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Exposure to Inequality, Human Capital Investment, and Labor Market Outcomes[*]

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Abstract

We estimate the effects of exposure to income and wealth inequality during adolescence on long-term educational and labor market outcomes. 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 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. Moreover, we find no evidence of systematic heterogeneity by socioeconomic background. Taken together, these findings provide reassurance that school integration policies mixing students from different socioeconomic backgrounds do not carry hidden long-run costs stemming from exposure to inequality. More broadly, they challenge the view that school-based exposure to peer inequality during adolescence is a causal driver of human capital accumulation or later-life mobility.

[*]The data used in this paper comes from the Swedish Interdisciplinary Panel (SIP), administered by the Centre for Economic Demography, Lund University, Sweden

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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. This question has grown more pressing as inequality has increased markedly in recent decades (Piketty and Saez, 2014), not only in Anglo-American settings but also in welfare states such as Sweden, where income and wealth inequality have risen steadily since the early 1990s (Roine and Waldenström, 2015; Lundberg and Waldenström, 2018). This paper provides new empirical evidence on this question, offering causal estimates of how exposure to inequality during adolescence affects educational investment and long-run labor market outcomes.

Theoretical work offers competing predictions about how inequality influences human capital investment. On the one hand, classical human capital theory implies that if inequality reflects returns to skills, it should incentivize educational investment (Becker, 1964). On the other hand, more recent contributions argue that growing inequality can lead to "economic despair" among individuals from lower-income backgrounds, as middle-class life appears increasingly out of reach (Kearney and Levine, 2016; Genicot and Ray, 2017). Such despair lowers educational aspirations, which in turn reduces human capital investment.[1] If this mechanism dominates, inequality not only reflects disparities but also perpetuates them by depressing educational investment among the less well-off, contributing to the Krueger–Gatsby Curve, which links greater inequality to weaker intergenerational mobility (Blanden et al., 2005; Corak, 2013; Durlauf et al., 2022; Cholli et al., 2024).[2]

The competing theoretical predictions highlight that the effect of inequality on human capital investment is ultimately an empirical question. Despite decades of debate, however, there is almost no causal evidence on whether exposure to inequality shapes educational or labor market trajectories. We address this gap by studying students in Sweden as they near the end of compulsory education, a critical point at which they must decide whether to pursue upper secondary schooling and, if so, which track to enter – decisions with lasting consequences for subsequent educational attainment and labor

[1] In the model of Genicot and Ray (2017), aspirations are socially determined: moderate gaps foster investment, while large gaps generate frustration and under-investment, leading to persistent inequality traps. Even within classical human capital theory, however, the response to higher expected returns may exacerbate inequality if wealthier parents are better positioned to invest in their children's education (Becker and Tomes, 1986).

[2] Income inequality may also reduce mobility by reinforcing social segregation (Chetty et al., 2022). The "economic despair" theory is further consistent with a large interdisciplinary literature documenting negative associations between inequality and educational attainment as well as other outcomes such as life satisfaction, mental health, crime, drug use, and social trust. One of the most influential contributions is Wilkinson and Pickett (2009), which has more than 17,000 citations on Google Scholar. For overviews of this literature, see Pickett and Wilkinson (2015); Bergh et al. (2016); Kim (2017); Polacko (2021); Kim et al. (2022).

market outcomes.

The school setting is especially relevant for studying the effects of inequality for two reasons. First, schools are central arenas of peer interaction, where adolescents spend much of their time with classmates from diverse socioeconomic backgrounds. A large literature shows that school-cohort peers influence outcomes ranging from academic achievement and educational choices to long-run earnings (e.g. Hoxby, 2000a; Lavy et al., 2012; Black et al., 2013; Carrell et al., 2018; Bietenbeck, 2020; Getik and Meier, 2023; Cattan et al., 2024). Adolescents are also especially sensitive to social comparisons, at a time when they become increasingly aware of their socioeconomic position.[3] Second, schools are a primary target of integration policies such as busing, catchment area reforms, and school choice that explicitly aim to mix students from different socioeconomic backgrounds (Deming et al., 2014; Angrist et al., 2022; Bjerre-Nielsen and Gandil, 2024). These policies rest on the idea of the school as a "melting pot," where students from different backgrounds meet and social barriers are broken down. Whether such exposure fosters aspiration and opportunity or instead reinforces disadvantage through exposure to inequality, however, remains an open empirical question.

Our analysis uses Swedish register data covering all students completing compulsory education between 1989 and 2013. Using detailed information on parents' income and wealth, we construct measures of inequality among students' school-cohort peers. For income, we compute the 90:10, 90:50, and 50:10 ratios, standard indicators of overall, upper-tail, and lower-tail inequality. For wealth, we focus on the 90:50 ratio, since the 10th percentile of assets is often zero, making lower-tail measures ill-defined. The outcomes we consider include GPA at the end of compulsory school, high school graduation, university enrollment and completion, and income up to age 35.

