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ABSTRACT

When Do Ordinal Ability Rank Effects Emerge? Evidence from the Timing of School Closures*

We leverage the timing of pandemic-induced school closures to learn about the emergence of ordinal rank effects in education. Using administrative data from Italian middle schools for four cohorts of students, our study reveals that disrupting peer interactions during the first year of middle school - when students are still unfamiliar with one another - substantially diminishes the impact of ordinal rank on test scores. Instead, later interruptions to peer interactions do not significantly affect the strength of these interpersonal comparisons.

JEL Classification: I21, I24, J24

Keywords: ability peer effect, ordinal ability rank, school closures, COVID-19

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1 Introduction

Interpersonal comparisons are a key feature of human interactions, and the school environment is no exception. A growing body of literature (see e.g. Elsner and Isphording, 2017, Murphy and Weinhardt, 2020, and the review by Delaney and Devereux, 2022) demonstrates that ordinal ability rank, by enhancing academic self-concept, is a type of ability peer effect that counters the positive spillover of high-ability peers (Bertoni and Nisticò, 2023) and holds significant relevance for educational outcomes.

Do rank effects still matter when peer interactions are suddenly limited? This paper examines how the sudden cessation of in-person schooling due to the pandemic altered the importance of interpersonal ability comparisons among classmates.

We estimate the effect of class ordinal ability rank on educational achievement using administrative data from Italian middle schools. Our identification strategy involves comparing the end-of-middle-school test scores of students with identical end-of-primary-school scores but different ordinal class ranks, influenced by variations in the higher-order moments of ability distributions across classes within a school cohort. We conduct our analysis across four cohorts of students, some unaffected by the pandemic and others impacted by school closures at different grades. This variation enables us to determine how the timing of sudden stops to peer interactions influenced the relevance of rank effects.

Consistent with the literature, we find significant and substantial rank effects for pre-pandemic cohorts, not exposed to school closures. For cohorts exposed to the pandemic, timing matters: only when closures occur during the first grade of middle school, when classmates are still unfamiliar with each other, the impact of ordinal rank on test scores is significantly weakened. For students affected in later grades, among whom rank is already more salient, the rank effect remains as pronounced as those in pre-pandemic cohorts.

Although our results arise from pandemic-induced school closures, they provide broader insights into the structure of ordinal rank effects. First, interpersonal ability comparisons depend on early interactions within a class, and their influence persists even if later peer interactions are reduced. Second, these comparisons have limited significance when early peer engagement is minimal, with later interactions only slightly affecting outcomes. From this perspective, our paper is close to Carneiro et al. (2024). Using the repeated random assignment of peers in each grade in Ecuadorian primary school, they show that the effect of rank among early peers is larger and more persistent than the effect of rank among later peers.

Our findings have implications for the design of educational programs that vary the schedule of peer interactions, such as blended programs. They reveal that the relevance of interpersonal comparisons decreases when peer interactions start late, while late interruptions to established interactions are not as relevant.

2 School closures in Italy

By forcing unanticipated school closures, the Covid-19 pandemic had disastrous impacts on children's education (see e.g. Agostinelli et al., 2022 for an assessment of impacts and mechanisms using a structural model). As learning is cumulative, the persistence of these effects is worrisome for the long-term outcomes of pupils. Italy was among the first countries affected by the Covid-19 pandemic. The first cases were reported in February 2020, and a national lockdown was imposed starting March 8. Students received only online - and mostly asynchronous - education for the remainder of the 2019/20 school year, which lasted approximately 14 weeks. During the 2020/21 school year, school closures varied by region, depending on the pandemic's intensity (see Aparicio-Fenoll, 2022). On average, schools were closed for 3.6 weeks, with a standard deviation of 3.3 weeks. Overall, Italy experienced some of the most prolonged school closures among European countries.

3 Data

We use population-level data on standardised education tests administered by INVALSI, the National Institute for the Evaluation of the Education System, in Italy. We match students' end-of-primary-school (5th grade) and end-of-middle-school (8th grade) test scores in math and reading. When students move from primary to middle school, between grades 5 and 6, they also change class peers. On average, approximately 75% of a student's middle school classmates were not in their primary school class.

