Exposure to Inequality, Human Capital Investment, and Labor Market Outcomes

Jan Bietenbeck[*], Matthew Collins[†], Petter Lundborg[‡], Kaveh Majlesi[§], and Adrián Nieto[¶]

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Abstract

We estimate the effects of exposure to economic inequality among school peers, measured using parental income and wealth, on long-term educational and labor-market outcomes in Sweden and the United States. Using detailed Swedish register data covering all students completing compulsory education between 1989 and 2013, we construct measures of inequality among students' school-cohort peers and exploit variation between cohorts within schools to identify plausibly causal effects. We find no evidence that exposure to peer inequality affects GPA, high school graduation, university enrollment, university completion, or income up to age 35. These null results are precisely estimated and robust to alternative measures of inequality, sample definitions, and specifications. They are also unlikely to reflect a design that is too weak to detect peer effects, as the same research design yields meaningful effects of several other peer characteristics. Moreover, we find no evidence of systematic heterogeneity by socioeconomic background. In contrast, using data from the United States, we estimate positive effects of upper-tail peer income inequality on college enrollment and adult earnings, particularly for students from lower socio-economic backgrounds. One possible explanation is that the wider income distribution and higher returns to education in the US make the economic rewards to educational investment more salient. Taken together, these findings suggest that exposure to economic inequality does not discourage educational investment, providing reassurance that school integration policies mixing students from different socioeconomic backgrounds do not carry hidden long-run costs stemming from exposure to peer inequality.

[*]Lund University, CESifo, IZA, and RFBerlin; email: jan.bietenbeck@nek.lu.se.
[†]University of Galway, Lund University and CESifo; email: matthew.collins@universityofgalway.ie.
[‡]Lund University, CESifo, RFBerlin, and IZA; email: petter.lundborg@nek.lu.se.
[§]Monash University, Lund University, CEPR, and IZA; email: kaveh.majlesi@monash.edu.
[¶]Lund University and IZA; email: adrian.nieto_castro@nek.lu.se.

1 Introduction

Inequality in income and wealth has become one of the defining issues in economics and politics, linked to debates about growth, opportunity, and social cohesion. A key concern is whether inequality affects the accumulation of human capital and thereby long-run economic mobility. Consistent with this concern, a large body of correlational evidence shows that inequality is associated with worse educational outcomes. Influential studies such as Wilkinson and Pickett (2009) and Pickett et al. (2024) go further and argue that this relationship reflects a causal effect of inequality. Yet this association may reflect other factors correlated with inequality, and credible causal evidence on whether inequality affects educational and labor-market trajectories remains limited.

This paper examines one potential channel through which inequality may affect human capital investment: exposure to inequality in a salient social environment. We study this channel among adolescents (and younger children) in schools, where students spend much of their time interacting with peers from different socioeconomic backgrounds. School cohorts thus constitute plausible reference groups for social comparisons and perceptions of attainable outcomes (Bowles and Carlin, 2020; Londoño-Vélez, 2022). Adolescence is also a period in which individuals become particularly sensitive to social comparisons and increasingly aware of their socioeconomic position.[1] Consistent with the importance of this environment, school peers have been shown to affect academic achievement, educational choices, and long-run earnings.[2] Schools are also central to integration policies that explicitly aim to mix students from different socioeconomic backgrounds (Deming et al., 2014; Angrist et al., 2022; Bjerre-Nielsen and Gandil, 2024).

How inequality among school peers affects human capital investment is theoretically ambiguous. From a human capital perspective, inequality may encourage educational investment if students interpret economic differences among their peers as signals of the returns to skill (Becker, 1964; Abramitzky and Lavy, 2014). Theories of socially determined aspirations, however, suggest that this effect may be non-monotonic: moderate gaps can encourage investment by making better outcomes salient and attainable, while large gaps can generate frustration and reduce investment (Genicot and Ray, 2017). In line with this, Kearney and Levine (2016) argue that greater inequality may generate

[1] Children and adolescents perceive economic differences from an early age. Young children recognize economic differences through visible cues such as clothing and housing (Ramsey, 1991; Rauscher et al., 2017), and by ages 7–10 many can rank occupations by income (Burgard et al., 1989; Emler and Dickinson, 1985). During adolescence, youths become increasingly attuned to social comparisons and more accurate in assessing their socioeconomic position (Mistry et al., 2015; Peretz-Lange et al., 2022; Hautala and Lehti, 2025). Recent survey evidence shows that adolescents aged 13–17 hold systematic perceptions of inequality and unequal mobility (Alesina et al., 2024); direct classroom evidence comes from Rao (2019), who shows that most children in grades 2–5 in elite Indian schools could identify which classmates were poor.

[2] See e.g., Hoxby (2000), Lavy et al. (2012), Black et al. (2013), Carrell et al. (2018), Bursztyn and Jensen (2015), Bursztyn et al. (2019), Bietenbeck (2020), Getik and Meier (2025), and Cattan et al. (2025).

"economic despair" among disadvantaged students when middle-class outcomes appear out of reach. The latter mechanisms imply that inequality may reduce human capital investment particularly among students from lower socioeconomic backgrounds, potentially contributing to the negative relationship between inequality and intergenerational mobility illustrated by the Great Gatsby Curve (Blanden et al., 2005; Corak, 2013; Durlauf et al., 2022; Cholli et al., 2024).

We investigate these competing predictions using detailed Swedish administrative data covering all students who completed grade 9, the final year of compulsory schooling, between 1989 and 2013. This is a critical stage for human capital investment, as students must decide whether to pursue upper secondary (high school) education and, if so, which track to enter—decisions with lasting consequences for subsequent educational attainment and labor-market outcomes. Using information on parental income and wealth, we construct measures of inequality among students' school-cohort peers. For income, we average each peer's parental income over the three years preceding completion of grade 9 and construct the 90:10, 90:50, and 50:10 ratios, capturing overall, upper-tail, and lower-tail inequality. For wealth, we use the 90:50 ratio because the lower tail often includes zero assets. We estimate the effects of exposure to peer inequality on students' GPA at the end of compulsory school, high school graduation, university enrollment and graduation, and income up to age 35.

To identify causal effects, we exploit variation in peer inequality across cohorts within the same school. This research design, pioneered by Hoxby (2000) and widely used in the peer effects literature (e.g. Lavy et al., 2012; Black et al., 2013; Carrell et al., 2018; Cattan et al., 2025), compares students attending the same school in different cohorts who are exposed to different levels of peer inequality. Because students typically remain with the same school-cohort peers throughout grades 7–9, and because Swedish compulsory schools do not track students by ability or socioeconomic background, our measure of school-cohort inequality captures sustained exposure to different socioeconomic peer environments rather than just ninth-grade exposure or classroom sorting. Our regressions control for school and cohort fixed effects as well as school-specific linear trends in order to account for persistent differences across schools, shocks common to cohorts, and school-specific trends over time. Our identifying assumption is that, conditional on these controls, variation in peer inequality across cohorts within schools is unrelated to other determinants of student outcomes. Consistent with this assumption, we find no systematic relationship between peer inequality and predetermined student characteristics, including parental income and education.

The results are striking in their consistency: we find no evidence that exposure to inequality among school peers during adolescence has economically meaningful effects on educational or labor-market outcomes. Across all measures of income and wealth inequality, the estimates are close to zero and precisely estimated. For example, increasing the

90:50 parental-income ratio by one standard deviation of the variation used for identification implies an estimated rise in university enrollment of just 0.08 percentage points, with a 95 percent confidence interval of 0.02 to 0.14 percentage points. These findings hold across a broad set of outcomes, including grades, high school graduation, university enrollment and graduation, and adult income.

We next examine whether the average effects conceal heterogeneous responses predicted by theories of aspirations and social comparison. These theories suggest that inequality may be particularly discouraging for disadvantaged students, for whom large socioeconomic gaps can make upward mobility appear less attainable. We therefore split students by parental income, wealth, and education, but find no evidence that exposure to inequality systematically harms students from lower socioeconomic backgrounds. We then ask whether inequality matters more in disadvantaged schools, where affluent peers may be especially salient and high-status outcomes may seem less attainable. Crossing student socioeconomic status (SES) with school SES, we find little evidence of systematic effects even in these settings. Allowing for non-linear effects yields the same conclusion. Thus, greater peer inequality appears neither to stimulate human capital investment through higher perceived returns to skill nor to suppress it through economic despair.

Two sets of findings help establish that these null estimates are informative. First, the identifying variation in peer inequality is economically substantial. For the 90:50 income ratio, a one-standard-deviation increase in the variation used for identification corresponds, holding median peer parental income fixed, to roughly a 27 percent increase in the income gap between median-income and high-income peers. Thus, the identifying variation reflects meaningful differences in inequality among school peers rather than small fluctuations in their economic circumstances. Second, our research design is capable of detecting peer effects in general. Applying the same within-school, between-cohort design to other dimensions of peer composition, we find economically meaningful effects of peers' immigrant background, parental education, and predicted ability on student outcomes.

Our findings are robust to a broad range of alternative measures and specifications. We obtain similarly small estimates when measuring inequality using housing wealth, a more visible dimension of living standards; restricting the analysis to smaller cohorts or same-gender peers to better approximate students' relevant comparison groups; excluding outliers; adding sibling fixed effects; and controlling for peers' average parental income or wealth. We also examine whether the effects of inequality depend on the timing or persistence of exposure. Using measures of peer inequality in grades 3, 6, and 9, we find little evidence that earlier or more sustained exposure meaningfully affects subsequent educational outcomes. Similarly, inequality among upper secondary school peers has little effect on later educational or labor-market outcomes.

To assess whether the findings extend to a setting with substantially higher levels of

inequality, we complement the Swedish analysis with evidence from the National Longitudinal Study of Adolescent to Adult Health (Add Health) in the United States. Add Health allows us to compare students in different grades within the same school at baseline, yielding a related within-school, across-grade design rather than the cohort-over-time design used in Sweden.

In the United States, we estimate positive effects of upper-tail peer income inequality on college enrollment and adult earnings. For example, a one-standard-deviation increase in the residualized 90:50 parental-income ratio increases college enrollment by approximately 0.95 percentage points and adult earnings by approximately 1.7–2.2 percent. The positive educational effects are particularly pronounced among students without university-educated parents. One interpretation is that the wider income distribution and higher returns to human capital in the United States make the economic gains from educational investment more visible, providing stronger incentives to invest in education that may outweigh any potential discouragement arising from larger income differences. Related evidence from Chetty et al. (2022) shows that friendships across socioeconomic backgrounds are strongly associated with upward income mobility in the United States, although our analysis examines a different form of socioeconomic exposure. In Sweden, by contrast, a more compressed income distribution may make the economic rewards to educational investment less salient, while also reducing the potential for discouragement arising from large socioeconomic gaps.

It is important to note that our analysis does not directly distinguish between these mechanisms, and the differences in findings between the two countries cannot be attributed solely to income dispersion, given their distinct institutional environments and empirical designs. More broadly, our results suggest that the effects of exposure to peer inequality may depend on the economic and social context in which that exposure occurs.

Beyond their immediate setting, our findings speak to broader debates about inequality and opportunity. For education policy, they provide reassurance that efforts to mix students from different socioeconomic backgrounds do not necessarily impose hidden costs on students' motivation or educational outcomes through increased exposure to inequality. Schools are a particularly salient and policy-relevant environment in which adolescents encounter inequality, making them a natural testing ground for these theories and our results provide little support for direct exposure to peer inequality as a major channel through which inequality reduces aspirations or weakens mobility.

Our paper contributes to research across the social sciences on the relationship between inequality and human capital investment. In a series of influential studies, Wilkinson and Pickett (2007, 2009) and Pickett et al. (2024) document negative correlations between income inequality and student performance across countries and U.S. states. Using within-state variation over time, Mayer (2001) finds that greater inequality is associated with higher educational attainment among high-income children but lower at-

tainment among low-income children, while Kearney and Levine (2016) find that greater inequality increases high school drop-outs among low-SES adolescents.[3] These patterns are consistent with theories in which inequality shapes aspirations and perceived opportunities. However, state-level changes in inequality may coincide with changes in other determinants of educational outcomes. We contribute by providing plausibly causal estimates of the effects of direct exposure to inequality within school peer groups on long-run human capital outcomes.

We also contribute to the literature on inequality and social mobility (e.g. Corak, 2013; Chetty et al., 2014). Chetty and Hendren (2018) show that children from lower-income families have lower adult incomes when they grow up in more unequal areas, while emphasizing that these associations do not identify causal effects of inequality. One proposed mechanism is that inequality discourages human capital investment among disadvantaged children, thereby reducing upward mobility. Our analysis tests a specific version of this channel—whether direct exposure to inequality among school peers discourages human capital investment—and finds no support for it.

Finally, our paper relates to the literature on peer effects in schools.[4] Prior work has examined effects of peer ability (e.g. Lavy et al., 2012; Bietenbeck, 2020), peer demographic characteristics (e.g. Hoxby, 2000; Hoxby and Weingarth, 2005; Lavy and Schlosser, 2011; Brenøe and Zölitz, 2020; Figlio et al., 2024), peer personality (Bietenbeck, 2025), disruptive peers (e.g. Figlio, 2007; Carrell et al., 2018), and exposure to peers with an elite education (Cattan et al., 2025). Related work highlights how peer pressure and social image concerns shape educational investment decisions (Bursztyn and Jensen, 2015; Bursztyn et al., 2019). Most closely related, Rao (2019) exploits the policy-induced integration of poor students into elite Indian schools and finds substantial effects on wealthy students' social preferences and interactions, but comparatively modest effects on academic achievement. We add to this literature by studying a distinct dimension of peer composition: economic inequality in schools, and its effects on human capital investment and long-run labor-market outcomes.

2 Background

2.1 The Swedish education system

The Swedish compulsory-school system provides a useful setting for our research design because students progress through stable school cohorts, school assignment is primarily residence-based, and formal tracking by ability or socioeconomic background is absent.

[3] We find a similar descriptive pattern in Sweden: Figure A.1 shows that children from low socioeconomic backgrounds are more likely to drop out of high school if they attend compulsory schools with greater income inequality.

[4] For overviews, see Sacerdote (2011) and Paloyo (2020).

During our study period, children in Sweden were required to complete nine years of compulsory schooling (*grundskolan*), beginning in the fall of the year they turned seven and ending in the year they turned sixteen. Compulsory schooling was divided into three stages: grades 1–3, 4–6, and 7–9. Schools could offer all three stages or only one or two of them. Grade retention or advancement was extremely rare, so cohorts typically advanced together and peer groups remained stable within each stage.

