A big fish in a small pond: Ability rank and human capital investment[*]

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Abstract

We study the impact of a student's ordinal rank in a high school cohort on educational attainment several years later. To identify a causal effect, we compare multiple cohorts within the same school, exploiting idiosyncratic variation in cohort composition. We find that a student's ordinal rank significantly affects educational outcomes later in life. If two students with the same ability have a different rank in their respective cohort, the higher-ranked student is significantly more likely to finish high school, attend college, and complete a 4-year college degree. These results suggest that low-ranked students under-invest in their human capital even though they have a high ability compared to most students of the same age. Exploring potential channels, we find that students with a higher rank have higher expectations about their future career, and feel that they are being treated more fairly by their teachers.

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1 Introduction

The characteristics of classmates are among the decisive factors for parents when choosing a school for their child. It is commonly believed that children learn and achieve more when they are surrounded by high-ability classmates. In this paper we explore a channel that runs counter to the positive impact of high-ability peers: a student's ordinal ability rank in her peer group. Smart students who have a low rank in their peer group — a small fish in a big pond — may erroneously conclude that they have a low absolute ability, and thus under-invest in their human capital. Psychologists have labeled this phenomenon the big-fish-in-a-little-pond effect (Marsh, 1987).

In this paper, we test whether being a *big fish* in a high school cohort affects the critical transition period from high school to college. Consider two students, Jack and Jim, who have the same absolute ability, but a different rank in their respective high school cohort: Jack is among the students with the lowest ability in his cohort, while Jim is among the brightest students in his cohort. In other words, Jim is a big fish in a small pond. Here we analyze whether Jim is more likely than Jack to finish high school, attend college, and complete a 4-year college degree.

To identify a causal effect, we exploit idiosyncratic changes in the cohort composition within the same school over time. We argue that, conditional on attending a given school, the cohort composition is exogenous to the student. Entering a school in a given cohort is mainly determined by a student's birth date, and thus beyond the influence of parents or students.

We use data from the National Longitudinal Study of Adolescent to Adult Health (AddHealth), a representative survey that tracks students in the US from middle and high school to their mid-30s, and contains rich information on cognitive skills and educational outcomes. Key to our identification strategy is that AddHealth covers multiple cohorts within the same high school, allowing us to exploit the within-school variation in cohort composition. Moreover, the survey includes an age-specific standardized ability test, which makes cognitive ability comparable within and across schools, as well as across cohorts. Based on these test scores, we rank students within a school cohort, and standardize the rank to cohort size, such that the variation in ordinal rank is only driven by differences in the ability distribution across cohorts.

Our central finding is that a student's ability rank in a high school cohort has a strong impact on educational outcomes later in life. A one-decile increase in a student's rank position — the difference between the first- and the third-best student in a grade of 20 students — increases high-school completion rates by half a percentage point, and both college attendance rates and 4-year-degree completion rates by one percentage point. Given that cognitive ability, parental education, as well as school and cohort characteristics are held constant, these are large effects. Within a school cohort, the effect is non-linear; it is virtually zero in the lower half of the ability distribution, and strongly positive in the upper half.

While our estimation strategy rules out that the results are driven by selection into schools, the identification may be threatened by school-specific cohort characteristics that are systematically related to educational attainment. One potential confounder is average peer ability, which has been shown to improve student performance. Our baseline specification would not be able to fully disentangle the negative rank effect from the positive impact of average peer ability. To net out the direct influence of average cohort characteristics on educational attainment, we apply a more demanding specification that includes school-by-cohort fixed effects. This approach absorbs the mean differences across cohorts within a school and identifies the effect only through differences in the variance of the ability distribution across cohorts. The results are unchanged, suggesting that the effect of rank on educational attainment is not biased by school-specific cohort characteristics.

In theory, this result can be explained by at least four mechanisms. First, the rank may provide students with a noisy signal of their own ability. Students may conclude from a low relative ability that they have a low absolute ability. If a low perceived ability translates into low expected returns to college, students may choose not to go to college. Second, rank can affect intrinsic factors. Students with a higher rank may be more motivated and self-confident, and hence put more effort into their studies, which then translates into a higher chance of going to college. Third, a student's environment may be responsive to a student's rank. Teachers, family, and friends may offer more support to high-ranked students, leading to better grades and a higher chance of going to college. Finally, the result could be explained by selective college admission policies. Colleges often observe a student's GPA rank within a cohort, which is correlated with our ability measure. If admissions officers give priority to students with a higher rank regardless of the school quality, or if colleges automatically admit the top 10% of a school cohort, then this can explain the effect.