Our dataset includes four cohorts of students who took the 8th grade test in May 2018, 2019, 2021, and 2022. The test was not administered in 2020 due to school closures. We use scores in math and reading, which are standardised by subject and cohort. While the 2018 and 2019 cohorts were not exposed to the pandemic before taking their 8th grade test, the 2021 cohort experienced the pandemic during grades 7 and 8, midway through middle school, while the 2022 cohort faced it during grades 6 and 7, just after starting

middle school.

Our sample is limited to students attending schools in the Centre-North of Italy. Southern regions are excluded due to the absence of 5th grade scores for many students, also caused by school strikes. For example, in 2015, nearly 40% of Southern Italian students had missing records. Following class formation rules in Italy (see D.M. 331/98), we also exclude classes with more than 30 pupils or fewer than 10 pupils, as well as schools with only one class, due to lack of variation between classes. Our final sample includes data on 1,016,806 students across 59,051 classes in 4,307 schools.

We use the 5th grade scores, pre-determined with respect to middle school class assignment, and 8th grade class identifiers g to compute student i's ordinal ability rank in subject s (math or reading). In cases of ties, we assign the minimum rank value unless otherwise stated. To facilitate comparability across classes of different sizes, we use a percentile measure of rank, normalizing the ordinal rank n_{igs} by class size N_{gs}. Rank is thus defined as:

$$\text{Rank}_{i,g,s} = \frac{n_{i,g,s} - 1}{N_{g,s} - 1}$$

Additionally, we use information on students' gender, immigrant status, and a dummy variable indicating whether at least one parent works in a teleworkable occupation, which might allow them to spend more time with their children (Aparicio-Fenoll, 2022). We also incorporate a comprehensive index of Socio-Economic Status (SES) that aggregates data on parental education, occupation, and several home facilities conducive to learning. Descriptive statistics are reported in Table 1.

Table 1: Summary statistics (Grades 5 to 8)

Academic year	2017/18		2018/19		2020/21		2021/22	
Variable	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Grade 8 math INVALSI score	0	1	0	1	0	1	0	1
Grade 8 reading INVALSI score	0	1	0	1	0	1	0	1
Ordinal grade 8 class rank based on the grade 5 math INVALSI score	0.500	0.305	0.500	0.304	0.500	0.305	0.500	0.305
Ordinal grade 8 class rank based on the grade 5 reading INVALSI score	0.500	0.305	0.500	0.305	0.500	0.305	0.500	0.305
Grade 5 math INVALSI score	0	1	0	1	0	1	0	1
Grade 5 reading INVALSI score	0	1	0	1	0	1	0	1
Female	0.502	0.500	0.500	0.500	0.496	0.500	0.496	0.500
Immigrant	0.111	0.314	0.119	0.323	0.125	0.330	0.145	0.352
Socioeconomic status index	0	1	0	1	0	1	0	1
A parent has a teleworkable job	0.712	0.453	0.709	0.454	0.712	0.453	0.700	0.458
Observations	234,872		259,130		257,838		264,966	

Notes: one observation per student. Test scores and the socio-economic status indexes are standardised by cohort.

4 Empirical Strategy

4.1 Identification

Students' ordinal ability rank in a class depends on their own ability and on the distribution of ability among their class peers. Our empirical analysis leverages idiosyncratic variation in the ordinal rank of schoolmates with the same 5th-grade ability score placed in classes with different distributions of 5th-grade ability scores. This variation arises from the arguably random sampling of small classes from the pool of students within a school cohort.

Threats to the validity of our design relate to the sorting of schoolmates across classes as well as to our ability to flexibly map the link between 5th- and 8th-grade student ability. To address these concerns, we include class-by-cohort-by-subject fixed effects in our model, which eliminate issues related to sorting across classes within school cohorts. The remaining variation depends on differences in the higher-order moments of the distribution of peers' subject-specific abilities across classes, as the within-class transformation removes differences in mean ability across classes. We further allow for a very flexible mapping between students' own 5th-grade score and the 8th-grade score by including in our model a vector of school-by-cohort-by-subject specific cubic polynomials in the 5th-grade score. We investigate the impacts of the pandemic on the relevance of ability

comparisons within classes by repeating this approach for each cohort and comparing the cohort-specific rank effects.

In a robustness check, we also exploit the availability of data for two subjects per student to run a within-pupil regression. This approach allows us to compare differences in rank in math and reading for the same student, isolating the effects due to subject-specific differences in the ability distribution within the student's class.

