Reference Dependent Aspirations and Peer Effects in Education*

Marco Fongoni[1], Jonathan Norris[2], Agnese Romiti[2], and Zhan Shi[2]

[1]Aix-Marseille Univ, CNRS, AMSE
[2]University of Strathclyde

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Abstract

We study the long-run effects of income inequality within adolescent peer compositions in schools. We propose a theoretical framework based on reference dependence where inequality in peer groups can generate aspiration gaps. Guided by predictions from this framework we find that an increase in the share of low income peers within school-cohorts improves the educational outcomes of low income students and has negative effects on high income students. We further document a range of evidence that corroborates these results, including that they are distinct from peer non-linear ability effects. We then find that social cohesion, through better connections in the school network, has an important role in mitigating the effects of peer inequality. Our results provide evidence on the role of inequality in peer groups for long-run educational outcomes, while also demonstrating that there is potential to avoid these consequences.

Keywords: Peer Effects, Inequality, Education
JEL-Codes: I21, I24, I29, J24

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*Corresponding author: Jonathan Norris (jonathan.norris@strath.ac.uk). Marco Fongoni (marco.fongoni@univ-amu.fr), Agnese Romiti (agnese.romiti@strath.ac.uk), and Zhan Shi (zhan.shi@strath.ac.uk). Special thanks are due to Kirill Borusyak, Benjamin Elsner, Lukas Kiessling, David Ribar, and participants at the 37th National Conference of Labour Economics for helpful comments and suggestions. This research uses data from Add Health, a program project directed by Kathleen Mullan Harris and designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill, and funded by grant P01-HD31921 from the Eunice Kennedy Shriver National Institute of Child Health and Human Development, with cooperative funding from 23 other federal agencies and foundations. Information on how to obtain the Add Health data files is available on the Add Health website (http://www.cpc.unc.edu/addhealth). No direct support was received from grant P01-HD31921 for this analysis. Fongoni acknowledges funding from the French government under the "France 2030" investment plan managed by the French National Research Agency (reference: ANR-17-EURE-0020) and from Excellence Initiative of Aix-Marseille University - A*MIDEX

1 Introduction

Attainment of higher education has become an increasingly important concern for inequality. Labour market returns to college completion can be large over the life course (Barrow and Malamud, 2015), and in general, returns are increasing in the cognitive and noncognitive skills that develop through effortful participation in education (Deming, 2017; Heckman, Humphries, and Kautz, 2014). In the midst of this, students form aspirations or goals for their educational attainment that may reflect their advantage or disadvantage and put students with a low socio-economic standing (SES) at risk of being disincentivized in their efforts and choices (Carlana, La Ferrara, and Pinotti, 2022a; Guyon and Huillery, 2021). We know less, however, about how exposure to inequality among students affects their effort and long-term educational attainment.

In this paper, we study income inequality within adolescent peer compositions in schools. We ask how its effect on long-run college completion varies across students' positions in the income distribution and show that these are distinct from peer non-linear ability effects. This is motivated by evidence that inequality is predictive of dissatisfaction and risky behaviours, especially among those with a low socio-economic standing (SES) (Balsa, French, and Regan, 2014; Payne, Brown-Iannuzzi, and Hannay, 2017), and from field experiments where moving disadvantaged children to better neighbourhoods generates null or even negative effects among those moving in adolescence (Chetty and Hendren, 2018a; Chetty, Hendren, and Katz, 2016). Or, 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.[1] 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." Although anecdotal, this story illustrates a larger point that the stress from the realization of inequality may create frustration or even adjust the reference points for success and the hope to reach it.

We draw empirical predictions from a theoretical model that builds on the idea of aspiration gaps (see, e.g. Genicot and Ray, 2017). In our model, students form a reference point for aspirations based on both their capacity to transform effort into outcomes and on the social environment defined by the educational capacity of their peers. We define our concept of capacity as the combined set of abilities and opportunities that enable a student to perform. This implies, for instance, that a student with the raw ability to achieve higher education may still have a low capacity due to facing severe stressors at home. Thus, the distribution of capacities in the social environment captures not only inequality in ability but also important dimensions such as inequality in noncognitive skills, in opportunities,

[1] Episode 550: Three Miles available at https://www.thisamericanlife.org/550/three-miles.

and in cognitive load.[2] We then interpret income and the distribution of peer income as measures that can capture inequality over many of these dimensions and one that is likely salient to adolescents.[3] We base this on evidence that SES is positively related to skill trajectories over childhood (Doepke, Sorrenti, and Zilibotti, 2019; Falk et al., 2021) and evidence that the conditions of poverty tax mental bandwidth (Haushofer and Fehr, 2014; Lichand and Mani, 2020; Mani et al., 2013). Thus, within school income inequality can capture within school inequality in capacities.

In our theory, students have aspirations, partly determined by the distribution of capacity among their peers. Lower capacity students are more likely to fall behind their aspirations than higher capacity students. This results in a negative aspiration gap and frustration for low capacity students and a positive aspiration gap for high capacity students. Shifts in the distribution of peer capacity will then have non-linear effects across students. An increase in this distribution raises aspirations, leaving low capacity students even more frustrated, reducing their effort, while high capacity students will become more motivated, increasing their effort. Students in the middle of the capacity distribution may be arrayed more closely around their aspiration reference point, balancing positive and negative effects to an average effort response of zero. Thus, our model suggests very different responses depending on a student's capacity that we empirically investigate around household income.

Empirically we operationalise our predictions using the leave-one-out share of low income peers in a student's school cohort to capture shifts in the degree of peer inequality. We use the share as our theoretical predictions are about shifts in the distribution, which the peer mean of income may not well capture.[4] We then test the effects from the share of low income peers on college completion across the distribution of students' own-household income. Drawing on our theoretical framework, our reduced form empirical predictions for an increase in the share of low income peers are (i) low income students experience positive effects improving on college completion, (ii) middle income students have on average null effects, and (iii) higher income students experience negative effects. For low income students, our prediction implies they are more likely to fall below the aspiration reference point for a given distribution of peers. Therefore, an increase in the share of low income peers reduces the size of their negative aspiration gap, leading to more educational effort where effort in turn leads to better long-run outcomes. Predictions for middle and high income students also similarly follow from our theoretical framework.

[2]Cognitive load represents a tax on mental bandwidth due to exposure to, and often sustained, environmental stressors (Mani et al., 2013).

[3]We discuss this in more detail in Section 3.1 and provide empirical evidence that links these concepts.

[4]We discuss this point again in the data section but note here that the interaction of the peer mean with the peer standard deviation may also capture this, but would be difficult as we need to assess the heterogeneity in the effect across students' position in the income distribution.

To identify our effects, we leverage within school, across cohort variation and flexibly control for the effect of students' household income. We compare students in the same school who have similar household incomes and similar characteristics but who face 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, avoiding contamination of our treatment effect which is split across students' position in the income distribution.[5] Our data is from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health samples students attending the same schools and provides a representative sample of US students during the 1994/95 school year who were in grades 7-12. These students were followed over time allowing us to observe eventual educational attainment.

We find empirical results that match our predictions: increasing the share of low income peers (i) increases the likelihood of completing college for low income students (bottom 20th percentile), (ii) has null effects on middle income students, and (iii) decreases the likelihood of completing college for high income students (top 20th percentile). Our effects on the bottom and top income students are sizeable. A standard deviation shift of a 20% increase in the share of low income peers raises the propensity to complete college by 3.6pp for the bottom and decreases it by 4.1pp for the top. These effects are sizable and of similar magnitude of other interventions aimed at fostering human capital investment. An example is from Pell Grants which offer means-tested financial assistance. The magnitude of our effects are comparable to the effects of financial assistance of about $1,000 in Pell grants to high-school students.[6] Our results are confirmed by a wide range of robustness checks lending strong support to our key predictions. Moreover, we find no evidence these effects are driven by either ability ranks or non-linear peer ability effects. Further, we find no evidence the effects are driven by teacher-student relationships, as opposed to our proposed aspiration gaps mechanism.

In Sections 4.3, 4.4, and 4.5 we turn to mechanisms through short-run measures to test whether effort responses to changes in the share of low income peers match our expectations. We look at measures of academic performance using high school transcript data on Add Health participants, at self-reported risky behaviours, and at self-efficacy – self-esteem, relative intelligence, and depressive symptoms. We find a strong pattern of results across these outcomes consistent with our proposed theory and expectations.

[5]Note, we additionally aim to avoid competing mechanisms from potential rank concerns, e.g., ability rank, or changes in peer behaviour, which in Section 3.2 we suggest can be captured by including the leave-one-out peer standard deviation in logged household income interacted over the students' position in the income distribution.

[6]This intervention is not entirely comparable with ours as they focus on college enrollment rather than college completion, see section 4.1 for more detail.

We also look at beliefs and expectations about college in Section 4.5. Here we find that low income students improve in terms of these outcomes in response to our treatment. Our theory is static and in it low capacity students with more lower capacity peers have smaller aspiration gaps due to a lower aspiration environment. However, note that if they face a sizable negative gap from a high aspiration environment, they can be frustrated which dynamically could lead to giving up. We conjecture that in a dynamic process being able to maintain higher effort and better self-efficacy can lead to a higher level of beliefs – dynamically to better aspirations – and our effects from an increase in the share of low income peers are consistent with this.

As stated earlier, we interpret peer income as a measure that can capture a variety of capacity dimensions. An important note, is that we do not aim to uncover the specific feature captured by our non-linear peer income effects, e.g., such as from the peer distribution of cognitive load or opportunities. Rather, we see income as a salient measure, likely observable to students, that allows us to broadly capture the non-linear effects of peer inequality which are motivated by our theory. We do, though, carefully show that our results are not driven by non-linear peer ability effects, which have been observed in the literature (Booij, Leuven, and Oosterbeek, 2017; Feld and Zölitz, 2017). Thus, an ability tracking scheme would not be enough to remove the consequences of peer income inequality. Put differently, it may not be possible to track on all the dimensions that family income captures and that may generate aspiration gaps among students.

What then can improve schools' ability to support disadvantaged students faced with inequality? Our theory suggests this lies partly in the ability to reduce the weight students put on peer distributions when forming reference points. Improving the social environment through friendships and social cohesion is one contender. A recent literature on social cohesion shows that it can improve students perception of their environment (Alan et al., 2021b), help the performance of disadvantaged children (Alan et al., 2021a), and that for people with a low SES friendships with people from a higher SES is an important predictor of income mobility (Chetty et al., 2022). Moreover, we keep our model simple to focus the intuition, but it could be that students do not know their true capacity and have formed beliefs that correlate with their family income. They would then update their beliefs such as in Kiessling and Norris (2022). In this case, factors that helped students believe they are nearer or further from the reference point – such as social cohesion in schools – would then either mitigate or exasperate the consequences of inequality. Using friendship nominations, we show in Section 5.1 that social integration through friendships moderates the effects from the share of low income peers on college completion. This holds for both low and high income students. Programs to improve social cohesion in schools may then have an important role in moderating aspiration gaps from inequality.

Finally, in Section 5.2, we assess additional layers of heterogeneity in our treatment effects across student characteristics. To do this, we employ the recent development of a machine learning approach known as causal forests to recover nonparametric estimates of the treatment effect at the individual level (Athey and Imbens, 2016; Athey, Tibshirani, and Wager, 2019; Athey and Wager, 2019). We first show that the pattern in these estimates across low, middle, and high income groups matches that from our baseline results. Last, we tend to find generally similar results across student characteristics with some heterogeneity by gender and dual versus single parent homes.

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 can cause low SES families to struggle with investing in their children's skills relative to high SES families (Doepke, Sorrenti, and Zilibotti, 2019; Doepke and Zilibotti, 2017). Additionally, neighbourhood 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 inequality within peer groups in schools. Furthermore, our results offer an additional explanation for why the benefits of moving to a better quality neighbourhood have been found to be mitigated if a child moves at a later age (Chetty and Hendren, 2018b; Chetty, Hendren, and Katz, 2016). During adolescence, the weight placed on peer compositions for forming aspirational reference points may become stronger and consequentially inequality within peer groups more salient as a source of information.

This study further relates to a growing literature on the long-run effects of school environments and peer compositions. These include effects from teacher quality (Chetty, Friedman, and Rockoff, 2014; Rothstein, 2017), smaller classes (Angrist and Lavy, 1999; Angrist et al., 2019; Chetty et al., 2011; Krueger and Whitmore, 2001), school spending (Jackson, Johnson, and Persico, 2015), and tracking students by ability (Duflo, Dupas, and Kremer, 2011; Guyon, Maurin, and McNally, 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 tends to run through dimensions unrelated to test score value added. Our study can help shed light here, as this fits with our results on schools with better social cohesion representing where and when disadvantaged students may not be harmed by aspiration gaps in a more advantaged environment.

In terms of peer compositions, we focus attention on the long-run effects from variation in inequality within peer groups. Thus we contribute to a recently developing evidence based on the long-run effects of peers, which finds that disruptive peers during childhood decrease earnings in adulthood (Carrell, Hoekstra, and Kuka, 2018), that for females labour force participation is affected by the labour force participation of peers' mothers (Olivetti, Patacchini, and Zenou, 2020), that high school peers' gender affects choice of college major (Anelli and Peri, 2019), and that among low ability students ability rank in high school affects mental health in adulthood (Kiessling and Norris, 2022).

Somewhat more related to our study in terms of peer compositions, Bifulco et al. (2014) and Bifulco, Fletcher, and Ross (2011) find that peers' parental education effects educational choices in the near term but that these effects fade out over time. Additionally, Gagete-Miranda (2020) and Norris (2020) find positive spillovers from friends' educational attitudes on one's own-attitudes. Our mechanism and empirical results are distinct and focus on how inequality can relate to aspirational reference points such that aspiration gaps can generate very different effects from peer group inequality across the distribution of students' family income.

We also relate to the broader range of studies on peer effects. Particularly, Booij, Leuven, and Oosterbeek (2017) and Feld and Zölitz (2017) find that the effects from peer academic performance on own-performance are non-linear around ability such that low-ability students can be harmed by higher-ability peers. Our aspiration gaps mechanism provides intuition that can help explain these results but we also show that our results are not described by peer non-linear ability effects. More broadly peer effects have been examined on a wide range of topics. A non-comprehensive summary of these studies are that: there is a positive relationship between peers' persistence and academic achievement (Golsteyn, Non, and Zölitz, 2021), low-achieving Kindergarten peers affect non-cognitive skill development (Bietenbeck, 2020), and a range of studies have examined the consequences of peer gender compositions (Black, Devereux, and Salvanes, 2013; Borbely, Norris, and Romiti, 2021; Gong, Lu, and Song, 2021; Lavy and Schlosser, 2011).

Another strand of studies on peer effects looks at students' ability rank among their peers. These studies find a positive link between rank and educational outcomes (Delaney and Devereux, 2021; Elsner and Isphording, 2017; Elsner, Isphording, and Zölitz, 2021; Murphy and Weinhardt, 2020) and future labour market earnings (Denning, Murphy, and Weinhardt, 2021) and negative links with risky behaviours, bullying, and depressive symptoms (Comi et al., 2021; Elsner and Isphording, 2018; Kiessling and Norris, 2022; Pagani, Comi, and Origo, 2021). As we study peer effects across the income distribution, it is important for us to control out rank effects as a potential competing mechanism that could explain our effects rather than aspiration gaps. We do this in multiple ways – discussed in Section 3.2 – and find no evidence that rank effects explain our results. Overall, we add to the broad literature in peer effects by highlighting how reference dependence in aspirations can generate non-linear responses to inequality in peer groups and empirically find supportive evidence in the short- and long-run.

2 Model

2.1 Preferences

Students are endowed with initial capacity θ and are assumed to be heterogeneous on this dimension. We define capacity as the combined set of factors that enable a student to translate effort into outcomes. Later, empirically, we interpret family income as a measure that captures many of these dimensions, which implies that the distribution of capacity a student faces in school captures within school income inequality. Now, we formalize our theoretical intuition.

