The good, the bad, and the average: Class rank and the transition to the labor market

Águeda Solís Alonso*

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Abstract

Using university administrative data matched with census student surveys, this paper studies the effects of class rank on job preferences, labor market prospects, and early career outcomes. I find that being top of the class affects career prospects and increases academic orientation, pushing graduates to pursue further education and to postpone labor market entry. Higher-ranked students are willing to accept more precarious employment contracts in exchange for jobs that align better with their studies, suggesting a role for intrinsic motivation. Effects are stronger for males and peer groups with higher average ability, indicating that peer competition influences rank effects.

Keywords: rank, peer effects, higher education, prospects
JEL codes: I21, I26, J24

*PhD Candidate, University of Turin and Collegio Carlo Alberto, agueda.solisalonso@carloalberto.org

1 Introduction

"There is no quality in this world that is not what it is merely by contrast. Nothing exists in itself."
— Herman Melville

Information on quality or performance is usually presented to us in the form of rankings. Whether it is about sports teams classifications, financial performance of firms, song charts, university rankings or peer-reviewed journals, comparisons and ranks emerge naturally and inevitably among groups, and are key to our understanding of the world. Rankings are particularly salient in educational contexts, where individuals interact frequently for extended periods of time, and constantly receive signals about their own performance and that of their peers through tests, evaluations, and grades. Moreover, students often face choices regarding educational investments and career choices under uncertainty about their payoffs, increasing the scope to rely on these signals to form beliefs about their ability.

In this paper, I study how a student's rank within their Master's peers affects their career prospects and choices. I combine administrative data on 10 cohorts of master's degree graduates with graduation and post-graduation surveys, which allow me to explore their preferences and intentions as well as early labor market outcomes. I exploit the quasi-randomness in peer composition, controlling for cohort, degree, and individual characteristics including ability, to identify the effects of ability rank on educational investments (academic performance and choice to pursue further education), labor market participation and earnings, and job preferences regarding job characteristics, types of employment contracts, and reserve wages. Estimates show that being top of the class increases academic orientation, pushing graduates to pursue further education and to postpone entry into the labor market. Ability rank increases the willingness to accept more precarious types of contracts in exchange for having a job that is coherent with the studies. Taken together, these findings suggest that relative position influences labor outcomes not only through educational attainment, but also by shaping career prospects and job preferences, a channel not previously documented. Importantly, rank effects are substantially stronger for males, helping explain part of the gender gap in doctoral enrollment, and in contexts where competition among peers might be more intense. My results are robust to the different identification assumptions, specifications, and measurement issues discussed in the related literature.

This study relates to the literature on rank effects, a specific type of peer effect first studied and identified by Cicala, Fryer and Spenkuch (2018) and Murphy and Weinhardt (2020), who established the fact that, independent of the effect of absolute ability, relative ability during school has an impact on future outcomes. This pattern has also been discussed in other disciplines under the name of "big fish-little pond effect" (Marsh and Parker, 1984), arguing that students form beliefs about ability based on a comparison group, and that this self-concept affects educational and labor outcomes through motivation and effort.

Since the establishment of this branch of the literature, rank effects have been identified on a variety of contexts and outcomes.[1] Closer to the current study is the subset of works on rank effects in university, and those focusing on labor market outcomes. Relative position among peers has been documented to have positive effects on credit attainment (Bertoni and Nisticò, 2023), subject and major choices (Elsner, Isphording and Zölitz, 2021), academic performance and degree attainment (Payne and Smith, 2020), and time to graduation and occupational prestige (Ribas, Sampaio and Trevisan, 2020). Similarly, rank effects identified in previous stages of education have been documented to impact earnings (Denning, Murphy and Weinhardt, 2023; Dadgar, 2024).

Most research on rank effects in higher education has focused on short-term outcomes related to academic performance and attainment. This paper contributes to the literature by examining outcomes related to job preferences, previously unexplored due to data limitations in administrative sources. By matching survey data to university records, I show that students who rank at the top among their peers not only perform better academically, but also display different career aspirations: they place a higher value on job-study alignment, and are more likely to postpone labor market entry and pursue doctoral studies. This finding extends previous work that has documented effects of rank on university enrollment and completion, and provides the first evidence that doctoral enrollment is similarly shaped by relative performance. More broadly, these findings complement the literature on rank effects in earlier stages of education, suggesting that rank-induced accumulation of human capital and delayed labor market entry may help explain effects on earnings later in life.

The remainder of this paper is structured as follows: Section 2 presents the setting, the data, and the identification strategy, Section 3 discusses the results, Section 4 explores potential mechanisms, Section 5 addresses some sensitivity checks, and finally Section 6 concludes.

[1] See Delaney and Devereux (2022) for a review of the literature.

2 Empirical strategy

2.1 Setting

This paper evaluates the research question in the context of a public university in North Western Italy, the University of Turin. Primarily located in the fourth most populated city of Italy, this is a relatively large university with around 80,000 enrolled students, which offers degrees in all disciplines except for engineering.

Like most public universities in Italy, the university under analysis is a non-selective institution, with generally non-competitive admission requirements and relatively low tuition fees[2], which results in a high heterogeneity in terms of the socio-economic background of the students, and a composition of students which is very similar to the national average. Despite some mobility, especially from the South of the country to the central area or the North, geographical position largely determines the choice of university. As such, this university has a central role in the local labor market, accruing 60% of the university students in the region where it is located, and with around 70% of students being legal residents of the region before starting their university degrees.

Tertiary education in Italy is mostly organized in 3-year Bachelor's degrees and 2-years Master's degrees, or single-cycle degrees, usually lasting for 5 years and equivalent to a Master's (common for subjects such as law, medicine, or pharmacy). Possibly because of the low economic and academic entry barriers, around two-thirds of Bachelor's graduates complete further education (OECD, 2020), a figure in line with, yet slightly higher than, other OECD countries. For this reason, Master's Degrees are the relevant qualification to study the transition into the labor market in this context.

Like in most European countries, students enroll in a specific field of study and follow a standard study plan. As per law mandate, at least 50% of the degree credits are common within each program. All students registered for a given course share a class, such that the relevant peer group in this context comprises the cohort of students undertaking the same Master's program.

Programs are typically small, with a median size of 18, and an average of 38. Small class sizes limit the scope for endogenous sorting into subgroups,

[2] According to the Italian Ministry of University and Research, around one-third of students is exempt from paying tuition, with fees being proportional to family wealth. For those who do pay them, the average tuition fee was 1281 euro in the academic year 2022/2023, representing 3% of the average household earnings.

and mitigate the downward bias in the estimates that arises from using class as the peer group, instead of unobserved subgroup structures.

2.2 Measuring ability: high-school diploma grade

An ideal measure of human capital should be salient enough so that individuals have some understanding of where they stand in relation to their peer group. At the same time, to allow for comparison and ranking across individuals, it should be standardized and not easily manipulated or confounded by unobservable characteristics. In practice, the generally available measures of ability introduce a trade-off between salience and standardization: grades are highly salient, but may not be comparable across schools or instructors, while standardized tests, if available, may not be salient to students, particularly if they are low-stakes. With these considerations, I leverage the high-school diploma grade, also known as *maturità* (maturity diploma) or *Esame di Stato* (State Exam), as my preferred measure of ability.

At the end of secondary education, the year of their 19th birthday, students take a national standardized exam that grants access to tertiary education. The exam consists of three written tests and an oral examination. One of the tests, common to all students, is on Italian language, while the other two depend on the specific high school track followed (for instance, Greek and Latin for classic studies high schools, maths and physics for scientific high schools). The exams are written centrally at the Ministry of Education and are the same for all students. A committee, specific for each class and composed by the same number of teachers from the high school and external ones (usually three of each), grades the exams.

The final grade is obtained by adding the scores in the three written tests (up to 15 points each, 45 in total), the score of the oral examination (up to 30 points), and up to 25 points which are determined from the grades of the last three years of education. The final grade is expressed in a 100 points-scale, with a minimum passing grade of 60, and it is usually published on each high school's scoreboard. Hence, students know both how they scored, and how they scored relative to peers from the same high school.

