

Weinhardt, Felix; Murphy, Richard

Conference Paper

Top of the Class: The Importance of Ordinal Rank

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel -
Session: Investments into Human Capital: Governments, Parents and Students, No. D14-V3

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Weinhardt, Felix; Murphy, Richard (2016) : Top of the Class: The Importance of Ordinal Rank, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2016: Demographischer Wandel - Session: Investments into Human Capital: Governments, Parents and Students, No. D14-V3, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel und Hamburg

This Version is available at:

<https://hdl.handle.net/10419/145626>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Top of the Class: The Importance of Ordinal Rank *

This version: February 2016

Abstract

This paper establishes a new fact about educational production: ordinal academic rank during primary school has long-run impacts on later achievement that are independent from underlying ability. Using data on the universe of English school students, we examine a setting in which the same baseline score on a national standardized test can correspond to different ranks among students situated in different primary school classes, where we calculate ranks using this baseline score. Institutional factors cause students to be re-assigned to a new set of secondary school peers and teachers that are unaware of the student's prior ranking. We find large and significant positive effects on test scores and subject choices during secondary school from experiencing a high primary school rank, conditional on the underlying primary baseline score. The effects are especially large for boys, contributing to an observed gender gap in end-of-high school STEM subject choices. Merged survey data suggest that the development of confidence is a likely mechanism.

Keywords: rank, non-cognitive skills, peer effects, productivity

JEL Classification: I21, J24

*We thank Esteban Aucejo, Thomas Breda, David Card, Andrew Clark, Susan Dynarski, Ben Faber, Mike Geruso, Eric Hanushek, Brian Jacob, Pat Kline, Steve Machin, Magne Mogstad, Imran Rasul, Jesse Rothstein, Olmo Silva, Kenneth Wolpin, Gill Wyness, and participants of the CEP Labour Market Workshop, UC Berkeley Labour Seminar, the Sussex University, Queen Mary University and Royal Holloway-University departmental seminars, the CMPO seminar group, the RES Annual Conference panel, IWAAE, the Trondheim Educational Governance Conference, the SOLE conference, CEP Annual Conference, the UCL PhD Seminar, the BeNA Berlin Seminar, IFS seminar and the CEE Education Group for valuable feedback and comments. All remaining errors are our own.

1 Introduction

Educational achievement is among the most important determinants of welfare, both individually and nationally, and as a result there exists a vast literature examining educational choices and production. Yet to date, lasting effects of ordinal academic ranks (conditional on ability) have not been considered. Why might rank matter? It is human nature to make social comparisons in terms of characteristics, traits and abilities (Festinger, 1954). When doing so, individuals often use cognitive shortcuts (Tversky and Kahneman, 1974). One such shortcut would be to use simple ordinal rank information instead of more detailed cardinal information. Indeed, it has recently been shown that ordinal rank, in addition to relative position, is used when individuals make comparisons with others and so affects happiness and job satisfaction (Brown et al., 2008; Card et al., 2012). Extending this concept to a school setting may result in educational investment decisions being made according to class rank rather than to students' absolute or relative abilities. The goal of this paper is to establish if –and why– a student's academic ranking in a class has long running implications on their future academic performance.

This paper proposes a novel approach for estimating rank effects conditional on ability that relies on naturally occurring distributional differences in peer ability across classes. Applying this approach to census data from England, we find that students that are highly ranked in a subject during primary school go on to have higher test scores in that subject throughout the rest of compulsory education, and that boys are reacting more strongly. Moreover, rank during primary school is predictive of subject specialization during secondary education. From here, we explore a range of potential mechanisms for these effects and through combining administrative test score data with student surveys, we provide evidence that class rank leads to confidence. Using a basic model we derive and implement a test to determine if this relationship is reflective of students learning about their own abilities, or impacting on non-cognitive skills and learning costs directly. The finding that 'misplaced' confidence does not lead to a reduction in test scores provides support for the latter.

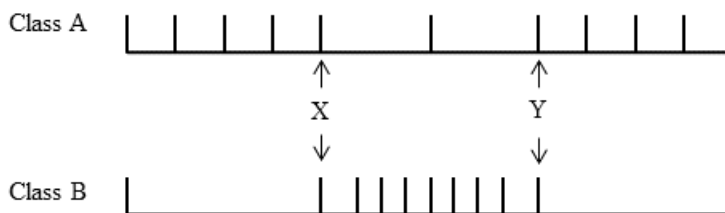
Besides improving our general understanding of human capital formation through documenting a new factor, which, to a greater or lesser extent, is likely to exist in all education settings, we go on to show that rank affects the gender gap in Science Technology Engineering and Mathematics (STEM) specializations. This is because girls have higher ranks (and ability) in languages during primary school. The resulting rank differences in primary education partly explain the gender gap in the take up of STEM qualifications towards the end of compulsory education, which are prerequisites to STEM majors. Finally, we propose that our findings provide potential explanations to reconcile some of the results in the selective schools, affirmative action, and neighborhood effects literatures and have important policy implications regarding student confidence and school choice.

Several challenges combine to make the causal effects of rank on an individual's later outcome difficult to identify. First, a naive regression of future outcomes on prior rank will be confounded as an individual's rank in a task will necessarily be correlated with their ability and hence with later outcomes. Controlling for ability, while still having variation in rank, is therefore key. Sec-

ond, there are the standard problems of sorting, reflection and unobserved common shocks. Our strategy and setting allows us to address all of these issues.

To address the identification issue of class rank being highly correlated with ability, this paper presents a novel empirical strategy that exploits variation in rank for a given ability. Our estimation of the rank effects relies on idiosyncratic variation in the distribution of primary school peer quality, which arises naturally because primary school classes are small and students vary in ability. Figure 1 provides a stylized illustration of this. The figure shows two classes of eleven students, with each mark representing a student’s test score, which are increasing from left to right and used to measure ability. The classes are very similar having the same mean, minimum, and maximum student test scores. However, two students with the same absolute and relative-to-the-mean test score, can still have different ranks, which can be calculated directly from the test scores. For example, a student with a test score of Y in Class A would have a lower rank ($R=5$) than the same test score in Class B ($R=2$). Similarly, a test score of X would be ranked differently in Classes A and B.

Figure 1: Rank Dependent on Distribution of Test Scores



Notably, this variation will occur across any classrooms, both within and across cohorts, schools and subjects. For example, in one cohort a student with a test score of 80 would be the second of their class, while in the next cohort the same score would place them fifth. Similarly, Class A and Class B of Figure 1 could represent the same set of students in two different subjects, or two classes in the same cohort and subject but in different schools. Critically, this variation allows us to condition on very flexible measures of student achievement when estimating the rank effects.

In practice, rank in primary school and baseline achievement will be measured using an external test taken at the end of primary school. One concern with measuring rank based on achievement scores is that a student who attended a good primary school and scored 80 may have lower ability than a student who attended a bad primary school and scored 80. The concern is that the student who scored 80 at the good primary school will have a lower rank and be of lower unobserved ability, thereby driving a correlation between rank and unobserved ability even conditional on test scores. To account for any mean differences in ability across these levels, we always include as controls fixed effects at the school-subject-cohort (SSC) level.¹ To re-emphasize, we are using the across class variation in test score distributions to separate rank and relative achievement effects, while including class fixed effects. These fixed effects only remove the between class differences in long run attainment due to any class level factor that enters additively and affects all

¹ In our data, the median primary school has only 27 students per cohort, so SSC fixed effects are best thought of as class fixed effects.

students similarly. The identification of rank comes from the remaining variation in the test score distributions.

To address the latter identification challenges of sorting and reflection and unobserved common shocks, we exploit unique features of the English educational system. In England, unlike the US for example, there is a large re-mixing of students between the primary and secondary phase of education, with the average primary school cohort of 27 students sending them to six different secondary schools, and secondary schools receiving students from 16 different primaries. The implication is that while primary school peers determine an individual's class rank during primary school, students have on average 87 percent new peers in secondary school. This re-mixing into secondary schools allows us to overcome major concerns about reflection and common shocks. This is because secondary peers who went to different primary schools were subject to different unobserved primary school shocks. In addition, two students with identical test scores, but who attended different primary schools to attend the same secondary school, may have different primary school ranks, but would have the same rank in secondary school. Therefore, conditioning on prior test score will account for secondary school rank based inputs, such as tracking.

In our main analysis, we use administrative data covering over 2.25 million students across five cohorts from the end of primary school to the end of secondary school. In England, all students are tested in English, Math, and Science at the end of primary school (age 11), and twice during secondary school (ages 14 and 16). These national assessments are marked externally from the school and are intended to be absolute measures of individual achievement; hence, scores are not set to a curve at class, school, district, or country level. We use the age-11 baseline score as external measure of achievement that does not directly rely on rank and therefore use it to rank every student among their primary peers in these subjects. Neither the students nor teachers are informed of this ranking based on these age 11 test scores. Rather, we take this rank measure as a proxy for perceived ranking due to interactions with peers over the previous six years of primary school, as we expect test performance to be highly correlated with everyday classroom performance.² Our outcome variables are national assessments in secondary school at age-14, age-16 and final year subject choices at age 18. We regress these on primary rank controlling for the age-11 individual baseline score. Additional parameters include a set of student observable characteristics: gender, Free School Meals Eligibility (FSME), Special Educational Needs (SEN), student ethnicity, and the SSC fixed effects. In our most demanding specifications we additionally include student fixed effects to account for any unobserved individual level characteristics or shocks, by utilizing the test scores and ranks of each student across three subjects.³

Our main finding is that conditional on flexible externally marked baseline score and SSC ef-

² In English Primary schools it is common for students to be seated at tables of four and for tables to be set by pupil ability. Students can be sat at the 'top table' or the 'bottom/naughty table'. This would make class ranking more salient and could assist students in establishing where they rank amongst all class members through a form of batch algorithm, e.g. 'I'm on top table, but I'm the worst, therefore I'm fourth best.' We discuss in Section 3.3 how our estimates would be affected if students wrongly perceived their real rank and provide empirical evidence in Section 6 that students have awareness of their rank.

³ This assumes any impacts are equal across subjects. Testing this, we find that the effect of rank is indeed qualitatively very similar in different subjects. As we also discuss in Section 4.1, the inclusion of the student effects changes in interpretation of the rank parameter, as any potential spillovers from being ranked highly in a different subject have been removed.

fects, students with a higher academic rank in a subject during primary school achieve higher test scores in that subject during secondary school. The effects of rank that we present are sizable in the context of the education literature, with a one standard deviation increase in rank improving age 14 and age 16 test scores by about 0.08 standard deviations. Estimates that account for individual unobservables, including average ability and rank spillovers, are smaller. Here, a one standard deviation increase in rank improves subsequent test scores in that subject by 0.055 within student standard deviations. These effects vary by pupil characteristics, with boys being more affected by their rank than girls throughout the rank distribution, and with students who are FSME not being negatively impacted by being below the median, but gaining relatively more from being ranked highly.

To gauge the long-run implications of a student's rank in primary school, we estimate the impact on the choice of subjects taken at the end of secondary school. England has an unconventional system where students typically choose to study only three subjects in the last two years of secondary school. These subject choices have long lasting repercussions, as not choosing any STEM subjects at this point removes the option of studying them at university. Here, we find that conditional on achievement, being at the top of the class in a subject during primary school rather than at the median increases the probability of an individual choosing that subject by almost 20 percent. We will argue that this highlights an undiscovered channel that contributes to the well-documented gender gaps in the STEM subjects and the consequential labor market outcomes (Guiso et al., 2008; Joensen and Nielsen, 2009; Bertrand et al., 2010).

At this point we perform a number of robustness checks to address remaining concerns that would challenge the interpretation of the rank parameter. First, we test the assumption that parents are not sorting to primary schools on the basis of rank. The second is a broader issue of measurement error in the baseline test scores that could give rise to long-run correlations between individual unobserved ability and secondary outcomes, even conditional on the primary baseline score. We provide estimates for the extent to which these effects would impact on our estimates through a series of data simulations and placebo tests. Based on these additional tests, we conclude that the primary rank effects that we document to affect secondary attainment and subject choices are genuine.

After explaining our novel approach and new findings, we take some first steps towards the understanding of the economic mechanism behind our estimated rank effect. We consider mechanisms of competitiveness, parental investment, environment favoring certain ranks, and confidence. By combining the administrative data with survey data of 12 thousand students with direct measures of subject specific confidence, we find that conditional on achievement those that are ranked higher among their peers in a subject have higher confidence in that subject. In parallel to what we find with test scores, we also find that boys' confidence is more affected by their school rank than girls'.

This higher confidence could be indicative of two channels. First, confidence is reflective of students learning about their own strengths and weaknesses in subjects, which is driven by their test scores and class rankings. This is analogous to other proposed mechanisms of social interaction (Azmat and Iriberry, 2010; Ertac, 2006), but instead of students only using their test scores and

cardinal relative positions, they now also use their ordinal ranking. Students then use this information to infer their education production function across subjects, and then make effort investment decisions. Alternatively, consistent with the “Big Fish Little Pond” literature (Marsh et al., 2008, 1988), confidence due to their rank could improve non-cognitive skills, which then lowers the cost of effort (or increases productivity) in that subject (O’Mara et al., 2006). Using a stylized model of effort allocation across subjects, we derive a testable hypothesis of misinformation to distinguish between these alternative explanations and find some evidence in favor of non-cognitive skills. This evidence is in line with the psychological literature which finds that children’s social environment during the primary school years is the most important for developing a sense of self concept (Rubie-Davies, 2012; Tidemann, 2000; Leflot et al., 2010).

It is important to point out that this paper is complementary to –but distinct from– a number of existing literatures. First, there is a long-standing interest regarding the theories and empirical manifestations of social interactions and peer effects, where group characteristics or outcomes causally influence individual outcomes (Sacerdote, 2001; Hoxby and Weingarth, 2005; Whitmore, 2005; Kremer and Levy, 2008; Carrell et al., 2009; Lavy et al., 2012).⁴ Second, the literature on status concerns and relative feedback. Tincani (2015) and Bursztyrn and Jensen (2015) find evidence that students have status concerns and will invest more effort if gains in ranks are easier to achieve. These results are similar to findings from non-education settings where individuals may have rank concerns such as in sports tournaments (Genakos and Pagliero, 2012), and in firms with relative performance accountability systems (i Vidal and Nossol, 2011). We differ from this literature because we estimate the effects of rank in a new environment, where status concerns or information about prior ranks do not matter.⁵ Third, there is a literature that examines the introduction of relative achievement feedback measures in education settings. Bandiera et al. (2015) find that the provision of feedback improves subsequent test scores for college students. Specifically relating to relative feedback measures Azmat and Iriberry (2010) and Azmat et al. (2015) find that their introduction during high school increases productivity in the short run. In contrast, this paper is not examining the reaction to a new piece of information but how student react to their ranking that they themselves perceive throughout primary school.

The most closely related literature is that on rank itself. These papers account for relative achievement measures and estimate the additional impact of ordinal rank on contemporaneous measures of well-being (Brown et al., 2008; Luttmer, 2005) and job satisfaction (Card et al., 2012). We contribute to this literature by establishing effects on objective outcomes over a longer time period. Finally, in addition to the direct contribution to the broad peer effects literature, we discuss in our conclusions that our findings provide potential explanations to reconcile some of the so-far mixed results in the related selective schools, affirmative action, and neighborhood effects literatures.

⁴ Of the eight categories of peer effects put forward by Hoxby and Weingarth (2005) rank impacts would most closely be associated with that of invidious comparisons.

⁵ The reasons why we do not study rank effects on contemporaneous outcomes are twofold. First, we regard these as impossible to identify non-experimentally and potentially by generated by different mechanisms. Second, even if identification of contemporaneous effects was possible, these might fade out over time and thus be economically less important.

The remainder of the paper is laid out as follows: Section 2 discusses the empirical strategy and how the rank parameter is identified. This is followed by a brief description of the English educational system, the administrative data, as well as the definition of rank used. Section 4 sets out the main results, as well as the nonlinearities and heterogeneity by gender and parental income. Section 5 discusses and tests threats to identification, such as endogenous sorting and measurement error. Section 6 also discusses potential mechanisms and provides additional survey evidence. Finally, in Section 7, we conclude by discussing other topics in education that corroborate with these findings and possible policy implications.

