Income Composition and Peer Effects in Education*†

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Abstract

We study the long-run effects of income differences within peer compositions. An increase in the share of low-income peers within school-cohorts improves the educational outcomes of low-income students and negatively affects high-income students. We show this pattern is not explained by commonly observed mechanisms. We then propose a model based on reference-dependent preferences and social comparison that rationalizes our findings, highlighting the role of frustration or motivation depending on students' relative income. We also provide evidence consistent with this mechanism. Finally, we show that better connections in school can help avoid unintended consequences of income differences among peers.

Keywords: Peer Effects, Education, Income Composition, Reference Dependence
JEL-Codes: I21, I24, I29, J24

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1 Introduction

The effects of exposure to differences in income for students are less clear than are the sources of inequality. A wide literature indicates that students' outcomes are influenced by peers (Sacerdote, 2014), but the reactions to differences in peer income composition are less understood. This is important, as such differences may shape students' aspirations, self-perception, and perceived social mobility. Even when average peer characteristics remain constant, who the peers are - and the relative position of each individual within that group - might meaningfully affect how students engage with their education. Peer income composition could work through well-known channels that income may capture, such as the ability distribution, behavior, teachers, or other characteristics (Billings and Hoekstra, 2023; Booij et al., 2017; Duflo et al., 2011; Feld and Zölitz, 2017; Carrell et al., 2018). Alternatively, income composition may draw students' attention to disparities in opportunity, and this can trigger different reactions, even when keeping students' ability fixed. Low-income students exposed to more advantaged peers may react feeling discouraged by the lack of opportunities that is made transparent by observing their more fortunate peers. On the other hand, they could be inspired to put more effort to achieve more, seeing the more fortunate peers as role models. In this paper, we examine which mechanism is at work by studying how changes in the composition of peer income affect students' long-run educational attainment and short-run performance.[1]

We make two main contributions. First, we empirically show that changes in peer income compositions affect educational attainment heterogeneously across adolescent students from lower to higher income families. We find that low-income students benefit from being exposed to a higher share of low-income peers in terms of likelihood of completing university, whereas high-income students are hurt. Further, we show evidence that this pattern is not likely to be explained by a range of mechanisms discussed in the literature. Second, to help rationalize our results, we propose a novel theoretical framework where students' choice of effort is influenced by income-based social comparisons. Subsequently, we provide some empirical evidence in support of the key mechanisms highlighted by our model: depending on a student's relative position in the income distribution, social comparison based on income can be either motivating, or can otherwise lead to frustration and discouragement. Finally, we conclude by highlighting a path through social cohesion and integration that may help mitigate the consequences of income differences among peers.

Empirical analysis. Our first contribution is to investigate how changes in peer income distributions during adolescence affect university completion, across students' family income. To capture changes in income distributions, we use data from the National Longitudinal Study of Adolescent to Adult Health (Add Health) and the leave-one-out share of low-income peers in students' school-cohort. We choose this broad reference group based on our motivation that

[1] As an anecdotal example, consider a story told by the "This American Life" radio program about a group of high school students attending school in one of America's poorest congressional districts taken to visit a nearby elite private school (Episode 550: Three Miles available at **https://www.thisamericanlife.org/550/three-miles**). Their reactions, described by a teacher, tell a powerful story (Greenbaum, 2015). "They felt like everyone was looking at them. And one of the students started screaming and crying. Like, this is unfair. This is – I don't want to be here. I'm leaving."

income composition may be an important part of the general environment students are exposed to. In Section 2, we discuss more on this choice, and later, in Section 7 we compare it against more refined peer reference groups.

We use a within school, across cohort design and effectively compare students in the same school, who have similar family incomes and other characteristics, face similar school-cohort variances in the income distribution, but are crucially facing differences in the share of low-income peers across their cohorts. The key assumptions are that unobserved selection factors into schools are fixed at the school level and that our flexible own-income controls fully capture the link between students' family income and their outcome. Based on these assumptions, we avoid contamination of the peer income effect which is split across students' position in the income distribution. We discuss in detail our identification strategy and assumptions in Section 3. Moreover, because part of our motivation here is that income composition can change environments, we condition on the leave-one-out standard deviation in school-cohort incomes. Hence, shifts in the share of low-income peers capture real differences in peer composition. These stronger differences may represent more salient changes in the environment.[2]

Our main results reveal a clear non-monotonic pattern. Among students in the bottom 20th percentile of the income distribution, a standard deviation increase of 20% in the share of low-income peers *increases* the propensity to complete university by 3.6 percentage points (pp). For students in the top 20th percentile of income, this same change *decreases* university completion by 4.1pp, while middle income students have estimated *null* effects.[3] Furthermore, we confirm that these results are robust to a wide range of checks. In Section 4.3, we then turn to assess whether this pattern is explained by common mechanisms proposed in the peer effects literature, such as non-linear peer ability effects, teacher and parental responses, and disruptive peer behavior. We find no evidence that the effects we identify are explained by these mechanisms, suggesting that students' responses to differences in income can be significant but not adequately addressed by the existing literature.

Theoretical framing and mechanisms. In the second part of the paper, we advance a novel theoretical model of student effort choice that offers a lens to rationalize the patterns we observe. We consider students who have different capacities for translating effort into an educational outcome. Capacity is a broader construct than just raw ability, encompassing a combination of factors, enabling a student to achieve outcomes. In this sense, we think of differences in capacities as capturing inequality of opportunity. Importantly, we consider income as one such factor that is both salient and observable in school. A central component of our theory is then the idea that social comparison among students based on income can generate both frustration and motivation depending on a student's relative position in the income distribution.

In our model, students compare realized outcomes in relation to a reference point for educational attainment, which we assume to be influenced by the capacity distribution of their peers: an indicator for what others can achieve. We show that for students with sufficiently high capacities (and therefore income), an increase in the share of low-income peers implies an

[2]This is what is meant above on facing similar school-cohort variances in the income distribution.
[3]Using the share of high income peers instead of the low income share, returns similar, mirror image results.

increase in perceived inequality of opportunities which leaves them further ahead of their peers. This generates loss of motivation, lower effort, and ultimately lower educational attainment. On the other hand, those students with sufficiently low capacities will see this as a reduction in the inequality of opportunities and will feel less frustrated as they are now less far behind their peers, leading to greater motivation, effort, and educational attainment. Middle-capacity students might experience both situations, rationalizing an average null effect for this income group.

Our model provides an understanding for how changes in the exposure to income differences among peers can generate unintended consequences for students' educational outcomes. Moreover, it also highlights a potential mechanism based on students' motivation (or frustration) when choosing effort. To investigate this further, in Section 6 we look at empirical evidence on performance in high school, based on transcript data and measures of self-esteem, relative intelligence rating, mental health, and motivation. Once again, we find a heterogeneous pattern: low-income students experience a strong, positive effect on performance and improvements in self-esteem and relative self-intelligence rating, while higher income students exhibit an increase in depressive symptoms and decreases in motivation. Altogether with the main results on long-run educational attainment, our evidence is well explained by a mechanism where disparities in income can create contextual effects in the school environment that are unintended.

Social cohesion and integration. Finally, in Section 7, we turn to an extension exploring what may improve the ability of schools to support disadvantaged students faced with inequality. Recent work shows that better connectivity (friendships) in school networks improves students' perception of school climate (Alan et al., 2021b) and improvements in social cohesion improve students' outcomes (Alan et al., 2021a). Thus, we propose that better connections in the school network can reduce the effects from changes in peer income composition. Intuitively, better cohesion may work against the effects of income differences by allowing students to put less weight on peer income when forming reference points or by learning about their peers' true abilities, feeling involved, and thereby more competitive. Using friendship nominations, we show descriptive evidence that social integration through friendships – either better centrality or more cross income group links – moderates the effects from the share of low-income peers on university completion. This holds for both low- and high-income students. We view this evidence as descriptive, but pointing to an important area for further work and policy. Our findings suggest that attempts to expose students to different income backgrounds must be coupled with efforts to improve social integration. Doing so, may help avoid unintended consequences stemming from reference dependence and inequality of opportunities among students.

Related literature. Our study relates to a literature on the consequences of inequality for skill development. Much of this literature has focused on how environments during early life affect skill development (for a review see Heckman and Mosso, 2014) and how inequality leads to different incentives for skill investments across low and high SES families (Doepke and Zilibotti, 2017; Doepke et al., 2019). Additionally, neighborhood inequality has long lasting effects on economic mobility (Chetty and Hendren, 2018a), and children gaining entrance just on the margin to higher quality middle schools in Mexico have been found to achieve lower conscientiousness scores and to shift aspirations away from academics toward vocational tracks (Fabregas, 2022). We contribute to this literature by highlighting the consequences of unabated income differences within peer groups in schools. Furthermore, our results offer an additional explanation for why the benefits of moving to a better quality neighborhood are diminished if a child moves at a later age (Chetty et al., 2016; Chetty and Hendren, 2018b).

Our study further relates to a growing literature on the effects of school environments and peer compositions. These include effects from teacher quality (Chetty et al., 2014; Rothstein, 2017), smaller classes (Angrist and Lavy, 1999; Krueger and Whitmore, 2001; Chetty et al., 2011; Angrist et al., 2019), school spending (Jackson et al., 2015), and tracking students by ability (Duflo et al., 2011; Guyon et al., 2012). Related to these, a recent study by Jackson et al. (2022) finds that the benefits of attending an effective high school for disadvantaged students runs through dimensions unrelated to test score value added. Our study can help shed light here, as this fits with our results on social cohesion representing where and when disadvantaged students may not be harmed by exposure to income differences among peers. Moreover, with college students Londoño-Vélez (2022) finds evidence that high income students shifted towards more diverse social networks and support for redistribution when exposed to a higher share of low income students. Our discussion relates to this through how social cohesion in the ability of low and high incomes students to connect can remove the barriers potentially created by reference dependence due to differences in opportunity.

We also contribute to a broad literature on the effects of peers. A non-comprehensive summary of studies on short-run influences of peers includes the link between peers' persistence and academic achievement (Golsteyn et al., 2021), exposure to low-achieving peers in Kindergarten (Bietenbeck, 2020), spillovers in educational attitudes among friends (Gagete-Miranda, 2020; Norris, 2020), and the effects of peer gender compositions (Lavy and Schlosser, 2011; Black et al., 2013; Gong et al., 2021; Borbely et al., 2023). Studies on the long-run effects of peers include disruptive peers (Carrell et al., 2018), working mothers within peer groups (Olivetti et al., 2020), peer gender effects on university major (Anelli and Peri, 2019), peers' parental education (Bifulco et al., 2011, 2014), peer deprivation and risky behaviors (Balsa et al., 2014), and the effects of high school ability rank on mental health in adulthood (Kiessling and Norris, 2022). More relatedly, Cattan et al. (2023) find elite peers in Norway to positively affect enrollment in elite schools and externally assessed exams.

Our focus is distinct in the literature and demonstrates that peer compositions, or differences in opportunity, can have important and very different effects across the distribution of students' family income. We examine this within the US context, where inequality can be high.[4] The role of peers in generating frustration or competition may be especially salient in a relatively unequal context, where students can be placed much further away from their reference point than in a more equal environment. Thus, context may matter in shaping peer effects. Evidence from group based games in psychology is suggestive of such contextual responses, showing

[4] Higher prevalence and salience of inequality in the US is particularly true when compared to countries such as Norway - the country examined in Cattan et al. (2023) - where the ratio between the top and bottom decile of the disposable income distribution is twice as big in the US than in Norway (6.3 vs 3.1, OECD 2018).

that when informed about the degree of income inequality in a group individuals with a low socioeconomic status (SES) take more risks and report less satisfaction (Payne et al., 2017).

Broadly speaking our work relates to a Rawlsian sense of justice and how context may make this salient. In Rawls' work, justice requires the least well off in society to have equality of opportunity (Rawls, 1972). If inequality relates to injustice, then it would not be surprising that exposure to inequality will trigger reactions. We explore this empirically through difference in income among students and frame the consequences in a behavioral model of social comparisons. Frustration as a response to differences in income, particularly among low-income students, could additionally help explain why programs relocating adolescents from disadvantaged to advantaged areas have not always found success.[5] Such interventions involve exposing students to a different distribution of income both for the lower- and higher-income students. Our results are consistent with peer composition creating contextual effects on students, our theory rationalizes this, and our evidence on mechanisms adds further support. Moreover, our evidence on social cohesion suggests that better connections in the school can mitigate the detrimental effects we find from exposure to peers with widely different incomes further emphasizing that context can shape peer mechanisms.

2 Data and variables

We use restricted data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a longitudinal study representative of middle and high schools in the United States in the mid-1990s. Add Health has several useful features. First, it covers multiple cohorts within schools, which we need for our empirical strategy of exploiting variation within schools across cohorts. Second, a representative set of students from each cohort was first interviewed in 1994/95, when the majority of students were between 12 and 18 years old, and followed for five waves until 2016-2018. Third, it includes students' household income, allowing us to observe within school income composition. Our measure to capture changes in the income composition of peers is the share of low-income peers within each student's school-cohort. We then compare long-run educational outcomes and short-run mechanisms, as the composition of peer income changes relative to a student's position in the income distribution. One drawback to Add Health is that we only observe a sample of students within school-cohorts. However, later we will show our results are robust to an extensive set of checks around this issue.

2.1 Family Income

Income is based on a parental survey for each adolescent. Typically, the mother answered this survey, and we use the reported household income from wave I of this survey to measure students' family income. Of the 20,745 respondents in wave I, household income data is missing for 5,394 individuals, representing approximately 26% of the sample. We address concerns related to this missing data later in sampling robustness checks.

[5] For instance, in the Moving to Opportunity experiment adolescent movers experienced on average null or even negative effects (Chetty et al., 2016), while the integration of poor students into elite schools in Delhi improved some pro-social outcomes among existing students but appears to have harmed performance (Rao, 2019).

2.2 Income, Skills, Well-Being, and Opportunity

Before proceeding, we provide some descriptive patterns around income and its association with skills and well-being. Thus, here we demonstrate, unsurprisingly, that family income captures a wide variety of factors that allows effort to be translated into success. This is not merely about ability but also about opportunity.

In Figure 1, we look at the link between income and each of ability, depressive symptoms, and parental investments. We measure cognitive ability using the Add Health Picture Vocabulary Test (PVT) score.[6] Consistent with evidence in the literature on skill trajectories and income (Doepke et al., 2019; Falk et al., 2021), we observe a positive relationship between PVT scores and income that persists when conditioning on school fixed effects (Figure 1a). Next, even holding ability constant, low-income may reduce well-being through mental health. Adolescents exposed to multiple stressors are at a greater risk of experiencing higher depressive symptoms (Thapar et al., 2012), and the conditions of poverty increase uncertainty, adding greater stress (Haushofer and Fehr, 2014; Lichand and Mani, 2020; Mani et al., 2013). Low-income conditions may then expose a student to more stressors, leading to more depressive symptoms, which can reduce motivation and beliefs about the returns to effort (De Quidt and Haushofer, 2019). Supportive of this assertion we see that lower income students tend to score higher on depressive symptoms than do wealthier students (Figure 1b) using the Center of Epidemiologic Studies Depression Scale (CES-D, Radloff, 1977).[7] Finally, we see that lower income students receive fewer monetary investments from parents (Figure 1c), which connects to opportunity. This pattern holds even after conditioning on school fixed effects and PVT scores, implying they are not simply reflecting endogenous school sorting or ability.

The patterns we find are consistent with a multi-dimensional interpretation of what income captures. Exposure to changes in income composition may then signal to adolescents their relative opportunity, leaving an open question on how they will respond educationally across the income distribution.

2.3 Definition of low-income peers

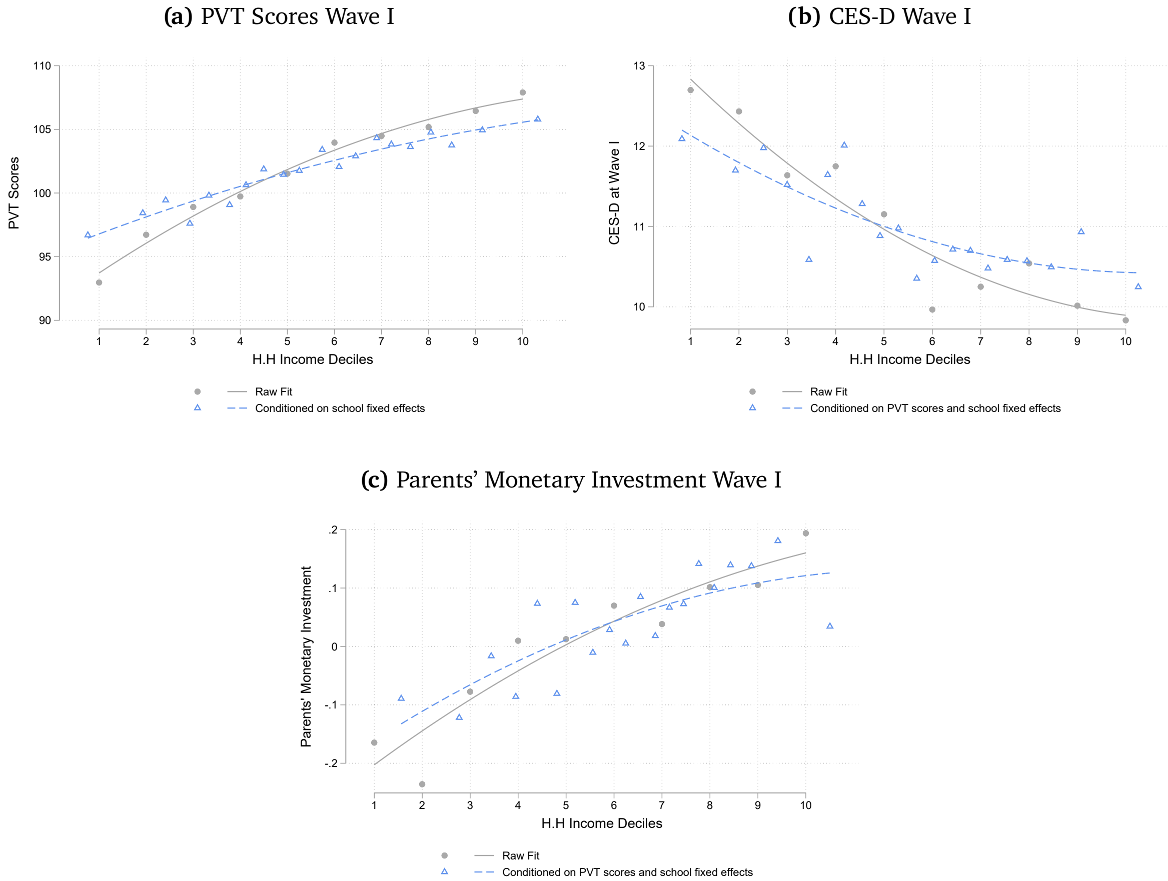
We focus our analysis on changes in the share of low-income peers. Thus, first, we define low-income households at wave I of the survey when students were in grades 7 – 12 with the majority of them (72%) in grades 9 – 12. We will refer to grades as cohorts. To define low-income households, we first include households below the 1994 poverty threshold for a given family size. Second, we additionally include households who are not below the poverty threshold but who are in the bottom third of the income distribution for each family size.[8] We use this definition to balance sample size for the low-income category against miss-classification and

[6] PVT scores in Add Health have been used for ability in a range of papers. Kiessling and Norris (2022) provide discussion on what it measures and show evidence that it is a stable measure of ability.

[7] The CES-D is a often used measure of depression in psychiatric epidemiology. This is a scale measure based on self-reported items that are 1-5 with higher values implying more depressive experiences. AddHealth contains 19 of the 20 items on the full scale for which we follow the literature and collect these into a sum. See Kiessling and Norris (2022) for more description and a lengthy discussion about the CES-D score in AddHealth and see the Appendix Table B.3 for a list.

[8] Family sizes of 8 or more people are grouped together.

Figure 1. PVT Scores, CES-D, and Parents' Monetary Investment by Household Income Deciles



Notes: For each household income decile, we report bin scatter plots with a quadratic fit line of PVT scores in panel (a), CES-D scores in panel (b) and parental monetary investment in panel (c). The bin scatter plot in panel (a) presents a quadratic fit line before and after conditioning on school fixed effects. Bin scatter plots in panel (b) and (c) present quadratic fit lines before and after conditioning on PVT scores and school fixed effects.

to make sure our peer measure has good support. In robustness checks, we explore alternative definitions and provide more discussion.

Next, we define our peer measure as the leave-one-out share of low-income peers at the school-cohort level. On average, this measure has a 35% share of low-income peers, and it provides near full support (see Appendix Figure B.1a). Additionally, after the inclusion of school and cohort fixed effects, we still maintain considerable variation to identify our effects of interest (see Appendix Figure B.1b). We use this definition to efficiently capture shifts in the distribution of peer incomes based on being around a larger share of lower-income peers versus medium to higher income peers. The mean itself may not capture sufficient variation if what matters is how far one is from their peers, something we discuss more in Section 4.[9]

We use the school-cohort as the peer reference group, because we want to define the general environment of peer income composition that students are exposed to. Later, in Section 7, we

[9]Interacting the peer mean with the peer standard deviation of income is another possibility to capture strong changes in the distribution, but this will considerably strain our data, because we aim to disaggregate effects across students' own position in the income distribution. We did check results using this approach and found consistent, though less efficient evidence.

compare this against more refined peer reference groups. At that point, we provide intuition and expectations on why the broader environment captures effects that refined groupings may not, and we then provide evidence for these expectations. Finally, we also checked our later baseline analysis by defining the share of high-income peers, in place of the low-income share, using either the top tertile or top 20% to define the share. We find the results are the same only flipped in sign, as it should be, relative to using the share of low-income students, thus the choice is to focus on the share of low income students is not consequential to the eventual interpretation.[10]

2.4 Educational outcomes

To assess the long-run consequences of exposure to income differences among peers during adolescence, we focus on whether or not a student has completed at least a university bachelor's degree or higher by wave IV of the survey when respondents are on average 28 years old (range: 24-34).[11] We focus on such long-run educational outcome for most of our results. Later, we also assess some short-run outcomes on performance in high school. For participants who agreed, Add Health collected their full high school transcript data at wave III. We calculate cumulative GPA excluding courses taken in years prior to the survey year of our treatment. We also construct indicators for whether the student took advanced courses in Math, Science, and English.

