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The effect of ordinal rank in school on educational achievement and income in Sweden

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Abstract

This study examines the influence of students' ordinal positions in the distribution of grades in their ninth-grade school cohort on subsequent educational and labor market outcomes using population-wide data for Sweden. The identification strategy uses differences between students' ranks in their school and their ranks in the country-wide ability distribution after conditioning on school-cohort fixed effects and school-level grade distributions. The findings reveal an advantage of occupying a higher rank in school with respect to educational and labor market accomplishments in adulthood, whereas a lower rank yields adverse consequences. Contrary to findings from the United States, no effect is found for students situated in the middle of the rank distribution. This study also shows that ordinal rank effects are more pronounced for students with lower socio-economic status and for female students at the top of their school ability distribution. This study highlights the importance of students' rank positions in determining their future academic and professional outcomes.

Keywords: education, income, ordinal rank, peer effects.

JEL-codes: I20, I23, I28

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Introduction

Peer interactions in school play a key role in shaping students' educational and labor market outcomes. One important example of such peer effects is related to a student's ordinal rank among classmates. Research shows that ranking among the lowest-performing students negatively impacts short-term outcomes such as income at age 24-27 (Denning et al., 2023) , test scores, subject choice in upper-secondary school (Murphy & Weinhardt, 2020), mental health (Kiessling & Norris, 2023; Kim & Liu, 2023), conscientiousness (Pagani et al., 2021) , and fertility and family patterns (Andersson et al., 2025). Conversely, being among the top performers tends to yield positive effects, including higher income (Denning et al., 2023), a greater likelihood of choosing prestigious STEM degrees (Delaney & Devereux, 2021), and better health outcomes (Kiessling & Norris, 2023; Kim & Liu, 2023) . These effects are linked to mechanisms such as the Big Fish–Little Pond effect (Marsh, 1987) , and differential teacher and parent attention, encouragement, and resources based on students' relative rank (Eble & Hu, 2017; Lavy et al., 2012; Lim & Meer, 2020; Megalokonomou & Zhang, 2024) . These mechanisms shape students' self-concept and the teaching methods they are exposed to, often favoring high-ranking students and disadvantaged low-ranked students.[2]

Although the rank effect is well established in many contexts, less is known about its impact on students' long-term outcomes. This study addresses that gap by focusing on outcomes such as average labor market income between ages 35 and 38, a strong proxy for lifetime income (Böhlmark & Lindquist, 2006; Haider & Solon, 2006), as well as replicating key educational outcomes in the Swedish context: the likelihood of choosing an science track in upper-secondary school, completing upper-secondary education, and years of schooling by age 33. Drawing on full-

[2] This paper does not aim to identify mechanisms; see (Murphy & Weinhardt, 2023) for a detailed discussion and empirical tests. Prior research has suggested several channels through which relative rank may matter. For instance, being among the lowest-performing students in a school can reduce self-confidence, social status (Denning et al., 2023; Pagani et al., 2021; Megalokonomou & Zhang, 2024)) , motivation and effort and even mental health (Kiessling & Norris, 2023; Kim & Liu, 2023) . Such effects may not only lower short-term performance but also diminish future effort (Denning et al., 2023). Rank may also shape teacher behavior: lower-ranked students might receive either additional support or, conversely, less attention and encouragement if teachers perceive them as having lower potential (Eble & Hu, 2017; Lavy et al., 2012; Lim & Meer, 2020). By contrast, high-ranking students may benefit from higher expectations and enriched learning opportunities (Carneiro et al., 2023; Pop-Eleches & Urquiola, 2013).

population administrative data from eight cohorts of Swedish 9th graders, I estimate the effect of students' ordinal rank in 9^{th} grade on long-term labor market and educational outcomes.

To identify the causal effect of ordinal rank on long-term outcomes, this study follows the empirical approach of (Denning et al., 2023), which builds on (Murphy & Weinhardt, 2020). Rank is defined by a student's position in the GPA distribution at the end of grade 9 within their school and cohort. While students may have similar academic ability (proxied by national GPA percentile), their ordinal rank can differ depending on the grade distribution in their school, which varies across schools and years. This setup allows for comparisons between students with equal ability but different ranks due to school assignment. The empirical model includes school-by-cohort fixed effects and controls for national ability rank, as well as interactions between school grade distribution type and student ability to mitigate confounding from school-level sorting. Following Denning et al., 2023, ordinal rank is modeled both linearly and as a set of twenty ventile dummies, allowing for the estimation of non-linear effects across the rank distribution. The use of national population register data covering eight full student cohorts enables robust subgroup analysis by gender, immigration background, and parental SES. In addition, the study constructs rank separately by gender within schools, enabling analysis of same-gender peer comparisons, which recent work suggests may be especially salient (e.g., Goulas et al., 2024). Furthermore, measurement at the end of grade 9 ensures that students are aware of their position during a high-stakes period for educational choices, aligning with research suggesting that older students internalize rank more strongly (Elsner et al., 2021).

The study finds that a higher ordinal rank in school has an impact on long-term labor market outcomes. Interestingly, the effect is non-linear. Moving up a rank has a larger impact on future earnings in both the lower and upper tails of the distribution, whereas in the middle of the distribution, the effect of a marginal increase in the rank is zero. This non-linear pattern diverges from the findings of Denning et al. (2023), who report a uniformly positive linear relationship between rank and income in the U.S.

Ordinal rank also shows positive effects on several educational outcomes, particularly among high-ability students. It increases the number of completed years of education by age 33 and raises the likelihood of enrolling in a science track in upper-secondary school, although it does not significantly influence upper-secondary completion rates. The effects are especially pronounced among high-ability students from low-income backgrounds, suggesting that ordinal position may be particularly consequential for disadvantaged groups. Gender-disaggregated analyses reveal that girls benefit more from higher ordinal rank than boys, particularly when ranks are constructed within gender. Overall, the results suggest that ordinal rank effects are not uniform but rather concentrated in the tails of the ability distribution, and that especially girls predominantly compare themselves to other students with the same gender.

Several robustness checks were conducted to assess the reliability of the results. First, the analyses were replicated separately for small and large schools, confirming that the findings are not driven by school size. Second, subject-specific grades in Mathematics and Swedish were included as additional covariates to test whether performance in key subjects mediates the observed effects, I found that, while the effects are smaller the pattern remains the same. Third, I addressed the coarseness of the GPA measure, which generates ties in the rank distribution, by replicating the analysis using 20 GPA categories (following Denning et al., 2023) instead of the 50 used in the baseline specification. The results were consistently larger across outcomes, indicating that the baseline estimates are conservative, while the overall non-linear patterns remain unchanged. In all cases, the main results remained stable, supporting the robustness of the estimated effects.

To validate the identifying assumptions, both linear and non-linear balance tests were conducted. These tests assess whether ordinal rank is correlated with predetermined student characteristics. In the preferred specification, which uses rich interactions between student ability and school types, coefficients on background characteristics are consistently close to zero across the rank distribution. This suggests that the model successfully controls for observable confounders and reduces the risk of bias from unobserved factors. In addition, non-linear balance checks are reported as robustness tests.

This study contributes to literature on ordinal rank in several ways. First, by using population-wide administrative data from multiple linked sources, it is possible to examine a broader set of outcome variables than in previous research. In particular, students' income is measured between ages 35 and 38, a period that closely approximates lifetime earnings (Haider & Solon 2006; Böhlmark &

Lindquist 2006), whereas prior studies have typically focused on earnings at earlier ages, such as 24-27 (Denning et al. 2023). Second, the study focuses on Sweden, in contrast to much of the existing research that centers on the United States. Sweden's more compressed income distribution and later tracking in the education system make it a distinct context. As a result, the effects of ordinal rank in Sweden may deviate from the linear patterns observed elsewhere. Third, the study benefits from rich parental background data. Both parents' highest levels of education are drawn from education agency records, providing an objective measure of educational attainment. In addition, income data from tax records are available for the year in which the student turned 15, offering a detailed picture of family socioeconomic status. Furthermore, by constructing ranks separately within gender, this study shows that girls' outcomes are particularly sensitive to comparisons with other girls, while boys respond more strongly at the lower end of the male distribution.[3]

These findings highlight the long-term relevance of students' relative standing within their school cohort for both educational and labor market outcomes. The effects are particularly pronounced among girls and high-ability students from disadvantaged backgrounds. This has important implications for educational policy, as it suggests that ordinal rank, independent of absolute ability, can shape students' trajectories well into adulthood. Such results underscore the role of peer comparison dynamics in shaping self-concept, motivation, and opportunity, consistent with theories like the Big Fish–Little Pond effect.

A key comparison is with (Denning et al., 2023), who examine ordinal rank effects in the U.S. using third-grade standardized test scores and find a uniformly positive, linear relationship between rank and later earnings at ages 24–27. In contrast, this study measures rank at the end of grade 9, when Swedish students receive their first official registered grades, and tracks outcomes much later in life, earnings between ages 35 and 38, a stronger proxy for lifetime income. Methodologically, while Denning et al. relies on subject-specific ranks and ability ventiles, this study constructs ranks from overall GPA and uses the full distribution of grades (50 national ranks). Substantively, the Swedish setting differs in two important ways: first, compulsory

[3] Mouganie & Wang (2020) also construct gender-specific peer comparisons, focusing on how exposure to high-performing female peers in mathematics affects girls' STEM track choices in Chinese high schools.

schooling continued with a common curriculum until grade 9, and tracking into academic versus vocational upper-secondary pathways only occurred at age 16, later than in the U.S.; second, Swedish grading during this period followed a norm-referenced system, which may have made competition more pronounced at the tails of the distribution, since all schools were bound by the same relative grading framework. These institutional differences help explain why the effects found here are smaller and concentrated at the lower and upper tails of the ability distribution, rather than spread across the entire spectrum as in the U.S. case. By extending the analysis to a distinct educational system and to later-life outcomes, this study provides new evidence on how rank effects depend on institutional context and persist into mid-adulthood.

The rest of the paper is organized as follows. In the next section, I present a short description of the data. Section 3 describes the empirical strategy in more detail. In section 4, I examine tests of the internal validity of my identification strategy. The main results are presented in section 5. Results on heterogeneous effects are presented in section 6. Section 7 demonstrates the robustness of my findings, and section 8 concludes.

2. Data

This section provides a concise overview of compulsory schools in Sweden and presents the data and main variables used in this study. The analysis draws on comprehensive Swedish register data encompassing all students who completed compulsory school (grade 9) between 1990 and 1997, corresponding to the cohorts born from 1974 to 1981.

Descriptive statistics for the variables are presented in Table 1, while Appendix D contains detailed descriptive statistics and information about the data sources.

2.1 Compulsory school in Sweden

During the study period (1990–1997), the Swedish education system consisted of nine years of compulsory schooling, followed by optional upper secondary education (three years) and enrollment in a university or college. Compulsory schooling began at age seven and included elementary school (year 1–6) and secondary school (year 7–9). Nearly all students completed compulsory school (Halldén, 2008; Stanfors, 2000), and the vast majority continued to upper secondary education. In year 9, students could choose between vocational and academic upper secondary tracks, with admission based on geographical proximity and their final GPA. From 1992, students were able to choose schools outside their residential areas (Edmark et al., 2014), coinciding with the introduction of privately operated voucher-funded schools – a shift from the earlier system of residence-based enrollment. [4] Teachers assigned grades based on exam results and coursework throughout the year.

2.2 Rank, ability and school types

In the forthcoming analysis (Section 3), two key variables are introduced: students' ordinal rank within their school cohort and their ability rank in the nation-wide cohort. Both are based on final grade 9 GPAs, calculated across 16 subjects graded on a 1–5 scale. During the study period, grades followed a norm-based system designed to approximate a normal distribution centered on 3. While standardized exams informed grading in core subjects, no centralized enforcement ensured adherence to the distribution. Teachers assigned grades based on exams, attendance, and overall academic performance.

Student *i*'s rank within their school *s* and cohort *c* is calculated as $R_{isc} = \frac{n_{isc}-1}{N_{isc}-1}$, where $n_{isc}$is the student's GPA rank within the school and cohort, and, N_{ist} is the number of students in that school-cohort. This produces a rank value between 0 and 1.

To operationalize relative positions, I create 20 dummy variables based on students' ordinal rank within their school-cohort. Students are sorted by GPA and grouped into 20 equal-sized bins, each representing 5% of the distribution from lowest to highest GPA. This "rank measure" allows inclusion of all schools, including those with as few as 20 students.

To capture academic ability at the national level, I construct an "ability measure" by ranking students across the entire country-cohort using 9th-grade GPA (scaled from 1.0 to 5.0 with one decimal). This results in 50 ability ranks from lowest to highest GPA. Both rank and ability measures are based on the same GPA data, but while the ability distribution reflects a cohort-wide, approximately normal curve, the rank variable reflects within-school variation. In some models, I

[4] The school choice reform was implemented in 1992. Prior to the reform, students were restricted to enrolling in schools within their vicinity.

also include Mathematics and Swedish grades separately to more precisely capture subject-specific ability.

School types are defined based on each school-cohort's GPA distribution—specifically its mean and variance. Schools are then grouped into four or ten categories, ordered by GPA mean or variance. Descriptive statistics for each school type are provided in Appendix Table D.3.