Admission to public schools was largely determined by residential proximity, with municipalities prioritizing students living nearby. Although families could express preferences for other schools, admission remained subject to capacity and school placement rules. Independent schools (*friskolor*) could use regulated admission criteria, such as waiting lists and sibling priority, but could not charge fees or select students by academic ability. On average, they enrolled around six percent of students during our study period. As a result, the socioeconomic composition of school cohorts was shaped to a large extent by neighborhood demographics and birth year.

At the end of compulsory school, students chose whether to apply to high school (*gymnasium*), where admission was based on 9th-grade GPA. Students could choose between academic and vocational programs, with the former preparing for higher education.[5] These decisions were therefore high-stakes choices, with lasting consequences for subsequent educational attainment and labor-market outcomes.

A key institutional feature for our empirical analysis is the absence of formal tracking in compulsory school. If schools assigned students to classes by ability or socioeconomic background, inequality could be concentrated within particular classrooms, making school-cohort inequality a less informative measure of students' peer environment. However, no such formal tracking existed in the Swedish compulsory school system during our study period. Class placement was administrative, driven mainly by balancing class sizes and scheduling needs, and largely unrelated to family background. This institutional feature makes school-cohort inequality a relevant measure of the socioeconomic environment students faced, rather than an artifact of ability or socioeconomic sorting across classrooms.

Together, these features make Sweden well suited for studying how exposure to peer inequality during compulsory schooling affects later educational and labor-market outcomes.

2.2 Income and wealth inequality in Sweden

Sweden has historically stood out for its relatively low income inequality. The deep recession of the early 1990s, however, marked a turning point, triggering a sharp and persistent

[5] A reform in 1994 extended vocational programs from two to three years and introduced a broader set of core academic subjects, enabling students to qualify for university admission if they completed additional coursework.

increase in inequality. In the wake of this recession, lower-income workers experienced large earnings losses, while higher-income workers were less affected (Friedrich et al., 2022). Although inequality fell somewhat thereafter, it never returned to pre-recession levels. By international standards, Sweden still appears relatively equal: in 2023, the 90:10 ratio of household disposable income was 3.3, compared with 3.8 in Germany and 6.4 in the United States (OECD, 2025). Yet, as we show in Section 4, these national figures mask substantial variation in inequality across school cohorts, with some experiencing levels of inequality that approach those observed in far more unequal societies.

Wealth inequality presents a different picture. Wealth disparities have increased considerably since 2000 (Lundberg and Waldenström, 2018), and Sweden today ranks among the most unequal countries in the developed world (World Inequality Database, 2025), with the top 10 percent holding about 68 percent of net wealth in 2023, similar to the United States at 70 percent and substantially above Germany at 59 percent. At the school-cohort level, inequality in parental assets also varies sharply, ranging from highly compressed to extremely unequal distributions. This variation provides an informative setting in which to test whether exposure to inequality in adolescence has lasting effects on educational investment and later-life mobility.

3 Data

3.1 Sources and definition of key variables

We use individual-level data from the Swedish Interdisciplinary Panel (SIP), which links multiple administrative registers covering all individuals born since 1973 and follows them until 2017. The data provide detailed information on 9th-grade school cohort composition, parental income and wealth, educational attainment, and adult income. Below, we describe the key variables used in our analysis.

Income inequality We measure income inequality among each student's 9th-grade school peers, excluding the student herself, following the standard practice in the peer-effects literature to avoid any mechanical correlation between her own parental income and the peer inequality measure. We link each peer to their parents' total income, which includes income from salaries, business profits, pensions, and sickness benefits, as well as capital income from interest and dividends.[6] To smooth transitory fluctuations, we average parents' total income over the three full calendar years before the cohort completes 9th grade. Using this measure, we compute the 90:10, 90:50, and 50:10 income ratios, capturing overall, upper-tail, and lower-tail inequality, respectively.[7]

[6] All income and wealth measures are deflated to the year 2000 using Statistics Sweden's Consumer Price Index.

[7] Percentile ratios are widely used in the inequality literature because they provide an intuitive and transparent way of describing the distribution (Autor et al., 2008; Gordon and Dew-Becker, 2007;

Because students typically remain with the same school-cohort peers throughout grades 7–9, these measures capture sustained exposure to the socioeconomic distribution of the peer group over the final stage of compulsory school rather than a transitory ninth-grade exposure.

Wealth inequality To calculate wealth inequality, we use information on peers' parental wealth. Wealth data were collected for tax purposes between 1999 and 2007, after which the wealth tax was abolished. The total assets variable we use captures the sum of market-valued real, financial, and other assets. For each student, we sum the total assets of both parents at the end of the year preceding their 9th-grade graduation. We then calculate the 90:50 wealth ratio among each student's 9th-grade school peers, excluding the student herself. We do not use the 90:10 and 50:10 wealth ratios since, in most years, bank accounts are not reported for those with small deposits. As a result, we observe the tenth percentile of peers' parental wealth as zero for 37% of our sample.

Human capital investment We measure the effects of exposure to inequality on a range of educational outcomes that reflect human capital investment: 9th-grade GPA (standardized to have a mean of 0 and a standard deviation of 1 within national cohorts), high school graduation, academic-track high school graduation, university enrollment, and university graduation.

Labor market outcomes We construct two measures of adult income: average total income (earned and capital income) at ages 28–30 and at ages 33–35, respectively. Since the SIP includes income only through 2016, the former measure is available for cohorts born up to 1986, while the latter is available for cohorts born up to 1981.

Background variables In balancing and robustness checks, we use a set of predetermined student characteristics: age in 9th grade (in years), a dummy for female, a dummy for being a first- or second-generation immigrant, mother's age at birth, family size, parental years of education, and parental income averaged over the three years preceding 9th grade.

3.2 Sample selection and summary statistics

We construct two distinct samples to accommodate differences in data availability. For the income inequality analysis, we focus on cohorts completing 9th grade between 1989 and 2013. The starting year, 1989, corresponds to the graduation year of the first cohort included in the SIP data (born in 1973), and we include cohorts through 2013, with educational outcomes observed through 2017. These restrictions yield a sample of 2,487,434

Meyer and Sullivan, 2017). Unlike summary measures such as the Gini coefficient, which compresses the full distribution into a single index, percentile ratios allow us to distinguish between inequality driven by gaps at the bottom (50:10), the top (90:50), or across the entire distribution (90:10). This distinction is particularly relevant in our setting, since adolescents may be most sensitive to visible gaps either at the lower or upper tail of their school-cohort distribution rather than to abstract aggregate measures.

students. In contrast, for the wealth inequality analysis, we restrict the sample to cohorts completing 9th grade between 2000 and 2008, as parental wealth data are available only from 1999 to 2007 and wealth inequality is measured in the year preceding 9th-grade graduation. This sample comprises 959,659 students.[8]

Table 1 presents summary statistics for the two samples. Both samples are very similar in terms of students' sociodemographic characteristics. The average 9th-grade cohort size is approximately 112 students in both samples. Regarding income inequality, the median 90:10 income ratio is 3.05, while the mean is much higher at 116.58 due to outliers driven by very small incomes at the 10th percentile. We retain these outliers in the main analysis to reflect the full distribution of observed inequality but show in our robustness analyses that excluding them does not affect our conclusions. The median 50:10 income ratio is 1.94 (mean: 53.01), while the median 90:50 income ratio is 1.54 (mean: 1.60). These median values are broadly comparable to the corresponding ones at the national level, which are also reported in the table.

For wealth inequality, the median 90:50 wealth ratio is 2.55, while the mean is considerably higher at 35.08, reflecting extreme values of the ratio; again, we show that excluding these outliers does not change our conclusions.

In terms of outcomes, 80 percent of students graduate from high school, and 45 percent graduate from an academic-track high school program that prepares them for university. Just under one quarter of students graduate from university within our sample period. In the income inequality sample, average income is 203,566 SEK at ages 28–30 (corresponding to 30,692 USD at 2024 prices) and 250,797 SEK at ages 33–35 (37,813 USD), whereas in the wealth inequality sample average income at ages 28–30 is 219,866 SEK (33,149 USD).[9]

3.3 Add Health: U.S. sample

To assess whether our findings extend beyond the Swedish context, we use the National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a longitudinal study of U.S. adolescents enrolled in grades 7–12 in 1994–95 who have been followed into adulthood. The study began with a school-based sample of 80 high schools and 52 feeder schools. The baseline in-school survey covered 90,118 students, of whom a subsample participated in in-home interviews. In total, 20,745 adolescents completed the Wave I in-home interview, and parents of 17,670 respondents completed a separate questionnaire, which we use to measure parental income. Our analysis combines these

[8] We implement some additional sample restrictions. In the income inequality sample, we exclude 167,250 individuals with missing values for any inequality measure, educational outcome, or balancing variable, as well as individuals who are the sole observation in their school cohort. In the wealth inequality sample, the corresponding exclusions amount to 11,916 individuals. The adult-income analyses use smaller samples because these outcomes are available only for earlier cohorts.

[9] Income at ages 33–35 is not available in the wealth inequality sample.

baseline data with information from subsequent waves to estimate the effects of exposure to peer inequality during adolescence on educational and labor-market outcomes.[10]

Because Add Health observes students in multiple grades within the same schools at baseline, we can construct grade-specific measures of peer inequality. Parental income is measured from the parent questionnaire as total pre-tax household income in 1994, including earnings, welfare benefits, dividends, and other sources. Analogously to our Swedish analysis, for each student we calculate the 90:10, 90:50, and 50:10 ratios of parental income among students in the same school and grade, excluding the student herself. Unlike in the Swedish registers, these measures are based on sampled respondents with available parental-income information rather than the entire school-grade population. On average, each respondent's peer-inequality measures are therefore calculated using approximately 55 other sampled students in the same school-grade cell, compared with an average total enrollment of around 280 students per school-grade cell.

The longitudinal follow-ups provide measures of both educational attainment and labor-market outcomes. We obtain high-school GPA from transcript data collected as part of Wave III (2001–02). We measure high-school graduation (including equivalent credentials such as the GED), college enrollment, and bachelor's degree completion by Wave IV (2008–2009), and earned income at three points in time: Wave IV (2008–09, ages 24–32), Wave V (2016–18, ages 33–43), and Wave VI (2022–25, ages 39–51). Because outcome availability differs across waves, our regression samples vary by outcome.

Table 2 presents summary statistics for the Add Health analysis samples. The samples are broadly similar in observed background characteristics. Importantly, peer income inequality is substantially higher than in Sweden. Across the outcome-specific samples, the median 90:10, 90:50, and 50:10 parental-income ratios are approximately 5.7, 2, and 2.8, respectively, all substantially larger than the corresponding median ratios in Sweden.

4 Empirical strategy

The key empirical challenge in estimating the causal effects of exposure to inequality is that inequality is not randomly assigned. Across schools, inequality is likely correlated with other determinants of students' outcomes, including school quality, neighborhood characteristics, immigrant composition, and peers' socioeconomic background. We therefore exploit within-school, between-cohort variation in peer inequality, comparing students who attend the same school but happen to belong to cohorts with different levels of inequality. By making comparisons within schools, we hold fixed persistent characteristics of the school and, given the close link between residential location and school

[10] Because our analysis requires non-missing parental income, school-grade identifiers, peer-inequality measures, and follow-up outcomes, our analysis samples are smaller than the full sample. We describe these outcome-specific samples in Table 2.

assignment in our setting, much of the persistent neighborhood environment as well.

Building on Hoxby (2000), we estimate:

$$Y_{ics} = \beta INEQ_{-ics} + \delta_c + \lambda_s + c \times D_s + \varepsilon_{ics}, \tag{1}$$

where Y_{ics} is an educational or labor-market outcome for student i in cohort c and school s; $INEQ_{-ics}$ measures inequality among i's school-cohort peers; δ_c denotes cohort fixed effects; λ_s denotes school fixed effects; $c \times D_s$ denotes school-specific linear cohort trends; and ε_{ics} is the error term. We estimate Equation 1 by ordinary least squares and cluster standard errors at the school level.

The coefficient β identifies the causal effect of exposure to peer inequality under the assumption that, conditional on school and cohort fixed effects and school-specific trends, cohort-to-cohort variation in inequality within schools is unrelated to other determinants of student outcomes. School fixed effects absorb persistent differences across schools, cohort fixed effects absorb shocks common to a graduation cohort, and school-specific trends allow schools to follow different linear trajectories over time. Importantly, identification does not require other dimensions of peer composition to remain mechanically constant. Rather, changes in peer inequality must not systematically coincide with changes in other factors that independently affect student outcomes.

A particularly relevant concern is that changes in inequality may coincide with changes in the average level of peer resources. We therefore also estimate specifications that directly control for peers' average parental income or wealth. A second concern is that the school cohort may be broader than the set of peers with whom a student regularly interacts. We address this by restricting the sample to smaller schools and by measuring inequality within school-cohort-by-gender cells.

4.1 Validity of the identifying assumption

We assess the plausibility of our identifying assumption using balancing tests. In particular, we test whether school-cohort inequality predicts predetermined student characteristics using the specification in Equation 1. If changes in inequality across cohorts within schools are indeed quasi-random after conditioning on our controls, they should not systematically predict student background. Table 3 reports the results, which are consistent with our identifying assumption: across 33 regressions, the estimated relationships are generally economically negligible. Some coefficients are statistically significant, which is unsurprising given our very large sample, but we find no systematic pattern indicating that cohorts exposed to greater inequality differ meaningfully in their predetermined characteristics. Consistent with this evidence, our main estimates are essentially unchanged

when we condition directly on the full set of student background characteristics.[11]

4.2 Magnitude of the identifying variation

A natural concern is that most inequality may arise across schools rather than across cohorts within schools, leaving little variation for identification. This is not the case. In the Swedish data, the standard deviation of the 90:50 parental income ratio falls from 0.28 to 0.16 after residualizing the measure with respect to school fixed effects, cohort fixed effects, and school-specific linear trends. Thus, a substantial amount of variation remains within schools across cohorts. The corresponding percentage reductions in the standard deviations of the other Swedish inequality measures are 31 percent for the 90:10 income ratio, 27 percent for the 50:10 income ratio, and 18 percent for the 90:50 wealth ratio.