2 A Rank-Augmented Education Production Function

We use the standard education production function approach to derive a rank-augmented value added specification to identify the effect of primary school rank on subsequent outcomes.⁶ Our basic specification is the following:

$$Y_{ijksc} = \beta_{\text{Rank}} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}) + \mathbf{x}'_i \boldsymbol{\beta} + \mathbf{d}_{jsc}' \boldsymbol{\gamma} + \varepsilon_{ijksc} \quad (1)$$

where:

$$\varepsilon_{ijksc} = \tau_i + \pi_{ksc} + v_{ijksc}$$

Y_{ijksc} denotes the national academic percentile rank of student i , from primary school j , in subject s and cohort c attending secondary school k in time period t . In this setting we only have two time periods, with the initial $t - 1$ representing the primary school period and t representing the secondary school period; for ease of exposition we drop the t superscript from the specifications and use superscripts for $t - 1$. In our data these correspond to national tests at age-11 ($t - 1$) and either age-14, age-16 or age-18 (all t). Student achievement is determined by a series observable and unobservable characteristics and shocks. Y_{ijsc}^{t-1} captures all factors up to the end of primary school such as student ability, parental investment, school inputs and peer effects that have impacted on student achievement. In our regressions, we allow for a cubic relationship with prior test scores.⁷ We also condition on \mathbf{x}_i , which is a vector of the observable permanent characteristics of the student which are allowed to affect individual test score growth. Moreover, we include primary SSC dummies, \mathbf{d}_{jsc} , allowing for long run mean impacts of being in a certain primary school-subject-cohort. Finally, the remaining unobservable factor ε_{ijksc} , is comprised of three components; unobserved individual specific shocks that occur between $t-1$ and t , τ_i ; the overall impact of attending a secondary school k in the second period t on test scores in subject s in cohort c , π_{ksc} ⁸; and an idiosyncratic error term v_{ijksc} . To allow for unobserved correlations in all of our estimations we cluster the error term at the level of the secondary school.

⁶ To see a full derivation from a more basic model see Appendix A.1

⁷ As a robustness check in Section 5 we also use a fully flexible measure $f(Y_{ijsc}^{t-1}) = \sum_{p=1}^{100} (\beta_p M^p)$, where M^p are dummy variables to allow for a different effect at each national test score percentile p , instead of the third-order polynomial in age-11 test scores. Since this does not affect our estimates we prefer the more parsimonious specification.

⁸ In Section 6.2 we discuss results that additionally account for secondary-subject-class-level effects.

The parameter of interest is β_{Rank} , which is the effect of having rank R_{ijsc}^{t-1} , in subject s in cohort c and in primary school j on achievement in that subject during secondary school. We construct R_{ijsc}^{t-1} based on the external end-of-primary school test scores Y_{ijsc}^{t-1} that we know for every student, as explained in detail in Section 3.3.⁹ The inclusion of the primary SSC dummies d_{jsc} makes the prior end of primary test scores account for the relative (cardinal) achievement within a class, and hence giving the rank parameter an ordinal interpretation. Figure 1 provides a graphical illustration of this and of the remaining variation used to estimate β_{Rank} . As we can see, for any given test score, students can have a range of different ranks even if average peer quality is identical.

In our most flexible specifications, we further augment these regressions by including the average student growth rate across subjects directly through dummy variables for each individual, n_i , to recover individual growth effects σ . This get us:

$$Y_{ijksc} = \beta_{Rank} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}) + n_i' \sigma + d_{jsc}' \gamma + \omega_{ijksc} \quad (2)$$

where:

$$\omega_{ijksc} = \pi_{ksc} + v_{ijksc}$$

This individual growth term absorbs all time-invariant individual characteristics x_i from specification 1 as well as the unobserved individual component τ_i from the error term.¹⁰ This specification effectively compares relative rankings across different classes, controlling for individual national subject-specific achievement as well as averaging out individual-specific growth rates. To reiterate, the variation in ranks that is used here in order to estimate β_{Rank} still relies on the naturally arising higher-moments of classroom rank distributions that occur because classes are small and draw from a large population. However, in contrast to specification 1, any individual characteristic that is not realized in age 11 test scores but contributes towards average age 14 test scores is now accounted for, such as competitiveness or secondary school attended, as long as the effects are not subject specific.

The inclusion of the student fixed effects also changes the interpretation of the rank estimate. Specification 1 allows spillover effects from being ranked highly in one subject onto others. Any estimation, which allows individual growth rates during secondary school, would absorb any spillover rank effects between subjects within students. It is for this reason, along with the additional accounting for other co-varying factors, why we would expect the coefficient of the rank effect in specification 2 to be smaller than those found in specification 1. Here, the rank parameter only represents the increase in test scores in a subject due to subject-specific rank, as any general gains across all subjects due to rank are absorbed by the student effect. As a result, the rank estimate of specification 2 can be interpreted as the extent of specialization in subject s due to primary school rank in that subject.

Finally, to investigate potential non-linearities in the effect of ordinal rank on later outcomes,

⁹ Any positive impact of rank during primary school on student primary school test scores would downward bias our results as it would be captured by higher age-11 test scores and thus lower age-11 to age-14 value added.

¹⁰ Note that our estimation sidesteps the standard issue of including a lagged dependent variable and individual effects simultaneously Nickell (1981), as the individual effects are recovered from test scores across subjects, rather than from average test scores over time.

(i.e. effects driven by students who are top or bottom of the class), we replace the linear ranking parameter with indicator variables according to vingtiles in rank $\sum_{\lambda=1}^{20} I_{is}^{\lambda}$ plus additional mutually exclusive indicator variables for those at the top and bottom of each school-subject-cohort. This can be applied to all the specifications presented. In the case of specification 1, this results in the following estimation equation:¹¹

$$Y_{ijksc} = \beta_{R=0} Bottom_{ijksc}^{t-1} + \sum_{\lambda=1}^{20} \left(\beta_{\lambda} I_{is}^{t-1, \lambda} \right) + \beta_{R=1} Top_{ijksc}^{t-1} + f(Y_{ijksc}^{t-1}) + \mathbf{n}'_i \boldsymbol{\beta} + \mathbf{d}_{jsct}' \boldsymbol{\gamma} + \omega_{ijksc} \quad (3)$$

where:

$$\omega_{ijksc} = \pi_{ksc} + v_{ijksc}$$

Given this structure the conditional independence assumption that needs to be satisfied for estimating an unbiased rank parameter is the following:- conditional on prior test scores (accounting for all non-time varying effects and inputs to age 11), student characteristics, and primary SSC level effects, we assume there would be no expected differences in the students' secondary school outcomes except those driven by rank. The remaining concern is that (subject-specific) unobserved effects or shocks may be at the individual subject level and transitory in the sense that they occur during primary school, but do not get measured by the end of the primary school baseline score, and then affect test scores later on. Examples for such non-linear and transitory shocks are primary school teachers teaching to specific parts of the distribution whose impact is only revealed in secondary school, non-linear transitory peer, or any other transitory effects potentially generated through measurement error in the age 11 tests.

To give a short preview, in Section 5.1 we perform the following checks: We address the concern about endogenous sorting to schools by merged-in survey data to show that subject-specific parental occupations, which predict test scores across subjects, are orthogonal to rank. Next, we provide the following evidence: first, we demonstrate through simulations and placebo tests that large non-standard peer effects and teacher effects would not generate these effects; second, we show that measurement error in the measurement of ability through test scores would at worst downward bias our results; and third, we present a series of robustness checks including replacing the "cubic" with "flexible" controls for age-11 test scores, re-estimating on a sample of small primary schools, and determining if these effects are mainly driven by the variance of primary peer test scores. All in all, we find no evidence that would challenge the causal interpretation of the long-run effect of primary rank.

¹¹ Estimates are robust to using deciles in rank rather than vingtiles and can be obtained upon request. In the case of ties in test scores both students are given the lower rank.

3 Institutional Setting, Data, and Descriptive Statistics

3.1 The English School System

The compulsory education system of England is made up of four Key Stages (KS). At the end of each stage, students are evaluated in national exams. Key Stage Two (KS2) is taught during primary school between the ages of seven and 11. English primary schools are typically small with the median size of a cohort being 27 students. The mean primary school class size also is 27 students (Falck et al., 2011) and so when referring to cohort-level primary school rank in a subject, this is equivalent to the class rank in that subject. At the end of the final year of primary school, when the students are aged 11, they take KS2 tests in English, math and science. These tests are externally graded on absolute attainment on a national scale of between zero to 100.

Students then transfer to secondary school, where they start working towards the third Key Stage (KS3). During this transition, the average primary school sends students to six different secondary schools, and secondary schools typically receive students from 16 different primary schools. Hence, upon arrival at secondary school, the average student has 87 percent new peers. This large re-mixing of peers is beneficial, as it allows us to estimate the impact of rank from a previous peer group on subsequent outcomes. Importantly, since 1998, it is unlawful for schools to select students on the basis of ability; therefore, admission into secondary schools does not depend on end-of-primary KS2 test scores or student ranking.¹² This means that KS2 is a low-stakes test with respect to secondary school choice. Key Stage 3 (KS3) takes place over three years, at the end of which all students take KS3 examinations in English, math, and science at age fourteen. Again, KS3 is not a high-stakes test and is externally marked out of 100.

At the end of KS3, students can choose to take a number of subjects (GCSEs) for the Key Stage 4 (KS4) assessment, which occurs two years later at the age of 16 and marks the end of compulsory education in England. The final grades for KS4 consist of nine levels (A*, A, B, C, D, E, F, G, U), to which we have assigned points to grades according to the Department for Education's guidelines (Falck et al., 2011). However, as students have some discretion in choosing the subjects they study and at what level, these GCSE grade scores are inferior measures of student achievement compared to KS3 examinations, which are on a finer scale and are compulsory. Therefore, as students are tested in the same three compulsory subjects in KS2 and KS3, we prefer KS3 as the main outcome measure for the purpose of our study. However, we also present results for the high-stakes KS4 examinations.

After KS4, some students choose to stay in school to study A-Levels which are a precursor for university level education. This constitutes a high level of specialization, as students typically only complete A-Levels in three subjects out of a set of 40.. For example, a student could choose to study biology, economics, and geography, and not English or math. Importantly, students' choices of subjects limit their choice-sets of majors at university, and so will have longer run effects on career choice. For example, chemistry as an A-Level is required to apply for medicine degrees and

¹²The Schools Standards and Framework Act 1998 made it unlawful for any school to adopt selection by ability as a means for allocating places. A subset of 164 schools (5%) were permitted to continue to use selection by ability. These Grammar schools administer their own admission tests independent of KS2 examinations and are also not based on student ranking within school.

math is a prerequisite for studying engineering.¹³ To study the long run impact of primary school ranking on students, we also examine the impact of rank on the likelihood of choosing to study that subject at A-Level.

3.2 Student Administrative Data

The Department for Education (DfE) collects data on all students and all schools in state education in England in the National Pupil Database (NPD).¹⁴ This contains the school's attended and demographic information (gender, Special Educational Needs (SEN), Free School Meals Eligible (FSME) and ethnicity). The NPD also tracks student attainment data throughout their Key Stage progression in each of the three compulsory subjects.

We extract a dataset that follows the population of five cohorts of English school children. This begins at the age of 10/11 in the final year of primary school when students take their Key Stage 2 examinations through to age 17/18 when they complete their A-Levels. The age 11 exams were taken in the academic years 2000/2001 to 2004/2005; hence, it follows that the age 14 examinations took place in 2003/2004 to 2007/8 and that the data from completed A-Levels comes from the years 2007/08 to 2010/12.¹⁵

First, we imposed a set of restrictions on the data to obtain a balanced panel of students. We use only students who can be tracked with valid age 11 and age 14 exam information and background characteristics. This comprises 83 percent of the population. Secondly, we exclude students who appear to be double counted (1,060) and whose school identifiers do not match within a year across datasets. This excludes approximately 0.6 percent of the remaining sample (12,900). Finally, we remove all students who attended a primary school whose cohort size was smaller than 10, as these small schools are likely to be atypical in a number of dimensions. This represents 2.8 percent of students.¹⁶ This leaves us with approximately 454,000 students per cohort, with a final sample of just under 2.3 million student observations, or 6.8 million student subject observations.

The Key Stage test scores at each level are percentalized by subject and cohort, so that each individual has nine test scores between zero and 100 (KS2, KS3, and KS4). This ensures that students of the same nationally relative ability have the same national percentile rank, as a given test score could represent a different ability in different years or subjects. Thereby allowing test score comparisons to be made across subjects and across time. This does not impinge on our estimation strategy, which relies only on variation in test score distributions at the SSC level.

Table 1 shows descriptive statistics for the estimation sample. Given that the test scores are represented in percentiles, all three subject test scores at age 11, 14, and 16 have a mean of around 50, with a standard deviation of about 28.¹⁷ Almost sixty percent of students decide to stay and

¹³ For the full overview of subjects that can be chosen, see: <http://www.cife.org.uk/choosing-the-right-a-level-subjects.html>

¹⁴ The state sector constitutes 93% of the student population in England.

¹⁵ The analysis was limited to five cohorts as from year 2008/9 the external age 14 examinations were replaced with teacher assessments.

¹⁶ Estimations using the whole sample are very similar, only varying at the second decimal point. Contact authors for further results.

¹⁷ Age-16 average percentile scores have lower averages due to the less precise underlying grading scheme.

continue their education until the A-Levels, which are the formal gateway requirement for university admission. Of the many subjects to choose from, about 14 percent choose to sit an A-Level exam in English, while in math and science, the proportions are about 9 percent and 11 percent, respectively.

Information relating to the background characteristics of the students is shown in the lower panel of Table 1. Half of the student population is male, over four-fifths are white British, and about 15 percent are Free School Meal Eligible (FSME) students, a standard measure of low parental income.

The within student standard deviation across the three subjects, English, math, and science, is 12.68 national percentile points at age 11, with similar variation in the age 14 tests. This is important, as it shows that there is variation within student which is used in student effects regressions.

3.3 Measuring Ordinal Rank

As explained in Section 3.1, all students take the end-of-primary national exam at age 11. These are finely and externally graded between zero and 100. We use these scores to rank students in each subject within their primary school cohort.

As previously noted, primary schools are small. However, since there exists some differences in school cohort sizes, in order to have a comparable local rank measure across schools, we cannot use the ordinal rank directly. Instead, we transform the rank position into a local percentile rank using the following formula:

$$R_{ijsc} = \frac{n_{ijsc} - 1}{N_{jsc} - 1}, R_{ijsc} \in \{0, 1\} \quad (4)$$

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 set is n_{ijsc} , which increases in test score to a maximum of N_{jsc} . R_{ijsc} is the cohort-size adjusted ordinal rank of the student that we use in the estimations. For example, a student who is the best in a cohort of twenty-one students ($n_{ijsc} = 21$, $N_{jsc} = 21$) has $R_{ijsc} = 1$ and so does a student who is the best in a cohort of 30. Note that this rank measure will be approximately uniformly distributed, and bounded between zero and 1, with the lowest rank student in each school cohort having $R = 0$. Similar to the non-transformed ordinal rank position, this transformed ordinal rank score does not carry any cardinal information (i.e. information about relative ability distances). For the ease of exposition, in the remainder of this paper we will refer to R_{ijsc} as the ordinal rank, rather than as the local percentile rank or as the cohort-size adjusted ordinal rank. Panel B of Table 1 shows descriptive statistics for the rank variable.

Given this measurement of rank by the researchers, it is relevant to consider how students will measure their academic rank, too. In fact, while we as researchers have full access to the test score data, rather than receiving these finely graded scores, students are instead given only one of five broad attainment levels. The lowest performing students are awarded level 1. The top performing students are awarded level 5. These levels are broad coarse measures of achievement, with 85% of students achieving levels 4 or 5. Students are not told their precise scores because the age 11 test (and the subsequent age 14 test) are a non-high stakes exams for students, and are mainly

used by the government as a measure of school effectiveness.¹⁸

For our context, this means that students do not know their underlying exact scores in these Key Stage exams, which we use to calculate their local ranks. However, we put forward that students infer their rank positions in class through repeated interactions and comparisons between the same group of students throughout the six years of primary school, along with repeated teacher feedback.¹⁹ We, therefore, use our calculated rank as a proxy for students' perceived rank throughout primary school in each subject. While we cannot know if students' academic rank based on Key Stage test scores is a good proxy for student perceptions, we have three facts that support this claim. First, there is a longstanding physiological literature that has established that individuals have accurate perceptions of their rank within a group (e.g. [Anderson et al., 2006](#)). Second, we find using merged survey data (described in Section 3.4) that conditional on test scores, students with higher ranks in a subject have higher confidence in that subject. And third, if individuals do not have a good perception of the rankings, then we would not expect to find an impact of our rank measurement at all. To this extent, the rank coefficient β_{Rank} from Section 2 would be attenuated and we are estimating a reduced form of perceived rank using actual rank.

3.4 Survey Data: The Longitudinal Study of Young People in England

Additional information about a subsample of students is obtained through a representative survey of 16,122 students from the first cohort. The Longitudinal Survey of Young People in England (LSYPE) is managed by the Department for Education and follows a cohort of young people, collecting detailed information on their parental background, academic achievements and attitudes.