2.3 Outcome variables

In this study, I examine four long-term outcomes, all measured during the students' 30s. The first outcome is years of education, defined as the total number of years based on the highest completed educational level. This variable likely correlates with ordinal rank, as high-ranked students may be more inclined to pursue higher education, while low-ranked peers may be less likely to do so. During the study period, nearly all students completed compulsory schooling, and the average number of years of completed education was 13.2. Approximately 25% obtained some form of post-secondary degree. The second outcome is the completion of upper secondary education, equivalent to at least 12 years of schooling. This outcome is especially relevant for students with low ordinal rank, where even small improvements in rank may reduce the dropout risk. In contrast, students with high rank are generally unlikely to drop out. In the sample, about 86% had completed upper secondary school by age 33. The third outcome captures the *educational track* selected in the first year of upper secondary school and is defined as attending a science or a vocational program. As shown in Table 1, 22% enrolled in a science track, and 37% in vocational training. Prior research suggests that higher ordinal rank is linked to selecting more academically demanding tracks (Denning et al., 2023; Facchinello, 2020; Murphy & Weinhardt, 2020), motivating separate analysis of this outcome. The final outcomes are based on the average income between ages 35 and 38.[5] The ordinal rank may influence income both indirectly, through education level and field of study, and directly, via beliefs, motivation, or expectations. Two measures are used: (1) the natural log of average income and (2) the income rank, constructed by dividing the cohort's income distribution into 100 percentiles.

[5] It is essential to consider an average over multiple years due to income volatility, as indicated in previous research.

2.4 Control variables

The specification includes individual, family, and demographic background controls to account for student characteristics and to examine whether rank effects differ by gender, parental education, and income. All variables are measured in grade 9, when rank is defined. Parental income is calculated as the sum of mothers' and fathers' earnings, expressed in logarithmic form, and households are grouped into four income quartiles. Parental education is measured analogously: parents' total years of schooling are summed up and divided into four ranked categories. Immigrant background is captured by an indicator for being born outside Sweden, together with a control for age at immigration, which has been shown to influence educational outcomes. Finally, birth year and month are included to account for relative age at school entry, as prior research documents systematic variation in academic performance by month of birth.

3 Empirical strategy

This paper estimates the causal effect of students' ordinal rank, measured by their position in the grade distribution within their 9th-grade school cohort, on medium- and long-term educational and labor market outcomes. I compare students with similar academic ability, but different ordinal ranks due to plausibly random variation in school-level grade distributions. Because students with equal ability are distributed across schools, their relative rank can differ despite identical performance. Figure 1 illustrates this point using the analysis data. It shows that a student with median national ability may be ranked anywhere from 7 to 13 depending on their school. This random variation in ordinal rank in the school, among students with identical ability rank in the national distribution, is what enables causal identification, provided that many schools and cohorts are observed.

Delaney & Devereux (2022) review methods used in the rank-effect literature and conclude that the design by Denning et al. (2023) is the most robust and conservative for register data. I adopt their approach here. The model includes school-cohort fixed effects to account for unobserved heterogeneity and adds school-type-by-ability interactions to capture systematic contextual effects.

Following Denning et al. 2023, I model outcome y of student i in school s and cohort c as follows:

$$y_{isc} = g(\boldsymbol{R_{isc}}) + A(a_{isc}) + S_{sc} + \gamma\, \boldsymbol{X_{is}} + \varepsilon_{isc} \quad (1)$$

In Equation 1, $g(.)$ is a function of ordinal rank, R_{isc}, $A(.)$ is a function of student ability, a_{isc}, $S_{sc}$is a school-by-cohort fixed effect, and $\boldsymbol{X_{is}}$ is a set of control variables that includes student background characteristics when finished grade nine. The ordinal rank function g(.) can take two forms: The first specification treats the GPA rank R_{isc} as a continuous variable and includes it linearly. The second uses the ordinal nature of rank by creating twenty indicator (dummy) variables, each covering a 5-percentile bin (ranks 1–20) within each school–cohort distribution. Rank 10 is the omitted reference group.

To obtain an unbiased estimate of the effect of ordinal rank, it is crucial to control student ability. The reason is that rank and ability are highly correlated, and ability likely has a direct effect on outcomes. As described in Section 2, I proxy student i's ability (a_{isc}) using their national GPA rank within their cohort. This "national rank" is based on the same 9th-grade GPA used for school rank and is constructed by sorting all students in a cohort from lowest to highest GPA and dividing them into 50 equal-sized groups, each representing 2% of the distribution. [6]

A potential concern is that ordinal rank may correlate with the distribution of grades within a school. For example, students with identical GPA can have different ordinal ranks depending on the variance of their school's grade distribution. As noted by Denning et al. (2023) and Booij et al., 2017 , this correlation risks confounding the causal effect of ordinal rank with school-level factors.[7] To address this, I include school-type-by-ability interactions in the specification. The full details of the classification strategies, together with an illustrative example, are provided in Appendix A.

In equation 1, S_{sc} denotes school-by-cohort fixed effects, which account for all unobserved time-invariant and time-varying characteristics of each school cohort. Following Denning et al. (2023),

[6] I chose 50 ranks because 50 ranks cover all possible GPA positions, since ninth grade GPA ranges from 1.0 to 5.0 in the data and is only reported with one decimal place.

[7] This bias is discussed at length in Booij et al. (2017) and Denning et al. (2023).

the rank and school effects are assumed to enter additively. Additional individual-level controls include gender, immigration status, parental education (four levels), and parental income (four levels), as described in the data section. These components together form the basis for the main estimation equation:

$$y_{isc} = \sum_{r=1,r\neq 10}^{20} \beta_r\, I_r \; + \sum_{d=1}^{D} \sum_{a=1}^{50} \beta_{ad}\, I_{isc} + S_{sc} + \gamma\, \boldsymbol{X_{is}} + \varepsilon_{isc} \quad (2)$$

3.1 Identifying variation

As discussed in the previous section, various sets of dummy variables can be included in the estimation equation to control for student ability and school grade distribution types. A key question is whether sufficient identifying variation remains after including these controls. The identification strategy relies on conditionally random variation in grade distributions across schools. If too many controls are added, such that the residual variation in school grade distributions is minimized, then students with the same ability will have identical ordinal ranks, and the effect of rank cannot be estimated. Thus, the challenge is to define school types that render grade distributions "sufficiently" similar across schools, without eliminating the variation needed for identification.

This problem involves a trade-off: omitting key controls risks bias due to confounding school characteristics, while over-controlling may attenuate the rank effect by removing meaningful variation. To address this, Section 4 presents a series of balancing tests to assess how different model specifications affect the relationship between student characteristics and ordinal rank.

To illustrate the remaining identifying variation, Figure A1 shows the relationship between student ability (national rank) and ordinal rank across four school types: low mean/low variance, low mean/high variance, high mean/low variance, and high mean/high variance. In all four types, ordinal rank varies at each level of student ability, indicating that identifying variation remains even after accounting for school type and ability. Finally, I examine how the residuals from Equation 3 vary across the ability distribution to assess the extent of unexplained variation in rank.

$$R_{isc} = \sum_{d=1}^{16} \sum_{a=1}^{50} \beta_{ad}\, I_{isc} + S_{sc} + \gamma\, \boldsymbol{X_{is}} + \varepsilon_{isc} \quad (3)$$

The standard deviation of ε_{isc} at each point in the ability distribution reflects the variation in ordinal rank after controlling other factors. Figure 2 shows the standard deviation of ordinal rank and its residuals plotted against the ability distribution. The standard deviation of ordinal rank follows an inverted U-shape, with higher average values. Using the residuals from regression 4, the variation remains stable across the ability spectrum, averaging 0.7 standard deviations. This variation, which corresponds to an average 3.5% change in class position, is used in the main model, indicating that a student's ordinal rank can shift by 0.7 rank positions at any given ability level.

3.2 Measuring rank in grade 9

Ordinal rank is determined based on the student's final GPA in compulsory school, which is measured during the spring of grade 9, when students are typically 16 years old. Previous studies have often utilized rank measurements taken at younger ages. However, employing rank measured at this older age has both advantages and disadvantages. Older students have a better understanding of their relative position among their peers within the school and comprehend the significance of their rank, especially as they are about to compete for slots in upper secondary tracks. This observation aligns with the findings of (Elsner et al., 2021) , who suggest that older students place greater importance on their rank compared to younger students.[8] However, ordinal rank measured in year 9 may already reflect the cumulative effects of earlier rankings. As a result, the grading year 9 could underestimate the true impact of the rank or its compounded effects over time. Consequently, the results presented here are likely lower bounds of the true rank effects.

3.3 Teacher grading

As discussed in the data section, using teacher-graded GPA as a measure of student ability has both advantages and disadvantages. While it captures multiple dimensions of human capital, it also

[8] It is noteworthy that this relevance is more pronounced in the Swedish context, as students receive their first grades in grade 8.

introduces potential measurement errors. This section addresses two concerns regarding teacher grading.

First, grading generosity may vary between teachers. If generosity is consistent within a school and cohort, the school-by-cohort fixed effects in the regression model address this issue. However, if generosity varies between classrooms within the same cohort, this could bias the estimate of the ordinal rank effect. Since students in year 9 typically are taught by multiple teachers in different subjects, this concern is mitigated. A robustness check using data from small schools, where the same teachers likely grade all students, further addresses this issue.

Second, the school's ability distribution could influence grading. While the grading system was designed to align with the nationwide GPA distribution, teachers may still be influenced by the local distribution. For example, students at the bottom of the distribution in a school may receive lower grades, while top performers may receive slightly higher grades, even if their abilities are identical to those of students in other schools. If anything, this may likely result in a downward bias in the ordinal rank effect, as lower-ranked (higher-ranked) students may appear to have lower (higher) GPAs than they would if their grades were truly reflecting their position in their cohort nation-wide. However, the fact that the analysis compares students with equal GPAs but different school-level ranks, helps mitigate these biases. To account for this, the empirical model includes controls for school-level ability distribution.

3.4 Limitations:

A limitation of the study is that rank is measured at the school rather than classroom level. If students primarily compare themselves with their classmates, the estimated effects may understate the true impact of relative rank. Even though students during this period were able to choose between standard and advanced level English and Mathematics, students typically remained in the same class from year 7–9 without ability-based tracking. In addition, subject teachers often taught multiple classes within a school under the nationally norm-referenced grading system. These features help moderate but do not eliminate the concern that classroom-level rank may be more salient than school-level rank. For the 1990–1997 cohorts, no standardized tests or official grades were available before year 8, as earlier grading was only reintroduced in 2012/13. Grades in year 8 therefore represent students' first formal academic assessments, which simultaneously reduces concerns about endogenous sorting but restricts the ability to control for pre-treatment.

4 Balance test

Before estimating the regression as specified in equation 2, this section conducts an informal test to assess whether the controls in the regression equation adequately address potential omitted variable bias. This is done by testing whether various pre-determined student characteristics are uncorrelated with ordinal rank after adding different sets of controls to the model. If these characteristics remain uncorrelated, it can be reasonably assumed that unobserved student characteristics are also uncorrelated with ordinal rank, conditional on the included controls. This would suggest that the model is likely to yield the causal effect of ordinal rank. The test is conducted in two formats: a linear balance check and a non-linear check.

4.1 Linear balance test

In conducting the linear balance test, I estimate Equation 2, substituting the outcome variable with various student characteristics. Following Denning et al. (2023), I use five alternative specifications. The rationale is that if background characteristics are not correlated with ordinal rank, then unobserved characteristics are likely unrelated as well. This helps identify the specification that provides estimates of ordinal rank's impact that are plausibly unaffected by bias from unobserved variables.

Table 2 presents the ordinal rank coefficient estimates for several dummy variables, including gender, parental education (high/low), parental income (high/low), and immigrant status. Panel A shows estimates when only ability is included as a control without interaction with school types. Panel B incorporates quartiles of school mean GPA interacted with ability (resulting in four school types × 50 ability types). Panel C uses deciles of school mean GPA (500 dummies), Panel D uses quartiles of school GPA variance (200 dummies), and Panel E uses deciles of school GPA variance (500 dummies). The final panel combines quartiles of both GPA variance and mean (800 dummies), as used by Denning et al. (2023). In the last two specifications, no coefficients are statistically significant, indicating that student background characteristics are uncorrelated with linear rank.

4.2 Nonlinear balance check

As a robustness check, I assess the potential non-linear relationship between ordinal rank and predetermined characteristics, I re-estimated model 3, substituting the outcome variables with student background traits. As in Table 2, statistically insignificant coefficients suggest that, conditional on included controls, ordinal rank is uncorrelated with observed characteristics, allowing for causal interpretation. Figure A2 in the appendix displays the estimates from three model specifications: (1) no controls, (2) controls for ability, and (3) controls for ability and 16 school types. The latter two correspond to specifications (a) and (f) in Table 2. The x-axis in each panel represents the student's ordinal rank (in 5% increments), while the y-axis shows the coefficient estimates. Note that the scale varies across figures, as the dependent variables differ in scale.

The results demonstrate that when no controls are included, there is clear correlation between ordinal rank and student characteristics. Once ability is accounted for, most estimates approach zero. In the final specification, which includes interactions between ability and 16 school types (800 dummies), the coefficients are near zero across almost all ranks. This indicates that these controls effectively account for observable background characteristics. It is therefore likely that unobserved traits are also largely accounted for, justifying the use of this specification in the main analysis.

5 Results

This section presents estimates of the effects of ordinal rank on future educational and labor-market outcomes. Two types of estimators are considered, linear and non-linear. For the non-linear effect, where separate coefficients are estimated for each ventile of the school by cohort rank distribution, the results are presented visually because of the large number of coefficients.

5.1 Linear effect of ordinal rank

The estimates for ordinal rank are presented in Table 3. Each row shows how the outcome in question changes when a student's rank increases by one position within the school. Note that a one-rank increase corresponds to a 5% shift in the student's position, as the ordinal rank variable is defined using ventiles of the school-by-cohort distribution. The effect size is calculated by dividing the estimates by the average of the respective outcome variable. Columns (a) to (f) represent specifications (a) to (f) outlined in Section 4, where each specification uses different numbers of dummies to capture ability and school types. Specification (f) is the preferred model.