4.2 Estimation

We bring this approach to the data by stacking the data for our four cohorts and estimating the following linear model using Ordinary Least Squares (OLS):

$$Y_{i,s}^{8} = \beta_c \text{Rank}_{i,g,s} + \alpha_{g,c,s} + f_{p,c,s}(Y_{i,s}^{5}) + \epsilon_p \tag{1}$$

where $Y_{i,s}^{8}$ represents the 8th-grade score of student i for subject s (math or reading). We allow the effect of $Rank_{i,g,s}$, β_c, to be specific to each cohort c; $\alpha_{g,c,s}$ is a vector of class-by-cohort-by-subject fixed effects; $f_{p,c,s}(Y_{i,s}^{5})$ indicates a vector of school-by-cohort-by-subject cubic polynomials in the 5th-grade score of student i in subject s (where p stands for school, or *plesso* in Italian); and ϵ_p is an idiosyncratic error term, which we allow to be clustered at the school level. On top of allowing us to cluster standard errors by schools (and not, less conservatively, by school-cohorts), stacking the data across cohorts also has the advantage that we can formally test the statistical significance of the difference in the rank effects across cohorts. Given that we compare effects across multiple cohorts (and in the balancing analysis reported below across outcomes as well) we conservatively adjust inference for multiple testing and report False Discovery Rate-adjusted q-values instead of standard p-values (Anderson, 2008).

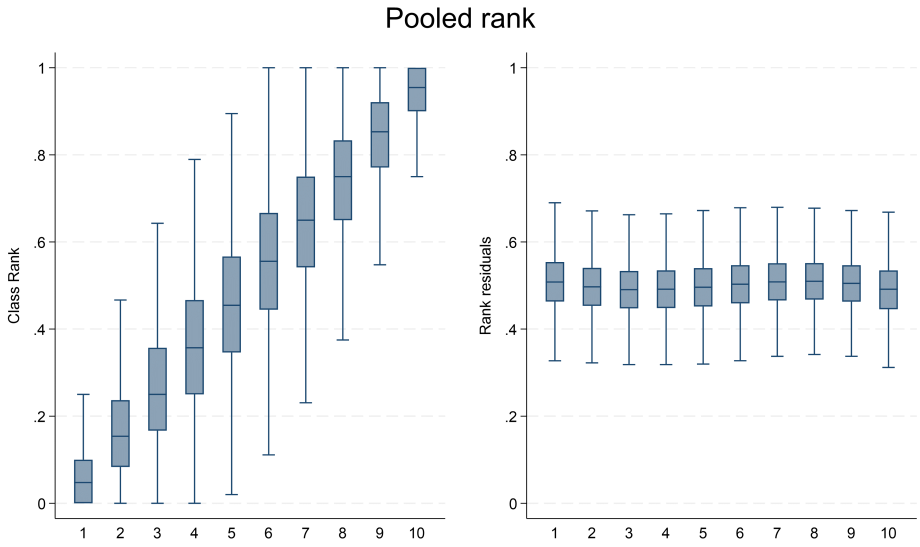
5 Results

5.1 Variability

Figure 1 shows significant variation in ability rank among students within the same decile of the 5th-grade INVALSI test score distribution. This variation is evident both in the

raw data and when considering the residuals from a regression of rank on the fixed effects and 5th-grade score controls included in our specification.

Figure 1: Box plots of observed and residual variation in the classroom rank



Notes: the left panel of the figure shows the class rank distribution for each decile of the 5th-grade INVALSI test score, pooling across subjects. The right panel displays the distributions of the residuals from a regression of the rank on class-by-cohort-by-subject fixed effects and school-by-cohort-by-subject cubic trends in the 5th-grade score. We re-centred the residuals at the mean rank value observed in the sample. The lower and upper bound of each box display the 25th and 75th percentile rank, and the top and bottom ends of each line represent the smallest and largest observed rank.

5.2 Balancing

Table 2 corroborates the validity of our identification strategy by reporting the results of a series of balancing tests. We carry out these tests by estimating our main equation with gender, immigrant background, parental tele-workability, and the SES index as outcomes. Consistent with the quasi-random assignment of students across classes, we find negligible and insignificant rank effects on these pre-class assignment student characteristics.

