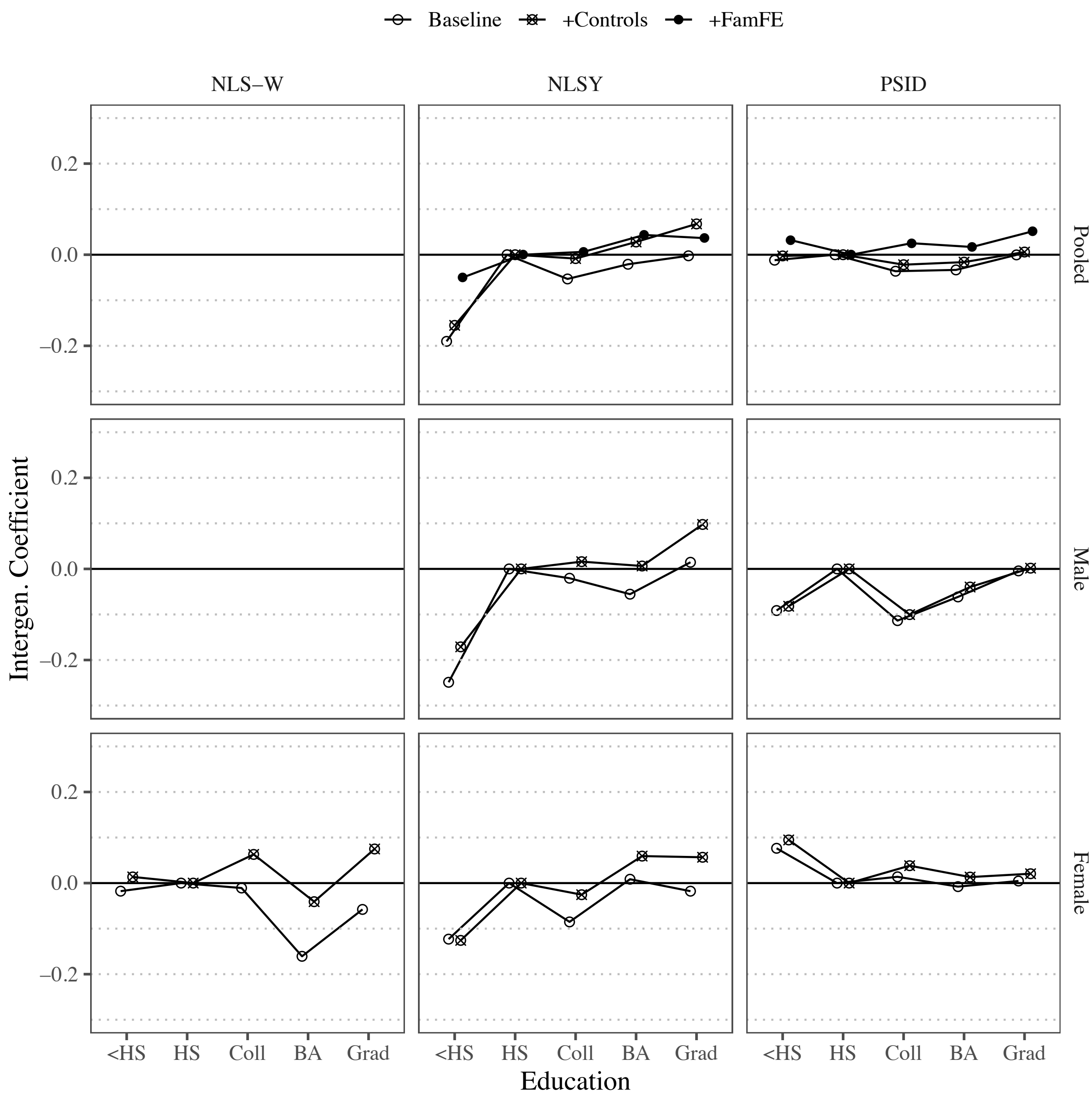

Figure S5. Parent Occupation Effects with Controls for Selection. Results based on three Bayesian hierarchical models: the baseline specification, one with additional controls for confounding, and one with family fixed effects (gender pooled analyses only). Elasticities are recentered around the baseline estimates for high school completion to facilitate comparison. Parent occupational status is rescaled to 20-point units (0-5).

