An Interventional Approach to Primary and Secondary Class Effects on Intergenerational Mobility*

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Abstract

The theoretical distinction introduced by sociologists of education between primary and secondary class effects on educational outcomes helps researchers disentangle distinct mechanisms underpinning class-based educational inequalities. Research capitalizing on these concepts has, however, been inattentive to the multiple pathways that characterize attainment inequalities, net of demonstrated ability, and further reveals little about the contribution of such effects to intergenerational persistence more broadly. In this paper, I provide a new approach for conceptualizing primary and secondary effects within a causal framework. I propose two novel types of 'secondary' intervention on educational outcomes which map onto two distinct forms of policy intervention designed to break the link between social origin and education. My proposed approach engages with one of the primary contributions of the original theory - namely, the distillation of one mechanism underpinning class educational inequalities particularly amenable to policy intervention - while integrating this framework within the broader research agenda of education's role in intergenerational mobility. Second, I demonstrate how researchers can quantify the impact of these educational interventions on mobility using observational data. I illustrate my approach by showing the utility of distinguishing between these two types of intervention in understanding the sources of intergenerational income persistence using the NLSY97 cohort.

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One of the strongest predictors of adult socio-economic attainment is your family income during childhood. On average, a 10 percentile point increase in parent income rank is associated with a 3.41 percentile increase in a child's income rank in adulthood (Chetty et al., 2014, 2020). This association between parental income and adult attainment - intergenerational persistence - is important because it helps quantify the degree of opportunity in society (Hout, 1988; Breen, 2004). When intergenerational persistence is high, individuals from low-income households tend to stay poor, and those from wealthier households tend to stay wealthy. By contrast, when intergenerational persistence is low - that is, when mobility is high - individuals' socio-economic outcomes are to a greater degree independent of the socio-economic advantages or disadvantages that characterized their upbringing.

The education system plays a vital role in social reproduction. Sociologists typically discuss the role of education in social mobility in terms of what is called the 'OED triangle': that is, the triangle of associations between origins, education, and destinations (see Blau and Duncan, 1967). In this model, origin status affects destination status not only directly but also via educational attainment: children from high-income families are likely to attain higher levels of education than their socio-economically disadvantaged peers (Ziol-Guest and Lee, 2016; Duncan et al., 2017); in turn, higher levels of education lead to higher returns in the labour market (Autor et al., 2008; Baum et al., 2010). On the one hand, because of these twin processes, a major portion of the association between parental and child income is mediated through educational attainment (Blau and Duncan, 1967; Bloome et al., 2018). On the other hand, these facts point towards the education system, and in particular, class-based inequalities in educational outcomes, as a key site of policy intervention if we wish to break the link between social origin and social destination.

While both of these components of the OED triangle have received sociological attention, a particularly longstanding theoretical tradition in sociology has focused on explaining the mechanisms by which class origin impacts on educational attainment. One such line of research focuses on the distinction between class inequalities in educational outcomes produced from primary effects (class effects on academic performance) and those produced from secondary effects (class effects on individuals' probability of making an educational transition, *net* of performance) - that is, into the direct and indirect effects of class origin. (Boudon, 1974; Breen and Goldthorpe, 1997). It is hypothesized that the mechanism underpinning secondary effects is the differential cost-benefit decision calculus individuals of different social origins make when making an educational transition. An important take-away of the empirical finding across national contexts of the existence and large importance of secondary effects is that this mechanism of inequality is especially amenable to policy intervention. While it is difficult to imagine how we might reduce performance effects given the years of cumulative and multidimensional (dis)advantage among children from different socio-economic groups that impact attainment, a more realistic form of intervention might target resource and informational constraints at educational transition junctures (Jackson et al., 2007a; Jackson, 2013).

Understanding the policy importance of secondary effects requires a causal assessment to be made, by asking questions such as, "what would inequality by class background look like if policymakers disrupted secondary class effects?" Yet, while stratification scholars have recognized the potential policy importance of secondary effects, few studies have provided the causal foundations necessary to address this sort of question. In particular, the lack of recognition of a number of causal and non-causal pathways interferes with a straightforward interpretation of the direct and indirect coefficients, and gives us little ground for understanding how disrupting the cost-benefit adjudication assumed to underpin the secondary effect of class background would affect educational inequalities in the way desired. Given that a predominance of primary or secondary effects in educational inequalities would produce radically different policy implications, it is clear we need a better way to ascertain their importance for producing inequalities. This paper aims to fill this gap.

Specifically, I make two contributions. The first is to recast the traditional primary and secondary effects literature into a causal-prescriptive framework aimed explicitly at informing policy interventions. My proposed approach engages with one of the primary contributions of the original theory - namely, the distillation of one mechanism underpinning class educational inequalities particularly amenable to policy intervention - while shifting the focus of the theory from considering the role of secondary class effects on educational outcomes to interrogating their role in intergenerational mobility. Asking how we might conceptualize the contribution of the primary and secondary aspects of educational attainment inequalities to intergenerational mobility enables us to understand the contribution of these education-based mechanisms to broader inequalities in life chances, as well as the prescriptive value of intervening to disrupt primary or secondary effects from a policy. Despite the common concern of the primary and secondary effects and the social mobility literature with issues of equality of opportunity and the influence of ascribed characteristics on life chances, these two areas of scholarly inquiry have to date largely run on separate tracks; the former treats education as an outcome variable, while the latter treats education as a mediator of the association between origins and destinations. In addition, my proposed framework speaks directly to a recent literature on interventional effects, which interrogates the gap that would persist between demographic groups after some intervention to a manipulable treatment (Jackson and VanderWeele, 2018; Lundberg, 2021). I propose two novel types of intervention to secondary class effects on educational outcomes ('secondary interventions'): 'weak' secondary interventions capture the cost-benefit mechanisms with which the original theory is concerned, while 'strong' secondary interventions capture the broader causal processes that shape class effects on educational outcomes, net of performance.

Second, I demonstrate how we can quantify the impacts of disrupting secondary effects on intergenerational mobility. I elaborate a method that shows how we can quantify the impact of various educational interventions on intergenerational mobility, and illustrate the proposed framework using data from the National Longitudinal Survey of Youth, 1997 (NLSY97) cohort. I find that the intergenerational income mobility after a weak secondary intervention that most closely captures the original theory does very little to enhance mobility rates. By contrast, interventions targeted at the strong secondary effect, which entails a much larger set of mechanisms and carries a radically different set of policy implications, has the capacity to have a greater impact on mobility. This finding suggests that the traditional primary-secondary effects theory overstates the importance of secondary effects, as traditionally conceptualized, on mobility from a policy perspective; specifically, interventions addressing the cost-benefit calculus said to underpin secondary effects per se is unlikely to boost intergenerational income mobility in the United States. To promote mobility, public investments in higher education should be targeted towards eliminating strong secondary effects.

Secondary class effects on *educational* outcomes

Empirical evidence on the existence of secondary effects

One of the most robust sociological findings across a range of industrialized countries has been the stubborn persistence of inequalities in college enrollment and completion among individuals from different class backgrounds. While inequalities have declined in some countries over the latter half of the 20th century (Breen et al., 2009), class educational inequalities remain stark and persistent. Parental income inequalities also display some heterogeneity: college enrollment is most sensitive to income increases among individuals from lower socio-economic backgrounds (Lundberg and Brand, 2022).

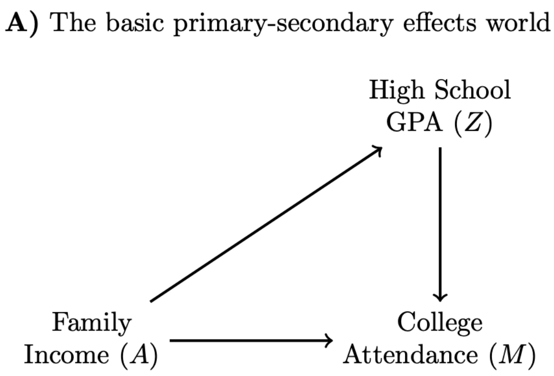
Sociologists interested in understanding inequality in educational opportunity have typically drawn a distinction between two distinct mechanisms first articulated by Boudon (1974). First, primary effects refer to the effects of social background on academic performance - the fact that children from higher class backgrounds, on account of their superior economic, social, and cultural resources, tend to outperform their disadvantaged peers in standardized tests and public examinations.[1] By contrast, secondary effects are the effects of class background on the decision an individual, in conjunction with their parents, teachers and peers, makes about whether to transition to a higher level of education, *conditional* on prior performance (for instance, whether or not to continue to university education conditional on high school GPA (e.g. Breen and Goldthorpe, 1997; Jackson, 2013)). Class inequalities in educational outcomes can then be understood as being produced from these two sources. Indeed, a salient finding from the empirical literature on primary and secondary effects is that, across a range of countries and time periods, working-class children are less likely than their advantaged peers to transition to a higher level of education, even when they have the same level of academic performance as these advantaged peers (Jackson et al., 2007a; Jackson, 2013; Morgan, 2012) This approach is formalized in the directed acyclic graphs (DAGs) presented in Figure 1. Consider the top DAG (A), which illustrates the assumed data-generation process in many applications of primary and secondary effects. Let A denote family income, Z be a measure of high school GPA, and M be an indicator denoting whether an individual transitions to college. In the language of mediation analysis, family income or social class background, can then affect individuals' decisions at an educational juncture both indirectly through performance, $A \to Z \to M$ as well as directly, net of performance, $A \to M$.

A large number of papers have taken up the issue of testing the existence of both primary and secondary effects and, in particular, assessing their relative importance in producing inequalities in educational attainment. In an important study, Erikson et al. (2005) obtained an estimate of the contributions of primary and secondary effects to inequalities in educational outcomes in the UK by combining the choice distribution of students of one class with the performance distribution of students of another to produce a counterfactual transition rate. This enabled the authors to obtain-an estimate for the proportion of students in a given class who would have made an educational transition had they maintained their performance distribution but having the same choice distribution as

[1] In this paper, I refer to social background broadly as material circumstances of upbringing, and use this term interchangeably with class origin and family income. Although I operationalize social origin in terms of family income, the approach I outline is of course generalizable to other domains of childhood material advantage.

students from another class, and compare this odds ratio with the observed odds ratio. The authors concluded that secondary effects account for around 45% of the salariat-to-working-class inequality (or 30% if social class is proxied by the binary variable of those with a degree versus those with no qualification) for the 1986 cohort (Erikson et al., 2005; Jackson et al., 2007a).

While the UK has received by far most attention on primary and secondary effects, several studies have extended these initial pieces to other national contexts. Perhaps most comprehensively, Jackson's (2013) volume finds than all educational transitions in all modern societies secondary as well as primary effects are involved in educational inequalities: young people of more advantaged social origins tend to make more ambitious educational choices than do individuals of less advantaged origins with similar levels of previous academic performance at each major educational transition (see also Morgan et al. (2013) for the US case). The relative importance of secondary effects is approximately one-third or higher, implying the substantial contribution of secondary effects of origin to overall inequalities in educational attainment. Specifically, in Italy and the Netherlands, over 90% of the IEO at the transition to university between the highest and lowest social background groups is determined by secondary effects, and in other countries such as the US and Germany around half or more of the total IEO is estimated to be attributable to secondary effects (Jackson and Jonsson, 2013).



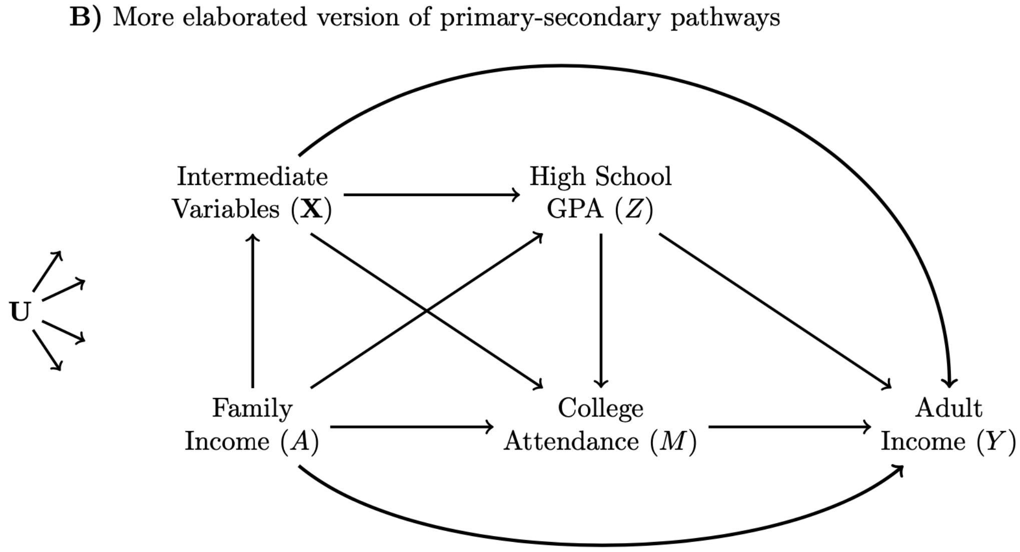


Figure 1: Two DAGs showing alternative models of class inequalities in educational attainment. The first corresponds to the simple model of primary and secondary effects as described in the literature (e.g. Jackson et al., 2007a; Jackson, 2013). The second corresponds to the more elaborated (and realistic) setting where we have two additional vectors: pre-family income unobserved confounders $\mathbf{U}$, as well as a set of intermediate variables $\mathbf{X}$, which encompasses a range of aspects of social disadvantage during upbringing. Note that $\mathbf{U}$ has forward paths to every vertex in the DAG.

“Secondary effects are conducive to policy intervention”: on the substantive interpretation of secondary effects

One important payoff to making a distinction between these two types of effects is that it draws attention to the distinct processes underlying different aspects of educational inequality, and thus different policy solutions. In particular, primary effects on performance are understood as the consequence of a complex interaction between the cultural, economic and social resources of children and their families and the educational system: the superior economic resources at the disposal of better-off families, and the higher cultural fluency of better educated parents and their familiarity with the schooling system, for example (e.g. Lareau, 2003; Schneider et al., 2018). By contrast, class differences in educational decisions at transitions can be seen as stemming from an evaluation of costs and benefits of the various educational options available - decisions conditioned by class-specific resources and informational constraints – and on the ‘perceived probabilities of more or less successful outcomes’ of those in different classes (Breen and Goldthorpe, 1997, p.276).

The argument goes as follows: young people, together with their parents and friends, set the costs, including the opportunity costs, of staying on in education against the expected benefits, especially regarding their future employment possibilities. Differences in choices, leading to secondary effects, then come about because for young people of more advantaged social origins the benefits of staying on and in general of making more ambitious educational choices tend to outweigh the costs involved far more often than for those of less advantaged origins. For example, education costs provide a key source of discrepancy in attainment as they decrease the proportion of working-class families whose resources will meet the costs of further education (in addition to indirect earnings-forgone through pursuing an educational pathway) (Goldthorpe, 2007). Central to this process is the fact that the more advantaged individuals’ social origins are, the higher the level of educational attainment that is likely to be required in order to give them good chances of maintaining their parents’ position. Drawing on a principle axiom of prospect theory - which assumes that individuals weigh losses higher than gains of equal magnitude (Kahneman and Tversky, 1979; Kahneman, 2011) - individuals’ mobility strategies are taken primarily to be driven by risk-averse choices at educational transitions. Thus, the primary motivation for those of advantaged origins to progress in the educational system as far as ever is possible can then be seen as a result of the need to avoid downward social mobility. As Breen et al. suggest (Breen et al., 2014, p. 266), in commenting on their finding of no relationship between risk aversion and staying on in education among students of more advantaged origins, "this arises because irrespective of risk tolerance" such students are always likely to stay on "as a way of ensuring that they reproduce their advantaged background." Such an interpretation of the direct effect of social origin on college enrollment behavior is consistent with quasi-experimental evidence on the causal role of family income on college enrollment, such as through examination of the effects of cash transfers to those at the threshold of eligibility for such transfers (Dynarski, 2003; Manoli and Turner, 2018).