Students choose effort e to achieve an educational outcome y, realized attainment, given by $y = y(e, \theta) = \theta e$. Further, each student has an individual aspiration level a over the educational outcome y. We define this aspiration level a as the aim or goal a student forms for their outcome. We assume students' preferences to be characterised by the following additively separable utility function:

$$u(e, y, a) = b(y) - c(e) + \mu(y - a), \tag{1}$$

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; and $\mu(y - a)$ captures the effect of aspirations over outcomes on a student's utility. We assume μ to be a reference-dependent gain-loss function, such that $\mu''(y - a) < 0$ if $y > a$ (i.e. concavity over gains) and $\mu''(y - a) > 0$ if $y < a$ (i.e. convexity over losses):

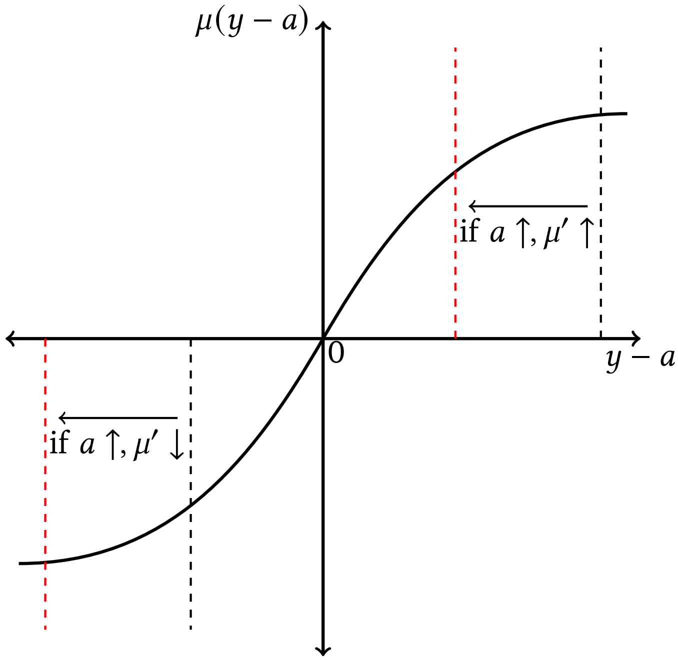
$$\mu(y - a) = \begin{cases} [y - a]^{\beta} & \text{if } y \geq a \\ -[a - y]^{\beta} & \text{if } y < a; \end{cases} \tag{2}$$

where $\beta \in (0, 1)$.[7] The properties of μ imply that whenever a student exceeds their aspirations, $y > a$, they perceive additional satisfaction form achieving y, which positively affects utility; and whenever a student falls short of their aspirations, $y < a$, they instead perceive a sense of frustration, which negatively affects utility. Moreover, whenever $y > a$,

[7] This formulation is in the spirit of Kahneman and Tversky (1979) and Tversky and Kahneman (1991) value function under riskless choice. 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 the qualitative predictions of our theory. 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 aspirations.

an increase in aspirations increases the marginal benefit of an additional unit of effort, and increasing effort will increase utility. Instead, whenever $y < a$, an increase in aspirations would decrease the marginal benefit of an additional unit of effort, implying that decreasing effort will increase utility: as the aspirations gap widens, higher frustration dampens the incentive to exert effort. The effect of an increase in aspirations on μ is shown in Figure 1.

Figure 1. Reference Dependence and Aspirations



2.2 Aspirations and students' effort

In this section we formally characterise how aspirations affect a student's choice of effort.

Consider a student endowed with capacity θ and with aspiration a, that needs to choose effort e to maximise their utility as given by (1). The first-order conditions characterising this maximisation problem are given by:

$$\alpha[\theta e]^{\alpha-1}\theta - e + \beta[\theta e - a]^{\beta-1}\theta = 0 \qquad \text{if } y > a \tag{3}$$

$$\alpha[\theta e]^{\alpha-1}\theta - e + \beta[a - \theta e]^{\beta-1}\theta = 0 \qquad \text{if } y < a \tag{4}$$

Denote the solution by $\tilde{e}(\theta, a)$, which is the level of effort at which the marginal benefit of exerting effort are equal to the marginal cost. Nevertheless, since the marginal benefit of effort crucially depend on the aspirations gap $y - a$, the properties of $\tilde{e}$ might differ depending on whether aspirations are frustrated, $y < a$, or exceeded, $y > a$.

To see this, consider a student with sufficiently low aspirations so that, for given θ, aspirations are exceeded $y > a$ at the optimal effort $\tilde{e}$. Denote the optimal effort in this case as $\tilde{e}(\theta, a)^+$, which is the solution to (3). To understand how aspirations can affect behaviour in this case, suppose that this student's aspiration will increase. For example, they may increase when the student is in a better environment for education. As aspirations increase, the marginal benefit of increasing effort is higher, implying that the student will exert more effort. However, effort is costly and a student's capacity is limited by θ, which implies that there is an aspiration threshold a^* beyond which utility is maximised by the solution to (4) which we denote as $\tilde{e}(\theta, a)^-$. Aspirations here become frustrated, and the aspirations gap becomes negative. Moreover, as aspirations increase further, the marginal benefit of putting more effort are now lower, implying that the student will begin to exert less effort.

These considerations lead to the following proposition.

Proposition 1. *For a given θ, there exists a unique aspiration level a^* such that: if $a < a^*$, aspirations are exceeded, and the student's effort increases as aspirations increase; if $a > a^*$, aspirations are frustrated, and the student's effort decreases as aspirations increase; if $a = a^*$ the student's educational outcome is maximised.*

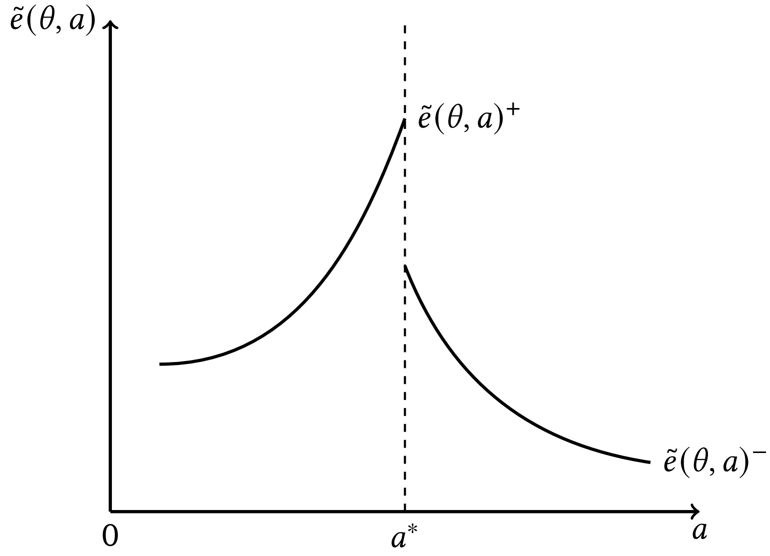
Proposition 1 formally establishes that a student's behaviour, in terms of effort, depends on their aspiration level in relation to some threshold a^*. When aspirations are exceeded, an increase in aspirations will foster an increase in effort. However, there is an aspirations level beyond which aspirations become frustrated and the student begins to lose motivation to exert effort. As aspirations increase further, effort decreases, widening the negative aspirations gap and increasing the frustration. Since effort is increasing in aspirations for all $a < a^*$ and decreasing in aspirations for all $a > a^*$, it follows that effort, and the achieved educational outcome y, are maximised when $a = a^*$.

The relationship between student's effort and aspirations, for a given θ, is plotted in Figure 2. Notice that since students are heterogeneous in θ, this implies that there also exists a distribution of a^*: each student has a different aspirations threshold, depending on their capacity.

2.3 Aspirations formation

We want to examine the effect of peers on individual students' aspirations and educational outcomes. To do so, we focus on a model of aspirations formation which explicitly considers the influence of a student's social environment on aspirations (in the spirit of Genicot and Ray (2017)), and abstract from determinants of aspirations that are internal to the individual (as formally analysed in Dalton, Ghosal, and Mani (2016)). Denote the distribution of capacities that characterises an individual student's social environment or network by F^θ. We assume that aspirations are a continuous and non-decreasing function of a student's capacity θ and of the capacity distribution F^θ. In particular, we consider

Figure 2. Optimal Effort and Aspirations



the following functional form:

$$a = a(\theta, F^\theta) = \gamma\theta + [1 - \gamma]\mathbb{E}\theta, \tag{5}$$

which implies that aspirations are a weighted average of θ and the mean of F^θ, denoted by $\mathbb{E}\theta \equiv \int \theta \, dF^\theta(\theta)$.[8] Hence, i) an increase in a student's capacity increases aspirations; and ii) for a given individual θ, a student surrounded by peers with higher capacities will have higher aspirations than a student surrounded by peers with relatively lower capacities. Within this specific formulation, $\gamma \in (0, 1)$ captures the relative importance of a student's own capacity *versus* their social environment on the formation of aspirations.

2.4 Peer effects through aspirations on students' effort

In this section we formally establish how changes in the social environment of students will affect their aspirations, and how this, in turn, will affect their choice of effort.

To begin with, consider the following proposition, which recasts the results established so far in terms of students' capacity endowment, also considering the effect that a student's own capacity has on effort through their aspiration formation as given by (5).

Proposition 2. *For a given F^θ, there exists a threshold θ^* such that, if $\theta < \theta^*$ then aspirations are frustrated, while if $\theta > \theta^*$ aspirations are exceeded.*

Proposition 2 states that students with lower capacities are more likely to be in the frustration zone, in which $y(\tilde{e}^-, \theta) < a$, than students with higher capacities, for which

[8]Note, this formulation is in the spirit of the aspiration formation model of Genicot and Ray (2017), and it satisfies all its properties. Crucially, it satisfies both "scale-invariance" and "social monotonicity".

$y(\tilde{e}^{+}, \theta) > a$. This result bears important implications for the effect of an increase in aspirations on students' behaviour and educational outcomes. Setting higher goals and aspirations 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.

We can then use the result established in Proposition 2 to classify students in terms of their relative position in the capacity distribution. More precisely, for a given $h > 0$, where h is large enough, we define "low capacity" students those endowed with $\theta < \theta^{*} - h$, "high capacity" students those endowed with $\theta > \theta^{*} + h$, and "middle capacity" students those endowed with $\theta \in [\theta^{*} - h, \theta^{*} + h]$.

Proposition 3. *Consider a shift in the capacity distribution from F^{θ} to $\hat{F}^{\theta}$, where $\hat{F}^{\theta} < F^{\theta}$, and such that $a(\theta^{*} + h, \hat{F}^{\theta}) < a^{*}$. Aspirations will increase across students. Moreover, low capacity students will decrease their effort, high capacity students will increase their effort, while the effect on middle capacity students is ambiguous: those endowed with $\theta < \theta^{*}$ will decrease their effort, while those endowed with $\theta \geq \theta^{*}$ will increase their effort, as long as $a(\theta, \hat{F}^{\theta}) < a^{*}$.*

Proposition 3 links the previous observation with one specific way in which aspirations can change by the same amount across students, that is, via an increase in the capacity of all students in a given social environment or network. In such a case, as average capacity increases, low capacity students will loose motivation (the marginal benefit of an additional unit of effort is lower) and decrease effort as the aspirations gap widens. High capacity students will instead gain motivation, and increase their effort. For middle capacity students the effect of an increase in aspirations is ambiguous, as some of these students will loose motivation and decrease effort, while some others will gain motivation and increase effort. Moreover, among this latter group, there will be a fraction that had aspirations in the proximity of a^{*} from below, and for whom an increase in aspirations due to an increase in peers' capacity will push them beyond a^{*} and make them switch from satisfaction to frustration.

2.5 Empirical Predictions

We draw empirical predictions from our theoretical model. First, we think of capacity as an increasing function of family income where importantly income may be a salient feature to children in school and it has direct implications for inequality. This idea is based on the fact that SES is positively related to skill trajectories (Doepke, Sorrenti, and Zilibotti, 2019; Falk et al., 2021) and negatively correlated with cognitive load (Haushofer and Fehr, 2014; Lichand and Mani, 2020; Mani et al., 2013). We further provide supportive empirical evidence linking capacity to income (see section 3.1). We will then proxy low (high) capacity with belonging to the bottom (top) part of the household income distribution. Second, we will formalise the capacity distribution, and the shift thereof students are exposed to, by using the share of peers from low-income families.

Our theory predicts that low-capacity students are more likely to be in the frustration zone, where the aspiration gap is negative, than high-capacity students, for which aspirations are likely to be exceeded and the aspiration gap is positive. Middle-capacity students are likely to experience either frustration or satisfaction. As we link capacity with income, these predictions also apply on the income dimension. Thus, considering an increase in the share of low-income peers (which would correspond to a shift from F^{θ} to $\breve{F}^{\theta}$, where $\breve{F}^{\theta} > F^{\theta}$) students' aspirations will decrease, and our theory would imply the following:

Prediction 1. *For students belonging to the lower end of the household income distribution, an increase in the share of low-income peers would induce a reduction in aspiration gaps and ultimately higher educational attainment.*

Prediction 2. *For students belonging to the higher end of the household income distribution, an increase in the share of low-income peers would induce an increase in aspiration gaps and lower educational attainment.*

Prediction 3. *For students around the middle of the household income distribution, an increase in the share of low-income peers would induce both decreasing and increasing aspiration gaps, which may on average balance towards a weaker, or null, effect on educational attainment.*

3 Data and Empirical Strategy

3.1 Data

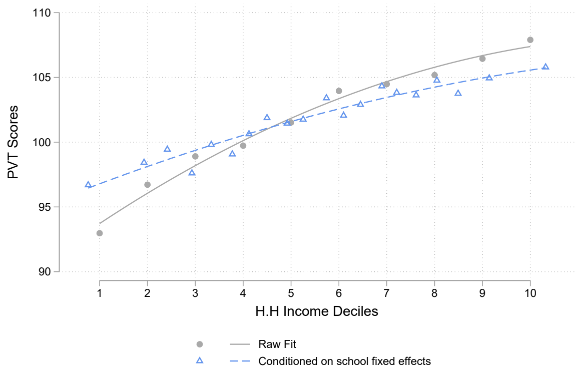
We use restricted data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a longitudinal study of a nationally representative middle and high schools in the United States. For our analysis, the Add Health dataset has several key features. First, it covers multiple cohorts within schools, which we need for our empirical strategy exploiting variation within schools across cohorts. Second, a representative set of students from each cohort is sampled. Third, students were 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. Hence, we can follow the development of adolescents' educational outcomes. Fourth, the dataset includes household incomes, which we will use to proxy capacity and to measure peer inequality through the share of low-income peers within each student's school-cohort.

Linking income and capacity. We think of capacity as a broad construct spanning many things including ability, noncognitive skills, cognitive load, and differences in opportunities. We use household income at the wave I survey to proxy our theoretical concept of capacity. As discussed in the introduction, low-income is linked to lower skill trajectories partly due to unequal environments and investments in early life skill development (Doepke, Sorrenti, and Zilibotti, 2019; Falk et al., 2021).

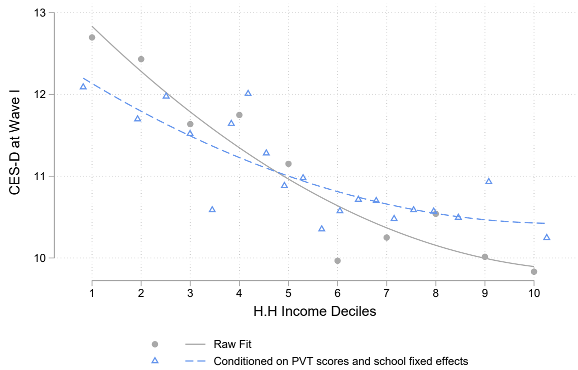
In Figure 3a, we use the AddHealth Picture Vocabulary Test (PVT) score – taken by all respondents at wave I – as a proxy for cognitive skills and plot the association with household income deciles. Both in the raw data and after conditioning on school fixed effects, we find that higher income deciles are linked to higher cognitive skills and low income deciles to lower cognitive skills, which is in support of our assumption about skill trajectories and income.