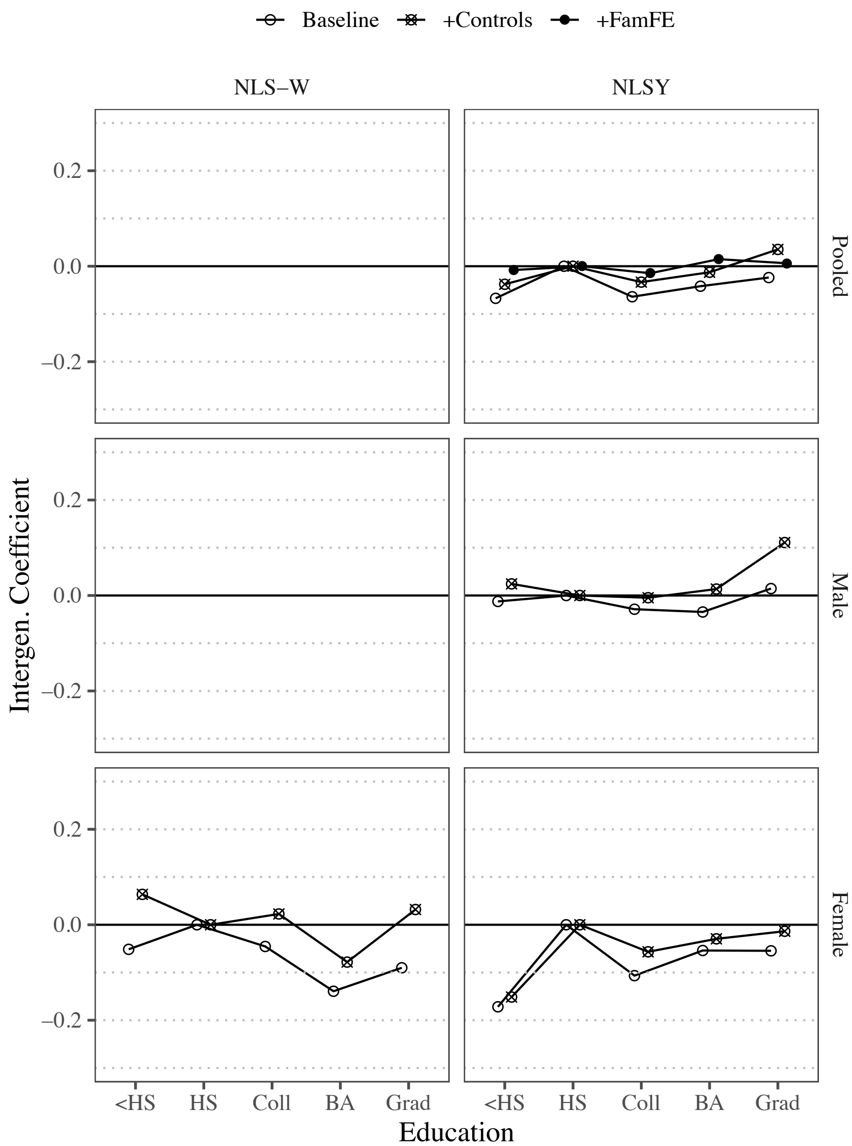

Figure S6. Sensitivity to Age at Survey. Elasticities presented from the baseline and control models for the NLS-W and NLSY cases who were 14-17 at the time at which controls were measured, presented alongside the estimates from the full samples (All). All estimates come from Bayesian models that adjust for measurement error in parent income.

