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ABSTRACT

Rank Effects in Education: What Do We Know So Far?*

In recent years there has been a plethora of empirical papers by economists concerning the effects of academic rank in school or college on subsequent outcomes of students. We review this recent literature, describing the difficult identification and measurement issues, the assumptions and methodologies used in the literature, and the main findings. Accounting for ability or achievement and across a range of countries, ages, and types of educational institutions, students that are more highly ranked in their class or their grade have been found to have better long-term outcomes. The effect sizes are generally large when compared to magnitudes found for other factors and interventions. Rank effects can provide useful insight into other educational phenomena such as the extent to which students benefit from high ability peers and the presence of a gender gap in STEM. However, the state of knowledge has probably not reached the point where the empirical findings from this literature have practical implications for policy intervention to improve outcomes of students.

JEL Classification: I21, J16, J24

Keywords: academic rank, education, human capital, social comparisons, peer effects, STEM

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Introduction

Economists and other social scientists have long been interested in understanding how students are affected by the characteristics, abilities, and achievements of other children in their class in school or college. While children may be expected to perform better when surrounded by able, high-achieving classmates, this effect may be offset or negated by a tendency for students to do better when they rank higher in the class. Students may compare their abilities with that of other students in the class and, hence, their rank may affect their perception of their ability, their self-confidence, and their behaviors. This is often referred to as the big-fish-little-pond effect as initially characterized in psychology by Marsh (1987). While some older literature in economics has considered rank effects (Zax and Rees, 2002), the new economics literature on rank can be traced to the seminal working papers by Cicala et al. (2011) and Murphy and Weinhardt (2013) that were the first of a deluge of recent papers by economists concerning the effects of academic rank in school or college on subsequent outcomes of students.

We review this recent literature, describing the difficult identification and measurement issues that face researchers, the assumptions and methodologies used in the literature, and the main findings. Accounting for ability or achievement levels and across a range of countries, ages, and types of educational institutions, students that are more highly ranked in their class or their grade have been found to have better long-term outcomes. The effect sizes are generally large when compared to magnitudes found for other factors and interventions. Rank effects can provide useful insight into other educational phenomena such as the extent to which students benefit from high ability peers and the presence of a gender gap in STEM. However, the state of knowledge has probably not reached the point where the empirical findings from this literature have practical implications for policy interventions to improve outcomes of students.

The structure of this review is as follows: In the next section, we define the rank effect that is the target of empirical research, and in Section 2, we outline the related identification issues. Section 3 describes some relevant measurement issues and Section 4 analyzes the main empirical findings in the literature. Various potential mechanisms underlying rank effects are discussed in Section 5. Section 6 considers the wider applicability of the rank findings and Section 7 concludes and considers where the literature may be heading.

1. What is a rank effect?

When studying rank, researchers typically rank students based on their performance in tests of aptitude or achievement. Their fundamental goal is to test whether, all else equal, having a higher rank affects subsequent student outcomes. For now, we refer to the test score as measuring human capital broadly defined; in Section 4, we discuss how this encompasses measures of both aptitude and achievement.

For the most part, researchers have not been able to measure how students rank within individual classrooms. Instead, most of the literature measures the academic rank of each student amongst children in the same year group in the same school. This is referred to as rank in the school-cohort and we use it as the baseline measure in this review. Because a simple ordinal rank measure would not be comparable across school-cohorts of different sizes, when economists consider rank effects, they percentilize the rank as follows:

$$Rank = \frac{(n_i - 1)}{(N_i - 1)}$$

where n_i is the student's ordinal rank in the school-cohort and N_i is the number of students in the school-cohort.[1] The percentile rank measure is approximately uniformly distributed, and

[1] In the event of ties, strategies used vary. Some papers assign the average rank to each individual in the tie while other papers instead give the highest ranking or the lowest ranking to all students who tie, measured amongst the

is bounded between 0 and 1, where 0 denotes the lowest ranked student in a school-cohort and 1 denotes the highest ranked student. When we refer to rank in this review, it implies this measure of percentile rank.

The effect of rank can be broadly defined as encompassing any reason why rank affects later outcomes. It is a rank effect if having a higher rank increases confidence and leads students to study more and do better, or to hang out with and be influenced by other higher ranked students, or to be less likely to engage in disruptive behavior. If teachers and parents provide more support and encouragement to higher ranked students which leads them to do better, this is also a rank effect. Likewise, it is a rank effect if a higher rank in a subject in school causes a student to believe that their comparative advantage is in that area and to subsequently choose it as a specialization in college. Rank effects can even be somewhat mechanical if colleges are more likely to admit students who have higher rank in high school. As is discussed in Section 5, researchers have made significant progress in determining the important mechanisms through which rank affects later student outcomes.

As mentioned earlier, much research focuses on peer effects – how school children are influenced by their classmates (see Sacerdote (2011) for an excellent discussion of what constitutes a peer effect). The effect of rank is a particular type of peer effect that is a function of the student's own level of human capital as well as that of their peers. Conceptually, the definition of a rank effect relates to how a student's outcomes change in response to a change in his/her rank, holding constant the distribution of peers in his/her class. However, if the peer distribution is unchanging and student human capital is fixed, the rank of the student cannot change. As such, we cannot treat rank as being distinct from other

group who are tied. Most papers report robustness tests with multiple approaches to ties and the exact treatment does not appear to be important for the results found.

peer effects. Rather, a working definition of the effect of rank requires restrictions about what types of peer effects are admissible in the analysis.

Another way of approaching this issue is to consider whether there exists an ideal experiment that would identify the effect of rank. Probably the best we could do is to randomize identical students into many classes that are identical in every way except that they differ in terms of the distribution of human capital of their students. This would provide variation in rank for otherwise identical students who face otherwise identical conditions. However, it would still be impossible to distinguish between rank and other peer effects using this experiment without making assumptions about the nature of the peer effects. We discuss this issue at length in the next section.

2. Identification of the rank effect

Identifying the effect of rank in real world datasets involves distinguishing the effects of rank from those of many other factors. More highly ranked students tend to have higher levels of human capital that may have a direct effect on outcomes, abstracting from any rank effects. More highly ranked students may also differ in terms of their other observed characteristics (such as gender or race) or unobserved characteristics; these individual characteristics may directly affect outcomes. Student rank could also be correlated with teacher quality or other classroom factors. Additionally, as referenced in Section 1, rank is correlated with peer characteristics and identifying a rank effect requires placing restrictions on other peer effects.

