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No Kid Is an Island: Peer Effects and Intergenerational Mobility

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September 29, 2026

Abstract

Economic inequalities persist from one generation to the next. To what extent do segregated social interactions reproduce disparities in parental resources? Exploiting within-school, across-cohort variation in schoolmates' parental backgrounds among Danish high-school students, I show that a $1 increase in average schoolmate parental earnings raises annual adult earnings by approximately $0.09. Peer exposure accounts for approximately 9% of intergenerational persistence among children from the highest-earning families. I further show that former schoolmates remain connected in the labor market: as their careers advance, they facilitate access to higher-paying firms and provide more attractive outside options. A formal mediation analysis combining the exposure design with variation in the timing of schoolmates' managerial promotions attributes approximately 40% of the exposure effect on earnings at ages 35–39 to this labor-market channel. The findings identify peer exposure as a determinant of intergenerational mobility and show how segregated interactions can transmit economic advantage beyond the family.

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I thank Giacomo De Giorgi, Enrico Moretti and Frederic Robert-Nicoud for precious guidance and support. Gratitude is also extended to Ainoa Aparicio-Fenoll, Pietro Biroli, Sydnee Caldwell, Hilary Hoynes, Patrick Kline, Joan Llull, Michele Pellizzari, Ricardo Perez-Truglia, Jörn-Steffen Pischke, Luigi Pistaferri, Pauline Rossi, Jan Stuhler, Aleksey Tetenov, Michela Tincani, Harrison Wheeler and Danny Yagan for insightful conversations. This project aslo benefited from constructive comments from participants at numerous conferences and seminars such as AlpPop Conference, IRLE Seminar at UCBerkeley, CEA Annual Meeting, SOLE Annual Meeting, WEAI Annual Meeting, EEA Annual Meeting, EALE Annual Conference, AIEL Conference, the Barcelona School of Economics PhD Jamboree, UniTo-CCA PhD Workshop in Economics, Brucchi-Luchino Labor Economics Workshop and Winter Meeting of the Econometric Society. Special thanks are due to Mauricio Prado, Battsita Severgnini, and Copenhagen Business School for their generous support in facilitating access to the data. All errors are my own. I gratefully acknowledge financial support from the GSEM - IEE and the Fonds Genèral at the University of Geneva.

1 Introduction

The relevance of parents' choices and endowments for children's well-being has been central to economic research since Becker and Tomes (1979). It has received renewed attention as empirical work has documented substantial persistence in economic outcomes across generations (Chetty et al., 2014; Mazumder, 2005; Corak, 2013). Families, however, are not children's only formative environment. As children grow up, interactions with neighbors, friends, and schoolmates can shape their development and future opportunities.

The starting point of these interactions is itself influenced by parental characteristics. Families sort across neighborhoods and schools, and parents value the composition of the social environments their children enter (Benabou, 1993; Durlauf, 1996; Landersø and Heckman, 2017; Abdulkadiroglu et al., 2020; Eshaghnia et al., 2023). Consequently, differences in parental resources are reproduced in children's social environments: children from higher-SES families are more likely to meet peers whose parents enjoy similar advantages.

In this paper, I investigate whether exposure to peers from advantaged parental backgrounds has lasting effects on children's economic outcomes. Using plausibly exogenous variation in high-school composition across cohorts in Denmark, I find that a $1 increase in schoolmates' average parental earnings raises annual adult earnings by $0.09.

This effect has direct implications for intergenerational mobility. When parental backgrounds are positively correlated within peer groups, children from affluent families may benefit both from their own parents' resources and from exposure to better-resourced peers. Children from less affluent families are less likely to receive either advantage. I find that in Denmark peer exposure accounts for approximately 9% of intergenerational persistence among children near the top of the parental-earnings distribution, where sorting is strongest. A quantification exercise replacing Danish school sorting with the degree of residential sorting observed across U.S. Census tracts implies that intergenerational persistence would be about 7% higher.

What explains the exposure effect? Peers with higher-SES parents may promote skill formation (Fruehwirth and Gagete-Miranda, 2019), alter social norms (Tincani, 2024), provide information about consequential choices (Cattan et al., 2025; Bellue, 2023), or facilitate ac-

cess to jobs through labor-market networks. Using Danish administrative registers, I follow students' education and careers through age 39. I find little evidence that the earnings effect operates primarily through educational attainment. Instead, former schoolmates remain important labor-market ties: they facilitate access to firms, and their career advancement generates wage gains for their peers.

To quantify this channel, I develop a formal mediation analysis that exploits variation in the timing of former schoolmates' promotions to managerial positions. The design combines the paper's within-school, across-cohort variation in peer exposure with a staggered difference-in-differences design around peers' promotions. The resulting decomposition attributes approximately 40% of the total exposure effect on earnings at ages 35–39 to access to labor-market opportunities associated with former peers' career progression.

These findings imply that social interactions contribute to the persistence of inequality across generations. Separating their role from family background and other environmental factors is important for understanding why opportunities differ across neighborhoods and other social settings (Chetty and Hendren, 2018). The results also point to a trade-off between social segregation and intergenerational mobility and motivate further study of policies that increase interaction across socioeconomic groups.

The mechanism also matters beyond distributional questions. Limited evidence of human-capital gains, together with evidence on job referrals and outside options, suggests that peer exposure affects not only workers' productivity but also how they find jobs and bargain over wages.

The first part of the paper estimates the causal effect of peer exposure using within-school variation in cohort composition, following Hoxby (2000).[1] Comparing students in different cohorts of the same school removes systematic differences across schools. School-specific composition trends further isolate cohort-level deviations that were unlikely to be anticipated by students and their families. Consistent with this identifying assumption, these deviations are unrelated to a broad set of predetermined student characteristics. The estimates are also robust to cohort-by-municipality fixed effects that absorb local shocks

[1]Related applications include Black et al. (2013); Carrell et al. (2018); Brenøe and Zölitz (2020); Cattan et al. (2025); Mertz et al. (2024).

capable of affecting both parental and adult earnings.

The second part quantifies how peer exposure affects intergenerational mobility and how this contribution depends on segregation. I first show that the parent–child earnings relationship becomes steeper when peer exposure has a larger causal effect and children's parental backgrounds are more positively correlated with those of their peers. Sorting across Danish high schools is concentrated at the top of the parental-earnings distribution, so peer exposure contributes most to persistence among high-earning families where it explains 9% of the transmisison of inqeualities. I then benchmark Danish school sorting against residential sorting across U.S. Census tracts using Opportunity Atlas data and find that intergenerational mobility would be 7% lower had Danish students sorted into higschool by parental earnings to the same extent as US residents sort into Census tracts.

The third part studies the mechanisms linking peer exposure to adult earnings. A one-standard-deviation increase in schoolmate parental earnings raises annual earnings by about \$710 and hourly wages by \$0.17. It increases the probability of holding a managerial position and the firm pay premium by about 0.2 percentage points each, while the estimated effect on college completion is small and statistically imprecise. Motivated by this pattern, I study former schoolmates' connections and career progression.

First, I show that former schoolmates provide access to their employers. I compare moves to a firm employing an actual schoolmate with moves to a firm employing an *almost schoolmate*, defined as a student who attended the same high school in the adjacent cohort. This comparison implies that high-school connections account for workplace changes for approximately 1.4% of the students in the sample. These ties also help low-SES students enter firms employing high-SES former schoolmates.

Second, I show that peers' career progression has wage spillovers consistent with improved outside options. In a staggered difference-in-differences design, a former schoolmate's promotion to manager raises peers' hourly wages by 4% on average in the following years. Wage gains remain among workers who do not join the promoted peer's firm and are larger when the peer is promoted at a high-wage firm, a pattern consistent with outside offers strengthening workers' bargaining positions (Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Bagger et al., 2014).

The final empirical part brings these estimates together in a mediation analysis. The main goal of this section it to estimate the portion of the returns to peers exposure that is due to improved connections to peers careers on the labor market. I show that the target parameter can be expressed as combination of the change in access to managerial peers caused by exposure to higher-SES schoolmates with the earnings response to a peer's earlier promotion. Identification uses the assumptions underlying the main exposure design, conditional parallel trends around peer promotions, mediator monotonicity, and homogeneity of the mediator effect between students whose access is shifted by exposure and those whose access would not change. The estimates imply that improved access to labor-market networks accounts for approximately 40% of the exposure effect on earnings at ages 35–39.

The paper contributes first to research on intergenerational mobility by identifying social interactions as a source of access to opportunity. Chetty et al. (2022a) use Facebook friendship data to document a negative spatial relationship between network segregation and mobility in the United States. I complement this evidence with a research design that identifies the causal effect of high-school peer exposure on adult earnings and labor market outcomes, separately identifying a new mechanism: peers as ties on labor markets. The paper also contributes to research on neighborhood effects (Chetty and Hendren, 2018; Alesina et al., 2021; Deutscher, 2020; Aloni and Avivi, 2024) by isolating peer composition from local economic conditions, and it is closely related to Cattan et al. (2025), who show that classmates' parental ties facilitate access to elite colleges.

Second, the paper contributes to research on social ties in labor markets. Since Granovetter (1983), this literature has studied how worker connections facilitate access to jobs (Hensvik and Skans, 2016; Glitz and Vejlin, 2021). Recent work shows that parents shape access to firms, occupations, and high-paying jobs (Kramarz and Skans, 2014; Staiger, 2023; Ventura, 2024; Dobbin and Zohar, 2023; Forsberg et al., 2024). I show that advantages associated with parental background also spill over to schoolmates through job access and wage bargaining. This mechanism is particularly relevant when segregated networks concentrate referrals and reinforce inequality (Calvó-Armengol and Jackson, 2007; Bolte et al., 2026).

Third, I enrich the standard within-school across-cohort identification strategy (Hoxby, 2000) for peer exposure formalizing and estimating a mediation analysis that exploits the

differential timing of a mediator (former schoolmates' promotions to managerial positions) to identify the portion of the exposure effect that operates through the mediator (labor-market connections). In doing this I provide an original contriution to the literature on mediation analysis (Imai et al., 2010; Huber and Frölich, 2020) by combining the exposure design with a staggered difference-in-differences design around peer promotions. The approach is related to the literature on staggered difference in difference designs Callaway and SantAnna (2021), from which innherits the assumption of conditional parallel trends and to recent work in this area Schenk (2025), which I adapt to a setting where a researcher assumes conditional independency of the treatment of interest (within-school across cohort) and has access to a staggered implementation of the mediator.

Finally, the paper adds to evidence on the long-run effects of childhood peers. Carrell et al. (2018) show that exposure to disruptive elementary-school peers lowers adult earnings, while Fruehwirth and Gagete-Miranda (2019) finds that classmates' parental education affects children's educational outcomes. I document a complementary channel in which high-school peers shape adult earnings through persistent labor-market connections.

The rest of the paper follows the empirical sequence described above. Section 2 presents the institutional setting, data, and sample. Section 3 develops the exposure design and reports the main estimates. Section 4 quantifies the implications for intergenerational mobility. Sections 5 and 6 examine education, labor-market outcomes, job access, and wage spillovers. Section 7 presents the mediation analysis, followed by the discussion and conclusion.

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[2].

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

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[3].

2.2 Sample Selection

Administrative registers covering the universe of the Danish population from 1980 to 2023 are the primary data sources of this paper. The sample includes the students who enrolled in a Danish high school from 1988 to 2000[4]. 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 $460,032$ students who enrolled in Danish high schools from 1988 to 2000, I exclude $34,752$ (5.9%) who enrolled before the age of 15 or after the age of 18, $1,972$ students from cohorts where more than 50% of their peers enrolled outside the same age range and $8,282$ students who attended schools with fewer than 4 consecutive years of observation. Moreover, I drop $12,696$ with missing information on adult or parental earnings. The final sample consists of $402,369$ students from 348 schools across 13 cohorts.

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

[4]These cohorts are chosen to include the children born between 1970 and 1985. Those are the individuals observed until the age of 39 in 2023, the last year of observation.

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 35 to 39, as reported in tax registers. For ease of interpretation, earnings are deflated for inflation and reported as of 2015 USD dollars.

Individual Earnings - Ranks 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.[5]

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 16 years of life. I then construct parental earnings as the average among the parents of each child.

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 $402,369$ students enrolled in 13 cohorts across 348 schools. The average student has 145 schoolmates; her parents earn $\sim \$48,600$ per year, and she earns $\sim \$60,000$ per year by the age of $35-39$. Comparing children from different backgrounds, 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: $\$16,323.98$ or 13.36 percentile ranks. Second, differences in parental earnings are mirrored in schoolmates' parental earnings. The difference between the schoolmates' parental earnings

[5]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).

of children from the top and the bottom quartile is $\$5,077.66$ and 6.37 percentile ranks, amounting to $\sim 10\%$ of the difference in their own parental background.

[Table 1 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[6]. 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. 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.

[6]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.

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).[7] 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.[8]

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

[7] 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. (2025) and Mertz et al. (2024).

[8] In subsection 6.2, I exploit time variation schoolmates' promotions to identify their effect on peers' wages, separately from exposure to different parental backgrounds.

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 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, 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 2 reports the coefficients from a balance test regressing school composition on several predetermined characteristics, including school-specific time trends.[9] Table 4 reports the same coefficients in table format. As shown in the last column of Table 4, which includes school-specific time trends, only 1 of the 21 variables considered

[9] 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.

reports a correlation with the residuals of school composition statistically different from zero at conventional confidence levels.

[Figure 2 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 5, 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 5 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 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 Figure 3, 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 6, the coefficient of interest is robust to inclusion of

cohort-by-municipality fixed effects in the regression. I interpret these results as supporting the identifying assumption that correlated shocks are unlikely to drive the results.

3.3 Results

[Table 7 HERE]

Table 7 displays the main results of the paper, reporting OLS estimates of model (1). Earnings are measured in 2015 USD in columns (1)–(4) and as percentile ranks in columns (5)–(8). The results are comparable across the two measures.

Columns (1) and (5) report the association between own parental earnings and adult earnings without controlling for peer exposure. Columns (2) and (6) add schoolmates' parental earnings. The decline in the own-parent coefficient reflects the positive correlation between students' and schoolmates' parental backgrounds. Columns (3) and (7) add school and cohort fixed effects, while columns (4) and (8) additionally include school-specific linear cohort trends. These final columns are the preferred specifications because the trends absorb gradual changes in sorting into schools, as discussed in Section 3.2.

