Does Educational Attainment Tend Towards Egalitarianism? Evidence from a Population Transitions Model*

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Abstract

Many scholars characterize educational attainment as a process that "tends towards egalitarianism" across the life course, based on the highly-replicated finding that family background effects on attainment appear to decline across educational transitions. Yet, since such descriptive transitions models typically measure disparities at a given educational level among only the "survivors" of all prior transitions, they fail to capture aspects of educational inequality arguably of greatest theoretical and policy interest. By proposing a population transitions model via a potential outcomes framework, I ask how educational inequality would evolve counterfactually over the life course for the entire high school-going population. Drawing on data from the NLSY97, I challenge the notion that individuals are decreasingly constrained by social origin over the educational life course: while only 35% of individuals from the lowest parental income group are expected to graduate college, that same figure is 76% for individuals from the highest parental income group. More straightforwardly, BA inequalities parallel inequalities in high school completion, a constancy across transitions not revealed by descriptive models. To highlight the policy implications of these results, I estimate hypothetical BA attainment inequalities under different degrees of college seat expansion. Contrary to expectations from descriptive transitions models, I show that even extreme higher-educational expansion would do little to reduce BA inequalities overall.

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The extent to which educational attainment is associated with one's social background informs a multitude of theoretical, philosophical, and policy issues. Levels of educational inequality in a given society, and their change over time, carry implications for questions of economic efficiency and fairness, the extent to which society lives up to the ideal of an 'education-based meritocracy,' and shifts in the allocation of educational credentials and opportunities over time.

The task of defining and measuring such inequalities has, however, been long-contested by social scientists. In the 1960s, stratification scholars sought to characterize changes in the effect of a wide range of components of social background on educational attainment across 20th century cohorts using a continuous measure of educational attainment ("years-of-schooling completed") (Duncan, 1967; Blau and Duncan, 1967; Sewell et al., 1969; Hauser and Featherman, 1976).[1] Despite the advances of these early pieces, the measure of inequality employed left them ill-equipped to characterize inequalities at different stages in the educational life course, as well as changes in these inequalities across cohorts. Different educational stages may exhibit different degrees of inequalities, be influenced by distinct institutional and political sources - such as the shift to post-industrialism (Treiman, 1970; Bell, 1973) - that affect these inequalities, and further alter the processes underpinning the intergenerational transmission of advantage in different ways (Mare, 2011; Mare and Chang, 2022).

In a series of landmark papers, Mare (1979; 1980; 1981) proposed an alternative analytical approach - one which examined inequalities in different schooling transitions, rather than in individuals' highest grade of schooling attained - with the promise of tackling these and related issues. Widely known as the "Mare Model" of educational transitions, the approach has become widespread in sociology, making decisive contributions to our understanding of patterns of educational inequality in cross-section, and how inequalities compare across time and place. In particular, it has led to the highly-replicated finding in countries including the US, UK and Israel that the predictive impact of social origin on attainment appears to decline across sequential educational transitions (Mare, 1980; Shavit and Blossfeld, 1993; Breen et al., 2009; Blossfeld et al., 2015; Wilbur and Roscigno, 2016). This stylized fact has led some scholars to characterize educational attainment as a process that "tends towards egalitarianism" across the life course, and argue that higher educational expansion may itself be sufficient to free individuals from the constraints of their social origin and enable them to attain further educational success irrespective of their social background (Müller and Karle, 1993;

[1] In this paper, I refer to "social background", "class origin", and "family background" interchangeably to refer to the set of socio-economic (dis)advantages experienced during upbringing. My empirical analyses employ "parental income rank" as one indicator of these socio-economic (dis)advantages.

Stolzenberg, 1994; Hout, 2007).

Nevertheless, such an approach shows only how inequality unfolds across educational transitions for the select group of individuals observed to actually progress through these transitions. As such, it is unclear whether the transformation of class-based inequalities across educational levels reflects the increasing social distance between parents and children across the life course and the impact of higher educational institutions on inequalities, or, if it instead reflects the positive selection of low-income youth: the fact that low-income students who successfully overcome the obstacles to higher education participation (Bailey and Dynarski, 2011; Jackson, 2013; Lucas, 1996) are more advantaged on non-economic axes compared with their non-college going counterparts (see especially Bowen and Bok, 1998; Cameron and Heckman, 1998, 2001; Jack, 2015, 2019). If the latter accounts for the observed decline in social background effects across transitions, extant measures of educational inequality will conflate patterns of inequality meaningful for assessment about life course progression and the role of educational organizations in producing inequality with patterns of selection. Further still, it could overstate the importance of higher-educational expansion for mitigating cumulative educational inequalities (Blau and Duncan, 1967; Hout, 1984, 1988; Mare, 2011; Torche, 2011), and could lead researchers to mistake changing patterns of selectivity across cohorts for meritocratic shifts in educational processes. Given these ambiguities, it is unclear whether extant empirical practice adequately captures aspects of educational inequality of greatest theoretical and policy interest.

To address this gap, I propose a "population transitions model" via a potential outcomes framework. This model makes a population level assessment about how educational inequalities unfold across the life course, by asking how inequality would counterfactually unfold if we considered the same set of hypothetical people at all transition point - that is, the resultant inequality we would observe at a given educational transition among individuals in a hypothetical experiment that sent a random sample from the full population of high school goers through the entire sequence of prior transitions. Compared with what I label a "descriptive transitions model," a model which describes how inequality unfolds only for the select group of students actually observed to progress through transitions, a population model estimates components of inequality that speak to the broader range of theoretical and policy concerns considered above. Using data from the 1997 cohort of the National Longitudinal Study of Youth (NLSY97), I use the case of parental income gaps in educational attainment to ask, Do higher educational levels truly exhibit less inequality in attainment than earlier transitions?

My proposed method builds upon a series of contributions that make important traction in adjusting for selectivity biases for pre-college educational transitions (Buis, 2011; Karlson, 2011; Lucas et al., 2011), but prioritizes instead how higher educational inequalities unfold. Such an examination is crucial not only because of the importance of later educational attainment such as BA completion for lower-income students' upward mobility, but because of the especially pernicious consequences of college non-completion for low-income youth (e.g. Zhou, 2021; Payne, 2022). My approach is further distinguished from previous work by explicitly targeting counterfactual, population level quantities which speak directly to a range of policy- and theoretically-relevant issues. These quantities can be seen as extensions of the "gap-closing estimand" introduced recently in sociology (Lundberg, 2022) to longitudinal settings, and build upon recent insights in sociology about the importance of adjusting for time-varying confounding in observational settings, both in higher education settings (Zhou, 2021; Zhou and Pan, 2021) as well as in different substantive contexts (see e.g. Wodtke et al., 2011; Wodtke, 2013; Lawrence and Breen, 2016).

The results challenge the notion that individuals are decreasingly constrained by their social origin as they progress through educational systems, and suggest that existing approaches to the study of educational transitions risk mischaracterizing educational expansion's equalizing potential. In particular, inequalities in college completion appear especially understated if assessed via a descriptive transitions model: the population transitions model reveals that inequalities in BA completion parallel extant inequalities in high school completion. Estimating hypothetical BA attainment inequalities under different degrees of college seat expansion I show that, contrary to expectations from descriptive transitions models, even extreme higher-educational expansion would do little to reduce BA attainment inequalities overall. These findings reveal new and important dimensions of inequality reproduction across the educational life course, and trouble the widely-cited conclusion that college is the "great equalizer," after whose attainment neither labor market nor educational outcomes depend much on social origin (Hout, 1984; Torche, 2011). They further suggest the importance of pursuing a population level approach for examining how life course educational inequalities evolve across cohorts meaningful for assessment about meritocratic educational shifts.

Educational Inequalities Across the Life Course

Descriptive Transitions Models of Inequality

By the 1960s, a "years of schooling" approach to studying educational inequality, which operationalized socio-economic status (SES) effects on educational attainment via ordinary least squares (OLS) regressions of years of schooling on SES variables, had become a standard tool for education stratification researchers seeking to establish patterns and trends in educational inequality (Blau and Duncan, 1967; Duncan, 1967; Duncan et al., 1972; Jencks et al., 1972; Sewell and Hauser, 1975). In a series of papers, Mare (1979; 1980; 1981) offered a landmark critique of this approach, proposing an alternative framework that would revolutionize the way stratification scholars approached the study of educational inequality. Rather than assessing overall inequality in number of grades completed, Mare proposed examining schooling inequalities as a *set* of inequalities at sequential educational transitions among individuals who have completed all prior transitions. Such an approach, Mare contended, mapped better onto the time-varying processes that characterize educational progression, shed light on which stages of education exhibit greater levels of inequalities, and permitted investigation into the institutional and political sources of inequalities at each individual stage and into the processes underpinning intergenerational transmission of advantage (Mare, 2011). As Mare wrote (1980, p. 295) in his original paper on the subject, a transitions approach to the study of educational inequality is important because "all phases of schooling may not require the same familial resources and structural advantages."

Mare's (1980) landmark finding was that the predictive ability of social origin declined over schooling transitions from the completion of elementary school to the attendance of graduate school. While Mare focused on educational inequalities for early 20th-century US birth cohorts, his finding of waning social background effects across transitions has been reproduced both for more recent US cohorts (Wilbur and Roscigno, 2016), as well as in a range of other countries such as France, Britain, Germany, Israel and Japan (Falcon and Bataille, 2018; Shavit and Blossfeld, 1993).

Transitions Models and Selectivity

By taking seriously the structure of educational continuation and decision-making, the Mare Model is attractive for its simplicity in better capturing the changing contours of educational stratification across the life course, compared with prior, years-of-schooling approaches. Such an approach has important merits on its own terms for particular research questions, such as those concerned with decomposing trends in inequalities and mobility over time (e.g. Bailey and Dynarski, 2011; Bloome et al., 2018).

Yet, an analysis of inequalities among the sample of individuals observed at each transition (the "risk set") only reveals inequalities among the select population observed to transition through different educational levels. This is because the risk set is a select population which likely differs across transitions: students who enroll in later stages of schooling are the "survivors" of a larger group of students who completed prior transitions (Mare, 1993). Importantly, selection into each risk set may well be correlated with family background, and increasingly so across transitions, given the structural obstacles disadvantaged children face in attaining higher levels of education on account of their socio-economic disadvantage (see especially Bowen and Bok, 1998; Cameron and Heckman, 1998, 2001; Jack, 2015, 2019). Mare (1980, p. 299) noted this selection bias issue in his earliest work - in particular, how selection on mental ability could alter his substantive conclusions:

> Thus the process of differential attrition itself may be sufficient to weaken the effects of a measured social background factor, father's schooling, on an unmeasured intervening factor, mental ability, and thereby to attenuate the observed reduced-form effect of father's schooling over levels of schooling. This implies that the lower-background-status members of a cohort will improve their ability composition over levels of schooling more than their high-status counterparts. Their relative gain in ability improves their relative chances for continuing to subsequent levels of schooling.

I illustrate this bias graphically in the top panel of Figure 2, which shows two DAGs for the hypothesized causal relationships between social background, indexed by parental income rank, R (a uniform variable taking values from 0 to 1), an early-years covariate such as cognitive ability (X), high school completion (A_1) and college attendance (A_2) in the top panel. Suppose we wish to compare extreme inequality (between the lowest and highest parental income ranks) in high school completion among the whole population, $\mathbb{P}[A_1 = 1|R = 1] - \mathbb{P}[A_1 = 1|R = 0]$, with inequality in college attendance *among high school graduates*, $\mathbb{P}[A_2 = 1|A_1 = 1, R = 1] - \mathbb{P}[A_2 = 1|A_1 = 1, R = 0]$. Suppose also that both R and X are positively correlated with both A_1 and A_2. Conditioning on A_1 in this latter quantity amounts to conditioning on a collider variable (Elwert and Winship, 2004), which artificially distorts the association between R and X (right DAG): given the obstacles that lower-income youth face in completing high school A_1, the lower-income students who make this transition may indeed "improve their ability composition over levels of schooling more than their high-status counterparts". This selection process could mechanically make the estimated inequality at the second transition ($\mathbb{P}[A_2 = 1|A_1 = 1, R = 1] - \mathbb{P}[A_2 = 1|A_1 = 1, R = 0]$) smaller than the estimated inequality at the first ($\mathbb{P}[A_1 = 1|R = 1] - \mathbb{P}[A_1 = 1|R = 0]$) even if inequality would not truly decline across these two transitions if we considered the same set of individuals for high school completion and for college attendance.

AO: would you do anything to make Fig 1 bottom panel clearer (re which are the colliders etc)?

Such a pattern of class-based selectivity may be especially pronounced at later educational stages. In his original paper on the topic, Mare (1980) found that inequalities in college graduation rates (among college-goers) were considerably smaller than inequalities in high school graduation rates (among high school goers). Yet, assessing college graduation inequalities among the risk set for this transition may be especially vulnerable to sample selection biases of the type discussed above. In contemporary America, college attendance is far from universal: only 41% of individuals born around around 1980 attended a 4-year college by age 22, compared with the high school completion rate of 85% among those same cohorts.[2] Figure 1, bottom panel, shows the analogous selection bias considered above, now examining inequalities in BA completion A_3, by parental income rank R, when we also consider a pre-college covariate such as income (Z).

Barriers to college entry for those from lower-income backgrounds are well-known (Jackson et al., 2007; Jackson, 2013; Morgan, 2012), but may have in fact increased in recent years alongside the "newly hostile regime" from the 1980s in the US that saw a dramatic lowering in public support for higher education, manifest in the increase in eligibility requirements for the Pell grant program and rates for Guaranteed Student Loans (GSLs), as well as a dramatic increase in tuition rates and in the overall the cost of college (Lucas, 1996). Indeed, the gap in college entrance between the lowest and highest parental income quartiles increased from thirty-nine to fifty-one percentage points between cohorts born in the early 1960s and those born some 20 years later (Bailey and Dynarski, 2011). Given the barriers to college attendance for low-income youth, we might expect the pattern of class-based selectivity described above to be especially pronounced into college, as opposed to into earlier educational transitions.

Are Individuals Decreasingly Constrained by Origin through Educational Systems?

As a result of these selection processes, it is impossible to determine from extant work whether the "waning coefficients" pattern across transitions reflects the inequalities that would be experienced by the whole population if this population were to progress, contrary to fact, through the sequence of prior transitions. Or, if it instead reflects the changing selectivity of individuals, since each transition compares a relatively more selective group (on characteristics correlated with further school continuation) of individuals from disadvantaged backgrounds to a relatively less selective group of individuals from advantaged backgrounds. Given the prevalence of such an approach to the analysis educational inequalities, such ambiguity carries through into important theoretical and policy

[2] Author estimates using the NLSY97.

concerns.

In particular, it is impossible to discern whether this pattern reflects a set of causal processes, such as the increasing social distance between parents and children across the life course and the impact of higher educational institutions on inequalities, on the one hand, or simply reflects the selectivity pattern described above, on the other. If students are decreasingly economically and socially dependent on their parents as they age, then parental resources decline in value for attainment across transitions, breaking the association between parental income and attainment across the educational life course (Müller and Karle, 1993). Yet, extant approaches cloud true life course and institutional processes with selection processes, and so leave us unable to adjudicate between these two possibilities.

Further, this selection pattern leaves us unable to accurately evaluate broader social good goals of higher educational levels and their associated institutions, in particular their role in either hampering or promoting equality by social origin. Consider the college completion transition. It is well-known that BA attainment is crucial for labor market success, and may even be especially important for leveling up individuals from poor as opposed to high-income backgrounds (Karlson, 2019, but see Zhou, 2019 and Fiel, 2020). Yet, college attendance itself is no guarantee of college completion: recent work suggests that college attendance without completion has particularly pernicious consequences for low-income youth (e.g. Zhou, 2021; Payne, 2022), who are often saddled with debt and have markedly worse labor market outcomes than their high-income, non-completer peers. Thus, an important question is: How much of a guarantee is a low-income individual's college attendance for their college completion, and how does this guarantee compare with that of a high-income student? Whether these two students have comparable guarantees of completion provides an overall assessment of the equity and efficiency of the higher-education system writ large, and in turn facilitates comparison of higher-education sectors across countries and time periods. By failing to adjust for selection, existing work fails to inform us about these issues.

Without an honest assessment of patterns of educational inequalities at a given educational level in a way that informs us about true degree of inequality at that level absent selection, we cannot begin to make an assessment about changes in these inequalities over time. A particularly prominent concern in early stratification scholarship was whether the effect of social background on educational attainment weakened over time or differed across societies with varying degrees of post-industrialization (Parsons, 1970; Treiman, 1970), especially amidst the post-industrial shift towards achievement-based allocation principles in the occupation system, and corollary expansion of the (higher-)education system, which researchers anticipated would lead to an increase in educational opportunity across cohorts. While the majority of these early studies, employing a years-of-schooling measure of educational inequality, emphasized constancy in stratification processes across US birth cohorts (Duncan, 1965, 1967; Featherman and Hauser, 1975; Hauser and Featherman, 1976; Halsey et al., 1980), cross-US cohort comparisons using the Mare approach showed that different levels of schooling exhibit different trends in inequality, with cross *increases* in inequality in intermediate (end of high school to college completion) educational transitions (Mare, 1981; Hout et al., 1993; Shavit and Blossfeld, 1993; Lucas et al., 2011; Wilbur and Roscigno, 2016, though see Lucas (1996)). Yet, insofar as the degree of selection into different education levels changes over time, such comparisons could well conflate changes in the degree of selectivity over time, especially as a growing proportion of successive cohorts attending college implies a decline in the selectivity of college-goers (Shavit and Blossfeld, 1993, ch. 1), with changes in educational inequality meaningful for assessment about meritocratic educational shifts. A first order issue must be to clarify the type of inequality most meaningful in capturing stratification processes distinct from patterns of selectivity.[3]

Ambiguity about the role of selection in driving the apparent decline in background effects across transitions is not only consequential for theoretical reasons, however, but also because it carries important policy implications. Indeed, certain scholars have taken the pattern of waning coefficients to imply that processes of educational expansion, particularly in the upper tiers of the education system, would be sufficient for equalizing educational opportunities (e.g. Hout, 2007), since entrance to higher educational levels implies a process of selection and attainment grounded primarily on merit and achievement which sees the "socioeconomic liberation of college graduates from their status origins." (Stolzenberg, 1994, p. 1068, see also Treiman, 1970; Hout et al., 1993; Mare, 1979; Torche, 2011). Certainly, this pattern seemed to accord with late theories of industrialization, which predicted that technological advances and educational expansion would induce a shift from ascriptive- to merit-based allocative processes in both the educational system and labor market (Treiman, 1970; Bell, 1973). Yet, such a policy prediction may well be misplaced if observed lower social background effects on attainment at higher educational levels reflect lower-income students' hyper-selection, rather than a true causal effect of educational expansion. If this is true, then mechanical expansion of the post-secondary system would not, by itself, guarantee a reduction in post-secondary completion in-

[3]Further, Mare argued that the conclusion of stability of class background across cohorts reported by prior authors was in fact a methodological artifact (Mare, 1981): OLS coefficients on social background variables decline across subsequent cohorts as a result of increasing education participation across class groups, even absent any change in the true association between social background and attainment.

equalities, and could even cause BA completion inequalities to increase.

Given these ambiguities, it is unclear whether extant empirical practice captures components of educational inequality of greatest theoretical and policy interest. Such approaches can be contrasted with one that makes an assessment about how educational inequalities truly unfold across the life course at a *population level*. A "population transitions model" captures the thought experiment described in the Introduction: imagine sampling a small group from the high school going population, intervening to ensure that all these individuals complete a given sequence of educational transitions, and then calculating the inequality in the share of those individuals who make the next educational transition. Most importantly, such an approach involves a comparison of inequalities across transitions *for the same set of individuals*, and so is better equipped to address questions about components of inequality that speak to the questions considered above.