Denote the outcome of interest (later educational achievement, future behavior, or future earnings) as Y. In general, we can write that $Y = f(R, H, X, S, C)$ where R is the rank of the student's human capital in the school-cohort, H is human capital of the student, X are student-specific factors, S are school-specific variables and C are cohort-specific variables.

While the timing of measurement of human capital and rank is an important issue, we postpone discussion about it until Section 3. In practice, researchers assume additive separability between *R* and the other elements while often allowing for interactions between *H*, *X*, *S,* and *C*.

Given human capital (*H*) is highly correlated with school-cohort rank, a key to isolating the effect of rank is to control flexibly for *H* and most empirical studies do so by including a polynomial in the test score. For the purposes of this exposition, we assume that *H* is measured discretely (as a test score, *h*) and a full set of indicator variables are included to control for *H*.[2] This leaves two first order identification challenges: (1) distinguishing the effect of rank from other possible peer and classroom effects, and (2) possible correlation of rank with other individual characteristics, some of which may not be observed by researchers.[3]

For clarity, we first discuss the identification challenges due to peer and classroom effects; later, we discuss the role of individual specific variables (and their potential interactions with *H*).

2.1 Distinguishing rank effects from other peer and school-cohort effects

Because rank for any individual depends only on their school-cohort and their test score, we begin by abstracting from individual-level variation and consider variation by school (*s*), by cohort (*c*), and by test score (*h*). Denoting the percentile rank as R, we write the relationship between the outcome and rank as

$$\boldsymbol{Y_{sch} = \alpha + \beta R_{sch} + v_{sch}} \qquad \textbf{(1)}$$

[2] Considering the cardinal measure of human capital as being discrete is not restrictive as the number of possible test scores can be thought of as being very large so that the measure is a percentile (as is the case in several of the empirical papers on rank).
[3] Measurement error also poses challenges; we postpone discussion of it until Section 3.

One can think of the error term v_{sch} as encompassing a wide range of possible peer effects related to human capital (or, indeed, classroom effects that are not due to peers but to teachers or some other factor). If v_{sch} is treated nonparametrically and represented by a full set of school-by-cohort-by-test score indicators, then β is unidentified because rank for any individual depends only on their school-cohort and their test score. Thus, identification requires restrictions on v_{sch}, and hence, on admissible peer and other school-cohort level effects.

We consider three common types of assumptions that have been made in the recent literature:

Assumption 1: $$\boldsymbol{E(v_{sch}|s,c,h) = \gamma_h + \theta_c + \mu_s} \quad \textbf{(2)}$$

This assumption states that differences in the error term across combinations of test scores, schools, and cohorts can be summarized by additive school, cohort, and test score effects. Cohort effects (θ_c) allow for differences in abilities or interests by cohort. Test score effects (γ_h) allow differences by the absolute level of human capital. Likewise, school effects (μ_s) denote time-invariant school-specific factors such as the location of the school. Given Assumption 1, the inclusion of indicators for test score, school, and cohort provides consistent estimation of rank effects. Many sources of variation provide identification of rank in this setup with the most obvious one being that because there is idiosyncratic variation in the characteristics of students who enter the school in different years, students with the same test score who are in the same school will have different ranks because they happen to be in different cohorts.

Assumption 1 is violated if the outcome is correlated with factors that are specific to a particular cohort within a school such as the quality of instruction they receive. Also, peer characteristics that affect outcomes but are conceptually distinct from rank will differ across cohorts in the same school as, conditional on test scores, students who are highly ranked will tend to be in low-achieving school-cohorts. Therefore, most rank papers make a weaker assumption, assumption 2 below:

Assumption 2: $$\boldsymbol{E(v_{sch}|s,c,h) = \gamma_h + \theta_{sc}} \quad \textbf{(3)}$$

This assumption states that differences in the error term across combinations of test scores and school-cohorts can be summarized by an additive school-cohort effect and an additive test score effect. Given Assumption 2, the presence of indicators for test score and indicators for school-cohort provide consistent estimation of rank effects as they eliminate potential confounders that have the same effect on all members of the school-cohort. Therefore, inclusion of school-cohort indicators ensures that rank estimates are not biased by correlations of rank with school-cohort-specific factors such as the quality of teachers and peers (provided these factors have the same effect on all students).

Assumption 2 implies that, other than rank, functions of interactions between school-cohorts and test scores do not belong in the model. This allows the identification of rank from cases where differences in rank across test scores are not homogenous across schools. Suppose we think of the test score as measured in letter grades. Consider two schools that have different test score distributions so that going from an A grade to a C grade in one school leads to rank falling by 0.5, while going from an A grade to a C grade in the other school leads to rank falling by 0.25. There is identifying variation in rank as the differences in rank between the two schools is not the same for each letter grade. So long as rank cannot be written as the sum of a school-cohort effect and a test score effect, the effect of rank is identified. In the appendix, we give some simple examples to show that, under Assumption 2, there are numerous sources of identification including differences in variances and higher-order moments of human capital across school-cohorts.

While Assumption 2 is much weaker than Assumption 1, it can still be considered a strong assumption as it does not allow peer effects to be heterogeneous by individual human capital as emphasized by Booij et al. (2017) and Bertoni and Nistico (2019). A weaker assumption is assumption 3, below:

Assumption 3: $$\boldsymbol{E(v_{sch}|s,c,h) = \gamma_h + \theta_{sc} + fsch} \quad \textbf{(4)}$$

where $fsch$ represents parametric functions of sc and h. Because these functions are parametric, identification of rank effects is still possible from idiosyncratic variation in rank that is orthogonal to these functions. In estimation, researchers have parameterized $fsch$ in two main ways.

The first approach is to include various sets of interactions of h with school characteristics such as the level of advantage of the school.[4] These interactions allow the effect of h on the outcome to vary with school characteristics. This may be important in certain applications. For example, Denning et al. (2021) mention that, in low-achieving schools in Texas, students with greater human capital are more likely to later attend two-year college, but the human capital gradient is negative for this outcome in high-achieving schools.

