No Kid Is an Island: Intergenerational Mobility and Peer Effects

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Abstract

Economic inequalities persist from one generation to the next. To what extent is this due to children's social interactions replicating parental disparities? While existing research documents parental investments and local economic conditions as crucial, this paper studies the role of peer exposure for social mobility. Exploiting within school across cohort exogenous variation in schoolmates' parental background among Danish high school students, I show that a $1 increase in average schoolmates' parental earnings results in a $0.08 increase in adult earnings. This effect is as large as 42% of the parent-child correlation in earnings. I find that former schoolmates are connected on labor market networks: as their career advances, they open doors to higher paying firms and provide more attractive outside options to their peers.

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1 Introduction

Parental investments are key to children's development (Becker and Tomes, 1979; Carneiro et al., 2021). Yet, kids often interact with peers from similar socioeconomic backgrounds, as parents sort across schools and neighborhoods. How does exposure to peers from families of different income levels influence future earnings? While research has explored the impact of local economic conditions on intergenerational mobility (Chetty and Hendren, 2018) and the role of peers in educational achievement (Sacerdote, 2011), much less is known about how social interactions transmit income inequality through generations.

I estimate the impact of exposure to peers from different parental backgrounds exploiting exogenous variation in high school composition across different cohorts as in Hoxby (2000). A \$1 increase in schoolmates' average parental earnings results in a \$0.08 increase in annual adult earnings. This effect is as large as 42% of the observed parent-child earnings correlation.

Children may benefit in multiple ways from peers with higher-earning parents, who often possess more human capital (Adermon et al., 2021) and better-paying jobs (Dobbin and Zohar, 2023; Forsberg et al., 2024). Exposure to such peers can promote skill spillovers and open doors to higher-paying jobs. I find limited evidence that this exposure boosts educational attainment. Instead, by examining wage and career paths, I show that former schoolmates leverage these connections to enter better-paying firms and benefit from peers' career advancements.

These findings indicate that social interactions contribute to the persistence of inequalities across generations. Identifying this mechanism is crucial for understanding intergenerational mobility. For instance, disparities in opportunities across social settings, such as neighborhoods (Chetty and Hendren, 2018), may be influenced by peer exposure. These insights highlight the policy relevance of fostering interactions among children from diverse backgrounds to promote access to opportunities.

In the first part of the paper, I estimate the causal effect of peer exposure exploiting variation in school composition as suggested by Hoxby (2000).[1] This approach addresses the key concern of *endogenous sorting* into schools: if students choose schools based on traits correlated with individual outcomes, such as ability, the observed relationship between peers' parental income and individual earnings could be spurious. To address this concern, I focus on variation among students who attended the same school but in different cohorts. Additionally, to isolate unexpected variation in school composition, I rely on idiosyncratic, cohort-specific deviations from school time trends, which I argue are unanticipated and exogenous from the students' perspective.[2]

A second concern relates to the possibility of *correlated shocks* that could influence individual outcomes similarly among group members, independent of social interactions. For instance, an increase in local labor demand could raise both peers' parental earnings and one's own earnings, even without peer effects. I demonstrate that the main results remain robust when controlling for cohort-by-municipality fixed effects, indicating that local economic fluctuations are not driving the findings. In the paper, I address similar concerns and present evidence supporting that *correlated shocks* are not affecting the results.

I find that a $1 increase in schoolmates' average parental earnings results in a $0.08 increase in adult yearly earnings. This effect is as large as 42% of the parent-child earnings correlation.[3] Children from different parental backgrounds experience a similar exposure effect. However, the effect is nonlinear. The effect of each extra unit of earnings on schoolmates' parental earnings' is decreasing in the level of schoolmates' parental earnings. Thus, a marginal change in peer exposure has a larger effect on adult earnings for children exposed to lower-SES peers.

[1]This approach has been extensively applied in the literature. Among others: Black et al. (2013); Carrell et al. (2018); Brenøe and Zölitz (2020); Cattan et al. (2022); Mertz et al. (2024).

[2]I provide several robustness checks to support this assumption, among others, showing that deviations in cohort composition are uncorrelated with students' predetermined characteristics.

[3]The results are robust to alternative measures of individual earnings such as percentile ranks.

What is the mechanism driving this effect? To explore whether it is driven by increased educational opportunities or improved access to higher-paying jobs, I analyze the impact of exposure to higher-SES peers on both outcomes. A one-standard-deviation increase in average schoolmates' parental earnings has a small positive effect on the probability of obtaining a college degree (+0.9 p.p., +1.3%) and a more significant effect on the probability of working at higher-paying plants, with a +0.2 p.p. increase (+10%) in the plant-specific wage premium at the age of 30.[4]

Motivated by these findings, I focus on the role of labor market interactions in determining exposure effects in the second part of the paper. I first document stark differences in access to higher-paying firms and occupations by parental background. Next, I show that *social ties* formed among schoolmates persist in the labor market, leading to spillovers of the advantages inherited from their parents.

First, school connections determine access to jobs. Comparing the probability of joining the plant of an *actual* schoolmate versus *almost* schoolmates (i.e. those who attended the same school in the cohort immediately before or after), I find that 1.4% of students join a firm due to a high school social tie.[5] Moreover, while I find that *connected hires* are more frequent among students from similar parental backgrounds, I document how low-SES students also join high-SES peers' plants because of exposure in high school.

Second, workers benefit from the career advancements of their former schoolmates. As highlighted by Manski (1993), identifying the impact of a peer's achievement (such as a promotion to a managerial position) is challenging because shared characteristics within the group may influence such an event. To address this challenge, I develop a novel research design that exploits variation in the timing of peers' promotions. While common group characteristics may affect the probability of a promotion, some varia-

[4]Plant-specific wage premia are computed as in Abowd et al. (1999) and Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

[5]This approach applies to the school setting identification designs that use variation in the timing of employment within a firm to test for the role of coworkers as social ties on the labor market (Hensvik and Skans, 2016; Caldwell and Harmon, 2019; Glitz and Vejlin, 2021).

tion in promotion timing is likely idiosyncratic. Leveraging this exogenous variation in a difference-in-differences framework, I find that a schoolmate's promotion increases peers' hourly wages by \$1.53 in the subsequent years. I present evidence suggesting that this effect is consistent with peers offering outside options triggering wage negotiations, as suggested by job search models featuring on-the-job search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014).

This paper contributes to the intergenerational mobility literature by highlighting how social interactions affect access to opportunities. While seminal theoretical works (Benabou, 1993; Durlauf, 1996) underscore the incentives for parents to form homogeneous peer groups due to the impact of their investments on neighboring children,[6] evidence on the relationship between peer exposure and social mobility is scarce due to extensive data requirements and significant identification challenges.

In a major contribution to address data limitations, Chetty et al. (2022a) collect Facebook friendship data for the U.S. to construct large-scale measures of network segregation, revealing a negative correlation between social network segregation and intergenerational mobility rates across space. My paper contributes to this literature by exploiting a different data source and focusing on high school exposure, showing that exposure to higher-SES peers has a causal effect on adult earnings.

Moreover, an important strand of research examines how the economic opportunities of children are shaped by the neighborhoods they grow up in (Chetty and Hendren, 2018).[7] I add to this body of work by highlighting social interactions as a key factor in social mobility, distinct from the influence of local economic conditions. Specifically, the findings of this paper suggest that the characteristics of individuals residing in different communities are likely to play a role, as *potential peers*, in determining the

[6] Consistently, Abdulkadiroglu et al. (2020) and Eshaghnia et al. (2023) show that parents value peer exposure in selecting neighborhoods and schools.

[7] *Neighborhood effects* are identified using plausibly random variation in children's ages at the time of moving, following the approach by Chetty and Hendren (2018), and replicated with similar findings in Africa (Alesina et al., 2021), Australia (Deutscher, 2020), and Israel (Aloni and Avivi, 2024).

disparities in access to opportunities observed across neighborhoods.[8]

Finally, I consider my findings closely related to Cattan et al. (2022), who show that Norwegian high school classmates enhance access to elite colleges if their parents are alumni of those institutions. My paper differs in that it focuses on the transmission of earnings and access to jobs rather than on education and access to colleges.

The second strand of literature this paper contributes to is the extensive research on the role of social ties in labor markets. Since Granovetter (1983), there has been considerable focus on how social ties among workers facilitate access to jobs, for example, providing potential employers with information on candidates' productivity (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). More recently, a growing body of work is exploring how parents influence access to firms (Kramarz and Skans, 2014; Staiger, 2023), occupations (Ventura, 2024), and higher-paying jobs (Dobbin and Zohar, 2023; Forsberg et al., 2024). This paper adds to this literature by demonstrating how peer exposure affects job access, potentially passing on advantages inherited from parents to schoolmates.

A third contribution of this paper is the development of a novel identification strategy that leverages the timing of managerial promotions to identify their effects on former schoolmates. This approach addresses an identification issue related to the *reflection problem* formalized by Manski (1993).[9] Group characteristics could confound the relationship between promotions and peers' wages. Assuming groups with and without promotions would have the same wage growth absent the promotion, the timing differences allow identification of effects independent of group composition in a difference-in-differences framework.[10]

[8] Several structural models aim at identifying the role of peer effects in determining social mobility (Fogli and Guerrieri, 2019; Agostinelli et al., 2020; Eckert and Kleineberg, 2021; Chyn and Daruich, 2023). This paper complements this strand of work by developing a research design aimed at identifying the causal effect of interest exploiting a natural experiment.

[9] Manski (1993) examines the impact of peers' outcomes on the same outcome for an individual, whereas I focus on how a distinct peer outcome (promotion) affects a specific individual outcome (wage).

[10] I apply to this setting the empirical specifications and inference techniques from recent advances in difference-in-differences designs with staggered treatments, as in Callaway and Sant'Anna (2021).

This method exploits variation in the timing of the event of interest, while existing approaches typically rely on variation in the probability of such events through instruments (Moffitt, 2001; Lalive and Cattaneo, 2009; Rossi and Xiao, 2023) or network structure (Bramoullé et al., 2009; De Giorgi et al., 2010). To my knowledge, the closest related exercise is Caldwell and Harmon (2019), who use time variation of a continous measure of workers' outside options in a model with worker fixed effects. While the event I examine (a peer's promotion) is more narrowly defined than theirs (any job transition among peers), my framework allows testing for potential pre-trends.

This paper's final contribution is identifying long-lasting peer effects stemming from exposure to High School peers. The results of this paper add to a large body of evidence on peer effects. In this direction, the closest results to this paper are in Carrell et al. (2018), who finds evidence of reduced earnings as a consequence of exposure to *disruptive peers* in elementary school and Fruehwirth and Gagete-Miranda (2019), who finds that kindergarteners whose classmates' parents have higher education, have higher educational outcomes.

These results enhance our understanding of social interactions as a key factor in intergenerational mobility. On one side, the causal effect of peer exposure identified in this paper and the observed school segregation by parental earnings suggests that social interactions reinforce inequalities. At the same time, this finding can inform policy discussions aimed at improving access to opportunities, such as school desegregation.

The rest of the paper is organized as follows: Section 2 describes the sample and the institutional framework. Section 3 presents the empirical strategy and the results concerning the effect of exposure to schoolmates. Section 4 presents descriptive evidence on the differential access to higher-paying firms by parental background. Section 5 presents the results on labor market networks and the effect of peers' promotions on wages. Finally, section 6 discusses the results, and section 7 concludes.

2 Institutional Framework and Sample Description

2.1 Institutional Setting: Danish High-Schools

Danish students complete compulsory education by the 9^{th} grade, typically at age 16. Attendance of an extra 10^{th} grade is optional. After finishing lower secondary education, students can choose to either enroll in a high school that grants access to tertiary education, attend a vocational school, or discontinue their education[11].

High school programs range from 2 to 3 years, depending on the track chosen, and aim to prepare students for tertiary education or entry into the labor market. They are organized into four principal tracks, each featuring distinct curricula. These tracks are tailored to prepare students for university-level studies or provide more technical and business-oriented training. Each track includes compulsory courses and elective subjects, allowing for some degree of curricular flexibility.

Admission to high school is conditional upon the successful completion of lower secondary education. Students submit ranked preferences for high schools and tracks. School placement is determined at the national level based on students' residential addresses: when preferred schools are oversubscribed, students are assigned to a similar school within their district. Although high schools are self-governing institutions, their funding primarily comes from state transfers, and tuition fees are either absent or minimal[12].

2.2 Sample Selection

Administrative registers covering the universe of the Danish population from 1980 to 2019 are the primary data sources of this paper. The sample includes the students who

[11] In the years considered in this paper, roughly 45.6% of the students enrolled in a high school by the age of 19.

[12] According to OECD statistics, 3% of the expenditure on upper and lower secondary education is directly financed by households (Nusche et al., 2016).

enrolled in a Danish high school from 1997 to 2007[13]. Parents and schoolmates are identified through family and school records, respectively. Additionally, earnings are tracked annually using tax records, while labor market outcomes are observed each year through employer-employee matched data for both the children and their parents.