To identify causal effects, we exploit within-school, between-cohort variation in peer inequality. This research design, first proposed by Hoxby (2000b) 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., 2024), compares students in the same school who are exposed to different levels of inequality depending on their cohort. By including school and cohort fixed effects as well as school-specific linear trends, we account for persistent differences across schools and common time trends. From the perspective of an individual student, the residual variation in inequality is plausibly random. We validate this assumption by showing that predetermined characteristics, such as parental income and education, are

[3] Children as young as 3–5 years recognize economic differences through visible cues like clothing and housing (Ramsey, 1991; Rauscher et al., 2017). By age 7–10, many can rank occupations by income (Burgard et al., 1989; Emler and Dickinson, 1985). During adolescence, youths become particularly attuned to social comparisons and more accurate in assessing their socioeconomic position, a pattern documented both in high-inequality countries and in more egalitarian Nordic contexts (Mistry et al., 2015; Peretz-Lange et al., 2022; Hautala and Lehti, 2025). Relatedly, recent studies show that local social environments shape perceptions of inequality and preferences for redistribution (Londoño-Vélez, 2022; Domènech-Arumí, 2025).

uncorrelated with school-cohort inequality. Moreover, the within-school variation we exploit is substantial and comparable in magnitude to between-school differences.

The results are striking in their consistency: we find no evidence that exposure to inequality during adolescence affects long-term educational or labor market outcomes. Across all measures of income and wealth inequality, estimates are close to zero and precisely estimated, allowing us to rule out even modest effects. The null findings hold across a broad set of outcomes—including grades, high school completion, university enrollment and graduation, and adult income. Even when we allow for non-linearities, comparing students exposed to the highest levels of inequality with those in more equal cohorts, the results remain null. These findings challenge both sides of the theoretical debate: inequality neither spurs greater investment by signaling higher returns to skills, nor generates lasting economic despair that discourages human capital accumulation.

We next examine whether effects vary across students or school contexts. Splitting students by parental income, wealth, or education, we find no evidence that exposure to inequality differentially affects those from lower- versus higher-SES families. To probe whether inequality matters more in disadvantaged schools, we extend this analysis by dividing the sample between high- and low-income schools. This 2×2 design, which crosses student SES with school SES, approximates the segregated school environments of more unequal societies, where students in disadvantaged neighborhoods rarely encounter upper-middle-class peers. Yet even in these settings, we find little evidence of systematic effects. The one exception is modest negative impacts for high-income students in low-income schools, consistent with mechanisms of social mismatch, though these estimates are small in magnitude and do not alter the broader conclusion.

In additional analyses, we address potential threats to validity and scope. First, our measures of inequality may not fully capture the relevant peer environment, but results remain null when restricting the sample to smaller cohorts or when defining inequality at the school-cohort-by-gender level. Second, disparities in wealth among peers might not always be salient. To address this, we extend our analysis to housing wealth—a more visible dimension of living standards—and again find null results. Third, concerns about external validity are mitigated by the fact that findings hold even for cohorts exposed to very high levels of income inequality and for wealth inequality, which in Sweden is among the highest in the developed world. Fourth, the results are robust to controlling for family fixed effects, exploiting sibling comparisons to absorb all shared family background factors. Fifth, we show that the findings are not confounded by correlated peer factors: controlling for average parental income or wealth, as well as other background characteristics, leaves estimates unchanged. Sixth, the null effects are not due to a general absence of peer influences: using the same design, we replicate strong and precisely estimated effects of other peer characteristics such as gender composition, immigrant share, parental education, and predicted ability. Finally, while most variation in inequality arises across schools due to sorting, the identifying within-school variation is substantial and comparable to between-school differences, underscoring that the absence of effects reflects substance rather than limitations of research design.

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. More generally, the results challenge theories that view direct exposure to inequality as a central driver of reduced aspirations and weaker mobility. Schools are a particularly salient and policy-relevant environment in which adolescents encounter inequality, making them a natural testing ground for these theories. The fact that we find no effects in this setting suggests that if inequality shapes long-run mobility, it must operate through other channels.

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 on standardized tests across countries and U.S. states. Moving beyond correlations, Mayer (2001) and Kearney and Levine (2016) use state fixed effects to study the effects of income inequality on educational outcomes. Mayer (2001) finds that growing up in a state with high economic inequality increases educational attainment for high-income children while lowering it for low-income children, exacerbating inequality. Similarly, Kearney and Levine (2016) show that individuals from low socioeconomic backgrounds are more likely to drop out of high school in states with greater income inequality. These findings are consistent with theoretical predictions by Genicot and Ray (2017) and Kearney and Levine (2016), which propose that rising inequality fosters economic despair and reduces human capital investment among individuals from low-income backgrounds. While the use of state fixed effects helps address some confounding, it cannot fully account for the possibility that changes in state-level inequality correlate with changes in other determinants of educational outcomes. We extend this literature by providing causal estimates of the effects of inequality exposure on long-run human capital.[4]

We also contribute to a growing body of work on the relationship between inequality and social mobility (e.g. Corak, 2013; Chetty et al., 2014). Chetty and Hendren (2018) show that growing up in areas with higher income inequality is associated with reduced adult income for children from lower-income families but caution that these associations do not establish causal effects of income inequality. One proposed explanation for these

[4] Interestingly, 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 attended compulsory schools characterized by greater income inequality.

patterns is that higher income inequality discourages human capital investment among lower-income individuals, reducing their chances of upward mobility. Our analysis tests this channel directly, and the findings suggest otherwise: we do not find any impact of inequality exposure on the outcomes of adolescents from lower-income families.