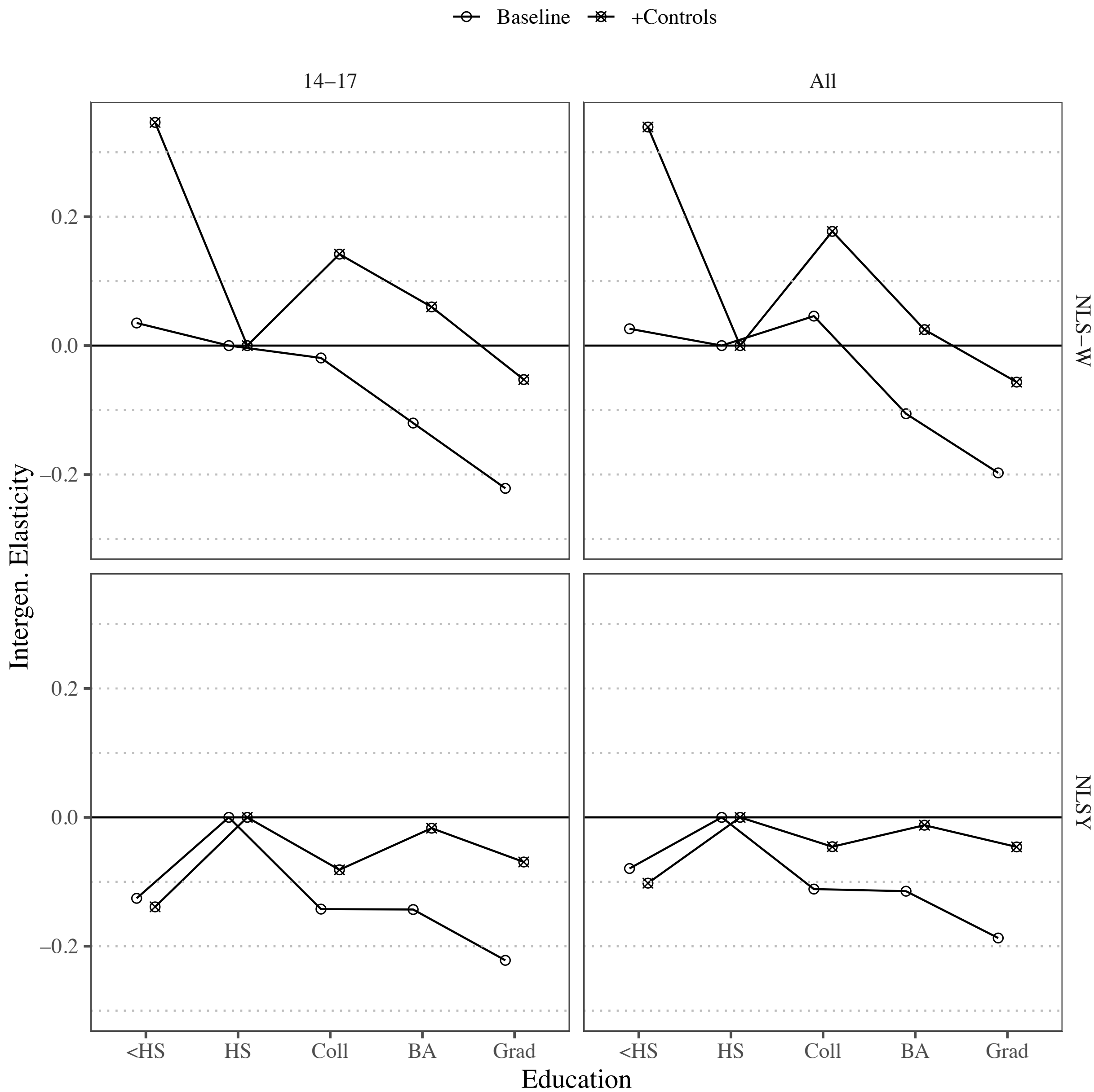

Figure S7. Intergenerational Elasticity Estimates, Torche Replication Sample, without Measurement Error Adjustments. Torche estimates are from her original paper (Torche 2011, Table 5). Based on specifications analogous to those summarized in Figure 4, but using the average of available parent income measures without further adjustments for measurement error. The baseline and control estimates are from random effects models fit via MLE, and the fixed effects estimates are from models fit via OLS.