Of the 387, 061 students who enrolled in Danish high schools from 1997 to 2007, I exclude 22, 848 (5.9%) who enrolled after the age of 19, and 9, 496 (1.3%) with missing information on adult or parental earnings. Additionally, I drop 1, 775 (0.4%) students from cohorts where more than 50% of their peers enrolled after age 19, and 2, 078 (0.5%) students who attended schools with fewer than 4 consecutive years of observation. The final sample consists of 350, 864 students from 339 schools across 11 cohorts.

2.3 Measures of Earnings and Peer Exposure

Individual Earnings Earnings are measured as the average annual earnings from the main occupation and self-employment before tax from age 28 to 32, as reported in tax registers. For ease of interpretation, earnings are reported as of 2015 USD dollars.

Individual Earnings - Ranks As high-SES individuals engage in longer education, measuring earnings in levels at the age of 30 might not capture differences in lifetime earnings. To address concerns due to differences in lifecycle wage profiles, I construct percentile ranks of individual earnings (as defined above) relative to the in-sample distribution of children born in the same cohort.[14]

Parental Earnings For each parent, I measure earnings as the average yearly earnings from the main occupation and self-employment over their child's first 18 years of life. I then construct parental earnings as the average among the parents of each child.

[13]These cohorts are chosen to include the children born between 1980 and 1987. Those are the individuals observed until the age of 32 in 2019, the last year of observation.

[14]Evidence from Sweden suggests that the rank-rank correlation is less sensitive to measurement issues due to different lifecycle wage profiles (Nybom and Stuhler, 2016).

Parental Earnings - Ranks I construct percentile ranks of parental earnings (as defined above) relative to the in-sample distribution of children born in the same cohort.

School-Cohort Schoolmates are identified as the children who enrolled at the same school and track in the same cohort as the child of interest, excluding the child itself.

2.4 Descriptive Statistics

The main characteristics of the sample are reported in Table 1. The sample includes $350,864$ students enrolled in 11 cohorts across 339 schools. The average student has 152 schoolmates; her parents earn $\sim$ \$47,500 per year, and she earns $\sim$ \$44,000 per year by the age of $28-32$.

[Table 1 HERE]

Table 2 reports conditional means of adult, parental, and schoolmates' earnings by quartile of parental earnings. Two main facts emerge from the table. First, children from higher-income parents tend to have higher earnings themselves. The difference in earnings between children from the top and the bottom quartile is substantial: \$10,872.59 or 9.26 percentile ranks. Second, differences in parental earnings are mirrored in schoolmates' parental earnings. The difference between the schoolmates' parental earnings of children from the top and the bottom quartile is \$5,656.8 and 7.12 percentile ranks, amounting to $\sim$ 10% of the difference in their own parental background.

[Table 2 HERE]

Finally, Figure 1 plots the conditional means of schoolmates' parental earnings (blue dots) and neighbors' parental earnings (orange circles), by percentile of own parental earnings[15]. Most of the segregation in schoolmates' parental earnings emerges at the right end of the earnings distribution, and the same pattern is observed for neighbors.

[15] For each student, I define as her neighbors the kids who live in the same municipality and enroll in a high school in the same year as herself.

These facts are informative on the institutional context of Danish high schools: free access to education is likely to mitigate segregation, especially at the bottom of the earnings distribution, while residential segregation potentially drives sorting into more homogeneous peer groups at the top of the distribution.

[Figure 1 HERE]

On the one hand, the correlation of parental backgrounds among schoolmates challenges the identification of exposure effects, as it suggests that unobservable characteristics of students' families might be correlated among schoolmates. On the other hand, if exposure to peers matters, school segregation results in children from higher-income families enjoying a double advantage: the first from their own family background and the second from the peers they are exposed to. The research design presented in the next section addresses the main threats to identification by comparing students who sorted in the same High School in different cohorts.

3 Peer Exposure

3.1 Research Design

Random assignment of children to schools would serve as the ideal experiment to estimate exposure effects. In the absence of such an experiment, unobserved determinants of outcomes might be correlated within groups because of endogenous sorting or group-level correlated shocks. Individuals sharing similar unobserved characteristics (such as ability) might sort in the same group or common shocks (such as changes in local economic conditions) might simultaneously affect group members and generate correlation in outcomes even in the absence of peer effects.

To address this issue, I follow a within-school across-cohort design, as introduced by Hoxby (2000).[16] Namely, I compare cohorts within the same school and leverage as identifying variation only the deviation from the school-specific time trend of school composition. To do so, I estimate the following model:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 \bar{X}_{-i} + Z_i' \delta + \gamma_{s(i)} + \tau_{s(i)} c(i) + \varepsilon_i. \tag{1}$$

Y_i and X_i represent earnings for each child i and her parents, respectively; $\bar{X}_{-i}$ is the leave-one-out mean of i's schoolmates' parental earnings; $c(i)$, $s(i)$ denote the cohort and the school of individual i, respectively. Hence, $\gamma_{s(i)}$ is a set of school fixed effects, and $\tau_{s(i)} c(i)$ represents school-specific time trends. Finally, Z_i is a vector of predetermined individual characteristics such as gender, year of birth, and year of birth of each of the parents. The main coefficient of interest (β_2) captures the marginal effect of peer's parental earnings on individual earnings. As such, this parameter is a combination of the direct effect of exposure to peers from different SES and the indirect effect emerging from peers' achievements.[17]

[16] Among several subsequent applications of the same identification, the closest to mine are Black et al. (2013), Carrell et al. (2018), Brenøe and Zölitz (2020), Cattan et al. (2022) and Mertz et al. (2024).

[17] In subsection 5.2, I exploit time variation schoolmates' promotions to identify their effect on peers' wages, separately from exposure to different parental backgrounds.

The main identifying assumption is that deviations from school-specific time trends in school composition are as good as random from the individual's perspective. I argue that the identifying variation captures deviations from expected school composition partially arising from idiosyncratic shocks to birth timing, which affect the composition of schoolmates without triggering parental responses like moving away from districts experiencing adverse shocks. In the following section, I provide evidence supporting this claim and further present findings that rule out *correlated shocks* experienced simultaneously by group members as a potential driver of the results.

3.2 Support for the validity of the identifying assumption

In this section, I provide several pieces of evidence supporting the identifying assumption that deviations from school-specific time trends in school composition are uncorrelated with unobserved determinants of students' earnings.

First, Table Table 3 presents a comparison of the standard deviation of $\bar{X}_{-i}$ and of its residuals obtained from regressing the same variable on school-specific time trends. While most of the variation in peers' parental background is captured by time trends, residuals account for one-fourth of the variation in school composition. This is the identifying variation leveraged in the main specification.

[Table 3 HERE]

Second, as shown in Figure 3, the distribution of the residuals of school composition is well approximated by a normal distribution, supporting the intuition that deviations from expected school compositions are as good as random.

[Figure 3 HERE]

Third, individual predetermined characteristics, including own parental earnings, are uncorrelated with the residuals in school composition. One could consider this as a balance test: if students were to anticipate deviations from school composition, their characteristics would be correlated with such deviations. For example, children from higher earnings or better-educated parents would enroll in school cohorts experiencing positive deviations from their expected composition. Figure 5 reports the coefficients from a balance test regressing school composition on several predetermined characteristics, including school-specific time trends.[18] Table 6 reports the same coefficients in table format. As summarized in Table 5, only 1 of the 28 variables considered report a correlation with the residuals of school composition statistically different from zero at 99% confidence level.

[Figure 5 HERE]

[Table 4 HERE]

Fourth, cohort-specific deviations from school time trends are uncorrelated over time. This result confirms that residual variation in school composition does not follow a predictable pattern, reinforcing the idea that students are unlikely to anticipate these changes. In Table 7, I present evidence from school-specific time series regression testing for autocorrelation in cohort composition. Upon inclusion of linear time trends, only 4.5% of the schools in the sample exhibit a correlation over time in school composition statistically different from zero at 90% confidence level.

[Table 7 HERE]

Overall, I interpret the evidence collected so far as supporting the identifying assumption that deviations from school-specific time trends in school composition are as good as random from the individual's perspective. However, a further potential concern involves *correlated shocks* at the group level, potentially driving deviations in school composition and in individual earnings simultaneously.

Such group-level shocks might involve changes in school policies following the intake of a higher earnings cohort. However, the centralized funding of Danish High Schools

[18] I standardize dependent variables and include as a regressor the school level average parental earnings to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by Guryan et al., 2009 and applied to a similar context by Brenøe and Zölitz, 2020.

is designed to equalize access to resources between schools and is financed through national-level taxation, thus making this event unlikely. To further address this concern, I estimate the effect of shocks from adjacent cohorts regressing adult earnings on school composition in adjacent years. As shown in Table 8, shocks to previous (and future) cohorts do not affect earnings. To drive the results, correlated shocks at the school level should affect the cohort of interest and leave no trace on subsequent cohorts. I interpret this as evidence that group-level shocks do not drive the results at the school level.

Alternatively, cohort-specific fluctuations of local economic conditions might affect both parental earnings and children's outcomes. However, the long time span over which parental earnings are measured is likely to capture permanent earnings rather than transitory fluctuations. Consistently, as shown in Table 9, the coefficient of interest is robust to inclusion of cohort-by-municipality fixed effects in the regression. Overall, I interpret these results as supporting the identifying assumption that correlated shocks are unlikely to drive the results.

3.3 Results

[Table 10 HERE]

Table 10 displays the main results of this paper, reporting OLS estimates of the coefficients of model (1). Earnings are measured as percentile ranks in columns (1) – (4) and in 2015 USD in columns (5) – (8). Results are reassuringly comparable across the two measures.

In column (1), the coefficient on parental earnings is the rank-rank coefficient measuring parent-child correlation in earnings.[19]Each extra percentile in parental earnings is associated with 0.16 increase in children earnings. Column (2) reports the cor-

[19]While this number is slightly lower than in similar studies (Landersø and Heckman, 2017), one has to consider that the sample of this paper is not representative of the entire population of Denmark, but of the set of students who enrolled in high school.

relation of adult earnings with parental earnings and peers' parental earnings. The decrease in the magnitude of the coefficient of parental earnings from column (1) to column (2) highlights the positive correlation between own and peers' parents. Finally, column (3) includes school and cohort fixed effects, and column (4) includes school-specific linear time trends. My preferred specification is in column (4), as including school-specific time trends is more likely to prevent the results from endogenous sorting into schools as described in section 3.2. The coefficient of interest is positive and statistically different from zero at the 99% confidence level in all specifications.

Two main results derive from the estimates in Table 10. First, the coefficient of interest is positive and statistically different from zero at 99% confidence level suggesting that peer exposure affects adult earnings. A 1 percentile increase in schoolmates' average parental earnings results in a 0.067 percentile increase in adult yearly earnings. When earnings are measured in nominal terms, a $1 increase in schoolmates' average parental earnings results in a $0.08 increase in adult yearly earnings. Second, the magnitude of the effect is 41.6% of the parent-child correlation in earnings when earnings are measured in percentile ranks and 42.8% when earnings are measured in nominal terms. Overall, the results suggest that exposure to schoolmates' parental earnings is a statistically significant and quantitatively important determinant of adult earnings.

3.4 Effect Heterogeneity and Nonlinearity

[Figure 6 HERE]

Figure 6 documents the heterogeneity of the exposure effect on students from different levels of parental earnings. The graph reports the point estimate and the 90% confidence intervals of the marginal impact of exposure to schoolmates' parental earnings implied by including in the model from eq. 1 a complete set of interaction dummies for each tercile of the distribution of parental earnings. The effect is homogeneous with respect to parental background: an average extra unit in peers' parental earnings benefits children from different parental backgrounds similarly.

[Figure 7 HERE]

However, the exposure effect is nonlinear. Figure 7 plots the marginal effects and the relative 90% confidence intervals from estimating a version of the model in eq. 1 where a quadratic polynomial for average schoolmates' parental earnings is included. The effect is evaluated at different levels of exposure to peers' parental earnings[20]. The effect is decreasing in the level of peers' parental earnings, being indistinguishable from zero for children exposed to the highest decile of the distribution of parental earnings.

Overall, the results suggest that exposure to schoolmates' parental earnings is a significant determinant of adult earnings. The magnitude of the effect is such that for a given difference in parental earnings, a change in exposure of the same magnitude would close 42% in the earnings gap. Moreover, the decreasing marginal effect of average school composition suggests that interventions aimed at desegregating schools might achieve higher levels of aggregate earnings by reallocating low-SES students between the most segregated schools, from schools with the worst average composition to those with the best average composition. This would improve peer exposure, where it has the higher marginal effect, and worsen peer exposure in schools where effects are more attenuated.

Given the magnitude of the estimated effect, understanding how exposure to schoolmates' parental earnings influences adult earnings is crucial. In the next section, I will provide evidence on the impact of exposure to schoolmates on education and labor market outcomes.

[20]The levels are chosen as the deciles of the average school composition and are reported as labels in the graphs after rounding them to the closest hundreds.