Finally, our paper relates to the broader literature on peer effects in schools.[5] Prior research has examined the effects of peer ability (e.g. Lavy et al., 2012; Bietenbeck, 2020), peer demographic characteristics (e.g. Hoxby, 2000a; Hoxby and Weingarth, 2005; Lavy and Schlosser, 2011; Brenøe and Zölitz, 2020; Figlio et al., 2024), peer personality (Bietenbeck, 2025), exposure to disruptive peers (e.g. Figlio, 2007; Carrell et al., 2018), and exposure to peers with an elite education (Cattan et al., 2024), among other factors. We add to this literature by studying how economic inequality among school peers affects adolescents' human capital investment and long-term labor market success.

2 Background

2.1 The Swedish education system

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 that cohorts 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 students were formally free to apply to any school, the scope for selective enrollment was limited. Independent schools (*friskolor*), which were permitted to set their own admission rules but could not charge fees or select students by ability, enrolled only around six percent of students during our period of study. As a result, the socioeconomic composition of school cohorts was shaped primarily 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.[6] These decisions were therefore high-stakes choices, with lasting consequences for subsequent educational attainment and labor market outcomes.

Finally, a potential concern for our empirical analysis is that schools might track

[5] For overviews of this literature, see Sacerdote (2011) and Paloyo (2020).

[6] 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.

students into classes by ability or socioeconomic background, thereby concentrating inequality within classes and leaving little variation at the cohort level. However, no such 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 ensures that socioeconomic diversity was realized at the cohort level, making school-cohort inequality the relevant margin of exposure.

Together, these institutional characteristics, including stable school-cohort composition, residence-based school assignment, and the absence of tracking, make Sweden an especially well-suited environment for estimating the causal effects of exposure to inequality during adolescence.

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 rise 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 has never returned to pre-recession levels. By international standards, Sweden still looks equal: in 2019, the 90:10 disposable income ratio was 3.3, compared to 3.7 in Germany and 6.3 in the United States (OECD, 2025). Yet, as we show in Section 4, these national figures mask substantial variation across school cohorts, with some experiencing levels of inequality that approach those observed in far more unequal societies.

Wealth tells a different story. Sweden today ranks among the most unequal countries in the developed world (World Inequality Database, 2025), and wealth disparities have grown steadily since 2000 (Lundberg and Waldenström, 2018). At the school-cohort level, wealth inequality varies sharply, ranging from highly compressed to extremely unequal distributions. This variation provides an especially 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 both earned income from salaries, business profits, pensions, and sickness benefits and capital income from interest and dividends.[7] 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.[8]

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 the sum of market-valued real, financial, and other assets. For each student, we sum parents' wealth 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 education outcomes that reflect human capital investment: 9th-grade GPA (standardized to have a mean of 0 and a standard deviation of 1), high school graduation, completion of an academic-track high school program, university enrollment, and university graduation.

Labor market outcomes We construct two measures of adult income: average total income (earned and capital income) at age 28–30 and at age 33–35. Since the SIP includes income only up until 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

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

[8] 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; 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. In addition, percentile ratios are less sensitive to extreme outliers than the Gini and remain robust when applied to moderately sized cohorts, making them well suited for our school-level analysis.

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 end at 2013 to ensure that key education outcomes can be reasonably observed through 2017. These restrictions yield a sample of 2,487,434 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.[9]

Table 1 presents summary statistics for the two samples. Both samples are very similar in terms of students' socio-demographic characteristics. The average 9th-grade cohort size is 112 students in both samples. Regarding income inequality, the median 90:10 income ratio is 3.05, while the mean is significantly 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; however, robustness analyses excluding them confirm that the results remain unchanged. 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 close to the corresponding ones at the national level, which are shown in the table beneath the sample-specific income ratios.

For wealth inequality, the median 90:50 wealth ratio is 2.55, while the mean is considerably higher at 35.08, reflecting extreme wealth values; 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 complete 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 age 28–30 (corresponding to 30,692 US$ at 2024 prices) and 250,797 SEK at age 33–35 ($37,813), whereas in the wealth inequality sample average income at age 28–30 is 219,866 SEK ($33,149).[10]

[9] We implement some additional sample restrictions. In the income inequality sample, we exclude 167,250 individuals with missing values for any of our measures of inequality, our outcome variables or variables used in our balancing tests, or who are the only individual observed in their school cohort, due to singular fixed effects. In the wealth inequality sample, this leads to the exclusion of 11,916 individuals.

