Rather First in a Village than Second in Rome? The Effect of Students' Class Rank in Primary School on Subsequent Academic Achievements*

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Abstract

Is it better to be first in a village than second in Rome, as Caesar claimed? Peer groups can impact later outcomes through two distinct yet related channels: the group's intrinsic quality and one's relative position within this group. The Italian public school setting is an advantageous quasi-laboratory to investigate this question. Using panel data on Italian students over 2013-2019, we compare the effect of a student's relative position in their peer group (class rank) to the effect of class quality in primary school on later academic outcomes. We design a new strategy to identify the rank effect by leveraging two sets of scores: grades on a national standardized test and grades on class exams. Standardized test grades are used to control for ability, alongside student fixed effects. Class grades are used to construct the class rank. We exploit the variation in rank coming from differences in teachers' grading pattern and offer evidence that our measure of rank is as good as random, once we control for our proxies for ability. We find that ranking at the top of the class compared to the bottom in primary school is associated with a gain of 8.1 percentiles in the national standardized grade distribution in middle school and 7.6 in high school. We further show that Caesar was misguided: the effect of a one standard deviation increase in rank amounts to 20% of the effect of a similar increase in class quality, conditional on the rank. Finally, using an extensive student survey, we establish that the rank effect is mediated through student sorting into better high schools and higher interest in academic subjects, self-esteem, peer recognition, and career prospects.

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> We are told that, as Caesar was crossing the Alps and passing by a barbarian village which had very few inhabitants and was a sorry sight, his companions asked with mirth and laughter, 'Can it be that here too there are ambitious strifes for office, struggles for primacy, and mutual jealousies of powerful men?' Whereupon Caesar said to them in all seriousness, 'I would rather be first here than second at Rome.'
>
> Plutarch, *Parallel Lives*

Introduction

Is it better to be first in a village than second in Rome, as Caesar claimed? In other words, what matters the most: the intrinsic quality of the reference group or one's relative position within this group? Both are determined by levels of resources, such as ability, human capital accumulation, parental wealth or income, and are thus hard to disentangle.[1] Ideally, one would hold the quality of the reference group and individual level of resources constant while changing relative positions within the group. In this respect, the Italian school system emerges as a pertinent case study. First, students are regularly evaluated through class exams within a well-defined reference group. Second, the resulting class rank provides them with a salient metric to assess their relative position within this group. Third, grades on national standardized tests provide a comparable measure of ability, whose resulting ranking within classes may differ from the ranking known to students.

In this paper, we pursue two objectives using panel data on Italian public school students observed in primary, middle, and high schools. The first objective is to estimate the effect of a student's relative position within her primary school class on her subsequent academic performance. To this end, we draw on the seminal work of Murphy and Weinhardt (2020) to suggest a new strategy to identify the effect of class rank which we then compare to the effect of class quality. The second objective is to analyze the mechanisms that mediate the rank effect by exploiting answers to a rich questionnaire on students' motivations and aspirations.

A student's class rank depends on her own ability which can in turn affect future outcomes: to identify a rank effect, we first need to control for individual ability. Conditional on individual ability, a student's class rank can still vary with the quality of her reference group, notably her class peers' abilities. However, using class peers' abilities as a source of variation would be spurious. For example, consider two classes, A and B, with equal

[1] Most theories on inequalities and intergenerational mobility focus on the impact of levels of resources and not on the impact of relative position, conditional on levels of resources. See Mogstad and Torsvik (2021) for a review of these theories since Becker and Tomes (1986, 1979).

mean ability distributions but with A's variance being higher than B's. A student with above-the-mean ability will rank higher in B than in A. A student with below-the-mean ability will rank lower in B than in A. Therefore, individual ability still affects the rank through its interaction with the class ability distribution, leading to omitted variable bias. Accurate identification of the class rank effect requires another source of variation to allow rank to vary while accounting for student's ability as well as higher-order peer effects.

In this regard, Italy provides an advantageous quasi-laboratory. First, students are regularly evaluated through national standardized tests, which we can use to control for both student ability and class ability distribution. The National Institute for the Evaluation of the Italian Education System (Invalsi) administers yearly standardized tests at multiple points in a student's education: twice in primary school, once at the end of middle school, and twice in high school. The subjects tested are Italian and Math.[2] These tests are proctored and graded by a different teacher than the one instructing students in the specific subject. Grading follows a precise rubric, consistent across Italian school districts, and results are not communicated to students. The resulting standardized test grades are comparable across the country and provide us with a proxy for ability in primary school, alongside student fixed effects, and a measure of academic performance in middle and high schools. Second, teachers have ample discretion in choosing how to grade their students in their own courses. We assume that, conditional on ability, class grades are random, thereby introducing another source of variation in students' class ranks that we can use to identify a rank effect. Class grades are usually disclosed by teachers to students, making the resulting students' relative positions salient. Lastly, teachers and students are randomly assigned to classes within primary schools. Therefore, conditional on attending a given school, students do not self-select into a class based on their expected rank or the expected teacher grading pattern.

Our identification strategy of the rank effect builds on the analysis of two distinct sets of scores: grades on national standardized tests to obtain a proxy for ability and grades on class exams to construct class ranks. We contend that class grades can be considered random conditional on ability because they are teacher specific. Therefore, variation in a student's class rank now comes from three sources: her ability, the ability distribution of her class, and teachers' idiosyncratic grading patterns. Two similarly able students may be ranked differently in classes exhibiting the same ability distribution due to teacher-specific grading. This allows us to identify a rank effect under relaxed assumptions compared to existing rank literature, notably regarding higher-order peer effects.

We suggest two main robustness checks to address two concerns. First, the rank

[2] English has been tested since 2019 but this does not concern the cohorts we use in this paper.

based on class grades might capture ability more accurately than the rank based on standardized test grades. Second, our proxies for ability might not suffice to ensure that the rank is orthogonal to ability. The first robustness check exploits the fact that the rank based on class grades is visible while the rank based on standardized grades is invisible. We show that, unconditionally, the rank based on standardized test grades and the rank based on class grades have the same impact on future outcomes. When we control for our ability proxies, the effect of the rank that is invisible almost vanishes while the effect of the rank that is visible persists. As any omitted variable, especially ability, would not exhibit such asymmetry, we conclude that the rank based on class grades is orthogonal to ability and operates through a channel that affects students' perceptions. The second robustness check further verifies that the rank based on class grades does not capture any remaining effect stemming from ability. In our main specification, we show that class rank in primary school is strongly correlated to previous measures of ability, thereby confirming that teachers' ranking is not random and reflects ability. However, once we control for our ability proxies, this correlation disappears.

Our findings show that the rank effect is large. Our main specification estimates that ranking at the top of class compared to the bottom in primary school is, *ceteris paribus*, associated with a gain of 8.2 percentiles in the national standardized grade distribution in middle school and 7.6 in high school. To assess the importance of this relative position effect with respect to an absolute position effect, we compare the rank effect to a class quality effect (Chetty et al., 2011). We find that the impact on future academic performances of increasing the rank by one standard deviation is about 20% as strong as the effect of increasing class quality by one standard deviation, conditional on the rank. Although Caesar appears to have been misguided (it is better to be second in Rome than first in a miserable village), this result shows that relative position matters substantially.

To understand what may explain such a lasting effect of rank on academic outcomes, we turn to explore the mechanisms that may be at play. We leverage a unique dataset consisting of a survey covering the 2018 cohort of students in their second year of high school. This detailed questionnaire (Questionario di Contesto) was administered as part of the 2018 Invalsi test. Students were asked questions about a wide range of different topics related to school and beyond, such as their self-esteem, parental support, career expectations, and relationships with peers. Students had to state how much they agreed with a series of statements related to each topic, using a six-point Likert scale. We find that the rank significantly improves students' self-esteem, career prospects, confidence in their ability in Italian and Math, relationships with their peers, and the way they perceive the education system. It also significantly impacts how they sort into high schools, with higher-ranked students self-selecting into better high schools.

Relative position in primary school class has lasting and profound academic and psychological effects on students. It is no wonder that class quality impacts students' later outcomes more, but these findings raise interesting questions. For instance, would reinforcing the salience and impact of rank among students be a way to reduce disparities in class quality?

Related Literature. Our work contributes to the vivid literature examining the rank effect in school. The first strand of rank papers uses randomized experiments to underpin the impact of rank disclosure (Goulas and Megalokonomou, 2021; Azmat and Iriberri, 2010). These studies find that revealing rank to students has a positive impact on performance, achievements, and earnings of those at the top of the ranking distribution. A second line of research exploits across-cohort variations to identify a rank effect (Elsner and Isphording, 2018, 2017) based on a strategy pioneered by Hoxby (2000). The assumption is that, conditional on attending a given school, the cohort composition is as good as random since it is determined by the timing of birth and local age cut-offs. The ranking is found to positively impact students' performance and to negatively affect anti-social behaviors.

Our paper most closely relates to a third line of recent literature spawned by the seminal work of Murphy and Weinhardt (2020). The strategy employed by these papers (Rury, 2022; Carneiro et al., 2022; Denning et al., 2021; Elsner et al., 2021; Pagani et al., 2021; Yu, 2020) is as follows: as classes vary in size and students' abilities, two similarly able students in two different classes will end up with a different rank. Consistent with previous research, these papers find that ranking in early stages of education affects subsequent performance and achievements and influences personality traits and behavior. In their review of the rank literature, Delaney and Devereux (2022) however point out to a limitation inherent to this strategy, namely the difficulty to sort out the rank effect from other intricate peer effects even "randomizing identical students into many classes that are identical in every way except that they differ in terms of the distribution of human capital of their students", which is, in their view, "the best [one] could do." Our new identification strategy suggests a way to plausibly control for peer effects as it does not require variations in the ability (i.e., human capital) distribution of peers for identification.

In fact, our work stands out in five important respects. First, we propose a new strategy in which the rank disclosed to students can vary even in classes whose ability distribution is the same. We use two sets of scores, standardized test grades to measure underlying ability and class grades to construct a salient measure of rank, to exploit an additional source of variation to identify the rank effect. As class grades are random conditional on ability, rank can vary even after controlling for higher order peer effects.

Other papers use standardized test grades[3] to obtain a proxy for the actual class rank, inheriting the limitation underlined by Delaney and Devereux (2022). Thanks to our new data, we can also offer evidence that we can more completely control for ability and other peer effects, thus increasing confidence in interpreting the rank effect. Second, our measure of rank is more likely to be salient to students than one based on national test grades and, thereby, to credibly affect students' perceptions.[4] Third, we use administrative data at the class level, which allows us to account for unobservables, such as teacher quality, peer effects, or class size, as well as estimating the quality of the group of reference.[5] Fourth, we compare the rank effect to the group quality effect, allowing us to put the former's magnitude in perspective. Finally, we explore the mechanisms mediating the rank effect by uniquely exploiting a rich questionnaire administered to all Italian students in a given year, thus shoring up the external validity of our conclusions.

Our paper also speaks to the inequality literature. Most theories focus on absolute levels of resources, such as human capital accumulation, parental wealth or income (Lee and Seshadri, 2019; Becker et al., 2018; Becker and Tomes, 1986, 1979). Empirical research has shown that absolute levels, whether individual or of the reference group, affect later outcomes. Evidence on impacts of the quality of the reference group abounds. For instance, neighborhood quality impacts future earnings and college attendance rates (Chetty and Hendren, 2018a,b), kindergarten class quality influences future earnings (Chetty et al., 2011), and inter-generational mobility in the US is strongly dependent on geographical location (Chetty et al., 2014). Research has also highlighted the importance of individual level of resources. It has for example been shown that inherited wealth plays an important role in shaping income inequalities (Piketty, 2014; Piketty and Zucman, 2014). Hvidberg et al. (2022) argue that one's position within the income distribution can shape views on the fairness of inequalities in society. Carneiro et al. (2021) show that children whose parents have relatively high income in early-childhood and low income in middle-childhood fare better in terms of later education and income than children whose parents exhibit the opposite income profile. In this regard, our paper suggests a way to measure the

[3]Murphy and Weinhardt (2020) and Pagani et al. (2021) actually use grades obtained at end-of-year national exams rather than standardized test grades.

[4]Interestingly, Yu (2020) observes a "perceived rank" on a 5-point scale (1 indicating "among the lowest" and 5 "among the highest"), which is self-reported by students in a survey sample in China comprised of 2,472 individuals. He shows a moderate correlation ($\rho = 0.68$) between rank based on standardized test grades and self-perceived rank. While using the same identification strategy as in Murphy and Weinhardt (2020), he nonetheless uses this survey-based data to explore the role played by saliency in potential mechanisms for the rank effect.

[5]Other papers usually observe school-level data. Notable exceptions are Pagani et al. (2021) and Yu (2020), who have access to class-level data although based on surveys respectively comprised of 4,161 and 2,472 students.

impact of relative position in groups that are clearly defined and in which the comparison metrics are transparent. We are also able to compare this impact to that of the quality of the reference group while exploring plausible pathways through which the rank effect may play out.

Paper organization. The rest of the paper is structured as follows. Section 1 describes the Italian school system and Invalsi tests. Section 2 presents our data, the sample we use in the article as well as some details on the computation of key variables. Section 3 lays out our empirical strategy and our contribution to the rank literature. Section 4 presents our main result on the effect of rank on subsequent academic outcomes. Section 5 describes our main robustness checks. Section 6 compares the rank effect with the effect of class quality. Section 7 explores the mechanisms that may mediate the effect of the rank on student performance. Section 8 concludes.

1 Institutional Context

1.1 A Primer on the Italian School System

The Italian school system consists of three different ***sections***. *Primary school* (Grades 1 to 5) lasts five years and starts at age 6. *Middle school* (Grades 6 to 8) lasts three years and starts at age 12. *High school* (Grades 9 to 13) lasts five years and starts at age 14. While every primary and middle school student shares the same core curriculum across the country, there exist three main different types of high schools into which students can self-select: academic ("Liceo"), technical ("Istituto Tecnico") and vocational ("Istituto Professionale"). Students in the academic track can specialize into different sub-tracks: humanities ("Liceo Classico") and scientific ("Liceo Scientifico") are the most common but languages and art also exist. High-schools are usually specialized in one sub-track. The technical track can focus either on technological or business subjects. The vocational track is centered around a core subject (tourism, industrial process, agriculture, etc.). The academic track prepares students to go to university and is typically chosen by students coming from higher socio-economic background (Brunello and Checchi, 2007). The technical and vocational tracks include students from lower socio-economic backgrounds as well as a higher share of children of immigrants and less women (Table 1). Importantly, high-school track choice is strongly correlated with the probability of subsequently attending university: Carlana (2019) notes that only 1.7% of vocational high-school students enrolled in universities in 2016 while 32.3% of technical high-school students and 73.7% of academic high-school students did.

Some important features of the Italian educational system are worth highlighting.

First, in primary and middle school, class composition is determined by school principals and according to guidelines set by law. Specifically, within a school, classes should be balanced in terms of gender and socio-economic status. Principals usually seek to ensure comparability across classes and heterogeneity within classes (Carlana, 2019).

Second, class composition is *stable within sections*: from Grades 1 to 5, Grades 6 to 8 and grade 9 to 13 students remain with largely the same peers.[6] The reshuffle occurs when going from primary to middle school and from middle school to high school and is substantial. In our main dataset, a student has an average of 64% (IQR = 57%-78%) new peers in middle school and 82% (IQR = 78%-90%) in high school. This guarantees that our primary school rank is not correlated with rank in middle or high school, allowing us to claim that our primary school rank effect does not capture a contemporaneous rank effect.

Third, each class is assigned to a Math and an Italian teacher by principals. The teacher-students match is also stable within sections (Barbieri et al., 2011). Each individual teacher typically has a large amount of discretion in how to organize educational activities and grading their pupils. The grades we observe in Grades 5 are the average of grades obtained at a series of written and oral teacher-specific tests over the course of the first semester. They range from 0 to 10 by step of 0.5. Importantly, attendance, participation or a student's general behavior are evaluated by a distinct grade and should in principle not be factored in the Italian or Math grade.

Class grades and the resulting rank are thus class-specific measures. Students know their scores and how they rank within their class, or have a good sense thereof, as scores are usually publicly released to students at the end of each semester. In other words, class grades and ranks are very likely to be known to students and the most relevant metric for peer comparison.

1.2 Invalsi Test and Student Questionnaire

In the spring of each year, the National Institute for the Evaluation of the Italian Education System (Invalsi) administers standardized tests in Italian and Math in Grades 2, 5, 8, 10 and 13. The tests are presented to students as ability tests and are not compulsory. Hence, we do not get to observe 100% of the students but only those who took the test.