Figure 3. PVT Scores, CES-D, and Parents' Monetary Investment by Household Income Deciles

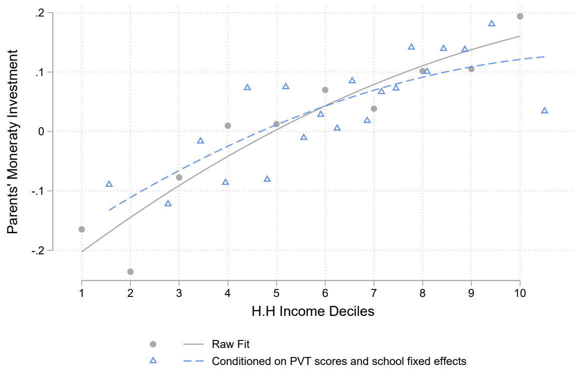
(a) PVT Scores Wave I



(b) CES-D Wave I



(c) Parents' Monetary Investment Wave I



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.

Our concept of capacity, however, is broader than just cognitive ability. Being low-income likely captures other factors that may be observable and salient to adolescents – such as being poor while observing peers with new fashion or expensive cars – based on which they may draw an inference about their relative opportunity. Moreover, being a low-income student may restrict capacity despite ability through facing greater cognitive scarcity. The conditions of poverty put individuals under more uncertainty and greater stress, which can detract attention or diminish cognitive resources harming performance (Haushofer and Fehr, 2014; Lichand and Mani, 2020; Mani et al., 2013). Thus, even students who have a strong raw ability can be harmed in their capacity to translate effort into outcomes when they live in low-income conditions.

Supportive of this assumption, we see in Figure 3b that lower income students tend to score higher on depressive symptoms than do wealthier students using the Center of Epidemiologic Studies Depression Scale (CES-D, Radloff, 1977).[9] Next, in Figure 3c we see that lower income students receive fewer monetary investments from parents which connects to opportunity. These patterns continue to hold even after conditioning on school fixed effects and PVT scores, implying they are not simply reflecting ability. Further, the behavioural science literature finds that adolescents exposed to multiple stressors are at a greater risk for higher depressive symptoms (Thapar et al., 2012). If conditions of low-income expose a student to more stressors, thereby higher cognitive load, we would then expect these students to have higher scores on the CES-D scale. Higher depressive symptoms may then harm student capacity to perform well in school, as a loss of motivation is a behavioural component of depressive symptoms (De Quidt and Haushofer, 2019). The patterns we see here are consistent with this and suggest that low income indeed may capture a variety of sources that matter for our theoretical concept of capacity. Next, we discuss how we define low-income to calculate the share of low-income peers.

Definition of low-income peers. Our theory is about shifts in the distribution, which the mean income of peers may not well capture, thus we focus on the share of low-income peers.[10] We define low-income households at wave I of the survey when students were in grades 7 – 12 and the majority (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

[9]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.

[10]As noted earlier, interacting the peer mean with the peer standard deviation of income is another possibility but may considerably strain our data, because we need to disaggregate effects across students' own position in the income distribution.

who are not below the poverty threshold but who are in the bottom third of the income distribution for each family size.[11] Next, our treatment of interest is then calculated as the leave-one-out share of low-income peers at the school-cohort level. This measure on average returns about 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).

Different definitions of this share are feasible, and in later robustness checks, we will consider multiple iterations. However, note there is a trade-off. Restrict on smaller portions of the income distribution and we may focus more directly on the poorest but have too little variation within schools to be efficient. Open up the definition to wider portions of the income distribution and we then likely miss-classify many students. Our definition balances this, focuses on a reasonably low portion of the distribution partitioned by family sizes, and it obtains a fair degree of within school variation.

Educational outcomes. As our primary outcome, 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).[12] We focus on the long-run educational outcome for our baseline results and through our robustness checks. We then turn to 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.

Sample selection and summary statistics. Columns (1) - (4) of Appendix Table B.1 present summary statistics of our sample in wave I. 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).[13] For this sample, these steps leave us with complete information on the share of low income peers – our treatment. 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,

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

[12]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.

[13]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 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 they are on average 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 college-educated households, and students have on average 34% of peers from low-income families. Moreover, the mean college 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). Moreover, to give a sense of the full sample before dropping observations, we compare means in the Appendix Table B.1 for each variable before and after our sample selection criteria. Though most of the mean differences are statistically significant from zero, we observe relatively small absolute mean differences. We interpret our analytic sample as representative of the full population. 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, measures for risky behaviours, and measures of self-efficacy.

3.2 Empirical Strategy

We need to surmount two hurdles to identify effects from the share of low-income peers. One, unaccounted for selection into schools will likely bias our estimates. Two, our theory predicts the peer effect is heterogeneous to own-capacity, which we operationalize as own-income, 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.

Main specification. We begin with the following specification:

$$
\begin{aligned}
Y_{ics} = {} & SLP_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\} \alpha_k \\
& + ln(Inc)\_SD_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\} \beta_k \\
& + f(ln(Inc_{ics})) + \boldsymbol{X}_i^{'} \gamma_1 + \boldsymbol{X}_{-i}^{'} \gamma_2 + \boldsymbol{XSD}_{-i}^{'} \gamma_3 + \theta_{ics} + \epsilon_{ics},
\end{aligned}
\tag{6}
$$

where Y_{ics} denotes the college graduation of student i in cohort c and school s; SLP_{-ics} denotes the leave-one-out percentage share of peers from low-income households in cohort c and school s. Following the predictions from our theory, we disaggregate the effect (α_k) from the share of low-income peers over deciles of own-household income. If our estimates are causal and driven by our proposed aspiration gaps mechanism, then we expect the following: increases in SLP_{-ics} will (i) have a positive effect for students on the lower end of the income distribution (Prediction 1), (ii) have negative effects for students on the upper end (Prediction 2), and (iii) have a null effect for students in the middle section (Prediction 3).[14] While we begin by disaggregating our effects over deciles, we will later relax this and make our specification more parsimonious.

We further include as a control the leave-one-out standard deviation of peers logged household income, which we also disaggregate across income deciles, as Tincani (2018) shows that higher order moments of peer distributions can exert separate effects. This variable is indeed correlated with our share measure; however, it is useful, because including it may capture a ranking mechanism if part of the effect from exposure to the peer income distribution stems from rank concerns in ability and income is correlated with ability. In expanded specifications, we will additionally include ability rank disaggregated over the income distribution and later assess a wide range of checks against non-linear peer ability effects. Moreover, this dispersion measure may also capture behavioral mechanisms separately from our share effects, if those mechanisms correlate with the peer standard deviation of the income distribution.

Next, and importantly, we flexibly control for non-linear effects from own-income by including a cubic polynomial in logged household income. We then control for a set of exogenous demographics and characteristics in $\boldsymbol{X}_i$.[15] In our preferred specification, we further 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.[16] 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. Finally, to focus on within school, across cohort variation we have school and cohort fixed effects given by $\theta_{ics} = \mu_c + \delta_s$. The error term is ϵ_{ics}.

[14]Recall for those in the middle of the income distribution, who are proxying those around the middle of the capacity distribution, they are likely to be spread just around the aspiration reference point and thus this shift is positive for some and negative for others leading on net null effect.

[15]These are gender, age and age squared, indicators for race (Asian, Black, Hispanic, White, Other), and 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.

[16]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.

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.

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 is that we have exogeneity conditional on a rich set of controls and fixed effects, implying that[17]

$$E\left[\epsilon_{ics}|SLP_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\}, \boldsymbol{X_i'}, \boldsymbol{X}_{-i}^{'}, \boldsymbol{XSD}_{-i}^{'}, \theta_{ics}\right] = 0. \tag{7}$$

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 $SLP_{-ics} \times \sum_{k=1}^{3} \mathbb{1}\left\{IncGroup = k\right\}$. In either case, our assumption really rests on two critical components. One, that we adequately capture the relationship between our outcome and own-income, and two, that we cut any link between determinants of selection into schools and our treatment.

For the first, we use a flexible specification in own-income with a cubic polynomial. However, we can easily test for sensitivity here, and in later checks, we expand this up to a sixth degree polynomial or replace the polynomials with ventile fixed effects. Additionally, our demographic and characteristic controls include multiple measures that likely capture further layers to socio-economic standing.

For the second, selection factors likely correlate with SLP_{-ics}. We show evidence of this in the Appendix Figure B.2. This is a scatter plot of SLP_{-ics} against school mean income sorted 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. We can, however, be more restrictive and relax our assumption

[17] We could also include $ln(Inc)\_SD_{-ics} \times \sum_{k=1}^{10} \mathbb{1}\left\{IncDecile = k\right\}$ in this expectation. We do not to keep things concise. Moreover, in general, we include these controls to capture potential mechanisms other than aspiration gaps that we discussed above, rather than as crucial to identification.

in case parents select partly based on school trends. To do so, in some specifications we include school trends via $\delta_s \times c$ or in other specifications school specific income trends.

Further specifications. We also want to ensure that our treatment effect estimates are not capturing a relationship between ability and college graduation nor capturing ability rank effects, particularly as there is now a significant literature demonstrating the consequences of ranks (Delaney and Devereux, 2021; Denning, Murphy, and Weinhardt, 2021; Elsner and Isphording, 2017, 2018; Elsner, Isphording, and Zölitz, 2021; Kiessling and Norris, 2022; Murphy and Weinhardt, 2020; Pagani, Comi, and Origo, 2021). In fact, when estimating the effect of peer mean ability, omitting ability rank leads to a downward bias (Bertoni and Nisticò, 2019), while the inclusion of rank itself can be misspecified if not accounting for heterogeneous responses to the ability distribution by one's own ability (Denning, Murphy, and Weinhardt, 2021). This raises a number of points that we can and do explore with sensitivity testing.

First, we already flexibly control for income and this should capture non-linearities in ability given a positive correlation between the two.[18] To further control for ability effects, we can supplement our main specification with flexible polynomials in each respondent's score on Add Health's revised version of the Picture Vocabulary Test (PVT) score. Second, we can use the PVT score around peers. In the Appendix, Figure B.3b we show that indeed ability school-cohort rank based on PVT scores has a strong, positive relationship with logged household income; although, once we control for ability (Figure B.3c) this relationship is cut. Thus, our flexible controls for own-income should generally control out ability and rank effects but we can add both polynomials of PVT scores and PVT rank – which we will also disaggregate over income groups – as additional controls in a check on our main specification. In further robustness checks, we will allow for a range of non-linear peer ability effects and eventually include both income rank and ability rank effects. Our baseline specification and results on the share of low income peers are never sensitive to these tests.

Our discussion above revolves around functional form restrictions and demonstrating that controls should capture these competing rank or peer non-linear ability effects. Yet, we reiterate here our comments from the introduction on what we intend to capture in our effects of interest. We see family income as a particularly salient feature to adolescents that can capture a wide range of underlying factors related to capacity. We do not aim to uncover each of these features that may be relevant in our effects, e.g., such as from the peer distribution of cognitive load or opportunities. Rather, we see income as a salient measure that allows us to broadly capture the non-linear effects of peer inequality which are motivated by our theory.

[18]As we already show in Figure 3, and repeat in the Appendix Figure B.3a with a scatter plot and line of best fit, there is a clear linear relationship between PVT scores and logged household income.

We will later implement a series of robustness checks to further check the sensitivity of results. Now, we turn to balance testing to assess the reasonableness of our identification assumptions and then turn to the baseline results.

Balancing test. We present our balance tests in Table 1. Each cell in columns (1) - (5) 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. We also add school-specific cohort trends in even numbered columns to relax our identification assumption around linear trends. In columns (3) - (5), 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 (6) - (8), we repeat this but using 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.

Table 1. Balancing test

	Share of Low Income Peers		Share × B20	Share × M	Share × T20	Log(Inc)SD × B20	Log(Inc)SD × M	Log(Inc)SD × T20
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.001	0.001	0.001	-0.000	0.005*	-0.000	0.001	0.001
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.009)
White	-0.000	0.000	-0.003	0.000	-0.003	0.011	0.001	-0.012
	(0.002)	(0.001)	(0.005)	(0.003)	(0.005)	(0.009)	(0.005)	(0.010)
College-educated Parents	-0.002	-0.001	-0.006	-0.002	0.002	-0.002	-0.001	-0.005
	(0.001)	(0.001)	(0.006)	(0.002)	(0.004)	(0.009)	(0.003)	(0.006)
Raised by a Single Parent	0.000	0.000	0.004	0.000	-0.005	0.000	-0.002	-0.009
	(0.002)	(0.001)	(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)	(0.000)	(0.000)
First-born child	0.003**	0.002**	0.002	0.003*	0.006**	0.001	0.007**	0.005
	(0.001)	(0.001)	(0.003)	(0.002)	(0.003)	(0.005)	(0.003)	(0.005)
Child of an Immigrant	-0.001	-0.000	0.004	-0.002	-0.001	-0.011	-0.003	0.005
	(0.002)	(0.001)	(0.004)	(0.003)	(0.004)	(0.007)	(0.005)	(0.011)
Household receives food stamps	-0.001	-0.002	0.003	0.003	-0.013	0.006	-0.000	0.013
	(0.002)	(0.002)	(0.003)	(0.005)	(0.024)	(0.006)	(0.006)	(0.038)
Household size	0.001	-0.000	0.001	0.001	0.000	-0.001	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)
Function of Log Household Income	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School and Grade FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School-specific Corhot Trends	No	Yes	No	No	No	No	No	No
Observations	11165	11165	2180	6920	2065	2180	6920	2065

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are in parentheses and clustered at the school level. Columns (1)-(2) use the analytic sample and columns (3)-(5) and columns (6)-(9) split the analytic sample by the bottom 20th percentiles household income, the 30th-70th percentiles, and top 20th percentiles households income.

4 Results

Our theoretical framework generates a set of predictions on the effects from the share of low income peers that we outlined in Section 2.5. Empirically, we then test 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.[19] Our hypothesis based on Prediction 1 is that students at lower income deciles are more likely to fall into the area where aspirations are frustrated ($y(\tilde{e}^{-}, \theta) < a$), thus an increase in the share of low income peers will increase their effort and on the margins increase ($\alpha_k > 0$) the probability they continue education and graduate university. Based on prediction 2 our hypothesis is that students at higher income deciles are more likely to fall into the area where aspirations are exceeded ($y(\tilde{e}^{+}, \theta) > a$), thus an increase in the share of low income peers will decrease their effort and on the margins decrease ($\alpha_k < 0$) the probability they continue education and graduate university. Finally, our hypothesis based on Prediction 3 is that students around the middle of the income distribution may be more likely to fall just around the aspiration reference point. Thus, for these students, we expect some experience increasing and others decreasing aspiration gaps from shifts in the share of low income peers that would on average balance towards a null effect.