[10] Income at age 33–35 is not available in this sample

4 Empirical strategy

The key empirical challenge in estimating the causal effects of exposure to inequality is that inequality is not randomly assigned. Specifically, the level of inequality in adolescents' social environment likely correlates with numerous factors influencing long-term outcomes, such as school and neighborhood quality. To address this challenge, we build on Hoxby (2000a) and estimate specifications that isolate the effects of exposure to inequality by exploiting variation between cohorts within the same schools:

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

where Y_{ics} is an educational or labor market outcome for student i in cohort c and school s; $INEQ_{-ics}$ measures school-cohort-level inequality among i's peers; δ_c is a vector of cohort fixed effects; λ_s is a vector of school fixed effects; $c \times D_s$ is a school-specific linear cohort trend; and ε_{ics} is the error term. We estimate these specifications using ordinary least squares and cluster standard errors at the school level to account for possible correlation in the error terms within schools.

The coefficient of interest in equation 1 is β, which captures the effect of exposure to inequality on the outcomes of interest. Estimates of this parameter will be unbiased if cohort-to-cohort variation in inequality within schools is random, conditional on the included controls. All our specifications include school fixed effects, which account for persistent differences in inequality and outcomes across schools, cohort fixed effects, which control for common trends across cohorts, and school-specific linear trends, which capture unobserved, time-varying school characteristics that might influence both inequality and outcomes. In robustness specifications, we additionally control for a range of student background characteristics.

We provide evidence supporting the validity of our empirical strategy in Table 2. In these balancing tests, we estimate the effect of school-cohort inequality on a set of predetermined student characteristics using the specification in equation 1 (without student background controls). The underlying idea is that if the variation in inequality is quasi-random after conditioning on our controls, it should not systematically predict student background. Across 33 regressions, virtually all of the coefficients on the inequality measures are economically negligible, although some achieve statistical significance due to the large sample size. We show in a robustness check that our results hold when we control for all of these background variables. These findings support our identifying assumption that differences in inequality between cohorts within the same school are quasi-random.[11]

[11] As an additional check, we run the same balance tests using lagged inequality (i.e., the level of inequality in the previous cohort). The results are similar: lagged inequality does not systematically predict student background, suggesting that parental sorting based on earlier cohorts is unlikely to drive our findings (Table A.1).

While the within-school, between-cohort approach thus allows us to isolate plausibly random variation in exposure to inequality, it has several potential limitations. First, one concern is that the analysis focuses on inequality at the school-cohort level rather than broader measures, such as national or neighborhood inequality. Yet this is precisely the setting where inequality is particularly salient, and where a large body of evidence shows that children and adolescents are acutely aware of economic differences among their peers (Emler and Dickinson, 1985; Burgard et al., 1989; Mistry et al., 2015; Rauscher et al., 2017; Peretz-Lange et al., 2022).[12] Second, the school cohort may be too large an aggregation, as students do not interact with all peers in their cohort. To address this, we conduct robustness analyses in which we restrict the sample to smaller schools with fewer students per cohort and measure inequality at the school-cohort-by-gender level, based on the intuition that adolescents are more likely to interact with peers of the same gender. Third, inequality among school-cohort peers may be correlated with other peer characteristics, which could confound our estimates. In particular, income (wealth) inequality may be correlated with average income (wealth) among peers. To address this concern, we conduct tests in which we control for peer background characteristics, including average income (wealth) of peers' parents.

Finally, a natural concern is that, because of sorting, most inequality arises across rather than within schools, leaving limited or unrepresentative variation to identify causal effects. However, this is not the case. For instance, the standard deviation of the 90:50 parental income ratio decreases only by 42 percent, from 0.28 to 0.16, when controlling for school and cohort fixed effects and school-specific trends, indicating that a meaningful amount of variation persists.[13] We further corroborate this in Appendix Figure A.2, which plots the time series of income and wealth inequality for 10 randomly selected schools and reveals a large amount of within-school between-cohort variation. Moreover, Panel A of Figure A.3 compares the distribution of within-school, between-cohort (red) and between-school (blue) variation in the 90:50 income ratio. The two distributions are very similar, indicating that the identifying variation in our design is not markedly smaller than the overall variation across schools.

[12] Since school assignment is mainly determined by residential proximity, neighborhood inequality is correlated with school-level inequality. Using parishes to approximate neighborhoods, we find that the correlation between the 90:50 income ratio at the school-cohort level and the 90:50 income ratio at the parish-cohort level is 0.65 (the parish-cohort level distribution includes extreme outliers, so we exclude the top 2 percent of values for this calculation). The corresponding correlations for the other measures of inequality are: 0.44 (90:10 income ratio), 0.40 (50:10 income ratio), and 0.33 (90:50 wealth ratio).

[13] The corresponding percent decrease in the standard deviations of the other inequality measures are: 31 percent (90:10 income ratio), 27 percent (50:10 income ratio), and 18 percent (90:50 wealth ratio).

5 Results

We start by providing descriptive evidence on the relationship between inequality exposure and education and labor market outcomes, before moving on to our causal estimates. Table 3 reports linear regressions of student outcomes on cohort-level inequality measures, estimated without additional control variables. 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 wealth ratio, are small. By contrast, the 90:50 income ratio stands out. A higher 90:50 ratio is linked to better grades and substantially higher probabilities of both academic 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 systematically stronger and *positive* associations—especially for educational transitions and earnings. To test whether these associations reflect causal effects, we now turn to our main estimates, which isolates quasi-random variation in inequality across cohorts within schools.