4 Education and Labor Market Outcomes

4.1 Parent-Child Correlation

In this section, I provide evidence on the vertical correlation of education and labor market outcomes with parental earnings: children from higher-earnings parents are more likely to obtain a College degree and have higher-paying jobs.

Table 11 documents education levels and labor market outcomes at the age of 30 for the whole sample and conditioning on terciles of parental earnings. As shown in the table, 73.2% of the students eventually obtain a university degree. This probability increases with parental earnings: children from the highest tercile are 15.7 *p.p.* (24%) more likely to get a university degree than children from the bottom tercile. When labor market outcomes are considered, a similar pattern emerges. Higher-SES children are 4.3 *p.p.* (5.8%) more likely to be employed and, conditional on being employed, to have managerial position 0.4 *p.p.* (18.1%). Finally, they work at higher-paying plants. By the age of 30, the difference in plant-specific pay premia between low and high-SES is 2.8 *p.p.* (350%).[21] These differences are reflected by differences in hourly wages, which are $3.26 (11.2%) higher for children from families at the top tercile of the earnings distribution than those from the bottom tercile.

[Table 11 HERE]

Do any of these differences spill over to schoolmates upon exposure? As children from higher-earning parents have higher levels of human capital and higher-paying jobs, does exposure to higher-SES schoolmates result in higher levels of education or better labor market outcomes? In this section, I estimate the effect of peer exposure on education and labor market outcomes by the age of 28-32.

[21]Plant-specific pay premia are estimated by decomposing wages for the entire population of Danish workers from 2000 to 2019 into the plant and worker-specific components as proposed by Abowd et al. (1999) and implemented among others by Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

4.2 Exposure Effects on Education and Labor Market Outcomes

Table 12 reports OLS estimates of the coefficients in eq. 1 where the dependent variable is replaced with the outcome of interest and parental earnings are standardized to have mean zero and standard deviation one. The lower panel of the table reports a different set of regression coefficients where all the independent variables are interacted with dummies for each tercile of parental earnings. For ease of comparison, the first column of the table reports the effect of a one standard deviation increase in peer exposure on lifetime earnings in nominal terms. A one-standard-deviation increase in average schoolmates' parental earnings increases lifetime earnings by \$596.32 (1.3%). When other outcomes are considered, the same difference in exposure results in a positive effect on the probability of obtaining a college degree (+1.2 p.p., +1.6%), an increase in hourly wages of \$0.30(1.1%), an increase in the probability of having a managerial position 0.4 *p.p.* (18.1%) and an increase in plant-specific pay premium of 0.2 *p.p.* (10.0%). When the effect is considered by terciles of parental earnings, while the point estimates suggest a larger effect of exposure on education for high-SES and on labor market outcomes for low-SES children, the confidence intervals are large, and one cannot reject the null hypothesis of homogeneity of the effect across terciles of parental earnings.

[Table 12 HERE]

While showing that exposure to higher-SES peers has a positive effect on education, potentially via spillovers in human capital formation as in Fruehwirth and Gagete-Miranda (2019) or transmission of information as suggested by Cattan et al. (2022), the results in Table 12 stress the importance of labor market outcomes as a key mechanism in the transmission of inequalities across generations. Given a $\$8,178.66$ earnings premium for college-educated individuals in the sample, a back-of-the-envelope calculation suggests that a 1.2 p.p. increase in the probability of obtaining a college degree due to exposure to higher-SES peers would result in a \$98.15 increase in earnings, amounting to 16.5% of the effect measured on hourly wages. I interpret this as evidence of alternative mechanisms, along with spillovers on educational achievement, driving the effect of peer exposure on adult earnings. The next section of the paper investigates the role of labor market interactions among former schoolmates in this process.

5 Former Schoolmates as Weak Ties on the Labor Market

Do friendships formed in school impact career paths and job prospects? Can classmates help gain access to higher-paying companies and higher wages? In this section of the paper, I show that former schoolmates' career paths are interconnected: 1.4% of the students in the sample secure a job at a company due to school connections, and the average worker experiences a $3.32 (+10.0%) increase in hourly wage as a peer is promoted to a managerial position.

5.1 Connected Hires

I begin by examining the effect of school connections on access to jobs. Do schoolmates facilitate access to the firms where they are employed? If so, do low-SES students leverage their high-SES former schoolmates' networks to help bridge the labor market outcome gaps identified in the previous section?

Identifying workplace changes driven by social network effects is challenging due to the endogenous nature of social interactions. Social relationships are often characterized by homophily, whereby individuals within a group tend to have correlated observable and unobservable characteristics. As a result, individuals might join the same firm because they share characteristics that make the firm appealing to them (e.g. skills or location) rather than because of social connections.

High Schools provide a natural experiment to address this identification challenge. *Almost* schoolmates (i.e., individuals who enrolled in the same high school in adjacent cohorts) are likely comparable to *actual* schoolmates, as they sorted into the same High School at a similar time. However, they are less likely be social ties.[22] Therefore, comparing the share of individuals who join the plant of an actual schoolmate to the share of individuals who join the plant of an almost schoolmate is informative on how

[22] This approach applies to the school setting identification designs that use variation in the timing of employment within a firm to test for the role of coworkers as social ties on the labor market (Hensvik and Skans, 2016; Caldwell and Harmon, 2019; Glitz and Vejlin, 2021).

many job switches are due to school connections. Intuitively, if the share of connected switches is higher for *actual* schoolmates than for *almost* schoolmates, this suggests that social connections formed in high school are a determinant of workplace changes.

I apply the following procedure. First, I compute the share of individuals who join a plant where any of her *actual* schoolmates is employed. Then, for each cohort within each school, I randomly draw without replacement a set of *almost* schoolmates as large as the number of *actual schoolmates* and compute the share of individuals who joined a plant where any of their *almost* schoolmates are employed. To avoid simultaneous moves confounding the measurement, I condition on the joined peer being employed at the receiving plant for at least one year before the switch. I consider as a workplace the plant[23] of main employment in November of each year and focus on changes in workplaces happening from the 4^{th} to the 14^{th} year since enrollment in high school.

By conducting independent draws of *almost* schoolmates and computing the share of students who join the plant of an *almost* schoolmate at each draw, I construct a counterfactual distribution representing the share of switches that would have been directed at schoolmates under the null hypothesis of no network effects. I then use this counterfactual distribution to test whether the share of connected switches is higher than in the absence of network effects, computing p-values as the share of draws that resulted in a lower probability than the realized one.

[Table 12 HERE]

Figure 9 reports the main results of the analysis. Orange bars show the probability of joining a the plant of an *actual* schoolmate. Blue bars display the probability of joining the plant of an *almost* schoolmate as the average across 1,000 independent draws of almost schoolmates from adjacent cohorts. Probabilities are computed separately for each tercile of parental earnings. Network effects are positive and statistically signifi-

[23]Plants are assigned a unique identifier by DST. While changes of identifiers over time for the same establishment are not infrequent, they affect firms of *actual* and *almost* schoolmates at the same rate and thus are not a concern.

cant for the subpopulations considered.

Former schoolmates facilitate access to their plants. As shown in panel (*a*) of the table, 12.9% (15.5%) of low-SES (high-SES) joined the plant of a former schoolmate from 4 to 14 years after enrollment in high school. Out of these changes in workplace, 1.18*p*.*p*. (7.6%) and 1.56*p*.*p*. (10.1%) are driven by social connections developed in high school by low and high SES respectively.

Switches to peers' workplaces are clustered by parental background. Panel (*b*) displays the probability of joining a peer's plant, conditional on such peer having parents from the top tercile of the distribution of parental earnings. High-SES children are 70% more likely to join a peer from the same parental background than low-SES children. Despite this fact, students from all parental backgrounds do join high-SES plants because of school connections.

Differences in access to workplaces where high-SES peers work reflect differences in access to high-paying plants.[24] Panel (*c*) shows the probability of joining a peer at a high-wage plant, confirming that high-SES children are 55% more likely to join such establishments than low-SES children. However, school connections drive 15.4% (0.46*p*.*p*.) of such switches for low-SES and 17.3% (0.66*p*.*p*.) for high-SES workers. This suggests that while high-SES children are more likely to join high-wage plants, low-SES children are still able to leverage school connections to access high-wage workplaces.

This result is confirmed even when we restrict our focus to switches aimed at high-wage plants connected by a high-SES peer. Panel (*d*) shows the probability of joining a high-SES peer at a high wage plant. Also, for this restricted set of switches, school connections are a statistically significant determinant of workplace changes for both high and low-SES children.

Overall, school exposure is a significant determinant of workplace changes. High school connections determine a change in workplace directed at a plant where a for-

[24]High wage plants are defined as the establishment whose plant fixed effect is at the top quartile of the national distribution of plant-specific AKM fixed effects as in Abowd et al. (1999).

mer schoolmate is employed for 1.4% of the individuals in the sample. Moreover, while high-SES students are more likely to leverage the connections developed in school and join high-SES students at high-wage firms, school connections open door to higher paying jobs for low-SES students too.

An unresolved question from this analysis concerns the magnitude (and direction) of wage gains resulting from workplace changes facilitated by school connections. Developing an identification strategy to address this issue is a potential avenue for future research. However, the evidence presented so far indicates that school connections influence access to employment opportunities. In the next section, I focus on a direct implication of this finding: if peers offer job opportunities, do they also improve peers' bargaining power in wage negotiations when their career prospects improve, such as after a promotion to a managerial role?

5.2 Wage Spillovers

Social ties might facilitate career advancements even without attracting peers to their firm. Since peers facilitate access to new job opportunities, as shown in the previous section, workers may leverage their peers' outside options to negotiate higher wages with their current employers, even without changing workplaces. Workers receiving an outside offer from a former schoolmate employed at a different firm may use it as a bargaining tool, as their current employer may find profitable to match the offer to retain the worker by raising their wage. This mechanism is standard in job-search models that feature on-the-job search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014).

The ideal experiment to test for spillovers from peers' outside options would involve exogenous variation in peers' career trajectories. However, in natural settings, such variation is likely spurious due to self-selection into peer groups or endogenous peer effects affecting peers' careers. For example, the promotion of a worker to a managerial position is likely to be associated with individual earnings determinants, such as human capital or skills. These characteristics may be correlated among peers due to the sorting of similarly skilled individuals into the same group. Or, even absent such sorting, prior exposure to the same peers may have influenced individual productivity, confounding the observed relationship between peers' labor market outcomes and individual wages.

This identification problem has been formalized by Manski (1993) as the *reflection problem*. Possible solutions have been proposed exploiting exogenous variation in individual outcomes due to exposure to group-level shocks (Moffitt, 2001; Lalive and Cattaneo, 2009; Rossi and Xiao, 2023) or non-overlapping peers (Bramoullé et al., 2009; De Giorgi et al., 2010). In this section, I propose a different approach by exploiting exogenous variation in the timing of peers' outcome realizations. Intuitively, while previous exposure to peers might influence the probability of being promoted to a managerial position, some variation in the timing of promotions is likely to be exogenous to individual and group characteristics. Exploiting this exogenous variation, the comparison of wages of individuals who already experienced a peer's promotion and those who have not yet identifies the effect of peers' labor market outcomes on individual wages.

Namely, I estimate the following model of difference-in-differences:

$$W_{sc,t} = \alpha_{sc}^{\tau} + \alpha_{t}^{\tau} + \sum_{l} \delta_{l}^{\tau} \left(M_{sc}^{\tau} \cdot \mathbb{1} \left\{ t = \tau + l \right\} \right) + \epsilon_{sc,t}. \tag{2}$$

Where $W_{sc,t}$ represents the average wage of the members of group sc (those who attended high school s in cohort c) at year t, M_{sc}^{τ} is a dummy variable equal to one if the group sc experienced a peer's promotion at year τ, and 0 if it did not yet. I consider only the first promotion to manager for each group, and I exclude the individuals who became managers from the sample. The coefficient δ_{l}^{τ} measure the effect of a peer's promotion on individual wages l years after the treatment, for those who experienced a promotion in year τ. I follow the procedure in Callaway and Sant'Anna (2021) to estimate the coefficients of interest for each year of treatment τ and aggregate them to compute dynamic treatment effects. The identifying assumption is that in the counterfactual scenario where the peer's promotion did not occur, the average wage of the group would have changed by the same amount as the average wage of the groups that had not yet experienced the peer's promotion.

[Figure 11 HERE]

Figure 11 plots the estimated dynamic treatment effects of a peer's promotion to manager on individual wages. The graph reports the point estimates and the 95% confidence intervals of the coefficients δ_{l}^{τ} in eq. 2 for each year after the treatment l, aggregated across different years of treatment τ as in Callaway and Sant'Anna (2021). The results suggest that a peer's promotion to manager has a positive effect on individual wages, resulting in a \$3.32 increase in hourly wages in the years following the promotion of a peer. Moreover, the absence of difference in trends between treated and control groups in the years before the treatment is reassuring about the validity of the identifying assumption.