Passing the exam is a prerequisite not only for accessing higher education but also for the vast majority of jobs. While the grade is not used in the admission process in universities, which implement their own entry tests in degrees with limited amount of seats, economic benefits such as scholarships, university fee reductions and vouchers for the purchase of cultural goods and services are determined on the basis of having a high mark (typically never lower than 90/100). When it comes to employment opportunities, until 2015 there were also minimum grade requirements for accessing public employment, ranging from 70 to 80. Since then, although no longer a requirement, extra points are awarded to candidates in a wide array of public calls for vacancies in the public sector, the police and armed forces, and in private companies with public ownership. Given the high-stakes nature of the exam, it has considerable social and psychological salience, making it suitable as a measure to capture perceived relative ability[3].

Since grading depends on the committees, and a part of the score is based on high school grades, it is not a fully standardized score, raising concerns about its validity to make comparisons across individuals. Random measurement error would introduce attenuation biases in the estimation, giving as a result lower bound estimates. When it comes to nonrandom error, given that high school grades are established by different teachers across three years, and that the grading committees include members external to the high-school, concerns should be limited. Although there is a widespread perception that grading standards are more lenient in the South of the country, I do not find evidence of systematic differences within my sample between local students and students who come from a different region, who represent around one-fifth of the sample.

Proof of the validity of the diploma grade as a measure of ability in this context is its high correlation with academic performance during the master (0.72), which is also highly significant (S.E. of 0.008). Nevertheless, in Section 5, I explore the sensitivity of my results to systemic measurement error, and to leveraging two alternative measures of ability: academic performance at the current (Master's) and previous (Bachelor's) degree, potentially more salient at labor market entry than high-school diploma, albeit less standardized[4].

2.3 Data

The source of data for the analysis is a combination of administrative and survey data on Master's graduates compiled by the AlmaLaurea university consortium.

[3]While information on perceived rank is not available, a number of papers find a strong and positive correlation between perceived and actual rank (Pagani, Comi and Origo, 2021; Elsner and Isphording, 2017; Yu, 2020), suggesting that students are able to form an accurate idea of their rank through interaction with their peers, even when they are not provided with explicit information.

[4]Nation-wide mandatory standardized testing of mathematical and language competences during primary and secondary education was introduced in academic year 2008-2009, and hence only existing for cohorts that graduate university after 2017.

Every year, the consortium conducts graduates' profile surveys among the newly graduated regarding their experience during the studies and their career plans, and surveys former graduates one, three, and five years after graduation about their entry into the labor market and their employment conditions. This data is then combined with administrative records regarding their academic performance and demographic data (gender, age, province of residence, parental occupation and educational attainment, and secondary education results).

I follow ten cohorts of graduates from all the Master's degrees offered by the institution between 2008 and 2018, which amounts to 49,179 individuals across 243 different degree programs. The graduates' profile survey is a mandatory step in the graduation request procedure, ensuring a census coverage, and although there is no formal requirement to fulfill them after receiving the degree, response rates remain significantly high one year after graduation, with 82.23% of the surveyed responding.

The main outcomes used in this paper are variables measured both at graduation and one year afterwards. At graduation, I analyze variables related to academic performance during the Master's degree, such as GPA, and graduation with Honors (*Cum Laude*), as well as academic orientation, defined as their expressed interest in pursuing doctoral studies. Moreover, I study earnings prospects, measured by the survey item "what is the minimum monthly wage that you would be willing to accept for full-time employment?", which was introduced in the survey in 2015. I also investigate how these prospects and further education intentions map into real outcomes, by looking into self-reported enrollment into a PhD and average monthly earnings one year after graduating.

As secondary outcomes in order to investigate mechanisms, I make use of two sets of questions from the graduates' profile survey. In these questions, graduates are asked to provide answers regarding their preferences for different job characteristics, scaled from 1 (not important at all) to 5 (very important), and regarding their willingness to accept a specific type of employment contract, from "definitely would not accept" (1) to "would definitely accept" (5).

Table 1 reports summary statistics for the sample.

Table 1: Descriptive Statistics

	(1)		(2)		(3)		(4)	
	All		Above class median		Below class median		Difference in means	
Variable	N	Mean	N	Mean	N	Mean	Difference	P-value
Gender: Female	49,179	0.63	22,523	0.69	26,656	0.58	0.116	0.000
First-generation university graduate	46,437	0.65	21,450	0.67	24,987	0.64	0.035	0.000
Academic highschool	49,167	0.81	22,520	0.80	26,647	0.81	-0.001	0.692
Academic highschool - humanities	49,167	0.19	22,520	0.19	26,647	0.20	-0.008	0.019
Academic highschool - sciences	49,167	0.46	22,520	0.42	26,647	0.50	-0.082	0.000
Highschool diploma grade	47,776	83.35	22,523	92.86	25,253	74.88	17.978	0.000
Social class: burgeoise	34,771	0.26	16,034	0.24	18,737	0.28	-0.040	0.000
Social class: middle class	34,771	0.33	16,034	0.33	18,737	0.32	0.013	0.009
Social class: lower burgeoise	34,771	0.21	16,034	0.21	18,737	0.20	0.015	0.001
Social class: working class	34,771	0.19	16,034	0.20	18,737	0.18	0.021	0.000
Resident in the region	49,179	0.78	22,523	0.78	26,656	0.78	-0.006	0.104
Resident in a different region	49,179	0.21	22,523	0.22	26,656	0.20	0.024	0.000
Age at graduation	49,179	26.71	22,523	26.24	26,656	27.10	-0.863	0.000
Duration of studies (in years)	49,179	3.62	22,523	3.52	26,656	3.70	-0.176	0.000
GPA (1-30 scale)	49,107	27.34	22,500	27.82	26,607	26.94	0.880	0.000
Graduated with Cum Laude	49,179	0.33	22,523	0.45	26,656	0.23	0.223	0.000
Reserve wage	17,560	1288.80	8,132	1264.45	9,428	1309.80	-45.346	0.000
Wants to pursue further education	46,236	0.43	21,382	0.44	24,854	0.42	0.021	0.000
Wants to do a PhD	46,437	0.10	21,450	0.11	24,987	0.10	0.016	0.000
Is doing a PhD	40,343	0.05	18,806	0.06	21,537	0.04	0.017	0.000
Working	40,419	0.56	18,837	0.56	21,582	0.57	-0.017	0.001
Average monthly earnings(t+1)	22,059	1130.62	10,158	1119.64	11,901	1139.99	-20.345	0.006
Gap between reservation and actual wage	7,325	-94.20	3,391	-81.72	3,934	-104.96	23.239	0.094
Job search (in months)	15,138	2.82	6,928	2.79	8,210	2.85	-0.065	0.206

Note: Table includes the number of observations and mean for key individual characteristics, including Highschool diploma grade, the measure of ability used throughout this paper, and main outcome variables. Columns (2) and (3) report the same statistics broken down by whether the individual is above/below his classmates' median ability, while column (4) presents the difference in means between the previous columns and the p-value associated with a two-sided mean comparison test.

2.4 Identifying rank effects

This paper aims at identifying the effects of class rank on prospects and short-term labor market outcomes by exploiting variation in peer composition.

I use the high-school diploma grade to compute student's relative ability among their university peers. This measure is generally taken three years before Master's students start interacting with each other. While I have no information on high-school attended, it is likely that the Master's peer group differs substantially from the high-school peer group, reducing concerns about reflection and interference from class-level shocks.