[6] Exceptions to class composition stability arise when a student repeats a year, moves in/out of the area or is transferred to another school by their parents (Carlana, 2019), which can be considered a marginal event. However, in Italy, grade repetition is almost non-existent in primary school and rare in middle school (Salza, 2022). It becomes considerably higher in high school: 16% of the students enrolled in Grade 10 in 2018 had repeated at least one year.

This point in developed further in Section 2. Importantly, except in Grade 8,[7] students are not informed about their performance on the test and test results do not play a role in middle school or high school enrollment determination (Carlana, 2019).

Their content is close to that of the OECD-Pisa test and consists in multiple-choice as well as open-ended questions (Angrist et al., 2017). Tests are proctored and graded by a different teacher than the one instructing students in the specific subject (Lucifora and Tonello, 2020). Grading is anonymous and follows a precise rubric that is common across the country (Carlana, 2019).

Since its implementation in 2009/2010, Invalsi test has been widely covered in the media and is looked at by parents when selecting schools,[8] as results have been made public since 2014. To discourage cheating, Invalsi devised an algorithm yielding a Cheating Propensity Indicator CPI_{cgs}, by class c, grade g and subject s (Lucifora and Tonello, 2020). If the CPI is above 50%, scores are not returned to classes and are excluded from the computation of the school average. If it is less than 50%, scores are "corrected" i.e., deflated by $(1 - CPI_{cgs})$ at the class level. Importantly, this "correction" is intended as a sanction to discourage cheating and is not a way to recover what students' performances would have been in the absence of cheating. In this paper, we keep classes with a cheating probability below 50% i.e., those which are more likely to have played by the rules than not,[9] and we also use non-corrected scores. In the paper, and in line with the literature, we turn raw Invalsi scores into percentalized scores, after excluding classes with a cheating probability larger than 50%.

In 2018, Invalsi administered a questionnaire on top of the test. It asked students about many topics related to school: parental support received in relation to their studies, confidence felt about Italian or Math, relationships with peers, career expectations, self-esteem, etc. The answers are numerical on a 6-point scale, with 1 indicating students strongly disagree with the statement and 6 that they totally agree.

[7]Grade 8 Invalsi tests are higher stakes as they have counted towards one-sixth of students' final score at the end of middle-school since 2017. That may explain why we find a slightly higher rank effect on Grade 8 standardized scores in the 2019 cohort than in the 2018 cohort, although not significantly different at the 5% level (p-value = 1.67).

[8]Primary and middle school choice is principally based on residency criteria, but parents can still choose among the set of local schools. The latter can thus deploy strategies to attract more students as resources are mainly a function of student enrollment. High-schools are free to compete to attract students (Lucifora and Tonello, 2020).

[9]The fraction of students in classes with a high likelihood of cheating at baseline is tiny: 90% of students are in classes with a cheating probability lower than 7% in Grade 5 and 8% in Grade 10. Our results are robust to using lower thresholds or if we restrict the sample to classes with a 0% cheating probability.

2 Data and Descriptive Statistics

In this paper, we mainly rely on two datasets in which we observe Italian students at three points in time. The first data covers the cohort that was in Primary school (Grade 5) in 2013, in Middle school (Grade 8) in 2016 and in High school (Grade 10) in 2018. The second dataset covers the following cohort i.e., from Grade 5 to Grade 10 over 2014-2019.

2.1 Overview of the Dataset

We observe all the students that satisfy two conditions: (i) being in High school (Grade 10) in 2018 or 2019 and (ii) sitting the Invalsi test that same year. As explained in Section 1.2, this test is not mandatory: Table 2 shows that 93.8% of Italian students in Grade 10 took it in 2018 and 93.7% in 2019. The dataset is then constructed in a backward way : data about their performances in Middle school (Grade 8) and Primary school (Grade 5) are retrieved, so that a student is always observed in High school but may or may not be observed in earlier Grades. In particular, we do not observe repeating students: a student who repeated a class will have taken six years to go from Grade 5 to Grade 10 instead of five years. If she is in the 2018 Cohort for instance, this implies that she was not in Grade 8 in 2015 but in Grade 7 and thus not observed in previous years.[10] The remaining missing students have simply not showed up to the test.

We have access to a variable that records the fraction of students that took the test by class so that we can back out the actual number of students that should have been observed in Grade 5 and Grade 8.[11] Table 2 shows that students of our dataset accounted for 89.8% of Grade 5 students in 2013 and 87.8% in 2014.

2.2 Class and School Characteristics

In Italy, class size is set by law. The *Decreto del Presidente della Republica 81/2009* set new bounds on class size in 2009/2010 but was rolled out one grade per year starting in Grade 1 (Angrist et al., 2017). The cohorts we observe are thus subject to the old regulation, established by the *Decreto Ministeriale 331/98*. In primary schools, classes

[10] This reasoning stands for any students that repeated or skipped a year or more.

[11] The actual number of students showed in Table 2 gives us an indication about the retention rate, as only grade repetition or skipping can explain it. We cannot observe if a student skipped a grade but it is quite rare in Italy. We can see thus see that the retention rate is approximately 15.9% between Grade 8 and Grade 10 and 21.2% between Grade 5 and Grade 10, which is in line with Salza (2022). Among students we observe in our sample, the retention rate is smaller: 12.5% between Grade 8 and Grade 10 and 15.7% between Grade 5 and Grade 10. This suggests that those who did not take the test in Grade 10 are on average more likely to have repeated a year and can be considered as weaker academically.

should not have fewer than 9 students and no more than 28 students.[12] Classes in schools located in "disadvantaged areas"[13] are not subject to these limits. Another reason explains why the lower bound is not reached by all classes: class size limits are to be satisfied at the beginning of each section: therefore, if a student leaves her school to go to another one, she will not be replaced and class size may fall below legal requirement. Finally, enforcement of the law is not perfect, as there is no sanction for schools that do no comply with it. Table 3 shows the distribution of class and school size by grades.

It is noteworthy that the high-school size distribution is very skewed. As school is compulsory until age 16, municipalities have to ensure that the different types of high-schools are accessible to local students. As middle school track is common to all students and there are three different types of high-school and many sub-tracks within each type, it is no surprise that there are a fair number of small high-schools with few classes within each.

2.3 Sample Selection

As our goal is to estimate the effect of class rank in primary school, measuring it correctly is crucial. Our dataset suffers from the fact that not every student is observed in Grade 5 and there is uncertainty about where missing students would have stood in both the ability distribution and the class score distribution. Table 4 shows that, in Grade 5, we observe on average more than 90% of the students per class.

To ensure the validity of our results, we proceed to some adjustments. We start by restricting our sample to classes for which we observe more than 90% of the students in Grade 5. Given the mean size of primary school classes, we miss one student on average. Second, we exclude Grade 5 classes whose size is below the 10^{th} percentile of the distribution in each cohort.[14] Third, we exclude primary schools with less than two classes satisfying the previous two criteria. Fourth, we remove classes in which we do not observe both Italian and Maths class grades for every student. This ensures that we accurately compute the class rank. Five, so as to keep a balanced sample, we remove students with at least one standardized score missing in Grade 8 and Grade 10. Table 5 shows the number of students, classes and schools that remain after selection. We are left

[12] The Decree requires that class size be between 10 and 25, with a discretionary margin of 10% below or above these limits.

[13] According to the Decree, these are schools in small islands, in mountain municipalities, in areas inhabited by linguistic minorities, in areas at risk of juvenile deviance or characterized by the significant presence of pupils with particular learning and schooling difficulties.

[14] This is mainly to avoid factoring in too small classes. Results are robust to the relaxation of this constraint.

with 178,764 students in the 2018 Cohort and 155,044 students in the 2019 Cohort.

The reason why we carefully select our sample based on Grade 5 characteristics is that Grade 5 is our pivot year. The only variables from Grade 8 and Grade 10 that we use are the percentalized standardized grades (defined in Section 2.4) that are computed from the non-restricted sample at the national level. Leaving out students from Grade 8 and Grade 10 is thus not going to bias our results. The remaining descriptive statistics and the main results are computed on this restricted sample.

As shown in Table 9, students in the restricted sample do not differ much from other students observed in Grade 5, alleviating concerns that our restriction could have led to select a specific sub-sample of students. Students in the restricted sample (Panel A) are slightly less likely to be immigrant than in the overall sample (Panel B) but are otherwise comparable on other characteristics. Notice that both Panel A and Panel B exhibit a mean socio-economic status (SES) of 0.1. This indicator is computed and standardized by Invalsi so that, at the population level, the mean is zero and the standard deviation is 1 (Masci et al., 2018).[15] Students observed in Grade 5 are thus, on average, of a slightly higher socio-economic background than the rest of the population, which is consistent with the fact that these are the students who did not repeat a year over the course of their studies.

Finally we observe in Table 6 that students in the restricted sample are more likely to end up in an academic high-school than in the unrestricted sample (Table 1), again consistent with the fact that we select students that are expected to be slightly better than the non-selected ones.

2.4 Scores

In this paper, we use two sets of scores: class grades (assigned by teachers) and standardized test grades (from Invalsi).

Regarding class grades, whose distribution is shown in Table 8, our premise is that missing students are at the bottom of the class grade distribution. As they are either those who repeated at least a year over the course of their study or who decided not to take standardized tests, this seems a sensible first assumption. To alleviate concerns about its validity, we nonetheless present three sets of robustness checks in Appendix C. First, we perform the analysis on the set of classes whose coverage is 100%. Second, we

[15] Invalsi computes the SES from three elements: parental years of education, parental occupational status and a measure of household possession, taking into account the number of books, access to the Internet, whether students have access to a dedicated desk to work, whether they have their own bedroom, etc.

drop the bottom 10% of students in each class whose coverage is 100% and rerun the analysis. Third, we assign random class grades to missing students,[16] recompute the rank within classes and run the analysis with the new ranks. All these checks yield results that are very close to our main results.

We percentalize standardized test grades, following the literature. Importantly, the percentalized standardized grades are computed before we proceed to sample selection so as to keep an accurate measure of where students fall in the national distribution. Prior to sample selection, the distribution of percentalized standardized grades should thus exhibit a uniform distribution in Grades 5, 8 and 10. Table 7 shows that, in Grade 5, standardized test grades are distributed uniformly. This indicates that, by restricting the sample, we do not leave out a specific part of the ability distribution at the national level. For the later grades, our sample restriction leads to a slightly skewed distribution. This is expected: the restricted sample leaves out any student who missed at least one standardized test or repeated at least a year i.e., those likely to be academically weaker.

2.5 Rank Computation

We now explain how we construct the measures of rank in primary school classes. The rank based on standardized test grades will only be used for robustness checks in Section 5. Whenever we mention rank without more precision, we mean rank based on Primary school class grades.

Following the literature,[17] we construct our measure of rank as follows:

$$R_{ics} = \frac{N_c - n_{ics}}{N_c - 1} \tag{1}$$

where n_{ics} is the ordinal rank of student i in class c and subject s and N_c is class c size. For instance, the best student in class c would have $n = 1$ and thus $R = 1$ while the weakest would have $n = N_c$ and thus $R = 0$. In case of ties, we assign the mean rank (Denning et al., 2021). This is the most neutral way of proceeding: this allows us to ensure the average rank of a class is the same as that of a class of similar size with no ties,[18] while not arbitrarily breaking ties. We thus use this rank measure in our main specification.[19]

[16] As far as standardized test grades are concerned, since they are computed according to a national scale, missing students at the class level is not a concern.

[17] See Delaney and Devereux (2022) for a review.

[18] If, say, there are 10 students in a class and no tie, the average rank will be 5.5. If every student got the same score and we assign them the mean rank, the average rank will be 5.5 too. Assigning them the top or bottom rank would lead to an average rank of 1 or 10 respectively.

[19] We discuss this point more at length in Appendix B in which we show that we are likely to estimate a lower bound of the true effect.

Notice that, if we observed every student and there was no tie, the way we construct our rank variable would make it a uniformly distributed variable whose support is $(0, 1)$.

Figures 1 shows the distribution of our class rank measure, based on class grades. Figure 2 shows a similar distribution but for the rank measure based on standardized grades. As expected, both are very close to a uniform distribution but are slightly skewed. This is because we consider that unobserved students rank are at the bottom of the class (see Section 2.3).

3 Empirical Strategy

Our goal is to estimate the effect of the rank on subsequent academic achievements. To do so, we draw on the seminal work of Murphy and Weinhardt (2020), which sparked the development of an active literature relying on their empirical strategy (Denning et al., 2021; Elsner et al., 2021; Rury, 2022). To make our contribution in this area clearer, we shall first briefly describe this strategy and recall what the main assumptions are. We shall then compare it to the ideal experiment and explain how our dataset allows us to get closer to it.

3.1 Statement of the Problem

The identification strategy developed by Murphy and Weinhardt (2020) relies on variations in ability distributions across classes. They assume that student i in class c gets a rank R^S_{ic} which is a function of her ability a_i and the class ability distribution $F_c(.)$:

$$R^S_{ic} = g(a_i, F_c(.)) \tag{2}$$

Their identification strategy relies on the fact that two identically able students can end up with different ranks if the distribution F_c they face is different. If F_c is random, one can observe variations in R_{ic} conditional on a_i and measure a rank effect.

In its most general form, the problem can be stated as follows:

$$S^g_{ics} = f(R^S_{ics}, a_i, \theta_{cs}, x'_i\gamma) + \varepsilon_{ics} \tag{3}$$

where for student i in class c studying subject s, S^g_{ics} is the percentalized standardized score in grade $g \in \{8, 10\}$, a_i her ability, θ_{cs} a class-by-subject fixed effect and x_i some observables (gender, socio-economic status..). Importantly, in the rank literature, R^S_{ics} is a proxy for the class rank in primary school, as it is computed from primary school standardized test grades and not class grades actually assigned to students by teachers.

Any estimation requires to impose some structure on f. In particular, it is common to rewrite Equation 3 as:

$$S^g_{ics} = \alpha + \beta R^S_{ics} + h(S^5_{ics}) + \theta_{cs} + x'_i\gamma + \varepsilon_{ics} \tag{4}$$

where h is a polynomial and percentalized standardized test grades in Primary school, S^5_{ics}, are used as a measure of ability. Claiming that the rank effect is identified in Equation 4 requires:

$$\mathbb{E}[\varepsilon_{ics}|R^S_{ics}, a_i, \theta_{cs}, x'_i\gamma] = 0 \tag{5}$$

Equation 5 assumes that the rank is uncorrelated with student's unobservables or class features that are not accounted for by the inclusion of the class fixed effect. The latter only controls for class-level shocks (such as disruptive peer, teacher quality..) that affect all students in a similar fashion. But, as pointed out by Denning et al. (2021), this specification does not account for higher-order peer effects.

As an example, consider classes 1 and 2. Both have the same mean in abilities but the variance of 1 is lower than 2's, as depicted in Figure 3. Consider students X and Y, respectively in class 1 and 2, both with ability A points above the mean. Then the rank of X is higher than Y's (Figure 3(a)). On the contrary, if both have ability A points below the mean, X's rank is lower than Y's (Figure 3(b)). To avoid omitted variable bias, one has to account for the effect of the interaction between the variance of the class ability distribution and a student's ability, since it impacts the rank differently depending on ability. But this does not suffice: this problem arises for any moment of the distribution and could be wholly tackled only by constraining the two distributions to be identical. But holding both individual ability a_i and the class ability distribution F_c fixed, effectively bars identification since this precludes any variation in R^S_{ic} from Equation 2.

3.2 Our Contribution

Ideally, one would look at two classes 1 and 2 in which the ability distribution would be the same. How each student ranks in the ability distribution of their class would be unknown to them but known to the econometrician. The rank disclosed to students would be a noisy function of their ability, thereby introducing randomness in the rank and leading similarly able students in classes 1 and 2 to end up with different *visible* ranks.

At this point, it is worth emphasizing again that all the recent rank literature only deals with one set of scores that is used both to measure ability and to rank students. As standardized test grades and the resulting ranking are unlikely to be known to students, the rank measure is taken as a proxy for the "true" rank computed from class grades (Denning et al., 2021). In contrast, our observing two sets of scores, one coming from standardized tests and the other being assigned by teachers gives us greater flexibility and may allow us to mimic this experiment. Indeed, we can now dissociate the measure of ability from the measure of rank.