[Figure 12 HERE]

To gain further insight into how a peer's promotion affects own wages, Figure 12 shows the effect of a peer's promotion to manager on individual wages, estimated separately for individuals who worked at the same establishment as the promoted peer in any period after the promotion (*joiners*) and for those who did not (*not joiners*). While the most significant wage gains are realized by the individuals who join the promoted peer, also those who do not join the new manager realize a wage gain of $1.62 in the years following the promotion. This indicates that the impact of a peer's promotion on an individual's wages may not only result from directly joining the promoted peer but also from the promotion's influence on the individual's ability to negotiate wages.

[Figure 13 HERE]

Finally, outside options improve bargaining positions the more appealing they are: everything else equal, a worker who is poached from a higher-wage firm can obtain a higher wage increase if her employer wants to retain the match. To test this implication, Figure 13 reports the effect of a peer's promotion, estimated separately for peer groups whose manager was promoted at a plant from the top and the bottom tercile of the distribution of plants' pay-premia.[25] The effect of a peer's promotion is larger when the promoted peer is employed at a high-wage plant. This result is in line with the interpretation of peers' promotion as an improvement in the outside option of the individual: the higher the productivity of the firm where the peer is promoted, the more attractive the outside option from a potential job offer and the larger the realized wage gains.

Overall, the results of this section suggest that social connections developed in high school provide outside options that individuals can leverage to negotiate higher wages.

[25] Plant-specific wage premia are computed as in Abowd et al. (1999) and Card et al. (2013). They can be interpreted as the percentage increase in wage paid to a worker upon employment at a given plant.

When a school connection gets promoted to a managerial position, her peers see a persistent increase in hourly wages, also conditional on not joining the promoted peer. Moreover, the wage gains are larger when the promoted peer is employed at a high-wage firm.

6 Discussion

This paper investigates peer exposure as a determinant for social mobility: parental inequalities are transmitted to children's peers via social spillovers. A $1 increase in schoolmates' parental earnings results in a $0.08 increase in adult yearly earnings. Moreover, stark differences in access to higher-pay jobs between children from different parental backgrounds are coupled with schoolmates facilitating access to jobs and higher wages. In this section, I will relate the results of this paper to existing literature and the institutional context in which they are found, illustrating how they advance our understanding of the determinants of international mobility and which questions are left open for future research.

Where children are raised significantly impacts their success: this has led economists to study *neighborhood effects* (Chetty and Hendren, 2018; Chyn, 2018; Alesina et al., 2021; Deutscher, 2020; Mogstad and Torsvik, 2021; Aloni and Avivi, 2024). At the same time, parents also value *with whom* their children interact when selecting neighborhoods and schools (Heckman and Landersø, 2022; Abdulkadiroglu et al., 2020; Eshaghnia et al., 2023). This paper contributes to this literature by providing causal evidence of the importance of social interactions in transmitting earnings across generations. When considered in the context of this literature, the results suggest that that *neighborhood effects* are partly driven by the social networks they expose children to.

High schools are a compelling setting to study peer exposure on adult outcomes. Considerable evidence has been collected on the crucial experience of high school years for individual development. For example, Cattan et al. (2022) documents the role of high schools in shaping children's educational choices, Carrell et al. (2018) shows how disruptive schoolmates affect lifetime earnings, and Black et al. (2013) and Brenøe and Zölitz (2020) show how high school gender composition affects long-run economic outcomes. At the same time, high schools are one of the institutional contexts where social networks are formed. In particular, Chetty et al. (2022b) document how cross-SES friendships are formed in U.S. high schools, stressing the role of *exposure* in offsetting the natural tendency of creating links within SES clusters. The results of this paper confirm this hypothesis, showing that social ties developed in high school are long-lasting and determine access to jobs and higher wages. This makes the question of how to design schools to foster cross-SES interactions a key policy target.

One limitation of the approach of this paper is that the endogenous behavioral responses of agents exposed to different peers remain unobserved. For instance, endogenous changes in parenting styles may counteract the effects of desegregation policies, as high-SES parents may prevent their children from interacting with schoolmates from lower-SES backgrounds (Agostinelli et al., 2020; Doepke and Zilibotti, 2017).[26] Nonetheless, the nonlinearities in the exposure effect identified in this paper suggest potential gains from a policy aimed at reallocating low-SES students between the most segregated schools, from those with the worst average composition to those with the best average composition. This would improve peer exposure where it has the higher marginal effect, and worsen peer exposure in schools where effects are more attenuated. However, addressing the nonlinearity of the exposure effect within contexts of endogenous network formation remains an open question for future research.

It is also important to consider the specificity of the Danish institutional context to draw conclusions on the validity of the present results in different settings. Providing equal access to education in Denmark is likely limiting students' segregation by parental earnings across schools. Still, schoolmates' parental background do affect

[26] A related but broader question lies in which interventions might be developed to limit such behavioral responses. For example, suppose parents restrict cross-SES interactions due to concerns about potential spillover effects related to risky behaviors. In that case, it may be worthwhile to couple desegregation policies with initiatives to curb these behaviors among students.

Danish adult earnings. This highlights the importance of considering the role of social networks in shaping economic outcomes, which might play an even more critical role in contexts where heterogeneity in education prices increases the correlation of parental backgrounds among schoolmates.

A key contribution of this paper is the analysis of the different roles high school peers play in fostering social mobility. I document how exposure to higher-SES peers leads to a limited increase in the probability of obtaining a college degree, but it significantly affects wages and access to higher-paying firms. It is instructive to consider these findings in relation to the institutional context of Danish upper secondary education. Danish high schools are not compulsory and are designed to prepare students for college. As such, they might attract students already inclined to pursue further studies, making them less influenced by peer exposure. Nevertheless, the findings on the significance of school connections for labor market outcomes indicate that the influence of peers extends beyond educational choices and has a broader impact on labor market success.

In particular, children exposed to higher-SES peers gain access to higher-paying firms and earn higher wages. These findings align with emerging literature showing significant differences in labor market outcomes based on parental background. For example, children often inherit occupations and employers from their parents (Kramarz and Skans, 2014; Staiger, 2023; Ventura, 2024), and those from higher-SES families tend to access higher-paying firms (Dobbin and Zohar, 2023; Forsberg et al., 2024). This paper highlights similar patterns and emphasizes the potential role of social exposure in reinforcing these differences. Children from high-SES families are not only more likely to be employed at high-paying firms, but they also benefit from being exposed to peers from similar high-SES backgrounds, as they facilitate access to higher-wage firms through their social networks.

In the last part of the paper, I show how school connections open doors to jobs and generate wage spillovers. These results are consistent with job search models that feature on-the-job search (Postel-Vinay and Robin, 2002; Cahuc et al., 2006). Workers can achieve wage gains by receiving attractive outside offers and using them to negotiate higher wages with their current employer. My findings are consistent with those of Bagger et al. (2014), who show that outside options are key for young Danish workers in advancing up the job ladder early in their careers. While I cannot rule out alternative mechanisms, such as peers providing information to update biased beliefs on the wage distribution, the causal evidence of spillovers from schoolmates collected in this paper highlights the lasting importance of social connections in accessing opportunities and achieving higher wages.

Finally, the results of this paper are to be considered as a novel addition to an existing literature on different types of peer effects affecting long term economic outcomes. Important results on the role of peers in shaping aspirations (Genicot and Ray, 2020), social norms (Bursztyn et al., 2018), expectations (Bellue, 2023), human capital (Fruehwirth and Gagete-Miranda, 2019) and social capital (Cattan et al., 2022) are complementary to those of this paper and stress different, but similarly important channels linking exposure to social mobility.

This paper's findings show how segregation in social interactions reinforces inequalities across generations. These insights deepen our understanding of the forces shaping social mobility and provide guidance for policies aimed at reducing inequality. Prominent policy options include school desegregation and the strategic design of shared spaces and leisure activities that promote interactions among individuals from diverse socioeconomic backgrounds.

However, two caveats must be considered when considering policy implications. First, the causal effects identified in this paper may not be policy-invariant. For instance, the impact of a large-scale peer redistribution policy could be less significant than suggested, as families may adopt more authoritarian parenting styles to mitigate perceived adverse effects from interactions with lower-SES peers. Second, any consideration of optimal policies should be framed within a clear normative statement of the policy objectives. While various arguments can be made in favor of reducing inequalities, it is beyond the scope of this paper to take a stand in this debate. Ultimately, the contribution of this paper is positive in its nature, emphasizing how social interactions facilitate the transmission of inequalities across generations.

7 Conclusions

This paper identifies the impact of interactions across socioeconomic groups on children's future earning potential. Exposure to peers from higher-SES families is shown to positively influence adult earnings, with a $1 increase in the average parental earnings of schoolmates resulting in an $0.08 increase in yearly adult earnings. While peer exposure has a limited effect on educational attainment, the connections formed in school persist into the labor market, facilitating access to higher-paying jobs and generating spillovers from peers' promotions. These findings highlight the value of policies aimed at fostering interactions among students from diverse backgrounds, such as school desegregation and the design of safe shared spaces. Overall, the insights from this paper identify a critical determinant of social mobility that can inform policy discussions and serve as a foundation for future research on the role of social interactions in shaping economic outcomes.

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Data Source

Danish Administrative Data: DST https://www.dst.dk/da/

Tables

Table 1: Descriptive Statistics

	Mean	Standard Dev.	N
Female	0.57	(0.50)	350,864
N of Schoolmates	152.55	(76.82)	350,864
Earnings 28-32	43,924.34	(26,113.03)	350,864
Earnings 28-32 (Rank)	50.47	(28.91)	350,864
Father - Earnings When Kid 0-18	60,690.43	(44,381.77)	345,848
Mother - Earnings When Kid 0-18	34,713.53	(19,286.28)	350,503
Parental Earnings	47,433.09	(25,891.19)	350,864
Parental earnings (Rank)	50.50	(28.87)	350,864

Note: The table presents summary statistics for the main sample of students enrolled in at a Danish high school from 1997 to 2007 and their parents, including the sample meana and standard deviation, along with the sample size (N) for each variable analyzed.

Table 2: Conditional Earnings

	Quartile of Parental Earnings			
	Q1	Q2	Q3	Q4
Parental earnings	23,284.68	40,763.86	50,543.88	75,141.88
Parental earnings (Rank)	12.56	38.48	63.64	88.12
Earnings 28-32	38,218.18	42,840.12	45,548.68	49,090.77
Earnings 28-32 (Rank)	43.56	49.33	52.82	56.18
SM Par. earnings	45,362.70	45,960.48	47,393.41	51,019.50
SM Par. earnings (Rank)	47.80	48.58	50.70	54.92
N	87,719	87,715	87,718	87,712

Note: The table presents average earnings outcomes for students in the sample, their parents and their schoolmates' parents, conditional on quartile of parental earnings. Earnings are measured in nominal terms (adjusted as 2015 USD) or in percentile ranks computed w.r.t. the distribution of students born in the same cohort belonging to the sample.

Table 3: Residual Variation in Schoolmates' Parental Earnings

	mean	sd	count
Schoolmates' Parental Earnings	50.50	9.29	350,821
Schoolmates' Parental Earnings - residual (linear trend)	-0.00	2.62	350,821
Schoolmates' Parental Earnings - residual (nonlinear trend, 2nd order)	0.00	2.41	350,821
Schoolmates' Parental Earnings - residual (linear trend, 3rd order)	0.00	2.23	350,821
Schoolmates' Parental Earnings - residual (moving avg.)	0.00	2.61	216,270

Note: The table presents descriptive statistics on schoolmates' parental earnings. Schoolmates parental earnings are measured as the leave-one-out average earnings of the parents of each schoolmate, excluding own parents. Parental earnings are measured as percentile ranks of earnings w.r.t. the sample distribution of parental earnings of students born in the same year and the average from age 0 to 18 of the child is computed. The table reports mean and standard deviations of the residuals of the same measure as resulting from a regression on school specific, linear and nonlinear time trends.

Table 4: Parental Earnings are Orthogonal to (residuals of) Peers' Parental Earnings

	(1)	(2)	(3)	(4)
	Par. Earnings	Par. Earnings	Par. Earnings	Par. Earnings
Schoolmates' Par. Earnings	0.044***	0.007	0.039***	0.006
	(0.012)	(0.011)	(0.011)	(0.011)
Observations	350821	350821	345801	345801
School and time FE	Yes	Yes	Yes	Yes
Individual and school controls	No	No	Yes	Yes
School time trend	None	Linear	None	Linear
F-stat for joined significance of controls			0	0
P-value of parental background	0	.527	0	.59

SEs in parentheses are clustered at the school level.