2.5 Sample selection and summary statistics

Summary statistics for our sample are reported in Columns (1) - (4) of Appendix Table B.1. We first drop observations with missing household income, missing school and cohort identifiers, missing family size, individuals older than 19 at wave I, and individuals from schools with fewer than 20 students in total and 5 students per cohort (6,433 observations).[12] These steps leave us with complete information on the share of low-income peers. Next, we drop those missing information on education level at wave IV (3,174 observations), leaving us 11,165 students in our analytic sample. For all other controls, we impute them to either 0 for discrete variables or to the mean for continuous variables and control for corresponding missing indicators in all specifications.

In our analytic sample, 52% are female and the average age is 15.5 years old in wave I. The majority of students are white (59%), about 17% report at least one foreign born parent, 38% of all students come from university-educated households, and students have on average 34% of peers from low-income families. Moreover, the mean university graduation rate by wave IV (collected in 2008) in our sample is 33%, which is similar to the national average of 29.4% at the time of the survey (U.S. Census Bureau, 2022).

[10] Results using the share of high income students are unreported but can be made available.

[11] While there is a wave V, attrition at this wave was much more severe. Our results, though, are very similar if using the wave V sample and education information.

[12] Family size is important for how we define low-income peers thus we drop those missing family size. The restrictions on school and cohort size are standard in the literature using Add Health for peer effect analysis (see Elsner and Isphording, 2018; Kiessling and Norris, 2022).

We also compare the analytic sample means in the Appendix Table B.1 before and after reweighting using the cross-sectional weights AddHealth provides. These weights bring the sample in-line with the national distribution, and on our key variables, we observe barely any absolute differences between the unweighted and weighted means. While there are differences for ethnicity, we later will show that our baseline analysis is robust to using these weights. Overall, we interpret our analytic sample as representative of the full population on our outcome, treatment, and key variables. Additionally, we provide summary statistics for outcomes that we use in later analyses in the Appendix Table B.2. These include our measures taken from the high school transcript data and measures of self-esteem, beliefs, and mental health that we later use to assess frustration and motivation as mechanisms.

3 Empirical strategy

We need to surmount two hurdles to identify effects from the share of low-income peers. One, selection into schools would likely bias our estimates, if unaccounted for. Two, responses to peer income composition may be heterogeneous to own-income given the stark differences in opportunity that income can create. Thus, we need to disaggregate effect estimates for the share of low-income peers over the household income distribution and avoid contamination from any non-linear effects that stem from income. We address these problems through (i) using a within school, across cohort design with school and cohort fixed effects commonly deployed in the peer effects literature (e.g., see Sacerdote, 2014) and (ii) highly flexible controls for own-income.

3.1 Main specification

We begin with the following specification:

$$\begin{aligned} Y_{ics} = {} & SLP_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\}\alpha_k \\ & + SD(ln(Inc))_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\}\beta_k \\ & + f(ln(Inc_{ics})) + \boldsymbol{X}_i^{'}\gamma_1 + \boldsymbol{X}_{-i}^{'}\gamma_2 + \boldsymbol{XSD}_{-i}^{'}\gamma_3 + \theta_{ics} + \epsilon_{ics}, \end{aligned} \tag{1}$$

where Y_{ics} denotes the university graduation of student i in cohort c and school s and SLP_{-ics} denotes the leave-one-out percentage share of peers from low-income households in cohort c and school s. The coefficients α_k are the marginal effects of SLP_{-ics} at each income decile. We take this as our starting point based on both classical reasons to expect non-linear peer effects (e.g., differential responses to peer ability or by teachers, etc.) and the motivation discussed in the introduction around the potential for different peer compositions to induce both motivation or frustration among students depending on their relative position in the income distribution. Note that while we begin with the disaggregation across deciles of income, based on results from this we then turn to a more parsimonious specification disaggregating over income groups defined as the bottom two deciles, the middle, and top two deciles. In this case, we replace the by decile interaction with $SLP_{-ics} \times \sum_{k=1}^{3} \mathbb{1}\{IncGroup = k\}$.

We further include a measure for the dispersion of income in peer groups, the leave-one-out standard deviation of peers logged household income, which we also disaggregate across income deciles. The purpose for including this is to capture potential ranking mechanisms through controlling for differences in dispersion. Changes in effort due to differences in, for example, ability rank or social status rank could arise independently from our primary focus on the peer effect of exposure to income compositions (Elsner and Isphording, 2018; Kiessling and Norris, 2022). Indeed, Tincani (2018) shows that higher order moments of peer distributions can exert different effects. Thus, this control is primarily about interpretation, while it is exogenous under the same assumptions that our share of low-income variable will be.

Next, for identification, it is important to control for own income, as well as school and cohort fixed effects. We thus flexibly control for non-linear effects from own-income by including a cubic polynomial in logged household income. We use this polynomial approach to maintain efficiency rather than including deciles indicators. However, we show in robustness checks that our results are not sensitive to higher order polynomials in income, nor are they sensitive to going beyond decile fixed effects by controlling for income ventile fixed effects. To focus on within school, across cohort variation we have school and cohort fixed effects given by $\theta_{ics} = \mu_c + \delta_s$.

We then control for a set of exogenous demographic and family background characteristics in the vector $\boldsymbol{X}_i$.[13] In our preferred specification, we supplement these controls by adding peer leave-one-out means for some of these characteristics ($\boldsymbol{X}'_{-i}$), as a way to capture other potential mechanisms that may run through peer compositions.[14] We also add peer leave-one-out standard deviations ($\boldsymbol{XSD}'_{-i}$) for continuous characteristic controls (age and family size) to further capture potential effects from second moments of peer compositions. The error term is ϵ_{ics}.

We could restrict our data further and estimate our effects on sub-samples of own-income. This would allow all covariates to vary by each sub-sample, but the sample sizes would prevent efficient estimation. Thus, we begin with the analytic sample and in a later robustness check consider sub-sample restrictions.

Our data allows us to test against a rich set of mechanisms that could describe our results apart from our preferred interpretation. We will later add the peer mean of income and the peer ability composition, as well as ability rank, interacted with income groups to test whether our effects are driven by changes in peer quality. We will show our results are not explained by these factors, suggesting our control for income dispersion may have already pulled these mechanisms out. Moreover, controlling for income dispersion may also capture behavioral mechanisms separately from our share (SLP_{-ics}) effects, if those mechanisms correlate with the peer

[13]These are gender, age and age squared, indicators for race (Asian, Black, Hispanic, White, Other), an indicator for being the child of an immigrant, the family size, indicators for parents' highest degree (less than high school, high school/GED, some college, college degree, postgraduate degree), and an indicator for being raised in a single parent household.

[14]Note that we exclude peer controls in parental education as these could create collinearity problems with our share of low-income peers. We have included them (indicators for whether parents have completed high school, some college education, or post graduate education) in unreported results and they did not change our baseline result but we believe they over-control.

standard deviation of the income distribution. Later, we directly add peer behavior measures as further checks on whether income picks up other mechanisms. As we will show, we will find our results are not explained by these mechanisms leading us to provide a modelling framework that founds clearly our preferred interpretation.

3.2 Identifying assumption

In order to identify the causal effects from the share of low-income peers over the income distribution, α_k, the share has to be as good as randomly assigned. Our assumption, shared with all school-cohort based designs, is that we have exogeneity conditional on a rich set of controls and fixed effects.

Identification really rests on two critical components. First, we must adequately capture the relationship between our outcome and own-income, and second, we must cut any link between the determinants of selection into schools and our treatment. For the first, we use a flexible specification in own-income with a cubic polynomial. In later checks, we expand this up to a sixth degree polynomial or replace the polynomials with income ventile fixed effects.

For selection into schools, we assume that it is captured by school and cohort fixed effects, as unaccounted for selection may correlate with SLP_{-ics}. We demonstrate the point in the Appendix Figure B.2, using a scatter plot of SLP_{-ics} against the mean income within each school. We sort school mean income from low to high among those in the bottom two income deciles (panel (a)), the middle deciles (panel (b)), and the top two deciles (panel (c)). In each case, we see that the raw, uncontrolled correlation is clearly negative. We then show these same scatter plots after removing school fixed effects. Though mechanical, as mean school income is a fixed factor, the plots illustrate our identification strategy showing that with school fixed effects this link is now cut and will also be cut for all other unobserved factors common at the school level. Moreover, we can see that in each segment of the income distribution there remains variation in the residual SLP_{-ics} that we leverage to identify our effects.

Our assumption here implies that parents select into schools based on fixed school factors thereby the school fixed effects remove all unobserved selection factors. We also relax this assumption in some specifications in case parents select schools partly based on school trends, adding these via $\delta_s \times c$ or in other specifications adding school specific income trends. Moreover, later we explore an extensive set of robustness checks demonstrating that our results are not sensitive to a range of concerns and are unlikely to be spurious (via placebo testing).

3.3 Balancing test

We now test for evidence consistent with our identification assumptions using balance tests presented in Table 1. Each cell in columns (1) - (4) presents a regression of our treatment variable of interest on each row variable. In each test, we control for a cubic in logged household income and school and cohort fixed effects, as these are crucial to our identification. In columns (2) - (4), we restrict the sample around the bottom 20th, the middle, and the top 20th of own-household income to check that our identification assumption is still reasonable within these important groups. Finally, in columns (5) - (7), we repeat this but use the peer standard deviation of logged household income to show that even our additional peer income controls are reasonably exogenous.

Consistent with quasi-random assignment of peers, we observe that most characteristics are not related to our treatment variables. Only the indicator for whether a student is the first-born child seems to be associated with a higher share of low-income peers. Yet, given the number of tests performed is relatively high and the coefficient is small (amounting to less than one percentile score) we interpret the balancing check as strongly consistent with quasi-random assignment of peers.[15]

Table 1. Balancing test

	SLP_{-ics}	$SLP_{-ics} \times$ B20	$SLP_{-ics} \times$ M	$SLP_{-ics} \times$ T20	Log(Inc)SD × B20	Log(Inc)SD × M	Log(Inc)SD × T20
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	0.001	0.001	-0.000	0.005*	-0.000	0.001	0.001
	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.009)
White	-0.000	-0.003	0.000	-0.003	0.011	0.001	-0.012
	(0.002)	(0.005)	(0.003)	(0.005)	(0.009)	(0.005)	(0.010)
College-educated Parents	-0.002	-0.006	-0.002	0.002	-0.002	-0.001	-0.005
	(0.001)	(0.006)	(0.002)	(0.004)	(0.009)	(0.003)	(0.006)
Raised by a Single Parent	0.000	0.004	0.000	-0.005	0.000	-0.002	-0.009
	(0.002)	(0.003)	(0.002)	(0.005)	(0.005)	(0.003)	(0.008)
Birth weight (ounces)	0.000	0.000	0.000	0.000	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
First-born child	0.003**	0.002	0.003*	0.006**	0.001	0.007**	0.005
	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.005)
Child of an Immigrant	-0.001	0.004	-0.002	-0.001	-0.011	-0.003	0.005
	(0.002)	(0.004)	(0.003)	(0.004)	(0.007)	(0.005)	(0.011)
Household receives food stamps	-0.001	0.003	0.003	-0.013	0.006	-0.000	0.013
	(0.002)	(0.003)	(0.005)	(0.024)	(0.006)	(0.006)	(0.038)
Household size	0.001	0.001	0.001	0.000	-0.001	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)
Function of Log Household Income	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School and Grade FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11165	2180	6920	2065	2180	6920	2065

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. SLP_{-ics} denotes the leave-one-out percentage share of peers from low-income households in cohort c and school s. Standard errors are in parentheses and clustered at the school level. Columns (1) use the analytic sample; columns (2)-(4) and columns (5)-(7) split the analytic sample by the bottom 20th percentile of household income, the 20th-80th percentiles (endpoints are not included), and the top 20th percentile of households income.

4 Results: long-run effects on educational attainment

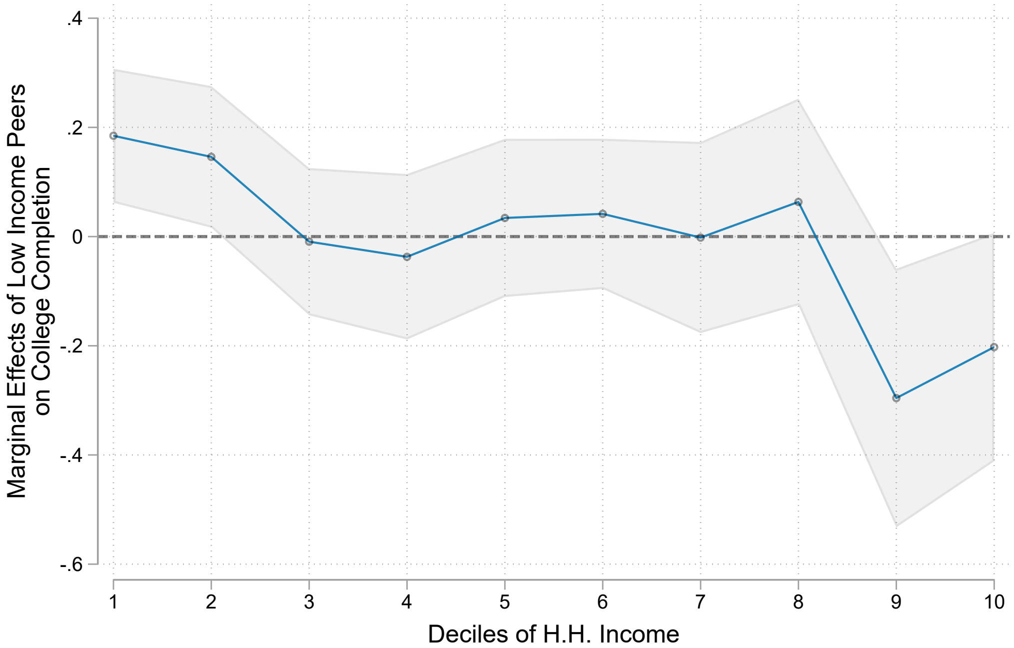
In this section we begin by testing the effect from a shift in the share of low-income peers on the probability a student completes a university degree or higher by the wave IV survey across students' positions in the income distribution. Next, we test for whether our results on peer income composition can be explained by other peer effects mechanisms advanced in the literature, and conclude with a set of robustness checks.

[15]The significant, positive estimate on first-born does show up both on the average of SLP_{-ics} and on SLP_{-ics} for the top-20 income group. We do not think that this is a concern. First-born children often get better resources (Black et al., 2018), thus if anything, we may have predicted an opposite sign effect here. Again, the magnitudes are small, go against our effects and are not persistently significant in columns (2), (5), or (7). Finally, we have confirmed that including or excluding it from our controls does not change our baseline nor mechanism results.

4.1 Baseline results

We begin by studying the marginal effects from a student's share of low-income peers at wave I on their probability of completing university by wave IV. We use our preferred specification, as discussed in Section 3, to calculate the marginal effects (α_k) at each decile of the own-household income distribution at wave I. Figure 2 reports the results. In all our analyses, we cluster standard errors at the school level. We find positive and significant effects for lower-income students (bottom two deciles), null effects over the middle, and negative and significant effects for higher-income groups (top two deciles).[16] These results suggest that shifts in peers income composition can create different responses across the income distribution.

Figure 2. The share of low-income peers and effects on university completion over deciles of the own-household income distribution



Notes: This figure presents the marginal effects on the probability of completing university by wave IV of the survey from the leave-one-out mean (share) of low-income peers in the same-high school and cohort (wave I). The effects are calculated at each decile of the own-household income distribution at wave I.

To empirically shed light on the sharp cutoffs in effects, we calculate the gap between the individual logged household income and the school-cohort peer mean of logged household income to give the percentage difference (gap: $ln(Inc_{ics}) - ln(Inc_{-ics})$). In the Appendix Figure B.4, we present plots of the interquartile range, median, and mean for this gap over household income deciles. We see that students in the first two deciles are much further behind their peers than better off students. Even students in the third decile are considerably less far behind their peers than those in the first two deciles. Next, for the top household income deciles, we see that those in the ninth and tenth deciles are consistently much further ahead of their peers. Overall, we think these patterns on distance support our findings in Figure 2, and suggest that being further

[16]We also conduct the same analysis using the share of high-income peers as our treatment variable and we get a mirror image of Figure 2

away from one's peers drives the effects This does not, however, explain why being further away to either side should matter.

In the next section we turn to a more parsimonious specification, while in Section 4.3 we show that our empirical results are not explained by a variety of mechanisms that can be drawn from the literature. Hence, in Section 5 we develop a model of social comparison and students' effort choice that can rationalize our findings, and subsequently provide some evidence in support of its main mechanisms.

4.2 Baseline results: parsimonious specification

Based on the results by decile, we group the distribution of own-household income into the bottom 20th, middle, and top 20th. We then use these groups to disaggregate the effect from the share of low-income peers. In Table 2, we present the results across multiple specifications in columns (1) - (6), finding stable results across specifications. Interpreting our preferred specification (column 2), we find that for the bottom 20th of the household income distribution in high school, a 100% shift in the share of low-income peers yields a 18 percentage point (pp) increase in the likelihood of holding at least a four year degree by wave IV. For the middle group, we find null effects, and for the top 20th of household income the marginal effect is a 25pp decrease. A 100% shift, however, is not realistic. Interpreting these in standard deviation shifts (a 20% shift) translates the effect for the bottom 20th into a 3.6pp increase and for the top 20th into a 4.1pp decrease.

The estimates for the bottom and top 20th groups are significantly different across all specifications. One concern is that multiple hypothesis testing within and across specifications could lead to false rejections of the null (Clarke et al., 2020). To account for this, in the Appendix, Table D.1, we report Romano Wolf p-value adjustments across all specifications based on a block cluster bootstrap around schools. Although we obtain higher p-values, our results remain statistically significant at the 5% level for the bottom 20th group and at the 10% level for the top 20th group.

To give some context to the effect estimates, we compare them to the average probability of having at least a four year degree split across income groups. The overall average in our analytic sample is 33%, which breaks into 15% for those in the bottom 20th of the household income distribution in high school, 31% for the middle group, and 59% for the top 20th group. Thus, for the bottom 20th students the effect from the share of low-income peers amounts to a 24% increase from the group mean, whereas the effect within the top 20th group is only about 7%. We also compare these effects to conditional university completion gaps over gender and socioeconomic differences, detailed in the Appendix, Figure B.5.[17] These results are sizeable for low-income students and of similar magnitude to other interventions targeting low-income families and their children. For perspective, the magnitude of our effects is comparable to financial assistance programs, such as the Social Security Student Benefit Program, a large financial assistance program paid to children of deceased, disabled, or retired Social Security

[17]The standardized effect for the bottom 20th group amounts to about half of the gap between females and males, around 40% of the gap between university and non-university parents, and is similar in size to the gap between single and two-parent homes. Comparisons are similar looking at the top 20th group.

Table 2. Baseline effects on university completion: Share of low-income peers

	University Graduate					
	(1)	(2)	(3)	(4)	(5)	(6)
$SLP_{-ics} \times$ Bottom 20	0.18**	0.18**	0.16**	0.19***	0.27***	0.22**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)	(0.10)
$SLP_{-ics} \times$ Middle	0.01	0.02	0.01	-0.00	0.07	-0.02
	(0.07)	(0.07)	(0.06)	(0.06)	(0.09)	(0.07)
$SLP_{-ics} \times$ Top 20	-0.25**	-0.25**	-0.27**	-0.27**	-0.19	-0.29**
	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)	(0.13)
Peer Log(Inc) (SD)	Yes	Yes	Yes	Yes	Yes	Yes
Own-Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
School and Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Peer Effects (means)	No	Yes	Yes	Yes	Yes	Yes
Peer Effects (SD)	No	Yes	Yes	Yes	Yes	Yes
Own-Ability Polynomials	No	No	Yes	Yes	Yes	No
Ability Rank $\times$ Income Position	No	No	No	Yes	Yes	No
School-specific Cohort Trends	No	No	No	No	Yes	No
School-specific Income Trends	No	No	No	No	No	Yes
Mean University Graduation	0.33	0.33	0.33	0.33	0.33	0.33
Observations	11,165	11,165	11,165	11,165	11,165	11,165
R^2	0.241	0.243	0.263	0.264	0.273	0.253
Difference between B20 and T20	<0.001	<0.001	<0.001	<0.001	<0.001	0.002

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. SLP_{-ics} denotes the leave-one-out percentage share of peers from low-income households in cohort c and school s. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Estimates of marginal effects of SLP_{-ics} are for those in the bottom 20th percentile of household income, in the middle, and finally in the top 20th percentile of household income. Peer Log(Inc) (SD) denotes the standard deviations of peer log income. We always include a 3-degree polynomial of log household income in the own characteristics control. Ability rank means the ability rank within school cohorts.

beneficiaries in the US to finance post-secondary education (Dynarski, 2003). Effect estimates suggest that an offer of $1,000 in grant aid corresponds to an increase in the probability of high-school students attending university by about 3.6pp (Dynarski, 2003).

Next, in columns (3) - (4), we check our results against the inclusion of flexible controls for own-ability and rank. We include a quartic polynomial in the PVT scores and control for the peer (school-cohort) leave-one-out mean as well as the standard deviation in PVT scores (column 3). We also check that our effects are not driven by a rank mechanism, as a wide literature illustrates the importance of relative ability (Bertoni and Nisticò, 2019; Denning et al., 2021; Murphy and Weinhardt, 2020; Elsner and Isphording, 2017). Thus, we next add the PVT school-cohort rank, which we disaggregate by students' position in the bottom 20th, middle, or top 20th income group (column 4).[18] Our key results on the bottom and top 20th groups remain consistent and significant.