Two main results derive from the estimates in Table 7. First, the coefficient of interest is positive and statistically different from zero at the 99% confidence level, indicating that peer exposure affects adult earnings. A one-percentile increase in schoolmates' average parental earnings raises adult earnings rank by 0.069 percentiles. In levels, a \$1 increase in schoolmates' average parental earnings raises annual adult earnings by \$0.092. Second, the level estimate is approximately 42% of the parent–child earnings coefficient. Overall, the estimates identify exposure to schoolmates' parental earnings as a statistically significant and quantitatively important determinant of adult earnings.

3.4 Effect Heterogeneity and Nonlinearity

[Figure 4 HERE]

[Table 8 HERE]

Figure 4 documents how the exposure effect varies across groups of students and schools. For each dimension, the graph reports the point estimates and 90% confidence intervals of the marginal effect of schoolmates' parental earnings, obtained by interacting peer exposure in eq. 1 with group indicators; the underlying estimates and the tests for differences across groups are reported in Table 8. Three patterns stand out. First, the effect is larger for boys than for girls ($0.160 vs. $0.041 per $1 of schoolmates' parental earnings; the difference is significant at the 90% level), and for children of college-educated parents than for children whose parents have no college degree ($0.142 vs. $0.031; significant at the 99% level). Second, the effect is concentrated in smaller schools: it amounts to $0.173 in cohorts of fewer than 90 students and to $0.036 in larger ones (significant at the 90% level), consistent with closer interactions among schoolmates when peer groups are smaller. Third, point estimates rise with own parental earnings, from $0.056 for children in the bottom tercile to $0.118 for those in the top tercile, but these differences, as well as those by school location and school income, are not statistically significant. Hence, exposure to higher-earning peers benefits children from all parental backgrounds, with no evidence that returns are larger for children from low-earning families.

[Figure 5 HERE]

However, the exposure effect is nonlinear. Figure 5 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[10]. The effect is decreasing in the level of peers' 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,

[10]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.

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.

4 Peer Exposure and Intergenerational Mobility

The estimates above imply that sorting in social exposure can alter intergenerational mobility. When children from high-earning families are systematically exposed to peers from similarly advantaged families, the positive exposure effect reinforces the transmission of economic advantage across generations. This section quantifies this contribution with a simple accounting framework that combines three objects: the parent–child earnings gradient, the sorting of children into peer groups by parental earnings, and the causal return to schoolmates' parental earnings. Throughout, x denotes a child's parental earnings rank, and all earnings are measured in percentile ranks.

4.1 Accounting Framework

Intergenerational persistence. Following the literature on intergenerational mobility (Chetty et al., 2014), persistence is summarized by the rank–rank relationship between children's adult earnings Y_i and parental earnings X_i:

$$Y_i = \alpha_0 + \alpha_1 X_i + u_i, \tag{2}$$

where α_1 measures how much of the parental advantage is transmitted to children: a one-percentile increase in parental earnings raises the expected adult earnings rank by α_1 percentiles.

Sorting into peer groups. The degree of sorting is captured by the relationship between a child's parental earnings and those of her schoolmates. Because sorting in Danish high schools is concentrated at the top of the parental-earnings distribution (Figure 1), I allow this relationship to be quadratic:

$$\bar{X}_{-i} = P(X_i) + v_i, \qquad P(x) = \rho_0 + \rho_1 x + \rho_2 x^2. \tag{3}$$

The slope $P'(x) = \rho_1 + 2\rho_2 x$ measures how much more advantaged a child's peers become as her own parental earnings increase. Without sorting, $P'(x) = 0$ and all children are exposed

to the same peers on average.

Returns to exposure. To let the return to peer exposure vary with own parental background, I augment the model in eq. 1 with an interaction between own and schoolmates' parental earnings:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 \bar{X}_{-i} + \beta_3 \bar{X}_{-i} X_i + Z_i'\delta + \gamma_{s(i)} + \tau_{s(i)} c(i) + \varepsilon_i, \tag{4}$$

so that the causal return to an extra percentile of schoolmates' parental earnings for a child with parental rank x is $\beta_2 + \beta_3 x$. As in the main specification, β_2 and β_3 are identified from within-school, across-cohort deviations in school composition.

Decomposition. Substituting eq. 3 into eq. 4 and differentiating with respect to parental earnings yields the local parent–child gradient

$$\frac{\partial\, \mathbb{E}[Y_i \mid X_i = x]}{\partial x} = \underbrace{\beta_1 + \beta_3 P(x)}_{\text{direct parental effect}} + \underbrace{(\beta_2 + \beta_3 x)\, P'(x)}_{\text{sorting in peer exposure}}. \tag{5}$$

The second term is the contribution of peer exposure to persistence: moving up the parental-earnings distribution, children are exposed to peers with higher parental earnings at rate $P'(x)$, and each additional percentile of peers' parental earnings raises adult earnings by $\beta_2 + \beta_3 x$. This term vanishes when there is no sorting or when peer exposure has no effect on earnings. Relative to the observed rank–rank slope, the share of intergenerational persistence explained by peer exposure at parental rank x is

$$s(x) = \frac{(\beta_2 + \beta_3 x)(\rho_1 + 2\rho_2 x)}{\alpha_1}. \tag{6}$$

Removing this component from the observed gradient gives the counterfactual parent–child relationship that would prevail absent sorting in peer exposure, $\tilde{Y}(x) = \alpha_0 + \alpha_1\big(1 - s(x)\big)x$.

4.2 Estimates for Denmark

In the Danish sample, the estimated rank–rank slope is $\hat{\alpha}_1 \approx 0.18$: expected adult earnings increase from about the 42^{nd} percentile for children from the poorest families to the 59^{th} percentile for children from the richest families.

Figure 6 plots the estimated sorting profile $\hat{P}(x)$. Peers' parental earnings are essentially flat, around the 48^{th} percentile, for children in the bottom fifth of the parental-earnings distribution ($\hat{\rho}_1 \approx -0.047$, $\hat{\rho}_2 \approx 0.0013$, with a minimum around the 18^{th} percentile), and then rise steadily to about the 57^{th} percentile at the top. The implied sorting slope $\hat{P}'(x)$ is close to zero at the bottom, about 0.08 at the median, and about 0.21 at the top of the distribution.

Figure 7 plots the estimated return to exposure $\hat{\beta}_2 + \hat{\beta}_3 x$. The return increases with parental earnings, from about 0.04 at the bottom ($\hat{\beta}_2 \approx 0.040$) to about 0.09 at the top ($\hat{\beta}_3 \approx 0.0005$). At the median, it is close to the linear estimate of 0.069 reported in Table 7. Consistent with Figure 4, the confidence intervals are wide and the difference in returns across parental backgrounds is imprecisely estimated.

[Figure 6 HERE]

[Figure 7 HERE]

Combining these estimates, Figure 8 reports the share $\hat{s}(x)$ of intergenerational persistence attributable to peer exposure over the parental-earnings distribution. The contribution is negligible at the bottom, where sorting is absent, and increases steadily toward the top, where both the sorting slope and the return to exposure are largest. Peer exposure accounts for approximately 9% of intergenerational persistence at the 90^{th} percentile of parental earnings and for about 11% at the very top.

[Figure 8 HERE]

Figure 9 translates the decomposition into a counterfactual parent–child earnings gradient. The solid line shows the observed relationship $\hat{\alpha}_0 + \hat{\alpha}_1 x$, while the dashed line shows the counterfactual $\tilde{Y}(x)$ that removes the component generated by sorting in peer exposure.

The two lines coincide in the bottom half of the distribution and diverge at higher parental ranks: at the top, sorting in peer exposure raises expected adult earnings by about two percentile ranks. Peer sorting thus strengthens persistence where Danish families are most advantaged.

[Figure 9 HERE]

4.3 Benchmarking against U.S. Residential Sorting

The same framework can be used to assess how persistence would change under stronger sorting. I replace the Danish sorting profile with the degree of residential sorting observed across U.S. Census tracts. The U.S. analogue of $P(x)$ is the relationship between a child's parental-income rank and the average parental-income rank of the other children growing up in the same tract. This relationship cannot be estimated directly from public data, but its slope can be recovered from tract-level statistics in the Opportunity Atlas. The intuition is simple: if families were randomly distributed across tracts, a child's parental rank would say nothing about her neighbors' parental ranks, and the slope would be zero; if tracts were perfectly segregated, neighbors would have exactly the same parental rank, and the slope would be one. In general, the slope equals the share of the total variation in parental rank that occurs *between* tracts rather than within them. Because parental ranks are uniformly distributed nationally, this share only requires the dispersion of mean parental rank across tracts, which the Opportunity Atlas reports. The implied slope is $\rho^{US} \approx 0.30$, so that $P_{US}(x) \approx 35 + 0.30\,x$: a 10-percentile increase in parental income rank is associated with an increase of about 3 percentiles in the mean parental rank of a child's neighbors. The full derivation is reported in Appendix B. The U.S. sorting slope is steeper than the Danish one over most of the distribution. Holding the Danish returns to exposure and the Danish rank–rank slope fixed, the ratio between persistence under U.S. sorting and observed persistence at parental rank x is

$$R(x) = 1 + \frac{(\beta_2 + \beta_3 x)\big(P'_{US}(x) - P'(x)\big)}{\alpha_1}. \tag{7}$$

Figure 10 plots $\hat{R}(x)$. Under U.S.-style sorting, persistence would be about 8% higher for children below the 60^{th} percentile of parental earnings, and the increase would fall to about 4% at the top. The increase is smaller at the top because Danish schools are already strongly sorted in that part of the distribution. This exercise should be interpreted as an accounting counterfactual: it holds the estimated exposure response fixed while changing the degree of sorting.

[Figure 10 HERE]

5 Education and Labor Market Outcomes

5.1 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 9 documents education levels and labor market outcomes at ages 35–39 for the whole sample and conditioning on terciles of parental earnings. As shown in the table, 72.5% of the students eventually obtain a university degree. This probability increases with parental earnings: children from the highest tercile are 13.6 *p.p.* (20.6%) 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 2.2 *p.p.* (2.7%) more likely to be employed and, conditional on being employed, 1.3 *p.p.* (48.1%) more likely to hold a managerial position. Finally, they work at higher-paying plants. At ages 35–39, the difference in plant-specific pay premia between low and high-SES is 4.1 *p.p.*: children from the bottom tercile work at plants paying 0.5% below average, while children from the top tercile work at plants paying 3.6% above average.[11] These differences are reflected by differences in hourly wages, which are \$4.85 (15.2%) higher for children from families at the top tercile of the earnings distribution than those from the bottom tercile.

[11] 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.

[Table 9 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 at ages 35–39.

5.2 Exposure Effects on Education and Labor Market Outcomes

Table 10 reports OLS estimates of the coefficients in eq. 1, replacing the dependent variable with each education or labor-market outcome and standardizing parental earnings to have mean zero and standard deviation one. The lower panel interacts peer exposure with indicators for terciles of own parental earnings. A one-standard-deviation increase in average schoolmate parental earnings raises annual earnings by \$709.65 (1.17%) and hourly wages by \$0.17 (0.51%). It increases the probability of holding a managerial position by 0.2 percentage points (7.09%) and the firm pay premium by 0.2 percentage points (13.79%). By contrast, the estimated effect on college completion is only 0.2 percentage points and is statistically imprecise. The tercile-specific estimates suggest larger earnings and wage effects among students from high-earning families, but the confidence intervals do not support a precise conclusion about heterogeneity across parental backgrounds.

[Table 10 HERE]

The small and imprecise college estimate provides limited support for education as the principal mechanism, although it does not rule out human-capital spillovers (Fruehwirth and Gagete-Miranda, 2019) or information about educational choices (Cattan et al., 2025). The clearer effects on wages, managerial positions, and firm pay premia instead point toward labor-market interactions. The next section examines whether connections among former schoolmates facilitate job access and transmit the returns to peers' career advancement.

6 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.

6.1 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.[12] 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 many job switches are due to school

[12]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).

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[13] 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.

[Figure 11 HERE]

Figure 11 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 significant for the subpopulations considered.

[13]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.

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.[14] 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 former 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.

[14]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).

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?

6.2 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:

$$W_{sc,t} = \alpha_{sc}^{\tau} + \alpha_{t}^{\tau} + \sum_{l} \delta_{l}^{\tau} \left(M_{sc}^{\tau} \cdot \mathbb{1}\{t = \tau + l\} \right) + \epsilon_{sc,t}. \tag{8}$$

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 SantAnna (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 12 HERE]

Figure 12 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. 8 for each year after the treatment l, aggregated across different years of treatment τ as in Callaway and SantAnna (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 13 HERE]

To gain further insight into how a peer's promotion affects own wages, Figure 13 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 14 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 14 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.[15] 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. When a school connection gets promoted to a managerial position, her peers see a persistent increase

[15]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.

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.

7 Mediation Analysis

The previous sections establish two facts. First, exposure to schoolmates from higher-earning families raises adult earnings (Section 3). Second, the career progression of former schoolmates generates wage gains for their peers (Section 6.2). This section connects the two and asks how much of the return to peer exposure operates through the career progression of former schoolmates.

7.1 Motivating Evidence

The starting point is who becomes a manager. Panel (a) of Figure 15 plots the share of students holding a managerial position at each age between 25 and 35, separately for students from low- and high-SES families. Promotions to managerial positions begin in the mid-twenties, and a gap by parental background opens up around age 30: by age 35, 6.5% of high-SES students are managers, compared with 4.2% of low-SES students.

Because high-SES students are more likely to be promoted, students who attend school with high-SES peers are more likely to see a former schoolmate reach a managerial position early in their careers. Panel (b) of Figure 15 plots the share of students who have at least one former schoolmate in a managerial position by each age, separately for students exposed to high-SES peers and to low-SES peers (the top and bottom quartile of schoolmates' parental earnings, respectively). At age 25 the two groups differ little (38% vs. 32%). The gap then widens quickly: by age 30, 91% of students exposed to high-SES peers have a former schoolmate who is a manager, compared with 70% of students exposed to low-SES peers. By age 35, almost all students in both groups have experienced a peer promotion (98% vs. 89%).