The second method is to include interactions of h with the mean of h in the school-cohort and interactions of h with the variance (or standard deviation) of h in the school-cohort and also even to include triple interactions of h with the mean and variance (or standard deviation) of h in the school-cohort (Bertoni and Nistico, 2019). Alternatively, different types of school-cohort distributions could be determined based on their mean, variance, or higher order moments and h could be interacted with indicator variables for the type of distribution (Denning et al., 2021). Controlling for these additional interactions reduces the identifying variation in rank and the hope is that the remaining identifying variation is largely

[4] For example, Delaney and Devereux (2021) interact test scores (measured using indicators for letter grades) with school-cohort size, with whether the school is a same-sex or mixed-sex school, and with the type of school (whether it is a fee-paying school, whether it is a disadvantaged school, and whether it is a Secondary, Vocational, Comprehensive, or Irish-language school).

idiosyncratic and can be treated as quasi-random. However, it does create a “black box” feel to identification as the remaining sources of identifying variation are far from transparent.

2.2 Correlation of rank with unobserved individual characteristics

The identification issues raised in the previous subsection exist whether or not students are randomly assigned to school-cohort groups as there is the same necessity to place restrictions on the role of peer and other school-cohort effects. However, random assignment to school-cohort groups implies that a student’s characteristics are uncorrelated with those of other students in the group and, hence, rank is conditionally uncorrelated with unobserved individual characteristics. This is not the case when school-cohorts form endogenously, and we now turn to this case.

Introducing an individual-level error component, a benchmark estimating equation is

$$\boldsymbol{Y_{isc} = \alpha + \beta R_{isc} + X_{isc}\delta + Z\gamma + \varepsilon_{isc}} \qquad \textbf{(5)}$$

Here *X* denotes individual characteristics such as gender and age and *Z* captures measures of human capital, school-cohort fixed effects, and possibly interactions of school-cohort characteristics with human capital. In the absence of random assignment, the rank of individual i (R_{isc}) may be correlated with the error, ε_{isc} if variances or higher order moments differ across school-cohorts and different types of children sort differently across types of school-cohorts. Denning et al. (2021) provide useful examples of scenarios in which sorting of students leads to correlations between individual characteristics and rank. Such correlations will lead to inconsistent estimates.

In Section 2.1, we discussed how the addition of interactions between h and features of the distribution of h within the school-cohort (such as the mean and variance) is important to allow for heterogeneous peer effects. Denning et al. (2021) show that these types of interactions can also be motivated by sorting concerns as, in the absence of random assignment, variation in the distribution of h across school-cohorts can lead to correlations of rank with unobserved child characteristics, conditional on h. Therefore, they suggest an estimation strategy of including interactions of h with indicator variables for the type of school-cohort distribution – they create 16 types based on quartiles of the mean human capital and quartiles of the variance of human capital in the school-cohorts. This potentially ameliorates sorting issues by limiting identifying variation in rank to that within similar school-cohort distributions. Thus, in the absence of random assignment, there is more than one reason to include interactions of human capital with features of the school-cohort distribution when estimating rank effects.

An additional strategy to deal with sorting is to use a rich set of controls for individual characteristics encompassing demographics such as gender, race, and age. It is probably also very useful to have a rich set of human capital controls as whatever ability/achievement measure is used to construct rank is unlikely to capture all factors that determine later outcomes. This makes it quite likely that rank may be correlated with unobserved components of human capital. For example, suppose people with higher ranks are better at mathematics than others, even conditional on the human capital measure. If mathematical ability is critical to determining later outcomes but is not controlled for, then it may appear that rank matters when it does not. The best solution to this problem is to control for a wide range of measures of absolute ability/achievement but often researchers do not have access to more than the one that is used to calculate rank.

It may also be appropriate to broaden the constituents of X to include measures of how the student ranks in terms of some individual characteristics, such as age rank (whether the oldest, second oldest, etc. in the school-cohort), as future human capital accumulation may depend both on the age of the student and on the age of the student relative to others in the school-cohort group. Given that rank in terms of human capital may be correlated with rank in terms of other characteristics, omitting these terms is a potential source of bias. However, this issue is underexplored in the literature as few papers assess the effects of adding controls for the rank of covariates.[5]

One further possible approach to accounting for individual characteristics is to include student fixed effects. This is only feasible in special circumstances such as in Murphy and Weinhardt (2020) where they have measures of student achievement in three subjects and study the effect of rank in each subject on later performance in that subject. In this case, including individual fixed effects comes at the cost of absorbing any growth in achievement due to rank that is common across all subjects and so will tend to lead to underestimation of rank effects.

In addition to controlling for observed individual characteristics, many studies attempt to assess whether there is likely to be bias due to omitted characteristics. Based on the premise that the correlations of rank with observed characteristics speak to correlations of rank with unobserved ones, balancing tests of the sort

$$\boldsymbol{X_{isc} = \delta + \pi R_{isc} + Z\mu + \epsilon_{isc}} \qquad \textbf{(6)}$$

provide a useful diagnostic where the null hypothesis is that $\pi = 0$. Ultimately, however, it is preferable to have randomly assigned groups so that sorting is not a problem that needs to be addressed.

3. Measurement Issues

In this section, we consider some measurement issues: First, should human capital be measured before the student is exposed to the peer group that is used to determine his/her

[5] One exception is Devereux and Delaney (2021) who include the rank of overall achievement as a control when testing the effects of student rank in mathematics and English. However, they do not include controls for the rank of age or of other covariates.

ranking? Second, should researchers construct rank using a measure of ability or of achievement? Third, what are the effects of measurement error in human capital and rank?

3.1 Timing of measurement of human capital

As rank is likely to affect human capital accumulation, it is advantageous if human capital is measured when the student has a different peer group to that used to construct rank. Indeed, many studies measure human capital in a period prior to the formation of the peer group used to measure rank as this avoids the circularity of rank affecting human capital that then affects rank that then affects human capital and so on.

Often, the available data do not allow a measure of human capital that preexists the peer group used to construct the rank variable. There is no fundamental identification problem with measuring human capital after the peer group forms as one can still ascertain the effect on later outcomes of rank at the point of measurement, conditional on human capital at the point of measurement. However, estimated effects of rank may differ based on the exact timing of measurement; for example, they may depend on the length of time between the formation of the school-cohort group and the measurement of human capital. Interestingly, in their study using Texas school data, Denning et al. (2021) find that while rank affects human capital accumulation, this does not lead to systematic changes in rank.

3.2 *Ability versus achievement*

Researchers have used human capital variables that encompass measures of ability and/or achievement, where the general understanding is that ability is somewhat innate and based on underlying cognitive skills while achievement reflects effort and engagement in addition to ability. There are advantages and disadvantages to having a measure that is more a marker for underlying ability rather than achievement.