As explained in Section 1, standardized test grades are unknown to students and graded externally according to a very precise rubric that is common across the country (Carlana, 2019). In addition to student fixed effects, they serve as a measure of subject-specific ability.[20] We contend that they standardized test grades constitute an appropriate measure of subject-specific ability. First, standardized tests in primary school have no stake: they are conducted for informational purposes and the results do not matter to students. It is highly unlikely students cram for it, their performance thus better reflecting their underlying ability. Second, they are graded anonymously and by teachers that do not instruct the students they grade, making grades free of teacher bias as well as comparable across the country.

Class grades are known to students and are used to construct the measure of rank. The variation we rely on to identify the rank effect is now two-dimensional: it originates from both the variation in ability distributions and the variation in class grade distributions due to teachers.

We can thus identify a rank effect, even accounting for higher-order peer effects. Class grades are teacher-specific and, as such, can be considered as random, conditional on ability. The randomness indeed comes from two sources. First, it can stem from an idiosyncratic mapping from abilities to grades that is teacher-specific. Class grades and ranks could differ because teachers vary randomly: they can be lenient (say, give 10 to everyone) or strict (e.g., give a low maximum grade), irrespective of their students' abilities. Second, teachers' mapping from ability to grades is noisy and may incorporate information unobservable to the econometrician. The more elements are taken into account to determine grading, the more discretion teachers have.[21] As explained in Section 1, the grades that we observe are the averages of oral and written tests that are taken all along the first semester. It is likely that a teacher's judgement of a student is affected by the latter's behavior and influences the grading, especially regarding oral evaluations. We thus consider that the rank based on class grades is randomly assigned, conditional on ability.[22] In Section 5, we present robustness checks to rule out concerns that (i) class

[20]One might be worried about the temporal mismatch, as standardized tests are taken in the second semester while we observe class grades from the first semester. We show in Appendix E that this does not affect our results.

[21]Dee et al. (2019) show for instance that teachers tend to make use of that discretion.

[22]As students are randomly assigned to classes and teachers *at the beginning* of primary school, there can possibly be no selection based on how strict or lenient a teacher would be perceived in her grading.

grades would be a more accurate measure of ability than standardized test grades and (ii) that our ability controls do not suffice to ensure the rank based on class grades is orthogonal to ability.

Finally, our empirical strategy crucially relies on the fact that, conditional on ability (as measured by standardized test scores) and class scores, there is enough variation in rank. Figures 4 and 5 show this is the case.

3.3 Other Threats to Identification

The Italian setting allows us to bypass another major source of concern: non-random sorting of students. Following Denning et al. (2021), we can distinguish between active and passive sorting. Active sorting would transpire if students could self-select into classes on the basis of their expected ranks. Passive sorting would occur if students with certain characteristics would systematically enroll in classes whose ability distributions are different. In both cases, there would be a risk of omitted variable bias.

As Italian students are by law randomly allocated to classes and teachers at the beginning of primary school, both types of sorting are most improbable. To provide suggestive evidence that no sorting occurs we perform Pearson Chi-Square tests on dimensions that we can observe: gender, socio-economic status (SES) and standardized test grades in primary school. Results are shown in Appendix A and shores up the claim that students do not sort into classes in primary school.

3.4 Main Specification

The main specification reads:

$$S_{ics}^{g} = \alpha + \beta R_{ics} + h(S_{ics}^{5}) + g(C_{ics}) + \theta_{cs} + \gamma_i + \varepsilon_{ics} \tag{6}$$

where for student i in class c and subject s, S_{ics}^{g} is the percentalized standardized test grade in Grade $g \in \{5, 8, 10\}$, R_{ics} is the class rank in primary school, C_{ics} is the class grade in primary school, h and g are quartic polynomials,[23] θ_{cs} a primary school class-by-subject fixed effect and γ_i a student fixed effect.

Notice that this specification resembles the standard one in the literature but differs from it in three important ways. First, we now use a rank computed from class grades. It is likely to be salient to students and thus to actually affect their perceptions. Second, we control for class grades to account for the absolute effect of the grade *per se*, as students also compare their grades to the absolute scale. Failing to do so may lead to omitted

[23] Our results are robust to the use of polynomials of other orders.

variable bias if higher grades have in themselves a positive effect on future outcomes (e.g., it is more rewarding to get a 8 than a 6, which may impact students' perceptions irrespective of the rank). Third, in addition to the percentalized standardized score in Grade 5 that we use to control for subject-specific ability, we add a student fixed effect to beef up our control for unobserved ability across subject.

4 Results

4.1 Effect of the Rank on Subsequent Academic Outcomes

Our main results are presented in Table 10. The effect of the rank measured from class score is large: table 10 shows that, *ceteris paribus*, ranking at the top of class compared to bottom in the last year of primary school leads to a gain of 8.2 percentiles in the national standardized grade distribution in middle school and 7.6 in high school. This is equivalent to moving up by about 35,000 ranks nationally.

This translates into a potent impact in terms of standard deviation of the distribution: all else being equal, going from the bottom to the top of the class causes a gain of almost a quarter of a standard deviation in the standardized test grade distribution at the national level (0.23 in middle school and 0.28 in high school).

4.2 Non-Linear Effects of the Rank

To explore potential non-linear rank effects, we run the following specification:

$$\begin{aligned} S_{ics}^{g} = \alpha + & \sum_{\substack{1 \leq k \leq 20 \\ k \neq 10}} \beta_k \mathbf{1}[d_{ics} = k] + \beta_{top} \mathbf{1}[R_{ics} = 1] + \beta_{bot} \mathbf{1}[R_{ics} = 0] \\ & + f(S_{ics}^{5}) + h(C_{ics}) + \theta_{cs} + \gamma_i + \varepsilon_{ics} \end{aligned} \tag{7}$$

where d_{ics} is the ventile of student's i ordinal rank in class c and subject s.

The reference group is the 10th ventile (i.e. 45-50th percentiles). We observe that the effect of the rank is linear across the distribution, although coefficients around the median are more imprecisely estimated. This suggests that the rank may be less salient to students that stand in the middle of the class distribution.

We further investigate heterogeneous rank effects by estimating Equation 7 on different subset of students. First, we compare the impact of the rank on women and men. Figure 7 shows that they appear to be affected similarly, although the negative impact of very low rank seems to be attenuated for women. Second, we look at how immigration status might affect the rank effect. Again, Figure 8 shows that no there is no significant difference, even if point estimates suggest that the rank effect might be exacerbated for immigrants, i.e., they benefit more from being at the top and suffer more from being at the bottom. Finally, we compare students whose SES is below the national median and those whose SES is above. As shown on Figure 9, we do not detect any significant differences.

5 Main Robustness Checks

5.1 Is Class Rank Orthogonal to Ability?

In spite of our novel identification strategy and the resulting new specification, not being able to perfectly observe ability remains a challenge. Indeed, if standardized test grades are a noisy measure of ability, it could still be the case that more able students obtain a higher rank. As they are more likely to perform better in subsequent grades, this would lead to a biased rank estimate. We suggest two main robustness checks to address this concern.

The first robustness check addresses the concern that the rank based on class grades might better capture ability than the rank based on standardized test grades while proving that the former operates through a channel that affects students' perceptions and is orthogonal to ability. The second robustness check offers additional evidence that our controls adequately capture ability by showing that the rank variable is uncorrelated with previous measures of ability.

5.1.1 Visible vs. Invisible Ranks

This robustness check exploits the symmetry of two ranks which differ only to the extent that one is visible and the other invisible. Indeed, we observe two scores for each student: one that is never reported to students (standardized test grades) and one that is reported to students (class grades). Both of these scores are naturally correlated with students' underlying abilities and preparation, and hence may be directly related to students' later outcomes. They result in two ranks: one that is *visible* (rank based on class grades) and one that is *invisible* (rank based on standardized test grades). Intuitively, the rank visible to the student may affect his or her performance by changing their own perceptions and behaviors, while the rank invisible to the student will serve as a placebo test to ensure that our other controls adequately capture any remaining relationship between students' ranks and later outcomes.

We start by checking that, absent any control for ability, the visible and invisible ranks affect later outcomes in the same way. We estimate the following specification:

$$S^g_{ics} = \alpha + \beta R_{ics} + \delta R^S_{ics} + \theta_{cs} + \varepsilon_{ics} \tag{8}$$

where notations are the same as in Equation 6 and R^S_{ics} is the rank based on standardized test grades for student i in class c studying subject s.

Panel A of Table 11 reports $\hat{\beta}$ and $\hat{\delta}$ estimated from Equation 8. We observe that coefficients are almost the same, thereby ruling out concerns that the rank based on class grades may better capture ability than the rank based on standardized grades.

We then turn to look at how coefficients on both ranks change when we include the controls of the main specification. We thus run the following:

$$S^g_{ics} = \alpha + \beta R_{ics} + \delta R^S_{ics} + h(S^5_{ics}) + g(C_{ics}) + \theta_{cs} + \gamma_i + \varepsilon_{ics} \tag{9}$$

Panel B of Table 11 reports $\hat{\beta}$ and $\hat{\delta}$ estimated from Equation 9. We observe that the estimate the effect of invisible rank is either close to 0 and imprecisely estimated (Column (1)) or greatly reduced (Column (2)). In contrast, the estimate of the effect of the visible rank is the same as the one resulting from the main specification (Table 10). This suggests that, once we include our controls, the visible rank is orthogonal to the invisible rank.

We also estimate the following non-linear specification:

$$\begin{aligned} S^g_{ics} = \alpha &+ \sum_{\substack{1 \le k \le 20 \\ k \ne 10}} \beta_k \mathbf{1}[d_{ics} = k] + \beta_{top}\mathbf{1}[R_{ics} = 1] + \beta_{bot}\mathbf{1}[R_{ics} = 0] \\ &+ \sum_{\substack{1 \le k \le 20 \\ k \ne 10}} \delta_k \mathbf{1}[d^S_{ics} = k] + \delta_{top}\mathbf{1}[R^S_{ics} = 1] + \delta_{bot}\mathbf{1}[R^S_{ics} = 0] \\ &+ f(S^5_{ics}) + h(C_{ics}) + \theta_{cs} + \gamma_i + \varepsilon_{ics} \end{aligned} \tag{10}$$

where notations are the same as in Equation 7 and where d^S_{ics} is the ventile of student's i standardized grade rank in class c and subject s. Figure 10 plots $\hat{\beta}_k$ and $\hat{\delta}_k$. We observe a pattern that is consistent with the average effect estimated previously: in Grade 8, the effect of the invisible rank is non-significant at every ventile. In Grade 10, the invisible rank is significant above the 15th ventile, although the estimated effect remains negligible compared to that of the visible rank. This could be due to a slight correlation between the two ranks at the upper tail, as a student who ranks high in the standardized test grade distribution of her class is unlikely to rank low in the class grade distribution.

To summarize, we start from two totally symmetric scores, except that one is visible to students while the other is not. At baseline, without any control, both ranks have the same effect on subsequent outcomes, suggesting that they both convey the same measure of ability. Once we introduce controls for ability and class grades, the rank based on class grades (the visible rank) still exhibits a strong effect while the effect of the rank based on standardized grades (the invisible rank) vanishes away. As any omitted variable (particularly ability) would not exhibit this asymmetry aspect, this is evidence that the rank based on class grades is operating through a channel that changes students' perceptions and is orthogonal to ability.

Finally, the coefficient estimates of the rank based on class grades do not change before and after the inclusion of the rank based on standardized test grades, suggesting that our main estimates adequately capture the effect of students' change of perceptions.

5.1.2 Is Rank Correlated with Ability?

This robustness check uses a companion dataset, in which we observe students in Grade 2 in 2013, Grade 5 in 2016 and Grade 8 in 2019. This allows us to retain Grade 5 class rank as a pivot while ensuring its suitability as a rank measure uncorrelated with ability and checking the stability of our results on another cohort. We proceed to the same selection as for the main dataset (see Section 2.3).

First, we want to make sure that, unconditionally, Grade 2 standardized grades convey some measure of ability. Figure 11 plots the unconditional mean and standard deviation of Grade 5 class rank as a function of ventiles of Grade 2 standardized test grades. We see that Grade 5 class rank and Grade 2 standardized test grade exhibit a strong and linear relationship.

We then turn to estimate the correlation between Grade 5 class rank and Grade 2 standardized test grades, once we account for all the controls of our main specification. We thus estimate Equation 6 using Grade 2 standardized grades as the dependent variable. Results are displayed in Table 12. Column (1) of Table 12 shows that the coefficient on Grade 5 class rank is both close to zero and imprecisely estimated (p-value = 0.42): the rank variable is uncorrelated with a previous measure of ability, assuaging concerns that it would still capture some ability. On the other hand, Column (2) of Table 12 shows that the effect of the rank on Grade 8 standardized scores is stable: the estimate on this cohort is not significantly different from the coefficient we measured in Column (1) of Table 10 at the 5% level (p-value = 0.05).[24]

[24] The slight difference between the two coefficients may stem from a composition effect: our main sample is restricted to students that have not repeated a year between Grade 5 and Grade 10 and accounts for 40% of the actual number of students in Grade 5 in years 2013/2014. Most students repeat a year between Grade 8 and Grade 10 so this robustness analysis effectively includes students that might be academically weaker (the robustness analysis sample accounts for 57.5% of the actual number of students in Grade 5 in 2016).

5.2 Controlling for Ability Distribution

One of the main contributions of this paper is that we can estimate a rank effect while accounting for higher-order peer effects. To investigate how much of the rank effect is driven by the shape of the ability distribution (as measured by standardized test grades), we run the following specification:

$$S^{g}_{ics} = \alpha + \beta R_{ics} + \left(\sum_{k=2}^{7} \beta^{(k)} m^{(k)}_{cs} \right) \times R_{ics} + h(S^{5}_{ics}) + g(C_{ics}) + \theta_{cs} + \gamma_i + \varepsilon_{ics} \quad (11)$$

where m^{k}_{cs} is the moment of order k of the ability distribution in subject s and class c. We then look at how the rank effect is affected. To do so, we re-evaluate it for different values of these seven moments: mean, 25th, 50th and 75th percentiles.

As shown on Table 13, the effect of the rank largely remains in the same ballpark. We can thus be confident that the effect we measure is not mainly driven by the interaction between individual ability and the class ability distribution.

6 Rank Effects and Class Effects

We have shown that the rank effect is significant and provided evidence that it is orthogonal to ability and captures a change in students' perceptions. In this section, we seek to quantify the importance of the rank effect relative to the class quality effect. To extend the metaphor, we have estimated that, conditional on being in a miserable village or in Rome, it is better to be higher ranked. We now want to compare this effect to the one of being in a miserable village compared to being in Rome, conditional on the rank.

To do so, we resort to a strategy similar to Chetty et al. (2011): we use the class-by-subject fixed effect estimates from Equation 6 as a measure of all class inputs (peer effects, teacher effects, size, amenities..) affecting subsequent academic performances. As shown in Figure 12, the distribution of primary school class fixed effects on middle school performances is very close to a normal (Figure 12a). This is not quite the case regarding primary school class fixed effects on high school performances but the QQ-plot shows that the distribution does not exhibit outliers (Figure 12b).

As shown in Table 14, increasing class quality by one standard deviation is associated with a gain of 11.8 national percentiles in the standardized test grade distribution in Middle school and 12.8 in High school. On the other hand, an increase in class rank by one standard deviation results in a gain of 2.2 national percentiles in the standardized test grade distribution in Middle school and 2.0 in High school. Therefore, the rank effect corresponds to about 15-20% of the class quality effect: to compensate a decrease in one standard deviation in class quality, the class rank would need to increase by five standard deviations. As the class rank follows a uniform distribution with support on (0,1), this means that the negative impact of a one standard deviation decrease in class quality cannot even be compensated by going from bottom to top of the class.

What drives Primary school class quality? To offer a tentative answer, we look at the correlation between class quality and the following observable characteristics at the class level in Primary school: fraction of women and immigrants, size, and average student socio-economic status.[25] Table 15 reports the results, after we standardized all variables. We observe that class quality is strongly and positively correlated with the average student socio-economic status in the Primary school class. The fraction of women appears to be slightly positively correlated to class quality while the fraction of immigrants is negatively correlated with performances in Middle school but positively correlated with performances in High school. Lastly, we observe that class quality is also positively correlated to class size. We conclude that Primary school classes that are more gender-diverse and where peers come from a higher socio-economic background ensure higher performances in the future.