4.1 Long-run effects on educational attainment

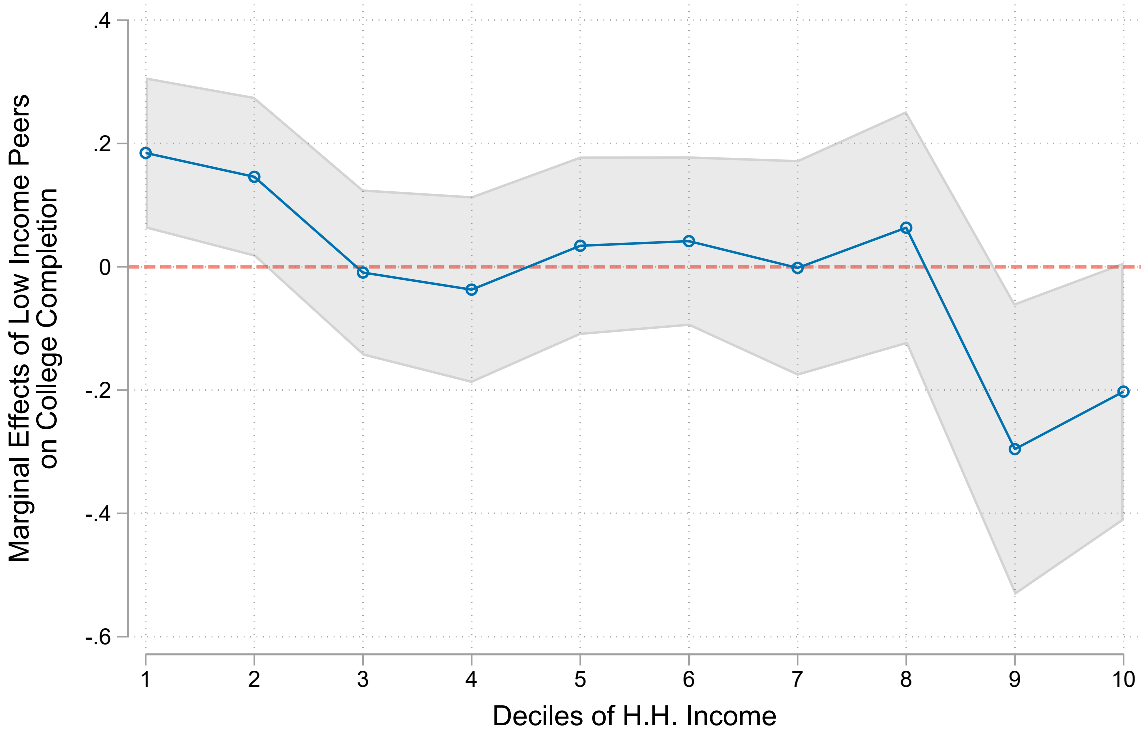
Baseline results. We begin by studying the marginal effects from a student's share of low-income peers at wave I on their probability of completing college by wave IV. The marginal effects (α_k) are calculated at each decile from the distribution of own-household income at wave I using our preferred specification, as discussed in Section 3.2. Figure 4 reports the results. Standard errors are clustered at the school level here and in all results to follow. Consistent with our predictions 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).

Our theory does not, and is not meant to, predict precisely where in the income distribution we would expect a transition to a null effect from the low and high income ends. Our theory rather tells us to look at low, middle, and high income separately in terms of assessing the treatment effect. Thus, based on our results disaggregating by deciles we move to a more parsimonious specification.

We next 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

[19] We focus on university completion as this is a more comprehensive measure of educational attainment encompassing both enrollment and retention.

Figure 4. The share of low-income peers and effects on college completion over deciles of own-household income



Notes: This figure presents the marginal effects on the probability of completing college 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 from the distribution of own-household income at wave I.

income peers. In Table 2, we present these results across multiple specifications moving from less restrictive in column (1) to the most restrictive in columns (5) - (6).

Our results match our predictions and are persistent and rather stable across specifications. Interpreting our preferred specification (column 2), we find that a 100% shift in the share of low income peers yields a 18 percentage points (pp) increase in the likelihood of holding at least a four year degree by wave IV for those in the bottom 20th of household income distribution in high school. 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 unlikely to be realistic. Interpreting these in standard deviation shifts (20%) translates the effect for the bottom 20th into a 3.6pp increase and for the top 20th a 4.1pp decrease.

Turning to inference, our key results are statistically significant and 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, Romano, and Wolf, 2020). Our inference, however, is robust to a Romano Wolf p-value adjustment – reported in the Appendix, Table D.1.[20]

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

	University Graduate					
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × 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} × 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} × 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 × 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 percentiles household income, in the 30th-70th percentiles, and finally in the top 20th percentiles 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.

To give some context to these effects, we compare them to conditional college completion gaps over gender and socioeconomic differences, which we report in the Appendix, Table B.4. The standardized effect estimate for the bottom 20th group of 3.6pp amounts to about half of the gap between females and males, around 40% of the gap between college and non-college parents, and is similar in size to the gap between single and two-parent homes. Comparisons are similar looking at the top 20th group.

These results are sizeable and of similar magnitude to other interventions targeting low-income families and their children. As a comparison, the magnitude of our effects are comparable to the effects of Pell Grants, the largest means-tested financial assistance for post-secondary students in the US (Dynarski, 2003). Effects estimates for these suggest

[20] We use the Romano Wolf routine based on a block cluster bootstrap around schools.

that an offer of $1,000 in grant aid increases the probability of attending college by high-school students 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. For own-ability, 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). As we discussed in Section 3, we want to check that our effects are not driven by a rank mechanism. Thus, we next add the PVT school-cohort rank, which is also disaggregated by students' position in the bottom 20th, middle, or top 20th income group (column 4).[21] Our key results on the bottom and top 20th groups remain consistent and significant.

Up until now we have assumed that selection factors are captured by the school fixed effects. Now, we relax this assumption with school-specific trends in columns (5) - (6). First, in column (5), we include the expansive specification from column (4) and allow for a linear trend within schools. This specification relaxes the identification assumption but is the most restrictive on the data. Second, in column (6), we use our preferred specification to be more parsimonious, but allow for there to be 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 the bottom 20th household income group at high school improves on wave IV income in response to an increase in the share of low income peers. For the top 20th group, we see null effects on wave IV income. Our theory does not necessarily make predictions on income, but while we do not see effects on students in the top 20th of parental income, the income results are suggestive that indeed the lower parental income students benefit both in educational attainment and future income.[22]

Non-linearity in peer ability. As mentioned before, we use income as a proxy to capture the non-linear effects of peer inequality. Yet, we have not fully ruled out the possibility that our results might be driven by non-linear effects from the peer ability composition (Booij, Leuven, and Oosterbeek, 2017; Feld and Zölitz, 2017).[23] In Table 3, we explore this

[21]In a additional robustness checks to come, we will go even further and allow for a wide range of non-linear peer ability effects and also consider income rank effects.

[22]While the top 20th group does not see a corresponding drop in income, as may be expected based on the college completion results, it may be that those from higher parental income backgrounds are better positioned to maintain higher income regardless of their college 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.

[23]This literature suggests that nonlinear peer ability effects may be driven by changes in teaching practice that are more or less conducive to different ability groups, or by other factors more directly related to peer interactions. Thus, as we include controls for nonlinear peer ability effects in Table 3, we may further capture some of these competing mechanisms.

more closely. We begin by adding to our preferred specification peer mean ability – based on PVT scores – and the standard deviation of peer ability interacted with own-income positions in column (2). Next, in column (3), to introduce peer ability heterogeneity around own-ability, we add interaction terms of peer mean ability, peer SD ability, and own-ability. Going further, in columns (4) - (5) we consider interactions of quartiles of the 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, Murphy, and Weinhardt (2021) aimed at more fully capturing potential non-linear effects from reactions to the distribution of ability in the school. Across all of these specifications our estimates on the share of low income peers remain remarkably consistent with our baseline estimates.

Table 3. Accounting for non-linearity in peer ability

	University Graduate						
	Non-linear peer ability effect					Rank effect	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SLP_{-ics} × Bottom 20	0.18**	0.18***	0.17**	0.17**	0.17**	0.22***	0.23***
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
SLP_{-ics} × Middle	0.02	0.00	0.01	0.02	0.02	-0.00	-0.01
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)
SLP_{-ics} × Top 20	-0.25**	-0.28**	-0.27**	-0.26**	-0.26**	-0.27**	-0.29**
	(0.11)	(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
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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 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, which is presented in column (1) of this table. 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, as we study peer effects across the income distribution, it is important to consider rank effects as a potential competing mechanism or source of bias if it correlates with our particular peer composition. While we have already flexibly allowed for ability rank effects in Table 2, we re-consider ranking concerns in columns (6) - (7) of the Appendix Table 3. We do this by allowing for both ability and income rank effects disaggregated across the income distribution. As is shown in columns (6) - (7), our results are not sensitive to ability nor income rank effects. Thus, we conclude that our main results on the share of low income peers are distinct from and insensitive to both non-linear peer ability and rank effects.

Altogether, the results in Figure 4, Table 2, and Table 3 are consistent with expectations based on our theory. Further, our results suggest the presence of strong non-linear effects stemming from peer income inequality that have not previously been assessed in the literature. Next, we turn to a series of robustness checks and then to explore mechanisms more closely.

4.2 Robustness checks

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

Definitions for 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 percentiles of the income distribution for a given family size, (ii) below 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 to introduce measurement error by misclassifying a larger share of students as low income peers when they are not, implying it should return smaller effects. Moreover, definitions that shrink the size of the low income peer groupings have another tradeoff in that 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 college 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 a 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 variable, 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 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 college 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 analysis is unbiased.

Attrition. In wave IV, after near 14 years of the treatment in wave I, about 78 % of the baseline sample remains.[24] Appendix Table C.5 shows that attrition patterns do not differ by treatment status across own-household income groups regardless of the

[24]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.

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.[25] 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.

4.3 Measures of effort: high school performance

In our theory, the aspiration gap mechanism affects the choice of effort. Empirically, if this mechanism is at work and income indeed proxies capacity, then we should see that an increase in the share of low income peers increases effort for those with lower income and decreases it for those with higher income. To proxy effort, we look at short-run observable measures. Fortunately, Add Health collected high school transcripts for all participants who were in the wave III survey, who agreed, and for whom the transcripts were capable of being accessed.[26]

We leverage this data in two ways. One, 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.[27] Second, 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. Therefore, we construct separate indicators for whether someone took 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.[28] Finally, given that there is both 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.

In Table 4, we report results on GPA and taking advanced courses. In column (1), we begin by using self-reported GPA from the wave I survey. Here we find null effects across income groups, but the pattern is as expected with a positive point estimate for the bottom 20th and a negative estimate for the top 20th group. In column (2), we turn to the transcript cumulative GPA. As the share of low income peers increases, we find a strong, positive increase in GPA for the bottom 20th group. They improve in their actual GPA consistent with our prediction for a reduction in the aspiration gap. Recall

[25]We then replicate our results with IPW weights using the specifications in column (2) and column (6) of Table 2.

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

[27]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.

[28]For math, this is defined by taking pre-calculus or calculus. For science, it is whether one took advance science or physics. For English, it is whether one took an honors English class.

that we expect the bottom 20th income group to encompass those likely already behind the aspiration reference point in our theory. Thereby, a mechanism for the increase in the share of low income peers is that it captures a reduction in this reference point and thus a reduction in their aspiration gap improving the marginal benefit of effort.

Next, on the transcript GPA we do see some positive effect for the middle income group, but it is a smaller effect. Intuitively, we expect this group to fall more closely around the reference point, thus it is possible that reductions in aspiration gaps could slightly outweigh effects from those with an increase in the gap. Nevertheless, the results are still consistent with a smaller impact on this middle group. Finally, we see no effect on the top 20th, but the effort choices for this group may run through different channels. Recall that this group has on average a higher raw ability level, which may allow them to maintain grades even if they engage in other behaviors detrimental to their future educational success.

Table 4. GPA and Advanced Courses

	GPA		Advanced Courses			
	Self	Transcript	Math	Science	English	More than one
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	0.05	0.81***	0.36***	0.25	0.13	0.47***
	(0.15)	(0.25)	(0.13)	(0.16)	(0.22)	(0.17)
SLP_{-ics} × Middle	-0.07	0.49**	0.08	0.01	0.05	0.14
	(0.13)	(0.21)	(0.12)	(0.14)	(0.23)	(0.14)
SLP_{-ics} × Top 20	-0.18	0.04	0.10	-0.30*	0.23	-0.00
	(0.17)	(0.29)	(0.15)	(0.18)	(0.25)	(0.17)
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	11074	7297	7309	7277	5183	7318
R^2	0.197	0.278	0.255	0.216	0.259	0.244

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 (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. Column (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 column (2) - (6). We trim our data to our analytic sample as in Table 2.

We now turn to the advanced courses in columns (3) - (6). Here we find that 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 for an increase in the share of low income peers and on science the point estimate is positive and rather large. Also, this group has a significant and positive increase in their likelihood to take more than one advanced subject. The middle group returns null results and the top 20th group has mainly null results with a marginally significant negative effect on taking advanced sciences. While we do not find much in the way of effects for the middle and top groups, the results for the bottom 20th income group are consistent with improvements in effort.

We also repeat the Romano Wolf p-value adjustment we conducted at the baseline to check that our inference in Table 4 is not driven by multiple hypothesis test bias. We report these in the Appendix Table D.2 and find that our main results on the bottom 20th group survive this adjustment.

Finally, for our analysis here we have restricted the sample to those present in our baseline analysis, meaning we drop those who are missing data for college completion. In the Appendix Table E.1, we also report these same 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. Overall, we see persistent evidence that particularly the bottom 20th income group responds positively to an increase in the share of low income peers.

4.4 Measures of effort: 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 (Conti, Heckman, and Urzua, 2010; Cutler and Lleras-Muney, 2010; Kenkel, Lillard, and Mathios, 2006), as well as evidence that the stringency of education dampens risky behaviour (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 behaviour in terms of reduced return to human capital. In Table 5, we assess our effects of interest on a range of self-reported indicators for risky behavior.

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, these patterns match expectations given our preceding results.

Table 5. 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.

What is the qualitative explanation? As far as risky behavior inversely proxies effort, then our predictions remain the same as those described for high school performance. Thus, as a shift in peer composition externally manipulates the educational reference point, we would expect more effort for low income adolescents as the share of low income peers rises via a decrease in the aspiration gap. This would be consistent with a reduction in risky behavior. Conversely, this external shift in the reference point for high income adolescents widens their positive aspiration gap potentially leading them to place time into alternative pursuits. This would be consistent with an increase in risky behavior.

4.5 Effects on Mental Health and Beliefs

To this point, our discussion has been rooted in the consequences that inequality can have on externally manipulating educational reference points around aspirations. Our aspiration gap mechanism focuses on this external, exogenous shift in aspiration points via exposure to inequality in peer compositions, which in turn can affect effort and long-term educational outcomes. Importantly, this does not mean aspiration and belief updating stops here. In fact, one can think of a more general, dynamic setting in which effort and realised outcomes—even in the short term—are also part of the aspirations formation process (see Dalton, Ghosal, and Mani, 2016). There we can expect a positive feedback loop between effort and aspirations. For instance, an exogenous reduction in the aspiration gap due to low-income peers which results in higher effort, and therefore in the achievement of better educational outcomes, may in turn positively affect students' own capacity – or their beliefs about their capacity – ultimately increasing their aspirations.[29] In what follows, we attempt to uncover this positive feedback loop by exploring how our aspirations gap mechanism affects mental health, beliefs, and students' desire/expectations about going to college.

We measure mental health and self-perceptions based on contemporaneous, at wave I, measures of self-esteem, relative intelligence (self) rating, and depressive symptoms via the CES-D scale. In columns (1) - (3) of Table 6 we test how the share of low income peers affects these measures across students in the bottom 20th, middle, and top 20th income groups. We find that students in bottom 20th improve on mental health and self-perception, consistent with a reduction in aspiration gaps and frustration that our model predicts. We continue to find null effects for middle income students, and among the top 20th income students we find mostly null effects with some evidence for an increase in depressive symptoms as captured by CES-D scores. This increase in the CES-D score for high income students is consistent with our theory, as behavioural symptoms of depression include a loss of motivation and withdrawal of effort (De Quidt and Haushofer, 2019).

Table 6. Mental Health, Beliefs, and Expectations

	Mental Health and Beliefs			Desires and Expectations		
	Self-Esteem	Intelligent Feeling	CESD	Want College	Likely College	Expect College
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	1.75**	0.34*	0.94	0.40**	0.36	0.76**
	(0.85)	(0.20)	(1.44)	(0.18)	(0.22)	(0.36)
SLP_{-ics} × Middle	0.98	-0.01	0.74	0.28*	0.13	0.41
	(0.80)	(0.18)	(1.06)	(0.16)	(0.20)	(0.33)
SLP_{-ics} × Top 20	0.15	-0.00	3.11*	-0.08	-0.05	-0.12
	(1.04)	(0.26)	(1.78)	(0.20)	(0.23)	(0.39)
Mean Dep Var	28.56	3.9	11.02	4.46	4.19	8.65
Observations	11134	11151	11154	11142	11134	11134
R^2	0.088	0.111	0.092	0.107	0.168	0.155

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 show the effects of share of low income peers on self-efficacy in column (1) - (3) and on beliefs and expectations about college in column (4) - (6). We include self-esteem, perception of relative intelligence, and CES-D under mental health. Column (4) uses a 1-5 scale outcome on wanting to go to college, while column (5) uses a 1-5 scale outcome on the likelihood a student places on going to college. College Expectation in column (6) is the sum of the wanting and likelihood to go to college.

