Class Rank and Long-Run Outcomes

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Abstract

This paper considers a fundamental question about the school environment – what are the long run effects of a student's ordinal rank in elementary school? Using administrative data from all public school students in Texas, we show that students with a higher third grade academic rank, conditional on ability and classroom effects, have higher subsequent test scores, are more likely to take AP classes, graduate high school, enroll in college, and ultimately have higher earnings 19 years later. Given these findings, the paper concludes by exploring the tradeoff between higher quality schools and higher rank.

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Keywords: rank, education, subject choice

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1. Introduction

There is a large literature examining peer effects in education. This literature typically focuses on either the benefits of high performing peers (Sacerdote, 2001; Whitmore, 2005; Kermer and Levy, 2008; Carrell et al., 2009; Black et al., 2013; Booji et al., 2017), or the negative effects of having disruptive peers (Hoxby and Weingarth, 2006; Lavy et al., 2012; Carrell and Hoekstra, 2010; Carrell et al., forthcoming). However, there is another mechanism, where having lower-performing peers could *improve* student outcomes—namely a student' rank**.** [1] We will explore a student's ordinal rank in their classroom and the persistence of this effect on their outcomes into adulthood. Rank is an appealing attribute to study because it naturally occurs in any group of people.

We consider a student's rank in third grade (8 to 9 years old), independent of their achievement, on short and long run outcomes. We use the universe of public school students in Texas from 1994-2006 and combine this with an identification strategy that leverages idiosyncratic variation in rank. We find that a student's rank in third grade impacts grade retention, test scores, AP course taking, high school graduation, college enrollment, and earnings up to 19 years later.[2]

Academic achievement and rank are highly correlated and so we use the method developed in Murphy and Weinhardt (2014) to isolate the effect of a student's rank. To identify this effect, we use idiosyncratic variation in the distribution of test scores across schools, subjects, and cohorts. In particular, we define a student's achievement by their test score expressed as a percentile of the state population. We then compute a student's rank within their school, subject, and cohort and express it as a percentile. The thought experiment is to compare students who have equivalent math achievement (defined below) but differ in their rank in their school. Consider the following hypothetical: two students in successive cohorts of the same size and mean attainment, at the same school, who have the same math

[1] This can occur through various channels. These channels can be categorized as internal (learning about ability, development of non-cognitive skills) and external (parental and school investments).

[2] Some work has studied the introduction of relative achievement feedback measures in education settings. Azmat and Iriberri (2010) find that providing information on relative performance feedback during high school increases productivity of all students when they are rewarded for absolute test scores. In contrast, Azmat et al. (2015) find relative feedback in college causes significant short run decreases in student performance, but no long run effects.

achievement (as measured by their place in the state-wide distribution). Because school cohorts are small relative to the state cohort, there will be variation in the test score distribution such that one student may be the fifth best student in the class and the other may be the eighth. This is the idiosyncratic variation we leverage to identify the effect of rank.

A concern is that despite having the same absolute achievement measures, students may be in very different school contexts. To account for any mean shifting factors, we include fixed effects at the elementary school-subject-cohort (SSC) level. These fixed effects remove the between SSC-group differences in long run attainment growth due to any group-level factor that enters additively and affects all students similarly, such as measurement issues (bad weather on the test day), or the school environment (mean ability of the students in the classroom, impact of the teacher, school infrastructure, etc.). A simple example of this is presented in Panel A of Figure 1, where each mark represents a student's test score. Classes A and B have different test score distributions and differ in mean attainment by an amount θ. Accounting for this difference, we can make a comparison between student i in Class A with student i^* in Class B-θ who have the same relative test score, but have different class ranks due to variation in the test score distribution.

We isolate the effect of rank under the assumption that a student's rank conditional on test score is exogenous. Further, we must assume that we correctly model the true relationship between achievement and future outcomes. We provide evidence in support of both of these assumptions. The large number of classes in our administrative data allows us to estimate very flexibly the relationship between achievement and later outcomes. We can even condition on non-parametric functions of baseline achievement when estimating the rank effect. Panels B and C in Figure 1 provides example classrooms in Texas that demonstrate the idiosyncratic variation in the test score distribution across primary schools can create such a situation.

We perform a battery of robustness checks to establish the underlying assumptions are valid including: higher order polynomials specifications of achievement, non-parametric controls of achievement, estimates for small schools, where there is likely to be one classroom per grade, among others checks. We also show that rank and achievement have different relations to the various outcomes we study, sometimes with effects in the opposite direction, to clarify that the rank effects are genuine and not picking up effects of mis-specified achievement.

We test for heterogeneous rank effects by gender, parental income, and race. We find the impact of rank on male and female students to be very similar, regardless of outcome. In contrast, we find that disadvantaged students (non-white or Free and Reduced Price Lunch) are significantly more affected by rank than their advantaged counterparts. This is seen throughout a student's life, affecting eighth grade test scores, high school graduation, college enrolment, and earnings.

A natural question to ask is, why would third grade rank impact these outcomes? One possibility is that humans think in terms of heuristics (Tversky and Kahneman, 1974), and so use ordinal rank position rather than the more detailed cardinal position within a group. Alternatively, ordinal rank may be easier to observe than cardinal position. Regardless of the precise behavioral origins of the effect, its impact can be seen in the findings that an individual's ordinal position within a group predicts well-being (Luttmer, 2005; Brown et al., 2008) and job satisfaction (Card et al., 2012), conditional on cardinal measures of relative standing. Hence, rank may also impact investment decisions and subsequent productivity. In the education literature, this is known as the Big-Fish-Little-Pond effect where individuals gain in confidence, when they are highly ranked in their local peer group (for a review see Marsh et al., 2008).[3] Parents, teachers or education system could contribute to the Big-Fish-Little-Pond effects—the effect does need to be driven entirely by students. We are agonistic as to what is driving the effects in this paper. Instead, we establish the lasting impact of elementary school rank on long run outcomes.

Recent studies have documented that a student's relative rank matters independent of achievement to short run outcomes. Murphy and Weinhardt (2014) document the effect of primary school rank, independent of achievement, on high school test scores and confidence. A subsequent set of papers by Elsner and Isphording applies the same idea to the United States using data from the National Longitudinal Study of Adolescent to Adult Health (AdHealth) to study effects of contemporaneous high school rank on high school completion,

[3] In the economics literature this has been referred to as the invidious comparison peer effect by Hoxby and Weingarth (2006).

college going (2017a), and health outcomes (2017b).[4] We extend this literature by using administrative data on three million individuals to look at the long-term effects of third grade rank on adult outcomes. Explicitly, we consider a student's rank younger at ages than have previously been considered (age 8-9) on outcomes up to 19 years later, instead of effects of rank two to six years later.[5]

Our findings contribute to a growing literature that documents childhood conditions affect adult outcomes. These conditions range from a child's health (Oreopoulos et al., 2008), where a child lives (Chetty et al., 2016), the quality of a student's teacher (Chetty et al., 2014), size of a student's classroom (Chetty et al., 2011), the age of a student when they start school (Black et al., 2011), and the presence of disruptive peers (Carrell et al., forthcoming; Bietenbeck, forthcoming) among others. We add to this list that a child's rank in their third grade classroom, independent of their achievement, has meaningful effects on education and earnings in adulthood.[6]

We find that rank effects are larger for historically disadvantaged groups such as non-white students or students eligible for FRPL. One implication of this is that, unlike linear in means peer effects, where moving students between groups would have no net impact, re-arranging students with rank in mind could improve overall outcomes. However, this sort of exercise merits caution because the changes in classroom distribution will have general equilibrium effects not accounted for in this paper (Carrell et al., 2013). The more practical implication of this finding is to caution enthusiasm for programs that move disadvantaged students into situations where they will be the lowest ranked student. The extensive literature on selective schools and school integration has shown mixed results from students attending selective or predominantly non-minority schools (Angrist and Lang, 2004; Clark, 2010; Cullen

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[4] The AdHealth home-survey only contains a sample of 34 students of each school cohort, which are used to compute a measure of rank. (Elsner and Isphording 2017a, 2017b). More recently there are a set currently unpublished working paper estimate the impact of rank within college on contemporaneous outcomes in various countries (Elsner et al. 2018; Payne and Smith, 2018; Ribas et al. 2018).

[5] Murphy and Weinhardt (2014) consider the effect of rank on test score 3 and 5 years later. Elsner and Isphording (2017a) consider rank in high school on college enrollment and graduation—approximately 2 to 8 years later. Elsner and Ishpording (2017b) consider the effect of rank on risky behavior as reported 18 months later. Murphy and Weinhardt (2014) consider rank at age 10-11. Elsner and Isphording consider rank at age 14-18.

[6] We find positive effects of higher rank on a range of outcomes with one exception - highly ranked students are less likely to obtain a bachelor's degree, despite being more likely to enroll in college. Obtaining a bachelor's degree is increasing in third grade achievement, but has an inverse-u relationship with third grade rank. We show that these high ranked students may overmatch and enroll in college majors with higher quality peers.

et al., 2006; Kling et al., 2007; Dobbie and Fryer, 2014; Bergman, 2018). Our findings would speak to why the potential benefits of prestigious schools may be attenuated among these marginal/bussed students.

Finally, we address the parental question of which school should parents send their child to, given the existence of the rank effects. We find choosing a school solely on the basis on mean peer achievement, rank effects reduce 39 percent of the potential gains for median performing students in the state. In contrast, choosing on the basis of SSC value added, rank effects only reduce the gains from choosing a better school by 12 percent.

The rest of the paper is structured as follows: Section 2 briefly sets out the empirical design. Sections 3 describes the data. Section 4 presents the results, robustness tests and heterogeneity. Section 5 discusses implications for school choice. Lastly, Section 6 concludes.

2. Empirical Design

We closely follow the method of Murphy and Weinhardt (2014), this method is ideal for large administrative data with many small groupings of students. This is to ensure that we have sufficient variation; specifically, for a given test score we have a range of different rankings.

For an illustration of the variation we use consider Panel A of Figure 1, representing two classes and the distribution of test scores within each. The mean attainment of students is higher in class B by an amount θ, this could be due to having better infrastructure, teachers, conditions on test day, or other school level factors. To account for any underlying differences between classes we condition on school-subject-cohort (SSC) fixed effects.[7] This means that we would then be effectively comparing students with the same score, relative to the mean of their peers, students i and i^*, in classes A and B-θ. However, the distribution of test scores remains different, therefore students with the same relative test score will have different ranks.

This means that we would then be effectively comparing students with the same score, relative to the mean of their peers, but with a different rank due to variation in the test score

[7] For the purpose of conciseness throughout the paper we refer to School-Subject-Cohort (SSC) groups as classes. We do not have class group level information for these students. However in the robustness section we estimate the impact of rank in schools with under 30 students, where the SSC will likely reflect a class and all the findings hold (Figure 12)

distribution. Therefore, the rank parameter only picks up information about ordinal position, and not about cardinal position relative to the class mean. In other words, if cardinal measures of relative achievement were sufficient to explain student outcomes, the rank parameter would be insignificant. A significant rank parameter implies that humans additionally use ordinal rank information to make decisions.[8]

We will exploit the differences in the test score distributions across schools, cohorts, and subjects. This follows a similar strategy of Hoxby (2000), among others, and compares the outcomes of students in adjacent cohorts within the same school. The difference is that the variable of interest in these papers typically varies at the school-cohort level e.g. proportion female, whereas in this paper there is variation in treatment (rank) within each school-subject-cohort. We illustrate the variation in rank we use for a given test score relative to the mean in the lower Panels of Figure 1. Panel B replicates the stylized example from Panel A using seven elementary school classes in Math from our data. Each class has a student scoring 22 and 38 and have a mean test score of 30. Four of the classes have a student scoring 35, however the different test score distributions mean each student has a different rank. Panel C includes math scores from all elementary schools, demeaned at the school-cohort level. The red line represents where each of these students scoring 35 would appear (5 points above the mean). We can see that there is considerable variation in rank for a given cardinal distance to the class average. There is sufficient naturally occurring variation in rank for a given test score throughout the achievement distribution, which allows us to include even non-parametric controls for the primary school baseline achievement measure. We discuss the formal assumptions needed for identification in section 3.2.

2.1. Specification

To estimate the impact of rank on a range of later outcomes we use the following specification

$$Y_{ijsc} = \rho R_{ijsc} + f\left(T_{ijsc}\right) + \boldsymbol{X_i}'\beta + \theta_{jsc} + \epsilon_{ijsc} \tag{1}$$

where Y_{ijsc} is the outcome of student i who attended elementary school j in subject s from cohort c. This will be a function of academic rank, R_{ijsc}, in third grade, a flexible measure of

[8] In this paper we make no attempt to assign who is making the decisions which impact student outcomes. Murphy and Weinhardt (2018) provide evidence that neither parents or teachers change their investment in students during secondary school on the basis of primary school rank, conditional on ability. In contrast, survey evidence implies that students' confidence is impacted by their ordinal rank.

third grade test scores $f(T_{ijsc})$ in each subject, observable student demographic information $\boldsymbol{X_i}$, and set of elementary SSC fixed effects, θ_{jsc}.

Since we have student achievement and rank information in two subjects (math and reading), we stack the data over subjects for our primary analysis. Our preferred specification investigates potential non-linearities in the effect of ordinal rank on later outcomes, by replacing the linear ranking parameter with indicator variables according to quantiles in rank.

$$Y_{ijsc} = \sum_{n=1,n\neq 10}^{20} I_n R_{ijsc}\,\rho_n + f(T_{ijsc}) + X_i{}'\beta + \theta_{jsc} + \epsilon_{ijksc} \qquad (2)$$

For many outcomes, especially longer-run outcomes, the effects seem to be largely nonlinear. Hence, we will primarily focus on equation 2. We model $f(T_{ijsc})$ many ways, but our preferred specification controls for test scores non-linearly using twenty indicators for a student's achievement according to their ventile position in the state-wide achievement distribution. Note all standard errors are clustered to allow for any spatial and temporal autocorrelation within elementary school j. This is broader than the unit of randomization which is the school-subject-cohort level jsc, and so is more conservative as it also allows errors to be serially correlated across cohorts and subjects within a school.

In summary, if students, parents, or teachers react to ordinal information as well as cardinal information, then we would expect rank to have a significant effect on later achievement when estimating these equations. This is what is picked up by the ρ parameter. The following sections discuss identification, the setting, and how rank is measured before we turn to the estimates.