To interpret this variation economically, consider the 90:50 parental income ratio. Its mean in the Swedish sample is 1.60, and median peer parental income is 392,033 SEK. A one-standard-deviation increase in the residualized 90:50 ratio raises the ratio from 1.60 to 1.76. Holding median parental income fixed, this corresponds to an increase in the 90th-percentile parental income from about 627,000 SEK to 690,000 SEK, or an increase in the 90th-to-median income gap of about 63,000 SEK. Relative to the baseline gap, this is a 27 percent increase. The identifying variation therefore corresponds to economically meaningful differences in the peer income distribution, rather than small residual fluctuations.

The magnitude of the within-school variation is also visible in Appendix Figure A.2, which plots the time series of income and wealth inequality for 10 randomly selected schools. Panel A of Figure A.3 provides a more systematic comparison by plotting the distribution of within-school, between-cohort variation in the 90:50 income ratio alongside the corresponding between-school variation. The two distributions are similar, indicating that the identifying variation is not markedly smaller than the variation across schools.

5 Results

We start by providing descriptive evidence on the relationship between inequality and education and labor market outcomes, before moving on to our causal estimates. Figure 1 shows that the negative association between income inequality and educational outcomes documented in previous research (Wilkinson and Pickett, 2009; Pickett et al., 2024) is also

[11] We also run balancing checks using inequality in the preceding cohort within the same school as the treatment. This addresses the concern that families may sort across schools in response to the socioeconomic composition—and especially the inequality—of earlier cohorts. As shown in Table A.1, lagged inequality likewise does not systematically predict student background, providing little evidence of such anticipatory sorting.

evident in our setting. Panel A plots the proportion of students in the 2000 ninth-grade cohort who subsequently graduated from high school against income inequality across Swedish provinces in 2000. Inequality is measured by the 90:50 income ratio among the population of each province. The figure shows a clear negative correlation between inequality and high school graduation.

Panel B presents a similar relationship at the school-cohort level, which is the level of analysis in our study. It plots the proportion of students graduating from high school against the 90:50 ratio of peers' parental income for students completing ninth grade in 2000. Schools are categorized into 20 bins based on their level of peer income inequality, and both inequality and high school graduation are averaged across schools in each bin. Similarly to Panel A, there is a clear negative association between inequality and high school graduation.

Table 4 extends the descriptive analysis by reporting unconditional regressions of student outcomes on school-cohort-level inequality measures. Overall, the signs are mixed and no consistent pattern emerges across measures. Most coefficients for the 90:10 and 50:10 income ratios, as well as the 90:50 wealth ratio, are small. By contrast, the 90:50 income ratio exhibits stronger associations with several outcomes. Consistent with Figure 1, it is negatively associated with high school graduation, although the estimate is not statistically significant. However, higher inequality according to this measure is linked to better grades and substantially higher probabilities of both academic-track high school graduation and university enrollment. In sum, while the unconditional regressions do not reveal a consistent relationship between inequality and outcomes overall, the 90:50 ratio shows stronger and mostly positive associations. To test whether these relationships reflect causal effects, we now turn to our main estimates, which exploit variation in inequality across cohorts within schools.

5.1 Causal estimates

Table 5 presents our main estimates of the effects of exposure to inequality on human capital investment and labor-market outcomes, using the empirical specification outlined in Equation 1. The results are striking: we find no evidence that inequality has economically meaningful effects on 9th-grade GPA, high school graduation, university enrollment or graduation, or adult income. This pattern holds broadly across all four measures of income and wealth inequality, and the estimates are in most cases only a fraction of the magnitudes observed in the descriptive analysis. These findings stand in sharp contrast to earlier evidence linking greater inequality to lower human capital investment and mobility.

Importantly, our estimates are also sufficiently precise to rule out economically meaningful effects for key outcomes. For example, the estimated effect of the 90:50 income

ratio on university enrollment is 0.005 (SE = 0.002). An increase in the 90:50 ratio of 0.16 points, corresponding to one standard deviation of the variation used for identification, implies an effect on university enrollment of only 0.08 percentage points, and the 95 percent confidence interval rules out effects larger than 0.14 percentage points. To provide an additional intuitive benchmark, the national 90:50 income ratio is approximately 2.3 in the United States and 1.7 in Sweden, a difference of 0.6 points. Applying this difference as an illustrative benchmark implies an effect on university enrollment of 0.3 percentage points, with an upper 95 percent confidence bound of approximately 0.5 percentage points. Thus, even when benchmarked against the much larger U.S.–Sweden difference in inequality, the implied effects remain small.

5.1.1 Heterogeneity by parental SES

One possible explanation for the discrepancy with previous research is heterogeneity: while exposure to inequality might encourage students from higher-income families to invest more in education, it could generate "economic despair" and reduce investment among those from lower-income backgrounds. To test this hypothesis, we divide students into two groups based on their parents' income rank relative to the parental income distribution of the preceding cohort at the same school for the income inequality analysis, and their wealth rank for the wealth inequality analysis.[12] We then separately estimate effects for students with below- and above-median parental income or wealth.

The results in Table 6 provide little support for this explanation: across nearly all outcomes and measures of inequality, the coefficients remain close to zero, though some reach statistical significance given the large sample size. For example, among lower-income students, a standard deviation increase in the 90:50 income ratio implies an increase in university enrollment of just 0.16 percentage points, an economically small and positive effect. This differs from the negative relationship between inequality and educational investment among disadvantaged youth documented at the state level by Kearney and Levine (2016) and Mayer (2001).

Overall, we find little evidence that exposure to inequality affects human capital investment or labor-market outcomes for students from either lower- or higher-income backgrounds, or from lower- or higher-wealth families. This conclusion is reinforced by alternative specifications that define parental rank using the national rather than school-cohort distribution (Appendix Table A.3) and by analyses that split the sample by parental education instead of income or wealth (Appendix Table A.4).

[12] Because rank is mechanically related to inequality within the same school-cohort cell, we calculate rank using the distribution of parental income and wealth from the previous cohort in the same school. This necessitates excluding observations for which the previous-cohort distribution is unavailable, slightly reducing the sample size.

5.1.2 Heterogeneity by school SES

The analysis by parental SES shows no systematic differences in how inequality exposure affects students from lower- versus higher-income families. Yet the effects of inequality may also depend on the socioeconomic composition of students' peer environments. To investigate this possibility, we classify school cohorts as high- or low-income based on whether their average parental income is above or below the median across school cohorts. We then distinguish students with parental income above and below their school-cohort median, yielding a 2×2 design that crosses student SES with the socioeconomic composition of their school cohort.

Appendix Tables A.5 and A.6 report the results. Although some negative effects emerge for higher-income students, particularly on adult income, we find no consistent pattern across groups, outcomes, or inequality measures. In particular, there is little evidence that exposure to inequality systematically harms lower-income students, even in disadvantaged school cohorts.

5.1.3 Non-linear effects

Another potential explanation for the null results so far is that the effects of inequality are non-linear, with linear models masking offsetting impacts at different points of the distribution. For example, moderate inequality may raise aspirations by exposing students to more advantaged peers, whereas larger disparities may make similar achievements seem unattainable, discouraging educational investment. To test this, we classify school-cohort inequality into quartiles and estimate effects separately for intermediate (Q2–Q3) and high (Q4) inequality, using the lowest quartile (Q1) as the reference group. The results in Table 7 provide little evidence of systematic non-linearities. While some coefficients are statistically significant, effect sizes are generally small and inconsistent across outcomes. In particular, we find no consistent evidence of adverse effects on educational attainment or adult income among students exposed to the highest levels of peer inequality. These findings suggest that our main conclusions are not driven by imposing a linear relationship between peer inequality and student outcomes.

5.1.4 Can our research design detect peer effects?

A natural concern is that our null estimates may reflect limitations of the within-school, between-cohort design rather than an absence of effects of inequality. In particular, cohort-to-cohort variation in peer composition may be too noisy, or the inclusion of school fixed effects and school-specific trends may leave too little identifying variation to detect meaningful peer effects.

To assess this possibility, we apply exactly the same research design to peer characteristics for which the existing literature provides strong reasons to expect effects: the

female share, immigrant share, predicted ability, and parental education of school-cohort peers. Appendix Table A.2 shows economically meaningful effects across most of these dimensions, with 18 of 28 estimates being statistically significant. A higher female share increases adult income, while a higher immigrant share lowers grades and reduces rates of high-school graduation and university enrollment and graduation. Higher shares of university-educated parents and higher predicted peer ability increase rates of high school graduation and university enrollment and graduation. Overall, these results suggest that differences in peer composition shape the social environment in ways that matter for students' outcomes.

More generally, these findings speak directly to the concern that idiosyncratic cohort-to-cohort fluctuations are behaviorally irrelevant because students respond only to broader or more persistent social environments. The evidence points against this concern: the same within-school, between-cohort variation generates meaningful effects for several economically and socially important peer characteristics. This also mitigates concerns that cross-grade spillovers make own-cohort composition uninformative, since such spillovers would tend to attenuate the estimated effects of other peer characteristics as well. Thus, the within-school, between-cohort design is capable of detecting peer influences in our setting, including influences related to peers' socioeconomic background. What appears not to matter is inequality in peers' parental income or wealth.

This interpretation is further supported by how the identifying variation in inequality compares with that of other peer characteristics. As shown in Figure A.3, substantial within-school, between-cohort variation remains in inequality, even when compared with the corresponding between-school variation. The figure also shows that within-school variation is even more limited relative to between-school variation for several other peer characteristics, including gender composition and immigrant share, for which we nevertheless detect meaningful effects. Together, these comparisons make it unlikely that the null estimates for inequality simply reflect a research design that is too weak to detect peer effects.

5.2 Robustness checks

We next present results from additional analyses that address concerns related to measurement limitations, sensitivity to outliers, selection into schools, and potential confounding by student background and average peer resources.

5.2.1 Measurement of inequality

A potential limitation is that our measures of inequality may not fully capture the relevant peer environment, as students do not interact with all members of their school cohort. To address this concern, we implement two alternative specifications. In Appendix Table A.7,

we restrict the analysis to schools whose ninth-grade cohort size never exceeds 120 during our study period. The smaller average cohort sizes of 68 and 70 in the income and wealth inequality samples, respectively, make it more likely that students interact with most of their school-cohort peers. Alternatively, in Appendix Table A.8, we measure inequality at the school-cohort-by-gender level. Since adolescents are more likely to interact with same-gender peers, this may better approximate their relevant comparison groups. In both cases, the results remain close to zero.

We also consider whether total wealth is too opaque to be noticed by adolescents. If so, inequality in total assets may not capture the dimension of economic differences that students actually perceive. We therefore re-estimate our models using inequality in housing wealth. Housing wealth is likely to be more visible because it is reflected in the size, location, and characteristics of peers' homes, and therefore provides a more salient indicator of differences in living standards. As reported in Appendix Table A.9, the results are similarly close to zero.

5.2.2 Outliers

As described in Section 3.2, some of our inequality measures exhibit large outliers, which could unduly influence our estimates. In Appendix Table A.10, we address this issue by excluding the top 2 percent of observations in terms of inequality in each regression. This results in some slightly larger, though still small, estimates that vary in sign across measures of inequality and outcomes. Overall, our main conclusions are robust to the exclusion of outliers.

5.2.3 Selection into schools

Another potential concern is that families sort into schools, or time school enrollment, based on expected peer inequality. Such sorting would bias our estimates if families that choose different peer environments also differ in unobserved ways that affect children's outcomes. Following the logic of sibling comparisons in Figlio et al. (2024), we address this concern by estimating sibling fixed-effects models that compare siblings from the same family who are exposed to different levels of peer inequality during adolescence. These models absorb all time-invariant family characteristics, including persistent parental preferences, resources, and school-choice tendencies. They therefore help assess whether our estimates are driven by selection of different types of families into schools with different levels of peer inequality. As shown in Appendix Table A.11, the sibling fixed-effects estimates broadly resemble our baseline results and remain near zero across most outcomes. This suggests that persistent family-level selection is unlikely to drive our findings.

5.2.4 Accounting for student background and average peer resources

To assess whether our estimates reflect differences in students' own family backgrounds or peers' average economic resources rather than peer inequality, we next augment the baseline specification with additional controls. Appendix Table A.12 reports regressions that add predetermined characteristics of the individual student: age in 9th grade, gender, immigrant background, mother's age at birth, family size, parental education, and parental income. Appendix Table A.13 instead adds peers' average parental income in the income-inequality regressions and peers' average parental wealth in the wealth-inequality regressions. The inclusion of these controls leaves our estimates essentially unchanged, consistent with the balancing evidence for predetermined student characteristics reported in Table 3.

5.3 Cumulative exposure and timing

Our baseline analysis measures peer inequality toward the end of compulsory schooling, when students are approaching consequential educational choices. While this is a natural point at which to study exposure, inequality experienced earlier in childhood or across multiple stages of schooling may have different effects on aspirations and educational investment. To investigate this possibility, we use a separate administrative dataset that allows us to observe students and their school peers in grades 3, 6, and 9.[13] We examine both average exposure across these three grades and the effects of exposure at each grade separately. The analysis focuses on students completing compulsory school between 2016 and 2023, for whom information on all three stages is available.

We first examine whether cumulative exposure to peer inequality affects educational outcomes. Table 8 reports estimates using inequality averaged across grades 3, 6, and 9, controlling for school fixed effects and school-specific linear trends at each stage. Although several coefficients are statistically significant, their magnitudes are generally small, providing little evidence that exposure to inequality across multiple stages of schooling meaningfully affects subsequent educational attainment.

We next investigate whether the effects of inequality depend on the timing of exposure. Table 9 includes inequality in grades 3, 6, and 9 jointly, allowing us to estimate the effect of exposure at each stage conditional on exposure at the others. For the 90:50 income ratio, grade-9 exposure is associated with small reductions in high school graduation, academic-track high school graduation, and university enrollment. However, the estimates are economically small, and no consistent pattern emerges across grades or inequality measures. Additional specifications examining exposure in grades 3 and 6 sep-

[13] These analyses use a separate administrative dataset that combines compulsory-school registers covering grade 3 from 2010 to 2023 and grades 6 and 9 from 2012 to 2023, together with information on subsequent educational outcomes.

arately (Appendix Tables A.14 and A.15), as well as inequality among high school peers (Appendix Table A.16), yield similarly small effects. Overall, we find little evidence that peer inequality has economically meaningful effects on educational investment, even when exposure occurs earlier in childhood or spans multiple stages of schooling.

6 Evidence from the United States

We next examine whether our findings from Sweden extend to the United States, where income inequality and returns to education are substantially higher. These differences may strengthen competing responses to peer inequality. On the one hand, exposure to more affluent peers may raise educational aspirations by making the potential rewards to human capital investment more salient. On the other hand, larger economic disparities may discourage students if they perceive the circumstances of their more advantaged peers as unattainable.