5.1 Causal estimates

Table 4 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 affects 9th-grade GPA, high school graduation, university enrollment or completion, or adult income. This null finding holds consistently 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. Importantly, our estimates are precise enough to rule out even modest effect sizes. For example, for university enrollment, the estimated effect of the 90:50 income ratio is 0.005 (SE = 0.002). Given that the 90:50 ratio is 2.3 in the U.S. and 1.7 in Sweden—a difference of 0.6 points, according to OECD data—this implies that growing up in a more unequal school environment like the U.S. rather than Sweden increases university enrollment by no more than 0.5 percentage points with 95 percent confidence. For the 90:10 ratio—which is 6.3 in the U.S. and 3.3 in Sweden, a difference of 3.0 points—we can rule out that U.S.-level inequality affects university enrollment by more than 0.000003 percentage points. These findings stand in sharp contrast to earlier studies that document negative effects of inequality on human capital accumulation and economic mobility.

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 within the school-cohort distribution for the income inequality analysis, and their wealth rank for the wealth inequality analysis.[14] We then separately estimate effects for students with below- and above-median parental income or wealth.

The results in Table 5 provide no 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. To illustrate magnitudes, consider the U.S.–Sweden difference in the 90:10 income ratio of 3.0 points (6.3 vs. 3.3). Our estimates imply that exposure to U.S.-level inequality would increase college enrollment for low-income students by at most 0.000009 percentage points. This contrasts sharply with Kearney and Levine (2016), who estimate that a one-point increase in the 90:10 ratio reduces U.S. college enrollment among low-income youth by 4.1 percentage points—equivalent to a 12.3 percentage point decline when moving from Swedish to U.S.-level inequality.

Overall, we find no 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.2) and by analyses that split the sample by parental education instead of income or wealth (Appendix Table A.3).

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 this does not rule out heterogeneity along another important dimension: the type of school a student attends. The salience and consequences of inequality are likely shaped not only by a student's own socioeconomic background but also by the socioeconomic context of their peers. Low-income students in high-income schools may experience inequality more acutely, as visible gaps in resources and lifestyles sharpen feelings of relative deprivation. Conversely, high-income students in low-income schools may stand out and experience social isolation and pressure to downplay advantages. In more advantaged schools, high-income students may instead see their advantages reinforced.

To test these mechanisms, we extend our heterogeneity analysis by splitting the sample between high- and low-income schools and further distinguishing between students

[14] 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 dropping one graduation cohort, slightly reducing the sample size.

above and below the school-cohort median. We do so by splitting the sample at the median of school-cohort average income. This 2×2 design crosses student SES with school SES, allowing us to approximate the segregated school environments of more unequal societies, such as the United States, where students in disadvantaged neighborhoods rarely encounter upper-middle-class peers. By focusing on Sweden's poorest schools, this analysis also provides a bridge for comparability across contexts with very different degrees of segregation. As shown in Tables A.4 and A.5, however, we find no consistent or systematic effects across most groups, reinforcing our conclusion that exposure to inequality does not generally shape educational or labor market outcomes. The one notable exception is modest negative effects for high-income students in low-income schools, consistent with mechanisms of social mismatch and peer disadvantage. Yet these effects are limited in size and scope and do not alter the broader conclusion.

Taken together, these findings suggest that even in contexts resembling the highly segregated school environments of more unequal countries, exposure to peer inequality exerts little causal influence on long-term educational or labor market trajectories—strengthening the external validity of our results.

5.1.3 Non-linear effects

One 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. To test this, we classify school-cohort inequality into quartiles and estimate effects separately for middle (Q2–Q3) and high (Q4) inequality, using the lowest quartile (Q1) as the reference group. The results in Table 6 provide little evidence of such non-linearities. While a few coefficients are statistically significant, effect sizes are small and inconsistent across outcomes. Importantly, even students exposed to the highest levels of peer inequality show no systematic differences in educational attainment or adult income. This suggests that our null results are not simply an artifact of linear modeling or of Sweden's relatively modest overall inequality: if exposure to inequality had meaningful effects, they would likely appear here—but they do not.

5.2 Robustness and external validity

We next present results from additional analyses that address concerns related to measurement, outliers, selection into schools, and the validity of our research design.

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 test this, we implement two alternative specifications. In Appendix Table A.6, we restrict the analysis to schools with a maximum cohort size of 120. The reduced 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. As an alternative, in Appendix Table A.7, we measure inequality at the school-cohort-by-gender level. Since adolescents are more likely to interact with same-gender peers, this arguably captures a more socially relevant dimension of inequality. In both cases, the results remain close to zero.

We also consider whether overall financial wealth is too opaque to be noticed by adolescents. If so, inequality in total assets may not be the right measure. We therefore re-estimate our models using inequality in housing wealth, a more visible and salient indicator of peers' living standards. As reported in Table A.8, the results remain null.