To highlight potential trade-offs more concretely, we look into how a student's class rank in Primary school relate to the socio-economic status of her peers. Table 16 shows the correlation between class rank and the class mean SES, controlling for student's Primary school standardized test grade and SES.[26] We observe that an increase by one standard deviation in the class mean SES is associated with a decrease by 0.13 standard deviation in student's class rank. Table 17 shows that, controlling for student's Primary school class rank, standardized test grade and SES, an increase by one standard deviation in the class mean SES leads to a gain of 2.44 percentiles in the national distribution of standardized test grades in Middle school and 3.66 percentiles in High school. Using estimates from the previous part, we can conclude that, all else being equal, increasing the class mean SES by one standard deviation yields a net gain of 2.15 percentiles in the national distribution of standardized test grades in Middle school and 3.40 in High school. This result, which echoes what sociology has highlighted for long (Mayer and Jencks, 1989; Meyer, 1970), is interesting in two respects. It suggests that there may be a relative deprivation effect of sorts: conditional on one's SES, having peers of higher SES is detrimental to one's class rank. However, this effect is dwarfed by the overall gain of interacting with higher SES

[25]We do not include class mean standardized test grades in Primary school as it is impossible to disentangle what stems from peers' baseline abilities from what is due to a teacher effect. However, when we include the class mean standardized test grades, we observe a strong and positive correlation with class quality.

[26]Notice that data about SES is missing for 9,157 students in our main sample, hence the fact that the regression is run on 649,302 observations only.

students.

7 Mechanisms

We have shown that there is a trade-off between class rank and class quality. The latter matters more in magnitude but the former still has a sizable effect. We now turn to investigate what mechanisms may mediate the rank effect.

7.1 Sorting in Middle School and High School

We first set out to analyze the influence of class rank in primary school on choices of middle and high schools. Since 2014, schools are legally required to make their standardized test grades public at the school level and this has become a way for schools to attract students and increase enrollment (Lucifora and Tonello, 2020). An important feature of the Italian education system is that students self-select into middle and high schools. Middle school enrollment is mainly based on residency criteria but students can still choose among different local schools. They have much more leeway in selecting their high-schools, by type (academic, vocational or technical) and location. High-schools are also specialized: for instance, academic high-schools can be focused on either humanities or science. Importantly, high-schools usually do not discriminate on the basis of grades, unless there is an oversupply of students.

To construct a measure of school quality, we use an additional datasets, which closely resembles the one we use for our main analysis. It tracks students from an earlier cohort, which was in Grade 8 in 2014, Grade 10 in 2016 to Grade 13 in 2019. Our measure of school quality is constructed using standardized test scores in 2014 for middle schools and 2016 for high schools. We compute the average by school and subject and percentalize the resulting measure so that our school quality metric lies between 0 and 100. Importantly, following Invalsi guidelines in computing the scores that then made public, we exclude classes with a probability of cheating larger than 50% from the computation at the school level.[27]

We estimate Equation 6 with school quality by subject as the dependent variable. Results are displayed in Table 18. As expected, the rank has little effect on the quality of middle schools but a sizable one on the quality of high-school: going from last to first in one's primary school class leads to attending a high-school whose average achievement at standardized tests is 4.5 percentiles higher.

[27] This results in 35,486 students missing from our main sample. Their schools were excluded from this analysis as they were not allowed to publicly release their standardized test results.

7.2 Motivation

To further explore motivation mechanisms, we exploit students' answers to the *Questionario di Contesto*. As described in Section 1, this questionnaire covers several main topics: how students perceive school, how interested they are in Italian and Math, the amount of support they receive from their parents, etc.[28]. On each main topic, students are asked several questions to which they have to answer using a six-point Likert scale. We average the scores by topic[29] and percentalize them at the national level, prior to dropping any student.

Importantly, only questions about subject interest are subject specific, allowing us to use the main specification (Equation 6) with student fixed effects. For the other channels, we will have to use a version of the main specification without student fixed effects:

$$M^{10}_{ic} = \alpha + \beta R_{ics} + h(S^5_{ics}) + g(C_{ics}) + \theta_{cs} + x'_i\zeta + \varepsilon_{ics} \tag{12}$$

where for student i in class c studying subject s, M^{10}_{ict} is the percentalized average score to questions on topic t in Grade 10, h a quartic polynomial, θ_{cs} a class-by-subject fixed effect and x'_i are students' observables (gender, socio-economic status, immigrant status). We look into the effect of the rank in Italian and Math separately on each topic.

This questionnaire was administered only in 2018 due to the uproar it stirred in the Italian media and the negative press coverage it received. Therefore, we restrict our analysis of motivation to the 2018 cohort. We apply the same criteria of sample restriction as described in Section 2.3, with two modifications: we now keep students even if we do not observe Grade 8 standardized scores and we drop those for whom we do not observe scores for each topic of the Questionnaire.

Table 19 shows that the restricted sample is comprised of students with a slightly higher SES on average and that are less likely to be immigrant and more likely to be women. The reason may be that students included in the restricted sample are those observed also in Grade 5 and thus substantially less likely to have repeated a year. For reference, we first run Specification 6 on the new sample to verify that the effect of the rank on Grade 10 standardized scores is in line with our main estimates. We find that going from last to first of one's primary school class leads to a 6.4 gain in national percentiles in Grade 10 standardized scores. This coefficient is not statistically different from the main estimate (p-value = 0.22).

[28] The detail of the questions is given in Appendix F.

[29] Our results are robust to using the sum of scores by topic as dependent variable.

7.2.1 Subject Confidence

In this section, we explore how the rank in each subject affects students' interest and confidence in Italian and Maths. As questions are similar for both subjects, we can keep on using Specification 6. We will also compare the results with those obtained using Specification 12 to gauge whether we can infer anything meaningful from regressions that do not include student fixed effects. Figure 13 shows that the distribution of Percentalized Subject Confidence Score distribution is slightly skewed compared to a uniform distribution. This is also expected given the selection of the sample of analysis (students that are likely better at school than those who are left out, and thus probably more interested in subjects studied).

The rank seems to have a very strong effect on the confidence of student in the subject studied. Going from first to last of one's primary school class leads to a 3.9 gain in percentiles in the national distribution of subject confidence scores. Coefficients with or without student fixed effects are not significantly different (p-value = 0.8). Interestingly, being an immigrant or a woman seems to be associated with a substantially higher subject confidence. The coefficient on socio-economic status is as expected.

7.2.2 Parental Support

We now turn to explore how primary school ordinal rank affects parental support. A priori, the effect is not obvious. One may expect that students with higher rank would prompt their parents to provide them with more support, due to higher returns. It could also be that parents are less involved with higher ranked students because they feel they do not need to be encouraged. Parents may also be induced to support their children more strongly if they perceive them as struggling in school. As shown in Table 22, a higher rank appears to be negatively correlated with parental support, although the coefficient is imprecisely estimated.[30] Women and students from higher socio-economic background appear to receive more parental support while immigrants significantly less.

This result prompts us to explore non-linearities in the rank effect on parental support. We estimate Equation 7 using the degree of parental support as dependent variable. Figure 14 shows that coefficients remain imprecisely estimated all along the rank distribution. However, a pattern is noticeable regarding rank in Math: it seems that parents of students ranking higher in Math support them less. This would be consistent with parents estimating they do not need to help successful children with a subject that may look arduous

[30] Results do not change when we condition on parents' occupation or education.

7.2.3 Self-Esteem

One channel through which class rank in Primary school may have a lasting impact on later outcomes is confidence about one's intellectual abilities. Indeed, rank at an early age can shape one's conceptions about one's intellectual worth. Table 23 shows that, in Italian or Math, a higher class rank leads to significantly higher levels of self-confidence, with the rank in Italian being twice as more impactful as the rank in Math. Furthermore, being an immigrant does not significantly affect self-esteem but being a woman is associated with lower levels of confidence. Students with a higher socio-economic status also display significantly higher levels of self-confidence.

7.2.4 Peer Recognition

As class rank is publicly known among students, it may be a metric used to compare to each other beyond school achievements. The standing associated with one's class rank may thus affect a student's relationship with her peers.

Table 24 shows that it is the case. Primary school rank appears to significantly and positively impact students' relationships with their peers. Again, the rank in Italian seems to have more impact than the rank in Math. We also observe that being women or an immigrant leads to reporting significantly lower levels of peer recognition. In contrast, higher SES is correlated with higher peer recognition.

7.2.5 Perception of School

The last mechanism we explore is the perception that students have from school. Questions related to this topic have two different scales: some questions are negative, with the highest score indicating that students have a hard time at school, and some are positive, with the highest score indicating that students enjoy their time at school. For this reason, we group them into two categories: "bad perception" and "good perception".

Interestingly, Table 25 shows that a high rank in Maths does not lead to a better perception of school, while a high rank in Italian does. This may be related to the fact that Italian is perceived as the main subject in Italy. But overall, the rank leads to a significant increase on good perception and a significant decrease on bad perception.

8 Conclusion

In this paper, we contribute to the inequality literature by showing that, ceteris paribus, relative position impacts future outcomes. We estimate that student class rank in primary school has a strong effect on later academic achievements in middle school and high school. The rank operates through channels affecting students' perceptions, enhancing their interests in the subjects studied, their self-confidence and their career prospects, as well as prompting them to sort into better high schools. Although the quality of the group of reference matters more than one's relative position within that group (the rank effect amounts to about 20% of the class quality effect), the rank effect is too sizable to be neglected.

Our main contribution is to define a new way of estimating the effect of relative position. We exploit unique features of the Italian public school setting to suggest an identification strategy relying on random variations in the rank while allowing to control for individual ability and higher-order peer effects.

As we find that the rank effect exhibits non-linearities, our results point to possible policies. One might think of disclosing ranks only for top students, to retain the positive effect of ranking high while not discouraging weaker students. Surveying students to understand how salient their rank is and whether misperception might matter as well as conducting a randomized control trial to understand how rank disclosure policies might affect students' perceptions and future outcomes appear to be promising avenues for future research.

More generally, our work suggests that relative position effects should be taken into account when thinking about inequalities. It remains to be seen whether rank especially matters in the school system or its effects are comparable in other context, such as income comparisons.

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9 Tables

9.1 Institutional Context

Table 1: Students' Characteristics by High School Type

	High School Type		
	Academic	Technical	Vocational
Demographics			
Socio-Economic Status	0.32	-0.20	-0.58
	[0.95]	[0.92]	[0.94]
% of Immigrant	0.14	0.23	0.35
	[0.34]	[0.42]	[0.48]
% of Women	0.60	0.31	0.43
	[0.49]	[0.46]	[0.50]
Observations			
Number of Students	526,747	299,809	163,631
Fraction	53.2%	30.3%	16.5%

Note: This table shows the fraction and characteristics of students enrolling into the three main types of high-school. The measure of Socio-Economic Status is a continuous variable with higher values indicating a higher socio-economic background. It is described in more details in footnote 12 of Section 2.3. Note that the total number of observations for the 2018 and 2019 cohorts is 997,555 but data about high-school track is missing for 7,368 students.

9.2 Data and Descriptive Statistics

Table 2: Standardized Test Attendance

	Panel A: 2018 Cohort		
	Grade 5 (2013)	Grade 8 (2015)	Grade 10 (2018)
Actual Number of Students	413,596	442,971	529,039
Observed Number of Students	371,574	407,211	496,331
Fraction Missing	10.2%	8.1%	6.2%
	Panel B: 2019 Cohort		
	Grade 5 (2014)	Grade 8 (2016)	Grade 10 (2019)
Actual Number of Students	424,668	452,153	535,132
Observed Number of Students	372,668	419,239	501,224
Fraction Missing	12.2%	7.3%	6.3%

Note: The actual number of students is the number of students that could have been observed if all Italian students had taken the test and none of them had repeated or skipped a year. It is compared with the number of students observed every year in our dataset. The increase in the number of students over time is due to repetition: as the likelihood of repeating a year is higher in middle and high school, the number of students in a given year mechanically increases in higher grades. See Section 1 for details.

Table 3: Class and School Size Distribution

	Mean	Std. Dev.	25th Perc.	Median	75th Perc.
Primary School (Grade 5)					
Actual Class Size	14.6	5.0	11	15	18
Observed Class Size	12.9	4.8	10	13	16
Actual School Size	60.0	34.2	32	58	83
Observed School Size	53.3	30.7	29	51	73
Number of Classes Per School	4.1	2.1	2	4	6
Middle School (Grade 08)					
Actual Class Size	16.6	4.7	14	17	20
Observed Class Size	15.3	4.8	12	15	19
Actual School Size	77.2	47.6	46	69	98
Observed School Size	71.3	45.0	41	63	91
Number of Classes Per School	4.6	2.4	3	4	6
High School (Grade 10)					
Actual Class Size	20.5	5.1	18	21	24
Observed Class Size	19.2	5.4	16	20	23
Actual School Size	134.2	99.1	24	139	204
Observed School Size	125.8	94.7	23	128	192
Number of Classes Per School	6.6	4.4	1	7	10

Note: This Table reports the distribution of class and school sizes per Grade. See Section 1 for details.

Table 4: Class Coverage

	Mean	**Std. Dev.**	**Min**	**p25**	**Median**	**p75**	**Max**	**N**
Panel A: 2018 Cohort								
Grade 5	91.3%	8.7	5%	87%	93.3%	100%	100%	28,825
Grade 8	92.8%	7.5	27.3%	89.5%	95%	100%	100%	26,748
Grade 10	93.4%	9.7	4.2%	90.5%	95.8%	100%	100%	25,723
Panel B: 2019 Cohort								
Grade 5	90.1%	9.8	4.2%	84%	90.9%	95.7%	100%	28,649
Grade 8	93.5%	6.8	25%	90.5%	95.2%	100%	100%	27,130
Grade 10	93.2%	9.7	3.6%	90%	95.7%	100%	100%	26,226

Note:

Table 5: Characteristics of the Selected Sample

	Number of Students	Number of Classes	Number of Schools
Panel A: 2018 Cohort			
Grade 5	178,764	13,461	4,230
Grade 8	178,764	21,855	5,144
Grade 10	178,764	24,220	3,557
Panel B: 2019 Cohort			
Grade 5	155,044	11,138	3,751
Grade 8	155,044	20,390	4,904
Grade 10	155,044	24,316	3,573

Note:

Table 6: Students' Characteristics by High School Type

	Academic (1)	Technical (2)	Vocational (3)
Demographics			
Socio-Economic Status	0.34	-0.17	-0.54
	[0.93]	[0.90]	[0.91]
% of Immigrant	0.08	0.13	0.19
	[0.27]	[0.34]	[0.40]
% of Women	0.61	0.32	0.48
	[0.49]	[0.47]	[0.50]
Observations			
Number of Students	200,556	96,535	36,717
Fraction	60.1%	28.9%	11.0%

Note: This table shows the fraction and characteristics of students enrolling into the three main types of high-school in the restricted sample. The measure of Socio-Economic Status is a continuous variable with higher values indicating a higher socio-economic background. It is described in more details in footnote 12 of Section 2.3.

Table 7: Percentalized Standardized Test Score Distribution

	Mean	Std. Dev.	p25	p50	p75
Panel A: Grade 5 Invalsi Scores					
Italian	50.28	28.83	25.00	50.00	75.00
Maths	50.48	28.78	26.00	50.00	75.00
Panel B: Grade 8 Invalsi Scores					
Italian	52.43	28.61	28.00	53.00	77.00
Maths	52.16	28.70	28.00	53.00	77.00
Panel C: Grade 10 Invalsi Scores					
Italian	54.69	28.03	31.00	56.00	79.00
Maths	54.31	28.35	31.00	56.00	79.00

Note:

Table 8: Class Score Distribution

	Mean	Std. Dev.	Min	p25	p50	p75	Max
Panel A: Grade 5 Class Score							
Italian	7.96	1.02	1.00	7.00	8.00	9.00	10.00
Maths	8.03	1.05	1.00	7.00	8.00	9.00	10.00
Panel B: Grade 8 Class Score							
Italian	7.28	1.15	1.00	6.00	7.00	8.00	10.00
Maths	7.13	1.34	1.00	6.00	7.00	8.00	10.00
Panel C: Grade 10 Class Score							
Italian	6.47	1.04	1.00	6.00	6.50	7.00	10.00
Maths	6.15	1.41	1.00	5.00	6.00	7.00	10.00

Note:

Table 9: Students Characteristics

	Mean	Std. Dev.	N
Panel A: Restricted Sample			
% of Women	0.51	0.50	333,808
Age	10.91	0.33	331,480
Socio-Economic Status	0.10	0.97	324,651
Both Italian-born Parents	0.82	0.39	333,808
At Least 1 Italian-born Parent	0.89	0.31	333,808
Panel B: All Students Observed in Grade 5			
% of Women	0.51	0.50	744,242
Age	10.92	0.33	739,311
Socio-Economic Status	0.10	0.98	705,858
Both Italian-born Parents	0.78	0.41	744,242
% of Immigrants	0.14	0.35	744,242

Note:

9.3 Results

Table 10: Effect of Primary School Class Rank

	Performance in...	
	Middle School (Grade 8)	High School (Grade 10)
	(1)	(2)
Class Grade Rank	8.169***	7.623***
	(0.619)	(0.594)
Observations	667,616	667,616
Mean	52.30	54.50

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Middle School (Grade 8) or High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 6. Quartic in class grades and standardized test grades, Primary School class-by-subject fixed effects and students fixed effects are included. Data are stacked by subject (Italian or Math), so there are two observations per student.