2.2. Identification

We must make two assumption for our estimates of ρ or ρ_n to be causal. The first assumption is about identification. Let $Y_{ijsc}(r)$ be the distribution of potential outcomes for an outcome Y as a function of potential rank r. Formally,

A1: $$E[Y_{ijsc}(r)|A_{ijsc}, X_{ijsc}, \theta_{jsc}, R_{ijsc}] = [Y_{ijsc}(r)|A_{ijsc}, X_{ijsc}, \theta_{jsc}]$$

This assumption is that the distribution of potential outcomes are conditionally orthogonal to observed rank. This assumption is motivated by the thought experiment that a student rank is "as good as" random after controlling for test scores, SSC fixed effects, and student demographics. If students sort into class on the basis of mean characteristics or outcomes of the SSC, this would be captured by θ_{jsc}. Further, $f(T_{ijsc})$ captures effects on later outcomes driven solely by student achievement.

Violations of A1 would occur if student sorted into SSCs based on what their rank would be. Observing student rank in a SSC before enrolling in the SSC would be difficult for three reasons. First, this would require parents to know the ability of their child and of the potential peers in each of the potential schools. Second, we will show that there is considerable cohort to cohort variation within a school such that knowing previous cohort distributions would not be sufficient to predict rank accurately. Third, student grade retention, grade acceleration, or transferring schools all change the composition of a school cohort and will change the ranking of students as a cohort moves from one grade to the next. Hence, it is difficult for a parent to predict the third grade rank of their child in various SSCs.

Further, Hastings et al. (2009) show that parents prefer schools that have high mean performance. They also show that higher ability students' parents are more likely to prefer schools with higher mean achievement than the parents of low-ability students. This implicitly goes against sorting to schools for higher ranks.[9]

We illustrate the difficulty of sorting into a SSC based on rank as well as the amount of natural occurring variation in rank in Figure 2. In Figure 2 we focus on students at the median of the state test distribution. The horizontal axis indexes schools-subject groups. Within each school-subject, we plot the rank of a student who scored at the state-wide median in their school-subject cohort for all cohorts observed at that school. We sort schools based on the average rank of the statewide median student over all cohorts. Hence, schools on the left are relatively low-performing because students at the median have relatively high rank.

[9] Texas implemented the Top Ten Percent Rule in 1998. This gave high school students in the top decile of their class automatic admission to any public university in Texas. Student rank for this rule was determined in 11th or 12th grade. Cullen et al. (2013) document that some students changed their enrollment behaviour in response to this rule. However, the number of students was small—Cullen et al. (2013) estimate that 211 students per cohort changed the high school they attended and that this was driven by students opting out of magnet schools and into their assigned public school. This sorting is very unlikely to be driving 3rd grade sorting into SSCs for a number of reasons. First, some of the cohorts we examine were before the implementation of the top 10 percent rule. Second, performance in elementary and middle schools does not directly factor into the calculation of top ten percent status. Third, 3rd grade students are at least six years away from entering high school and so the decision is likely not salient.

The vertical thickness of the distribution indicates the support throughout the rank distribution of approximately 20 percentiles in each school-subject. This means that within school, there is considerable variation in where the median student would rank across cohorts. In fact, the within school standard deviation of a student with the median statewide test score is 0.08. Further, the average within-school-subject difference in rank between highest rank and lowest rank for the median student is 0.17.

This figure demonstrates that within school-subjects, there is a lot of variation in observed rank across cohorts. Hence, knowing a students' exact rank (conditional on achievement and school subject averages) would be very difficult. Hence, sorting on the basis of rank would be difficult.

Our second assumption, A2, is that we correctly specify the relationship between outcomes Y and $R_{ijsc}, X_i, T_{ijsc}, \theta_{jsc}$. Formally:

A2: $$E\left[Y_{ijsc}(r)\middle|A_{ijsc}, X_{ijsc}, \theta_{ijsc}, R_{ijsc}\right] = f(R_{ijsc}) + f\left(T_{ijsc}\right) + X_i{}'\beta + \theta_{jsc} + \eta_{ijsc}$$

$$\text{and } \eta_{ijsc} \perp R_{ijsc} .$$

In the above equation η_{ijsc} is specification error. We must assume that this error is uncorrelated with rank. A special case is that $\eta_{ijsc} = 0$ which says that we correctly model the true relationship between outcomes Y_{ijsc} and $R_{ijsc}, X_i, T_{ijsc}, \theta_{jsc}$. If there is specification error, that is if $\eta_{ijsc} \neq 0$, we still recover the causal effect of rank as long as the specification error is uncorrelated with observed rank R_{ijsc}. A similar assumption is required for many empirical settings such as differences in differences.[10]

We model the relationship between achievement and outcomes in many ways and find consistent result. This suggests that assumption A2 is likely to hold. This assumption highlights the benefit of using large data sets that allow for very flexibly estimates of the relationship between achievement and outcomes.

[10] Murphy and Weinhardt (2018) show that random noise in the ability measure can lead to a downward bias in the rank estimate. Moreover, in section 5.5. below we present and discuss results where the effects of rank and of academic performance for college going can have opposing effects. This is additional evidence that rank has an independent effect and is not merely measuring academic performance.

3. Data

The data we use in this study is the de-identified data from the Texas Education Research Center (ERC), which contains information from a number of state level institutions.[11] Data concerning students' experience during their school years cover the period 1994–2012, although the primary estimating sample will focus on 1995–2008. These data contain demographic and academic performance information for all students in public K–12 schools in Texas provided by the Texas Education Agency (TEA). These records are linked to individual-level enrollment and graduation from all public institutions of higher education in the state of Texas using data provided by the Texas Higher Education Coordinating Board (THECB). Ultimately, these records are linked to students' labor force outcomes in years 2009-2017 using data from the Texas Workforce Commission (TWC). This contains information on quarterly earnings, employment and industry of employment for all workers covered by Unemployment Insurance (UI).[12]

3.1. Constructing the Sample

The sample used for this analysis consists of students who took their third grade state examinations for the first time between 1995 and 2008.[13] We focus on students taking their third grade exam for the first time to alleviate concerns regarding the endogenous relationship between class rank and previous retention. We focus on students taking their exams in English, rather than Spanish. During this period, the third grade students took tests annual reading and math assessments, although the testing regime changed.[14] Consequently, we percentilize student achievement by subject and cohort. This ensures that the test score distribution for each subject is constant for each cohort. For each student we generate a rank within their elementary school cohort for math and reading based on their test scores including those who had been retained.

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[11] For more information on the ERC see https://research.utexas.edu/erc/

[12] Unemployment insurance records include employers who pay at least $1,500 in gross earnings to employees or have at least one employee during twenty different weeks in a calendar year regardless of the earnings paid. Federal employees are not covered.

[13] Students are defined as taking their third grade exam for the first time if the student was observed not being in the third grade in the previous year.

[14] Until 2002, the Texas Assessment of Academic Skills (TAAS) was used. Starting in 2003, the Texas Assessment of Knowledge and Skills (TAKS) was used. The primary differences had to do with which grades offered which subject tests. This does not affect this study substantively as all students took exams in math and reading for 3rd and 8th grade

We link students to subsequent outcomes including performance in reading and math in eighth grade. In order to not conflate the impacts of retention on test scores, we only estimate the impact on eighth grade test scores on those who took the test on time. We also consider classes taken in high school including Advanced Placement courses, and graduation from high school. We then consider whether students enroll in a public college or university in Texas (separately by two year and four schools), if they declare a STEM major and whether student the student graduates from college. Lastly, we look at the probability of earnings and the probability of having positive UI earnings.

For binary outcomes such as AP course taking, high school graduation, and college enrollment, we define the variable as 1 for the event occurring in a school covered by our data and 0 otherwise. For eighth grade test scores, we only consider students who took eighth grade tests. For earnings, we consider both average earnings including zeroes as well as excluding zeroes.

To maximize the sample, we consider as many cohorts as possible for each outcome. This means that we have more cohorts for outcomes closer to third grade and fewer cohorts for later outcomes. For K-12 and initial college attended outcomes we have 13 cohorts of students who took their third grade tests between 1994 and 2006, 6,117,690 student subject observations. For graduating college in four years we have 10 cohorts (1994-2003) totalling 4,573,672 student-subject observations. For graduating in 6 years and post college outcomes for individuals aged 23-27 we have 8 cohorts (1994-2001) or 3,597,340 student-subject observations, and 6 cohorts or 2,647,240 students for graduating with a BA within eight years. This explains the discrepancy in sample size across different outcomes.

Table 1 presents summary statistics. The sample is 47 percent white, 35 percent Hispanic, and 15 percent Black. 70 percent of students in the sample eventually graduate from a Texas public high school. 46 percent of students attend a public university or college in the year after "on time" high school graduation.[15] Within three years of on time high school graduation, 23 percent attending a public four-year institution in Texas and 31 percent attending a Texas

[15] On time graduation is defined as graduation if a student did not repeat or skip any grades after grade 3.

community college. When students are 23-27 years old, 65 percent have non-zero earnings, where the average non-zero earnings is $24,818 in 2016 dollars.

3.2. Rank Measurement

We rank each student among their peers within their grade at their school according to their scores in standardized tests in each tested subject. Simply, a student with the highest test score in their grade will have the highest rank. However, a simple absolute rank measure would be problematic, because it is not comparable across schools of different sizes. Therefore, like state test scores we will percentilize the rank score individual *i* with the following transformation:

$$R_{ijsc} = \frac{n_{ijsc}-1}{N_{jsc}-1}, \qquad R_{ijsc} = [0,1]$$

where N_{jsc} is the cohort size of school j in cohort c of subject s. An individual's *i* ordinal rank position within this group is n_{ijsc},, which is increasing in test score. Here R_{ijsc} is the standardized rank of the student which we will used for our analysis. For example, a student who had the second best score in math from a cohort of twenty-one students (n_{ijsc}=20, N_{jsc}=21) will have R_{ijsc}=0.95. This rank measure will be approximately uniformly distributed, and bounded between 0 and 1, with the lowest rank student in each school cohort having R=0. In the case of ties in test scores, each of the students with the same score is given the mean rank of all the students with that test score in that school-subject-cohort. However, our results are similar if we break ties by assigning students the bottom rank, randomly break the ties, or only consider "on time" students in the third grade class.[16] We will calculate this rank measure for each student for each standardized test they participate in.

Note that this is our measure of the academic rank of a student within their class. Students will not necessarily be told their class rank in these exams by their teachers, nor do we believe that students care particularly about their ranking in these low-stakes examinations. Rather we interpret our test score rank measure as a proxy for their day-to-day academic ranking in their class. Students learn about their rank through repeated interactions throughout elementary school with their class peers, who answers the most questions or gets the best

[16] Our main analysis limits the sample to students who took the their third grade test on time. However, their actual classroom consists of students who are on time and students who are not. We show that our results are very similar when we calculate rank only using students taking third grade on time. Results using different methods to break ties are available in Appendix Table 1.

grades in assignments.[17] Similar arguments can be made for teachers or parents learning about the rank of students.

4. Results

We will primarily present results of specification 2 by plotting the estimate coefficients, ρ_n, along with the 95 percent confidence intervals. All estimates will be relative to the ventile that includes students ranked from 45-50 in their class. Because there are many estimates for each outcome, we present the results visually.[18]

4.1. K-12 Outcomes

We first consider the probability of repeating third grade in Figure 3. Figure 3 panel A shows that lower ranked students are more likely to repeat third grade even after conditioning on achievement. The effects are unsurprisingly coming from the lower ranked students. Moving a student from being ranked last to being ranked in the lower 25th percentile reduces the probability for retention by roughly 4 percentage points. Given the mean retention rate of 1.6 percent, this represents a sizable shift. There is a discontinuous jump for those in the lowest ventile of the rank distribution, which is double that of the next highest ventile (5.4 percent versus 2.7 percent). This result shows that rank affects how students are treated by their schools, independent of their ability.

We next examine the effect of third grade rank on achievement in eighth grade where achievement in eighth grade is measured in state percentiles. Figure 3 Panel B shows an approximately linear effect of rank in third grade on academic performance. Moving from the 25th percentile to the 50th percentile in rank improves performance by approximately 2.5 percent. This is similar to the estimates in Murphy and Weinhardt (2014) that consider outcomes at comparable ages in England, finding the same change in rank at the end of

[17] We use rank in third grade as it is the earliest measure of ranking available. It is be possible to estimate the impact of later grade ranks on outcomes. However, as we find that future test scores are increasing in previous rank, it means that later rank itself is an outcome and rank is self-perpetuating. Hence, it is difficult to determine when a child's ranking has the largest impact. Ideally, we would rank students based on a before school measure of achievement.

[18] The corresponding estimates and standard errors are available in table form upon request.

primary school (age 10/11) improves performance national test scores at age 13/14 by 1.9 percent.[19]

The results on eighth grade test scores are not novel, but they do corroborate that similar rank effects occur in different educational systems, establishing the external validity of each estimate. Moreover, they do provide a mechanism for the later outcomes we observe. In particular, student achievement in eighth grade is correlated with many outcomes including high school achievement, class taking, college enrollment and success, and labor market outcomes.

We also consider whether a student takes advanced placement courses in Figure 4. In the first two panels of Figure 4 (A & B), we use our standard specification where the two observations for math and reading for each student are stacked and so we are estimating the mean rank coefficients. We see that elementary school achievement rank linearly affects the probability of taking AP Calculus or AP English. In both cases, the point estimates of combined rank effects in math and reading are similar for AP Calculus and AP English. With the exception that there is a discontinuous jump in the probability of taking AP Calculus if the student is in the top ventile of their elementary school. Note however, the baseline rate for taking AP Calculus and AP English for our sample is 8.4 percent and 19 percent respectively.

The second two panels of Figure 4 (C & D) we consider the effect of rank separately by school subject. Here we run specifications where we control for achievement in third grade math and reading separately and simultaneously allowing there to be a different rank effect for math and reading. In Panel C, a higher rank in math causes more students to take AP Calculus. Most of this effect occurs for students above the median in rank, whereas below the median there are small difference in the probability of taking AP Calculus. In contrast a student's rank in reading has very little effect on taking AP Calculus for students with rank above the median. For students below the median, low ranks have small effects on taking AP Calculus. In Panel D, rank in Math again has a stronger effect than rank in reading. However,

[19] We only consider students who took the test in 8th grade "on time." However, rank causes some students to be retained. Hence, the estimates of the effect on 8th grade test scores are difficult to interpret. No simple correction can be used to address this problem, so we present results for on-time 8th graders but note the difficulty in interpreting these results.

rank in reading does positively affect taking AP English Courses. This is evidence that any rank effects are subject specific and have spillover effects into other subjects.

The final set of K-12 outcomes we consider is whether a student graduated from high school. The time frame we consider is within within three years of "on time" high school graduation. On time graduation is defined by nine years after their third grade to avoid issues of grade retention, standing at 70 percent in our sample. The impact of third grade rank can be seen in Panel A of Figure 5. A higher rank makes students more likely to graduate from high school. The effect is non-linear, coming primarily from students who are above the median in class rank. There is no discernable difference from being last in third grade to being at the 25th percentile. In contrast, the benefit of rank increases from 2.1 percent at the 75th percentile to 7 percent for those being top of their third-grade class.