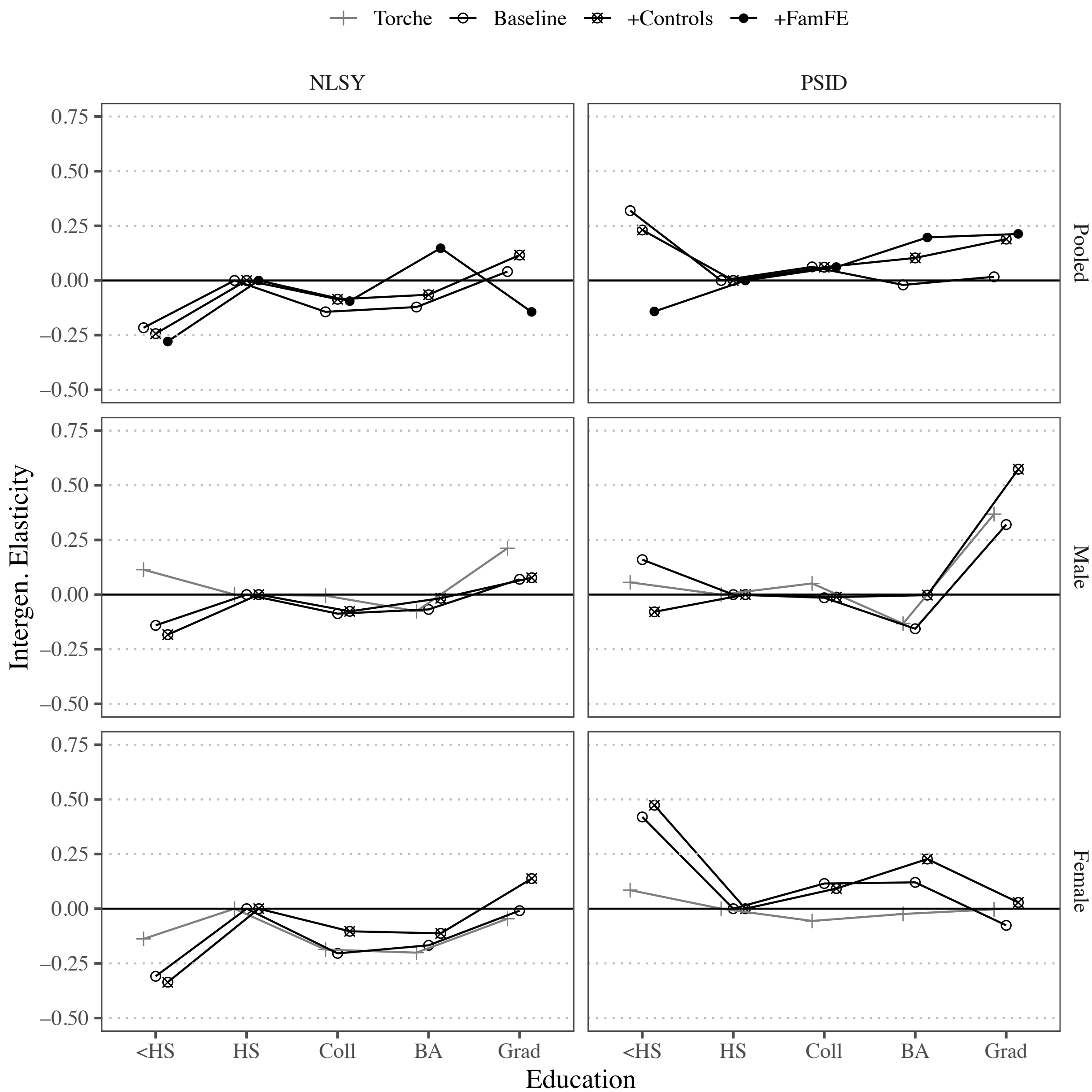

Figure S8. Intergenerational Elasticity Estimates, Torche Replication Sample, Adjusted for Measurement Error. Torche estimates are from her original paper (Torche 2011, Table 5). Other results based on three Bayesian hierarchical models of a sample intended to approximate Torche's: the baseline specification, one with controls for confounding, and one with controls and family fixed effects (gender pooled analyses only). Elasticities are recentered around the baseline estimates for high school education to facilitate comparison.