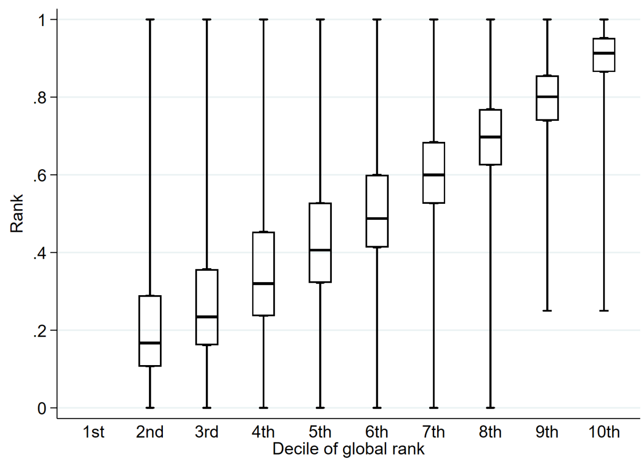
In order to compute a rank measure that is comparable across classes with different sizes, I compute the percentile rank measure:

$$Rank_{dch} = \frac{n_{dch} - 1}{N_{dc} - 1}$$

Where n_{dch} stands for the position of individuals with ability level h from degree d in cohort c (or class dc, since, commonly, all students taking a course are together in the same class). Ties are not corrected for, meaning that n_{dc} reflects the number of people in the same class who have a strictly higher ability level plus one, and those with the same level share a rank. N_{dc} stands for class size. The resulting number ranges from zero to one, with zero representing the highest ranked student (the one with the highest ability), and one the lowest ranked student. To improve the interpretability of the model results, I multiply $Rank_{dch}$ by minus ten. The associated coefficient can then be interpreted as the effect of going up by one decile in the peer ability distribution.

The identification strategy relies on comparing students with the same underlying ability who end up in different rank positions because of quasi-random variation in their peer groups. The first requisite then is that there is sufficient variation in rank conditional on ability. Figure 1 plots the relation between class and global (considering the full sample) ranks. As expected, there is a positive relationship between both, with students that rank high among their peers being more likely to rank high in the overall ability distribution. Nevertheless, conditional on ability, there is substantial variation in relative rank, with students on the 2nd to 8th decile of the global distribution ranking in all positions of their class distribution. In particular, the standard deviation of rank controlling for measured ability is of 1.33, which for the average class of size 38 translates into a variation of around

Figure 1: Local vs. global rank



Note: Figure reports the relationship between the local rank (defined at the class level) and the global ability rank (across all degrees and cohorts). The horizontal borders of the boxes indicate the 25th and 75th quintile respectively, while the line inside the box indicates the median. Brackets indicate the minimum and the maximum value of the local rank, by global rank deciles.

5 positions in the class ranking, and for the median class with 18 students, into a variation of 2 positions approximately.

Table 2: Raw and Residual Variation of Rank

	Mean	SD
Ability rank	-4.57	3.08
Residual rank, net of measured ability	0.00	1.33

Note: Table reports descriptive statistics for class rank, before and after removing the effect of measured ability.

To properly isolate the causal effect of rank from the influence of ability and other potential confounders, such as degree and cohort factors, these elements have to be properly accounted for in the empirical estimation. As it's common in the literature (Delaney and Devereux, 2022; Elsner and Isphording, 2017), I flexibly control for my measure of ability through a third-order polynomial to account for its high correlation with class rank.

Additionally, identification requires addressing two additional challenges: distinguishing rank effects from other peer effects, and accounting for potential sorting of individuals into certain degrees or cohorts.

Because ranking high among peers and having low-skilled classmates can be seen as two sides of the same coin (Bertoni and Nisticò, 2023), assumptions need to be imposed on peer effects in order to identify class rank effects. Denoting the relationship between outcomes and rank as

$$Y_{dch} = \alpha + \beta R_{dch} + v_{dch} \tag{1}$$

where R_{dch} stands for our rank measure and v_{dch} for the error term, which depend on the degree (d), cohort (c) and ability level (h) of the individual. The error term includes any possible peer effects, or other class-level effects related to instructors or different factors. Identification requires restrictions on v_{dch}, and hence on the sort of peer and class effects that are assumed in the model.

In this paper, I assume the error term can be summarized by an additive classroom (dc) effect and an additive ability effect, so that $E(v_{dch}|d,c,h) = \gamma_h + \theta_{dc}$. This assumption allows for cohort-varying effects within a particular degree and for peer effects different than rank effects, but implies that peer effects are homogeneous across different levels of human capital.

Identification comes from comparing cases where differences in rank across ability levels are not homogeneous across peer groups, in other words, from differences in the variance and higher moments of the human capital distribution across classes. For an illustrative example, see Figure A.1 in Appendix A.1.

Imposing a weaker assumption, while allowing for the inclusion of the interaction of class and human capital effects, reduces the identifying variation and obscures its interpretation. For this reason, estimations following different versions of this assumption have been relegated to a series of sensitivity checks in Section 5.

Additionally, causal identification requires quasi-random assignment of individuals to peer groups. If this assumption is violated and individuals have a preference for ranking high among their peers (for status reasons) or if peer groups form endogenously, for instance on the basis of gender and social background, there will be a correlation between rank and individual characteristics. In this case, rank effects would be inconsistenly estimated. For this reason, I include controls for individual characteristics in my baseline model, and I implement an alternative strategy to deal with potential sorting in Section 5: including interactions of the ability measure with class distribution types (Denning, Murphy and Weinhardt, 2023).

With the previous considerations, my preferred specification to estimate the causal effects of class rank on aspirations, job preferences and short-term labor market outcomes is the following:

$$Y_{dch} = \alpha + \beta R_{dch} + f(H) + \gamma' \mathbf{X} + \sigma_{dc} + e_{dch} \quad (2)$$

Where Y_{dch} represents one of the outcomes described in Subsection 2.3, $f(H)$ is a third-order polynomial of the human capital measure, $\mathbf{X}$ stands for a set of individual characteristics such as gender, age at graduation, parental education and region of residence, and σ_{dc} are degree-by-cohort (class) fixed effects. The main coefficient of interest is β, which should be interpreted as the average causal effect of moving up by one decile in the class ability rank. Standard errors are clustered at the class level, the level at which rank is allocated.

As a further check for the absence of sorting, I run a version of my preferred specification, Equation 2, using as dependent variables the available individual characteristics instead of including them as controls.

Table 3: Randomisation check: Effect of Rank on Individual-Level Characteristics

	(1) Gender: Female	(2) Age at graduation	(3) Resident in the region	(4) Academic hs - humanities	(5) Academic hs - sciences
Rank	-0.00	-0.03	-0.00	0.00	0.00
	(0.00)	(0.02)	(0.00)	(0.00)	(0.00)
	(6) Social class: burgeoise	(7) Social class: lower burgeoise	(8) Social class: middle class	(9) Social class: working class	(10) First-gen
Rank	-0.00	-0.00	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Note: Table displays the effect of class ability rank on individual characteristics, after controlling for a third-degree polynomial of the ability measure and class fixed effects, in a similar fashion to the main specification Equation 2. Clustered standard errors are reported between parentheses.

Results, displayed in Table 3, do not suggest evidence of sorting. Under the assumption that unobservable characteristics behave the same way, sorting should not be a concern in this setting. As additional support of this claim, in Section 5 I discuss the sensitivity of my estimates to the exclusion of these controls.

3 Results

3.1 Main results

Table 4 reports my main estimates of the effect of class rank on academic performance, motivation and labor participation based on Equation 2.

Table 4: Rank effects on academic performance and motivation and labour supply

Panel A: at graduation				
	(1) GPA (18-30 scale)	(2) Graduated Cum Laude	(3) Wants to do a PhD	(4) Reserve wage
Rank	0.047***	0.015***	0.008***	-7.12*
	(0.008)	(0.003)	(0.002)	(3.95)
N	47,124	47,169	47,169	19,005
R^2	0.467	0.212	0.215	0.204
Panel B: one year after graduation				
	(5) Employment	(6) PhD enrollment	(7) Avg. monthly earnings	
Rank	-0.014***	0.006***	-6.79	
	(0.003)	(0.002)	(4.32)	
N	37,900	37,837	20,490	
R^2	0.148	0.219	0.271	

Note: Table displays the coefficients associated to class rank for separate regressions, together with the associated sample size (N), and the adjusted R-squared. Outcomes are variables related to academic performance and motivation and labor supply. Columns (1) - (4) reflect outcomes measured at graduation, while columns (5) - (7) refer to outcomes measured one year after. Reserve wage is only available for cohorts who graduated after 2014. All regressions control for a third-degree polynomial of the ability measure, individual controls (gender, age at graduation, region of residence and parental educational attainment) and class fixed effects. Clustered standard errors are reported between parentheses. *, **, *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

Results show that ranking one decile closer to the top of the class translates into an improvement of 0.047 points (in a 30-points scale) in grade point average, which at the same time increases the probability of graduating with Latin Honors (*Cum Laude*) by 1.5 percentage points. These improvements in academic performance are accompanied by increases in the self-reported interest in pursuing further studies, in particular a doctoral degree, an interest that appears to largely materialize one year after, with an increase of 0.6 percentage points in the probability of being enrolled into these type of studies, and a decrease of 1.4 percentage points in the probability of being employed. While a one-decile improvement in class rank induces graduates' willingness to accept lower salaries, with a fall of 7.12 euros in their reservation wage, this difference is not reflected in actual monthly earnings, for which the coefficient is similar in size (-6.79), but statistically indistinguishable from zero.