A Population Transitions Model

The population transitions model described in the previous section can now be formalized mathematically as follows. As previously, suppose we index class origin by parental income rank R, taking values from 0 to 1. Further, let A_{k+1} denote the $k+1$th educational transition, and let overbar notation denote an individual's transition history up to the kth transition, e.g. $\overline{1}_k = \{A_1 = 1, \ldots, A_k = 1\}$ denotes that an individual progressed from the 1st through the kth educational transitions. In my empirical analysis, I assign high-school completion to be my first educational transition (A_1), given the almost universal nature of high school attendance in the contemporary US. Thereafter, I analyze three further educational transitions: college attendance, college completion, and graduate school attendance, denoted, respectively by A_2, A_3, and A_4, although the exposition below is general enough that any sequence of transitions could be analyzed via the framework.

To formalize the population transitions model, I use standard potential outcomes notation, such that $A_{k+1}(\overline{1}_k)$ denotes an individual's potential transition status at transition A_{k+1} were that individual to progress through the entire sequence of prior transitions 1 through k. As an illustration, $A_3(\overline{1}_2) \triangleq A_3(A_1 = 1, A_2 = 1)$ denotes an individual's potential BA completion status were that individual to complete high school as well as to attend college.

Before proceeding, we must specify a working definition of inequality for comparisons across educational transitions. Discretizing parental income (a continuous variable) into deciles, for instance, and comparing inequalities across deciles would lead to an unwieldy number of contrasts at a given transition as well as across transitions. To sidestep this issue, I assume a parametric model for how the probability of success at a given educational level varies across parental income ranks. Specifically, I assume that inequality in both the *observed* and *potential* probability of success at a given transition is linear in parental income rank, such that we can characterize inequality at that transition via a two-dimensional summary measure: an intercept and a slope.[4]

Under such a linear model, consider parent income inequality in the *observed* probability of completing transition A_{k+1} among all individuals *who have completed the prior k transitions*, namely,

$$\mathbb{P}[A_{k+1} = 1|\overline{1}_k, R] = \beta_0^k + \beta_1^k R, \tag{1}$$

which I refer to as a "conditional inequality". I refer to the set of conditional inequalities under all educational transitions of interest as a "descriptive transitions model". Conditional inequality can be compared with inequality in the *potential* probability of completing transition A_{k+1} *among the whole population:*

$$\mathbb{P}[A_{k+1}(\overline{1}_k) = 1|R] = \beta_0^{*k} + \beta_1^{*k} R, \tag{2}$$

which I refer to as a "controlled inequality," extending the notion of counterfactual inequalities (Lundberg, 2022; Zhou, 2019) to a series of treatments (in our case, education transitions).[5] The set of controlled inequalities across all educational transitions considered characterize a "population transitions model," because they describe how educational inequality would evolve if we considered the same set (i.e., the entire population) of individuals at each transition. Compared with controlled inequalities, which consider counterfactual outcomes among the whole population ($\mathbb{P}[\cdot|R]$), conditional inequalities consider observed outcomes among individuals who have made all prior transitions ($\mathbb{P}[\cdot|\overline{1}_k, R]$).

Comparing (i) β_0^* with (ii) β_0 contrasts (i) the counterfactual probability of the most disadvantaged individuals (those with a parental income rank of 0) making the $k+1$th transition in the hypothetical experiment, with (ii) the observed probability of the most disadvantaged individuals, who are currently observed to make the first k transitions, making the $k+1$th transition. Meanwhile, com-

[4]Formally, these quantities are defined as linear projections of individuals' observed transition status and potential transition probability (given parental income rank) onto parental income rank (see Appendix A for further details).

[5]Notice that $A_0 = \emptyset$, such that for $k = 1$, the relevant population quantity is simply the descriptive quantity $\beta_0^1 + \beta_1^1 R$.

paring (i) β_1^* with (ii) β_1 contrasts the difference in counterfactual probability of making the $k+1$th transition between individuals from the lowest and highest parental ranks in the experiment with (ii) the difference in the observed probability of making the $k+1$th transition between individuals from the lowest and highest parental ranks among individuals who have made the prior k transitions transition. Further, comparing β_1^{*k} across different educational levels enables an assessment of how much attainment inequality would occur as and when a random sample of the full population of high school goers, as opposed to a select group of high school goers, encounter them.

Identification and Estimation

The parameters capturing controlled inequalities at a given transition (β_0^{*k} and β_1^{*k}) under the population transitions model are population-based quantities because they capture how much inequality would result at a given educational transition were the entire population (that is, of all high school goers) to progress, contrary to fact, through the entire sequence of prior transitions. Since controlled inequalities are defined in terms of a hypothetical experiment, and are thus not observed under the status quo. Nevertheless, they can be identified using observational data under the assumptions of "sequential ignorability", "consistency", and "system invariance."

I defer most technical details to Appendices A and B, but the broad ideas behind this approach can be outlined as follows. To state sequential ignorability formally, let X be a set of confounders for the effect of the first transition of interest (high school completion) on the second (college attendance). Additionally, let $\overline{Z}_k = (Z_1, \ldots, Z_k)$ denote a vector of post-treatment confounders, that is, confounders for the effect of each transition on the next. For example, Z_1 denotes a set of variables that may confound the effect of college attendance on college completion; Z_2, a set of potential confounders for the effect of college completion on graduate school attendance, etc. Importantly, and as will be discussed in the following sections, the set Z_k is undefined for individuals who do not make the *kth* transition (cf. Stolzenberg, 1994). Sequential ignorability can formally be expressed as follows:

$$A_{k+1}(\overline{1}_k) \perp\!\!\!\perp A_k | X, \overline{Z}_{k-1}, \overline{A}_{k-1} \tag{3}$$

$\forall k \in \{0, \ldots, K\}$ with $A_0 = Z_0 = \emptyset$ and where $R \in X$. This assumption states that each individual's potential transition value is independent of observed prior transition value conditional on prior covariates, and would be satisfied in Figure 2, which shows a directed-acyclic graph (DAG) in which no unobserved confounding exists for any of the educational transition effects.

[Insert Figure 2]

The consistency assumption, which is standard in the causal inference literature, connects observed outcomes to potential outcomes, and implies that individuals' potential transition value $A_{k+1}(1_k)$ is fixed. Finally, system invariance connects potential outcomes under the status quo to outcomes under the hypothetical experiment, by stipulating that individuals' potential outcomes are unchanged under the experiment (Opacic et al., 2023). This assumption would be violated, for example, if the changing prevalence of individuals at college under the experiment altered processes of stratification within higher-education institutions, for instance, by diluting college resources vital for low-income students, in turn lowering their BA completion rate. The system invariance assumption can thus be made more plausible by interpreting parameters pertaining to the hypothetical experiment locally: as the resultant inequality we would observe were a small, random sample of individuals to progress through different educational levels (cf. Lundberg, 2022).

Under these assumptions, controlled inequalities can be identified using the g-formula, which has typically been applied in the case of time-varying treatments (see Robins (1986; 1997), as well as e.g. Wodtke et al. (2011) and (Lawrence and Breen, 2016) for applications in sociology). Appendix A demonstrates the differences between the population and descriptive transitions models differ under this identifying formula, and shows mathematically why the g-formula sidesteps the selection issues that plague conditional inequalities in descriptive transitions models. While sequential ignorability is a weaker identification assumption than one that relies on a single set of confounders for all transitions, this assumption is still strong and unverifiable: we can never be certain that we have accounted for all relevant confounders in our model. To test the robustness of my causal claims to this identification assumption, in Appendix I, I report a sensitivity analysis that assesses the sensitivity of my main results to different degrees of unobserved confounding under certain assumptions about the nature of confounding.

The descriptive parameters (β_0^k, β_1^k) that characterize conditional inequality at the $k+1$th transition, for each of the $k = 1, \ldots, K$ transitions of interest, can be estimated by simply regressing individuals' outcomes for a given transition on parental income rank, among individuals who have made all prior transitions via OLS. To estimate the parameters $(\beta_0^{*k}, \beta_1^{*k})$ characterizing controlled inequality at the $k+1$th educational transition, I employ a nonparametric estimation approach which draws on a debiased machine learning (DML) algorithm (see Rotnitzky et al., 2017; Chernozhukov et al., 2017).

While several approaches could in principle be used for estimation, including g-computation (Robins 1986; 1997) and marginal-structural-models (MSMs) (VanderWeele et al., 2014), the use of DML for fitting models is particularly attractive in our context since ML algorithms are highly robust to model misspecification; as I describe in the following section, I employ a wide range of individual-, family,-school- and college-level confounders. While increasing the number of theoretically-relevant confounders increases the plausibility of the sequential ignorability assumption, it comes at the increasing risk of model misspecification bias when using parametric estimators due to the high number of possible functional forms (interactions and nonlinearities) that may exist in the data. A nonparametric estimation approach sidesteps these issues by learning such functional forms from the data.

The DML algorithm consists of the following two steps:

1. Estimate a Neyman orthogonal "signal" for the (unconditional, or population) counterfactual probability of success at the $k+1$th transition.

The Neyman orthogonal signal for each individual is a function of that individual's observed covariates, and consists of two of more "nuisance functions": functions necessary for estimating the causal effect of interest but not of substantive interest by themselves. For example, the Neyman orthogonal signal for the parameters $(\beta_0^{*1}, \beta_1^{*1})$, which capture the controlled inequality in college attendance we would observe were a random sample of individuals drawn from the entire population to complete high school, is

$$\mathbb{E}[A_{2i} \mid X_i, A_{1i} = 1] + \frac{\mathbb{I}(A_{1i} = 1)}{\mathbb{P}[A_{1i} = 1 \mid X_i]}(A_{2i} - \mathbb{E}[A_{2i} \mid X_i, A_{1i} = 1]). \tag{4}$$

We see that computation of the signal in Equation 4 requires estimating two nuisance functions, $\mathbb{E}[A_{2i} \mid X_i, A_{1i} = 1]$ and $\mathbb{P}[A_{1i} = 1 \mid X_i]$, and then obtaining predicted values at each individual's observed X_i. Computation of the signals for the controlled inequality in college completion and for graduate school attendance similarly requires estimating of (two or more) nuisance functions and obtaining predicted values at observed values of covariates (see Appendix B for the relevant signals). Throughout the analyses that follow, I use ML estimators to fit these nuisance functions. To reduce bias induced by the over-fitting of traditional machine-learning estimators, I follow standard practice in using a procedure called "cross-fitting" for estimation of these signals, which entails using different subsamples for fitting the nuisance functions and obtaining predicted values (Chernozhukov et al., 2017).

2. Estimate $(\beta_0^{*k}, \beta_1^{*k})$ by regressing the relevant Neyman orthogonal signal for the given transition

on parental income rank.

Valid standard errors for $(\beta_0^{*k}, \beta_1^{*k})$ can then be obtained via the "sandwich" estimator of the corresponding regression coefficients. The DML approach is further advantageous in that it can be used to obtain valid standard errors that are also in general smaller than those that would be obtained via alternative estimation approaches. Full details about this DML procedure are provided in Appendix B.

Comparison with Existing Approaches

My approach builds upon a series of contributions that make important traction in assessing inequalities at pre-college educational transitions in ways that adjust for class-based selectivity. In particular, across the high school graduation to college entry transitions, Lucas and colleagues (Lucas, 2001; Lucas et al., 2011) showed that, for US cohorts who were college sophomores between 1966 and 1990, coefficients on various measures of social background fail to wane across these two transitions after controlling for a range of family demographics and high school test scores, replicating the finding that even when not adjusting for these covariates, the effect of social background is constant across these transitions (see Lucas, 1996).

In addition to its substantive focus on attainment across the entire educational life course, my approach can be distinguished from this prior work on three methodological levels. Primarily, my approach differs in being the first to define model-free estimands directly capturing a population level quantity. As Mare (2011, p. 242) pointed out, "even when the concern about bias is limited to the problem of selective attrition in school transition models, an estimated empirical relationship can be properly said to be "biased" only if there are clearly defined parameters for which an unbiased estimate is in principle attainable." In consequence, quantities estimated by prior approaches are substantively ambiguous. Consider the regression-based approach pursued by Lucas and collaborators (Lucas et al., 2011) to assess inequalities in A_3, BA completion. Under this approach, the researcher would fit a model of A_3 on confounders X and $A_2 * R$ among units observed to complete high-school (i.e., with $A_1 = 1$), and argue that this effectively deals with the selection issue discussed above. The coefficient on $A_2 * R$ does not, however, estimate the population quantity β_1^{*2}, since by adjusting for covariates (Z_1) that may lie on the causal path $A_1 \rightarrow Y$ and $A_1 \rightarrow A_2 \rightarrow Y$, this approach blocks an important component of the composite effect of A_k on A_{k+1}, as well as the association between R and Z. Instead, the coefficient on $A_2 * R$ captures how much inequality would result in the experiment if we simultaneously equalized their values of all covariates X.

Second, my approach employs an identification strategy that facilitates inclusion of a distinct set of observed time-varying confounders at each educational transition. Such an inclusion in fact facilitates weaker *identification* assumptions compared with prior approaches. In particular, prior approaches relying on either standard ignorability assumptions based on observed characteristics (e.g. Lucas et al., 2011), or on a fixed-effects design that requires no within-sibling-pair in factors predictive of educational progression (Mare, 1993), implicitly assume away dynamic selection of individuals into and out of educational transitions.[6] Yet, such an assumption is at odds with both the logic of educational transition models, whereby certain variables at a given transition such as college GPA are defined only for those individuals who have made the prior transition (cf. Stolzenberg, 1994), as well as with a vast literature highlighting the effects of educational organizations on academic performance and other resources (e.g. Downey and Condron, 2016; Meyer, 1997; Jennings et al., 2015; Raudenbush and Bryk, 1986).

Finally, compared with prior *modeling* strategies for selection adjustment in educational transition models, which rely on the specification of a parametric form for either the distribution of the error term (Holm and Jæger, 2011; Buis, 2011, 2013) or rely on parametric selection-on-observables in either a logit or probit model (Lucas et al., 2011), the population transitions model facilitates a wholly nonparametric estimation procedure. Such an approach is useful because it rests on exploiting empirical patterns of confounding across transitions observed in data, rather than relying on an a priori specification of observed (or unobserved) selection patterns (c.f. Lundberg et al., 2020). This focus on dealing with observed rather than with unobserved confounding can be seen as responding to calls to address bias in educational transition models via the analysis of "measured heterogeneity" (Mare, 2011), rather than, for example, assuming a particular structure of unobserved heterogeneity. As Mare (1993) himself wrote: "The direction of the distortion of observed effects by underlying heterogeneity [in the Mare model] must be resolved empirically."

Data

To examine descriptive and population educational transitions models, I draw on data from the National Longitudinal Survey of Youth 1997 (NLSY97), a longitudinal study that began with a nation-

[6] In Appendix D, I demonstrate how a standard (single-confounding set) ignorability approach could lead to misleading conclusions about controlled inequalities, compared with a weaker, sequential ignorability approach.

ally representative sample of individuals at ages 12-17 in 1997 who were interviewed annually up to 2011 and biennially afterwards. This longitudinal component is crucial for modeling educational transitions, since a cross-sectional analysis of the population risks comparing individuals from different birth cohorts with highly variable "exposure periods" to any given level of education.[7] In the analyses that follow, I use the case of parental income as to assess whether educational inequalities truly wane across the life course, but in Appendix H, I replicate my main analyses replacing parental income with parental assets and education, respectively.

I construct four sets of variables: educational transitions, a set of confounders for the effect of high school graduation on subsequent transitions, and a single set of intermediate confounders for the effect of BA completion on graduate school attendance, and parental income rank. As described previously, I assign high school completion (by age 22) to be my first educational transition, and thereafter analyze college attendance, college completion, and graduate school attendance. Specifically, these consist of three indicator variables denoting whether the respondent had attended a four-year college by age 22 (either via direct enrollment or via transferring from a two-year college), whether the respondent had received a BA degree by age 29, and whether the respondent had enrolled in a graduate level program by age 29, respectively. I assume that all individuals who make a given educational transition made all previous educational transitions. Thus, I code a respondent as a high school graduate (i.e., $A_1 = 1$) if the individual had either graduated high school by age 22, attended a four-year college by age 22, received a BA degree by age 29, or attended graduate school by age 29, and as a high school dropout otherwise (i.e., $A_1 = 0$), and adopt an analogous coding strategy for college attendance (A_2), and BA completion (A_3). Thus, by construction, I assume away cases in which an individual makes a particular educational transition without having made *all* previous transitions (e.g. if an individual enrolls in college without having completed high school), an approach which serves as a reasonable approximation to reality. In Appendices F and G, I replicate my main analyses using alternative age cutoffs for college attendance and completion, as well as defining college attendees as only those individuals who directly enrolled in a four-year college (i.e., did not transfer from a two-year college). The results are similar across these alternative educational definitions.

I include a large array of background characteristics (X) in estimating my population transitions model. Note that I now consider X to be a fuller set of covariates compared with the single, abil-

[7] As Lucas et al. (2011) note, lengthy risk periods allow education transitions coefficients to become contaminated by life-cycle factors.

ity confounder X used in the toy example previously. These include basic demographic variables (gender, race, ethnicity, age at 1997), socioeconomic background (parental education, parental income, parental assets, co-residence with both biological parents, presence of a paternal figure, rural residence, southern residence), ability (percentile score on the ASVAB test) and behavior, high school GPA, an index of substance use, an index of delinquency, whether the respondent had any children by age 18), and peer and school-level characteristics (college expectation among peers, and three dummy variables denoting whether the respondent ever had property stolen at school, was ever threatened at school, and was ever in a fight at school). Parental education is measured using mother's years of schooling; when mother's years of schooling is unavailable, it is measured using father's years of schooling. Parental income is measured as the average annual parental income from the earliest five survey waves (1997-2001); both parental income and parental assets are transformed to 2019 dollars using the PCE, and I transform parental income into a (ranked) uniform variable between 0 and 1.

My identification and estimation strategy enables the inclusion of a distinct set of observed intermediate confounders for each transition to adjust for selection processes that may confound the causal effects of each transition on subsequent transitions. Under the directed acyclic graph (DAG) shown in Figure 2, which shows a set of hypothesized causal relationships between each of the transition-varying confounders, the assumption of sequential ignorability would be satisfied. In practice, I assume that the set of Z_1 confounders is empty, implying that individuals' potential BA completion status $A_3(\overline{1}_2)$ is conditionally independent of college attendance $(A_2 = 1)$ given high school graduation ($A_1 = 1$), background characteristics (X) and parental income rank (R).

By contrast, I do not assume away the set of Z_2 confounders (which, henceforth, I label Z), and include two postsecondary characteristics in my models to adjust for confounders of the effect of BA completion on graduate school attendance, namely, field of study, and college GPA. Specifically, I use college self-reported major field of study, drawing on the NLSY survey instrument asking about major choice in each month in which the respondent was enrolled in a college, and using a dummy variable to denote whether whether a respondent majored in a STEM or non-STEM field by age 29. Finally, college GPA is measured using the respondent's cumulative GPA from the Post-Secondary Transcript Study.

I handle missing components of background characteristics (X) and intermediate variables (Z) using multivariate imputation with ten imputed data sets. To each imputed dataset, I apply the DML algorithm described previously using a composite machine-learning model known as a "super-learner" (Van der Laan and Rose, 2011) to estimate each regression function, and using five-fold cross-fitting (Chernozhukov et al., 2017). Appendix B gives further details about the particular models I fit under my assumed data-generating process. Finally, I obtain overall point estimates and standard errors by adjusting estimates obtained on each imputed dataset using Rubin's (2004) method.

Results

Descriptive Statistics

Figure 3 shows conditional means of respondent attributes X and Z by parental income group, progressively restricting the sample from (i) the full population (of high school goers), to (ii) high school (HS) completers, (iii) college attendees, and to (iv) BA completers. As discussed in the preceding section, I include these covariates in my models to satisfy the sequential ignorability assumption necessary to identify the population transitions model; by extension, these covariates are also those on which lower-income students are likely increasingly positively selected across transitions. As discussed previously, if low-income students are increasingly positively selected on these attributes across transitions (that is, if the association between parental income and these attributes declines across transitions), then this pattern of selectivity could at least in part for the near monotonic decline in background effects across transitions observed in descriptive transitions models. Thus, Figure 3 provides an assessment of how the association between parental income and these attributes changes across sequential transitions.