6.1 Empirical strategy and identifying variation

Our U.S. analysis follows the same general empirical approach as the Swedish analysis, but exploits a different source of variation. Because Add Health observes students in multiple grades within the same schools during the 1994–95 school year, we compare students attending different grades within the same school, rather than different cohorts attending the same school over time. We estimate Equation 1, treating grades as cohorts for notational consistency. Cohort fixed effects therefore correspond to grade fixed effects, while school-specific linear cohort trends correspond to school-specific linear grade profiles. Identification comes from variation in peer inequality across grades within schools, conditional on these controls. Similar within-school, across-grade designs have been used in previous studies of peer effects with Add Health data (e.g. Bifulco et al., 2011; Kiessling and Norris, 2023).

An important feature of the U.S. setting is the substantially greater dispersion of parental incomes among school peers than in Sweden. The mean 90:50 parental income ratio is 2.14 in Add Health, compared with 1.60 in Sweden. Thus, the income gap between the 90th percentile and the median, expressed relative to median income, is approximately 90 percent larger in the United States. The standard deviation of the 90:50 ratio after residualizing school and cohort fixed effects and school-specific linear trends is also considerably larger in Add Health (0.50 versus 0.16). Appendix Figure A.4 illustrates the substantial variation remaining across grades within schools, alongside the corresponding between-school variation. Although some of the greater residual variation in Add Health may reflect sampling noise in the measurement of peer inequality, the U.S. setting nevertheless provides an opportunity to study exposure to considerably larger

economic differences among peers.

Our identifying assumption is that, conditional on school and grade fixed effects and school-specific grade trends, differences in peer inequality across grades within schools are unrelated to other determinants of student outcomes. We assess its plausibility through balancing tests reported in Table 11.[14] Although several coefficients are statistically significant, their magnitudes are generally small. The results thus provide limited evidence of systematic sorting across school-grade cohorts. As a further robustness check, we show that the estimates for college enrollment and adult earnings remain positive and statistically significant when we control directly for predetermined student characteristics (Appendix Table A.21).

6.2 Results

Table 10 presents our main estimates. In contrast to the predominantly null findings from Sweden, exposure to greater peer income inequality in the United States has positive effects on several educational and labor-market outcomes, particularly for upper-tail inequality as measured by the 90:50 income ratio. A one-unit increase in this ratio raises high school GPA by 0.024 points and the probability of college enrollment by 1.9 percentage points, while the estimated effects on high school graduation and bachelor's degree completion are small and statistically insignificant. To interpret the magnitudes, a one-standard-deviation increase in the residualized 90:50 ratio implies an increase in college enrollment of 0.95 percentage points, equivalent to approximately 10 additional college entrants per 1,000 students. The corresponding effect on GPA is economically small.

We also find positive effects on adult earnings. The estimated coefficients on the 90:50 income ratio are 0.034, 0.035, and 0.045 for log earnings measured in Waves IV, V, and VI, respectively, all statistically significant. A one-standard-deviation increase in peer inequality therefore implies increases in annual earnings of approximately 1.7, 1.8, and 2.2 percent across the three waves. Evaluated at the corresponding sample means, these estimates correspond to approximately \$600, \$980, and \$1,590 in additional annual earnings, respectively. Positive effects are thus evident from young adulthood through middle age. Moreover, the earnings results are not confined to upper-tail inequality: in Wave VI, a one-standard-deviation increase in the 90:10 and 50:10 income ratios is estimated to raise earnings by approximately 3.4 and 2.2 percent, respectively. Allowing for non-linear effects, we find that the positive effects on both educational attainment and earnings are most pronounced among students exposed to the highest levels of peer inequality (Appendix Table A.17).

We also examine whether the effects differ by socioeconomic background. Appendix

[14] The balancing tests use the Wave IV education sample. Each regression includes observations with non-missing values for the predetermined characteristic used as the dependent variable, so sample sizes vary across outcomes.

Tables A.18, A.19, and A.20 report results by parental income rank and parental education. The clearest differences emerge by parental education. Among students without university-educated parents, a one-unit increase in the 90:50 ratio raises high school GPA by 0.102 points, college enrollment by 4.3 percentage points, and bachelor's degree completion by 2.5 percentage points. The corresponding estimates for students with university-educated parents are close to zero. The earnings estimates for students without university-educated parents are also positive, with statistically significant coefficients in Waves V and VI implying increases of approximately 3.3 and 3.9 percent for a one-standard-deviation increase in peer inequality. These subgroup patterns suggest that students from less-educated families may respond particularly strongly to exposure to affluent peers.

Overall, the U.S. results contrast with the predominantly null findings from Sweden. Greater exposure to peer income inequality, particularly upper-tail inequality, appears to encourage college enrollment and increase subsequent earnings, with suggestive evidence of stronger educational responses among students from less-educated families. One possible explanation is that the wider income distribution and higher returns to education in the United States make the economic rewards to educational investment more salient, so that upward aspirations outweigh potential discouragement effects. However, our analysis does not directly identify these mechanisms, and differences between Sweden and the United States cannot be attributed to inequality alone, given differences in their institutional settings, data, and empirical designs. The findings therefore suggest that the effects of exposure to peer inequality may depend importantly on the broader economic and social environment.

7 Conclusion

We study how exposure to economic inequality among school peers during adolescence and younger ages affects educational attainment and labor market outcomes. Using detailed Swedish register data, we construct measures of income and wealth inequality among school-cohort peers and exploit within-school, between-cohort variation to estimate plausibly causal effects. We find that exposure to peer income or wealth inequality does not meaningfully influence key outcomes, including high school graduation, university enrollment and completion, or adult income. These null effects are precisely estimated, robust across multiple definitions of inequality, and do not mask substantial differences in responses across socioeconomic groups. Importantly, they also do not appear to arise from an inability of the research design to detect peer effects: using the same identifying variation, we find meaningful effects of several other peer characteristics.

In contrast, our complementary analysis using U.S. Add Health data reveals positive associations between exposure to peer income inequality and educational outcomes,

particularly college enrollment, as well as higher earnings in adulthood. The educational responses are especially pronounced among students without university-educated parents. These findings suggest that the consequences of exposure to peer inequality may differ substantially across economic and institutional contexts.

Our findings have implications for school integration policy. Many countries use busing, catchment-area reforms, school choice, or related policies to increase socioeconomic mixing across schools. Such policies are often motivated by the potential benefits of exposing disadvantaged students to higher-achieving peers, better-resourced schools, and broader social networks. At the same time, greater socioeconomic mixing may increase students' exposure to differences in family resources and living standards, potentially generating unintended costs through social comparison, relative deprivation, or reduced aspirations. Our findings provide little evidence for this particular potential cost of socioeconomic integration. In Sweden, greater inequality among school peers does not appear to harm disadvantaged students' educational attainment or later-life economic outcomes. The positive effects observed in the United States further suggest that exposure to economic differences among peers need not discourage educational investment.

The contrast between Sweden and the United States may also help illuminate the mechanisms through which exposure to inequality affects human capital investment. In a setting with a wider income distribution and greater returns to human capital investment, such as the United States, differences in peers' economic circumstances may make the potential rewards to educational effort more salient, strengthening incentives to invest in human capital. In Sweden, the more compressed income distribution may weaken the incentive effects of exposure to inequality, while also limiting the scope for discouragement arising from seemingly unattainable economic outcomes. The predominantly null Swedish findings could therefore reflect weaker or offsetting mechanisms, whereas the positive U.S. associations are consistent with incentives and upward aspirations outweighing any discouragement effects. These interpretations remain tentative, however, since we cannot directly distinguish the underlying mechanisms, and the two countries differ in important respects beyond their income distributions.

More broadly, our findings suggest that direct exposure to socioeconomic inequality within schools does not necessarily undermine educational investment or subsequent economic outcomes. This is informative for theories emphasizing social comparison and economic despair, but does not imply that inequality operating through neighborhoods, labor markets, or broader societal conditions is without consequence. Schools provide a particularly relevant setting for examining these theories, however, as adolescents interact repeatedly with peers from different socioeconomic backgrounds and make consequential educational decisions during this period. Our findings highlight the importance of the broader economic and institutional environment in shaping responses to inequality.

Acknowledgments and Data Sources

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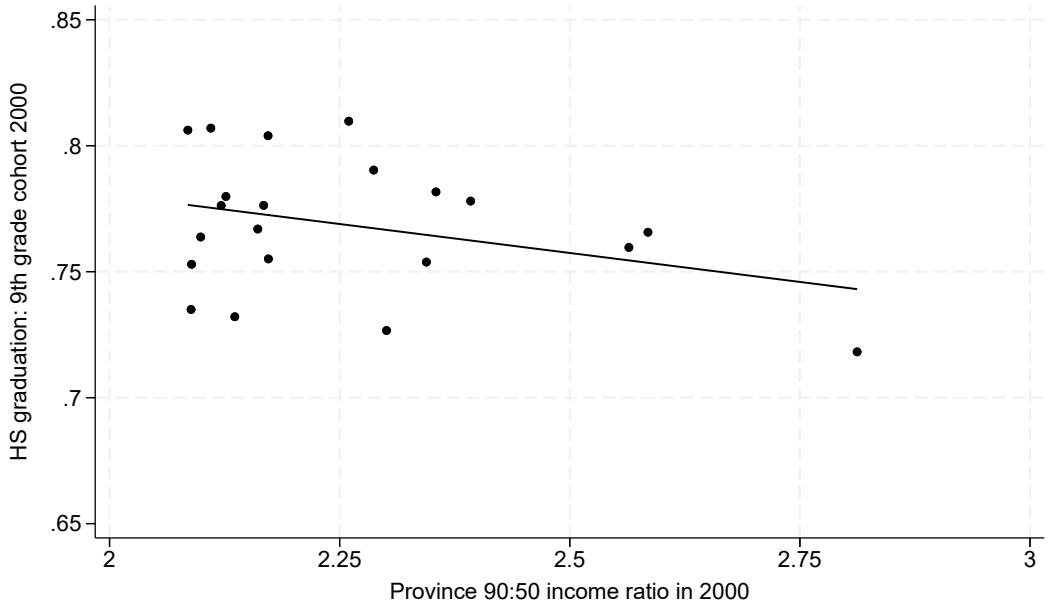
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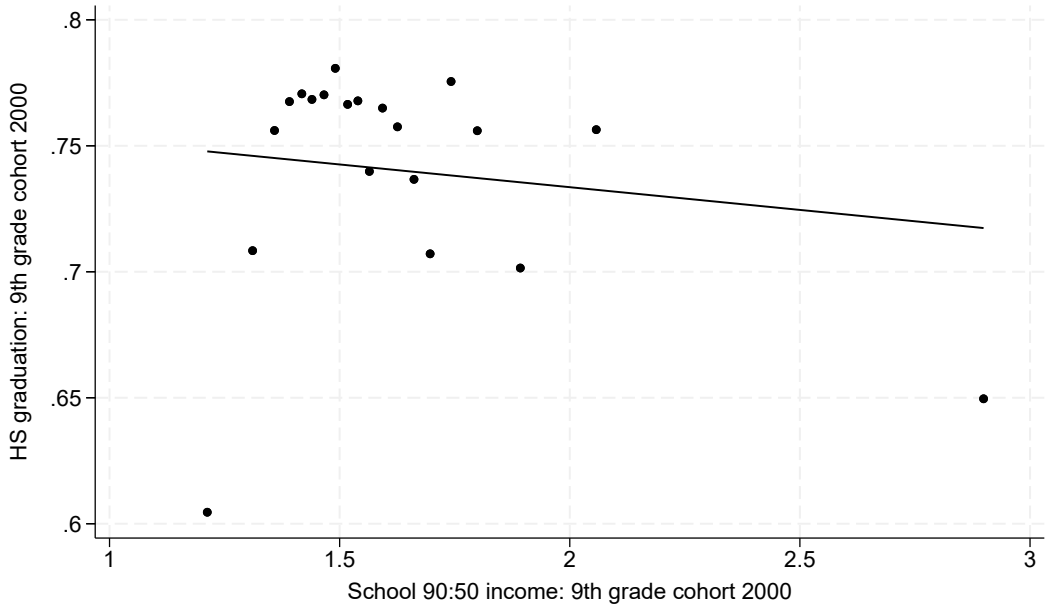
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Tables and Figures

Figure 1: Income inequality and high school graduation



(a) Panel A: By Swedish province



(b) Panel B: By school (binned)

Notes: Panel A plots the proportion of students graduating from high school among the 2000 9th grade cohort against income inequality in 2000 across Swedish provinces. Inequality is measured using the 90:50 income ratio among the population in each province. Panel B plots the proportion of students graduating from high school against school-cohort income inequality among the 2000 9th grade cohort. Inequality is measured using the 90:50 income ratio among school-cohort peers' parental income. Schools are categorized into 20 bins according to the 90:50 income ratio. High school graduation and income inequality are averaged across schools in each bin. In both figures, the solid line represents a linear fit of the relationship between high school graduation and income inequality.

Table 1: Summary statistics

	Income inequality sample (N=2,487,434)			Wealth inequality sample (N=959,659)		
	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median
	(1)	(2)	(3)	(4)	(5)	(6)
Socio-demographic characteristics						
Age in 9th grade	16.01	0.22	16.00	16.00	0.23	16.00
Female	0.49	0.50	0.00	0.49	0.50	0.00
Immigrant background	0.10	0.30	0.00	0.10	0.30	0.00
Mother's age at birth	28.12	5.07	28.00	28.48	5.09	28.00
Family size	2.83	1.39	3.00	3.06	1.36	3.00
Mother's years of education	12.11	2.46	12.00	12.30	2.40	12.00
Father's years of education	11.74	2.63	11.00	11.83	2.54	11.00
Parental income	442,898.03	709,219.84	392,032.56	469,757.67	987,817.93	417,698.47
Cohort level measures						
Cohort size	112.55	45.98	112.00	112.21	48.29	111.00
90:10 Income ratio	116.58	11,609.26	3.05			
90:50 Income ratio	1.60	0.28	1.54			
50:10 Income ratio	53.01	4,660.12	1.94			
90:50 Wealth ratio				35.08	1,509.62	2.55
National 90:10 Income ratio[a]	3.75	0.63	3.76			
National 90:50 Income ratio[a]	1.68	0.07	1.67			
National 50:10 Income ratio[a]	2.22	0.28	2.20			
National 90:50 Wealth ratio[a]				3.46	0.05	3.45
Outcome variables						
9th grade GPA (standardized)	0.03	0.98	0.08	0.04	0.97	0.08
Graduated from high school	0.80	0.40	1.00	0.79	0.41	1.00
Graduated from academic high school	0.45	0.50	0.00	0.43	0.50	0.00
Enrolled at university	0.48	0.50	0.00	0.49	0.50	0.00
Graduated from university	0.23	0.42	0.00	0.24	0.43	0.00
Average income age 28-30[b]	203,565.72	228,230.22	201,226.06	219,865.51	343,303.16	219,087.75
Average income age 33-35[b]	250,796.98	533,214.90	233,178.88			

Notes: The table shows summary statistics for the income inequality sample (columns 1-3) and wealth inequality sample (columns 4-6). Immigrant background is an indicator taking value 1 if an individual is a first- or second-generation immigrant and 0 otherwise. Family size is measured as the total number of children in an individual's family. Parental income is calculated as total parental income, averaged across the three years prior to the year in which a student completed 9th grade. Parental wealth is calculated as total parental wealth in the year prior to the year in which a student completed 9th grade. 9th grade GPA is standardized at the national cohort level.