While social policy does target the mechanisms producing primary effects in the form of educational resources, the extent of cumulative (dis)advantage that underpins primary effects is surely less amenable to policy intervention than the cost benefit calculus underpinning secondary effects. One could envisage such policy interventions in the form of informational campaigns, as well as financial incentives and support. In short, choice may be more readily influenced than performance. We may be able to credibly infer what would gaps in adult income between individuals from different parental income groups look like if individuals from less advantaged backgrounds would 'exploit' their demonstrated academic ability as advantaged children at educational transitions - if we were to 'neutralize' secondary class effects on educational outcomes. This fact is one often made in empirical analyses of secondary and primary class effects. For instance, as Jackson (2013) writes in her volume (see also (see also Jackson et al., 2007b):

> Aside from the utility of primary and secondary effects for explaining IEO [inequality of educational opportunity], the concepts are highly relevant to policy. Just as the explanatory tools differ depending on whether the focus is on primary or on secondary effects, the appropriate policy interventions will also differ depending on which type of effect is to be addressed...if differences in the choices made by students at the same level of performance have a significant impact, policies aimed at changing constraints and incentives hold more promise [than early years interventions].

Despite the important insight that the distinction between primary and secondary effects can bring to understanding the mechanisms underpinning class inequalities in educational attainment, there are several limitations to the traditional definition of these effects from a causal inference perspective that need to be addressed if the theory is to be useful from a policy perspective. In particular, it is unclear whether the "direct effect" of family income truly captures the cost-benefit calculus that informs the interpretation of this "effect", which is often captured by means of a regression coefficient in a model of college attendance on family income and high-school GPA. In fact, there are several reasons for questioning whether alternative social mechanisms may be underlying this direct effect, resulting from the fact that family income is highly correlated with other aspects of socio-economic advantage.[2]

There are several reasons for thinking that financial constraints may not be of primary importance in shaping college enrollment behavior, motivated by the fact that lower-income students often qualify for needs-based grants that may make (Choy and Berker, 2003; Houle, 2014). Certainly, several studies both in the US and UK provide suggestive evidence that income-based factors, including upfront costs as well as prospective debt, may not feature as saliently in individuals' educational decision-making as the traditional theory presumes: educational enrollment among lower-income students does not seem to be as elastic to tuition fees as the theory presumes. This then points us to a set of potential alternative explanations for the purported direct effect of family income on college attendance, namely, that students' college enrollment cost-benefit adjudications are informed by alternative aspects of disadvantage, such as by school contexts, peer groups, or neighborhoods.

First, the parents of low-income students may play an important role in shaping the college-going preferences their children. Several qualitative and ethnographic studies emphasize the difficulties encountered when parents who do not themselves have a college degree attempt to guide their children through various school and college milestones (e.g. Tornatzky et al 2002). In particular, as Hoxby and Turner (2013) write, these parents, may fail to differentiate between different types of colleges, such as those offering 2 versus 4-year degrees and those with high versus low graduation rates, and those with large versus few institutional resources. Moreover, these parents may themselves be unaware of financial aid packages available at particular institutions, and even less so than their children, who have an important additional informational source: the school. In consequence, these parents may focus on colleges with a low sticker price (such as a 2-year college), even if families might incur greater costs attending these colleges on net, or alternatively encourage their children to pursue alternative (non-educational routes).

Moreover, disadvantaged school and neighborhood contexts, which are strongly correlated with

[2]While it is technically possible to distinguish between studies that focus on individuals' application behavior from those that examine enrollment behavior, I argue that from a theoretical perspective it does not necessarily pay to do so. In particular, while one can distinguish between the direct costs of application from the costs of enrollment, clearly, perceptions about risk and future payoff and foregone labour market earnings will influence application behaviour as much as it will influence enrolmment behaivour.

family income, may shape college enrollment behavior, net of its effects on GPA, by leading to important informational constraints. Such constraints alter individuals' perceptions of the cost of college, and may further deter lower-income students from applying to college due to the seemingly burdensome paperwork and up-front fees required in an application, even if such students wish to attend college. In particular, the facts that high-achieving, low-income rarely apply to selective colleges that on-net cost less due to generous funding packages and financial aid programs (and in large part 'under-match' to lower-ranked schools that will cost them more overall), and that, once enrolled in a selective school, these individuals progress and graduate at the same rates as their high-income peers, suggest that such groups are using unreliable information to make college application decisions (Hoxby and Avery, 2012). Patterns of under-matching are also evident for mid-achieving, low-income students (Bowen et al., 2009; Smith et al., 2013). Experimental research has also demonstrated the importance of interventions that include semi-customized information on the application process (on the net costs of different colleges, suggested application behavior, and and low-paperwork fee waivers) or that aim to reduce complexity, misperceptions and uncertainty in the application process by guaranteeing aid and a fast-tracked application process, for increasing lower-income students' application and enrollment outcomes (Hoxby and Turner, 2013, 2015; Dynarski et al., 2021).[3]

Certainly, regarding the school environment, research suggests that lower-income individuals' peers and mentors do not know about funding support available to students. Schools with larger numbers of low-income students often have higher rates of teacher attrition and have fewer qualified teachers (for instance, with a college degree), as well as counselors who are unaccustomed to advising students who wish to attend (selective) colleges (Borman and Dowling, 2008; Bischoff and Owens, 2019; Hoxby and Avery, 2012). Geography may also play an important role in driving informational constraints in several additional ways. Comparing the profiles of low-income high-achievers who do and do not apply to selective colleges, Hoxby and Avery (2012) find that the

[3]Some scholars argue that informational constraints alone might not be enough to constrain application behavior is advanced by scholars who emphasize instead the importance of reducing complexity, misperception and uncertainty in the application process. Most recently, Dynarski et al. find that randomization of a college aid package guaranteeing aid and a fast-tracked application process to individuals in Michigan worked to halve the college attendance advantage of high socioeconomic status students, net of SAT scores, and in fact to reverse the effect for competitive colleges. By contrast, previous studies that have provided only information about aid eligibility, with no similar commitment, have substantively insignificant effects on application behavior (Bettinger et al., 2012; Bergman et al., 2019).

former group are concentrated in a small number of private high schools in the main cities of urban with at least one, and often several, selective colleges, while the latter are geographically dispersed, and often one of only several high-achieving, low-income students in the area. Thus, much of the targeted outreach work undertaken by selective colleges (visits by admissions staff to high schools, campus visits by students, after school college access programs, and the like), which occurs in areas where these colleges are located, are unlikely to reach lower-income students.

Beyond such informational constraints fostered within and outside of the school environment, the poorer neighborhoods and school contexts in which lower-income students are often located may affect college application and enrollment behavior in several broader ways. Social isolation theories of neighborhood disadvantage, for instance, argue that networks in poor neighborhoods contain few members of the middle-class or of mainstream society; in turn, this leads to the development of neighborhood subcultures - such as cultures that are oppositional to mainstream orientations regarding education and employment - into which young individuals are socialized (Wilson, 2011, 2012). Contextual socialization effects may also present themselves in a weaker form.Weaker expressions of social isolation theory argue that poorer neighborhoods may be characterized by wider array of competing and conflicting cultural models as a result of greater diversity of resident demographics; the result of such cultural heterogeneity is a greater diversity of educational goals among adolescents in these neighborhoods, as well as a weaker relationship between adolescents' goals, such as going to college, and their realized educational behaviors, even net of family and school socio-economic resources (Harding, 2011).[4]

[5] These perspectives accord with classical models of status attainment, which emphasized the

[4]This latter consequence is hypothesized to result not only from a lack of information about admissions and financial aid processes, but also from the social support for a greater variety of educational goals. As Harding aptly puts it, "such a neighborhood social environment will provide a 'weak signal" about the costs and benefits of various possible decisions".

[5]Instead of focusing on the social connections between neighborhood residents and larger society Social disorganization theories highlight processes that are endogenous to the neighborhood (Sampson et al., 1997; Sampson, 2012; Sharkey and Faber, 2014): the higher racial diversity and higher rates of residential mobility of poorer neighborhood leads to weaker social ties between residents; the resultant lower rates of "intergenerational closure" (that is, social cohesion among parents of school children in a community) presents young individuals with inconsistent set of educational values and afford parents lower capacity to regulate children's educational behaviours (e.g. Coleman, 1988; Morgan, 2005). These processes might plausibly affect students' educational decision-making. Yet, at least insofar as academic performance is concerned, empirical research has found inconsistent evidence on the existence of social organization and isolation effects. Empirical research has not found consistent evidence in support of positive effects of social closure on academic attainment (Morgan and Sørensen, 1999), and both ethnographic and quantitative research

role of socialization in accounting for individuals' educational and occupational outcomes (Sewell et al., 2018). "Significant others", such as teachers, school peers, and adult residents in a neighborhood, form expectations about whether a student will attend college; the student, it is assumed, internalizes these expectations as aspirations, which in turn leads them to embrace a particular educational identity (college-going/non-college-going, for instance) and to alternative educational outcomes. According to its proponents, this model of attainment assumes that significant others' expectations are a function of both the student's characteristics (their observed performance and socio-economic status), as well as exogenous sources. For example, independently of a student's academic performance, the educational expectations of peers and teachers' are likely to be formed by the educational trajectories of previous cohorts of students in a given school.

Most importantly, the above social processes can be said to capture mechanisms that pertain to broader aspects of disadvantage - such as neighborhood context - that are correlated with family income: having a low family income is predictive of living in particular social contexts which might affect information available as well as exposure to certain peer groups, cultures and facilitatory adults. In other words, they capture the indirect effect of income on college enrollment behavior (net of high-school GPA) that operate through social disadvantage, and as such do not capture the pure secondary cost-benefit effect that is strictly class-related. The ideas articulated in the above can be formalized in the more elaborated causal DAG (B) below, which also suggests some additional issues with the traditional primary-secondary effects conceptualization. Compared with that in Figure 1, the DAG below features two additional vectors representing an unobserved vector $\mathbf{U}$ denoting cultural resources as well as genetic inheritance, which affects all other sets of variables in the DAG, and a vector of 'intermediate' variables $\mathbf{X}$ on the causal path from family income to performance (such as educational expectations, school type and neighborhood quality). Note that our outcome is now adult income than educational transition, in order to motivate the approach I take below in terms of disrupting intergenerational persistence.[6] If we are to understand the causal structure as more complex than the top DAG presumes, then a causal interpretation of primary and secondary effects quickly becomes far less straightforward.

has found little evidence in support of the existence of neighborhood "ghetto" cultures that are antithetical to mainstream views about education and work (Edin and Kefalas, 2011; Newman, 2009; Young, 2011; Tyson et al., 2005; Harris and Robinson, 2007).

[6]Note in addition that, while $\mathbf{X}$ is multivariate, we can be agnostic about the causal relationships among its constitutive variables. While I also assume that A, $\mathbf{X}$ and Z precede M, a temporal ordering of A, $\mathbf{X}$ and Z is not required to define and identify my proposed interventional effects.

First, in addition to the two causal pathways elaborated in (A) we have an additional causal path $A \rightarrow \mathbf{X} \rightarrow M$; this pathway captures the processes described above that, as Morgan (2012) writes, cannot simply be considered a mechanistic elaboration of Boudon's conception of secondary effects understood as simply class-specific cost benefit analyses. Instead, it is a 'separate component of the net association between class and college entry that is best attributed to a broad structural interpretation' (p. 33). Thus, while if we model our data using the naive DAG in Panel A, this path will be absorbed into the secondary effect, yet these variables are not explicit components of the choice process that is thought to generate the causal secondary effects suggested by Boudon.

Two additional issues are especially salient. First, in this more elaborated DAG, disparities in high school GPA, college attendance and adult income across family income groups arise in several ways. To begin with, there are forward paths emanating from family income: for instance, family income might affect the type of neighborhood a child grows up in or the quality of school they attend, as well as college attendance and high-school GPA.[7] However, there are also unobserved variables $\mathbf{U}$ such as parental ability affect both parental income A as well as intermediate variables $\mathbf{X}$ such as child ability and college attendance. Crucially, this is a non-mediating path - it does not capture the effect of family income on $\mathbf{X}$. There are good reasons to be concerned about the fact that $\mathbf{U}$ may not be fully measured in observational settings. As far back as the 1990s, studies such as those by Mayer (1997) provided evidence suggesting that coefficients on family income on educational and other social outcomes may be confounded. More recent work on the dynamic selection of individuals into and out of neighborhoods also adds credence to the observation that family income coefficients are likely to be artificially inflated by sources of disadvantage that are not directly income-related (e.g. Wodtke et al., 2011). If researchers do not observe all of the components in $\mathbf{U}$, then they will be unable to identify the causal effect of family income or class. Second, this DAG makes clear that high-school GPA is a collider variable with respect to parental income A and other characteristics of upbringing $\mathbf{X}$ (Elwert and Winship, 2004; Zhou, 2019). Thus, conditioning on high-school GPA in order to examine the direct effect of family income on college attendance (the secondary class effect) induces a spurious association between family income and other characteristics of upbringing, and could artificially increase or decrease the estimated secondary effect. This spuriousness might be interpreted as follows: given the obstacles in terms of family and school disadvantage that lower-income students must often overcome in order to perform well academically,

[7] In the formal language of DAGs, we now have 4 backdoor paths from A to M: $A \leftarrow \mathbf{U} \rightarrow M$, $A \leftarrow \mathbf{U} \rightarrow \mathbf{X} \rightarrow M$, $A \leftarrow \mathbf{U} \rightarrow Z \rightarrow M$, $A \leftarrow \mathbf{U} \rightarrow \mathbf{X} \rightarrow Z \rightarrow M$, and $A \leftarrow \mathbf{U} \rightarrow Z \leftarrow \mathbf{X} \rightarrow M$.

low-income high-performing students may be positively selected in terms of residential context, school type and peers, variables which themselves affect propensity to attend college ($\mathbf{X} \rightarrow M$). Thus, the secondary effect is likely to be attenuated overall.

In short, traditional studies on the importance of secondary class effects on educational outcomes lack the causal foundations necessary to address causal, and by extension policy-relevant claims. Given that a predominance of primary or secondary effects in educational inequalities would produce radically different policy implications, it is clear we need a better way to ascertain their importance for producing inequalities. While some studies have recognized some of the conceptual issues with the traditional framework as detailed here (e.g. Morgan, 2012), no previous work to my knowledge has attempted to empirically adjudicate between the traditional and alternative interpretations of the estimated secondary effect. In the following sections, I elaborate a reconfiguration of the traditional primary and secondary effects theory from a descriptive to causal (prescriptive) framework which speaks more directly to specific policy interventions.

An interventional approach to primary and secondary effects in intergenerational mobility

We can define two secondary interventions through a field-experimental ideal

Figure 1b motivates the approach I pursue here. As detailed above, one major limitation of the original primary-secondary effects framework is its lack of a clearly manipulable variable, ambiguity about potential non-causal relationships between family income and college attendance, and thus intrinsic inability to inform researchers about a hypothetical world under an intervention to neutralize primary or secondary class effects. As a corrective, therefore, I propose treating an educational transition as a clearly definable treatment variable whose effect we wish to causally identify for prescriptive, policy purposes. In this way, family income becomes simply a demographic marker, not treated causally but as a label of two or more groups defined by their social background. As we will later see, such a framework enables us to be agnostic about the causal ordering of parental income and other socio-demographic aspects of upbringing.

In addition to sidestepping the causal identification difficulty built into the original theory, this approach speaks to a broader literature on intergenerational mobility, where education is treated as a mediator of the association between origins and destinations (e.g. Figure 1), rather than as an outcome variable, as in the primary and secondary effects literature. Despite the common concern of the primary and secondary effects and mobility literature with issues of equality of opportunity and the influence of ascribed characteristics on life chances, these two areas of scholarly inquiry have to date largely run on separate tracks. Asking how we might conceptualize the contribution of the primary and secondary aspects of educational attainment inequalities to intergenerational mobility enables us to understand the contribution of these education-based mechanisms to broader inequalities in life chances, as well as the prescriptive value of intervening to disrupt primary or secondary effects from a policy perspective.[8] This predictive value speaks directly to one of the core advantages from distilling primary and secondary effects, namely, the latter's particular amenability to policy intervention.