Two patterns are of particular note. First, for certain background attributes, such as parental assets, parental education, and high school type, this association appears unchanged across transitions, implying a lack of differential selectivity by parental income on these attributes. On the other hand, the association between parental income and other background attributes is highly changes dramatically across subsequent transitions. In particular, the high-income "advantage" on many school-related attributes diminishes significantly across transitions. For example, in the full population, the difference in average ASVAB percentile score between the most advantaged and disadvantaged children (in terms of parent income rank) is $69.00 - 24.8 = 44.13$, while among BA completers, this advantage shrinks to $77.05 - 54.97 = 22.08$. Similar convergence in the above attributes appears for school-related measures of violence and disruption, educational expectations among peers, whether the respondent had children by age 18, and on delinquency and substance abuse scores (see Appendix C). In fact, there is almost no difference in the probability that the respondent had any children by age 18, and in whether the child had ever been involved in a fight at school between the highest and lowest income children among BA completers.

Second, the degree of attenuation across transitions depends on the transition contrast examined. In particular, the degree of attenuation in the association between parental income and several characteristics is much greater at the between the high school completion and college attendance transitions than it is at across other pairs of transitions considered: low-income college-goers improve on their ability than their non-college going, low-income peers, far more than college graduates improve on their ability than their college dropout peers, and far more than high school completers improve on their ability compared with high school goers. In short, Figure 3 provides preliminary evidence in support of the selectivity hypothesis, that the association between family background and attributes predictive of educational success attenuates across transitions. Further, it gives suggestive evidence that such patterns of selectivity might be most pronounced among college attendees. In the following sections, I explore selection-adjusted estimates of income inequalities across educational transitions to characterize the extent to which educational attainment truly becomes more egalitarian across transitions.

[Insert Figure 3]

Does Educational Attainment Tend Towards Egalitarianism?

Figures 4 and 5 display my estimates of conditional and controlled inequalities at the high school, college attendance, BA completion, and graduate school attendance transitions. Specifically, Figure 4 shows the estimated intercepts and slopes for "conditional inequalities" at each educational transition (β_0^k and β_1^k) as well as for "controlled inequalities" (β_0^{*k} and β_1^{*k}), while Figure 5 displays the predicted probabilities of making a given transition by parental income rank implied by the descriptive and population transition models. In the figures, I distinguish intercept from slope estimates by shape (intercept estimates are circles while coefficients are triangles), and distinguish controlled from conditional inequalities by color (descriptive parameters are colored in blue, and causal parameters, in green). Exact point estimates and standard errors are shown in Appendix C.

Examining first the conditional inequalities at each educational transition, we can see that the estimated slopes (β_1^k) increase across the first two transitions (high school and college), and then decrease monotonically thereafter. Specifically, a one unit increase in parental income rank is associated with an increased probability of high school completion by 0.32. Substantively, this means that individuals from the lowest and highest parental income ranks have in excess of a 32 percentage point advantage in their probability of high school completion: as captured by the intercept term for high school completion, β_0^1, only 69% of individuals from the lowest parental income rank are expected to complete high school; by contrast, there is an essentially universal completion rate among those from the highest parental income rank.[8] Among individuals who graduate high school, the parental income rank advantage associated with college attendance (β_1^2) is even higher, at 0.59. The lower intercept term (β_0^2) of 0.17, compared with the first transition, reflects the low rate of college attendance from students from the lowest income backgrounds. This increase in the demonstrated advantage of social origin across the high school completion and college attendance transitions replicates for a later cohort a pattern found for birth cohorts from the 1950s and 1960s. In particular, Lucas (1996) and Lucas et al. (2011) found a similar increase in socio-economic advantage across the high school completion and college entry transitions for 1980 sophomores (using the High School and Beyond (HS&B) dataset) (see Ahituv and Tienda (2004) for female respondents in the NLSY79).[9]

Turning next to inequalities in BA completion among college-goers, Figure 4 reveals a dramatic decline in the advantage of social origin on making this transition compared with making the transition to college, having made all prior transitions: the slope β_1^3 declines from 0.59 to 0.29, and is also lower than the equivalent slope for high school completion. Finally, considering inequalities in graduate school attendance among BA completers, we see a further marked decline in the advantage of social origin; the slope declines to be negative, though is not statistically distinguishable from 0.

The right panel side of Figure 4 and right panel of Figure 5 show the corresponding results under the proposed population transitions model: i.e., one that adjusts for selection processes into each educational transition. To recall, these population parameters capture the degree of inequality we would observe at a given transition under the hypothetical experiment in which a random sample of high school goers progress sequentially through all prior transitions. Two patterns are of particular note.

First, controlled and conditional inequalities are remarkably similar across several transitions (high school completion to college attendance and BA completion to graduate school attendance). In particular, while controlled inequality in college attendance (β_1^{*2}) is estimated to be 3 percentage

[8]Note that my linear specifications of conditional and controlled inequalities should be seen as an approximation, as they do not respect the constraint that probabilities are bounded between 0 and 1.

[9]Lucas (1996) found that the increase in social background coefficients across the high school graduation to college attendance transitions holds only when college attendance is defined as four-year attendance only; broadening the definition of college entry to include 2 year colleges resulted in roughly constant social background effects across these two transitions.

points higher than conditional inequality in college attendance (β_1^2, 0.62 rather than 0.59), taking into account uncertainty in this point estimate (a 95% confidence interval for the conditional inequality estimate is $[0.56, 0.63]$), we cannot conclude that the selection-adjusted, controlled inequality estimate is statistically different from the non selection-adjusted conditional inequality estimate of 0.59. The slight decline in the predicted probability of college attendance for individuals from the lowest parental income rank under the population model (β_0^{*2}) compared with that same probability in the descriptive model β_0^2 (0.17 versus 0.12) is, however, indicative that all high school completers are somewhat selected on attributes, compared with their non-high school completer peers, but this selection does not appear to be correlated with social origin. Second conditional and controlled inequalities in graduate school attendance are highly similar, and indicate a very slight low-income advantage in terms of pursuing this postsecondary transition, although the point estimate is not distinguishable from zero.

Second, and most importantly, the association between parental income and probability of BA completion strengthens significantly in the population transitions model. The population model intercept β_0^{*3} (representing the predicted probability of BA completion among low-income children with parental income rank of 0) changes dramatically compared with that under the descriptive model (β_0^3), from approximately 0.51 to 0.35. There is also a corresponding increase in the estimated slope (β_1^{*3} vs β_1^3), from 0.29 to 0.41, signifying a much greater degree of inequality in BA completion in the population than descriptive model - that is, once we adjust for selection. In particular, the intercept and slope in the population model makes the positive selection of low-income students into college especially evident: while the predicted probability of BA attainment among individuals from the highest parental income rank decreases by only 4 percentage points for the highest-income students ($0.80 - 0.76$), the predicted probability of BA attainment among individuals from the lowest income rank (as captured by the intercept terms) falls over four times as much across the two models.

Taken together, the results suggest that failing to adjust for selection into different educational levels risks mischaracterizing educational institutions' equalizing potential. While there is no obvious monotonic decline across all transitions, concordant with prior work on birth cohorts just prior to that of the NLSY97 (Lucas, 1996; Lucas et al., 2011), the descriptive transitions model underestimates the degree of inequality in degree completion generated at the college level by over 12 percentage points: the 41 percentage point inequality in completion between those from the lowest and highest income groups generated by colleges exceeds (point-wise) inequalities generated by high school in terms of high school completion rates, providing further evidence that inequalities in fact increase, rather than decrease, over the educational life course. In Appendix I, I test the sensitivity of my main set of results to unmeasured confounding by extending a general technique proposed by VanderWeele (2010) and VanderWeele and Arah (2011) in the context of mediation analysis. Under the requisite assumptions, these population estimates are highly robust to patterns of unobserved confounding.

[Insert Figure 4]

[Insert Figure 5]

Why are BA Completion Inequalities So Large?

The sustained degree of inequality across the high-school and BA completion transitions revealed by the population model tallies with a large research literature demonstrating that a range of class-correlated, pre-college resources consequential for on-campus trajectories, such as financial, cultural and academic factors.

In particular, beyond the direct advantages conferred by financial resources in terms of bolstering college performance and increasing probability of completion (Armstrong and Hamilton, 2013; DeSimone, 2008; Stuber, 2009, 2011; Jack, 2019; Bound et al., 2012), cultural advantages cut across economic disparities: those students who are unfamiliar with dominant middle class culture, for example on account of their attendance at public, under-resourced high schools (Jack, 2015, 2019), often display lower levels of academic performance and higher rates of dropping out (e.g. Carter, 2005; Collier and Morgan, 2008; Charles et al., 2009; Stephens et al., 2012; Calarco, 2014; Manski et al., 1983). Further still, it is well-known that pre-college measures of ability are highly predictive of BA attainment, even net of financial and cultural resources. In particular, a one standard deviation increase in high school GPA has been associated to be predictive of an increase in graduation rates of up to 20 percentage, although "only a small portion of the gap in college graduation rates between high- and low-income students can be explained by their differences in academic preparation" (Bowen and Bok, 1998) - a fact which attests to the high ability level of low-income college-goers.

What is the relative importance of these resources in driving BA completion inequalities? To address this question, I estimate a set of "controlled inequalities" which additionally control for a set of pre-college resources. Controlling for pre-college resources in this way provides an estimate of the extent to which controlled inequalities can be statistically explained by class differences in these resources.[10] I fit three separate regression models where I additively control for parental assets,

[10]Specifically, I estimate the quantity $\mathbb{P}[A_3(\overline{1}_k) = 1|R, W] = \tilde{\beta}_0^{*2} + \tilde{\beta}_1^{*2}R + \boldsymbol{W}\vec{\gamma}$,where $\boldsymbol{W}$ is the

individual ability (percentile score on the ASVAB test), and high school type (whether an individual ever attended a private or a Catholic school), to capture in a broad way the additional contribution of parental financial resources, pre-college ability, and cultural resources, respectively.

Table 1 presents the results from these regressions. We see that parental assets and individual ability statistically explain almost half of controlled inequality in BA completion: the controlled inequality estimate (β_0^{*2}) as presented in the prior section of 0.41 falls to 0.21 after controlling for parental assets and for this measure of student ability. Indeed, once we account for these two variables, type of school attended explains very little of the parental income gap in completion probability. Despite the fact that those who attended a Catholic school tend to have a higher probability of BA completion, as seen by the positive coefficient on this variable, attendance at such schools is not strongly correlated with family background, leading to a low explanatory power of school type for the parental income gap in college effects on completion. In short, the results underline the importance of pre-college resources in explaining why college attendance overly benefits individuals from high-income backgrounds, echoing results from recent work that also highlights the role of early childhood characteristics and experiences in explaining racial disparities in college completion rates (Zhou and Pan, 2021).

[Insert Table 1]

How Would College Enrollment Expansion Affect Completion Inequalities? A Counterfactual Analysis

Inequalities in BA degree attainment in the population overall are the product of inequalities in bachelor's degree completion among college-goers and of inequalities in individuals' college attendance rates.[11] Given this relationship, the low association between social background and BA completion implied by descriptive transitions models might lead some to believe that a mechanical, non-preferential expansion of the post-secondary education system would lead to a reduction in BA attainment inequalities overall. Certainly, this prediction would accord with that of some scholars, who took the lower degree of educational inequality observed among college-goers to mean that higher-educational expansion would be sufficient for equalizing educational opportunities (Stolzen-

design matrix of pre-college resources, and where $\tilde{\beta}_0^{*2}$ is the pre-college resource-adjusted analog of the controlled inequality parameter β_0^{*2} considered previously.

[11] This is because the probability of BA completion A_3 given parental income rank R, $\mathbb{P}[A_3 = 1|R = r]$, is equal to $\mathbb{P}[A_3 = 1, A_2 = 1|R = r] = \mathbb{P}[A_3 = 1|A_2 = 1, R = r]\mathbb{P}[A_2 = 1|R = r]$.

berg, 1994, p. 1068, see also Treiman, 1970; Hout et al., 1993; Mare, 1979; Torche, 2011; Hout, 2007). Yet, in light of my finding that controlled inequalities in BA completion, which capture inequalities in BA completion were a random sample of individuals to progress through to college, parallel the large, extant inequalities in high school completion, it is questionable whether such a mechanical expansion would in fact have positive effects on BA completion inequalities overall.

To address the question of how educational expansion could affect BA inequalities by parental income, I now conduct a counterfactual analysis that examines overall inequalities in BA completion in a set of hypothetical worlds where the proportion of college-goers is incrementally increased from its observed quantity of 41% up to 81%. The quantity 81% reflects a world in which there is extreme college expansion but where high school completion processes are unchanged, that is, one in which in which $95\% = \mathbb{P}(A_2 = 1|A_1 = 1) = \frac{\mathbb{P}(A_2=1)}{\mathbb{P}(A_1=1)} = \frac{81\%}{85\%}$ of high school completers are 4-year college-goers. Following Ciocca Eller and DiPrete (2018), I refer to overall BA inequalities by parental income in the population overall as "BA attainment inequalities."

As previously, let A_1 and A_2 denote high school completion and 4-year college attendance, respectively, and X denote all pre-high school completion covariates, and parent income rank be denoted by R. Let $\pi(X, R) \triangleq \mathbb{P}(A_2 = 1|A_1 = 1, X, R)$ denote the probability of college attendance given pre-college covariates, parental income rank, and college attendance (i.e., the "propensity score"). In each hypothetical world, then, I envisage a college expansion intervention that indiscriminately scales each student's odds of college attendance, $\frac{\pi(X,R)}{1-\pi(X,R)}$, by some constant δ, but that leaves high school completion processes untouched. I then find a series of constants δ that correspond to a series of hypothetical worlds in which the proportion of college-goers is increased in percentage point intervals from 41% to 81%. In each world, the constant can be used to estimate individuals' counterfactual propensity score under the given expansion.[12] Finally, and following Lundberg (2022), I estimate the counterfactual association between social origin and BA completion (BA attainment inequality) by means of a weighted regression of BA completion on parental income in the full population, where the weights are given by the ratio of the counterfactual to the factual estimated propensity scores if an individual completed high school and 0 otherwise, i.e.,

[12]Since such an intervention corresponds to a counterfactual propensity score of $\pi(X, R) \triangleq \mathbb{P}^*(A_2 = 1|A_1 = 1, X, R) = \frac{\delta\pi(X,R)}{\delta\pi(X,R)+1-\pi(X,R)}$ (Kennedy, 2019), for each hypothetical world, I find the constant that produces the desired proportion of college-goers c in the population overall by numerically solving the equation $\mathbb{E}_n[\hat{\pi}^*(X, R) \cdot \hat{\mathbb{P}}(A_1 = 1)] = c$, where $\mathbb{E}_n$ denotes empirical expectation and $\hat{}$ denotes an estimated factual or counterfactual propensity score.

$$\text{weight}_i = \begin{cases} \frac{\hat{\pi}^*(X,R)}{\pi(X,R)}, & \text{if } A_1 = 1 \\ 0, & \text{else.} \end{cases} \tag{5}$$

Estimating BA attainment inequality using the weights specified in Equation 5 identifies the hypothetical causal effect of educational expansion on BA inequality because it simulates hypothetical inequalities using "controlled inequalities" at the BA level. In other words, each simulated value of BA attainment inequality at different rates of college attendance employs a causal quantity. To demonstrate how a descriptive transitions model, which is based on "conditional inequalities" at different educational levels, could wrongly inform us the effect of a mechanical post-secondary expansion on BA attainment inequality, I replicate this entire procedure but using a misspecified propensity score (i.e., one that does not condition on the full set of pre-high school completion covariates, and only on parental income rank given attainment of the prior education transition). In other words, I repeat the entire prior procedure, replacing $\pi(X, R)$ with $\pi(R) \triangleq \mathbb{P}(A_2 = 1|A_1 = 1, R)$. This procedure deliberately fails to identify the causal effect of college at each rate of post-secondary enrollment, and is designed to mimic conclusions that could be reached about education's equalizing potential under a descriptive transitions model.

Figure 6 plots estimates of the BA attainment and completion inequalities in each of the hypothetical worlds I consider, that is, under varying degrees of college enrollment using conditional inequalities (left panel) and controlled inequalities (right panel). Beginning with the left panel, which uses conditional inequalities to assess the effect of expansion on BA attainment inequality, we see that a mechanical expansion of higher-education seats does in fact appear to dramatically lower BA inequality. Higher education expansion from its status quo level to a world in which 81% of individuals attend college monotonically decreases attainment inequality by almost 20 percentage points. Yet, turning to the right panel, which displays analogous estimates of counterfactual attainment inequalities but obtained under a model that correctly adjusts for class-based selection, shows that this conclusion is misleading. The left-most point estimates, corresponding to a hypothetical world in which approximately 41% of the high school-going population enroll in a 4-year college, capture the origin-BA completion association under the status quo, while the far-most right points estimate this association under extreme college expansion, when 81% of the full population are 4-year college-goers. We see that as the proportion of college-goers increases, the association between parental income and BA completion increases. This increase is non-monotonic and appears to subside very marginally only when approximately 70% of the population attend college. At any rate, these results unequivocally challenge the idea that mechanical higher-education expansion would in any way *decrease* the association between family background and BA completion, and demonstrate the importance of the population transitions model for making more accurate claims about the equalizing effects of higher-education expansion on inequalities.

[Insert Figure 6]

Conclusion

In a series of landmark papers, Mare (1979; 1980; 1981) revolutionized the conceptual and methodological standard for studying inequality within educational systems, pioneering a set of findings that indicated increasing educational egalitarianism across the life course, and the promise of higher-educational institutions in particular in freeing individuals from the constraints of their social origins. Using a novel, nonparametric approach that builds on the latest thinking in potential outcomes as well as in recent development in machine-learning in causal inference settings in sociology (e.g. Ahearn et al., 2022; Brand et al., 2021), this paper has addressed whether the stylized finding of waning inequality is due to selection bias or a true reduction in inequality across transitions when we consider the same set of hypothetical people (i.e. all high school goers in the population) at all transition points. While drawing on the important insights by scholars over the years on methods to address the biases involved in the classical educational transitions models, my proposed approach is the first to examine inequalities at the college level and beyond. In addition to this substantive focus, my approach is distinguished from prior approaches to selection-adjustment for early educational transitions by explicitly targeting a set of population-based quantity that directly speak to a set of important theoretical and policy-related issues. Most fundamentally, this reformulation provides a novel way to conceptualize and measure educational inequalities in a model-free manner, one that enables proper adjudication as to whether the finding of waning inequality is due to selection bias or a true reduction in inequality across transitions.

I find that descriptive transitions models, which fail to adjust for selection into different educational levels, risk mischaracterizing how educational inequality unfolds across the life course. Drawing on data from the NLSY97 cohort, the population transitions model exposes a far greater degree of inequality experienced at the college level that is disguised by descriptive transitions models. The degree of inequity in BA completion between individuals from the lowest and highest parental-income ranks is dramatically under-estimated when we fail to adjust for lower-income students' selectivity into college; disparities in BA completion in fact parallel extant inequalities in high school completion. This finding that college attendance is no panacea for educational inequality illuminates an additional, yet under-stated, inequality-producing mechanism in the "college as great equalizer" debate: individuals from high-income backgrounds not only benefit the most in terms of adult income attainment from a BA degree (Zhou, 2019; Fiel, 2020), but college attendance alone guarantees such individuals a much higher chance of degree completion compared with their lower-income peers.