Next, in columns (4) - (6) we turn to self-reported stated desires to go to college and expectations to go to college. Here we find that students in the bottom 20th of the income distribution significantly improve in their desire and, while not significant, also have a positive point estimate for their expectation. For the top 20th group, the results

[29]Although we do not provide a formal treatment of this mechanism, our aspirations formation process in (5) is consistent with this interpretation if we consider a dynamic setting in which a student's own capacity (or belief about own capacity) is endogenous to effort, i.e. $\theta_{t+1} = f(e_t), f' > 0, \theta_0$ given. In such a setting, higher effort today, due to an exogenous reduction in aspirations gap, would result in both higher capacity *and* aspirations in the future.

are not significant. Nevertheless, all the point estimates here are negative, consistent with our expectations. Also, these measures are imperfect proxies of aspirations that likely contain significant measurement error leading our results to be underestimated. Overall our effects in this Section are consistent with the positive feedback loop between effort and aspirations outlined above, but we do not draw strong conclusions and look at these results as suggestive.

4.6 Alternative mechanisms

Disruptive peers. Thus far we have found consistent patterns with our theoretical framework on the aspiration gaps mechanism. However, we do not fully rule out other potential mechanisms. One potential competing mechanism would come from changes in disruptive behaviour. This may be particularly true if those from more disadvantaged backgrounds have a higher likelihood of disruptive behaviours (Carrell, Hoekstra, and Kuka, 2018; Zhao and Zhao, 2021), although this need not be the case and a higher share of low income peers is not guaranteed to increase misbehaving in the peer group. In Table 7, we outline the sign predictions we have discussed and those from additional potential mechanisms.

Table 7. Sign predictions of competing mechanisms

	Low Income	Middle Income	High Income
Aspiration Gaps	+	null	–
Disruptive Peers	–	–	–
Teacher–Student Relations	+	ambiguous	ambiguous

Notes: In this table, we outline empirical sign predictions for an increase in the share of low income peers based on competing mechanisms.

Disruptive behavior from peers causes harm to academic achievement both in the short and in the long run (Carrell and Hoekstra, 2010; Carrell, Hoekstra, and Kuka, 2018; Kristoffersen et al., 2015; Zhao and Zhao, 2021), and the evidence points toward it having negative effects on student outcomes at each point in the income distribution (Carrell and Hoekstra, 2010; Carrell, Hoekstra, and Kuka, 2018).[30] This suggests that for low income students our aspiration gaps mechanism generates opposite sign predictions, while for high income students both mechanisms point in the same direction. Middle income students who we conjectured would have null effects from the aspiration gaps

[30]Carrell and Hoekstra (2010), and Carrell, Hoekstra, and Kuka (2018) are the only two studies we are aware of evaluating the effects of disruptive peers on student outcomes across the income distribution. Carrell, Hoekstra, and Kuka (2018) is the only study examining long-term student outcomes, such as college 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.

mechanisms would still potentially face negative effects from misbehaving peers. As far as changes in the share of low income peers also picks up some shift in misbehaviour, then our takeaway here is that a positive point estimate for the low income students would be consistent with our proposed mechanism and not with changes in misbehaviour among peers, while for high income students a negative point estimate could be consistent with either or both. Although, we reiterate here that our flexible income controls and our disaggregation over income of the peer dispersion (SD) of logged household income may already have picked up a mechanism via peers' disruptive behaviour.

Next, we look for evidence more specifically around controlling for peer misbehaviour. We explore our treatment effects after accounting for proxies of same school cohort peers' disruptive behaviour in Table 8. Our first measure is the share of peers who score above the median on a delinquency scale.[31] Our second measure is the share of peers who reported getting into a fight at school, and our third measure is the share of peers facing home disruption.[32] In all cases, we disaggregate the peer effect by a student's own-position in the income distribution. We report our results for both University Graduation and also for the transcript GPA measure. On both outcomes, we find highly consistent estimates for the share of low-income peers across the income distribution regardless of the peer disruption measure we control for.[33] These measures of disruptive peers are imperfect and potentially endogenous, yet we find no evidence that our main treatment effects are sensitive to their inclusion, suggesting our baseline treatment effects are not driven by changes in disruptive behaviour.

Teachers. Another candidate is that teachers may change behaviour in response to a change in the share of low income peers. The previous literature on peer effects in education often posits that teacher expectations and effectiveness might vary by classroom composition to meet the needs of the students (Aucejo et al., 2022; Duflo, Dupas, and Kremer, 2011; Papageorge, Gershenson, and Kang, 2020). Alternatively, teachers' implicit stereotypes regarding different income groups may obstruct their interaction with students (Carlana, 2019; Carlana, La Ferrara, and Pinotti, 2022b). When the share of low-income peers increases within a given school-cohort, teachers may be required to devote more attention to low-income students and adapt their teaching practices and expectations accordingly. Such a response may benefit low-income students and offer an alternative mechanism to aspiration gaps. How such a mechanism may impact middle or high income students is somewhat more ambiguous as it will depend on whether the

[31]Add Health dedicates a sub-section of the survey to self-reported delinquency where respondents filled out their answers privately through an audio computer assisted program. The scale is the sum of the delinquency items. We list each of these with their summary statistics in the Appendix Table B.3.

[32]Home disruption is defined based on the parent survey, typically filled out by the mother, and is equal to one if the parent answered as having binged more than five alcoholic drinks in the last month, reports discussing separation with their partner, or reported frequently arguing with their partner.

[33]See Appendix Table E.2 for the results including the estimates for the disruptive peer measures.

Table 8. Accounting for disruptive peers

	University Graduate			Transcript GPA		
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	0.18**	0.18**	0.17**	0.97***	0.90***	0.85***
	(0.07)	(0.07)	(0.07)	(0.24)	(0.29)	(0.25)
SLP_{-ics} × Middle	0.02	0.02	0.02	0.53**	0.46**	0.49**
	(0.06)	(0.06)	(0.07)	(0.20)	(0.21)	(0.21)
SLP_{-ics} × Top 20	-0.24**	-0.23**	-0.26**	0.11	0.03	0.04
	(0.11)	(0.11)	(0.11)	(0.29)	(0.29)	(0.29)
Share of High Delinquency Peers × Income Position	Yes	No	No	Yes	No	No
Self Delinquency Scale × Income Position	Yes	No	No	Yes	No	No
Share of Peers Fighting at School × Income Position	No	Yes	No	No	Yes	No
Self Fighting at School Scale × Income Position	No	Yes	No	No	Yes	No
Share of Peers with Home Disruption × Income Position	No	No	Yes	No	No	Yes
Self Home Disruption × Income Position	No	No	Yes	No	No	Yes
Transcript Non-reponse Weights	No	No	No	Yes	Yes	Yes
Mean Dep Var	0.33	0.33	0.33	2.41	2.41	2.41
Observations	11165	11123	11161	7297	7267	7293
R^2	0.251	0.247	0.244	0.297	0.283	0.281

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. See Appendix Table E.2 for the coefficients on the disruptive peers measures. Delinquency scale is the sum of 15 items presented in Table B.3. High delinquency is defined as above median children among the delinquency scale distribution. Fighting at school indicator is equal to one if the last physical fight the child had occured at school. Home disruption is defined based on the parent survey, typically filled out by the mother, and is equal to one if the parent answered as having binged more than five alcoholic drinks in the last month, reports discussing separation with their partner, or reported frequently arguing with their partner.

attention shift to low-income students comes at their expense or not. Table 7 outlines these sign predictions.

Empirically, if the effects from the share of low-income peers are partially driven by teachers, we would expect to observe a significant effect on teacher-student interactions. We focus on four items that relate to teacher-student 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. In Table E.3, we report the effect from a change in the share of low income peers on teachers outcomes using our preferred specification from the baseline. We find null effects on teacher-student interactions, which suggests that responses by teachers is not driving our effects.

In the U.S. educational context without fixed classrooms, students typically change classrooms during high school due to changing class enrollment. 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 behaviour and teaching practice at the same time. Given this assumption and our results in this section, we expect that teacher attributes and behaviours are unlikely to drive our effects from the share of low income peers on educational attainment.

5 Moderating Mechanisms and Discussion

5.1 Integration into social networks

The way aspirations are formed could depend on more than the students' exposure to a given degree of income equality. This conjecture goes beyond our theory. Nevertheless, it could be important for policy. For instance, if some factors mitigate the role inequality plays in forming beliefs about where one falls relative to an aspiration point, then these channels will help us understand how to circumvent the role of inequality when integrating students across the household income distribution.

One candidate here is social integration. For example, Alan et al. (2021a) find that in ethnically mixed schools in Turkey – who experienced influxes of refugee children – an intervention to improve students' understanding of each other improved social cohesion. This included a reduction in social exclusion and an improvement in Turkish language skill among refugee children relative to those in control schools. Moreover, Alan et al. (2021b) find that students who are better connected in their school-class network perceive their social environment in a more positive light. Thus, for aspiration gaps, our conjecture is that students' educational aspirations may be less affected by the income distribution when they are more socially integrated into the school.

We now provide some evidence on this conjecture. We first use the school friendship network data available in Add Health to assess how our effects change by network centrality. Second, we compare our baseline effects against redefining the peer reference group on sharing similar characteristics where friendship ties are more likely.

Heterogeneity by network centrality. We use students' Bonacich centrality to test whether social connectedness moderates the effects of income inequality. Bonacich centrality is an index score that takes into account students' direct and indirect friendship links in the school (Bonacich, 1987). This measure may be endogenous, and we caution interpretation. Yet, it can provide us with suggestive evidence on whether social integration has a role to play in shifting students' perception of where they fall relative to an aspiration reference point as income inequality changes in their peer group.

First, we take a simple approach, splitting our sample by whether a student has an above or below median centrality score. In Figure 5a, we report the effect from a change in the share of low income peers on college completion using our preferred specification from the baseline but split on the high/low centrality sub-samples. For the bottom 20th and top 20th income groups, the effects appear to be driven by those with low centrality.

The coefficients are not significantly different across the high/low splits, but the pattern of results is suggestive that low centrality students respond the strongest to changes in the share of low income peers.

Second, the centrality measure is a continuous index, thus rather than splitting the sample here we interact centrality with the share of low income peers across the bottom 20th, middle, and top 20th income groups.[34] Based on a regression with this continuous interaction we then calculate the marginal effects from the share of low income peers on college completion at the 25th, 50th, and 75th percentiles of centrality and report these in Figure 5b.[35] The estimates here are more efficient and we see that the strongest effects appear over those with low centrality. The effects then taper off to null effects at higher degrees of centrality.

Again, centrality may be an outcome, so we should interpret these results with caution. We do show in the Appendix, Table E.4 that centrality and other measures of friendships do not respond to our treatment at any of the income groups we study. Thus, it appears the role of social cohesion may be more a moderator of our treatment than an outcome. Nevertheless, we interpret this as suggestive. Yet, if better social ties improve social cohesion as the literature suggests, then our findings here imply that better social ties can divert attention from the peer income composition when forming aspirations. The consequences of inequality within a group for our aspiration gap mechanism then depends on the degree of social cohesion. Next, we take a different approach that is less likely to suffer from endogeneity concerns.

Peer reference groups based on more similar peers. We define peer reference groups based on all students in a given cohort in the same school. To measure the share of low income peers, we can more precisely define the peer reference group around students in the same school and cohort who share characteristics. Here, we explore how our results change when we allow for finer subgroups in the effect from the share of low income peers. More specifically, 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.[36]

Our theory does not make predictions on the relevant reference group, but we can define two competing hypotheses. One, a more similar group of peers will make the shifts in income inequality within the group more salient. In this case, more refined groupings should dominate the effects. Second, and alternatively, due to homophily in

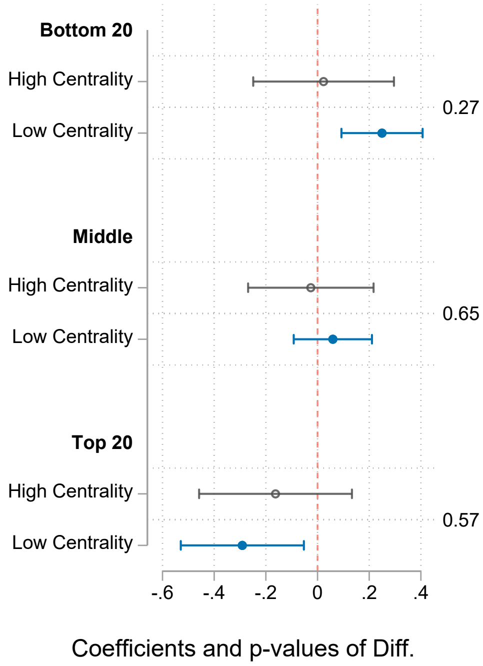
[34]While we do not interact centrality with all covariates in the model, thus more restrictive than subsample splits, we do additionally interact it with the peer standard deviation in the log of household income across the bottom 20th, middle, and top 20th income groups.

[35]The middle income group is included but omitted from the figure for clarity.

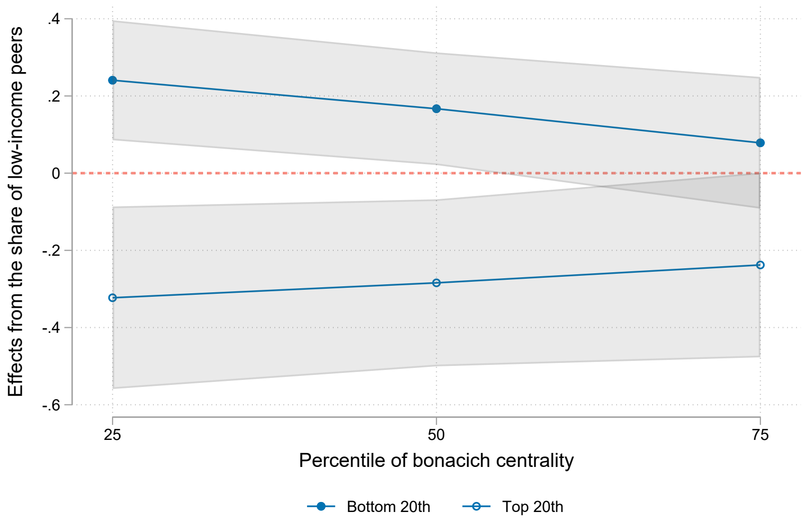
[36]This is a "horse race", as we include both our baseline peer reference group definition and a more refined grouping in the same regression.

Figure 5. College completion: heterogeneity by network centrality

(a) Sample split by centrality



(b) Marginal effects with a continuous interaction



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 calculate marginal effects for the share of low income peers at the 25th, 50th, and 75th percentile of centrality based on an interaction with centrality as a continuous variable. Here we include the middle income groups but omit them from the figure for brevity. We always include school and cohort fixed effects as in column (2) of Table 2. P-values of differences are presented at the side.

more refined peer groupings students have more friendships in these groups; thereby, the aspirations reference point – and students' perception of their relative position to it, i.e., beliefs – form around features other than the income distribution that then mitigates the aggregate effects we observe at the baseline. The second hypothesis here would be consistent with our suggestive evidence on network centrality.

In the Appendix Figure E.1, we report the results for the bottom and top 20th income groups on college completion as the outcome.[37] The baseline effect, using the same school and cohort peer definition, remains similar across specifications. However, the additional results based on more restricted peer group definitions have small and insignificant effects for both the bottom 20th and top 20th income groups. Thus, these results are consistent with the second hypothesis – friendships in more similar peer groupings divert attention from income inequality for aspirations – and also consistent with our evidence around network centrality.