In columns (5) - (6), we now add school-specific trends to relax the assumption that selection factors are fully captured by the school fixed effects. First, in column (5), we include the

[18]In an additional checks against alternative mechanisms (see Section 4.3), we will go even further and allow for a wide range of non-linear peer ability effects and also consider income rank effects.

expansive specification from column (4) and allow for a linear trend within schools. This specification allows for families to select into schools based on time-varying factors that we proxy with linear-trends at school level, but is the most restrictive on the data. Second, in column (6), we use our preferred specification as in column (2) but allow for school specific trends across our defined income groups. In both cases, we find very similar results to those in our simpler specifications.

Finally, we consider a different outcome by using the natural log of individual income at wave IV. These results are reported in the Appendix Table B.4. We find that wave IV income improves for the bottom 20th household income group at high school in response to an increase in the share of low-income peers, while for the top 20th group, we see null effects on wave IV income. Note, that for top income students, the effect size on university completion as a percent of the mean is much smaller than it is for lower-income students. Also, it may be that those from higher parental income backgrounds are better positioned to maintain higher-income regardless of their university completion status. This question is beyond the scope of our paper. Nevertheless, the pattern of results suggests strong effects for the bottom 20th group that are different from the experience of the top 20th group.

Altogether, the results here suggest the presence of strong, heterogeneous effects stemming from peer income composition. We further examine additional heterogeneity within each income group across student characteristics using a causal forest (Athey et al., 2019). See the Appendix Section F for a detailed description of the method and results. We find that our pattern of results on the effects from the share of low income peers across income groups remains consistent when estimated with a causal forest (see the Appendix Figure F.1a). Additionally, we see that these peer effects within income groups are generally persistent across other student characteristics, and even across the ability distribution.

Next, we examine whether our results could be driven by alternative peer mechanisms, such changes in peer quality or ability, by controlling for the peer mean of income and peer ability and a range of additional factors. We then conclude by testing the credibility of our baseline results through a series of robustness checks in Section 4.4.

4.3 Results explained by common peer effects in the literature?

Later we will provide a model along with empirical evidence to rationalize the effects we find through an interpretation of reference dependence and motivation or frustration. Before doing so, we first examine whether commonly observed mechanisms in the literature can explain the patterns we observe on income differences in peer groups. We investigate whether our heterogeneous peer income effects seem to pick up the following mechanisms: *heterogenous responses to differences in peer quality; responses from teachers to changes in peer income composition; changes in disruptive behaviors; and parental responses to changes in the peer income composition.*

Peer quality and rank. One possibility is that changes in the share of low income peers captures changes in peer quality that the literature has shown effects students non-linearly by ability type (Booij et al., 2017; Duflo et al., 2011; Feld and Zölitz, 2017). This literature finds that non-linear peer ability effects may stem from changes in teaching practices that are more or less conducive to different ability groups. It also points out factors directly related to peer interactions – help studying, better information, etc. – that can generate differential responses to peer ability. Generally, the evidence suggests that students do not benefit from mixing by ability, implying that tracking by ability can be optimal. Our results on the share of low-income peers could be explained by this type of mechanism given the correlation between family income and ability. However, here we find no evidence for this.

In Table 3, we control for non-linear peer ability effects in several ways. We begin, in column (1), by adding to our preferred specification the peer mean of income interacted with own-income positions, because changes in the peer mean of income could capture peer ability composition. In column (2), we directly control for peer mean ability – based on PVT scores – and the standard deviation of peer ability interacted with own-income positions. Next, in column (3), we introduce peer ability heterogeneity around own-ability by adding interaction terms of peer mean ability, peer SD ability, and own-ability. In columns (4) - (5), we consider interactions of quartiles of a school's position in the school mean ability distribution and likewise for the school's position in the SD ability distribution. This is motivated by suggestions in Denning et al. (2021) aimed at capturing more effectively potential non-linear effects from reactions to the distribution of ability in the school. Across all of these specifications our estimates of the share of low-income peers remain remarkably consistent with our baseline estimates.

Table 3. Accounting for heterogeneous responses to peer quality and rank

	University Graduate						
	Non-linear peer ability effect					Rank effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SLP_{-ics} × Bottom 20	0.22**	0.18***	0.17**	0.17**	0.17**	0.22***	0.23***
	(0.10)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
SLP_{-ics} × Middle	0.06	0.00	0.01	0.02	0.02	-0.00	-0.01
	(0.09)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
SLP_{-ics} × Top 20	-0.20*	-0.28**	-0.27**	-0.26**	-0.26**	-0.27**	-0.29**
	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.12)	(0.12)
Peer Effects (means)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Effects (SD)	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Log(Inc) (means) × Income Position	Yes	No	No	No	No	No	No
Own-Ability Polynomials	No	Yes	Yes	Yes	Yes	No	Yes
Peer Ability (means & SD) × Income Position	No	Yes	No	No	No	No	No
Peer Ability (means) × Peer Ability (SD) × Own-Ability	No	No	Yes	No	No	No	No
School Ability Quartiles (means) × Own-Ability	No	No	No	Yes	Yes	No	No
School Ability Quartiles (SD) × Own-Ability	No	No	No	No	Yes	No	No
Income Rank × Income Position	No	No	No	No	No	Yes	Yes
Ability Rank × Income Position	No	No	No	No	No	No	Yes
Observations	11165	11165	11165	11165	11165	11164	11164
R^2	0.243	0.263	0.263	0.264	0.264	0.243	0.264

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Standard errors are in parentheses and clustered at the school level. School ability quartiles (means) are the quartiles of schools based on the school-level peer mean ability. School ability quartiles (SD) denote the quartiles of schools based on the school-level standard deviations of peer ability. Income rank denotes the rank of household income within school cohorts while ability rank denotes the rank of ability within school cohorts.

Finally, ability rank effects are known to exist separately from standard ability effects possibly from a social comparison or a learning about ability mechanism (Elsner and Isphording, 2017; Kiessling and Norris, 2022). Thus, we expand our specification to account for ability ranks. While we have already flexibly allowed for ability rank effects in Table 2, we reconsider ranking concerns by allowing for both ability and income rank effects disaggregated across the income distribution. As is shown in columns (6) - (7) of the Table 3, our results are not sensitive to ability or income rank effects. Thus, our main results on the share of low-income peers appear distinct from, and insensitive to, both heterogenous responses to peer ability and rank effects.

Teachers, distuptive peers, and parents. Beyond non-linear peer ability and rank effects, we examine a broader set of mechanisms that could potentially account for the observed heterogeneous effects. These include changes in teacher behavior, shifts in disruptive peer behavior, and parental responses to peer income composition. We provide a thorough discussion of each of these and report our results in the Appendix, Section E.1. Across all analyses, we find no evidence that the heterogeneous effects from the share of low income peers documented in Sections 4.1 and 4.2 are explained by this wide range of plausible mechanisms.

4.4 Robustness checks

In this section, we report a series of additional analyses to probe the robustness of our results.

Definitions of the share of low-income peers. We define low-income households as those whose household income is either below the 1994 poverty threshold or in the bottom third of the income distribution for a given family size. We then calculate the leave-one-out share of low-income peers at the school-cohort level based on this definition. Yet, other definitions of low-income households are conceivable for assignments of the share of low-income peers for those students who are in the same school cohort and have the same household income. For instance, we could define low-income households based on (i) the bottom 20th percentile of the income distribution for a given family size, (ii) below the median of the income distribution for a given family size, or (iii) the bottom third of the household income distribution based on school region, school urbanicity, and family size (grouping households whose family size is equal or larger than 5).

Of these, we expect most results to be similar except for the below median definition, which might introduce measurement error by misclassifying a larger share of students as low-income peers when they are not, implying it should return smaller and less precise effects. Moreover, definitions that shrink the size of the low-income peer groupings have another tradeoff, as they reduce the degree of variation available within schools, thereby potentially yielding less efficient results. In Appendix Table C.1, we compare results from these different definitions. We find similar effects across definitions except for the below median definition, where we find weaker effects as expected, and some less efficient results where the definitions are more stringent. Importantly, the results – absent the definition by the median – are stable. Generally, our current definition of low-income households seems reasonable to capture the stratification of household income.

Non-linearity in household income. In our main specification, we adopt a cubic polynomial in logged household income to take the relation of university graduation and own-income into account. Yet, one might worry that we have not captured all the relevant non-linearity between our outcome and logged household income. In Appendix Table C.2, we therefore examine different polynomials up to the sixth order. We find that our results are highly robust regardless of the degree we control for. Moreover, we include a specification with indicators for each ventile level of the logged household income, which non-parametrically controls for different own-income levels, and find our results remain unchanged.

Subsample by income groups. In our main specification, we disaggregate our results by own-household income groups for being in the bottom 20th, the middle, and the top 20th. While we gain efficiency from this specification, we do not allow all covariates to vary by each subsample. In Appendix Table C.3, we examine the consistency of our results by splitting the sample over each of the income groups we use. We start from our baseline specification and then add a quartic own-ability polynomial and the school-cohort ability rank as an additional check. We find that our subsample results for the share of low-income peers are consistent with our main results. While the results slightly lose some efficiency, we find the point estimates are quite stable and robust.

Placebo tests. Our identification strategy assumes that the share of low-income peers is as good as randomly assigned conditional on own income and school and cohort fixed effects. One way to test against failures of this assumption is with placebo tests. In Appendix Table C.4, we reproduce our main specification results with an alternative treatment variable and then with an alternative outcome variable. As for the placebo treatment, we take the share of low-income peers within the same school but from a different cohort with a 1-year or 2-year time gap. As for the placebo outcome, we use an indicator for ever repeating a grade in the past. This is a pre-treatment placebo outcome. Given that our identification assumptions hold, we would not expect a link to past repetition of school grades. For the bottom 20th group of own-household income, both placebo tests yield an expected zero. For the top 20th group, we do find some correlation between the placebo treatment and our university graduation outcome, but this effect disappears once we control for school-specific income trends. As is shown in column (6) of Table 2, our point estimates stay consistent when we control for school-specific income trends. These placebo tests are highly consistent with our identifying assumptions and suggest that our main model is well identified.

Attrition. In wave IV, approximately 14 years after the treatment in wave I, about 78% of the baseline sample remains.[19] Appendix Table C.5 shows that attrition patterns do not differ by

[19]Note that the baseline sample is defined after our initial set of sample selection criteria but before dropping those missing information on education level at wave IV.

treatment status across own-household income groups, regardless of the school and cohort fixed effects we control for. We further assess the robustness of our results to accounting for attrition in two ways. First, we calculate treatment effects using inverse probability weighting (IPW), where the weights are calculated as the predicted probability of being in the wave IV follow-up sample (based on the main specification controls and an additional variable for whether the family was willing to move).[20] Second, we use the wave IV sampling weights provided by Add Health to adjust for non-response in longitudinal models. Our results survive parametric corrections for attrition using either IPW or sampling weights in Wave IV.

Random sampling of students per school. The impact of sampling errors on estimates is not entirely clear in a nonlinear model with group means. We approach this in two ways. First, we follow Sojourner (2013), and re-weight the share of low income peers by the observed share of students in each school-cohort and also control for this observed share as a covariate. The results are reported in Table C.6 with additional discussions also in the Appendix. We find that our conclusions remain the same, and if anything, our results are under-estimated for both the bottom and top 20th income groups.

Second, we assess the consequences of observing a random sub-sample of students per school using a simulation. The data generating process (DGP) is specified in Appendix C and is based on our estimated values for the share of low income peers. We simulate 500 schools of 240 students and decrease the share of students in our sample from full saturation, where all students in a school are sampled, to a situation where we observe only 10% of all students.[21] Our simulations show that it leads to attenuation for the bottom and top 20th income group students, while the middle income group shows a small upward bias (see Appendix Figure C.1). We then repeat the simulations based on estimates for subgroups by income and find that sampling variation attenuates the estimates when the true coefficient is non-zero for both the bottom and top 20th income groups, and has no effect on the middle income group where the true effect is set to 0 (see Appendix Figure C.2).

Measurement error in income. We then turn to measurement error in our income measure. Specifically, we assess how our estimates change when we introduce noise to the measure of income, i.e., we measure $ln(\tilde{Inc}) = ln(Inc) + \phi \cdot v$, where $ln(Inc) \sim \mathcal{N}(3.5, 0.85)$, $v \sim \mathcal{N}(0, 0.85)$, and $\phi \in [0, 1]$. Thus $\phi = 0$ corresponds to situations where we have no measurement error, while $\phi = 1$ corresponds to situations where we allow as much measurement error as noise in our income measure. This measurement error then translates into measurement error in the share of low income peers that each student faces. We then combine the measurement error in income with random sampling of students, taking the situations with 100% and 30% sampling, respectively. In Table C.7, we report estimates of the effect for the share of low income peers among the bottom 20th, middle, and top 20th income groups along with the ratio of the estimated effect to the true coefficient in parentheses. For the middle group, the ratio is not

[20] We then replicate our results with IPW weights using the specifications in column (2) and column (6) of Table 2.
[21] Our approach is adapted from the designs used by Elsner and Isphording (2017) and Kiessling and Norris (2022) to assess measurement error for ability rank effects.

reported because the true coefficient is 0. Our simulation results show that for the bottom 20th and top 20th income groups the effects are attenuated, resulting in an underestimation of the true effects.

Next, we propose a novel explanation to rationalize our findings. It is based on reference dependence and the idea that social comparison among students can generate both frustration and motivation depending on a student's relative position in the income distribution.

5 A model of social comparison and student effort

We propose a theoretical model of student effort choice towards the achievement of an educational outcome. The model provides a lens to understand how exposure to income differences may generate the patterns we have observed.

Our model is based on two premises. First, we think of income as a salient and observable characteristic signaling students' capacity, in-line with the evidence we present in Section 2. We think of capacity here as reflecting differences in opportunity even when holding raw ability fixed. Second, we consider a possible non-monotonicity in the effect that social comparison on this capacity dimension can have on students' behavior. Our model provides a novel perspective on how exposure to income differences, and therefore to inequality of opportunities, may affect students' effort and their educational attainment. Thus, we consider how income differences among peers may affect the contextual environment students live within.

To capture this, we build on reference dependence (Kahneman and Tversky, 1979; Tversky and Kahneman, 1991) and social comparison (see e.g. Clark et al. (2010) and Card et al. (2012)). More precisely, we assume that a student's reference point for educational attainment is, at least in part, determined by the capacity distribution of their peers—a proxy for what others can achieve. Further, in the spirit of Genicot and Ray's (2017; 2020) model of socially determined aspirations, we provide a framework in which changes in the capacity distribution of peers can have heterogeneous effects across students depending on their relative position in the income distribution.

We place particular emphasis on the effects of changes in the composition of peers' family income on students' behavior. The theoretical framework explains a contextual effect, but alternatively, our framework can also be considered a reduced-form model of students' best response with non-linear peer effects in which students use peers' income as a salient and observable indicator of what others can achieve.[22] In fact, the reference point for educational attainment in our model is an artifact which, together with our assumption of reference-dependent preferences, enables us to capture the effect of inequality in opportunities on a student's utility.

[22]An extension of our framework could also incorporate the possibility that students' reference point for social comparisons is influenced by their beliefs about peers' effort (in addition to peers' capacities). This would then generate strategic complementarities between students' effort, as in game-theoretic foundations of social interaction peer effects (see e.g. Boucher et al. (2024)). Such an extension would, however, require a more specific form of the students' reference point than the one we consider below, as well as the characterization of an equilibrium in which students' beliefs are consistent. We believe this extension to be interesting, but also beyond the scope of the model developed in this paper. Moreover, it can be deduced that the predictions we derive to rationalize our empirical findings, which are based on changes in the composition of peers' capacities, will also hold in a more elaborated model with strategic complementarities in effort.

5.1 Preferences

Students are endowed with initial capacity θ defined as the combined set of factors that enable a student to translate effort into educational outcomes.[23] In particular, we assume that capacity is a strictly increasing function of both ability s and income I. That is, $\theta \equiv \theta(s, I) > 0$, with $\theta_s > 0$ and $\theta_I > 0$, and that the only source of heterogeneity in capacity in our model is income.[24] Denote the distributions of income and capacity by F^I and F^θ respectively. Our assumption implies that F^θ is a transform of F^I: the distribution of capacities a student faces in school captures differences in within-school income compositions.

Students choose effort e to achieve an educational outcome y, realized attainment, given by $y \equiv y(e, \theta) = \theta e$. Further, to capture the effect of social comparison and inequality of opportunity on behavior, we assume that students compare their realized outcome in relation to a reference outcome r which is influenced by the capacity distribution they face. In particular, we assume r to be positively related to someone's own capacity as well as to the capacity of their peers. More formally, $r \equiv r(\theta, F^\theta)$ with the following properties: i) $r_\theta > 0$; ii) $r(\theta, \hat{F}^\theta) > r(\theta, F^\theta)$ when $\hat{F}^\theta$ first-order stochastically dominates F^θ; and iii) $r(\lambda\theta, F^{\lambda\theta}) = \lambda r(\theta, F^\theta)$ for all $\lambda > 1$, where $F^{\lambda\theta}$ denotes the distribution of θ when all capacities increase by λ. This last assumption implies that if all capacities increase by the same proportion, then r increases by the same proportion.[25]

Students' preferences are characterized by the following additively separable utility function:

$$u(e, y, r) = b(y) - c(e) + \mu(y - r), \tag{2}$$

where $b(y) = y^\alpha$, $\alpha \in (0, 1)$, captures the benefit from achieving the outcome y; $c(e) = e^2/2$, is the cost of effort (where the marginal cost is normalized to e); and $\mu(y - r)$ captures the effect of social comparison over outcomes on a student's utility. We assume μ to be a reference-dependent gain-loss function, such that $\mu''(y - r) < 0$ if $y > r$ (i.e. concavity over gains) and $\mu''(y - r) > 0$ if $y < r$ (i.e. convexity over losses):

$$\mu(y - r) = \begin{cases} [y - r]^\beta & \text{if } y \geq r \\ -[r - y]^\beta & \text{if } y < r; \end{cases} \tag{3}$$

where $\beta \in (0, 1)$.[26] The properties of μ, combined with our assumptions on r, are a central component of our model, capturing the effect of inequality of opportunity, due to income differences

[23] Since our results are not explained by non-linear peer ability effects, we want to use the theory as a lens to think about our empirical findings.

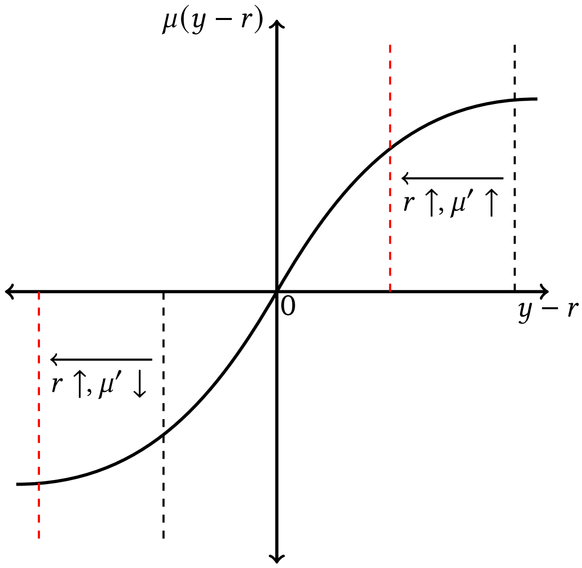
[24] This assumption is made for simplicity and to capture the fact that income is more salient and easily observable than ability. For instance, in the absence of complete information about peers' abilities, this assumption would imply that students' use income as a proxy for capacity. This set up also enables a closer matching between our theoretical model and the empirical analysis, in which we control for peers' ability.

[25] Our formulation of the properties of r is inspired by the model of socially determined aspirations in Genicot and Ray (2017, 2020). In particular, $r(\theta, F^\theta)$ satisfies both "scale-invariance" and "social monotonicity".

[26] This formulation is in the spirit of Kahneman and Tversky (1979) value function under riskless choice (Tversky and Kahneman, 1991). In particular, our function μ displays both "reference dependence" and "diminishing sensitivity", but it does not feature "loss aversion". Note that while both reference dependence and diminishing sensitivity are crucial ingredients of our model, the consideration of loss aversion—despite adding one additional parameter and layer of complexity—would not affect our qualitative predictions. Moreover, while there is ample evidence of the existence of loss aversion in the evaluation of monetary/material payoffs, less is known about its role in less tangible domains such as that of educational outcomes.

among peers, on students' behavior. Figure 3 plots μ as a function of $y-r$ when θ and e are fixed, and shows the partial effect of an increase in the reference outcome r on the slope of μ: the marginal returns of effort that stem from reference dependence. For instance, consider a student with a relatively high θ' and such that $y > r$, implying they perceive additional satisfaction from achieving the educational outcome y (the student is in the gain domain, the upper-right panel of Figure 3). In this case, an increase in peer income, and therefore peer capacity, generates an increase in the marginal returns to effort (r increases and the slope of μ becomes steeper), and an increase in effort will increase utility. As we will formally establish later, this effect can be interpreted as greater motivation stemming from a reduction in inequality of opportunity between the student and their peers. Instead, consider a student with a relatively low θ'' and

Figure 3. The Gain-loss Function



such that $y < r$, implying they perceive a sense of frustration, which negatively affects utility (the student is in the loss domain, the lower-left panel of Figure 3). Here, an increase in peer income generates a decrease in the marginal returns to effort (r increases, but the slope of μ becomes flatter), implying that decreasing effort will increase utility: as the inequality in opportunity between the student and their peers widens, higher frustration dampens the incentive to exert effort.

In the remainder of this section we will formally characterize the consequences of these changes in the reference outcome on a student's choice of effort. Subsequently, we will establish how shifts in peers' income can affect effort differently depending on the student's relative position in the income distribution.