[Figure 15 HERE]

Two lessons follow. First, exposure to high-SES peers mainly *advances the timing* of the first peer promotion, rather than determining whether a peer promotion happens at all. Second, since a peer's promotion raises wages (Section 6.2), an earlier peer promotion can translate into higher earnings at a given age. The mediation analysis below quantifies this

channel. The raw differences in panel (b) partly reflect sorting into schools; in the analysis, the shift in the timing of peer promotions is estimated with the same within-school, across-cohort design used for the main results.

7.2 Target Parameter

I formalize the question with potential outcomes. For each student i, let $P_i \in \{r, r'\}$ denote exposure to low-SES peers (r, bottom quartile of schoolmates' parental earnings) or high-SES peers (r', top quartile), and let S_i denote the age at which a former schoolmate of i is first promoted to manager. Let $Y_{i,t}(p, s)$ be the earnings of student i at age t if she were exposed to peers p and her first peer promotion occurred at age s, and let $S_i(p)$ be the age of her first peer promotion under exposure p. Observed outcomes are $Y_{i,t} = Y_{i,t}(P_i, S_i(P_i))$ and $S_i = S_i(P_i)$.

The target parameter is the *mediated effect*

$$\theta_t(r, r') = \mathbb{E}\big[Y_{i,t}\big(r', S_i(r')\big) - Y_{i,t}\big(r', S_i(r)\big) \mid P_i = r'\big]. \tag{9}$$

In words, consider a student who went to school with high-SES peers. θ_t compares her actual earnings at age t with the earnings she would have had if she had kept exactly the same schoolmates, but her first former schoolmate had been promoted at the age at which this would have happened had she been exposed to low-SES peers. All other channels through which peers may matter—human capital spillovers, social norms, information about educational choices—are held fixed at the high-exposure level; only the timing of the first peer promotion changes. θ_t therefore measures the part of the exposure effect that operates through earlier access to former schoolmates in managerial positions.

The total effect of exposure on the treated, $\beta_t(r, r') = \mathbb{E}[Y_{i,t}(r', S_i(r')) - Y_{i,t}(r, S_i(r)) \mid P_i = r']$, can then be written as the sum of the mediated effect and a direct effect δ_t that

captures all remaining channels:

$$\beta_t(r,r') = \underbrace{\mathbb{E}[Y_{i,t}(r', S_i(r')) - Y_{i,t}(r', S_i(r)) \mid P_i = r']}_{\theta_t(r,r')\text{: mediated effect}} + \underbrace{\mathbb{E}[Y_{i,t}(r', S_i(r)) - Y_{i,t}(r, S_i(r)) \mid P_i = r']}_{\delta_t(r,r')\text{: direct effect}}. \tag{10}$$

7.3 Identification

The difficulty in estimating θ_t is that it involves $S_i(r)$, the timing of the first peer promotion that a student exposed to high-SES peers would have experienced with low-SES peers, which is never observed. The key to identification is that promotion timing is ordered: exposure to high-SES peers can only move a student's first peer promotion earlier, one year at a time. The mediated effect can then be built up age by age. For each age q, two questions are asked: (i) for how many students does exposure to high-SES peers bring the first peer promotion forward, so that it has occurred by age q rather than after q? and (ii) how much does having that promotion one year earlier raise their earnings? The mediated effect is the sum, over ages, of the product of the two:

$$\theta_t(r,r') = \sum_{q=1}^{T} \pi_c^q(r,r')\, \tau_{t|c}^q(r,r'). \tag{11}$$

Shift in the probability of a peer promotion. $\pi_c^q(r,r')$ is the share of students for whom exposure to high-SES peers shifts the first peer promotion across age q. It equals the causal effect of exposure on the probability of having experienced a peer promotion by age q, i.e. the causal counterpart of the gap in panel (b) of Figure 15. I estimate it with the within-school, across-cohort exposure design of eq. 1, using as outcome an indicator for having a former schoolmate in a managerial position by age q.

Effect of an earlier peer promotion. $\tau_{t|c}^q(r,r')$ is the earnings effect at age t of experiencing the first peer promotion at age q rather than $q+1$. I estimate it with a conditional difference-in-differences design that builds on the staggered design of Section 6.2, comparing students whose first peer promotion occurs at age q with otherwise similar students whose first peer promotion occurs one year later. Figure A1 in the Appendix reports the resulting

event-study estimates. Before the promotion, the earnings of the two groups evolve in parallel, with pre-promotion coefficients fluctuating around zero and showing no trend. After the promotion, the earnings of students whose peer was promoted earlier increase, by between \$300 and \$670 per year over the following five years.

The mediation interpretation adds two assumptions to those used in the component designs. First, mediator monotonicity requires exposure to higher-SES peers not to delay the first peer promotion. Second, an effect-transportability condition requires the promotion effect estimated among individuals observed with a promotion at q to represent the effect for individuals whose promotion timing is shifted across threshold q by peer exposure. The full potential-outcomes setup, threshold decomposition, identification conditions, estimators, and school-clustered inference are presented in Appendix A.

7.4 Results

The age profiles support the proposed mechanism. The total exposure effect on earnings becomes positive in the late twenties and grows as former schoolmates progress in their careers. Exposure to high-SES peers also raises the probability of having a former schoolmate in a managerial position. Finally, the event-study estimates show an earnings increase following a peer's promotion, with the largest effects in the first years after the event.

Figure 16 brings these pieces together. Orange markers report the total effect of exposure to high-SES peers on yearly earnings at each age, β_t; green markers report the mediated component θ_t attributed to earlier access to former schoolmates in managerial positions. The total effect becomes positive around age 27 and grows to more than \$1,000 per year in the mid-thirties. The mediated component is close to zero until the early thirties and becomes economically meaningful precisely when former schoolmates begin entering managerial positions, reaching about \$500–\$600 per year between ages 33 and 37. Over ages 35–39, the ages at which adult earnings are measured in the main analysis, the total effect averages about \$1,190 per year and the mediated component about \$470 per year. The decomposition therefore indicates that approximately 40% of the return to peer exposure operates through increased access to attractive labor-market opportunities associated with former peers' career advancement.

[Figure 16 HERE]

8 Discussion

This paper investigates peer exposure as a determinant of social mobility: differences in parental resources extend beyond the family through social interactions. A $1 increase in schoolmates' parental earnings raises annual adult earnings by approximately $0.09. Moreover, differences in access to high-paying jobs by parental background coexist with an active role for schoolmates in facilitating job access and generating wage gains. This section relates the results to the existing literature and discusses their interpretation and external validity.

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. (2025) 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).[16] 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 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

[16] 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.

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., 2025) 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.

9 Conclusions

This paper identifies the effect of interactions across socioeconomic groups on children's adult earnings. A \$1 increase in the average parental earnings of schoolmates raises annual adult earnings by approximately \$0.09. Peer exposure has little precisely estimated effect on college completion, while school connections persist in the labor market, facilitate access to higher-paying firms, and transmit gains from peers' career advancement. The mobility accounting exercise attributes approximately 9% of intergenerational persistence among children from high-earning Danish families to peer exposure and implies that persistence would be about 7% higher under the degree of sorting observed across U.S. Census tracts. The mediation analysis attributes approximately 40% of the return to peer exposure at ages 35–39 to labor-market opportunities associated with former peers' career advancement. Together, these results identify social interactions as a channel through which segregation reproduces

economic advantage across generations.

A Formal Mediation Framework

This appendix presents the potential-outcomes framework underlying the mediation exercise in Section 7. The treatment is exposure to peers from different parental backgrounds, and the mediator is the timing of the first managerial promotion among an individual's former schoolmates. The framework accommodates an ordered, event-time mediator and connects the paper's within-school exposure design to the promotion-timing design.

A.1 Setup and target parameter

Let $Y_{i,t} \in \mathbb{R}$ denote the earnings of individual i at time $t \in \{0, \ldots, T\}$. At $t = 0$, the individual receives a time-invariant treatment $P_i \in \mathcal{P}$ and subsequently experiences an event-time mediator $S_i \in \mathcal{T} \equiv \{1, \ldots, T, \infty\}$. The value $S_i = \infty$ indicates that the event does not occur during the observation window. In the application, $P_i = r'$ denotes exposure in the top quartile of the leave-one-out distribution of schoolmate parental earnings and $P_i = r$ denotes exposure in the bottom quartile. The mediator S_i is the year in which the first former schoolmate is promoted to a managerial position.

Let $Y_{i,t}(p, s)$ be the potential outcome at t under treatment p and mediator date s, and let $S_i(p)$ be the potential mediator date under treatment p. Consistency gives

$$Y_{i,t} = Y_{i,t}\big(P_i, S_i(P_i)\big), \qquad S_i = S_i(P_i).$$

For a change in peer parental background from r to r', define the average total, mediated, and direct effects on the treated as

$$\beta_t(r, r') \equiv \mathbb{E}[Y_{i,t}(r', S_i(r')) - Y_{i,t}(r, S_i(r)) \mid P_i = r'], \tag{12}$$

$$\theta_t(r, r') \equiv \mathbb{E}[Y_{i,t}(r', S_i(r')) - Y_{i,t}(r', S_i(r)) \mid P_i = r'], \tag{13}$$

$$\delta_t(r, r') \equiv \mathbb{E}[Y_{i,t}(r', S_i(r)) - Y_{i,t}(r, S_i(r)) \mid P_i = r']. \tag{14}$$

By construction, $\beta_t(r, r') = \theta_t(r, r') + \delta_t(r, r')$. The parameter of interest is $\theta_t(r, r')$, the component of the treatment effect operating through treatment-induced changes in peer-

promotion timing.

A.2 Threshold decomposition

For every threshold $q \in \{1, \ldots, T\}$, define

$$\Delta_{i,t}^{q} \equiv Y_{i,t}(r', q) - Y_{i,t}(r', q+1),$$

with the convention that $T + 1 = \infty$. For any two mediator dates k and k', the difference in potential outcomes can be written as the sum of the effects of crossing the intervening thresholds:

$$Y_{i,t}(r', k') - Y_{i,t}(r', k) = \sum_{q=1}^{T} \Delta_{i,t}^{q} \left(\mathbf{1}\{k' \leq q < k\} - \mathbf{1}\{k' > q \geq k\} \right). \tag{15}$$

Substitution into the definition of $\theta_t(r, r')$ gives a decomposition with terms for movements in both directions across each threshold. The following monotonicity condition removes movements in the direction opposite to the treatment.

Assumption A1: Mediator monotonicity. For $r' \geq r$,

$$S_i(r') \leq S_i(r) \qquad \text{for every } i.$$

Thus, exposure to higher-SES peers may accelerate the first peer promotion but does not delay it. Under A1,

$$\theta_t(r, r') = \sum_{q=1}^{T} \underbrace{\mathbb{E}\left[\Delta_{i,t}^{q} \mid P_i = r',\ S_i(r') \leq q < S_i(r)\right]}_{\tau_{t|c}^{q}(r,r')} \underbrace{\mathbb{P}[S_i(r') \leq q < S_i(r) \mid P_i = r']}_{\pi_{c}^{q}(r,r')}. \tag{16}$$

The first term is the earnings effect of moving the mediator from $q+1$ to q among individuals induced to cross threshold q. The second term is the share of such threshold crossers.

Define the cumulative mediator indicator $M_i^q(p) = \mathbf{1}\{S_i(p) \leq q\}$. Monotonicity implies

$$\pi_c^q(r, r') = \mathbb{E}[M_i^q(r') - M_i^q(r) \mid P_i = r'] \,. \tag{17}$$

Identification of the crosser share therefore reduces to identification of the treatment effect on the probability that a peer promotion has occurred by threshold q.

A.3 Identifying the crosser share

Let X_i denote the continuous leave-one-out average parental earnings of individual i's schoolmates. The discrete treatment regimes r and r' correspond to the bottom and top quartiles of this exposure. Let $F_p(x)$ be the distribution of X_i within regime p, with mean μ_p, and let $M_i^{*q}(x)$ be the potential cumulative mediator under continuous exposure x.

Assumption A2: Linear continuous-exposure response. For every threshold q and exposure level x,

$$\mathbb{E}[M_i^{*q}(x) \mid \alpha_i^q] = \alpha_i^q + \gamma^q x,$$

over the support of X_i. This condition yields

$$\pi_c^q(r, r') = \gamma^q(\mu_{r'} - \mu_r) \equiv \gamma^q \Delta_\mu. \tag{18}$$

Let H_i contain school fixed effects, cohort fixed effects, and school-specific linear cohort trends. Write the population projections

$$X_i = H_i' b^X + Z_i, \qquad \alpha_i^q = H_i' b^q + e_i^q.$$

The observed cumulative mediator satisfies

$$M_i^q = H_i' b^q + \gamma^q X_i + u_i^q.$$

Assumption A3: Quasi-random cohort composition. For every threshold q,

$$\mathbb{E}[Z_i u_i^q] = 0.$$

After removing the components in H_i, residual variation in peer composition must be unrelated to residual determinants of peer-promotion timing. Under A2 and A3, OLS applied to the residualized equation identifies γ^q, and

$$\widehat{\pi}_c^q(r, r') = \widehat{\gamma}^q \widehat{\Delta}_\mu.$$

A.4 Identifying the effect of earlier peer promotion

Let $\Delta Y_i^{t,q} = Y_{i,t} - Y_{i,q-1}$ and let W_i contain predetermined covariates used for adjustment. Consider the difference in outcome changes between individuals whose first peer promotion occurs in q and those whose first promotion occurs in $q+1$, reweighted so that the latter group has the same distribution of W_i as the former.

Assumption A4: Promotion-timing design. The promotion has no effect before it occurs; conditional on W_i, the groups with $S_i = q$ and $S_i = q+1$ would have experienced the same earnings growth in the absence of the promotion at q; and the comparison group has positive probability over the covariate support of the treated group.

Under A4, the adjusted difference-in-differences contrast identifies

$$\tau^q_{t,\text{DiD}}(r') = \mathbb{E}[Y_{i,t}(r', q) - Y_{i,t}(r', q+1) \mid P_i = r',\ S_i(r') = q]\,. \tag{19}$$

The decomposition in eq. (16) requires this effect for threshold crossers rather than for individuals observed with $S_i(r') = q$.

Assumption A5: Effect transportability to threshold crossers. For every q and t,

$$\mathbb{E}[Y_{i,t}(r', q) - Y_{i,t}(r', q+1) \mid P_i = r',\ S_i(r') = q] \tag{20}$$

$$= \mathbb{E}[Y_{i,t}(r', q) - Y_{i,t}(r', q+1) \mid P_i = r',\ S_i(r') \le q < S_i(r)]\,. \tag{21}$$

Under A5, $\tau^q_{t,\mathrm{DiD}}(r') = \tau^q_{t|c}(r, r')$.