5.2 Causal forest: heterogeneous effects

We now want to compare effects within each income group across dimensions of student characteristics. Doing so can be helpful to policy or future work in terms of better understanding for whom these effects are most salient, but our main result is already heterogenous by income groups and digging deeper may be difficult or risk spurious results. Therefore, we implement the recently developed method of a causal forest, which is a machine learning approach to recovering heterogeneous treatment effects (Athey and Imbens, 2016; Athey, Tibshirani, and Wager, 2019; Athey and Wager, 2019).

Causal forests aim to partition the data across observable covariates to maximize the variance in treatment effects, repeat this many times (growing a forest), and aggregate treatment effects for each observation across the forest. Thus, we recover conditional average treatment effects (CATEs) over partitions of exogenous controls that generates a wide range of heterogeneity in the predicted treatment effects for each individual. With a continuous variable, as we have, these are partial effects heterogeneous to unique partitions of the covariate space, but we will continue to refer to the CATE for simplicity.

We provide more details on causal forests and our implementation in the Appendix Section F. Here we note two additional points. First, we want to examine heterogeneity in the predicted treatment effects within income groups, thus we run our causal forest separately for each income group and then stack the results. Second, prior to the causal forest, we remove school and cohort fixed effects within each income group, residualizing the outcome (completed college by wave IV), the share of low income peers (treatment), and each of our student characteristics to remove confounding effects and focus on characteristics of the student.

We first demonstrate that the pattern in the CATEs across income groups matches closely to our previous results. Panel (a) of Figure 6 reports these results. For the bottom

[37]Again, the middle income group is always included but we omit their results here as those are null effects and to maintain brevity.

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.

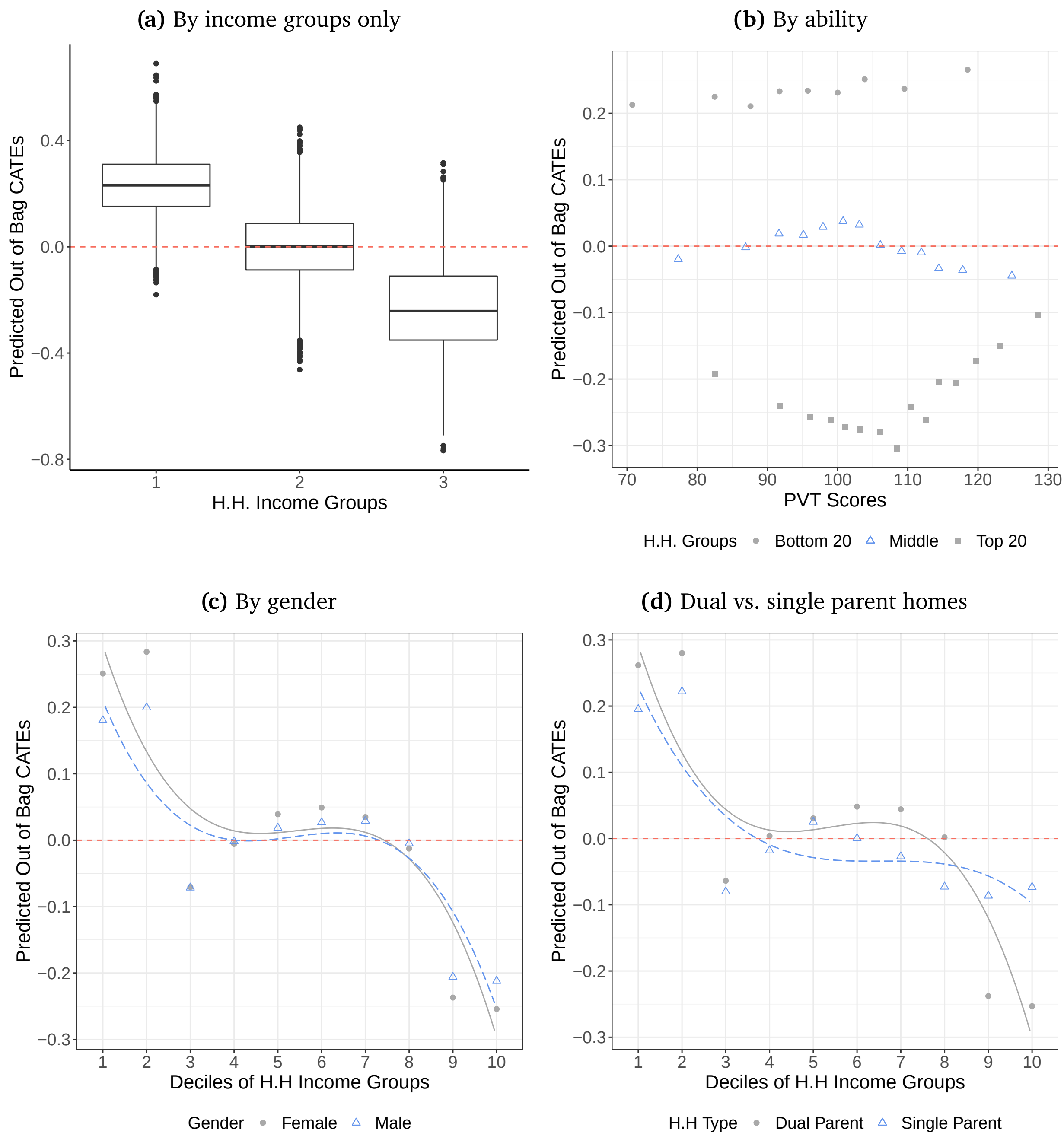
Next, in panel (b), we check whether our results vary over cognitive ability. We have already discussed the link between income and ability and we have controlled flexibly for ability and school-cohort ability rank. It could be, however, that only a portion of the ability distribution drives our results. For instance, Carlana, La Ferrara, and Pinotti (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), 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.

Finally, we turn to gender and dual vs. single parent homes. In the Appendix Table F.1, we split each income group by those with a high or low CATE (above or below the median) and then test mean differences in having a high or low CATE across our student characteristics. For many of these, the differences are minimal, but we do find interesting patterns around gender and dual vs. single parent homes, which we explore further in Figure 6.

In panel (c), we report binscatter plots across income deciles split by gender, and in panel (d), we report the same split by dual vs. single parent homes. 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. In the

Figure 6. Causal forest heterogeneity in CATEs by income groups



Notes:

Appendix Table F.1, we further show that these differences are significant even after adjusting for multiple hypothesis test bias.

Students from dual parent homes exhibit a similar pattern, with particularly stronger effects among the top 20th. 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.

6 Conclusion

We examine the role of inequality within peer compositions in schools on long-run educational attainment. We provide, to our knowledge, the first evidence about how the long-run effect of within-school inequality on college completion varies depending on the student's position in the household income distribution. Our empirical analysis is motivated by a theoretical model based on two key ideas: aspirations affect students' effort in different ways depending on how they are positioned relative to them; and external factors, such as the distribution of income in a student's social environment, are an important determinant of these aspirations in the spirit of Genicot and Ray (2017)'s model of aspiration formation.

Our model rests on the idea that students form a reference point for aspirations as a weighted function of their own capacity to turn effort into outcomes and the capacity of their peers. The model produces some clear predictions that we then test in our empirical analysis: as long as students form aspirations based on their peers capacity, then a downward shift in the peer capacity distribution will reduce individual aspirations. However, depending on the position of the student on the capacity distribution, low- and high-capacity students will experience opposite effects from this shift in terms of their aspiration gaps and effort. The former are likely to lie in the area where aspirations are frustrated, therefore a downward shift in the peer capacity distribution will increase their educational attainment by reducing their aspiration gap and increasing their effort. On the contrary, high capacity students likely lie in the area where aspirations are exceeded, therefore the same downward shift will increase their aspiration gaps and in turn reduce their educational attainment.

We test these theoretical predictions by using the share of low-income peers in a student's cohort within school to model the shift in school peer inequality, arguing that income represents a broader and more encompassing measure of capacity. We then examine how peer distributional shifts affect college completion to a different extent across the distribution of students own-household income. In order to identify these effects, we leverage within school, across cohort variation and additionally flexibly control for students' household income in addition to a rich vector of individual characteristics.

Our results strongly support the theoretical predictions of our model. We find that low-income students benefit from an upward shift in the share of low-income peers in terms of increased likelihood of college completion, middle-income students are unaffected, and high-income students experience a reduced likelihood of college completion. These findings are consistent with our theoretical model and 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 college by 3.6pp for the bottom income students and decreases it by 4.1pp for the top income students.

We find that our long-run effects are matched by short-run effects on effort responses measured by GPA score and whether a student took advance course work that are consistent with our theoretical predictions, in particular for low-income students. Similarly, we find suggestive evidence of effects on risky behaviours that are consistent with our expectations based on the theoretical predictions. We also explore other outcomes that we argue might be affected by our treatment such as proxy for self-efficacy, and find a pattern of results that is consistent with low-income peers likely reducing frustration and in turn increasing self-efficacy for low-income students. In addition, we examine effects on beliefs and expectations about college and find that low income students improve in these outcomes in response to our treatment. We argue that this may be consistent with a dynamic, positive feedback loop between effort, beliefs, and aspirations.

Additionally, we examine whether social cohesion can moderate the consequences of inequality in peer groups. We find evidence this is indeed the case, suggesting that when students are better integrated into social networks at school that these inequality effects become less important. This is important for policy as is suggests that aspiration gaps around inequality may be subverted without a need to change the composition of students. Finally, we employ the recent development in machine learning techniques known as causal forest to explore further layers of heterogeneity in our treatment effects. Our results show an interesting pattern of heterogeneity by gender and dual versus single parent homes.

Our study suggests that aspiration gaps from inequality can have important effects on human capital accumulation and in turn generate further inequality across the income distribution. However, these effects may not be destiny. We find that social integration through friendships moderates our effects of low-income peers on college completion. This points to a potential role for policies fostering social cohesion to mitigate the consequences of inequality in peer groups. In addition, we believe our study helps shed light on mechanisms alleviating the consequences of inequality: affecting the weight students place on the peer environment in forming aspirations or helping students get nearer to their aspirations.

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Appendix

A Mathematical Proofs

Proof of Proposition 1. To begin with, it is useful to summarise the properties of the functional forms adopted in the model of Section 2, that is, $b(0) = 0, b'(y) > 0, b''(y) < 0$, and $\lim_{y\to\infty} b'(y) = 0$; $c(0) = 0$, $c'(e) > 0$, $c''(e) > 0$, and $\lim_{e\to\infty} c'(e) = \infty$; and $\mu(0) = 0$, $\mu'(y-a) > 0$, $\mu''(y-a) < 0$ if $y > a$ (concavity) and $\mu''(y-a) > 0$ if $y < a$ (convexity), and $\lim_{y\to a} \mu'(y-a) = \infty$. All functions are continuous, and twice differentiable, the only exception being μ which is not differentiable at $y = a$. Next, we proceed by analysing 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 $y > a$. By definition, $\tilde{e}(\theta, a)^+$ is the level of effort at which the first-order condition given by (3) is satisfied. Since $u_{ee} = b''(y)\theta^2 + \mu''(y-a)\theta^2 < 0$, where $\mu''(y-a) < 0$ when $y > a$, we conclude that u is strictly concave in e. This, together with the fact that as e gets smaller, so that y approaches a from above then $\lim_{y\to a} u_e = \infty$, and as e gets larger then $\lim_{e\to\infty} u_e = -\infty$, enables us to conclude that $\tilde{e}(\theta, a)^+$ exists, it is strictly positive, and that it is the unique global maximum of u. Moreover, note that $u_{ea} = -\mu''(y-a)\theta > 0$. Hence, implicit differentiation allows us to deduce that $\tilde{e}_a^+ = -u_{ea}/u_{ee} > 0$, which implies that $\tilde{e}(\theta, a)^+$ is increasing in a.

Case of $y < a$. Here, by definition $\tilde{e}(\theta, a)^-$ would be the level of effort at which the first-order condition given by (4) is satisfied. However, $u_{ee} = b''(y)\theta^2 + \mu''(y-a)\theta^2$, the sign of which remains ambiguous, since $\mu''(y-a) > 0$ when $y < a$. Hence we cannot conclude whether u is concave or convex in e in the domain of losses. To proceed, we assume that the parameters of the model (namely α and β) are such that $b''(y) + \mu''(y-a) < 0$, which would indeed ensure that $u_{ee} < 0$. Note this corresponds to the assumption that μ is 'not too convex' in the domain of losses, or, that b is 'concave enough'. Nevertheless note that as e gets smaller then $\lim_{e\to 0} u_e = \infty$, and that as e gets larger so that y approaches a from below then $\lim_{e\to a} u_e = \infty$. These together imply that even if $u_{ee} < 0$ we cannot be sure that a solution exists, or if it exists, we cannot be sure that it is unique. To proceed, we assume that if more than one solution exists in this case, the student will choose the one that yields the highest output. While if no solution exists, the student will choose $\tilde{e}(\theta, a)^+$ (which always exists). Finally, consider the case in which two local solutions exist: one such that $y > a$ and one such that $y < a$. In this case, we assume the student will choose the one that yields the higher utility, in line with utility maximisation. To prove that $\tilde{e}(\theta, a)^-$ is decreasing in a it is then sufficient to show that $u_{ea} = -\mu''(y-a)\theta < 0$, which follows from the fact that $\mu''(y-a)$ when $y < a$. Hence, implicit differentiation yields $\tilde{e}_a^- = -u_{ea}/u_{ee} < 0$.

Next, we prove that a^* exists and that it is unique. Consider the interval $[0, \underline{a}]$, such that aspirations are satisfied when $a \in [0, \underline{a}]$ and both $\tilde{e}(\theta, a)^+$ and $\tilde{e}(\theta, a)^-$ exist. Application of the envelope theorem allows us to conclude that both $u(\tilde{e}(\theta, a)^+, \theta, a)$

and $u(\tilde{e}(\theta,a)^-,\theta,a)$ are decreasing in a. This, together with the fact that $\tilde{e}(\theta,a)^+$ is increasing in a, $\tilde{e}(\theta,a)^-$ is decreasing in a, implies that there exists an $a > 0$ at which $u(\tilde{e}(\theta,a)^+,\theta,a) = u(\tilde{e}(\theta,a)^-,\theta,a)$ (for which we assume the solution to be given by $\tilde{e}(\theta,a)^+$). Denote this aspiration level as a^* and further note that if $a > a^*$ then it must be that $u(\tilde{e}(\theta,a)^+,\theta,a) < u(\tilde{e}(\theta,a)^-,\theta,a)$, implying that the solution switches from being $\tilde{e}(\theta,a)^+$ to $\tilde{e}(\theta,a)^-$ and aspirations are frustrated. Next, since $\tilde{e}(\theta,a)^-$ is decreasing in a it implies that as we increase a further beyond a^* aspirations will remain frustrated. The same logic applies for all $a \in [0,a^*]$, since as we increase a, and $\tilde{e}(\theta,a)^+$ is increasing in a, aspirations remain satisfied in this range. This implies that a^* is unique.

To conclude, we prove that output is maximised when $a = a^*$. For a given capacity θ define the level of effort at which aspirations are attained, $y = a$, as $\bar{e}(\theta,a) \equiv \frac{a}{\theta}$ Hence, when a student exceeds aspirations, $y > a$ $(a < a^*)$, their optimal effort $\tilde{e}(\theta,a)^+$ is such that $\tilde{e}(\theta,a)^+ > \bar{e}(\theta,a)$ and when they fall short of aspirations, $y < a$ $(a > a^*)$, their optimal effort is such that $\tilde{e}(\theta,a)^- < \bar{e}(\theta,a)$. This implies that $\tilde{e}(\theta,a)^+ > \tilde{e}(\theta,a)^-$ in the neighbourhood of a^*, and since $\tilde{e}(\theta,a)^+$ is increasing in a it follows that $\tilde{e}(\theta,a)$, and therefore y, is maximised when $a = a^*$. ■

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

Consider $\bar{e}(\theta,a(\theta,F^\theta))$ for a given capacity θ, and distribution F^θ, and denote by $F^{\theta,\lambda}$ the distribution of θ where all capacities are shifted by a factor $\lambda > 1$, such that $F^{\theta,\lambda}(\lambda\theta) = F^\theta(\theta)$. It follows that the mean of $F^{\theta,\lambda}$ is given by $\lambda\mathbb{E}\theta$. This implies that $a(\lambda\theta,F^{\theta,\lambda}) = \gamma\lambda\theta + [1-\gamma]\lambda\mathbb{E}\theta = \lambda a(\theta,F^\theta)$. Moreover, for any θ, if $\hat{F}^\theta$ is a distribution that first-order stochastically dominates F^θ (strictly), that is $\hat{F}^\theta < F^\theta$, it follows that $\mathbb{E}\hat{\theta} \equiv \int \theta \, d\hat{F}^\theta(\theta) > \mathbb{E}\theta$, implying that $a(\theta,\hat{F}^\theta) > a(\theta,F^\theta)$. These considerations imply that, for $\theta_2 = \lambda\theta_1$, with $\lambda > 1$, then:

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

Hence $\bar{e}$ is decreasing in θ.