9.4 Main Robustness Checks

Table 11: Robustness Check: Class Grade Rank vs. Standardized Grade Rank

	Performance in...	
	Middle School (Grade 8)	High School (Grade 10)
	(1)	(2)
	Panel A: Unconditional	
Class Grade Rank	34.40***	31.08***
	(0.142)	(0.151)
Standardized Grade Rank	36.65***	34.61***
	(0.139)	(0.129)
	Panel B: Conditional on Grades and Student FEs	
Class Grade Rank	8.192***	7.556***
	(0.619)	(0.594)
Standardized Grade Rank	-0.352	1.024***
	(0.334)	(0.320)
Observations	667,616	667,616
Mean	52.30	54.50

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Middle School (Grade 8) or High School (Grade 10) class level. Panel A reports estimates $\hat{\beta}$ and $\hat{\delta}$ from Equation 8, where only Primary School class-by-subject fixed effects are included. Panel B reports estimates $\hat{\beta}$ and $\hat{\delta}$ from Equation 9, where Primary School class-by-subject fixed effects, student fixed effects, and quartic in class grades and standardized test grades are included. Data are stacked by subject (Italian or Math), so there are two observations per student.

Table 12: Robustness Check: Primary School Class Rank Effect

	Standardized Test Grades in..	
	Primary School (Grade 2)	Middle School (Grade 8)
	(1)	(2)
Class Grade Rank	-0.667	6.375***
	(0.832)	(0.662)
Observations	467,334	467,334
Mean	50.58	55.49

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Primary School (Grade 2) or Middle School (Grade 8) class level. This Table reports estimates $\hat{\beta}$ from Equation 6. Quartic in class grades and standardized test grades, Primary School class-by-subject fixed effects and students fixed effects are included. Data are stacked by subject (Italian or Math), so there are two observations per student.

Table 13: Effect of the Ability Distribution on Rank Estimates

Grade	**Baseline**	**Evaluated at the ... of $\sum_{k=2}^{7} \beta^{(k)} m_{cs}^{(k)}$**			
		Mean	**25th percentile**	**50th percentile**	**75th percentile**
Middle School (Grade 8)	8.17	8.46	7.70	8.59	9.34
High School (Grade 10)	7.64	7.48	6.66	7.22	8.04

Note: This Table compares the baseline estimated rank effect from Equation 6 to the estimated rank effect from Equation 11, i.e., once the first seven moments of the class ability distribution have been factored in.

9.5 Rank Effect vs. Class Effects

Table 14: Rank Effect vs. Class Effect

	Performance in ..	
	Middle School (Grade 8)	High School (Grade 10)
Class Rank	2.17	2.03
Class Quality	11.46	12.37
Ratio	18.9%	16.4%

Note: This Table reports the effect on subsequent academic achievements of one standard deviation increase in the Primary school class rank and in the Primary school class quality. These effects are computed from Equation 6, with the class quality effect being estimated from class-by-subject fixed effects. See Section 6 for details.

Table 15: Correlation Between Class Quality and Class Observables

	Primary School Class Effect on Performance in..	
	Middle School	High School
	(Grade 8)	(Grade 10)
	(1)	(2)
Mean SES	0.263***	0.400***
	(0.00447)	(0.00426)
Fraction of Immigrants	-0.0695***	0.0355***
	(0.00434)	(0.00414)
Fraction of Women	0.0217***	0.0396***
	(0.00431)	(0.00411)
Class Size	0.0472***	0.0356***
	(0.00448)	(0.00427)
Observations	49,198	49,198

Note: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. This Table reports correlations between observables at the Primary school class level and class effects on performance in Middle school (Column (1)) and High school (Column (2)). All variables are standardized. See Section 6 for details.

Table 16: Correlation Between Class Rank and Class Mean SES

	Class Grade Rank
	(1)
Mean SES	-0.135***
	(0.00187)
Observations	649,302

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Primary school (Grade 5) class level. This Table reports correlations between Primary school class mean SES and student's Primary school class rank based on class grades. Both variables are standardized. Controls for student's SES and Primary school standardized test grades are included. See Section 6 for details.

Table 17: Correlation Between Performance and Class Mean SES

	Primary School Class Effect on Performance in..	
	Middle School (Grade 8)	High School (Grade 10)
	(1)	(2)
Mean SES	2.437***	3.659***
	(0.0588)	(0.0488)
Observations	649,302	649,302

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Middle School (Grade 8) or High School (Grade 10) class level. This Table reports correlations between Primary school class mean SES and student's standardized test grades in Middle school (Column (1)) or High school (Column (2)). The class mean SES variable is standardized. Controls for student's SES and Primary school standardized test grades are included. See Section 6 for details.

9.6 Mechanisms

Table 18: Impact of Class Rank on School Quality

	Quality of..	
	Middle School (Grade 8)	High School (Grade 10)
	(1)	(2)
Class Grade Rank	0.835***	4.552***
	(0.320)	(0.440)
Observations	596,644	596,644
Mean	51.35	52.05

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the Middle School (Grade 8) or High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 6 with a measure of school quality used as the dependent variable. See Section 7.1 for details. Quartic in class grades and standardized test grades, Primary School class-by-subject fixed effects and students fixed effects are included. Data are stacked by subject (Italian or Math), so there are two observations per student. Note that 35,486 students of our main sample are missing. The discrepancy is due to the fact that we exclude schools in which too many classes cheated: in this case schools cannot publicly report their standardized test results. See Section 1 for details.

Table 19: Students Characteristics

	Mean	Std. Dev.	Obs.
2018 Cohort: Restricted Sample			
Socio-Economic Status	0.10	0.96	174,098
% of Women	0.52	0.50	174,098
% of Immigrants	0.08	0.27	174,098
2018 Cohort: All Sample			
Socio-Economic Status	0.01	1.00	457,146
% of Women	0.49	0.50	496,331
% of Immigrants	0.20	0.40	496,331

Note: This Table compares the selected sample for the mechanism analysis of Section 7.2 to the baseline sample of students observed in High school (Grade 10) in 2018. See Section 7.2 for details.

Table 20: Effect of Primary School Class Rank

	Performance in High School (Grade 10) (1)
Class Grade Rank	6.379***
	(0.822)
Observations	348,196
Mean	55.51

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 6 on a sample of students from the 2018 Cohort. See Section 7.2 for details. Quartic in class grades and standardized test grades, Primary School class-by-subject fixed effects and students fixed effects are included. Data are stacked by subject (Italian or Math), so there are two observations per student.

Table 21: Effect of the Rank on Subject Interest

	Subject Interest (1)	Subject Interest (2)
Class Grade Rank	3.571***	3.961***
	(0.792)	(1.264)
Female	4.213***	
	(0.117)	
Socio-Economic Status	2.280***	
	(0.0637)	
Immigrant	1.216***	
	(0.221)	
Observations	348,196	348,196
Mean	52.35	52.35
Student FE	No	Yes

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 12 (Column (1)) and Equation 6 (Column (2)) using a measure of subject interest as dependent variable. See Section 7.2 for details. Quartic in class grades and standardized test grades and Primary School class-by-subject fixed effects are included in both specifications. Student fixed effects are included in Column (2) specification. Data are stacked by subject (Italian or Math), so there are two observations per student.

Table 22: Grade 5 - Class Level

	Parental Support (1)	Parental Support (2)
Math Rank	-1.178	
	(1.054)	
Italian Rank		0.494
		(1.086)
Female	1.925***	2.814***
	(0.143)	(0.143)
Socio-Economic Status	5.683***	5.507***
	(0.0791)	(0.0800)
Immigrant	-6.233***	-5.924***
	(0.281)	(0.283)
Observations	174,098	174,098
Mean	52.01	52.01

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 12 using a measure of parental support as a dependent variable. The variable of interest is the Primary school class rank in Math (Column (1)) or in Italian (Column (2)). See Section 7.2 for details. Quartic in class grades and standardized test grades and Primary School class-by-subject fixed effects are included.

Table 23: Grade 5 - Class Level

	Self-Esteem (1)	Self-Esteem (2)
Maths Rank	2.888***	
	(1.047)	
Italian Rank		5.249***
		(1.063)
Female	-2.892***	-4.928***
	(0.141)	(0.140)
Socio-Economic Status	3.737***	3.592***
	(0.0795)	(0.0801)
Immigrant	-0.222	0.322
	(0.286)	(0.287)
Observations	174,098	174,098
Mean	51.40	51.40

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 12 using a measure of self-esteem as a dependent variable. The variable of interest is the Primary school class rank in Math (Column (1)) or in Italian (Column (2)). See Section 7.2 for details. Quartic in class grades and standardized test grades and Primary School class-by-subject fixed effects are included.

Table 24: Grade 5 - Class Level

	Peer Recognition (1)	Peer Recognition (2)
Maths Rank	2.083*	
	(1.087)	
Italian Rank		4.177***
		(1.102)
Female	-5.279***	-5.670***
	(0.151)	(0.150)
Socio-Economic Status	2.565***	2.508***
	(0.0822)	(0.0828)
Immigrant	-4.654***	-4.368***
	(0.284)	(0.286)
Observations	174,098	174,098
Mean	52.07	52.07

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 12 using a measure of peer recognition as a dependent variable. The variable of interest is the Primary school class rank in Math (Column (1)) or in Italian (Column (2)). See Section 7.2 for details. Quartic in class grades and standardized test grades and Primary School class-by-subject fixed effects are included.

Table 25: Grade 5 - Class Level

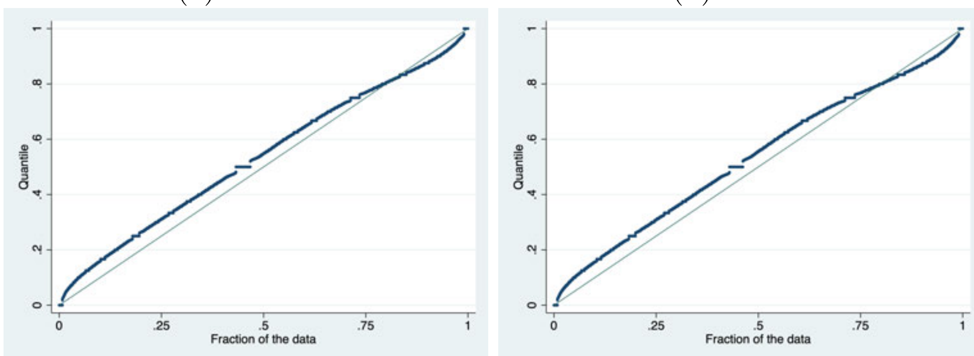
	Bad Perception (1)	Good Perception (2)	Bad Perception (3)	Good Perception (4)
Maths Rank	-4.207***	0.716		
	(1.071)	(1.085)		
Italian Rank			-4.143***	3.047***
			(1.082)	(1.096)
Female	-8.925***	5.232***	-8.977***	5.183***
	(0.147)	(0.149)	(0.146)	(0.149)
Socio-Economic Status	-2.311***	2.280***	-2.217***	2.218***
	(0.0809)	(0.0818)	(0.0814)	(0.0823)
Immigrant	-0.804***	-1.465***	-0.995***	-1.412***
	(0.286)	(0.286)	(0.287)	(0.288)
Observations	173,782	173,773	173,782	173,773
Mean	48.41	52.06	48.41	52.06

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the High School (Grade 10) class level. This Table reports estimates $\hat{\beta}$ from Equation 12 using a measure of school perception as a dependent variable. The variable of interest is the Primary school class rank in Math (Column (1)) or in Italian (Column (2)). See Section 7.2 for details. Quartic in class grades and standardized test grades and Primary School class-by-subject fixed effects are included.

10 Figures

10.1 Data and Descriptive Statistics

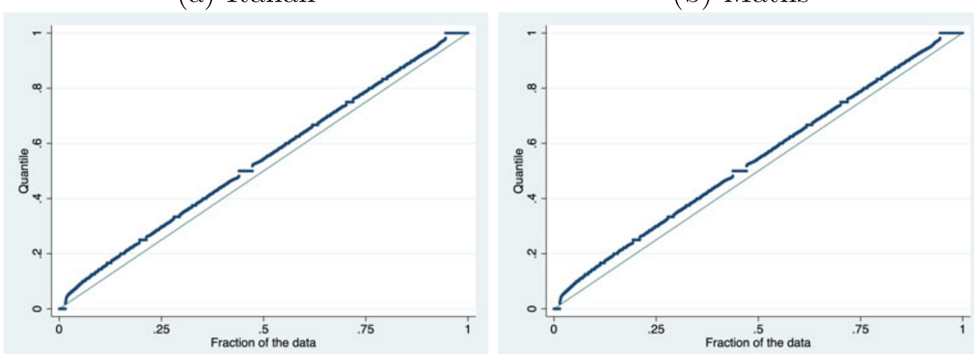
(a) Italian (b) Maths



Note:

Figure 1: QQ-plot of Class Rank Quantiles based on Class Scores

(a) Italian (b) Maths

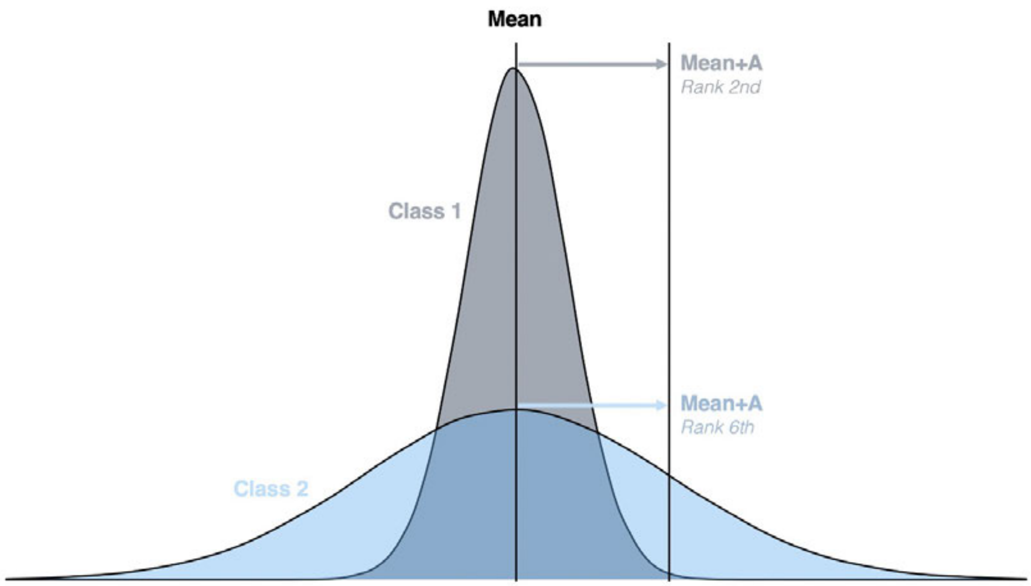


Note:

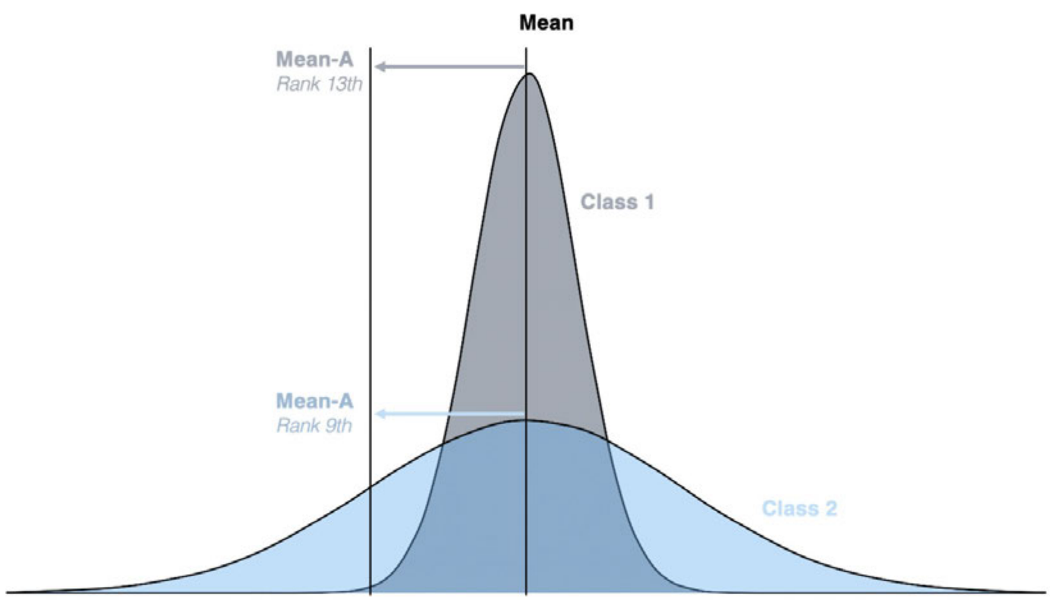
Figure 2: QQ-plot of Class Rank Quantiles based on Standardized Scores

10.2 Empirical Strategy

(a) Above-the-Mean Abilities



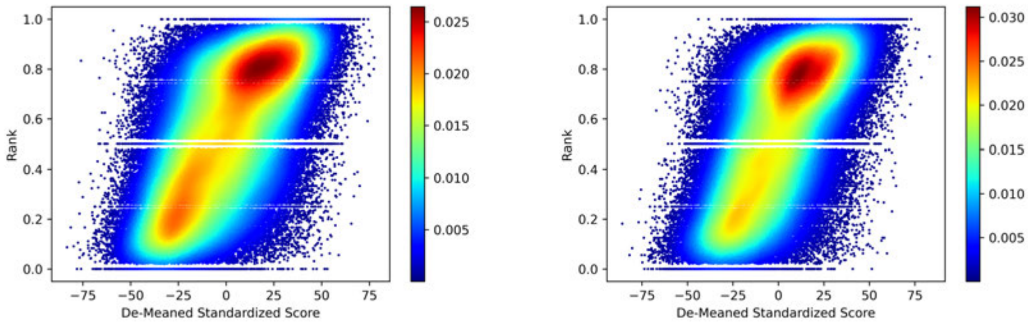
(b) Below-the-Mean Abilities



Note: This Figure illustrates the example developed in Section 3.1.