Assumptions A2 and A3 parallel the linear-response and quasi-random composition restrictions of the main exposure design. Assumption A4 contains the standard conditions for the conditional difference-in-differences promotion design. A1 and A5 are specific to the mediation interpretation.

A.5 Estimation and inference

For each threshold q, estimate γ^q by OLS from

$$\widetilde{M}^q_i = \gamma^q \widetilde{X}_i + u^q_i, \tag{22}$$

where tildes denote residuals after projection on H_i. Estimate $\tau^q_{t,\mathrm{DiD}}$ by a covariate-balanced difference-in-differences regression comparing $S_i = q$ with $S_i = q + 1$ among individuals in the high-exposure regime. The threshold-specific and aggregate mediated effects are

$$\widehat{\theta}^q_t(r, r') = \widehat{\gamma}^q \widehat{\Delta}_\mu \widehat{\tau}^q_{t,\mathrm{DiD}}, \qquad \widehat{\theta}_t(r, r') = \sum_q \widehat{\theta}^q_t(r, r'). \tag{23}$$

Inference uses the delta method while retaining the covariance between the mediator-response and promotion-effect estimates and across thresholds. Let $\widehat{\psi}^{\gamma,q}_i$ and $\widehat{\psi}^{\tau,q,t}_i$ be the influence-function contributions for $\widehat{\gamma}^q$ and $\widehat{\tau}^q_{t,\mathrm{DiD}}$. Treating Δ_μ and the balancing weights as fixed finite-population quantities gives

$$\widehat{\psi}^{\theta,t}_i = \Delta_\mu \sum_{q=1}^{t} \left(\widehat{\tau}^q_{t,\mathrm{DiD}} \widehat{\psi}^{\gamma,q}_i + \widehat{\gamma}^q \widehat{\psi}^{\tau,q,t}_i \right). \tag{24}$$

These contributions are summed within schools to construct the school-clustered variance estimator used for the mediation estimates.

B Recovering U.S. Residential Sorting from Tract-Level Means

This appendix derives the U.S. sorting coefficient ρ^{US} used in Section 4.3 from publicly available tract-level statistics in the Opportunity Atlas. Throughout this appendix, parental income ranks are normalized to $[0, 1]$, as in the Opportunity Atlas; the coefficient is invariant to rescaling ranks to percentiles.

Target parameter. Let $x_i \in [0, 1]$ denote the parental income rank of child i, and let g index Census tracts, with n_g children in tract g. The leave-one-out mean parental rank among the other children in i's tract is

$$\bar{x}_{-i,g} = \frac{1}{n_g - 1} \sum_{j \in g,\, j \neq i} x_j.$$

The U.S. analogue of the sorting profile in eq. 3 is the linear projection $\bar{x}_{-i,g} = \rho_0^{US} + \rho^{US} x_i + u_i$, whose slope is

$$\rho^{US} = \frac{\mathrm{Cov}(x_i, \bar{x}_{-i,g})}{\mathrm{Var}(x_i)}.$$

Decomposition into between- and within-tract variation. Let $\mu_g = \mathbb{E}[x_i \mid g]$ be the mean parental rank in tract g, and write $x_i = \mu_g + e_i$ with $\mathbb{E}[e_i \mid g] = 0$. By the law of total variance,

$$\mathrm{Var}(x_i) = \mathrm{Var}(\mu_g) + \mathbb{E}\left[\mathrm{Var}(x_i \mid g)\right].$$

Since the tract total can be written as $n_g \mu_g$, the leave-one-out mean satisfies

$$\bar{x}_{-i,g} = \frac{n_g \mu_g - x_i}{n_g - 1} = \mu_g - \frac{e_i}{n_g - 1},$$

so that

$$\mathrm{Cov}(x_i, \bar{x}_{-i,g}) = \mathrm{Var}(\mu_g) - \mathbb{E}\left[\frac{\mathrm{Var}(x_i \mid g)}{n_g - 1}\right].$$

The exact sorting coefficient is therefore

$$\rho^{US} = \frac{\mathrm{Var}(\mu_g) - \mathbb{E}\left[\mathrm{Var}(x_i \mid g)/(n_g - 1)\right]}{\mathrm{Var}(x_i)}.$$

When tracts contain many children, the leave-one-out correction is negligible and

$$\rho^{US} \simeq \frac{\mathrm{Var}(\mu_g)}{\mathrm{Var}(x_i)}. \tag{25}$$

The slope of neighbors' mean parental rank on own parental rank is thus approximately the share of the total variation in parental rank that occurs *between* tracts.

Calibration. In the Opportunity Atlas, parental income ranks are approximately uniformly distributed at the national level, so that $\mathrm{Var}(x_i) = 1/12$. The child-population-weighted standard deviation of mean parental rank across Census tracts is $SD(\mu_g) = 0.157$, so that $\mathrm{Var}(\mu_g) = 0.157^2 = 0.0246$. Eq. 25 then gives

$$\rho^{US} \simeq \frac{0.157^2}{1/12} = 0.296.$$

Because ranks are uniform with mean 0.5, the implied sorting profile is $\mathbb{E}[\bar{x}_{-i,g} \mid x_i] \simeq 0.5 + 0.296\,(x_i - 0.5)$. In percentile ranks, this corresponds to $P_{US}(x) \simeq 35.2 + 0.296\,x$: a 10-percentile increase in parental income rank is associated with an increase of about 3 percentiles in the mean parental rank of other children living in the same Census tract.

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Data Source

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

Tables

Table 1: Descriptive Statistics

	Q1	Q2	Q3	Q4	Total
A: Children					
Female	0.61	0.61	0.57	0.53	0.58
	(0.49)	(0.49)	(0.50)	(0.50)	(0.49)
Earnings	52,588.00	57,443.87	61,597.26	68,911.98	60,135.06
	(36,302.44)	(32,411.51)	(37,169.54)	(57,685.38)	(42,487.55)
Earnings (rank)	43.79	48.55	52.36	57.15	50.46
	(28.15)	(27.51)	(28.20)	(30.11)	(28.93)
College	0.63	0.68	0.73	0.79	0.71
	(0.48)	(0.47)	(0.44)	(0.41)	(0.46)
B: Parents					
Earnings	23,547.18	41,131.76	51,504.80	78,237.64	48,604.60
	(10,435.14)	(3,501.41)	(4,146.52)	(40,113.71)	(28,799.32)
Earnings (rank)	13.00	38.00	63.00	88.00	50.50
	(7.21)	(7.21)	(7.21)	(7.21)	(28.87)
College	0.30	0.37	0.56	0.74	0.49
	(0.46)	(0.48)	(0.50)	(0.44)	(0.50)
C: Peers' Parents					
Earnings	46,800.59	47,156.58	48,584.22	51,878.25	48,604.86
	(6,913.26)	(6,455.49)	(6,954.77)	(9,259.89)	(7,740.24)
Earnings (rank)	48.10	48.66	50.76	54.47	50.50
	(8.17)	(7.83)	(8.22)	(9.51)	(8.82)
College	0.46	0.46	0.50	0.55	0.49
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)
Observations	100598	100592	100592	100587	402369

Note: The table presents summary statistics for the main sample of students enrolled at a Danish high school and their parents, by quartile of parental earnings, including the sample mean and standard deviation (in parentheses), along with the sample size for each group.

Table 2: Conditional Earnings

	Q1	Q2	Q3	Q4	Total
Share Peers Q1	0.28	0.26	0.24	0.22	0.25
	(0.09)	(0.08)	(0.08)	(0.08)	(0.09)
Share Peers Q2	0.26	0.27	0.25	0.22	0.25
	(0.08)	(0.08)	(0.07)	(0.08)	(0.08)
Share Peers Q3	0.24	0.25	0.26	0.25	0.25
	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
Share Peers Q4	0.22	0.22	0.25	0.32	0.25
	(0.11)	(0.11)	(0.12)	(0.15)	(0.13)
School Size	139.23	142.00	147.37	153.87	145.62
	(70.13)	(69.09)	(65.27)	(61.37)	(66.79)
Observations	100598	100592	100592	100587	402369

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

	SD	Residual SD		
Peers' Par. - Ranks	8.82	3.15	2.99	2.61
		(36%)	(34%)	(30%)
Peers' Par. - USD	7740.24	3001.69	2702.43	2376.14
		(39%)	(35%)	(31%)
% Peers Low T.	11.40	5.93	5.36	4.44
		(52%)	(47%)	(39%)
% Peers Mid T.	7.60	5.06	4.94	4.27
		(67%)	(65%)	(56%)
% Peers Top T.	14.30	5.51	4.39	4.01
		(39%)	(31%)	(28%)
School FEs		✓	✓	✓
Cohort FEs			✓	✓
School T. Trends				✓

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 16 of the child is computed. The first column reports the standard deviation of each measure; the remaining columns report the standard deviation of its residuals from regressions on the fixed effects and school-specific time trends indicated at the bottom of the table (in parentheses, as a share of the raw standard deviation).

Table 4: Balance Test – Extended Version

		(1)			(2)			(3)		
Panel A: Overall	Par. Earnings, USD	0.239	(0.030)	[0.000]	0.006	(0.002)	[0.002]	-0.001	(0.001)	[0.491]
	Only Child	0.044	(0.008)	[0.000]	0.000	(0.001)	[0.528]	0.000	(0.000)	[0.411]
	First Born	0.035	(0.005)	[0.000]	0.001	(0.001)	[0.098]	0.001	(0.001)	[0.145]
Panel B: Mothers	Missing Parental ID	-0.003	(0.002)	[0.174]	-0.001	(0.001)	[0.527]	-0.000	(0.001)	[0.629]
	Teen Parent	-0.022	(0.003)	[0.000]	-0.001	(0.001)	[0.213]	-0.000	(0.000)	[0.492]
	Missing Education	0.010	(0.005)	[0.049]	-0.001	(0.001)	[0.218]	0.000	(0.001)	[0.616]
	High School	-0.070	(0.011)	[0.000]	0.001	(0.001)	[0.372]	0.001	(0.001)	[0.192]
	College	0.194	(0.022)	[0.000]	0.002	(0.001)	[0.003]	0.001	(0.001)	[0.356]
	Employed	0.061	(0.007)	[0.000]	0.006	(0.001)	[0.000]	0.002	(0.001)	[0.001]
	Full-time Work	0.056	(0.006)	[0.000]	0.004	(0.001)	[0.001]	0.001	(0.001)	[0.222]
	Managerial Position	0.037	(0.004)	[0.000]	0.002	(0.001)	[0.023]	0.001	(0.001)	[0.467]
	Hourly Wage	0.128	(0.017)	[0.000]	0.001	(0.001)	[0.098]	0.001	(0.001)	[0.283]
Panel C: Fathers	Missing Parental ID	0.004	(0.004)	[0.284]	-0.002	(0.001)	[0.054]	-0.001	(0.001)	[0.427]
	Teen Parent	0.001	(0.004)	[0.829]	-0.002	(0.001)	[0.067]	-0.001	(0.001)	[0.399]
	Missing Education	0.009	(0.005)	[0.083]	-0.002	(0.001)	[0.054]	-0.000	(0.001)	[0.885]
	High School	-0.110	(0.016)	[0.000]	0.001	(0.001)	[0.406]	0.000	(0.000)	[0.646]
	College	0.225	(0.027)	[0.000]	0.002	(0.001)	[0.003]	0.001	(0.001)	[0.101]
	Employed	0.018	(0.007)	[0.009]	0.004	(0.001)	[0.000]	0.001	(0.001)	[0.128]
	Full-time Work	0.030	(0.008)	[0.000]	0.003	(0.001)	[0.000]	0.000	(0.001)	[0.485]
	Managerial Position	0.075	(0.011)	[0.000]	0.003	(0.001)	[0.007]	0.001	(0.001)	[0.273]
	Hourly Wage	0.189	(0.023)	[0.000]	0.003	(0.001)	[0.001]	0.001	(0.001)	[0.417]

Note: The table reports coefficients from separate regressions regressing (standardized) schoolmates' parental earnings on several (standardized) measures of predetermined characteristics. For each specification, the coefficient, its standard error (in parentheses) and p-value [in brackets] are reported. All regressions include controls for own parental earnings, cohort fixed effects and school-level average realizations of 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).

Table 5: Residuals in School Composition are Uncorrelated Over Time

Significant at	# Schools	% of Schools
90%	34 / 348	9.77%
95%	14 / 348	4.02%
99%	1 / 348	0.29%

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 the residuals of school composition. School composition is measured as the average parental earnings of students enrolled in each school and cohort. The table reports the number (and the share) of schools for which the AR(1) coefficient is statistically different from zero at the 90%, 95% and 99% confidence level.

Table 6: Higher Order Time Trends and Alternative Specifications

	Baseline	Cohort × Mun. (Age 16)	Cohort × Mun. (HS)	Quadratic Trend	Cubic Trend	Lag + Lead	Lag Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 2015 USD							
Par. Earnings	0.219***	0.221***	0.219***	0.219***	0.219***	0.258***	0.229***
	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)
Peers' Par. Earnings	0.092**	0.116***	0.108**	0.083**	0.083**	0.069*	0.063**
	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)
Panel B: Percentile Ranks							
Par. Earnings	0.121***	0.121***	0.121***	0.121***	0.121***	0.125***	0.127***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peers' Par. Earnings	0.069***	0.078***	0.070***	0.056***	0.056***	0.069***	0.070***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	380958	372127	380958	380958	380958	254243	313208

Note: Estimates from separate OLS regressions. The dependent variable is children's adult earnings, measured in 2015 USD in Panel A and in percentile ranks of the cohort-specific distribution in Panel B. Column (1) reports the baseline specification. Columns (2) and (3) add cohort-by-municipality fixed effects, where municipality is defined as the municipality of residence at age 16 and in the year of high-school enrollment, respectively. Columns (4) and (5) replace linear school-specific time trends with quadratic and cubic trends. Columns (6) and (7) additionally control for the composition of adjacent cohorts (lags and leads, and lags only). All specifications include controls for gender, parents' year of birth, year of birth, teenage parenthood and parents' years of education. SEs in parentheses are clustered at the school-cohort level (cohort level in columns (6)–(7)). * p<0.10, ** p<0.05, *** p<0.01.