Next, from Proposition 1, we know that when a student exceeds their aspirations, $a < a^*$ their optimal effort is $\tilde{e}(\theta,a(\theta,F^\theta))^+ > \bar{e}(\theta,a(\theta,F^\theta))$; and when they fall short of aspirations, $a > a^*$ their optimal effort is $\tilde{e}(\theta,a(\theta,F^\theta))^- < \bar{e}(\theta,a(\theta,F^\theta))$. Hence, since $\bar{e}$ is increasing in a, there exists a unique $\bar{e}^* \equiv \frac{a^*}{\theta}$ (where a^* is defined in Proposition 1) such that for all $a < a^*$, $\bar{e}(\theta,a(\theta,F^\theta)) < \bar{e}^*$ (and aspirations are exceeded), and for all $a > a^*$, $\bar{e}(\theta,a(\theta,F^\theta)) > \bar{e}^*$ (and aspirations are frustrated). This, along with the fact that

$\bar{e}$ is decreasing in θ, can be used to deduce that if aspirations are frustrated for some $\theta_1 \leq \theta^*$, so that $\bar{e}(\theta_1, a(\theta_1, F^\theta)) > \bar{e}^*$, then they are also frustrated for all $\theta < \theta_1$; while if aspirations are exceeded for some $\theta_2 \geq \theta^*$, so that $\bar{e}(\theta_2, a(\theta_2, F^\theta)) < \bar{e}^*$, then they are also exceeded for all $\theta > \theta_2$. ■

Proof of Proposition 3. First note that if $\hat{F}^\theta < F^\theta$, then $a(\theta, \hat{F}^\theta) > a(\theta, F^\theta)$ for any given θ, hence, aspirations increase for all students. Next, consider low capacity students, for which $a > a^*$. If $a(\theta, \hat{F}^\theta) > a(\theta, F^\theta)$ then $\tilde{e}(\theta, a(\theta, \hat{F}^\theta))^- < \tilde{e}(\theta, a(\theta, F^\theta))^-$. While aspirations increase, effort decreases. Note, this is true for any $a(\theta, \hat{F}^\theta)$ since a is increasing in F^θ. Then consider high capacity students, for which $a < a^*$. If $a(\theta, \hat{F}^\theta) > a(\theta, F^\theta)$ then $\tilde{e}(\theta, a(\theta, \hat{F}^\theta))^+ > \tilde{e}(\theta, a(\theta, F^\theta))^+$. Both aspirations and effort increase (and we assumed that $\hat{F}^\theta$ is such that $a(\theta^* + h, \hat{F}^\theta) < a^*$, that is, aspirations will remain satisfied even to the least capable student of the high capacity students—and note that even a^* is a function of θ). Finally, consider middle capacity students. There is a fraction of these students endowed with $\theta \in [\theta^* - h, \theta^*)$ for which $a > a^*$, which implies they behave the same as low capacity students. However, there is also a fraction of these students endowed with $\theta \in [\theta^*, \theta^* + h]$ whom will increase their effort only as long as the increase in aspirations is such that $a(\theta, \hat{F}^\theta) < a^*$, while they will decrease their effort if the increase in aspirations is such that $a(\theta, \hat{F}^\theta) > a^*$. ■

B Additional Tables and Figures

Table B.1. Summary statistics

	Analytic Sample = 11,165				Full Sample		
	Mean	SD	Min	Max	Mean	Mean diff.	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Outcome and Treatment							
College Graduate in wave IV	0.33	0.47	0	1	0.32	0.01	0.01
Share of Low Income Peers (SLP_{-ics})	0.34	0.20	0	1	0.35	-0.01	0.00
B. Student Characteristics							
Logged Household Income	3.56	0.84	0	7	3.52	0.04	0.00
Female	0.52	0.50	0	1	0.51	0.01	0.00
Age	15.47	1.68	11	19	15.66	-0.19	0.00
Hispanic	0.15	0.35	0	1	0.17	-0.02	0.00
White	0.59	0.49	0	1	0.52	0.07	0.00
Black	0.20	0.40	0	1	0.22	-0.02	0.00
Asian	0.05	0.21	0	1	0.07	-0.02	0.00
Other Races	0.02	0.13	0	1	0.02	0.00	0.08
Family Size	3.79	1.21	2	12	3.77	0.02	0.19
Child of and Immigrant	0.17	0.38	0	1	0.22	-0.05	0.00
Less than HS Parents	0.10	0.30	0	1	0.13	-0.03	0.00
HS or GED Parents	0.29	0.46	0	1	0.30	-0.01	0.16
Some College Parents	0.22	0.42	0	1	0.21	0.01	0.02
College Parents	0.25	0.43	0	1	0.23	0.02	0.00
Postgraduate Parents	0.13	0.34	0	1	0.12	0.01	0.07
Single Parent Household	0.30	0.46	0	1	0.32	-0.02	0.00
Grade 7	0.14	0.35	0	1	0.13	0.01	0.00
Grade 8	0.14	0.35	0	1	0.13	0.01	0.00
Grade 9	0.19	0.39	0	1	0.17	0.02	0.00
Grade 10	0.20	0.40	0	1	0.19	0.01	0.21
Grade 11	0.18	0.38	0	1	0.18	0.00	0.46
Grade 12	0.15	0.35	0	1	0.16	-0.01	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. Column (5) presents the mean of full sample available from the dataset. Each variable has around 20,000 observations in the full sample. Column (6) shows the difference in means and column (7) presents the p-values from the mean-comparison tests.

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. Risky Behaviours				
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	3
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	6
C. Self Efficacy				
Self esteem (Kaufman)	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
How much wanting to go to college	4.46	1.01	1	5
How likely will go to college	4.19	1.12	1	5
Standardized college Expectations	0.02	0.99	-3	1
Observations	11165			

Notes: This table presents additional summary statistics on GPA and advanced courses taking in Table 4, risky behaviours in Table 5, and self efficacy in Table 6 after restricting to our analytic sample.

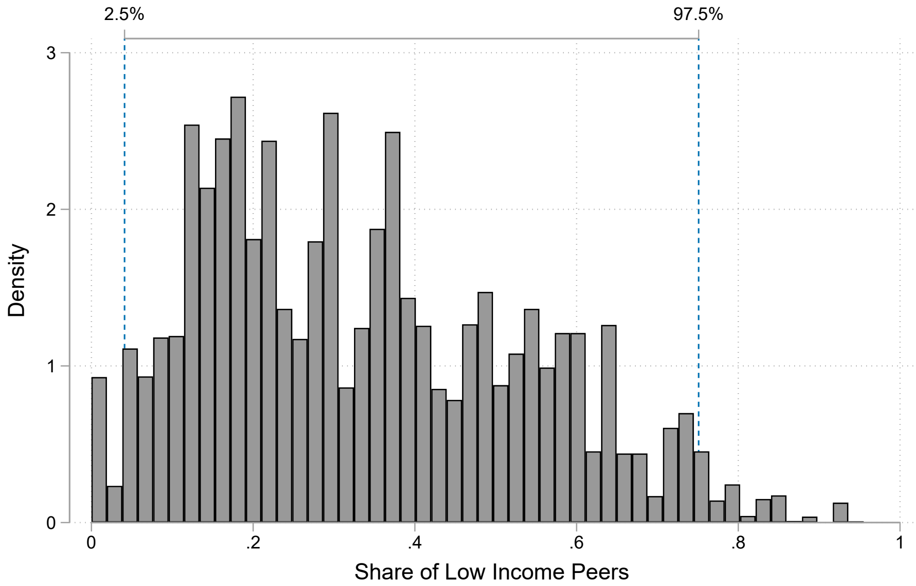
Table B.3. Delinquency scale summary statistics

	Mean	SD	Min	Max
Paint graffiti or signs	0.14	0.49	0	3
Deliberately damage property	0.25	0.60	0	3
Lie to your parents or guardians	0.94	1.05	0	3
Take things from a store without paying	0.40	0.80	0	3
Get into a serious physical fight	0.45	0.77	0	3
Hurt someone badly	0.24	0.59	0	3
Run away from home	0.10	0.38	0	3
Drive a car without permission	0.14	0.48	0	3
Steal something worth more than 50 dollars	0.08	0.38	0	3
Go into a house to steal	0.08	0.37	0	3
Threaten to use a weapon to get something	0.06	0.31	0	3
Sell marijuana or other drugs	0.14	0.54	0	3
Steal something worth less than 50 dollars	0.34	0.77	0	3
Take part in a group fight	0.26	0.60	0	3
Act loud, rowdy, or unruly in a public place	0.73	0.91	0	3
Observations	11125			

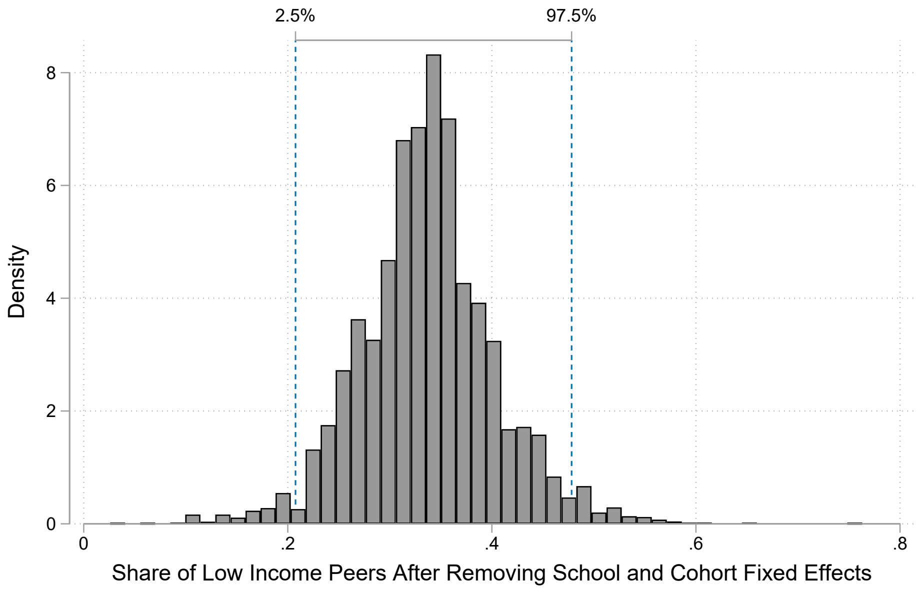
Notes: This table presents summary statistics for the components of delinquency scale after restricting to our analytic sample.

Figure B.1. Variation in Share of Low Income Peers

(a) Raw variation



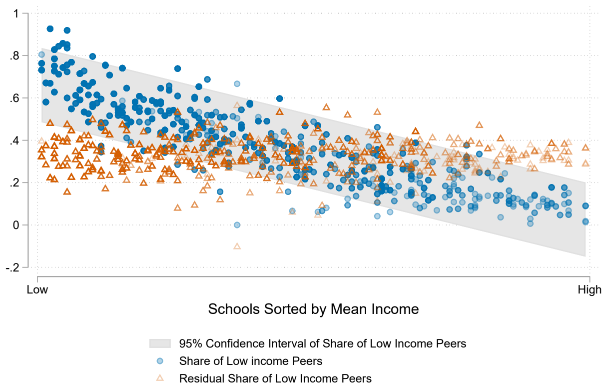
(b) Variation post removal of school and cohort fixed effects



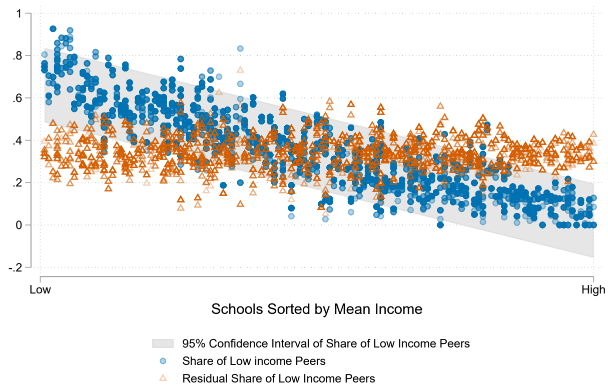
Notes: This figure presents a histogram of the share of low income peers in our analytic sample. Panel (a) reports the variations in the sample, and panel (b) reports this variation after removal of school and cohort fixed effects with the sample mean added back to place it on the same scale as panel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

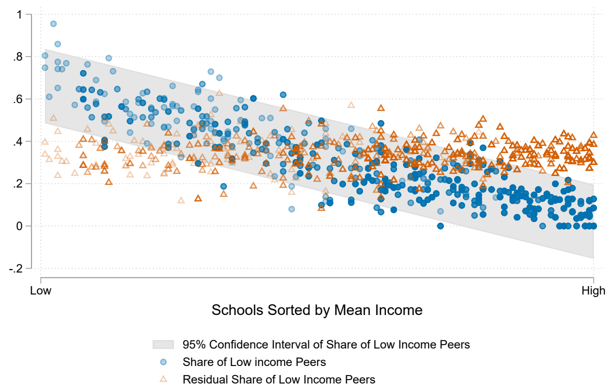
(a) Bottom 20



(b) Middle



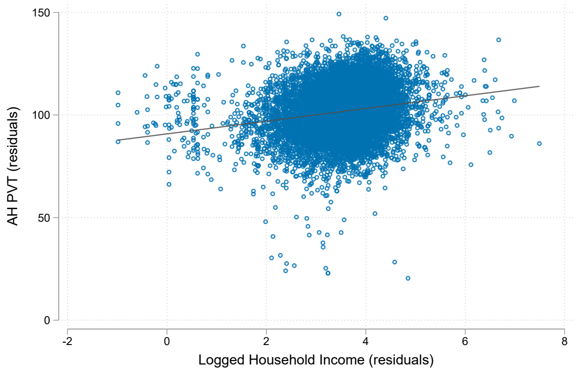
(c) Top 20



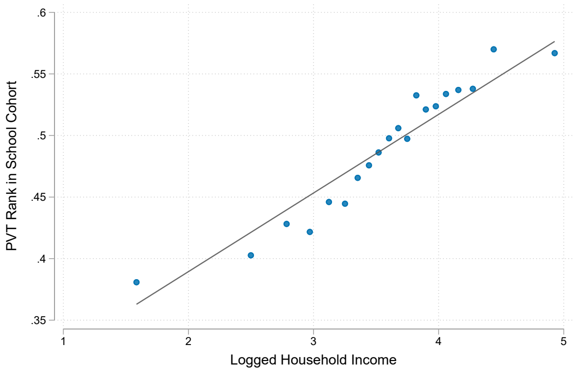
Notes: These figures present the share of low income peers and its residual after removal of school fixed effects with the sample mean added back to it for the bottom 20th, middle, and top 20th of the household income distribution by schools. Schools are sorted based on the mean logged household income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

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

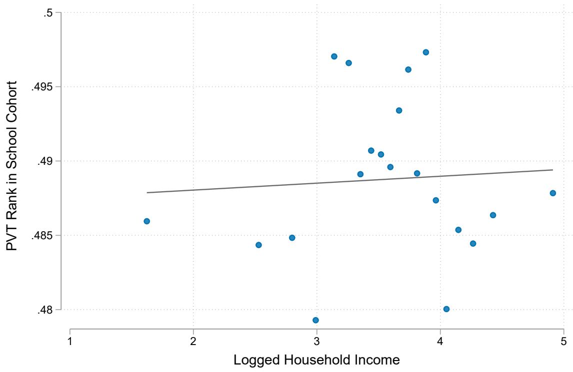
(a) PVT and ln(Income)