In order to make sense of the magnitude of these average effects, it is useful to put into perspective what a decile represents in this sample. In an average class with 38 students, each rank decile represents roughly 4 classmates, hence moving up one decile typically means outperforming about 4 more classmates.

Estimates of the effect of rank on grades are qualitatively in line with previous findings, albeit relatively lower in magnitude. Differences in setting, measurement of human capital, timing of measurement of key variables, and specification are the potential driving forces behind the divergences. The 0.047 points increase in GPA represents 0.049 standard deviations, a third of the effect of 0.149 found by Denning, Murphy and Weinhardt (2023) for primary students in Texas (US) five years after ability measurement (same lapse as in this case). Closer to my estimates are those of Elsner, Isphording and Zölitz (2021), who find class rank in a specific subject increases grades in follow-up courses by 0.0403 standard deviations, or Payne and Smith (2020)'s documented effect of 0.083 standard deviations one year after entering university in Ontario, suggesting that class rank effects may have a greater impact on earlier stages of education. The exception to this pattern is Ribas, Sampaio and Trevisan (2020), who relying on a different identification strategy based on a discontinuity in class assignment rules, find an effect of 0.336 standard deviations among the students of a Brazilian elite university.

On the other hand, the null effect on average monthly earnings contrasts with Denning, Murphy and Weinhardt (2023) and Dadgar (2024)'s positive estimates. Divergence with Denning, Murphy and Weinhardt (2023) could be explained by differences between the US and Italy in the labor market prospects of recent graduates, or by differences in the sample of interest. While they study all elementary school students, my analysis is focused on Master's degree graduates, a more selected sample in which variation in labor market outcomes might not be as marked. Regarding Dadgar (2024) instead, in this case earnings are measured in the mid-to-late thirties, a sample roughly ten years older than mine, hence likely driving this difference.

It is not possible to compare the remaining results with the existing literature. To the best of my knowledge, no effects on willingness to pursue doctoral studies, PhD enrollment, or reserve wages have been documented before. Nevertheless, rank effects on educational attainment have been found in different contexts (Payne and Smith, 2020; Elsner and Isphording, 2017; Dadgar, 2024; Denning, Murphy and Weinhardt, 2023), thus suggesting some degree of external validity of these effects.

3.2 Gender heterogeneity

The previous subsection describes the finding that class rank induces changes in academic performance, academic motivation, and the decision to postpone labor market entry. A related relevant question then is whether these effects differ between males and females. The evidence provided by the existing literature has so far been mixed, with some studies finding stronger effects for males (Bertoni and Nisticò, 2023; Murphy and Weinhardt, 2020; Elsner, Isphording and Zölitz, 2021), and others documenting statistically similar patterns (Denning, Murphy and Weinhardt, 2023; Payne and Smith, 2020).

Females, despite being over-represented in higher education, and at the top of the grades distribution, are less likely to continue their education towards a doctoral degree. In Italy, the percentage of females with a Master's Degree that enroll in a PhD is 18% , which is 24 percentage points below the same share for males. This makes the transition from Master's Degrees to Doctoral Degrees the stage towards an academic career in which the drop in the share of females is the most pronounced: the percentage of females falls from roughly 70% at the Master degree level to 50% at the doctoral stage, followed by the transition from Assistant Professor to Professor, in which the share of females falls from 46% to 36%[5].

While the topic of gender differences in promotions in the academic profession has received considerable attention from the research standpoint (Bosquet, Combes and Garcia-Penalosa, 2013; Ginther and Kahn, 2004, 2009), particularly in the Italian context (De Paola and Scoppa, 2015; De Paola, Ponzo and Scoppa, 2015; Bagues, Sylos-Labini and Zinovyeva, 2017; Zinovyeva and Bagues, 2015), little is known about what shapes the decision to pursue doctoral studies, and about the possible gender differences in this choice.

Existing literature has found that beliefs on ability and returns are determinants of the decision to pursue further education (Boneva, Golin and Rauh, 2022; Belfield et al., 2020; Boneva and Rauh, 2017). If there are

[5]Figures are slightly different in other European countries, with females being generally less over-represented in higher education. The corresponding fall at the Doctoral stage is of 11 pp, slightly below the drop at the transition to Professorship (14 pp). However, data from the European Commission (Commission, for Research and Innovation, 2021) confirm the pattern that the PhD is the stage at which female underrepresentation in academia starts. See Appendix A.2.

differences in how males and females interpret class rank, or in their job preferences, we could expect to find asymmetric results of class rank on the decision of getting a doctoral degree, and career choices in general.

Regarding the formation of beliefs, Chevalier et al. (2009) finds that females rely less on peer comparison to form beliefs about their ability. Similarly, the literature on gender differences in belief updating has found that women respond less to positive feedback (Möbius et al., 2022) and more to negative feedback (Shastry, Shurchkov and Xia, 2020), although results generally depend on the nature of the task and the gender stereotype associated with it (Bordalo et al., 2019; Coffman, Collis and Kulkarni, 2024).

When it comes to preferences, the literature on occupational segregation and gender gaps has highlighted gender differences in attitudes towards risk and competition (Cortes and Pan, 2018; Niederle and Vesterlund, 2007), relative importance of earnings versus pro-social aspects of jobs (Lepinteur and Nieto, 2025), valuation of non-wage characteristics such as commuting time or work-life balance (Fluchtmann et al., 2024), or in the intensity of these preferences (Lordan and Pischke, 2022). If females perceive the academic career as risky, competitive, or lacking work-life balance, they may be less inclined to pursue a PhD. Moreover, anticipated discrimination can be an additional discouraging factor (Lepage, Li and Zafar, 2025).

Both factors point to the same hypothesis: rank effects on academic performance and motivation are expected to be larger for males than for females. This hypothesis is supported by my findings, as shown in Table 5. The size of the estimates is consistently larger for males for all outcomes except for Reservation wages. The differences are statistically significant for GPA, interest in doing a PhD and actual PhD enrollment, for which the males' estimate more than doubles the females', suggesting that differential responses to class rank could contribute to the observed drop in the share of females from the Master's level to the Doctoral level.

Some back-of-the-envelope calculations might be useful to understand the contribution of this differential response to the gap in academic aspirations. If females' response to class rank had the same intensity as males' (that is, if their marginal effect on interest in a PhD was 1 percentage point instead of 0.6), the share of females in the potential pool of PhD applicants (assuming people who report being interested in doing a PhD actually apply for admission) would increase by 5 percentage points, roughly 534 people, representing a 20% increase of the baseline. Assuming that PhD selection committees don't have a preference for candidates of one gender conditional on their quality, and that the total number of available funded PhD positions is fixed, the share of females enrolled in a PhD would increase by approx-

Table 5: Rank effects on academic performance and motivation and labour supply, by gender

Panel A: at graduation	(1) GPA (18-30 scale)	(2) Graduated Cum Laude	(3) Wants to do a PhD	(4) Reserve wage
Gender: Male × Ability rank	0.063***	0.017***	0.010***	-5.79
	(0.008)	(0.003)	(0.002)	(4.05)
Gender: Female × Ability rank	0.036***	0.013**	0.006***	-8.08*
	(0.008)	(0.003)	(0.002)	(4.17)

Panel B: one year after graduation	(5) Employment	(6) PhD enrollment	(7) Avg. monthly earnings
Gender: Male × Ability rank	-0.017***	0.010***	-8.981*
	(0.003)	(0.002)	(4.69)
Gender: Female × Ability rank	-0.012***	0.004**	-5.47
	(0.003)	(0.002)	(4.37)

Note: Table displays the coefficients associated to class rank for separate regressions. The main explanatory variable, *rank*, is interacted with an indicator for graduates' gender. Outcomes are variables related to academic performance and motivation and labor supply. Columns (1) - (4) reflect outcomes measured at graduation, while columns (5) - (7) refer to outcomes measured one year after. Reserve wage is only available for cohorts who graduated after 2014. All regressions control for a third-degree polynomial of the ability measure, individual controls (gender, age at graduation, region of residence and parental educational attainment) and class fixed effects. Clustered standard errors are reported between parentheses. *, **, *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

imately 8 percentage points, closing around two-thirds of the gap between the share of female PhD students (50%) and the share of female Master's Degrees graduates (63%).