Note: The dependent variable in all columns is the percentile of parental earnings. All columns include the leave-one-out average of parenalt earnings at the school level, to account for negative mechanical bias due to using leave-one-out measure of peer characteristcs, as suggested by Guryan, Kroft, and Notowidigdo (2009) correction method. Individual controls included in Columns (3)-(4) include fixed effects for gender, year of birth and mother and father age at birth. The p-value reported in the last line refers to the coefficient on peers' parental earnings. All variables are standardized. SE clustered at the school level are reported in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Predetermined characteristcs are Orthogonal to (residuals of) Peers' Parental Earnings

	N of test with $H_0 : \beta = 0$ is rejected			N of tests
	P-value<.1	P-value<.05	P-value<.01	
School FE	13	12	9	28
	(46.42%)	(42.85%)	(32.14%)	(100.00%)
School time trend	4	3	1	28
	(14.28%)	(10.71%)	(3.57%)	(100.00%)

Note: This Table shows aggregate results from separate OLS regressions reported in Table 6. All regressions include cohort fixed effects and school fixed effects. The first row of the table refers to regressions which do not include school-specific time trends. The second row of the table refers to regressions which include school-specific time trends. The table reports the number (and the share in parentes) of variables which report correlation with the leave one out average of peers' parental earning different from zero at 90%, 95% and 99% condifence level.

Table 6: Balance Test - Extended Version

School FEs			School Time Trend			
b	SE	P-value	b	se	P-value	Characteristic
0,006	0,001	0,000	0,001	0,001	0,321	Parental Earnings
-0,004	0,001	0,000	-0,001	0,001	0,323	Age of Enrollment
0,001	0,001	0,318	0,000	0,001	0,551	Fisrt Born
0,000	0,001	0,768	0,000	0,001	0,391	N of Siblings
0,003	0,001	0,024	0,000	0,001	0,784	Yrs. Educ. - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
0,001	0,001	0,467	-0,001	0,001	0,451	Plant Size - F
-0,001	0,001	0,116	-0,001	0,001	0,344	Teenage Par. - F
-0,003	0,001	0,000	-0,001	0,000	0,026	Missing Edu. - F
-0,001	0,001	0,103	-0,001	0,001	0,209	Missing Parent- F
0,001	0,001	0,293	0,001	0,000	0,100	Plant size: 1 to 49 - F
0,001	0,001	0,376	0,000	0,000	0,863	Plant size: 50 to 200 - F
0,000	0,001	0,774	0,000	0,000	0,506	Plant size: 200+ - F
-0,003	0,001	0,000	-0,001	0,001	0,036	Missing plant ID - F
0,004	0,001	0,000	0,000	0,001	0,695	Yrs. Educ. - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
0,002	0,001	0,024	0,000	0,001	0,780	Firm Size - M
-0,001	0,001	0,109	-0,001	0,001	0,334	Teenage Par. - M
-0,003	0,001	0,000	-0,001	0,001	0,062	Missing Edu. - M
0,000	0,001	0,525	0,000	0,001	0,737	Missing Parent- M
0,002	0,001	0,000	0,001	0,000	0,004	Plant size: 1 to 49 - M
-0,001	0,001	0,302	-0,001	0,000	0,161	Plant size: 50 to 200 - M
0,000	0,001	0,521	0,000	0,001	0,995	Plant size: 200+ - M
-0,004	0,001	0,000	-0,001	0,001	0,066	Missing plant ID - M

Note: The table reports coefficients from separate regressions regressing (standardized) schoolmates parental earnings on several (standardized) measures of predetermined characteristcs. All regressions include include controls for own parental earnings, cohort fixed effects and school-level average realizations of parental earnings to to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by Guryan et al. (2009) and applied to a similar context by Brenøe and Zölitz (2020). The first column reports the coefficient peer's parental earnings from a regression including school fixed effects, the fourth column reports the coefficient peer's parental earnings from a regression including school-specific linear time trends.

Table 7: Residuals in School Composition are Uncorrelated Over Time

	N of test with $H_0 : \beta = 0$ is rejected			N of tests
	P-value<.01	P-value<.05	P-value<.1	
None	3	17	14	332
	(0.9%)	(5.1%)	(4.2%)	(100%)
Linear	3	10	15	332
	(0.9%)	(3%)	(4.5%)	(100%)
Quadratic	3	12	12	332
	(0.9%)	(3.6%)	(3.6%)	(100%)
Cubic	4	5	9	332
	(1.2%)	(1.5%)	(2.7%)	(100%)

Note: This Table shows aggregate results from separate school-specific time series regressions. All regressions test for the school specific AR(1) coefficient of the correlation over time in school composition. School composition is measured as the average parental earning of students enrolled in each school and cohort. The first row of the table refers to regressions which do not include school-specific time trends. The latter rows of the table refers to regressions which include school-specific time trends. The table reports the number (and the share in parentes) of variables which report correlation with the leave one out average of peers' parental earning different from zero at 99%, 95% and 90% condifence level.

Table 8: Adjacent Cohorts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\tilde{X}_{i,t-3}$	-0.040**						
	(0.018)						
$\tilde{X}_{i,t-2}$		-0.039*					
		(0.022)					
$\tilde{X}_{i,t-1}$			0.018				
			(0.020)				
$\tilde{X}_{i,t}$				0.069***			
				(0.022)			
$\tilde{X}_{i,t+1}$					-0.010		
					(0.021)		
$\tilde{X}_{i,t+2}$						0.031	
						(0.025)	
$\tilde{X}_{i,t+3}$							-0.029
							(0.025)
Observations	254007	285381	317009	350821	312911	277114	242501

SEs in parentheses are clustered at the school-cohort level.

School FEs and school-specific time trends are included.

* p<0.10, ** p<0.05, *** p<0.01

Note: Each column reports estimates from separate OLS regressions. Dependent variable is children earnings by the age of 28-32. All earnings are expressed in ranks with respect tot the cohort-specific national distribution. $\tilde{X}_{i,t+c}$ is the average of parental earnings among students who enrolled in the same school as i, c years after the actual cohort of i. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

Table 9: Higher Order Time Trends and Alternative Specifications

	(1)	(2)	(3)	(4)	(5)
Parental earnings (Rank)	0.146***	0.146***	0.146***	0.161***	0.145***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
SM Par. earnings (Rank)	0.068***	0.047**	0.034	0.077**	0.090***
	(0.021)	(0.021)	(0.023)	(0.030)	(0.021)
SM Par. earnings (Rank, moving average)				-0.026	
				(0.041)	
Observations	345801	345801	345801	213168	345439
School FE	Yes	Yes	Yes	No	Yes
Cohort FE	Yes	Yes	Yes	No	Yes
School t trend (1st order)	Yes	Yes	Yes	No	Yes
School t trend (2nd order)	No	Yes	Yes	No	No
School t trend (3rd order)	No	No	Yes	No	No
School×Municipality	No	No	No	No	Yes
R^2	0.10	0.10	0.11	0.07	0.11

Note: Estimates from separate OLS regressions. dependent variable is children earnings by the age of 28-32. All earnings are expressed in ranks with respect tot the cohort-specific national distribution. All speifications include controls for year of birth, mother age at birth, father age at birth, gender and cohort size. Municipality is defined as the municipality of recidence in the year of enrollment. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

Table 10: Main Results

	Ranks				2015 *USD*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Par. Earnings	0.161***	0.157***	0.145***	0.145***	0.176***	0.173***	0.163***	0.163***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.024)	(0.026)	(0.028)	(0.028)
Schoolmates' Par. Earnings		0.046*	0.068***	0.067***		0.040	0.094***	0.075***
		(0.024)	(0.018)	(0.021)		(0.032)	(0.020)	(0.024)
Observations	345834	345791	345791	345791	345834	345791	345791	345791
Cohort FE	No	No	Yes	Yes	No	No	Yes	Yes
School FE	No	No	Yes	Yes	No	No	Yes	Yes
School Time Trend	No	No	No	Yes	No	No	No	Yes
R^2	0.07	0.07	0.10	0.10	0.07	0.07	0.10	0.10

Note: Estimates from separate OLS regressions. dependent variable is children earnings by the age of 28-32, measured in percentile ranks of the distribution of students born in the same year in columns (1) – (4) and in 2015 USD in columns (5) – (8). All specifications include controls fixed effects for year of birth, mother age at birth, father age at birth and gender. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

Table 11: Education and Labor Market Outcomes: SES Gradient

	Par. Earnings: Tercile			
	1	2	3	All
College	0.653	0.731	0.810	0.732
	[115,792]	[115,783]	[119,289]	[350,864]
College, STEM	0.098	0.112	0.141	0.117
	[115,792]	[115,783]	[119,289]	[350,864]
Employed	0.729	0.789	0.772	0.763
	[115,792]	[115,783]	[119,289]	[350,864]
Manager	0.018	0.020	0.022	0.020
	[84,375]	[91,340]	[92,127]	[267,842]
Hourly Wage	28.895	29.800	32.156	30.325
	[84,375]	[91,340]	[92,127]	[267,842]
Plant Wage Premium (AKM)	0.008	0.014	0.036	0.020
	[81,899]	[89,100]	[90,478]	[261,477]
Top Tercile Plant Wage Premium (AKM)	0.462	0.480	0.568	0.505
	[81,899]	[89,100]	[90,478]	[261,477]

Note: The table presents average outcomes for students in the sample, measured at the age of 30 year sold. Sample averages conditional on tercile of parental earnings are reported, with the number of observations in each cell reported in square brakets. College is a dummy variable equal to one if the individual has completed a College degree. College, STEM is a dummy variable equal to one if the individual has completed a College degree in the fields of science, technology, engeneering or mathematics. Employed is a dummy variable equal to one if the individual is employed at the age of 30. Manager is a dummy variable equal to one if the individual is employed as a manager at the age of 30, the variable is defines only for employed individuals. Hourly Wage is the hourly wage at the main occupation at the age of 30, the variable is defines only for employed individuals. Top quartile and Top decile are dummy variables equal to one if the individual is employed at a plan whose AKM fixed effect (as in Abowd et al. (1999)) is in the top quaritle or decile of the national distribution of plant fixed effects, respectively, the variable is defines only for employed individuals

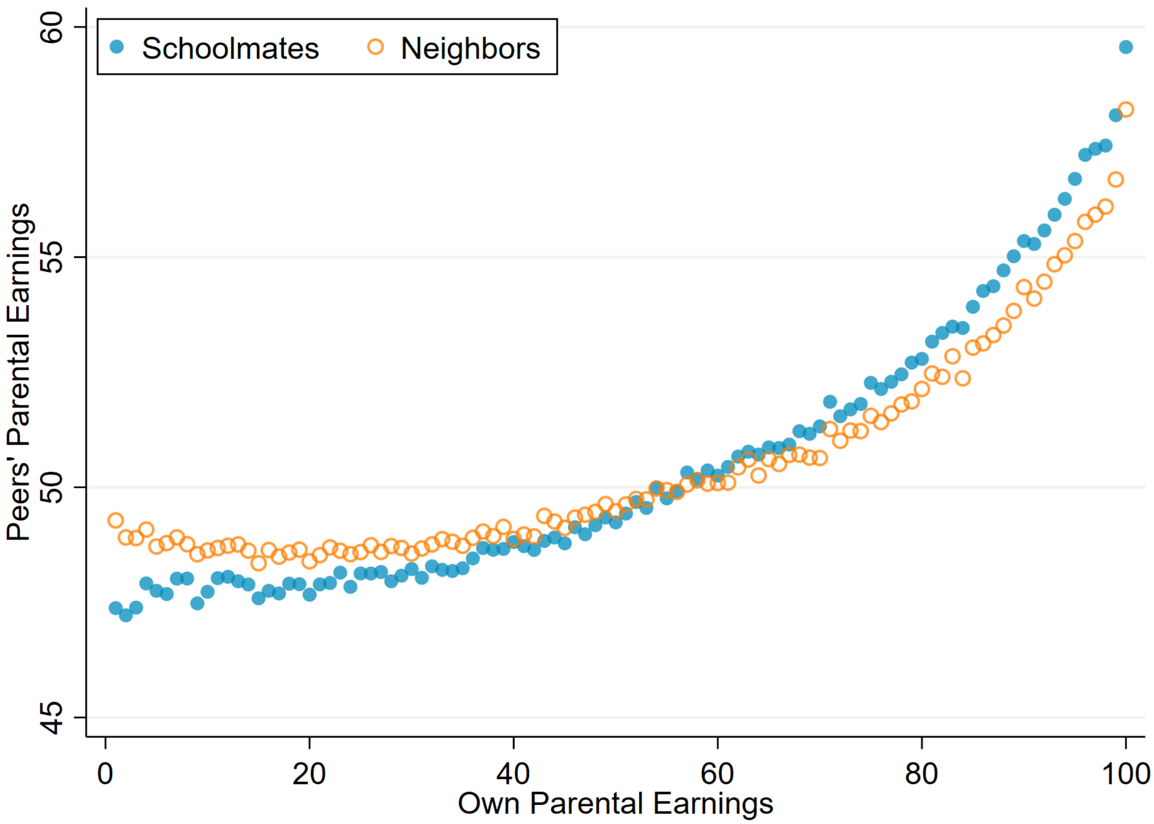
Table 12: Education and Labor Market Outcomes: Exposure Effect