We merge survey responses with our administrative data using a unique student identifier. This results in a dataset where we can track students from a primary school, determine their academic ranks, and then observe their later measurements of confidence and attainment, allowing us to test if rank affects confidence conditional on attainment. This survey also contains detailed information on the occupation of each parent, which we will later use to test parental sorting.²⁰

At age 14 the students are asked how good they consider themselves in the subjects English, math, and science. We code five possible responses in the following way: 2 "Very Good"; 1 "Fairly Good"; 0 "Don't Know"; -1 "Not Very Good"; -2 "Not Good At All". We use this simple scale as a measure of subject specific confidence. While it is more basic than surveys that focus on confidence, it does capture the essence of the concept.

The matching between the NPD and LSYPE is based on a common unique identifier. However, the LSYPE also surveys students attending private schools that are not included in the national datasets. In addition, students that are not accurately tracked over time have been removed, in

¹⁸The students also appear not to gain academically just from achieving a higher level. Using a regression discontinuity design across these achievement levels, where the underlying national score is the running variable, shows no gains for those students who just achieved a higher level.

¹⁹In English Primary schools it is common for students to be seated at tables of four and for tables to be set by pupil ability. Students can be sat at the 'top table' or the 'bottom/naughty table'. This could assist students in establishing where they rank amongst all class members through a form of batch algorithm, e.g. 'I'm on top table, but I'm the worst, therefore I'm fourth best.'

²⁰This is the first research to merge LSYPE responses to the NPD for primary school information.

total 3,731 survey responses could not be matched. Finally, 1,017 state school students did not fully complete these questions, and so could not be used for the confidence analysis. Our final dataset of confidence and achievement measures contains 11,558 student observations. Even though the survey does not contain the attitude measures of every student in a school cohort, by matching the main data, we will know where that student was ranked. This means we are able to determine the effect of rank on confidence, conditional on test scores and SSC fixed effects.

The bottom panel of Table 1 shows descriptive statistics for the LSYPE sample, which we use to estimate rank effects on confidence directly. The LSYPE respondents are representative of the main sample, although the mean age 11 test scores are slightly lower and the proportion of FSME is higher than the national at 18.6 percent and 14.6 percent, respectively.

4 Estimation

Before turning to the estimation results, we illustrate the variation in rank we use for a given test score relative to the mean demonstrated in Figure 1. Figure 2 replicates the stylised example from Figure 1 using six primary school classes in English from our data. Each class has a student scoring the minimum, maximum, 92 and have a mean test score of 55 (as indicated by the dashed grey line) in the age 11 English exam. Given the different test score distributions each student scoring 92 has a different rank. This rank is increasing from School 1 through to School 6 with ranks R of 0.83, 0.84, 0.89, 0.90, 0.925 and 0.94 respectively, despite all students having the same absolute and relative to the class mean test scores. Figure 3 extends this example of the distributional variation by using the data from all primary schools and subjects in our sample. Here, we plot age 11 test scores, de-measured by primary SSC, against the age 11 ranks in each subject. The vertical thickness of the distribution of points indicates the support throughout the rank distribution. For the median student in a class, we have wide support for in-sample inference from $R = 0.2$ to $R = 0.8$.

4.1 Effect of Rank on Age-14 Test Scores

To begin the discussion of the results, we present estimates of the impact of primary school rank on age 14 test scores. The estimates are reported in the first three columns of Table 2, with the specifications becoming increasingly flexible, moving to the right from just conditioning on prior test scores and SSC effects to the inclusions of student characteristics (ethnicity, gender, ever FSME, SEN), and individual effects. All specifications allow for up to a cubic relationship with age 11 test scores.

Column (1) is the most basic specification. It shows that the effect of being ranked top compared to bottom *ceteris paribus* is associated with a gain of 7.95 national percentile ranks (0.29 standard deviations). When accounting for pupil characteristics, there is an insignificant change to 7.96. Given the distribution of test scores across schools, very few students would be bottom ranked at one school and top at another school. A more useful metric is to describe the effect size in terms of standard deviations. A one standard deviation increase in rank is associated with increases in later test scores by 0.085 standard deviations or 2.36 national percentile points. This is comparably large in comparison with other student characteristics typically included in growth specifications:

females' growth rate is 1.01 national percentile points higher than males, and FSME students on average have 2.96 national percentile points lower growth rate than non-FSME students.²¹

Column 3 shows the estimates that also include student fixed effects (specification 2). Recalling from Section 2, conditioning on student effects allows for individual growth rates, which absorb all student level characteristics. Since students attend the same primary and secondary school for all subjects, any general school quality or school sorting is also accounted for. Just as before, any long run subject specific primary school quality effects are accounted for by the primary SSC effects.²²

As expected, the within student estimate is considerably smaller as the student effect also absorbs any spillover effects gained through high ranks in other subjects, and is only identifying the relative gains in that subject. The effect from moving to the bottom to top of class *ceteris paribus* increases the national percentile rank by 4.56 percentiles, as we see in column (3) of Table 2. To make a comparison in terms of standard deviations, this effect is scaled by the within student standard deviation of the national percentile rank (11.32). Conditional on student and SSC effects, the maximum effect of rank is 0.40 standard deviations. This is a very large effect, but a change from last to best rank within student represents even more of an extreme treatment. It is more conceivable for a student to move 0.5 rank points (e.g., being at the 25th percentile in one subject and 75th at another). Our estimates imply that these students would improve their test scores in that subject by 0.20 standard deviations. In terms of effect size, given that a standard deviation of the rank within student is 0.138 for any one standard deviation increase in rank, test scores increase by about 0.056 standard deviations.²³

If there were any general gains through achieving a high rank in one subject, this would be absorbed in the within student estimates. Making a difference between columns (2) (7.96) and (3) (4.56) can be interpreted as an upper bound of the gains from spillovers between subjects. A more detailed interpretation of the differences in effect size are provided at the end of Section 6.

4.2 Effect of Rank on Key Stage 4 Outcomes (Age 16)

We extend the scope of the analysis on primary school rank from comparable age 14 tests to high stakes national exams at age 16 in columns (4) to (6) of Table 2. The three core subjects (English, math, and science) are again tested at age 16. These outcomes are not the main focus of our paper for the reasons outlined in Section 3.1. The impact of primary school rank on test performance has only marginally dropped in all specifications between ages 14 and 16. Being at the top of class compared to the bottom during primary school increases age 16 test scores by 6.47 percentile points compared with 7.96 at age 14. This shows remarkably little fade out over time, given that the students are in a new peer environment. We continue to observe that a one standard

²¹Including the rank parameter in this specification reduces the Mean Square Error by 0.31. This is more than the reduction from allowing for a gender growth term (0.25) or an ethnicity growth term (0.28).

²²In addition, the student fixed effects assume similar effects across subjects. Re-estimating column (2) for subjects separately, we find relatively similar effect sizes of 8.826, s.e. 0.187 (Maths); 8.441, s.e. 0.188 (Science) and 7.646, s.e. 0.201 (English). Of course, it is not possible to estimate column (3) that includes the student fixed effects separately by each subject.

²³For students with similar ranks across subjects the choice of specialization could be less clear. Indeed, in a sample of the bottom quartile of students in terms of rank differences, the estimated rank effect is 25% smaller than those from the top quartile. Detailed results available on request.

deviation increase in rank improves later test scores by 1.80 national percentiles. Similarly, the impact on test scores using the within student variation has decreased, but remains significant.

4.3 Effect of Rank on A-Level Choices (Age 18)

After the examinations at age 16, students can choose to stay in school and study for A-Levels which are the key qualifications required to study any associated subjects at university. To this end, we estimate the impact of primary school rank in a specific subject on the likelihood of choosing to study that same subject for A-Levels.²⁴

These results are presented in columns (7) to (9) of Table 2, with a binary outcome variable being whether or not the student completed an A-Level related to that subject. In this linear probability model, conditional on prior test scores, student characteristics and SSC effects, students at the top of the class in a subject compared to being ranked at the bottom are 3.9 percentage points more likely to choose that subject as an A-Level. This is on a baseline take-up rate among these subjects of 10.5 percent. A student who is at the 75th rather than 25th rank percentile in a subject would, therefore, be 18.6 percent more likely to choose a course related to that subject for an A-Level. Interestingly, for A-Level choice, there is no significant difference when additionally including student effects. This could reflect the lack of spillovers across these range of subjects, given that the choice of subjects is limited to three, and that students could potentially take multiple A-Levels within one subject (e.g. biology, physics, chemistry).

4.4 Non-Linearities and Heterogeneity

The specifications thus far assumed that the effect of rank is linear. However, it is conceivable that the effect of rank changes throughout the rank distribution (Brown, 2011). To address this, we allow for non-linear effects of rank by replacing the rank parameter with a series of 20 indicator variables in the vingtiles in rank, plus top and bottom of class dummies (see specification 3).

The equivalent estimates from specification 1 and 2, without and with student fixed effects, are presented in the first panel of Figure 4. The effect of rank appears to be linear throughout the rank distribution, with small flicks in the tails. This indicates that the effect of rank exists throughout. Students ranked just above the median perform better three years later than those at the median. The within student estimates are smaller in magnitude throughout and have less of a gain for being top of the class.

We now turn to how the effects of rank vary by student characteristics. We estimate these effects using the student fixed effects specification with non-linear rank effects in addition to interacting the rank variable with the dichotomous characteristic of interest. These student characteristics are gender and FSME status. For each variable, we plot the partial predictions of the effect of rank on test scores for both groups, illustrating how the different groups react to primary school rank.²⁵

²⁴Students that did not take on any of the core subjects or have left school are included in the estimations.

²⁵The student characteristics themselves are not included in the estimations, as they are absorbed by the student effects. These characteristics interacted by rank, however, are not absorbed by student effects, because there is variation within the student due to having different ranks in each subject.

The second panel in Figure 4 shows how rank relates to the gains in later test scores by gender. Males are more affected by rank throughout 95 percent of the rank distribution. Males gain four times more from being at the top of the class, but also lose out marginally more from being in the bottom half. This is within student variation in later test scores, and therefore the coefficient could be interpreted as a specializing term, implying that prior rank is associated with a stronger specializing effect on males than females.

The third panel in Figure 4 shows that FSME students are less negatively affected by rank and more positively affected than Non-FSME students. Free School Meals Eligibility students with a high rank gain more than Non-FSME students, especially those ranked top in class who gain almost twice as much. We return to these results in Section 6, when we discuss separate mechanisms that might give rise to these effects.

5 Robustness

The causal interpretation of the rank estimates relies on two main assumptions. The first is the standard conditional independence assumption, that conditional on test scores, SSC effect and in some specifications individual effects, a student's rank is orthogonal to other determinants of the student's later achievement in that subject. The idea that this assumption rests on is that the across-classroom variations in rank that we are exploiting are arising naturally because classes are small. In the following Section 5.1, we explore the validity of this assumption by testing if parents who will generate high growth in a subject systematically sort their children to primary schools, such that their child will have a particularly high/low rank in that subject.

The second requirement is that these rank effects are not generated through the way in that we measure ability using the external achievement tests, which we also use to generate the rank measures. We need to ensure that neither measurement errors nor other transitory shocks would result in spurious rank effects. In Section 5.2, we discuss potential transitory peer effects, teacher effects and more classical measurement error. Providing reasoning, simulations of a data generating process as well as a number of placebo checks, we argue that our estimates are -at worst-attenuated.

Finally, in Section ?? we discuss further checks regarding functional form assumptions when controlling for the individual baseline achievement score, differences in school sizes, and effects of additionally controlling for some higher-moments of classroom ability distributions.

5.1 Rank-Based Primary School Sorting²⁶

We provide indicative evidence that the conditional independence assumption is satisfied by showing that a student's rank is orthogonal to a strong pre-determined subject-varying determinant of a student's later achievement. A prime example of such subject-specific unobservables is the occupational background of the parents. Children of scientists may both have a higher initial achievement and a higher learning curve in science throughout their academic career, due

²⁶We also discuss -and reject- rank-based sorting to secondary schools, which would be an outcome rather than a confounding factor, in Section 6.2.

to parental investment or inherited ability. The same could be said about children of journalists for English and children of accountants in math. This will not bias our results, as long as conditional on age 11 test scores, parental occupation is orthogonal to primary school rank. However, if these parents sort their children to schools such that they will be the top of class and also generate higher than average growth then this would be problematic. Therefore we test for paternal sorting to schools on the basis of rank.²⁷

We test for this by using the LSYPE sample, which has information on parental occupation. All parental occupations are classified into English, math, science, or “other”. Then, an indicator variable is created for each student subject if they have a parent who works in that field.²⁸ To test if this is a good indicator of parental transfer of abilities by subject, we first regress age 11 test scores on parental occupation, school subject effects, and student effects (Table 3, Panel A). This shows that after accounting for average individual achievement, this measure of parental occupation is a significant predictor of student subject achievement. Then, using rank as the dependent variable, we test for a violation of the orthogonality condition in Panel B of Table 3. Here, we see that while parental occupation predicts student achievement by subject, it does not predict rank conditional on test scores. This implies that parents are not selecting primary schools on the basis of rank for their child. This does not rule out other co-varying factors that may bias the results, but it provides us with confidence that this likely large factor of parental influence does not.

5.2 Test-Scores as a Measure of Ability

We use age 11 test scores as a measure of student ability. The Key Stage 2 test scores are particularly good measures of ability, as they are finely graded, and their only purpose is to gauge the ability of the student on an absolute metric. This means that students are not marked on a curve; hence, test scores are not a function of rank. However, there may be factors that cause these test scores to be a poor measure of ability. As we simultaneously use these test scores to determine rank and as a measure of prior achievement, these factors are of concern to our paper. We consider three cases where test scores do not reflect the underlying student ability, peer effects, teacher effects and other transitory shocks (measurement error).

Peer Effects: There are a multitude of papers estimating the impact of peers on student attainment (Sacerdote, 2001, 2011; Carrell et al., 2009, 2013; Falk and Ichino, 2006). To the extent that peer effects are sizable, they could have meaningful impacts on our results, because they would

²⁷Typically, parents try to get their child into the best school possible in terms of average grades, which would work against any positive sorting by rank, as higher average achievement would decrease the probability of their child having a high rank. However, if parents want to maximize their child’s rank in a particular subject, this can bias the results. To do this, they need to know the ability of their child and of all potential peers by subject. This is unlikely to be the case, particularly for such young children who have yet to enter primary education at age four. Parents can infer the likely distributions of peer ability if there is autocorrelation in student achievement within a primary school. This means that if parents know the ability of their children by subject, as well as the achievement distributions of primary schools, they could potentially select a school on this basis.

²⁸Parental Standard Occupational Classification 2000 grouped in Science, Math, English and Other. Science (3.5%); 2.1 Science and technology, 2.2 Health Professionals, 2.3.2 Scientific researchers, 3.1 Science and Engineering Technicians. Math (3.1%); 2.4.2 Business And Statistical Professionals, 3.5.3 Business And Finance Associate Professionals. English (1.5%); 2.4.5.1 Librarians, 3.4.1 Artistic and Literary Occupations, 3.4.3 Media Associate Professionals. Other: Remaining responses.

simultaneously impact on a student’s rank and age 11 test scores. Consider the situation where an individual has poorly performing peers during primary school. They would likely achieve a lower score than they otherwise would and have a higher rank. If peer effects are transitory, when the student goes on to attend secondary school, which in expectation will have average peers, they will achieve test scores appropriate to their ability. This means that a student with bad primary peers will have a high rank and also high gains in test scores. In our value-added specification, this would generate a rank effect, even if rank had no impact on test scores. Note that this is only an issue if peer effects are transitory. If primary peers have a permanent impact on student attainment, then conditioning on the end of primary test scores will have fully accounted for them.

In Appendix A.2 we show that these peer effects can generate the appearance of rank effects using a data generating process, wherein we specify large transitory peer effects and no impact of rank. However, after taking estimates from 1000 simulations, we go on to show that when controlling for mean SSC effects, the estimated false rank effect becomes negligible. Note that we are cautious and we set the peer effects to be 20 times larger than those found by Lavy et al. (2012) using the same English census data and school setting. Moreover, we are testing using simulated data, both where the peer effect is linear in means and when it is a highly non-linear. The simulations and further discussion can be found in Appendix A.2 and Appendix Table A.1.