5.2 Capacities, Peers, and Students' Effort

Consider a student endowed with capacity θ, facing capacity distribution F^{θ}, and with reference outcome r, that needs to choose effort e to maximize their utility as given by (2). The first-order conditions characterizing this maximization problem are given by:

$$\alpha[\theta e]^{\alpha-1}\theta + \beta[\theta e - r]^{\beta-1}\theta = e \qquad \text{if } y > r, \tag{4}$$

$$\alpha[\theta e]^{\alpha-1}\theta + \beta[r - \theta e]^{\beta-1}\theta = e \qquad \text{if } y < r, \tag{5}$$

where the left-hand side captures the marginal benefit of exerting effort, while the right-hand side is the marginal cost. The solution, denoted by $\tilde{e}(\theta, r)$, is the level of effort at which these are equal.[27] Because the marginal benefit of effort crucially depends on the gap $y - r$, we know from the preceding discussion that the properties of $\tilde{e}(\theta, r)$ might differ depending on whether the student is experiencing frustration $y < r$, or greater motivation, $y > r$, in achieving the educational outcome y.

To see this, consider a student that is currently perceiving additional satisfaction so that $y > r$ at the optimal effort, which is the solution to (4) and denoted by $\tilde{e}^+ \equiv \tilde{e}(\theta, r)^+$. To understand how changes in the reference outcome can affect behavior in this case, suppose that r increases. For example, this could stem from the student being exposed to peers with higher income, and therefore higher capacities. In this case, the marginal benefit of increasing effort is higher, implying that the student will exert more effort to achieve a better educational outcome. However, higher effort is increasingly costly and the marginal benefit of achieving better outcomes decreases. As we formally establish below, this implies there exists a threshold reference outcome r^* beyond which utility is maximized by the solution to (5), denoted by $\tilde{e}(\theta, a)^-$. In this case, $y < r$, the student perceives frustration, and further increases in r will decrease the marginal benefit of effort, resulting in lower effort and worse educational outcomes.

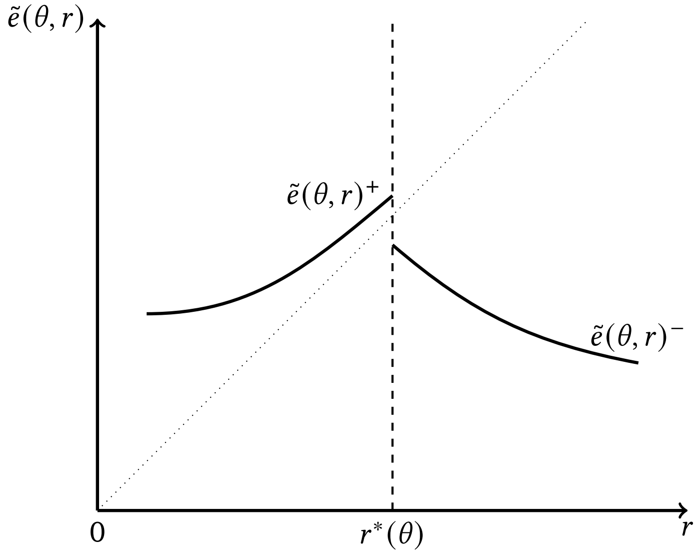
Proposition 1. *For a given θ, there exists a unique reference outcome r^* such that: if $r < r^*$, then $y(\tilde{e}^+, \theta) > r(\theta, F^\theta)$ and optimal effort $\tilde{e}(\theta, r)^+$ is increasing in r; and if $r > r^*$, then $y(\tilde{e}^-, \theta) < r(\theta, F^\theta)$ and optimal effort $\tilde{e}(\theta, r)^-$ is decreasing in r. Moreover, $r^* \equiv r^*(\theta)$ is increasing in θ.*

Proposition 1 establishes that the effect of changes in a student's reference outcome is non-monotonic: effort and educational outcomes are increasing in r for all $r < r^*$ and decreasing in r for all $r > r^*$. This relationship is plotted, for a given θ, in Figure 4. In fact, since students are heterogeneous in θ, due to differences in income, there exists a distribution of r^*: each student has a different reference threshold depending on their capacity, and the greater is their capacity, the higher this threshold will be. Intuitively, the greater is a student's capacity, the larger the increase in their peers' capacity, and therefore r, will have to be before they feel frustrated by their peers having greater opportunities to achieve higher outcomes.

The results established in Proposition 1 characterize a mapping between effort and reference outcomes, by taking as given an individual student's capacity θ. However, both the reference outcome $r(\theta, F^\theta)$ and the reference threshold $r^*(\theta)$ are functions of θ. This suggests that for a given distribution of capacities F^θ, whether a student perceives satisfaction, or a sense of frustration, crucially depends on their capacity in relation to the ones of their peers, that is,

[27] Our assumptions over μ imply that there may be at most two solutions when $y < r$. To proceed, we only consider the one according to which a student's effort would be decreasing in its marginal cost: a student with higher marginal cost would exert less effort than a student with lower marginal cost (note, however, that marginal cost is normalized to e in this model for simplicity). See Appendix A for more details.

Figure 4. Optimal Student Effort



it depends on their position in the income distribution. The following proposition formally establishes this role for a student's initial capacity endowment.

Proposition 2. *For a given F^θ, there exists a unique threshold θ^* such that, for all $\theta < \theta^*$ then students are frustrated, while for all $\theta > \theta^*$ then students are satisfied.*

Proposition 2 states that students with lower capacities are more likely to be in the frustration zone than students with higher capacities. This result bears important implications for the effect of changes in the composition of the peer capacity distribution on students' behavior and educational outcomes. For instance, being exposed to peers with higher capacities and opportunities may be beneficial for students at the highest end of the capacity distribution, but detrimental for students at the lowest end of the capacity distribution.

5.3 Predictions and Empirical Counterpart

At this stage, it is possible to use the results just established to illustrate how our model can rationalize our empirical findings. First, note that our assumptions on the determinants of students' capacities immediately imply that, for a given income distribution F^I, there exists a unique threshold income, which we denote by I_F^*, such that students with income $I < I_F^*$ are frustrated, while students with income $I > I_F^*$ are in the satisfaction zone. This also implies that we can express the reference outcome in terms of income: $r = r(I, F^I)$. Next, we can classify students in terms of their relative position in the income distribution F^I. For a given $\varepsilon > 0$, where ε is large enough, denote with $I_F^l \equiv I_F^* - \varepsilon$ and with $I_F^h \equiv I_F^* + \varepsilon$, and define "low income" students those endowed with $I \leq I_F^l$, "high income" students those endowed with $I \geq I_F^h$, and "middle income" students those endowed with $I \in (I_F^l, I_F^h)$.

In the next proposition, we characterize the response of students to a change in the composition of the income distribution they face, which mimics our empirical analysis. In particular, we will do a comparative statics exercise across the income groups defined above, where we shift the income distribution from F^I to G^I such that G^I contains a larger share of low income peers. Hence, we consider a distribution G^I that is first-order stochastically dominated by F^I. In our model this implies that students' reference outcome will be lower, with heterogeneous effects across the income distribution. For simplicity, we will assume that even the richest of the low income students remains frustrated.

Proposition 3. *Consider a shift in the income distribution from F^I to G^I, such that $G^I > F^I$ and $r(I_F^l, G^I) > r^*$. Low capacity students will increase effort and achieve better educational outcomes, high capacity students will decrease effort and achieve worse educational outcomes, while the effect on middle income students is ambiguous: while those endowed with $I \in (I_F^*, I_F^h)$ will decrease effort, those endowed with $I \in (I_F^l, I_F^*)$ will increase effort, only as long as $r(I, G^I) > r^*$.*

Proposition 3 establishes the existence of heterogeneous effects of a change in the composition of peers' income on students' educational attainment, which is conditional on their relative position in the income distribution. Through the lens of our model we can interpret this result as follows. An increase in the share of low income peers will reduce the inequality of opportunity from the perspective of low income students, who will now feel less frustration and greater motivation (as the marginal benefit of effort is greater), leading to an increase in effort and higher educational outcomes. On the other hand, from the perspective of high income students there is an increase in the inequality of opportunity which leaves them even "further ahead of their peers". This generates a loss of motivation (as the marginal benefit of effort is smaller) and a drop in effort, which ultimately translates into lower educational attainment. For middle income students, the effect is qualitatively ambiguous: some of these students will see a reduction in the inequality of opportunity and feel less frustrated, while others although feeling satisfied to be ahead, will loose motivation and decrease their effort.

This result rationalizes our empirical finding that, controlling for students' ability, an increase in the share of low-income peers has positive effects on low-income students, negative effects on high-income students, and null effects on middle-income students (see Figure 2 and Table 2 in Section 4). Moreover, our theoretical model suggests a potential mechanism based on student effort and generated by heterogeneous effects on students' motivation and frustration depending on their relative position in the income distribution. In the next section, we investigate the empirical plausibility of this mechanism.

6 Results: effort, frustration, and motivation

In this section we aim to provide some empirical support to the main mechanism highlighted by our model. Hence, we now examine short-run outcomes related to high-school performance, followed by measures related to frustration and motivation.

6.1 High-school performance

Although we lack a good measure of pure effort, Add Health has excellent measures of high-school performance from transcript data, which we use to proxy effort. Our baseline results on university graduation and our model predictions suggest there should be non-monotonic effects on performance. We start with self-reported grades and then use high school transcripts collected by Add Health for all participants in the wave III survey, who agreed, and for whom the transcripts were accessible. To overcome attrition at wave III and from wave III into the transcript sample, Add Health constructed specific non-response weights for the education transcript data, which we use in the following analysis.[28] We calculate each person's cumulative GPA from the year of their wave I survey (time of our treatment) to the end of high school.[29] Also, we construct separate indicators for whether someone chose to take an advanced course in Math, in Science, or in English anywhere from the time of their wave I survey to the end of high school.[30]

In Table 4, we report effect estimates for a shift in the share of low-income peers using our baseline specification. With self-reported GPA (column 1), we observe null effects, but with transcript cumulative GPA (column 2), we observe a strong, positive increase in GPA for the bottom 20th group. We also see a positive, but smaller, effect for the middle-income group and a null for the top 20th.

Table 4. GPA and advanced Courses

	GPA		Advanced Courses				GPA		
	Self	Transcript	Math	Science	English	More than one	Transcript	Transcript	Transcript
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
SLP_{-ics} × Bottom 20	0.05	0.81***	0.36***	0.25	0.13	0.47***	0.75***	0.96**	0.60**
	(0.15)	(0.25)	(0.13)	(0.16)	(0.22)	(0.17)	(0.24)	(0.43)	(0.28)
SLP_{-ics} × Middle	-0.07	0.49**	0.08	0.01	0.05	0.14	0.49**	0.33	0.51**
	(0.13)	(0.21)	(0.12)	(0.14)	(0.23)	(0.14)	(0.20)	(0.27)	(0.25)
SLP_{-ics} × Top 20	-0.18	0.04	0.10	-0.30*	0.23	-0.00	0.02	0.23	-0.03
	(0.17)	(0.29)	(0.15)	(0.18)	(0.25)	(0.17)	(0.28)	(0.40)	(0.39)
$Peer\ \ PVT_{-ics}$							-0.01**	-0.00	-0.02**
							(0.01)	(0.01)	(0.01)
$Peer\ \ PVTSD_{-ics}$							0.01	0.00	0.01
							(0.01)	(0.01)	(0.01)
Edu non-response weights	NA	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ability Tracking Split	NA	NA	NA	NA	NA	NA	NA	Yes	No
Mean Dep Var	2.77	2.41	0.40	0.45	0.23	0.59	2.41	2.41	2.40
Observations	11074	7297	7309	7277	5183	7318	7297	4409	2771
R^2	0.20	0.28	0.25	0.22	0.26	0.24	0.32	0.31	0.33

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Column (1) shows the effects of share of low-income peers on self-reported GPA from Wave I In-Home data while column (2) shows the effects on average GPA calculated from the first interviewed year to the end of the high school from Wave III high school transcript data. Columns (3) - (6) show the effects of share of low-income peers on the taking rate of advanced courses of Math, Science, English, and if ever took more than one advanced course. Columns (7) - (9) control for the distribution of peer ability where PVT is Picture Vocabulary Scores and SD is standard deviation. In columns (8) and (9), we stratify the sample by schools who report to use ability tracking for English and Language Arts. We use specific educational sampling weights constructed to adjust for transcript non-response as well as survey non-response in column (2) - (9). We trim our data to our analytic sample as in Table 2.

[28] Wave III was collected over 2001 and 2002 with participants in young adulthood aged roughly 18-24.

[29] For example, this means that for someone in 10th grade at the wave I survey we calculate their GPA from 10th-12th, while for someone in 12th at the wave I survey we use only their 12th grade scores.

[30] Core required credits for graduation are set by each state, but advanced courses are often at the choice of the student in an effort to pursue University entrance. For Math, this is defined by taking pre-calculus or calculus. For Science, it is whether one took advanced science or physics. For English, it is whether one took an honors English class.

We then look at the choice to take advanced courses (columns 3 - 6). The bottom 20th income group continues to respond positively to an increase in the share of low income peers. They are significantly more likely to take advance Math and to take more than one advanced subject. We see no change for the middle income group, and the top 20th group have mainly null results with a marginally significant negative effect on taking Advanced Sciences. We also repeat the Romano Wolf p-value adjustment conducted at the baseline to check that our inference is not driven by multiple hypothesis test bias (see Table D.2 in the appendix). We find that the key results here for the bottom 20th group survive this adjustment.[31]

While the results here point toward effort responses, they could rather be explained by grading on a curve. If low-income students tend to have lower grades than high income students, then having more low-income students in a cohort means that these students are on average in classes with lower overall grades. In this case, grading on a curve would make these students appear to have higher grades. To check this, in columns (7) - (9) we compare students who face similar distributions of ability in their school-cohort – controlling for both the peer leave-one-out mean in PVT scores and its standard deviation – but who have variation in the share of low-income peers. The idea here is that they will, on average, face similar grade distributions, thereby making it unlikely that the remaining effects from shifts in the share of low-income peers are due to grade inflation

The effect estimates for the share of low-income peers upon controlling for the peer ability distribution (column 7) remain essentially unchanged. We also see that an increase in the peer mean of ability (PVT scores) leads to weakly lower GPA (about 6.7 points lower for a standard deviation increase in peer ability). This negative effect on peer ability would be consistent with a grading on the curve mechanism. If so, then this effect on peer ability should disappear in schools which track by ability. In column (8), using schools who report to track by ability on English and Language Arts, we see this is indeed the case.[32] Moreover, the effects from the share of low income peers remain consistent across this stratification (columns 8 and 9). Overall, we see no evidence that our results are driven by a grading on a curve rather than an effort or motivation mechanism.[33]

6.2 Frustration and motivation

Our model implies that changes in income differences can affect students through frustration and motivation. To proxy these, we use measures of self-esteem, relative intelligence (self) rating, depressive symptoms (the CES-D scale), and a scale we formed for motivation.[34] Details

[31]We have restricted the sample to those present in our baseline analysis, meaning we drop those who are missing data for university completion. In the Appendix Table E.4, we also report the results where we include even those who are not present in the baseline analytic sample. These are generally similar to our results in Table 4 and qualitatively yield similar conclusions.

[32]The survey does not provide information about whether schools track by ability on other dimensions.

[33]We also explore outcomes on self-reported risky behaviors. The evidence on risky behaviors is inefficient, with a clearer suggestion of an increase in risky behavior for adolescents from higher income families as the share of low income peers increases, while we see null or negative effects on lower income adolescents. We describe these results in more detail in the Appendix Section E.3.

[34]Our motivation scale is an aggregate of two questions about how often the student has problems paying attention in school and getting their homework done. We scale these so that higher values imply less trouble. We recognize that this may also capture effort but it also can capture a degree of motivation.

for these are reported in Table B.3 of the Appendix. We see self-esteem and depressive symptoms as particularly good proxies, because they relate to pessimistic beliefs on the returns to effort, which, if too low, can lead to withdrawal (De Quidt and Haushofer, 2019; Kiessling and Norris, 2022).

In Table 5, we show that the effects from the share of low-income peers on these measures are non-monotonic across students' income groups. Students in the bottom 20th improve on self-esteem (column 1, significant) and self-perception of intelligence (column 2, weakly significant), while we continue to find null effects for middle-income students. Students in the top 20th see an increase in depressive symptoms (column 3, weakly significant) and a decrease in our measure of motivation (column 4, significant).

Table 5. Frustration and motivation

	Self-Esteem	Intelligent Feeling	CES-D scale	Motivation
	(1)	(2)	(3)	(4)
$SLP_{-ics} \times$ Bottom 20	1.75**	0.34*	0.94	-0.10
	(0.85)	(0.20)	(1.44)	(0.17)
$SLP_{-ics} \times$ Middle	0.98	-0.01	0.74	-0.21
	(0.80)	(0.18)	(1.06)	(0.16)
$SLP_{-ics} \times$ Top 20	0.15	-0.00	3.11*	-0.52***
	(1.04)	(0.26)	(1.78)	(0.19)
Mean Dep Var	28.56	3.9	11.02	28.56
Observations	11134	11151	11154	11164
R^2	0.088	0.111	0.092	0.069

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the school level. School and cohort fixed effects are included in all specifications. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. *Self-esteem* is measured from 7-items that we base on discussions in (Rosenberg, 1989) and higher values imply better self-esteem. *Intelligent feeling* is a student's perception of their relative intelligence. The *CES-D scale* measures depressive symptoms where higher values imply worse mental health. Finally, *motivation* is composed of students' report on a 0-4 scale of how frequently they do not pay attention in school and a second 0-4 scale on not getting homework done. We reverse code these so that higher values imply they pay more attention and get homework done more frequently and then take the average of these two. Details of those variables can be found in the Appendix, Table B.3.

Interpreting in aggregate across all four measures in Table 5, the effects we observe here are consistent with the predictions of our model. Changes in income composition generate heterogenous patterns of frustration and motivation. Also, our model is unique in the non-monotonic predictions it makes. For instance, if changing income composition only changed the structure of academic rank, then higher income students are likely to improve in rank as the share of low income peers increases. No mechanism in the literature on ranks (Murphy and Weinhardt, 2020; Elsner and Isphording, 2017; Kiessling and Norris, 2022) predicts worse outcomes among the top students.[35] Introducing social comparisons via reference-dependent preferences, as in our model here, brings this to focus and the empirical patterns are supportive its predictions.

[35]Also, as discussed earlier, we have controlled in several ways for rank effects and did not find them to explain our results for either the bottom or top income students.

7 Social cohesion: avoiding harm from income differences

In this final section, we ask what can be done to mitigate harmful effects from exposure to income differences. Matching lower income students to better environments may in fact be desirable but only if it opens opportunities. Theoretical work on networks suggests that homophily can prevent the flow of information and opportunity across groups (for a review, see Jackson, 2021). For instance, a low-income student placed into a higher income school where the network is highly segmented by income groups, will be less likely to have network links with high income students and therefore not receive information nor experience any complementaries in effort. We view this as a low social cohesion environment consistent with an observed link between perceptions of school climate and network centrality (Alan et al., 2021b). Moreover, recent evidence shows that improving social cohesion improves student outcomes for both worse and better off students (Alan et al., 2021a).

Better network integration could dampen the social comparison mechanism highlighted by our model, which could generate the non-monotonic pattern we observe. This could work through simply allowing a student to put less weight on their peers' income distribution when doing social comparisons. It also may allow students who are unsure about the true abilities and opportunities of their peers to learn more and feel more involved and competitive. In light of our model, on the low-income side, students would then feel less frustrated and, on the high income side, less likely to lose motivation.

We test these implications by splitting our sample based on data from students' friendship nominations within the school. This is, of course, a descriptive exercise. It is beyond the scope of this paper to deal with the endogeneity of friendship nominations. Nonetheless, this is instructive for future work and points toward a hopeful direction. In Figure 5, we split the effects from the share of low income peers on university graduation based on having high versus low network centrality[36] and links outside of a student's own income group. We interpret having a high centrality and having a high number of links outside own income group as representing a high social cohesion environment where information and opportunity is more likely to be shared.

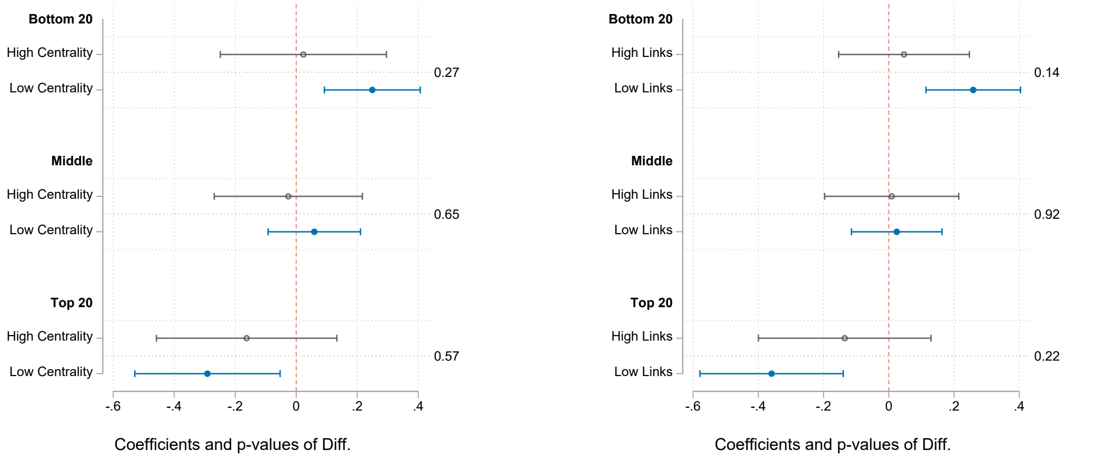
Across both measures of social cohesion and integration, we observe a similar pattern. When a student has better network links, the effects from the share of low income peers are near zero and insignificant. However, when network links are poor, then the effects from the share of low income peers are large, significant, and aligned with our previous results for the bottom and top 20th income groups. We must be cautious here, as limited statistical efficiency prevents us from drawing strong conclusions about heterogeneity. However, taken together, this descriptive evidence suggests that social cohesion may moderate the observed results and potentially harmful effects of exposure to income differences among peers.