Figure 3: Higher Order Peer Effects on Rank

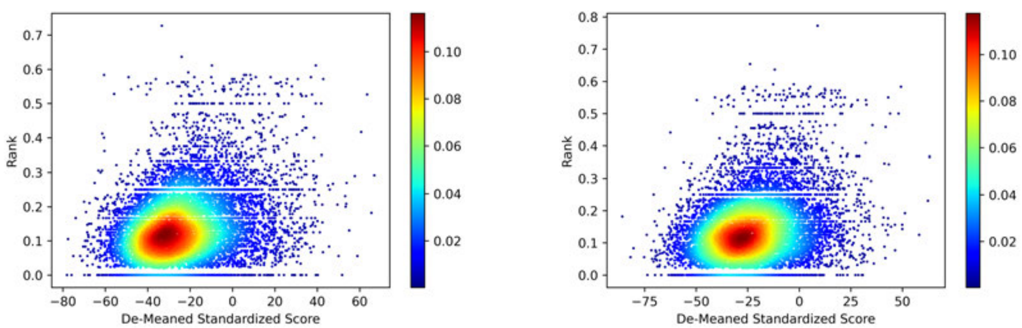
(a) Italian (b) Maths



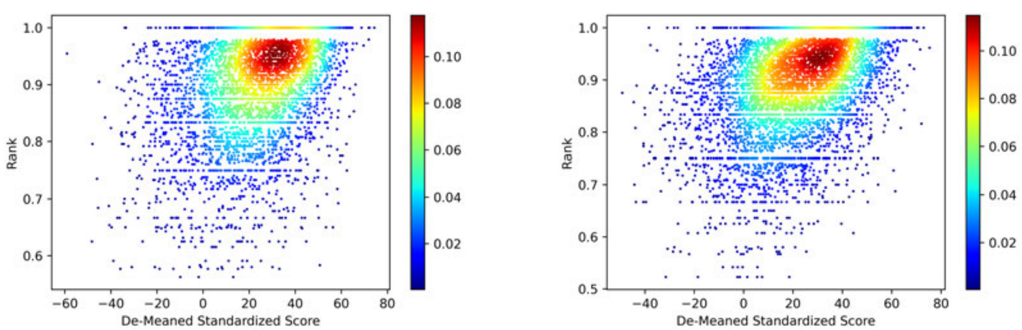
Note:The x-axis shows the de-meaned standardized test grades and the y-axis the corresponding class rank based on class grades.

Figure 4: Grade 5 Class Rank Distribution

(a) Italian (Class Gradfe = 6) (b) Maths (Class Grade = 6)



(c) Italian (Class Grade = 10) (d) Maths (Class Grades = 10)

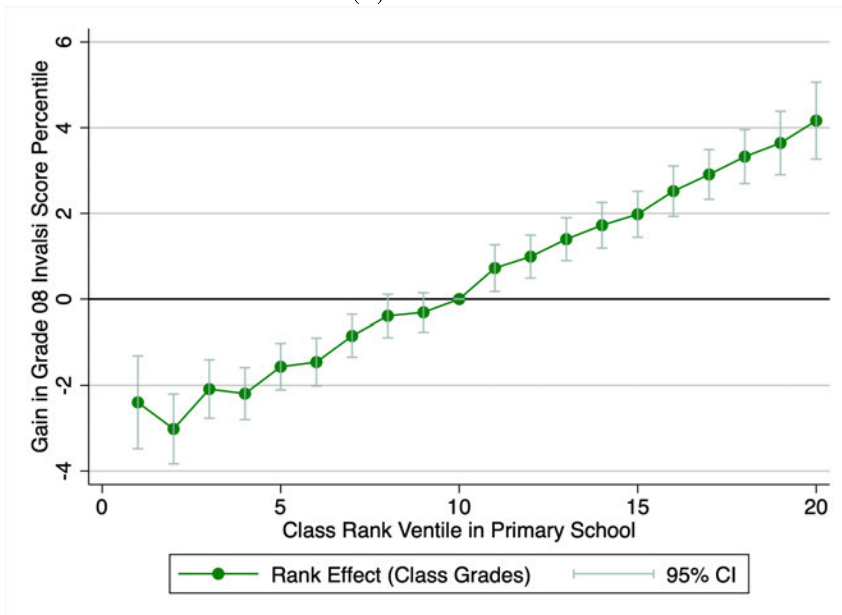


Note: The x-axis shows the de-meaned standardized test scores and the y-axis the corresponding class rank based on class grades for students who got a class score of 6 or 10.

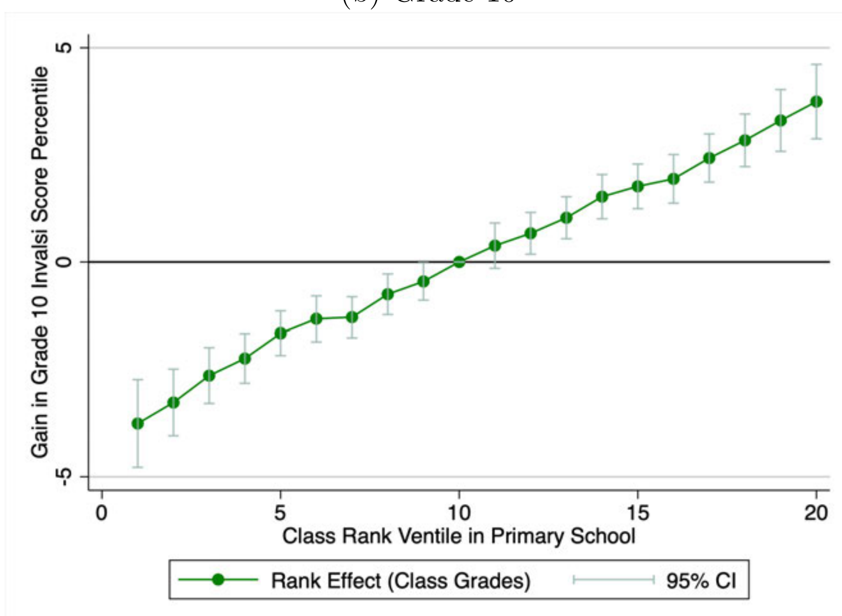
Figure 5: Grade 5 Class Rank Distribution Conditional on Class Score

10.3 Results

(a) Grade 8



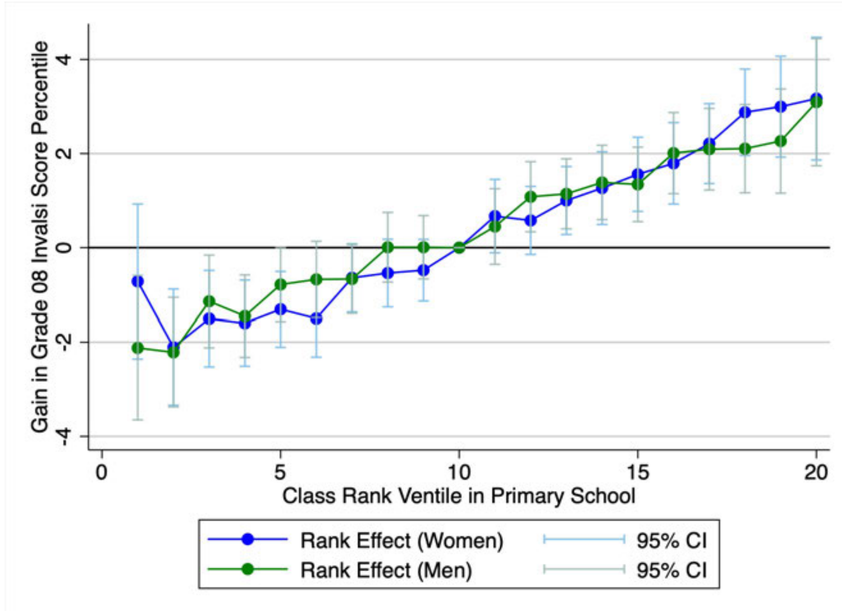
(b) Grade 10



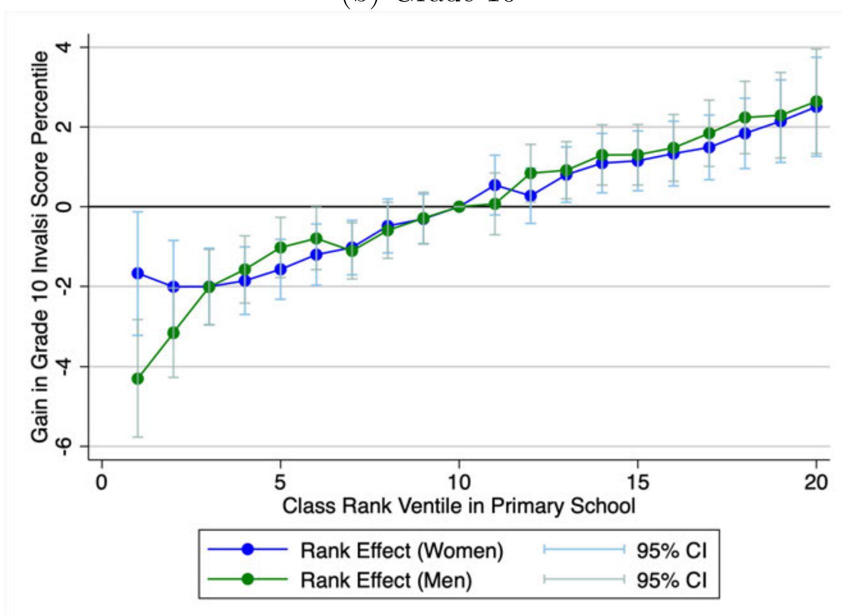
Note:

Figure 6: Non-Linear Rank Effects

(a) Grade 8



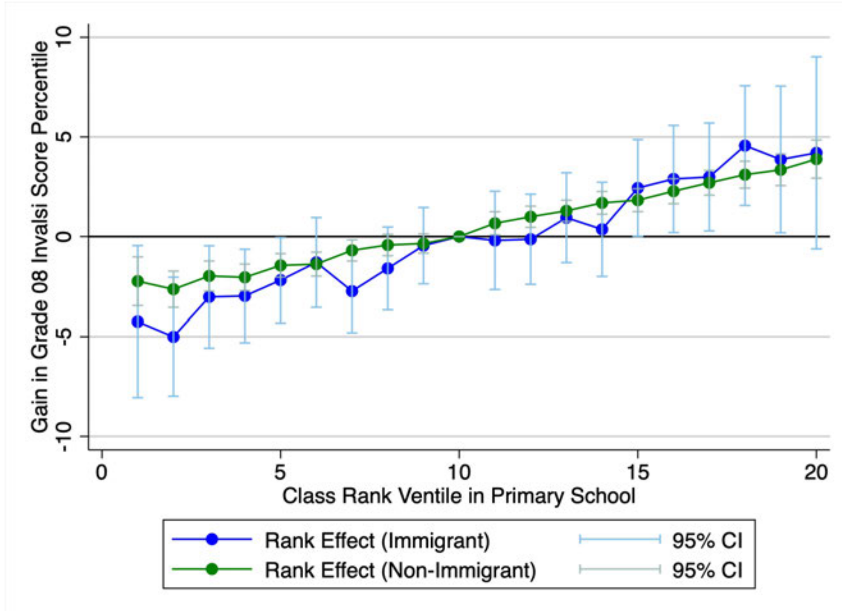
(b) Grade 10



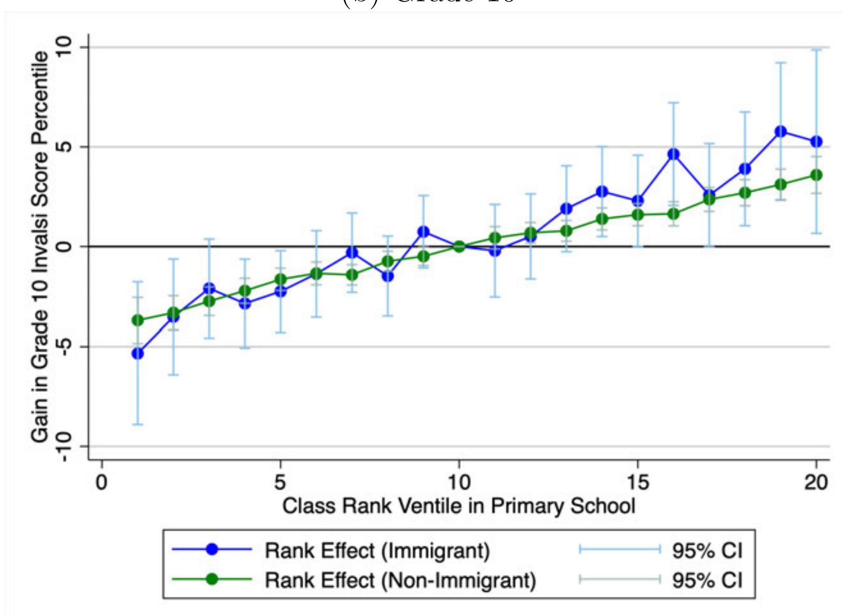
Note:

Figure 7: Non-Linear Rank Effects - By Gender

(a) Grade 8



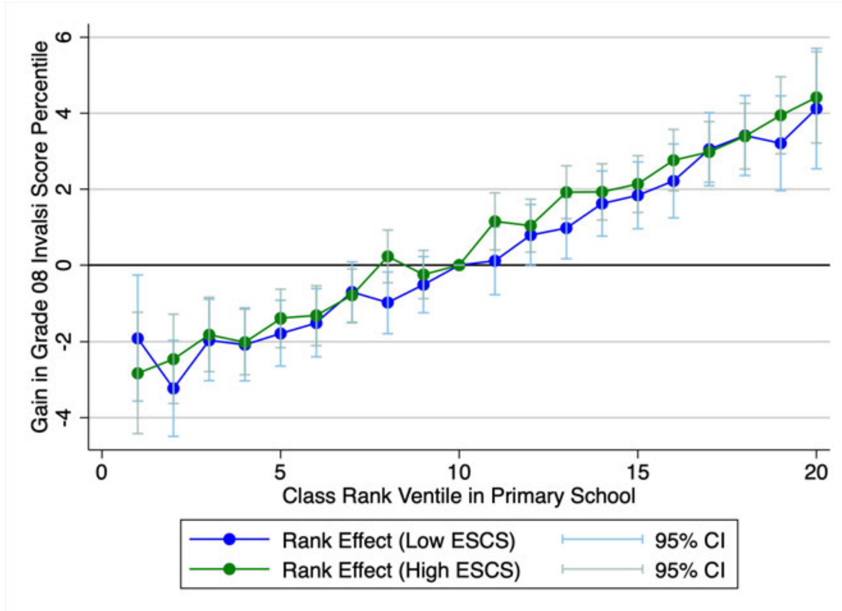
(b) Grade 10



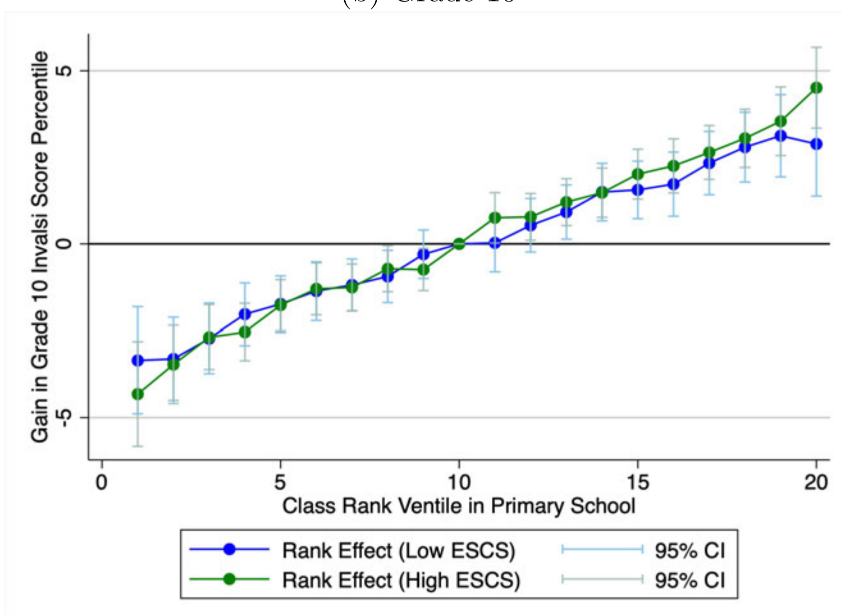
Note:

Figure 8: Non-Linear Rank Effects - By Immigration Status

(a) Grade 8



(b) Grade 10

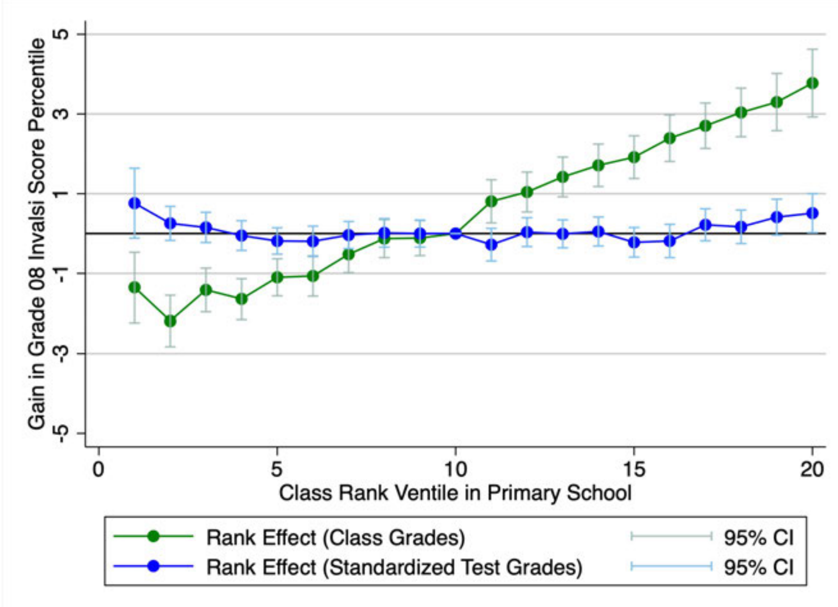


Note:

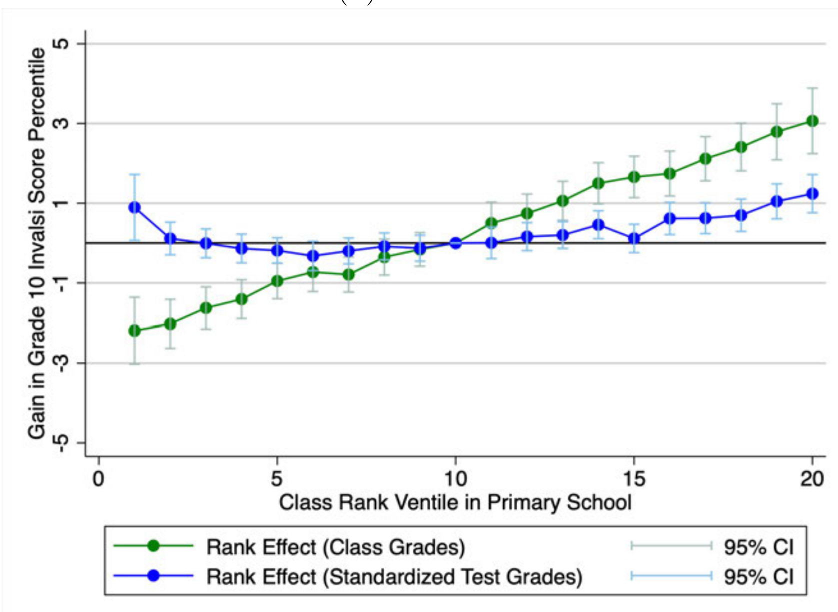
Figure 9: Non-Linear Rank Effects - By Socio-Economic Status

10.4 Main Robustness Checks

(a) Grade 8

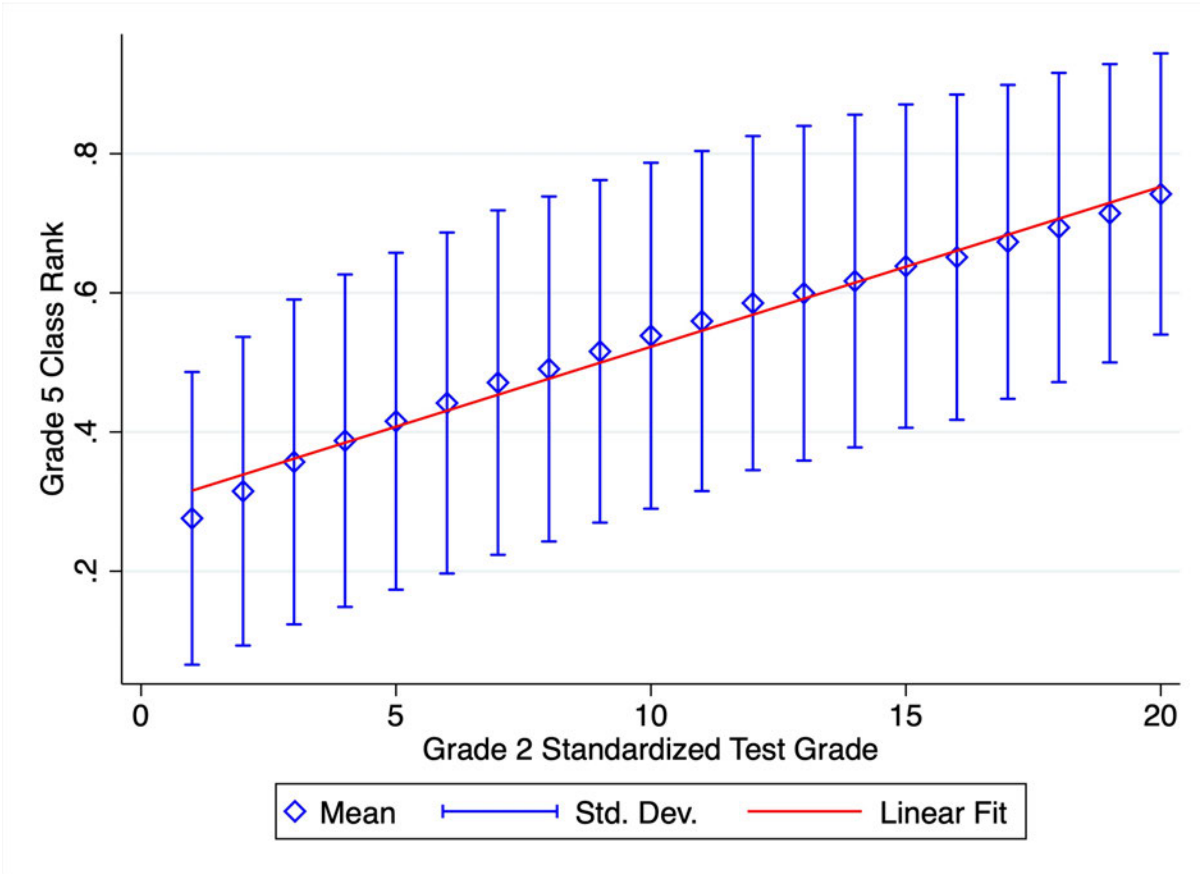


(b) Grade 10



Note:

Figure 10: Non-Linear Rank Effects -
Class Grade Rank vs. Standardized Grade Rank

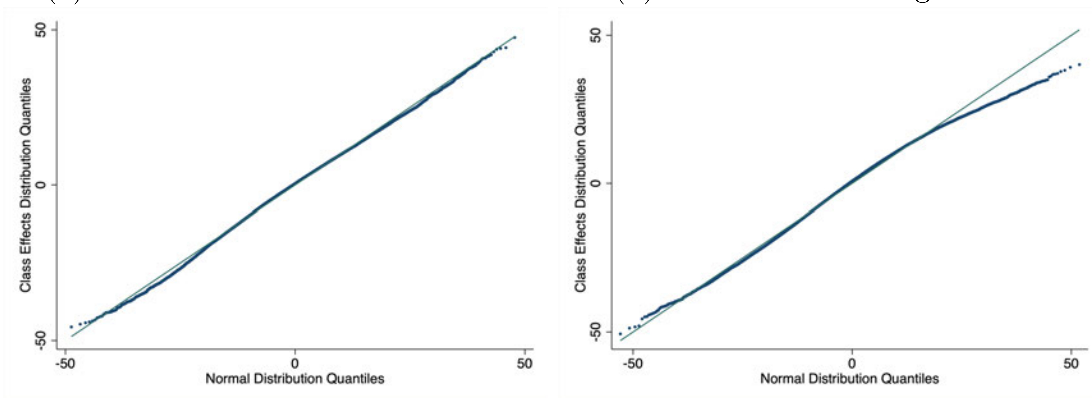


Note:

Figure 11: Correlation Between Grade 2 Standardized Test Grades and Grade 5 Class Ranks

10.5 Rank Effect vs. Class Effects

(a) Performance in Middle School (b) Performance in High School

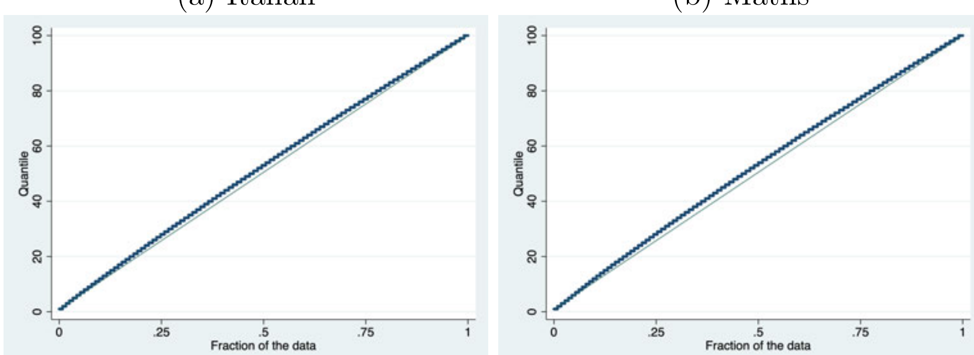


Note: These plots show how the quantiles of the distribution of Primary school class effects compare to that of a normal distribution with the same mean and variance. Class effects on Middle school performance exhibit a distribution very close to that of a normal. Class effects on High school performance exhibit a distribution with thinner tails than that of a normal, assuaging any concerns that the variance could driven by outliers.

Figure 12: Distributional Diagnostic Plot of Primary School Class Effects on Later Performance

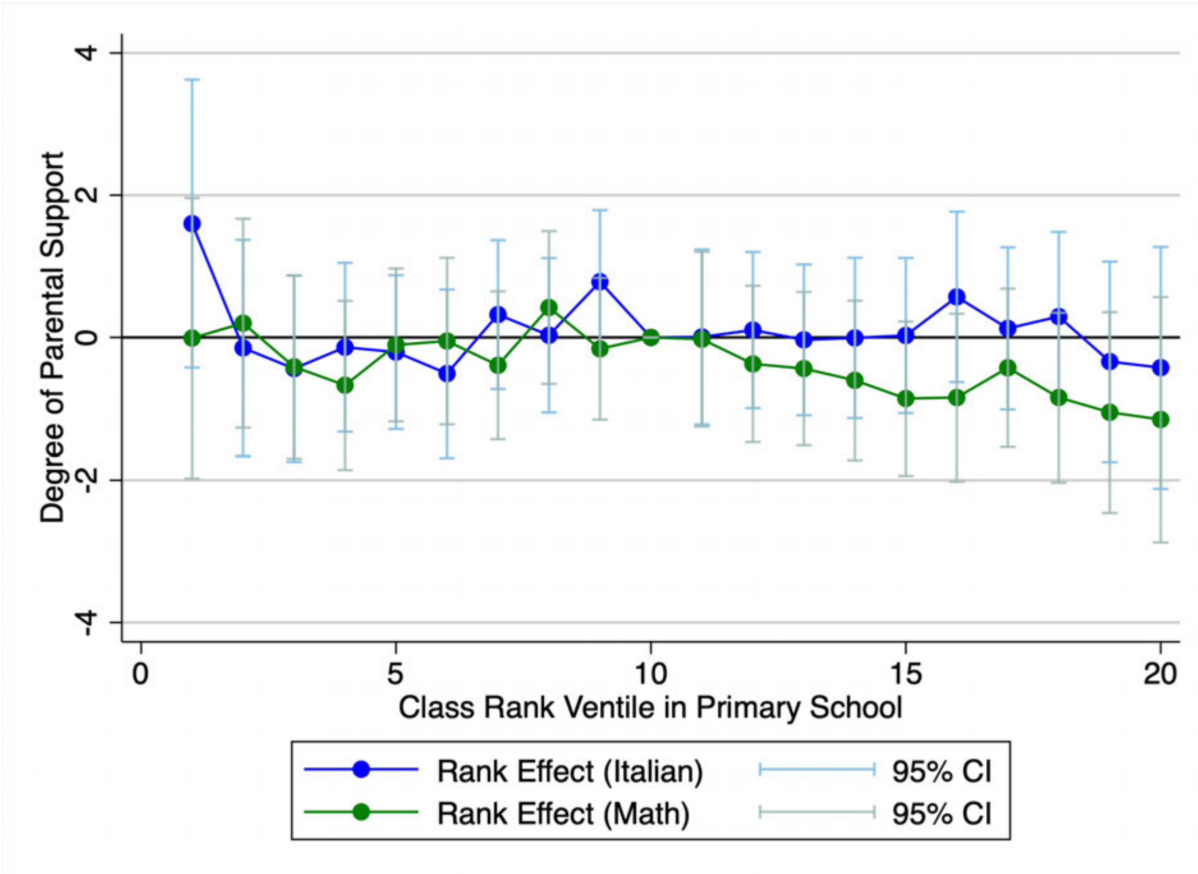
10.6 Mechanisms

(a) Italian (b) Maths



Note:

Figure 13: QQ-plot of Subject Confidence Score Quantiles



Note:

Figure 14: Non-Linear Rank Effect on Parental Support

Appendices

A Balance of Characteristics within Schools

As noted in Section 1, principals are supposed to ensure that, within primary schools, classes are balanced along certain dimensions, particularly gender and SES. SES being a continuous variable, we group students into deciles. We want to check the extent to which schools comply with the law and student assignment to classes are statistically independent from students' characteristics. To do so, we perform a series of Pearson Chi-Square Tests at the school level on the following characteristics: gender and SES. The null hypothesis is that the composition of classes is balanced at the school level. In Table 26, we report the fraction of tests whose p-value is below 5%. We observe that primary school classes indeed appear to be balanced .