Teacher Effects: Alternatively, instead of peers influencing the transformation of ability to test scores, it is possible that teachers teach in such a way as to generate false positive rank results. For this to occur, it would only require teachers to have different transformation functions of student ability into test scores. Let us consider a simple case, where test scores y_{ij} are determined by mean teacher effects μ_j , and linear teacher transformations b_j of student ability a_{is} , $y_{ij} = \mu_j + b_j a_{ij} + e_{ij}$. Here, variation in the teachers’ production functions will generate differences in the observed test scores for a given ability. If teachers varied only in level effects μ_j then these differences would be captured by the SSC effects, which allow for mean differences in future achievement. However, if teachers also vary in their transformative approach b_j , that would not be captured with these fixed effects, as different students within a class will be affected differently. If there is variation in b_j , then the same test score will not mean the same things in different schools, and rank will preserve some information on ability, which test scores will not.

To test if this kind of spreading of test scores is driving the results, we run a series of placebo tests. We randomly reassign students to cohorts, such that students will appear in the same schools they attended, but will only have a one in five chance to be in their actual ‘correct’ cohort. Assuming that this transformative teacher effect (b_j) is time invariant, a student of a given initial ability in one cohort would get the same test score no matter what cohort they attended.²⁹ If it is the teacher specific transformations that are generating the results, then the remixing across cohorts will have no impact on the rank parameter, as rank will contain the same residual information in each cohort. Row 2 of Table 4 shows the mean coefficients and standard errors from 50 randomization of students within schools across cohorts. Note that the rank parameters are significantly smaller and approximately one fifth of the previous size. As we use five cohorts of data, students

²⁹This assumption is reasonable in the English setting as teachers teach certain year groups rather than following a single cohort through primary school.

will, on average, randomly have a fifth of their true peers in each cohort, and so we would expect this proportion of the rank effect to remain.

Next, row 3 shows an equivalent pair of estimates, which randomly allocates students to primary schools within a cohort. Here, students are very unlikely to be assigned to a class with their actual peers. As expected, this generates precise zero effects. Reflecting the ranking of students in different schools has no impact. This also implies that there are no mechanical relationships between rank and later achievement driving these results.

Measurement Error: In addition to peer or teacher effects, the end of primary school test scores may not be a good measure of student ability due to other transitory shocks that generate measurement error. This is especially problematic for our paper, as it generate a mechanical relationship between student rank and gains in test scores. For example, a negative transitory shock to primary test scores would decrease our measure of a student's rank, but we would also see that student having a higher growth rate in achievement between primary and secondary school. Therefore, students with a randomly lower test score will have a lower rank and a higher growth rate, which can generate a downward bias to our rank effect. To gauge the extent of this measurement error issue, we again simulate a data generating process, where we allow for test scores to be a pure and highly noisy measures of ability, with 20 percent of the test score variation caused by random noise (Appendix A.3 and Appendix Table A.2). This shows that normally distributed individual-specific measurement error works against finding any effects. We then show that the inclusion of SSC effects reduces the downward bias, and becomes negligible with the inclusion of individual effects..

5.3 Other Checks: functional form, class size, high-variance classrooms

To address potential remaining concerns, we present three further robustness checks.

First, one may be concerned that the functional form used to account for prior achievement is not sufficiently flexible and not allowing the academic growth rates to differ sufficiently. To address this, we replace the set of cubic controls for prior achievement with a fully flexible specification using a separate indicator for each age 11 test percentile. These results are shown in row 4 of Table 4, where we can see that this does not have a large impact on the rank parameter, reducing it from 7.960 (0.145) to 7.662 (0.145).

Second, the estimates presented have used students attending primary schools with cohort sizes of greater than 10. As the median cohort size of primary schools is 27, we argue that cohorts are effectively equivalent to classes. Given that the maximum class size for 11-year old students is 30. Row 5 presents estimates that restrict the sample to only those students who attended a primary school of less than 31 students. Here, the estimates fall slightly to 6.48, and the standard errors increase.

Third, how much of these effects are driven by the variance of ability within a class, which may make it harder to teach in general? The concern is that high-variance classrooms could generate spurious rank effects because teachers have to decide to teach to specific parts of the distribution, and thereby induce a non-linearity in the mapping of unobserved student ability into student achievement at the end of primary school that we pick up in our rank estimates. While the long

run impact of variance itself is already accounted for by the primary SSC effects, we can include an interaction of the standard deviation of class test scores with each individual rank, to estimate how the impact of rank varies with variance. Row 6 presents estimates of the rank effect predicted at the mean class standard deviations. As may be expected, the class fixed effects estimate falls to 5.71 (0.156), as some of this variation is generating the rank effects. The inclusion of the class standard deviation interactions, however, has a noticeably smaller impact on the within student estimates that fall to only 4.33 (0.12) from 4.56 (0.107). In addition, rank effects are present throughout the variance distribution. At the 25th and 75th percentiles of the classroom standard deviation of test scores, the respective rank effects are 6.29 and 5.34 for the SSC fixed effects specification and 4.27 and 4.39 when additionally accounting for individual fixed effects. The take-away from this exercise is that (1) the rank effects are not generated through differences in variances across classrooms as they remain relatively stable along this dimension, especially in specifications including a student fixed effects, and (2) rank effects are slightly *smaller* in classrooms with a higher variance of peer ability, which is the opposite of the prediction if we are concerned about teachers deciding to teach to specific parts of the ability distribution in high-variance classes and thereby generating the effects.

We believe these additional pieces of evidence on the functional form for controlling for past achievement, class sizes and differences in the variances in achievement distributions across classrooms, we believe that our estimated rank effects are genuine.

6 Mechanisms

A number of different mechanisms may be reacting to rank to produce these results. These include competitiveness, environmental favors to certain ranks, external (parental) investment by task, students learning about their ability, and development of non-cognitive skills. In the following, we discuss how each might coincide with the findings presented so far.

6.1 Mechanism 1: Competitiveness

Recent research has shown that students may have rank concerns during primary school, and that they adjust their efforts accordingly (see [Tincani \(2015\)](#) or [Hopkins and Kornienko \(2004\)](#) for studies of effects of rank concerns). In our setting, if students work harder during primary school because of these concerns, it will be reflected in higher end-of-primary school achievement scores, which we control for. However, if the goal of an individual is to be better than their peers (maximize rank) at minimal cost of effort, this can produce a subset of our results, but not the full pattern.

In particular, students attending primary schools where they face little competition for being at the top of the class will have to spend less effort and, thereby, obtain a lower test score while still remaining at the top. Then, when facing a more competitive secondary school environment, these previously “lazy” students may exert more effort and will appear to have high growth in test scores. This would generate a positive rank effect as those at the top of class have high gains in test scores. However,

if rank concerns during primary school are driving the results, we would only expect to see these effects near the top of the rank distribution, as this only applies to students who exceed their peers and so get a lower than would be expected age 11 test scores due to lowered effort. All those in the remainder of the distribution would be applying effort during primary school to gain a higher rank. Consequently, we would not expect to see an effect for these students. Given the result that the rank effect is seen throughout the rank distribution, it is unlikely that this type of competition mechanism is driving this long-run effect.

It could still be the case that primary school subject rank is positively correlated with the degree of competitiveness of the student. However, in the specification that includes student fixed effects, any general competitiveness of an individual will be accounted for. This competitiveness would need to vary by subject. As previously mentioned, any factor that varies by student across subjects conditional on prior test scores could confound, or in this case, explain the results.

6.2 Mechanism 2: External (parental) investment by task

The parents may react to the academic rank of their child by altering their investment decisions. Parents can assist the child at home with homework or with other extra-curricular activities, or choose a school specializing in a certain subject. If the parents know that their child is ranked highly in one subject, they may have a tendency to encourage the child to do more activities and be more specialized in this subject. Note that as we are controlling for student effects, this will need to be subject specific encouragement, rather than general encouragement pertaining to schoolwork. This mechanism assumes that parents react to achieved primary school rank rather than to prior preferences.³⁰

We test to see if this type of secondary parental sorting generates the results in two ways. First, we remove all students who attended secondary schools labeled specialist in English, math, or science from the sample and re-estimated the effects. This consists of 8 percent of all secondary schools at the age of entry, with 575,000 students in our sample. The removal of these students has a negligible impact on the rank effects (Row 7, Table 4). Parents may not be reacting to the labels, but choosing schools with high actual gains in certain subjects. If the rank effect is due to this type of parental sorting, then additional conditioning on secondary SSC effects may reduce the size of the rank parameter. In the final row of Table 4, we see that these additional controls only have a small impact on the rank effect. These results imply that parents are not reacting to the rank of their child when choosing a secondary school; hence, secondary school effects are not driving the results.

6.3 Mechanism 3: The environment favors certain ranks

Another possible explanation for this finding is that the environment may favor the growth of certain ranks of students. As an example, one can imagine primary school teachers teaching the

³⁰ For this to be the case, it would require parents to want to specialize their child at the age of 11, rather than to improve their child's weakest subject. If parental investment is focused on the weaker subject (Kinsler et al., 2014), this will reverse the rank effect for these students. Instead of parents reacting to rank through increased parental investments, parents may respond by sending their child to a school that specializes in that subject area.

low-ability students if faced with a heterogeneous class group.³¹ If this were the case, teachers may design their classes with the needs of the lowest ranked students in mind. This means that these students can achieve higher age 11 test scores than they otherwise could, but retain their low ranking. Again, if gains in test scores are transitive, this can generate a positive rank effect. When the previously low-ranked students attend secondary school, they would have the least growth in test scores because of their inflated age 11 test scores. This requires primary school teachers to be decreasingly effective with the rank of students.³² However, if the effects are mainly due to the teacher focusing on the low ranks, we will not necessarily expect to see the stark differences by gender or free school meal status. Given these inconsistencies and specificity, we have doubts that this is the dominant reason for the effect, but we cannot exclude this mechanism.

6.4 Mechanism 4: Student Confidence

A simple mechanism of the rank effect is that being highly ranked among peers makes an individual more confident in school generally or in a specific subject. We will first provide evidence that rank, conditional on test scores, increases confidence and follows similar patterns of heterogeneity found with the rank effects. We then try to determine how increased confidence improves student test scores by proposing two channels and testing them against each other.

6.4.1 Survey Results

We link the administrative data to the LSYPE data which contains questions regarding student confidence in each subject at the age of 14, with our main estimation sample. This allows us to test directly if rank position within primary school has a lasting effect on this measure of subject confidence, conditional on attainment. The specifications are equivalent to (1) and (2) with the dependent variable now being student confidence. Since this survey was run for only one cohort, the SSC effects are replaced by school-by-subject effects.

Panel A of Table 5 presents these results and demonstrates that conditional on age 11 test scores, students with a higher primary school rank position are significantly more likely to say that they are good in that subject at the 10 percent level (column 1). The impact of moving from the bottom of the class to the top is 0.196 points on a five-point scale (20 percent of standard deviation). This suggests that students develop a lasting sense of strengths and weaknesses depending on their local rank position, conditional on relative test scores.

The LSYPE survey was conducted on a sample of students from 24 percent of all secondary schools immediately prior to the age 14 tests. As many primary schools feed students into each secondary school, there are only very few students per primary school in this survey (4.5 students,

³¹ We have run estimations controlling for the within school-subject-cohort variance to take into account that high variance classes may be more difficult to teach. However, these cannot include SSC or student effects, and thus the estimates should not be cleanly interpreted as ordinal rank affects. Therefore these specifications only allowed for general school effects or no school effects. The inclusion of a school-subject-cohort variance into these specifications does not significantly alter the rank parameter. Our findings can be presented upon request.

³² Note if primary teachers taught to the median student, those at both extremes would lose out. So instead of a linear effect, we would find a U-shaped curve with both students at the bottom and the top of the distribution gaining relatively more during secondary school.

conditional on at least one student being in the survey). This severely limits the degrees of freedom in each primary school-subject group. To obtain a clearer view of the effect of rank on confidence, we also estimate how rank based on age 14 test scores within a secondary school-subject affects contemporaneous subject confidence conditional on achievement. This provides us with an average of 20 surveyed students per school.³³

In Panel B, where we see that conditional on secondary school-subject effects, moving from the bottom to the top of the class improves confidence by 0.43. Allowing individuals to have different general levels of confidence, which we control for with the student fixed effect, reduces the parameter to 0.38, but it still remains significant at 1 percent (column 3). As the students had not taken their age 14 exams at the time of the survey, this shows that individual's confidence can be influenced by rank without being told their test scores. These rankings, derived from the age 14 test scores, are representative of the students' interactions with their peers throughout their secondary school activities.

We examine the heterogeneity of these effects by estimating the effect of age 14 rank on confidence separately by gender (Panel C). We find that the effect on male confidence is five times larger than the effect on females ($\hat{\beta}_{rankmale} = 0.61$, $\hat{\beta}_{rankfemale} = 0.12$), which mirrors the results we find for the effect of rank on later test scores. Unfortunately, due to the smaller sample size of the LSYPE, we are unable to produce the effects non-linearly or by FSME status.

The effects of secondary school rank on confidence are large and we could expect the contemporaneous effect of primary rank on confidence at age 11 to be even larger, as academic confidence is thought to be more malleable at this age (Tidemann, 2000; Rubie-Davies, 2012; Leflot et al., 2010). Overall, given the effects of rank on the direct measures of student confidence and the heterogeneity of effects found in the main results, it seems likely that non-cognitive skills matter.

6.4.2 Learning or Cost-Shifting?

We conclude the paper by testing how this increased confidence improves test scores by comparing two channels. The first is that students use their ordinal class ranking to learn about their own subject specific abilities. Are they relatively more productive in math or English? This is similar to the model proposed by Ertac (2006). Students then use this information when making effort investment decisions across subjects. Critically, this mechanism does not change an individual's education production function, only their perception of it. We will argue below that this feature allows us to test the learning model.

The second is that a student's ordinal ranking during primary school has an impact on their confidence in a subject, or overall academically. In the educational literature, this is known as the Big-Fish-Little-Pond-Effect, and it has been found to occur in many different countries and institutional settings (see Marsh et al. (2008) for a review).³⁴ This confidence can differ over tasks, so

³³The reason why we do not look at the effect of KS3 age 14 rank on later outcomes is due to the tracking by subject in secondary school, which will be related to rank. This is not an issue with primary school rank, because even if there were tracking in primary schools, when moving to secondary school, students with the same test scores (but different primary ranks) would be assigned to the same track.

³⁴The psychological-education literature uses the term self-concept, which is formed through our interactions with the environment and peers O'Mara et al. (2006). Individuals can have positive or negative self-concept about different

a student can consider themselves good in English, but still bad in math (Marsh et al., 1988; Yeung and Lee, 1999). Confidence then generates non-cognitive skills in a subject such as confidence, resilience, and perseverance Valentine et al. (2004). The importance of such non-cognitive skills for both academic attainment and non-academic attainment is now well established (Borghans et al., 2008; Lindqvist and Vestman, 2011; Heckman and Rubinstein, 2001). We propose to model these increased non-cognitive skills as a decrease in the costs of effort for that task.³⁵

A basic behavioral model is provided in Appendix A.4, where students want to maximize total grades for a given total cost of effort, and where they have differential abilities and cost of effort for each subject. Students who have a high rank in a subject during primary school have a lower cost of effort in that subject in secondary school. This shifts the students' isocost line out along the axis for this subject. Consequently, they are able to reach higher isoquant, and will optimally invest more effort in that subject (Appendix Figure A.1, Panel A). If there are any general gains in confidence that can reduce the cost of any academic effort and cause a parallel shift out of the isocost line, more effort would be allocated to all subjects. Note that with this channel, it will be impossible for students to misallocate effort across subjects, as they are perfectly informed about their costs and abilities.

Under the learning hypothesis, students use local rank information to make effort investment decisions across subjects. Assuming that students want to maximize grades for minimum cost of effort, they would be applying more effort to subjects where their perceived ability is highest.³⁶ However, students with larger differences between local and national percentile ranks (in absolute terms) would have more distorted information about their true abilities, assuming national test scores are a good measure of ability. These students would then be more likely to misallocate effort across subjects, thereby achieving lower average grades compared to students whose local ranks happen to closely align with national ranks. Conversely, if the rank effects were caused by actual changes in the costs associated with the education production function, even if local rank is different from national rank, this would not lead to a misallocation of effort in terms of maximizing grades.

We do not have direct data on perceptions versus reality of costs. However, we can test for misallocation of effort by examining how average grades achieved are correlated with the degree of 'misinformation'. More precisely, we compute a measure of the degree of misinformation for students in each subject using their local rank $R_{ijsc}^{t-1} \in \{0, 1\}$ and national percentile rank $Y_{ijsc}^{t-1} \in \{0, 100\}$ at age 11. Both are uniformly distributed and, therefore we simply define the degree of misinformation Mis_{ijsc}^{t-1} as the absolute difference between the two after re-scaling percentile rank:

$$Mis_{ijsc}^{t-1} = |R_{ijsc}^{t-1} - \frac{Y_{ijsc}^{t-1}}{100}| \text{ where } Mis \in \{0, 1\} \quad (5)$$

aspects of themselves and students with a high self-concept would also develop positive non-cognitive skills..