For most of the estimates in Table 3, the effect size is small, though ordinal rank significantly influences the outcomes. For instance, in specification (f), the estimate for income rank is 0.16. This means that a 5% increase in a student's school rank results in a 0.16 increase in their income rank (out of 100). This effect size amounts to less than half a percent. Similarly small effect sizes are observed for other outcomes. In terms of specifications, the estimates are generally smaller when controlling for school variances (columns d and e) compared to when controlling for school means (columns b and c) or only ability (column a). Our preferred specification is (f), which accounts for both school variances and means interacted with student ability.

As discussed in the empirical strategy section, the small effects of ordinal rank in Table 3 may however be due to the rank effect of not being homogeneous across all positions in the rank distribution. If the effect is zero for certain ranks, the overall estimate will be diluted. To address this, we estimate the impact of ordinal rank in a non-linear format.

5.2 Non-linear effect of ordinal rank

Figure 3 presents the main results on the non-linear effects of ordinal rank on several educational and labor market outcomes. Each dot represents the estimated coefficient for one of 20 rank-based ventiles, with 95% confidence intervals. The reference category is the 10th ventile (roughly the middle of the within-school distribution), such that each coefficient reflects the effect of being in a given ventile relative to the 10th. Note that outcome scales differ across panels.

Panels (a) and (b) report effects on upper secondary school track choice. Panel (a) shows that ordinal rank positively affects the probability of entering the science track, but only among students near the top of the distribution. Moving from rank 15 (75th percentile) to the top rank increases the likelihood of science track enrollment by nearly 6 percentage points relative to the middle rank. This is consistent with the timing of the field choice decision, which occurs at the end of grade 9. In contrast, the likelihood of enrolling in a vocational track follows an inverted U-shape: increasing with rank among low-ranking students (below the 25th percentile) and declining at higher ranks (above the 75th percentile).

Panel (c) examines years of schooling by age 33. Ordinal rank has a positive and significant effect for students at the lower (below 15th percentile) and upper (above 80th percentile) tails of the distribution. For students in the middle, the rank coefficients are small and statistically indistinguishable from zero. This suggests that the rank effects on educational attainment are concentrated among students with either very low or very high relative standing. Panel (d) shows a strong rank effect on the probability of completing upper secondary education, particularly for students with very low ability. For example, increasing rank from 1 to 2 is associated with a roughly 25 percentage point increase in completion probability. For ranks 3 through 11, the effects are negligible, and above rank 12, the marginal impact is minimal. Panels (e) and (f) display effects on average income between ages 35 and 38, using log income and income rank, respectively. Both outcomes show a steep rank gradient at the bottom of the distribution, with diminishing returns at higher ranks. For instance, moving from rank 1 to 2 increases income rank by approximately one percentile, while moving from rank 19 to 20 increases it by only 0.5. [9]

To the best of my knowledge, the only study examining the non-linear effect of ordinal rank on income is Denning et al. (2023), who analyze income at age 24-27. In their study, top-ranked students (ranks 19–20) earned log-incomes roughly 0.08 higher than median-ranked peers, while the lowest-ranked students (rank 1) saw gains of 0.12 relative to rank 10. In this study, the corresponding effects are slightly smaller, approximately 0.06 and 0.10, respectively. For students in the middle of the distribution, Denning et al. find a small (0.01) positive effect, whereas this study finds no statistically significant differences.

To address concerns about attrition and measurement of labor market outcomes, Appendix C presents results for alternative earnings measures (log income, income rank, and absolute income including zero earners) at ages 25–29, 30–34, and 35–38. The results confirm that the non-linear rank effects are robust across measures and age groups

[9] Part of the effect may be mechanical: moving up a rank at the top of the income distribution typically requires a much larger absolute income increase than moving up a rank near the bottom.

6 Heterogeneity effect

This section examines whether the effects of ordinal rank vary by student background. The heterogeneity analysis focuses on the non-linear specification and includes four dimensions: gender, immigration status, parental income, and parental education. Results are shown in Figures A.3–A.7.

Figure A.3 shows that female students respond slightly more than males to improvements in ordinal rank, particularly with respect to income and years of education. For other outcomes, gender differences are modest. To explore whether students compare themselves primarily to same-gender peers, I re-estimate equation 2 separately by gender, using within-gender rank as the explanatory variable. That is, a girl's rank is measured relative to other girls, and a boy's rank relative to other boys. Figure A.4 displays these estimates: the left panel shows outcomes by girls' rank among girls, and the right panel shows boys' rank among boys. For girls, within-gender ranks strongly predict science track enrollment, years of education, and income rank at the top of the distribution. The patterns are like those based on overall ordinal rank, though the effect on vocational track enrollment is smaller and statistically insignificant. Among boys, within-gender rank matters more at the lower end of the distribution. Lower-ranked boys are significantly less likely to complete upper secondary education, more likely to avoid the vocational track, and have lower income. For instance, being in the bottom 5% of the male cohort reduces the probability of completing upper secondary school by 8 percentage points, compared to a 3-percentage point reduction for girls.

Figure A.5 reports rank effects by immigration status. Among immigrants, rank effects are strongest at the top of the distribution, especially for income. Among native-born students, rank has a stronger influence in the lower part of the distribution. For vocational track enrollment, ordinal rank has no significant effect among immigrants, while for native students the effect follows an inverted U-shape.

Figures A.6 and A.7 examine heterogeneity by parental income and education. The most notable differences appear in income and upper secondary school completion. Students from low-income families benefit more from high ordinal rank. For example, increasing school rank from the 90th to the 100th percentile (rank 18 to 20) is associated with a rise in income rank from roughly 2 to

5, a shift of three ranks, equivalent to about 6 percent of the rank distribution. Similarly, low-income students ranked in the top 15 percent of their school have a substantially higher probability of completing upper secondary education.

7 Robustness check

In this section, I conduct a set of robustness checks to address potential concerns raised in the empirical strategy section and further validate the estimators.

7.1 School size

The ideal setting for identifying ordinal rank effects is at the classroom level, where students are more likely to interact and compare themselves directly with peers. Since classroom-level data are unavailable, I examine heterogeneity by school size as a proxy. [10] This serves two purposes. First, smaller schools approximate classroom-level environments, where students are likely to share teachers and have more direct peer comparisons. In most small schools, students are taught by the same teacher across subjects and grades, reducing within-school heterogeneity in grading practices. Second, analyzing large schools helps address potential measurement error in ability. Larger schools contain more students per rank, which improves precision by averaging across students with similar underlying ability. This reduces attenuation bias from noisy GPA-based rank measures. Figure A.8 shows the estimated rank effects for small and large schools, respectively. Across nearly all outcomes, estimates are larger in large schools, particularly for students in the top quartile distribution of the rank (above the 75th percentile). These findings align with those reported by Denning et al. (2023) and suggest that ordinal rank effects may be even more pronounced when ability is more precisely measured.

7.2 Ability measurement

To account for students' underlying academic ability, I use their national GPA rank as the primary control. As a robustness check, I also include subject-specific grades in Swedish, English, and

[10] Large school is defined as having enrollment (number of students) above the year-specific mean across schools.

Mathematics—core subjects that are highly predictive of educational performance. Each grade is coded as a set of categorical indicators (1–5), with English and Mathematics distinguished by course level; separate dummies are included for each level to capture grading variation across tracks.

Figure A.9 presents the results from these alternative specifications. Incorporating subject grades slightly attenuates the estimated effects of ordinal rank, particularly for students in the top of the school distribution. This suggests that part of the rank effect may be capturing subject-specific ability, but the core findings remain robust. In sum, the inclusion of detailed subject grades has a modest mitigating impact on the estimated relationship between school rank and later outcomes.

7.3 GPA ties

A further robustness check concerns the coarseness of the GPA measure, which generates ties in the rank distribution. The baseline specification uses 50 GPA categories. To assess robustness, I replicate the analysis using 20 GPA categories, following Denning et al. (2023). Figure A.10 presents results for key outcomes under both specifications. The estimates based on 20 categories are consistently larger across outcomes, indicating that the baseline specification is conservative. Importantly, the overall patterns of non-linear rank effects are preserved, confirming that GPA ties do not bias the results in a way that undermines the main conclusions.

8 Conclusion

This article examines the causal impact of a student's ordinal rank within their school cohort in grade nine on subsequent educational and labor market outcomes. Building on the methodology introduced by Denning et al. (2023), this study analyzes long-term labor market outcomes, such as earnings at age 35-38, and focuses on a new context – Sweden.

The results indicate that while higher ordinal rank is generally associated with improved long-term outcomes, the effects are not linear across the ability distribution; rather, they are concentrated at the lower and upper tails. This finding contrasts with earlier studies suggesting more uniform rank effects across the distribution (see Delaney & Devereux, 2022 for a review). In the Swedish context, rank effects are concentrated at the extremes of the within-school ability distribution. Small changes in ordinal rank can have substantial consequences for students' educational trajectories and earnings, particularly among students at the bottom or top of their school's performance hierarchy. For example, moving from rank 15 to 20 (the top 25%) increases the likelihood of enrolling in the science track in upper secondary school by nearly 6 percentage points. Similarly, rank effects on years of schooling by age 33 are evident only for students below the 15th percentile or above the 80th percentile. Rank also affects completion of upper secondary school, with the strongest impact observed among the lowest-ranked students: moving up just one rank increases the probability of completion by nearly 3 percentage points.

The effects of ordinal rank are heterogeneous across student subgroups. Rank effects on income (ages 35–38) are strongest among high-ability students from low-income and immigrant backgrounds. In contrast, for students from higher-income families and native-born backgrounds, the impact of rank is more pronounced in the lower portion of the ability distribution. Gender differences also emerge: high-ranking girls benefit more from their ordinal position than boys, particularly in academic outcomes. The analysis further shows that these gendered effects are largely driven by within-gender comparisons. Girls' outcomes respond more strongly to their rank among other girls, and the same pattern holds for boys. For instance, a boy ranked in the lowest 5% of his school cohort is nearly 8 percentage points less likely to complete upper secondary school than a peer just one rank higher; for girls, the comparable figure is 3 percentage points.

A limitation of this study is that ordinal rank is measured at the school level rather than within classrooms, which may be the more salient reference group for students. In addition, Swedish students first receive official grades only at the end of grade 8, so the effects estimated here may capture both the impact of rank itself and the cumulative influence of earlier, informal comparisons. These factors suggest that the results likely represent a conservative estimate of the true magnitude of rank effects.

Compared to existing U.S. evidence (e.g., Denning et al. 2023), the estimated effects in this study are smaller and concentrated in different parts of the distribution. Several factors may account for this divergence. First, the Swedish setting evaluates rank effects at grade nine - when students receive their first official registered grades - whereas the U.S. study assesses rank in grade three. Second, grading in Sweden is based on teacher-assigned GPA, while the U.S. evidence uses standardized test scores. These institutional differences imply that the current estimates likely represent a lower bound of the true impact of ordinal rank.

From a policy perspective, the implications of ordinal rank effects are not straightforward, in part due to limited understanding of the underlying mechanisms. Possible channels include differential teacher attention, peer comparisons, self-confidence, and academic self-concept: mechanisms well-documented in the psychological literature (e.g., Marsh, 1987). A key question for future research is whether the benefits of higher ordinal rank can be leveraged for students from disadvantaged backgrounds without imposing costs on others. At minimum, educators and policymakers should recognize the risks associated with low ordinal ranks and consider interventions to mitigate their long-term consequences.

During the preparation of this work, the author used *ChatGPT 5* to improve the language and readability of the text. After using this tool, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

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Tables and Figures

Tables:

Table (1) Descriptive statistics

	Mean	Standard Deviation	Observations
Female share	48.9	49.99	794076
Father income log	8.9	2.18	754112
Mother income log	8.6	2.14	727636
Father years of education	11.2	2.86	726405
Mother years of education	11.3	2.54	757699
Born Outside of Sweden	7.5	26.33	794076
Parent born Outside of Sweden	13.0	33.59	794076
GPA	3.2	0.70	794076
English (advanced)	3.0	0.90	211072
English	3.3	0.87	536233
Mathematics (advanced)	3.0	0.95	309044
Mathematics	3.3	0.90	445494
Swedish	3.2	0.90	770575
STEM	22.4	41.67	276062
Vocational Track	37.3	48.35	276062
Income log (age 35-38)	7.6	1.00	684328
Years of Schooling	13.1	2.17	756340
Finished USS	86.1	34.60	794076
Number of students in each school	121.8	40.30	794076
Number of students in each rank	6.1	2.01	794076
Observations	794076		

Note: This table contains descriptive statistics of the variable used in the study. It covers all students in compulsory school between 1990 and 1997

Table (2) Balance Test

	Male	Low parent education	High parent education	Low income	High income	Immigrant
A. Un-interacted						
Rank	-0.35***	-0.32***	0.44***	-0.26***	0.33***	-0.060***
	(0.0031)	(0.0029)	(0.0043)	(0.0028)	(0.0037)	(0.0028)
B. School Mean Quartiles						
Rank	-0.056***	-0.0064	0.019	0.0068	0.012	0.0028
	(0.016)	(0.013)	(0.014)	(0.015)	(0.013)	(0.0053)
C. School Mean Deciles						
Rank	-0.054***	-0.014	0.032**	0.0042	0.019	0.0035
	(0.016)	(0.013)	(0.014)	(0.015)	(0.014)	(0.0055)
D. School Variance Quartiles						
Rank	-0.026*	0.020	-0.018	0.010	-0.0098	0.000071
	(0.015)	(0.012)	(0.014)	(0.014)	(0.013)	(0.0047)
E. School Variance Deciles						
Rank	-0.018	0.016	-0.016	0.0088	-0.0061	-0.00092
	(0.015)	(0.012)	(0.013)	(0.014)	(0.013)	(0.0047)
F. School Mean quartiles and variance quartiles						
Rank	-0.00081	-0.012	0.010	0.0076	0.020	-0.00024
	(0.016)	(0.013)	(0.013)	(0.015)	(0.013)	(0.0052)