Table 7: Peer Exposure and Adult Earnings

	2015 USD				Ranks			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Par. Earnings	0.237***	0.228***	0.220***	0.219***	0.139***	0.128***	0.122***	0.121***
	(0.042)	(0.043)	(0.044)	(0.044)	(0.002)	(0.002)	(0.002)	(0.002)
Peers' Par. Earnings		0.212***	0.122***	0.092**		0.177***	0.085***	0.069***
		(0.038)	(0.038)	(0.039)		(0.018)	(0.018)	(0.018)
Observations	380987	380958	380958	380958	380987	380958	380958	380958
Cohort FE	No	No	Yes	Yes	No	No	Yes	Yes
School FE	No	No	Yes	Yes	No	No	Yes	Yes
School Trend	No	No	No	Yes	No	No	No	Yes
R^2	0.09	0.09	0.11	0.11	0.12	0.13	0.15	0.15

Note: Estimates from separate OLS regressions. The dependent variable is adult annual earnings measured in 2015 USD in columns (1)–(4) and as a percentile rank within birth cohort in columns (5)–(8). Parental earnings and schoolmates' parental earnings use the corresponding units. All specifications include fixed effects for year of birth, mother age at birth, father age at birth and gender. SEs in parentheses are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.01.

Table 8: Heterogeneity of Exposure Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Peers' Par. Earnings	0.160***	0.031	0.056	0.173***	0.099**	0.128***
	(0.055)	(0.043)	(0.043)	(0.064)	(0.046)	(0.043)
× Female	-0.119*					
	(0.069)					
× College Edu. Parents		0.111***				
		(0.026)				
× Mid Par. Earnings			0.039			
			(0.024)			
× Top Par. Earnings			0.062			
			(0.048)			
× ≥ 90 Peers				-0.137*		
				(0.078)		
× Urban School					-0.044	
					(0.085)	
× Rich School						-0.050
						(0.065)
Observations	380958	380958	380958	380958	380958	380958
Cohort × Group FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
School Trend	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.11	0.11	0.11	0.11	0.11	0.11

Note: Estimates from separate OLS regressions of eq. 1, where schoolmates' parental earnings are interacted with indicators for the group reported in each row, and cohort fixed effects are allowed to vary by group. Adult earnings and parental earnings are measured in 2015 USD. The coefficient on Peers' Par. Earnings is the effect for the reference group: boys in column (1), children whose parents have no college degree in column (2), children in the bottom tercile of parental earnings in column (3), cohorts of fewer than 90 students in column (4), non-urban schools in column (5) and low-income schools in column (6). Interaction terms report the difference relative to the reference group. All specifications include school fixed effects and school-specific linear time trends. SEs in parentheses are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.01.

Table 9: Education and Labor Market Outcomes: SES Gradient

	Parental Earnings Tercile			
	Bottom	Middle	Top	Total
Education				
High School	0.965	0.982	0.986	0.978
	[136,813]	[132,781]	[132,775]	[402,369]
College	0.660	0.722	0.796	0.725
	[136,813]	[132,781]	[132,775]	[402,369]
College: STEM	0.151	0.153	0.169	0.158
	[90,340]	[95,838]	[105,641]	[291,819]
Labor Market				
Employed	0.817	0.858	0.839	0.838
	[136,813]	[132,781]	[132,775]	[402,369]
Hourly Wage	32.008	33.614	36.862	34.162
	[116,202]	[117,875]	[116,686]	[350,763]
Manager	0.027	0.031	0.040	0.033
	[136,813]	[132,781]	[132,775]	[402,369]
Senior Employee	0.342	0.406	0.459	0.402
	[136,813]	[132,781]	[132,775]	[402,369]
Firm Pay Premium (AKM)	-0.005	0.008	0.036	0.013
	[123,276]	[123,010]	[121,770]	[368,056]

Note: The table presents average outcomes for students in the sample, measured at ages 35–39. Sample averages conditional on tercile of parental earnings are reported, with the number of observations in each cell reported in square brackets. 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, engineering or mathematics. Employed is a dummy variable equal to one if the individual is employed. Manager is a dummy variable equal to one if the individual is employed as a manager; Hourly Wage is the hourly wage at the main occupation; these variables are defined only for employed individuals. Firm Pay Premium is the AKM fixed effect (as in Abowd et al. (1999)) of the plant where the individual is employed; the variable is defined only for employed individuals.

Table 10: Education and Labor Market Outcomes: Exposure Effect

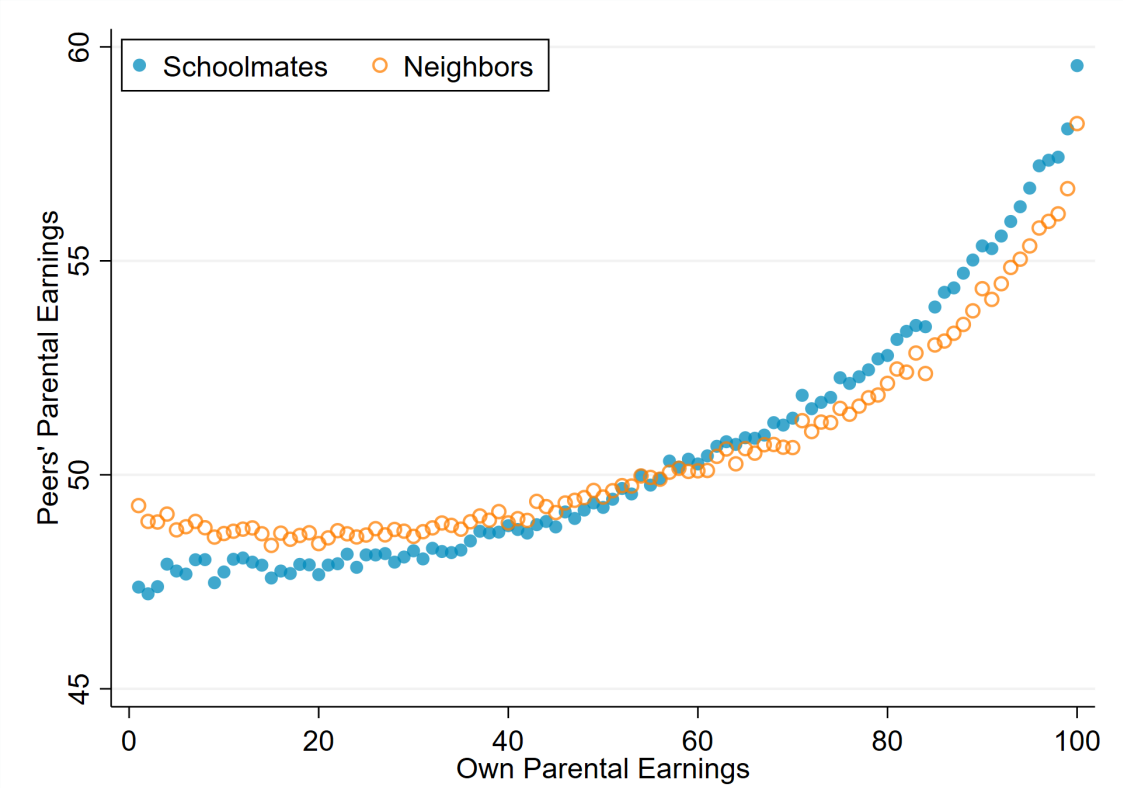
Panel A	Earnings	High School	College	STEM College	Employed	Wage/h	Manager	Senior Employee	Firm AKM
SMs P. Earn.	709.651**	0.002***	0.002	0.001	0.001	0.173*	0.002**	0.003	0.002**
	(304.254)	(0.001)	(0.002)	(0.002)	(0.002)	(0.092)	(0.001)	(0.003)	(0.001)
Observations	380958	380958	380958	380958	380958	333497	380958	380958	349314
R^2	0.11	0.02	0.11	0.08	0.01	0.07	0.02	0.09	0.10
Mean D.V.	60433.2	.98	.73	.12	.84	34.21	.03	.4	.01
Effect Size	1.17 %	.25 %	.32 %	.5 %	.13 %	.51 %	7.09 %	.86 %	13.79 %

Panel B	Earnings	High School	College	STEM College	Employed	Wage/h	Manager	Senior Employee	Firm AKM
× Low P. Earn.	380.624	0.002**	0.002	-0.002	-0.003	0.083	0.003***	0.001	0.001
	(314.365)	(0.001)	(0.003)	(0.002)	(0.003)	(0.105)	(0.001)	(0.003)	(0.001)
× Mid P. Earn.	712.781*	0.002**	0.002	0.002	0.000	0.039	0.002**	0.004	0.001
	(367.614)	(0.001)	(0.003)	(0.002)	(0.003)	(0.113)	(0.001)	(0.003)	(0.001)
× Top P. Earn.	954.823***	0.002***	0.002	0.001	0.003	0.349***	0.001	0.004	0.003***
	(344.948)	(0.001)	(0.002)	(0.002)	(0.002)	(0.099)	(0.001)	(0.003)	(0.001)
Observations	380958	380958	380958	380958	380958	333497	380958	380958	349314
R^2	0.11	0.02	0.11	0.08	0.01	0.07	0.02	0.09	0.11
Mean D.V., Low P. Earn.	53987.71	.97	.66	.1	.82	32.01	.03	.35	-.01
Effect Size	.71 %	.25 %	.31 %	-1.72 %	-.37 %	.26 %	11.91 %	.23 %	-20.8 %
Mean D.V., Mid P. Earn.	59567.42	.98	.72	.11	.86	33.62	.03	.41	.01
Effect Size	1.2 %	.21 %	.3 %	2.03 %	.03 %	.12 %	7.9 %	.87 %	10.25 %
Mean D.V., Top P. Earn.	67537.33	.99	.8	.13	.84	36.88	.04	.46	.04
Effect Size	1.41 %	.24 %	.22 %	.76 %	.41 %	.95 %	3.54 %	.96 %	7.75 %

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 eq. 1, while Panel B reports the coefficients for the same model upon inclusion of interaction terms with dummies for parental earnings' terciles. All specifications include 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.

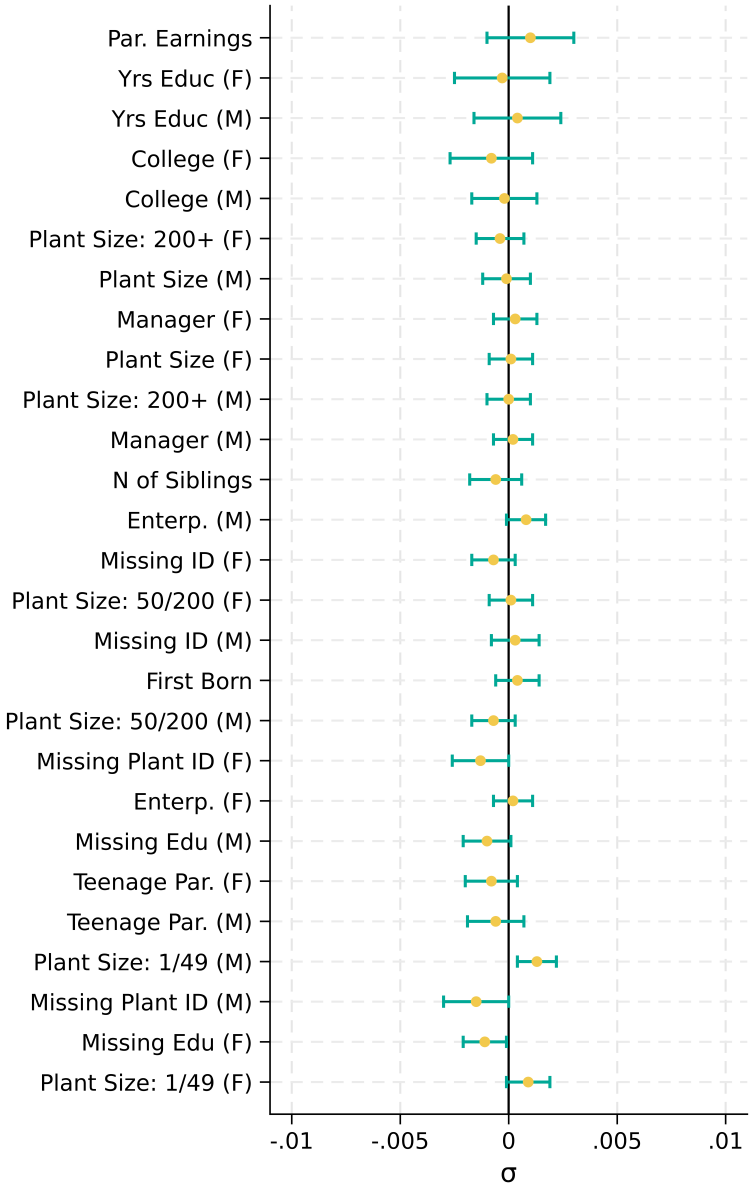
Figures

Figure 1: Sorting across Schools and Neighborhoods



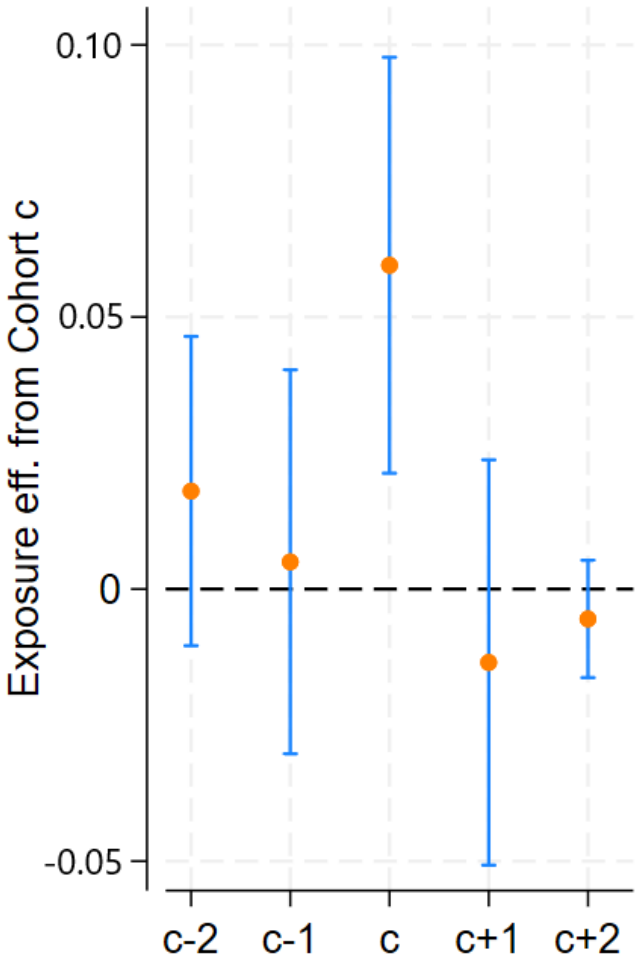
Note: The graph plots the sample average of schoolmates' and neighbors' parental earnings, conditional on the percentile of the in-sample parental earnings distribution. Peers' parental earnings are also measured in percentile ranks of the in-sample parental earnings distribution. Blue dots represent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