³⁵ Confidence may instead affect a student's ability in a task rather than cost of effort. This would lead to the same predicted changes in the effort ratios and empirical results. If we had time use-data we would be able to differentiate between these causes, however given the data available, we are unable to determine if it is costs or abilities that are affected.

³⁶ Assuming diminishing returns to effort in each subject so that all effort isn't allocated into just one subject.

This measure takes the value zero for students where their local rank happens to correspond exactly to the national rank. A large value, on the other hand, indicates large differences between local and national rank. Averaging this metric across subjects within student provides a mean indicator of the degree of misinformation for each student. To test directly whether or not a student with a large amount of disinformation does significantly worse, we use a specification similar to specification 1 but with the by subject variation removed, as we are examining the effect on average test scores. We estimate the following specification:

$$\bar{Y}_{ijc}^{t-1} = \beta_{Rank} \bar{R}_{ijc}^{t-1} + f(Y_{ijc}^{t-1}) + \beta_{Mis} \bar{M}is_{ijc}^{t-1} + \mathbf{x}'_i \boldsymbol{\beta} + \mathbf{j}'_{jc} \boldsymbol{\varphi} + \eta_{ijc} \quad (6)$$

where

$$\eta_{ijc} = \tau_i + v_{ijc}$$

Here, \bar{Y}_{ijct} denotes the average test scores across subjects in period t , \bar{R}_{ijc}^{t-1} is average rank in primary school, $\bar{M}is$ the additional misinformation variable, \mathbf{x}_i a vector of individual characteristics and \mathbf{j}_{jc} primary school-cohort fixed effects. If the amount of misinformation causes them to misallocate effort over subjects we expect $\beta_{Mis} < 0$. Alternatively, the null hypothesis that local rank causes changes to the actual production function means $\beta_{Mis} = 0$.

$$H_1 : \text{Learning } \beta_{Mis} < 0$$

$$H_0 : \text{Null } \beta_{Mis} = 0$$

We obtain the following estimates using our full sample of 2,271,999 students. For benchmarking purposes, we first estimate a version of specification 6 without the additional misinformation variable (Table 6). The effect of average rank on average test score is estimated at 13.1, and is highly statistically significant. Column (2) adds the coefficient for the effect of misinformation, which is estimated to be small negative and statistically insignificant while the rank parameter remains almost unchanged. Given this specification, we fail to reject the null hypothesis that the amount of misinformation does not cause students to misallocate effort. Therefore, we conclude that the learning mechanism alone is unlikely to generate our results, though we fully acknowledge the limitations of this test.

Given this finding, we turn back to re-interpret the main results. The cost of effort model is consistent with the empirical results found in Section 4. The smaller estimates from the pupil fixed effects specification (2) will have general confidence effects absorbed and, therefore, will only be picking up the effect of within student reallocation of effort across subjects. Specification 1, which only contains SSC effects, implicitly allows spillovers between subjects within a student. It can be interpreted as a general spillover of confidence effects across subjects, and are, therefore, accordingly larger (Appendix Figure A.1, Panel C).

We have also seen that males are more affected by rank throughout 95 percent of the rank distribution, gaining significantly more from being at the top of the class, and losing out marginally more from being in the bottom half. The coefficient can be interpreted as a specializing term,

implying that the prior rank has a stronger specializing effect on males than females. The stronger positive effects for males may also be caused by them perceiving themselves as higher ranked than they actually are. We are estimating the effect of perceived rank using information on the actual rank, as already discussed at the end of Section 3.3.

Finally, the third panel in Figure 4 shows that FSME students are less negatively affected by rank, and more positively affected than Non-FSME students. One potential interpretation of this is that these students already have low confidence in their abilities and so do not suffer from being lowly ranked. Moreover, the shallower gradient for Non-FSME students may also lead to an interpretation that they are less affected by class rank, as these students may have their academic confidence being more affected by factors outside of school.

7 Conclusion

This paper established a new result, showing that rank position within primary school has significant effects on secondary school achievement and the likelihood of completing STEM subjects at the end of secondary school. There is significant heterogeneity in the effect of rank, with males being influenced considerably more. Moreover, a higher rank seems also linked to important non-cognitive skills, such as confidence.

What are the policy implications of these findings? With specific regard to education, these findings leads to a natural question for a parent deciding on where to send their child (in partial equilibrium). Should my child attend a “prestigious school” or a “worse school” where she will have a higher rank? Rank is just one of the many factors in the education production function. Therefore, choosing solely on the basis of rank is unlikely to be a correct decision.

To gauge the relative importance of rank for choosing the school, we make some comparisons of effect sizes found by the literature. The authors are currently not aware of any study that identifies the effectiveness of schools in terms of standard deviations³⁷; therefore, we use estimates on the impact of teachers as an indicative measure for the effects of school quality for this benchmarking exercise. A teacher who is one standard deviation better than average improves student test scores by 0.1 to 0.2 standard deviations (Aaronson et al., 2007; Rivkin et al., 2005). Comparatively, we find that a student with one standard deviation higher rank in primary school will score 0.08 standard deviations better at age 14.³⁸

Our results also show that primary school rankings affect A-Level subject choices and so have long run implications as A-Level choices are linked directly to university admissions. Of course, if males and females, on average, had the same primary rankings across all subjects, it would be less clear why our findings might matter for subject choices and access to STEM degrees. However, the average primary boy’s (girl’s) rank in English is 0.440 (0.535), in math 0.515 (0.468), and in science 0.477 (0.474). Taking absolute differences in combined math and science versus English ranks, boys

³⁷ Evaluations of school effectiveness using admission lotteries (e.g. Abdulkadiroglu et al., 2011; Angrist et al., 2010; Fryer and Dobbie, 2011) are comparing effectiveness between types rather than the whole distribution of effectiveness.

³⁸ Note that these are still not directly comparable because the effect of the teacher is annual and fades out, whereas the rank treatment lasts the duration of primary school (5 years) and the effect is found three years later.

have a 0.166 higher rank in STEM subjects than girls. This difference would mean that males are about 0.66 percentage points more likely choose a STEM A-Levels, conditional on achievement. Given the low share of students taking STEM subjects, if we were to equalize the primary rankings in subjects across genders, this would reduce the total STEM gender gap by about seven percent. One direct way for this equalization of rankings across subjects to be achieved would be to have primary schools separated by gender, ensuring there will be same amount of females and males being on top of the class in STEM and English.

Further policy recommendations would hinge on the precise channels through that the rank effects operate, which future research should examine in more detail. If rank effects for example operate through non-cognitive skills, there would be general implications for productivity and informational transparency. To improve productivity, it would be optimal for managers or teachers to highlight an individual's local rank position if that individual has a high local rank. If an individual is in a high-performing peer group and therefore may have a low local rank but high global rank, a manager should make the global rank more salient. For individuals who have low global and local ranks, managers should focus on absolute attainment, or focus on other tasks where the individual has higher ranks.

Besides policy implications, our findings also help to reconcile a number of topics in education.

These persistent rank effects could partly speak towards why some achievement gaps increase over the education cycle. Widening education gaps have been documented by race (Fryer and Levitt, 2006; Hanushek and Rivkin, 2006, 2009). With rank effects small differences in early overall attainment could negatively affect general academic confidence, which would lead to decreased investment in education and exacerbate any initial differences. A similar argument could be made for the persistence of relative age-effects, which show that older children continue better compared to their younger counterparts (Black et al., 2011). In the case of gender, a gap occurs by subject, where males are overly represented in STEM subjects by the age of 18, despite girls outperforming boys at early ages in these subjects but in particular in languages (Burgess et al., 2004; Machin and McNally, 2005).

Finally, research on selective schools and school integration have 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). Many of these papers use a regression discontinuity design to compare the outcomes of the students that just passed the entrance exam to those that just failed. The common puzzle is that many papers find no benefit from attending these selective schools.³⁹ However, our findings would speak to why the potential benefits of prestigious schools may be attenuated through the development of a fall in confidence among these marginal/bussed students, who are also necessarily the low-ranked students. This is consistent with Cullen et al. (2006), 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" (page 1194).

³⁹ Similar effects are found in the Higher Education literature with respect to affirmative action policies (Arcidiacono et al., 2012; Robles and Krishna, 2012).

References

- Aaronson, D., Barrow, L., and Sander, W. (2007). Teachers and Student Achievement in the Chicago Public High Schools. *Journal of Labor Economics*, 25:95–135.
- Abdulkadiroglu, A., Angrist, J. D., Dynarski, S. M., Kane, T. J., and Pathak, P. A. (2011). Accountability and Flexibility in Public Schools: Evidence from Boston's Charters And Pilots. *Quarterly Journal of Economics*, 126(2):699–748.
- Anderson, C., Srivastava, S., Beer, J. S., Spataro, S. E., and Chatman, J. A. (2006). Knowing your place: Self-perceptions of status in face-to-face groups. *Journal of Personality and Social Psychology*, 91(6):1094–1110.
- Angrist, J. D., Dynarski, S. M., Kane, T. J., Pathak, P. A., and Walters, C. R. (2010). Inputs and impacts in charter schools: Kipp lynn. *American Economic Review*, 100(2):239–43.
- Angrist, J. D. and Lang, K. (2004). Does School Integration Generate Peer Effects? Evidence from Boston's Metco Program. *American Economic Review*, 94(5):1613–1634.
- Arcidiacono, P., Aucejo, E., and Spenner, K. (2012). What happens after enrollment? an analysis of the time path of racial differences in gpa and major choice. *IZA Journal of Labor Economics*, 1(1):5.
- Azmat, G., Bagues, M., Cabrales, A., and Iriberry, N. (2015). What You Know Can't Hurt You, For Long: A Field Experiment on Relative Performance Feedback. Mimeo, Queen Mary University.
- Azmat, G. and Iriberry, N. (2010). The importance of relative performance feedback information: Evidence from a natural experiment using high school students. *Journal of Public Economics*, 94(7-8):435–452.
- Bandiera, O., Larcinese, V., and Rasul, I. (2015). Blissful Ignorance? A Natural Experiment on the Effect of Feedback on Students' Performance. *Labour Economics*, 34:13–25.
- Bertrand, M., Goldin, C., and Katz, L. F. (2010). Dynamics of the gender gap for young professionals in the financial and corporate sectors. *American Economic Journal: Applied Economics*, 2(3):228–55.
- Black, S. E., Devereux, P. J., and Salvanes, K. G. (2011). Too Young to Leave the Nest? The Effects of School Starting Age. *The Review of Economics and Statistics*, 93(2):455–467.
- Borghans, L., Duckworth, A. L., Heckman, J., and ter Weel, B. (2008). The economics and psychology of personality traits. *Journal of Human Resources*, 43(4).
- Brown, G., Gardiner, J., Oswald, A., and Qian, J. (2008). Does wage rank affect employees well-being? *Industrial Relations: A Journal of Economy and Society*, 47(3):355–389.
- Brown, J. (2011). Quitters Never Win: The (Adverse) Incentive Effects of Competing with Superstars. *Journal of Political Economy*, 119(5):982 – 1013.
- Burgess, S., McConnell, B., Propper, C., and Wilson, D. (2004). Girls Rock, Boys Roll An Analysis of the Age 14-16 Gender Gap in English Schools. *Scottish Journal of Political Economy*, 51(2):209–229.
- Bursztyjn, L. and Jensen, R. (2015). How Does Peer Pressure Affect Educational Investments? *The Quarterly Journal of Economics*, 130(3):1329–1367.
- Card, D., Mas, A., Moretti, E., and Saez, E. (2012). Inequality at work: The effect of peer salaries on job satisfaction. *American Economic Review*, 102(6):2981–3003.

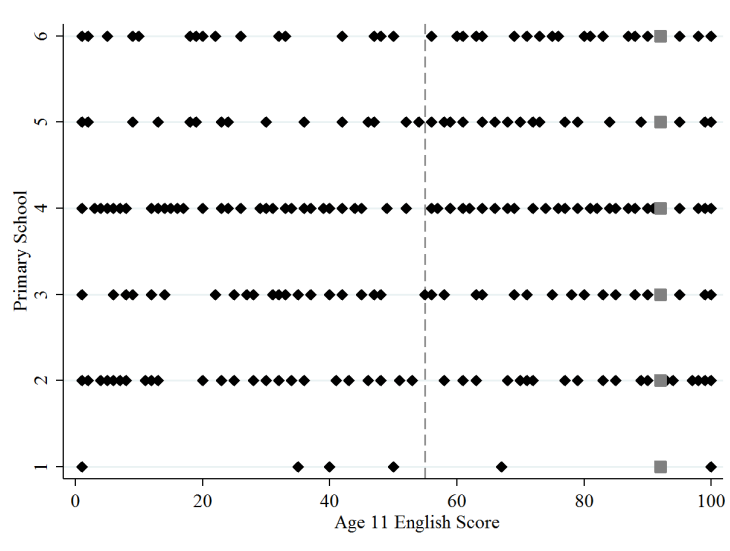
- Carrell, S., Fullerton, R. L., and West, J. (2009). Does your cohort matter? measuring peer effects in college achievement. *Journal of Labor Economics*, 27(3):439–464.
- Carrell, S. E., Sacerdote, B. I., and West, J. E. (2013). From Natural Variation to Optimal Policy? The Importance of Endogenous Peer Group Formation. *Econometrica*, 81(3):855–882.
- Clark, D. (2010). Selective schools and academic achievement. *The B.E. Journal of Economic Analysis & Policy*, 10(1):1–40.
- Cullen, J. B., Jacob, B. A., and Levitt, S. (2006). The Effect of School Choice on Participants: Evidence from Randomized Lotteries. *Econometrica*, 74(5):1191–1230.
- Ertac, S. (2006). Social Comparisons and Optimal Information Revelation: Theory and Experiments. Mimeo, University of California Los Angeles.
- Falck, O., Gold, R., and Heblich, S. (2011). Class Size and Education in England. Research Report DFE-RR169, Research Report.
- Falk, A. and Ichino, A. (2006). Clean Evidence on Peer Effects. *Journal of Labor Economics*, 24(1):39–58.
- Festinger, L. (1954). A theory of social comparison processes. *Human Relations*, 7:117–140.
- Fryer, R. and Dobbie, W. (2011). Are High-Quality Schools Enough to Increase Achievement Among the Poor? Evidence from the Harlem Children’s Zone. *American Economic Journal: Applied Economics*, 3.
- Fryer, R. G. and Levitt, S. D. (2006). The Black-White Test Score Gap Through Third Grade. *American Law and Economics Review*, 8(2):249–281.
- Genakos, C. and Pagliero, M. (2012). Interim Rank, Risk Taking, and Performance in Dynamic Tournaments. *Journal of Political Economy*, 120(4):782 – 813.
- Guiso, L., Monte, F., Sapienza, P., and Zingales, L. (2008). Culture, gender, and math. *Science*, 320(5880):1164–1165.
- Hanushek, E. A. and Rivkin, S. G. (2006). School Quality and the Black-White Achievement Gap. NBER Working Papers 12651, National Bureau of Economic Research, Inc.
- Hanushek, E. A. and Rivkin, S. G. (2009). Harming the best: How schools affect the black-white achievement gap. *Journal of Policy Analysis and Management*, 28(3):366–393.
- Heckman, J. J. and Rubinstein, Y. (2001). The Importance of Noncognitive Skills: Lessons from the GED Testing Program. *American Economic Review*, 91(2):145–149.
- Hopkins, E. and Kornienko, T. (2004). Running to keep in the same place: consumer choice as a game of status. *American Economic Review*, 91(2):1085–1107.
- Hoxby, C. and Weingarth, G. (2005). Taking race out of the equation: school reassignment and the structure of peer effects. Technical report, Department of Economics, Harvard University.
- i Vidal, J. B. and Nossol, M. (2011). Tournaments without prizes: Evidence from personnel records. *Management Science*, 57(10):1721–1736.
- Joensen, J. S. and Nielsen, H. S. (2009). Is there a causal effect of high school math on labor market outcomes? *Journal of Human Resources*, 44(1):171–198.