*Notes: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. This table shows balance tests from regressions of student characteristics on rank and 50 ability-category indicators under various specifications (cf. Denning et al., 2020, Table 2). Panel A: ability not interacted with school type. Panel B: ability interacted with quartiles of school mean GPA. Panel C: ability interacted with deciles of school mean GPA. Panels D and E: ability interacted with quartiles and deciles of school variance, respectively. Panel F: ability interacted with both school mean and variance quartiles (16 dummies).*

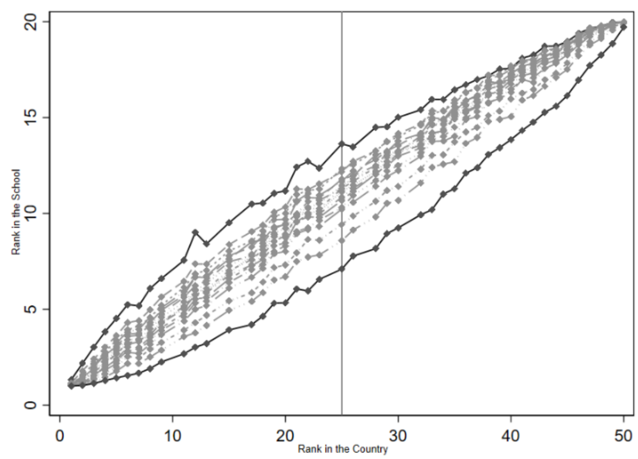
Table (3) Linear Results

	(a)	(b)	(c)	(d)	(e)	(f)
Income Rank						
School Rank	0.21***	0.20***	0.22***	0.22***	0.17***	0.16***
	(0.036)	(0.032)	(0.034)	(0.035)	(0.034)	(0.034)
Effect size (% of mean)	0.34	0.32	0.35	0.35	0.27	0.26
Income Log						
School Rank	0.0067***	0.0073***	0.0068***	0.0064***	0.0060***	0.0056***
	(0.0014)	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0014)
Effect size (% of mean)	0.084	0.093	0.086	0.081	0.077	0.071
Years of Schooling						
School Rank	0.020***	0.029***	0.032***	0.0062**	0.0043	0.015***
	(0.0027)	(0.0029)	(0.0030)	(0.0028)	(0.0029)	(0.0030)
Effect size (% of mean)	0.15	0.22	0.24	0.047	0.033	0.12
Finished Upper Secondary School						
School Rank	0.0014**	0.0018***	0.0022***	0.0015***	0.0016***	0.0022***
	(0.00055)	(0.00061)	(0.00062)	(0.00059)	(0.00060)	(0.00063)
Effect size (% of mean)	0.16	0.21	0.26	0.18	0.18	0.25
Science Track						
School Rank	0.0058***	0.0063***	0.0063***	0.0029***	0.0028***	0.0039***
	(0.00097)	(0.0011)	(0.0011)	(0.00099)	(0.00099)	(0.0011)
Effect size (% of mean)	2.60	2.82	2.83	1.31	1.24	1.73
Vocational track						
School Rank	0.0054***	-0.0013	-0.0018	0.0057***	0.0058***	0.0014
	(0.0012)	(0.0013)	(0.0014)	(0.0013)	(0.0013)	(0.0014)
Effect size (% of mean)	1.44	-0.35	-0.49	1.52	1.55	0.38

Notes: Standard errors in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Each coefficient shows the linear effect of a one-rank increase in students' ordinal position within their school-cohort distribution (equivalent to a 5% shift in rank). All models include controls for ability, school–cohort fixed effects, parental income, parental education, gender, and immigration status. Columns (a)–(f) correspond to alternative specifications described in Section 4, which differ in the number of interactions used to capture school type and ability. Specification (f) is the preferred model. Effect sizes are calculated relative to the sample mean of each outcome.

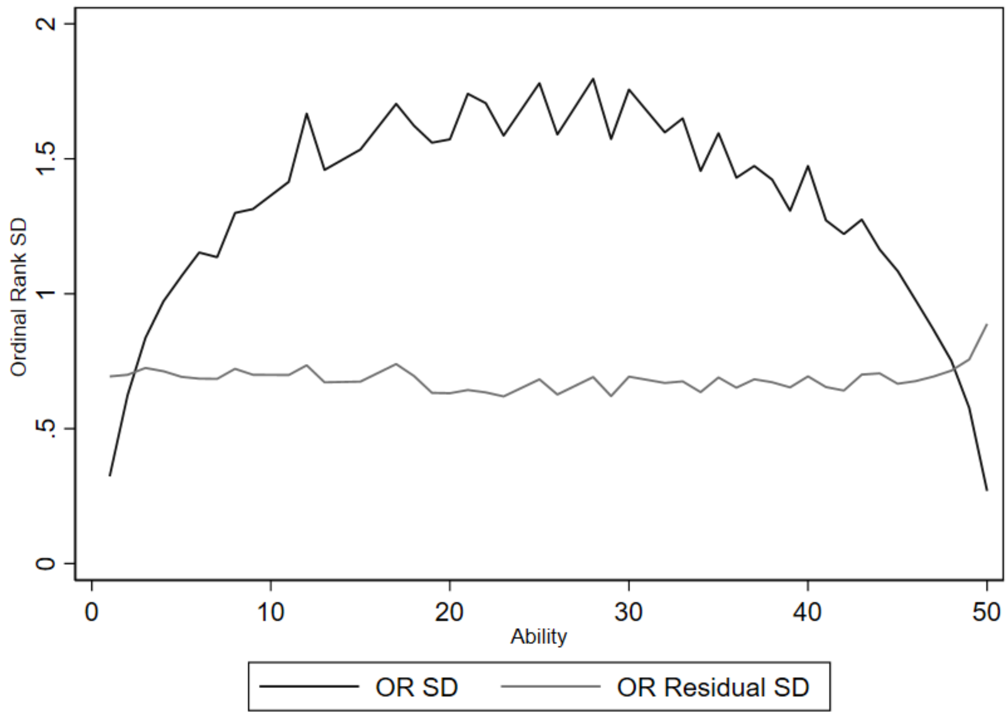
Figures:

Figure (1) Relationship between ability and school rank



Notes: The y-axis shows a student's within-school rank (1 = lowest, 20 = highest). The x-axis shows national ability rank (1 = lowest, 50 = highest), proxied by Grade-9 GPA rank. Each dot is the mean within-school rank for all students at a given national rank. Gray lines connect these means for schools grouped into 20 school groups (from low to high mean) based on each school's average within-school rank (from low to high). The vertical line marks the median of the national ability distribution.

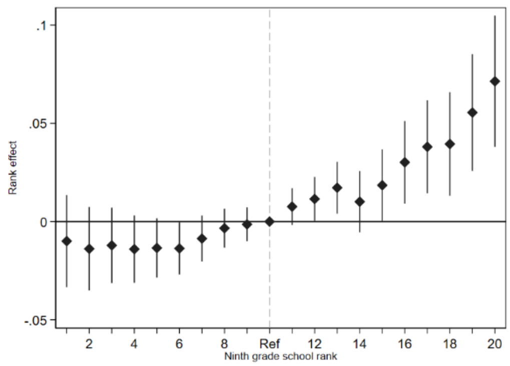
Figure (2) The standard deviation of ordinal rank by the ability



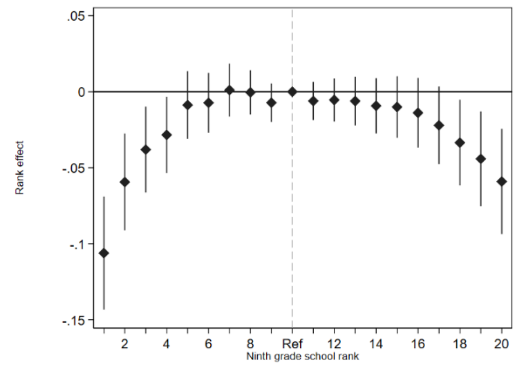
Note: This figure shows the standard deviation of the students' ordinal rank (black line) and the residuals (grey line) of ordinal rank. When calculating the residual of ordinal rank, the rank is used as a dependent variable regressed on ability, school type, pre-characteristics control, and school-cohort fixed effect.

Figure (3) The effect of ninth grade ordinal rank on different outcomes

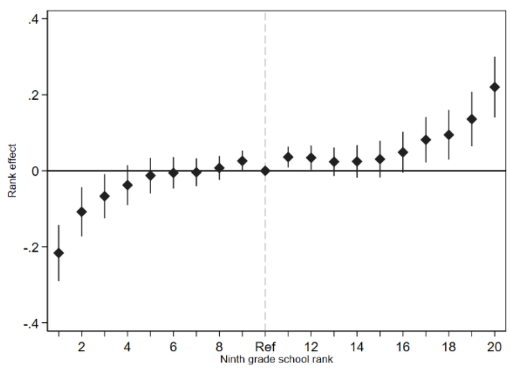
(a) Science Track



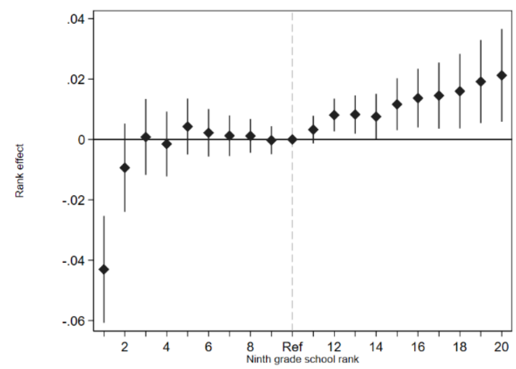
(b) Vocational Track



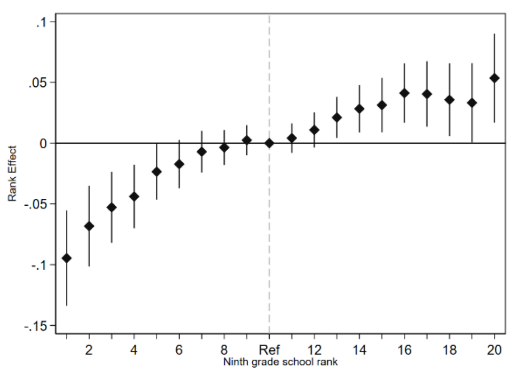
(c) Years of Schooling age 33



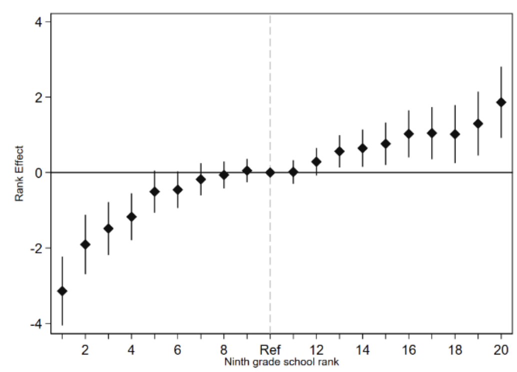
(d) Finished Upper Secondary school



(e) Income log, age 35-38



(f) Income Rank age 35-38



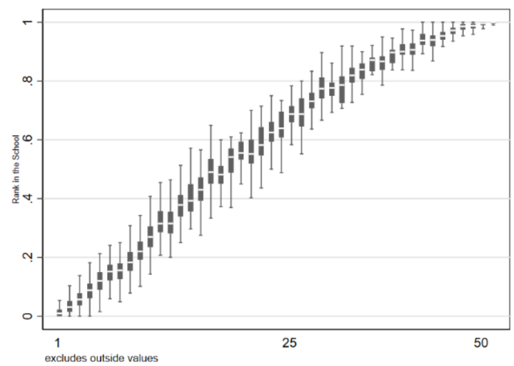
Note: Each dot in the figures marks the estimates of 1 to 20 school rank with 95% confidence interval. The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on mean and variances) interact with the pupils' national rank. The following controls are included in the model: immigration status, gender, parental education, parent income, and school-cohort fixed effect.