In summary, a student's rank in third grade independently affects grade retention, testing performance 5 years later, class selection, and ultimately graduation. As we examine longer term outcomes, these changes throughout schooling will be some of the channels that affect things such as college education and earnings. Many of these findings are novel in and of themselves—in particular, a student's rank in their elementary school classroom at age 8 or 9 affects their probability of graduation from high school.[20]

4.2. College Outcomes

Given that rank in third grade impacted outcomes during high school, examining college entry is a natural next step. Figure 6 presents enrollment in any public college in Texas. The relationship between rank and college enrollment is a bit puzzling. There is a slightly u-shaped relationship with low ranked students more likely to attend college than those around the median, and then large positive effects on enrollment for students in the top of their class. Note there is a positive relationship between third grade test scores (i.e. state rank) and college enrollment for low ranked students (Appendix Figure 1.A). The fact that low rank has a positive effect on college attendance, and a low-test score has a negative effect clearly indicates that the rank effect is not just picking up unobserved ability.

[20] Elsner and Isophording (2017a, 2017b) document that a student's rank in high school affects graduation from high school and risky behavior, rather than their rank in a previous setting.

To understand this pattern better, we consider enrollment in two-year and four-year schools separately in Figure 7. Figure 7 Panel A considers enrollment in two-year institutions where the effects on enrollment are again U-shaped. Low ranking increases the probability of going to community college; similarly, high rank increases the probability of attending community college. The effects sizes are large relative to the baseline community college enrollment rate. A student in the 90th percentile for rank is nearly 4 percentage points more likely to attend community college than a student with the median rank. The baseline rate is 31 percent. Figure 7 Panel B shows enrollment in a four-year institution and finds enrollment is increasing in rank, and this is driven by very high and very low-ranking students. Elementary school rank appears to have little impact on weather a student attends a four year college if they are within the 25th to 75th percentile.

The patterns in Figure 7 are consistent with some low ranked students enrolling in two-year institutions rather than four-year institutions. However, low rank induces some students to attend two-year institutions that would not have attended any college. This may be the result of tracking students into different trajectories based on rank. These different educational paths could encourage students to attend two-year schools where more vocational training is available.

Once at college students can declare a major. Given the significant returns to STEM majors we now estimate the ultimate impact of third grade rank on the probability of a student declaring a STEM as their first major. Figure 8 Panel A shows that there is a strong positive relationship between students' rank in elementary school and their likelihood of choosing a STEM major.[21] Students in the bottom 10 percentiles of the classroom are at least 1 percentage point less likely compared to the median student with a baseline enrollment of 4.1 percent. Like AP choice in high school, major choice is likely to be impacted by students rankings in particular subjects, and this previous estimate is the average effect of both reading and math ranks. To explore this Panel B of Figure 8 presents the impacts of third grade math and reading rank on declaring a STEM major. Here we find that the relationship is driven by math rank,

[21] We code students who declare a STEM major as 1 and students who do not as 0. Hence, estimates will conflate the effect of rank college enrollment and declaring a STEM major.

with top ranked math students being 1.5 percent more likely to choose STEM. In contrast top ranked reading students are less likely to choose a STEM major.

Continuing with post-secondary outcomes, the first column of panels in Figure 7 considers graduation with a bachelor's degree within various time frames—4 years (Panel A), 6 years (Panel B), and 8 years (Panel C) after "on-time" graduation from high school.[22] In all cases, there is an unusual pattern where rank has an inverted-U relationship to graduation with a bachelor's degree. It is not surprising that rank below the median causes students to graduate at lower rates. We have already seen that bottom ranked students were 1.3 percent less likely to enroll in a 4-year college, but here we see that they are 2.5 percent less likely to have a bachelor's degree within four years, indicating that rank continues to have an impact. What is more unexpected is that higher ranked students are *less* likely to graduate with a bachelor's degree.

One potential explanation of this is that having a high rank may lead to "overmatch" where students attend colleges where they are not prepared. This is consistent with overconfidence arising from rank. This deleterious effect of rank contrasts with other outcomes including high school graduation, class taking, etc. Evidence of this mechanism can be found in Appendix Figure 1 Panel B, which again shows that the relationship between third grade achievement and college graduation is always positive. Here we can see that high achieving students are more likely to obtain a bachelors degree within 6 years, whereas high ranking students are less likely to obtain that qualification. Appendix Figure 2 provides additional evidence consistent with overmatch, by showing that conditional on achievement, students with higher third grade rank enroll in colleges with higher achieving peers, as defined by mean third grade state percentile.

4.3. Labor Market Outcomes

We consider the effects of rank on a range of labor market outcomes. First, we examine employment outcomes for students ages 23-27 (or 15-18 years after third grade). We consider average annual earnings between the ages for eight cohorts of students who took their third

[22] Bachelor degrees are only available at 4-year institutions.

grade examinations between 1994-2001 (employment years 2009-2016).[23] The first panel in Figure 10 considers the probability of having positive earnings from age 23-27. A value of one would mean having positive recorded in each of these years, a value of 0 would mean having no recorded earnings between ages 23 and 27. The pattern in Panel A suggests there is not much effect of rank on the probability of having positive earnings until class rank is above the 80th percentile at which point rank positively affects the probability of having earnings. Because we have earnings from Unemployment Insurance, this effect could be a few things. First, it could be a labor supply response where it is an actual increase in positive earnings. Second, it could be an increase in the probability of staying in state or working in sectors covered by unemployment insurance.[24] People with higher educational attainment are more likely to migrate (Greenwood 1997). This suggests that students with third grade rank may be more likely to leave the state due to their higher academic performance. However, we find that higher rank leads to an *increase* in the probability of observing earnings. This pattern suggests the effect on earnings is likely a labor supply response.

The remaining panels of Figure 10 refer to the effect of rank on earnings. Panel B shows that increasing rank increases average annual earnings between the ages of 23-27. Low ranked students have meaningful earnings penalties, earning $1,200 per year less than the median ranked student. High ranked students see increases in earnings as well, but the effect is concentrated among the highest ranked students. The lower two panels show the effect on non-zero earnings and log earnings (Panels C and D respectively). Conditional on having positive earnings, we find the negative impact of having a low rank is exacerbated. Students at the bottom now earn $2,500 less per year, compared to students in the middle of the rank distribution. Taking the log of average annual earnings shows that rank affects the log of average earnings throughout the distribution of rank.

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[23] Note, not all individuals have four years over which to average employment. Those from the most recent cohort only have one year of labor outcomes, for instance. We average over all years from age 23 to 27 for which we have earnings.

[24] Unemployment Insurance records cover employers who pay at least $1,500 in gross earnings to employees or have at least one employee during twenty different weeks in a calendar year. Government employees are not covered. Andrews et al. (2016) uses Census data to show that for students who attended the two flagship universities in Texas, there does not appear to be a systematic difference in earnings for those in state versus those who leave the state.

Taken together, rank in third grade affects labor market outcomes. Moving from the 25th percentile to the 75th percentile in rank causes log earnings to increase by approximately 7 log points.

4.4. Robustness

We show that our results are robust to several alternative specifications and samples. First, we model the relationship between achievement and outcomes using various functional forms. Figure 11 presents results for four main outcomes, eighth grade test scores, graduation from high school, the probability of attending any college, and real earnings. The point estimates are displayed for various controls for student achievement. We model student achievement using various polynomials from first order to a sixth order. Additionally, we control for achievement using ventiles in student achievement (our preferred specification). The results are substantively similar once achievement is controlled for with a quadratic.[25] Hence, our results are not dependent on the functional form chosen to model achievement. Moreover, Appendix Figure 3 shows that our estimates are not dependent on the conditioning set of demographics, by estimating the rank effects with and without them.

One data limitation is that we do not observe which classroom students are taught in for third grade students. Hence, our main results use fixed effects for school-subject-cohort (SSC), which we have been referring to as a class. As a robustness check, we estimate the effects separately on SSCs with fewer than 30 third graders in a school-subject-cohort. These schools are likely to have one classroom of third graders. Figure 12 presents the point estimates by SSC size. In particular, we present results for under 30, under 60, and under 90 students in a SSC as well as our main specification with all SSCs. We show that our results do not vary meaningfully when we focus on small SSCs. The notable exception is the impact on college attendance. Here, for smaller SSCs there is a negative impact on any college attendance for being below the median.

In our main specification, we handle ties in rank by assigning students the mean of the rank. We consider other methods including breaking ties including assigning the lowest rank,

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[25] The fact that only allowing for a linear impact of percentile rank leads to different estimates may be reflective of the underlying ability distribution being normally distributed, which we have transformed into a uniform distribution through percentalization.

randomly breaking ties, and a rank only among students who are "on-time" in third grade. Our results are qualitatively similar regardless of our method of dealing with ties. The results tend to be slightly smaller when we break ties randomly which we attribute to the introduction of noise into our measure of rank (See Appendix Table 1).

We also perform our analysis on a consistent subsample. That is, we fix the sample as the cohorts who we observe for the most distant outcome (earnings age 23-27) and estimate all of the outcomes on that sample. Our results are qualitatively similar with larger standard errors as would be expected (See Appendix Figure 4).

Rank and achievement are measured using the same test score; as a result, measurement error in test scores would generated correlated measurement error in the rank variable, which could affect the interpretation of rank effects. Murphy and Weinhardt (2018) show that additive random noise would non-linearly downward bias the effect of class rank depending on the extent of the measurement error. Murphy and Weinhardt (2018) shows that additional measurement error equal to 30 percent of the original standard deviation (and recalculating the ranks) reduces the rank coefficient by 34 percent.

4.5. Heterogeneity

In this section we explore the heterogeneity of the rank effects the four main outcomes; 1) eighth grade test scores; 2) Graduating High School; 3) Enrolling in College; and 4) Real Earnings 23-27. We explore this for three pre-defined variables; race (white/non-white), gender (male/female), and free and reduce price lunch eligibility in third grade (eligible/non-eligible). We present the estimates for each of these categories in Figures 13, 14 and 15.

First, is the impact of third grade rank different for white and non-white students? Panels A and B of Figure 14 show that a student's class rank is more impactful, both positively and negatively, for non-white students. Bottom ranked white students achieve 4.9 percentile points lower on eighth grade test scores, whereas the equivalent non-white students have a reduction of 6.9 percentile points. At the top of the distribution white students gain by 7.3 percentile points whereas non-white students gain by 8.6 percentile points. These differences are starker when we look at high school graduation. Here, previously low ranked white students are no less likely to graduate, but non-white students are 3.7 percentage points less likely. In contrast, top ranked non-white students are 9 percent more likely to graduate, whereas white students only gain by 3.1 percentage points.

A similar pattern can be seen with the any college enrollment outcome. Panel C shows that the positive estimates for having a low rank on college attendance are all driven by white students. Non-white students are less likely to go to any type of college as a result of rank, if they had a below median rank in the third grade. These patterns could be explained with low ranked students replacing 4-year college attendance with 2-year colleges, and non-white students replacing college attendance with no college attendance. Non-white students gain more from having a high rank compared to their white counterparts. As may be expected the impact on earnings follows that of college attendance, in that non-white students are significantly more impacted by third grade rank than white students.

In contrast to the large differences by race there is no evidence for heterogeneity with respect to gender (Figure 14). This is different to Murphy and Weinhardt (2018) who show that boys react more strongly to rank during high school in England.

Finally, the heterogeneity of estimates with regards to FRPL students mirror those of white/non-white students, with the more disadvantaged group being more effected by rank than their counterparts (Figure 15). This is true for test scores, graduation, college enrollment and earnings. This is in accordance with Murphy and Weinhardt (2018) who find that Free School Lunch students gain more form being highly ranked in England. One explanation is that these sets of students are have low academic confidence or a different information set about the achievement, and weight their school experience more heavily than non-disadvantaged students. This has important consequences for optimal classroom composition, which we discuss below.

5. Discussion of the size of Rank Effects and School Quality

We estimate the effects of rank net of SSC fixed effects. Traditional peer effects suggest that better peers should help performance. However, we show that having more better peers also has a negative effect by lowering rank. In this section we quantify the effects of rank as compared to the benefits of having better peers and school environments.

Consider the following thought experiment. A parent may move their child to a "better" school. This would come with a decrease in their child's rank and a likely increase in the quality of their child's peers. What would be the net gain in test scores from such a move?

To operationalize this, we categorize elementary schools into "good", "bad" and "average" in terms of their third grade achievement (Table 2). To do this, we regress third grade test scores on indicators for a student's third grade school. This fixed effect captures many things including, student ability, peer effects, resource differences, parental investments, etc. Each of these will reflect the average difference in third grade test scores between primary schools and should not be considered causal, but may reflect what parents consider when choosing an elementary school, as they are relatively easy to observe. Similarly, we calculate elementary school's third to eighth grade unconditional value added by controlling for students' third grade achievement. The standard deviation of these value added measures is .055. So, if a student moves to a one standard deviation better school, she receives a bump of .055 in eighth grade test scores. To gauge the benefit of attending an elementary school with better attainment we also record the mean value added of "good", "bad" and "average" schools in terms of attainment. It appears that schools with higher mean achievement also have higher third-to-eighth grade value added, although these gains are only half the size compared to if parents were selecting schools on the basis of value added, 0.026 versus 0.055 (final row of Table 2).

To ascertain the net benefits of attending these schools net of rank effects, we need to consider how a student's rank would change. The majority of Table 2 consists of presenting the mean rank of students in each state achievement ventile in each school type. For example, consider the median student at the tenth achievement ventile. If they attended an average elementary school in terms of third grade achievement, their expected class rank would be 0.479. Whereas if they attended a "bad" school they would have an expected rank of 0.613, and 0.346 if they had attended a "good" school. We can see that there is a clear trade off in terms of rank and the quality of school, when measured in absolute achievement. As may be expected this rank-quality tradeoff is higher when elementary school quality is measured in mean achievement compared to value added. When parents move their child from a "bad" value added school to a “good value added school, the loss in rank is only -0.155. If they instead use mean achievement the change in rank is larger at -0.267 (Table 2 row 10).

We can see that students from higher-up in the state achievement distribution also have higher ranks in their classes. There is not much difference in terms of expected rank at an average school, independent whether school quality is measured in terms of value added or absolute attainment throughout the achievement distribution. However, this thought experiment clearly shows that the decrease in rank from moving from a bad to a good school is always smaller when considering schools in terms of their value added. Moreover, the loss of rank from attending a ‘better’ school is largest for the students near the middle of the distribution.