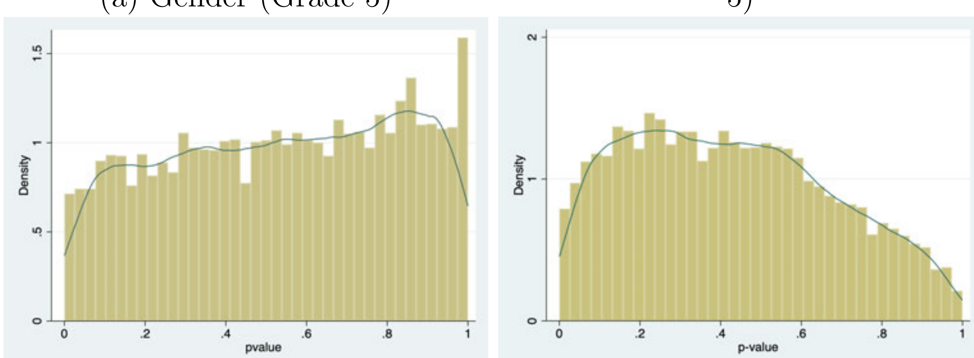
Table 26: Chi-square Tests

	Fraction of Significant Tests	
	Primary School	High School
Gender	3.6%	41%
Socio-Economic Status	4%	7.1%

Note: This table reports the fraction of significant tests at 5%.

The distribution of p-values are plotted on Figure 15.

(a) Gender (Grade 5) (b) Socio-Economic Status (Grade 5)



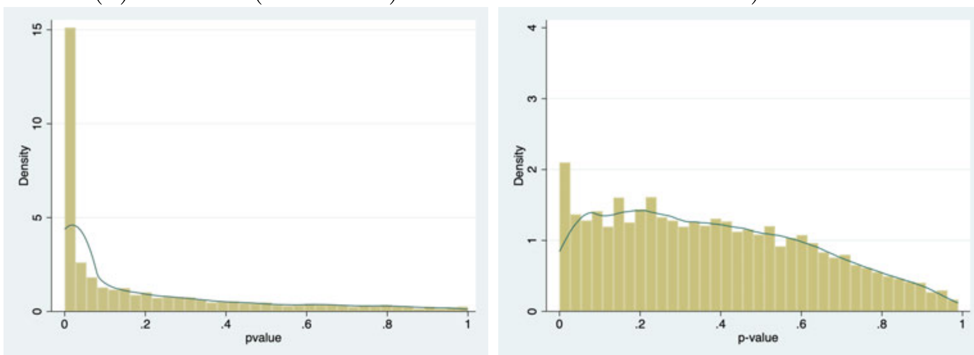
Note:

Figure 15: Grade 5 Class Composition Balance Check

In high school, where students have much more leeway to choose their classes, we can see that independence along the gender dimension is no longer satisfied.

(a) Gender (Grade 10)

(b) Socio-Economic Status (Grade 10)



Note:

Figure 16: Grade 10 Class Composition Balance Check

B Rank and Ties

As we observe a lot of ties in our data, we have to be careful in the way we deal with them. As explained in Section 2, we followed the literature and assigned the mean rank to ties. For instance, if 2 students got 10 and 3 got 8, the first two are assigned rank 2 and the last three are ranked 5. This allows us to "normalize" the sum of the ranks by class and keep class of the same size comparable. However, it is legitimate to wonder what would be the result of treating ties differently, especially assigning the group top or bottom rank. In the previous example, the first three would rank first if assigned the top rank and third if assigned the bottom rank.

We are wary of using these two other methods as this would lead to create an artificially high number of top or bottom students, giving some classes disproportionate weight. If, say, all students in a class of 20 got the same grade, we would thus consider we have either 20 top or 20 bottom students while they were arguably no better than the class average and were not distinguishable from each other on this dimension. We expect these methods to yield much lower coefficients, as they essentially defuse the rank effect by artificially polarizing ranks.

However, we want to make sure that we are not over-estimating the rank effect due to the presence of ties. Our investigation is twofold. We first explore how a "compressed" rank distribution can affect the magnitude of the rank effect. We take advantage of our observing the standardized test distribution for which there are very few ties at the class level. We then reassign ranks based on standardized scores to mimic the distribution of ranks based on class scores.

Table 27: Rank Ties Example

Standardized Scores		**Class Scores**			
Score	Rank	Score	Mean Rank	Bottom Rank	Top Rank
100	1	10	1.5	2	1
95	2	10	1.5	2	1
93	3	9	3	3	3
75	4	8	4.5	5	4
68	5	8	4.5	5	4
56	6	7	7	8	6
53	7	7	7	8	6
43	8	7	7	8	6
29	9	6	9	9	9
23	10	5	10	10	10

Note:

Let's suppose that there are 10 students in a class. The standardized scores allow us to rank them with no ties but the distribution of class scores leads to ties, as summarized on Table 27. Importantly, a row does not identify a student: student A might be first at the standardized test but third in the class score ranking. We then proceed to construct a placebo standardized score rank to mimic ties occurring in the class score ranking. For instance, in this example, students ranked 1 and 2 based on standardized scores would be reassigned ranks 1.5 to create a placebo mean rank, ranks 2 to create a placebo bottom rank and ranks 1 to create a placebo top rank. The rank of the third would not change. Students ranked 4 and 5 would also be considered ties, as well as students ranked 6, 7 and 8. We estimate Equation 4 with these placebo ranks and we report $\hat{\beta}$s in Table 28. As anticipated, we observe that, in Grade 10, "compressing" the rank distribution leads to an underestimation of the effect of the rank,[31] suggesting that we estimate a lower bound of the effect. For Grade 8, imprecise estimation renders conclusion slightly more dubious - but we notice that coefficients are not significantly different from each other.

[31] Coefficients are all significantly different at the 5% level.

Table 28: Placebo Rank Results

	Grade 8				**Grade 10**			
	Baseline	Mean	Bottom	Top	Baseline	Mean	Bottom	Top
$\hat{\beta}$	0.318	0.406*	0.125	0.434**	1.673***	0.723***	0.512***	0.886***
	(0.338)	(0.224)	(0.191)	(0.217)	(0.324)	(0.220)	(0.187)	(0.211)

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the current Grade (8 or 10) class level. G5 Class * Subject and student fixed effects are included. Data are stacked by subject (Italian or Maths), so there are two observations per student. $\hat{\beta}$ is from Equation 4 with different rank variables: the standard one based on standardized scores (Baseline) and the placebo ones mimicking the rank distribution based on class scores.

Second, we use the fact that, in the limit, when there is no tie, the three measures of rank converge and should consequently yield the same result. We want to pin down this limit, to have a sense of how far from it our estimates stand.

To do so, we estimate Equation 6 on different subsamples of which are excluded students with too high a number of ties. We re-estimate the rank effect first excluding students tied with more than 4 people, then those tied with more than 3 people and finally on the small subset of students that are not tied or only have one tie.[32] Results are displayed in Table 29.

[32]Estimates for the subset of students that are not tied are highly imprecisely estimated for lack of observations and are thus not reported here.

Table 29: Rank Estimates Convergence

	Grade 8			Grade 10		
	Mean	Bottom	Top	Mean	Bottom	Top
At most one tie	9.54***	5.87***	8.70***	6.61***	5.43***	4.58**
	(2.65)	(2.28)	(2.33)	(2.62)	(2.22)	(2.30)
At most two ties	8.90***	4.61***	7.66***	6.79***	4.37***	4.98***
	(1.64)	(1.35)	(1.37)	(1.57)	(1.29)	(1.31)
At most three ties	9.47***	5.21***	6.67***	8.68***	5.56***	5.32***
	(1.19)	(0.95)	(0.94)	(1.14)	(0.90)	(0.91)
Baseline	8.17***	3.52***	3.66***	7.62***	3.46***	3.21***
	(0.62)	(0.40)	(0.42)	(0.59)	(0.38)	(0.41)

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Clustering at the current Grade (8 or 10) class level. G5 Class * Subject fixed effects are included. Data are stacked by subject (Italian or Maths), so there are two observations per student. $\hat{\beta}$ is from Equation 6, estimated on four different samples of students and with three different measures of rank.

We can see that, as expected, the three measures of rank yield somewhat converging estimates, as we exclude tied students from our analysis. The limit seems to be close to our original estimate: none of the estimates from the sample of students with at most one tie are significantly different from the main estimate at the 5% level. This alleviates concerns that our measure of rank could be driving our results.

Together with the previous part in which we showed that a compressed distribution leads to an underestimation of the effect, we are confident that the estimate we find is likely to be a lower bound to the true effect.

C On a Different Subset of Classes

In this Section, we look at how the main result is affected when we run the analysis on a different subset of classes. In light of the concerns laid out in Section 2 about unobserved students and the consequences it could have on the computation of rank, we perform the analysis on two other samples.

First, we restrict the sample of classes to those with 100% coverage so that we know that all students are observed and the rank is well-defined. Results are displayed in Table 30.

Table 30: Rank Effect on Fully Covered Classes

	G08 Standardized Score (1)	G10 Standardized Score (2)
	Panel A	
Class Rank	8.177***	7.620***
	(1.019)	(0.976)
Observations	225,702	225,702
Mean	51.91	53.13

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates from Equation 6 on the subset of classes in which all students are observed. Clustering at the current Grade (8 or 10) class level. Quartic in class scores or standardized scores and G5 Class * Subject fixed effects are included. Data are stacked by subject (Italian or Maths), so there are two observations per student.

Estimates are extremely close to those from our main sample.

Second, we test our that the assumption that students for whom we lack information are the weakest in their class. To do so, we keep fully covered classes but we drop students whose average class score is below the 10th percentile of their class average score distribution. This is pretty conservative as the mean coverage of classes in our sample is 95%. Results are displayed in Table 31.

Table 31: Rank Effect after Dropping Worst Students

	G08 Standardized Score (1)	G10 Standardized Score (2)
	Panel A	
Class Rank	9.402***	7.012***
	(1.260)	(1.200)
Observations	200,018	200,018
Mean	54.87	55.99

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates from Equation 6 on the subset of classes in which all students are observed. Clustering at the current Grade (8 or 10) class level. Quartic in class scores or standardized scores and G5 Class * Subject fixed effects are included. Data are stacked by subject (Italian or Maths), so there are two observations per student.

We can see that G8 estimate is slightly higher than the "true" one from Table 30 but the

G10 estimate is slightly lower. For neither Grades are estimates statistically different at the 10% level.

Third, we keep the sample of our main analysis but we now randomly assign class scores to missing students and recompute the ranks accordingly. We perform 50 Monte Carlo simulations and record $\hat{\beta}$ from Equation 6 at each iteration. Results are reported in Table 32.

Table 32: Monte Carlo Simulations - Missing Students

	Grade 8	Grade 10
$\hat{\beta}$	8.52***	7.16***
	(0.027)	(0.024)

Note: This table shows the result of 50 Monte Carlo simulations estimating Equation 6 after randomly allocating class scores to missing students and recomputing ranks accordingly. Standard error in parentheses.

Results are extremely close to that of the main analysis, assuaging concerns that assuming missing students are at the bottom of the class score distribution would significantly bias our results.

D Is the Rank Measure Context Specific?

Teachers have discretion in how to grade students but the 0-10 scale they use to do so is common across the country. This constrains the variability in class scores and one might wonder the extent to which the teacher effect is really at play. To test how context-specific the rank is, we randomly allocate students to placebo classes of the same size as the true classes. We then recompute their rank within these placebo classes and run the following specification:

$$S^{10}_{i\tilde{c}s} = \alpha + \beta R^5_{i\tilde{c}s} + h(S^5_{ics}) + g(C^5_{ics}) + \theta_{\tilde{c}s} + \gamma_i + \varepsilon_{i\tilde{c}s} \tag{13}$$

for student i studying subject s in class c ending up in placebo class $\tilde{c}$. Notice this is exactly the same as the main specification, except that classes are now randomly made up. Table 33 displays results obtained over 250 simulations: the median coefficient $\hat{\beta}$ along with 25th-75th percentile range (IQR) and the fraction of significant effect, after controlling for the False discovery rate at the 5% level.

Table 33: Monte Carlo Simulations

	Grade 8	Grade 10
Median $\hat{\beta}$	0.93	0.25
IQR	$[0.71, 1.22]$	$[-0.02, 0.52]$
Fraction of significant effect	49.2%	0%

Note: This table shows the result of 250 Monte Carlo simulations estimating Equation 13. FDR Control at the 5% level.

Interestingly, it seems that the rank measure in Grade 8 somehow captures an slight intrinsic component (the median estimate accounts for 11% of the main estimate in Grade 8), which may explain why the main estimate is larger in Grade 8 than in Grade 10. But, overall, this exercise confirms that the overwhelming part of the rank estimate is teacher-specific.

E Temporal Mismatch

As class scores are observed in January and Invalsi test is taken in April/May, there is a risk of temporal mismatch: we may be wary of our treatment (rank) affecting the covariate (ability measure). However, since the rank has a positive effect on subsequent academic achievements, controlling for same-year standardized test scores would make us underestimate the effect of the rank - thus providing us with a lower bound of the rank effect.

Table 34: Effect of Ability Measure on the Rank Effect

	Standardized Test Grades in Middle School (Grade 8)		
	(1)	(2)	(3)
Class Rank	6.375***	9.373***	6.414***
	(0.662)	(0.707)	(0.681)
Quartic in G5 Standardized Grade	Yes	No	Yes
Quartic in G2 Standardized Grade	No	Yes	Yes
Grade 2 Class-by-Subject FE	No	Yes	Yes
Observations	467,334	467,334	467,334
Mean	55.49	55.49	55.49

Note: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates from Equations **??**. Clustering at the current Grade 8 class level. Grade 5 Class × Subject fixed effects are included. Data are stacked by subject (Italian or Maths), so there are two observations per student.

Table 34 shows that it is indeed what we observe: controlling for ability using previous standardized previous standardized test scores leads to an overestimation of the rank effect

(Column (2)). Including them as additional control for ability yields a very close estimate to that we measure using the main specification.

F Questionario di Contesto

In this Section, we give the detail of the questions asked to students as part of the Questionnaire. They have to answer on a 1-6 point Likert scale.

F.1 Subject Interest

“Let’s talk about Italian/Math. Do you agree with the following sentences?

1. Totally disagree.
2. Slightly agree.
3. Moderately agree.
4. Agree.
5. Strongly agree.
6. Totally agree.

Questions:

- ”In general, I enjoy learning Italian/Math”
- ”I like to read Italian/Math books”
- ”I am happy to study Italian/Math”
- ”I am interested in learning Italian/Math well”
- ”I like to learn new topics in Italian/Math”
- ”I can’t wait to take Italian/Math lessons”

F.2 Career Prospects

“Thinking about your future, how true do you think these sentences are?

1. Totally false.
2. Slightly true.
3. Moderately true.

4. True.
5. Very True.
6. Totally true.

Questions:

- ”I will achieve the degree I want”.
- ”I will always have enough money to live”.
- ”In life I will be able to do what I want”.
- ”I'll be able to buy the things I want”.
- ”I will find a good job”.

F.3 Self-Confidence

“Thinking about yourself, how much do you agree with the following sentences?”

1. Totally disagree.
2. Slightly agree.
3. Moderately agree.
4. Agree.
5. Strongly agree.
6. Totally agree.

Questions:

- ”I'm able to think fast”.
- ”I think I'm a nice guy”.
- ”In the face of obstacles I work harder”.
- ”I usually have good ideas”.
- ”I learn new things with ease”.
- ”I know how to make others understand my point of view”.

F.4 Peer Recognition

“Thinking about your companions, how much do you agree with the following sentences?”

1. Totally disagree.
2. Slightly agree.
3. Moderately agree.
4. Agree.
5. Strongly agree.
6. Totally agree.

Questions:

- ”I think my teammates enjoy working with me”.
- ”In class, I feel accepted”.
- ”I can trust my companions”.
- ”I have fun with my companions”.
- ”In school, I have many friends”.

F.5 Perception of the School System

“Think about your experience in school. How true do you think these sentences are?

1. Totally false.
2. Slightly true.
3. Moderately true.
4. True.
5. Very True.
6. Totally true.

Questions:

- ”I want to stop going to school as soon as possible”.
- ”Going to school is an effort”.

- ”I feel fine at school”.
- ”I feel like I am wasting time at school”.
- ”At school, I get bored”.
- ”I have no reason to go to school”.
- ”At school I do interesting things”.

F.6 Parental Support

“How much do you agree with the following sentences?”

1. Totally disagree.
2. Slightly agree.
3. Moderately agree.
4. Agree.
5. Strongly agree.
6. Totally agree.

Questions:

- ”My parents are interested in what I do at school”.
- ”My parents encourage me to commit to studies”.
- ”My parents help me when I have difficulties at school”.
- ”My parents encourage me to be self-confident”.