These figures suggest a role for differential reactions to ability signals by gender, rather than differences in ability itself, in explaining the smaller predisposition of females toward academic careers. Unfortunately, whether these are driven by different preferences or differences in the formation of beliefs cannot be answered with the available data. Further research is needed to answer this question and help formulate effective policy that contributes to fixing the *leaky pipeline* in academia.

4 Mechanisms

Why does class rank affect academic performance and aspirations? The existing literature has proposed several potential explanations underlying the effects.

First, top students may have a higher preference for competition, be it driven by a pursuit of self-improvement, or by a concern to preserve their relative position in the class. Testing whether rank estimates are larger in contexts where students face higher levels of competition provides suggestive support for the hypothesis that competitive preferences may play a role in the results. Competition for being the best student is arguably more pronounced in classes with high-skilled peers, and the higher willingness to compete of males with respect to females is a consistent finding of the competitiveness literature (Niederle and Vesterlund, 2007; Buser, Niederle and Oosterbeek, 2014; Flory, Leibbrandt and List, 2015). Thus, if competition plays a role in rank effects, we should observe larger effects in classes with higher average ability and classes with a higher proportion of males. Table 6 collects the results of these heterogeneity analyses and confirms this intuition. Point estimates are consistently higher for classes with an average ability above the mean and those with a proportion of males above the median. The difference between both is statistically significant for the probability of employment in both cases, and for expected and actual earnings in the comparison between high-ability and low-ability classes.

Table 6: Mechanisms: competitiveness

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Rank (baseline estimates)	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Rank×class with high average ability	-0.015***	0.007***	0.046***	0.015***	0.008***	-7.86**	-6.98
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.98)	(4.37)
Rank×class with low average ability	-0.010***	0.003*	0.053***	0.012***	0.007***	-1.96	-6.74
	(0.003)	(0.002)	(0.011)	(0.003)	(0.002)	(4.73)	(4.61)
Rank×class with high prop. of males	-0.014***	0.007***	0.054***	0.016***	0.008***	-7.08*	-8.68*
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.96)	(4.46)
Rank×class with high prop. of females	-0.013***	0.005***	0.037***	0.013***	0.007***	-7.23	-4.42
	(0.003)	(0.002)	(0.009)	(0.003)	(0.002)	(4.47)	(4.41)

Note: Table reports estimates of class rank effects on the outcomes of interest for different types of classes, categorized according to their average ability measure, and by its gender composition.

Similarly, class rank could have an effect through other intrinsic factors, such as perceived ability and returns from education, or confidence, motivation and effort. If students infer from their rank that they are highly-skilled, they may also adjust their perceptions of their returns to work and to further education and decide to continue their studies. Similarly, students with similar ability but with a higher relative position may be more confident and motivated, and thus put more effort into studying and make more ambitious choices. This beliefs channel has received the greatest amount of support from the existing literature (Elsner and Isphording, 2017; Elsner, Isphording and Zölitz, 2021; Murphy and Weinhardt, 2020; Pagani, Comi and Origo, 2021). In this paper, I explore the role of academic motivation and engagement with the studies by exploiting a series of questions regarding the declared importance of different aspects of a job. Students are asked to rate in a scale from 1 (not important at all) to 5 (very important) the importance of earnings, career progression, stability and security, acquisition of professional skills, coherence with studies, match with cultural interests, independence and autonomy, and free time for choosing a job. Similarly, to explore the roles of perceptions on labor market returns, I use a set of questions about their willingness to accept different types of labor contracts. Additionally, I look at whether the results differ depending on the opportunity costs associated with pursuing a PhD. I classify degrees into high-opportunity-cost-degrees and low-opportunity-cost ones based on whether the average earnings of their graduates is above or below the median. Results displayed in Tables7, 8, and 9 provide support for the academic motivation channel, and against the hypothesis of the adjustment of perceived returns. Students who rank one decile higher among their peers than other students with similar levels of ability and socio-demographic characteristics report that coherence with studies is more important for them when choosing their future job. Although small in magnitude (1% of a standard deviation), this effect is statistically significant at the 10% level, and it's the only statistically significant difference in job preferences. The effect of class rank on the importance of earnings, although statistically indistinguishable from zero, is negative. Along these lines, effects of class rank are homogeneous along degrees with different average earnings, confirming that the observed class rank effects may not be driven by adjustments in the expected returns to education, as seen also in the effects on reserve wages.

When asked about their willingness to accept different types of contracts, students with a higher relative position are more willing to accept more precarious sorts of employment such as internships, apprenticeships, or project-based collaborations. Overall, these effects suggest that graduates with a better relative performance are willing to trade earnings and employment conditions for having jobs that match their interests and their studies.

Table 7: Rank effects: importance of aspects of a job

	(1)	(2)	(3)	(4)
	Earnings	Career progression	Stability and security	Gaining skills
Rank	-0.005	-0.004	-0.006	-0.003
	(0.005)	(0.005)	(0.005)	(0.003)
N	46,273	46,219	46,150	46,169
R^2	0.059	0.108	0.049	0.029
	(5)	(6)	(7)	(8)
	Coherence with studies	Cultural interests	Independence and autonomy	Free time
Rank	0.009*	0.004	0.001	-0.008
	(0.005)	(0.005)	(0.005)	(0.006)
N	46,190	46,042	46,109	46,094
R^2	0.095	0.064	0.028	0.035

Note: Table displays the coefficients associated to class rank for separate regressions, together with the associated sample size (N), and the adjusted R-squared. Dependent variables are self-reported importance of different aspects of a job, measured in a scale ranging from 1 (not important at all) to 5 (very important).All regressions control for a third-degree polynomial of the ability measure, individual controls (gender, age at graduation, region of residence and parental educational attainment) and class fixed effects. Clustered standard errors are reported between parentheses. *, **, *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

Table 8: Rank effects: willingness to accept employment types

	(1) Insertion	(2) Project-based	(3) Temporary	(4) Interim
Rank	0.016*	0.022**	-0.017	-0.034
	(0.008)	(0.010)	(0.032)	(0.039)
N	29,550	31,083	47,169	47,169
R^2	0.088	0.076	0.035	0.036

	(5) Part-time	(6) Full-time	(7) Internship	(8) Remote work
Rank	-0.021	-0.021	0.014*	0.004
	(0.032)	(0.021)	(0.007)	(0.036)
N	47,169	47,169	45,241	47,169
R^2	0.039	0.032	0.105	0.044

	(9) Occasional collaboration	(10) Increasing responsibilities	(11) Apprenticeship	(12) Self-employment
Rank	-0.098	-0.098	0.015**	-0.027
	(0.101)	(0.090)	(0.007)	(0.035)
N	14,183	16,086	45,140	47,169
R^2	0.054	0.047	0.118	0.038

Note: Table displays the coefficients associated to class rank for separate regressions, together with the associated sample size (N), and the adjusted R-squared. Dependent variables are self-reported willigness to accept different types of employment contracts, measured in a scale ranging from 1 (very unlikely) to 5 (very likely).All regressions control for a third-degree polynomial of the ability measure, individual controls (gender, age at graduation, region of residence and parental educational attainment) and class fixed effects. Clustered standard errors are reported between parentheses. *, **, *** indicate statistical significance at 10%, 5%, and 1% level, respectively.