Appendices

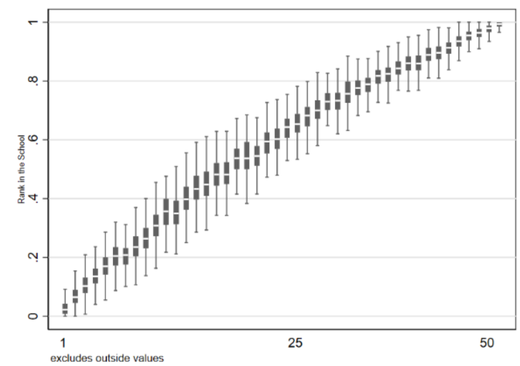
A. Additional Figures and Tables

Figure (A1): Relationship between ability and school rank in different types of schools

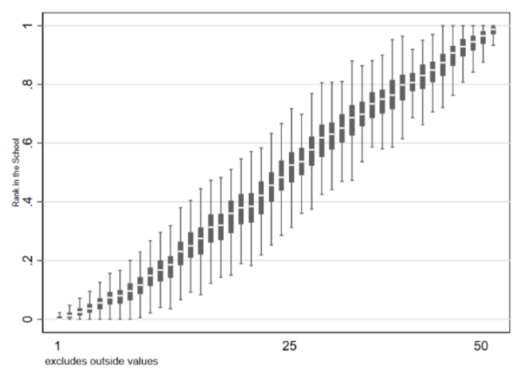
(a) Mean Q1- Variance Q1



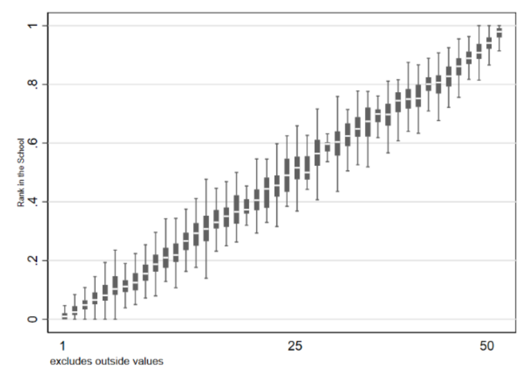
(b) Mean Q1- Variance Q4



(c) Mean Q4- Variance Q1

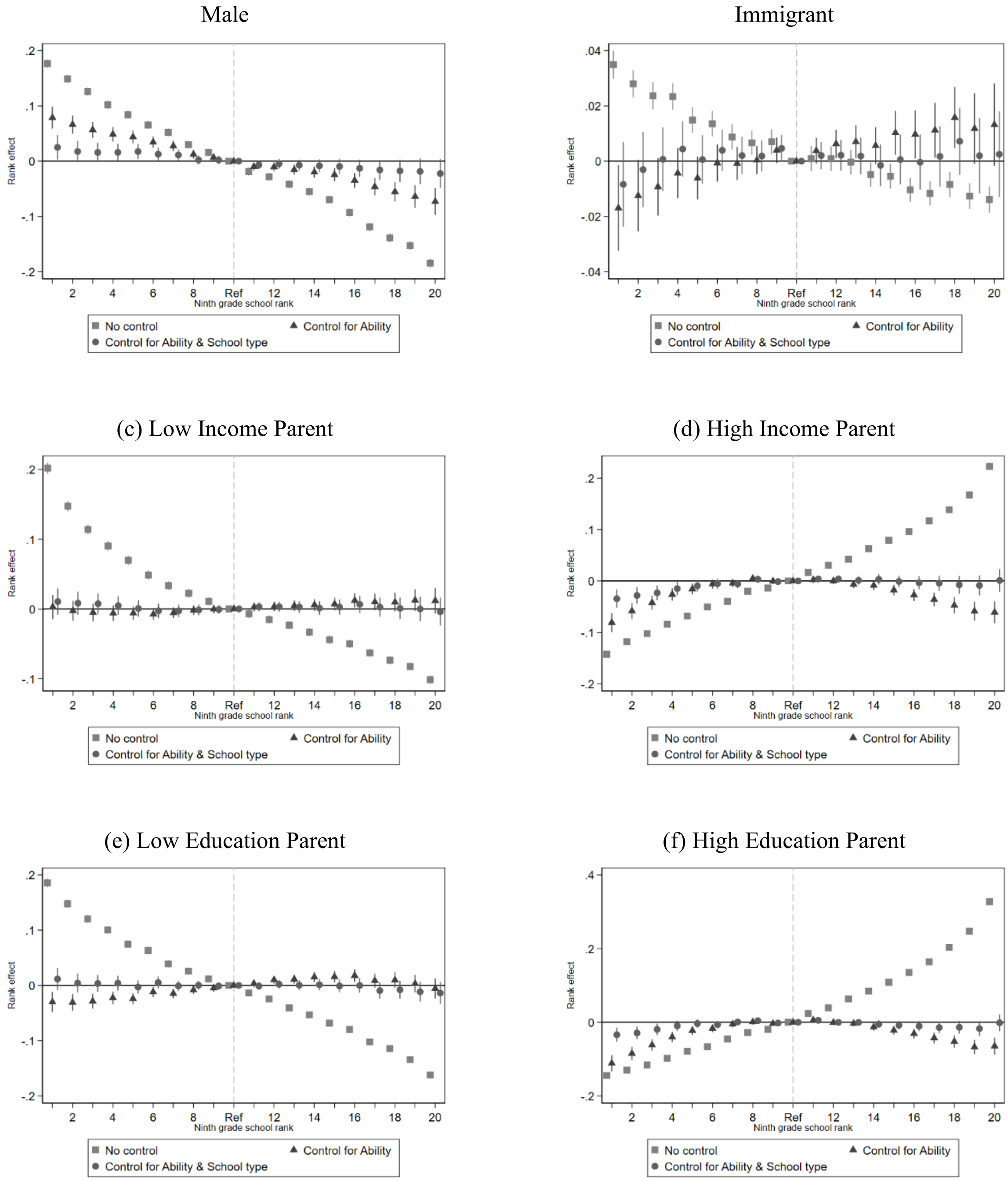


(d) Mean Q4- Variance Q4



Note: This figure shows the box plot of students' ability and their ordinal rank. Each box represents the median, 25 percentile, and 75 percentiles of ordinal rank for each ability point. Four types of schools are shown in panels: schools with low mean and low variance (panel a); schools with low mean and high variance (panel b); schools with high mean and low variance (panel c); and schools with high mean and high variance (panel d).

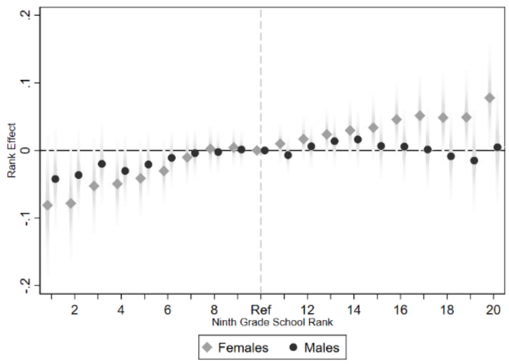
Figure (A2) Non-linear Balance Test



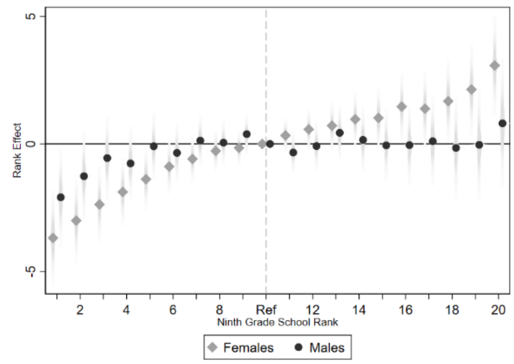
Note: This figure shows the non-linear balance test of different characteristics of the pupils across ranks. Each point is the estimate and 95% confidence interval of the corresponding rank. The green dots mark the estimates when no control is included in the model, the yellow dots mark the estimates when 50 dummies of ability are included in the model, and the blue dots mark the estimates when the interaction of school types and 50 dummies of ability were included.

Figure (A.3) Heterogeneity by Gender

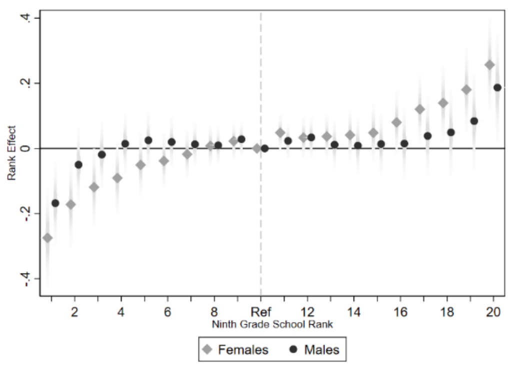
(a) Income log, age 35-38



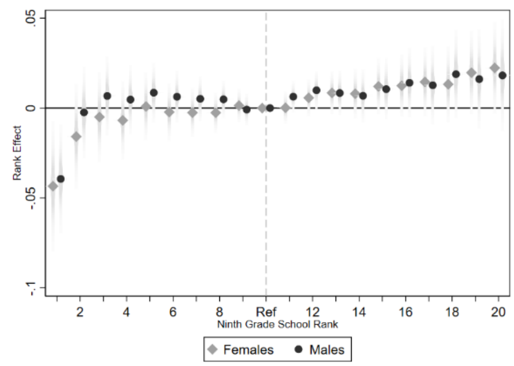
(b) Income Rank age 35-38



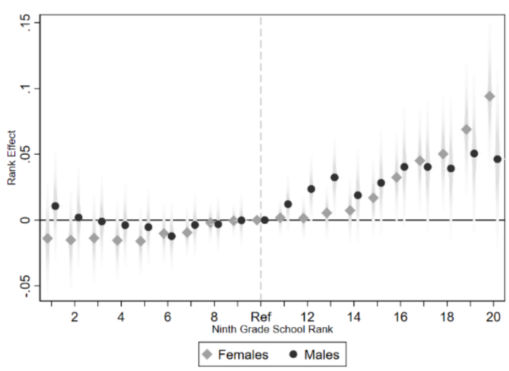
(c) Years of Schooling age 33



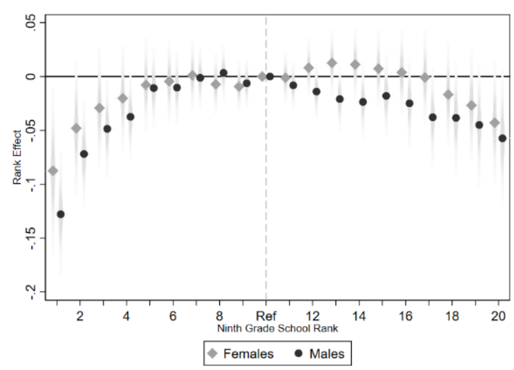
(d) Finished Upper Secondary school



(e) Attending Science Track



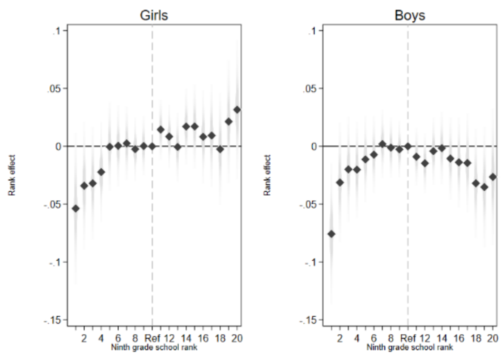
(f) Attending Vocational Track



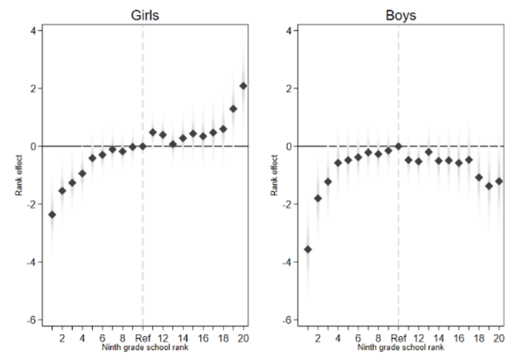
Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval for females (grey) and males (black). The 10th rank is the reference point.

Figure (A.4) Heterogeneity by Gender— Gender group

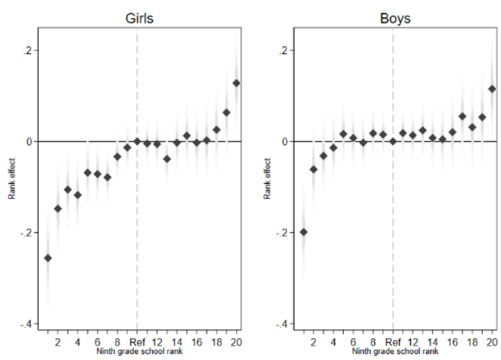
(a) Income log, age 35-38



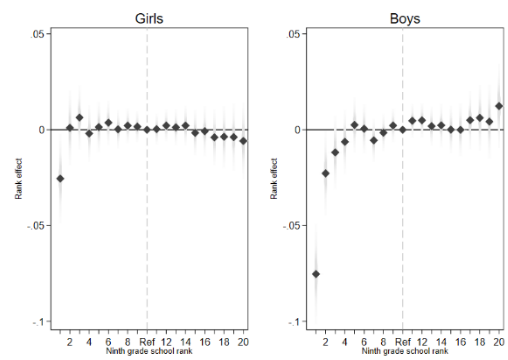
(b) Income Rank age 35-38



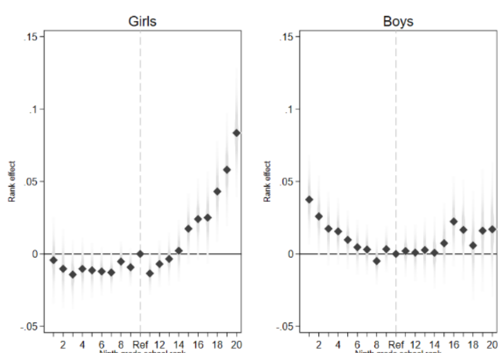
(c) Years of Schooling age 33



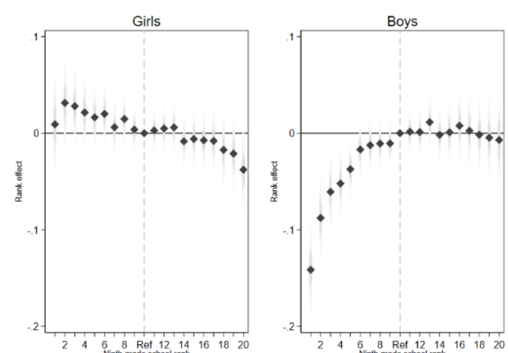
(d) Finished Upper Secondary school



(e) Attending Science Track



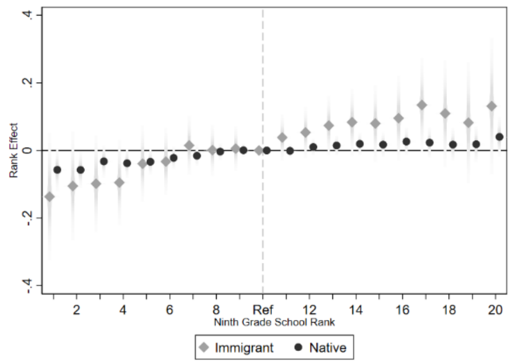
(f) Attending Vocational Track



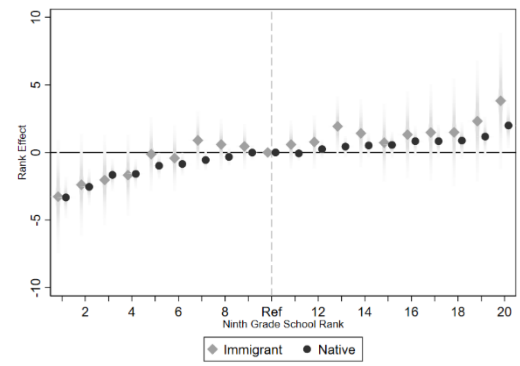
Note: This figure shows the estimates of girl's rank among girls (left panel) and estimates of boy's rank among boys (right panel) for different outcomes