Consider the set-up where a and a^* are two levels of family income we wish to compare, i.e. $A \in \{a, a^*\}$ with a^* representing the lower parental income group. Imagine drawing a sample from a population of interest, and then intervening to assign each an individual in the sample a level of education $M = m$. The value $Y(m)$ then denotes the potential value of adult earnings that individual i would take under that that level of education, such that $\mathbb{E}[Y(m)|A = a]$ denotes average adult income among units with $A = a$ if they were exposed to level m of education. Thus, under this setup, identifying the causal contribution of secondary effects then becomes an issue of asking: "how much would gaps in adult income by family income (i.e., intergenerational mobility) decrease if individuals' educational attainment were a function solely of primary, as opposed to both primary and secondary effects?". In other words, what would mobility look like in a world where the secondary mechanisms underpinning educational attainment ceased to exist? As we will see, 'neutralizing' secondary class effects in the context of an observational study amounts to asking what gaps in adult income would look like if we imposed a particular probability distribution of educational attainment among individuals of a given class background.

Having addressed one major weakness of the original theory, namely, the lack of a clear causal target, how might we consider the lack of clarity of the multiple pathways that the traditionally-defined secondary effect captures? Instead of assigning everyone to a particular level of education, my proposed approach centers around a "stochastic treatment assignment rule" which shuffles treatment assignments around units in the sample according to some function of observed covariates $\mathbf{G} = \mathbf{g}$. Such an assignment rule can be understood as the process of a researcher observing

[8]Such an approach is of course predicated on the labor market earnings advantage to college attendance.

any unit's covariate values and then assigning that unit to a treatment value $M = m$ probabilistically from a set of treatment values $m \in \{1, \ldots, M\}$ according to some probability distribution $P(m|\mathbf{g})_{m \in \mathcal{M}}$. In addition, let $\mathcal{M}_{|\mathbf{g}}$ denote a random draw from this distribution. I define two types of intervention, which I illustrate graphically in Figure 2:

1. First 'weak' secondary interventions can be understood as those effects that capture class-differentiated choice decisions at a juncture operating *net of* the effect of class origin on intermediate variables $\mathbf{X}$. In other words, such an intervention would not disrupt the path from family income to college attendance through $\mathbf{X}$; only directly net of $\mathbf{X}$. Let $P(M|\mathbf{X}, Z, a)$ denote the cumulative distribution function (CDF) of M (college attendance) among those with high school GPA grade $Z = z$, intermediate variables values $\mathbf{X} = \mathbf{x}$, and family income level $A = a$, and $\mathcal{M}_{|\mathbf{X},Z,a}$, a random draw from this distribution. Note that $\mathbf{X}$ and Z are random variables in this distribution, whereas A is fixed to $A = a$, indicating that the intervention randomly assigns college attendance M among individuals from all income backgrounds such that both (i) attendance follows the same distribution as is observed in the group $A = a$ within groups defined by Z and $\mathbf{X}$, and (ii) the relationship between Z and $\mathbf{X}$, on the one hand, and A, on the other, is unaffected. Therefore, the quantity

$$\psi_{a,a^*}^{\text{weak}} \triangleq \mathbb{E}[Y(\mathcal{M}_{|\mathbf{X},Z,a})|A = a^*] \tag{1}$$

reflects the expected mean among low-income individuals if their educational levels were drawn from $P(M|X, Z, a)$, i.e., the observed conditional distribution of education among high-income individuals. I call this type of quantity an "interventional mean" (a weak secondary interventional mean). Similarly, we can define the quantity $\psi_{a,a}^{\text{weak}} \triangleq \mathbb{E}[Y(\mathcal{M}_{|\mathbf{X},Z,a})|A = a]$ as the expected mean among high-income individuals if their educational levels were drawn from the observed conditional distribution of education among high-income individuals, with the same high school GPA (Z) score and value of intermediate variables $\mathbf{X}$.[9] The quantity

$$\Delta_{a,a^*}^{\text{weak}} \triangleq \psi_{a,a}^{\text{weak}} - \psi_{a,a^*}^{\text{weak}},$$

then captures the residual disparity in earnings between high- and low-income individuals after

[9] In principle, we could define alternative forms of weak intervention, for instance, those that impose among high-income individuals the observed conditional distribution of education among *low*-income individuals, i.e. $\psi_{a^*,a}^{\text{weak}} \triangleq \mathbb{E}[Y(\mathcal{M}_{|\mathbf{X},Z,a^*})|A = a]$.

a weak secondary intervention.[10]

2. By contrast, 'strong' secondary effects refer to interventions that would neutralize the path from parental income to college attendance net of high school performance that operates both through intermediate variables **X** as well as through other pathways. In other words, while I operationalize the pure secondary effect, which captures a class-situated cost-benefit analysis, as a residual term, net of additional covariates capturing additional aspects of socio-demographic (dis)advantage (blocked in a weak secondary intervention), the strong secondary intervention entails blocking an additional set of pathways from family income to college attendance. It is important to note that strong secondary interventions can in fact be further divided into two types of intervention in a way that weak secondary interventions cannot, which I discuss in Appendix B.

Let $\mathcal{M}_{|Z,a}$ denote a random draw from $P(M|Z,a)$, the distribution of M (college attendance) among those with high school GPA Z among those with family income level a, such that

$$\psi_{a,a^*}^{\text{strong}} \triangleq \mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a^*] \tag{2}$$

captures the expected mean among low-income individuals if their educational levels were drawn from $P(M|Z,a)$, i.e., the observed conditional distribution of education among high-income individuals within a given level of high-school GPA (a strong secondary interventional mean). Similarly, the quantity $\psi_{a,a}^{\text{strong}} \triangleq \mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a]$ captures the expected mean among high-income individuals if we intervened to send these individuals to college at the same rate as individuals from high-income backgrounds with the same high school GPA (Z) score (i.e. if we imposed the distribution of college attendance among high-income children with a particular GPA on high-income children).[11] Note in addition that the residual disparity between high- and low-income children, $\Delta_{a,a^*}^{\text{strong}} \triangleq \psi_{a,a}^{\text{strong}} - \psi_{a,a^*}^{\text{strong}}$, can be captured by $\mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a] - \mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a^*]$.

[10] Intuitively, setting M to a particular distribution is equivalent to assigning each individual a random draw from that distribution. Note in addition that, compared with other causal estimands, it is only meaningful in general to talk about interventional effects in the average, since each individual's $Y(\mathcal{M}_{|\mathbf{x},z,a})$ depends on the random quantity $\mathcal{M}_{|\mathbf{x},z,a}$

[11] Note that we could also write the weak and strong interventions, respectively, as $\theta_{a,a_1} \triangleq \mathbb{E}_{\mathcal{S}}[\bar{y}_{\mathcal{S},a_1}(\mathcal{M}_{|x,z,a})]$ and $\psi_{a,a_1} \triangleq \mathbb{E}_{\mathcal{S}}[\bar{y}_{\mathcal{S},a_1}(\mathcal{M}_{|x,a})]$, preserving the notation used in Lundberg (2021), where the expectation is defined over hypothetical repeated samples. I opt for population level quantities in the main text for notational simplicity, but I seek to make inferences only about a limited subsample, as I specify in the next section.

The advantages of the approach I propose above are twofold. First, when we consider A as simply a descriptive marker that informs a population disparity, we are not required to identify the 'effect' of income at all.[12] The approach therefore conveniently sidesteps identification issues of A on M present in the original primary-secondary effects framework; many of the backdoor paths considered by Morgan are simply not relevant to interventional effects.

Second, I have approached the issue of defining an intervention to secondary class effects on educational transitions by creating two distinct interventions. Clearly, defining a secondary intervention solely as an effort to neutralize the 'direct effect' of family income net of performance opens the question of how to treat variables ($\mathbf{X}$) on the (backdoor) pathway from A to M, college transition. As we can see in the DAG, there are two kinds of pathway from family income to college attendance: the pathways that operates through intermediate variables such as neighborhood type, and the pathways that operates directly, net of these intermediate variables. This observation then naturally leads us to consider two different types of secondary intervention on what the traditional literature labels the 'direct' pathway from class background to college attendance, net of academic performance. Importantly, each of these interventions deactivates both the forward and backdoor paths through $\mathbf{X}$ and net of $\mathbf{X}$. To recall, disparities in college attendance across family income groups, net of high-school GPA , arise in several ways. Unobserved variables $\mathbf{U}$ such as parental ability affect both parental income A as well as intermediate variables $\mathbf{X}$, which themselves affect M. Crucially, this is a non-mediating path - it does not capture the effect of family income on M via $\mathbf{X}$. In addition, there are forward paths emanating from family income: for instance, family income might directly affect the type of neighborhood a child grows up in or the quality of school they attend. As a result, the interventions I consider deactivate both the forward path(s) from A to M (net of high school GPA, and, in the case of the strong intervention, additionally, net of intermediate variables), and the backdoor paths from A to M. Specifically, the strong intervention which equalizes college attendance within high-school GPA groups deactivates the forward paths $A \rightarrow M$ and $A \rightarrow \mathbf{X} \rightarrow M$ as well as the backdoor paths $A \leftarrow \mathbf{U} \rightarrow M$ and $A \leftarrow \mathbf{U} \rightarrow \mathbf{X} \rightarrow M$, while the weak intervention deactivates only one forward path $A \rightarrow M$ and one backdoor path $A \leftarrow \mathbf{U} \rightarrow M$.[13]

[12]I adopt notation from the mediation literature concerned with effect identification of A to ease comparison, but A is simply indicative of a collection of individuals, and is not of direct causal interest

[13]In theory, it is difficult to construct a formal criterion for selecting variables that feature in$\mathbf{X}$ for the purpose of defining the weak intervention. I treat the strong secondary effect as operating like a residual category; the traditional definition of the secondary effect makes it too wide a residual, and it's better to pick out more intermediate variables so that the residual approximates better the "pure

Importantly, the two types of secondary intervention I propose here map onto different types of policy as might be delivered in practice. Figure 3 summarizes this mapping. First, weak secondary interventions can be considered the weaker form of intervention as they only block pathways from family income to attendance that operate net of structural aspects of socio-economic upbringing - i.e. pathways which are thought to capture the cost-benefit calculus aspect of educational transitions. Thus, an intervention of this sort would pertain to policy aimed directly to alter the cost-benefit calculus of individuals from different class backgrounds - for instance, to informational resources targeted at low-income children, or needs-based grants or financial incentives to apply or enroll in college. In other words, a weak secondary intervention might correspond to a supply-oriented policy (in the sense of increasing the number of low-income students who apply to college). By contrast, strong secondary interventions are the more radical since they are interventions that would not be sensitive to - or whose efficacy would not be shaped by - other aspects of disadvantaged students' upbringing environment. In other words, since they block the composite path from family income to attendance through both the cost-benefit calculus and structural aspects of upbringing (but not the path through GPA performance), they capture a world in which college admission depends only on high school performance and no other class-contingent factors. An intervention of this sort might, for example, refer to a set of targeted university admissions or quotas for a representative admission of individuals from different class origins, within GPA groups, although it is important to note that such an intervention eradicates entirely differences in college attendance by parental income (given GPA), irrespective of whether these differences result from class differences in application, admission, or matriculation.[14] Two points are of note here. First, both interventions operate at the level of college enrollment rather than of BA attainment, which make them distinct from the concept of 'controlled mobility' introduced in Zhou (2019).[15] Second, neither of these inter-

decision" process. However, to be sure, there could be intermediate variables that a researcher would not wish to include in $\mathbf{X}$, such as variables that directly capture short-term budget restraint and information about college. As a rule of thumb, variables that capture some portion of the hypothesized secondary mechanism (the cost-benefit calculus affecting college transition decisions) should not be contained within this set - for instance, variables that capture information about college. In practice with observed datasets, however, it is likely that the majority of observed social-background-related variables are appropriate to include in $\mathbf{X}$.

[14] This intervention is similar to the exercise undertaken by Chetty et al. (2020), who simulate counterfactual mobility rates under a hypothetical intervention to eradicate "under-matching" of lower-income students to high-tiered colleges, though differs in that (a) my intervention is at the college attendance level, and (b) my interventional effects are expressed as analytical estimands, rather than as a simulation algorithm.

[15] Moreover, because Zhou's (2019) intervention concerns BA completion, rather than college en-

ventions alter the association between family background and intermediate variables or high school GPA. The intervention is solely with respect to college attendance conditional on high school GPA (strong intervention), or conditional on both high school GPA and intermediate variables (weak intervention). Of course, policy interventions to equalize transition rates to higher education for those within the same GPA bracket are likely to be unsatisfactory for reducing class educational inequalities in general insofar as low-income students are constrained by their lower average GPA scores, as well to the extent that class inequalities in adult income persist among college graduands. [16]

rollment, it hinges on both ensuring access and ensuring completion among students. It could therefore be considered a composite secondary-primary intervention.

[16] These interventions are best interpreted as "stylized interventions", since the extent to which estimates of them using observational data would map onto true estimates if they were enacted in reality depends upon a number of factors, including the number of individuals subjected to the intervention, as well as the extent to which, for instance, the weak intervention successfully increases both application, admission and enrollment rates at colleges, among individuals from lower-income background, and the strong intervention increases not only admissions offices' behaviors, but also the application and enrollment behavior of lower-income students. At least insofar as high-achieving individuals are concerned (which constitute only one subgroup of the population to which my interventions are applicable), conditional on applying to a particular college, there appears to be no difference in admission and enrollment probability between high- and low-income students; the largest driver of family income inequality in seletctive college attendance is the gap in application rates between low- and high-income children.(Hoxby and Avery, 2012). While my mathematically-defined interventions do not map directly to policies, they provide guidance about what kinds of policies would be more helpful in improving mobility. If one is hesitant to interpret these estimands in this way, these estimands still have a legitimate interpretation in terms of the "contribution of a set of mechanisms" to intergenerational reproduction, which is, of course, my primary concern in this paper.

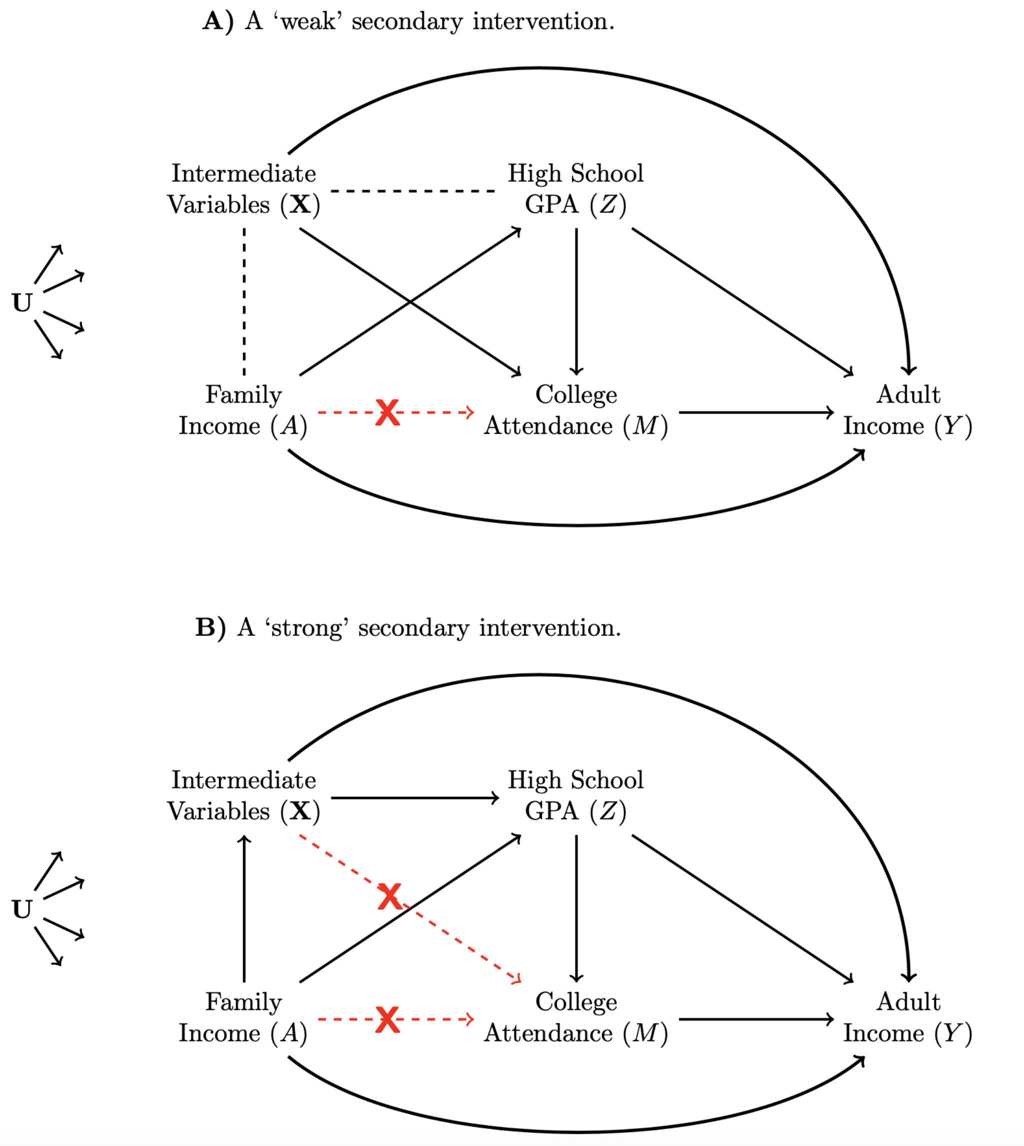


Figure 2: A ‘weak’ secondary intervention removes the association between family income A and its confounders **U** on college attendance, conditional on high school GPA Z *and* on intermediate variables **X** such as school type, neighborhood of origin, and peer expectations. A ‘strong’ secondary intervention removes the association between family income and its confounders on college attendance, conditional on high school GPA, but *unconditional* on intermediate variables. family income A purely as a demographic marker, the DAG accommodates unobserved cultural and genetic confounders **U** of the effect of family income on each set of variables in the model. Note that **U** has forward paths to every vertex in the DAG. Moreover, for expositional simplicity I illustrate the case where intermediate variables **X** are ‘post’-family income, though the flexibility of treating family income as a demographic marker means that my framework is agnostic about whether **X** occur before or after family income. The only chronological requirement is that A and **X** occur before Z, which in turn occurs before M, which occurs before Y.