How would these changes in ranks and school environments impact a student’s overall attainment? For this, we require one last piece of information, ρ from equation 1, the relationship between rank and eighth grade test score, which is 0.09. We can now calculate the impact on test scores.

Let us consider the case of parents of a median student (tenth ventile) considering ‘good’ or ‘bad’ schools in terms of mean third grade achievement. Sending the child to the better school, would lead to an increase of eighth grade test scores by 6.1 percentiles (using the associated value added scores from the bottom of Table 2 (0.026-(-0.035)). However, sending the child to the better school would reduce the students expected rank by 0.267. This would reduce the student’s eighth grade test score by 2.4 percentiles (0.09*0.267). Therefore, the rank effect has reduced the gains from attending a school two standard deviations better by 39 percent, resulting in a net gain would be 3.7 percentiles.

Alternatively, if parents were better informed and selected elementary schools on the basis of value added then there would be a smaller trade off in class rank (0.15) and larger increases in future test scores (0.11=0.055-(-0.055)). In this case, a median student attending a good school rather than a bad school would gain 11 percentile points in the eighth grade test score distribution. In contrast, the student would lose 1.4 percentiles due to their lower rank (-0.014=-0.15*0.09). Hence, if parents were to choose on the basis of value added, there would be a net gain of 9.6 percentiles.

In the case parents choosing on the basis of value added, the effect of school quality is roughly eight times the size of the rank effect. Note, while this rank effect is relatively small, this school quality measure encapsulates *all* observable and unobservable factors that contribute to student value added. Moreover, while choosing the best school for their child in value-added is what most parents try to achieve, value-added is difficult to observe. Basing school choice on the basis of mean achievement, we see that the importance of class rank is not inconsiderable, reducing any perceived benefits by up to 39 percent. The main message for parents weighing the tradeoff of rank is that choosing schools based on value-added is the best strategy.

Finally, what does the presence of rank effects mean for optimal classroom composition given the heterogeneity of the effects? We find that disadvantaged groups such as non-white students or students eligible for FRPL gain more from being highly ranked and lose more from being lowly ranked among their peers. Therefore, unlike linear in means peer effects, where moving students between groups would have no net impact, re-arranging students with rank in mind could improve overall outcomes. This would involve creating groups of students such that disadvantaged students predominantly have higher ranks than non-disadvantaged students. However, this sort of exercise merits caution because the changes in classroom distribution may be out of sample for the estimates in this paper (Carrell, et al., 2013).

This finding should caution enthusiasm for programs that move disadvantaged students into situations where they will be the lowest ranked student. The extensive literature on selective schools and school integration has shown mixed results from students attending selective or predominantly non-minority schools (Angrist and Lang, 2004; Clark, 2010; Cullen et al., 2006; Kling et al., 2007; Dobbie and Fryer, 2014; Bergman, 2018). Our findings would speak to why the potential benefits of prestigious schools may be attenuated among these marginal/bussed students. This is consistent with Cullen et al. (2006, p. 1194), who find that those whose peers improve the most gain the least: "Lottery winners have substantially lower class ranks throughout high school as a result of attending schools with higher achieving peers, and are more likely to drop out".

6. Conclusions

We demonstrate that a students' rank among their peers at a young age has long lasting impacts. This affects a student's performance in school including tests, courses taken, progress through toward graduation. Ultimately, it also affects student graduation from high school. Relative position affects the decision to enroll in post-secondary education. Most strikingly, it affects a student's real earnings in their mid-twenties. We find that a student enrolling in a class where they are at the 75th percentile rather than 25th in third grade increases their real wages between ages 23 and 27 by $1500 per annum, or approximately 7 percent. [26] For comparison, Carrell et al. (forthcoming) look at the long run impact of peers at the same ages, and find that being in a class of 25 with a student who was exposed to domestic violence reduces an individual's earnings by 3 percent.

Our findings add to a growing list of papers that demonstrate conditions for young children have long lasting consequences. In contrast to other papers that focus on policy differences that students face, we document the effect of an unavoidable phenomenon in groups—relative rank. Documenting these differences raises the question if policies explicitly focusing on lower ranked students rather than low ability students may raise student outcomes for some students. These policies need not replace policies focusing on low ability students but may serve as a useful complement.

In fact, some of the effect of rank may be coming via teachers and administrator interactions with students. We document that students are more likely to be retained in third grade which is a decision made not by the student but by teachers, administrators, and families.

Finally, we examine if and to what extent parent should consider rank effects when choosing the best school for their children. Critically, we document a trade-off from attending a school with high achieving peers: this mechanically lowers the rank of your own child. We examine this trade-off in detail based on the observed student and school allocations in Texas. We find that rank offsets about 40 percent of the benefits of school value added for the median

[26] Using the present discounted value of earnings of $522,000 as in Chetty et al. (2014), which follow Krueger (1999) in discounting earnings gains at a 3 percent real annual rate, we calculate that these rank differences would increases life time earnings by $36,540 in net present value. This figure is based on the point estimates from Chetty et al. (2014) Figure 10 Panel 5. The 5th ventile has a coefficient of -0.032 and the 15th ventile has an estimate of 0.035.

performing student, if parents choose schools based on mean peer achievement. Instead, if parents choose schools based on value-added, the offsetting effects of rank from attending a better school are much smaller.

Future research on rank should focus on the interaction between rank and policies that exaggerate or mediate the effects of rank. Future research should also consider the effect of rank in groups outside of school settings.

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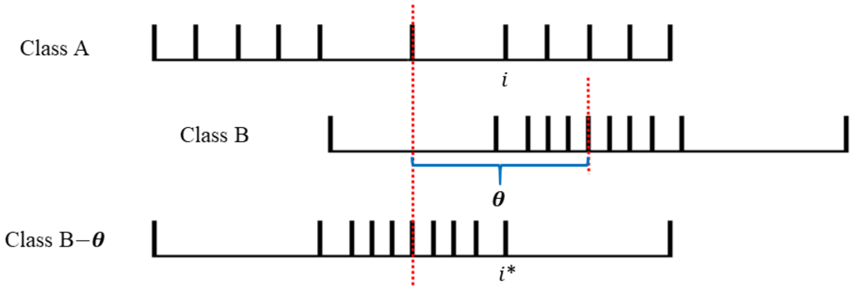
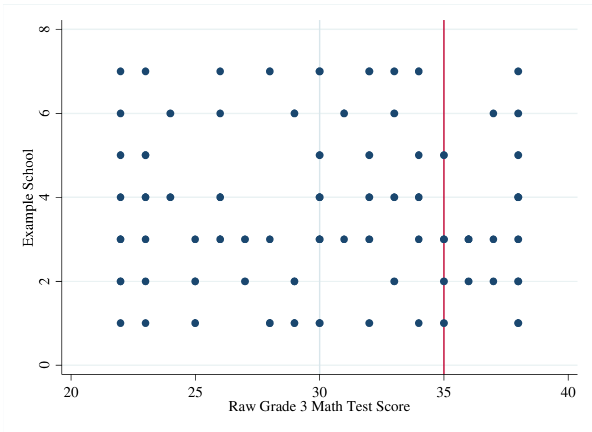
Figure 1: Test Score Distributions in Texan Elementary Schools

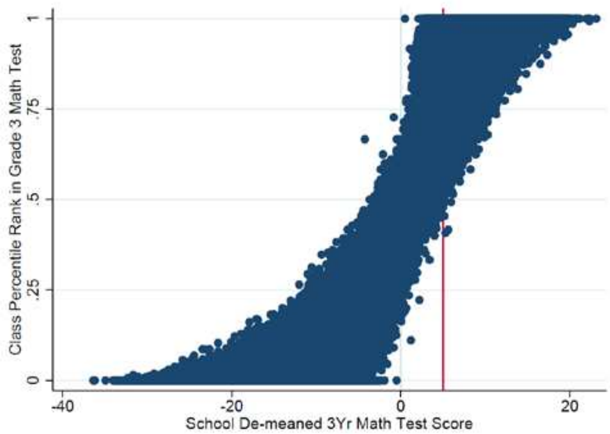
A: Illustrative Example



B: Test Scores distributions in schools with same mean, min and max

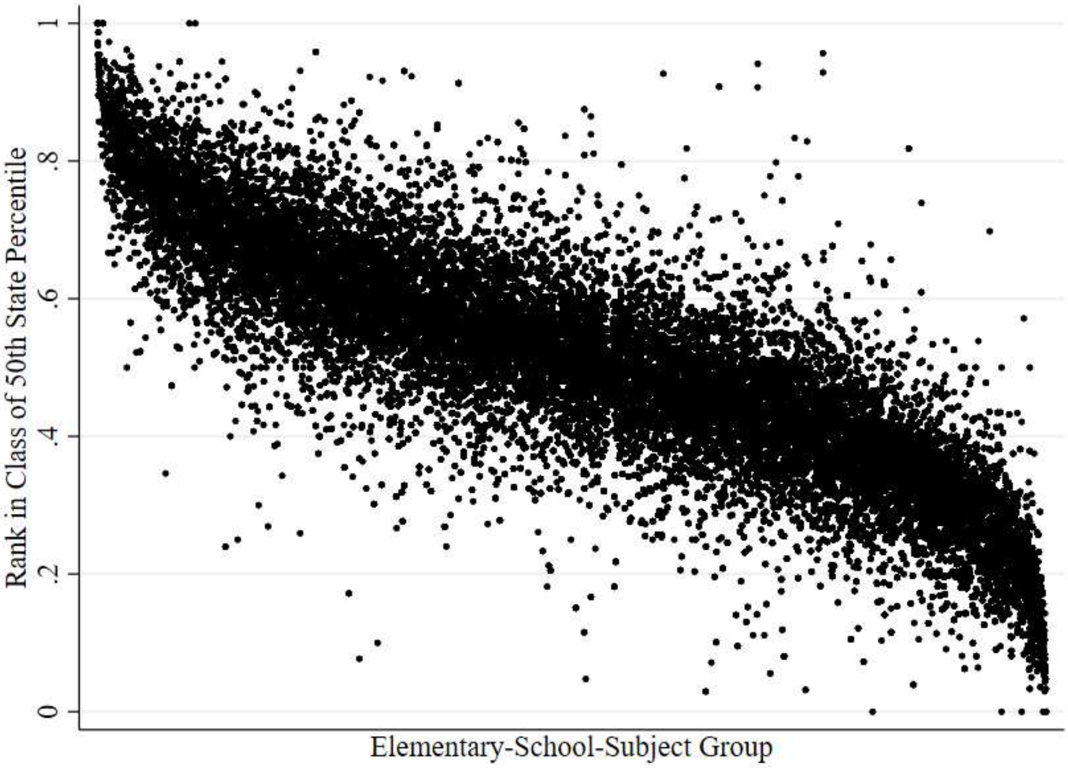


C: Variation in class rank conditional on test scores



Note: These figures are based on raw administrative data. This data has be perturbed in order to be FERPA compliant. This is showing the raw math scores in seven Texan elementary schools. Panel A is a illustrative example showing two classrooms with the same min, max, and mean scores where two students with the same achievement have different ranks. Panel B shows that such classrooms exist, presenting seven with the same mean, max, with students who have the same achievement having different rank . Panel C shows the different rank values these students with the same relative position within their classes have.

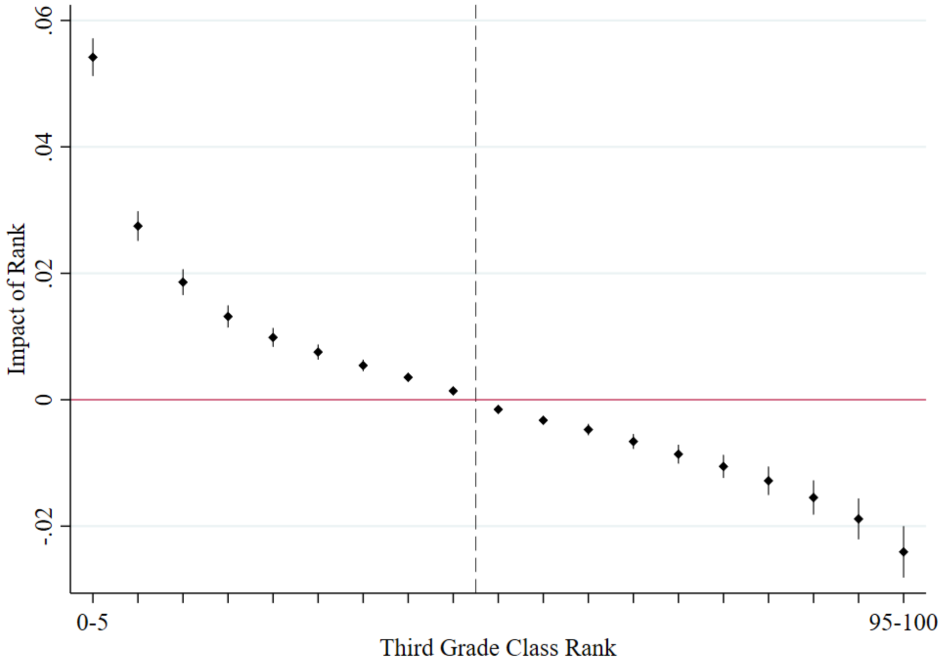
Figure 2: Common Support of Local Rank of Median Student Rank Within School-Subject Groups



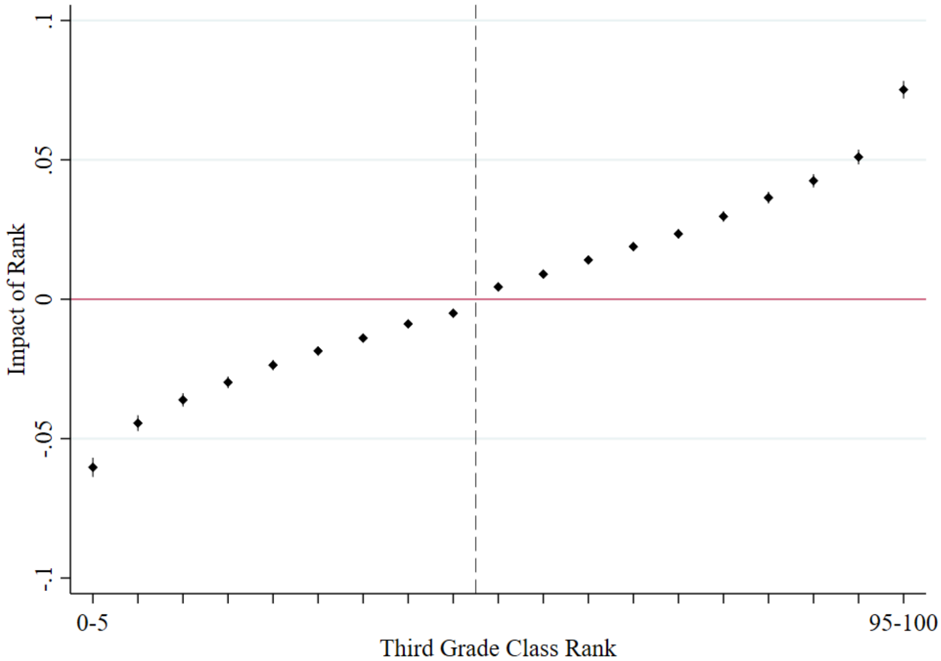
Note: This figure is based on raw administrative data. This data has be perturbated in order to be FERPA compliant. This figure plots the class rank of each student ranked at the 50th percentile in the state by school-subject groups over 13 cohorts. School-subject groups are sorted by mean rank of Y-axis over the 13 cohorts, such that school performance is increasing e.g. the first point is a student at the 50th percentile in the state, but in their school-subject group are ranked top. School-Subject Groups with no students at the 50th percentile are not plotted.