(b) PVT Rank and ln(Income)

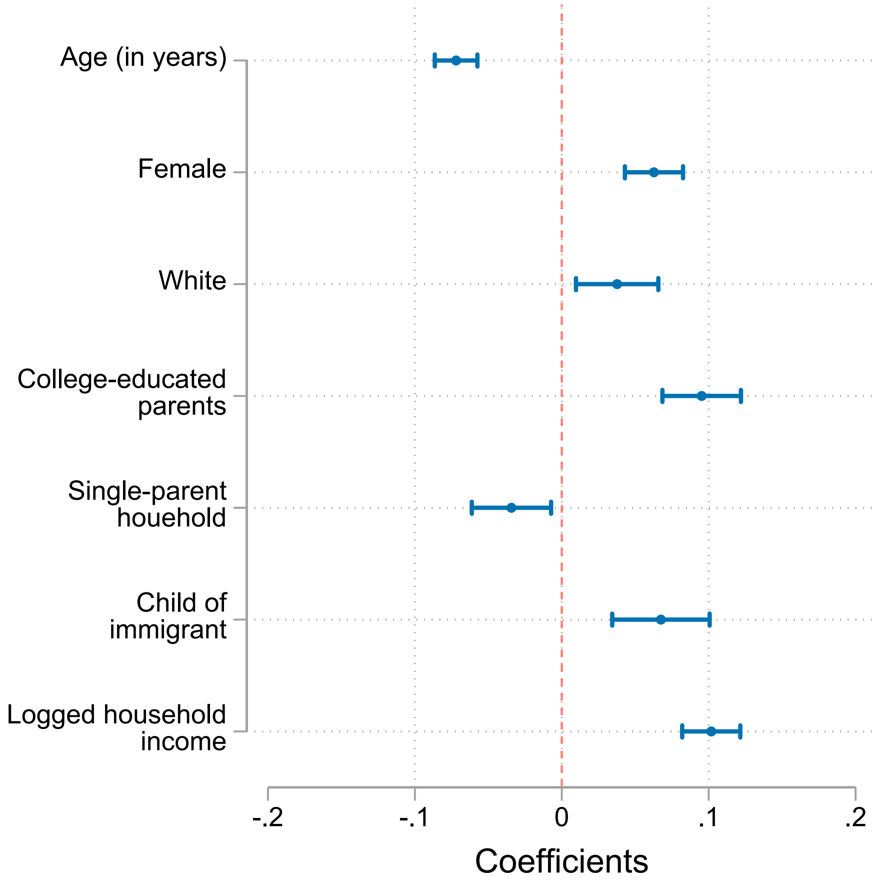


(c) PVT Rank and ln(Income): Control for PVT



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. Associations of Covariates with College 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 labour 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.1.

C Robustness Checks

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

	SLP_{-ics} × Bottom 20	SLP_{-ics} × Middle	SLP_{-ics} × 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} × Bottom 20			-0.08	0.05	0.19***	0.23**	0.26***
			(0.06)	(0.07)	(0.07)	(0.10)	(0.08)
SLP_{-ics} × Middle			-0.05	0.08	0.03	-0.01	0.04
			(0.05)	(0.07)	(0.06)	(0.07)	(0.07)
SLP_{-ics} × 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 of percentile household income, for the 30th-70th percentiles, and finally for the top 20th percentiles 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.

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, Romano, and Wolf, 2020) 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, Romano, and Wolf, 2020; Romano and Wolf, 2005) on different outcomes. This table provides p-values after controlling for the family-wise error rate.

E Mechanisms: Additional Results

Table E.1. 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.

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

	University Graduate			Transcript GPA		
	(1)	(2)	(3)	(4)	(5)	(6)
SLP_{-ics} × Bottom 20	0.18**	0.18**	0.17**	0.97***	0.90***	0.85***
	(0.07)	(0.07)	(0.07)	(0.24)	(0.29)	(0.25)
SLP_{-ics} × Middle	0.02	0.02	0.02	0.53**	0.46**	0.49**
	(0.06)	(0.06)	(0.07)	(0.20)	(0.21)	(0.21)
SLP_{-ics} × Top 20	-0.24**	-0.23**	-0.26**	0.11	0.03	0.04
	(0.11)	(0.11)	(0.11)	(0.29)	(0.29)	(0.29)
Share of High Delinquency Peers × Bottom 20	0.03			-1.02**		
	(0.12)			(0.39)		
Share of High Delinquency Peers × Middle	0.07			-0.37		
	(0.10)			(0.32)		
Share of High Delinquency Peers × Top 20	0.10			-0.50		
	(0.13)			(0.37)		
Share of Peers Fighting at School × Bottom 20		-0.08			-0.44	
		(0.09)			(0.46)	
Share of Peers Fighting at School × Middle		-0.06			0.07	
		(0.07)			(0.22)	
Share of Peers Fighting at School × Top 20		-0.11			-0.08	
		(0.12)			(0.30)	
Share of Peers with Home Disruption × Bottom 20			0.05			-0.42
			(0.08)			(0.33)
Share of Peers with Home Disruption × Middle			-0.02			0.01
			(0.07)			(0.21)
Share of Peers with Home Disruption × Top 20			0.07			-0.16
			(0.12)			(0.24)
Self Delinquency Scale × Income Position	Yes	No	No	Yes	No	No
Self Fighting at School Scale × Income Position	No	Yes	No	No	Yes	No
Self Home Disruption × Income Position	No	No	Yes	No	No	Yes
Transcript Non-reponse Weights	No	No	No	Yes	Yes	Yes
Mean Dep Var	0.33	0.33	0.33	2.41	2.41	2.41
Observations	11165	11123	11161	7297	7267	7293
R^2	0.251	0.247	0.244	0.297	0.283	0.281

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. Delinquency scale is the sum of 15 items presented in Table B.3. High delinquency is defined as above median children among the delinquency scale distribution. Fighting at school indicator is equal to one if the last physical fight the child had occured at school. Home disruption is defined based on the parent survey, typically filled out by the mother, and is equal to one if the parent answered as having binged more than five alcoholic drinks in the last month, reports discussing separation with their partner, or reported frequently arguing with their partner.

Table E.3. Teachers effects: share of low income peers

	Care Teachers	Close Teachers	Fair Teachers	Teacher Scale
	(1)	(2)	(3)	(4)
$SLP_{-ics} \times$ Bottom 20	-0.01	-0.33	-0.06	-0.19
	(0.22)	(0.20)	(0.18)	(0.20)
$SLP_{-ics} \times$ Middle	-0.11	-0.14	-0.07	-0.14
	(0.17)	(0.18)	(0.18)	(0.19)
$SLP_{-ics} \times$ Top 20	0.21	-0.08	0.00	0.05
	(0.24)	(0.21)	(0.21)	(0.22)
Observations	11110	11164	11162	11165
R^2	0.068	0.074	0.055	0.066

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. Each potential teacher channel variable is standardized.

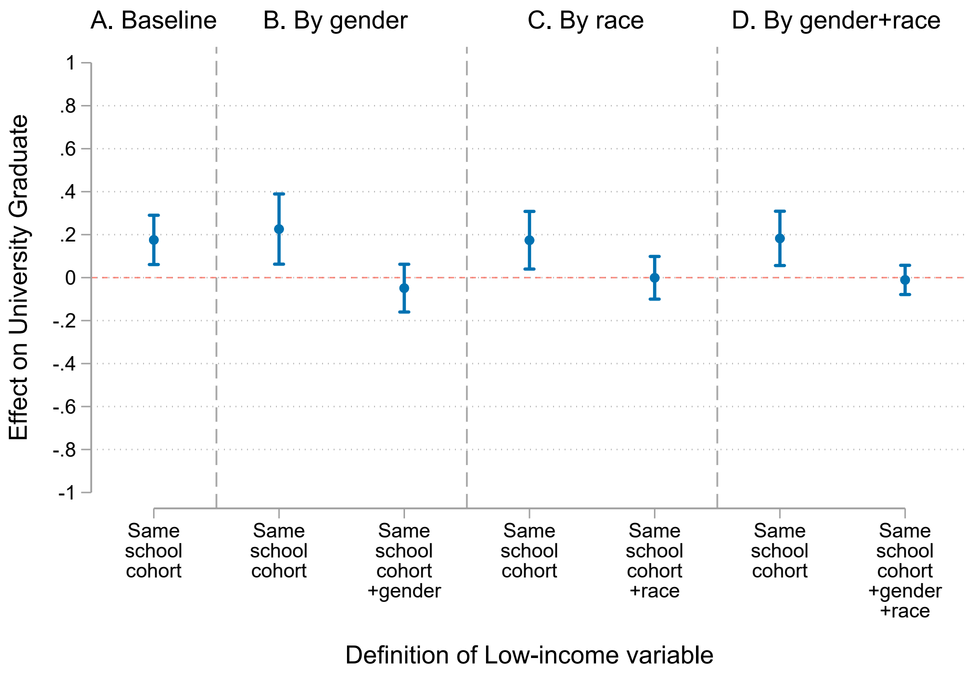
Table E.4. Network centrality effects

	Bonacich centrality	In-degree	Male reciprocates	Female reciprocates
	(1)	(2)	(3)	(4)
$SLP_{-ics} \times$ Bottom 20	0.17	0.09	0.05	0.07
	(0.27)	(0.24)	(0.17)	(0.15)
$SLP_{-ics} \times$ Middle	-0.05	-0.29	-0.08	0.09
	(0.27)	(0.22)	(0.14)	(0.12)
$SLP_{-ics} \times$ Top 20	0.22	-0.22	0.06	0.05
	(0.34)	(0.28)	(0.19)	(0.17)
Mean Dep Var	0.06	0.06	0.54	0.63
Observations	8,114	8,114	3,705	4,286
R^2	0.079	0.119	0.106	0.160

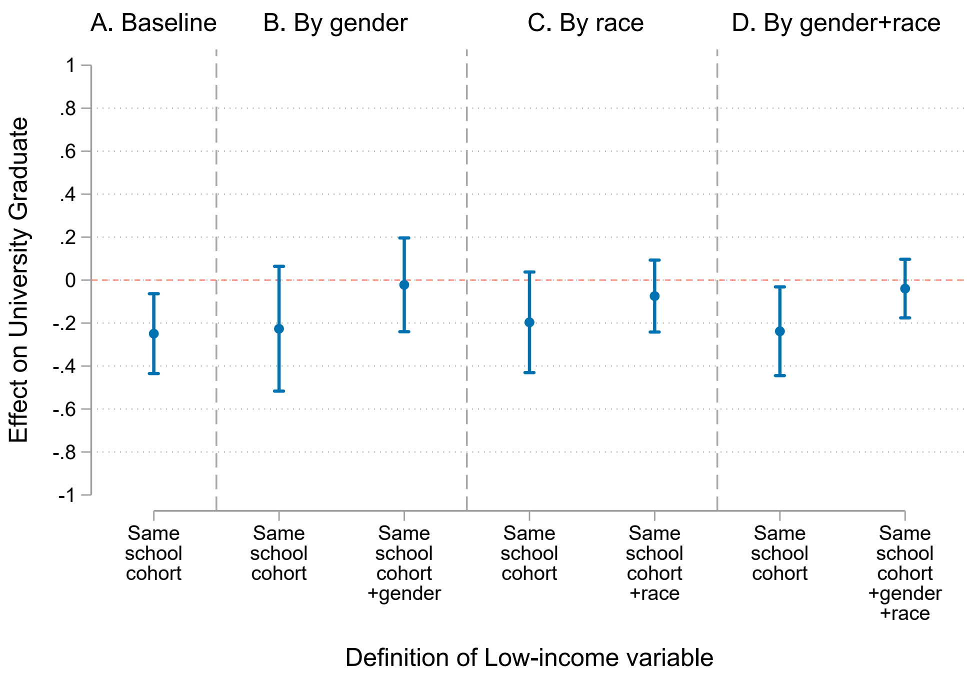
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. School and cohort fixed effects are included in all specifications. Standard errors are in parentheses and clustered at the school level. Each specification includes all controls as in our preferred baseline specification in column (2) of Table 2. Bonacich centrality and In-degree variables are standardized. Male reciprocates denotes that whether the person ego nominated as his/her best male friend nominated ego a friend.

Figure E.1. College 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 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), Athey, Tibshirani, and Wager (2019), and Wager and Athey (2018), 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 college 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, Tibshirani, and Wager (2019) show that treatment effect estimates under unconfoundness and "honesty" are asymptotically normal, allowing the calculation of confidence intervals.[1]

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

[1]This discussion omits complexities on tuning parameters discussed in Athey and Imbens (2016), Athey, Tibshirani, and Wager (2019), and Wager and Athey (2018).

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).[2] Finally, we stack the out of bag CATE estimates across income groups for analysis.

Our individual characteristics included in the causal forests correspond to those in the Appendix Table B.1. To be parsimonious, we then evaluate the variation in the CATEs across the set of characteristics listed in Table F.1. Within each income group, we form an indicator being above or below the median CATE in that group. In the table, we report the mean value of each student characteristic for those above the median (high CATEs) and those below the median (low CATEs) repeating this for each income group.[3] We also report the difference in means across high and low CATEs and 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 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. 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. We have discussed these in the main text.

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

[3]Our approach here is simlar to that of Carlana, La Ferrara, and Pinotti (2022a) except that we split across 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	0.304	0.386	-0.083	0.156	0.092	0.131	-0.040	0.031	0.038	0.023	0.014	0.297
HS or GED Parents	0.569	0.510	0.059	0.425	0.547	0.545	0.001	0.988	0.212	0.339	-0.127	0.003
College Parents	0.128	0.104	0.024	0.708	0.362	0.323	0.038	0.088	0.750	0.638	0.113	0.003
Missing Parents' Education	0.076	0.093	-0.017	0.819	0.035	0.043	-0.008	0.508	0.026	0.007	0.019	0.084
Female	0.700	0.395	0.305	0.003	0.525	0.515	0.010	0.912	0.458	0.565	-0.107	0.007
Age	15.293	15.719	-0.427	0.019	15.561	15.321	0.240	0.016	15.414	15.640	-0.225	0.125
Age Squared	236.608	249.861	-13.252	0.016	244.932	237.680	7.252	0.022	240.109	247.516	-7.406	0.088
Hispanic	0.240	0.227	0.014	0.889	0.120	0.169	-0.049	0.022	0.086	0.050	0.036	0.111
Black	0.325	0.363	-0.039	0.676	0.198	0.157	0.041	0.059	0.088	0.169	-0.081	0.003
Asian	0.028	0.021	0.007	0.876	0.057	0.042	0.015	0.127	0.076	0.051	0.024	0.297
Other Races	0.022	0.021	0.001	0.889	0.018	0.015	0.003	0.912	0.009	0.009	-0.000	1.000
Missing Races	0.000	0.001	-0.001	0.889	0.000	0.003	-0.003	0.092	0.005	0.000	0.005	0.297
Child of an Immigrant	0.228	0.211	0.017	0.889	0.153	0.182	-0.030	0.092	0.143	0.110	0.033	0.297
Missing Child of an Immigrant Info	0.002	0.003	-0.001	0.889	0.001	0.001	-0.000	1.000	0.000	0.001	-0.001	0.608
Single Parent Household	0.528	0.707	-0.179	0.003	0.203	0.327	-0.124	0.002	0.184	0.030	0.154	0.003
Family Size	3.925	3.365	0.560	0.003	3.981	3.624	0.357	0.002	3.910	3.890	0.020	0.895
Ability (AHPVT scores)	96.559	93.165	3.394	0.041	101.641	103.092	-1.451	0.051	108.697	105.749	2.948	0.003
Median CATEs	0.232				0.002				-0.242			
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 6 for reference.