The major advantage of an ability measure is that it is likely to be relatively fixed and so largely unaffected by peers. Therefore, the concern raised earlier that the human capital measure is itself influenced by peers (and hence rank) may be less pertinent. Unfortunately, it may be optimistic to believe that ability measures used in practice are unaffected by peers as they are often derived from low stakes standardized tests and scores may depend on attitude and effort as well as aptitude. It is also unlikely that any test score is unaffected by previous teaching and learning which themselves are likely to be impacted by peers (as teachers may adjust pedagogy to the group being taught). However, all these issues are likely to be greater for achievement measures as they are more malleable and may plausibly be responsive to peer influences and, hence, rank.

In contrast, an advantage of achievement measures is that achievement is likely to be more salient than ability as students may be aware of the performance of classmates as revealed by repeated testing. Salience is a major issue for the rank literature as studies generally assume that students have some knowledge of their rank despite rarely being provided with this information. If students do not know their rank, how can their behavior respond to it? To the extent that the actual rank differs from the rank perceived by the student, and that it is perceived rank that matters, rank estimates will tend to be attenuated.

Happily, there is some evidence that students know something of their rank with recent research finding a strong and positive correlation between objective and self-perceived ability rank in Italian primary schools (Pagani et al., 2021), in Chinese middle schools (Yu, 2020), and in U.S. high schools (Kiessling and Norris, 2020; Elsner and Isphording, 2017). While evidence is limited, salience of achievement rank is likely to be greater than that for ability.

3.3 Measurement error in human capital and rank

In all studies, mismeasurement of the human capital variable and, hence, rank is a concern. The fundamental issue is that we expect subsequent outcomes to depend on student rank based on true human capital rather than human capital as measured by the researcher. However, human capital is typically measured by a test of some sort that will tend to provide a noisy measure of the underlying true human capital.

There are many possible scenarios. A benign, albeit unlikely, case is that all students could have their human capital mismeasured by the same amount, perhaps because of the temperature on the day of the test, and so rank is unaffected. In this case, the presence of school-cohort fixed effects would control for this problem and there would be no bias in the rank estimate. More plausibly, some students will overperform and others underperform on the test and both human capital measure and its rank will be mismeasured. If what matters is the rank based on true rather than measured human capital, there will be biases.

Variations in test performance could be random (some students have a "bad day" while others have a "good day") or they could be systematic if, for example, more conscientious students try harder on the test than others and, hence, outperform their ability. Because mismeasurement affects rank through errors in both the student's own score and that of their classmates, this is a complex econometric problem. In practice, researchers have carried out simulations to characterize the likely resultant biases and have concluded that it leads to relatively small attenuation biases (Elsner and Isphording, 2017; Murphy and Weinhardt, 2020; Denning et al., 2021).[6]

[6] Murphy and Weinhardt (2020) show that multiplicative measurement error in human capital can lead to particular biases as variation in measured human capital in the tails of the distribution due to measurement error may not be accompanied by changes in rank. They demonstrate using simulations that, if measurement error is multiplicative, transforming the human capital variable into a uniform distribution can eliminate the bias.

4. Findings

The literature is broad and has used a wide variety of datasets and outcome variables and has measured rank across a range of different ages. As such, it is difficult to neatly characterize but we attempt to group studies that are broadly similar. We then give an overview of heterogeneities that have been found in rank effects and of how magnitudes of estimated rank effects compare to those of other educational inputs.

4.1 Effects of rank in school measured using idiosyncratic variation in administrative data

Because accurate measurement of rank requires knowledge of all students in a school-cohort, administrative datasets have been widely used in the rank literature. An advantage of these datasets is the typically large numbers of schools and students that enable reasonably precise estimation of rank effects based on idiosyncratic variation.

Murphy and Weinhardt (2020) use UK data to measure student achievement rank at the end of primary school. Even though students have a mostly different set of peers in secondary school, they find that students who are higher ranked in primary school achieve higher test scores during secondary school. Also, students who are ranked higher in a subject in primary school are more likely to complete that subject at A-Level. Using a similar design, Dadgar (2021) examines the effect of GPA rank at the end of compulsory schooling (about age 15) in Sweden on later outcomes as students move to vocational or upper secondary schools and on to the labor market. His findings are mixed with some positive effects of rank on educational outcomes and on earnings.

Comi et al. (2021) use Italian data to measure rank in high school based on school marks achieved at the end of junior high school. As in Murphy and Weinhardt (2020), the peer group is very different in the later stage of education; unlike in Murphy and Weinhardt (2020), rank is calculated relative to the students in the later education group rather than the earlier group. They find that students with higher rank are less likely to exhibit violent behavior in school. Using the same dataset and a similar setup, Pagani et al. (2021) find a positive rank effect on conscientiousness (but not on other personality traits).

Denning et al. (2021) use data from Texas schools to study the long-term effects of 3rd grade rank in mathematics and English. They find that higher ranked students have better subsequent test scores, are more likely to graduate from high school and later from college, and have higher earnings 19 years later. Once again, their study is subtly different from the others just described as the test scores are determined when the student is in the same peer group as when the rank is measured, and students continue with largely the same peer group for some time afterwards.

Cicala et al. (2018) examine how changes in rank, as students move from middle schools to high schools in New York, relate to changes in student behavior. They find that students whose rank declines are more likely to have "behavioral incidents". Once again, the setup here is different from the other papers as outcomes are related to changes in rank rather than rank itself.

4.*2 Research using idiosyncratic variation in the AddHealth dataset*

Some studies have used survey data from the *National Longitudinal Study of Adolescent to Adult Health (AddHealth)* dataset, a representative sample of U.S. adolescents in grades 7-12. This dataset has a very rich set of variables but, unlike administrative datasets, suffers from sampling error in rank as only a subset of students from each school-cohort (about one-third) are observed.[7] It also suffers from significant attrition between the first two

[7] Elsner and Isphording (2017, Online Appendix) show that sampling error of rank in the AddHealth leads to attenuation bias – they estimate the bias to be about 20% in their study.

waves.[8] The survey contains a nationally standardized cognitive test score that is measured at about age 16 and used to form ranks within high school school-cohorts. Elsner and Isphording (2017, 2018) find that students with a higher rank are more likely to complete high school and attend college and there is a negative effect of rank on the likelihood of smoking, drinking, having unprotected sex, and engaging in violence. Kiessling and Norris (2020) find that a higher rank is associated with better mental health.