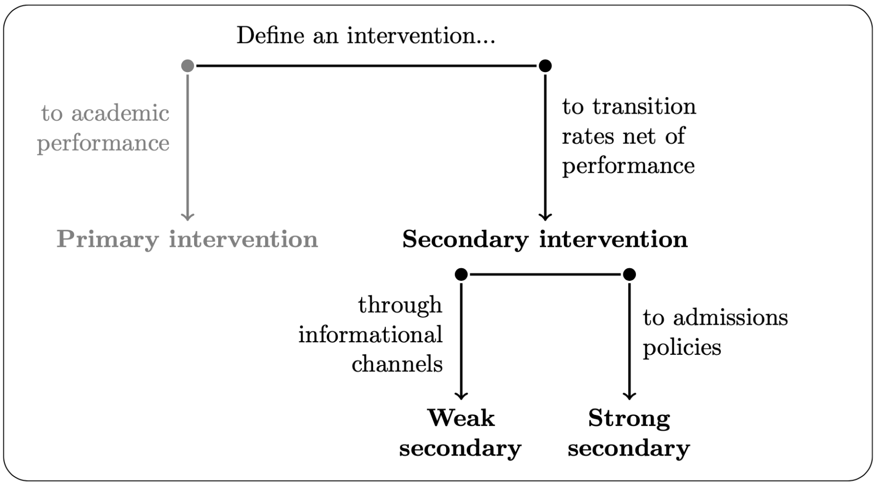


Figure 3: Educational interventions to reduce class-based inequalities in adult earnings operate at different levels. Most broadly, they can be designed to alter either individuals' educational performance or individuals' probabilities of transitioning to the subsequent stage of education (e.g. college), net of performance. These correspond, respectively, to primary- and secondary-based interventions, only the latter of which I consider in this paper. Secondary interventions can then be further divided into two types: (a) those that only block the direct pathway from parent income to college attendance net of both performance and intermediate variables such as school, neighborhood and other family characteristics (a weak secondary intervention), and (b) those that block the composite path from parent income to college attendance comprising both the direct effect in (a) and the path through intermediate variables. The important takeaway from this distinction is that weak and strong secondary interventions capture two different forms of policy intervention: whether we intervene to alter individuals' cost-benefit calculus for instance through an informational or grant-based approach (weak secondary), or through intervening to specify a particular admissions policy (strong secondary).

Identifying and estimating secondary interventional effects

Conceptualizing these interventions I propose as hypothetical experiments in the population is useful because it helps clarify our research goal, and is unbounded in scope. We may want to ask what mobility would look like under an intervention to the whole population of high-school goers. Of course, in practice, since we cannot change admissions policies twice over across colleges, approximating these counterfactual interventions using observational data is the most feasible way to estimate these estimands. While a clear advantage of the approach I propose compared with the traditional primary-secondary effects literature is the ability to sidestep identification of family income on educational transitions or on adult income,[17] this framework still requires identification

[17] Again, since A is used in purely a descriptive sense as a demographic marker, we do not require the assumptions of no unobserved treatment-mediator or treatment-outcome confounding. Note in addition that we are agnostic about the causal ordering of A and $\mathcal{M}_1$. A can be seen as

of the effect of college attendance M on adult earnings Y. This effect can be non-parametrically identified under conditional ignorability of the effect of college attendance on adult earnings.

Specifically, let $\tilde{P}(m|a,\mathbf{x},z)$ denote a (conditional) interventional distribution of interest, such that $\tilde{P}(m|a,\mathbf{x},z) = P(m|z,\mathbf{x},a)$ under the weak secondary intervention and $\tilde{P}(m|a,\mathbf{x},z) = P(m|z,a)$ under the strong secondary intervention. If we accept the causal structure of the world implied by the DAG in Figure 2, $Y(m) \perp M|A,\mathbf{X},Z$, and in order to identify the effect of M on Y we would need to control for confounders of the $M-Y$ relationship (that is, A, $\mathbf{X}$ and Z), so long as $\mathbf{U}$ does not affect both M and Y. Under this assumption of no unobserved confounding for the $M \rightarrow Y$ relationship conditional on family income, background characteristics and high-school GPA,[18] we can write $\mathbb{E}[Y(m)|A=a]$ as $\int \mathbb{E}[Y|A=a,\mathbf{x},z,m]dP(z,\mathbf{x}|a)$, and the weak and strong secondary interventional means (for low-income students) are thus identified as

$$\psi_{a,a_!} = \int \mathbb{E}[Y|a^*,x,z,m]d\tilde{P}(m|a,\mathbf{x},z)dP(z,\mathbf{x}|a). \tag{3}$$

For high-income students, the identification formula is identical, except for the fact that the expectation quantity and final density are replaced by $\mathbb{E}[Y|a,\mathbf{x},z,m]$ and $P(z,\mathbf{x}|a)$, respectively. Further details and a derivation of these identification formulas of these quantities are given in Appendix A. Note that this identification formula clarifies that neither weak nor strong secondary interventions alter the association between A and $(\mathbf{X},Z)$: the latter serve solely as variables we need to adjust for in order to identify the causal effect of M on Y.

The identification equation given above suggests that both weak and strong secondary interventions can be estimated via numerical integration or a Monte Carlo simulation approach (e.g. Imai et al., 2010). A limitation of this approach, however, is that estimates of these conditional density or probability functions tend to be noisy if any of the mediators are continuous or multivariate. Fortunately, we can rewrite both of the integrals that identify weak and strong interventions, respectively, in terms of probability functions that are more amenable to estimation. Using Bayes' rule, we can rewrite Equation 3 as a function of odds ratios of M, which suggests the following weighting estimator for the weak and strong secondary interventional means:

either causally prior to or causally post any of the component variables of $\mathcal{M}_1$ (e.g. family income determines the type of school a child attends, but parental education level or neighborhood type affects income (e.g. Wodtke et al 2011)). Either of these causal relationships may be true, but weak secondary interventions are identified in all cases.

[18] I also assume consistency and positivity (see Appendix A for further details).

$$\hat{\psi}_{a,a_1} = \mathbb{P}_n \left[\frac{Y\tilde{\Pr}(M = m|a, \mathbf{x}, z) / \Pr(M = m|a_1, Z, \mathbf{X})}{\mathbb{P}_n \tilde{\Pr}(M = m|a, \mathbf{x}, z) / \Pr(M = m|a_1, Z, \mathbf{X})} |a_1 \right], \tag{4}$$

where $\mathbb{P}_n[\cdot] = n^{-1} \sum_i [\cdot]$, and where $a_1 \in a, a^*$, for high- and low-income children, respectively. This method can be seen as an extension of the weighting-based estimators proposed in Vanderweele et al. (2014), and a proof is shown in Appendix A; since M is an indicator for college attendance, the density function $P(M|\mathbf{g})$ becomes the more easily estimable probability mass function, $\Pr(M = m|\mathbf{g})$. This estimator suggests the following procedure for estimating weak and strong secondary interventional means: first, estimate models for (1) M conditional on A and Z and (2) M conditional on A, Z and $\mathbf{X}$; second, a weighted average of observed Y among individuals at each level of family income, where the weights are the ratio of the predicted probabilities obtained from (1) and (2) evaluated at counterfactual and observed values of family income, respectively, constitute estimates of the strong secondary intervention; the same weighted average using the ratio of predicted probabilities from model (2) evaluated at counterfactual and observed values of family income, respectively, constitute estimates of the weak secondary intervention.

Standard errors for these quantities can be obtained via the heteroskedastic-consistent "sandwich" estimator or the parametric bootstrap. This procedure can easily be generalized to instances where A is multivariate, in which case the procedure is analogous but the contrasts a and a^* may refer to arbitrary levels of family income. Throughout the rest of the paper, I discretize family income into quintiles, and estimate secondary interventions by imposing among individuals from all parental income quintiles the distribution of college attendance observed among those in the 5th quintile - that is,$\psi_{5,a}$, $a \in \{1, \ldots, 5\}$. Because the identification formulae are agnostic about functional form, we can use any model, to estimate the propensity score models for the mediators; in the following, I use a logit model with all second order interactions to estimate the propensity scores involved in the estimator.

Comparison with existing approaches: educational expansion and decompositions

The weak and strong secondary interventions I have considered here capitalize on the mediating role of education in the transmission of socio-economic status over generations, by asking what would happen hypothetically to intergenerational mobility if policy-makers disrupted the origin-education (OE) association. These estimands are predicated on the fact that children from higher socio-economic backgrounds are more likely to attend and complete college than their disadvantaged peers, and rest on altering college attendance rates among individuals via competing assignment rules.

Asking what a counterfactual world would look like under these interventions is thus distinct from asking what would happen to rates of mobility if we expanded college access for all. This latter quantity is essentially that captured by Zhou's (2019) concept of 'controlled mobility'. Zhou's estimand reconfigures an earlier literature that explores the moderating role of education in the mobility process - that is, how social mobility varies across levels of education into a causal framework (Breen, 2010; Torche, 2011) - into a causal framework.

This framework makes clear that whether mobility would be higher than its present levels if we expanded educational access to all depends on whether attending higher education is more beneficial for disadvantaged youth - that is, whether there are higher "return to education" among children from disadvantaged social origins (Goldthorpe and Jackson, 2008; Hout, 2012). This is distinct from saying than that there is a larger observed association between family origin and adult attainment among BA holders, which might simply reflect the greater degree of selectivity among these individuals. Zhou (2019), however, finds no evidence of education's equalizing role; by contrast, the mobility-enhancing potential of the interventions I consider do not depend on higher-education's having higher effects for disadvantaged youth, since they are instead concerned with the mediating rather than moderating role of education in the mobility process.[19]

The idea of assigning a counterfactual probability distribution to educational attainment also relates to an older line of research which has sought to assess the "role" of education in the mobility process. Such approaches include estimating social fluidity with and without controlling for educational attainment (Goldthorpe and Mills, 2004), simulating the evolution of counterfactual origin–destination tables under different assumptions about educational change (Breen, 2010; Torche and

[19]There are two further differences of note between the secondary interventions I propose and Zhou's 2019 estimand of controlled mobility. First, my proposed interventions clarify that the intervention concerns transition to college (or college admission) rather than BA attainment (as considered in Zhou 2019). To be sure, an intervention to equalize BA attainment is the more radical, though such an intervention would hinge on ensuring completion among all students. This would be difficult, however, given that only 70% of all individuals enrolling at a 2- or 4-year college complete a degree, and such an intervention would need to manipulate not only college enrollment but also continuity - an intervention that arguably offers a vaguer directive for policy. Second, while controlled mobility sets the value of college attendance to be the same across all units, my proposed secondary interventions equalize the *distribution* of college attendance across groups, defined by values of the intermediate variables and high-school GPA, and thus entail that individuals from a particular demographic group are assigned a random draw from the full range of college attendance values, rather than just one.

Ribeiro, 2010; Pfeffer and Hertel, 2015), and path decompositions of the origin–destination odds ratios into direct and indirect (via education) pathways (Kuha and Goldthorpe, 2010; Breen and Karlson, 2014). All of these studies involve constructing a descriptive quantity which results from setting the educational distribution of lower-class groups to be the same as that of higher-class groups, e.g. $\int \mathbb{E}[Y \mid A = a^*, m] dP(m \mid A = a)$, where M denotes educational level. These quantities are similar to the interventional means featuring in my weak and strong secondary interventions, but differ in two respects. First, the probability distribution for college attendance in these descriptive decompositions is $P(m \mid A = a)$, whereas for strong and weak secondary interventions, this distribution is of the form $P(m \mid \mathbf{X}, Z, A = a)$ or $P(m \mid Z, A = a)$; second, and most importantly, educational attainment is not treated as a causal variable (whose effect is identified) in this descriptive quantity, and thus unlike the secondary interventional means, which have a causal interpretation, $\int \mathbb{E}[Y \mid A = a^*, m] dP(m \mid A = a)$ is a statistical parameter that should not be interpreted causally.

Data

To illustrate the proposed weak and strong secondary interventions, I draw on the National Longitudinal Survey of Youth 1997 (NLSY97), which began with a nationally representative sample of men and women at ages 12 to 18 in 1997. The population amenable to the interventions I consider are all students who completed a high school diploma or GED ('high school graduates'). I exclude students who dropped out of high school since they would be ineligible for college entry, and thus for the interventions I propose. I also use of college-level characteristics assembled by Zhou and Pan (Zhou and Pan, 2021), which makes use of the NLSY Geocode data and data from the IPEDS and the Opportunity Insights project (Chetty et al., 2020). I limit my analytical sample to white, black, and Hispanic respondents who had completed at least a high-school diploma or GED by age 22 who have not transferred to a 4-year college, and who have valid earnings information at ages 30-33 ($N = 4,640$).[20]

I measure parental income (A) as the average family income reported in the five earliest survey waves (1997 to 2001), and adult income by averaging respondent annual earnings between ages 30 and 33. Both variables are adjusted for inflation to 2019 dollars using the personal consumption ex-

[20] Removing 4-year college transfers from the analysis reduces the analytic sample by 27%. To assess the sensitivity of my results to the exclusion of this group, I replicate my main analyses on a sample including transfer students in Appendix D. My substantive findings are not sensitive to the exclusion of this group.

penditures index (PCE). I treat respondents' annual earnings as the sum of their self-reported wage and salary income and income from farms and businesses. Although total family income arguably captures a more complete picture of economic (dis)advantage in adulthood, focusing on individual income enables a more focused analysis for two reasons. First, because family income is function of extra-labour market processes such as assortative mating in addition to processes in the labour market, the counterfactual disparities I estimate would capture the effect of college on labour market and marital outcomes, which may differ and even counteract each other (Zhou, 2019) Second, for more global interpretations of the counterfactual disparities, family-level measures of adult income would require the stricter assumption that neither labour market nor marital market outcomes are a function of the proportion of individuals attending college, which is harder to maintain than solely the first component. As is common practice in the income mobility literature (Chetty et al., 2014, 2020; Bloome et al., 2018; Zhou, 2019), I transform both parental and respondent/adult earnings into their percentile ranks. This enables me to capture 'relative' rates of income mobility, which consider the intergenerational persistence of income net of overall changes in the marginal distribution of income over time and thus more directly measures equality of opportunity (Torche, 2015; Bukodi and Goldthorpe, 2018). I calculate the income ranks with respect to the population of high-school completers, and adjust them using the NLSY97 sampling weights. Compared with the NLSY79, which could also in theory be used to estimate the interventions I consider, the NLSY97 facilitates estimation of interventional effects that are likely to be closer to those we would observe if we undertook the ideal field experiment, since it traces the educational and labor market experiences of a younger cohort.[21]