Figure (A.5) Heterogeneity by Immigration status

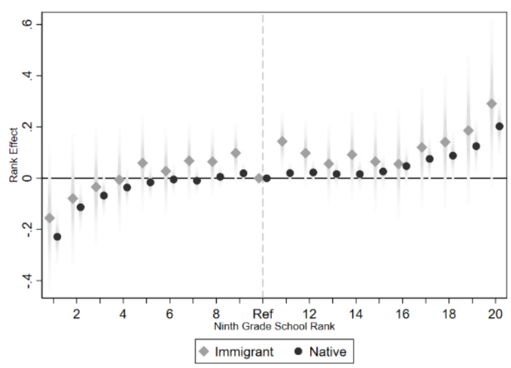
(a) Income log, age 35-38



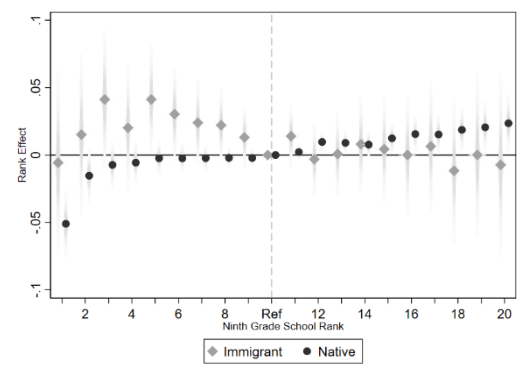
(b) Income Rank age 35-38



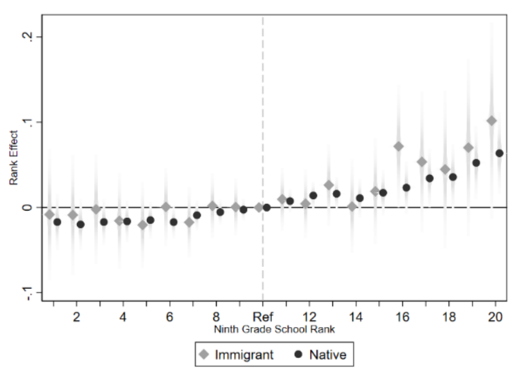
(c) Years of Schooling age 33



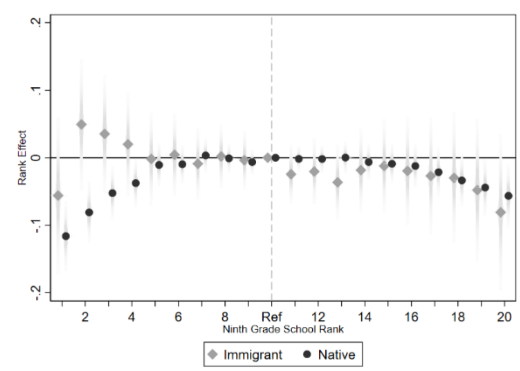
(d) Finished Upper Secondary school



(e) Attending Science Track



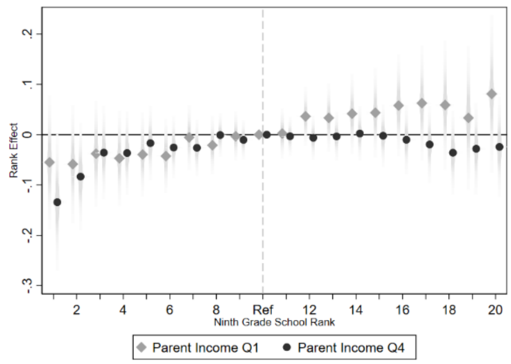
(f) Attending Vocational Track



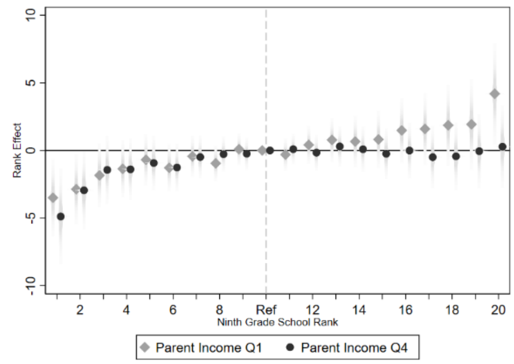
Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval for immigrant (grey) and Native (black). The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on means and variances) are interacted with the students' national rank. Gender of the student, parental education, parent income, and school-cohort fixed effects are included in the model.

Figure (A.6) Heterogeneity by Parent's Income

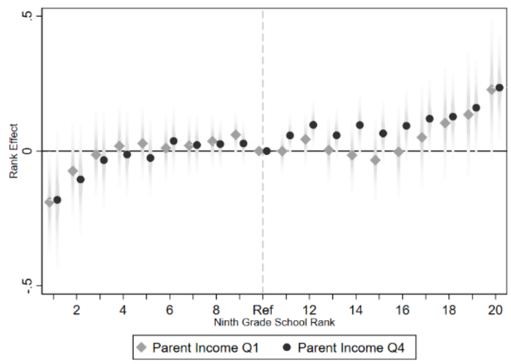
(a) Income log, age 35-38



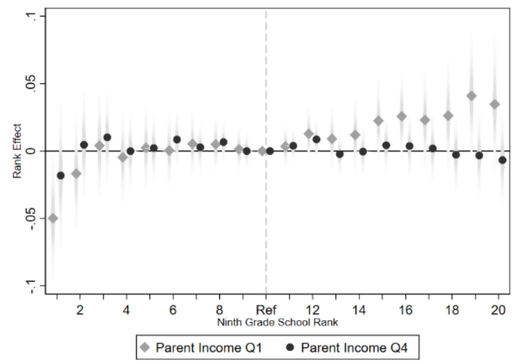
(b) Income Rank age 35-38



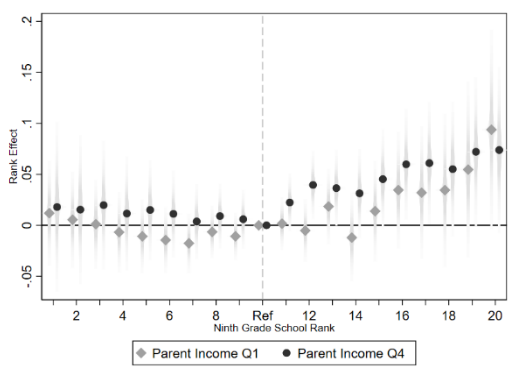
(c) Years of Schooling age 33



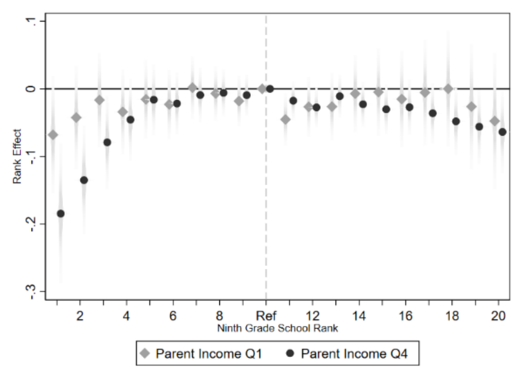
(d) Finished Upper Secondary school



(e) Attending Science Track

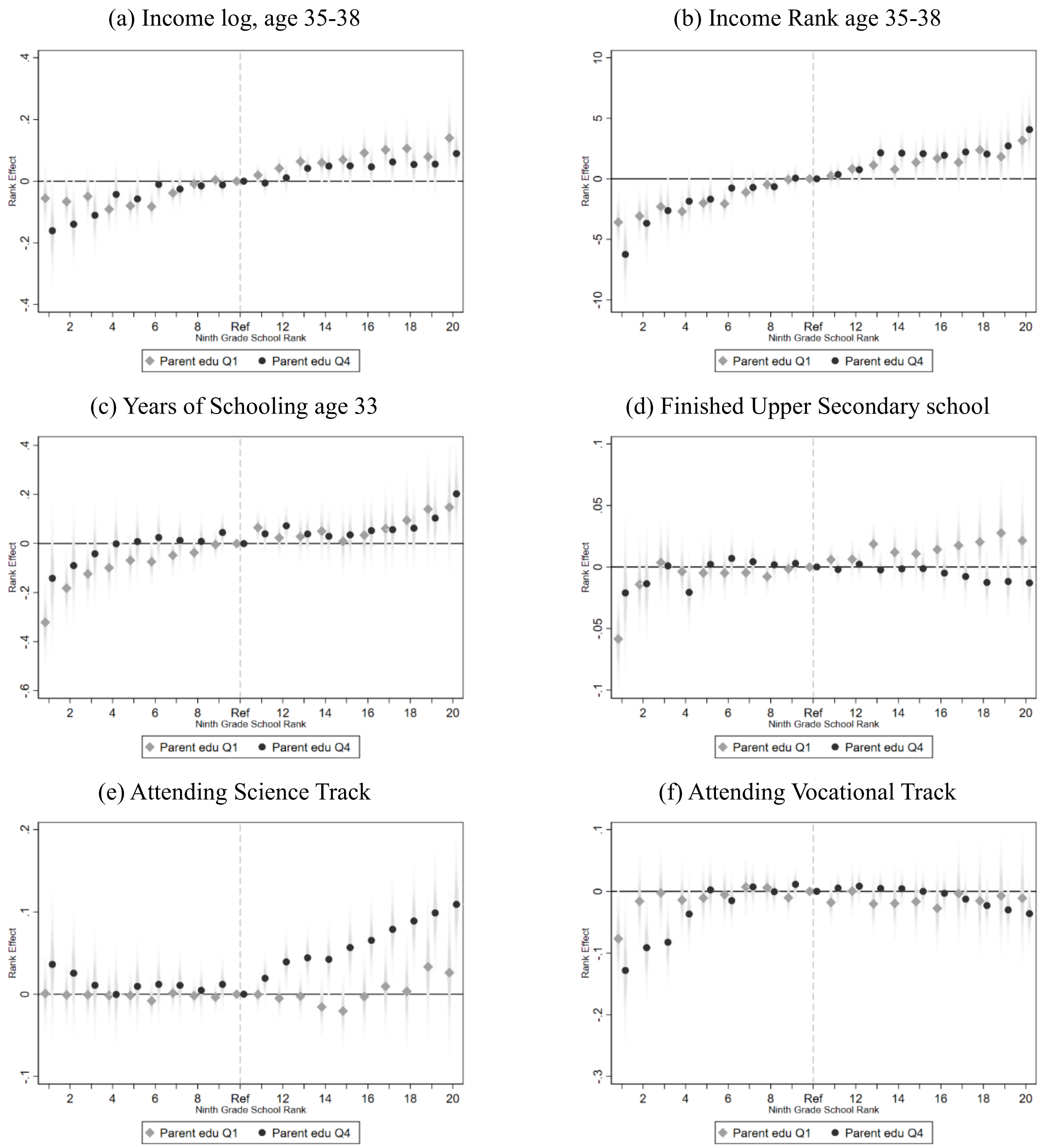


(f) Attending Vocational Track



Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval for low income parent (grey) and high-income parent (black). The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on mean and variances) interact with the students' national rank. Gender of the students, parental education, parent income, and school-cohort fixed effects are included in the model.

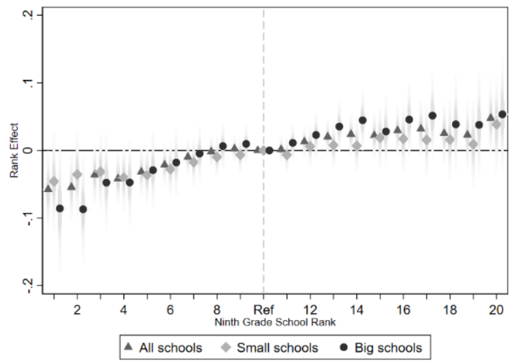
Figure (A.7) Heterogeneity by Parent's Education



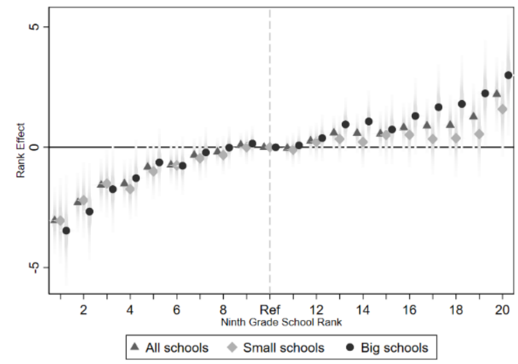
Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval for low-educated parent (grey) and high-educated parent (black). The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on mean and variances) are interacted with the students' national rank. Gender of the students, parental education, parent income, and school-cohort fixed effects are included in the model.