Figure 3 – Third Grade Rank on K-12 Outcomes, 1

A. Repeat Third Grade



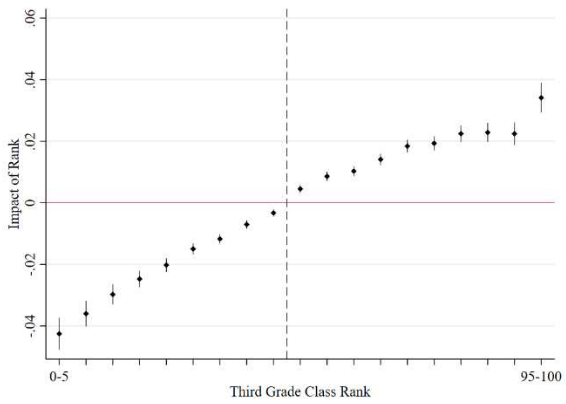
B. Eighth Grade Test Scores



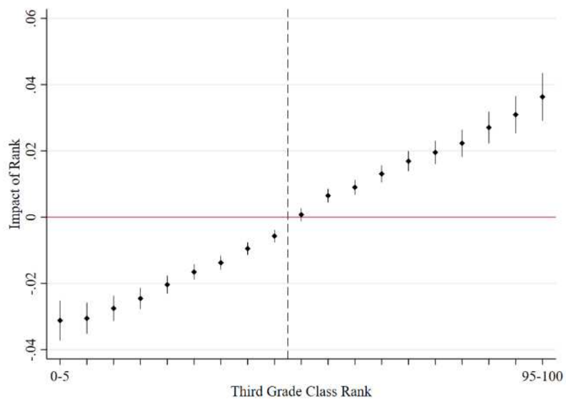
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. The mean retention rate is 1.6.

Figure 4 – Third Grade Rank on K-12 Outcomes, 2

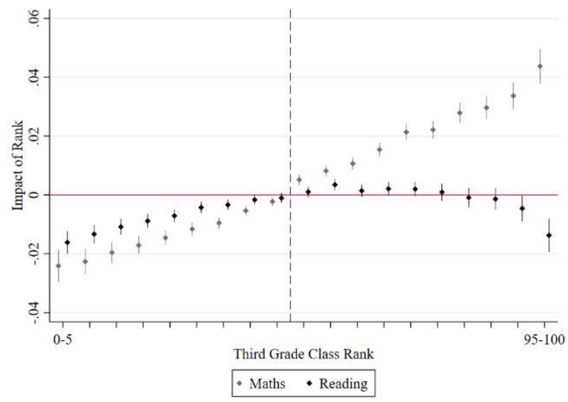
A. AP Calculus



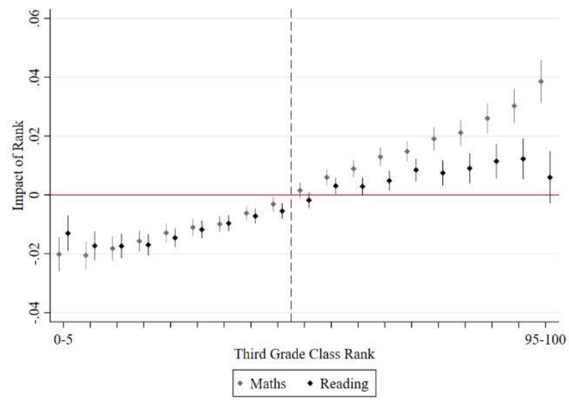
B. AP English



C. AP Calculus, subject-specific rank



D. AP English, subject-specific rank



Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. Panels C and D come from a specification which controls for achievement and rank in third grade in both subjects simultaneously.

Figure 5 – Third Grade Rank on High School Graduation

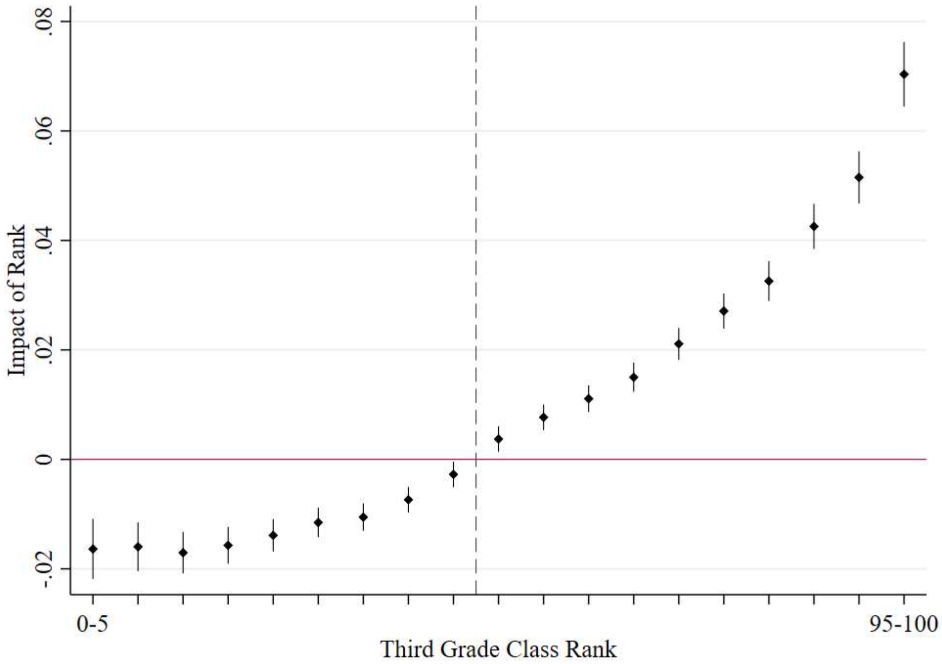
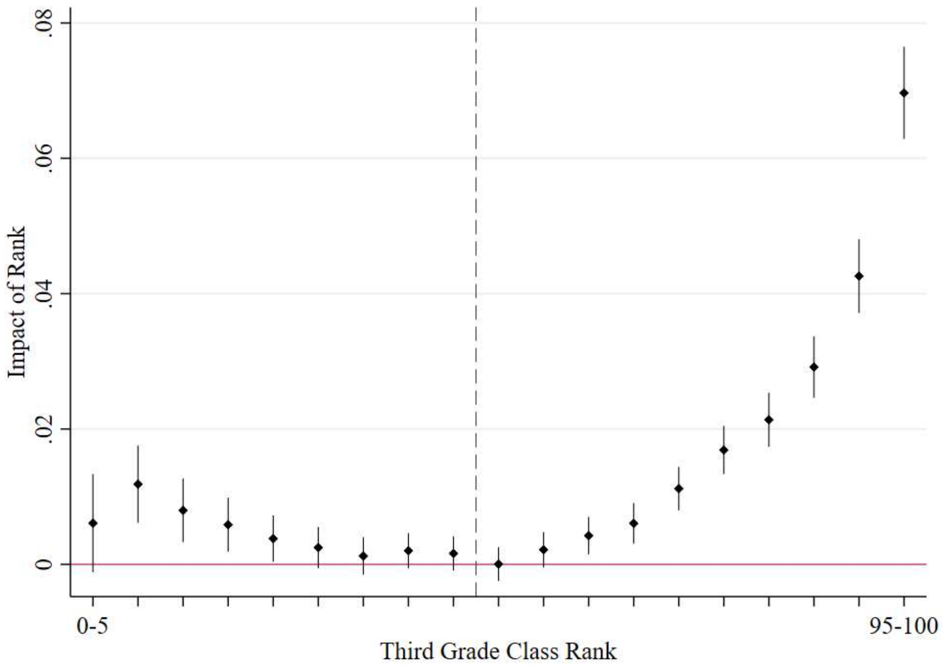


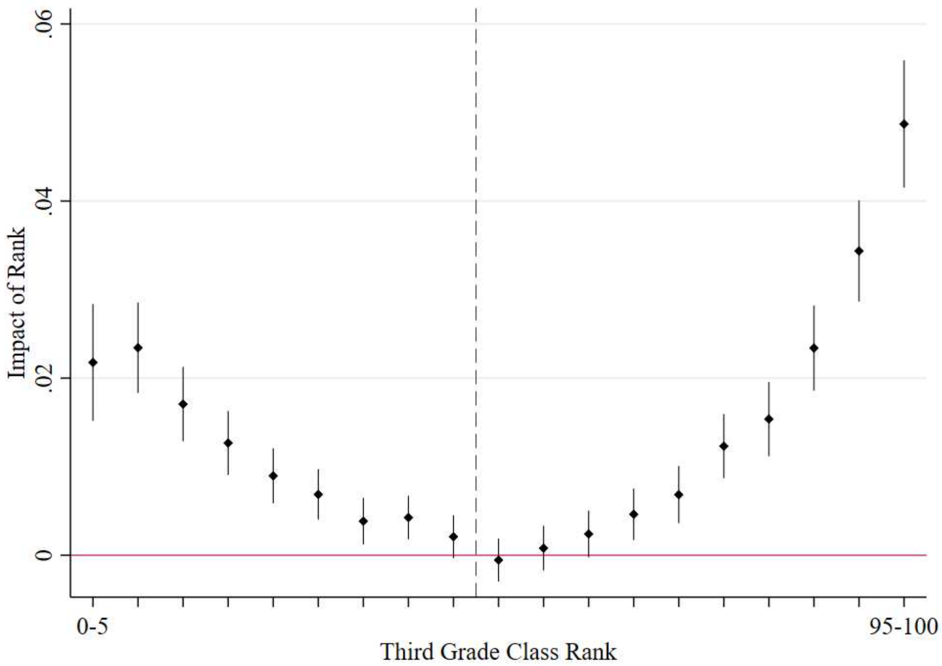
Figure 6 – Third Grade Rank on Enroll in College



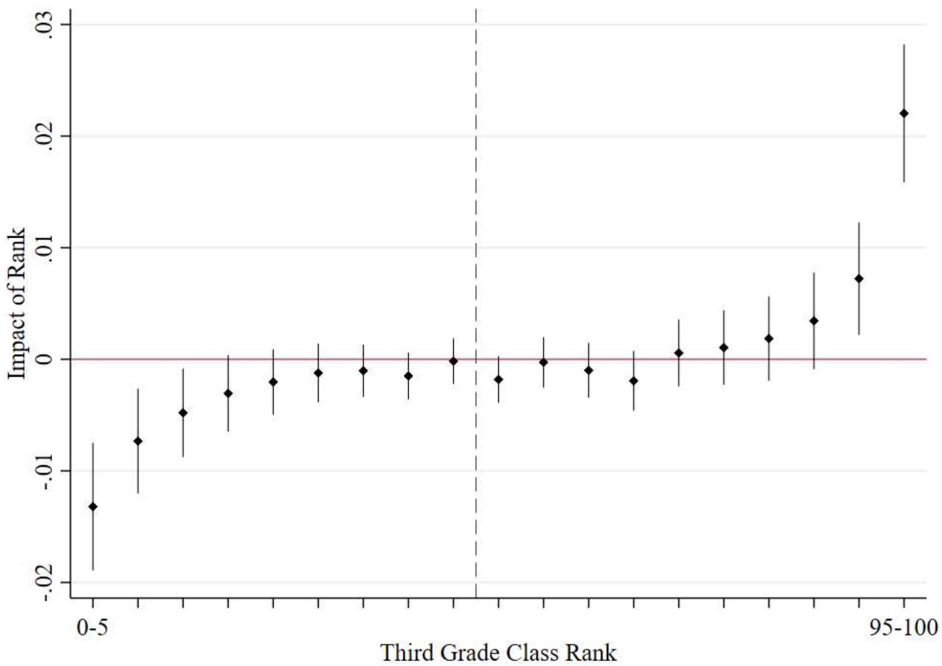
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. The mean high school graduation rate of 71 percent and a college enrollment rate of 47 percent.