We finally turn to a more plausibly exogenous approach. Throughout this paper the peer reference group has been set at the same school-cohort level. We now compare this against more refined peer groupings where stronger friendship ties are likely to exist due to homophily. If social integration mitigates harm from peer composition, then we should expect stronger peer

[36] We use Bonacich centrality, an index score that takes into account students' direct and indirect friendship links in the school (Bonacich, 1987).

Figure 5. University completion: heterogeneity by network centrality and school climate

(a) Sample split by centrality **(b)** Sample split by links outside own income group



Notes: This figure tests how different high and low network centrality students react to the share of low-income peers where we split the sample by those above or below the median centrality in panel (a). In panel (b) we do the test over friendships nominated by the students outside their own income group. We always include school and cohort fixed effects as in column (2) of Table 2. P-values of differences are presented at the side.

composition effects at the broader school-cohort level, where exposure to income differences signals more about the inequality in opportunity. Thus, we enrich our main specification and add a second share of low-income peers effects calculated (i) within school, cohort, and gender, (ii) within school, cohort, and race, or (iii) within school, cohort, gender, and race.[37] These results are reported in the Appendix, Figure E.1. In all cases, we find no effects on these more refined groupings, consistent with expectations based on more likely interactions in these groups, while our prior observed effects at the school-cohort level remain unchanged.

Our evidence throughout this paper contributes an important point to the literature: the composition of peer income can have non-monotonic effects on students educational outcomes that are not easy to reconcile under the lens of the existing peer effects literature. The mechanism we propose suggests that inequality of opportunities among students can generate feelings of frustration or motivation depending on a student relative position in the income distribution. Our results on social cohesion then point toward a path forward that policy can take: attempting to expose students to different income backgrounds requires coupling this with efforts to improve social cohesion to avoid reference dependence from inequality in opportunity.

8 Conclusion

Exposure to income differences among students may draw their attention to disparities in opportunity, in turn producing unintended consequences that are heterogeneous across the income distribution. Low-income students may realize they have fewer opportunities than their more fortunate peers, whereas high-income students can be motivated to do better if surrounded

[37] This is a “horse race”, as we include both our baseline peer reference group definition and a more refined grouping in the same regression.

by students with similar opportunities. In this paper, we empirically examine the role of changes in peer income composition on students' long-run educational attainment and their short-run performance, and how these change by own income.

We model the shift in school peer income differences using the share of low-income peers in a student's cohort within school. We then use this measure to examine how peer distributional shifts affect university completion and how these effects differ across the distribution of students own-household income. In order to identify these effects, we leverage within school, across cohort variation and flexibly control for students' household income. We also compare adolescents facing similar variances in the distribution of school-cohort income and additionally control for a rich vector of individual characteristics.

Our results show that low-income students benefit from an increase in the share of low-income peers, which positively affect their likelihood of university completion. Middle-income students experience on average null effects, and high-income students experience a reduced likelihood of university completion. These findings are robust to a rich battery of robustness checks. Our effects are sizable in magnitude: a 20% increase in the share of low-income peers raises the likelihood of completing university by 3.6pp for the bottom income students and decreases it by 4.1pp for the top income students. We also provide evidence that common mechanisms discussed in the peer effects literature do not explain our findings.

We then propose a novel theoretical framework that helps rationalize our results. We consider students with varying capacities for translating effort into educational outcomes, where capacity goes beyond the concept of raw ability and includes factors like opportunity and income. Students compare their outcomes to a reference point for educational attainment influenced by their peers' capacities. High-capacity (high-income) students perceive an increase in income differences when surrounded by a greater share of low-income peers, leading to lower motivation (less competition), effort, and attainment. Conversely, low-capacity (low-income) students see a reduction in income differences, resulting in higher motivation, effort, and attainment. Middle-capacity students may experience both situations, explaining an average null effect for this group. Hence, our model establishes that social comparison based on income can generate either frustration or motivation, depending on a student's relative position in the income distribution, and helps to understand potential unintended consequences from exposure to income differences among peers.

In further empirical analysis, we examine measures of performance, frustration, and motivation and find support for non-monotonic effects that are consistent with the theoretical predictions. Low-income students benefit from exposure to low-income peers in terms of short-term school performances, self-esteem and relative self-intelligent rating, whereas high-income students react with an increase in depressive symptoms and decreases in motivation.

Finally, we show descriptive evidence that the unintended effects of income differences among peers on students can be mitigated by social cohesion and a more integrated school environment. Social integration, measured through friendship nominations and cross-income group links, moderates the effects of low-income peers on university completion for both low- and high-income students. This suggests that policies fostering social cohesion can mitigate the consequences of exposure to peer group inequality. Overall, our evidence points to unintended consequences from exposure to peer income composition that policy needs to take into account in order for students to benefit from this integration.

Appendix: For Online Publication

A Mathematical Proofs

Proof of Proposition 1. To begin with, it is useful to summarize the properties of the functional forms adopted in the model of Section 5, that is, $b(0) = 0$, $b'(y) > 0$, $b''(y) < 0$, and $\lim_{y\to\infty} b'(y) = 0$, $\lim_{y\to 0} b'(y) = \infty$; $c(0) = 0$, $c'(e) > 0$, $c''(e) > 0$, and $\lim_{e\to\infty} c'(e) = \infty$; and $\mu(0) = 0$, $\mu'(y-r) > 0$, $\mu''(y-r) < 0$ if $y > r$ (concavity over gains) and $\mu''(y-r) > 0$ if $y < r$ (convexity over losses), and $\lim_{y\to r} \mu'(y-r) = \infty$. All functions are continuous, and continuously differentiable, the only exception being μ which is not differentiable at $y = r$. Next, we proceed by analyzing the properties of the solution for the case in which $y > a$, denoted by $\tilde{e}(\theta, a)^+$ and then for the case in which $y < a$, denoted by $\tilde{e}(\theta, a)^-$.

Case of $y > r$. By definition, $\tilde{e}(\theta, r)^+$ is the level of effort at which the first-order condition given by (4) is satisfied. Since

$$u_{ee} = [b''(y) + \mu''(y-r)]\theta^2 - c'' < 0$$

where $\mu''(y-r) < 0$ when $y > r$, we conclude that u is strictly concave in e. This, together with the fact that as e gets smaller (so that y approaches r from above), $\lim_{y\to r} u_e = \infty$ due to the fact that $\lim_{y\to r} \mu'(y-r) = \infty$, and as e gets larger, $\lim_{e\to\infty} u_e = -\infty$ due to the fact that $\lim_{e\to\infty} c'(e) = \infty$, enables us to conclude that $\tilde{e}(\theta, r)^+$ always exists, it is unique, and strictly positive. Moreover, note that

$$u_{er} = -\mu''(y-r)\theta > 0,$$

where $\mu''(y-r) < 0$ when $y > r$, enabling us to deduce by implicit differentiation that $\tilde{e}_r^+ = -u_{er}/u_{ee} > 0$, implying that $\tilde{e}(\theta, r)^+$ is increasing in r.

Case of $y < a$. By definition $\tilde{e}(\theta, r)^-$ would be the level of effort at which the first-order condition given by (5) is satisfied. However,

$$u_{ee} = [b''(y) + \mu''(y-r)]\theta^2 - c'',$$

the sign of which remains ambiguous, since $\mu''(y-r) > 0$ when $y < r$. Hence, we cannot conclude whether u is concave or convex in e in the domain of losses. Nevertheless, we can deduce that the marginal benefit of effort have a U-shape form, since $\lim_{y\to 0} b' = \infty$, $\lim_{y\to a} \mu' = \infty$ and

$$u_{eee} = \left\{[\alpha-2][\alpha-1]\alpha[\theta e]^{\alpha-3} + [\beta-2][\beta-1]\beta[r-\theta e]^{\beta-3}\right\}\theta^3 > 0.$$

This imply that we cannot be sure that a solution exists, or that if it does, that it is unique. To proceed, we denote the value of r at which the slope of the left-hand side of (5) is equal to the slope of the right-hand side, by $\hat{r}$. Formally, this is the value of r at which $u_{ee} = 0$. This value exists, and it is unique since

$$u_{er} = -\mu''(y-r)\theta < 0$$

when $y < r$. That is, as we increase r, the left-hand side of (5) will cross the right-hand side, and $\hat{r}$ is the value of r at which these are tangent. This imply that if $r < \hat{r}$ then there is no solution for the case of $y < r$ and the solution is $\tilde{e}(\theta, r)^+$; while if $r > \hat{r}$ then there are two solutions, one at which $u_{ee} > 0$ and one at which $u_{ee} < 0$. If we had modelled variable marginal cost (e.g. $c'(e) = \phi e$, $\phi > 0$, rather than ϕ being normalised to one as in the model section), $u_{ee} > 0$ would imply that $\tilde{e}(\theta, r)^-$ is increasing in its marginal cost, while $u_{ee} < 0$ would imply that $\tilde{e}(\theta, r)^-$ is decreasing in its marginal cost, which is the one we consider. Hence, if $r < \hat{r}$ there is no solution in the loss domain, and the student will choose $\tilde{e}(\theta, r)^+$ (which always exists); while if $r \geq \hat{r}$ there always exist two local solutions: one such that $y > r$ and one such that $y < r$. In this case, we assume the student will choose the one that yields the higher utility, in line with the principle of utility maximization. To prove that $\tilde{e}(\theta, r)^-$ is decreasing in r we use the fact that $u_{er} = -\mu''(y-r)\theta < 0$ which follows from the fact that $\mu''(y-r) > 0$ when $y < r$. Hence, implicit differentiation yields $\tilde{e}_r^- = -u_{er}/u_{ee} < 0$, implying $\tilde{e}(\theta, r)^-$ is decreasing in r.

Next, we prove that r^* exists and that it is unique. For this, it is sufficient to consider a situation in which $r \in [0, \underline{r}]$ where $\underline{r} > \hat{r}$ and suppose that $y > r$ such that the solution is $\tilde{e}(\theta, r)^+$ and that $u(\tilde{e}(\theta, r)^+, \theta, r) > u(\tilde{e}(\theta, r)^-, \theta, r)$. Application of the envelope theorem implies that both $u(\tilde{e}(\theta, r)^+, \theta, r)$ and $u(\tilde{e}(\theta, r)^-, \theta, r)$ are decreasing in r, where (from the first order conditions (4) and (5))

$$\begin{aligned}\frac{du(\tilde{e}(\theta, r)^+, \theta, r)}{dr} &= -\beta[\theta\tilde{e}^+ - r]^{\beta-1} = \alpha[\theta\tilde{e}^+]^{\alpha-1} - \frac{\tilde{e}^+}{\theta} \\ &< \alpha[\theta\tilde{e}^-]^{\alpha-1} - \frac{\tilde{e}^-}{\theta} = -\beta[r - \theta\tilde{e}^-]^{\beta-1} = \frac{du(\tilde{e}(\theta, r)^-, \theta, r)}{dr},\end{aligned}$$

and where the inequality follows from the concavity of b and the fact that $\tilde{e}^+ - \tilde{e}^- > 0$ at a given r. This implies that as we increase r, $u(\tilde{e}(\theta, r)^+, \theta, r)$ decreases faster than $u(\tilde{e}(\theta, r)^-, \theta, r)$, and that there exists a value of r, denoted by $r^* \equiv r^*(\theta)$, at which

$$u(\tilde{e}(\theta, r)^+, \theta, r) - u(\tilde{e}(\theta, r)^-, \theta, r) = 0,$$

(and for which we assume the solution to be given by $\tilde{e}(\theta, r)^+$). Further note that if $r > r^*$ then it must be that $u(\tilde{e}(\theta, r)^+, \theta, r) < u(\tilde{e}(\theta, r)^-, \theta, r)$, implying that the solution switches from being $\tilde{e}(\theta, r)^+$ to $\tilde{e}(\theta, r)^-$ and $y < r$. Next, since $\tilde{e}(\theta, r)^-$ is decreasing in r it implies that as we increase r further beyond r^* then y remains below r. The same logic applies for all $r \in [0, r^*]$, since as we increase r, and $\tilde{e}(\theta, r)^+$ is increasing in r, then y remains above r. This implies that r^* is unique.

To conclude, we prove that r^* is increasing in θ. From the definition of r^* above, implicit differentiation yields:

$$\frac{dr^*(\theta)}{d\theta} = -\frac{\dfrac{du(\tilde{e}(\theta, r)^+, \theta, r)}{d\theta} - \dfrac{du(\tilde{e}(\theta, r)^-, \theta, r)}{d\theta}}{\dfrac{du(\tilde{e}(\theta, r)^+, \theta, r)}{dr} - \dfrac{du(\tilde{e}(\theta, r)^-, \theta, r)}{dr}} > 0,$$

since the results above imply that the denominator is negative, while application of the envelope theorem implies that

$$\begin{aligned}\frac{du(\tilde{e}(\theta,r)^{+},\theta,r)}{d\theta} &= \left\{\alpha[\theta\tilde{e}^{+}]^{\alpha-1}+\beta[\theta\tilde{e}^{+}-r]^{\beta-1}\right\}\tilde{e}^{+}\\ &= \frac{\tilde{e}^{+}}{\theta}\tilde{e}^{+}\\ &> \frac{\tilde{e}^{-}}{\theta}\tilde{e}^{-}\\ &= \left\{\alpha[\theta\tilde{e}^{-}]^{\alpha-1}+\beta[r-\theta\tilde{e}^{-}]^{\beta-1}\right\}\tilde{e}^{-} = \frac{du(\tilde{e}(\theta,r)^{-},\theta,r)}{d\theta}.\end{aligned}$$

Hence, the numerator is positive, implying r^* is increasing in θ. ■

Proof of Proposition 2. This proof proceeds in two steps. First we show that the level of effort at which $y = r$, defined by $\bar{e}(\theta, r(\theta, F^\theta)) \equiv \frac{r(\theta,F^\theta)}{\theta}$, is decreasing in θ. Then we prove the existence and uniqueness of θ^*.

Consider $\bar{e}(\theta, r(\theta, F^\theta))$ for given capacity θ and distribution F^θ, our assumptions on r imply that, for $\theta_2 = \lambda\theta_1$ with $\lambda > 1$:

$$\begin{aligned}\bar{e}(\theta_2, r(\theta_2, F^\theta)) &= \frac{r(\theta_2, F^\theta)}{\theta_2}\\ &< \frac{r(\theta_2, \lambda F^\theta)}{\theta_2} = \frac{r(\lambda\theta_1, \lambda F^\theta)}{\lambda\theta_1} = \frac{\lambda r(\theta_1, F^\theta)}{\lambda\theta_1} = \bar{e}(\theta_1, r(\theta_1, F^\theta)).\end{aligned}$$

Hence $\bar{e}(\theta, r(\theta, F^\theta))$ decreasing in θ (and increasing in r). Next, from Proposition 1 we know that if $r < r^*$ then $\tilde{e}^+ > \bar{e}$, and if $r > r^*$ then $\tilde{e}^- < \bar{e}$. Hence, there exists a unique $\bar{e}^*(\theta, r^*(\theta))$ such that for all (θ, r) with $\bar{e}(\theta, r) > \bar{e}^*(\theta, r^*(\theta))$ then $r > r^*$ and $y < r$, while for all (θ, r) with $\bar{e}(\theta, r) < \bar{e}^*(\theta, r^*(\theta))$ then $r < r^*$ and $y > r$. This, along with the result established above that $\bar{e}(\theta, r(\theta, F^\theta))$ is decreasing in θ, can be used to deduce that if $\bar{e}(\theta, r(\theta, F^\theta)) > \bar{e}^*(\theta, r^*(\theta))$ for some $\theta_1 \leq \theta^*$ so that $r(\theta_1, F^\theta) > r^*(\theta_1)$, then this will be the case for all $\theta < \theta_1$; while if $\bar{e}(\theta, r(\theta, F^\theta)) < \bar{e}^*(\theta, r^*(\theta))$ for some $\theta_2 > \theta^*$ so that $r(\theta_1, F^\theta) < r^*(\theta_1)$, then this will be the case for all $\theta > \theta_2$. ■

Proof of Proposition 3. First note that if $G^I > F^I$, then $r(I, G^I) < r(I, F^I)$ for any given I, hence, the reference outcome decreases for all students. Next, consider low income students, for which initially $r(I, F^I) > r^*(I)$. If $r(I, G^I) < r(I, F^I)$ then $\tilde{e}(I, r(I, G^I))^- > \tilde{e}(I, r(I, F^I))^-$ by the results established in Proposition 1, since our assumption that G^I is such that $r(I_F^l, G^I) > r^*(I)$ ensures that this is true for any $r(I, G^I)$ with $I \leq I_F^l$: even the richest of the low income students remains frustrated. Then consider high income students, for which initially $r(I, F^I) < r^*(I)$. Since $r(I, G^I) < r(I, F^I)$ then $r(I, F^I) < r^*(I)$ and $\tilde{e}(I, r(I, G^I))^+ < \tilde{e}(I, r(I, F^I))^+$ by the results established in Proposition 1. Finally, consider middle income students. There is a fraction of these students endowed with $I \in (I_F^*, I_F^h)$ for which $r(I, G^I) < r^*(I)$, which implies they behave the same as high income students. However, there is also a fraction of these students endowed with $I \in (I_F^l, I_F^*)$ whom will increase their effort only as long as the decrease in r is such that $r(I, G^I) > r^*(I)$; while they will decrease their effort if the decrease in r is such that $r(I, G^I) < r^*(I)$. ■

B Additional Tables and Figures

Table B.1. Summary statistics

	Analytic Sample = 11,165				Reweighted to population		
	Mean	SD	Min	Max	Mean	SD	Mean diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Outcome and Treatment							
College Graduate in wave IV	0.33	0.47	0	1	0.30	0.46	0.03
Share of Low Income Peers (SLP_{-ics})	0.34	0.20	0	1	0.34	0.21	0.00
B. Student Characteristics							
Logged Household Income	3.56	0.84	0	7	3.55	0.84	0.01
Female	0.52	0.50	0	1	0.49	0.50	0.03
Age	15.47	1.68	11	19	15.31	1.78	0.16
Hispanic	0.15	0.35	0	1	0.11	0.31	0.04
White	0.59	0.49	0	1	0.71	0.45	-0.12
Black	0.20	0.40	0	1	0.14	0.35	0.06
Asian	0.05	0.21	0	1	0.03	0.16	0.02
Other Races	0.02	0.13	0	1	0.02	0.13	0.00
Family Size	3.79	1.21	2	12	3.76	1.22	0.03
Child of and Immigrant	0.17	0.38	0	1	0.13	0.34	0.04
Less than HS Parents	0.10	0.30	0	1	0.11	0.31	-0.01
HS or GED Parents	0.29	0.46	0	1	0.32	0.47	-0.03
Some College Parents	0.22	0.42	0	1	0.22	0.42	0.00
College Parents	0.25	0.43	0	1	0.23	0.42	0.02
Postgraduate Parents	0.13	0.34	0	1	0.11	0.32	0.02
Single Parent Household	0.30	0.46	0	1	0.30	0.46	0.00
Grade 7	0.14	0.35	0	1	0.18	0.38	-0.04
Grade 8	0.14	0.35	0	1	0.17	0.38	-0.03
Grade 9	0.19	0.39	0	1	0.17	0.38	0.02
Grade 10	0.20	0.40	0	1	0.16	0.37	0.04
Grade 11	0.18	0.38	0	1	0.14	0.35	0.04
Grade 12	0.15	0.35	0	1	0.15	0.35	0.00

Notes: Column (1) - (4) in this table present summary statistics for the sample in wave I of AddHealth after restricting to our analytic sample but before imputing the sample, which has 11, 165 observations left. Columns (5) and (6) present the mean and standard error of population reweighted from our analytic sample using the cross-sectional weights AddHealth provides. The ethnicity distribution after reweighting is similar to the population profile of United Sates in 1995 (of the Census, 1995). Column (7) shows the difference in means.

Table B.2. Additional summary statistics

	Mean	SD	Min	Max
A. GPA and Advanced Courses Taking				
Self-reported GPA at wave I	2.80	0.77	1	4
Transcript average GPA after treatment	2.44	0.89	0	4
Advanced Math courses taking	0.41	0.49	0	1
Advanced Science courses taking	0.46	0.50	0	1
Advanced English couses taking	0.24	0.43	0	1
Taking more than one advanced couses	0.60	0.49	0	1
B. Frustration and Motivation				
Self esteem	28.56	4.14	7	35
Intelligent feelings compared to others	3.90	1.08	1	6
CES-D mental health scale	11.02	7.46	0	54
Motivation	3.78	0.91	1	5
Observations	11165			

Notes: This table presents additional summary statistics on GPA and advanced courses taking in Table 4 and frustration and motivation measures in Table 5 after restricting to our analytic sample.