- Kinsler, J., Pavan, R., and DiSalvo, R. (2014). Distorted Beliefs and Parental Investment in Children. Mimeo, SOLE-JOLE 2015.
- Kling, J. R., Liebman, J. B., and Katz, L. F. (2007). Experimental Analysis of Neighborhood Effects. *Econometrica*, 75(1):83–119.
- Kremer, M. and Levy, D. (2008). Peer effects and alcohol use among college students. *The Journal of Economic Perspectives*, 22(3):189–206.
- Lavy, V., Silva, O., and Weinhardt, F. (2012). The Good, the Bad, and the Average: Evidence on Ability Peer Effects in Schools. *Journal of Labor Economics*, 30(2):367–414.
- Leflot, G., Onghena, P., and Colpin, H. (2010). Teacher-child interactions: Relations with children's self-concept in second grade. *Infant and Child Development*, 19(4):385–405.
- Lindqvist, E. and Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: Evidence from the Swedish enlistment. *American Economic Journal: Applied Economics*, 3(1):101–128.
- Luttmer, E. F. P. (2005). Neighbors as negatives: Relative earnings and well-being. *The Quarterly Journal of Economics*, 120(3):963–1002.
- Machin, S. and McNally, S. (2005). Gender and Student Achievement in English Schools. *Oxford Review of Economic Policy*, 21(3):357–372.
- Marsh, H., Seaton, M., Trautwein, U., Ludtke, O., Hau, O'Mara, A., and Craven, R. (2008). The Big Fish Little Pond Effect Stands Up to Critical Scrutiny: Implications for Theory, Methodology, and Future Research. *Educational Psychology Review*, 20(3):319–350.
- Marsh, H. W., Byrne, B. M., and Shavelson, R. J. (1988). A multifaceted academic self-concept: Its hierarchical structure and its relation to academic achievement. *Journal of Educational Psychology*, 80(3):366–380.
- Nickell, S. (1981). Teachers, Schools, and Academic Achievement. *Biases in Dynamic Models with Fixed Effects*, 49(6):1417–1426.
- O'Mara, A., Green, J., and Marsh, H. (2006). Administering self-concept interventions in schools: No training necessary? A meta-analysis. *International Education Journal*, 7:524–533.
- Rivkin, S. G., Hanushek, E. A., and Kain, J. F. (2005). Teachers, Schools, and Academic Achievement. *Econometrica*, 73(2):417–458.
- Robles, V. C. F. and Krishna, K. (2012). Affirmative Action in Higher Education in India: Targeting, Catch Up, and Mismatch. NBER Working Papers 17727, National Bureau of Economic Research.
- Rubie-Davies, C. (2012). Teacher Expectations and Student Self-Perceptions: Exploring Relationships. *Psychology in the Schools*, 43(5):537–552.
- Sacerdote, B. (2001). Peer effects with random assignment: Results for Dartmouth roommates. *Quarterly Journal of Economics*, 116(2):681–704.
- Sacerdote, B. (2011). Peer effects in education: how might they work, how big are they and how much do we know thus far. In Hanushek, E., Machin, S., and L., W., editors, *Handbook of the Economics of Education Volume 3*. Elsevier, Amsterdam.

- Tidemann, J. (2000). Parents' gender stereotypes and teachers' beliefs as predictors of children's concept of their mathematical ability in elementary school. *Journal of Educational Psychology*, 92(1):144–151.
- Tincani, M. M. (2015). Heterogeneous peer effects and rank concerns: Theory and evidence. Mimeo, University College London.
- Tversky, A. and Kahneman, D. (1974). Judgment under uncertainty: Heuristics and biases. *Science*, 185(4157):1124–1131.
- Valentine, J. C., DuBois, D. L., and Cooper, H. (2004). The relation between self-beliefs and academic achievement: A meta-analytic review. *Educational Psychologist*, 39(2):111–133.
- Whitmore, D. (2005). Resource and peer impacts on girls' academic achievement: Evidence from a randomized experiment. *American Economic Review*, P & P:199–203.
- Yeung, A. S. and Lee, F. L. (1999). Self-concept of high school students in china: Confirmatory factor analysis of longitudinal data. *Educational and Psychological Measurement*, 59(3):431–450.

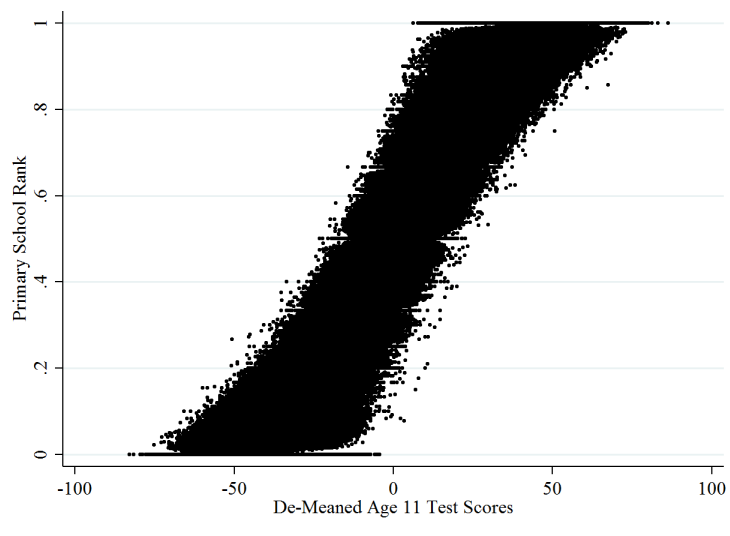
Figures

Figure 2: Test Score Distributions Across Similar Classes



Notes: Each diamond represents a student score and gray squares indicate all students who scored 92. The Y-axis represents six primary school English classes which all have a student scoring the minimum, maximum and have a mean test score of 55 (as indicated by the dashed grey line). Given the different test score distributions each student scoring 92 has a different rank. This rank is increasing from School 1 through to School 6 with ranks of 0.83, 0.84, 0.89, 0.90, 0.925 and 0.94 respectively, despite all students having the same absolute and relative to the class mean test score. Note that individual test scores have been randomly altered enough to ensure anonymity of individuals and schools. This is for illustrative purposes only and in no way affects the interpretation of these figures.

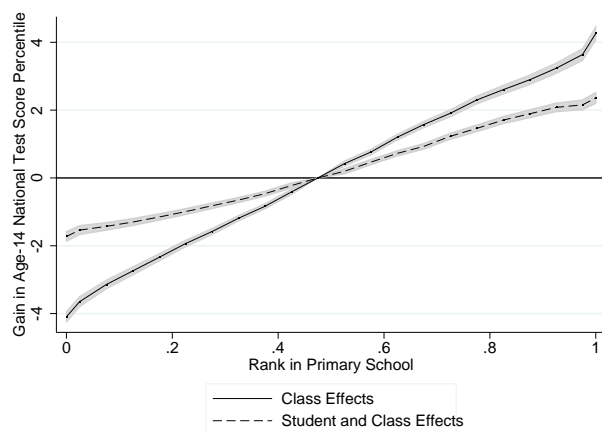
Figure 3: Rank Distributions Across School-Subject-Cohorts Subjects



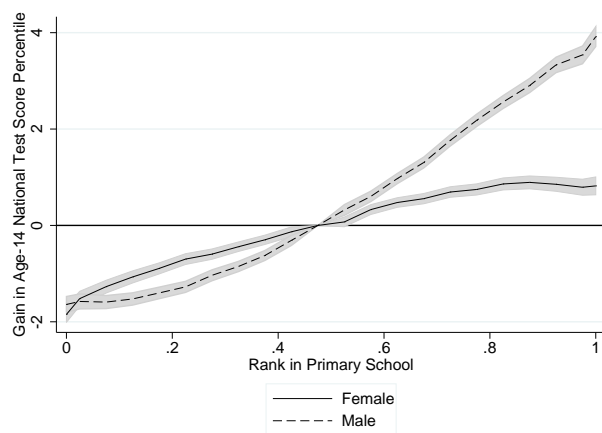
Notes: The Y-axis is the primary rank of students and the X-axis shows the de-meaned test scores by primary school-subject-cohort. Note that individual test scores have been randomly altered enough to ensure anonymity of individuals and schools. This is for illustrative purposes only and in no way affects the interpretation of these figures.

Figure 4: Impact of Primary Rank on Age 14 Test Scores

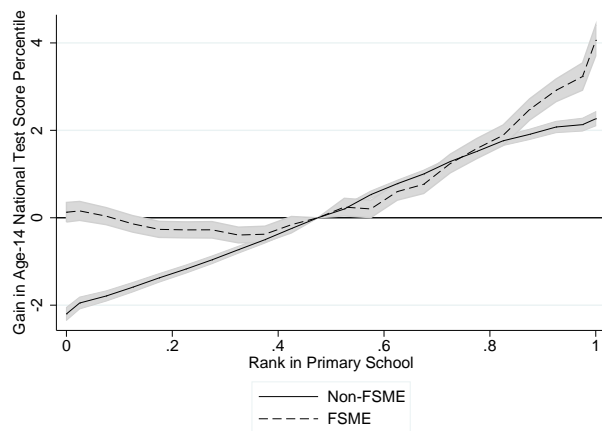
Figure 4.1: Non-Parametric Estimation



4.2: Non-Linear Effects by Gender



4.3 Non-Linear Effects by Free School Meal Eligibility



Notes: FSME stands for Free School Meal Eligible student. Effects obtained from estimating the effect of rank on Non-FSME (Female) students and the interaction term with FSME (Male) students. Non-linear effect with dummies for the vingtiles of rank plus a dummy for being top or bottom of school-subject-cohort. All estimates use subject specific rank and test score across three subjects and condition on Primary-subject-cohort group effects and student effects. Results are discussed in Section 4.4 in the text. Shaded area represent 95% confidence intervals.

Tables

Table 1: Descriptive Statistics of the Main Estimation Sample

	Mean	S.D.	Min	Max
Panel A: Student Test Scores				
<i>Age 11 Test Scores</i>				
English	50.285	28.027	1	100
Maths	50.515	28.189	1	100
Science	50.005	28.026	1	100
<i>Rank Characteristics</i>				
English	0.488	0.296	0	1
Maths	0.491	0.296	0	1
Science	0.485	0.295	0	1
Within Student Rank SD	0.138	0.087	0	0.577
<i>Age 14 Test Scores</i>				
English	51.233	28.175	1	100
Maths	52.888	27.545	1	100
Science	52.908	27.525	1	100
<i>Age 16 Test Scores</i>				
English	41.783	26.724	1	94
Maths	43.074	27.014	1	96
Science	41.807	26.855	1	94
<i>Age 18 Subjects Completed</i>				
English	0.123	0.328	0	1
Maths	0.084	0.277	0	1
Science	0.108	0.31	0	1
Panel B: Student Background Characteristics				
SEN	0.175	0.38	0	1
FSME	0.146	0.353	0	1
Male	0.499	0.5	0	1
Minority	0.163	0.37	0	1
LSYPE Sample				
<i>How good do you think you are at...</i>				
English	0.922	0.931	-2	2
Maths	0.937	0.92	-2	2
Science	0.896	1.012	-2	2
<i>Age 11 Test Scores</i>				
English	49.475	27.769	1	100
Maths	50.106	28.367	1	100
Science	48.685	28.296	1	100
<i>Age 14 Test Scores</i>				
English	50.671	27.999	1	100
Maths	52.992	27.608	1	100
Science	52.206	27.706	1	100
<i>Background Characteristics</i>				
SEN	0.166	0.372	0	1
FSME	0.186	0.389	0	1
Male	0.498	0.5	0	1
Minority	0.349	0.477	0	1

Notes: 6,815,997 student-subject observations over 5 cohorts. Cohort 1 takes Key Stage 2 (KS2) examinations in 2001 and Key Stage 3 (KS3) examinations in 2004, Key Stage 4 (KS4) in 2006 and A-Levels at age-18 in 2008. Test scores are percentalized tests scores by cohort-subject and come from national exams which are externally marked. Age-16 test scores mark the end of compulsory education. Age-18 information could be merged for a subsample of 5,147,193 observations from cohorts 2 to 5. The LSYPE sample consists of 34,674 observations from the cohort 1 who took KS2 in 2001 and KS3 in 2004. For a detailed description of the data see Section 3.

Table 2: Rank effects on Student Outcomes

Outcome	Age 14 Test Scores			Age 16 Test Scores			Complete Subject Age 18		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Primary Rank	7.946**	7.960**	4.562**	6.468**	6.426**	3.129**	0.041**	0.039**	0.039**
	<i>0.147</i>	<i>0.145</i>	<i>0.107</i>	<i>0.154</i>	<i>0.152</i>	<i>0.107</i>	<i>0.002</i>	<i>0.002</i>	<i>0.002</i>
Male		-1.007	..		-2.246**	..		-0.011**	..
		0.045	..		0.056	..		0.001	..
FSME		-2.962**	..		-4.230**	..		-0.034**	..
		<i>0.030</i>	..		<i>0.041</i>	..		<i>0.000</i>	..
SEN		-4.401**	..		-3.383**	..		-0.007**	..
		0.033	..		0.036	..		0.000	..
Minority		1.874**	..		4.274**	..		0.050**	..
		<i>0.052</i>	..		<i>0.082</i>	..		<i>0.001</i>	..
Cubic in Age 11 Test Scores	✓	✓	✓	✓	✓	✓	✓	✓	✓
Primary SSC Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
Secondary School Effects			Abs.			Abs.			Abs.
Student Effects			✓			✓			✓

Notes: Results obtained from nine separate regressions based on 2,271,999 student observations and 6,815,997 student-subject observations. In columns 1-3 the dependent variable is by cohort by subject percentalized KS3 test scores. In columns 4-6 the dependent variable is by cohort by subject percentalized KS4 test scores. In columns 7-9 the dependent variable is if the student completed an A-Level at age 18 in the corresponding subject. SSC effects are fixed effects for each school-by-subject-by-cohort combination. Coefficients in columns 3, 6 and 9 are estimated using Stata command `reghdfe` allowing two high dimensional fixed effects to be absorbed. Standard errors in italics and clustered at 3,800 secondary schools. "Abs." indicates that the effect is absorbed by another estimated effect. ** 1% sig.

Table 3: Balancing by Parental Occupation

	Primary	Primary-Student
	(1)	(2)
<i>Panel A: Effects on age-11 tests</i>		
Parental Occupation	7.722**	1.706*
	<i>0.840</i>	<i>0.783</i>
<i>Panel B: Effects on Ordinal Rank</i>		
Parental Occupation	-0.004	0.000
	<i>0.005</i>	<i>0.034</i>
Primary school-by-subject Effects	✓	✓
Student Effects		✓

Notes: Results obtained from regressions based on 31,050 subject-student observations for which parental occupations could be identified from the LSYPE. Detailed occupational coding available from the authors on request. Panel A has KS2 as dependent variable, in Panel B KS2 with polynomials up to cubic are included as controls. All regressions control for student characteristics and subject effects. Regressions in column (2) estimated using Stata command `reghdfe`. ** 1%, * 5% significant.

Table 4: Robustness

		Effect of Primary Rank	
		(1)	(2)
(1)	Main Specifications - Benchmark	7.960** (0.145)	4.562** (0.107)
(2)	Randomised Cohort Within School	2.132** (0.154)	0.869** (0.140)
(3)	Randomised School Within Cohort	-0.099 (0.130)	-0.227 (0.150)
(4)	Fully Flexible Age 11 Test Scores	7.662** (0.145)	4.402** (0.132)
(5)	Small Primary Schools (Single Class) Only	6.482** (0.176)	3.596** (0.144)
(6)	Accounting for Primary Class Variance	5.718** (0.156)	4.337** (0.118)
(7)	Excluding Specialist Secondary Schools	7.922** (0.155)	4.586** (0.112)
(8)	Accounting for Secondary-Cohort Subject FX	7.901** 0.146	4.470** (0.107)
Student characteristics		✓	Abs
Age 11 Test Scores		✓	✓
Primary school-by-subject-by-cohort Effects (SSC Effects)		✓	✓
Student Effects			✓

Notes: This table is discussed in Section 5 (row 1-6) and in Section 6.2 (row 7 and 8). Results obtained from 16 separate regressions. Rows 1, 2, 3, 4, 6 and 8 use the main sample of 6,815,997 student-subject observations. Row 5 uses a reduced sample of 2,041,902 student-subject observations who attended primary schools with cohort sizes of less than 31. Row 7 uses a reduced sample of 6,235,806 student-subject observations who did not attend a secondary school classified as specialist. The dependent variable is by cohort by subject percentalized KS3 test scores. Student characteristics are ethnicity, gender, free school meal (FSME) and special educational needs (SEN). SSC effects are fixed effects for each school-by-subject-by-cohort combination. Coefficients are estimated using Stata command `reghdfe` allowing two high dimensional fixed effects to be absorbed. Standard errors in italics and clustered at the secondary school level. ** 1% significance levels.