Because they distill social and causal processes, such as the increasing social distance between parents and children across the life course and the impact of higher educational institutions on inequalities, from the positive selection of low-income youth, I have argued that controlled inequalities are of greater theoretical and policy importance than the conditional inequalities estimated under descriptive transitions models. In particular, since controlled inequalities better capture policy implications about the causal effects of educational expansion on inequality, they facilitate evaluation of whether higher educational expansion is itself sufficient for equalizing educational opportunities (Stolzenberg, 1994, p. 1068, see also Treiman, 1970; Hout et al., 1993; Mare, 1979; Torche, 2011). Evaluating counterfactual BA attainment inequality under a series of hypothetical interventions to college attendance, I find, contrary to policy predictions derived from descriptive transitions models, that mechanical expansion of the post-secondary system would not, by itself, guarantee a reduction in post-secondary completion inequalities, and could even cause BA completion inequalities to increase.

This finding suggests that mechanical expansion of college seats would be misplaced as a substitute for class-based interventions to ensure degree completion. In particular, my set of analyses of "controlled inequalities" in BA completion which additionally adjust for a set of pre-college resources suggest that US colleges benefit high-income students on campus (in terms of BA completion) in large part because they currently serve individuals with high levels of assets as well as high academic abilities (Table 1). Policy interventions at the college-level that focus on financial support for low-resourced students, as well as academic support for individuals throughout college, would bolster lower-income students' BA completion rates.

At the same time, my results show that policy interventions at only the college-level would be insufficient for equalizing BA attainment rates marginally. Both the descriptive and population transitions models reveal a worrying degree of inequality in students' college-going behaviors, a result which suggests that examining class-base inequalities in students' decisions to attend college is also of primary importance if we are to combat cumulative inequalities over the life course. Future work would do well to examine this transition in greater depth. Such an analysis, for example, could consider both components of horizontal stratification at the high-school level, as well as the potentially distinct contributions of ability, motivations and educational aspirations to educational inequalities at this juncture. Additionally, assessing the role of 2-year college attendance and associates degree attainment in driving 4-year attendance inequalities - both in terms of differentially facilitating or impeding 4-year college attendance for individuals from different class backgrounds - is an important avenue for future work. Together, such analyses promise to shed light on the social processes and mechanisms that hinder access to college for the vast majority of lower-income youth and, more broadly, which constrain the education system from being the "great equalizer" many so hope it to be.

While the population transitions model arguably captures components of inequality of greater theoretical and policy interest than descriptive models, revealing important facts about the true degree and location of inequality, it inherits several limitations from descriptive transitions models that elide complexities in the US higher educational landscape. In particular, its treatment of educational transitions, such as college attendance, as binary neglects the importance of horizontal stratification in colleges that may lead to distinct patterns of inequality. Further, it does not address the unique educational experiences of two-to-four year college transfer students and educational inequalities emergent among these students' trajectories. In particular, the fact that nearly half of US BA graduates have at some point attended a two-year college behoves us to examine in what ways experience with the two-year education sector affects attainment inequalities. A natural extension of the present study would therefore entail a fuller treatment of different educational trajectories: an examination of the resultant inequalities under a population transitions model of different trajectories, and an evaluation of specific institutional features of the higher-education landscape that might hamper equalization of BA completion rates.

Such an exercise would also be important to direct at the graduate school level. In my main analyses, I find that BA completion marks a dramatic weakening of the association between parental income and post-college educational attainment. Yet, preliminary supplemental analyses (Appendix E) suggest that examining BA effects on graduate school attendance separately by program reveals distinct patterns of educational stratification depending on the graduate program considered. This surprising result seems to suggest that that the characteristics on which lower-income students select into BA completion are associated only with masters degree attainment, and not with any other type of graduate program (MD, JD, and PhD). A distinct set of career-oriented aspirations and attitudes may well be highly predictive of professional and managerial advanced degrees (e.g. Stolzenberg, 1994), but it would appear that such characteristics are somewhat orthogonal to the (more education-oriented) attitudes that are associated with masters degree attainment and on which lower-income students select into BA completion.[13] A more thorough examination of inequalities at the graduate level is needed, especially as graduate attendance (and completion) become more commonplace and the graduate school becomes a larger locus of (maximally-maintained) educational inequality (Lucas, 2001).

Finally, the conceptual approach introduced in this paper could be applied to study a broader set of inequalities and policy-relevant issues subject to analogous patterns of differential selectivity by majority-minority groups. For instance, the predictive efficacy of loans in equalizing educational decision-making between racial groups may be overestimated if evaluated exclusively on the subset of Black and White college attendees, rather than assessed via a population model (Witteveen, 2023). Beyond higher-education, while estimates of racial discrimination in police arrest typically assess racial inequalities through restricting to samples of individuals stopped by police (Neil and Winship, 2019; Knox and Mummolo, 2020; Knox et al., 2020), gauges of the true degree of discrimination in arrests and charges may be better informed by assessing the racial disparity in arrest we would observe if all Black and white individuals spotted by the police were stopped, rather than comparing the relatively over-selected group of Black individuals who are stopped with an under-selected group of Whites who are stopped. Given the prevalence of sample selection issues in studies of inequalities and discrimination in the education setting and beyond, the population inequalities approach introduced here offers an important conceptual tool for future researchers.

AO: does this final paragraph add much? Do the examples make sense?

[13] Though note that this result is especially curious when we consider Mullen et al.'s (2003) finding that characteristics such as educational expectations, college GPA, college selectivity and test scores - that is, those attributes on which we might expect lower-income college graduates to be especially positively selected - are more strongly associated with professional degrees and PhD program enrollment compared with enrollment in a masters degree.

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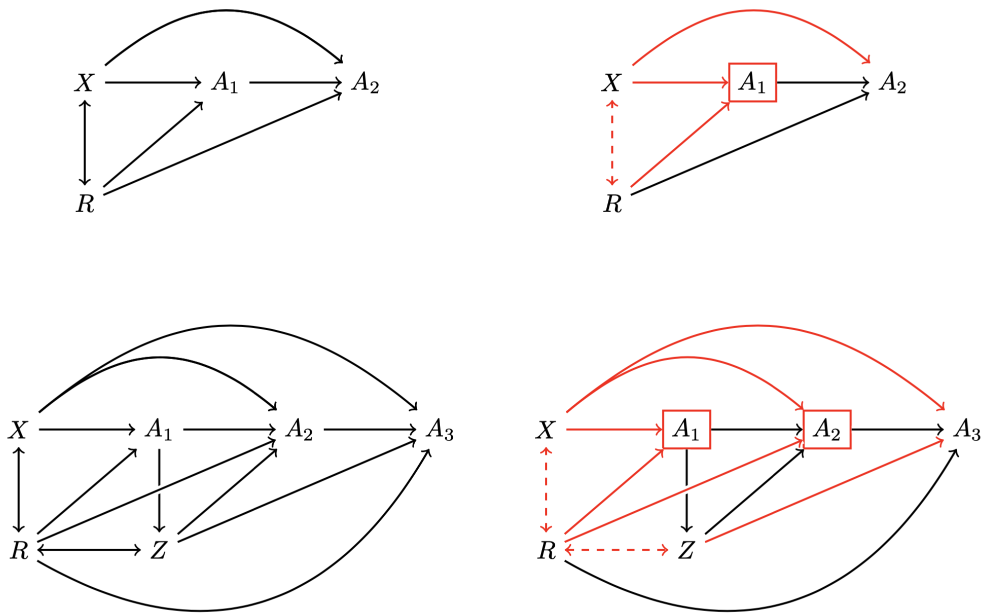

Figure 1: DAGs showing the hypothesized causal relationships between parental income rank R, pre-high school graduation confounders X, high school graduation A_1 and college attendance A_2 (top panel), and between these variables in addition to college completion A_3 and pre-college attendance confounders (bottom panel). In both panels I use a bidirectional arrow ($\longleftrightarrow$) between two variables to denote agnosticism about the causal relationship between these two variables. In each panel, the rightmost figure shows graphically the endogenous selection bias induced by examining the association between R and A_1 (top panel) and the association between R and A_2 (bottom panel) via conditioning on prior educational transitions (collider variables).

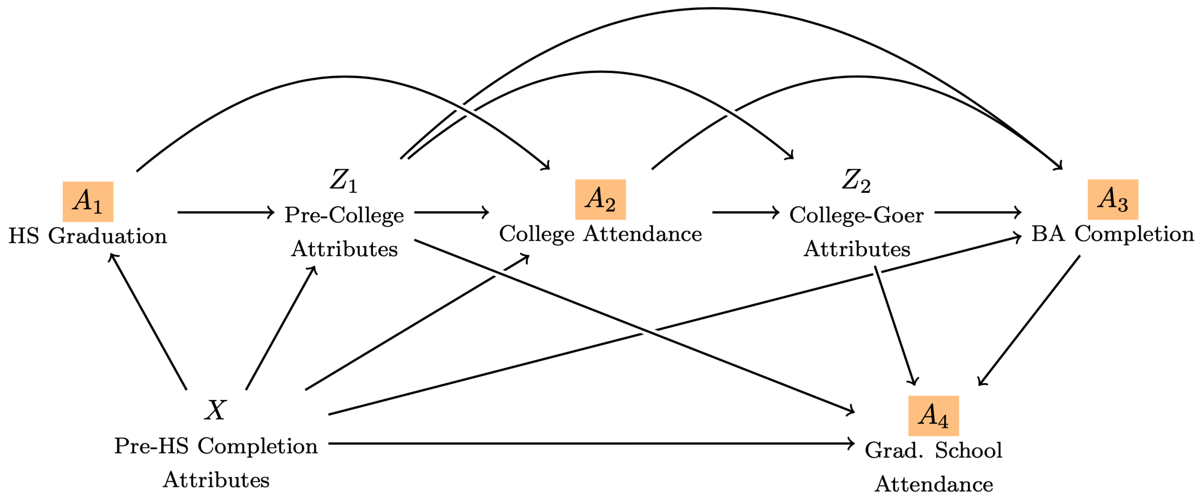

Figure 2: A DAG showing the hypothesized causal relationships between high school graduation A_1, college attendance A_2, BA completion A_3 and graduate school attendance A_4.

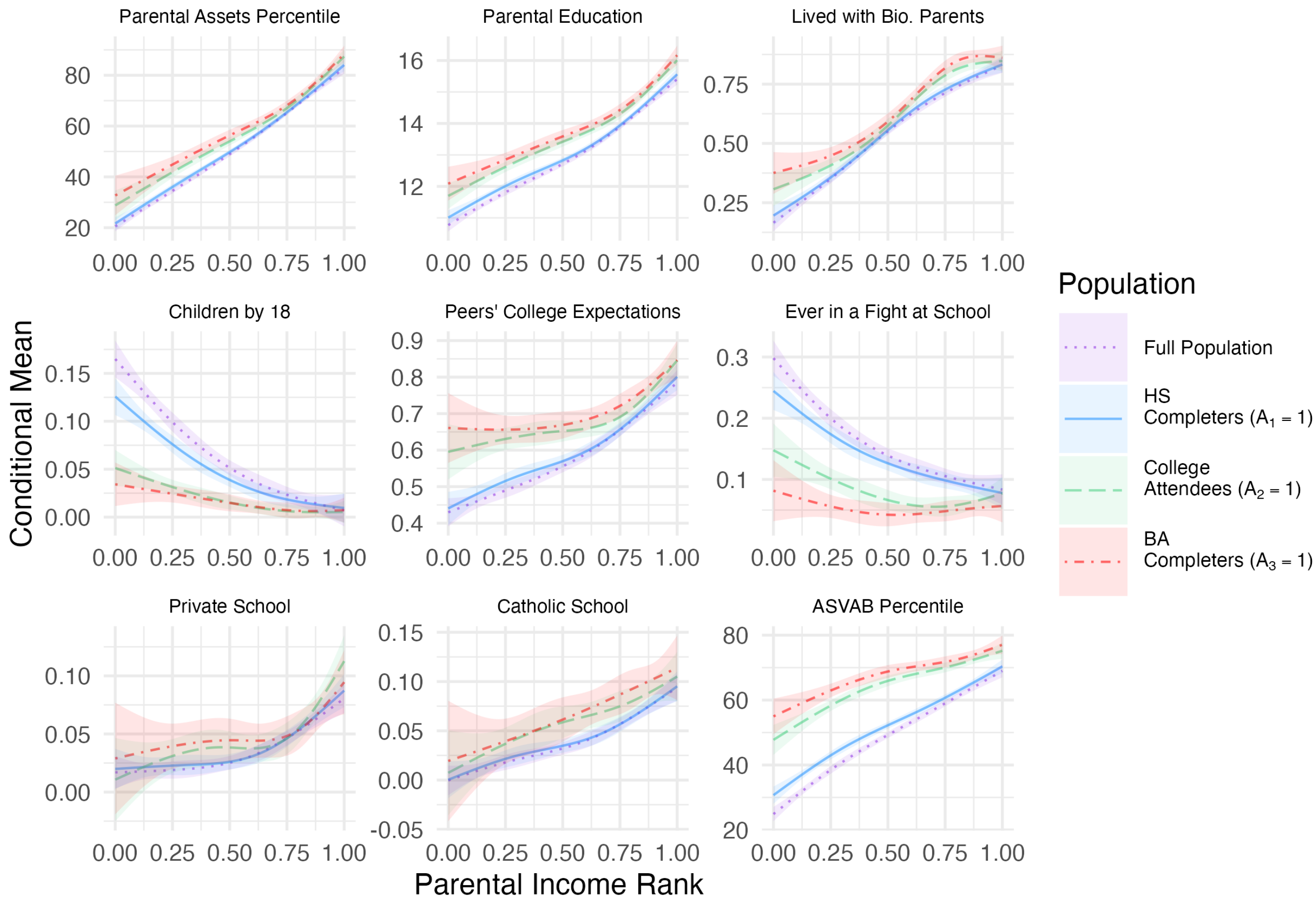

Figure 3: Conditional means of selected covariates, fitted as natural splines of parental income rank with 3 degrees of freedom, and adjusted using NLSY97 sampling weights.

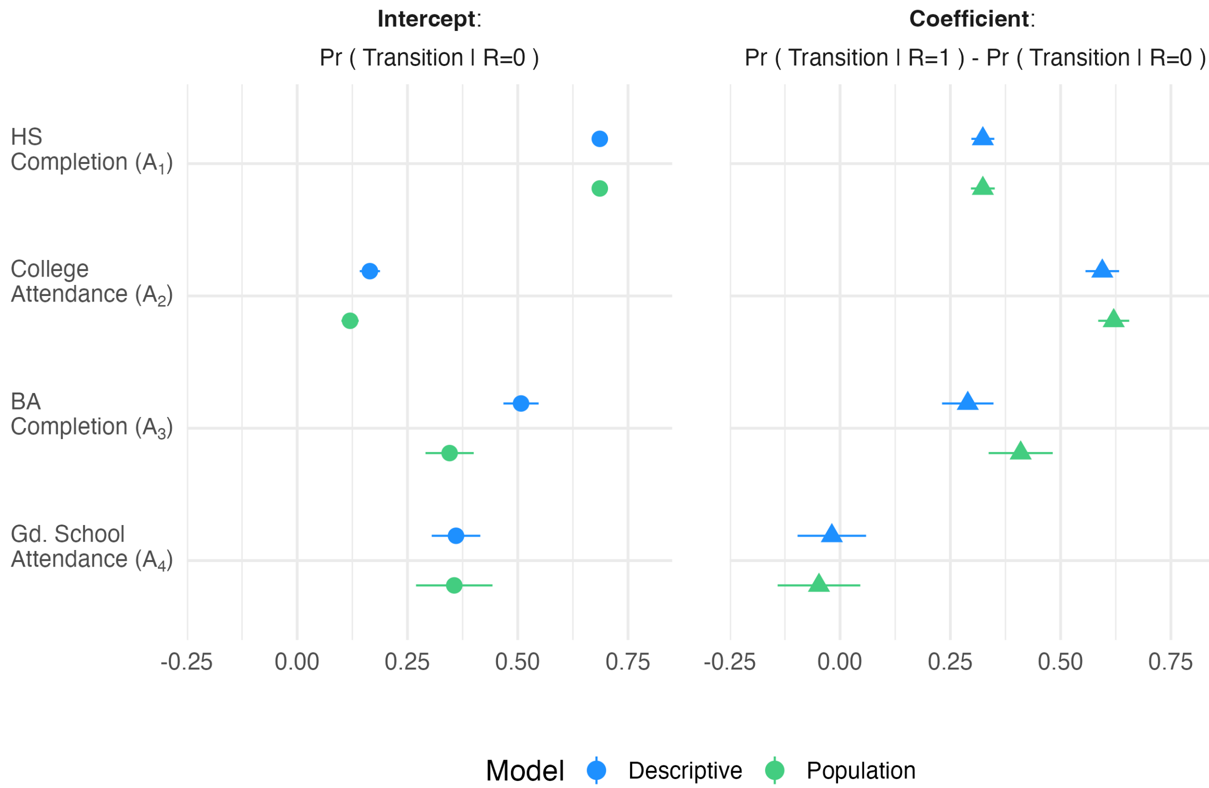

Figure 4: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models.

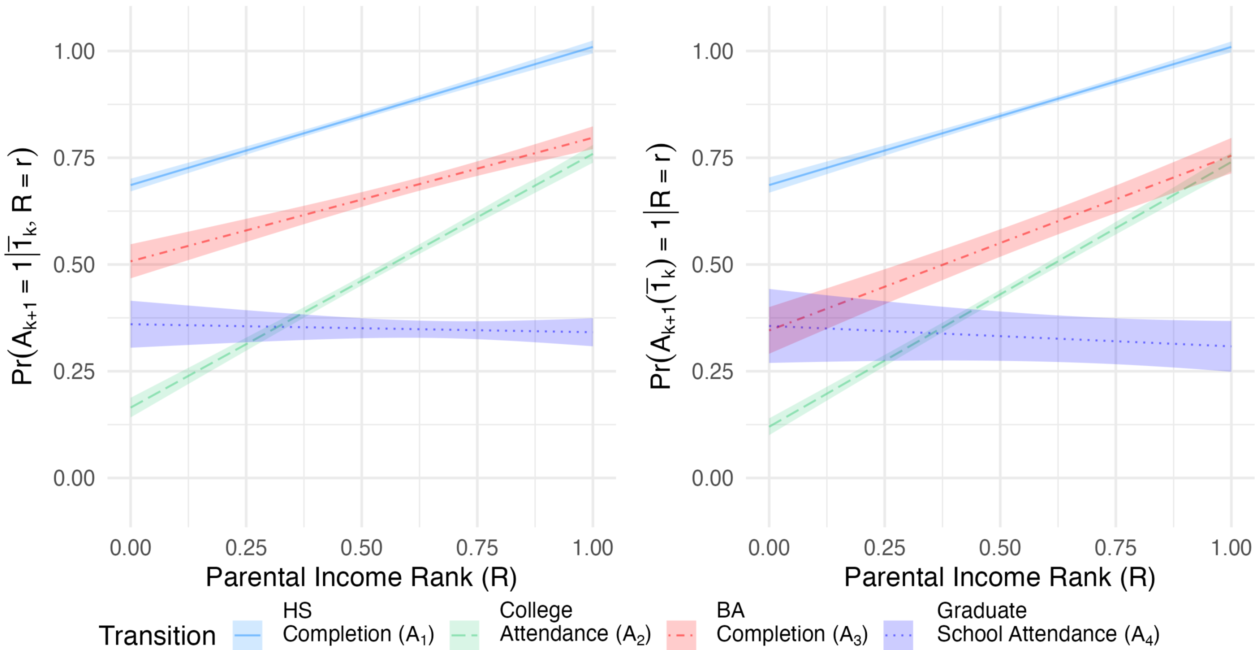

Figure 5: Predicted probabilities of making an educational transition under descriptive and population transition models. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

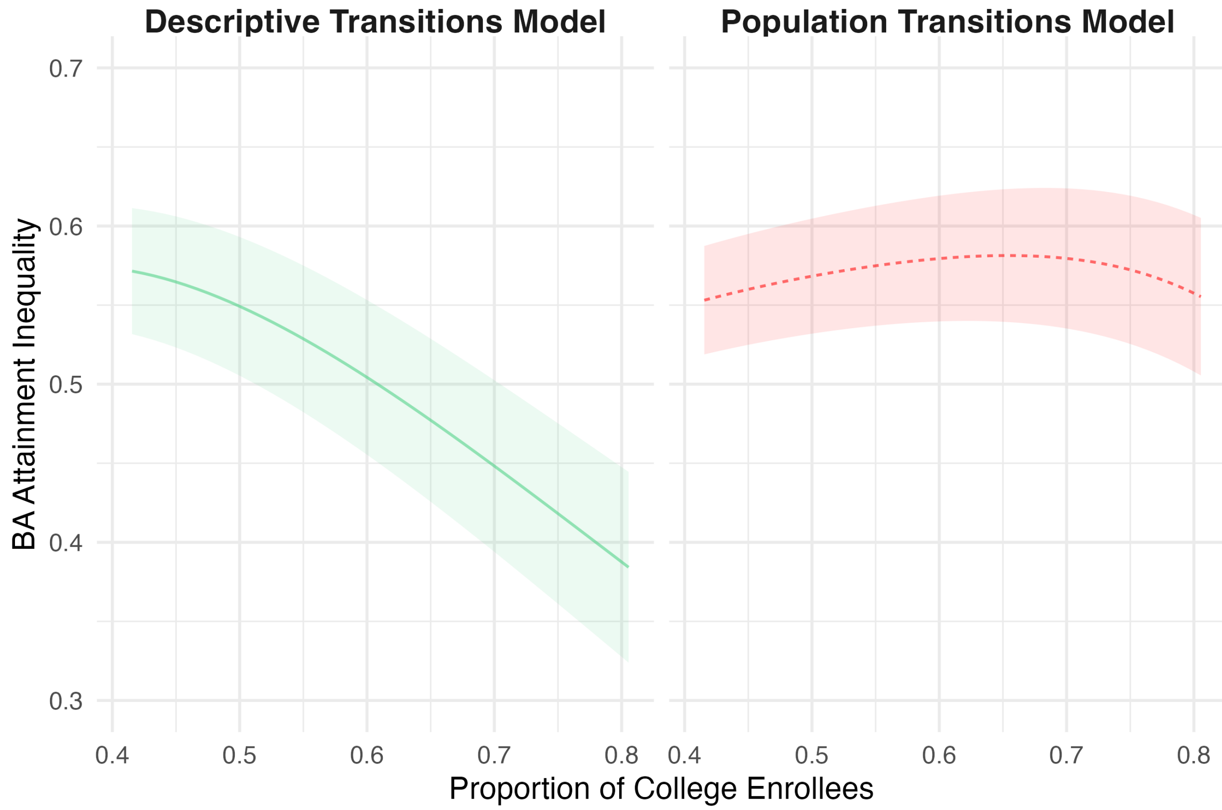

Figure 6: Effects of hypothetical college expansion on the association between social origin and BA completion, as estimated under the descriptive transitions model (left panel) and population transitions model (right panel).