Table 2: Balancing tests - rank effects on pre-determined covariates

Dependent variable	(1) Female	(2) Migrant	(3) ESCS score	(4) A parent has a teleworkable job
$Rank_{ig}$, 2018 cohort, pre-pandemic	-0.000	0.007	0.021	-0.001
	[0.999]	[0.999]	[0.999]	[0.999]
$Rank_{ig}$, 2019 cohort, pre-pandemic	-0.019	0.009	-0.012	0.008
	[0.690]	[0.791]	[0.999]	[0.999]
$Rank_{ig}$, 2021 cohort, closures in grades 7 & 8	0.020	-0.007	0.037	0.004
	[0.690]	[0.999]	[0.690]	[0.999]
$Rank_{ig}$, 2022 cohort, closures in grades 6 & 7	-0.001	0.002	0.034	0.002
	[0.999]	[0.999]	[0.999]	[0.999]
Observations	2,033,612	2,033,612	2,033,612	1,683,554

Notes: Estimates are obtained from Equation (1). Regression specifications include class-by-cohort-by-subject fixed effects and school-by-cohort-by-subject cubic trends in the 5th-grade score. The number of observations declines in column 4 due to missing values in parental occupations. Inference is robust to clustering by school and to the problem of multiple testing across outcomes and cohorts. Sharpened False Discovery Rate-adjusted q-values are reported in squared brackets. $***$ p < 0.01, $**$ p < 0.05, and $*$ p < 0.10.

5.3 Main Results

Column (1) in Table 3 reports the causal effects of ordinal ability rank on students' 8th grade test score, estimated using Equation (1). The results indicate a large, positive, and statistically significant effect of rank on 8th grade INVALSI score for the 2018, 2019, and 2021 cohorts. For these cohorts, increasing a student's rank by one standard deviation (0.3) results in an average test score gain of approximately 3% of a standard deviation.

Our main finding is that this effect is two-thirds smaller for students completing 8th grade in 2022. For this cohort, a 1SD increase in rank only improves test score by 0.09SD. Stacking data across cohorts allows us to test for the significance of the difference in rank effect across cohorts. We find that the effect for 2022 is statistically different from those for the other cohorts with a p-value of 0.006 (joint test). The other effects are instead not statistically different from each other.

The 2021 and 2022 cohorts of each school experienced school closures at the same time and for the same duration, but at different grades. As a result, students in the 2021 cohort had known their peers for more than one year when the pandemic began, while those in

the 2022 cohort had just started middle school and were less familiar with each other. This evidence leads us to conclude that the timing of interactions among peers is crucial for the emergence of rank effects: the significance of interpersonal comparisons diminishes when peer interactions begin late, whereas late disruptions to already established interactions are less impactful.

Table 3: Rank effects on 8th grade INVALSI score

Dependent variable: 8th grade INVALSI score							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$Rank_{ig}$, 2018 cohort, pre-pandemic	0.075***	0.074***	0.078***	0.058***	0.085***	0.057***	0.075***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]
$Rank_{ig}$, 2019 cohort, pre-pandemic	0.085***	0.081***	0.072***	0.080***	0.085***	0.115***	0.088***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]
$Rank_{ig}$, 2021 cohort, closures in grades 7 & 8	0.103***	0.082***	0.081***	0.103***	0.089***	0.120***	0.106***
	[0.001]	[0.001]	[0.006]	[0.001]	[0.001]	[0.001]	[0.002]
$Rank_{ig}$, 2022 cohort, closures in grades 6 & 7	0.028**	0.030**	0.006	0.042***	0.030**	0.013**	0.063***
	[0.019]	[0.025]	[0.172]	[0.002]	[0.021]	[0.010]	[0.002]
Individual controls	No	Yes	No	No	No	No	No
Class-by-cohort-by-subject fixed effects	Yes	Yes	Yes	Yes	Yes	No	Yes
Pupil and subject fixed effects	No	No	No	No	No	Yes	No
School-by-cohort-by-subject cubic trends in the 5th grade score	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rank value in case of ties	Mean	Mean	Minimum	Maximum	Mean	Mean	Mean
Schools in the sample	All	All	All	All	Balanced panel	All	All
Test score definition	Levels	Levels	Levels	Levels	Levels	Levels	Percentiles
Observations	2,033,612	1,683,554	2,033,612	2,033,612	1,737,960	2,033,612	2,033,612

Notes: Estimates are obtained from Equation (1). Individual controls include a female dummy, an immigrant dummy, the SES index, and a dummy for parents' teleworkability. The number of observations declines in column 2 due to missing values in parental occupations and in column 5 as we drop schools that are not present in all years. Inference is robust to clustering by school and to the problem of multiple testing across cohorts. Sharpened False Discovery Rate-adjusted q-values are reported in squared brackets. $***$ p < 0.01, $**$ p < 0.05, and $*$ p < 0.10.