Figure (A.8) Robustness check– school size

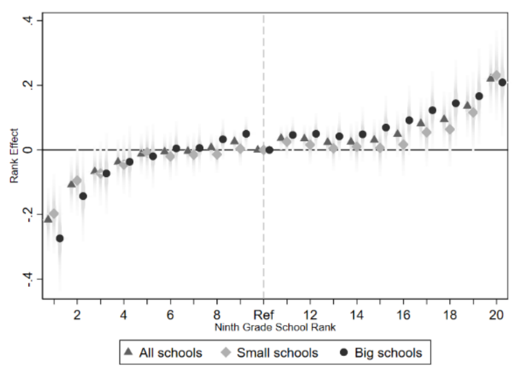
(a) Income log, age 35-38



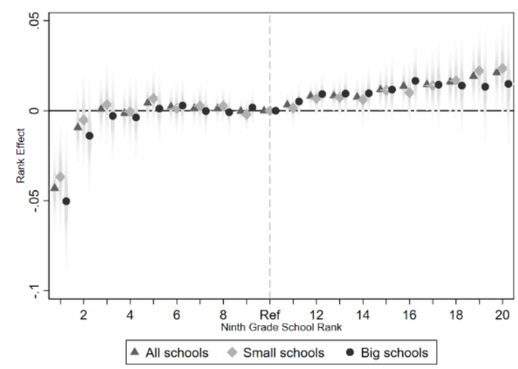
(b) Income Rank age 35-38



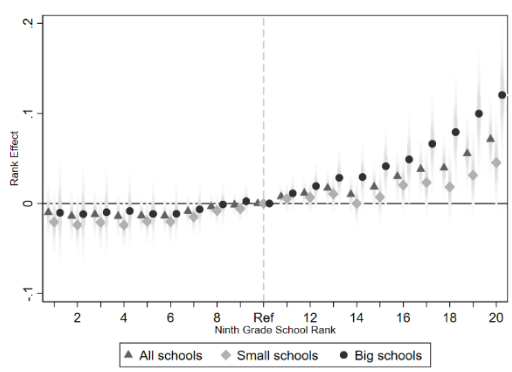
(c) Years of Schooling age 33



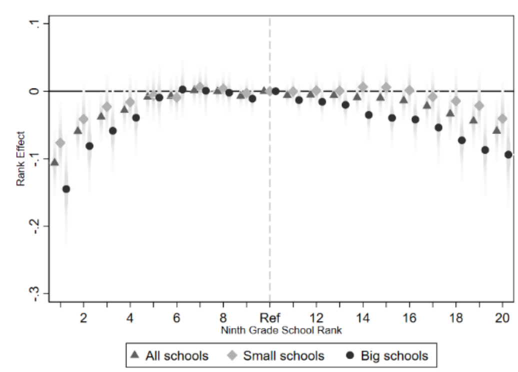
(d) Finished Upper Secondary school



(e) Attending Science Track

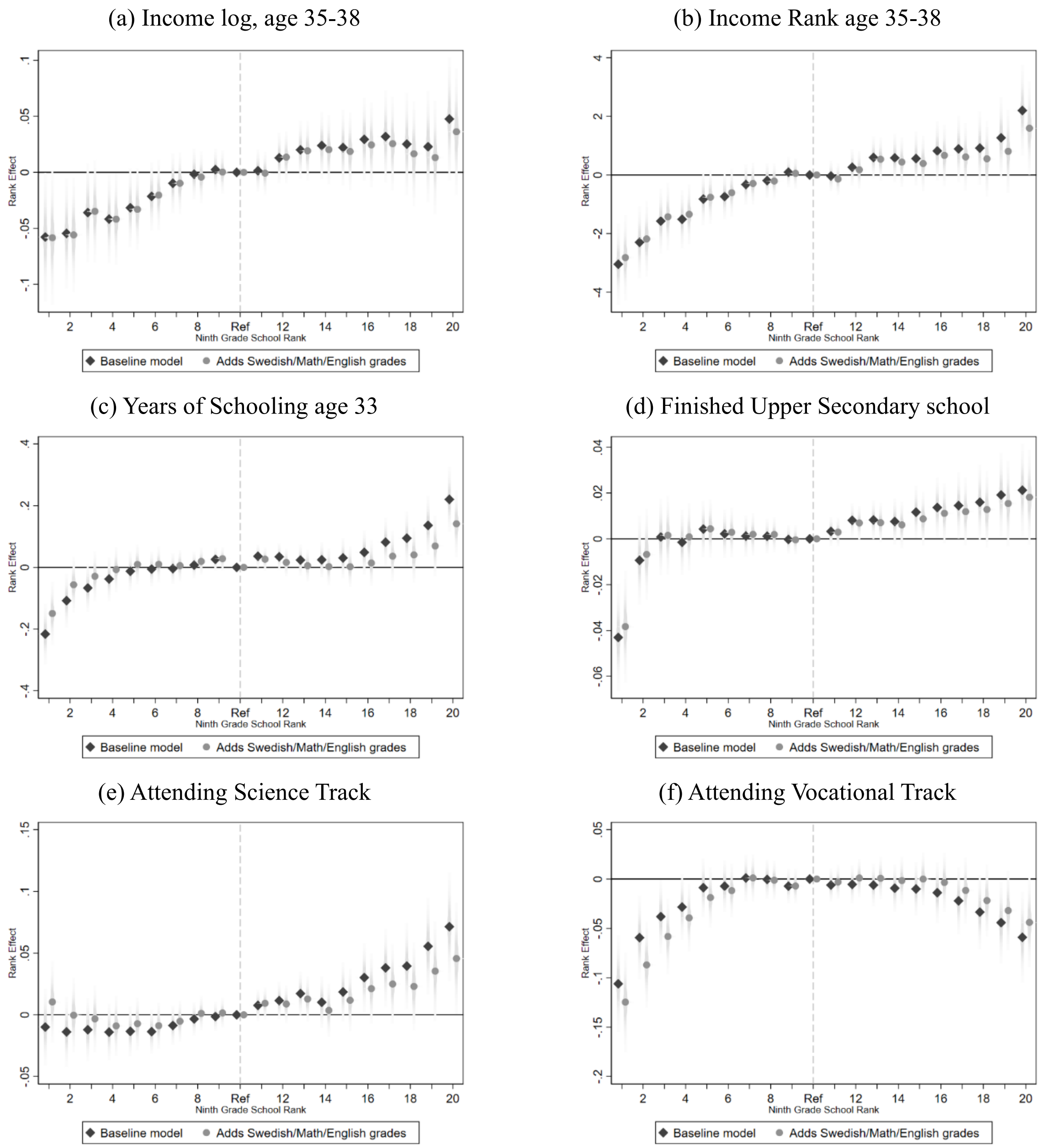


(f) Attending Vocational Track



Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval for small schools (grey) and big schools (black). Small (big)schools are defined as having less (more) than 120 students. The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on mean and variances) are interacted with the student's national rank. Gender of the students, parental education, parent income, immigration status and school-cohort fixed effects are included in the model.

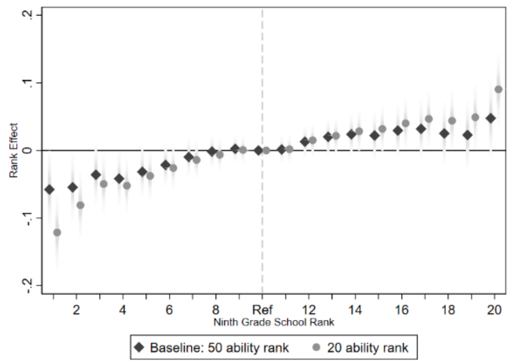
Figure (A.9) Robustness check—other grades



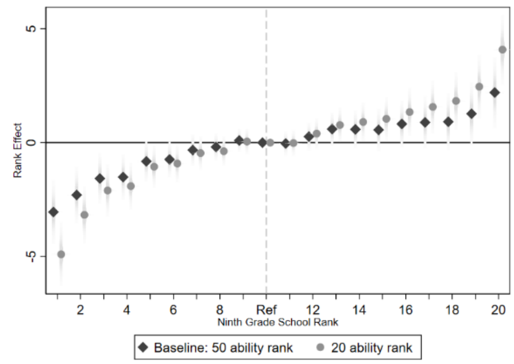
Note: Each dot in the figures shows the estimates of 1–20 school rank with 95% confidence interval. The grey dots shows the estimates when Swedish, Mathematics, and English are added to the main model. The 10th rank is the reference point. In all models, dummy variables for 16 types of schools (based on mean and variance of achievement) interact with the students' national rank. Gender of the students, parental education, parent income, and school-cohort fixed effects are included in the model.

Figure (A.10) Robustness check—GPA Ties (50 vs. 20 Categories)

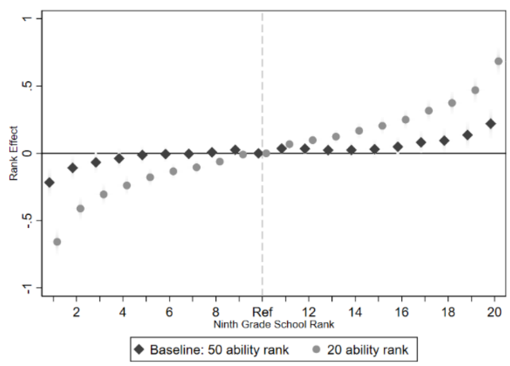
(a) Income log, age 35-38



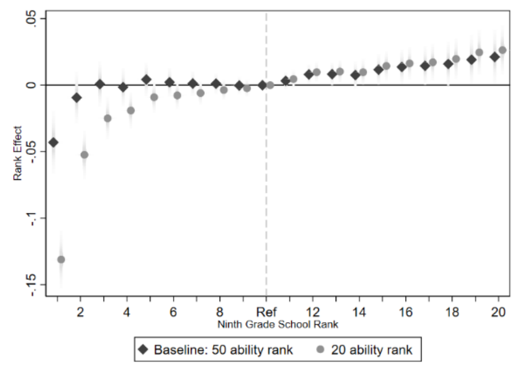
(b) Income Rank age 35-38



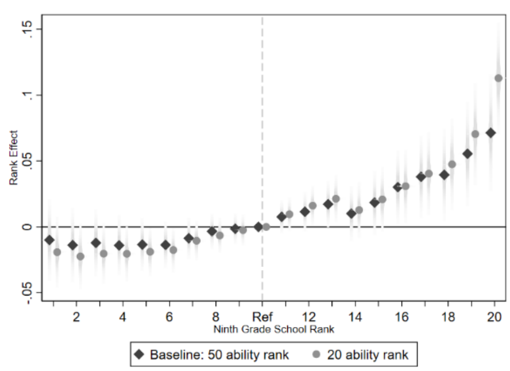
(c) Years of Schooling age 33



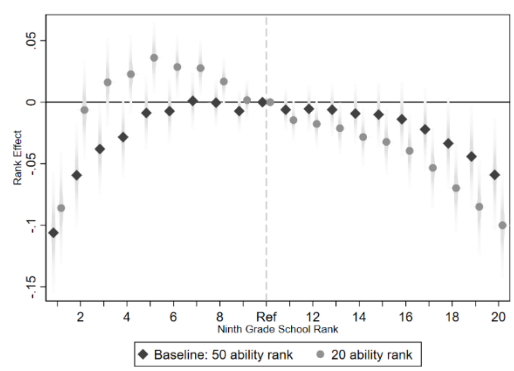
(d) Finished Upper Secondary school



(e) Attending Academic Track



(f) Attending Vocational Track



Note: Each dot in the figures shows the estimates of school rank (1–20) with 95% confidence intervals. Black diamonds indicate estimates from the baseline specification using 50 GPA categories, while grey circles show estimates using 20 GPA categories (following Denning et al., 2023). The 10th rank is the reference point. In all models, dummy variables for 16 school types (based on mean and variance of achievement) interact with students' national rank. Student gender, parental education, parental income, and school-cohort fixed effects are included.

Appendix B

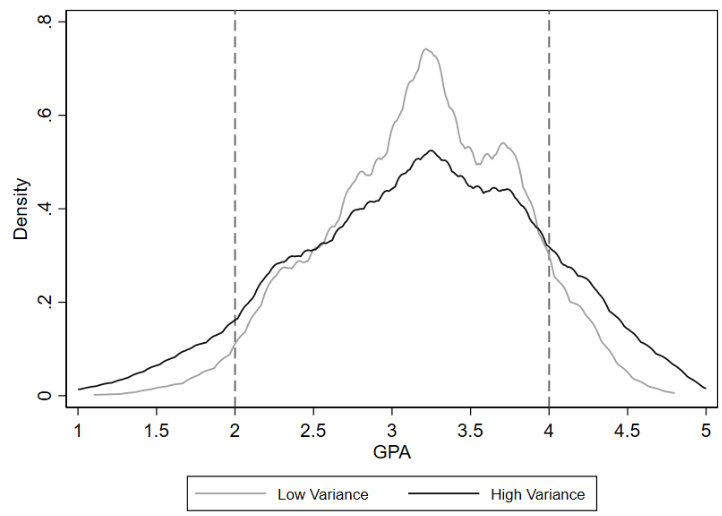
A potential source of bias arises because ordinal rank is partly determined by the distribution of grades within a school. To illustrate, consider two schools with the same mean GPA: one with high variance (School A) and one with low variance (School B). A student with a GPA of 4.0 may rank lower in School A than in School B due to greater dispersion, and the same applies for lower-GPA students. This implies that ordinal rank is shaped not only by a student's ability but also by school level grading distributions (Figure A.1). To address this, the empirical model incorporates a function that captures interactions between national ability rank and school type:

$$A(a_{isc}) = \sum_{d=1}^{D} \sum_{a=1}^{50} \beta_{ad} \, I_{isc} \qquad (2)$$

where I_{isc} is a dummy equal to 1 if student i in cohort c falls into national ability rank r (1–50). School type d ranges from 1 to D, based on the mean and variance of each school's GPA distribution. School types were classified using five approaches: Quartiles of the school-level GPA mean, quartiles of the GPA variance, deciles of the GPA mean., deciles of the GPA variance and combinations of quartiles for both mean and variance.