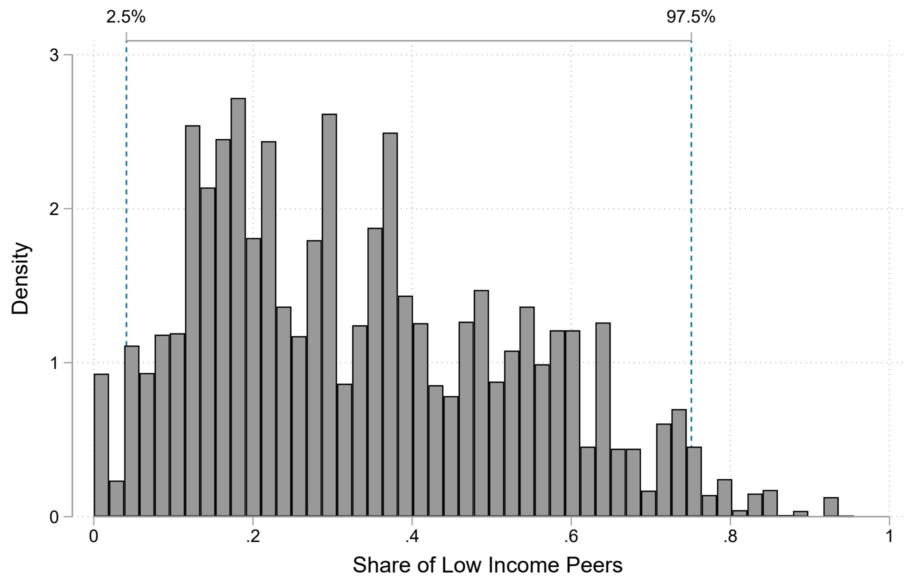
Table B.3. Frustration and motivation variables

	Original questions	Answer modalities	Final indicator
Self-Esteem	1. You have a lot of good qualities. 2. You are physically fit. 3. You have a lot to be proud of. 4. You like yourself just the way you are. 5. You feel like you are doing everything just about right. 6. You feel socially accepted. 7. You feel loved and wanted.	1. strongly agree 2. agree 3. neither agree nor disagree 4. disagree 5. strongly disagree	We reverse code the raw variables, then aggregate those 7 variables to get the self-esteem variable. Higher values imply higher self-esteem.
Intelligent Feeling	Compared with other people your age, how Intelligent are you?	1. moderately below average 2. slightly below average 3. about average 4. slightly above average 5. moderately above average 6. extremely above average	
CES-D scale	1. You were bothered by things that don't usually bother you. 2. You didn't feel like eating, your appetite was poor. 3. You felt that you could not shake off the blues, even with help from your family and your friends. 4. You felt you were just as good as other people. 5. You had trouble keeping your mind on what you were doing. 6. You felt depressed. 7. You felt that you were too tired to do things. 8. You felt hopeful about the future. 9. You thought your life had been a failure. 10. You felt fearful. 11. You were happy. 12. You talked less than usual. 13. You felt lonely. 14. People were unfriendly to you. 15. You enjoyed life. 16. You felt sad. 17. You felt that people disliked you. 18. It was hard to get started doing things. 19. You felt life was not worth living.	0. never or rarely 1. sometimes 2. a lot of the time 3. most of the time or all the time	We reverse code items 4, 8, 11, 15 and aggregate those 19 variables to get a final score ranging from 0 to 57, which higher scores indicating a higher propensity for despressive symptoms.
Motivation	1. During the 1994-1995 school year, how often did you have trouble paying attention in school? 2. During the 1994-1995 school year, how often did you have trouble getting your homework done?	0. never 1. just a few times 2. about once a week 3. almost everyday 4. everyday	We reverse code these raw variables and take the mean. Higher values imply less trouble/higher motivation.

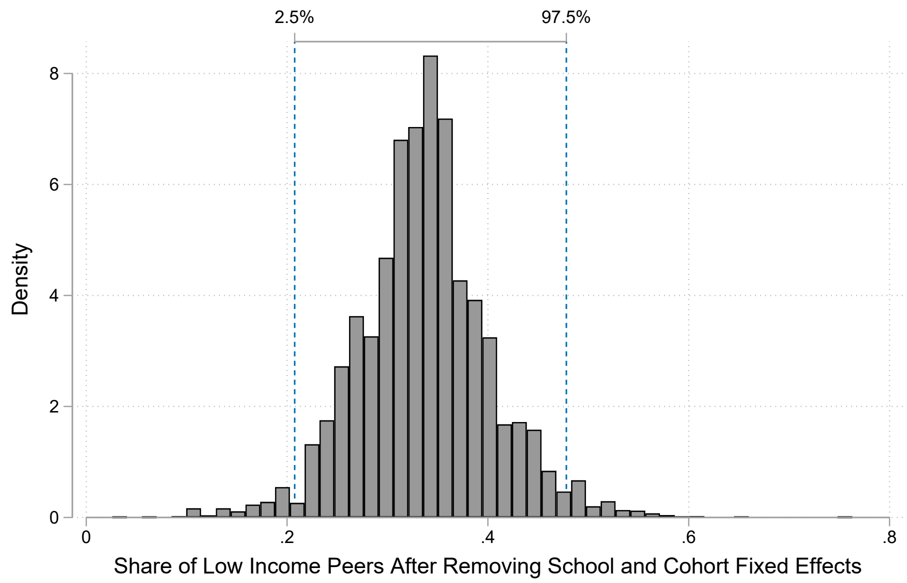
Notes: This table presents details of the construction of the frustration and motivation variables in Table 5.

Figure B.1. Variation in share of low-income peers

(a) Raw variation



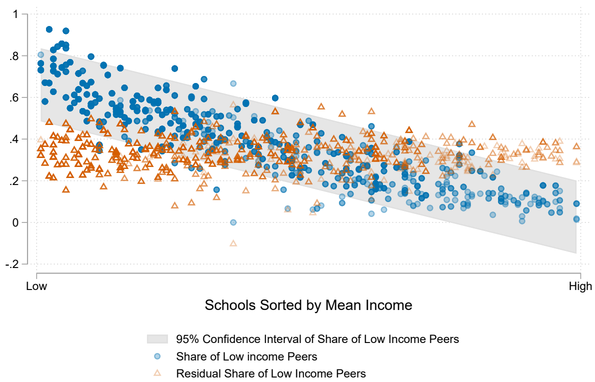
(b) Variation post removal of school and cohort fixed effects



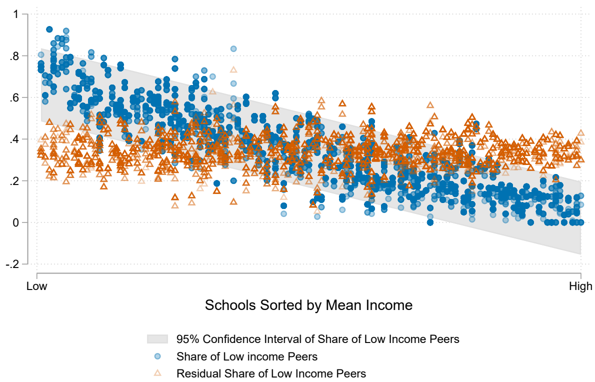
Notes: This figure presents a histogram of the share of low-income peers in our analytic sample. Panel (a) reports the variations in the sample, and panel (b) reports this variation after removal of school and cohort fixed effects with the sample mean added back to place it on the same scale as panel (a). Vertical lines denote the 2.5 and 97.5 percentiles.

Figure B.2. Variation between the share of low-income peers and school quality heterogeneous to own income groups conditional on school fixed effects

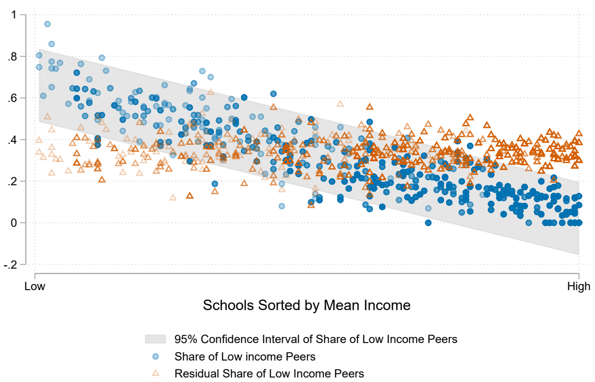
(a) Bottom 20



(b) Middle



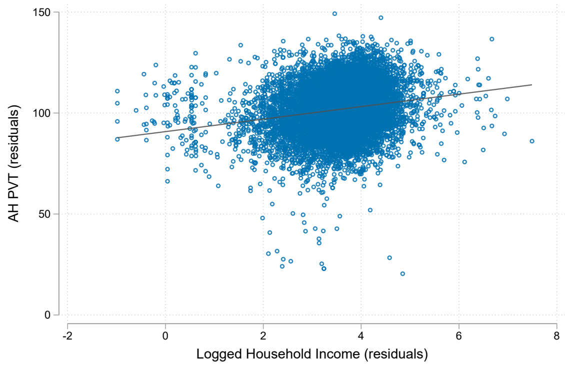
(c) Top 20



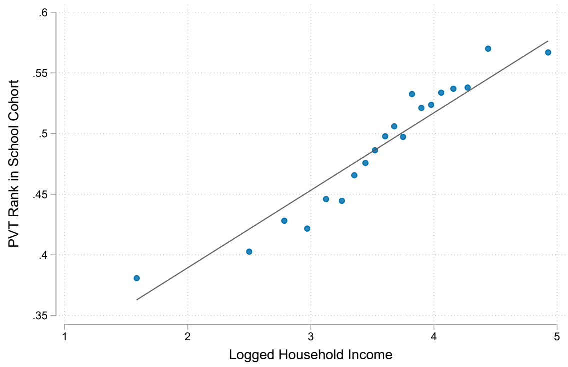
Notes: These figures present the share of low-income peers and its residual after removal of school fixed effects with the sample mean added back to it for the bottom 20th, middle, and top 20th of the household income distribution by schools. Schools are sorted based on the mean logged household income of students from the lowest to the highest.

Figure B.3. Associations: PVT scores, rank, and household income

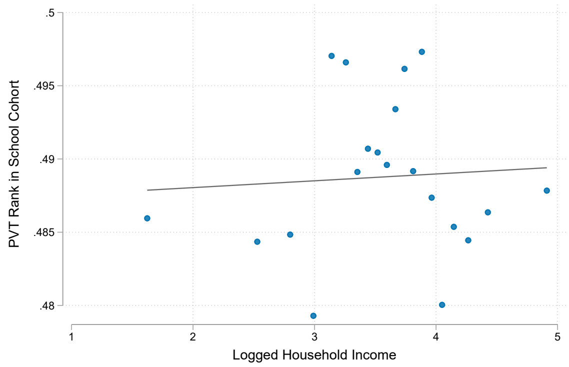
(a) PVT and ln(Income)



(b) PVT Rank and ln(Income)

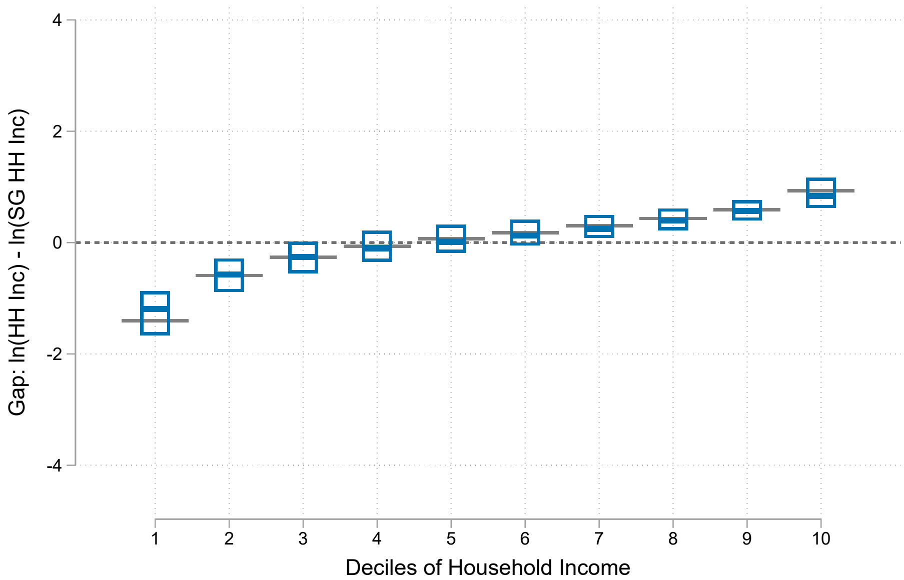


(c) PVT Rank and ln(Income): Control for PVT



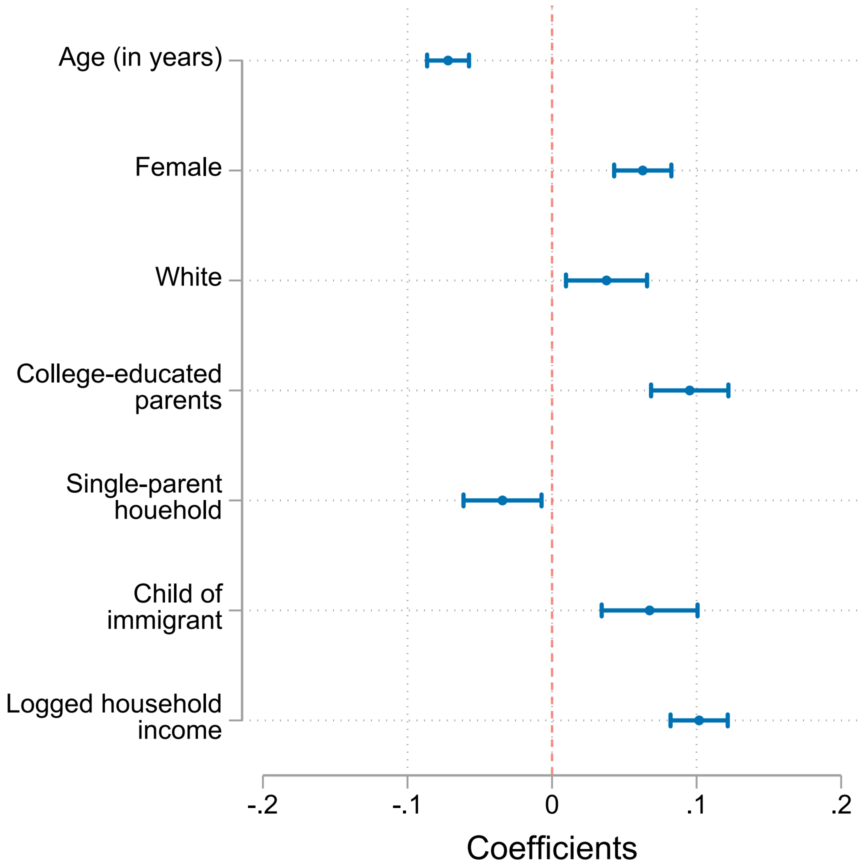
Notes: In all panels, we control for school fixed effects so associations are based on within school variation. Panel (a) reports a scatter plot and line of best fit between the residuals of the picture vocabulary test (PVT) scores and logged household income after removing school fixed effects. We add the full mean back to place the plot on the scale of the original variables. Panel (b) reports a bin scatter plot between the percentilized PVT school cohort rank based on the PVT scores and logged household income. Panel (c) reports the same as (b) but we control additionally for students' PVT scores.

Figure B.4



Notes: For each household income decile, this figure presents box plots of the interquartile range overlaid with lines for the mean and median.

Figure B.5. Associations of covariates with university completion



Notes: This figure presents a linear specification for logged household income and other characteristics. The base race in our specification is white, and we control for school and cohort fixed effects.

Table B.4. Long-run effects on labour market outcomes

	Wave IV Log Individual Income			
	(1)	(2)	(3)	(4)
SLP_{-ics} × Bottom 20	0.33	0.89***	0.79**	0.67*
	(0.25)	(0.29)	(0.38)	(0.39)
SLP_{-ics} × Middle	0.24	0.37*	0.33	0.30
	(0.15)	(0.21)	(0.30)	(0.19)
SLP_{-ics} × Top 20	-0.05	0.06	0.08	0.05
	(0.24)	(0.30)	(0.37)	(0.41)
School-specific Cohort Trends	No	No	Yes	No
School-specific Income Trends	No	No	No	Yes
Wave IV Sampling Weight	No	Yes	Yes	Yes
Mean Log Income	10.18	10.16	10.16	10.16
Observations	9919	9614	9614	9614
R^2	0.115	0.171	0.186	0.197

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. We trim our data to our analytic sample as in Table 2 and use Wave IV log household income as the long-run labor market outcome variable. We use Wave IV sampling weight to adjust the attrition in column (2) - (4). The sample weight was computed by the attrition for selecting schools and adolescents, as well as characteristics related to non-response. We further add school-specific cohort trends in column (3) and school-specific income trends in column (4). The result is consistent once we relax the sample size to the fully available sample in Table E.4.

C Robustness Checks

Table C.1. Robustness to different definitions for the share of low-income peers

	$SLP_{-ics} \times$ Bottom 20	$SLP_{-ics} \times$ Middle	$SLP_{-ics} \times$ Top 20
	(1)	(2)	(3)
Original	0.18**	0.02	-0.25**
	(0.07)	(0.07)	(0.11)
Bottom 20th Percentile	0.22***	-0.01	-0.32**
	(0.08)	(0.07)	(0.16)
Below Median	0.13**	0.03	-0.09
	(0.06)	(0.05)	(0.09)
By School Region and Family Size	0.18***	-0.00	-0.19*
	(0.06)	(0.06)	(0.11)

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. The first row shows the results of our original definition of the share of low-income peers. In the second row, we define the share of low-income peers as the share of peers in the bottom 20th percentile of household size for a given family size. In the third row, we define the share of low-income peers as the share of peers below the median of household income for a given family size. In the fourth row, we define the share of low-income peers as share of peers in the bottom 3rd of the household income distribution by school region, school urbanicity, and a family size indicator (whether the family size is larger than 4). Observations are equal to 11,165 as our analytic sample size in each specification.

Table C.2. Robustness to non-linearity in household income

	Iterations of LnHHInc Polynomials				Ventiles
	(1)	(2)	(3)	(4)	(5)
$SLP_{-ics} \times$ Bottom 20	0.18**	0.17**	0.16**	0.16**	0.16**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
$SLP_{-ics} \times$ Middle	0.01	0.02	0.02	0.02	0.03
	(0.07)	(0.07)	(0.07)	(0.07)	(0.06)
$SLP_{-ics} \times$ Top 20	-0.25**	-0.25**	-0.26**	-0.26**	-0.26**
	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)
$(\text{LnHHInc})^3$	-0.01***	0.01	0.10**	0.02	
	(0.00)	(0.01)	(0.05)	(0.17)	
$(\text{LnHHInc})^4$		-0.00**	-0.02**	0.00	
		(0.00)	(0.01)	(0.05)	
$(\text{LnHHInc})^5$			0.00*	-0.00	
			(0.00)	(0.01)	
$(\text{LnHHInc})^6$				0.00	
				(0.00)	
H.H. Income Ventiles	No	No	No	No	Yes
Observations	11165	11165	11165	11165	11165

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Column (5) includes household income ventiles to control for non-linearity.

Table C.3. Subsample analysis

	University Graduate					
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	0.27**			0.23*		
	(0.14)			(0.13)		
SLP_{-ics} × Middle		-0.03			-0.02	
		(0.08)			(0.08)	
SLP_{-ics} × Top 20			-0.34			-0.39*
			(0.21)			(0.21)
Own-Ability Polynomials	No	No	No	Yes	Yes	Yes
School-Cohort Ability Rank	No	No	No	Yes	Yes	Yes
Observations	2180	6920	2065	2180	6920	2065

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Columns (1) - (3) include all controls as in our preferred baseline specification in column (2) of Table 2. Columns (4) - (6) add additional controls as in our specification in column (4) of Table 2.

Table C.4. Placebo test

	Placebo treatment		Placebo outcome	
	(1)	(2)	(3)	(4)
SLP_{-ics} × Bottom 20	0.08	-0.08	-0.03	-0.11
	(0.06)	(0.10)	(0.06)	(0.12)
SLP_{-ics} × Middle	-0.04	-0.05	-0.00	0.01
	(0.05)	(0.05)	(0.05)	(0.05)
SLP_{-ics} × Top 20	-0.20**	0.03	-0.07	0.05
	(0.09)	(0.11)	(0.06)	(0.09)
School-specific Income Trends	No	Yes	No	Yes
Observations	11047	11047	11149	11149

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Columns (1) - (2) estimate the effects of the placebo share of low-income peers on the probability of graduating from university. The placebo share of low-income peers is defined using the share of low-income peers in another cohort within the same school. Columns (3) - (4) estimate the effects of actual share of low-income peers on the placebo outcome, which is an indicator of ever repeated a cohort. Column (2) and column (4) add the school-specific income trends to the baseline specification.

Table C.5. Attrition analysis and sampling weights

	Attrited in Wave IV				University Graduate IPW Adjusted		University Graduate Weighted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share of Low Income Peers	-0.05	0.07					
	(0.04)	(0.06)					
$SLP_{-ics} \times$ Bottom 20			-0.08	0.05	0.19***	0.23**	0.26***
			(0.06)	(0.07)	(0.07)	(0.10)	(0.08)
$SLP_{-ics} \times$ Middle			-0.05	0.08	0.03	-0.01	0.04
			(0.05)	(0.07)	(0.06)	(0.07)	(0.07)
$SLP_{-ics} \times$ Top 20			-0.05	0.10	-0.23**	-0.27**	-0.26*
			(0.06)	(0.09)	(0.11)	(0.13)	(0.14)
School and Grade Fixed Effects	No	Yes	No	Yes	Yes	Yes	Yes
School-specific Income Trends	No	No	No	No	No	Yes	No
Share Attrited	.22	.22	.22	.22	.22	.22	.22
Observations	14339	14339	14339	14339	11115	11115	10818
R^2	.026	.049	.027	.05	.24	.25	.27

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. The dependent variable in columns (1) - (4) is an indicator equal to one if an individual has attrited in wave IV and zero otherwise. Estimates of marginal effects are for the share of low-income peers in the bottom 20th percentile of household income, for the middle, and finally for the top 20th percentile of household income. In columns (5) - (6), we calculate treatment effects of the share of low-income peers on the probability of graduating from university using inverse probability weighting, where the weights are calculated as the predicted probability of being in wave IV follow-up sample based on the available baseline controls as in column (2) of Table 2. We further add the school-specific income trends to the baseline specification in column (6). We use Wave IV sampling weight designed for estimating single-level models to adjust the attrition in column (7). The sample weight was computed by the attrition for selecting schools and adolescents, as well as characteristics related to non-response.

Table C.6 presents the results adjusted for measurement error by weighting the share of low-income peers by the percentage of school-cohort peers observed in the analytic sample (as calculated from the in-school survey). Additionally, we control for the percentage of peers observed, following the methodology of Sojourner (2013). After applying the weighting, the point estimates become larger, but the confidence intervals still encompass the baseline point estimates. This suggests that, we are likely underestimating our baseline results due to sampling measurement error instead of overestimating.