	(1) Earnings	(2) College	(3) STEM	(4) Employed	(5) Manager	(6) Hourly Wage	(7) Plant FE	(8) Plant FE> $p(66)$
Panel A: Homogenous Effects								
SMs Par. Earn.	596.328**	0.012***	-0.000	0.003	0.003**	0.301***	0.002**	0.010***
	(188.773)	(0.003)	(0.002)	(0.003)	(0.001)	(0.098)	(0.001)	(0.004)
Observations	345791	345791	345791	345791	264504	264504	258232	258232
R^2	0.10	0.09	0.08	0.18	0.01	0.05	0.07	0.03
Mean D.V.	43934.34	0.73	0.12	0.76	0.02	30.33	0.02	0.50
Panel B: Heterogeneous Effects								
SMs Par. Earn.$\times$ Ter. = 1	638.92**	0.009*	-0.000	0.005	0.003*	0.396**	0.001	0.013*
	(297.544)	(0.006)	(0.004)	(0.005)	(0.002)	(0.195)	(0.002)	(0.007)
SMs Par. Earn. $\times$ Ter. = 2	522.56*	0.009*	-0.001	0.001	0.002	0.312**	0.005***	0.018***
	(281.563)	(0.005)	(0.004)	(0.005)	(0.002)	(0.152)	(0.002)	(0.007)
SMs Par. Earn. $\times$ Ter. = 3	571.67*	0.016***	-0.001	0.001	0.003*	0.256	0.001	0.005
	(342.054)	(0.004)	(0.003)	(0.004)	(0.002)	(0.184)	(0.002)	(0.006)
Observations	345782	345782	345782	345782	264497	264497	258223	258223
R^2	0.11	0.10	0.08	0.19	0.02	0.06	0.08	0.03
Mean D.V., Ter. 1	39071.45	.65	.1	.73	.02	28.89	.01	.46
Mean D.V., Ter. 2	44231.28	.73	.11	.79	.02	29.8	.01	.48
Mean D.V., Ter. 3	48337.05	.81	.14	.77	.02	32.16	.04	.56

Note: The table reports OLS estimates from the same model as in eq. 1, where earnings are measured in 2015 USD and the dependent variable is reported at the top of each column. Panel A reports the coefficients for the model as specified in 1, while Panel B reports the coefficients for the same model upon inclusion of interaction terms with dummies for parental earnings' terciles. All speifications include controls fixed effects for year of birth, mother age at birth, father age at birth and gender. SEs in parentheses are clustered at the school-cohort level. * p<0.10, ** p<0.05, *** p<0.01

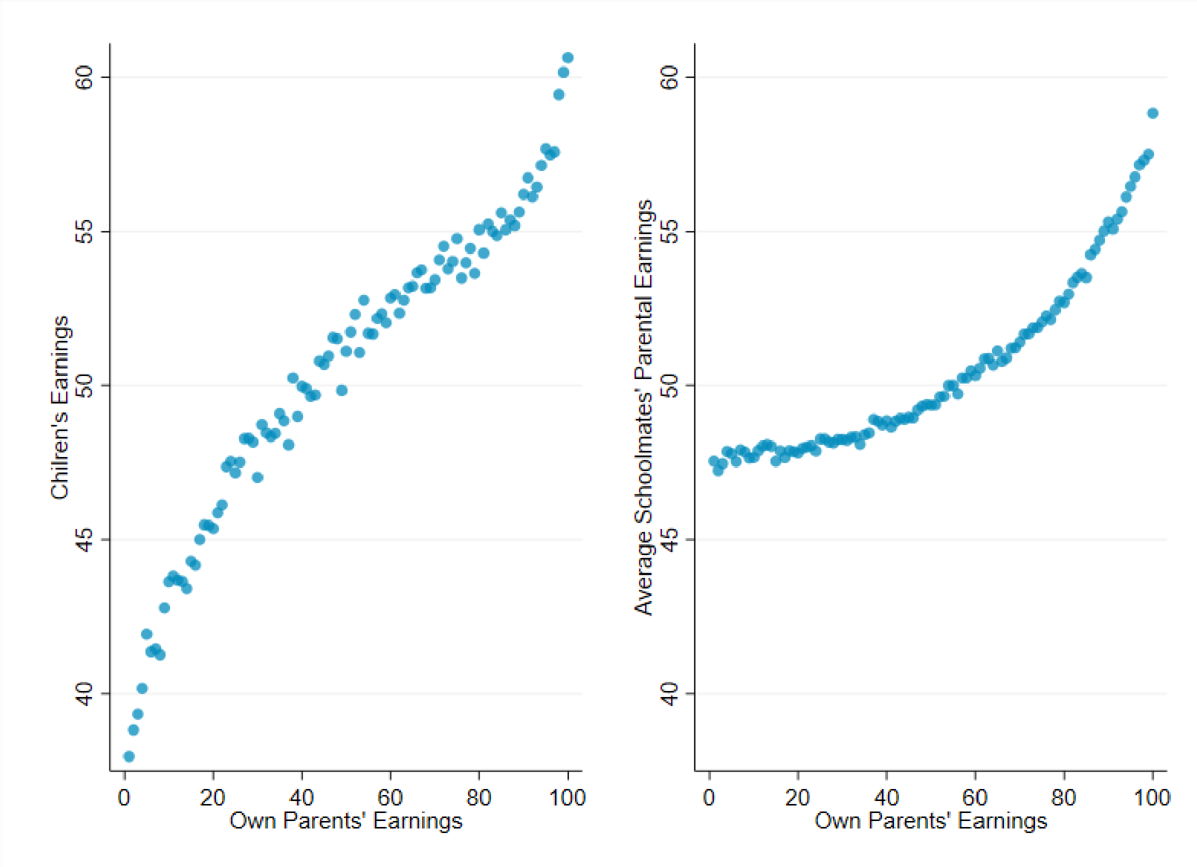
Figures

Figure 1: Sorting across Schools and Neighborhoods



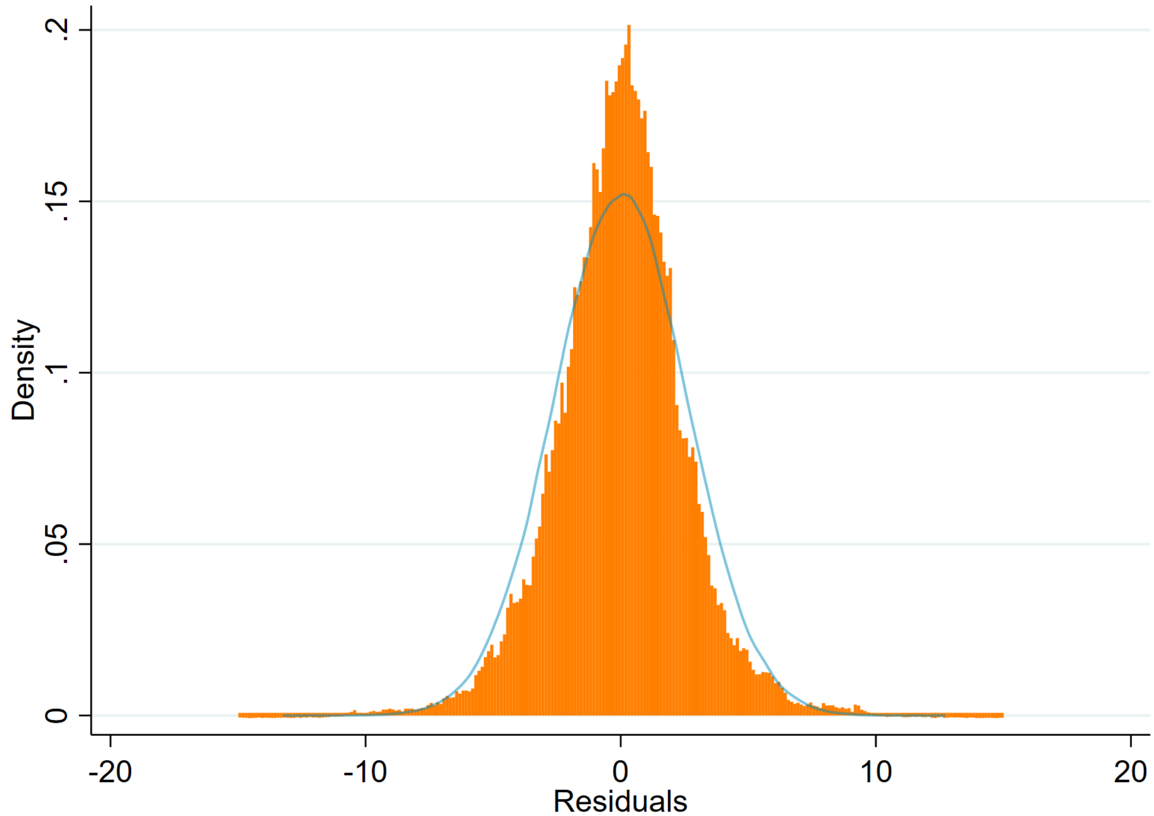
Note: The graph plots the sample average of schoolmates' and neighbors' parental earnings, conditional on the percentile of the in-sample parental earnings distribution. Peers' parental earnings are also measured in percentile ranks of the in-sample parental earnings distribution. Blue dots represent schoolmates' average parental earnings, orange circles represent neighbors' average parental earnigs. Neighbors are defined as individuals born in the same calendar year and registered as living in the same municipaility in the year of enrollment in high school.

Figure 2: Intergenerational Mobility and Peer Exposure



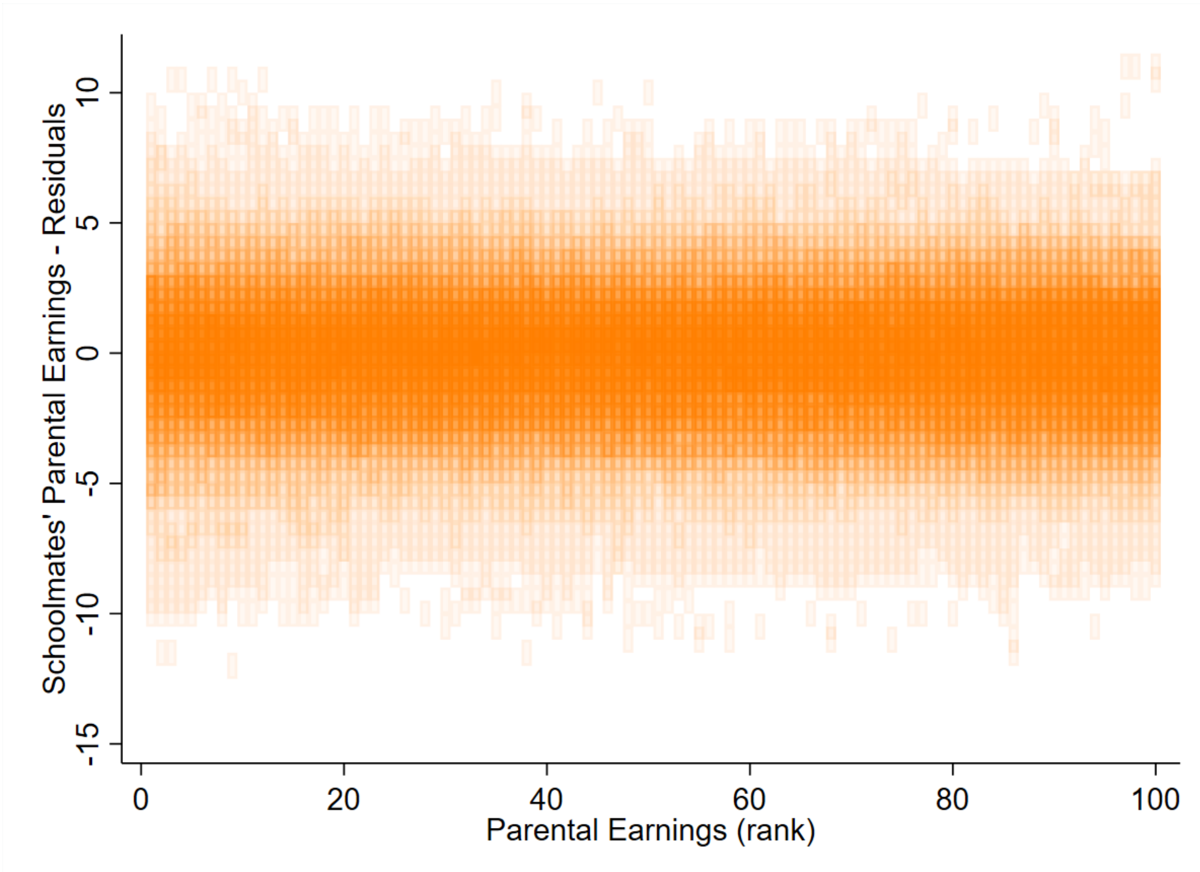
Note: The left graph plots the average earnings by the age 28-32 conditional on own parental earnings. The right graph plots the average peers' parental earnings conditional on own parental earnings. Peers' parental earnings are computed as the leave one out average of parental earnings among schoolmates. Earnings and parental earnings are measured in percentiles of the national earnings distribution.