4.3 Research using a randomized controlled trial

A recent paper by Carneiro et al. (2021) uses a unique experiment run on 202 schools in Ecuador whereby in each grade from kindergarten to 6th grade, students are randomly assigned to a classroom. Using test scores from the end of the previous grade, they calculate the rank of each student amongst their new classmates. They find that a higher classroom rank in mathematics has a significant effect on mathematics test scores at the end of the school year and that this effect gets larger over time. They also find that mathematics rank affects achievement in languages but do not find any evidence of an effect of rank in languages on either mathematics or language achievement.

4.4 Research using administrative data to study effects of subject rank on STEM

Researchers have been interested in whether student rank in mathematics and English in school influence whether they choose to do STEM subjects in college. Denning et al. (2021), using data from Texas, find that mathematics rank in 3rd grade has a positive effect on doing a STEM major in college. Delaney and Devereux (2021) use Irish administrative data

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[8] Another issue is that many outcome variables are self-reported and measurement error in the dependent variable can lead to bias if it is correlated with rank; if students report their behavior relative to others in their peer group and so higher-ranked students report their behavior to be better than it is, then this will tend to bias the effects of rank in a positive direction.

on college applicants to investigate whether, conditional on absolute achievement at the end of high school, within school-cohort rank in English and mathematics affects choice of college major. They find that higher rank in mathematics increases the likelihood of applying to a STEM college program and a higher rank in English decreases the probability of choosing STEM. Both studies rely on idiosyncratic variation in peer groups.

In contrast, Goulas et al. (2020) exploit random assignment of students to classrooms in high school in Greece and create the within-classroom ranking of the ratio of performance in STEM subjects relative to non-STEM subjects. They find that, conditional on absolute achievement, a relatively higher rank in STEM subjects increases the likelihood that female students apply to STEM college programs (they find no evidence of an effect for males).

4.5 Rank effects in college

While most research on rank effects in education has focused on schools, a few papers have studied rank effects in college. Bertoni and Nisticò (2019) exploit the random assignment of Dutch economics first-year college students to tutorial sessions and find that higher student rank in the session (measured based on high school GPAs) leads to more credits being attained in first year. Similarly, Elsner et al. (2021) use random assignment to teaching sections for business students in a Dutch university and find that higher rank increases performance and the probability of choosing related follow-up courses and majors. Like the Carneiro et al. (2021) paper mentioned above, a nice feature of their study is that students are randomized more than once so they can study dynamic effects. They find that effects of rank decline over time and that responses to rank changes are asymmetric: improvements in rank raise performance, while decreases in rank have no effect. Interestingly, the teaching sections are of only 7 weeks duration so rank matters even when students are grouped for only a short period.

Payne and Smith (2020) use Canadian administrative data from four universities to show that idiosyncratic variation in program rank (based on performance in the best six subjects in high school) has positive but small effects on GPA and degree attainment.

4.6 Heterogeneous effects

There is unlikely to be a single "causal effect" of rank as there are many changes to a school-cohort group that may change individual rank. For example, a student's rank may increase due to a lowly-ranked student joining the group or due to a highly ranked student leaving the group. Unless both changes (and potentially many more such changes) have the same effect on the outcomes of the student, there are heterogeneous effects of rank. Additionally, some students may be much more influenced by their rank than others and, even if all students are equally sensitive to their perceived rank, some students may have better knowledge of their rank than others and therefore be more responsive to their actual rank. These factors also imply heterogeneous effects of rank.

Increasing attention is being paid to the implicit weights used by regression when estimating a treatment effect that is potentially heterogeneous while including many control variables, including fixed effects (for example, Słoczyński, 2020). This is a relevant issue for the rank literature especially given that the number of students and amount of identifying variation in rank may differ greatly across human capital levels and school-cohorts. We are unaware of any rank studies that have calculated underlying regression weights so the relative contribution of different types of observations to estimation of the rank coefficient is unclear. However, researchers have gone someway down this road by doing a range of sample splits and allowing for non-linear and heterogeneous effects.

While the benchmark regression imposes a linear effect of rank, most papers examine whether rank effects are non-linear. While some studies find evidence for non-linearities, many find that the effects are approximately linear across the rank distribution with some small differences in the tails (Denning et al. 2021; Murphy and Weinhardt 2020; Elsner et al. 2021).[9]

Additionally, many researchers examine whether rank effects vary by student and school characteristics with a strong emphasis on whether rank effects differ by gender. It has been shown that boys are more competitive than girls (Buser et al. 2017) and have a higher willingness to compete (Niederle and Vesturlund, 2007) and therefore may be more responsive to their rank. However, findings in the literature are mixed with some papers finding that rank effects are larger for males (Murphy and Weinhardt, 2020; Delaney and Devereux, 2021; Elsner et al. 2021) while others find no statistically significant differences by gender (Carneiro et al., 2021; Payne and Smith, 2020; Denning et al. 2021; Pagani et al. 2021) or even find stronger effects for females (Goulas et al. 2020). Some papers also examine whether effects are different when ranks are calculated relative only to same-gender peers. Within-gender rank may be more salient if students tend to form social circles with students of the same gender or compare themselves to same-gender students. However, findings tend to be that within-gender rank effects are similar to or smaller than effects of overall rank (Delaney and Devereux, 2021; Pagani et al. 2021; Elsner et al. 2021; Elsner et al. 2017).

Some research has examined whether findings differ by race, socio-economic status, or student ability. Denning et al. (2021) find that non-white students and those availing of a free school lunch are more influenced by their rank. Similarly, Murphy and Weinhardt (2020) find that free school meal students gain more from being highly ranked than students who are

[9] Using Italian data, Fenoll (2021) finds that, even conditional on a linear control for percentile rank, being the best student in the class in primary school leads to increased test scores in secondary school. Using Swedish data, Dadgar (2021) finds the effects of rank are strongest in the tails of the rank distribution.

not eligible for a free school meal. Kiessling and Norris (2020) find the positive effects of rank on mental health are more pronounced for low ability students.

4.7 Magnitudes of rank effects relative to other effects

The literature has shown that rank has significant effects on test scores, college graduation probabilities, college major choice, earnings, risky behavior choices, and mental health. But are these effects large in magnitude and how do they compare to the effects of other factors? In this section, we give a few examples.