Table C.6. Sampling measurement error

	Baseline results on University Graduate	Adjust for % observed in in-home survey
	(1)	(2)
$SLP_{-ics} \times$ Bottom 20	0.18**	0.41**
	[0.04,0.31]	[0.05,0.77]
$SLP_{-ics} \times$ Middle	0.02	-0.04
	[-0.11,0.14]	[-0.41,0.34]
$SLP_{-ics} \times$ Top 20	-0.25**	-0.88**
	[-0.47,-0.03]	[-1.55,-0.20]
Observations	11165	9783

Notes: This table presents the baseline results and the results weighted by the percentage of school-cohort peers observed in the data. This information comes for the in-school survey which in some cases cannot be correctly matched to students in the in-home survey leading to a drop in observations for this analysis.

Simulations to assess measurement errors. We present simulations to assess the role of two forms of measurement error. We assume the following data generating process (DGP):

$$Y_{is} = 0.18 SLP_{is} \times B20_i - 0.25 SLP_{is} \times T20_i + 0.01 ln(Inc_i)$$

where Y denotes our outcome, SLP_{is} denotes the leave-one-out percentage share of peers from low-income households in their school cohort defined by the bottom third of the simulated income distribution, and Inc_i denotes a student's household income, which is randomly drawn from a log-normal distribution with the log-income mean of 3.5 and standard deviation of 0.85 ($ln(Inc) \sim N(3.5, 0.85)$), consistent with our analytical data. The indicator variables $B20_i$ and $T20_i$ flag observations in the bottom and top 20th deciles of the simulated income distribution. For the simulations, we use variation across schools abstracting away from multiple cohorts in each school. However, we model no selection effects into schools, thus variation across schools in our simulations is exogenous conditional on income. The parameters in the DGP ($\beta_1 = 0.18$, $\beta_2 = 0$, and $\beta_3 = -0.25$) are based on the specification shown in column (1) of Table 2.

First, we assess the consequences of observing a random subsample of students per school using Monte Carlo simulations. Based on our DGP we run 1000 repetitions with 500 schools/cohorts each and 240 students per school, re-drawing Inc_i and SLP_{is} at each repetition. We evaluate the simulated data with the following specification:

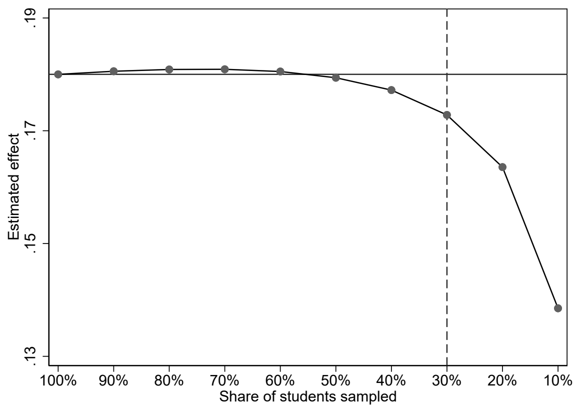
$$y = \beta_1 SLP_{ics} \times B20_i + \beta_2 SLP_{ics} \times Mid_i + \beta_3 SLP_{ics} \times T20_i + \gamma ln(Inc_i).$$

Additionally, we also evaluate it based on subgroups by income ($B20_i, Mid_i, T20_i$).

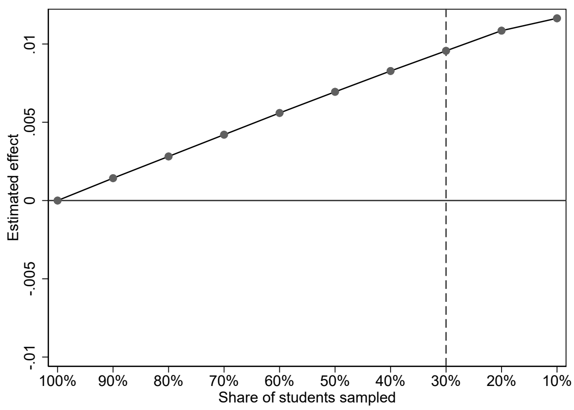
Second, we consider measurement error in our measure of income. Based on the same DGP but using mismeasured income we run the same regression as above at each repetition and combine this with the sampling error running a 100% sample and a 30% of the school sample. Results from the simulations are presented below.

Figure C.1. Simulations to assess bias due to random sampling within schools

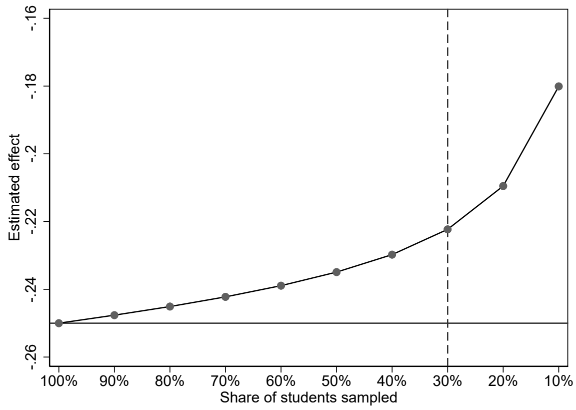
(a) Bottom 20 group



(b) Middle group



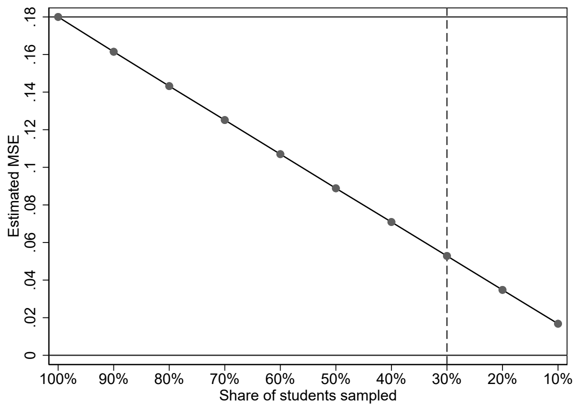
(c) Top 20 group



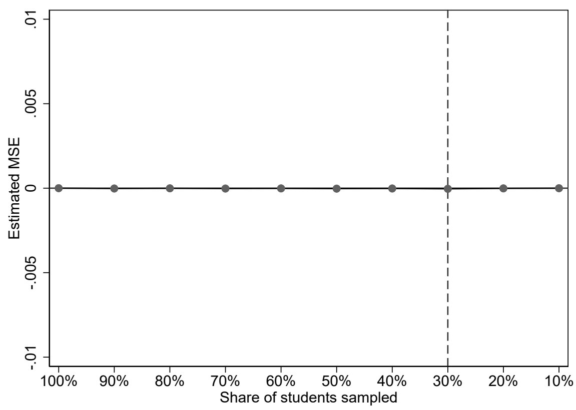
Notes: These figures present results from Monte Carlo simulations with 1000 repetitions of 500 schools for the bottom 20, middle, and top 20 groups respectively. The vertical dashed line of 30% is the average percentage of an Add Health school that was sampled.

Figure C.2. Simulations to random sampling within schools: subsample analysis

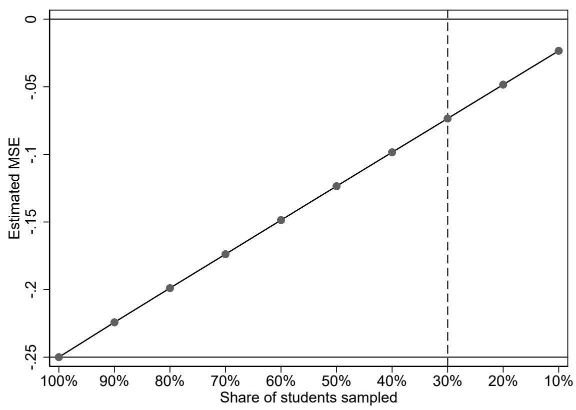
(a) Bottom 20 group



(b) Middle group



(c) Top 20 group



Notes: These figures present subsample analysis results from Monte Carlo simulations with 1000 repetitions of 500 schools for the bottom 20, middle, and top 20 groups respectively. The vertical dashed line of 30% is the average percentage of an Add Health school that was sampled.

Table C.7. Simulations to assess bias due to measurement error in income

Simulation: Measurement error in income

DGP: $Y_{is} = 0.18 SLP_{-is} \times B20_i - 0.25 SLP_{-is} \times T20_i + 0.01 ln(Inc_i)$;

$\widetilde{ln(Inc_i)} \sim \mathcal{N}(3.5, 0.85)$;

$\widetilde{ln(Inc_i)} \sim ln(Inc_i) + \phi \cdot v_i$; $\phi \in [0,1]$; $v_i \sim \mathcal{N}(0, 0.85)$;

Estimate: $Y_{is} = \beta_1 \widetilde{SLP_{-is}} \times B20_i + \beta_2 \widetilde{SLP_{-is}} \times Mid_i + \beta_3 \widetilde{SLP_{-is}} \times T20_i + \gamma \widetilde{ln(Inc_i)}$

	Measurement error (ϕ)					
100% sampling	0	0.2	0.4	0.6	0.8	1.0
$SLP_{-ics} \times B20$	0.18	0.09	0.04	0.01	0.00	-0.00
	(100%)	*(52%)*	*(22%)*	*(6%)*	*(0%)*	*(-2%)*
$SLP_{-ics} \times Mid$	0.00	-0.00	-0.00	-0.00	-0.00	-0.00
$SLP_{-ics} \times T20$	-0.25	-0.15	-0.09	-0.05	-0.03	-0.02
	(100%)	*(61%)*	*(35%)*	*(20%)*	*(13%)*	*(9%)*
30% sampling						
$SLP_{-ics} \times B20$	0.17	0.09	0.04	0.02	0.00	0.00
	(94%)	*(51%)*	*(23%)*	*(9%)*	*(3%)*	*(0%)*
$SLP_{-ics} \times Mid$	0.01	0.01	0.01	0.00	0.00	0.00
$SLP_{-ics} \times T20$	-0.22	-0.13	-0.07	-0.04	-0.03	-0.02
	(88%)	*(53%)*	*(30%)*	*(17%)*	*(10%)*	*(7%)*

Notes: This table presents the results of Monte Carlo simulations with 1000 repetitions of 500 schools each. Shares in parentheses report the ratio of the estimate to the true coefficient from the data-generating process. For the middle group, the ratio is not reported because the true coefficient is 0.

D Romano-Wolf p-value Adjustment

Table D.1. Romano-Wolf p-value adjustment for university graduation

	University Graduate					
	(1)	(2)	(3)	(4)	(5)	(6)
$SLP_{-ics} \times Bottom\ 20$						
Original p-value	0.015	0.012	0.013	0.006	0.004	0.027
Romano-Wolf p-value	0.028	0.026	0.026	0.010	0.010	0.044
$SLP_{-ics} \times Middle$						
Original p-value	0.854	0.810	0.922	0.986	0.396	0.783
Romano-Wolf p-value	0.948	0.926	0.948	0.982	0.521	0.926
$SLP_{-ics} \times Top\ 20$						
Original p-value	0.030	0.028	0.017	0.014	0.139	0.028
Romano-Wolf p-value	0.052	0.052	0.028	0.028	0.190	0.052

Notes: We use Romano and Wolf's step-down adjusted p-values to conduct multiple hypothesis testing (Clarke et al., 2020; Romano and Wolf, 2005) across specifications. This table provides p-values after controlling for the family-wise error rate. The specifications match specifications in our baseline Table 2.

Table D.2. Romano-Wolf p-value adjustment for GPA and advanced courses

	GPA		Advanced Courses			
	Self	Transcript	Math	Science	English	More than one
	(1)	(2)	(3)	(4)	(5)	(6)
$SLP_{-ics} \times Bottom\ 20$						
Original p-value	0.719	0.001	0.008	0.130	0.552	0.006
Romano-Wolf p-value	0.998	0.026	0.070	0.535	0.978	0.054
$SLP_{-ics} \times Middle$						
Original p-value	0.553	0.018	0.522	0.928	0.821	0.337
Romano-Wolf p-value	0.978	0.122	0.978	1.000	1.000	0.884
$SLP_{-ics} \times Top\ 20$						
Original p-value	0.304	0.891	0.494	0.089	0.356	0.994
Romano-Wolf p-value	0.858	1.000	0.968	0.413	0.892	1.000

Notes: We use Romano and Wolf's step-down adjusted p-values to conduct multiple hypothesis testing (Clarke et al., 2020; Romano and Wolf, 2005) on different outcomes. This table provides p-values after controlling for the family-wise error rate.

E Mechanisms and additional results

E.1 Results explained by alternative mechanisms?

We now describe our analysis testing whether our results on the share of low income peers capture dimensions beyond non-linear peer ability, which we address in the main text Section 4.3. Specifically, we explore reponses by teachers, disruptive peer behavior, and responses by parents.

E.1.1 Teachers

Responses by teachers that correlate with changes in the share of low-income peers could explain our results. As mentioned above, the literature on peer effects in education shows that teachers do change their behavior in response to classroom composition aimed at more effectively meeting students' needs (Duflo et al., 2011; Lee et al., 2014; Jackson, 2016; Aucejo et al., 2022; Papageorge et al., 2020). In this case, we would expect that as the share of low-income peers increases in a given school cohort, teachers may decide to devote more attention to them and also adapt their expectations and teaching practices accordingly. This will benefit low-income students, providing an explanation for our evidence on the bottom-20 students. However, the impact on middle or high-income students is somewhat ambiguous, as it will depend on whether the attention shift to low-income students comes at their expense or not. Moreover, predictions here for low-income students are not entirely clear. Alternatively, if teachers hold implicit stereotypes regarding different income groups, this may obstruct their interaction with students, acting to harm low-income students (Carlana, 2019; Carlana et al., 2022b).

Table E.1. Teachers effects: share of low-income peers

	Relationship with Teachers				University Graduation	
	Care Teachers	Close Teachers	Fair Teachers	Teacher Scale	Tracking	No Tracking
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	-0.01	-0.33	-0.06	-0.19	0.20*	0.15
	(0.22)	(0.20)	(0.18)	(0.20)	(0.11)	(0.09)
SLP_{-ics} × Middle	-0.11	-0.14	-0.07	-0.14	0.02	0.04
	(0.17)	(0.18)	(0.18)	(0.19)	(0.11)	(0.07)
SLP_{-ics} × Top 20	0.21	-0.08	0.00	0.05	-0.30*	-0.08
	(0.24)	(0.21)	(0.21)	(0.22)	(0.17)	(0.12)
Observations	11110	11164	11162	11165	6755	4265
R^2	0.068	0.074	0.055	0.066	0.227	0.254

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Standard errors are in parentheses and clustered at the school level. The relationship with teacher variables are standardized. Columns (6) - (7) return to University graduation as the outcome but the sample is stratified by schools who report they do (or do not) to use ability tracking for English and Language Arts. Note that ability tracking was reported in the school principle's questionnaire and only asked on this dimension.

We next look at further evidence for a teacher mechanism to explain our results. In the U.S. educational context, students typically change classrooms throughout the day as they switch between classes and do not necessarily stay with the same classmates. Thereby, we would expect a teacher driven mechanism for our effects to be dominant only if all, or the significant share, of the teachers in the same school-cohort update their behavior at the same time. This may translate into average shifts in teacher-student relationships heterogeneous to the income distribution, so we look at student-reported measures of these relationships.[1] The results, in the Table E.1 columns (1) - (4), suggest there no effects here.

Finally, we look at our baseline model for University graduation split by schools who use ability tracking for English and Language Arts.[2] The effects should disappear in schools that track by ability if increases in the share of low-income peers mainly captures optimization of instruction for low-income students. Our results in columns (5) - (6) of the Table E.1 are not consistent with this for low-income students, and while the point estimates are inefficient, suggest similar results across school types. The results for high-income students, however, suggest they are mainly present in tracking schools, thus there is likely some role for the optimization story, albeit not enough to explain the overall pattern we observe.

E.1.2 Disruptive peers

Another possibility is that an increase in the share of low-income peers also picks up a shift in disruptive behavior. Disruptive behavior causes harm to academic achievement both in the short and the long run (Carrell and Hoekstra, 2010; Carrell et al., 2018; Kristoffersen et al., 2015; Zhao and Zhao, 2021; Billings and Hoekstra, 2023). In this case, we would expect a negative effect of our peer treatment on educational attainment at each point of the income distribution (see evidence in Carrell and Hoekstra (2010); Carrell et al. (2018)).[3] Yet, in light of our baseline results, predictions based on the effect of an increase in disruptive behavior would only be able to explain our negative estimate on high-income students.

To assess this, we repeat our baseline regressions after also controlling for the share of peers who have fought at school disaggregated by a student's own-position in the income distribution. As our sample consists of adolescents, we see fighting at school as a particularly salient in-school disruption. Results are reported in the Table E.2. We estimate regressions first including both those who report having been in a fight and those who have not. We then drop fighters to avoid concerns over individual's choice to fight confounding the effects of peer disruption through spillovers (Billings and Hoekstra, 2023, e.g., see). We find highly consistent estimates for the share of low-income peers across the income distribution in all specifications, suggesting our baseline treatment effects are not driven by changes in disruptive behavior. We reiterate here that our flexible income controls and our disaggregation over income of the peer dispersion (SD)

[1] We focus on four items that relate to these interactions from the student self-reported questionnaire at wave I: whether teachers care about students, whether students have trouble getting along with teachers, whether teachers treat students fairly, and a mean scale of the above three items. Higher scores in these outcomes reflect better teacher-student interactions.

[2] Ability tracking is reported by school principles at a school wide level. Ability tracking is not asked for other dimensions.

[3] Carrell and Hoekstra (2010), and Carrell et al. (2018) are the only two studies we are aware of evaluating the effects of disruptive peers on student outcomes across the income distribution. Carrell et al. (2018) is the only study examining long-term student outcomes, such as university attendance or attainment of any degree. Their findings point to disruptive peers bringing about negative effects on both low- and high-income students. Carrell and Hoekstra (2010) confirms similar results on test scores in the short-run, though results are imprecisely estimated for the low-income group.

Table E.2. Disruptive peers: share of low-income peers

	University Graduate		
	(1)	(2)	(3)
Fight in School × Bottom 20	-0.03**	-0.03**	
	(0.02)	(0.02)	
Fight in School × Middle	-0.08***	-0.08***	
	(0.01)	(0.01)	
Fight in School × Top 20	-0.08***	-0.08***	
	(0.02)	(0.02)	
Share of Peers Fighting at School × Bottom 20	-0.01	-0.02	0.16
	(0.14)	(0.14)	(0.17)
Share of Peers Fighting at School × Middle	-0.16	-0.16	-0.03
	(0.12)	(0.12)	(0.15)
Share of Peers Fighting at School × Top 20	-0.73***	-0.55***	-0.38
	(0.19)	(0.20)	(0.24)
SLP_{-ics} × Bottom 20		0.18**	0.20**
		(0.07)	(0.09)
SLP_{-ics} × Middle		0.02	0.01
		(0.06)	(0.07)
SLP_{-ics} × Top 20		-0.22*	-0.15
		(0.11)	(0.12)
Observations	11123	11123	8358
R^2	0.25	0.25	0.25
Only Non-Fighters	No	No	Yes

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Fighting at school is an indicator is equal to one if the last physical fight the student had occurred at school. The share of peers fighting at school is a leave-one-out share calculated at the same school-grade level. We also control for the variance of fighting in school at the school-grade level as we do for income. In column (3), we restrict the sample only to those who report not having fought at school.

of logged household income may already have picked up a mechanism via peers' disruptive behavior and our results here are consistent with this interpretation.

E.1.3 Parental inputs

Another potential explanation for our results is through parental response to changes in the share of low income peers. Recent evidence in fact points to substitution effects between parental beliefs about school quality and parental time investments (Greaves et al., 2023). If parents can observe their child's peers and infer the distribution of peer quality (through peer income), they may react adjusting their inputs or parenting style.[4] If peer quality is viewed by parents as

[4]Recent literature examines how parental style can directly intervene in children's peer group formation (Agostinelli et al., 2020). However, we abstract from this mechanisms as both our theoretical framework and our identification strategy treat peers as exogenously determined.

a signal of school quality, parental response could in part compensate, or even dominate, the negative effect of a decrease in school quality (due to a higher share of low-income peers).[5]

Table E.3. Parental involvement

	School-related Involvement			Overall Involvement
	Mother	Father	Parents	Parents
	(1)	(2)	(3)	(4)
SLP_{-ics} × Bottom 20	-0.06	0.03	-0.10	0.02
	(0.19)	(0.24)	(0.19)	(0.19)
SLP_{-ics} × Middle	-0.03	-0.18	-0.13	0.02
	(0.16)	(0.21)	(0.16)	(0.16)
SLP_{-ics} × Top 20	-0.08	-0.32	-0.18	0.18
	(0.23)	(0.26)	(0.22)	(0.22)
Mean Dep Var	0.04	0.04	0.05	0.05
Observations	10699	8049	11073	11073
R^2	0.052	0.054	0.060	0.103

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. We use three measures: (a) talked about their schoolwork or grades, (b) worked on a project for school, and (c) talked about things they were doing in school to construct the school-related involvement scale for mothers and fathers. Scales for mothers and fathers are averaged to create a parent score. Aggregated involvement in column (4) is a composite scale of ten items including all activities such as going shopping, playing a sport, going to a religious service or church-related event, talking about someone they were dating, going to a movie, talking about a personal problem, and having a serious argument about their behavior. Each scale is standardized.

To explore this, we leverage three different measures of parental involvement from our survey, based on whether the child reported to have done any of the following activities with their parents: (a) talking about their school work or grades, (b) working on a project for school, and (c) talking about things they were doing in school. Then, we construct a school-related involvement scale and use it as an outcome. We also build a measure of overall involvement, given by a composite scale of ten items including several activities such as going shopping and playing a sport. Results of this exercise are reported in the Table E.3, where we see no response of parental involvement to variation in the share of low income peers across all different outcomes, suggesting that fluctuations in the share of low-income peers does not trigger any sort of parental response.

[5]Fredriksson et al. (2016) also provide evidence that the response of high-income parents is greater than that of other groups, when there is an increase in class size.