[a] National measures are summarized at the cohort level. The number of observations is 25 for each income ratio and 9 for the 90:50 wealth ratio. [b] Average income at age 28-30 is only observed for selected cohorts, reducing the number of observations to 1,256,167 in the income inequality sample and 274,171 in the wealth inequality sample. Average income at age 33-35 is only observed for selected cohorts in the income inequality sample, reducing the number of observations to 800,388, and for none of the cohorts in the wealth inequality sample.

Table 2: Summary statistics for the Add Health sample

Sample:	W3 transcripts	W4 education	W4 income	W5 income	W6 income
	(1)	(2)	(3)	(4)	(5)
Socio-demographic characteristics					
Biological sex (0 = Male, 1 = Female)	0.53	0.53	0.51	0.56	0.56
Birth year	1978.94	1978.93	1978.93	1978.95	1978.95
1st or 2nd gen. immigrant (0/1)	0.17	0.16	0.16	0.16	0.16
Mother's age at birth	25.45	25.24	25.29	25.41	25.32
Number of HH members	4.61	4.61	4.59	4.55	4.59
Number of siblings (inc. focal child)	2.46	2.46	2.45	2.43	2.45
Mother completed high school (0/1)	0.84	0.83	0.84	0.85	0.84
Mother completed university (0/1)	0.30	0.28	0.29	0.30	0.30
Father completed high school (0/1)	0.85	0.84	0.84	0.85	0.85
Father completed university (0/1)	0.34	0.33	0.33	0.35	0.35
HH Income (USD)	47677.68	46792.40	47363.97	49100.15	48427.20
Cohort-level measures					
90:10 income ratio	6.69	6.75	6.72	6.71	6.72
90:10: SD	4.01	4.15	4.16	4.13	4.12
90:10: Median	5.77	5.80	5.77	5.67	5.71
90:50 income ratio	2.13	2.14	2.13	2.13	2.14
90:50: SD	0.72	0.75	0.75	0.78	0.79
90:50: Median	2.00	2.00	2.00	2.00	2.00
50:10 income ratio	3.07	3.09	3.08	3.08	3.07
50:10: SD	1.30	1.37	1.38	1.31	1.30
50:10: Median	2.81	2.85	2.83	2.81	2.81
Outcome variables					
High School GPA (from W3)	2.57	2.59	2.60	2.67	2.65
Graduated High School by W4 [inc. cert./GED]	0.96	0.94	0.95	0.95	0.95
Enrolled in University/College by W4	0.70	0.66	0.68	0.70	0.70
Completed Bachelor Degree by W4	0.33	0.30	0.31	0.34	0.34
Earned income at W4 (USD)	36204.02	35080.15	37673.17	35429.44	35482.29
Earned income at W5 (USD)	57444.07	55643.46	57223.42	55989.92	56908.14
Earned income at W6 (USD)	71684.01	69967.78	72057.20	72643.61	70077.22
N	11,704	15,041	13,691	11,612	11,266

Notes: The table shows summary statistics for AddHealth sample. Each column corresponds to the estimation sample used for the indicated Add Health outcome or outcome family. The W3 transcript sample is used for high-school GPA; the W4 education sample is used for high-school graduation, college enrollment, and bachelor's degree completion; and the W4, W5, and W6 income samples are used for earned-income regressions in the corresponding waves. Peer inequality measures are calculated within school-grade cells at baseline, excluding the respondent herself. The table reports means except where rows indicate standard deviations or medians. Standard deviations are unconditional sample standard deviations. Background-variable means are based on available non-missing observations within each column-specific sample and may therefore use fewer observations than the column. Immigrant background is an indicator taking value 1 if an individual is a first- or second-generation immigrant and 0 otherwise. Household members reflects the total number of people living in the child's household. Household income is reported by one parent of the child for the household as a whole.

Table 3: Balancing tests

	Age in 9th grade	Female	Immigrant background	Mother's age at birth	Family size	Mother's years of education	Father's years of education	Parental income	Parental wealth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
90:10 income ratio	0.00000008	0.00000003	0.00000007*	0.0000002	–0.00000005	–0.0000005***	–0.0000002	–0.05	
	(0.00000005)	(0.00000002)	(0.00000004)	(0.0000005)	(0.00000009)	(0.0000001)	(0.0000003)	(0.03)	
90:50 income ratio	–0.0004	0.001	0.004**	0.02	0.0004	0.001	–0.002	–11648.8*	
	(0.001)	(0.002)	(0.002)	(0.02)	(0.006)	(0.01)	(0.01)	(6414.3)	
50:10 income ratio	0.0000002	0.00000008	0.0000002***	0.0000004	–0.00000002	–0.000001***	–0.0000005	–0.08	
	(0.0000001)	(0.00000005)	(0.00000007)	(0.000001)	(0.0000002)	(0.0000002)	(0.0000007)	(0.07)	
90:50 wealth ratio	0.0000003	–0.0000002	0.0000002	0.000002	–0.00000008	–0.000005**	–0.000005	–0.06	–0.02
	(0.0000004)	(0.0000004)	(0.0000004)	(0.000003)	(0.000002)	(0.000002)	(0.000003)	(0.2)	(1.4)
N income ratio regressions	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	959,659	959,659	959,659	959,659

Notes: The table shows estimates of regressions of predetermined student characteristics on school-cohort-level income and wealth inequality. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 4: Exposure to inequality, human capital investment, and labor market outcomes: Descriptive evidence

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	–0.0000005***	–0.0000002***	–0.00000009***	–0.0000001***	–0.0000001***	0.0000003	–0.00002**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
90:50 income ratio	0.3***	–0.006	0.2***	0.2***	0.005	–0.05***	0.1***
	(0.023)	(0.005)	(0.011)	(0.009)	(0.006)	(0.013)	(0.015)
50:10 income ratio	–0.000001***	–0.0000005***	–0.0000002***	–0.0000003***	–0.0000004***	0.0000006	–0.00004**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
90:50 wealth ratio	–0.000007***	–0.000003***	–0.000002***	–0.000002***	–0.000001***	–0.00002	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
N income ratio regressions	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	1,256,167	800,388
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,171	

Notes: The table shows estimates of the association between exposure to income and wealth inequality and education and labor market outcomes. Each coefficient comes from a separate regression. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 5: Exposure to inequality, human capital investment, and labor market outcomes

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	–0.00000004	–0.000000003	–0.00000004	0.00000001	–0.000000009	0.00000009	0.00001**
	(0.0000001)	(0.00000004)	(0.00000003)	(0.00000002)	(0.00000002)	(0.0000004)	(0.000006)
90:50 income ratio	0.008	–0.002	0.003	0.005**	0.003*	–0.01	–0.01
	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)	(0.010)	(0.01)
50:10 income ratio	–0.00000006	–0.00000002	–0.00000008	0.00000004	–0.000000002	0.0000001	0.00002*
	(0.0000003)	(0.00000009)	(0.00000009)	(0.00000005)	(0.00000004)	(0.0000007)	(0.00001)
90:50 wealth ratio	0.0000009	–0.0000002	–0.0000006	–0.0000001	–0.0000004	0.000009	
	(0.0000008)	(0.0000002)	(0.0000004)	(0.0000003)	(0.0000003)	(0.000007)	
N income ratio regressions	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	1,256,167	800,388
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,171	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 6: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income and wealth rank

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < median	–0.00000008	–0.00000004	–0.00000004	0.000000006	–0.00000002	0.00002	0.00001
	(0.0000001)	(0.00000005)	(0.00000003)	(0.00000004)	(0.00000002)	(0.00002)	(0.000009)
Parental income > median	0.00000007	0.00000008**	–0.00000002	0.00000005	0.00000002	0.00005***	0.00003**
	(0.0000002)	(0.00000003)	(0.00000007)	(0.00000007)	(0.00000003)	(0.00002)	(0.00001)
90:50 income ratio:							
Parental income < median	0.01*	–0.001	0.006*	0.010***	0.003	0.005	–0.009
	(0.007)	(0.003)	(0.003)	(0.003)	(0.002)	(0.02)	(0.02)
Parental income > median	0.002	–0.003	0.0009	0.003	0.003	–0.04***	–0.04*
	(0.006)	(0.002)	(0.003)	(0.003)	(0.003)	(0.01)	(0.02)
50:10 income ratio:							
Parental income < median	–0.0000002	–0.0000002	–0.0000001	0.00000004	–0.00000007	0.00002	0.00002
	(0.0000004)	(0.0000001)	(0.0000001)	(0.0000001)	(0.00000005)	(0.00003)	(0.00002)
Parental income > median	0.00000009	0.0000001*	–0.00000002	0.00000006	0.00000006	0.00009**	0.00005**
	(0.0000004)	(0.00000007)	(0.0000002)	(0.0000001)	(0.00000007)	(0.00004)	(0.00002)
90:50 wealth ratio:							
Parental wealth < median	0.000002	0.0000007**	–0.0000006	0.0000004	–0.0000006	–0.0000003	
	(0.000002)	(0.0000004)	(0.0000004)	(0.0000005)	(0.0000004)	(0.000003)	
Parental wealth > median	0.000002***	–0.0000007	–0.000001***	–0.000001*	0.0000002	–0.00003*	
	(0.0000007)	(0.0000005)	(0.0000004)	(0.0000006)	(0.0000009)	(0.00002)	
N income ratio	2,181,453	2,181,453	2,181,453	2,181,453	2,181,453	1,000,280	575,273
N wealth ratio	839,106	839,106	839,106	839,106	839,106	179,001	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes, separately for students with parental income and wealth below and above the school-cohort median. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 7: Exposure to inequality and education and labor market outcomes: non-linear effects

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Q2-3	0.003	0.002*	0.003***	0.003**	0.002*	–0.005*	0.004
	(0.002)	(0.001)	(0.001)	(0.001)	(0.0010)	(0.003)	(0.003)
Q4	–0.004	–0.001	–0.0001	0.002	0.003**	–0.005	–0.0005
	(0.004)	(0.002)	(0.002)	(0.002)	(0.001)	(0.005)	(0.006)
90:50 income ratio:							
Q2-3	0.005**	0.004***	0.005***	0.003**	0.0007	–0.0003	0.003
	(0.003)	(0.001)	(0.001)	(0.001)	(0.0010)	(0.003)	(0.003)
Q4	0.003	0.0005	0.003	0.005***	0.004***	–0.007	–0.010*
	(0.004)	(0.002)	(0.002)	(0.002)	(0.001)	(0.004)	(0.006)
50:10 income ratio:							
Q2-3	0.0007	0.001	0.002	0.0009	0.001	–0.003	0.003
	(0.003)	(0.0010)	(0.001)	(0.001)	(0.0009)	(0.003)	(0.003)
Q4	–0.009**	–0.0010	–0.002	–0.001	0.002	0.001	0.003
	(0.004)	(0.001)	(0.002)	(0.002)	(0.001)	(0.005)	(0.006)
90:50 wealth ratio:							
Q2-3	–0.007	–0.002	–0.001	–0.004*	–0.004**	–0.02*	
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.01)	
Q4	–0.01*	–0.002	0.0003	–0.0010	–0.002	0.005	
	(0.007)	(0.003)	(0.003)	(0.003)	(0.003)	(0.02)	
N income ratio regressions	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	1,256,167	800,388
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,171	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. For these regressions, we classify school-cohort inequality into quartiles and include two indicators for middle (Q2-3) and high (Q4) inequality as our main independent variables, using the lowest quartile (Q1) as the reference group. Means of school-cohort-level inequality for Q1/Q2-3/Q4 are: 2.21/3.05/372.23 (90:10 income ratio), 1.37/1.56/1.96 (90:50 income ratio), 1.51/1.93/148.85 (50:10 income ratio), 1.87/2.63/127.69 (90:50 wealth ratio). All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 8: Impacts of exposure to average inequality during Grades 3, 6, and 9

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university
	(1)	(2)	(3)	(4)
90:10 income ratio	0.0000003	–0.0000002	–0.00000007	–0.0000008***
	(0.0000005)	(0.0000003)	(0.0000004)	(0.0000002)
90:50 income ratio	0.002	–0.005***	–0.004***	–0.004**
	(0.002)	(0.001)	(0.002)	(0.001)
50:10 income ratio	0.0000005	–0.0000002	0.00000002	–0.000002***
	(0.000001)	(0.0000005)	(0.0000007)	(0.0000002)
Observations	503,178	323,177	323,177	323,177

Notes: The table shows estimates of the effects of average exposure to income inequality across Grades 3, 6, and 9 on education outcomes. Each coefficient comes from a separate regression. All regressions control for cohort fixed effects and school fixed effects and school-specific linear trends for the schools attended in Grades 3, 6, and 9. Standard errors, clustered by school, are reported in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 9: Impacts of exposure to inequality during G3, G6 and G9 – timing