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.9, we address this issue by excluding the top 2 percent of observations in terms of inequality in each regression. This results in slightly larger estimates for the 90:50 income ratio, suggesting small positive short-term impacts on high school outcomes but negative impacts on income at ages 28–30 and 33–35. However, estimates for all other inequality measures remain close to zero. Overall, we conclude that our findings are robust to the exclusion of outliers.

5.2.3 Selection into schools

Another potential concern is that students may sort into schools or enrollment cohorts based on expected peer inequality. To address this, we follow Figlio et al. (2024) and estimate sibling fixed-effects models that compare siblings exposed to different levels of inequality during adolescence. As shown in Appendix Table A.10, these within-family estimates closely resemble our baseline results and remain near zero across all outcomes, suggesting that unobserved family characteristics or selective enrollment are unlikely to drive our findings.

5.2.4 Other threats to the validity of the research design

We address several other potential concerns about the validity of our research design. First, we confirm that our estimates remain stable when adding additional controls. Appendix Table A.11 reports regressions including student background characteristics, while Appendix Table A.12 adds average peer parental income (in the income regressions) and average peer wealth (in the wealth regressions). Since we already showed in Table 2 that inequality exposure does not predict these characteristics, it is unsurprising that the inclusion of these controls leaves results unchanged.

Second, we examine whether our null results might reflect a general absence of peer effects in Swedish schools. To assess this,we replicate well-established peer effects in other domains—female share, immigrant share, predicted ability, parental education, and absolute income—using the same design.[15] As shown in Table A.13, we find economically meaningful and statistically significant peer effects across most dimensions, consistent with prior findings in the literature: 22 out of 35 estimates are statistically significant. This confirms that peer effects operate in the Swedish school context, but that inequality exposure does not produce similar effects, and also suggests that potential spillovers across grades, if present, are not strong enough to attenuate cohort-based peer influences toward zero.

Third, we assess whether the null results could be due to limited identifying variation once fixed effects and trends are absorbed. The within-school, between-cohort variation in peer inequality remains substantial and comparable in magnitude to between-school differences, and indeed exceeds the identifying variation for peer characteristics where we detect strong effects (gender composition, immigrant share, and parental income; see Figure A.3). This reinforces that the absence of results should be interpreted as substantive rather than a statistical artifact.

6 Conclusion

We study the causal effects of exposure to economic inequality during adolescence on long-term 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. Across all specifications, we find that exposure to inequality—whether measured in terms of income or wealth—does not 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 not explained by heterogeneous responses across socioeconomic groups.

The school context provides a particularly relevant setting for studying how exposure to inequality shapes life trajectories. Adolescence is a formative period in which students make critical educational decisions, and these choices are taken in environments where peers from diverse socioeconomic backgrounds interact daily. In such settings, economic inequality is highly salient, potentially shaping perceptions of opportunity, aspirations, and motivation. A growing literature shows that local social environments shape individ-

[15] Predicted ability is constructed by regressing Grade 9 GPA on predetermined characteristics—parental education, gender, immigrant background, and school fixed effects—and using the predicted values as a proxy for individual ability. The average predicted ability among school-cohort peers serves as the peer treatment.

uals' perceptions of inequality and their attitudes toward redistribution (Londoño-Vélez, 2022; Domènech-Arumí, 2025), underscoring the importance of the school as a site where inequality might be expected to leave lasting marks.

These results also have policy implications. Many countries pursue school integration policies designed to mix students from different socioeconomic backgrounds. Our findings provide reassurance that such policies do not, at least in this context, impose hidden costs on disadvantaged students by exposing them to inequality. More broadly, the evidence points away from school-based exposure as a central mechanism linking inequality to lower mobility.

More broadly, our findings challenge theories that view inequality exposure as a direct mechanism depressing aspirations and mobility through "economic despair." At the same time, they suggest that the consequences of inequality may be highly context-dependent. Sweden's egalitarian institutions and universal access to education may buffer students from the discouraging effects that inequality could generate in less redistributive environments. Our results also underscore the importance of careful identification in this area: correlations between inequality and mobility across places or over time may reflect channels other than direct peer exposure. By providing credible causal evidence, our study helps redirect the search for mechanisms through which inequality shapes life chances.

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Tables and Figures

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 siblings an individual has. 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: Balancing tests

	Age in 9th grade (1)	Female (2)	Immigrant background (3)	Mother's age at birth (4)	Family size (5)	Mother's years of education (6)	Father's years of education (7)	Parental income (8)	Parental wealth (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 3: 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	1,256,207	800,436
N wealth ratio regression	959,659	959,659	959,659	959,659	959,659	274,214	

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

	9th grade GPA (std.) (1)	Graduated from high school (2)	Graduated from academic high school (3)	Enrolled at university (4)	Graduated from university (5)	ln(Average income age 28-30) (6)	ln(Average income age 33-35) (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 5: Exposure to inequality, human capital investment, and labor market outcomes: heterogeneity by parental income and wealth rank

	9th grade GPA (std.) (1)	Graduated from high school (2)	Graduated from academic high school (3)	Enrolled at university (4)	Graduated from university (5)	ln(Average income age 28-30) (6)	ln(Average income age 33-35) (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.