Figure 2: Predetermined Characteristics are Orthogonal to (Residuals of) Peers' Parental Earnings



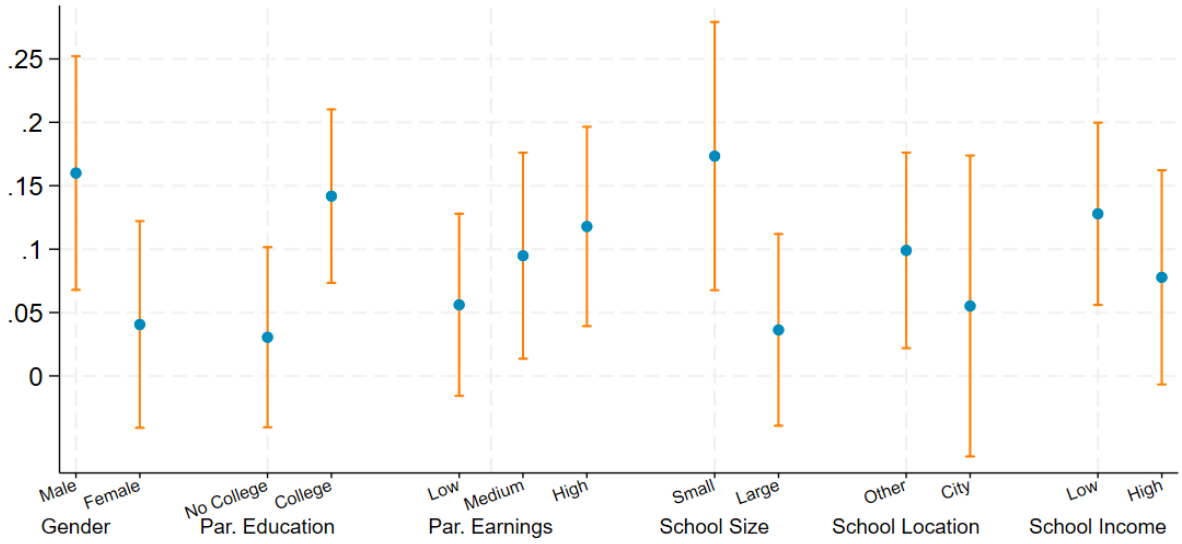
Note: The graph reports coefficients (and confidence intervals) from separate regressions regressing (standardized) schoolmates' parental earnings on different (standardized) measures of predetermined characteristics, including controls for own parental earnings, school-specific time trends and school-level average realizations of 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). (F) and (M) refer to fathers and mothers, respectively.

Figure 3: Adjacent Cohorts



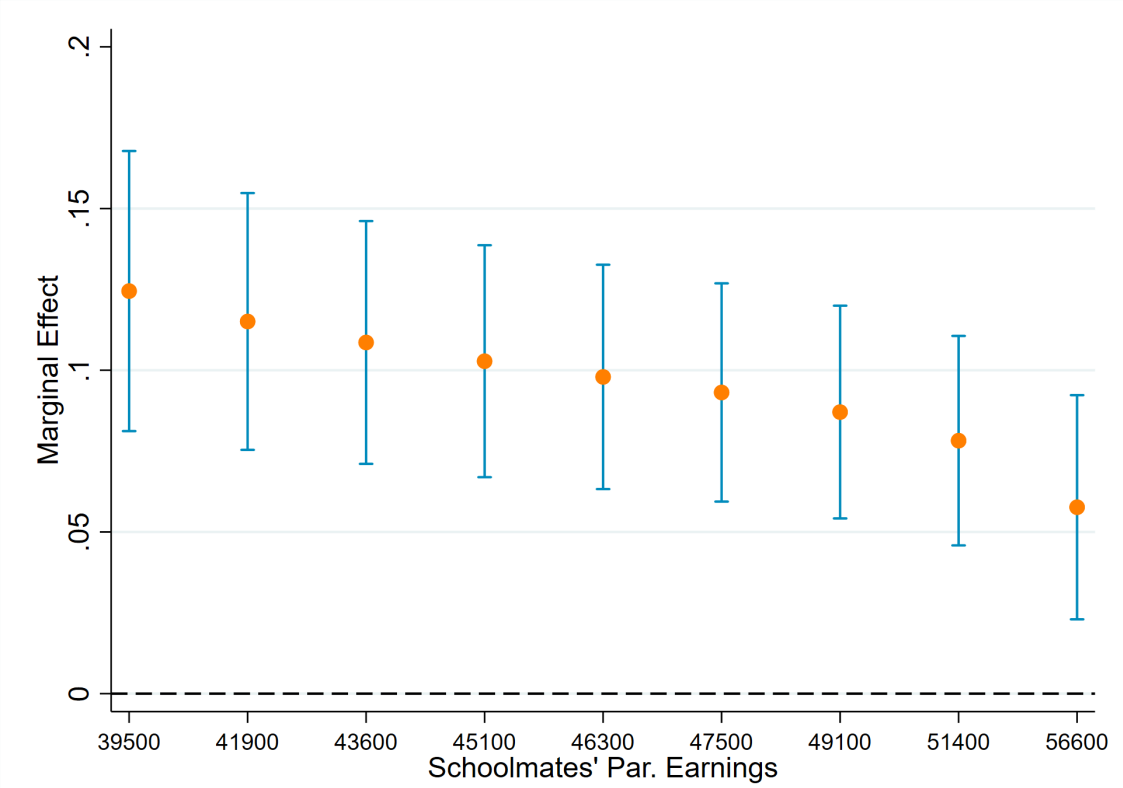
Note: The graph reports estimates (and confidence intervals) of the effect on children's adult earnings of $\tilde{X}_{i,t+c}$, the average of parental earnings among students who enrolled in the same school as i, c years after the actual cohort of i, for $c \in \{-2, -1, 0, 1, 2\}$. SEs are clustered at the school-cohort level.

Figure 4: Heterogeneity of Exposure Effects



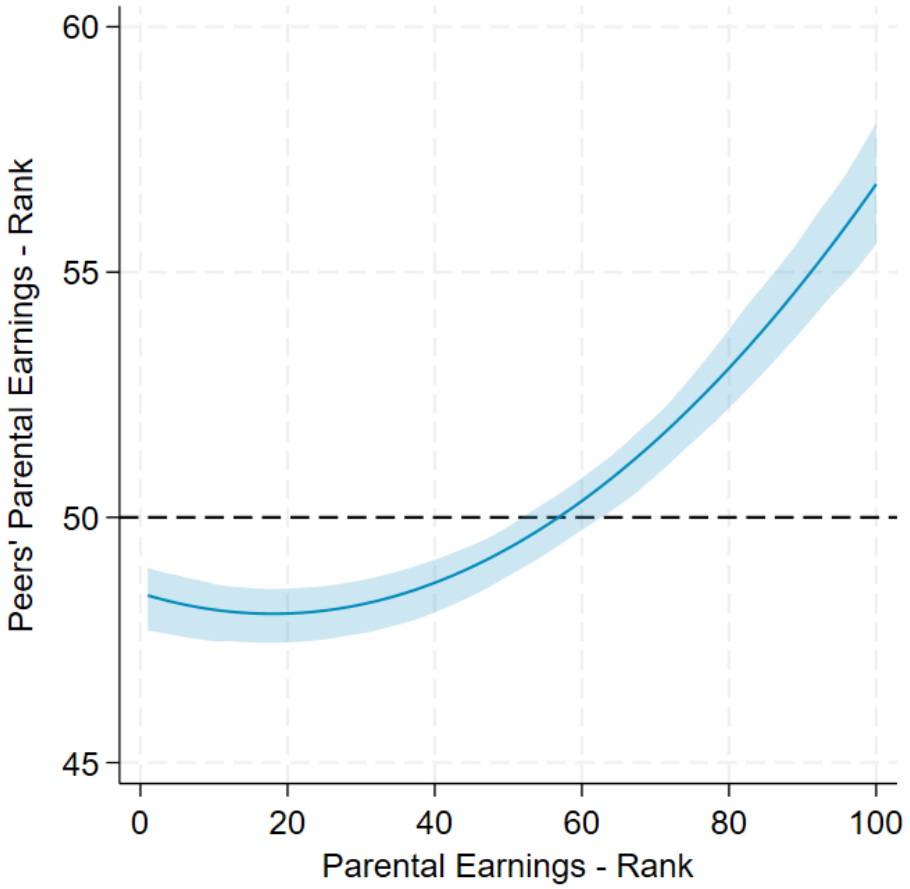
Note: The graph plots the marginal effect of schoolmates' parental earnings on own adult earnings (both in 2015 USD), along with 90% confidence intervals, separately for groups of students defined along six dimensions: gender, parental education (no parent vs. at least one parent with a college degree), tercile of own parental earnings, school size (cohorts of fewer than 90 vs. 90 or more students), school location (urban vs. other) and school income (low- vs. high-income schools). Each dimension comes from a separate estimation of eq. 1 in which schoolmates' parental earnings are interacted with group indicators, and cohort fixed effects are allowed to vary by group. All specifications include school fixed effects and school-specific linear time trends. Standard errors are clustered at the school level.

Figure 5: Decreasing Marginal Effects



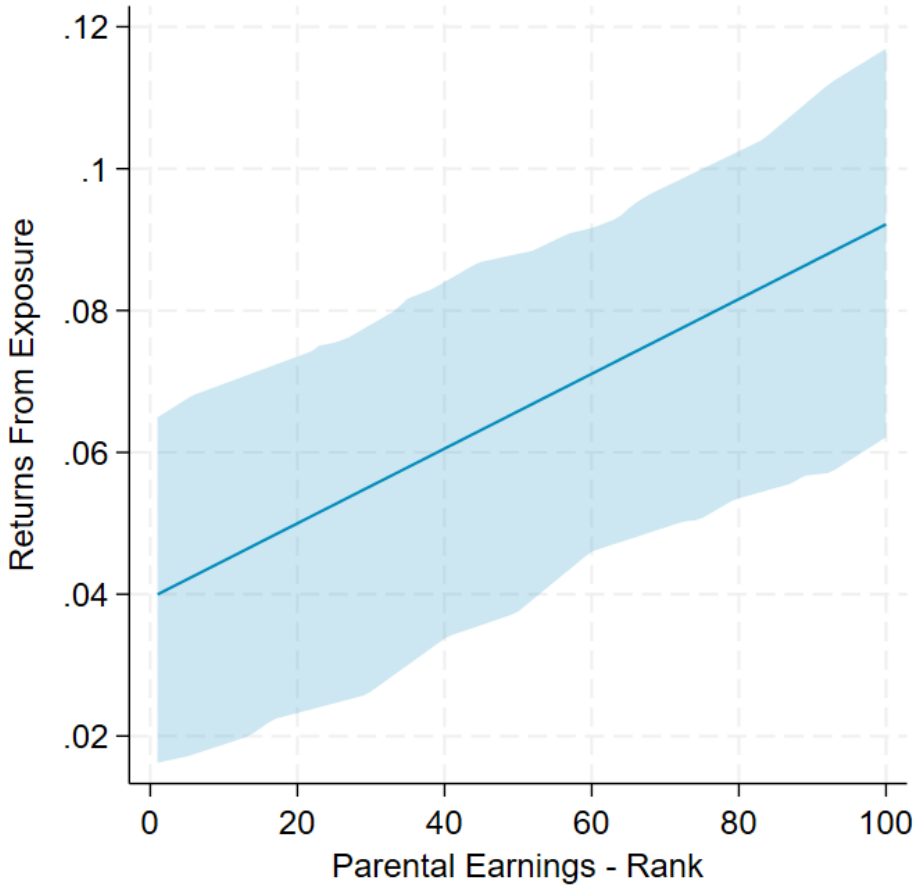
Note: The graph plots the marginal effects and the relative 90% confidence intervals from estimation of the model in eq. 1 where a quadratic polynomial for average schoolmates' parental earnings is included, evaluated at different levels of exposure to peers' parental earnings. The horizontal axis reports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

Figure 6: Sorting into Peer Groups by Parental Earnings



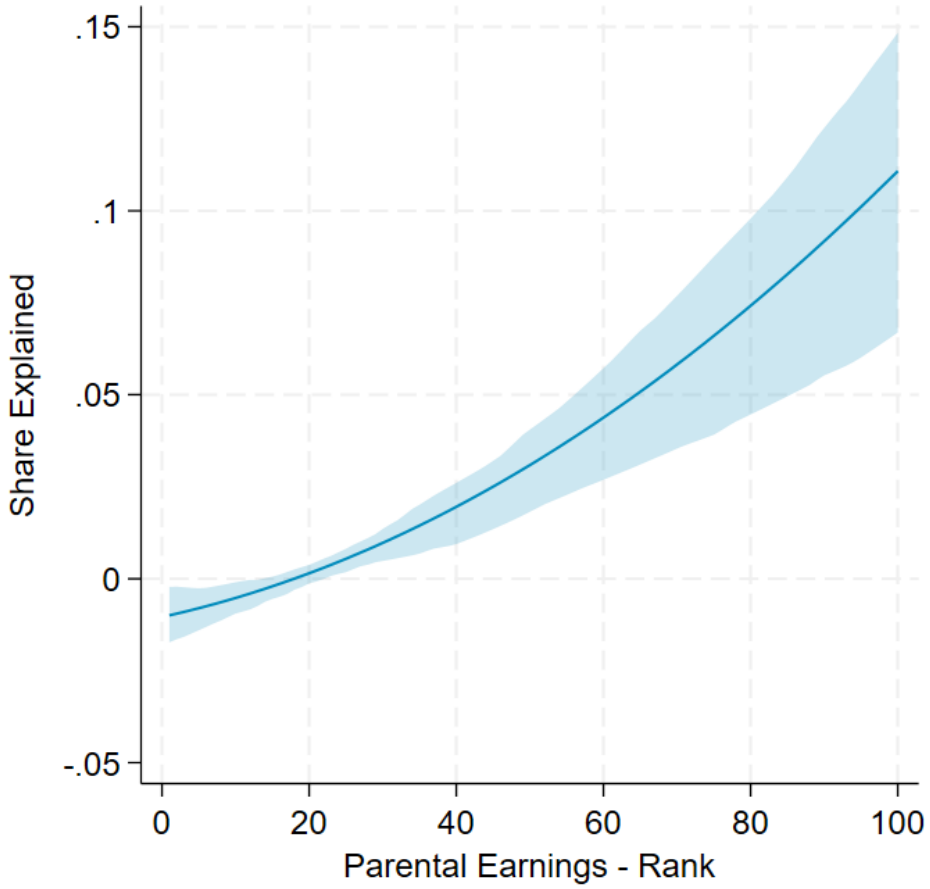
Note: The graph plots the estimated sorting profile $\hat{P}(x) = \hat{\rho}_0 + \hat{\rho}_1 x + \hat{\rho}_2 x^2$ from eq. 3, i.e. the expected leave-one-out average of schoolmates' parental earnings rank conditional on own parental earnings rank x. The shaded area reports confidence intervals. The dashed line marks the 50^{th} percentile, the expected peers' parental earnings rank in the absence of sorting.