Table 5: Student Confidence on Rank

	(1)	(2)
<i>Panel A: Subject Confidence on Age 11 Test Scores</i>		
Primary Rank	0.196 <i>0.117</i>	0.056 <i>0.18</i>
<i>Panel B: Subject Confidence on Age 14 Test scores</i>		
Secondary Rank	0.427** <i>0.099</i>	0.382** <i>0.155</i>
<i>Panel C: Subject Confidence on Age 14 Test scores by Gender</i>		
Secondary Rank – Male Students	0.530** <i>0.126</i>	0.606** <i>0.206</i>
Secondary Rank – Female Students	0.317* <i>0.166</i>	0.115 <i>0.233</i>
School-by-subject effects	✓	✓
Student Effects		✓

Notes: Results obtained from eight separate regressions based on 11,558 student observations and 34,674 student-subject observations from the LSYPE sample (17,415 female, 17,259 male). For descriptives, see Table 1. The dependent variable is a course measure of confidence by subject. All specifications in columns 1 control for observable student characteristics, these are absorbed by the student effect in column 3. Student characteristics are ethnicity, gender, free school meal (FSME) and special educational needs (SEN). Panel A conditions on age 11 test scores (cubic) and primary school by subject effects. Panels B and C condition on age 14 test scores (cubic) and secondary school by subject effects. Cohort effects are not included because the LSYPE data is only available for one cohort. Standard errors in parenthesis and clustered at 796 secondary schools ** 1% sig, * 5% sig.

Table 6: Is the Degree of Misinformation Harmful?

	Raw (1)	Primary (2)
Primary Rank	13.088** <i>0.269</i>	13.072** <i>0.269</i>
Misinformation	- -	-0.512 <i>0.312</i>
Student characteristics	✓	✓
Age 11 Test Scores (cubic)	✓	✓
Primary-cohort Effects	✓	✓

Notes: Results obtained from two separate regressions based on 2,271,999 student observations averaged over subjects where column (2) includes an additional explanatory variable on misinformation. The dependent variable is by cohort by subject percentalized average KS3 test scores. Rank is the average rank across English, Maths and Science in primary school. The misinformation measurement is the average absolute difference between local rank and national percentile rank for each student in the end-of-primary school test score. Student characteristics are ethnicity, gender, free school meal (FSME) and special educational needs (SEN). Coefficients are estimated using Stata command `reghdfe` allowing two sets of high dimensional fixed effects to be absorbed. Standard errors in italics and clustered at 3,800 secondary schools. ** 1% sig.

Appendix

A.1 Education Production Function Foundation to Empirical Specification

We use the standard education production function approach to derive a rank-augmented value-added specification that can be used to identify the effect of primary school rank, measured as outlined in Section 2 on subsequent outcomes.

To begin, we consider a basic contemporaneous education production function, using the framework as set out in Todd and Wolpin (2003). For student i studying subject s in primary school j , cohort c and in time period $t = [1, 2]$:

$$Y_{ijsc}^t = x_i' \beta + v_{ijsc}^t$$

$$v_{ijsc}^t = \mu_{jsc} + \tau_i + \varepsilon_{ijsc}^t$$

where Y denotes national academic percentile rank in subject s at time t and is determined by x_i a vector of observable non-time varying characteristics of the student and v_{ijsc}^t representing the unobservable factors. Here β represents the permanent impact of these non-time varying observable characteristics on academic achievement. There are two time periods: in period one, students attend primary school. In the second period, students attend secondary schools. The error term v_{ijsc}^t has three components. μ_{jsc} represents the permanent unobserved effects of being taught subject j in primary school s in cohort c . This could reflect the effect of a teacher being particularly good at teaching maths in one year but not English, or that a student's peers were good in English but not in science; τ_i represents permanent unobserved student characteristics, which includes any stable parental inputs or natural ability of the child; ε_{ijsc}^t is the idiosyncratic time specific error which includes secondary school effects. Under this restrictive specification only ε_{ijsc}^t could cause the national academic rank of a student to change between primary and secondary school, as all other factors are permanent and have the same impact over time.

This is a restrictive assumption, as the impact of observable and unobservable characteristics are likely to change as the student ages. One could imagine that neighborhood effects may grow in importance as the child grows older, and that the effects of primary school are more important when the child is young and attending that school. Therefore we extend the model by allowing for time-varying effects of these characteristics:

$$Y_{ijsc}^t = \beta_{Rank}^{t-1} R_{ijsc}^{t-1} + x_i' \beta + x_i' \beta_t + v_{ijsc}^t$$

$$v_{ijsc}^t = \mu_{jsc} + \mu_{jsc}^t + \tau_i + \tau_i^t + \varepsilon_{ijsc}^t$$

where β_t allows for the effect of student characteristics to vary over time. We have also introduced the parameter of interest β_{Rank} , which is the effect of having rank R_{ijsc}^{t-1} , in subject s in cohort c and in primary school j on student achievement in that subject in the subsequent period t . As we are interested in the longer-run effects of rank positions that students had during their early education stages, we therefore assume that there is no effect of rank in the first period $t = 0$ as there is no prior rank. We will hence be estimating β_{Rank}^{t-1} , the effect of primary school rank on period t outcomes. To simplify the notation the time subscript will be dropped, as only one rank parameter is estimated, β_{Rank} .

This specification also allows for the unobservables to have time varying effects. Again τ_i represents unobserved individual effects that capture all the time constant effects of a student over time; μ_{jsc} represents the permanent effects of being taught in a specific school-subject-cohort.

Now, additionally, we have τ_i^t and μ_{jsc}^t allowing for these error components to vary over time so that students can have individual-specific growth rates as they grow older, or that primary school teachers can affect the efficiency of their students to learn a certain subject in the future.

Given this structure we now state explicitly the conditional independence assumption that needs to be satisfied for estimating an unbiased rank parameter. Conditional on student characteristics, time varying and permanent primary school-by-subject-by-cohort (SSC) level and individual effects, we assume there would be no expected differences in students' outcomes except those driven by rank.

$$Y_i^t \perp R_i^{t-1} | x_i, \mu_{jsc}, \mu_{jsc}^t, \tau_i, \tau_i^t \in R$$

To achieve this we require the measures of all factors that may be correlated with rank and final outcomes. Conditioning on prior test scores will absorb all non-time varying effects as they will affect period-1 test scores to the same extent as period-2 test scores. Any input, observable or unobservable, that would affect academic attainment is captured in these test scores. Therefore we can express period two outcomes, age 14 test scores, as a function of rank, prior test scores, student characteristics and unobservable effects.

$$Y_{ijksc}^t = \beta_{Rank} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}(x_i' \beta, x_i' \beta, \tau_i, \mu_{jsc}, \tau_i^{t-1}, \mu_{jsc}^{t-1})) + x_i' \beta^t + \mu_{jsc}^t + \tau_i^t + \varepsilon_{ijksc}^t$$

Using lagged test scores means that the remaining factors are those that affect the learning in period 2, between ages 11 and 14 ($x_i' \beta^t, \mu_{jsc}^t, \tau_i^t$). In our regressions, we will allow the functional form of this lagged dependent variable to take two forms, either a 3rd degree polynomial or a fully flexible measure, which allows for a different effect at each national test score percentile. As we can observe certain characteristics and primary school attended, β^t and μ_{jsc}^t can easily be estimated. The interpretation of μ_{jsc}^t is that some primary schools are more effective at teaching for a later test than others, in a way that does not show up in the end-of-primary age-11 test scores.

With regard to recovering τ_i^t , the second period academic growth of individual i is below, but it is worth spending some time interpreting what the rank coefficient represents without its inclusion. Being ranked highly in primary school may have positive spillover effect in other subjects. Any estimation, which allows for individual growth rates during secondary school (second period), would absorb any spillover effects. Therefore, leaving τ_i^t in the residual means that the rank parameter is the effect of rank on the subject in question and the correlation in rank from the other two subjects, as we have test scores for English, mathematics and science.

In the second period the student will be attending secondary school k which may affect later test scores by subject, π_{ksc} , which is another component of the error term ε_{ijksc} , where $\varepsilon_{ijksc} = \pi_{ksc} + \varepsilon_{ijksc}$. As stated above conditional on time-varying student effects, prior subject test scores and the other stated factors, we do not expect that these components will be correlated with primary rank. This is primarily because general secondary school effects are absorbed by the time varying student effects. We will return to the issue of secondary school choice in Section 5.

The first two specifications we estimate will recover the effect of rank due to overall changes in effort which allow for spill-overs between subjects. These are the following:

$$Y_{ijksc}^t = \beta_{Rank} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}) + x_i' \beta^t + \mu_{jsc}^t + \varepsilon_{ijksc}^t \quad (7)$$

where

$$\varepsilon_{ijksc}^t = \tau_i^t + \pi_{ksc}^t + \varepsilon_{ijksc}^t$$

$$Y_{ijksc}^t = \beta_{Rank} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}) + x_i' \beta^t + \mu_{jsc}^t + \pi_{ksc}^t + \psi_{ijksc}^t \quad (8)$$

where

$$\psi_{ijksc}^t = \tau_i^t + \varepsilon_{ijksc}^t$$

We now further augment these regressions by including the average student growth rate across subjects to recover individual growth effects, τ_i^t . Note that despite using panel data, this estimates the individual effect across subjects and not over time. When allowing for student effects, we effectively compare rankings across classes, controlling for individual national subject-specific achievement as well as all potentially unobserved individual effects on value added that are constant across subjects. Any individual characteristic that is not realized in the age-11 test scores but contributes towards the age-14 test scores is accounted for, including the secondary school attended, as long as the effects are not subject specific.

$$Y_{ijksc}^t = \alpha_t + \beta_{Rank} R_{ijsc}^{t-1} + f(Y_{ijsc}^{t-1}) + \tau_i^t + \mu_{jsc}^t + \epsilon_{ijksc}^t \quad (9)$$

where

$$\epsilon_{ijksc}^t = \pi_{ksc}^t + \varepsilon_{ijksc}^t$$

In these specifications the rank parameter only represents the increase in test scores due to subject specific rank, as any general gains across all subjects would be absorbed by the student effect. This can be interpreted as the extent of specialization in subject s due to primary school rank. It is for this reason, and the removal of other covarying factors, why we would expect the coefficient of the rank effect in specification 9 to be smaller than those found in 7 or 8. Finally, to also investigate potential non-linearities in the effect of ordinal rank on later outcomes (i.e. effects driven by students who are top or bottom of the class), we replace the linear ranking parameter with indicator variables according to quantiles in rank plus additional indicator variables for those at the top and bottom of each school-subject-cohort (the rank measure is defined in Section 3). We allow for non-linear effects according to vintiles in rank, which can be applied to all the specifications presented. We drop the t superscripts for the ease of exposition in the main text.

In summary, if students react to ordinal information as well as cardinal information, then we expect the rank, in addition to relative achievement to have a significant effect on later achievement when estimating these equations. This is what is picked up by the β_{Rank} - coefficient.

A.2 Peer Effects

There are concerns that with the existence of peer effects, peer quality jointly determines both a student's rank position and their age 11 results. This mechanical relationship could potentially bias our estimation. This is because in the presence of peer effects, a student with lower quality peers would attain lower age 11 test scores and gain a higher rank than otherwise. Thus, when controlling for prior test scores in the age 14 estimations, when students have a new peer group, those who previously had low quality peers in primary school would appear to gain more. Since rank is negatively correlated with peer quality in primary school, it would appear that those with high rank will make the most gains. Therefore, having a measure of ability confounded by peer effects would lead to an upward-biased rank coefficient.

This situation may be present in our data. We propose a resolution through the inclusion of subject-by-cohort-by-primary school controls. These effects will absorb any average peer effects within a classroom. However, they will not absorb any peer effects that are individual specific. This is because all students will have a different set of peers (because they cannot be a peer to themselves). Therefore, including class level controls will only remove the average class peer effect. The remaining bias will be dependent on the difference between the average peer effect and the individual peer effect and its correlation with rank. We are confident that the remaining effect of peers on the rank parameter will be negligible, given that the difference between average and

individual peer effect decreases as class size increases. The bias will be further attenuated because the correlation between the difference and rank will be less than one, and both effects are small.

We test this by running simulations of a data generating process, where test scores are not affected by rank and are only a function of ability and school/peer effects. We then estimate the rank parameter given this data. We allow for the data-generating process to have linear mean-peer effects, as well as non-linear peer effects [Lavy et al. \(2012\)](#). We are conservative, and assume extremely large peer effects, allowing both types of peer effects to account for 10 percent of the variance of a student's subject-specific outcome. Given that the square root of the explained variance is the correlation coefficient, this assumption implies that a one standard deviation increase in peer quality improves test scores by 0.31 standard deviations. In reality, [Lavy et al. \(2012\)](#) find a 1sd increase in peers only increases test scores by 0.015 standard deviations, which is 20th of the size.

The data generating process is as follows:

- We create 2900 students attending 101 primary schools and 18 secondary schools of varying sizes.
- A range of factors are used to determine achievement. Each of these factors are assigned a weight, such that the sum of the weights equal one. This means that weights can be interpreted as the proportion of the explained variance.
- Students have a general ability α_i and a subject specific ability δ_{is} taken from normal distributions with mean zero and standard deviation one. Taken together they are given a weighting of 0.7 as the within school variance of student achievement in the raw data is 0.85. They are given a weight of 0.6 where rank effects exist.
- All schools are heterogeneous in their impact on student outcomes. These are taken from normal distributions with mean zero and standard deviation one. School effects are given a weighting of 0.1 as the across school variance in student achievement in the raw data is 0.15.
- Linear mean peer effects are the mean subject and general ability of peers not including themselves. Non-linear peer effect is the negative of the total number of peers in the bottom 5% of students in the population in that subject. Peer effects are given a weight of 0.1, which is much larger than reality.
- We allow for measurement error in test scores to account for 10% of the variance.
- We generate individual i 's test scores as a function of general ability α_i , subject specific ability δ_{is} , primary peer subject effects ρ_{ijs} or secondary peer subject effects σ_{iks} , primary school effects μ_j or secondary school effects π_k , age 11 and 14 measurement error ε_{ijs} or ε_{ijks} , and primary school Rank ω_{ijs} .

– Age 11 test scores

$$Y_{ijs}^t = 0.7(\alpha_i + \delta_{is}) + 0.1\mu_j + 0.1\rho_{ijs} + 0.1\varepsilon_{ijs}$$

– Age 14 test scores where rank has no effect (Panel A):

$$Y_{ijks}^{t-1} = 0.7(\alpha_i + \delta_{is}) + 0.1\pi_k + 0.1\sigma_{iks} + 0.1\varepsilon_{ijks}$$

– Age 14 test scores where rank has an effect (Panel B):

$$Y_{ijks}^{t-1} = 0.6(\alpha_i + \delta_{is}) + 0.1\pi_k + 0.1\sigma_{iks} + 0.1\omega_{ijs} + 0.1\varepsilon_{ijks}$$

We simulate the data 1000 times and each time estimate the rank parameter using the following specifications with and without school-subject effects, with and without school-subject effects.

$$Y_{ijks}^t = \beta_{Rank} Rank_{ijs}^{t-1} + \beta_{y1} Y_{ijs}^{t-1} + \varepsilon_{ijks}^t$$

The results from these estimations can be found in Appendix Table A.1. When rank does not have an effect (Panel A), we would expect $\beta_{Rank} = 0$. When it does (Panel B), $\beta_{Rank} = 0.1$. With these inflated peer effects sizes, we find that controlling for SSC effects removes enough of the positive bias to make the effect of peers negligible (Table A.1, column 3). If there are large non-linear peer effects, then this specification introduces a negative bias; therefore our results can be considered as upper bounds (Table A.1, column 3).

A.3 Measurement Error in Test Scores

Test scores are an imprecise measure of ability. Could this measurement error be driving the results? Given that rank and test scores will both be affected by the same measurement error (but to different extents due to heterogeneous test score distributions across classes), calculating the size of the bias is intractable. To gauge the potential effect of measurement error, we simulate the data generating process. This allows us to have a true measure of ability and a student test score of which 20 percent of the variation is measurement error. Comparing the estimates of the rank parameter both with and without measurement error provides an indication of the extent to which measurement error could be driving the results. Rank measurement is then derived from the noisy test score measure in both cases.