Figure 7 Third Grade Rank on College Enrollment

A. Enroll in Community College within 3 years of "on-time" high school graduation



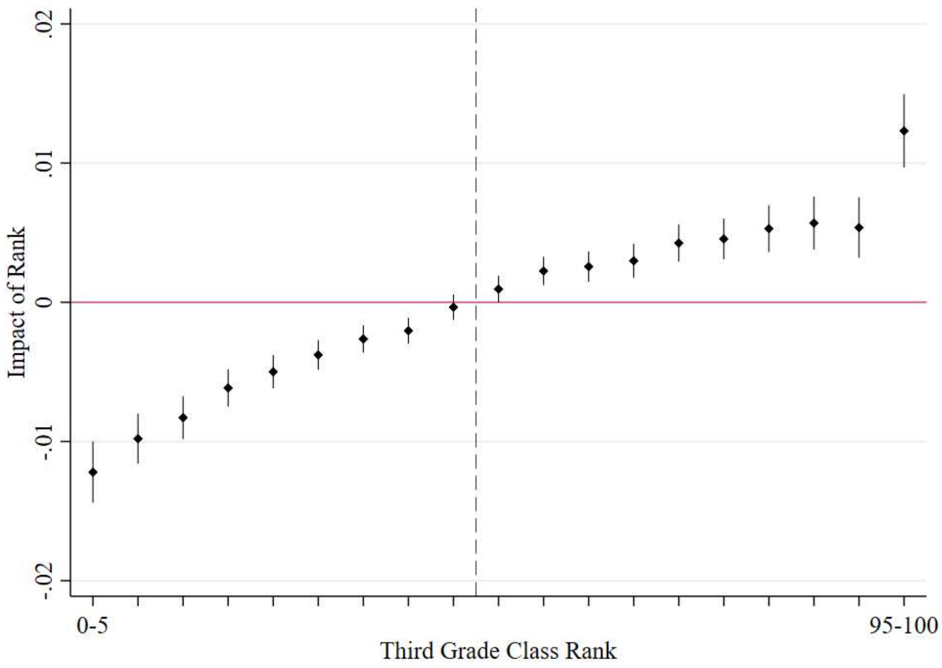
B. Enroll in 4 year within 3 years of "on-time" high school graduation



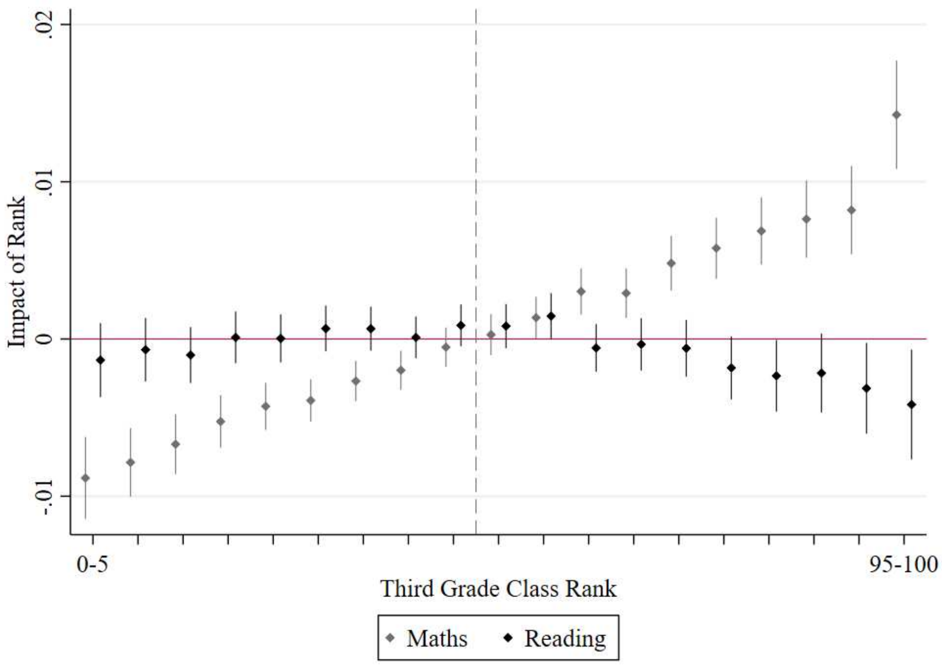
Note: These figures plot the coefficient for ventiles of class rank. The 45^{th}-50^{th} percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. The mean 2-year college enrollment rate is 31 percent and the mean 4-year college enrollment rate is 23 percent.

Figure 8 – Third Grade Rank on Major Choice

A. Declaring a STEM Major



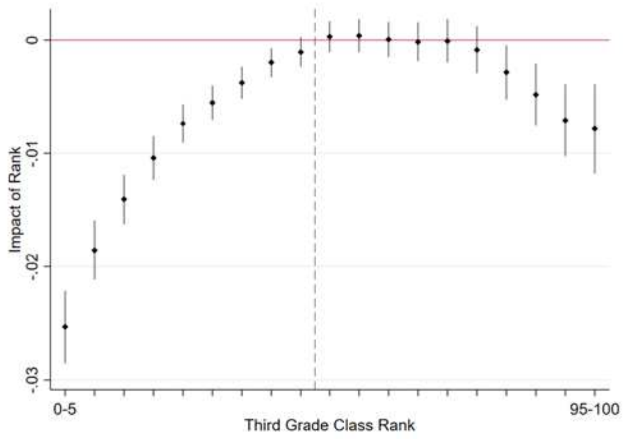
B. Declaring a STEM by subject specific rank



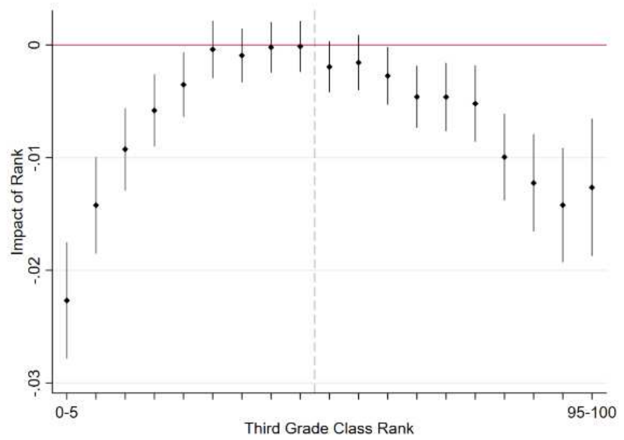
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. Panel B from a specification which controls for achievement and rank in third grade in both subjects simultaneously.

Figure 9 – Third Grade Rank on Bachelor's Degree Receipt

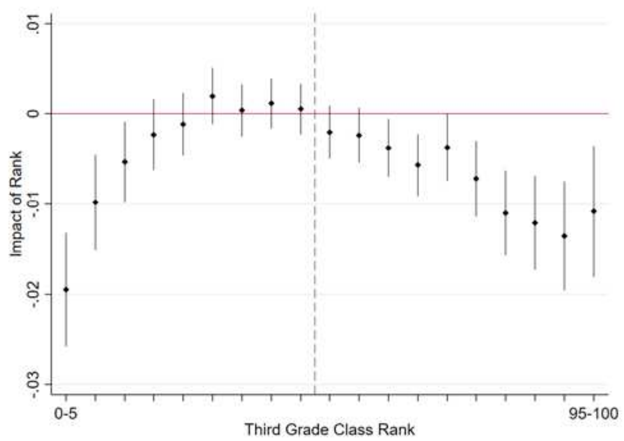
A. Graduate 4 Year College in 4 years



B. Grad 4 Year College in 6 years



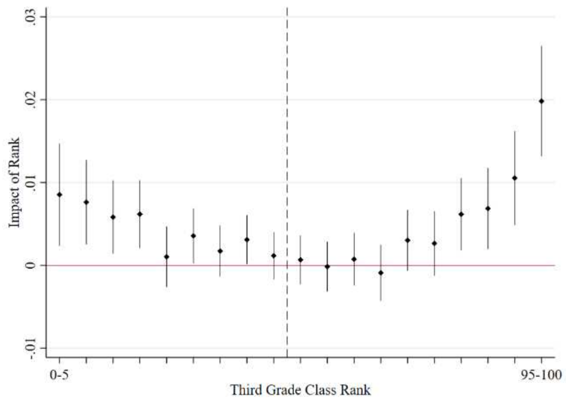
C. Grad 4 Year College in 8 years



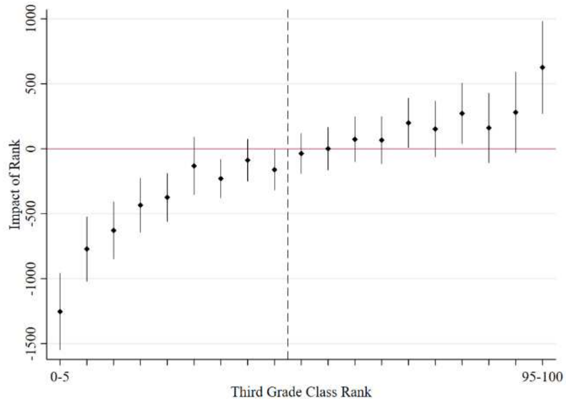
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Figure 10 – Third Grade Rank on Labor Market Outcomes (Age 23-27)

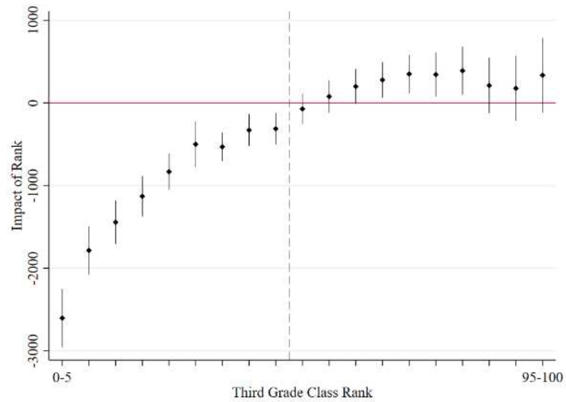
A. Positive Earnings



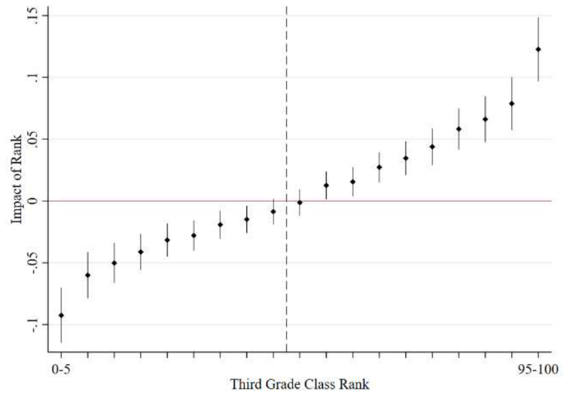
B. Average Earnings



C. Average Non-Zero Earnings



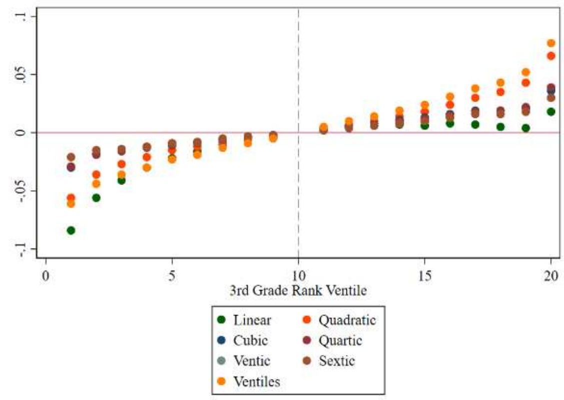
D. Log Average Earnings



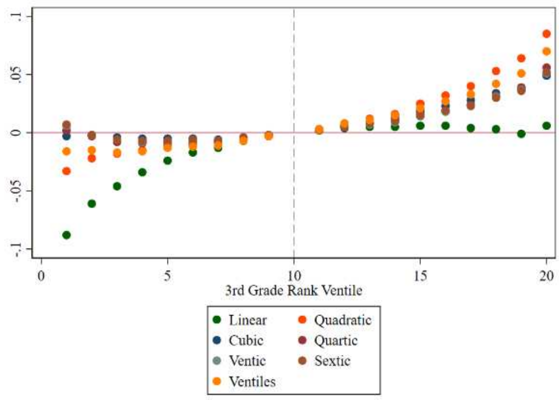
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. The mean positive earnings between 23-27 are \$24,912. Mean earnings are \$17,365.