Figure 7: Returns to Peer Exposure by Parental Earnings



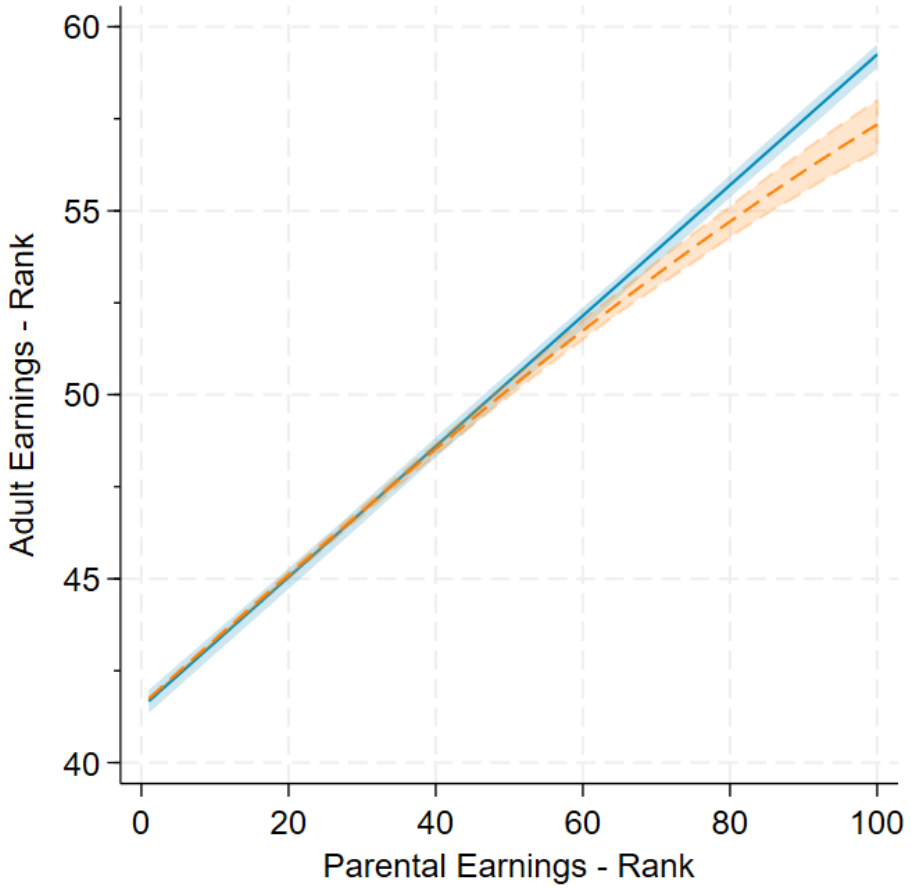
Note: The graph plots the estimated return to exposure $\hat{\beta}_2 + \hat{\beta}_3 x$ from eq. 4, i.e. the effect on the adult earnings rank of a one-percentile increase in schoolmates' average parental earnings, as a function of own parental earnings rank x. The shaded area reports confidence intervals.

Figure 8: Share of Intergenerational Persistence Explained by Peer Exposure



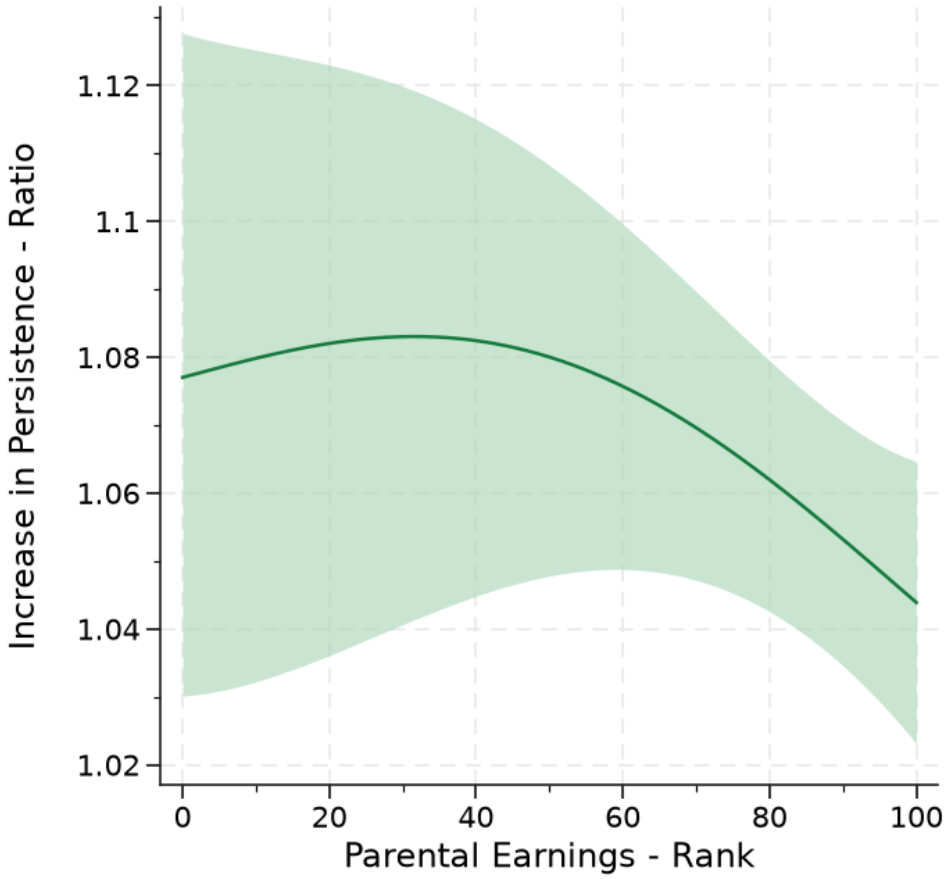
Note: The graph plots the estimated share $\hat{s}(x)$ of intergenerational persistence explained by sorting in peer exposure, as defined in eq. 6, as a function of own parental earnings rank x. The share combines the sorting profile in Figure 6, the returns to exposure in Figure 7 and the rank–rank slope from eq. 2. The shaded area reports confidence intervals.

Figure 9: Observed and Counterfactual Parent–Child Earnings Gradients



Note: The solid line plots the observed rank–rank relationship $\hat{\alpha}_0 + \hat{\alpha}_1 x$ from eq. 2. The dashed line plots the counterfactual relationship $\tilde{Y}(x) = \hat{\alpha}_0 + \hat{\alpha}_1(1 - \hat{s}(x))x$, which removes the share of persistence explained by sorting in peer exposure (Figure 8). Shaded areas report confidence intervals.

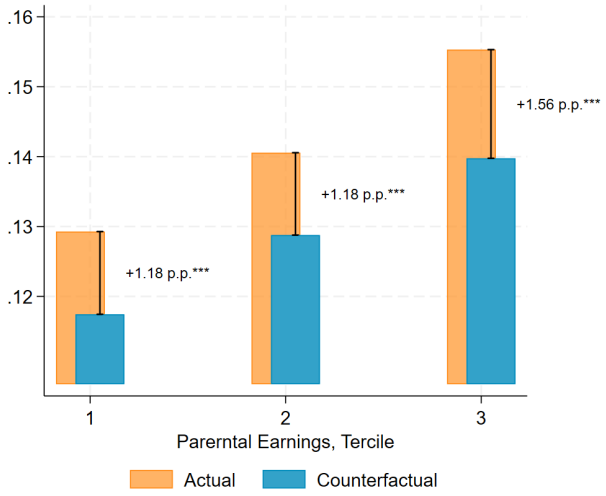
Figure 10: Change in Intergenerational Persistence under U.S.-Style Sorting



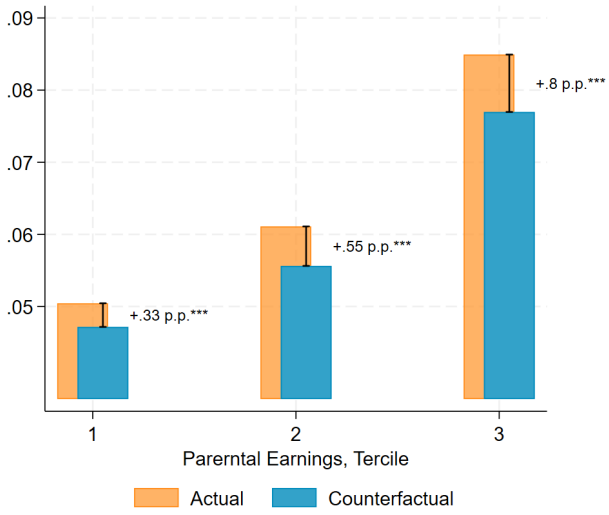
Note: The graph plots the ratio $\hat{R}(x)$ from eq. 7 between intergenerational persistence under U.S.-style sorting and observed persistence in Denmark, as a function of own parental earnings rank x. The U.S. sorting profile is the relationship between own parental-income rank and the average parental-income rank of other children in the same Census tract, $P_{US}(x) \approx 35.2 + 0.296\,x$, recovered from Opportunity Atlas tract-level data as described in Appendix B. Danish returns to exposure and the Danish rank–rank slope are held fixed. The shaded area reports confidence intervals.