Figure 3: Residuals are Normally Distributed



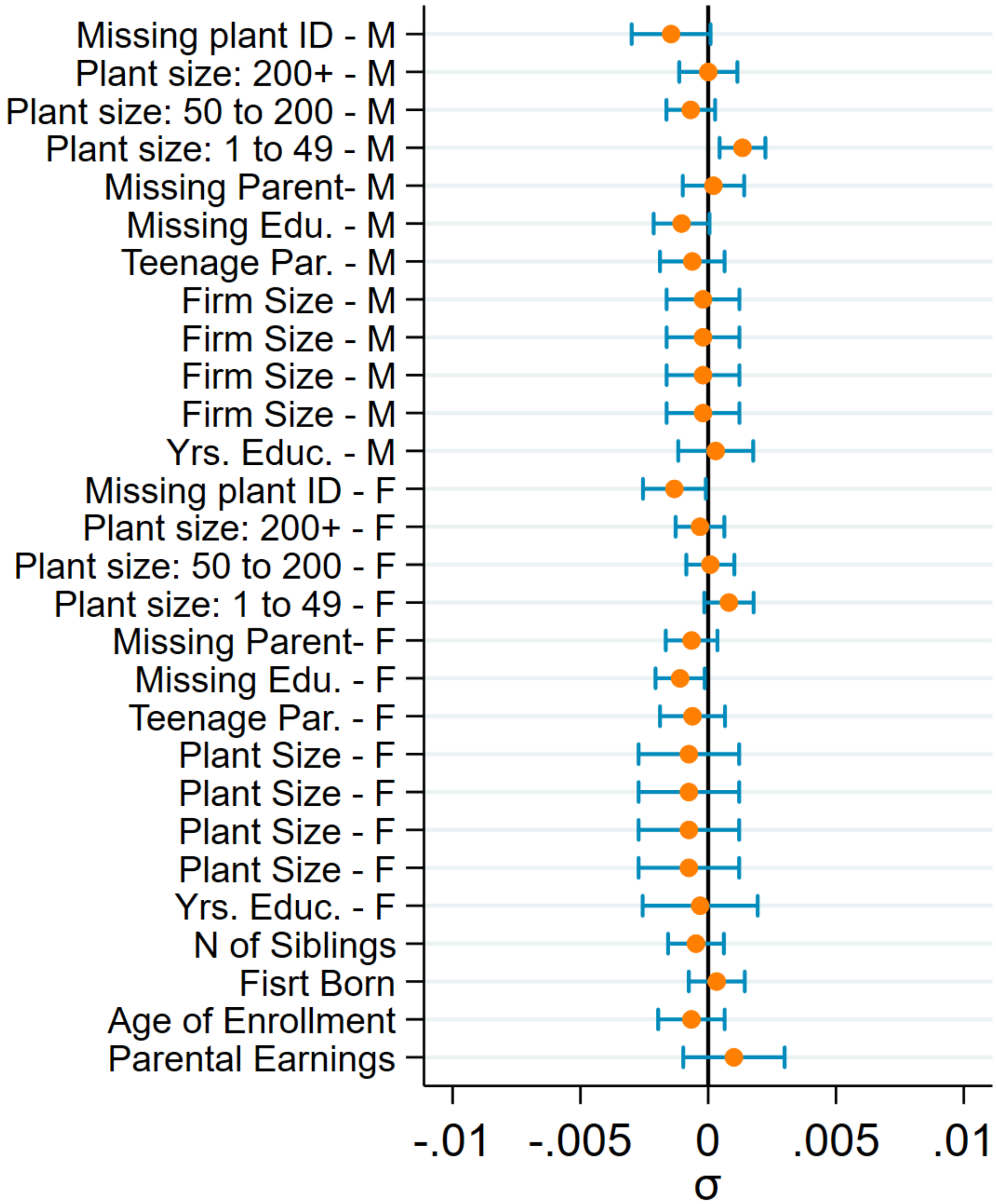
Note: The graph plots the empirical PDF of the residual of average schoolmates' parental background from a regression on school fixed effects and school specific linear time trends. Average schoolmates' parental background is defines as the leave-one-out average of schoolmates parental earnings in percentiles of the national distribution. The PDF of a normal distribution with the same mean and standard deviation as the resuduals is represented by the continous blue line.

Figure 4: Parental Earnings are Orthogonal to (residuals of) Peers' Parental Earnings



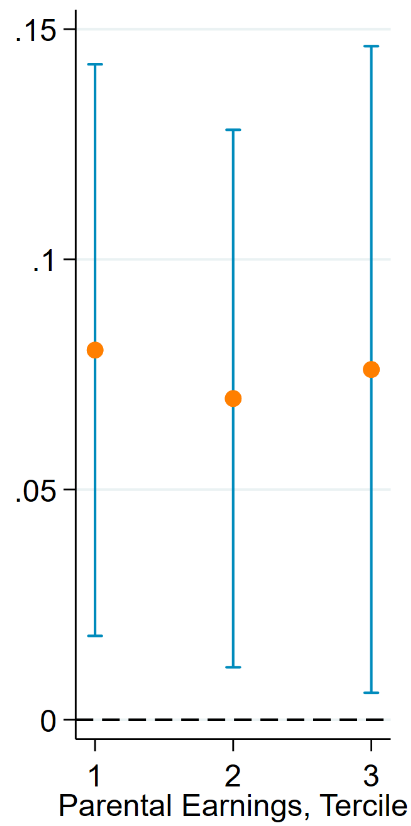
Note: The graph plots the empirical bivariate distribution of the residual of average schoolmates' parental background from a regression on school fixed effects (vertical axis) and school specific linear time trends and own parental background (horizontal axis). Average schoolmates' parental background is defines as the leave-one-out average of schoolmates parental earnings in percentiles of the national distribution.

Figure 5: Predermined Characteristcs are Orthogonal to (residuals of) Peers' Parental Earnings



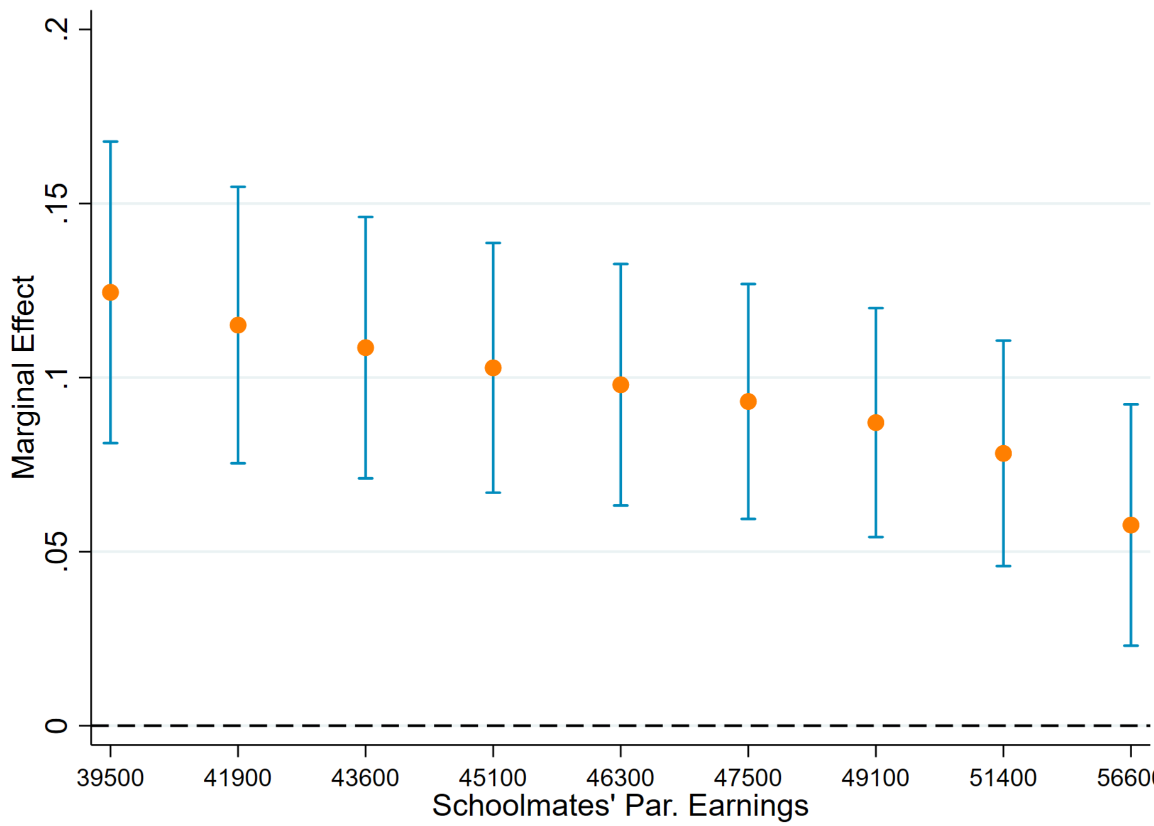
Note: The graph reports coefficients from separate regressions regressing (standardized) schoolmates parental earnings on different (standardized) measures of predetermined characteristcs, including controls for own parental earnings, school specific time trend and school-level average realizations of parental earnings to to control for mechanical negative correlation due to the leave-one-out nature of the measure considered, following a standard practice introduced by Guryan et al. (2009) and applied to a similar context by Brenøe and Zölitz (2020).

Figure 6: Heterogeneity by Parental Earnings



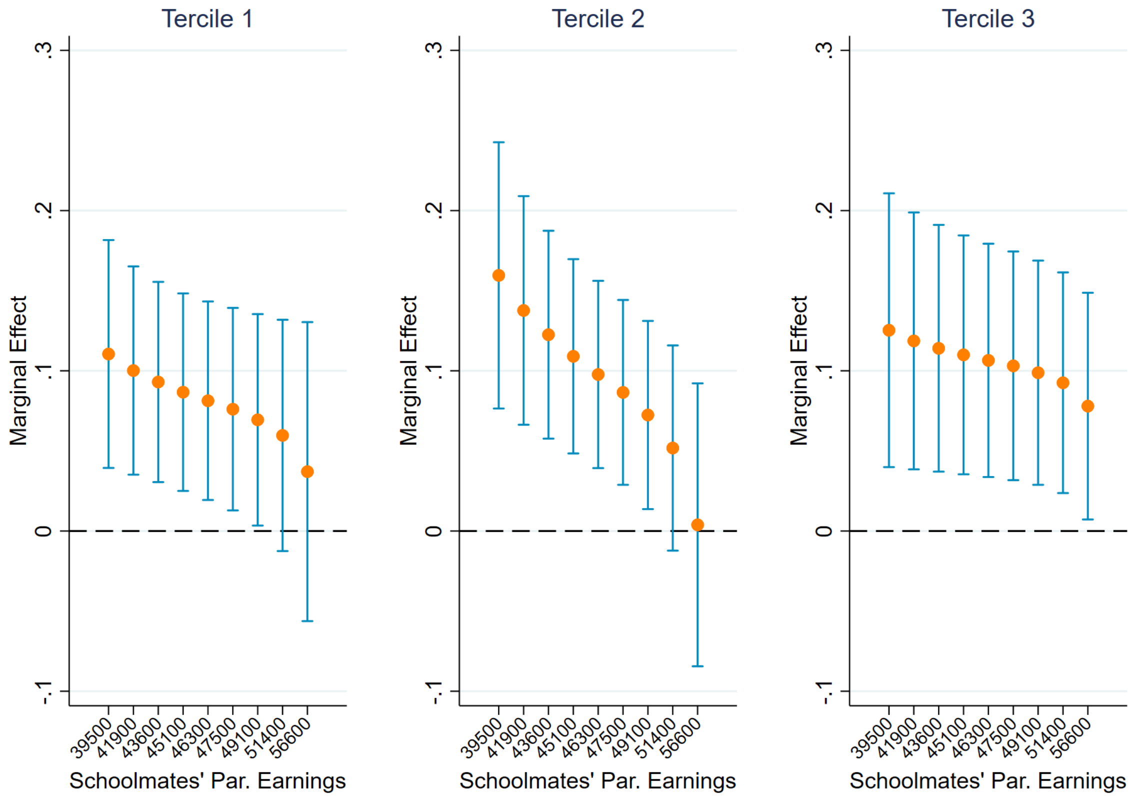
Note: The graph plots the marginal effect (along with 90% confidence intervals) of the coefficient measuring the impact of peers' parental earnings on own earnings from a version of eq. 1 which includes a full set of interactions between allt the independent variables and a set of dummies for each tercile of parental earnings. standard errors are clustered at the school level.

Figure 7: Decreasing Marginal Effects

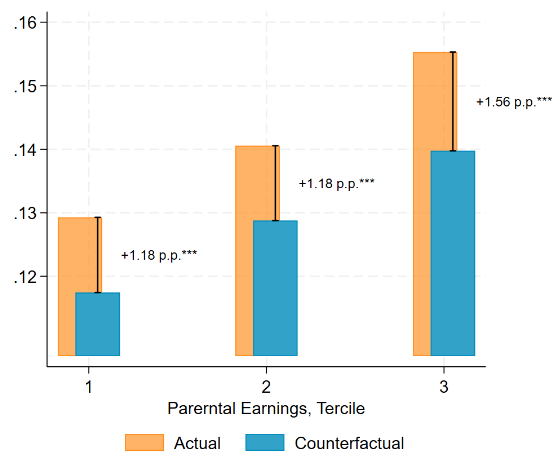


Note: The graph plots the marginal effects and the relative 90% confidence intervals from estimation of the model in eq. 1 where a quadratic polinomial for average schoolmates' parental earnings is included, evaluated at different levels of exposures to peers parental ernings. The horizontal axis report the deciles of the distribution of schoolmates parental earnings (rounded to the closest hundreds), at which the marginal effect is computed.