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university
	(1)	(2)	(3)	(4)
90:10 income ratio - G9	0.0000004***	0.0000002**	0.0000003***	–0.0000004***
	(0.0000001)	(0.0000001)	(0.00000003)	(0.00000002)
90:10 income ratio - G6	0.0000005	–0.0000002**	0.00000007	–0.0000002
	(0.0000003)	(0.0000001)	(0.0000004)	(0.0000001)
90:10 income ratio - G3	–0.00000002	–0.0000002	–0.0000002**	–0.0000003**
	(0.0000002)	(0.0000001)	(0.00000009)	(0.0000001)
90:50 income ratio - G9	–0.0009	–0.004***	–0.003***	–0.002***
	(0.001)	(0.0007)	(0.0008)	(0.0008)
90:50 income ratio - G6	–0.003	0.006*	0.002	0.001
	(0.006)	(0.003)	(0.004)	(0.004)
90:50 income ratio - G3	0.005**	–0.0004	0.001	–0.0007
	(0.002)	(0.001)	(0.002)	(0.002)
50:10 income ratio - G9	0.0000005***	0.0000002**	0.0000004***	–0.0000005***
	(0.0000001)	(0.0000001)	(0.00000005)	(0.00000003)
50:10 income ratio - G6	0.0000009	–0.0000004**	–0.0000001	–0.0000004**
	(0.0000006)	(0.0000002)	(0.0000007)	(0.0000002)
50:10 income ratio - G3	–0.0000001	–0.0000005	–0.0000005***	–0.0000006***
	(0.0000005)	(0.0000003)	(0.00000019)	(0.0000002)
Observations	503,178	323,177	323,177	323,177

Notes: The table shows estimates of the effects of exposure to income inequality in Grades 3, 6, and 9 on education outcomes. For each inequality measure and outcome, exposures in Grades 3, 6, and 9 are included jointly in the same regression. All regressions control for cohort fixed effects and school fixed effects and school-specific linear trends for the schools attended in Grades 3, 6, and 9. Standard errors, clustered by school, are reported in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Table 10: Exposure to inequality, human capital investment, and labor market outcomes - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	0.002	0.001	0.003	0.002	0.000	0.005	0.013***
	(0.004)	(0.0007)	(0.002)	(0.001)	(0.004)	(0.004)	(0.003)
90:50 income ratio	0.024**	0.007	0.019***	0.003	0.034**	0.035*	0.045***
	(0.01)	(0.005)	(0.007)	(0.005)	(0.02)	(0.02)	(0.01)
50:10 income ratio	–0.000	–0.000	0.004	0.004	–0.006	–0.002	0.024**
	(0.01)	(0.002)	(0.004)	(0.004)	(0.008)	(0.01)	(0.009)
N	11,704	15,041	15,041	15,041	13,691	11,612	11,266

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

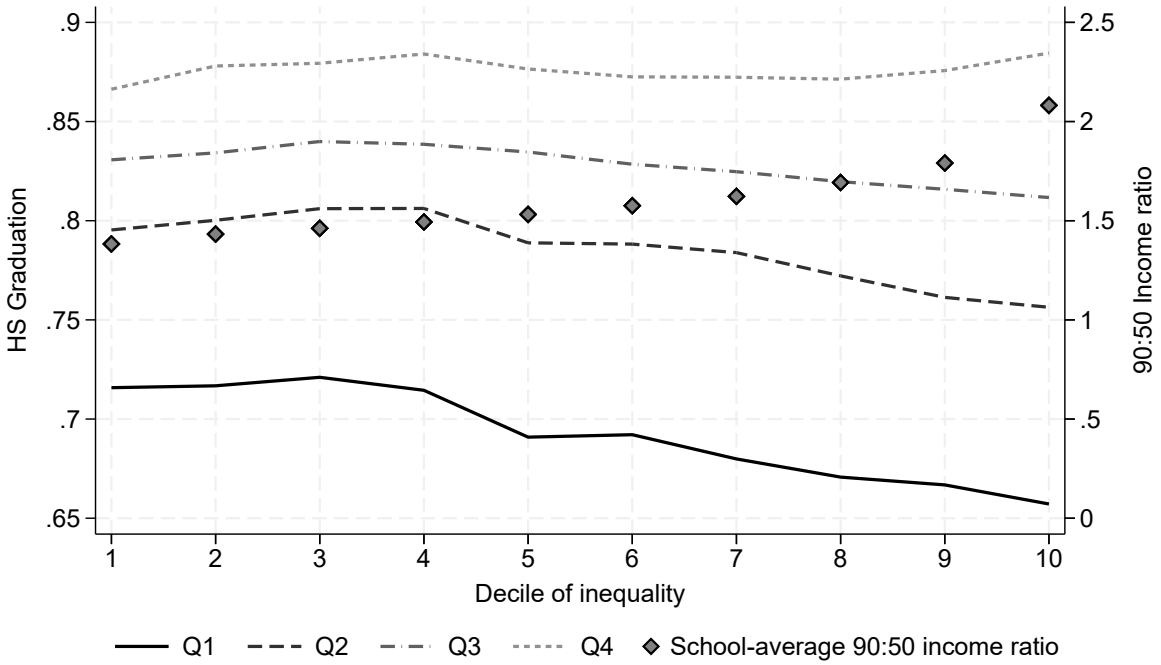
Table 11: Balancing tests - Add Health

	Birth year	Female	Immigrant status	Mother's age at birth	Family size	Mother HS	Mother Uni.	Father HS	Father Uni.	Parental Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
90:10 income ratio	0.003	0.001	0.00009	0.04**	–0.006	–0.0008	0.004**	0.002	0.0008	–314.8
	(0.002)	(0.002)	(0.001)	(0.02)	(0.004)	(0.001)	(0.002)	(0.001)	(0.002)	(211.3)
90:50 income ratio	0.03**	0.01**	–0.008	0.1	–0.02	0.007	0.02**	0.010*	0.01	–1654.0
	(0.01)	(0.007)	(0.005)	(0.08)	(0.02)	(0.007)	(0.007)	(0.006)	(0.009)	(1554.0)
50:10 income ratio	–0.004	–0.0003	0.002	0.09**	–0.01	–0.003	0.005	0.002	–0.005	–104.6
	(0.008)	(0.004)	(0.004)	(0.04)	(0.01)	(0.003)	(0.004)	(0.004)	(0.005)	(772.4)
N	15,036	15,041	15,041	13,404	15,041	13,549	13,549	9,971	9,971	11,397

Notes: The table shows estimates of regressions of predetermined student characteristics on school-cohort-level income inequality. Each coefficient comes from a separate regression. The sample used for this analysis is that for which we observe education outcomes in Wave 4. Sample sizes vary due to missing values in the outcome variables for each balancing test. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

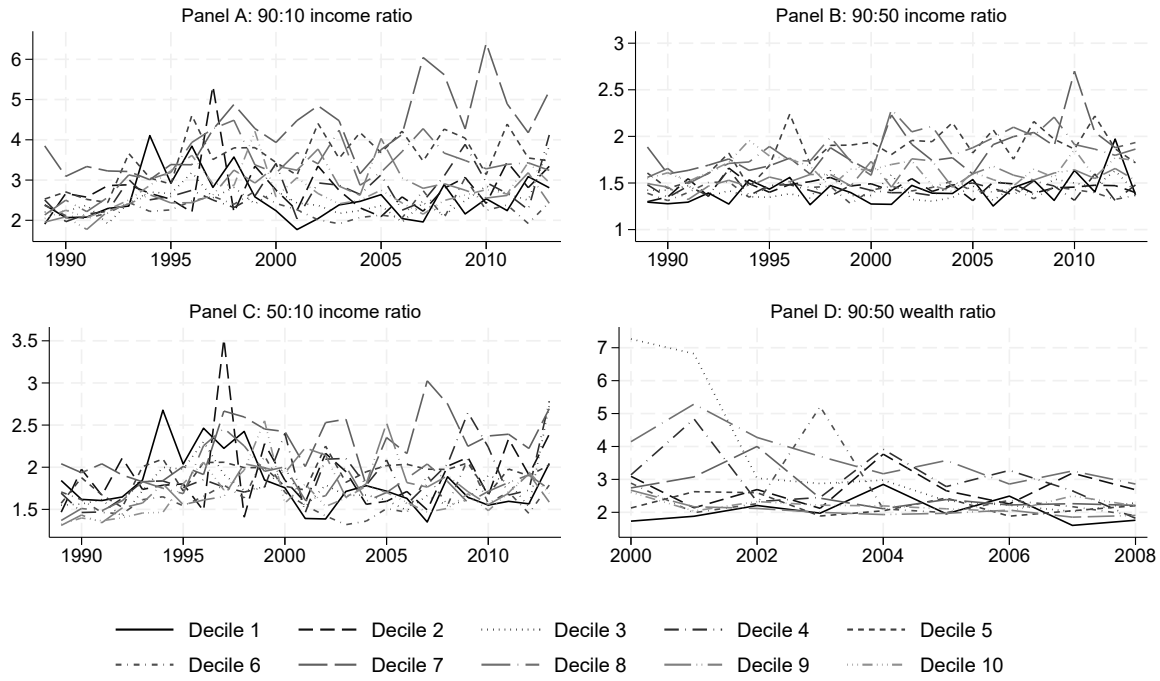
A Additional Results

Appendix Figure A.1: High school graduation by socioeconomic background and income inequality



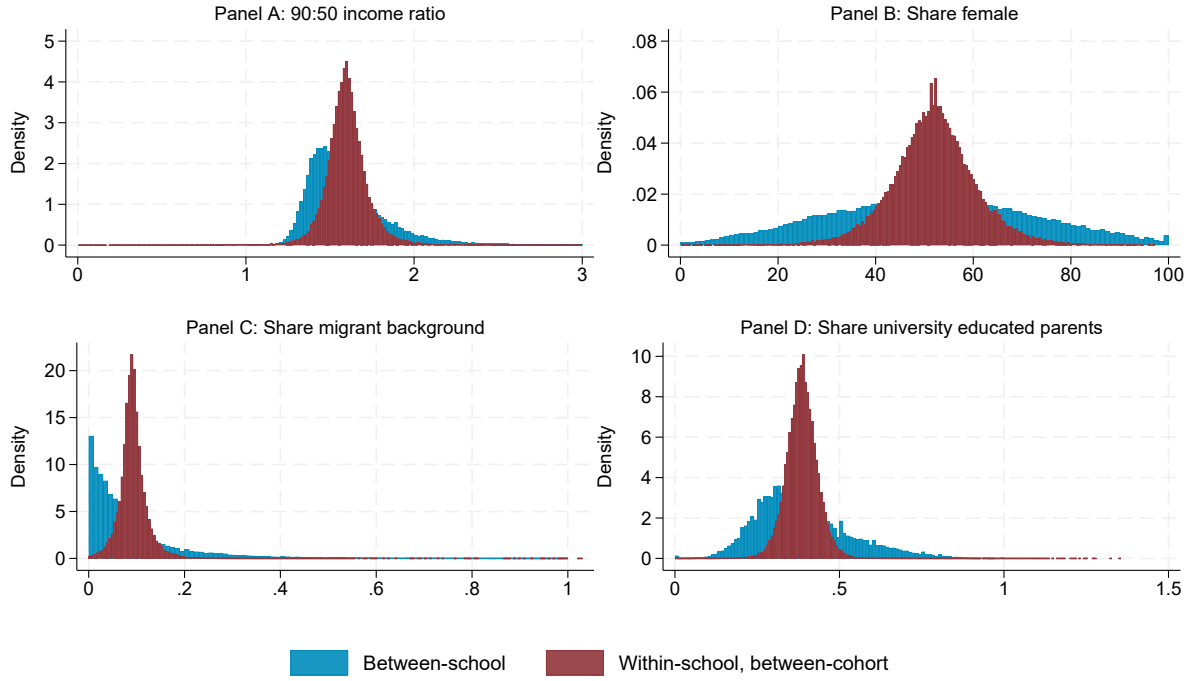
Notes: The figure presents the proportion of students graduating from high school according to socioeconomic background and income inequality. Socioeconomic background is measured in quartiles of parental income. Inequality is measured in deciles of the school-average 90:50 income ratio in the compulsory schools students attended. Lines plot the relationship between high school graduation and income inequality in compulsory school, separately for each quartile of socioeconomic background. Diamonds plot the average 90:50 income ratio for schools in each decile of inequality.

Appendix Figure A.2: Time series of income and wealth inequality for 10 schools



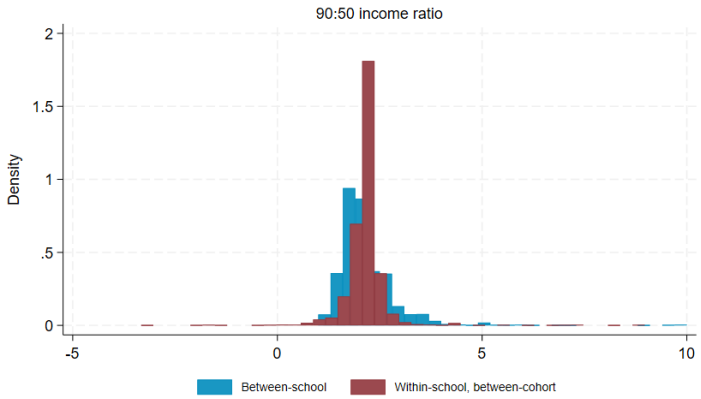
Notes: The figure shows the evolution of income and wealth inequality for ten schools in our sample. To select these schools, we first divided schools into deciles according to their average cohort size across all years. We then randomly selected one school from each decile. The graph plots the 90:10 income ratio (upper left panel), 90:50 income ratio (upper right panel), 50:10 income ratio (lower left panel), and 90:50 wealth ratio (lower right panel) for each of these schools.

Appendix Figure A.3: Within- and between-school variation



Notes: The figure compares across-school variation to within-school, between-cohort variation in the 90:50 income ratio, share of female peers, share of immigrant peers, and share of university-educated parents. Within-school, between-cohort variation is represented by residuals of the raw variables, net of school and cohort fixed effects and school-specific linear trends. Residuals are added to the sample mean for comparability.

Appendix Figure A.4: Within- and across-grade variation - AddHealth Data



Notes: The figure compares across-school variation to within-school, across-grade variation in the 90:50 income ratio. Within-school, across-grade variation is represented by residuals of the raw variables, net of school and cohort fixed effects and school-specific linear trends. Residuals are added to the sample mean for comparability.