In addition to parent and child income, I construct three further sets of variables: college attendance (M), intermediate variables $\mathbf{X}$ as well as high-school GPA variable Z, the latter two of which are used both to identify the effect of college attendance on earnings as well as to define the strong and weak secondary interventions. First, I measure college attendance as a binary variable denoting whether an individual has ever enrolled in a 4-year college $(M = 1)$ by age 22 or has completed high-school $(M = 0)$. Individuals who transition from 2- to 4-year colleges are qualitatively distinct from those who begin at 4-year colleges Ciocca Eller and DiPrete, 2018, and their inclusion in the analysis would necessitate some measure of associates degree (AA) performance considered

[21]Nevertheless, such an approach necessarily comes with a trade-off, since it only enables us to measure adult earnings in these cohort members' early thirties. As has been noted in the mobility literature, measures of adult income at younger ages are likely to act as poor proxies for permanent adult income, and thus misrepresent true rates of mobility (e.g. Blanden, 2013; Bloome et al., 2018).

in college admissions processes. I therefore exclude respondents who did not begin their postsecondary education as 4-year college enrollees ("four-year beginners"), although in the Appendix I repeat my main analyses for the full sample of high-school completers, including those who transition from a 2- to 4-year college. Second, I measure GPA using NLSY97 transcript data rather than self-reported GPA and curricular measures as a measure of academic performance, which increases confidence in the validity of my estimates.[22] Third, I construct a set of background and school characteristic variables as part of the intermediate variables $\mathbf{X}$ in my model. To recall, these variables are important for two reasons: first, they are necessary in order to plausibly estimate the effect of college attendance on earnings, which is only identified if I appropriately adjust for all confounding of the $M \rightarrow Y$ relationship. Second, these variables delineate a particular pathway through which family income is associated with college attendance, and as such play an important role in the interventions themselves. In particular, the weak secondary intervention I propose concerns equalizing college attendance within subgroups defined by both high school GPA *as well as* social-structural aspects of (dis)advantage that affect college attendance. Because I treat family income as purely a descriptive marker, we need not worry about whether such variables are strictly descendants of family income as opposed to pre-treatment. While in practice it is difficult to measure all of the components of this set of variables, the NLSY97 is advantageous as it contains a wealth of information on individuals' family and social background, making identification of the effect of college attendance on earnings more plausible. I include a range of variables that include demographic characteristics (age in 1997, gender and race), non-economic aspects of family background (parental education, which is measured using mother's years of schooling or, if missing, father's years of schooling, whether the respondent lived with both biological parents, presence of a father figure), ability, (percentile score on the ASVAB test), whether the respondent had any children by age 18, and neighborhood-, peer- and school-level characteristics such as southern or rural residence, college expectations among peers, and an indicator for whether the individual attended a private high school). I choose these variables to map as closely as possible onto the aspects of disadvantage discussed in the previous sections that are likely important pathways through which family income is

[22]One disadvantage of using high-school GPA as a measure of performance is the potential for measurement error that is correlated with family income (for example, higher-income students' GPAs may be systematically higher than lower-income students if high-schools in high-income neighborhoods are more prone to grade inflation than are lower-income schools. This might upwardly bias my estimates of secondary interventions. More standardized measures of performance, such as ACT or SAT scores, are missing for above 80% of individuals, thus invalidating their use for my empirical illustration.

associated with college attendance, net of high-school GPA. For instance, my family-level variables such as family structure and parental education serve to capture parental informational deficits and influences that are hypothesized to shape application behavior, while my neighborhood-, peer- and school-level variables are intended to capture college-related informational deficits that inhere in these environments.

In my empirical illustration, I also investigate secondary effects which might operate in terms of college quality selection; to this end, I construct a further variable among the college-goers in my sample indicating whether an individual attended a selective college by age 22. College selectivity is measured using a binary variable denoting whether the college is a "highly competitive" or "most competitive"college in Barron's Profile of American Colleges 2000. Since many respondents attended more than one college, I focus on the college in which the respondent had been enrolled for the longest time by age 29. I handle missing components of $\mathbf{X}$ and Z by multivariate imputation via chained equations, using ten imputed data sets, and adjust the standard errors of my estimates for multiple imputation using Rubin's (1987) method.

Results

Extant gaps by class background

To gain a sense of the extent of inequalities by parental income groups, Table 1 presents descriptive statistics of my analytic sample split by parental income quintile. A comparison between the lowest and highest parental income groups reveals considerable differences in conditional means across background characteristics, and educational and labor market outcomes. On all measured social background characteristics, poor children are substantially disadvantaged compared with children from high-income backgrounds. Higher-income groups are further constituted by a far greater proportion of white individuals than are lower income students. Regarding the family unit, poor children are far less likely to have been living with their biological parents, with both parents, and with parents who have a high level of education (measured in years); the difference in parental assets between the lowest and highest income groups is also particularly striking, demonstrating how economic disadvantage persists in multiple realms of upbringing. In early measures of academic attainment and advantage, poorer students also lag well behind their advantaged peers: the poorest students scored over 30 percentiles lower than the wealthiest on the ASVAB test, and further were far less likely to have attended a private high school.

Stark differences for children of different parental income brackets persist throughout young adulthood. The average credit-weighted GPA of a high school graduate from the bottom 20% of family earnings is 2.51, compared with 3.0 for individuals individuals in the top 20% of family earnings. Poorer children are also far less likely to have progressed beyond only high school education, with the poorest being 0.47 percentage points less likely to attend a 4-year college than the wealthiest; even if the poorest students do 'make it' to college, they are are less likely to have attended attended a "highly" or "most competitive" competitive college (Tiers 1 or 2 on the Barron Index of college selectivity) (13% as opposed to 58% for high-income children), less likely to have attended a college with high graduation rates upward mobility rates, and are also less less likely to have majored in a (lucrative) STEM field.

Importantly for present purposes, class origin inequalities also leave their mark on adult socio-economic outcomes. With the exception of a larger gap between the 1st and 2nd quintiles of parental income, the association between parental income and adult earnings is almost linear, with individuals from the richest socio-economic backgrounds earning on average earnings in the 61st percentile, versus the average earnings percentile of 39 of those from the bottom income quintile. In keeping with my focus on intergenerational income persistence, I focus on these percentile outcomes (specifically, of earnings) as my primary dependent variable in the analyses that follow (I also present estimates on log earnings in the Appendix), though for reference I also present average real dollars: the 61st percentile corresponds to annual earnings of $53,408, while the 39th percentile corresponds to annual earnings of $24,413. This backdrop of intense adult economic inequalities by parental income group motivates an exploration of what might happen to class-based inequalities under a series of counterfactual secondary interventions.

To highlight the stark disparities in social background characteristics, high-school performance and college attendance rates among individuals from lower and high class backgrounds that are at the heart of the interventions I have proposed above, Figure 4 displays inequalities in college transition probabilities across the entire parental income distribution, conditional on high-school GPA quintile. These residual inequalities capture the secondary "effects" of family income that my proposed interventions attack head on. We can see that, even among individuals who are equally strong performers, measured in terms of their high-school attainment, coming from a family whose income lies in the top versus bottom percentiles of the parental income distribution increases college attendance probability by at least 25 percentage points. Overall, secondary "effects" of family income appear to be linear for the majority of individuals, while for the top and bottom quantiles of high-school performers, parental income displays some interesting non-linearities. In particular, for individuals in the lowest quintile of high-school performance, college enrollment rates increase most steeply with income among individuals from the highest-income families, while for the top high-school performance, college attends seems most sensitive to income at the bottom of the socio-economic distribution. Of course, these figures do not speak to the causal effects of family income or of high-school GPA on college attendance, but they do tell us that, among students with the same high school GPA, the association between family income and college attendance is clearly still substantial. Clearly, the large gaps in educational attainment between family income groups is an important contributor to overall family income disparities in adult attainment. My focus on weak and strong secondary interventions places the role of educational attainment on overall adult disparities in income in plain view; their relative efficacy will depend on the extent to which the clear disparities in socio-demographic background characteristics mediate the association between family income and college attendance.

Table 1: Conditional means in educational, labour market and background characteristics, by family income (5 quintiles).

		Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Background	Age in 1997	15.06	15.07	14.97	14.99	14.93
Attributes	Female	0.52	0.51	0.48	0.46	0.48
	Black	0.37	0.24	0.15	0.10	0.06
	Household Net Worth	44,465	62,120	109,032	197,679	413,175
	Parental Education	11.37	11.85	12.62	13.32	14.82
	Lived with Biological Parents	0.23	0.32	0.48	0.66	0.80
	Father Figure Present	0.47	0.58	0.75	0.88	0.94
	Lived in Rural Area	0.24	0.23	0.32	0.29	0.26
	Lived in South	0.46	0.39	0.34	0.28	0.29
	Children by 18	0.13	0.08	0.07	0.03	0.01
	Substance Abuse Score	1.01	1.14	1.19	1.06	0.96
	Delinquency Score	1.47	1.50	1.51	1.26	1.05
	ASVAB Percentile	31.65	41.67	47.95	57.16	67.79
	Private High School	0.03	0.03	0.05	0.08	0.13
Labour	Annual Earnings	24,012	31,364	35,492	43,869	56,756
Market Outcomes	Log Annual Earnings	9.20	9.69	9.81	10.08	10.41
	Percentile Rank of Earnings	38.24	45.65	49.22	55.39	63.02
Educational	Credit Weighted GPA	2.45	2.61	2.70	2.89	3.11
Outcomes	Attendance	0.19	0.28	0.35	0.52	0.75
	Highly / Most Competitive College	0.10	0.17	0.24	0.37	0.63
	Stem	0.11	0.12	0.13	0.17	0.18
	Grad	0.43	0.46	0.48	0.51	0.56
	Success	0.22	0.24	0.25	0.26	0.30

Note: Quintiles 1:5 denote parental income quintiles. Conditional means are adjusted for multiple imputation via Rubin's (1987) method, and all statistics are calculated using NLSY97 sampling weights.

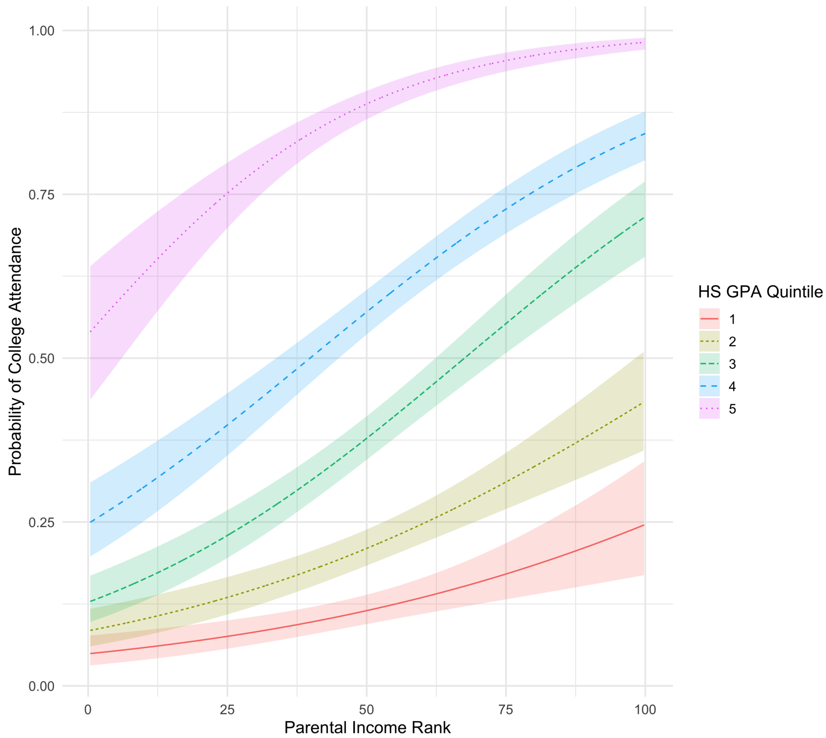


Figure 4: Predicted probability of college attendance as a function of parental income rank within high-school (HS) GPA quintile. Conditional means are fitted via a generalized additive model (GAM) which imposes a smooth functional form between college attendance and parental income rank that is allowed to differ by HS GPA quintile, adjusted by NLSY97 sampling weights. Ribbons represent 95 percent asymptotic confidence intervals.

Weak and strong secondary interventions

The leftmost panel of Table 2 presents current income gaps in adult percentile earnings rank by parental income quintiles, thus reproducing the 16th row of Table 1 above, while the second from leftmost panel documents the observed gap in adult earnings under coming from a high rather than low-income household in childhood. Extant class-based inequalities in adult socio-economic are stark: individuals coming from the lowest parental income quintile attain, on average, adult earnings in the 38th percentile, while those who were brought up in highest parental income quintile on average earn in the 63rd percentile. In other words, as shown in the second from leftmost panel, children from the richest income quintile end up approximately 24.8 percentiles higher in the income distribution on average relative to children from the lowest parental income quintile. Using the estimation strategy outlined above, I next estimate average earnings under a weak secondary intervention. Following Lundberg (2021), I interpret the interventions I consider as a local claim about hypothetical mobility rates in a small fraction of the population, rather about the whole population. Inference about the counterfactual world where we undertake such a set of interventions to college attendance will be strongest when we interpret it as a local claim about hypothetical mobility rates in a small fraction of the population, rather about the whole population.[23] Columns 3 to 6 of Table 2 report the results of this exercise, showing, in turn, the counterfactual counterparts of columns 1 to 2, as well as the the percentage reduction in the observed gap in earnings between high and all other parental income groups, as well as the increase in earnings under the counterfactual intervention. Across all parental income quintiles, the results suggest that expected income under a weak secondary intervention would be virtually unchanged - a result of both the very minor shifts in expected earnings under such a intervention, as well as the imprecision in the respective estimates. In fact, the largest shift in earnings to be expected under this intervention is an increase by 0.2 percentiles for those individuals coming from the third parental income quintile - a shift which is, however, statistically insignificant.

Next, I estimate the counterfactual expected earnings among different parental income quintiles under the strong secondary intervention. Table 3 echoes the structure of the results presented in Table 2, with the first two columns presenting observed income and gaps (versus the fifth parental income quintile), the next two columns showing the counterfactual earnings under this intervention, and the final two displaying the percent reduction in the observed income gap and increase in average earnings per quintile group, respectively.

As the results demonstrate, the strong secondary intervention on average compresses the earnings distribution to a far greater extent than the weak secondary intervention. Increases in earnings are noticeably larger for individuals in the 3rd and 4th parent income quintiles under the strong rather than weak secondary intervention, although estimation uncertainty does not allow us to conclude that these estimates are distinguishable from zero. Most strikingly, counterfactual earnings between the fifth and second quintiles decreases dramatically under a strong secondary intervention: individuals from this group would expect in excess of a 4 percentile point increase in their adult expected earnings in this hypothetical world, a statistically significant increase, and one that closes the gap in expected earnings rank almost entirely between the second and third quintiles.

[23] As Lundberg (2021) notes, this latter claim is often relevant from the perspective of policymakers, who cannot intervene on the whole population at once (p.12). Moreover, secondary interventions can readily be conceptualized at the school-level: one could imagine admissions policies or outreach programs changing at a set of schools, in which case the counterfactual mobility rates I estimate would apply to the subpopulations attending these particular schools.

This finding is interesting, especially when one considers the fact that individuals from the lowest income quintile are most disadvantaged across the vast majority of socio-demographic advantage indicators reported in Table 1.

In summary, my results accord with the expectations outlined in the previous sections, namely that weak secondary interventions that target directly the choice component of educational decision-making are far less effective at promoting intergenerational mobility rates than strong interventions that disrupt the cluster of additional mechanisms through which social background affects college attendance, net of demonstrated performance. As shown in Appendices C, D and E, this pattern is replicated across an alternative definition of intergenerational mobility (employing the intergenerational elasticity coefficient, IGE, which is the coefficient from a regression of log adult earnings on log parental income), alternative sample restrictions including the inclusion of 4-year transfer students, as well as across samples employing alternative cutoffs for college attendance.