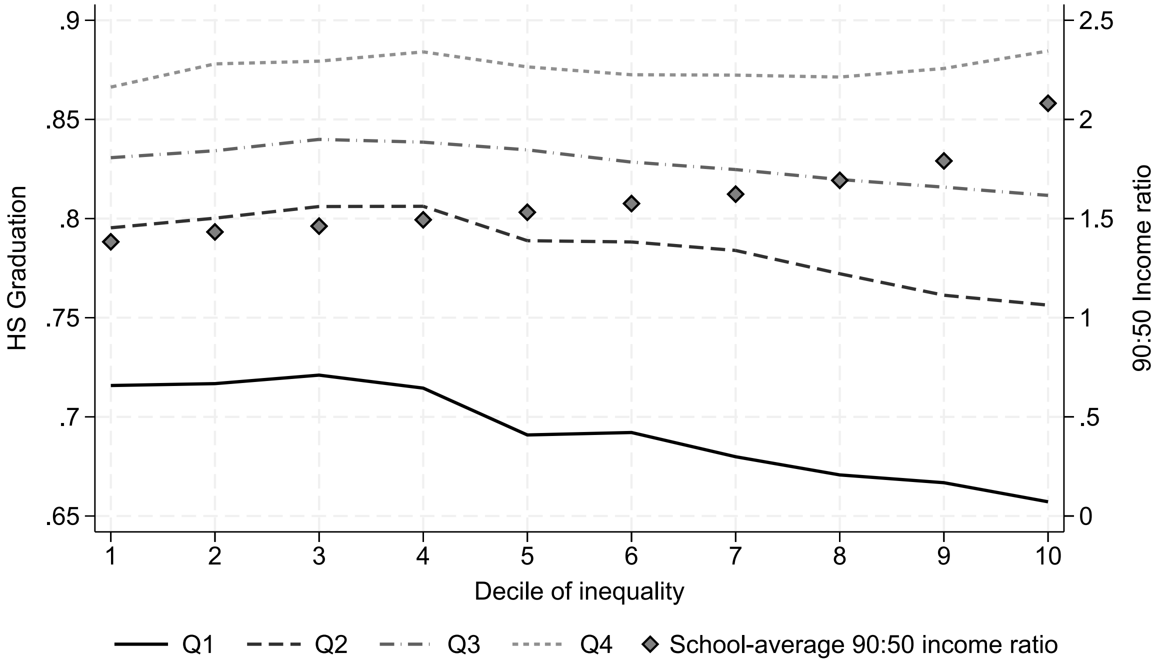
Table 6: Exposure to inequality and education and labor market outcomes: non-linear effects

	9th grade GPA (std.) (1)	Graduated from high school (2)	Graduated from academic high school (3)	Enrolled at university (4)	Graduated from university (5)	ln(Average income age 28-30) (6)	ln(Average income age 33-35) (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.