Appendix Table A.1: Balancing tests: Lagged inequality

	Age in 9th grade	Female	Immigrant background	Mother's age at birth	Family size	Mother's years of education	Father's years of education	Parental income	Parental wealth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
90:10 income ratio	0.0000002***	0.00000004***	0.0000002***	–0.00000006	0.0000004***	–0.0000006***	–0.00000005	–0.06***	
	(0.00000001)	(0.000000009)	(0.00000003)	(0.0000004)	(0.00000008)	(0.00000005)	(0.00000008)	(0.01)	
90:50 income ratio	–0.001	–0.002	0.001	0.01	0.003	–0.01	–0.02	–21378.8*	
	(0.001)	(0.002)	(0.001)	(0.02)	(0.006)	(0.01)	(0.01)	(11664.5)	
50:10 income ratio	0.0000003***	0.00000006***	0.0000003***	0.0000001	0.0000006**	–0.0000010***	–0.0000002	–0.1***	
	(0.00000003)	(0.00000002)	(0.00000006)	(0.0000009)	(0.0000002)	(0.0000001)	(0.0000002)	(0.03)	
90:50 wealth ratio	0.0000003	0.000003*	–0.000003*	0.000007	–0.000002	0.000007	0.000001	–0.4	0.6
	(0.000001)	(0.000001)	(0.000002)	(0.000007)	(0.000009)	(0.000006)	(0.000003)	(0.9)	(4.8)
N income ratio regressions	2,268,945	2,268,945	2,268,945	2,268,945	2,268,945	2,268,945	2,268,945	2,268,945	2,268,945
N wealth ratio regression	833,956	833,956	833,956	833,956	833,956	833,956	833,956	833,956	833,956

Notes: The table shows estimates of regressions of predetermined student characteristics on lagged school-cohort-level income and wealth inequality. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.2: Peer effects in other dimensions

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share female	–0.015	0.007	0.013*	0.004	–0.001	0.073***	0.112***
	(0.017)	(0.006)	(0.007)	(0.006)	(0.005)	(0.020)	(0.025)
Share immigrant backg.	–0.107***	–0.076***	–0.078***	–0.067***	–0.034***	–0.045	0.055
	(0.037)	(0.012)	(0.014)	(0.012)	(0.010)	(0.037)	(0.053)
Share parental university education	0.05**	0.04***	0.05***	0.05***	0.04***	–0.03	–0.06**
	(0.021)	(0.007)	(0.009)	(0.008)	(0.007)	(0.022)	(0.027)
(Predicted) Ability	0.015	0.056***	0.069***	0.066***	0.049***	0.026	–0.002
	(0.030)	(0.009)	(0.013)	(0.011)	(0.009)	(0.027)	(0.033)
N	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	1,256,167	800,388

Notes: The table shows estimates of the effects of exposure to various peer effects on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Predicted ability is estimated using the fitted values from a regression of 9th grade GPA on sex, immigrant status, mother's and father's highest level of education and school fixed effects. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.3: Exposure to inequality and education and labor market outcomes: heterogeneity by parental income and wealth rank (national distributions)

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < median	–0.00000001	–0.000000009	–0.00000001	0.00000001	–0.000000004	0.00001	0.00001*
	(0.0000001)	(0.00000003)	(0.00000003)	(0.00000003)	(0.00000001)	(0.00001)	(0.000008)
Parental income > median	0.0000001	0.0000001*	–0.0000001	0.00000007	–0.000000005	0.00010**	0.000009
	(0.0000002)	(0.00000008)	(0.0000001)	(0.0000001)	(0.00000004)	(0.00004)	(0.00001)
90:50 income ratio:							
Parental income < median	0.01	–0.0003	0.007*	0.007**	0.001	–0.01	–0.002
	(0.008)	(0.003)	(0.003)	(0.003)	(0.002)	(0.02)	(0.02)
Parental income > median	0.004	–0.003	0.0003	0.003	0.004	–0.02*	–0.05**
	(0.006)	(0.002)	(0.003)	(0.003)	(0.003)	(0.01)	(0.02)
50:10 income ratio:							
Parental income < median	0.00000006	–0.00000002	0.00000001	0.00000007	–0.000000009	0.00002	0.00002*
	(0.0000003)	(0.00000008)	(0.00000009)	(0.00000007)	(0.00000004)	(0.00002)	(0.00001)
Parental income > median	0.00000005	0.0000002	–0.0000003	–0.0000001	–0.000000004	0.0001*	0.00002
	(0.0000006)	(0.0000002)	(0.0000003)	(0.0000002)	(0.0000001)	(0.00008)	(0.00002)
90:50 wealth ratio:							
Parental wealth < median	0.000002	0.0000007**	–0.0000006	0.0000004	–0.0000006	–0.0000003	
	(0.000002)	(0.0000004)	(0.0000004)	(0.0000005)	(0.0000004)	(0.000003)	
Parental wealth > median	0.000002***	–0.0000007	–0.000001***	–0.000001*	0.0000002	–0.00003*	
	(0.0000007)	(0.0000005)	(0.0000004)	(0.0000006)	(0.0000009)	(0.00002)	
N income ratio	2,181,444	2,181,444	2,181,444	2,181,444	2,181,444	1,000,273	575,270
N wealth ratio	839,106	839,106	839,106	839,106	839,106	179,001	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes, separately for students with parental income and wealth below and above the national median. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.4: Exposure to inequality and education and labor market outcomes: heterogeneity by parental education

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Less than university	–0.00000006	0.00000003	–0.00000001	0.00000005*	–0.000000001	0.00002	0.000003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
University	0.0000001	–0.0000001	–0.00000007	–0.00000004	–0.000000007	0.0000003	0.00003**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
90:50 income ratio:							
Less than university	0.005	–0.002	0.004	0.006**	0.003	–0.02	–0.01
	(0.007)	(0.003)	(0.003)	(0.003)	(0.002)	(0.012)	(0.015)
University	0.006	–0.003*	0.0007	0.002	0.002	–0.01	–0.02
	(0.006)	(0.002)	(0.003)	(0.003)	(0.002)	(0.014)	(0.021)
50:10 income ratio:							
Less than university	–0.0000001	0.00000005	–0.00000003	0.0000001**	0.000000009	0.00002	0.000003
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
University	0.0000004	–0.0000002	–0.0000001	–0.00000006	6.15187e-10	0.0000006	0.00006**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
90:50 wealth ratio:							
Less than university	0.000001	–0.0000004	–0.0000005	–0.0000004	–0.0000001	0.000008	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
University	–0.0000009	0.0000009	–0.0000008	0.000001	–0.000001*	0.00001	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	
N income ratio	2,025,916	2,025,916	2,025,916	2,025,916	2,025,916	945,276	578,366
N wealth ratio	959,598	959,598	959,598	959,598	959,598	274,123	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. The sample is split into two groups: students with at least one parent who completed university, and students with no university-educated parent. The sample size is slightly reduced due to missing information on parental education. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.5: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income and wealth rank in low-income schools

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < median	–0.000000010	–0.00000006	–0.00000006	0.000000006	–0.00000004	0.00002	0.00001
	(0.0000002)	(0.00000006)	(0.00000004)	(0.00000006)	(0.00000002)	(0.00002)	(0.000009)
Parental income > median	0.00000009	0.00000009***	0.00000002	–0.00000001	0.00000003	0.00004**	0.00003**
	(0.0000002)	(0.00000003)	(0.00000009)	(0.00000007)	(0.00000003)	(0.00002)	(0.00001)
90:50 income ratio:							
Parental income < median	0.02	0.01*	0.010	0.01*	0.0006	–0.007	0.03
	(0.02)	(0.007)	(0.007)	(0.007)	(0.005)	(0.02)	(0.02)
Parental income > median	–0.02	–0.01*	–0.01**	–0.01*	–0.01***	–0.03*	0.0008
	(0.02)	(0.006)	(0.007)	(0.006)	(0.005)	(0.02)	(0.03)
50:10 income ratio:							
Parental income < median	–0.00000006	–0.0000001	–0.0000001	0.00000003	–0.00000009	0.00002	0.00002
	(0.0000003)	(0.0000002)	(0.0000001)	(0.0000001)	(0.00000006)	(0.00003)	(0.00002)
Parental income > median	0.0000002	0.0000002**	0.00000007	–0.00000002	0.00000006	0.00006*	0.00005**
	(0.0000004)	(0.00000007)	(0.0000002)	(0.0000001)	(0.00000006)	(0.00003)	(0.00002)
90:50 wealth ratio:							
Parental wealth < median	0.000002	0.0000006	–0.0000006	0.0000002	–0.0000007*	–0.0000002	
	(0.000002)	(0.0000004)	(0.0000005)	(0.0000005)	(0.0000004)	(0.000003)	
Parental wealth > median	0.000002***	–0.0000006	–0.000001***	–0.000001*	0.0000003	–0.00007	
	(0.0000007)	(0.0000005)	(0.0000004)	(0.0000006)	(0.0000010)	(0.0002)	
N income ratio	1,018,254	1,018,254	1,018,254	1,018,254	1,018,254	759,195	480,572
N wealth ratio	403,729	403,729	403,729	403,729	403,729	120,911	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes, separately for students with parental income and wealth below and above the school-cohort median. This analysis is conducted among school-cohorts with below median average parental income. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.6: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income and wealth rank in high-income schools

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < median	–0.0000002***	0.00000009***	0.000000007***	–0.000000003**	–0.00000001***	0.0009***	0.004
	(0.000000007)	(0.000000001)	(0.000000001)	(0.000000002)	(0.000000001)	(0.00003)	(0.01)
Parental income > median	0.0000003***	0.0000002***	0.000000004	0.0000003***	–0.00000002***	0.001	–0.009**
	(0.000000009)	(0.00000001)	(0.00000001)	(0.00000002)	(0.000000006)	(0.001)	(0.004)
90:50 income ratio:							
Parental income < median	0.009	–0.007*	0.003	0.007	0.002	0.01	–0.02
	(0.009)	(0.004)	(0.004)	(0.004)	(0.002)	(0.03)	(0.04)
Parental income > median	–0.003	–0.003	–0.003	–0.003	–0.001	–0.04*	–0.2***
	(0.007)	(0.002)	(0.003)	(0.003)	(0.003)	(0.02)	(0.04)
50:10 income ratio:							
Parental income < median	–0.000002***	0.000001***	0.00000006	0.000000003	–0.0000001***	0.003***	0.02
	(0.0000003)	(0.00000005)	(0.00000003)	(0.00000006)	(0.00000003)	(0.0001)	(0.03)
Parental income > median	0.000002***	0.000000005	–0.0000002	0.000002**	–0.0000003*	0.001	0.004
	(0.0000003)	(0.0000004)	(0.0000004)	(0.0000006)	(0.0000002)	(0.003)	(0.01)
90:50 wealth ratio:							
Parental wealth < median	0.0002***	0.0001***	0.000003	–0.00007***	0.000005	0.007***	
	(0.00004)	(0.00002)	(0.00001)	(0.00001)	(0.000009)	(0.0005)	
Parental wealth > median	0.00005***	0.00003***	0.00004***	0.00002*	0.00001**	–0.00004***	
	(0.00001)	(0.000003)	(0.000008)	(0.00001)	(0.000005)	(0.000005)	
N income ratio	1,163,139	1,163,139	1,163,139	1,163,139	1,163,139	241,016	49,066
N wealth ratio	435,348	435,348	435,348	435,348	435,348	58,033	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes, separately for students with parental income and wealth below and above the school-cohort median. This analysis is conducted among school-cohorts with above median average parental income. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.7: Exposure to inequality and education and labor market outcomes: robustness to restricting the sample to schools with a maximum cohort size of 120

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	0.00000002	–0.000000005	–0.00000003	0.00000001	–0.000000004	–0.0000001	0.00002
	(0.0000002)	(0.00000005)	(0.00000003)	(0.00000003)	(0.00000001)	(0.0000002)	(0.00001)
90:50 income ratio	0.004	–0.003	–0.0009	–0.001	0.0001	–0.01	–0.004
	(0.006)	(0.002)	(0.003)	(0.002)	(0.002)	(0.02)	(0.02)
50:10 income ratio	0.0000001	–0.00000005	–0.00000004	0.00000005	0.000000008	–0.0000002	0.00003
	(0.0000004)	(0.0000001)	(0.0000001)	(0.00000008)	(0.00000005)	(0.0000004)	(0.00002)
90:50 wealth ratio	0.000001	–0.0000002	–0.0000003	0.0000002	–0.0000001	0.000009	
	(0.0000009)	(0.0000003)	(0.0000004)	(0.0000003)	(0.0000002)	(0.000007)	
N income ratio regressions	830,104	830,104	830,104	830,104	830,104	352,621	211,632
N wealth ratio regression	407,550	407,550	407,550	407,550	407,550	110,358	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. The sample is restricted to schools which have a maximum 9th-grade cohort size of 120 across all years. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.8: Exposure to inequality and education and labor market outcomes: robustness to measuring inequality at the cohort*gender level

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	0.0000001**	0.00000002	0.00000004	0.000000010	0.00000002	0.00000006	0.00001
	(0.00000006)	(0.00000002)	(0.00000003)	(0.00000003)	(0.00000002)	(0.0000002)	(0.00001)
90:50 income ratio	0.008**	0.0001	0.004***	0.002	0.002	0.005	–0.01
	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)	(0.010)
50:10 income ratio	0.0000003**	0.00000004	0.00000007	0.00000001	0.00000005	0.00000003	0.00002
	(0.0000001)	(0.00000004)	(0.00000006)	(0.00000007)	(0.00000004)	(0.0000003)	(0.00002)
90:50 wealth ratio	–0.0000005	–0.0000003	–0.0000004***	–0.0000002*	–0.0000002**	–0.0000003	
	(0.0000004)	(0.0000002)	(0.0000001)	(0.0000001)	(0.0000001)	(0.0000007)	
N income ratio regressions	2,471,335	2,471,335	2,471,335	2,471,335	2,471,335	1,250,614	797,084
N wealth ratio regression	957,344	957,344	957,344	957,344	957,344	273,413	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. The sample size is slightly reduced as income ratios could not be calculated for some school*gender groups. For these regressions, we compute all inequality measures at the school-cohort-gender level. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.9: Exposure to inequality, human capital investment, and labor market outcomes: Housing wealth

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)
	(1)	(2)	(3)	(4)	(5)	(6)
90:50 housing wealth ratio	0.00003***	0.000005**	0.000004**	0.00001***	0.000005***	0.00002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	918,974	918,974	918,974	918,974	918,974	262,113

Notes: The table shows estimates of the effects of exposure to housing wealth inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.10: Exposure to inequality and education and labor market outcomes: excluding top 2% of the inequality distribution