The data generating process is as follows:

- 2900 students to 101 primary schools and 18 secondary schools of varying sizes.
- A range of factors are used to determine achievement. Each of these factors are assigned a weight, such that the sum of the weights equal one. This means that weights can be interpreted as the proportion of the explained variance.
- Students have a general ability α_i and a subject specific ability δ_{is} taken from normal distributions with mean zero and standard deviation one. Taken together they are given a weighting of 0.7 as the within school variance of student achievement in the raw data is 0.85. They are given a weight of 0.6 where rank effects exist.
- All schools are heterogeneous in their impact on student outcomes, which are taken from normal distributions with mean zero and standard deviation one. School effects are given a weighting of 0.1 as the across school variance in student achievement in the raw data is 0.15.
- We allow for measurement error in test scores to account for 20% of the variance, double the effect of any school subject effects.
- We generate individual i 's test scores as a function of general ability α_i subject specific ability δ_{is} , primary school effects μ_j or secondary school effects π_k , age 11 and 14 measurement error ε_{ijs} or q_{ijks} , and primary school Rank ω_{ijs}

– Age 11 test scores

$$Y_{ijs}^{t-1} = 0.7(\alpha_i + \delta_{is}) + 0.10\mu_j + 0.2\varepsilon_{js}$$

- Age 14 test scores where rank has no effect (Panel A):

$$Y_{ijks}^t = 0.7(\alpha_i + \delta_{is}) + 0.10\pi_k + 0.2\varepsilon_{ijks}$$

- Age 14 test scores where rank has an effect (Panel B):

$$Y_{ijks}^t = 0.6(\alpha_i + \delta_{is}) + 0.10\pi_k + 0.1\omega_{ijs} + 0.2\varepsilon_{ijks}$$

We simulate the data 1000 times and each time estimate the rank parameter using the following specifications with and without school-subject effects, controlling either for true ability ($\alpha_i + \delta_{is}$) or age 11 test scores.

$$Y_{ijks}^t = \beta_{Rank}Rank_{ijs}^{t-1} + \beta_{Ability}Ability_{ijs} + \varepsilon_{ijks}$$

$$Y_{ijks}^t = \beta_{Rank}Rank_{ijs}^{t-1} + \beta_{Ability}Ability_{ijs} + \sigma_{ijs} + \varepsilon_{ijks}$$

$$Y_{ijks}^t = \beta_{Rank}Rank_{ijs}^{t-1} + \beta_Y Y_{ijs}^{t-1} + \varepsilon_{ijks}$$

$$Y_{ijks}^t = \beta_{Rank}Rank_{ijs}^{t-1} + \beta_Y Y_{ijs}^{t-1} + \sigma_{ijs} + \varepsilon_{ijks}$$

The results of these specifications can be found in Appendix Table A.2. The ability specification produces unbiased results. When there is measurement error in the test score there is a downward bias to the rank effect when rank has an effect (Table A.2, Column 5, Panel B). We find that including SSC and student fixed effects removes this downward bias.

A.4 Model of Effort Allocation

To explicitly describe the mechanism, we put forward a basic behavioral model of how rank can affect later actions through changes in confidence. There are two stages: a learning stage followed by an action stage. In the learning stage, individuals of heterogeneous ability in different tasks are randomly allocated into groups. They perform tasks and compare their abilities relative to others in their group. This forms their task specific and general confidence. In the second stage, individuals are placed in a new peer group, where they perform the same tasks. The confidence formed in the first stage affects the costs of effort for each task in the second stage.⁴⁰ Individuals now allocate effort to each task to maximize output for a given level of effort and ability. In this simplified model we assume that individuals do not include later rank directly in their objective function.

Without losing generality, we apply this to the education setting where students vary in terms of ability across subjects and are randomly allocated to primary schools where their confidence in each subject if are formed during the first stage. This is generated through students interacting with their peers, such as observing who answers questions, and teacher grading. For the purposes of the model, we assume that students exert no effort during primary school, with outcomes being a product of ability and school factors.

⁴⁰ Confidence instead affect an agent's ability in a task rather than cost of effort. This would lead to the same predicted changes in the effort ratios and empirical results. Given the data available, we are unable to determine if it is costs or abilities that are affected. With information on time allocated on each task a positive relationship with rank would imply cost reductions, whereas no changes or decreases would imply gains in ability. We have chosen costs, as this is the more parsimonious and intuitive of the two.

In the second stage, we model students as grade maximizing agents for a given total cost of effort T_i and subject ability level A_{is} . The grade achieved Y_i , by a student i in subject s is a function of ability A_{is} and effort E_{is} according to a separable production function where there are decreasing returns to effort in each subject, $0 < \kappa < 1$. For simplicity of notation, assume that there are only two subjects, $s = e, m$. The productivity of effort is additionally affected by subject specific school factors μ_s . The total test score of individual i is the sum of this function over subjects, therefore, for student i in school μ_{is} the education production is:

$$Y_i = f(A_{ie}, E_{ie}) + f(A_{im}, E_{im}) = \mu_{ie} A_{ie} E_{ie}^\kappa + \mu_{im} A_{im} E_{im}^\kappa \quad (10)$$

This can be rearranged in terms of E_{ie} so that an isoquant Q_o can be drawn for a given total grades Y_i , subject abilities and school effects, and all the combinations of subject effort (see Figure A.1).

$$E_{ie} = \left(\frac{Y_i - \mu_{im} A_{im} E_{im}^\kappa}{\mu_{ie} A_{ie}} \right)^{1/\kappa} \quad (11)$$

A student's confidence in each subject generated in the first stage determines the student's cost of effort. Those with more confidence will find the cost of effort lower. For example, when faced with a difficult mathematics question, a student who considers herself good at mathematics would spend longer looking for a solution, as compared to another student who may give up. Therefore, the cost of subject effort c_s is a decreasing function of school subject rank R_s , $C_s = g(R_s)$ where $g' < 0$. We assume that the costs of subject effort are linear in the effort applied to that subject. We also allow for a general cost of effort C_{ig} , which varies across individuals according to general academic confidence and is a decreasing function of ranks in all subjects, $C_g = d(R_m, R_e)$ where $d'(R_s) < 0$ for $s = m, e$. This general cost function reflects a student's general attitude towards education, and is linear in the sum of effort applied across all subjects, $E_{im} + E_{ie}$. The total cost of effort T that a student can apply is fixed; however, the inclusion of a general cost of academic effort term, means that the total effort applied by a student is very flexible.

$$T_i \geq C_{im} E_{im} + C_{ie} E_{ie} + C_{ig} (E_{im} + E_{ie}) \quad (12)$$

This allows for an isocost line to be drawn using the cost of effort in each subject as the factor prices for a given total effort (see Figure A.1, Panel A). There is additionally a non-binding time constraint, normalizing the total time available to one, $E_{ie} + E_{im} < 1$. As standard, the solution is where the technical rate of substitution equals the relative factor prices (i.e. where the isoquant and isocost lines are tangential).

Therefore student i wants to maximize total grades by solving:

$$\max_{E_e, E_m} Y(E_e, E_m) = f(E_e) + f(E_m) = \mu_e A_e E_e^\kappa + \mu_m A_m E_m^\kappa$$

subject to:

$$T \geq C_e E_e + C_m E_m + C_g (E_m + E_e)$$

$$1 > E_e + E_m$$

$$l = Y - \lambda (T - C_e E_e - C_m E_m - C_g (E_m + E_e))$$

$$dl / (dE_e) = 0 \rightarrow \partial Y / (\partial E_e) = \lambda (C_e + C_g)$$

$$dl/(dE_m) = 0 \rightarrow \partial Y/(\partial E_m) = \lambda(C_m + C_g)$$

$$dl/d\lambda = 0 \rightarrow C_e E_{ie} + C_m E_m + C_g(E_m + E_{ie}) = T$$

$$dY/(dE_s) = \kappa \cdot \mu_s A_s E_s^{(\kappa-1)}$$

Therefore

$$\kappa \cdot \mu_s A_s E_s^{(\kappa-1)} = \lambda(C_s + C_g)$$

Where λ reflects the marginal grade per effort and $\lambda > 0$

$$\frac{\kappa \mu_e A_e E_e^{(\kappa-1)}}{(C_e + C_g)} = \lambda = \frac{\kappa \mu_m A_m E_m^{(\kappa-1)}}{C_m + C_g}$$

This gives

$$\frac{(C_e + C_g)}{(C_m + C_g)} = \frac{\mu_e A_e E_e^{(\kappa-1)}}{\mu_m A_m E_m^{(\kappa-1)}} \quad (13)$$

It is also clear that given this specification effort exerted in a specific subject is dependent on the student's ability and cost of effort in that subject and general confidence.

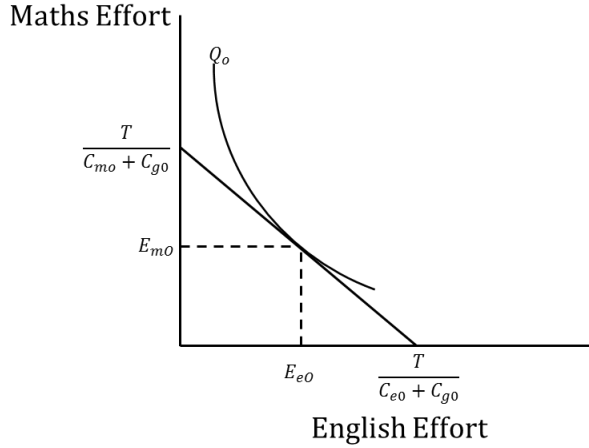
$$E_{is}^* = \left(\frac{\lambda(C_{is} + C_g)}{(\kappa \mu_s A_{is})} \right)^{1/(\kappa-1)} \quad (14)$$

In the above λ reflects the marginal grade per effort where $\lambda > 0$. As costs are decreasing in subject rank and $0 < \kappa < 1$, any increase in rank in subject s will increase the later effort allocated to that subject. A student who increases in confidence in English will now have a lower cost of learning English, thereby increasing their English to math effort ratio. The reduced costs also induce an income effect as more effort can be allocated for the same total effort costs. The isocost line shifts outwards and a higher isoquant can be reached (Figure A.1 Panel B). This student would now optimally chose to exert more effort in English ($E_1 > E_0$) and less effort in math ($M_1 < M_0$). As a result, the total grades that can be achieved for a given cost of effort and ability level is higher. This has yet to take into account the reduction in general academic costs of effort C_g , due to an increase in general academic confidence. This would reduce the cost of both subjects, leading to an income effect. This shifts the isocost curve out, increasing the maximum possible English and math efforts that could be allocated (Figure A.1 Panel C). Given this specification, the final effect on math effort is ambiguous, as it depends on the shape and position of the isoquants and the importance of general confidence. For the estimations that include student fixed effects, the individual effects absorb any individual general academic confidence gained by being ranked highly. These estimations are therefore equivalent to the case where C_g is fixed and we are just looking at the effect of allocation across subjects. The specifications that do not include individual effects instead do allow for spillover effects between subjects. Hence there can be general gains in confidence. This is the intuition for why the parameters recovered from the student effects estimations are smaller than those from the school cohort effect estimations. This two-subject example is for exposition only, but easily extends to the setting where an individual is maximizing total grades over three subjects.

Appendix Figures and Tables

Figure A.1: Optimal Allocation of Effort Across Subjects

Panel A

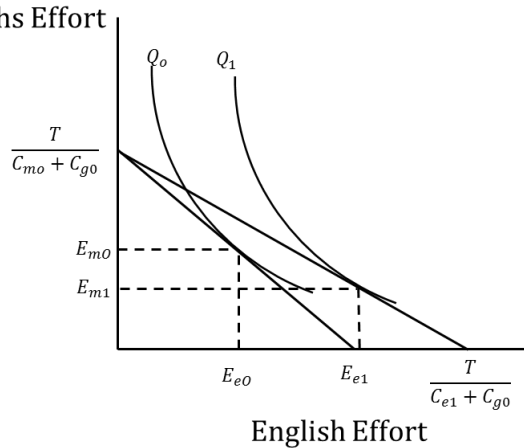


Isoquant Q_0 :

$$E_{e0} = \left(\frac{Y_0 - \mu_m \cdot A_m \cdot E_{m0}^k}{\mu_e \cdot A_e} \right)^{\frac{1}{k}}$$

Optimal English effort E_{e0} and math effort E_{m0} , given cost of English and math effort C_{e0} , C_{m0} . Marginal cost of effort equals marginal test score gain where isoquant and isocost curve are tangential.

Panel B

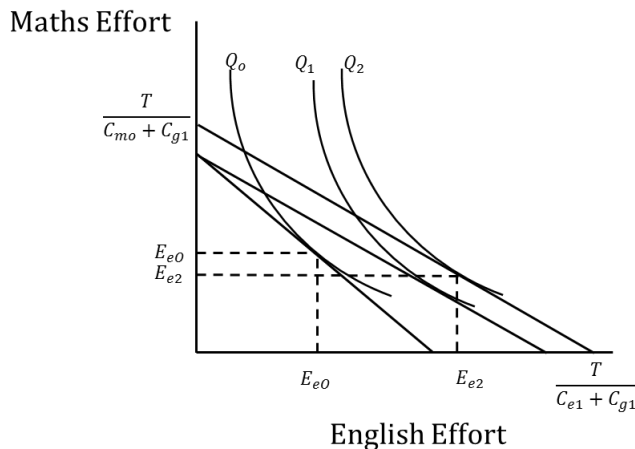


Isoquant Q_1 :

$$E_{e1} = \left(\frac{Y_1 - \mu_m \cdot A_m \cdot E_{m1}^k}{\mu_e \cdot A_e} \right)^{\frac{1}{k}}$$

A higher rank in English, improves English confidence and reduces cost of effort in English $C_{e0} > C_{e1}$. Shifts isocost line out to new intercept on English axis. Increase English effort $E_{e0} < E_{e1}$ and decrease math effort $E_{m0} > E_{m1}$

Panel C



Isoquant Q_2 :

$$E_{e2} = \left(\frac{Y_1 - \mu_m \cdot A_m \cdot E_{m2}^k}{\mu_e \cdot A_e} \right)^{\frac{1}{k}}$$

A higher rank in English, also improves general confidence and reduces cost of effort in both subjects $C_{s1} > C_{s2}$. Shifts isocost line out to new intercept on both axis. This increases effort applied in both subjects $E_{s1} < E_{s2}$.

Total effects: More effort applied to English $E_{e2} > E_{e0}$, ambiguous effect on math.

Table A.1: Simulation of Rank Estimation with Peer Effects

	<i>Mean peer effects</i>		<i>Non-linear Peer Effects</i>	
	(1)	(2)	(3)	(4)
<i>Panel A: Rank has no effect $\beta_{rank}=0.0$</i>				
Mean $\hat{\beta}_{rank}$	0.046	0.000	0.302	-0.041
Mean SE of $\hat{\beta}_{rank}$	0.014	0.018	0.015	0.019
SE of $\hat{\beta}_{rank}$	0.015	0.019	0.031	0.020
95% Lower Bound	0.015	-0.037	0.243	-0.079
95% Upper Bound	0.077	0.035	0.364	-0.003
<i>Panel B: Rank has an effect $\beta_{rank}=0.1$</i>				
Mean $\hat{\beta}_{rank}$	0.099	0.100	0.304	0.068
Mean SE of $\hat{\beta}_{rank}$	0.014	0.017	0.014	0.018
SE of $\hat{\beta}_{rank}$	0.015	0.018	0.027	0.018
95% Lower Bound	0.069	0.066	0.252	0.033
95% Upper Bound	0.129	0.133	0.358	0.104
KS2 and Rank	✓	✓	✓	✓
School-Subject-Effects		✓		✓

Notes: 1000 iterations, 95% confidence bounds are obtained from 25th and 975th estimate of ordered rank parameters.

Table A.2: Simulation of Rank Estimation with Measurement Error

	<i>Condition on true ability:</i>			<i>Condition on test scores:</i>		
	<i>No measurement error</i>			<i>Large measurement error</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Rank has no effect $\beta_{rank}=0.0$</i>						
Mean $\hat{\beta}_{rank}$	-0.001	0.000	0.000	-0.010	0.015	-0.000
Mean SE of $\hat{\beta}_{rank}$	0.021	0.020	0.025	0.029	0.034	0.041
SE of $\hat{\beta}_{rank}$	0.037	0.021	0.025	0.030	0.036	0.042
95% Lower Bound	-0.074	-0.039	-0.047	-0.068	-0.054	-0.082
95% Upper Bound	0.076	0.041	0.050	0.048	0.086	0.079
<i>Panel B: Rank has an effect $\beta_{rank}=0.1$</i>						
Mean $\hat{\beta}_{rank}$	0.099	0.100	0.100	0.053	0.113	0.100
Mean SE of $\hat{\beta}_{rank}$	0.021	0.020	0.025	0.027	0.032	0.038
SE of $\hat{\beta}_{rank}$	0.037	0.021	0.025	0.029	0.033	0.039
95% Lower Bound	0.026	0.061	0.053	-0.002	0.048	0.024
95% Upper Bound	0.176	0.141	0.150	0.109	0.179	0.176
Ability and Rank	✓	✓	✓	✓	✓	✓
School-Subject-Effects		✓	✓		✓	✓
Student Effects			✓			✓

Notes: 1000 iterations, 95% confidence bounds are obtained from 25th and 975th estimate of ordered rank parameters.