Table 2: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.20*** (1.12)	-24.83*** (1.47)	38.02*** (1.17)	-25.00*** (1.50)	-0.69%	-0.17 (0.38)
2	45.65*** (1.01)	-17.38*** (1.37)	45.60*** (1.07)	-17.42*** (1.40)	-0.25%	-0.04 (0.39)
3	49.22*** (0.96)	-13.80*** (1.36)	49.42*** (1.00)	-13.61*** (1.37)	1.42%	0.20 (0.35)
4	55.39*** (0.97)	-7.64*** (1.37)	55.39*** (0.98)	-7.63*** (1.36)	0.07%	0.01 (0.22)
5	63.03*** (0.92)		63.03*** (0.92)			-0.00 (0.00)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 3: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.20*** (1.12)	-24.83*** (1.47)	37.99*** (2.94)	-24.50*** (3.19)	1.32%	-0.20 (2.80)
2	45.65*** (1.01)	-17.38*** (1.37)	49.73*** (1.89)	-12.77*** (2.12)	26.51%	4.08** (1.68)
3	49.22*** (0.96)	-13.80*** (1.36)	49.46*** (2.15)	-13.04*** (2.36)	5.56%	0.24 (1.96)
4	55.39*** (0.97)	-7.64*** (1.37)	55.87*** (1.46)	-6.63*** (1.90)	13.25%	0.48 (1.12)
5	63.03*** (0.92)		62.50*** (1.10)			-0.53 (0.63)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

A robustness check: educational match

The traditional primary and secondary effects literature is concerned primarily with educational attainment defined in terms of educational access - that is, with respect to attendance at any particular educational level - and particularly for the transition to middle to high-school and from high-school to college. Such a focus is of course justified by the conceptual scope of this literature, which is concerned with differential class-based actions as they pertain to decision-making about continuing versus dropping out of education at any given level. Nevertheless, when we recast the primary and secondary effects literature in an interventional contexts, such a focus may be unsatisfactory for several reasons.

Decision-making about enrollment at any given level of education concerns not only a binary decision of whether to transition but also a qualitative one concerning the type of education that may be pursued. Amidst higher educational expansion in the US, graduates must increasingly choose whether to enter the labour market or to enroll in post-graduate education. Increasingly, therefore, educational attainment in the US has become a field of multiple levels with sequential transitions, all of which are independently consequential for individuals' labour market outcomes. Research on such 'matching' of individuals to colleges has shown that disadvantaged students tend to be undermatched. Despite the fact that their performance would permit such attendance, high-achieving, low-income rarely apply to selective colleges compared with their higher-income peers, despite the fact that attendance at such institutions would cost them less on net due to their generous funding packages and financial aid programs (Hoxby and Avery, 2012), as well as the fact that selective colleges are often found to benefit lower-income students more. Thus, any intervention to individuals' decisions about entry must account for how this intervention might affect matching processes. Theoretically, it is plausible that secondary effects operate in the matching process, to the extent that more selective colleges pose a greater cost to disadvantaged students and a greater benefit for upper-class individuals who seek to maintain their parents' high class position (and recognize that attending an elite school is a necessary requirement for accessing elite professional employment), and beyond college education in the postgraduate application process. However, despite recognition of the sustained importance of secondary effects for the matching process in recent empirical research (Bukodi et al., 2021), research on secondary effects has exclusively doused on one transition at a time. Recasting primary and secondary effects in an interventional framework begs the question: what would happen if we disrupted multiple secondary effects that operate in creating disparities in educational transitions and attainment throughout individuals' educational careers. [24] These insights suggest that, once we recast primary and secondary effects within an interven-

[24]Moreover, the sequential and cumulative nature of educational decisions/actions and inequalities extends of course to completion at a given level. The interventions I consider concern college

tional approach, it makes sense to consider interventions to both college attendance and college selectivity. Identification and estimation is very similar to the case of a a single secondary intervention to college attendance, though now we must fit several models for both college attendance and selectivity, and multiply the propensity scores (see Appendix A for further details).

Tables 4 and 5 show the results from a series of two secondary interventions to college attendance and college selectivity, where selectivity is defined as an indicator for whether the college attended is "highly" or "most competitive" competitive college on the Barron Index of college selectivity. Table 4 presents counterfactual earnings under a weak intervention to college attendance and both a weak (top panel) and strong (bottom panel) intervention to college selectivity, while Table 5 presents counterfactual earnings under a strong intervention to college attendance and both a weak (top panel) and strong (bottom panel) intervention to college selectivity.

From the first panel of Table 4, we see that a series of interventions designed to target income-related cost-benefit analyses regarding both individuals' decision to attend college *and* their decision to attend a (non-)selective college have little additional effect in compressing gaps in expected earnings compared with a sole weak intervention to college attendance. By contrast, a weak secondary intervention to college attendance, when coupled with a strong intervention to college selectivity, enhances the mobility-promoting potential of the weak secondary intervention at the attendance level, increasing expected earnings rank by at least half a percentile among the bottom four parent income quantiles (though, due to the small effect sizes, none of the differences in earnings are statistically distinguishable from zero). Turning to Table 5, from the first panel we see that a series of strong and weak secondary interventions to college attendance and selectivity, respectively, has much a similar effect on earnings gaps as a singular strong intervention to attendance. Strikingly, however, when we examine the bottom panel of this table, we see the enormous mobility-enhancing potential of a strong secondary intervention to both attendance and selectivity. In particular, such an intervention reduces the expected earnings gap between each and every parental income quintile group and the top income group, although due to the large standard errors of the counterfactual

enrollment, rather than completion. Much work has shown vast class- and race-based inequalities in college completion, and to the extent that many of the low-income individuals 'sent' to college under the interventions I propose subsequently drop out (see Ciocca Eller and DiPrete, 2018), the strong and secondary policies could be deemed relatively inefficient. While this is not my primary concern in this paper, it is also reasonable and important to ask how disrupting secondary effects on attainment would promote mobility if we further intervened to reduce inequalities in college completion rates - or if the inequality enhancing potential of secondary interventions might be reduced by failure to intervene at later stages.

earnings estimates, the majority are statistically insignificant. A lack of significance does not, however, imply no mobility-enhancing potential of a a series of strong secondary interventions, and it is of particular note that, under such an intervention, expected earnings gaps compared with the top income quintile are reduced by at least 10% for all groups. Especially striking is the fact that the gap between children from the second and fifth quintiles is reduced by nearly 40%, entailing a (statistically) significant increase of over 6 percentile points, constituting a 49% increase in expected earnings percentile relative to a singular strong intervention to college attendance.

Overall, these findings regarding the role of college selectivity in the intergenerational process underscore my earlier findings of the greater mobility-enhancing role of strong secondary interventions at any given educational transition, and further point to (a) the potentially more important role of weak secondary effects at the level of college selectivity, rather than of attendance, in fueling intergenerational inequalities, compared with (b) the potentially more important role of strong secondary effects at the level of college attendance in driving intergenerational persistence.

Table 4: Expected adult log income and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
	Weak (Attendance) and Weak (Selectivity)					
1	38.20*** (1.12)	-24.83*** (1.47)	38.07*** (1.17)	-24.95*** (1.50)	-0.50%	-0.12 (0.41)
2	45.65*** (1.01)	-17.38*** (1.37)	45.60*** (1.07)	-17.42*** (1.40)	-0.26%	-0.05 (0.39)
3	49.22*** (0.96)	-13.80*** (1.36)	49.40*** (1.00)	-13.63*** (1.37)	1.26%	0.17 (0.35)
4	55.39*** (0.97)	-7.64*** (1.37)	55.38*** (0.98)	-7.64*** (1.36)	-0.06%	-0.00 (0.23)
5	63.03*** (0.92)		63.03*** (0.92)			-0.00 (0.00)
	Weak (Attendance) and Strong (Selectivity)					
1	38.20*** (1.12)	-24.83*** (1.47)	38.74*** (1.56)	-23.79*** (2.05)	4.18%	0.54 (1.12)
2	45.65*** (1.01)	-17.38*** (1.37)	46.31*** (1.24)	-16.21*** (1.83)	6.69%	0.66 (0.77)
3	49.22*** (0.96)	-13.80*** (1.36)	50.08*** (1.19)	-12.45*** (1.73)	9.82%	0.86 (0.78)
4	55.39*** (0.97)	-7.64*** (1.37)	55.94*** (1.10)	-6.59*** (1.69)	13.75%	0.55 (0.60)
5	63.03*** (0.92)		62.53*** (1.30)			-0.50 (0.96)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 5: Expected adult log income and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
	Strong (Attendance) and Weak (Selectivity)					
1	38.20*** (1.12)	-24.83*** (1.47)	37.97*** (2.94)	-24.52*** (3.19)	1.24%	-0.22 (2.80)
2	45.65*** (1.01)	-17.38*** (1.37)	49.76*** (1.89)	-12.74*** (2.12)	26.71%	4.11** (1.68)
3	49.22*** (0.96)	-13.80*** (1.36)	49.43*** (2.15)	-13.07*** (2.36)	5.33%	0.21 (1.96)
4	55.39*** (0.97)	-7.64*** (1.37)	55.86*** (1.46)	-6.64*** (1.89)	13.12%	0.47 (1.12)
5	63.03*** (0.92)		62.50*** (1.10)			-0.53 (0.63)
	Strong (Attendance) and Strong (Selectivity)					
1	38.20*** (1.12)	-24.83*** (1.47)	40.43*** (4.16)	-21.87*** (4.75)	11.91%	2.23 (4.06)
2	45.65*** (1.01)	-17.38*** (1.37)	51.71*** (2.50)	-10.58*** (3.22)	39.09%	6.07** (2.38)
3	49.22*** (0.96)	-13.80*** (1.36)	50.75*** (2.36)	-11.54*** (3.03)	16.36%	1.53 (2.22)
4	55.39*** (0.97)	-7.64*** (1.37)	56.48*** (1.86)	-5.82 ** (2.77)	23.77%	1.09 (1.66)
5	63.03*** (0.92)		62.30*** (2.05)			-0.73 (1.89)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Conclusion

Boudon's fundamental recognition of the distinction between two different class-based mechanisms that may lead to inequality of educational inequalities by family background has led to a sustained effort among sociologists to test empirically the role of both primary and secondary effects in observed attainment gaps in education. Yet, empirical research on assessing the existence of primary and secondary effects has often been inattentive to the multiple causal and non-causal pathways that characterize attainment inequality, net of demonstrated ability, and has further run on parallel tracks to both research on assessing the degree of intergenerational (im)mobility, as well as experimental research on socio-economic constraints on educational decision-making. As a result, despite pretense to policy relevance of empirical research on secondary class effects, little is known about the true mobility-enhancing potential of policy interventions to secondary class effects.

To fill this gap, in this article I have established a framework to assess the contribution of secondary effects to intergenerational persistence, one which takes issues of causal identification the task of adjudicating between alternative explanations of secondary effects more seriously than present work. Specifically, my proposed framework formalizes how class effects on an educational transition, net of academic performance, may contribute to intergenerational mobility, and, by extension, how policy interventions to these effects may disrupt intergenerational persistence, by distinguishing between two types of 'secondary interventions': what I label 'weak' and 'strong' secondary interventions, both of which can be interpreted as a field-experimental ideal. After theoretically distinguishing between the distinct set of mechanisms that likely underpin secondary effects on attainment, I proposed a mathematical formalization of my framework that integrates these interventions, as well as additional causal and descriptive estimands that have been considered in the literature on the relationship between educational inequality and intergenerational persistence, under a common model. Both the theoretical arguments about the existence of alternative mechanisms underpinning the secondary effect and my mathematical formalization imply that strong secondary interventions are likely to be most effective with regard to the promotion of social mobility. Finally, I showed how my framework can be identified with observational data, proposed a weighting strategy that can be used for estimation in practice, and applied it to a nationally representative dataset. Empirical results provide support for the limitations of secondary effects with regard to the overall promotion of mobility.

My secondary interventions framework contributes to literature on sociology of education, intergenerational mobility on theoretical, empirical and methodological fronts. First, and theoretically, I have sought to improve upon the original primary-secondary effects theory by both challenging the oft-taken assumption about the interpretation of observed secondary effects, and by showing how debate about the role of mechanisms in producing educational inequalities can be conceptualized in terms of their role in intergenerational mobility more broadly. Second, my proposed framework, though fundamentally defined as theoretical estimands, in terms of hypothetical interventions at the population level, can be combined with a set of identification assumptions in order to be examined empirically using observational data. Empirical evidence confirms that weak secondary interventions - that is, those predicated on disrupting the cost-benefit mechanism traditionally assumed to underpin secondary effects - does far less for intergenerational mobility than would be assumed under the traditional framework. Moreover, my empirical analyses are the first to reveal the contributions of cumulative secondary effects in the intergenerational mobility process for this cohort in the United States: my results suggest that the weak and strong secondary effects may have comparatively larger or weaker effects at different educational transition points; weak secondary interventions appear to have greater mobility-enhancing potential at the level of college selectivity rather than of college attendance, while the opposite is true for strong secondary interventions.

Third, in recent decades, the sociological community, and social sciences more generally, has become increasingly interested in understanding the micro-level foundations of macro-level social phenomena. This focus on distilling the mechanisms underpinning various phenomena has garnered widespread theoretical attention, although arguably this theoretical impetus has not been matched by methodological rigor. In particular, typical approaches to mechanism-adjudication, for instance by comparing coefficients on models with and without putative mediators, rely on strong and likely implausible assumptions about functional-form and identification. Identifying the secondary effect is a case in point. In this paper, I have shown how a series of stochastic interventional estimands can inform the debate on mechanism adjudication. To my knowledge, this is the first study to use the interventional framework to test for the existence, and significance of distinct mechanisms underpinning observed social phenomena. By examining the case primary and secondary effects, this study provides future researchers with a methodological framework explicitly aimed at testing the extent to which micro-level processes give rise to broader patterns of inequality.

My secondary effects framework represents a first attempt to adjudicate between alternative mechanistic explanations for the existence of secondary class effects on educational attainment. Clearly, much more work needs to be done; in the following, I offer three suggestions for future work aimed at employing and extending my secondary effects interventional framework.

First, while my empirical analyses provide preliminary evidence in support of the higher mobility-enhancing potential of secondary effects, the proposed framework would benefit from more exacting empirical analyses characterized by larger sample sizes. Such an analysis would then facilitate the examination of subgroup-specific secondary effects. One such point of departure could be differences across racial-subgroups. In particular, it has been noted in several national contexts that secondary class effects appear to be weaker for members of minority racial-ethnic groups. This may result from several factors. On the one hand, ethnic minority groups may perceive themselves as requiring a higher level of qualification for attainment of a particular class position since they expect levels of ethnic discrimination in the labor market. Alternatively, given the significant obstacles that Black parents would have faced to make it to a similar class position as that of their white counterparts, we might expect Black parents with a particular income level to be more positively selected on attributes salient for their children's educational decision-making than whites. When reframed in terms of my proposed framework, one might therefore expect the set of pathways from family income to college attendance, net of both high school GPA and intermediate variables, to be weaker for Blacks than whites, and thus for a weak secondary intervention to be more effective for whites than Blacks. On the other hand, since Black children, on average, suffer from a broader set of structural disadvantages as compared with their white peers, even within the same income level - such as growing up in an impoverished neighborhood, being surrounded by lower-income peers and attending lower-performing high schools (e.g. Wodtke et al., 2011; Sampson, 2012), it is plausible that the composite set of paths from family income to college attendance through both this direct path *and* through intermediate variables is stronger effect for Blacks than whites, and thus, that a strong secondary intervention, which intervenes to block the composite path from family income to college attendance via (i) a class-based cost-benefit calculus and (ii) structural factors encapsulated in the set of intermediate variables, blocks a set of pathways that are presently more constraining for Blacks than for whites. Of course, whether these theoretical predictions in fact play out is a matter for future empirical investigation.

Second, a prominent theoretical concern surrounding concepts of primary and secondary effects relates to how to include what Erikson et al. (2005) label 'unobserved early choice components'. Individuals may make an (unobserved) anticipatory decision about a future educational transitional point some years before the actual transition occurs, and this anticipatory decision structures academic performance in the run-up to the transition: if students anticipate that they will not make an educational transition, they may then decide to 'work less hard and subsequently achieve a lower level of performance' (Jackson 2013: 17). There is, therefore, a further causal pathway not included in Jackson's model whereby social origin affects university admissions through an anticipated decision which impacts on academic performance (primary effects) – and which in turn impacts on differential college admissions rates. To the extent that such anticipatory decision-making is common behavior, ignoring the influence of anticipatory decisions is likely to downwardly bias the estimate of the secondary effect and upwardly bias the estimate of the primary effect – and thus underestimate the relative importance of secondary effects. To test how anticipatory decisions influence the relative importance of primary and secondary effects in class differentials in university admissions, research on primary and secondary effects has traditionally compared estimates derived using different measures of primary effects: while measures of performance taken close to the decision might be 'contaminated' by anticipatory decisions, measures of academic ability taken well before an educational decision is likely to be less prone to such 'contamination'. Such analyses have generally supported the existence of anticipatory decision-making. Future research using my framework could employ a similar strategy, or even develop form of sensitivity analysis to compare interventional distributions that do and do not account for anticipatory decision-making.