Figure 11: Probability of Joining a Plant Where a Schoolmate is Employed

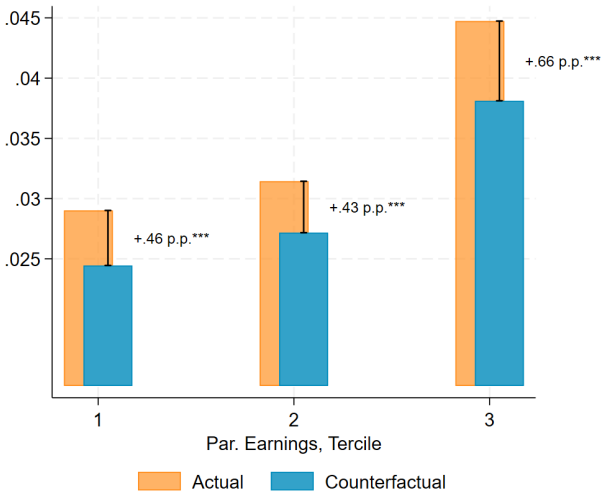
(a) Any Plant, Any Schoolmate



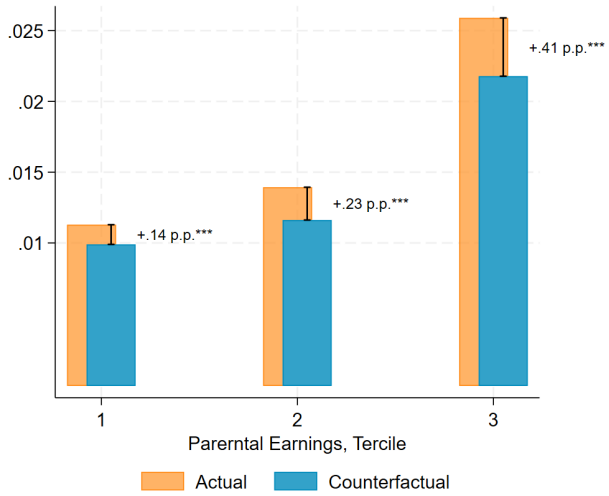
(b) Any Plant, High-SES Schoolmate



(c) High-Wage Plant, Any Schoolmate

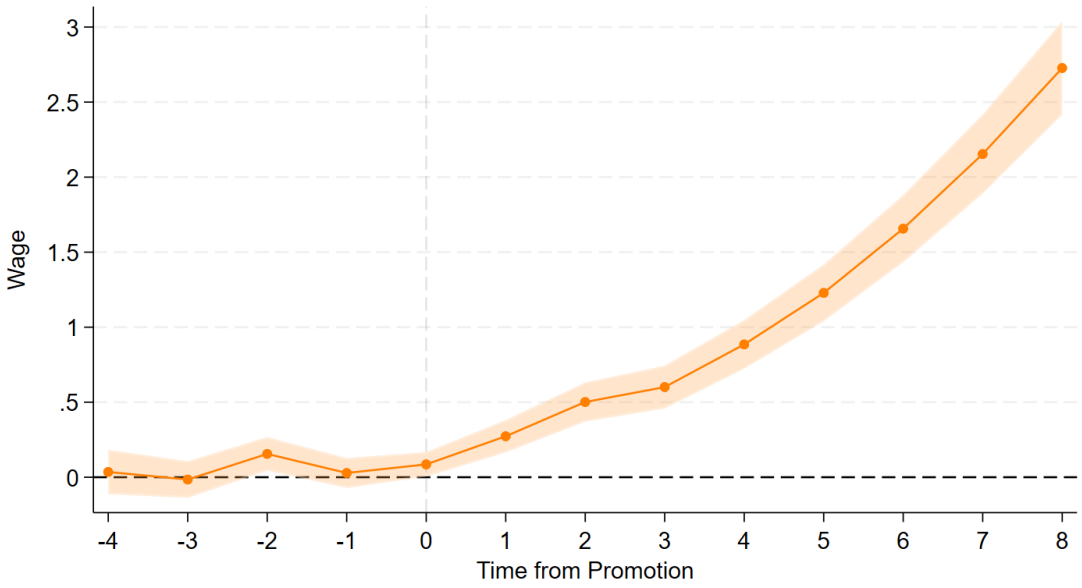


(d) High-Wage Plant, High-SES Schoolmate



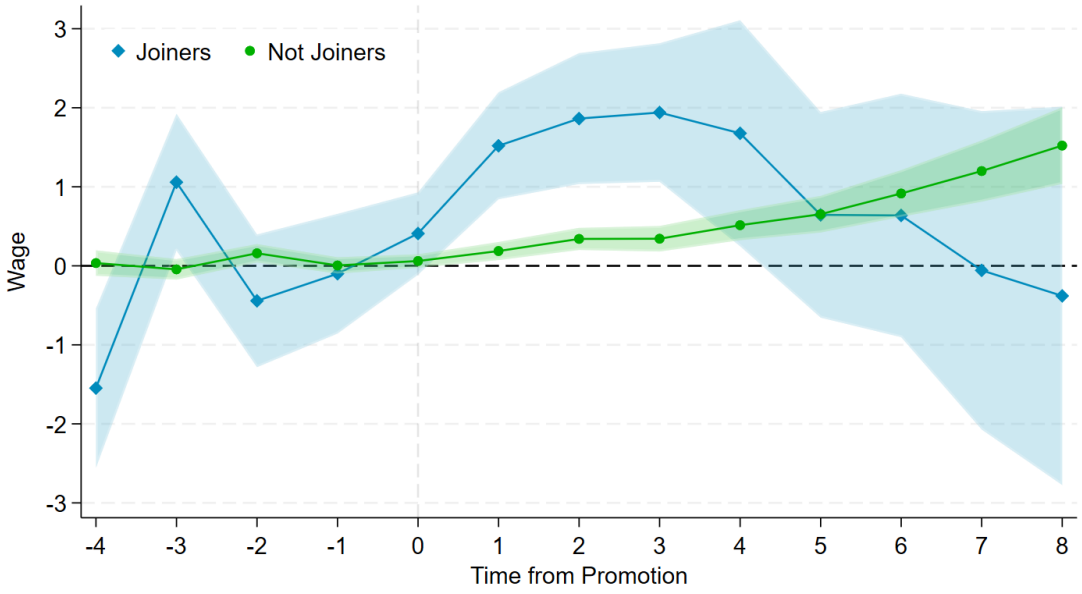
Note: The bar graphs display the probability of joining a plant where a schoolmate is employed, measured 4 to 14 years after high school enrollment. The probability is calculated as the share of individuals who join actual schoolmates' plants (orange bars, left) versus counterfactual schoolmates' plants (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 plant 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 within the top quartile of the national distribution. * p<0.10, ** p<0.05, *** p<0.01.

Figure 12: Effect of a Peer's Promotion on Wages



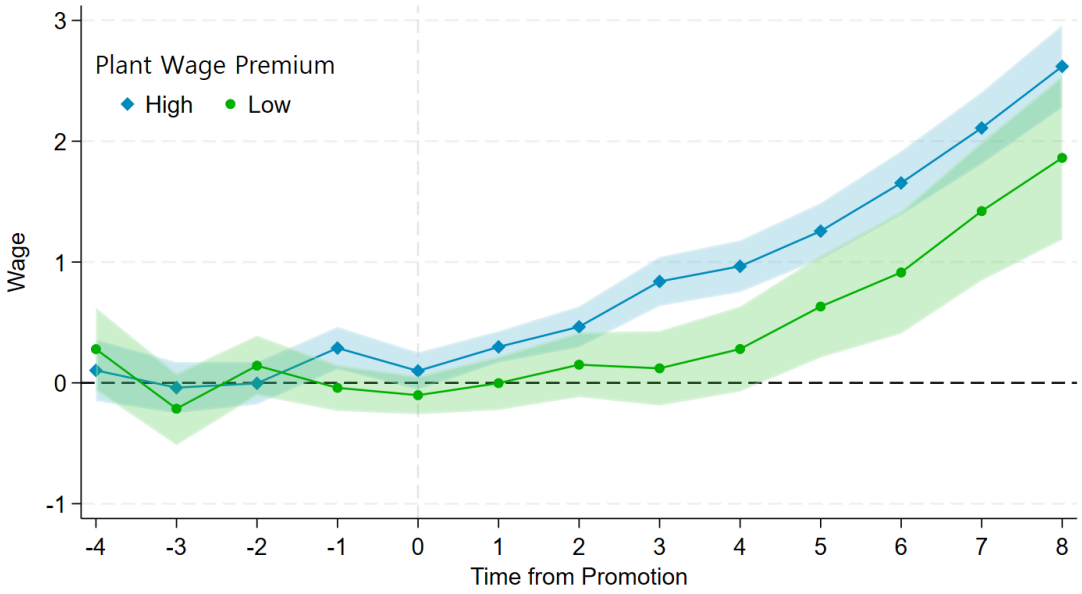
Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (8) as in Callaway and SantAnna (2021). Each group is composed of schoolmates who enrolled at the same high school in the same cohort, except 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 13: Effect of a Peer's Promotion on Wages: Joiners and Not Joiners



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 (8) as in Callaway and SantAnna (2021), estimated separately for joiners (blue diamonds) and not joiners (green circles). Each group is composed of schoolmates who enrolled at the same high school in the same cohort, except 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 of the promotion onwards; not joiners are the others.

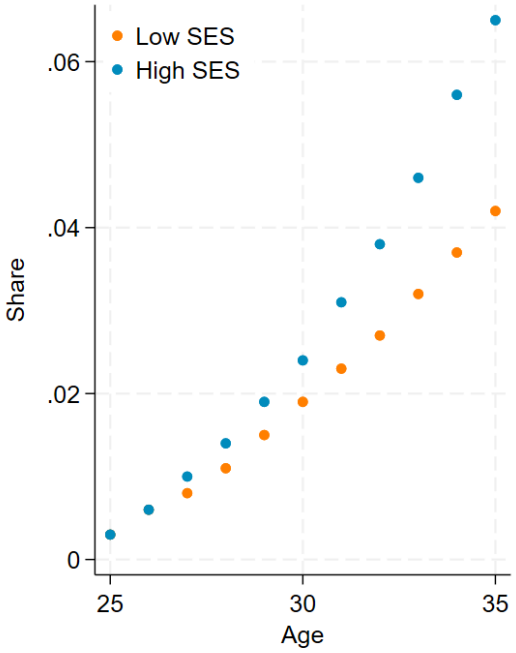
Figure 14: Effect of a Peer's Promotion on Wages: High- and Low-Wage Plants



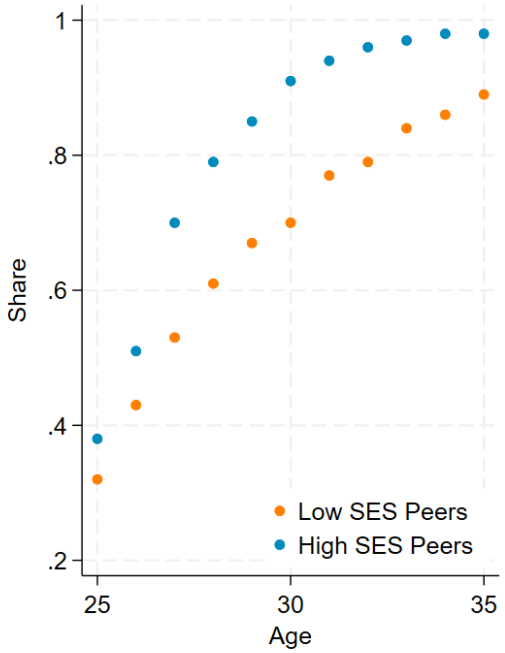
Note: The graph reports estimates of the effect of a schoolmate's promotion on own hourly wages for each period before and after promotion, obtained by estimating the model in equation (8) as in Callaway and SantAnna (2021), estimated separately for groups with high (blue diamonds) and low (green circles) outside options. Each group is composed of schoolmates who enrolled at the same high school in the same cohort, except 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.

Figure 15: Promotions to Managerial Positions

(a) Own Promotion, by Parental Background

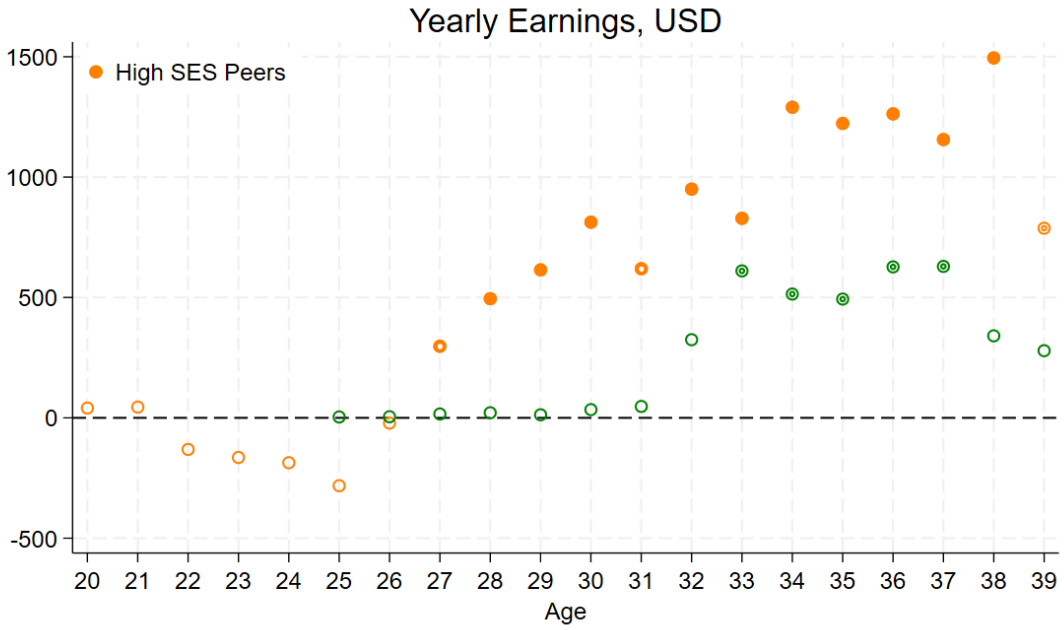


(b) Former Schoolmate Promoted, by Peer Exposure



Note: Panel (a) plots the share of students holding a managerial position at each age, separately for students from low- and high-SES families. Panel (b) plots the share of students with at least one former schoolmate in a managerial position by each age, separately for students exposed to low-SES peers (bottom quartile of the leave-one-out average of schoolmates' parental earnings) and to high-SES peers (top quartile). Both panels report raw shares, without controls.

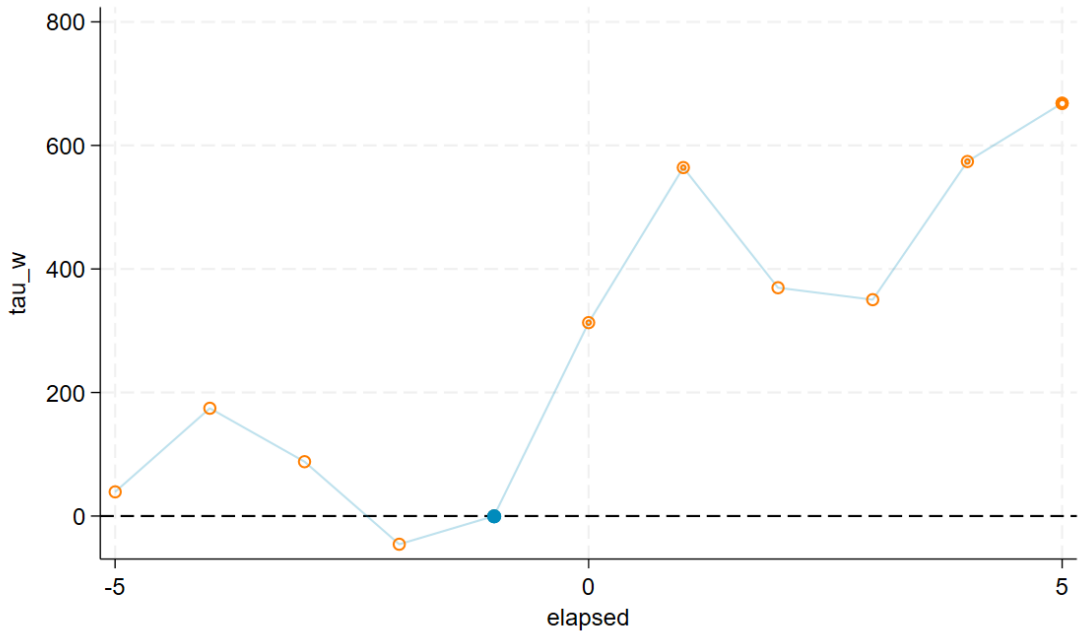
Figure 16: Total and Manager-Mediated Effects of High-SES Peer Exposure on Yearly Earnings



Note: The graph reports, at each age t between 20 and 39, the effect on yearly earnings (2015 USD) of exposure to high-SES peers (top quartile of the leave-one-out average of schoolmates' parental earnings) rather than low-SES peers (bottom quartile). Orange markers report the total effect $\beta_t(r, r')$, estimated with the within-school, across-cohort design of eq. 1. Green markers report the mediated effect $\theta_t(r, r')$, the component of the total effect that operates through the earlier timing of the first promotion of a former schoolmate to a managerial position, estimated as in eq. 11. See Section 7 and Appendix A for details.

Appendix Figures

Figure A1: Earnings Effect of a One-Year-Earlier Peer Promotion



Note: The graph reports event-study estimates of the effect on yearly earnings (2015 USD) of experiencing the first promotion of a former schoolmate to a managerial position at age q rather than at age $q+1$, obtained with the conditional difference-in-differences design described in Section 7.3 and Appendix A. Estimates are aggregated across promotion ages q and are reported by years elapsed since the earlier promotion. The year before the promotion (-1) is the reference period.