Table 9: Mechanisms: expectations on returns

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Rank (baseline estimates)	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Rank×high opportunity cost degree	-0.015***	0.006***	0.054***	0.014***	0.007***	-8.08**	-7.21
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.84)	(4.38)
Rank×low opportunity cost degree	-0.014***	0.006***	0.023***	0.014***	0.008***	-7.45	-5.35
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(4.58)	(4.34)

Note: Table reports estimates of class rank effects on the outcomes of interest for different types of degrees, categorized on the basis of graduates' average earnings.

Third, class rank could trigger an external response from parents, who may decide to increase their investments in their children, or from the academic environment, making additional or different resources available to the best-ranked students. Although my data does not allow to assess the potential role that professors or university resources may play, I leverage the available information on parental education and occupation to discern the possible mediator role of parental response. More educated and wealthy households may have a higher degree of awareness and involvement on their children's education (Houtenville and Conway, 2008; Cobb-Clark, Salamanca and Zhu, 2019; Kalil, Ryan and Corey, 2012) and more capacity to react to information about their performance (Graetz, Öckert and Skans, 2023). In this sense, I run two heterogeneity analyses by interacting the main variable of interest, *rank*, first with an indicator for social class based on parental occupation, and then with an indicator for whether at least one of the parents has achieved tertiary education. Nevertheless, it is unlikely that parents play a substantial role at this stage, given that they are probably unaware of their child's rank, and less influential than at earlier stages of education. In line with this, the outcomes for which rank effects diverge the most are those related to academic performance, GPA and the probability of graduating with honors, while for outcomes related to employment and further studies choices, rank effects seem not to depend on household economic or educational background.

Table 10: Mechanisms: parental involvement

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Rank (baseline estimates)	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Rank×working class	-0.016***	0.005**	0.021**	0.006*	0.005*	-10.578**	-5.669
	(0.004)	(0.002)	(0.010)	(0.003)	(0.002)	(5.296)	(5.810)
Rank×middle class	-0.018***	0.008***	0.042***	0.012***	0.007***	-5.511	-9.339
	(0.004)	(0.002)	(0.009)	(0.003)	(0.002)	(4.899)	(5.767)
Rank×lower burgeoise	-0.013***	0.005**	0.043***	0.013***	0.006**	-5.485	-7.247
	(0.004)	(0.002)	(0.010)	(0.003)	(0.002)	(5.034)	(5.715)
Rank×burgeoise	-0.020***	0.009***	0.055***	0.018***	0.008***	-11.166**	-10.383*
	(0.004)	(0.002)	(0.009)	(0.003)	(0.002)	(4.912)	(5.975)
Rank×first-gen	-0.017***	0.006***	0.037***	0.010***	0.006**	-6.095	-8.533
	(0.004)	(0.002)	(0.010)	(0.003)	(0.002)	(5.22)	(5.38)
Rank×no first-gen	-0.018***	0.008***	0.051***	0.016***	0.008***	-10.643**	-8.614
	(0.004)	(0.002)	(0.009)	(0.003)	(0.002)	(4.792)	(5.788)

Note: Table reports estimates of class rank effects on the outcomes of interest for different types of individuals, categorized on the basis of their household background and the educational attainment of their parents.

Finally, there could be concerns that the increased probability of being enrolled into a PhD could simply reflect that PhD admission committees use cohort rank as a measure of candidates' performance. I argue that it is unlikely that this is the sole driver of this effect, given that I also find an increase in the intentions of pursuing doctoral studies before graduation. Moreover, only 36% of the students who are doing a PhD are enrolled in the same university where they pursued their Master's Degree. While there is likely to be some overlap between Professors who teach at the Master Degree's and Professors who sit in the PhD admissions committee in the same department, and hence information on rank may be available and used to select candidates, this information is hardly available for other departments and universities.

Nevertheless, I provide an additional check in the fashion of Elsner, Isphording and Zölitz (2021). I modify my preferred specification and include the Master's Degree graduation mark as a control. If rank effects persist after controlling for the graduation mark, this is indicative of rank having an independent effect on PhD enrollment beyond acting as a proxy for quality for committees[6]. This analysis is presented in Table 11.

[6]Given that graduation marks are affected by rank, the inclusion of this variable as a control introduces post-treatment or bad control bias (Angrist and Pischke, 2009), hence the interpretation of the rank coefficient is no longer causal

Table 11: Mechanisms: University selection

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Rank (baseline estimates)	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Rank (controlling for final mark)	-0.014***	0.006***	0.027***	0.010***	0.007***	-7.14*	-7.20*
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(3.95)	(4.31)

Note: Table reports the baseline estimates of class rank effects on the outcomes of interest, and a comparison with the estimates after including the Master's Degree final mark as a control.

To summarize, the mechanisms for which I find a greater amount of support with the available data are competitiveness and motivation and effort, in line with previous findings of the related literature (Elsner and Isphording, 2017; Elsner, Isphording and Zölitz, 2021; Murphy and Weinhardt, 2020; Pagani, Comi and Origo, 2021). While my data does not allow me to test for the potential role of Professors' and university resources' response, this channel has been found to have a minor role in other contexts (Pagani, Comi and Origo, 2021).

5 Robustness checks

In this section, I test the sensitivity of my results to alternative identifying assumptions proposed by the literature, and to some critical issues related to the measurement of the main variable, class rank.

Table 12: Sensitivity to different specifications

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Baseline estimates	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Additive degree and cohort effects							
Degree and Cohort F.E.s	-0.009***	0.005***	0.018**	0.006**	0.007***	-7.97**	0.27
	(0.003)	(0.001)	(0.007)	(0.002)	(0.002)	(3.78)	(3.55)
+ Individual characteristics	-0.009***	0.005***	0.021***	0.006**	0.007***	-6.85*	-1.93
	(0.003)	(0.001)	(0.007)	(0.002)	(0.002)	(3.72)	(3.55)
+ Class ability	-0.012***	0.006***	0.043***	0.014***	0.008***	-7.35*	-6.98*
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.99)	(4.24)
+ Class heterogeneity	-0.012***	0.006***	0.043***	0.014***	0.008***	-7.35*	-6.90
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.99)	(4.24)
Parametric functions of class and ability							
H×class ability, H×class heterogeneity	-0.009***	0.005***	0.033***	0.018***	0.009***	-1.92	6.79
	(0.003)	(0.002)	(0.010)	(0.003)	(0.002)	(4.76)	(4.65)
H×class distribution types	-0.011***	0.007***	0.047***	0.021***	0.009***	-1.75	-2.57
	(0.003)	(0.002)	(0.006)	(0.003)	(0.002)	(4.44)	(4.52)
Global H decile dummies	-0.012***	0.006***	0.051***	0.014***	0.007***	-8.07**	-5.84
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(4.01)	(4.56)
Measurement of Rank							
Using Bachelor's grades	-0.000	0.011***	0.003	0.017***	0.014***	-4.38	-9.19*
	(0.004)	(0.002)	(0.011)	(0.004)	(0.003)	(4.69)	(5.28)
Using Master's grades	-0.002	0.007***	-	0.025***	0.008***	-22.42***	-10.42**
	(0.003)	(0.002)	-	(0.003)	(0.002)	(5.21)	(4.28)
Same-sex peer group	-0.007***	0.002	0.023***	0.007***	0.001	-4.31	-5.73*
	(0.002)	(0.001)	(0.006)	(0.002)	(0.001)	(3.04)	(3.17)
Based on standardized measure	-0.014***	0.006***	0.035***	0.008**	0.009***	-6.77	-7.59
	(0.004)	(0.002)	(0.010)	(0.003)	(0.002)	(5.22)	(5.38)

Note: Table reports estimates of class rank effects on the outcomes of interest across different model specifications.