A Identification and Estimation of Equation 3

In this section, I offer formal identification proofs of the 'weak' and 'strong' secondary interventions I propose, as well as proofs of the estimation strategies I undertake in the main text. Let A, X, Z, M and Y be as in the main text. Weak and strong interventions are identified under the assumptions of consistency, ignorability and positivity:

1. consistency: for any unit, if $M = m$, then $Y = Y(m)$;
2. ignorability for $M - Y$: $Y(m) \perp M | A, X, Z_1, Z_2$;
3. positivity: $p_{M|A,\mathbf{X},Z}(m|a, \mathbf{x}, z) > \varepsilon > 0$.

Under these assumptions, first, for the 'weak' secondary intervention among low-income individuals, $\mathbb{E}[Y(\mathcal{M}_{|\mathbf{X},Z,a})|A = a^*]$, we have:

$$
\begin{aligned}
&\mathbb{E}[Y(\mathcal{M}_{|\mathbf{X},Z,a})|A = a^*] \\
&= \int \mathbb{E}[Y(\mathcal{M}_{|\mathbf{x},z,a})|A = a^*, \mathbf{X} = \mathbf{x}, Z = z] dP(z|A = a^*, \mathbf{X} = \mathbf{x}) dP(\mathbf{x}|A = a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, \mathcal{M}_{|\mathbf{x},z,a} = m)] dP(\mathcal{M}_{|\mathbf{x},z,a} = m|A = a^*, \mathbf{X} = \mathbf{x}, Z = z) dP(z|A = a^*, \mathbf{X} = \mathbf{x}) dP(\mathbf{x}|A \\
&= \int \mathbb{E}[Y(m)|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, M = m] dP(m|\mathbf{X} = \mathbf{x}, Z = z, A = a) dP(z|A = a^*, \mathbf{X} = \mathbf{x}) dP(\mathbf{x}|A = a^*) \\
&= \int \mathbb{E}[Y|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, M = m] dP(m|\mathbf{X} = \mathbf{x}, Z = z, A = a) dP(z|A = a^*, \mathbf{X} = \mathbf{x}) dP(\mathbf{x}|A = a^*)
\end{aligned}
$$

Second, for the 'strong' secondary intervention among low-income individuals, $\mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a^*]$, we have:

$$
\begin{aligned}
&\mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a^*] \\
&= \int \mathbb{E}[Y(\mathcal{M}_{|z,a})|A = a^*, \mathbf{X} = \mathbf{x}, Z = z] dP(\mathbf{x}, z|A = a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, \mathcal{M}_{|z,a} = m] dP(\mathcal{M}_{|z,a} = m|A = a^*, \mathbf{X} = \mathbf{x}, Z =) dP(\mathbf{x}, z|A = a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, \mathbf{X} = \mathbf{x}, Z = z] dP(m|Z = z, A = a) dP(\mathbf{x}, z|A = a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, M = m] dP(m|Z = z, A = a) dP(z|A = a^*, \mathbf{X} = \mathbf{x}) dP(\mathbf{x}|A = a^*) \\
&= \int \mathbb{E}[Y|A = a^*, \mathbf{X} = \mathbf{x}, Z = z, M = m] dP(m|Z = z, A = a) dP(z|\mathbf{X} = \mathbf{x}, A = a^*) dP(\mathbf{x}|A = a^*)
\end{aligned}
$$

Identification for high-income individuals follows straightforwardly. Note the key distinction between this quantity and the identification result for the weak intervention considered above is simply that the cumulative distribution function (CDF) for college attendance in the strong secondary intervention lacks intermediate variables in the conditioning set, reflecting the fact that this intervention breaks the pathway from family income to college attendance via intermediate variables, while the weak intervention does not. Note additionally the relationship between parental income and $\mathbf{X}$ (intermediate variables) and Z (high school GPA) is preserved in both types of intervention. To provide an estimator for the weak and strong secondary interventions that avoids estimation of multiple densities, which may be multivariate or continuous, we can rewrite the integrals featuring in the identification formulae for weak and strong interventions given above using Bayes' rule, letting $\tilde{P}(m|a, \mathbf{x}, z)$ represent the interventional distribution of interest (i.e. the probability function representing either the weak or strong secondary intervention):

$$
\begin{aligned}
&\int \mathbb{E}[Y|a^*, \mathbf{x}, z, m] d\tilde{P}(m|a, \mathbf{x}, z) dP(z|\mathbf{x}, a^*) dP(\mathbf{x}|a^*) \\
&= \int \frac{y dP(y, \mathbf{x}, z, m|a) d\tilde{P}(m|a, \mathbf{x}, z) dP(z|a, \mathbf{x}) dP(\mathbf{x}|a) dP(a)}{dP(z|a, \mathbf{x}) dP(\mathbf{x}|a) dP(a)} \\
&= \int y dP(y, \mathbf{x}, z, m|a) \frac{d\tilde{P}(m|a, \mathbf{x}, z)}{dP(m|a, \mathbf{x}, z)} \\
&= \mathbb{E}\left[Y \frac{\tilde{f}(M|a, \mathbf{X}, Z)}{f(M|a, \mathbf{X}, Z)} |a \right].
\end{aligned}
$$

This expression suggests the method-of-moments estimator $\mathbb{P}_n \left[Y \frac{\tilde{f}(M|a, \mathbf{X}, Z)}{f(M|a, \mathbf{X}, Z)} |a \right]$, where $\mathbb{P}_n[\cdot] = n^{-1} \sum_i [\cdot]$.

To provide identification and estimation results for secondary interventions to both college attendance and college selectivity, we can adapt the notation introduced in the main text to write any stochastic assignment rule that randomly allocates individuals to any series educational attainments as a function of observed characteristics. As before, let $\tilde{P}(m|\mathbf{g})$ denote the cumulative distribution function (CDF) of attainment of a given level of education among those with observed characteristics $\mathbf{G} = \mathbf{g}$, and $\mathcal{M}_{|\mathbf{g}}$, a random draw from this distribution. Now, for any K educational transitions for which we seek to define a series of secondary interventions, denote a vector of interventional treatments by $\overline{M} = \{M_1, \dots M_K\}$ and $\overline{\mathcal{M}}_{|\overline{\mathbf{g}}}$ denote a random draw from the product of CDFs. Generally, then, we have the following formula for expected earnings in group $A = a^*$ under series of counterfactual secondary interventions:

$$\overline{\psi}_{a,a_1} = \mathbb{E}[Y(\overline{\mathcal{M}}_{|\overline{a}})|A = a^*], \tag{5}$$

which suggests a weighting estimator similar to that proposed in Equation 4:

$$\hat{\overline{\psi}}_{a,a_1} = \mathbb{P}_n\left[\frac{Y[\prod_{j=1}^{J}\tilde{P}_a(M_j = m_j)/\prod_{j=2}^{J}P(M_j = m_j|m_{j-1}, a_1, Z, X)dP(Z, X|a_1)]}{\sum[\prod_{j=1}^{J}\tilde{P}_a(M_j = m_j)/\prod_{j=2}^{J}P(M_j = m_j|m_{j-1}, a_1, Z, X)dP(Z, X|a_1)]}|a_1\right]. \tag{6}$$

One can generalize the above to the case of secondary interventions across multiple educational transitions. As before, let $P(M|u)$ denote the cumulative distribution function (CDF) of attainment of a given level of education among those with observed characteristics u, and $\mathcal{M}_{|u}$, a random draw from this distribution. Now, denote a vector of interventional treatments by $\overline{M}_K = \{M_1, \ldots M_K\}$ and $\overline{\mathcal{M}}_{|u}$ denote a random draw from the product of CDF; similarly, denote a vector of intermediate confounders for the k-th transition by $\overline{Z}_K = \{Z_1, \ldots Z_K\}$ a Generally, then, for an arbitrary set of weak and strong secondary interventions, we can write:

$$\begin{aligned}
&\mathbb{E}[Y(\overline{\mathcal{M}}_{|u}|A = a^*] \\
&= \int \mathbb{E}[Y(\overline{\mathcal{M}}_{|u})|A = a^*, X = x, \overline{Z}_k = \overline{z}_k]\prod_{k=1}^{K} dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, X = x, \overline{Z}_k = \overline{z}_k, \overline{\mathcal{M}}_{|u} = \overline{m}] \\
&\cdot \prod_{k=1}^{K} d\tilde{P}(\overline{\mathcal{M}}_{|u} = \overline{m}|A = a^*, X = x, \overline{Z}_k = \overline{z}_k)dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, X = x, \overline{Z}_k = \overline{z}_k]\prod_{k=1}^{K} d\tilde{P}(\overline{\mathcal{M}}_{|u} = \overline{m})dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*) \\
&= \int \mathbb{E}[Y(m)|A = a^*, X = x, \overline{Z}_k = \overline{z}_k, \overline{M}_K = \overline{m}_K]\prod_{k=1}^{K} d\tilde{P}(\overline{m}_k)dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*) \\
&= \int \mathbb{E}[Y|A = a^*, X = x, \overline{Z}_k = \overline{z}_k, \overline{M}_K = \overline{m}_K]\prod_{k=1}^{K} d\tilde{P}(\overline{m}_k)dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*).
\end{aligned}$$

It is then straightforward to show that the above quantity can be expressed in terms of ratios of counterfactual to observed mediator distributions, namely,

$$\int \mathbb{E}[Y|A = a^*, X = x, Z_k = z_k, M_k = m_k]\prod_{k=1}^{K} d\tilde{P}(\overline{m}_k)dP(z_k|m_{k-1}, z_{k-1}, x, A = a^*)dP(x|a^*)$$

$$= \mathbb{E}\left[Y \frac{\prod_{k=1}^{K} \tilde{f}(\overline{m}_k)}{\prod_{k=1}^{K} f(m_k|z_k, m_{k-1}, x, A = a)} |a^*\right],$$

where $\tilde{f}(\overline{m}_k)$ denotes a series of arbitrary interventional distributions for college attendance, selectivity, completion, etc.

B Two Forms of Strong Secondary Intervention

In the main text, I define a strong secondary intervention as the intervention that breaks all pathways from family income to college attendance net of high-school GPA, and imply that this is applicable to all individuals, irrespective of parental income status. However, in the following I discuss how it is in fact possible to further subdivide the strong form of intervention into two forms, which I call an AA-strong intervention and uniform-strong intervention, in a way that it is not possible to subdivide the weak intervention.

The first refers to cases where the strong intervention is applied to *both* high and low-income groups, while the second captures instances where the strong intervention is applied *only* to low-income groups. The latter quantity corresponds to an intervention targeted solely to alter the proportion of lower income children attending college while not altering admissions policies or behavior for high-income children, and as such represents a form of class-based affirmative action (AA). I refer to the former uniformally-applied intervention as the 'uniform-strong' intervention, and the latter as the 'AA-strong' intervention.

As a result, while in the main text I focus on a single contrast that captures the residual disparity after a strong secondary intervention, $\Delta^{\text{strong}}_{a,a^*} \triangleq \psi^{\text{strong}}_{a,a^*} - \psi^{\text{strong}}_{a,a^*}$, this quantity can in fact be captured by two contrasts:

$$
\begin{aligned}
&(a) \quad \mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a] - \mathbb{E}[Y(\mathcal{M}_{|Z,a^*})|A = a^*] \\
&(b) \quad \mathbb{E}[Y|A = a] - \mathbb{E}[Y(\mathcal{M}_{|Z,a})|A = a^*],
\end{aligned}
$$

which represent, respectively, (a) the expected disparity over repeated samples in income between high and low parental income groups after the strong intervention is applied to *both* high and low-income groups (the 'uniform-strong' intervention), and (b) the expected disparity over repeated samples in income between high and low parental income groups after the strong intervention is applied *only* to low-income groups (the 'AA-strong' intervention). The latter quantity corresponds to an intervention targeted solely to alter the proportion of lower income children attending college while not altering admissions policies or behavior for high-income children, and as such represents a form of class-based affirmative action (AA).

There is a subtle, but important, distinction between a strong secondary intervention that applies to all individuals and one that is only targeted towards lower income groups. To appreciate this distinction, let Δ^{tot} denote $\mathbb{E}[Y(1) - Y(0)|A = a^*]$ - that is, the conditional average treatment effect (CATE) of college attendance among individuals from advantaged class backgrounds. We can then write observed expected earnings among individuals from advantaged social origins as

$$\begin{aligned}
\mathbb{E}[Y|A = a^*] =& \mathbb{E}[Y(0)|A = a^*] + \mathbb{E}[M \cdot \Delta^{\text{tot}}|A = a^*] \\
=& \mathbb{E}[Y(0)|A = a^*] + \int \mathbb{E}[M \cdot \Delta^{\text{tot}}|A = a^*, Z = z] dP(z|a^*) \\
=& \int \Big[\mathbb{E}[Y(0)|A = a^*, Z = z] + \mathbb{E}[\Delta^{\text{tot}}|A = a^*, Z = z] \cdot \Pr(M = 1|A = a^*, Z = z) \\
& + \text{cov}[\Delta^{\text{tot}}, M|A = a^*, Z = z] \Big] dP(z|a^*) \\
=& \psi^{\text{strong}}_{a^*,a^*} + \mathbb{E}\Big[\text{cov}[\Delta^{\text{tot}}, M|A = a^*, Z]|A = a^* \Big]. \qquad (7)
\end{aligned}$$

In other words, a strong intervention that does not target higher-income groups means expected earnings among this group is equal to the sum of a strong secondary intervention that targets this group and the expectation of the covariance between the effect of college attendance and selection into college attendance; a strong intervention that targets this group removes this selection component due to random assignment of college attendance. In short, in the case of the strong secondary intervention, only the 'AA-strong' interventional estimand reduces to the average observed outcome among high-income children, while the 'uniform-strong' intervention does not. By contrast, there is only one possible variation of the weak secondary intervention among high-income groups, because observed earnings among high-income groups is in fact equal to counterfactual earnings among high-income groups under a weak secondary intervention. These distinctions are summarized in Figure 5. [25]

[25] This is because, by iterated expectations as well as the ignorability assumption $Y(m) \perp\!\!\!\perp M|A, X, Z$, $\mathbb{E}[Y|A = a] = \int \mathbb{E}[Y|A = a, x, z, m] dP(m|A = a, x, z) dP(x, z|a) = \int \mathbb{E}[Y(m)|A = a, x, z, m] dP(m|A = a, x, z) dP(x, z|a) = \psi^{\text{weak}}_{a,a}$.

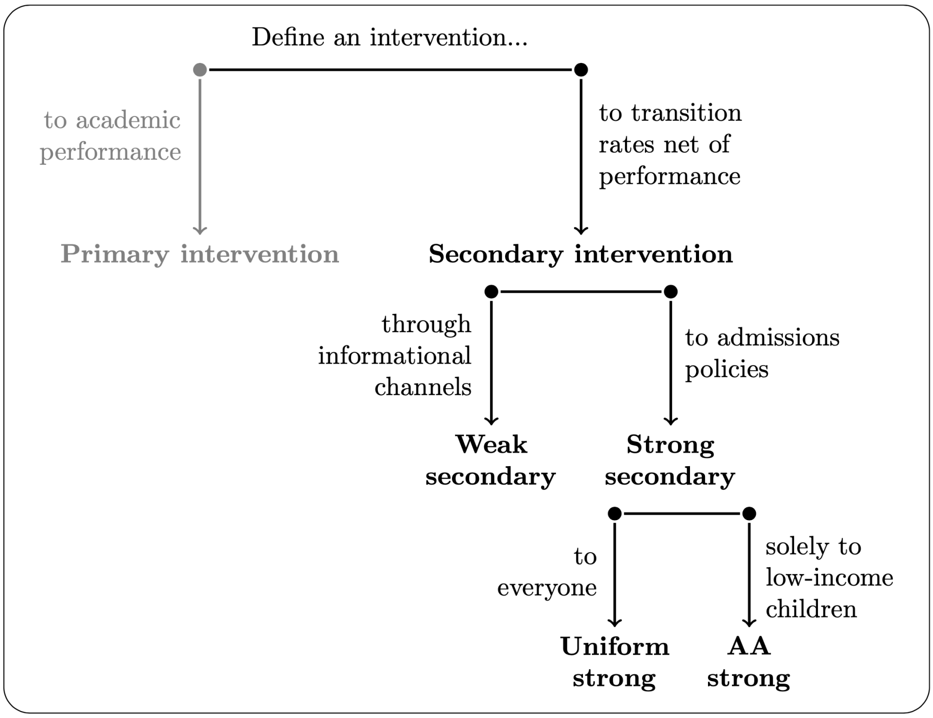


Figure 5: Secondary interventions can be further divided into two types: (a) those that only block the direct pathway from parent income to college attendance net of both performance and intermediate variables such as school, neighborhood and other family characteristics (a weak secondary intervention), and (b) those that block the composite path from parent income to college attendance comprising both the direct effect in (a) and the path through intermediate variables. Whether this admissions policy applies to everyone or just to disadvantaged children underpins the final distinction between 'uniform-strong' and 'AA-strong' interventions.