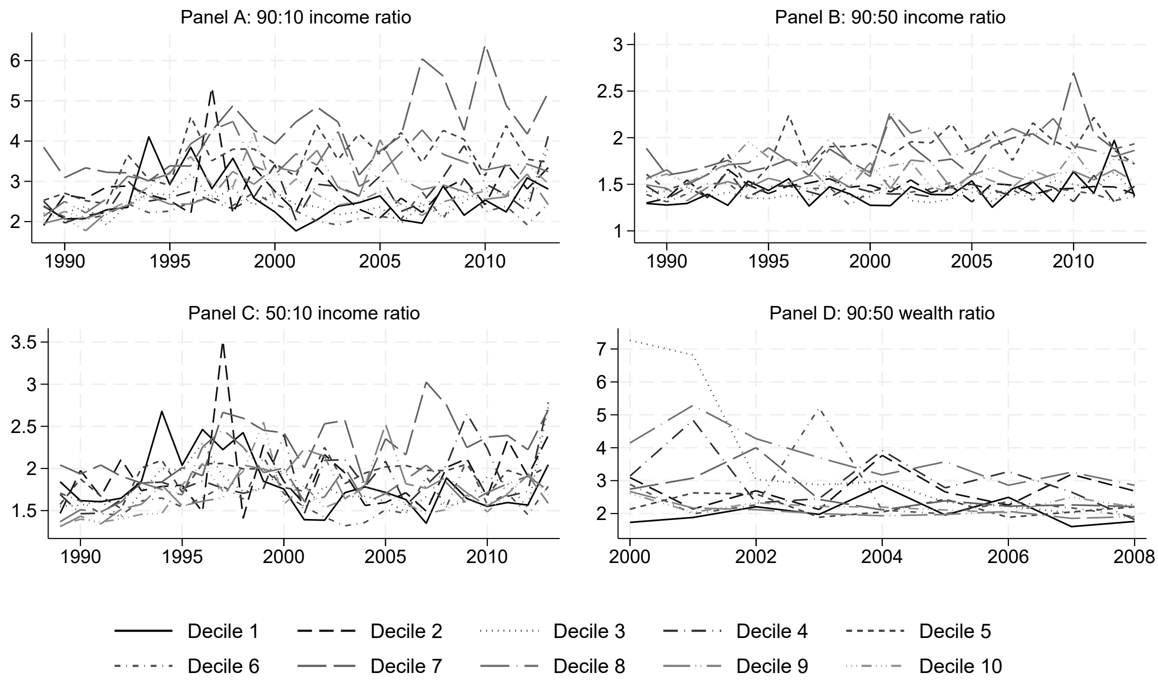
A Additional Results

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



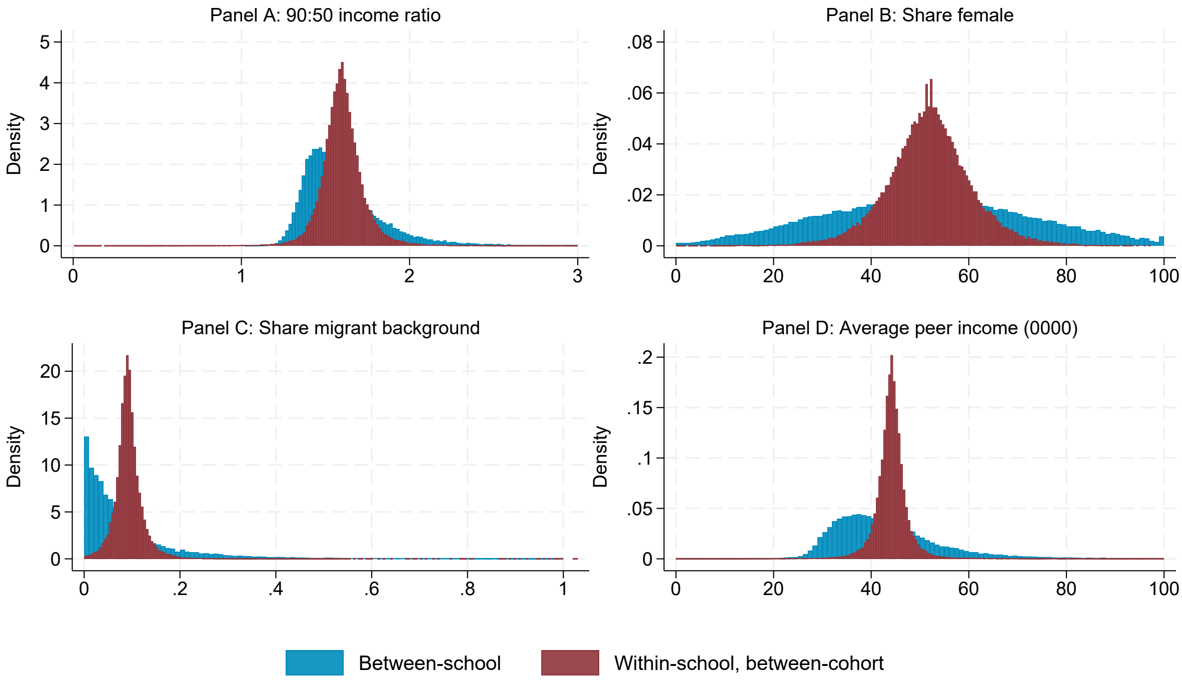
Notes: The figure presents the proportion of students graduating from high school according to socioeconomic background and income inequality. Socioeconomic background is measured in quartiles of parental income. Inequality is measured in deciles of the school-average 90:50 income ratio in the compulsory 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 average peer parental income. 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. Resisuals are added to the sample mean for comparability.

Appendix Table A.1: Balancing tests: Lagged inequality

	Age in 9th grade (1)	Female (2)	Immigrant background (3)	Mother's age at birth (4)	Family size (5)	Mother's years of education (6)	Father's years of education (7)	Parental income (8)	Parental wealth (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: 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.3: 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	–0.00000008	–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)
University	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:							
Less than university	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)
University	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:							
Less than university	–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)
University	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:							
Less than university	0.000001	–0.0000004	–0.0000005	–0.0000004	–0.0000001	0.000008	
	(0.000001)	(0.0000002)	(0.0000004)	(0.0000004)	(0.0000002)	(0.000007)	
University	–0.0000009	0.0000009	–0.0000008	0.000001	–0.000001*	0.00001	
	(0.000002)	(0.000001)	(0.000001)	(0.000001)	(0.0000008)	(0.00002)	
N income ratio	2,181,489	2,181,489	2,181,489	2,181,489	2,181,489	1,000,335	575,313
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.4: 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	1,163,139	1,163,139	1,163,139	1,163,139	1,163,139	241,016	94,688
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.5: 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	94,688
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.6: 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.7: 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 due to singular fixed effects when replacing school with school*gender fixed effects. 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.8: 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.9: 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.10: 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.11: 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.12: 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.13: Peer effects in other dimensions

	9th grade GPA (std.) (1)	Graduated from high school (2)	Graduated from academic high school (3)	Enrolled at university (4)	Graduated from university (5)	ln(Average income age 28-30) (6)	ln(Average income age 33-35) (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)
Avg. peer income (0000)	0.0003	0.0002***	0.0003**	0.0002**	0.00006	–0.000004	–0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Share 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. Standard errors, clustered by school, in parentheses. * $p<0.10$ ** $p<0.05$ *** $p<0.01$.