Figure 11 – Flexible controls for Achievement

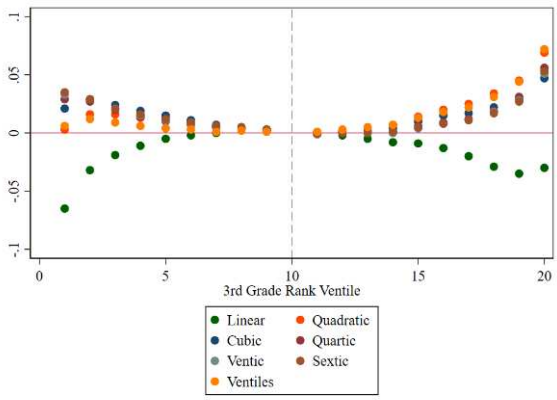
A. Eighth Grade Test



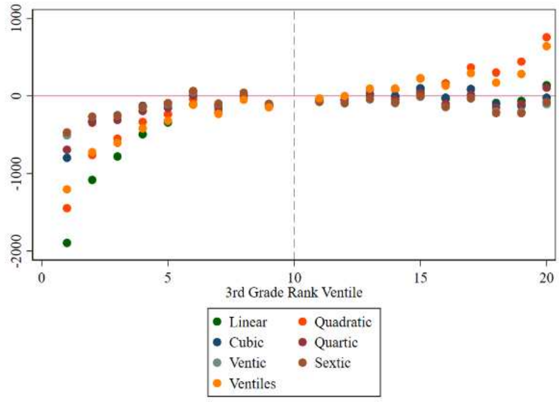
B. Ever Graduate HS



C. Any College

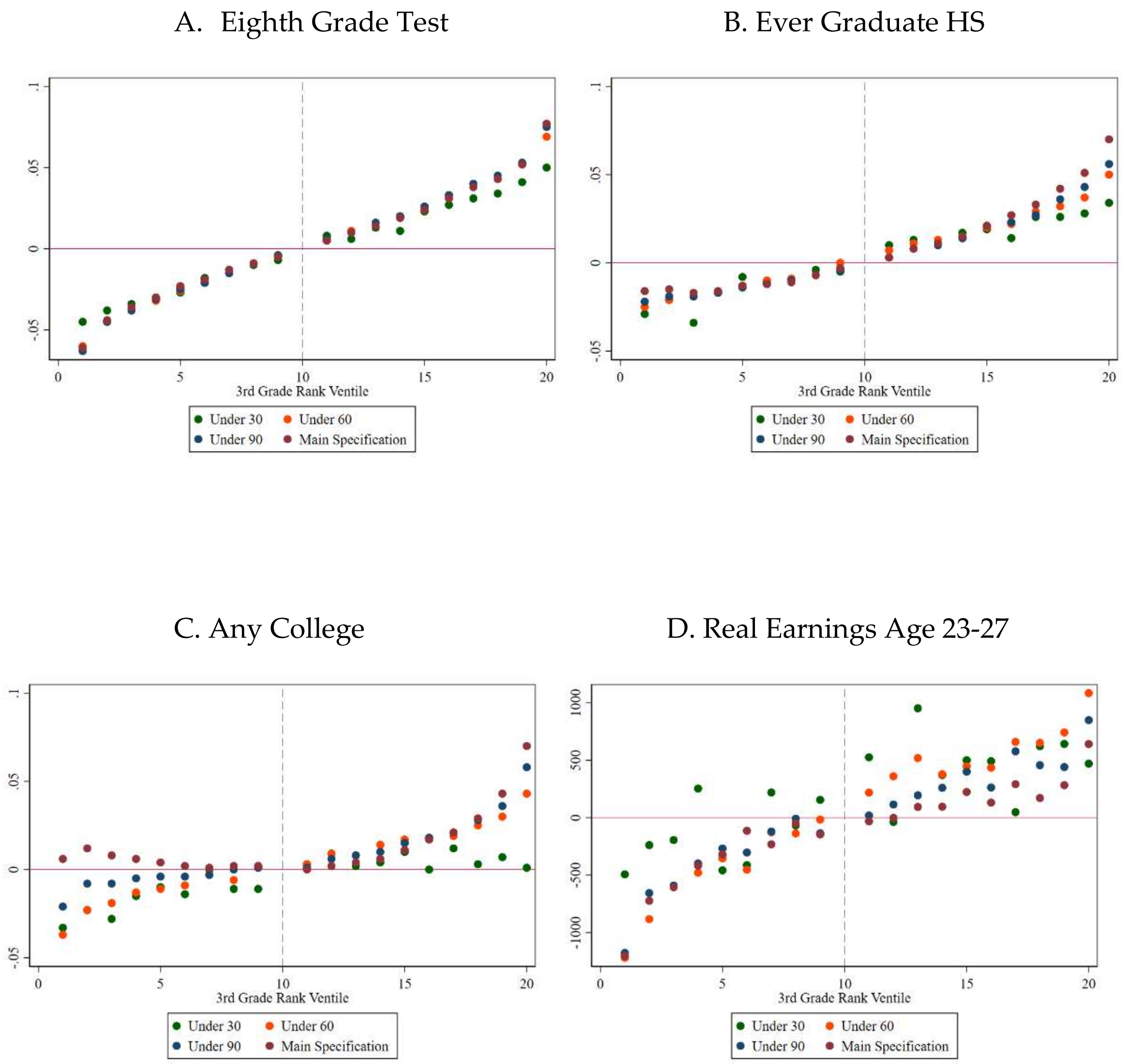


D. Real Earnings Age 23-27



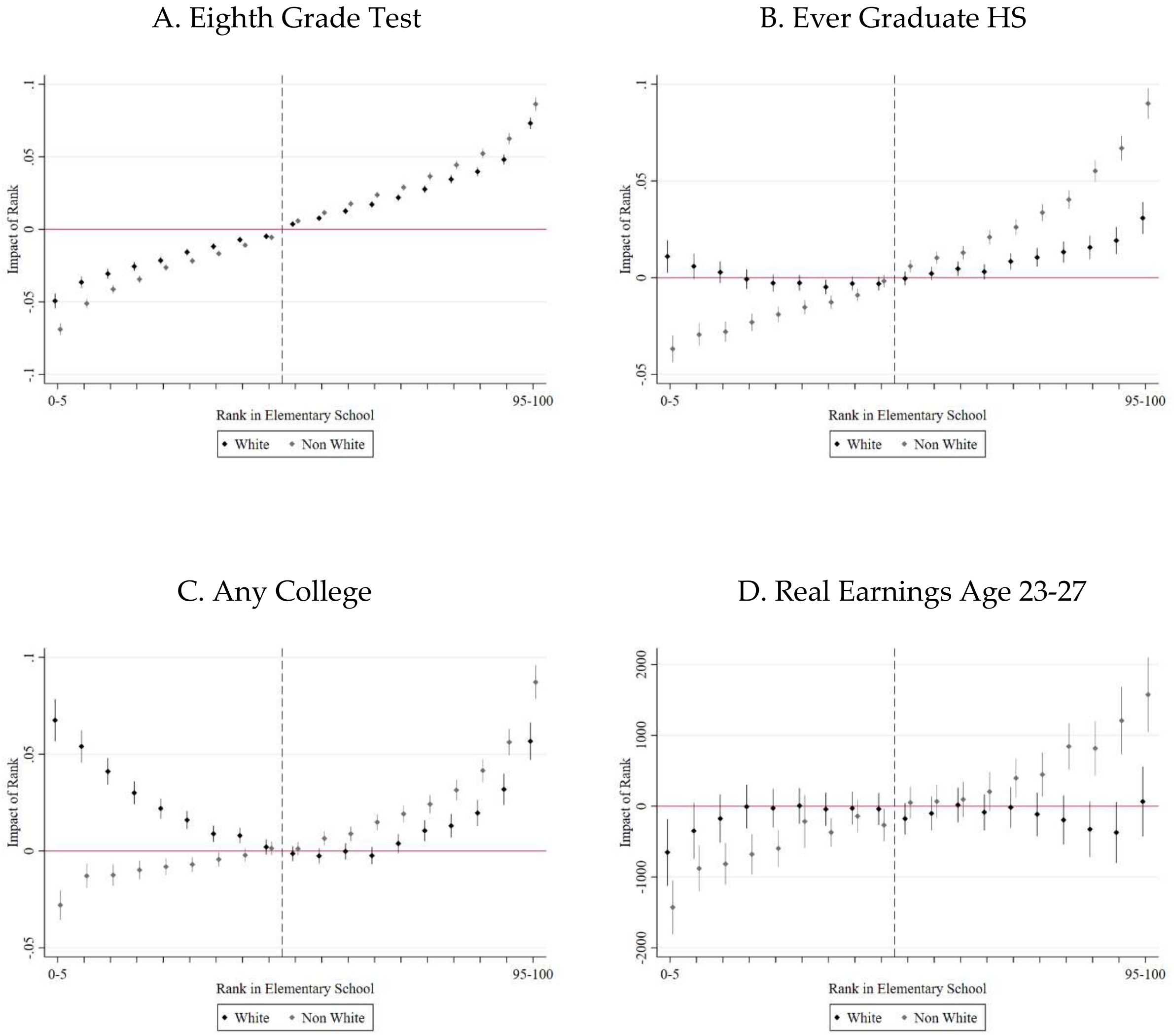
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Figure 12 – Results by Class Size



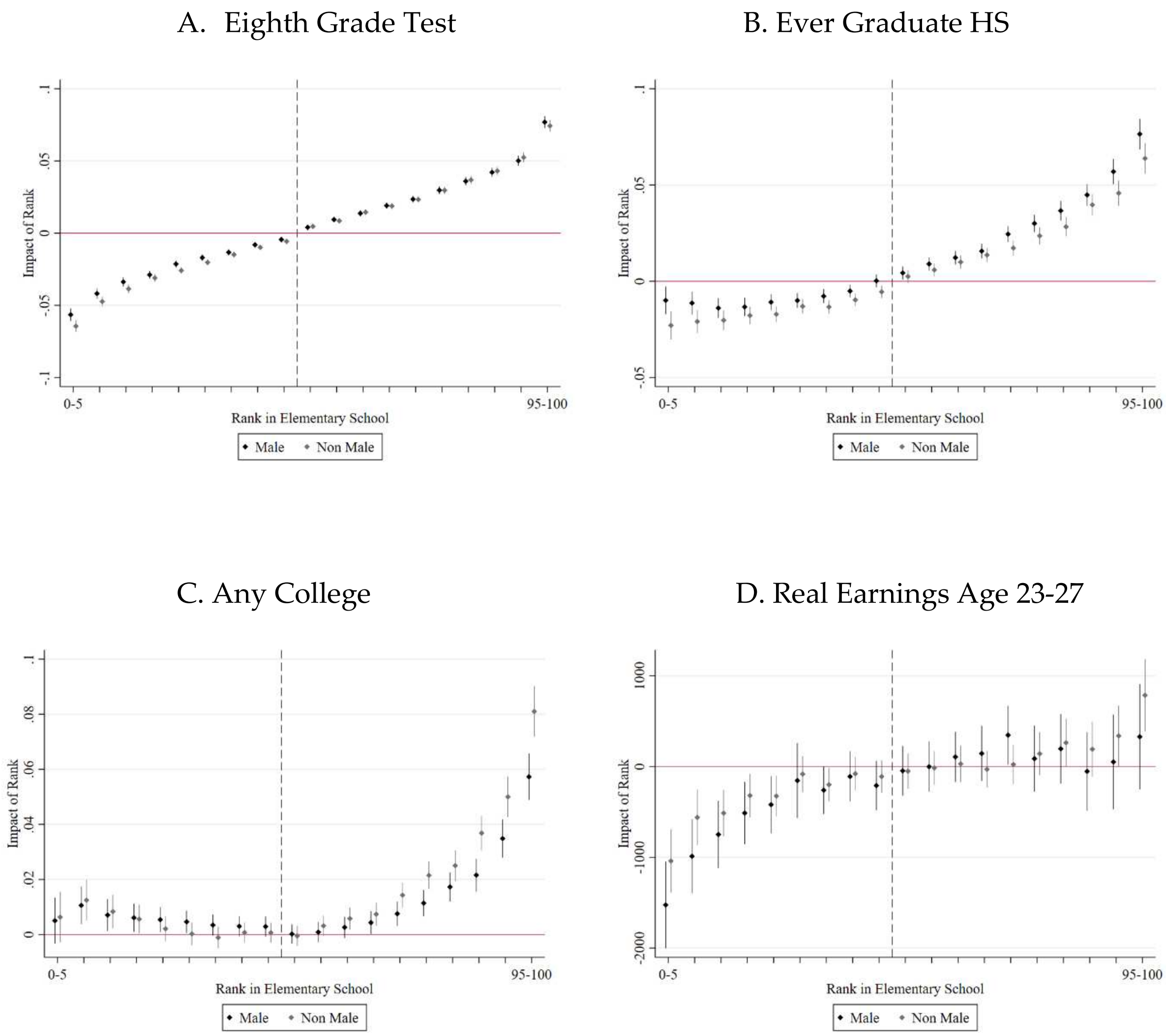
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Figure 13 – Heterogeneity by Race



Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

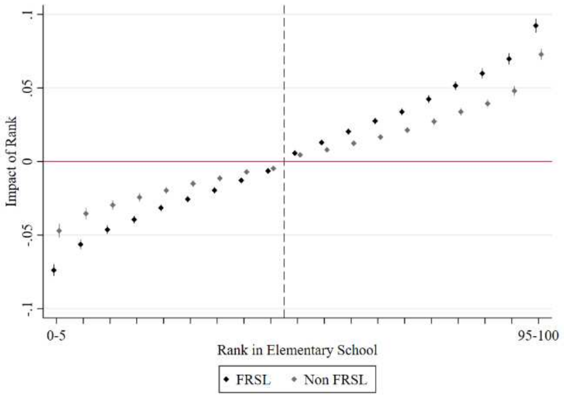
Figure 14 – Heterogeneity by Gender



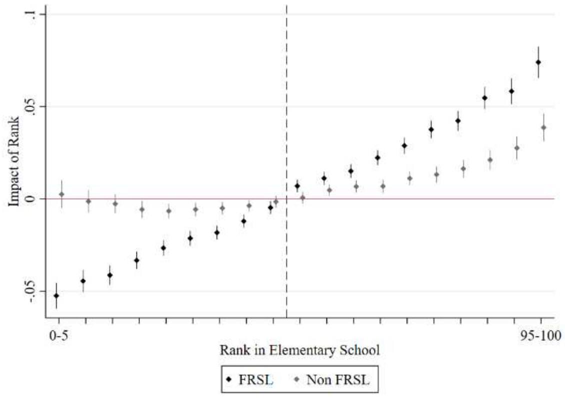
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Figure 15 – Heterogeneity by Free and Reduced-Price Lunch

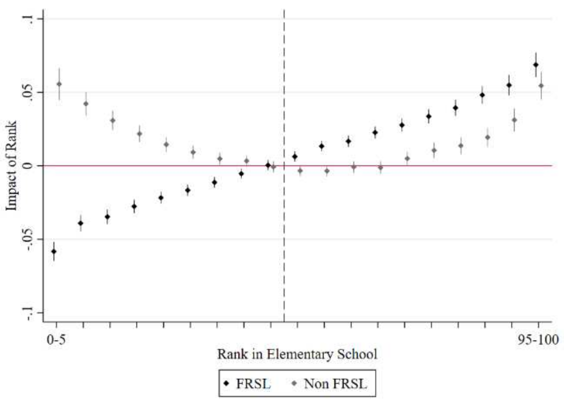
A. Eighth Grade Test



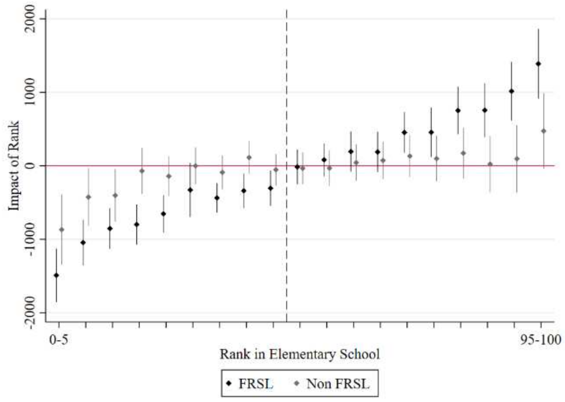
B. Ever Graduate HS



C. Any College



D. Real Earnings Age 23-27



Note: These figures plot the coefficient for ventiles of class rank. The 45^{th}-50^{th} percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Table 1: Summary Statistics

	Mean	Standard Deviation	Observations
Demographics – 13 Cohorts			
Male	0.50	0.50	6,117,690
Economic Disadvantage	0.49	0.50	6,117,690
English as a Second Language	0.12	0.32	6,117,690
White	0.47	0.50	6,117,690
Asian	0.03	0.17	6,117,690
Black	0.15	0.36	6,117,690
Hispanic	0.35	0.48	6,117,690
Size of Third Grade SSC	93.7	46.6	6,117,690
Repeat Third Grade	0.02	0.13	6,117,690
K-12 Outcomes – 13 Cohorts			
State Test Percentile, eighth grade	0.55	0.28	4,919,673
Ever Graduate High School	0.70	0.46	6,117,690
AP Calculus	0.08	0.28	6,117,690
AP English	0.18	0.39	6,117,690
Any College	0.46	0.50	6,117,690
Enroll, 4 yr college	0.23	0.42	6,117,690
Enroll, 2 year college	0.31	0.46	6,117,690
College Outcomes – 10/8/6 Cohorts			
Declare STEM Major	0.041	0.198	6,117,690
BA in 4 years	0.06	0.24	4,573,672
BA in 6 years	0.14	0.34	3,597,340
BA in 8 years	0.16	0.37	2,647,240
Age 23-27 Labor Outcomes – 8 Cohorts			
Non Zero Wages	0.65	0.43	3,597,340
Real Wages	17,300	24,093	3,597,340
Real Non-Zero Wages	24,818	25,372	2,652,284

Note: This table contains summary statistics for the main estimating sample of third graders from 1995-2008. Some outcomes are only available for early cohorts which generates the differences in sample size. Enroll, 4yr college means enrollment within 3 years of "on-time" high school graduation and is similarly defined for 2 year colleges.