Table 1: Estimated and slope for BA completion under population transitions model, with and without controlling for parental assets, student ability, and school type.

		Controlling for:		
Variable	(Unadjusted)	Assets	+ Ability	+ School Type
Parental Income	0.4094 (0.0355)	0.2998 (0.0355)	0.2140 (0.0611)	0.2089 (0.0615)
Parental Assets		0.1772 (0.0574)	0.1253 (0.0517)	0.1231 (0.0516)
Ability Score			0.2762 (0.0740)	0.2729 (0.0735)
Catholic School				0.0885 (0.0327)
Private School				0.0034 (0.0381)

Note: Numbers in parentheses are standard errors. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

A Nonparametric Representation of the Population Transitions Model and its Identification

In Figure 2 in the main text, I show the assumed data-generating process that satisfies sequential ignorability in the proposed population transitions model. Notably, a number of edges are structurally excluded from the DAG because of the sequential nature by which individuals make educational transitions (e.g., individuals can only progress to college if they have graduated high school; similarly, individuals can only complete a BA degree if they have progressed through the high school graduation and college attendance transitions).

This dependence structure can be formally described via the set of structural equations shown below. Here, $U = \{U_X, U_{A_1}, U_{Z_1}, U_{A_2}, U_{Z_2}, U_{A_3}, U_{A_4}\}$ is a vector of all unmeasured exogenous factors affecting the system; together with the set of functions f that are assumed to be non-stochastic but unknown, these fully define the joint distribution of potential outcomes in the system:

$$
\begin{aligned}
X &= f_X(U_X) \\
A_1 &= f_{A_1}(X, U_{A_1}) \\
Z_1 &= f_{Z_1}(X, U_{Z_1}) \\
A_2 &= f_{A_2}(X, A_1, Z_1, U_{A_2}) \\
Z_2 &= f_{Z_2}(X, Z_1, A_2, U_{Z_2}) \\
A_3 &= f_{A_3}(X, Z_1, A_2, Z_2, U_{A_3}) \\
A_4 &= f_{A_3}(X, Z_1, Z_2, A_3, U_{A_3}).
\end{aligned}
$$

Then, the population transitions model concerns the following counterfactuals:

$$
\begin{aligned}
A_2 &= f_{A_2}(X, 1, Z_1, U_{A_2}) \\
A_3 &= f_{A_3}(X, Z_1, 1, Z_2, U_{A_3}) \\
A_4 &= f_{A_3}(X, Z_1, Z_2, 1, U_{A_3}),
\end{aligned}
$$

where $R \subset X$. Here, the lack of all but proximal transitions from the structural equations results from the assumption of monotonicity of educational transitions, which states that for all $k \in \{0, \dots, K\}$, $A_k = 0 \implies A_{k+1} = 0$.

This series of structural equations satisfies the assumption of sequential ignorability ($A_{k+1}(a_k) \perp\!\!\!\perp A_k | \overline{Z}_k, A_{k-1}, X, \forall a, k \in [K]$), which, intuitively, states that the effect of each transition A_k on the next A_{k+1} is independent of the potential outcomes of A_{k+1}, conditional on all antecedent variables or there is no unobserved confounding for the effect of A_k on A_{k+1}).

To ease the following exposition, let $\tau^k(r) = \mathbb{P}[A_{k+1}(\overline{1}_k) = 1 | R = r]$, that is, the kth "transition effect" in the population transitions model among individuals from parental income group $R = r$. Under sequential ignorability, $\tau(r)$ is identified by the g-formula (Robins, 1986):

$$\tau^k(r) = \int_x \int_{\overline{z}_k} \mathbb{P}[A_{k+1} = 1 | x, \overline{z}_k, \overline{a}_k, r] \Big[\prod_{j=1}^{k} dP(z_j | x, \overline{z}_{j-1}, \overline{a}_{j-1}, r) \Big] dP(x|r), \tag{6}$$

where, again, $A_0 = \emptyset$, such that $\mathbb{P}[A_1 = 1 | R = r]$ is simply identified as $\mathbb{P}[A_1 = 1 | R = r]$. Note also that, for $k = 1$, the identification formula collapses to that for the conditional average treatment effect (CATE), such that $\mathbb{P}[A_2 = 1(A_1 = 1) | R = r] = \int \mathbb{P}[A_2 = 1 | A_1 = 1, x, R = r] dP(x | R = r)$, as shown above. My proposed approach is reminiscent of that suggested by Lucas and collaborators (Lucas, 2001; Lucas et al., 2011), who argued that the selection problem could be addressed by means of time / transition-varying controls from longitudinal data, such as GPAs from the most recent level of education.[14]

To appreciate the differences between the population and descriptive transitions models differ under this identifying formula we can compare (i) the conditional inequality $\mathbb{P}[A_2 = 1 | A_1 = 1, R = 1] - \mathbb{P}[A_2 = 1 | A_1 = 1, R = 0]$ considered previously (which captures inequality between ranks 0 and 1 in college attendance among high school completers) with (ii) the controlled inequality $\mathbb{P}[A_2(1) = 1 | R = 1] - \mathbb{P}[A_2(1) = 1 | R = 0]$. Assuming there exists a discrete variable X like ability which is positively associated with college attendance, $\mathbb{P}[A_2 = 1 | A_1 = 1, R = 1] - \mathbb{P}[A_2 = 1 | A_1 = 1, R = 0]$ can be written as

[14]Though no previous study evaluating the Mare Model has, to the best of my knowledge, employed transition-varying confounders. The "neo-classical" approach espoused by (Lucas et al., 2011) mentions the promise of time-varying confounders in education transitions research, though in practice, only controls for GPA in the 9th and 12th grades of high school appear in the modeling procedure, and both serve as confounders for a single transition: the effect of high school on college.

$$P[A_2 = 1|A_1 = 1, R = 1] - \mathbb{P}[A_2 = 1|A_1 = 1, R = 0] = \sum_x \mathbb{P}[A_2 = 1|A_1 = 1, R = 1, X = x] \cdot \mathbb{P}[X = x|A_1 = 1, R = 1] \\ - \sum_x \mathbb{P}[A_2 = 1|A_1 = 1, R = 0, X = x] \cdot \mathbb{P}[X = x|A_1 = 0, R = 0]. \quad (7)$$

By contrast, under sequential ignorability, $\mathbb{P}[A_2(1) = 1|R = 1] - \mathbb{P}[A_2(1) = 1|R = 0]$ can be written as

$$P[A_2(1) = 1|R = 1] - \mathbb{P}[A_2(1) = 1|R = 0] = \sum_x \mathbb{P}[A_2 = 1|A_1 = 1, R = 1, X = x] \cdot \mathbb{P}[X = x|R = 1] \\ - \sum_x \mathbb{P}[A_2 = 1|A_1 = 1, R = 0, X = x] \cdot \mathbb{P}[X = x|R = 0]. \quad (8)$$

The difference between these two expressions lies in the probability function for X: whereas the controlled inequality identification formula uses the probability function $\mathbb{P}[X = x|R = r]$, conditional inequality features $\mathbb{P}[X = x|A_1 = 1, R = r]$. This contrast emphasizes that controlled inequality speaks to a population quantity (in that it employs the distribution of X observed among the *whole population of individuals* with parental income rank $R = r$), while conditional inequality captures inequality among individuals observed to proceed through the prior transition (since it employs the distribution of X observed among the *high school completer population* with parental income rank $R = r$). This argument extends naturally to the case of higher-level transitions, where the density function in the g-formula ($\prod_{j=1}^{k} P(z_j|x, \bar{z}_{j-1}, \bar{a}_{j-1}, r)$) used to identify controlled inequalities is replaced by the density function $\prod_{j=1}^{k} P(z_j|x, \bar{z}_{j-1}, \bar{a}_k, r)$.

To recall from the main text, I characterize inequality in the counterfactual probability of success at a given educational transition by assuming a linear working model for how this probability varies by parental income rank R. Let $f(r, \beta^{*k}) = \beta_0^{*k} + \beta_1^{*k} r$ be the assumed working model. Then, $\tilde{\beta}^{*k} = (\tilde{\beta}_0^{*k}, \tilde{\beta}_1^{*k})$ is defined as the projection of $\tau^k(r)$ onto working model $f(r, \beta^{*k})$, namely,

$$\tilde{\beta}^{*k} = \underset{\beta^{*k} \in \mathbb{R}^2}{\operatorname{argmin}} \mathbb{E}[\big(\tau^k(R) - f(R, \beta^{*k})\big)^2], \quad (9)$$

such that $\tilde{\beta}^{*k}$ uniquely satisfies the moment condition $\mathbb{E}[g(\tilde{\beta}^{*k}, R)] = 0$, where the moment function $g(\beta^{*k}, R) = \frac{\partial g(R, \beta^{*k})}{\partial \beta}\big(\tau^k(R) - f(R, \beta^{*k})\big)$. Under the working model $f(r, \beta^{*k}) = \beta_0^{*k} + \beta_1^{*k} r$, $\tilde{\beta}^{*k}$ is the solution to $\mathbb{E}[g(\tilde{\beta}^{*k}, R)] = 0$, where $g(\beta^{*k}, R) = \begin{pmatrix} 1 & R \end{pmatrix}^\top \big(\tau^{(k}R) - f(R, \beta^{*k})\big)$.

B Further Details about Estimation

In this section, I detail my estimation strategy for the projection parameters of $\tau(r)$ onto R, $\tilde{\beta}^{*k} = (\tilde{\beta}_0^{*k}, \tilde{\beta}_1^{*k})$. In short, I obtain semi-parametric estimates of β^{*k} via a projection learner algorithm (see McClean et al., 2022), based on the the efficient influence function (EIF) for the moment condition $\mathbb{E}[g(\tilde{\beta}^{*k}, R)] = 0$, and combine this with cross-fitting (Chernozhukov et al., 2017). The (re-centered) EIF of the moment function $g(\beta^{*k}, R) = \begin{pmatrix}1 & R\end{pmatrix}^\top (\tau^k(R) - f(R, \beta^{*k}))$ is

$$\phi^k(X; \beta^{*k}) = \begin{pmatrix}1 & R\end{pmatrix}^\top (\psi^k(O; \eta) - f(R, \beta^{*k})),$$

where $\psi^k(O; \eta)$ is the recentered (true) EIF for the unconditional analog of $\tau^k(r)$, namely, $\mathbb{P}[A_{k+1}(\overline{1}_k) = 1]$. Here, I use $O = (X, \overline{A}_{K+1}, \overline{Z}_K,)$ to denote the observed data, and η to denote a set of nuisance functions. For the four transitions I consider in the model (high school completion, college attendance, college completion and graduate school attendance), there are three EIFs for the three relevant counterfactual quantities, $\mathbb{P}[A_{k+1}(\overline{1}_k) = 1]$, for $k \in \{1, 2, 3\}$, since the first transition has no relevant counterfactual). Under the l data-generating process assumed for the causal and non-causal pathways between educational transitions shown in Figure 2 in the main text (namely, assuming that a set of pre-high school completion characteristics X serve as confounders for the $A_1 - (A_2, A_3, A_4)$ relationships, that the set Z_1 is empty (i.e., that there are no distinct confounders for the effect of college attendance on BA completion in addition to X), and that a set of post-secondary confounders Z confound the $A_3 - A_4$ relationships), these EIFs are as follows:

$$\begin{aligned}
\psi^1(O; \eta) &= \mathbb{E}[A_2 \mid X, A_1 = 1] + \frac{\mathbb{I}(A_1 = 1)}{\mathbb{P}[A_1 = 1 \mid X]}(A_2 - \mathbb{E}[A_2 \mid X, A_1 = 1]), \\
\psi^2(O; \eta) &= \mathbb{E}[A_3 \mid X, A_2 = 1] + \frac{\mathbb{I}(A_2 = 1)}{\mathbb{P}[A_2 = 1 \mid X]}(A_3 - \mathbb{E}[A_3 \mid X, A_2 = 1]), \\
\psi^3(O; \eta) &= \mathbb{E}[\mathbb{E}[A_4 \mid X, Z, A_3 = 1] | X, A_2 = 1] \\
&\quad + \frac{\mathbb{I}(A_2 = 1)}{\mathbb{P}[A_2 = 1 \mid X]}(\mathbb{E}[A_4 \mid X, Z, A_3 = 1] - \mathbb{E}[\mathbb{E}[A_4 \mid X, Z, A_3 = 1] | X, A_2 = 1]) \\
&\quad + \frac{\mathbb{I}(A_3 = 1)}{\mathbb{P}[A_2 = 1 \mid X] \cdot \mathbb{P}[A_3 = 1 \mid X, A_2 = 1, Z]}(A_4 - \mathbb{E}[A_4 \mid X, Z, A_3 = 1]).
\end{aligned}$$

The first step in the projection learner algorithm is to compute estimates of $\phi^k(X; \beta^{*k})$, for $k \in \{1, 2, 3\}$; the second step is to solve the empirical analog of the population moment condition stated in Equation 9:

$$\mathbb{E}_n[\begin{pmatrix}1 & R\end{pmatrix}^\top (\psi^k(O;\hat{\eta}) - f(R,\hat{\beta}^{*k}))] = 0, \forall k \in \{1,2,3\}, \tag{10}$$

where $\mathbb{E}_n[\cdot]$ denotes empirical expectation. Formally, this algorithm can be expressed as follows:

1. Randomly split data into K folds: $\{S_1, ...S_k\}$;
2. For each fold, use the remaining $(k-1)$ folds (training sample) to fit a flexible machine-learning model for each of the nuisance functions η involved in the estimating equations.
3. For each observation in k (estimation sample), use estimates of the above models to compute estimates of $\psi^k(O;\hat{\eta}), \forall k \in \{1,2,3\}$.
4. In the full sample, estimate $\hat{\beta}^{*k}$ by solving the empirical moment condition stated in Equation 10.

This procedure of using flexible machine learning methods are used to estimate the nuisance functions combined with the cross-fitting procedure implemented in the algorithm above means that our estimates of $\hat{\beta}^{*k}$are not only robust to model misspecification but also efficient and asymptotically normal. Valid standard errors can then be obtained via the heteroskedasticity-consistent estimator, which is valid because the limiting distribution of $\sqrt{n}(\hat{\beta}^{*k} - \beta^{*k})$ is

$$\sqrt{n}(\hat{\beta}^{*k} - \beta^{*k}) \xrightarrow{d} \mathrm{N}(0, G^{-1}\mathbb{E}[\phi^k \phi^{k\top}]G^{-1}],$$

where $G \triangleq \mathbb{E}[\frac{\partial}{\partial \beta^*}\phi^k] = \mathbb{E}[\begin{pmatrix}1 & R\end{pmatrix}^\top \begin{pmatrix}1 & R\end{pmatrix}]$.

C Point Estimates and Standard Errors: Descriptive Statistics and Main Text Analyses

Table 2 below shows point estimates and standard errors plotted in Figure 4, while Table 3 shows conditional means of respondent background and college-level attributes by parental income group (dichotomized) for (i) the full population, as well as for restricting the sample to "survivors" of each educational transition I consider: (i) high school completers, (ii) college attendees, and (iii) college graduates.

Table 2: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models.

	Descriptive / Causal Parameter			
Transition	β_0	β_1	β_0^*	β_1^*
High School	0.6860 (0.0076)	0.3237 (0.0133)	0.6860 (0.0091)	0.3237 (0.0138)
College Attendance	0.1648 (0.0117)	0.5944 (0.0194)	0.1199 (0.0100)	0.6203 (0.0179)
BA Completion	0.5075 (0.0204)	0.2895 (0.0297)	0.3456 (0.0279)	0.4094 (0.0371)
Grad. School Attendance	0.3601 (0.0281)	-0.0187 (0.0396)	0.3561 (0.0442)	-0.0478 (0.0478)

Note: Numbers in parentheses are standard errors. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

Table 3: Conditional means in background and college-level attributes by parental income (dichotomized).