These approaches generate different numbers of school-type–by–ability dummies: 200 (4×50) for alternatives 1 and 2, 500 (10×50) for alternatives 3 and 4, and 800 (4×4×50) for alternative 5.

Figure (B1) Two schools with the same GPA mean and high and low variance



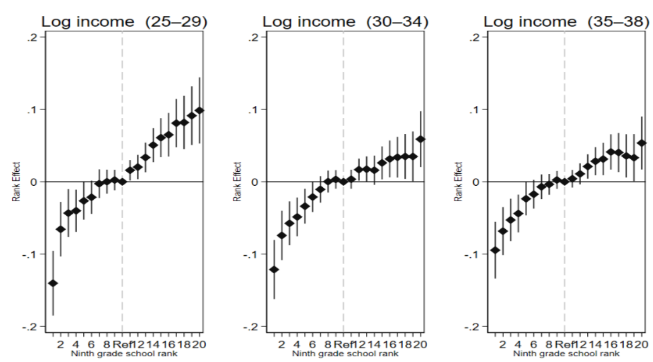
Note: This figure shows the grade distribution in two schools with the same mean GPA but with two variances: high and low. The black line is the distribution of high variance schools.

Appendix C. Earnings Outcomes

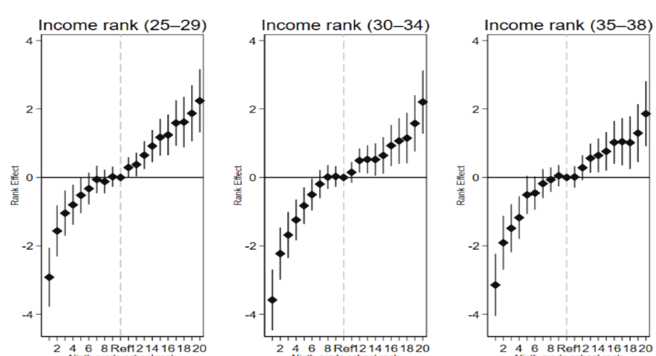
This appendix reports results for three alternative earnings measures across different age groups (25–29, 30–34, and 35–38): (i) log income, (ii) income rank within the national distribution, and (iii) average absolute income including zero earners. Figure C1 presents results for each outcome. Across all specifications, the non-linear rank effects observed in the main analysis are preserved. At the lower end of the rank distribution, negative rank effects become more pronounced at later ages, while at the upper end, positive effects are slightly larger but estimated with less precision. These findings suggest that the results are not sensitive to the choice of outcome measure and remain stable across different age windows.

Figure C1. Rank effects on earnings outcomes by age group

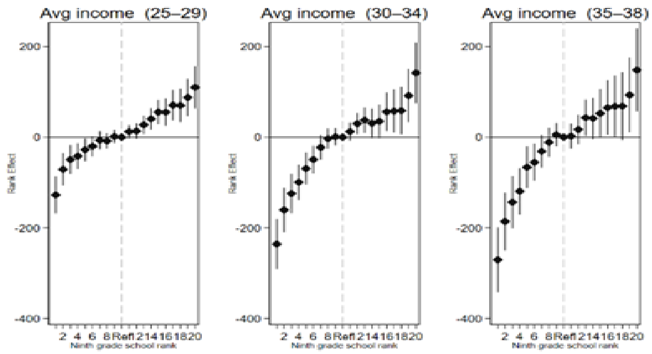
Panel A: Log income



Panel B: Income rank



Panel C: Average income



Notes: Each dot shows the estimated effect of school rank (1–20) with 95% confidence intervals. The 10th rank is the reference point. In all models, dummy variables for 16 school types (based on mean and variance of achievement) interact with students' national rank. Controls include student gender, parental education, parental income, and school-cohort fixed effects.

D Data description

This section explains the data used in this study. First, the information about how each variable was constructed is provided, followed by detailed descriptive statistics (Table

D.1 and Table 2.2).

D.1 Data description and variable definitions

D.1.1 GPA, ability, school rank, and school types

Final grades from grade nine, GPA:

Students were graded on a 5-point scale. The grade point average (GPA) is defined as the average of the subject grades, ranging from 1 to 5. The GPA is recoded with one decimal in the registry and is provided at a 50-level range, from 1–50.

• Swedish and Mathematics:

The final grade for mathematics, Swedish and English, from 1–5.

• School code:

Identifier of the schools, each school has one unique code.

• School rank among girls:

Sorting each female student in her female cohort and school and grouped in 20 equal groups, each group contains 5% of female students in the school-cohort.

• School Rank among boys:

Sorting each male student in his male cohort and school and grouped in 20 equal groups, each group contains 5% of male students in the school cohort.

• Country Rank (ability):

Sorting each student on GPA among his or her cohort and grouped in 50 groups. The groups are not of equal size.

• School type based on GPA mean:

To calculate this variable, the average of the school GPA is calculated. Then schools were sorted based on the mean from the lowest to the highest in 4 and 10 groups.

• School type based on GPA variances:

To construct these variables, first the average of the school GPA variance was calculated. Then schools were sorted based on the variances from the lowest to the highest in 4 and 10 groups.

• School size:

The number of students in each school and year indicates the school size.

D.1.2 Students' outcomes:

• Students' years of education:

To build students years of education, the students were followed until the age of 33 and the years of education were calculated based on the highest level of completed education. This is based on the latest degree (finished study) at the age of 33.

• Students earned income:

The average yearly earned income of the students between age 30 and 33. The analysis used the log of this variable.

• Students income rank:

I used student income rank for students when they were between 30 and 33 years old. To find the income rank, the income for all students in the same cohort was ranked from lowest to highest and grouped in 100 ranks. The percentile rank for the students was averaged between age 30 and 33.

• Students study track:

After finishing grade nine, students could choose to attend different tracks, which are categorized as vocational, academic, or preparatory. The preparatory track normally consists of a year of studies aimed at preparing the student to enter a regular vocational or academic track, and it is for students who lack sufficient qualifications (have too low grades) after grade nine. Each of the vocational and academic tracks is divided into sub-tracks. The vocational track includes a sub track related to industry and technology, health and childcare, and business and administration, and the science track consists of sub-tracks related to science, social sciences, and the arts.

D.1.3 Students demographic characteristics:

• Female:

A dummy variable is equal to one if the students is female, and zero otherwise.

• Immigration:

A dummy variable is equal to one if the student is born outside Sweden, and zero if the student is born in Sweden.

• Immigration year:

A set of dummy variables indicating the age of immigration to Sweden.

• Immigrant studying Swedish as a second language:

A dummy variable of one is used if an immigrant student was taking the course “Swedish as second language” and zero otherwise. This indicator is highly correlated with the “immigration year” as students who immigrated later in life more often attend this course.

• Student’s year and month of birth:

Dummy variables indicate each student’s month and year of birth.

D.1.4 Family characteristics

• Parents’ years of education:

The mother’s and father’s years of education when students were in grade nine. The variables are based on the highest level of completed education.

Parents’ income:

The mother’s and father’s incomes were measured when the students were in grade nine. Their incomes were summed and sorted from the lowest to the highest. Four dummies indicating the lowest (Q1) to the highest (Q4) income level were used.

D.2 Data sources

The data were obtained from registers from Statistics Sweden, for example Inkomst- och taxeringsregistret (IoT); Högskoleregistret (HR); Skolverkets elevregister (SE). Table D.1 shows the years that data were collected, the age of the students at the time the variable used, and the data sources.

Table (D.1) Data Sources

Variables	Year	Age of students	Register
GPA, Rank and Ability			
GPA	1990–1997	15/16	SE
Swedish 9th grade	1990–1997	15/16	SE
Math 9th grade (advanced)	1990–1997	15/16	SE
Math 9th grade (not advanced)	1990–1997	15/16	SE
English 9th grade (advanced)	1990–1997	15/16	SE
English 9th grade (not advanced)	1990–1997	15/16	SE
Rank in school (all)	1990–1997	15/16	SE
Rank in school (only girls)	1990–1997	15/16	SE
Rank in school (only boys)	1990–1997	15/16	SE
Rank in country (all)	1990–1997	15/16	SE
Outcomes			
Science track	1995–1997	15/16	HR
Vocational	1995–1997	15/16	HR
Years of education	2007–2014	33	SUN
Log income	2004–2014	35–38	IoT
Income rank	2004–2014	35–38	IoT
Control Variables			

Table (D.1) Data Sources

Variables	Year	Age of students	Register
Gender	—	—	IoT
Immigration	—	—	IoT
Year of birth	1974–1981	0	IoT
Month of birth	1974–1981	0	IoT
Mother level of education (4 levels)	1990–1997	15/16	SUN
Father level of education (4 levels)	1990–1997	15/16	SUN
Mother years of education	1990–1997	15/16	SUN
Father years of education	1990–1997	15/16	SUN
Parental household income (4 quantiles)	1990–1997	15/16	IoT
School dummies	1990–1997	15/16	SE
School size	1990–1997	15/16	SE
School type	1990–1997	15/16	SE

Note: This table reports the years that data were collected, the age of the students at the time of the variable used

Table (D.2) Descriptive statistics

Variable	Mean	SD	Min	Max	Obs
GPA	3.22	0.70	1.00	5.00	794,076
English (advanced)	2.99	0.90	1.00	5.00	211,072
English	3.32	0.87	1.00	5.00	536,233
Mathematics (advanced)	3.00	0.95	1.00	5.00	309,044
Mathematics	3.26	0.90	1.00	5.00	445,494
Swedish	3.19	0.90	1.00	5.00	770,575
Academic track	0.57	0.50	0.00	1.00	276,062
Social science	0.29	0.22	0.00	1.00	276,062
Science track	0.22	0.42	0.00	1.00	276,062
Vocational track	0.37	0.48	0.00	1.00	276,062
Vocational track: Technology	0.17	0.38	0.00	1.00	276,062
Vocational track: Health Care	0.10	0.30	0.00	1.00	276,062
Vocational track: Trade	0.10	0.30	0.00	1.00	276,062
Income (age 35–38, log)	7.87	0.84	0.00	11.72	719,713
Years of schooling	13.14	2.17	7.00	20.00	756,340
Finished upper-secondary school (%)	86.09	34.60	0.00	100.00	794,076
Female share (%)	48.87	49.99	0.00	100.00	794,076
Father income (log)	8.92	2.18	0.00	14.23	754,112
Mother income (log)	8.56	2.14	0.00	13.06	727,636
Father years of education	11.23	2.86	7.00	20.00	726,405
Mother years of education	11.32	2.54	7.00	20.00	757,699
Parent income Q1	19,940.25	28,324.35	0.00	106,991.40	193,395
Parent income Q2	47,236.09	50,784.44	1,739.00	157,236.00	193,392

Parent income Q3	62,938.19	66,818.10	2,712.80	199,728.81	193,390
Parent income Q4	90,613.31	100,444.99	3,512.20	1,550,109.00	193,392
Parent education Q1	17.11	1.76	14.00	20.00	181,642
Parent education Q2	20.91	0.88	19.00	22.00	174,572
Parent education Q3	23.55	1.12	22.00	26.00	175,557
Parent education Q4	28.97	2.59	26.00	40.00	176,264
Number of students in each school	121.83	40.30	1.00	296.00	794,076
Number of students in each rank	6.09	2.01	0.05	14.80	794,076

Note: This table contains detail descriptive statistics of the variable used in the study. It covers all students in compulsory school for the period 1990–1997

Table (D.3) Descriptive statistics—School types

	School Mean					School SD				
	Mean	SD	Min	Max	Obs	Mean	SD	Min	Max	Obs
Quartile 1	3.08	0.07	1.65	3.14	198,667	0.62	0.03	0.07	0.65	198,623
Quartile 2	3.17	0.02	3.14	3.20	198,726	0.67	0.01	0.65	0.69	198,518
Quartile 3	3.23	0.02	3.20	3.27	198,286	0.71	0.01	0.69	0.73	198,739
Quartile 4	3.38	0.11	3.27	3.99	198,397	0.76	0.03	0.73	1.23	198,188
Decile 1	3.03	0.08	1.65	3.09	79,936	0.59	0.03	0.07	0.62	80,209
Decile 2	3.11	0.01	3.09	3.13	79,481	0.64	0.01	0.62	0.65	79,505
Decile 3	3.14	0.01	3.13	3.15	79,124	0.65	0.00	0.65	0.66	78,509
Decile 4	3.16	0.01	3.15	3.18	79,152	0.67	0.00	0.66	0.68	79,406
Decile 5	3.19	0.01	3.18	3.20	79,700	0.68	0.00	0.68	0.69	79,512
Decile 6	3.21	0.01	3.20	3.22	79,467	0.69	0.00	0.69	0.70	80,228
Decile 7	3.24	0.01	3.22	3.25	79,048	0.71	0.00	0.70	0.72	78,779
Decile 8	3.27	0.01	3.25	3.29	80,888	0.73	0.01	0.72	0.74	79,468
Decile 9	3.33	0.02	3.29	3.37	78,337	0.74	0.01	0.74	0.76	79,747
Decile 10	3.48	0.11	3.37	3.99	78,943	0.79	0.03	0.76	1.23	78,705

Note: This table reports descriptive statistics of the school-level data for the period 1990–1997. Schools were categorized based on GPA mean (left panel) and GPA variances (right panel). The mean and the variance are sorted in quartiles and deciles.