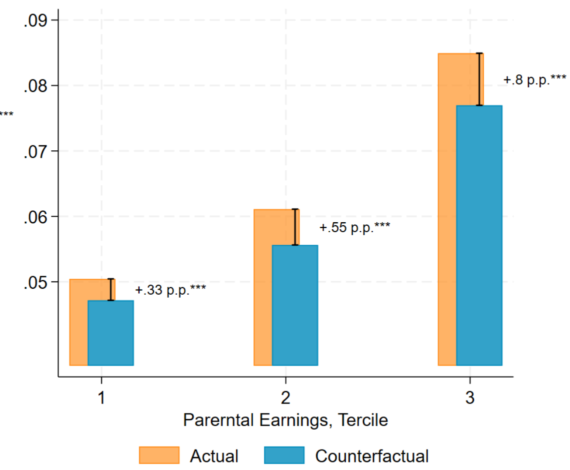
Figure 8: Decreasing Marginal Effects by SES



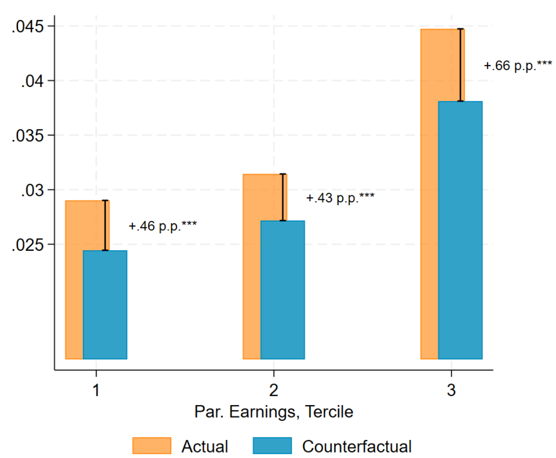
Note: The graph plots the marginal effects and the relative 90% confidence intervals from estimation of the model in eq. 1 where a quadratic polinomial for average schoolmates' parental earnings is included and a full set of interactions with dummies on tercile of parental earnings is included, evaluated at different levels of exposures to peers parental ernings and different tercile of parental earnings. The horizontal axis report the deciles of the distribution of schoolmates parental earnings (rounded to the closest hundreds), at which the marginal effect is computed.



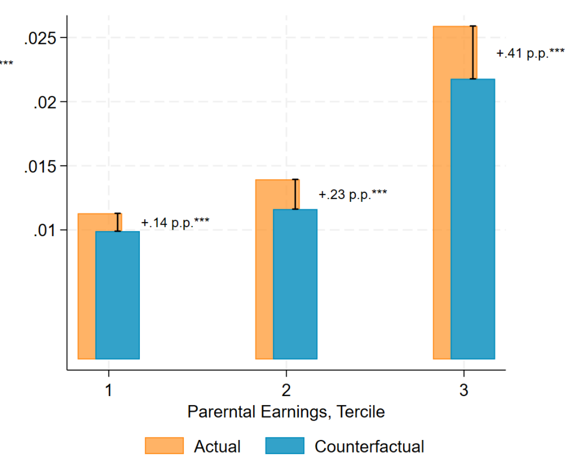
(a) Any Plant, Any Schoolmate



(b) Any Plant, High-SES Schoolmate



(c) High-Wage Plant, Any Schoolmate

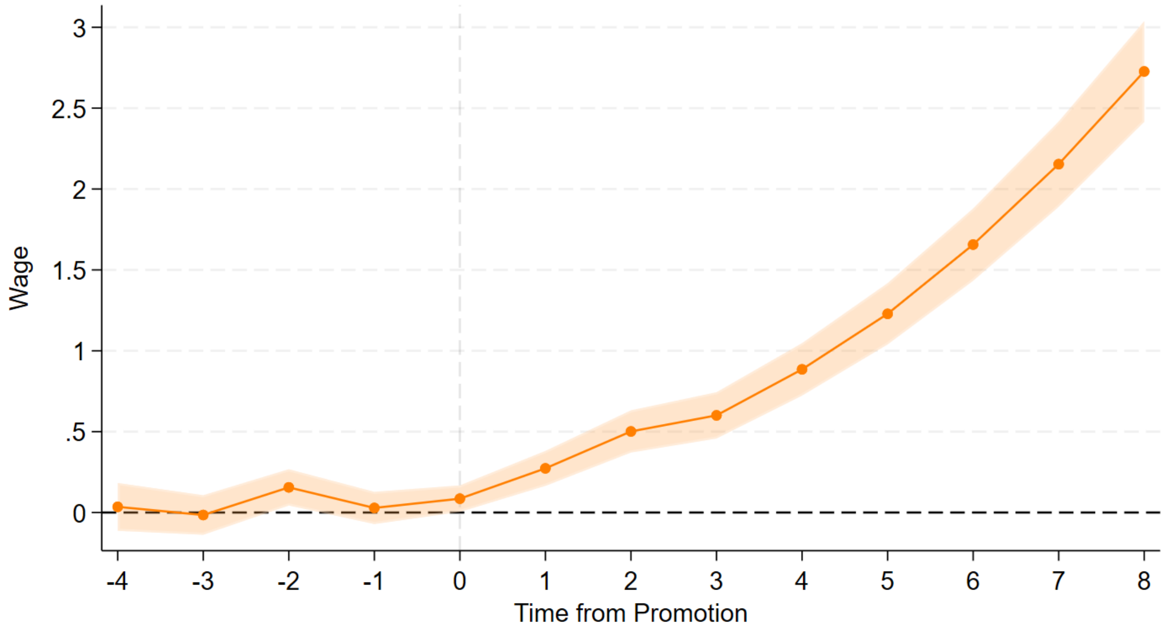


(d) High-Wage Plant, High-SES Schoolmate

Figure 9: Probability of Joining a Plant Where a Schoolmate is Employed

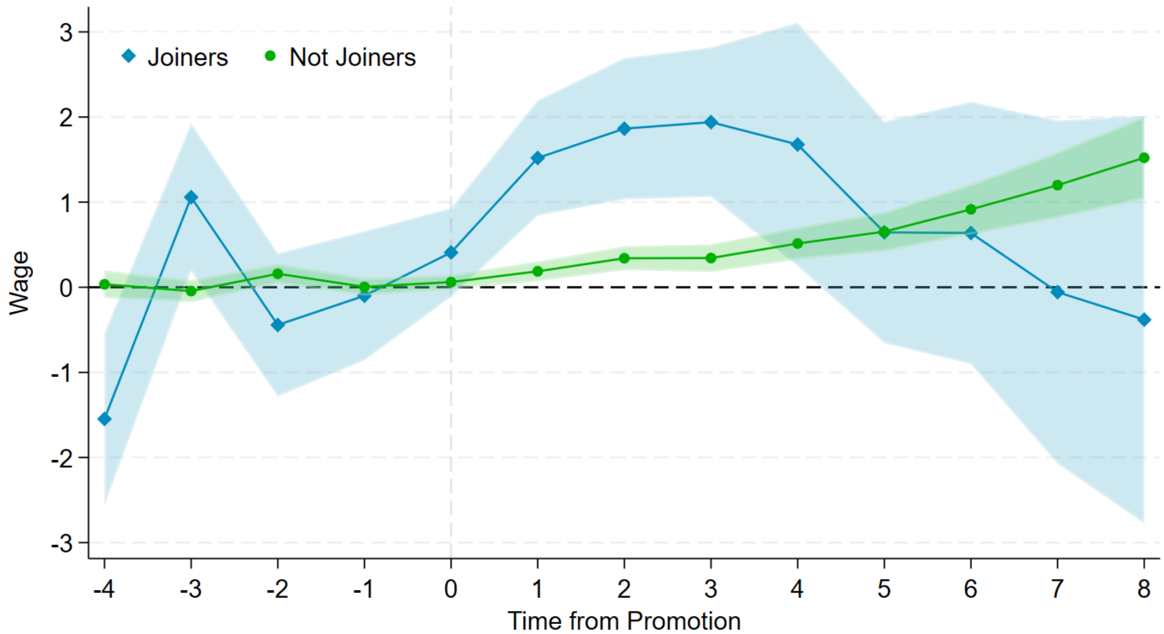
Note: The bar graphs display the probability of joining a plant where a schoolmate is employed, measured 4 to 14 years after high school enrollment. The probability is calculated as the share of individuals who join actual schoolmates' plants (orange bars, left) versus counterfactual schoolmates' plants (blue bars, right), averaged across 1,000 independent draws of almost schoolmates from adjacent cohorts. The plots show the difference in percentage points between the actual and counterfactual probabilities. P-values are computed by determining the share of counterfactual draws that result in a higher probability of joining an almost schoolmate compared to an actual schoolmate. Each panel computes the probability of joining any peer vs high-SES peers, at any firm or at a high-wage plant. High-SES peers are defined as children from parents at the top tercile of parental earnings, high wage plants are plants whose AKM fixed effect is whitin the top quartile of the national distribution. Significance levels are indicated as follows: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 11: Effect of a Peers' Promotion on Wages



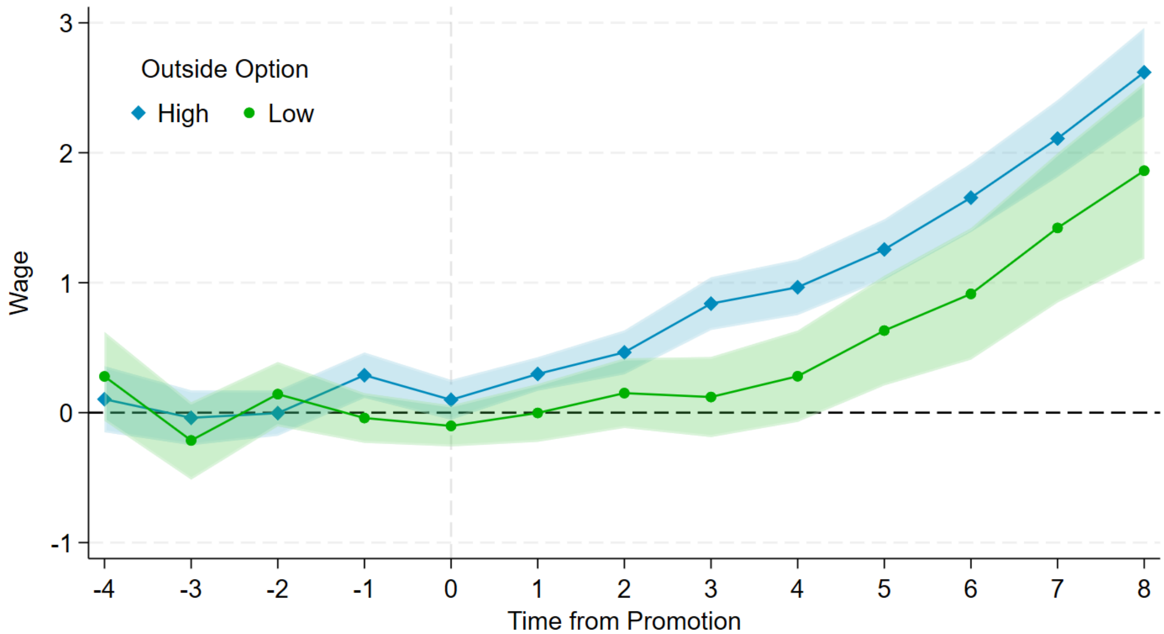
Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (2) as in Callaway and Sant'Anna (2021). Each group is composed by schoolmates who enrolled at the same high school in the same cohort, exept the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area.

Figure 12: Effect of a Peers' Promotion on Wages



Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (2) as in Callaway and Sant'Anna (2021), estimated separately for joiners (blue diamonds) and not joiners (green circles). Each group is composed by schoolmates who ernolled at the same high school in the same cohort, exept the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area. Joiners are the individuals who work at the same plant as the promoted manager at any point in time from the time on the promotion onwards, not joiners are the others.

Figure 13: Effect of a Peers' Promotion on Wages



Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (2) as in Callaway and Sant'Anna (2021), estimated separately for groups with high (blue diamonds) and low (green circles) outside options. Each group is composed by schoolmates who ernolled at the same high school in the same cohort, exept the first person becoming a manager. Each group is considered treated from the first year in which a member becomes a manager onwards. Confidence intervals at the 95% level are reported as a shaded area. Low and high outside options are distinguished based on the AKM plant fixed effect of the plant where the promoted manager worked. Groups are classified as having low outside options if the AKM plant fixed effect falls within the bottom tercile of the distribution, and high outside options if it falls within the top tercile.