		Full Population		HS Graduates		College Attendees		BA Completers	
		Low	High	Low	High	Low	High	Low	High
Background Attributes	Female	0.49	0.49	0.51	0.49	0.58	0.54	0.61	0.56
	Black	0.25	0.08	0.25	0.08	0.24	0.07	0.19	0.07
	Other	0.05	0.05	0.06	0.05	0.09	0.06	0.10	0.07
	Household Net Worth Percentile	65441.33	264497.94	73111.63	274161.43	112391.52	333207.71	131950.21	358926.12
	Parental Education	11.62	13.77	11.85	13.88	12.52	14.53	12.77	14.74
	Lived with Biological Parents	0.33	0.68	0.35	0.69	0.42	0.75	0.47	0.78
	Father Figure Present	0.58	0.89	0.59	0.89	0.63	0.91	0.64	0.92
	Lived in Rural Area	0.24	0.28	0.24	0.29	0.22	0.28	0.20	0.27
	Lived in South	0.40	0.30	0.38	0.30	0.38	0.29	0.35	0.29
	Children by 18	0.11	0.03	0.08	0.02	0.03	0.01	0.02	0.01
	Substance Abuse Score	1.13	1.04	1.07	1.03	0.83	0.85	0.78	0.80
	Delinquency Score	1.53	1.24	1.39	1.19	0.90	0.89	0.77	0.81
	75%+ of Peers Expected College	0.48	0.64	0.50	0.66	0.62	0.72	0.64	0.75
	90%+ of Peers Expected College	0.18	0.26	0.19	0.27	0.24	0.32	0.26	0.34
	Property Ever Stolen at School	0.25	0.22	0.25	0.22	0.22	0.20	0.20	0.18
	Ever Threatened at School	0.24	0.19	0.23	0.18	0.15	0.13	0.13	0.12
	Ever in a Fight at School	0.22	0.12	0.18	0.11	0.09	0.07	0.05	0.05
	Private School	0.02	0.05	0.02	0.05	0.02	0.06	0.03	0.06
	Catholic School	0.01	0.05	0.02	0.06	0.04	0.08	0.04	0.09
	ASVAB Percentile	36.16	57.41	41.38	59.64	57.43	70.43	62.15	72.70
College Level Attributes	STEM Major					0.18	0.18	0.17	0.18
	College GPA					2.58	2.84	2.99	3.09

Note: "Low" and "High" denotes individuals below and above median parental income, respectively. Conditional means are adjusted for multiple imputation via Rubin's (1987) method, and all statistics are calculated using NLSY97 sampling weights.

D Strength of Sequential Ignorability Approach

In the main text, I note that the sequential ignorability assumption is in fact weaker than an identification assumption that employs a single set of time-invariant confounders. To make these comments concrete, in this Appendix, I demonstrate the shortcomings of a "naive ignorability" approach in a longitudinal, educational transitions setting, and how such an approach can lead to different, and plausibly incorrect, conclusions about controlled inequalities, compared with conclusions reached under the sequential ignorability approach I pursue in my main analyses. Specifically, I compare controlled inequalities in graduate school attendance estimated under sequential ignorability (as in the main text) with controlled inequalities in graduate school that would be estimated under the stronger, "naive ignorability assumption

To recall, the sequential ignorability assumption (Equation 3 in the main text) states that each individual's potential transition value is independent of the prior transition, conditional on all antecedent transitions as well as on transition-varying confounders. This assumption therefore explicitly allows for the dynamic selection of individuals into and out of educational transitions. Moreover, it allows for the fact that certain variables may only be defined for those individuals who have made the prior transition (for instance, college GPA is undefined for non college-goers). In order to satisfy this assumption when estimating controlled inequalities in graduate school attendance, in my main analyses I include a large set of pre-high school completion confounders X in my models, in addition to a second set of post-college, pre-BA completion confounders Z (whether an individual declared a STEM major, and an individual's cumulative post-secondary GPA). Under sequential ignorability, and these two sets of confounders, controlled inequality in graduate school attendance, $\mathbb{P}[A_4(\bar{1}_3) = 1|R = r]$ is identified as

$$\mathbb{P}[A_4(\bar{1}_3) = 1|R = r] = \int \mathbb{P}[A_4 = 1|A_3 = 1, x, z, R = r] dP(z|A_2 = 1, x, R = r) dP(x|R = r).$$

By contrast, an ignorability assumption would require that each individual's potential transition value be independent of the prior transition, conditional on all antecedent transitions and *time-invariant* set of confounders that are defined for the entire population. In particular, for estimating controlled inequalities in graduate school attendance, we would have to assume that an individual's potential graduate school attendance status is independent of BA completion, given pre-high school completion confounders X.

By contrast, under the "naive ignorability" assumption, controlled inequality in graduate school attendance, $\mathbb{P}[A_4(A_3 = 1) = 1|R = r]$ is identified as

$$\mathbb{P}[A_4(\bar{1}_3) = 1|R = r] = \int \mathbb{P}[A_4 = 1|A_3 = 1, x, R = r]dP(x|R = r).$$

Unlike the sequential ignorability identification formula, this formula requires that BA completion be independent of potential graduate-school attendance, conditional on pre-high school completion characteristics X, and thus rules out post-college attendance characteristics Z that may confound the effect of college attendance on college-completion. Such an assumption seems unrealistic, because factors such as academic performance, motivation, and major choice are likely predictive of both BA attainment and post-tertiary progression.

To explore empirically the consequences of failing to adjust for transition-varying confounding, I re-estimate controlled inequalities in graduate school attendance using the DML procedure described previously but under the "naive ignorability" (I) assumption, and compare this with estimates of controlled inequality at this level obtained under my preferred sequential ignorability (SI) approach, as in the main text. Under the "naive ignorability" assumption, the EIF for the effect of BA completion on graduate school attendance reduces to

$$A_4^*(1,1,1) = \mathbb{E}[A_4 \mid X, A_3 = 1] + \frac{\mathbb{I}(A_3 = 1)}{\mathbb{P}[A_3 = 1 \mid X, A_2 = 1]}(A_4 - \mathbb{E}[A_4 \mid X, A_3 = 1]). \qquad (11)$$

Table 4 displays the estimated intercept and coefficient under (i) the descriptive transitions model (identical to those presented in the main text), (ii) the naive ignorability (I) approach to identifying the population transitions model, and (iii) my preferred sequential ignorability (SI) approach to identifying the population transitions model.

Turning first to the coefficients: while estimation uncertainty prevents us from definitively saying that the coefficients (under all three approaches) are non-zero, taking the point estimates at face value leads us different conclusions about the nature of inequality at the graduate school level. While β_1^4 is negative, implying that the inequality in graduate school attendance observed among the risk set of BA completers actually favors low income students. Nevertheless, under the "ignorability-only" approach, the estimated controlled inequality β_1^{*4I} switches sign compared, implying the opposite pattern. By contrast, under the weaker assumption of sequential ignorability, the same pattern of inequality as is observed under the descriptive model (favoring low income students) is observed, and the estimated slope β_1^{*4SI} more than doubles, compared with its descriptive analog, implying that lower income students are more advantaged in terms of graduate school attendance, and more so than we would conclude under the descriptive transitions model.

Perhaps more strikingly, the estimated intercepts differ substantially between the "ignorability-only" and sequential ignorability- approaches. The estimated probability of individuals from the lowest parental income rank under the descriptive model is 0.36, which is almost identical to the population model intercept obtained under sequential ignorability. By contrast, the equivalent quantity under the "ignorability-only" approach is 0.50, a vast over-estimation of the likely true value. The stark difference between the intercepts obtained under the two ignorability approaches, and limited difference in the slope estimates so obtained, suggests that college GPA and stem major are important confounders of the effect of BA completion on graduate school attendance for all individuals, but the pattern of confounding does not differ much by parental income rank.

Table 4: Estimated intercepts and slopes under the descriptive implementation of the Mare Model (β_0 and β_1) and the proposed population analog (β_0^{*k} and β_1^{*k}) under ignorability (I) and sequential ignorability (SI) identifying assumptions.

Transition	Descriptive / Ignorability Causal / Seq. Ignorability Causal Parameter					
	β_0	β_1	β_0^{*I}	β_1^{*I}	β_0^{*SI}	β_1^{*SI}
Grad. School Attendance	0.3601	-0.0187	0.4965	0.0134	0.3561	-0.0478
	(0.0281)	(0.0396)	(0.0241)	(0.0371)	(0.0442)	(0.0478)

Note: Numbers in parentheses are standard errors. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

E Stratification at the Graduate Level

In his original paper on educational transitions, Mare (1980) found that the parental income effect declined to the point of being negative for the transition from college to post-college (graduate) education - a finding reminiscent of Hout's (1988) observation that social background effects on adult occupational outcomes decline significantly among the college graduate population (see also Torche, 2011).

Examining in depth the relationship between social background and MBA program enrollment, Stolzenberg (1994) proposed a life course interpretation of the negative association between parental income and graduate attendance among college graduates, albeit without considering potential biases in his results owing to differential selectivity into this population subgroup. Finding that aspirations for post-college schooling, while strongly predictive of postgraduate attendance, are only very weakly associated with family background, he argues that aspirations for post-college continuation are moulded largely during college years, and without either much parental influence or with forms of parental influence that do not correlate with parental income.[15] Educational attainment beyond college could also reflect a distinct form of educational investment whose pursuit is largely determined by factors unrelated to attainment at preceding levels (for instance, attitudes towards work, and ambition for occupational advancement in careers for which a particular graduate degree provides requisite training) (e.g. Schein, 1974) . In turn, this could imply that the BA completion-graduate school attendance transition is not characterized by the differential selection problems present at earlier transitions: attributes on which low-income BA graduates are more positively selected than their higher-income peers, such as ability, motivation, external financial resources, and attitudes towards further school continuation, are not those attriburtes predictive of graduate school attendance[16]

One alternative possibility for the weakened effect of family background on post-college continuation results from the distinct diversity of post-collegiate schooling options compared with prior educational levels. The category of "graduate school program" encompasses fields of study and

[15] A similar argument is offered regarding financial disadvantage: college graduates lack the financial dependence on their parents "that would be necessary for the parental resources model to have much effect on post-collegiate school enrollment patterns." Stolzenberg (1994, p. 1044)

[16] Moreover, one interpretation Mare offered of his original finding is that children from the most economically advantaged backgrounds are less likely to make tertiary schooling transitions because they do not need further education in order to obtain high-paying employment, reliant as they are on bridging funds and network resources of their well-off parents.

training as diverse as law (JD), medicine (MD) , PhD programs, masters programs, and professional-oriented degrees such as MBA programs. Pooling over such a diverse range of educational types at the graduate level may risk masking important patterns of stratification that exist between family background and particular types of graduate program. Certainly, studies published after Mare's original paper found significant heterogeneity in family background effects in enrollment across different graduate programs: a zero effect of social background on MBA enrollment (Stolzenberg, 1994; Mullen et al., 2003) contrasts with a moderately positive effect on masters degree enrollment, and strengthening of the effect on professional and doctoral program enrollment Stolzenberg (1994).

Could this observed pattern reflect a true causal process rather than merely differential selection? Attitudes and aspirations, such as those about the type of work one wants to pursue, and those regarding education more broadly, likely motivate individuals to pursue a particular type of post-college education, rather than to pursue *graduate education* per se; the importance of different aspirations likely differs for different programs, and only certain of these attitudes and aspirations may be predictive of BA completion, and on which lower-income students are positively selected. For example, (Mullen et al., 2003) find that "setting one's sights on financial gain" greatly increases the probability that an individual will enroll in an MBA program, whereas such a factor has a negative effect on entering master's programs; further, it is plausible a priori that such an attitude is not necessarily more prevalent among low- than high-income graduates. Following this logic, in the analyses that follow, I disaggregate graduate school attendance into distinct types of graduate program in order to examine potential differential selectivity into each.

Conditional and controlled inequalities for different forms of graduate program attainment are shown in Table 5 and Figure 7 below. Examining first the conditional inequalities in Table 5 and the eft panel of Figure 7, the first striking result is that the slopes for MA / MSc tend in the opposite direction to those for JD and PhD attainment. The negative slope of -0.02 for MA / MSc attainment contrasts with the positive slopes for PhD and JD attainment (0.01 and 0.03, respectively). While we must attach some skepticism to these results due to their (relatively) large standard errors, these findings provide at least suggestive evidence that different student or educational processes might be work at the BA to graduate school transition. In particular, at face value, advantaged social origin does not predict masters degree pursuit among BA completers, while BA completers with higher parental incomes are more likely than their lower-income peers to pursue more lucrative advanced degrees. These descriptive findings certainly accord with those of Mullen et al. (2003), who found that the association between social background (measured in terms of parental education level) and postgraduate enrollment varies by type of graduate program, being strongest for professional and doctoral programs and weakest for master's programs.

To what extent might the differential selectivity of students from different income backgrounds drive these results? To address this question, we can turn next to the controlled inequalities presented in Table 5 and right panel of Figure 7. Strikingly, the slopes on parental income for all three types of graduate program considered are now all positive: coming from the highest rather than lowest parental income rank increases the probability of MA / MSc attainment by 3 percentage points, of PhD attainment by 1 percentage points, and of JD attainment by 2 percentage points. Also of note is that, after adjusting for selection into BA completion, the estimated intercepts across all types of graduate program completion fall substantially. For example, while approximately 26% of BA completers attain a masters degree (β_0), only 18% of BA completers would be expected to attain this degree under the hypothetical experiment to send a sample of high-school goers through college.

It is important to situate these results in the context of my main findings for conditional and controlled inequalities across educational transitions, which ignored horizontal stratification at the graduate levels. To recall from Figure 5, when we pool across graduate programs, controlled inequalities in graduate school attendance is negative. Yet, examining transition effects separately by graduate school program reveals a very different pattern of educational stratification - one that in fact reveals marked inequalities in BA completion's effect on further education.[17] In short, ostensible equality at the graduate school level masks inequalities in horizontal stratification; failure to account for horizontal dimensions of educational stratification risks mischaracterizing the degree of differential selection into distinct graduate programs and, by extension, the truenature of higher-educational inequality.

These findings should, however, be interpreted with some caution. First, I necessarily measure type of program attended by attainment of that program degree, as the NLSY97 does not distinguish between enrollees of a particular program and attainment of the associated degree. Such a procedure is limited in that it conflates the net effect of BA completion on program enrollment as well as the net effect of program enrollment on attainment of the corollary degree, but is partly justifiable by the fact that completion rates are overall higher for advanced degrees, than for a BA degree. Moreover, the outcomes I consider are not mutually exclusive.

[17] This finding can be explained by way of a "Simpson's Paradox," which explains why conditional and marginal associations may have opposite signs.

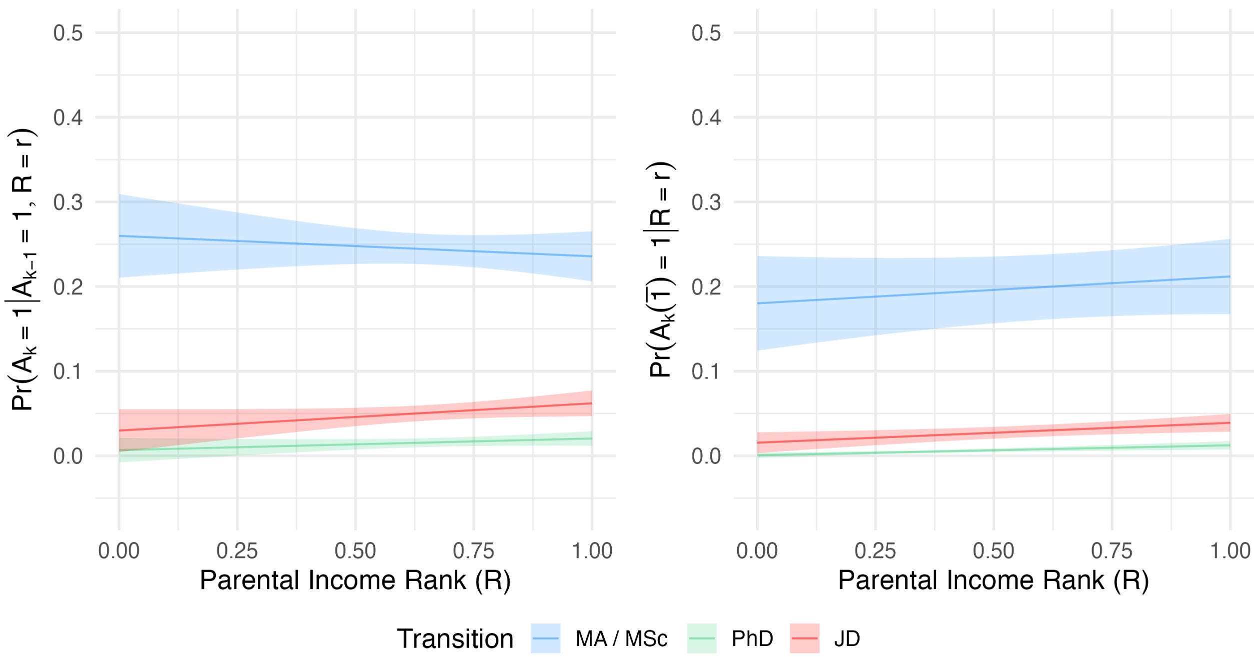

Figure 7: Predicted probabilities of making three types of graduate school transition under the descriptive implementation of the Mare Model and the proposed population analog. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

Table 5: Estimated intercepts and slopes under the descriptive implementation of the Mare Model (β_0 and β_1) and the proposed population analog (β_0^{*k} and β_1^{*k}).

	Descriptive / Causal Parameter			
Transition	β_0	β_1	β_0^*	β_1^*
MA / MSc	0.2599 (0.0252)	-0.0242 (0.0355)	0.1802 (0.0325)	0.0317 (0.0520)
PhD	0.0067 (0.0073)	0.0138 (0.0103)	0.0007 (0.0019)	0.0117 (0.0032)
JD	0.0298 (0.0129)	0.0322 (0.0182)	0.0155 (0.0060)	0.0235 (0.0090)

Note: Numbers in parentheses are standard errors. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

F Results Under Alternative Definitions of College Attendance

In the main analyses, I use age 22 as the cutoff to define college attendance, and include in this definition any individual who ever enrolled a 4-year college by age 22 either directly or via transferring from a two-year college (for individuals who did not either attain a BA degree or attend graduate school by age 29).

To test the robustness of my main results about conditional and controlled inequalities in BA completion to these coding decisions, I replicate estimates of conditional and controlled inequalities in BA completion using (i) alternative age cutoffs for defining college attendees including two-year transfer students from the definition of college attendance, and (ii) alternative age cutoffs for defining college attendees while excluding two-year transfer students from the definition of college attendance; in both cases, I keep BA attainment fixed at age 29. This latter replication exercise accounts for the fact that, as increasing the age cutoff for college attendance necessarily increases the heterogeneity of the treatment group of college-goers, since it includes more individuals who transferred from a two- to four-year college, and so attended a four-year college at a later age. This increase in heterogeneity is likely to be especially pronounced for lower-income students. Table 6 below shows how the proportion of college-goers that are two-year transfer students in my sample varies as the age cutoffs for defining college attendance is increased; this proportion increases from 0.23 when the cutoff for college attendance is defined to be age 20, to 0.35 when the cutoff for college attendance is defined to be age 29.

Table 6: Proportions of college-goers that are two-year transfer students, under alternative age cutoffs for defining college attendance.

	Age Cutoff					
	Age 20	Age 21	Age 22	Age 23	Age 24	Age 25
Proportion of 2-Year Transfers	0.23	0.26	0.3	0.31	0.33	0.35

Figure 8 shows intercepts and coefficients under the descriptive and proposed population transitions models, variously changing the cutoff for college attendance from age 25 to age 29, including two-year transfer students in the definition of college attendance, while Figure 9 shows the corresponding parameters when excluding two-year transfer students in the definition of college attendance. To recall, in the main text, the parameters are estimated when the age cutoff for college attendance is defined to be age 22, and when two-year transfer students are included in this definition): Under all specifications shown in these figures, we see that, for a given age cutoff definition, the results are consistent with the main finding reported in the main text: the intercept estimate (capturing the estimated BA completion probability for the lowest-income individuals) under the population model falls markedly compared with the descriptive transitions model, while the coefficient (capturing inequality in BA completion between the lowest and highest parental income ranks) increases. However, there is variation in the estimates across age cutoff definitions: increasing the age cutoff for college attendance lowers the estimated intercept under both the descriptive and population transitions models, but increases the estimated coefficients. For example, under the population model, 0.437 of individuals from the lowest parental income rank are expected to complete college when the college attendance age cutoff is defined to be age 20, but that proportion declines to 0.279 when that same cutoff is defined to be age 25 (a decline of 15.8 percentage points). By contrast, $0.437 + 0.336 = 0.773$ of individuals from the highest parental income rank under the population model are expected to be college completers when the college attendance age cutoff is defined to be age 20, but that proportion declines to $0.279 + 0.452 = 0.731$ when that same cutoff is defined to be age 25 (a decline of only 4.2 percentage points).