Murphy and Weinhardt (2020) find that a one standard deviation increase in rank in primary school increases test scores at ages 14 and 16 by approximately 8 percent of a standard deviation. Yu (2020) finds that a one standard deviation increase in rank in 7th grade in middle school increases mathematics, English, and Chinese test scores by 14, 18, and 20 percent of a standard deviation in 8th grade, respectively. In comparison, Rivkin et al. (2005) find that a one standard deviation increase in the average teacher quality in elementary school raises test scores by 11 percent of a standard deviation in mathematics and 9.5 percent of a standard deviation in reading. Given the importance we ascribe to teacher quality, it is noteworthy that the effect of rank is of a similar order of magnitude.

Denning et al. (2021) find an increase in rank of 25 percentiles in 3rd grade leads to an increase in 8th grade test scores (as measured in state test score percentiles) by 2.5 percentiles. This effect is close to the 4-percentile gap that exists between white and non-Hispanic Black students and the gap between non free lunch students and free lunch students. They also find that moving from the 25th to 75th percentile of third grade rank increases earnings at ages 23-

27 by about 7 percent, which is a typical estimate of the earnings return to a whole extra year of schooling. Once again, these comparisons suggests that the rank effect is large.[10]

Elsner et al. (2021) find that a one standard deviation increase in rank for students who are randomized into a tutorial session in college increases the grade at the end of the course by almost 7 percent of a standard deviation. In comparison, Feld et al. (2020) find that a tutorial instructor with a one standard deviation higher value-added increases student grades by only 2 percent of a standard deviation and Feld and Zolitz (2017) find that a one standard deviation increase in the average GPA of tutorial session peers increases grades by only 1.3 percent of a standard deviation. The rank estimates are similar to findings that a one standard deviation increase in professor quality increases grades by roughly 5 percent of a standard deviation (Braga et al. 2014; Carrell and West, 2010) and that a one standard deviation decrease in class size in college increases grades by 10 percent of a standard deviation (De Giorgio et al., 2012).

The size of rank effects on non-cognitive measures such as conscientiousness and violence are also large in comparison to the effects of other factors. Pagani et al. (2021) find that a one standard deviation increase in rank leads to an increase in conscientiousness by 14 percent of a standard deviation. Kiessling and Norris (2021) find that increasing rank by one standard deviation improves mental health by 6 percent of a standard deviation. In comparison, Marcus (2013) finds that one year after becoming unemployed, an individual's mental health deteriorates by 27 percent of a standard deviation and their spouse's mental health diminishes by 18 percent of a standard deviation. Elsner and Isphording (2018) find that a decile (about a standard deviation) increase in rank decreases the likelihood of smoking

[10] A further indication of the magnitude of rank effects is that, using administrative schooling data from the Netherlands, de Gendre (2021) finds that a one standard deviation increase in child rank in primary school decreases their younger sibling's test scores by around 2 percent of a standard deviation.

and drinking by 1.2 and 1.4 percentage points and report that this is similar in magnitude to the effects of increasing the proportion of peers who smoke or drink by 10 percentage points.

Our conclusion is that, while effect sizes differ across studies, rank effects are quite large when compared to the effects of other factors or treatments. In the next section, we discuss the underlying mechanisms that may generate these sizeable effects.

5. Why does rank matter?

The literature has identified several mechanisms that help explain why a student's academic rank affects test scores, earnings, and student behavior. They can be divided into two broad categories: (1) factors that affect the student's intrinsic beliefs and behaviors and (2) factors in the external environment that respond to a student's rank.

5.1 Self-confidence, motivation, effort, and non-cognitive skills

It is well established that students lack information about their ability (Zafar, 2011; Stinebrickner and Stinebrickner, 2012) and so may use their school-cohort rank to infer their true ability based on their relative ability.[11] Thus, higher ranked students have greater confidence in their academic ability, and this may lead them to exert more effort in their studies as the marginal cost of doing so is deemed to be low. Cicala et al. (2018) posit that students may use their rank to assess whether they have a comparative advantage in academics or as "troublemakers"; thus, rank may affect behavior and choice of peer group in school.

Several studies find these considerations to be relevant. Elsner and Isphording (2017) find that higher ranked students have higher expectations of their future career and higher

[11] Likewise, findings that subject ranks in school affect subsequent college major choices may arise because students infer their comparative advantage across subjects from their rank across subjects in school.

perceived intelligence. Murphy and Weinhardt (2020) find that higher ranked students have higher self-confidence and that the effect of rank on confidence is larger for boys than girls consistent with their findings of larger rank effects for boys. Finally, the findings of Carneiro et al. (2021) suggest that higher ranked students may have higher academic self-concept, Pagani et al. (2021) find that rank affects perceived ability and academic motivation, and Elsner et al. (2021) find that higher ranked students have higher expectations about their future grades. Therefore, it appears that self-concept or self-confidence is one channel through which rank effects operate.

Competitiveness may also play a role if students who discover that they are ranked high become motivated to work to preserve their high rank. Murphy and Weinhardt (2020) argue that if competitiveness was driving their results, the effects would predominantly be seen for the highest ranked students whereas they find positive effects on test scores across the rank distribution. In addition, Pagani et al. (2021) find a limited role of competitiveness as a potential mechanism while Elsner et al. (2021) find that a higher rank does not induce more hours of study. So, the literature has not found strong evidence for competitiveness as a mechanism.

5.2 Teacher and parental responses

Both teachers and parents may respond to a student's rank but the exact response is unclear a priori. Parents may invest more time if their child is higher ranked and shows an aptitude for a particular subject or less time if they feel that their child doesn't need the extra attention. Similarly, it is not clear whether teachers will devote more time to higher ranked students or pay them less attention if they have egalitarian preferences and would like to help the lower ranked students. Indeed, the findings in the literature are mixed.

Pop-Eleches and Urquiola (2013) find that teachers pay more attention to higher ranked students and that parents provide less help if their child is in a better school. Kinsler and Pavan (2021) find that parental beliefs about a child's cognitive skills respond to the child's relative position in their school. However, Murphy and Weinhardt (2020) find no evidence that parents respond to their child's rank and argue that parents may have little knowledge of the child's specific class rank. Elsner and Isphording (2017, 2018) and Pagani et al. (2021) also find little evidence that parents respond to their child's rank. Carneiro et al. (2021) find that higher ranked students are thought of as being high achievers by their future teachers. However, Pagani et al. (2021) find that teacher responses have a tiny albeit statistically significant effect and Murphy and Weinhardt (2020) and Elsner and Isphording (2017, 2018) find no evidence for teacher response to rank.