E.2 GPA and advanced courses with maximum sample

Table E.4. GPA and advanced courses: maximum sample estimates

	GPA		Advanced Courses			
	Self	Transcript	Math	Science	English	More than One
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	-0.02	0.71***	0.40***	0.30**	0.07	0.54***
	(0.14)	(0.24)	(0.12)	(0.15)	(0.20)	(0.16)
SLP_{-ics} × Middle	-0.11	0.57**	0.15	0.15	0.01	0.26*
	(0.12)	(0.22)	(0.11)	(0.14)	(0.21)	(0.14)
SLP_{-ics} × Top 20	-0.26*	0.01	0.14	-0.16	0.11	0.05
	(0.15)	(0.27)	(0.13)	(0.17)	(0.23)	(0.15)
Edu non-response weights	NA	Yes	Yes	Yes	Yes	Yes
Mean Dep Var	2.77	2.41	0.40	0.45	0.23	0.59
Observations	14185	8326	8343	8304	5937	8353
R^2	0.197	0.282	0.255	0.214	0.255	0.245

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Column (1) shows the effects of share of low-income peers on self-reported GPA from Wave I In-Home data while column (2) shows the effects on average GPA calculated from the first interviewed year to the end of the high school from Wave III high school transcript data. Columns (3) - (6) show the effects of share of low-income peers on the taking rate of advanced courses of Math, Science, English, and if ever took more than one advanced course. We use specific educational sampling weights constructed to adjust for transcript non-response as well as survey non-response in columns (2) - (6). We use our fully available sample in this table.

E.3 Risky behaviors

Effort in school may also be proxied by risky behaviors. Students who work harder at school may be less likely to engage in such behaviors and vice-versa. There is broad evidence that human capital investment reduces risky behavior (Kenkel et al., 2006; Conti et al., 2010; Cutler and Lleras-Muney, 2010), as well as evidence that the stringency of education dampens risky behavior (Hao and Cowan, 2019). This could be explained by time constraints in case of contemporaneous effects as well as expectation effects, if students anticipate the future cost of engaging in risky behavior in terms of reduced return to human capital.

Add Health provides a range of self-reported risky behaviors that we use from wave I. We assess our effects of interest on these behaviors in Table E.6. We expect these may measured with a degree of error that could obscure results and caution strong conclusions.

We assess drinking behavior in columns (1) - (3). Frequent drinking is an indicator for an above median report on frequency of drinking in the past year; drinking out is whether one drank without their parents present; and binge drinking is an indicator for having ever binged (5 or more) drinks in a single outing in the past year. Next, in columns (4) - (6), we have the number of days one smoked in the past year (column 4); an indicator for above median marijuana use (column 5); and an indicator for having used hard drugs (column 6). Finally, in column (7), we report a measure for having engaged in unprotected sex.

The results for the share of low-income peers have a generally consistent pattern across outcomes. Qualitatively we see mostly negative point estimates for the bottom 20th group and positive point estimates for the top 20th. Many of these are null effects, though not all, thus we do not want to over-interpret them. Nevertheless, the patterns here are consistent with our results on education and particularly show that even if the high income students did not suffer a significant drop in GPA, they still show behavioral patterns consistent with the result on long-term university graduation.

Table E.5. Risky Behavior Summary Statistics

	Mean	SD	Min	Max
Frequently drinking	0.17	0.38	0	1
Drinking with people other than family	0.41	0.49	0	1
Ever binge drinking	0.29	0.45	0	1
Standardized smoking days during the past month	-0.00	1.00	-0.49	2.51
Frequently using marijuana	0.14	0.34	0	1
Ever using hard drug	0.05	0.22	0	1
Standardized having unprotected sex recently	-0.00	1.00	-0.23	6.41
Observations	11165			

This table presents summary statistics for the risky behaviors in Table E.6 after restricting to our analytic sample. The smoking variable originally ranges from 0 to 30 days, and the unprotected sex variable ranges from 0 to 5 times. Both variables have many zeros (69.9% and 94.4%, respectively) and are highly right-skewed. We standardize them to mean 0 and standard deviation 1.

Table E.6. Risky Behavior Outcomes

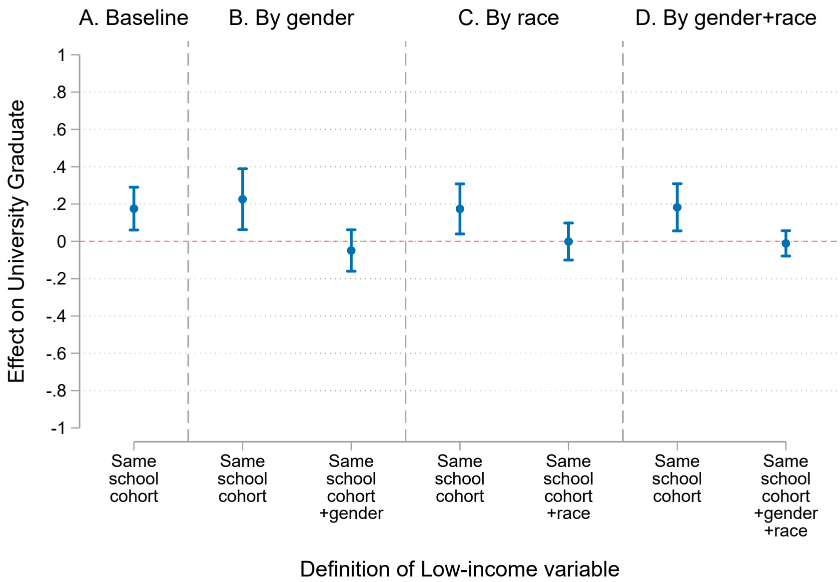
	Frequent Drinking	Drinking Out	Binge Drinking	Smoking	Marijuana	Hard Drug	Unprotected Sex
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SLP_{-ics} × Bottom 20	-0.11	-0.03	-0.09	-0.35*	0.02	-0.00	0.12
	(0.07)	(0.09)	(0.10)	(0.19)	(0.06)	(0.04)	(0.19)
SLP_{-ics} × Middle	-0.06	-0.02	-0.05	-0.05	0.06	0.07**	0.34**
	(0.06)	(0.07)	(0.08)	(0.16)	(0.05)	(0.03)	(0.17)
SLP_{-ics} × Top 20	0.05	0.09	0.10	0.29	0.11*	0.14***	0.46**
	(0.08)	(0.10)	(0.09)	(0.20)	(0.06)	(0.05)	(0.22)
Mean Dep Var	.17	.41	.29	0	.14	.05	0
Observations	11092	11101	10100	9502	11011	11021	11162
R^2	0.083	0.137	0.139	0.134	0.075	0.039	0.038

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. We trim our data to our analytic sample as in Table 2 and standardize smoking and unprotected sex outcomes to mean 0 and standard deviation 1.

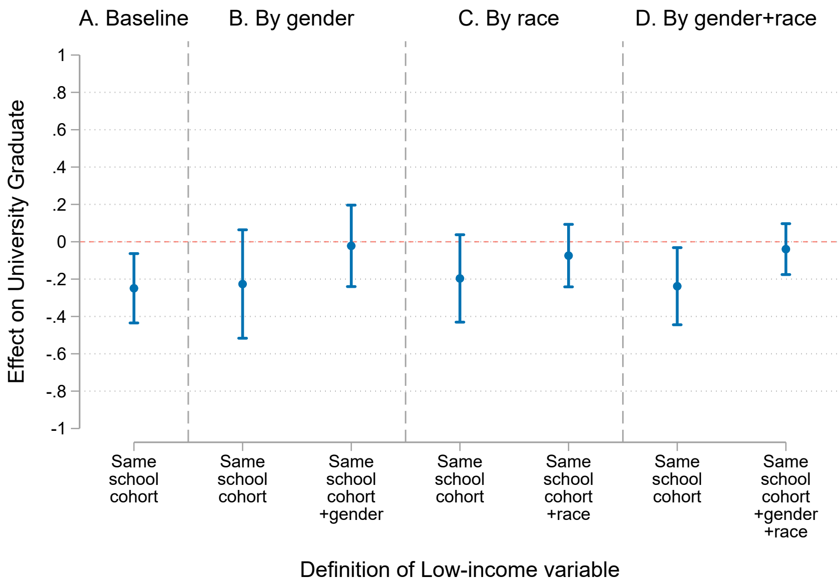
E.4 Social cohesion: additional results

Figure E.1. University completion: different definitions of peers groups

(a) Peer effect estimates for bottom 20



(b) Peer effect estimates for top 20



Notes: These figures tests how different definitions of peer groups compare against our baseline effects from the share of low-income peers on university graduation. We always include school and cohort fixed effects as in column (2) of Table 2. Panel A presents the estimates for students in the bottom 20th percentile of household income. Panels B presents the estimates for students in the top 20th percentile of household income. In each sub-panel, we include both definitions of the share of low-income peers in the regression. The middle-income students are included in the regression but we omit the estimates here as they are null effects.

F Heterogeneity via a Causal Forest

We want to examine heterogeneity across subgroups in our data that may be relevant for policy, e.g., by gender, single parent homes, and so forth. However, our main results are already heterogeneous by whether a student is from a low, middle, or higher-income family. Thus, further heterogeneity across many dimensions is difficult. While absent a larger sample there is no way to avoid this problem, we can use the recently developed, and data driven, causal forest approach to gain a better idea around how our effects differ across both observable dimensions in our data and the family income groups we have used throughout the paper.

Causal forests change the problem from estimating differences in effects across specific groups to nonparametrically recovering heterogeneous treatment effects across individuals. This approach, pioneered by Athey and Imbens (2016); Wager and Athey (2018); Athey et al. (2019), adapts regression trees to capture how treatment effects vary across partitions based on feasible combinations of observable control variables. With a binary treatment, this implies estimating differences in potential outcomes at realization of specific values among the observed controls yielding conditional average treatment effects (CATEs). In our case, we recover conditional average partial effects as $E[Cov[Y_i, W_i]|X_i]/Var[W_i|X_i]$ where Y_i is university graduation, W_i is the share of low-income peers, and X_i is our vector of exogenous individual characteristics. We will refer to these as CATEs for simplicity.

Causal forest works by growing trees. Put simply each tree is a partition of leaves whereby each leaf is a subset of observations with particular realizations of characteristics. Leaves are partitioned by maximizing the variance in treatment effects across partitions tuned with cross validation. In the "honest" implementation of Wager and Athey (2018), each tree is grown by randomly splitting the data into training and estimation subsets, using the training data to grow the tree, i.e., find the partitions, and the estimation sample to make the "out of bag" estimation of the treatment effects within partitions. The out of bag estimates are estimated on each leaf and then aggregated across trees. Importantly, Athey et al. (2019) show that treatment effect estimates under unconfoundness and "honesty" are asymptotically normal, allowing the calculation of confidence intervals.[6]

We employ causal forests but with two pre-step modifications. Note that causal forests rely on unconfoundness either via randomization or through conditioning. Thus, step one: we residualize Y, W, and each of our controls removing school and cohort fixed effects and we do this separately with the bottom 20th, middle, high-income groups. Next, we want to investigate heterogeneity within our already defined low, middle, and high-income groups due to our pre-existing focus on these groups motivated from our theory. Thus, step two: we run the causal forest on each of these income groups separately using the residualized variables from step one. Moreover, we employ cluster-robust random forests at the school level as shown in Athey and Wager (2019).[7] Finally, we stack the out of bag CATE estimates across income groups for analysis.

[6]This discussion omits complexities on tuning parameters discussed in Athey and Imbens (2016); Wager and Athey (2018); Athey et al. (2019).

[7]To implement, we use the *grf* package and *causal_forest* command in *R*.

We first demonstrate that the pattern in the CATEs across income groups matches closely to our previous results in panel (a) of Figure F.1. For the bottom 20th income group, the interquartile range falls entirely in the positive domain with a median of 0.234. The middle group falls right around zero. And, finally, the top 20th group has an interquartile range below zero with a median of −0.229.

Next, in panel (b), we check whether our results vary over cognitive ability. We have already discussed the link between income and ability and we have controlled flexibly for ability and school-cohort ability rank. It could be, however, that only a portion of the ability distribution drives our results. For instance, Carlana et al. (2022a) focus on a treatment applied to higher ability disadvantaged students who at pre-treatment tended to hold lower beliefs about their educational possibilities relative to more advantaged students of the similar ability. It is useful for policy then to understand whether an aspiration gap mechanism centers around certain portions of the ability distribution or is relevant across ability types. We, however, expect that this mechanism is relevant across cognitive ability types, per our arguments that capacity is broader than just cognitive ability, meaning students of different ability types are also faced with other skills and constraints that our mechanism can operate around.

In panel (b) of Figure F.1, we find rather homogeneous effects across the ability distribution (PVT scores) among the bottom 20th and middle-income groups. For the bottom 20th, effects are always positive and quite similar and for the middle-income group the CATEs are near zero and similar across ability. The top 20th group does show some heterogeneity with effects that are always negative but somewhat mitigated at the top end of the ability distribution. While these students may well have a very high capacity, this pattern is suggestive that very high ability students are likely to complete university for many other reasons or they place less weight on the social environment to determine their reference points. This is proxied by γ in our theory. Students with a high family income but who are not in the top of the ability distribution may still have higher capacity due to better opportunities – or alternatively have high beliefs due their family income such that their beliefs are above their true capacity – and may then be the ones who put more weight on the social environment to determine their reference points.

We then report binscatter plots across income deciles split by gender and by dual vs. single parent homes in panel (c) and (d) of Figure F.1. The effects are generally similar across genders but with females experiencing stronger, more positive, effects in the bottom 20th, and somewhat more negative effects in the top 20th. Students from dual parent homes exhibit a similar pattern, with particularly stronger effects among the top 20th.

Now we turn to evaluate the variation in the CATEs across the set of individual characteristics in Table F.1. Our individual characteristics included in the causal forests correspond to those in the Appendix Table B.1. We split each income group by those with a high or low CATE (above or below the median)[8] and then test mean differences in having a high or low CATE across student characteristics and report a p-value adjusted for multiple hypothesis test bias.

First, the median CATE in each income group matches our expectations and previous results. The median CATE is 0.234 for the bottom 20th, −0.002 for the middle, and −0.229 for the top 20th income group. Second, we see a number of significant differences across high and low

[8]Our approach here is similar to that of Carlana et al. (2022a) except that we split across income groups.

median groups in terms of characteristics. Many of these are minimal in magnitude; however, gender and single parent homes stand out.

We find that in the bottom 20th there are significantly more females and more students from dual parent homes with an above median CATE. For the top 20th, we continue to see significant heterogeneity by gender and single parent home status. These differences are significant even after adjusting for multiple hypothesis test bias. Here there is a higher share of females and students from dual parent homes with a below median CATE – as the median here is negative this implies they have a larger magnitude effect in absolute value.

In this case, a reasonable assumption is that adolescents in dual parent homes, and where incomes are high, likely have high capacity through a broader range of opportunities and fewer life stressors. Thus, these students would be farther ahead of their aspiration reference point as the share of low-income peers increases. We cannot, however, make conclusions here and look to these results as suggestive. Possibly a more important takeaway from this exercise is that our results overall are quite consistent across income groups.

Figure F.1. Causal forest heterogeneity in CATEs by income groups

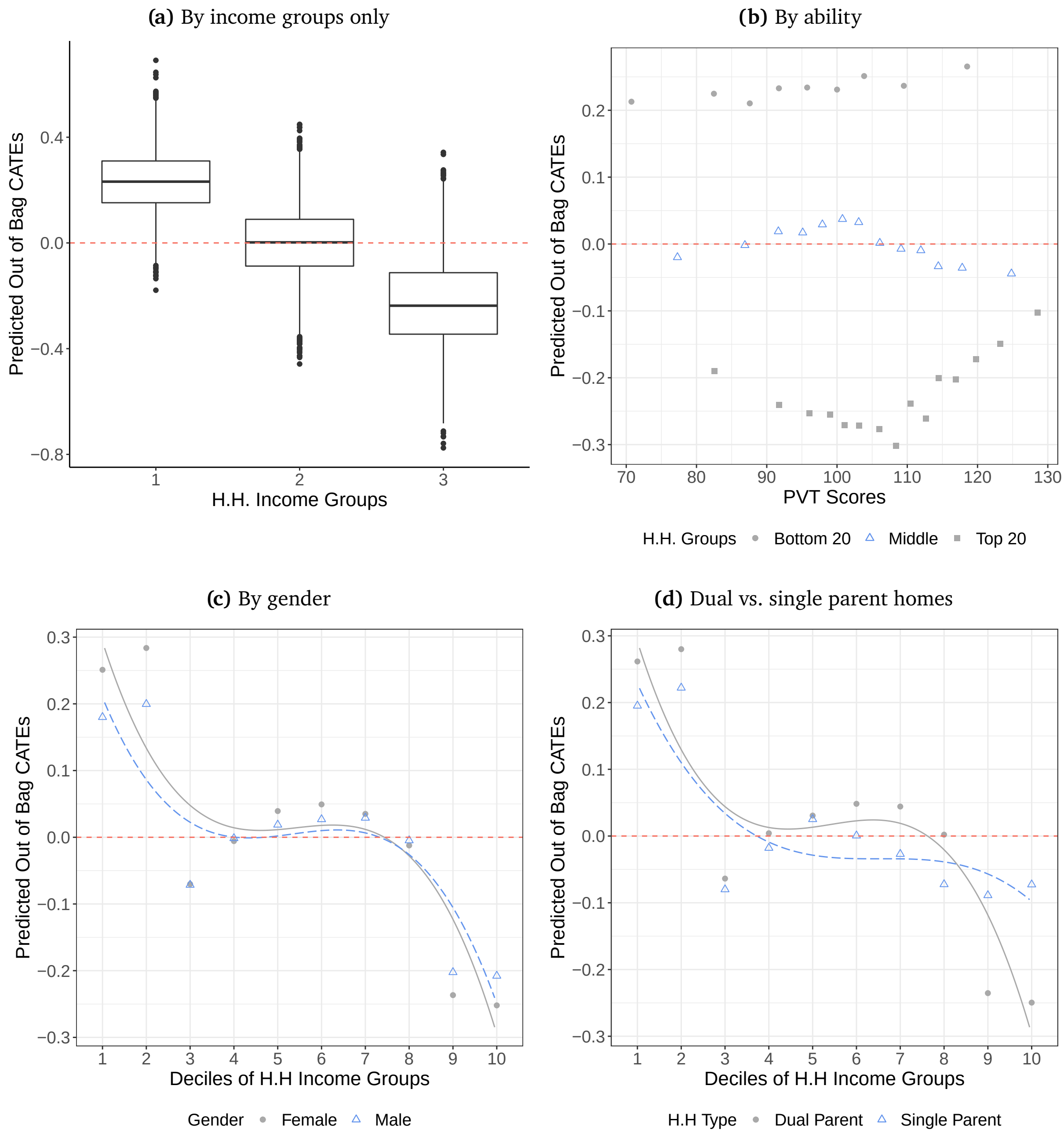


Table F.1. Causal forest: heterogeneity in the CATEs by individual characteristics

	Bottom 20				Middle				Top 20			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	High Predicted CATE	Low Predicted CATE	Diff.	Romano-Wolf p-value	High Predicted CATE	Low Predicted CATE	Diff.	Romano-Wolf p-value	High Predicted CATE	Low Predicted CATE	Diff.	Romano-Wolf p-value
Less than HS Parents	0.302	0.388	-0.086	0.130	0.091	0.132	-0.040	0.031	0.038	0.023	0.014	0.264
HS or GED Parents	0.572	0.507	0.064	0.368	0.546	0.547	-0.001	0.986	0.209	0.342	-0.133	0.003
College Parents	0.127	0.105	0.022	0.768	0.363	0.322	0.041	0.063	0.753	0.635	0.118	0.003
Missing Parents' Education	0.076	0.093	-0.017	0.800	0.035	0.043	-0.008	0.520	0.027	0.006	0.021	0.037
Female	0.700	0.395	0.305	0.003	0.525	0.516	0.008	0.920	0.459	0.564	-0.105	0.017
Age	15.298	15.714	-0.416	0.029	15.560	15.322	0.238	0.016	15.444	15.609	-0.165	0.264
Age Squared	236.792	249.677	-12.885	0.029	244.905	237.707	7.198	0.018	241.020	246.604	-5.583	0.209
Hispanic	0.238	0.229	0.008	0.962	0.120	0.169	-0.049	0.018	0.083	0.053	0.030	0.182
Black	0.326	0.362	-0.037	0.721	0.199	0.156	0.043	0.047	0.092	0.165	-0.073	0.017
Asian	0.028	0.021	0.007	0.876	0.057	0.042	0.014	0.135	0.075	0.052	0.022	0.264
Other Races	0.022	0.021	0.001	0.962	0.018	0.015	0.003	0.920	0.010	0.008	0.002	0.875
Missing Races	0.000	0.001	-0.001	0.883	0.000	0.003	-0.003	0.090	0.005	0.000	0.005	0.264
Child of an Immigrant	0.228	0.212	0.016	0.895	0.153	0.182	-0.030	0.090	0.138	0.115	0.023	0.270
Missing Child of an Immigrant Info	0.002	0.003	-0.001	0.962	0.001	0.001	-0.000	1.000	0.000	0.001	-0.001	0.584
Single Parent Household	0.528	0.707	-0.179	0.003	0.202	0.328	-0.126	0.002	0.187	0.027	0.160	0.003
Family Size	3.928	3.361	0.567	0.003	3.982	3.624	0.358	0.002	3.893	3.907	-0.014	0.875
Ability (AHPVT scores)	96.594	93.130	3.463	0.035	101.719	103.014	-1.295	0.047	108.521	105.925	2.595	0.017
Median CATEs	0.232				0.002				-0.237			
Observations	1090	1090	2180		3460	3460	6920		1033	1032	2065	

Notes: We report summary statistics as the mean for each characteristic split by those above or below the median of CATEs in a specific income group. We also report the difference between the means in columns 3, 7, and 11. Columns 4, 8 , and 12 show the Romano-Wolf p-values adjusted for multiple hypothesis testing. Note that for the top 20 group an above median (high) CATE would imply values closer to zero and a below median (low) CATE implies values that are more negative. See Figure F.1 for reference.

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