5.4 Robustness

Table 3 also shows that our main result survives several robustness tests: including gender, immigrant status, parental teleworkability, and SES as controls in the model (Column 2); using the mean (Column 3) or the maximum (Column 4) rank value instead of the minimum to handle ties; reducing the sample to include only a balanced panel of schools (Column 5); employing within-pupil regressions (Column 6); and using the standardised percentiles instead of the standardised levels of the score as a basis for the analysis (Column 7), as proposed by Murphy and Weinhardt (2020). The smaller rank effect for the

2022 cohort is qualitatively confirmed across all specifications. A remaining concern is that differences in rank effects across cohorts could date back to the pre-pandemic period, and shall not be attributed to the timing of school closures. For the cohorts exposed to the closures, we thus rely on pre-pandemic data and estimate effects of the 2nd-grade score-primary school class rank on students' 5th grade score. We find that the these rank effects are equal to 0.121 (p=0.002) for the 2021 and 0.141 (p=0.002) for the 2022 cohort, not statistically different from each other, dispelling this concern.

5.5 Heterogeneous Effects

Table 4 reports heterogeneous effects. To increase precision, we pool the 2018, 2019, and 2021 cohorts and compare them with the 2022 cohort. We find that, for the former set of cohorts, rank effects are larger among male, native, high-SES students, those with high 5th-grade score, and students whose parents had teleworkable jobs. The patterns observed for the 2022 cohort are comparable, but rank effects are reduced by roughly two-thirds for each group and are often statistically insignificant. We conclude that school closures reduced the relevance of interpersonal comparisons in a similar manner across subgroups. In unreported results, we assessed whether results differ between math and reading, but found no evidence of heterogeneous effects. Using data for the 2022 cohort, we also looked at whether rank effects vary by region depending on the duration of school closures in the 2020/21 school year, but again found no evidence of heterogeneous effects.

Table 4: Heterogeneity analysis

Sub-groups definition	(1) Female		(2) Immigrant		(3) Above median Grade 5 score		(4) Above median SES index		(5) A parent has a teleworkable job	
Cohorts	2018/19/21	2022	2018/19/21	2022	2018/19/21	2022	2018/19/21	2022	2018/19/21	2022
$Rank_{ig}$, group 0	0.082***	0.029*	0.094***	0.033*	0.082***	0.030*	0.045***	0.008	0.043***	0.006
	[0.001]	[0.096]	[0.001]	[0.073]	[0.001]	[0.097]	[0.001]	[0.430]	[0.001]	[0.602]
$Rank_{ig}$, group 1	0.093***	0.028*	0.044***	-0.004	0.093***	0.035*	0.127***	0.045**	0.097***	0.045**
	[0.001]	[0.096]	[0.001]	[0.681]	[0.001]	[0.097]	[0.001]	[0.011]	[0.001]	[0.027]

Notes: the table reports rank effects estimated for different cohorts (2018-2021 vs. 2022) and different sub-groups of the population. Estimates are obtained from Equation (1). Regression specifications include class-by-cohort-by-subject fixed effects and school-by-cohort-by-subject cubic trends in the 5th-grade score. Inference is robust to clustering by school and to the problem of multiple testing across cohorts. Sharpened False Discovery Rate-adjusted q-values are reported in squared brackets. $***$ p $<$ 0.01, $**$ p $<$ 0.05, and $*$ p $<$ 0.10.

6 Conclusions

This paper shows that the school closures prompted by the Covid-19 pandemic altered the importance of ability comparisons among classmates based on the timing of the disruptions. For students experiencing closures during their first year of middle school, when peer interactions are still developing, the impact of ordinal rank on test scores is limited. Conversely, for students affected in later grades, rank effects remain as pronounced as they were for pre-pandemic cohorts, even if peer interactions were muted for long time. These findings enrich our understanding of the education production function in learning programs with varied schedules of peer interaction - such as blended programs.

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