Overall, the evidence that teacher and parental responses to rank are important mechanisms is quite weak. However, institutional factors external to the student may matter. Calsamiglia and Loviglio (2019) show that in Catalonia where GPAs matter for college admissions, teachers in public schools appear to grade on a curve so having a lower class rank tends to reduce the GPA of students and, hence, their future educational options. This factor may be relevant in any educational system where school rank is a factor in college entry such as in Texas where the top 10% of students in each high school are guaranteed college admission. However, these institutional factors cannot explain rank effects on human capital accumulation in school.

6. Wider Implications of Rank Findings

The substantial effects of rank that have typically been found have broader implications and, in this section, we consider some of them.

6.1 Understanding peer effects findings

One valuable consequence of the rank literature is that it helps us better understand why exposure in school to high ability peers is not necessarily beneficial. The rank literature provides an explanation – the benefit of having stronger peers is partially (or fully) offset by the negative effect of being lower ranked. This is particularly the case when students whose achievement is just above a cutoff are admitted into a "better" school or class while students whose achievement is below the cutoff are not, enabling a regression discontinuity design to estimate the causal effect of attending the "better" school. In this setup, just barely qualifying for the "better" school or class implies having the lowest rank rather than the highest rank. Effectively, discontinuity designs speak to whether it is better to be the worst student in the "better" school or the best student in the "poorer" school. Clearly, in this context, rank effects may largely negate the value of being in the "better" school or class.[12] Indeed, Bertoni and Nistico (2019) demonstrate that the estimated effect of high ability peers is much more positive when regression controls are introduced for rank. Overall, rank effects imply that it is not necessarily better to have better peers and rationalizes the failure to find strong positive effects of high ability peers when account is not taken of rank in estimation.

6.2 Explaining gender gaps in STEM

Males are much more likely than females to choose STEM subjects and majors in college and this has been linked, in part, to boys being more likely to have a comparative advantage in mathematics rather than in verbal ability. In mixed-sex schools, boys have been found to average a higher rank in mathematics and a lower rank in English than have girls.

[12] Ribas et al. (2020) provide a nice illustration about how rank effects can negate the benefits of stronger peers using a discontinuity design with data from a Brazilian university. Denning et al. (2021) suggest that studies that instead use lotteries to determine who goes to what school tend to find stronger positive effects of school quality because, with a lottery, the correlation between rank and school quality is likely to be much weaker than in the discontinuity case.

Students may be uncertain about their ability and so may use rank to infer comparative advantage across subjects so, even conditional on absolute ability, students with relatively high rank in mathematics may be more likely to apply to STEM college programs. Thus, rank differences across subjects and genders can partially explain the gender gap in college STEM programs. Delaney and Devereux (2021) show that these rank differences can also rationalize some of the greater gender gap in STEM college applications among students in mixed-sex schools compared to same-sex schools – in same-sex schools, the average rank of boys and girls is, by definition, the same in each subject.

6.3 Policy implications for tracking versus mixing abilities

An important question for policy is whether it is better for schools to track students (so have students grouped by ability) or to have mixed-ability groupings. Class rank is not as highly correlated with ability in the former case as it is in the latter. If rank effects are homogenous, there is no scope to improve the aggregate effects of rank as increasing the rank of one student implies equivalent reductions in rank for one or more other students. However, if rank effects are heterogeneous one can improve matters by reassigning pupils across classes to reduce the rank of students who are not as sensitive to rank and increase the rank of students who are most influenced by rank. However, we suspect that attempts along these lines are unlikely to occur in practice for at least two reasons. First, patterns of heterogeneous rank effects are probably not sufficiently robust across studies to encourage their use in determining student allocation across classes. Second, as emphasized by Carrell et al. (2013), simulating policy outcomes from reduced form models can be misleading due to endogenous peer choices, even within groups. So, efforts to use regression rank estimates to optimize student allocations may not have the hoped-for positive effects on aggregate outcomes.

6.4 Implications for information provision

Schools can adjust how much information they provide to students and findings from the rank literature suggests that this may matter for student outcomes. Typically, educational institutions do not explicitly provide information about class rank, but students are likely to have a general sense of it from repeated interactions. There is probably little scope to reduce the amount of information students have about their rank but clearly schools could increase student knowledge about their rank.

In theory, a strategy of providing rank information only to high-ranking students could improve aggregate outcomes as we would expect high ranked students to benefit from this knowledge but low ranked students to be negatively affected. Indeed, if rank effects are heterogeneous, conceptually there is a case for providing more information on rank primarily to students who are highly ranked and who are also likely to be particularly sensitive to their rank. However, this type of policy is likely to be impractical and, even if it was feasible, might not be beneficial if heterogeneous effects of rank result from differences in knowledge of rank across students rather than differences in responses to perceived rank.

Overall, therefore, we do not believe that the rank literature has much to say about the practical benefits of providing information to students about their rank. However, some research speaks directly to this issue by studying interventions that provide rank information to students. Azmat and Iriberri (2010) find that providing relative performance feedback to Spanish high school students increases grades by 5 percent and this is significant across the whole distribution. Exploiting a similar experiment in Greece, Goulas and Megalokonomou (2021) find that the effects of rank information are positive for high achieving students but negative for low achieving students. Brade et al. (2020) find that providing rank information to first year college students increases subsequent performance but only for those who performed above average. There is also evidence that providing relative performance feedback tends to lead to poorer female performance relative to men (Cabrera and Cid 2017; Goulas and Megalokonomou, 2021). Overall, there are mixed findings about whether providing rank information leads to better academic outcomes for students.

Given that students may be in a school-cohort that is atypical in terms of the distribution of abilities of the students, a more promising intervention may be to provide students with more information about their ability or achievement relative to the broader population rather than to their school-cohort group. In general, this may have little aggregate effect as, to the extent that it reduces rank effects, it will benefit low ranked students and adversely impact high ranked students. However, there may be specific situations where students make suboptimal decisions because they infer their absolute ability from their school-cohort rank, such as shying away from STEM because they are not highly ranked in mathematics in their school-cohort even if their ability in mathematics is high relative to the population. In these cases, providing information about student ability or achievement in various subjects relative to the population in general may be a low-cost intervention that helps students to make better and more informed decisions.