This variation, produced when the age cutoff for BA attainment of age 29 is held constant as is the case here, reflects the facts that increasing the age cutoff for college attendance induces a larger number of low-, rather than high-, income individuals into college (given that the latter are more likely to attend at earlier years), individuals who are particularly susceptible to degree incompletion by age 29.[18] The lower intercept at higher age cutoffs for college attendance captures that the additional low-income individuals induced into college attendance are unlikely to complete their degree by age 29. The higher coefficient at higher age cutoffs reflects the fact that fewer high-income students are induced into college attendance when the cutoff is increased (i.e., the majority of high-income four-year attendees are early attendees, even when attendance is defined to be age 25).

[18]In particular, while 88% and 98% of respondents from the lowest and highest parental income quintiles, respectively, had attended college by age 20 among the college-going group defined by the age 22 cutoff, these same percentages are 78% and 96% among the college-going group defined by the age 25 cutoff. In other words, a much larger share of lower income students in the age 25 cutoff group for college attendees come from late college-goers, compared with their high-income peers.

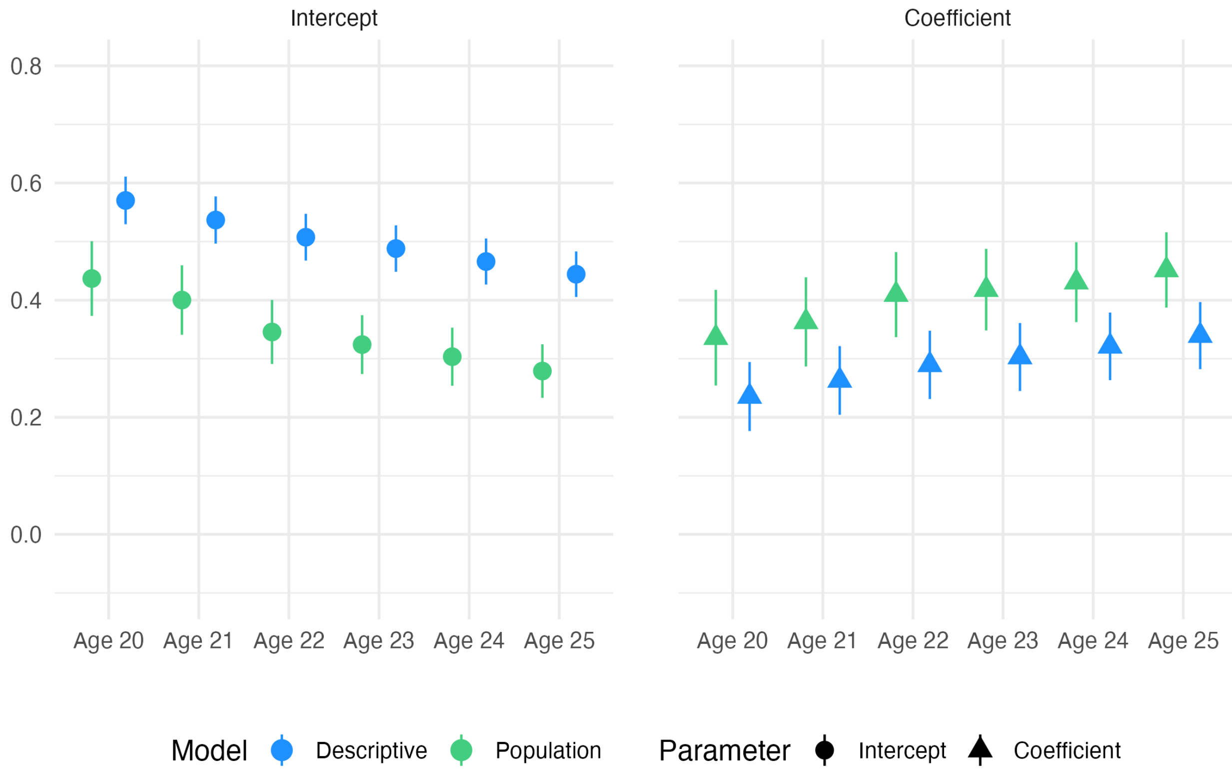

Figure 8: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models for BA completion (A_3) under different age cutoffs for college attendance (including two-year transfer students in the definition of college attendance).

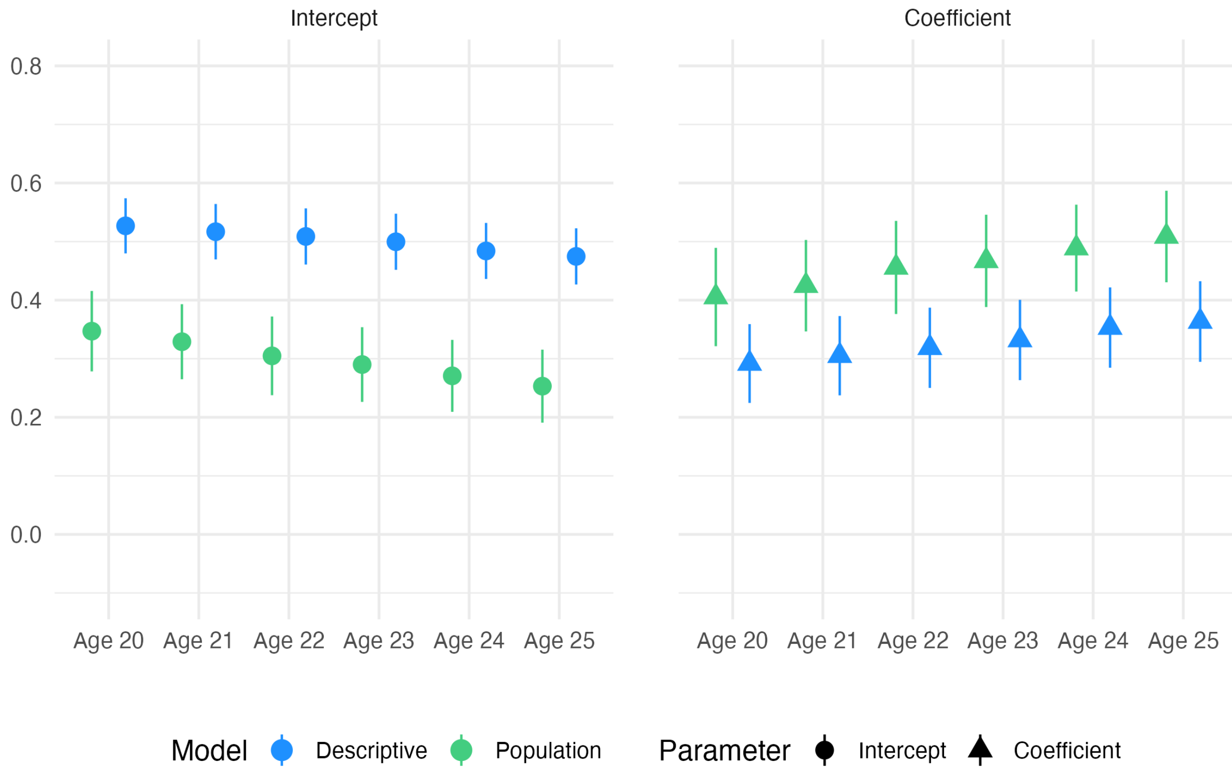

Figure 9: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models for BA completion (A_3) under different age cutoffs for college attendance (excluding college two-year transfer students from the definition of college attendance).

G Results Under Alternative Definitions of College Completion

In the main analyses, I use age 29 as the cutoff to define BA completers. This choice reflects the fact that low-income college-goers are more likely to have attended a 4-year college later in their educational careers, either after a period in the labor market, or after transferring from a 2-year college. Research also shows that lower-income individuals are also more likely to take a longer time to complete their degree than their higher-income peers (Oreopoulos and Petronijevic, 2013).

Figure 10 below shows intercepts and coefficients under the descriptive and proposed population transitions models, variously changing the cutoff for BA attainment from age 25 to age 29 (the latter being the cutoff used in the main analyses). We see that coefficient estimates are highly similar across alternative definitions of college completion. The range of the intercepts under both the descriptive and population transition models is larger, however, with a range of just under .1 when comparing the lowest and highest cutoffs. To recall, the the descriptive and population intercepts capture, respectively, the observed and counterfactual predicted probabilities of college completion for the lowest-income individuals. This increase in both these intercepts as the cutoff for BA completion is increased reflects the fact that low income individuals often enter college through a 2-year transfer route, and, once at a 4-year college, often take a longer to complete their degree than their high-income peers; increasing the cutoff for BA completion captures those groups of low-income students who take longer to complete their degree. By contrast, inequalities between the lowest and highest parental income groups are not dependent on age cutoffs for defining BA attainment.

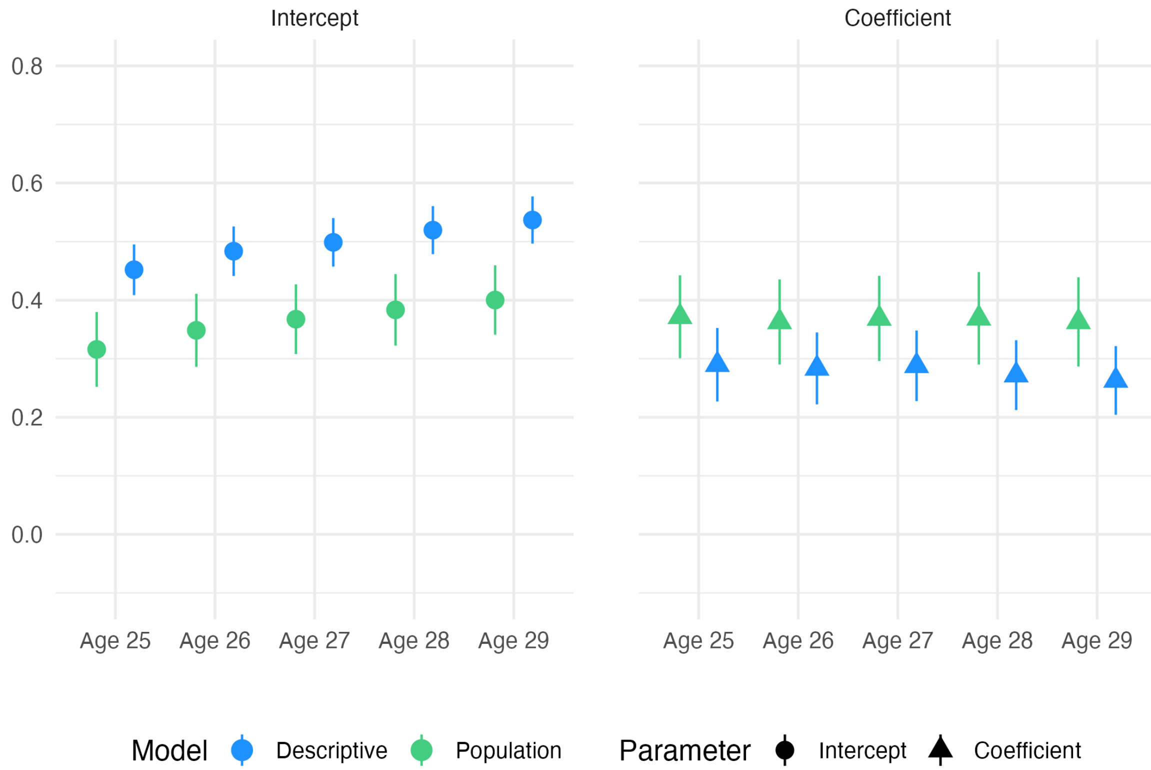

Figure 10: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models for BA completion (A_3) under different age cutoffs for time to completion.

H Results Under Alternative Definitions of Social Origin

In my main analyses, I focus in social background disparities in educational attainment where social background is operationalized as parental income rank. However, many studies of educational transitions inequalities typically report parameters for multiple measures of social background, and in this section I replicate the descriptive and population transition models for parental assets and parental education. Specifically, parental assets are measured as household net worth in 1997, and parental education is measured using mother's years of schooling, reflecting the demonstrated importance of mothers', as opposed to fathers', education for educational reproduction (Buis, 2013; Jerrim and Micklewright, 2011; Marks, 2008)). When mother's years of schooling is unavailable, it is measured using father's years of schooling.

The results, presented in Figures 11-14 below, show that results reported in my main analyses are almost identical for the first three transitions. However, across both parental assets and parental education, both the descriptive and population transition models suggest that BA degree attainment creates inequalities in graduate program enrollment. This contrasts with the finding for parental income rank, where the coefficient estimates for this last transition are negative (point-wise) and statistically indistinguishable from zero.

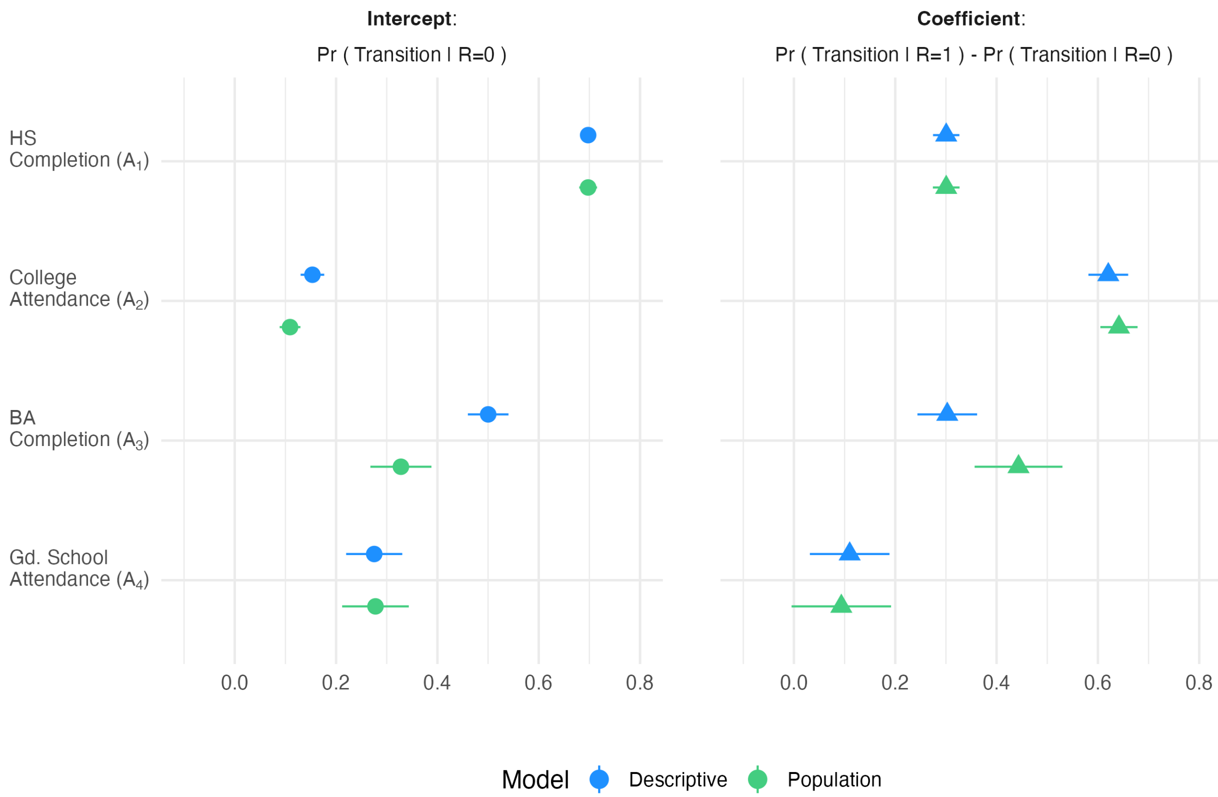

Figure 11: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models: parental assets.

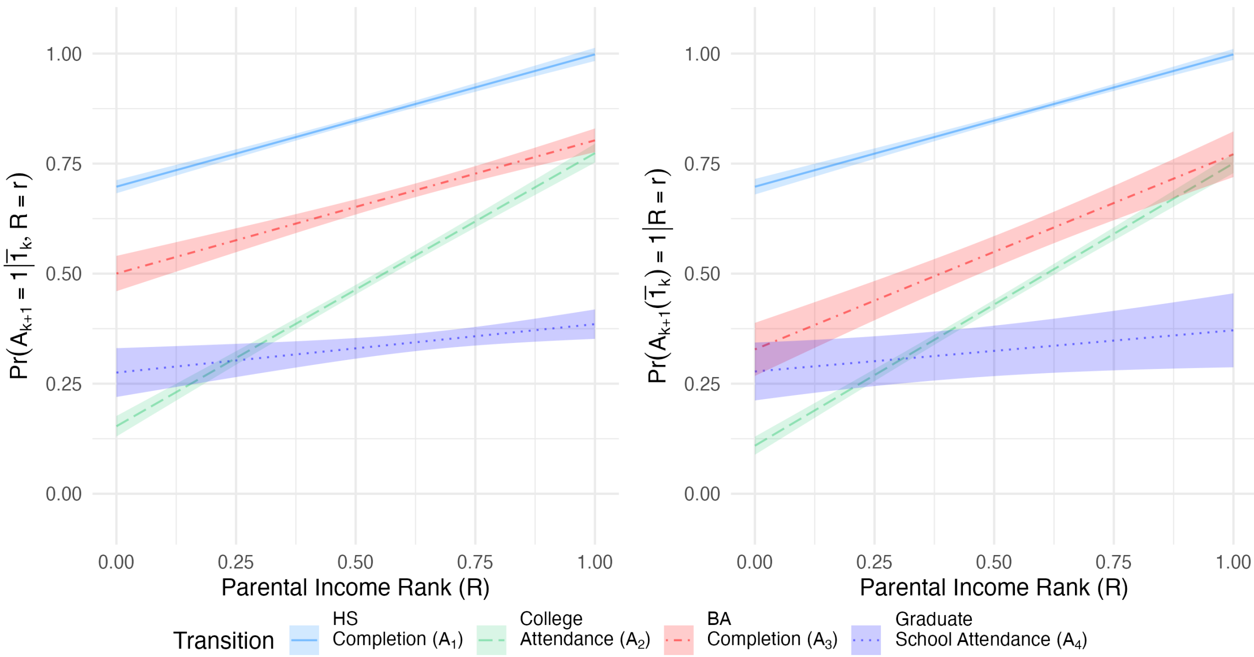

Figure 12: Predicted probabilities of making an educational transition under descriptive and population transition models: parental assets. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

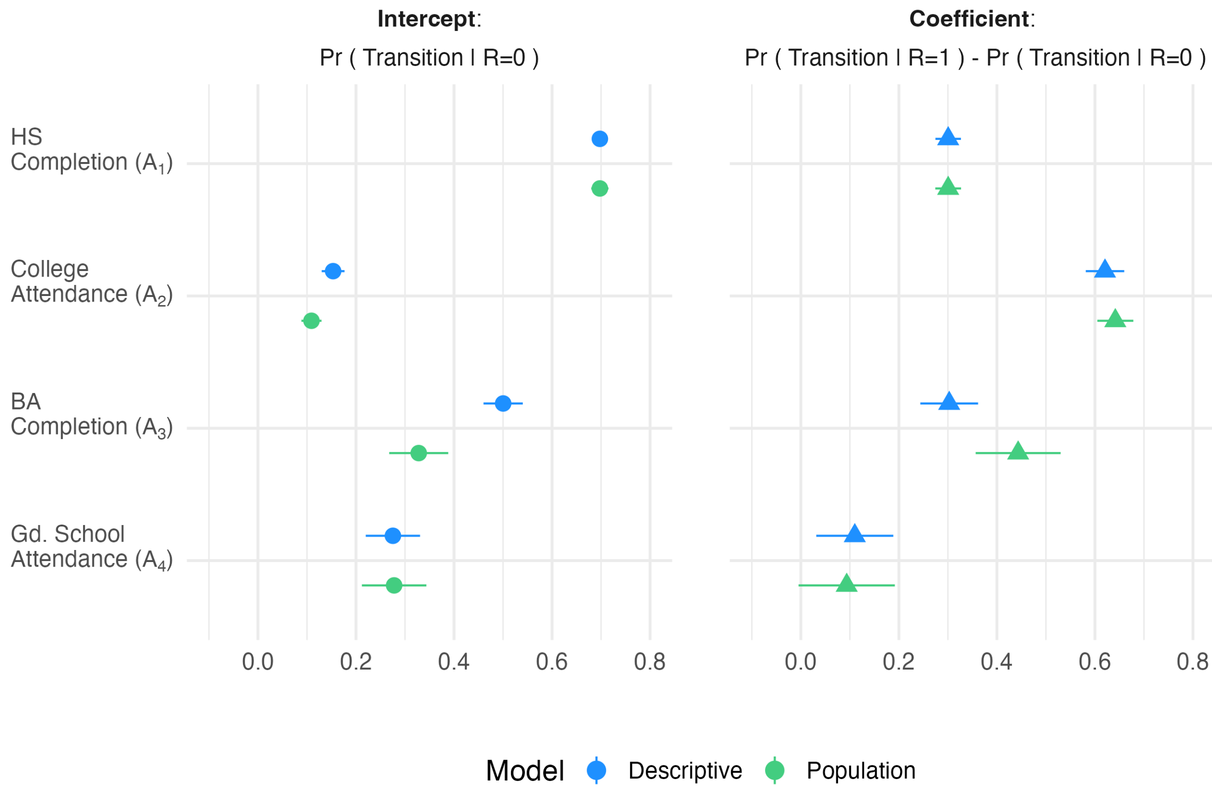

Figure 13: Estimated intercepts and slopes under descriptive (β_0 and β_1) and population (β_0^{*k} and β_1^{*k}) transition models: parental education.