In light of this distinction, we can re-evaluate the empirical results in the main text. The strong intervention I estimate in the main text (Table 3) is the "uniform strong intervention", which applies the intervention indiscriminate of class background (i.e., which imposes the distribution of college attendance conditional on high-school GPA observed among high-income children among *all* individuals, rather than just those individuals from non-high-income backgrounds). The alternative "AA-strong" intervention, which would only apply the intervention to low-income children, is omitted since the only difference from the uniform strong intervention results is the expected income rank among high-income students, which would be identical to the observed value of 63.03.

Of further note is the fact that, under a "uniform strong intervention", expected earnings rank among incomes from the top income quintile would possibly decrease (disregarding for a moment questions of estimation uncertainty), a decrease which is in fact, point-wise, larger than any of the expected changes in counterfactual earnings under a weak secondary intervention. This curious finding likely results from the result shown in Equation 7, namely that the counterfactual earnings among high-income groups under a strong intervention is equal to the observed earnings among this group minus the (expectation of) the covariance between college attendance and the effect of college attendance on earnings among this high income group, i.e. $\mathbb{E}[Y|A = a^*] - \mathbb{E}\Big[\text{cov}[\Delta^{\text{tot}}, M|A = a^*, Z]\Big]$, where Δ^{tot} denotes $\mathbb{E}[Y(1) - Y(0)|A = a^*]$, the conditional average treatment effect (CATE) of college attendance among individuals from advantaged class backgrounds). From this, we can deduce that this covariance term is positive - in other words, that, on average, those students from advantaged backgrounds who would benefit more from college attendance in terms of adult earnings are more likely to attend college than students from advantaged backgrounds who would benefit less from college attendance, even among individuals who have the same high-school academic performance. High income individuals, it would appear, have access to information about individual payoffs to college attendance, and use this information to inform college attendance decisions. In consequence, the uniform strong intervention that applies the strong intervention to all students regardless of class background would in fact serve to decrease earnings gaps overall, and more so than the AA-strong intervention that only intervenes on individuals from low-income backgrounds.

C Results on Intergenerational Income Elasticity (IGE)

In the main text, I examined the effects of weak and strong secondary interventions mobility on intergenerational mobility, using the rank-rank slope. The causal interpretation of the resulting estimates may, however, be criticized on account of their breaking the stable unit treatment value assumption (SUTVA; Rubin 1986): an individual's earnings rank is a function of both their own earnings and the earnings of everyone else, it depends on the educational attainment of themselves and others in the population. Alternatively, therefore, we can examine secondary interventions from the perspective of intergenerational income elasticity (IGE). The IGE measures the slope parameter from a regression of log adult income on log parental income; in keeping with my focus in the main analyses on intergenerational *persistence* - that is, on $1 -$ mobility, in the following analyses, I focus on persistence measured by $1 - \text{IGE}$. Because the logfunction is a rank-preserving transformation, parental income quintile groups are identical for estimating the counterfactual IGE and counterfactual rank-rank slope. Tables 6 and 7 below report counterfactual quintile-quintile log-log intergenerational persistence under strong and weak secondary interventions. Consistent with Tables 2 and 3 in the main text, strong secondary interventions are much more mobility-enhancing than are weak secondary interventions.

Table 6: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	9.20*** (0.07)	-1.21*** (0.08)	9.19*** (0.07)	-1.21*** (0.08)	-0.65%	-0.01
2	9.69*** (0.05)	-0.71*** (0.07)	9.69*** (0.06)	-0.72*** (0.07)	-0.22%	-0.00
3	9.81*** (0.05)	-0.59*** (0.07)	9.82*** (0.05)	-0.58*** (0.07)	1.39%	0.01
4	10.08*** (0.05)	-0.33*** (0.07)	10.08*** (0.05)	-0.33*** (0.07)	0.24%	0.00
5	10.41*** (0.04)		10.41*** (0.04)			0.00

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 7: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	9.20*** (0.07)	-1.21*** (0.08)	9.11*** (0.20)	-1.29*** (0.21)	-7.29%	-0.09
2	9.69*** (0.05)	-0.71*** (0.07)	9.93*** (0.10)	-0.48*** (0.11)	32.98%	0.24
3	9.81*** (0.05)	-0.59*** (0.07)	9.84*** (0.13)	-0.57*** (0.13)	4.19%	0.03
4	10.08*** (0.05)	-0.33*** (0.07)	10.10*** (0.08)	-0.31*** (0.09)	5.94%	0.02
5	10.41*** (0.04)		10.41*** (0.05)			0.00

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

D Results under Alternative Sample Restriction Criteria

In the main analysis, I restricted my sample to those respondents who began their post-secondary education at a 4-year college. This sample restriction is justified on the basis that individuals who transition from 2- to 4-year colleges are qualitatively distinct from those who begin at 4-year colleges Ciocca Eller and DiPrete, 2018, and their inclusion in the analysis would necessitate some measure of associates degree (AA) performance considered in college admissions processes. My main conclusions are, however, not sensitive to the exclusion of transfer students. Tables 8 and 9 report the impacts of secondary interventions for this larger population. As we can see, counterfactual rates of mobility under weak and strong secondary interventions display very similar patterns in full sample of high-school completers, including those who transition from a 2- to 4-year college. This set of results further indicates that, compared with a weak secondary intervention, strong secondary interventions are much more effective at promoting intergenerational mobility.

2

Table 8: Expected adult log income and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.87*** (0.99)	-22.44*** (1.25)	38.51*** (0.98)	-22.80*** (1.25)	-1.57%	-0.35
2	46.37*** (0.87)	-14.94*** (1.17)	46.09*** (0.87)	-15.22*** (1.16)	-1.85%	-0.28
3	50.95*** (0.85)	-10.36*** (1.14)	51.12*** (0.87)	-10.19*** (1.15)	1.65%	0.17
4	55.67*** (0.83)	-5.64*** (1.15)	55.71*** (0.83)	-5.60*** (1.14)	0.75%	0.04
5	61.31*** (0.77)		61.31*** (0.77)			-0.00

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 9: Expected adult log income and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.87*** (0.99)	-22.44*** (1.25)	39.75*** (1.56)	-21.71*** (1.82)	3.29%	0.88
2	46.37*** (0.87)	-14.94*** (1.17)	49.42*** (1.22)	-12.04*** (1.49)	19.44%	3.05
3	50.95*** (0.85)	-10.36*** (1.14)	52.14*** (1.30)	-9.31*** (1.56)	10.13%	1.19
4	55.67*** (0.83)	-5.64*** (1.15)	56.56*** (0.97)	-4.89*** (1.33)	13.25%	0.89
5	61.31*** (0.77)		61.45*** (0.90)			0.14

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

E Results under Alternative Definitions of College Attendance

In the main analysis, I code college attendees as those who had received a bachelor's degree by age 30. Tables G1 and G2 report the rank-rank slope estimates of conditional and controlled mobility where the age cutoff for defining college graduates is 25 and 35, respectively. We can see that our main results are fairly robust under alternative age cutoffs for defining college graduates.

In my main analyses, I use age 22 as the cutoff to define college-goers and non-college-goers. To assess the sensitivity of my results to this measurement choice, I conduct a series of additional analyses using alternative age cutoffs for college attendance, where those who attended a four-year college only after the age cutoff ("late college-goers") and those who attended a two-year college but not a four-year college ("two-year college-goers") are classified as high school graduates if they completed a high-school diploma by this age. Tables 10-19 below show my estimates of counterfactual gaps in adult earnings rank by parental income quintile across age cutoffs from 20-25, which reproduce my main finding of a larger impact of a strong secondary intervention on intergenerational mobility.

Table 10: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	39.68*** (1.17)	-23.54*** (1.48)	39.41*** (1.21)	-23.81*** (1.51)	-1.15%	-0.27 (0.37)
2	46.23*** (0.99)	-16.98*** (1.34)	45.96*** (1.03)	-17.26*** (1.36)	-1.61%	-0.27 (0.36)
3	50.38*** (0.97)	-12.84*** (1.33)	50.47*** (1.00)	-12.75*** (1.34)	0.71%	0.09 (0.31)
4	55.98*** (0.93)	-7.23*** (1.32)	56.04*** (0.94)	-7.18*** (1.31)	0.78%	0.06 (0.23)
5	63.22*** (0.89)		63.22*** (0.89)			-0.00 (0.00)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 11: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	39.68*** (1.17)	-23.54*** (1.48)	38.97*** (3.06)	-23.60*** (3.30)	-0.24%	-0.71 (2.83)
2	46.23*** (0.99)	-16.98*** (1.34)	49.31*** (1.94)	-13.25*** (2.13)	21.96%	3.08* (1.71)
3	50.38*** (0.97)	-12.84*** (1.33)	51.70*** (1.66)	-10.87*** (1.91)	15.37%	1.32 (1.41)
4	55.98*** (0.93)	-7.23*** (1.32)	57.32*** (1.40)	-5.25*** (1.82)	27.41%	1.33 (1.10)
5	63.22*** (0.89)		62.57*** (1.08)			-0.65 (0.63)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 12: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.90*** (1.16)	-23.97*** (1.46)	38.62*** (1.21)	-24.25*** (1.49)	-1.16%	-0.28 (0.40)
2	46.40*** (1.03)	-16.47*** (1.37)	46.18*** (1.07)	-16.69*** (1.39)	-1.32%	-0.22 (0.39)
3	49.85*** (0.96)	-13.02*** (1.32)	49.90*** (0.99)	-12.97*** (1.32)	0.38%	0.05 (0.32)
4	55.63*** (0.99)	-7.24*** (1.31)	55.69*** (1.00)	-7.18*** (1.30)	0.85%	0.06 (0.26)
5	62.87*** (0.90)		62.87*** (0.90)			-0.00 (0.00)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 13: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	38.90*** (1.16)	-23.97*** (1.46)	38.25*** (3.03)	-23.98*** (3.24)	-0.03%	-0.65 (2.83)
2	46.40*** (1.03)	-16.47*** (1.37)	49.86*** (1.73)	-12.37*** (1.99)	24.90%	3.46** (1.52)
3	49.85*** (0.96)	-13.02*** (1.32)	51.24*** (1.62)	-10.99*** (1.95)	15.57%	1.38 (1.33)
4	55.63*** (0.99)	-7.24*** (1.31)	56.83*** (1.50)	-5.40*** (1.88)	25.47%	1.20 (1.19)
5	62.87*** (0.90)		62.23*** (1.07)			-0.64 (0.64)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 14: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	35.52*** (1.04)	-12.95*** (1.70)	35.45*** (1.59)	-13.02*** (2.09)	-0.51%	-0.07 (1.21)
2	42.23*** (0.97)	-6.24*** (1.63)	42.97*** (1.46)	-5.50*** (1.94)	11.81%	0.74 (1.10)
3	45.16*** (1.02)	-3.31 ** (1.64)	45.39*** (1.23)	-3.08 * (1.77)	6.91%	0.23 (0.69)
4	48.76*** (1.18)	0.29 (1.77)	49.06*** (1.23)	0.60 (1.80)	105.53%	0.31 (0.38)
5	48.47*** (1.30)		48.47*** (1.30)			0.00 (0.00)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 15: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	35.52*** (1.04)	-12.95*** (1.70)	35.47*** (1.16)	-12.97*** (1.94)	-0.12%	-0.04 (0.49)
2	42.23*** (0.97)	-6.24*** (1.63)	42.57*** (1.01)	-5.87*** (1.80)	5.86%	0.34 (0.35)
3	45.16*** (1.02)	-3.31 ** (1.64)	45.28*** (1.08)	-3.16 * (1.82)	4.33%	0.12 (0.39)
4	48.76*** (1.18)	0.29 (1.77)	48.94*** (1.20)	0.50 (1.93)	71.52%	0.18 (0.33)
5	48.47*** (1.30)		48.44*** (1.50)			-0.03 (0.74)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 16: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	35.52*** (0.99)	-12.73*** (1.65)	35.79*** (2.36)	-12.46*** (2.71)	2.09%	0.27 (2.15)
2	41.80*** (1.01)	-6.45*** (1.68)	43.37*** (2.16)	-4.88 * (2.55)	24.36%	1.57 (1.94)
3	44.98*** (1.00)	-3.27 * (1.67)	45.33*** (1.48)	-2.92 (1.98)	10.80%	0.35 (1.10)
4	48.79*** (1.17)	0.54 (1.77)	49.66*** (1.31)	1.41 (1.86)	159.07%	0.86 (0.67)
5	48.25*** (1.34)		48.25*** (1.34)			0.00 (0.00)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin’s (1987) method.

Table 17: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	35.52*** (0.99)	-12.73*** (1.65)	35.36*** (1.13)	-12.82*** (2.11)	-0.73%	-0.17 (0.54)
2	41.80*** (1.01)	-6.45*** (1.68)	42.34*** (1.34)	-5.84*** (2.22)	9.50%	0.54 (0.93)
3	44.98*** (1.00)	-3.27 * (1.67)	45.06*** (1.10)	-3.12 (2.08)	4.62%	0.08 (0.50)
4	48.79*** (1.17)	0.54 (1.77)	49.03*** (1.21)	0.85 (2.12)	57.21%	0.24 (0.43)
5	48.25*** (1.34)		48.18*** (1.79)			-0.07 (1.19)

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin’s (1987) method.

Table 18: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) weak secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	36.87*** (1.11)	-26.30*** (1.45)	36.80*** (1.11)	-26.37*** (1.45)	-0.27%	-0.07
2	45.01*** (1.02)	-18.16*** (1.39)	45.37*** (1.04)	-17.81*** (1.40)	1.96%	0.36
3	48.88*** (1.00)	-14.29*** (1.37)	49.23*** (1.02)	-13.95*** (1.38)	2.39%	0.34
4	55.35*** (1.00)	-7.82*** (1.38)	55.46*** (1.00)	-7.72*** (1.38)	1.32%	0.10
5	63.17*** (0.94)		63.17*** (0.94)			-0.00

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

Table 19: Expected adult income ranking and observed gap (vs expected income among individuals from 5th parental income quintile) by parental income quintile under (a) no intervention (observed gap) and (b) strong secondary intervention (counterfactual gap).

	Observed Earnings	Observed Gap	Counterfactual Earnings	Counterfactual Gap	% Reduced (Gap)	Increase (Earnings)
1	36.87*** (1.11)	-26.30*** (1.45)	36.61*** (2.60)	-26.20*** (2.88)	0.38%	-0.26
2	45.01*** (1.02)	-18.16*** (1.39)	49.52*** (1.69)	-13.29*** (2.07)	26.82%	4.51
3	48.88*** (1.00)	-14.29*** (1.37)	48.40*** (2.28)	-14.41*** (2.50)	-0.84%	-0.48
4	55.35*** (1.00)	-7.82*** (1.38)	56.02*** (1.47)	-6.79*** (1.91)	13.15%	0.67
5	63.17*** (0.94)		62.81*** (1.16)			-0.36

Note: 1:5 denotes parental income quintile. *p<.10, **p<.05, ***p<.01 (two-tailed tests). Numbers in parentheses are bootstrapped standard errors (250 replications), adjusted for multiple imputation via Rubin's (1987) method.

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