7. Summary and future directions

We conclude by summarizing findings from the rank literature and assessing future directions the research may take. The main takeaway is that there is strong evidence across a range of institutions and countries for positive medium- and long-run effects of being more highly ranked in a school-cohort group or class. The effect sizes are generally large when compared to magnitudes found for other factors and interventions. While there are many potential mechanisms, evidence suggests that being ranked higher increases student confidence in their ability, leading to beneficial effects on their behaviors and expectations. Rank effects can provide useful insight into other educational phenomena such as the extent to which students benefit from high ability peers and the presence of a gender gap in STEM. However, the state of knowledge has probably not reached the point where the empirical findings from this literature have practical implications for policy intervention to improve outcomes of students.

One difficulty in summarizing the literature is that, due to many fundamental differences between studies, estimates are rarely directly comparable, and it is often difficult to distinguish differences due to institutional variation from those due to the conceptualization of the problem. As emphasized throughout this review, studies measure human capital at different student ages, there are variations in the peer groups used to rank students, and differences in the extent to which peer groups change between the measurement of rank and the realization of the outcome. These disparities imply that we would expect the effect of rank to vary depending on the exact set-up of the research study. For example, presumably rank should matter more if the student is exposed to the same group (and hence a similar rank) for a longer period. Additionally, if the outcome variable is measured at a point where the student has been exposed to several further peer groups, then this may reduce the effect of rank. To our knowledge, no study has provided an analysis of the persistence of rank over time, within and across peer groups, and this would provide useful evidence to enable comparison between estimates from various studies.

Helpfully, two recent studies provide estimates for the unusual scenario where students change peer group every year (Carneiro et al., 2021) or even more frequently (Elsner et al., 2021). Elsner et al. (2021) find that the effect of rank in a college tutorial section becomes smaller over time as students are randomly assigned to new teaching sections. In contrast, Carneiro et al. (2021) find that the effect of mathematics rank is largest in the earliest elementary school grades and this effect grows over time. They argue that their results are likely due to mathematics rank affecting academic self-concept which then has a persistent effect on learning in later grades. The difference in the findings of Elsner et al. (2021) and Carneiro et al. (2021) can perhaps be reconciled by the early childhood literature whereby “skills beget skills” and achievement gaps that are evident at early ages tend to widen over time (Cunha and Heckman, 2007). While these findings are useful, we expect that the literature will move towards modelling the effect of rank as a dynamic process in which students progress through several peer groups and are impacted by the various ranks they experience as they travel along their educational pathway.

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Appendix: Identification Examples with School-Cohort Effects

Consider, again, equation (1) where we abstract from individual-level variation and consider variation by school (s), cohort (c) and by test score (h) as rank for any individual depends only on their school-cohort and their test score:

$$\boldsymbol{Y_{sch} = \alpha + \beta R_{sch} + v_{sch}}$$

Assumption 2 in the text is that

$$\boldsymbol{E(v_{sch}|s, c, h) = \gamma_h + \theta_{sc}}$$

This states that differences in the error term across combinations of test scores and school-cohorts can be summarized by an additive school-cohort effect and an additive test score effect. It allows the outcome to differ systematically across school cohorts and to differ systematically by test score. However, it posits that, other than rank, functions of interactions between school-cohort and test score do not belong in the model. This allows the identification of rank from cases where differences in rank across test scores are not homogenous across schools. We provide some simple examples below where h is measured in letter grades.

Example 1: Rank Effect is Unidentified

We consider two school-cohorts. The numbers in the boxes below relate to the percentage of the school-cohort that has each letter grade:

School/Grade	A	B	C	D
1	25%	25%	25%	25%
2	25%	25%	25%	25%

We can then calculate the ranks for each letter grade in each school. We break ties by allocating the mean rank. The numbers in the boxes below are the ranks:

School/Grade	A	B	C	D
1	0.875	0.625	0.375	0.125
2	0.875	0.625	0.375	0.125

Here, rank is just a function of letter grade so rank effects are unidentified.

Example 2: Rank Effect is Unidentified

Here, the letter grade distributions have the same variance but different means:

School/Grade	A	B	C	D	E
1	25%	25%	25%	25%	0%
2	0%	25%	25%	25%	25%

We can then calculate the ranks for each letter grade in each school-cohort. We break ties by allocating the mean rank. The numbers in the boxes below are the ranks:

School/Grade	A	B	C	D	E
1	0.875	0.625	0.375	0.125	
2		0.875	0.625	0.375	0.125

In this example, rank is unidentified as it can be written as a linear additive function of letter grade indicators and a school-cohort indicator:

Rank = 0.875*I(A) + 0.625*I(B) + 0.375*I(C) + 0.125*I(D) – 0.125*I(E) + 0.25*I(2)

The lack of identification is also clear because differences in rank across letter grades are homogenous across the two school-cohorts – the difference at each letter grade equals 0.25.

Example 3: Rank Effect is Identified

Here, the letter grade distributions have the same mean but different variances (both distributions are symmetric):

School/Grade	A	B	C	D	E
1	20%	20%	20%	20%	20%
2	10%	20%	40%	20%	10%

We can then calculate the ranks for each letter grade in each school-cohort. We break ties by allocating the mean rank. The numbers in the boxes below are the ranks:

School/Grade	A	B	C	D	E
1	0.9	0.7	0.5	0.3	0.1
2	0.95	0.8	0.5	0.2	0.05

In this example, there is no way of writing the ranks as a linear additive function of letter grade indicators and a school-cohort indicator. Therefore, the effect of rank is identified. This is also clear because differences in rank across letter grades are not homogenous across the two school-cohorts.

Example 4: Rank Effect is Identified

In this example, the variance in letter grades is the same in both school-cohorts but the letter grade distribution is centered differently, and the shape of the distributions differ:

School/Grade	A	B	C	D	E
1	10%	40%	40%	10%	0%
2	0%	10%	40%	40%	10%

We can then calculate the ranks for each letter grade in each school-cohort. We break ties by allocating the mean rank. The numbers in the boxes below are the ranks:

School/Grade	A	B	C	D	E
1	0.95	0.7	0.3	0.05	
2		0.95	0.7	0.3	0.05

In this example, there is no way of writing the ranks as a linear additive function of letter grade indicators and a school-cohort indicator. Therefore, the effect of rank is identified. This is also clear because differences in rank across letter grades are not homogenous across the two school-cohorts.