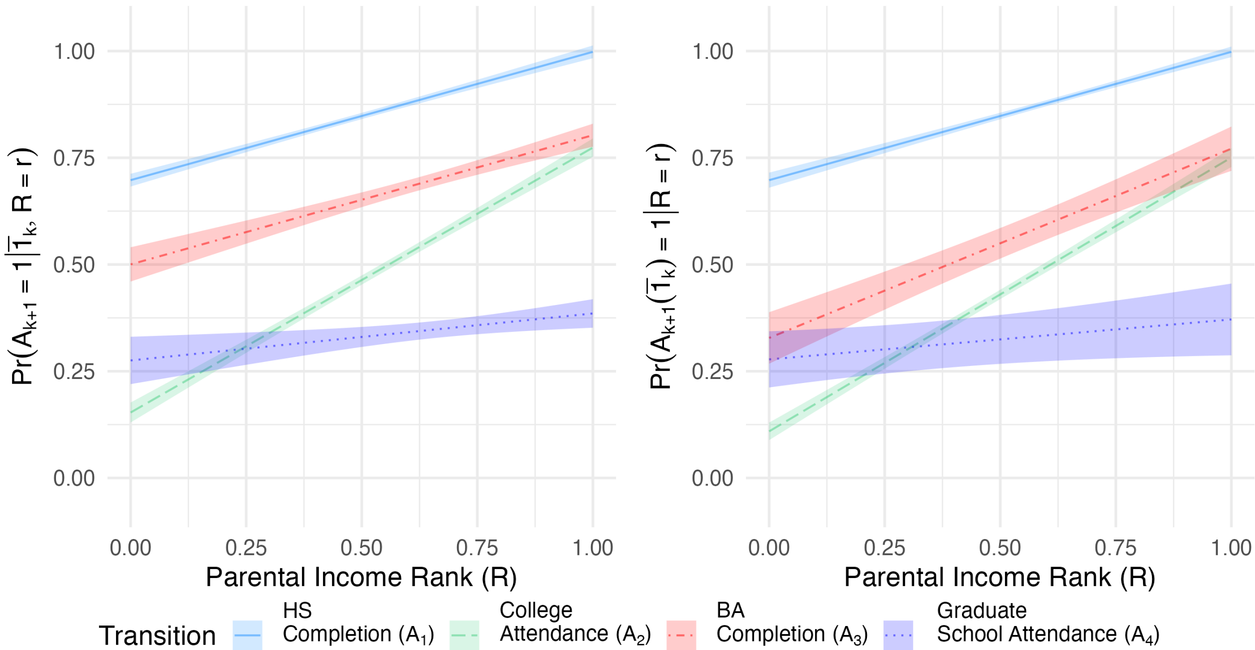

Figure 14: Predicted probabilities of making an educational transition under descriptive and population transition models: parental education. Point estimates and standard errors are derived from 10 imputed datasets using Rubin's (2004) method.

I Sensitivity Analyses

I.1 Results

As noted in the main text, I test the sensitivity of my main set of results to unmeasured confounding by extending a technique proposed by VanderWeele (2010) and VanderWeele and Arah (2011) in the context of mediation analysis, which varies the strength of a binary unobserved confounder's (U) association with the treatment (or mediator) and its association with the outcome of interest. I defer details of how this approach can be adapted to my population transitions model to Appendices I.2 and I.3, but here it suffices to note my main assumptions necessary to undertake such a test, namely, that (i) the effect of the unobserved confounder on the $k+1$th transition is positive and does not depend on any of the observed confounders, nor on parental income rank, (ii) the difference in prevalence of U between individuals who make the kth transition and those who do not, given other covariates, does not depend on values of these covariates and is linear in parental income rank.[19]

Importantly, this approach enables us to simulate a range of settings where the strength of the association between parental income rank and the unobserved confounder is differs across transitions, due to lower-income students' greater selectivity on U. Under these assumptions, we obtain a bias formula for $\mathbb{P}[A_k(\bar{1}) = 1|R]$ that is quadratic in R; taylor-expanding this expression about 0 returns a scalar first-order approximation of the bias of $\hat{\beta}_1^*$.

Figure 15 shows the bias-adjusted estimates of β_1^* under a range of potential values of η_1, where the parameters γ_0 and γ_1 are estimated via a linear regression of the signals for the kth transition effect on parental income rank, where δ_k is fixed at 0.15 and where η_0 varies in increments as η_1 varies. In other words, I assume that the unobserved confounder U is positively associated with both the probability of making both a given as well as a subsequent educational transition, and consider cases where the association between parental income rank and U (η_1) is either negative or positive. While, as 3 demonstrates, higher-income students have on average higher levels of observed attributes theoretically considered important for both attainment of a given transition and the next, but that as we progress across transitions, it is likely that lower-income individuals are increasingly *positively*

[19] Note that the approach I pursue differs from that advanced by Buis (2011) in the context of assessing unobserved heterogeneity in educational transition models by being totally nonparametric (in terms of being agnostic about estimation procedure undertaken), and assuming only independence between about the unobserved confounder, rather than structure in the form of its probability distribution. It also enables me to derive closed form analytical expressions for the bias, which is not the case for Buis.

selected on attributes predictive of educational attainment it makes sense to focus primarily on cases where η_1 is positive but not too far from zero.

Considering for illustration the bias-adjusted estimates of the inequalities that colleges generate in BA completion (second panel), we see that the difference in probability of observing the attribute U between the poorest and richest children would have to be in excess of 50 percentage points in order for the point estimate on this transition effect to be diminished to that observed under the descriptive transitions model (of .3). To put this into context, such an association would have to be stronger than heterogeneity in the causal effect of any educational transition (on the next) documented in prior sections.[20]

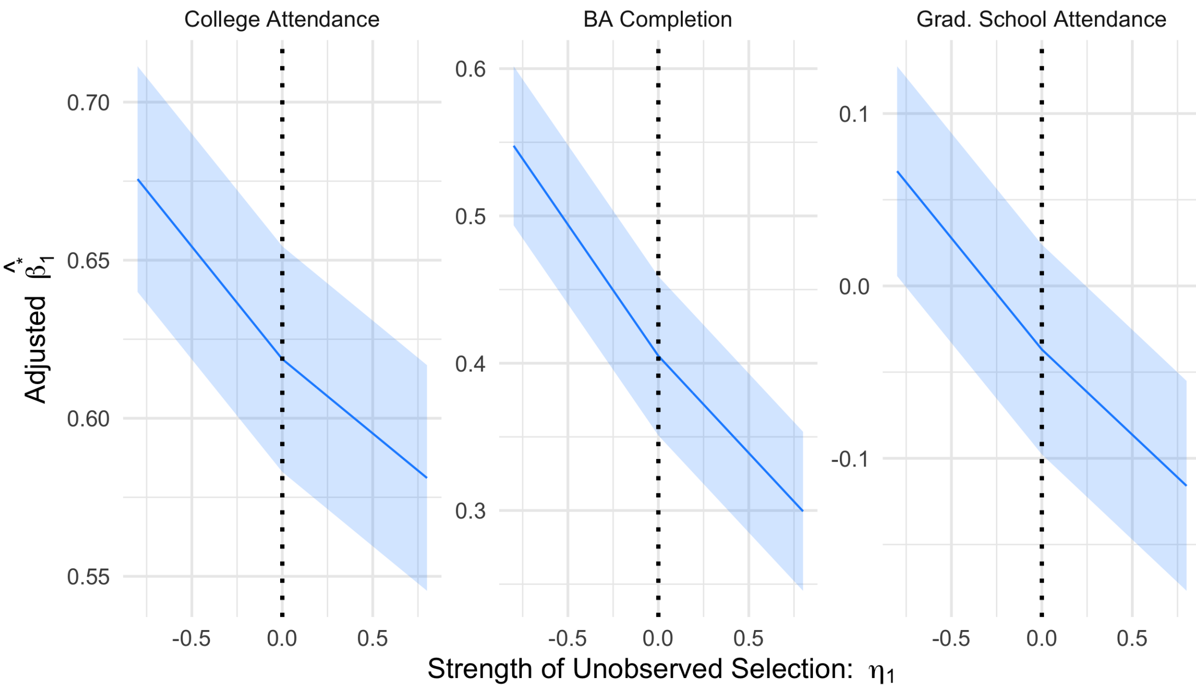

Figure 15: Adjusted estimates of the population parameter β_1^*, which corresponds to the "effect" of parental income rank on the probability of making a given transition under an intervention to send an individual through all prior transitions, under varying degrees of unobserved selectivity by family background, η_1.

[20]Specifically, I vary η_0 from 0.4 to 0, where 0.4 is the assigned value at the strongest negative value of η_1, and where 0 is attained when $\eta_1 = 0$. The logic behind this choice of intercept is the fact that, as the regression slope $\eta_1$increases from its most negative to a value of 0, the value of the intercept is likely to decrease.

I.2 Further Details about Sensitivity Analysis

Consider my primary estimand of interest - the net effect of a given educational transition on the next - and assume that this effect may be confounded by unobserved student characteristics that affect both the given transition as well as the next. I consider a binary unobserved confounder U such as cognitive ability or motivation that affects a given educational transition as well as attainment of the next. I make two simplifying assumptions about the interactivity of U with observed data $O = (X, \overline{A}_K, \overline{Z}_K)$: (i) $\mathbb{P}[A_{k+1} = 1|x, A_k = \overline{1}_k, \overline{z}_k, U = 1] - \mathbb{P}[A_{k+1} = 1|x, A_k = \overline{1}_k, \overline{z}_k, U = 0]$ does not depend on x and $\overline{z}_k$; (ii) $\mathbb{P}[U = 1|x, A_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U = 1|x, A_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 0]$ does not depend on x and $\overline{z}_k$. Under these assumptions, the bias for the estimate of $\mathbb{P}[A_{k+1}(\overline{1}) = 1]$ is given by (see Appendix I.3):

$$\alpha_{\mathrm{k}}\delta_{\mathrm{k}}(1 - \pi_k),$$

where $\pi_k = \mathbb{P}[A_k(\overline{1}) = 1]\forall k \in \{2, \ldots, K\}$ (i.e., the effect of the $(k-1)th$ transition on the kth transition, and where $\pi_1 = \mathbb{P}[A_1 = 1]$ (i.e. the marginal proportion of individuals in the population who complete the first transition, i.e. high school), and where α_k denotes the difference in prevalence of U between individuals who make the kth transition and those who do not, given other covariates, δ_k denotes the average difference in probability of making the next educational transition between those with and without U given other covariates,

Because my estimand, fundamentally, is concerned with effect heterogeneity by parental income background, we need to consider the above bias formula and its component terms as a function of parental income rank. Thus, $a_k = a_k(r)$, $\delta_{\mathrm{k}} = \delta_{\mathrm{k}}(r)$ and $\pi_{k-1} = \pi_{k-1}(r)$. Clearly, if each of these parameters are identical among individuals from different parental income backgrounds, then there will be no bias in my estimates of $\mathbb{P}[A_k(\overline{1}) = 1|R = r]$. Thus, while it is useful to consider cases where these bias parameters differ among parental income groups, because R (parental income rank) is a continuous variable, it is not straightforward to define rank-specific bias parameters; moreover, because my quantity of interest is a linear function of $\mathbb{P}[A_{k+1}(\overline{1}_k) = 1]$, where $A_1, \ldots A_{k+1}$ denote different educational transitions (e.g. high school, college attendance, college completion, GS attendance, etc) - i.e. the effect of a given educational transition on completion of the next. In particular, I am interested in heterogeneity in these expectations by parental income R, where I assume generally that these expectations are a linear function of g, i.e. $\mathbb{P}[A_{k+1}(\overline{1}_k) = 1|G = g] = \beta_0 + \beta_1 g$, we are interested in the bias of each of these regression coefficients. I focus on bias(β_1) in the following, as the intercepts estimated by my models have only a minor substantive interpretation.

To this end, I now two additional simplifying assumptions, namely that (a)$\mathbb{P}[A_{k+1} = 1|x, A_k = \overline{1}_k, \overline{z}_k, U = 1] - \mathbb{P}[A_{k+1} = 1|x, A_k = \overline{1}_k, \overline{z}_k, U = 0]$ does not depend on R, and further that (b) a_k and $(1 - \pi_k)$ are linear in R, i.e. $a_k = \eta_0 + \eta_1 R$ and $(1 - \pi_k) = \gamma_0 + \gamma_1 R$. Applying assumptions (a) and (b), it is easy to show thatbias$(\mathbb{P}[A_{k+1}(\overline{1}) = 1|R]$ is quadratic in R:

$$\text{bias}(\mathbb{P}[A_{k+1}(\overline{1}) = 1|R] = \delta_k[\eta_{k0}\gamma_0 + (\eta_0\gamma_1 + \eta_1\gamma_0)R + \eta_1\gamma_1 R^2]. \tag{12}$$

Since parental income rank R, by construction, lies between 0 and 1, it is appropriate to consider a first-order Taylor series expansion of this bias formula at $R = 0$:

$$\text{bias}(\mathbb{P}[A_{k+1}(\overline{1}) = 1|R]) \approx \delta_k\eta_0\gamma_0 + \underbrace{\delta_k(\eta_0\gamma_1 + \eta_1\gamma_0)}_{=\text{bias}(\hat{\beta}_1^*)} R,$$

where, as shown, the coefficient on R gives us a first-order approximation of the bias of $\hat{\beta}_1^*$.

I.3 Derivation of Bias Formulae for Sensitivity Analysis

In this section, I derive the bias for the effect of the kth educational transition on probability of making the next transition under some simplifying assumptions. Let $A_1, \ldots, A_K$ denote sequential transitions of interest, let $Z_1, \ldots Z_k$ denote distinct confounders of the effect of each transition on the next (i.e., Z_1 is a confounder for the effect of high school completion A_1 on college attendance A_2, Z_2 is a confounder for the effect of college attendance A_2 on college completion A_3, etc). Consider a binary unobserved confounder U that affects both the kth transition as well as the $(k+1)th$ transition. For each of the quantities $\mathbb{P}[A_{k+1}(\overline{1}_k) = 1]$,$k \in \{1, \ldots K\}$, we can write its bias as

$$
\begin{aligned}
&\text{bias}(\mathbb{P}[A_{k+1}(\overline{1}_k) = 1]) \\
&= \text{bias}(\mathbb{E}[A_{k+1}(\overline{1}_k)]) \\
&= \int \big(\mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U = 1] - \mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U = 0]\big) \\
&\cdot \big(\mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k]\big) \prod_{j=1}^{k} dP(z_j|\overline{A}_{j-1} = \overline{1}_{j-1}, \overline{z}_{j-1}), \quad (13)
\end{aligned}
$$

where $P(\cdot)$ denotes a probability measure. Notice next that the difference between the two conditional probabilities of U can be written as

$$
\begin{aligned}
&\mathbb{P}[U = 1|A_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k) \\
&= \mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \big(\mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 0)\mathbb{P}[A_k = 0|, \overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k] \\
&+ \mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathrm{\mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1]\mathbb{P}[A_k = 0|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k]}\big) \\
&= \big(\mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U = 1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 0]\big)\mathbb{P}[A_k = 0|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k).
\end{aligned} \quad (14)
$$

I next make two assumptions about the interactivity of U with observed data $O = (X, \overline{A}_K, \overline{Z}_K)$: (i) $\mathbb{E}[A_{k+1}|x, A_k = \overline{1}_k, \overline{z}_k, U = 1] - \mathbb{E}[A_{k+1}|x, A_k = \overline{1}_k, \overline{z}_k, U = 0]$ does not depend on x and $\overline{z}_k$; (ii) $\mathbb{P}[U = 1|x, A_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U = 1|x, A_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1]$ does not depend on x and $\overline{z}_k$. Under these assumptions, and plugging Equation 14 into the bias formula above (Equation 13), we have that

$$
\begin{aligned}
\text{bias}(\mathbb{E}[A_{k+1}(\overline{1})]) = \int & \big(\mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U=1] - \mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U=0]\big) \\
& \cdot \big(\mathbb{P}[U=1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U=1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 0]\big) \\
& \cdot \mathbb{P}[A_k = 0|, \overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k]\big) \prod_{j=1}^{k} dP(z_j|\overline{A}_{j-1} = \overline{1}_{j-1}, \overline{z}_{j-1}). \\
& = \big(\underbrace{\mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U=1] - \mathbb{E}[A_{k+1}|\overline{A}_k = \overline{1}_k, \overline{z}_k, U=0]}_{\triangleq \beta_k}\big) \\
& \cdot \big(\underbrace{\mathbb{P}[U=1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 1] - \mathbb{P}[U=1|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k, A_k = 0]}_{\triangleq \alpha_k}\big) \\
& \cdot \int \mathbb{P}[A_k = 0|, \overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_k] \prod_{j=1}^{k} dP(z_j|\overline{A}_{j-1} = \overline{1}_{j-1} \overline{z}_{j-1}) \\
& = \alpha_{\mathrm{k}} \beta_{\mathrm{k}} (1 - \pi_k),
\end{aligned}
$$

where $\pi_k = \mathbb{P}[A_k(\overline{1}_{k-1}) = 1] \forall k \in \{2, \ldots, k\}$ (i.e., the identified effect of the $(k-1)st$ transition on the kth transition, and where $\pi_0 = \mathbb{P}[A_1 = 1]$. Here, the second equality results from assumptions (i) and (ii), and the final equality follows from the fact that

$$
\begin{aligned}
& \int \mathbb{P}[A_k = 0|\overline{A}_{k-1} = \overline{1}_{j-1} = \overline{1}_{k-1}, \overline{z}_k] \prod_{j=1}^{k} dP(z_j|\overline{A}_{j-1} = \overline{1}_{j-1}, \overline{z}_{j-1}) \\
& = \int \mathbb{P}[A_k = 0|\overline{A}_{k-1} = \overline{1}_{k-1}, \overline{z}_{k-1}] \prod_{j=1}^{k-1} dP(z_j|\overline{A}_{j-1} = \overline{1}_{j-1}, \overline{z}_{j-1}) \\
& = 1 - \mathbb{P}[A_k(\overline{1}_{k-1}) = 1] \quad \text{(under sequential ignorability).}
\end{aligned}
$$

We note that $\mathbb{P}[A_k(\overline{1}_{k-1}) = 1]$, as well as its linear projection onto R (see Section I.2 above for more details) is estimable from the data under the sequential ignorability assumption.