I start by substituting degree-by-cohort fixed effects by separate degree and cohort fixed effects, effectively assuming that the error term in Equation 1 is composed by additive degree, cohort and ability effects. While this is a stronger assumption than the one used in my preferred specification, as it imposes the absence of effects of class-specific factors different than rank, comparing both still gives a useful intuition on which variables might be important for teasing out selection and other confounding effects from causal rank effects.

This specification tends to either leave estimates practically unchanged, as it is the case for interest in PhD and PhD enrollment, or provide downward-biased estimates for the rest of the variables. This suggests that there are peer effects in place for the affected outcomes, and that these peer effects have an opposite effect than that of rank.

Adding individual characteristics (gender, age at graduation, parental education and region of residence) as controls to the model does not affect the estimates. As discussed in Subsection 2.4, this confirms the little potential for concerns of selection into peer groups. On the other hand, adding average class ability as a control systematically brings the estimates closer to the baseline, making both sets of estimates statistically indistinguishable. This is in line with the findings of Bertoni and Nisticò (2023), evidencing that ranking high among peers and the average ability of peers are two sides of the same coin, and that average peer quality is an important confounder.

Turning into a weaker identifying assumption instead, I introduce parametric functions of class and ability effects into the model, allowing for heterogeity in rank effects across class characteristics. I start by replicating Bertoni and Nisticò (2023)'s approach by introducing interactions of my measure of ability with the class mean and the class variance. Estimates on the probability of employment, GPA and reserve wage slightly shrink in size, while PhD intentions and actual enrollment remain practically unchanged, and the effect on average monthly wages, while still statistically non-significant, changes sign and becomes positive. This suggests that, while PhD-related outcomes may not be responsive to class characteristics, the rest of outcomes may be.

Next, I follow Denning, Murphy and Weinhardt (2023) and categorize classes according to their ability distribution, creating 16 types by dividing the sample according to mean and variance quartiles. Conclusions are similar to the previous exercise, with point estimates being statistically indistinguishable from the baseline model.

The last specification check regards the flexible inclusion of ability effects to correctly account for its high correlation with the main variable of interest. In this sense, following Murphy and Weinhardt (2020), I substitute the third-order polynomial used in the baseline estimates with an indicator variable for each decile of the global ability distribution. Again, estimates remain practically unaltered.

Next, I test the sensitivity of my estimates to aspects related to the measurement of rank: the timing of measurement, the peer group that is considered for building the rank variable, and the potential threat of multiplicative error in the measurement of ability.

There is a trade-off between employing measures of ability that are measured before or after the peer group forms. While innate ability should remain more or less constant across the life cycle, rank can plausibly affect human capital accumulation. If rank affects human capital and human capital affects rank contemporaneously, this could introduce a reflection problem (Manski, 1993), which favors measures that precede peer group formation.

On the other hand, measures of ability that are taken after the peer group forms have the advantage that they might be more salient, as students are more aware of the current performance of their classmates, revealed through repeated evaluations, than of their past performance. Since perceived rank is what determines self-concept and behavior changes instead of actual rank, using a contemporary or a more recent measure may be more accurate. I propose two alternative measures of ability based on academic performance: the final grade achieved in the previous degree attained (Bachelor's Degree), and GPA of the Master's degree in course. In both cases, estimates on self-reported interest in PhD studies and actual enrollment remain unchanged, whereas the detrimental effect of class rank on employment becomes smaller and statistically insignificant. Effects on reserve and actual wages increase in size and become statistically significant when using current academic performance, with a decile increase translating into a fall of 22.42 euro in reserve wages, and a fall of 10.42 euro on average monthly earnings one year after graduation. When using Bachelor's grades, the effect on reservation wage falls in magnitude and loses significance, while the impact on actual earnings remains negative (9.19 euro) and statistically significant at the 10% level.

Previous research has argued that same-sex peers might be the relevant group for social comparison, making gender-specific ranks within the class more appropriate to capture these effects. Using same-sex peers to construct the rank measure causes a shrinkage of all estimates. A possible reason is that females, who make up almost two-thirds of the sample, and have on average a higher rank position, are less susceptible to rank effects, as suggested by the results of this paper.

Multiplicative measurement error occurs when the measurement error increases with distance from the average. In this context, because the sample includes only those who graduate, and not the entirety of the enrolled, and rank has been shown by the literature to impact graduation (Ribas, Sampaio and Trevisan, 2020; Dadgar, 2024), class rank is consistently overestimated. The overestimation of the explanatory variable can not be measured because of lack of information on the drop-outs. Nevertheless, this error introduces a downward bias in the estimates, underestimating the true effect size. Instead, measurement error for the ability measure could happen for instance if teachers grade on a curve, favoring the high achievers. In this case, in order to correct for multiplicative measurement error, I follow Murphy and Weinhardt (2020): I standardize the ability measure into a uniform distribution, I construct the class rank variable based on the standardized ability measure, and I re-run my preferred specification. Results, captured in the last row of Table 12, show that, while GPA, probability of graduating

Cum Laude, and reserve wage point estimates shift downwards, they remain statistically significant. The rest of the estimates remain unchanged.

Finally, I address concerns that rank effects may be non-linear. I substitute the continuous rank variable for decile indicators, and I plot the baseline effect against the estimates for each decile. Results, shown in Figure A.3 in the Appendix, do not suggest important non-linearities.

6 Conclusion

This paper provides new evidence on how class rank within university cohorts influences students' academic motivation, job preferences, and short-term labor market outcomes. Leveraging a rich dataset that combines administrative records and survey responses from ten cohorts of Master's graduates at a large Italian public university, I exploit quasi-random variation in peer composition to identify causal effects of relative ability rank.

I find that a student's relative position in the academic distribution has significant effects on their post-graduate decisions and behavior. Higher-ranked students are more likely to pursue further education, delay entry into the labor market, and accept less secure employment in favor of jobs that align with their field of study, suggesting that motivation and alignment with intrinsic interests play a significant role.

The influence of class rank is more pronounced in contexts where peer competition is arguably more salient, such as male-dominated classes and higher average ability peer groups, pointing to competitiveness and peer-driven motivation as key mechanisms. Importantly, females are less responsive to class rank than males, especially in terms of PhD aspirations and enrollment, suggesting that rank-driven self-perception may contribute to the observed drop in female representation at the doctoral level, even among high-performing graduates.

These findings contribute to the growing literature on ordinal rank and peer effects in education by documenting that relative academic position within peer groups has significant implications not only for educational attainment, but also for labor market prospects and job preferences. However, some limitations should be acknowledged.

First, while the choice of high-school diploma grade as a measure of ability has advantages in terms of timing and standardization, it is not free from criticism. Given that it is a measure of academic performance rather than ability per se, it can also reflect factors other than cognitive ability, such as soft skills, effort, or parental and school support. Moreover, differences in grading leniency across schools or high-school tracks could introduce measurement error and distort comparisons across individuals. While results remain robust to multiplicative measurement error corrections and to alternative measures of ability, results using a fully standardized measure of human capital could provide additional reassurance.

Second, my identification strategy relies on a measure of ability determined before university and controls for this measure and for unobservable class characteristics. While my results are robust to a battery of alternative specifications and assumptions, a remaining concern is that unobserved factors could be confounding the results. For instance, non-cognitive skills (e.g. grit, persistence, drive), personal networks, or teacher encouragement could be directly correlated both with class rank and human capital investment choices.

Third, findings underscore the importance of understanding how students interpret rank-related signals, and how these perceptions interact with preferences and stereotypes in shaping academic trajectories. Nevertheless, the lack of adequate data to delve into these mechanisms prevents this analysis. Future work should explore how interventions such as feedback framing, exposure to role models, mentoring, or career counseling might mitigate differential reactions to academic rank and support the academic and labor market outcomes of underrepresented groups.