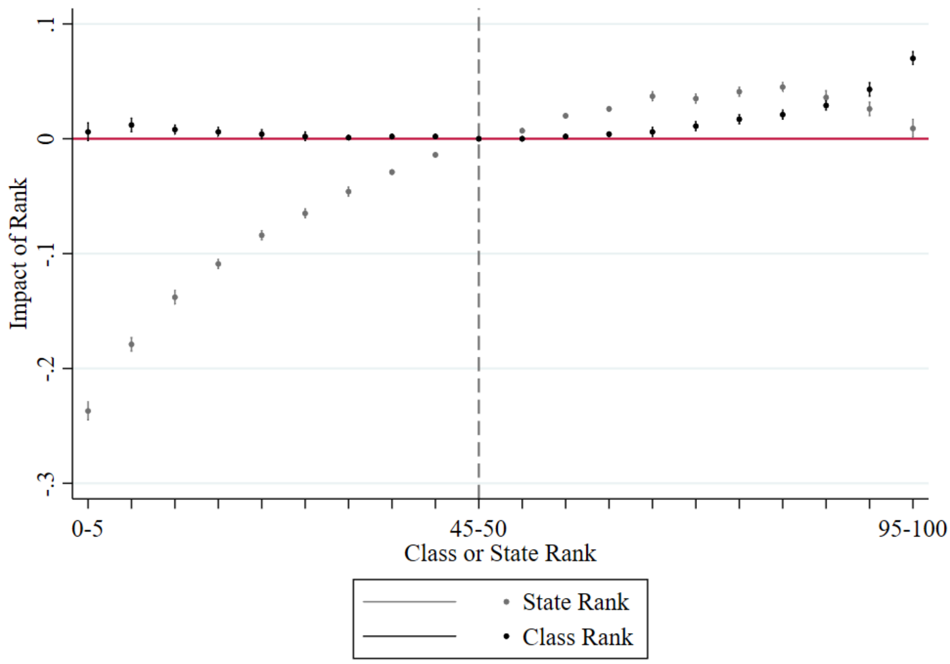
Table 2: The Distribution of Rank by Elementary School Effectiveness Measures

	Third Grade Attainment				Third to Eighth Grade Value Added			
State Ventile	Average School	Bad School	Good School	Rank Change	Average School	Bad School	Good School	Rank Change
1	0.028	0.054	0.014	*-0.040*	0.049	0.075	0.035	*-0.040*
2	0.074	0.137	0.039	*-0.098*	0.106	0.145	0.075	*-0.070*
3	0.123	0.211	0.068	*-0.143*	0.160	0.203	0.113	*-0.090*
4	0.172	0.279	0.100	*-0.179*	0.213	0.257	0.153	*-0.104*
5	0.222	0.341	0.133	*-0.208*	0.261	0.309	0.188	*-0.121*
6	0.272	0.401	0.170	*-0.231*	0.310	0.354	0.227	*-0.127*
7	0.323	0.458	0.210	*-0.248*	0.358	0.399	0.264	*-0.135*
8	0.374	0.509	0.250	*-0.259*	0.403	0.447	0.304	*-0.143*
9	0.426	0.565	0.294	*-0.271*	0.449	0.493	0.349	*-0.144*
10	0.479	0.613	0.346	*-0.267*	0.497	0.542	0.387	*-0.155*
11	0.527	0.650	0.390	*-0.260*	0.541	0.581	0.435	*-0.146*
12	0.577	0.703	0.452	*-0.251*	0.590	0.624	0.485	*-0.139*
13	0.627	0.737	0.495	*-0.242*	0.630	0.667	0.527	*-0.14*
14	0.685	0.787	0.561	*-0.226*	0.684	0.718	0.587	*-0.131*
15	0.726	0.810	0.613	*-0.197*	0.724	0.749	0.634	*-0.115*
16	0.784	0.858	0.685	*-0.173*	0.778	0.799	0.693	*-0.106*
17	0.828	0.889	0.745	*-0.144*	0.821	0.841	0.752	*-0.089*
18	0.876	0.916	0.808	*-0.108*	0.870	0.881	0.819	*-0.062*
19	0.930	0.995	0.889	*-0.106*	0.924	0.930	0.890	*-0.040*
20	0.962	0.973	0.934	*-0.039*	0.959	0.961	0.940	*-0.021*
Value Added	-0.001	-0.035	0.026	*0.061*	0.000	-0.055	0.055	*0.11*

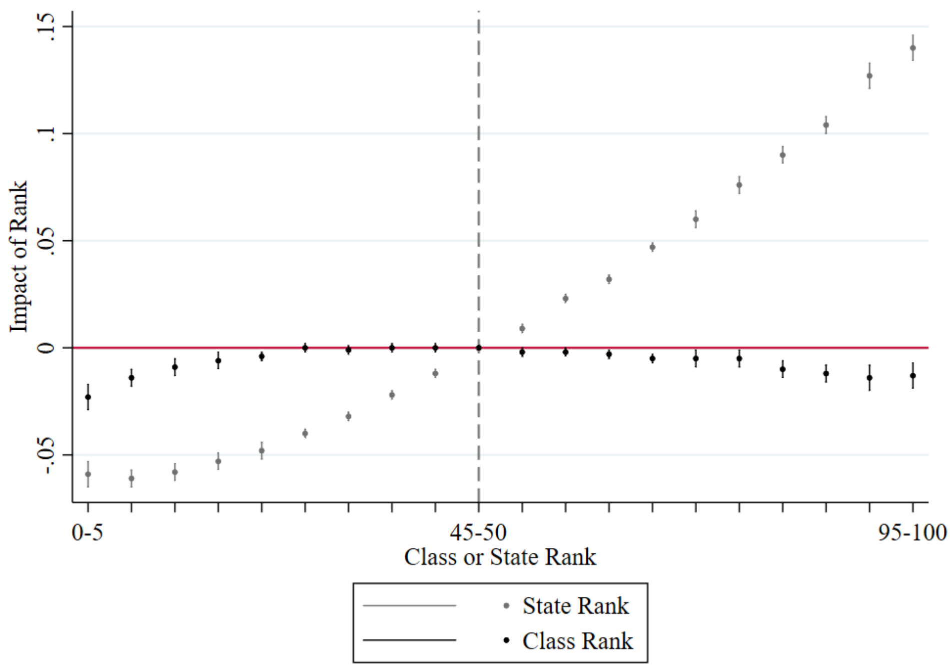
Note: This table categorizes elementary schools into good, bad and average in terms of average third grade attainment, and third to eighth grade value added. Good/bad are defined as being one standard deviation above/below the average (with tolerance of 0.0045). Each row the average class rank of students in this type of school for that ventile. The Rank Change column is the average rank change of students in that ventile from moving from a bad to a good school. The final row presents the third to eighth grade school level value added for this type of school. Value added calculated conditional on a cubic of third grade percentile.

Appendix Figure 1 - Relative Impact of State and Class Rank on

A. Attend Any College

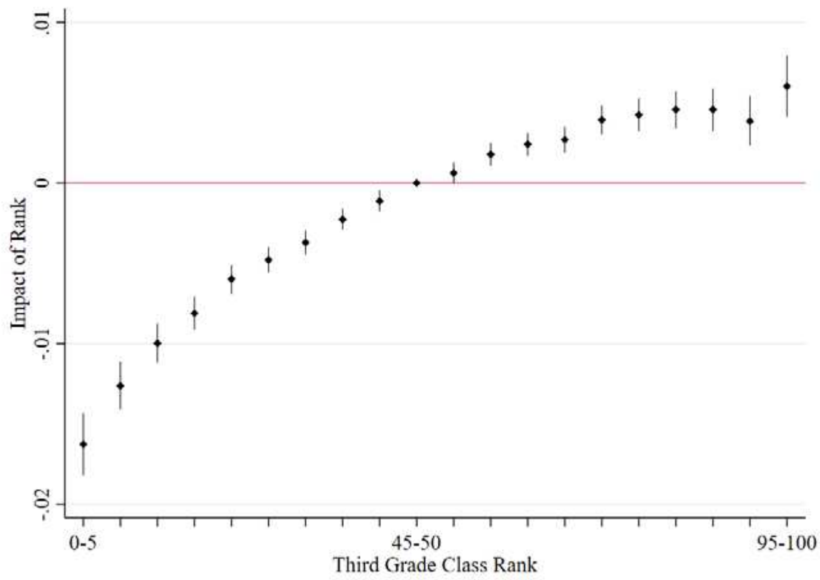


B. Obtain a Bachelors within 6 Years



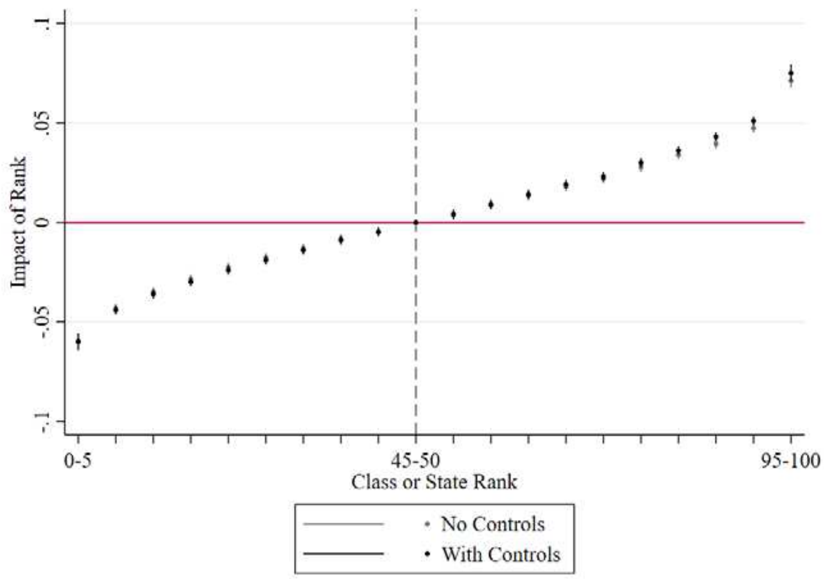
Note: These figures plots the coefficients for ventiles of class rank and the ventiles of achievement in the state rank distribution. Both sets of estimates are calculated simultaneously. The Class Rank estimates are the same as those presented in Figure 5.B and 7.C for Panels A & B respectively.

Appendix Figure 2 – Third Grade Rank on Four Year College Quality



Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. We define four-year college quality as the mean third grade state percentile of all students who have attended that college.

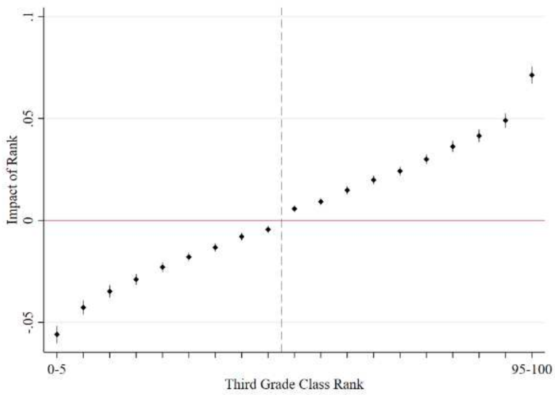
Appendix Figure 3 – Third Grade Rank on Eighth Grade Test Scores Conditional and Unconditional on Demographics



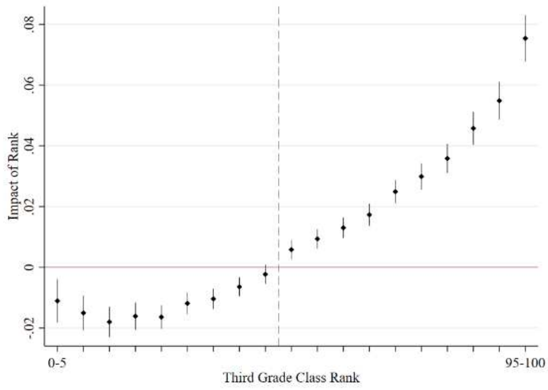
Note: These figures plot the coefficient for ventiles of class rank. The 45th-50th percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement. We define four-year college quality as the mean third grade state percentile of all students who have attended that college.

Appendix Figure 4 - Balanced Sample for Full Set of Outcomes

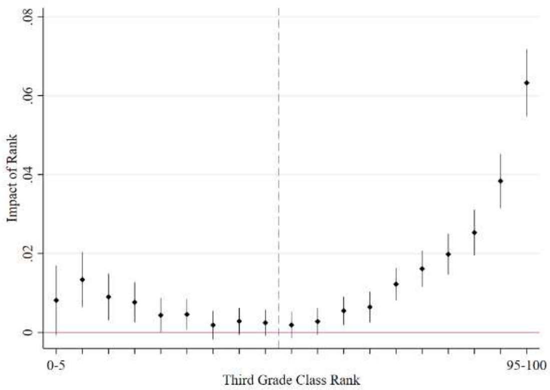
A. Eighth Grade Test



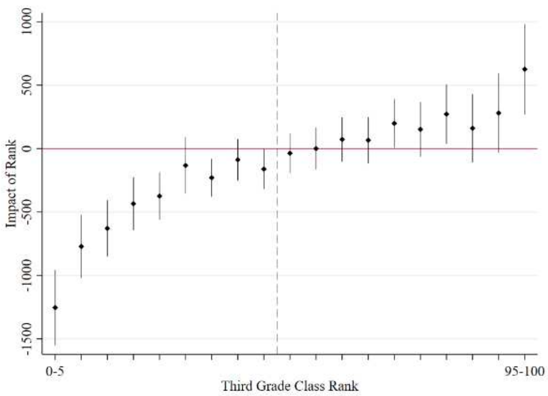
B. Ever Graduate HS



C. Any College



D. Real Earnings Age 23-27



Note: These figures are on the reduced balanced Sample for Full Set of Outcomes 8 cohorts Third Grade 1994-2001. These figures plot the coefficient for ventiles of class rank. The 45^{th}-50^{th} percentile is the omitted category. Estimates come from equation 2, which includes controls for race, gender, ESL status, and indicators for ventiles of student achievement.

Appendix Table 1 – Alternate Measures of Rank on Main Outcomes

	Method for Calculating Rank			
	Mean Rank	Bottom Rank	Random Rank	Ontime Students
	(1)	(2)	(3)	(4)
Outcome				
Test	0.039	0.057	0.029	0.040
Scores	*0.003*	*0.003*	*0.002*	*0.003*
Grad	0.040	0.060	0.030	0.040
HighSch	*0.004*	*0.004*	*0.003*	*0.004*
Any	-0.035	-0.007	-0.028	-0.034
College	*0.005*	*0.005*	*0.004*	*0.005*
Real	167.744	262.609	283.362	139.418
Wages	*247.347*	*242.651*	*209.923*	*244.035*

Notes: This table presents the rank estimates from 16 different regressions, using four different rank measures on four outcomes. Mean Rank – assigns the average rank to all students tied with the same score (this is the measure we use in the paper). Bottom Rank – assigns the bottom rank to all students tied with the same score. Random Rank – assigns a random rank to all students with tied with the same score. Ontime students – Assigns rank on a mean rank basis only among students who took their third grade exam on time, rather than all students in their class who took the exam.