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	–0.002***	–0.001***	–0.0009***	–0.0003	–0.00009	–0.002*	0.001
	(0.0006)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0009)	(0.001)
N	2,437,692	2,437,692	2,437,692	2,437,692	2,437,692	1,239,584	793,819
90:50 income ratio	0.01	0.003	0.010***	0.01***	0.008***	–0.02	–0.01
	(0.009)	(0.003)	(0.004)	(0.003)	(0.003)	(0.010)	(0.01)
N	2,437,688	2,437,688	2,437,688	2,437,688	2,437,688	1,244,273	797,319
50:10 income ratio	–0.003***	–0.002***	–0.002***	–0.0006	–0.0002	–0.002	0.003
	(0.001)	(0.0005)	(0.0005)	(0.0005)	(0.0003)	(0.002)	(0.002)
N	2,437,739	2,437,739	2,437,739	2,437,739	2,437,739	1,239,662	793,801
90:50 wealth ratio	–0.001	0.000007	–0.0005	–0.0006	–0.0005	0.005***	
	(0.0009)	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.002)	
N	940,466	940,466	940,466	940,466	940,466	268,956	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. The samples in these regressions exclude the top 2 percent of observations in terms of the relevant inequality measure. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.11: Exposure to inequality, human capital investment, and labor market outcomes: Family fixed effects

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	–0.00000003	0.00000001	0.00000002	0.00000004	0.00000002	0.00002	0.00002**
	(0.0000001)	(0.00000005)	(0.00000007)	(0.00000008)	(0.00000002)	(0.00001)	(0.00001)
90:50 income ratio	0.005	–0.0002	0.006*	0.005*	–0.003	–0.009	–0.01
	(0.006)	(0.002)	(0.003)	(0.003)	(0.003)	(0.01)	(0.02)
50:10 income ratio	0.000000007	0.00000005	0.00000006	0.00000009	0.00000007	0.00003	0.00004
	(0.0000003)	(0.0000001)	(0.0000002)	(0.0000002)	(0.00000005)	(0.00003)	(0.00002)
90:50 wealth ratio	0.000003**	0.0000005	–0.0000004	0.0000007	–0.00000008	0.0003	
	(0.000001)	(0.0000005)	(0.0000006)	(0.0000006)	(0.0000004)	(0.0002)	
N income ratio regressions	1,852,689	1,852,689	1,852,689	1,852,689	1,852,689	791,580	409,537
N wealth ratio regression	578,526	578,526	578,526	578,526	578,526	43,233	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for family fixed effects, school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.12: Exposure to inequality and education and labor market outcomes: controlling for background characteristics

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	0.00000003	0.00000002	–0.00000001	0.00000004	0.000000005	0.0000003	0.00002***
	(0.0000001)	(0.00000003)	(0.00000003)	(0.00000003)	(0.00000002)	(0.0000005)	(0.000007)
90:50 income ratio	0.007	–0.002	0.003	0.005**	0.003*	–0.01	–0.005
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.009)	(0.01)
50:10 income ratio	0.0000001	0.00000003	–0.00000002	0.00000010*	0.00000003	0.0000006	0.00003**
	(0.0000003)	(0.00000008)	(0.00000008)	(0.00000006)	(0.00000005)	(0.0000008)	(0.00001)
90:50 wealth ratio	0.000002*	–0.00000006	–0.0000002	0.0000003	–0.0000001	0.00001*	
	(0.0000010)	(0.0000003)	(0.0000003)	(0.0000003)	(0.0000002)	(0.000006)	
N income ratio regressions	2,487,434	2,487,434	2,487,434	2,487,434	2,487,434	1,256,167	800,388
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,171	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, school-specific linear trends, and additionally the student background characteristics shown in Table 1. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.13: Exposure to inequality and education and labor market outcomes: controlling for peers' parental characteristics

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	–0.00000004	–0.000000002	–0.00000004	0.00000002	–0.000000007	0.00002*	0.00001**
	(0.0000001)	(0.00000004)	(0.00000003)	(0.00000002)	(0.00000002)	(0.00001)	(0.000006)
90:50 income ratio	0.005	–0.004**	0.0008	0.004*	0.003	–0.01	–0.009
	(0.006)	(0.002)	(0.003)	(0.002)	(0.002)	(0.010)	(0.01)
50:10 income ratio	–0.00000005	–0.00000002	–0.00000007	0.00000005	0.000000003	0.00004	0.00002*
	(0.0000003)	(0.00000009)	(0.00000009)	(0.00000005)	(0.00000005)	(0.00002)	(0.00001)
90:50 wealth ratio	0.0000009	–0.0000002	–0.0000006	–0.0000001	–0.0000004	0.000009	
	(0.0000008)	(0.0000002)	(0.0000004)	(0.0000003)	(0.0000003)	(0.000007)	
N income ratio regressions	2,487,233	2,487,233	2,487,233	2,487,233	2,487,233	1,256,131	800,371
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,171	

Notes: The table shows estimates of the effects of exposure to income and wealth inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, school-specific linear trends, and additionally peers' average parental income (in the income inequality regressions) and peers' average parental wealth (in the wealth inequality regressions). Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.14: Exposure to inequality and education outcomes: Inequality among 3rd grade peers

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university
	(1)	(2)	(3)	(4)
90:10 income ratio	0.00000008	0.00000004	0.00000007	–0.00000000
	(0.00000008)	(0.00000005)	(0.00000006)	(0.00000004)
90:50 income ratio	0.002	0.0003	–0.0001	–0.00009
	(0.001)	(0.0009)	(0.001)	(0.001)
50:10 income ratio	0.0000001	–0.00000001	0.00000005	–0.00000008
	(0.00000011)	(0.0000001)	(0.00000009)	(0.0000001)
Observations	739,996	460,288	460,288	460,288

Notes: The table shows estimates of the effects of exposure to income inequality in 3rd grade on education outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.15: Exposure to inequality and education outcomes: Inequality among 6th grade peers

	9th grade GPA (std.)	Graduated from high school	Graduated from academic high school	Enrolled at university
	(1)	(2)	(3)	(4)
90:10 income ratio	0.0000005**	0.0000002*	0.0000001	0.00000003
	(0.0000002)	(0.00000009)	(0.00000007)	(0.00000003)
90:50 income ratio	0.0008	0.0001	0.0003	–0.0003
	(0.005)	(0.003)	(0.002)	(0.002)
50:10 income ratio	0.0000006	0.0000002	0.0000002	0.00000005
	(0.0000004)	(0.0000002)	(0.0000002)	(0.00000006)
Observations	780,775	520,708	520,708	520,708

Notes: The table shows estimates of the effects of exposure to income inequality in 6th grade on education outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.16: Exposure to inequality and education and labor market outcomes: Inequality among High School peers

	Graduated from high school	Graduated from academic high school	Enrolled at university	Graduated from university	ln(Average income age 28-30)	ln(Average income age 33-35)
	(1)	(2)	(3)	(4)	(5)	(6)
90:10 income ratio	–0.00000002	–0.00000000	–0.00000003**	–0.00000002*	–0.00000004	0.00000003
	(0.00000002)	(0.00000001)	(0.00000001)	(0.00000001)	(0.00000002)	(0.00000004)
90:50 income ratio	–0.0003	–0.0005	–0.0006	–0.0004	–0.006*	–0.004
	(0.0009)	(0.0009)	(0.001)	(0.001)	(0.003)	(0.003)
50:10 income ratio	–0.00000003	–0.00000001	–0.00000004**	–0.00000003**	–0.00000006	0.00000006
	(0.00000003)	(0.00000001)	(0.00000002)	(0.00000001)	(0.00000004)	(0.00000008)
Observations	2,180,251	2,180,251	2,180,251	1,813,832	1,039,142	476,136

Notes: The table shows estimates of the effects of exposure to income inequality in high school tracks on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school*track fixed effects, cohort fixed effects and school*track-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.17: Exposure to inequality, human capital investment, and labor market outcomes: non-linear effects - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Q2-3	0.008	0.001	0.011	–0.008	–0.059*	–0.064	0.012
	(0.02)	(0.008)	(0.01)	(0.02)	(0.03)	(0.04)	(0.04)
Q4	0.058*	0.024**	0.065***	0.018	–0.023	0.010	0.091*
	(0.03)	(0.01)	(0.02)	(0.02)	(0.04)	(0.05)	(0.05)
90:50 income ratio:							
Q2-3	0.015	0.010*	0.022	0.001	–0.014	–0.033	–0.005
	(0.03)	(0.006)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)
Q4	0.026	0.027***	0.053**	0.012	–0.007	0.054	0.043
	(0.04)	(0.009)	(0.02)	(0.02)	(0.04)	(0.06)	(0.05)
50:10 income ratio:							
Q2-3	0.001	–0.001	0.006	–0.006	–0.056*	–0.067*	–0.006
	(0.02)	(0.007)	(0.01)	(0.01)	(0.03)	(0.04)	(0.04)
Q4	0.057*	0.004	0.036**	0.023	0.021	0.037	0.090*
	(0.03)	(0.01)	(0.02)	(0.01)	(0.04)	(0.05)	(0.05)
N	11,704	15,041	15,041	15,041	13,691	11,612	11,266

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Coefficient in each column, in each panel, come from separate regressions. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.18: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income rank within cohort - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < cohort median	0.003	0.001	0.004	0.004	–0.005	0.001	0.005
	(0.007)	(0.001)	(0.003)	(0.003)	(0.006)	(0.007)	(0.008)
Parental income > cohort median	–0.006	–0.000	0.001	–0.004	0.004	0.011	0.014**
	(0.005)	(0.001)	(0.002)	(0.003)	(0.005)	(0.008)	(0.006)
90:50 income ratio:							
Parental income < cohort median	0.025	0.010*	0.020*	0.015	–0.021	0.046	0.035
	(0.02)	(0.006)	(0.01)	(0.01)	(0.03)	(0.04)	(0.05)
Parental income > cohort median	–0.028	–0.010	–0.004	–0.026**	0.017	0.002	0.001
	(0.03)	(0.008)	(0.010)	(0.01)	(0.03)	(0.04)	(0.03)
50:10 income ratio:							
Parental income < cohort median	0.004	0.001	0.007	0.010	–0.005	–0.013	0.011
	(0.02)	(0.003)	(0.007)	(0.009)	(0.02)	(0.02)	(0.02)
Parental income > cohort median	–0.007	0.003	0.007	–0.002	0.010	0.033	0.049***
	(0.01)	(0.002)	(0.007)	(0.009)	(0.02)	(0.02)	(0.02)
N	8,895	11,397	11,397	11,397	10,402	8,836	8,551

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.19: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income rank - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Parental income < median	0.001	0.001	0.003	0.003	–0.004	0.004	0.007*
	(0.006)	(0.001)	(0.003)	(0.002)	(0.005)	(0.005)	(0.004)
Parental income > median	0.004	–0.001	0.001	–0.004	0.017	0.024*	0.028**
	(0.007)	(0.003)	(0.003)	(0.005)	(0.01)	(0.01)	(0.01)
90:50 income ratio:							
Parental income < median	0.040*	0.016**	0.035***	0.022**	0.007	0.036	0.029
	(0.02)	(0.006)	(0.01)	(0.009)	(0.02)	(0.03)	(0.03)
Parental income > median	0.011	–0.009	–0.003	–0.026	0.044**	0.061*	0.062*
	(0.02)	(0.01)	(0.007)	(0.02)	(0.02)	(0.03)	(0.03)
50:10 income ratio:							
Parental income < median	–0.004	0.000	0.004	0.003	–0.009	0.003	0.016
	(0.01)	(0.002)	(0.005)	(0.005)	(0.01)	(0.02)	(0.01)
Parental income > median	0.010	0.001	0.009	0.011	0.026	0.014	0.056*
	(0.02)	(0.003)	(0.010)	(0.02)	(0.04)	(0.04)	(0.03)
N	8,895	11,397	11,397	11,397	10,402	8,836	8,551

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.20: Exposure to inequality and education and labor market outcomes: heterogeneity by parental education - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio:							
Less than university	0.001	0.001	0.002	0.001	0.007	0.001	0.017*
	(0.006)	(0.001)	(0.004)	(0.002)	(0.008)	(0.008)	(0.009)
University	–0.001	–0.001	0.002	–0.002	0.009	0.009	0.014**
	(0.004)	(0.0009)	(0.002)	(0.003)	(0.008)	(0.006)	(0.007)
90:50 income ratio:							
Less than university	0.102***	0.018**	0.043***	0.025**	0.054	0.067**	0.078*
	(0.03)	(0.008)	(0.01)	(0.01)	(0.05)	(0.03)	(0.04)
University	0.008	–0.005	–0.002	–0.008	0.013	0.007	0.026
	(0.02)	(0.006)	(0.006)	(0.01)	(0.03)	(0.02)	(0.03)
50:10 income ratio:							
Less than university	–0.022	–0.001	–0.007	–0.003	0.010	–0.029	0.023
	(0.01)	(0.004)	(0.010)	(0.005)	(0.02)	(0.02)	(0.02)
University	–0.002	–0.001	0.009	–0.000	0.033*	0.032	0.041*
	(0.01)	(0.002)	(0.007)	(0.010)	(0.02)	(0.02)	(0.02)
N	7,550	9,284	9,284	9,284	8,541	7,399	7,069

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.

Appendix Table A.21: Exposure to inequality and education and labor market outcomes: controlling for background characteristics - Add Health

Survey wave:	Wave III (2001–02)	Wave IV (2008–09)				Wave V (2016–18)	Wave VI (2022–25)
	High School GPA	Graduated from HS	Enrolled in Uni./ College	Has Bachelor's Degree	ln(Earned income)	ln(Earned income)	ln(Earned income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
90:10 income ratio	0.000	0.001	0.002	0.001	–0.000	0.004	0.012***
	(0.003)	(0.0007)	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)
90:50 income ratio	0.011	0.005	0.011**	–0.003	0.032*	0.037**	0.042***
	(0.01)	(0.004)	(0.005)	(0.004)	(0.02)	(0.02)	(0.01)
50:10 income ratio	–0.004	–0.000	0.003	0.003	–0.007	–0.006	0.023***
	(0.009)	(0.002)	(0.004)	(0.003)	(0.008)	(0.01)	(0.009)
N	11,704	15,041	15,041	15,041	13,691	11,612	11,266

Notes: The table shows estimates of the effects of exposure to income inequality on education and labor market outcomes. Each coefficient comes from a separate regression. All regressions control for school fixed effects, cohort fixed effects, and school-specific linear trends. Controls include birth year, sex, immigrant status, mother's age at birth, family size, mother's and father's education and household income at baseline. For categorical variables, missing values are coded as distinct values. For continuous variables, missing values are replaced with the sample mean and additional indicator variables are included for each variable to denote missing values. Standard errors, clustered by school, in parentheses. * p<0.10 ** p<0.05 *** p<0.01.