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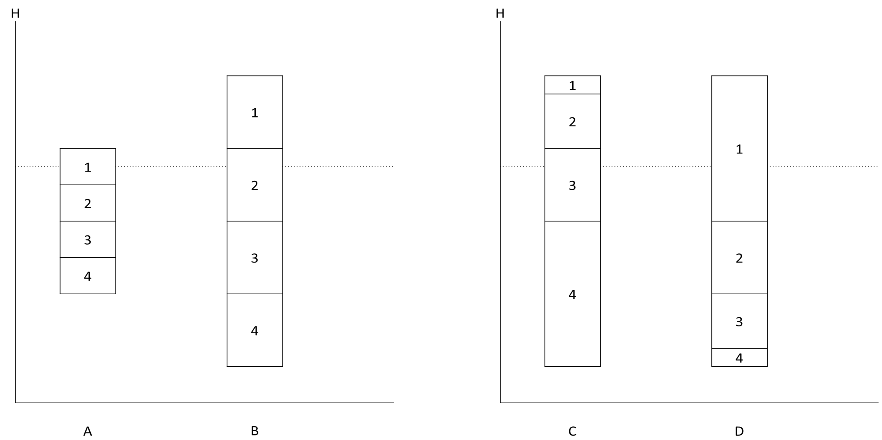
A Appendix

A.1 Identifying variation

Figure A.1: Identifying variation

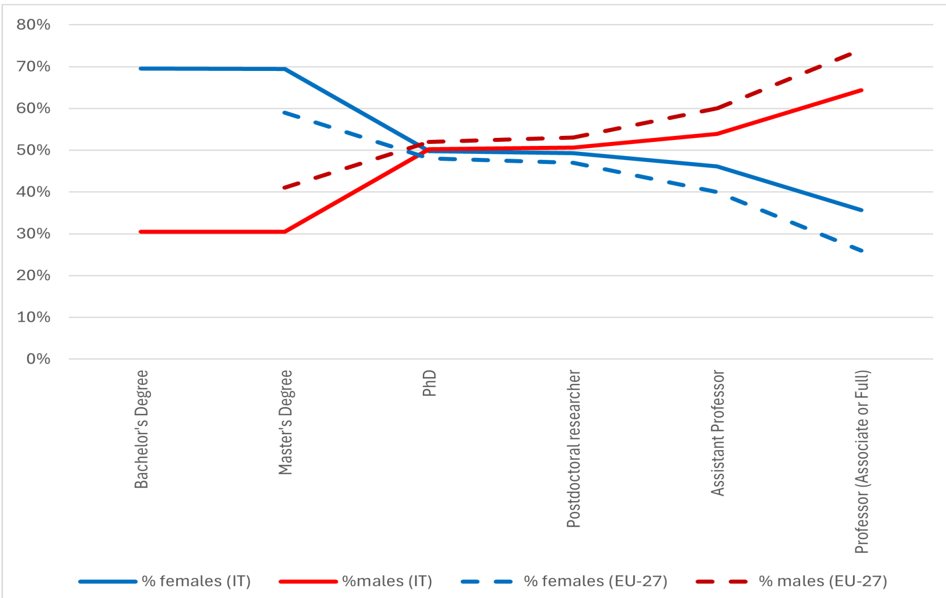


(a) (b)

Note: Figure shows four examples (A, B, C, and D) of ability distributions within a class, with the numbers referring to the quartiles of the distribution. Panel A presents a case in which two classes have the same average level of human capital and a different variance. Panel B presents a situation in which the two classes have the same variance, but the distribution is centered differently, with class C being skewed towards higher levels of ability, and class D being skewed towards the lower end of the distribution. In both cases, a student would end up having a different relative position with respect to her peers, even if her ability level remains constant.

A.2 The *Leaky pipeline* in Italy and in Europe

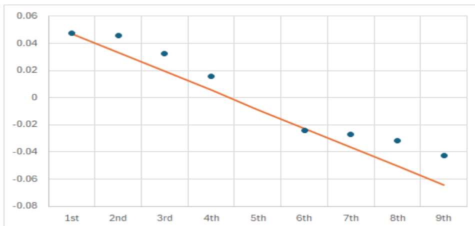
Figure A.2: Gender composition and career progression in the academic profession



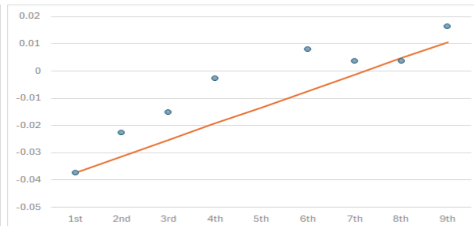
Note: Figure shows the share of females and males in all stages of the academic career, since post-mandatory tertiary education, until Professorship. Figures from Italy are taken from the Gender balance time series data of the Ministry of Universities and Research (Ministero dell'Università e della Ricerca , MUR) and refer to the year 2020, while figures for EU-27 are taken from the 2021 edition of the *She Figures* report (Commission, for Research and Innovation, 2021).

A.3 Additional robustness checks

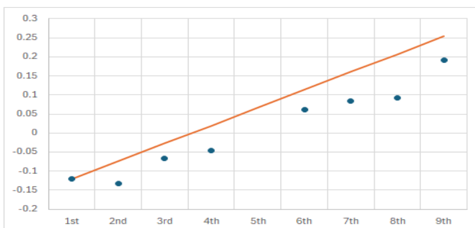
Figure A.3: Linear versus decile-specific rank effects



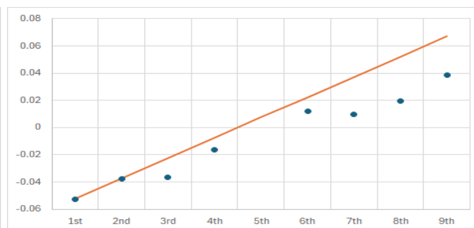
(a) Employment



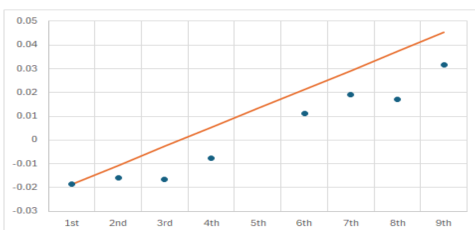
(b) PhD enrollment



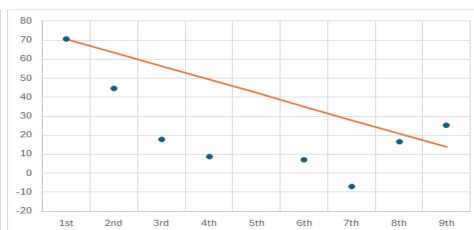
(c) GPA



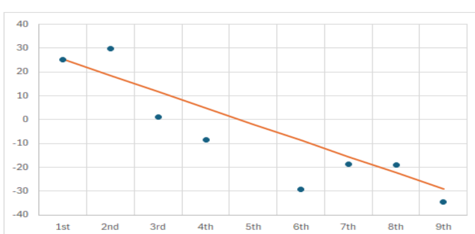
(d) Cum Laude



(e) Want to do a PhD



(f) Reserve wage



(g) Avg. monthly earnings

Note: Figures plot the baseline effect, depicted as the red sloped line, against estimates for each decile, obtained by substituting the continuous rank variable for decile indicators.

Table A.1: Sensitivity to different Rank specifications

	Employment	PhD enrollment	GPA	Cum Laude	Want to do a PhD	Reserve wage	Avg. monthly earnings
Baseline estimates	-0.014***	0.006***	0.047***	0.015***	0.008***	-7.12*	-6.79
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.95)	(4.32)
Using track rank	-0.009**	0.003*	0.030***	0.004	0.009***	-2.85	-5.92
	(0.004)	(0.002)	(0.010)	(0.003)	(0.002)	(5.39)	(5.15)
Using starting year to define cohort	-0.012***	0.005***	0.050***	0.017***	0.006***	-4.90	-7.57*
	(0.003)	(0.002)	(0.008)	(0.003)	(0.002)	(3.53)	(4.17)

Note: Table reports estimates of class rank effects on the outcomes of interest across different specifications of the main explanatory variable, $Rank_{hdc}$. The second row represents the results when switching from a "field" rank (number of people with a higher ability, plus one) to a "track" rank (number of people with a lower ability, plus one). The third row represents instead the main results when changing the cohort variable used to define the peer group, instead of the year of graduation, using the year of start of studies, calculated as the difference between graduation year and duration of studies.