While we are not able to fully disentangle these mechanisms, we can exploit the rich survey information in AddHealth to provide suggestive evidence that some channels are more important than others. We find strong evidence for the expected earnings channel. Applying the same empirical strategy as before, we find that a higher rank has an equally large effect on various measures of career expectations at the age of 16 as it has on the actual outcomes 12 years later. Moreover, we find that students with a higher rank are more optimistic, have a higher perceived intelligence, and put more effort into their studies, while we find no relationship between rank and various measures of well-being, happiness, and depression. In terms of support from their environment, students with a higher rank report a higher perceived support from their teachers, while the rank is not related to support from parents and friends. Finally, while we have no information on the type of college students are admitted to, we can exclude that the effect is purely driven by selective college admissions. When we run our baseline model and additionally control for GPA, the effect of the ability rank on educational attainment remains large and statistically significant, indicating that GPA-based college admissions explain only a fraction of the effect.

With this paper, we contribute to three strands of the literature. First, this paper extends the literature on ordinal rank and education outcomes. A large literature in psychology focuses on a student's self-concept, showing that students with a higher ordinal rank have a higher perceived ability in various school subjects (Marsh, 1987; Marsh *et al.*, 2007). The first rigorous causal estimate of ordinal rank on educational performance is provided by Murphy & Weinhardt (2014), who use administrative school data from the UK, and find a strong positive impact of ordinal rank in primary school on test scores in secondary school. Our paper uses a very similar research design, but departs from their study in two important dimensions. First, our data cover a longer time span, allowing us to estimate long-run effects of ordinal rank. Second, in addition to performance, we show that a student's ordinal rank affects critical choices during the transition from high school to college.

More broadly, this paper speaks to the literature on peer effects in education. So far, there is no consensus if and to what extent peers matter for student performance. While earlier studies have found a positive impact of higher peer quality on test scores, and affect later education choices, more recent studies show that peer effects are non-linear and can even be negative for some students.[1] The ordinal rank effect found in this paper provides one explanation for these ambiguous effects. The positive effect of having better peers can be offset by having a lower ordinal rank.

This paper also contributes to the literature on imperfect information and educational choices. The evidence shows that students have imperfect knowledge of their own ability (Stinebrickner & Stinebrickner, 2012, 2014; Zafar, 2011; Bobba & Frisancho, 2014), and are uncertain about their returns to education (Jensen, 2010; Attanasio & Kaufmann, 2015; Wiswall & Zafar, 2015). Our results suggest that the ordinal rank is one of the reasons why students have incorrect beliefs about their ability, and thus make suboptimal education choices.

2 Data and Descriptive Statistics

2.1 The AddHealth data

Our data is the restricted-use version of AddHealth, a representative longitudinal dataset of US middle and high schools. Four features of AddHealth are key to our study: first, it covers multiple cohorts within the same school. This is critical for identification, because we can compare students in adjacent cohorts within the same school, and exclude selection into schools as a main confounding factor. Second, within every school cohort, we observe a representative sample of students, from which we can construct the ability ranking. Third, the longitudinal set-up allows us to link the ordinal rank in high school to outcomes 12 years later, and to observe the critical transition from high school to tertiary education. Finally, the survey includes a standardized test that provides us with an objective measure of cognitive ability. Unlike in most other datasets, we can directly measure cognitive ability without having to resort to grades or

[1] The evidence for positive peer effects on student performance ranges from primary schools (Hanushek *et al.*, 2003; Ammermueller & Pischke, 2009) to high schools (Calvó-Armengol *et al.*, 2009; Imberman *et al.*, 2012) to college (Sacerdote, 2001; Zimmerman, 2003; Carrell *et al.*, 2009; De Giorgi & Pellizzari, 2014; Booij *et al.*, 2015). Bifulco *et al.* (2011); Patacchini *et al.* (2012) show that better peers also increase the likelihood of going to college. Studies that find a non-linear effect or zero effect are Lavy *et al.* (2012); Koppensteiner (2012); Carrell *et al.* (2013); Burke & Sass (2013); Pop-Eleches & Urquiola (2013); Abdulkadiroglu *et al.* (2014); Feld & Zölitz (2014); Tincani (2015); Tatsi (2015).

self-reported measures as proxies.

To date, four waves of AddHealth are available. The first wave was administered in 1994/1995, when students were between 13 and 18 years old. Follow-ups were run in 1996, in 2000/2001 when most students had left high school, and in 2008/2009, when most had entered the labor market. In the first wave, a representative sample was drawn among all public and private high schools in the US. Within each school, students from grades 7-12 were sampled. In total, we observe up to six cohorts within a school. All cohorts were interviewed at the same time, such that we only observe each cohort in one grade, i.e. we observe the 1994 entry cohort in grade 7, the 1993 entry cohort in grade 8, the 1992 entry cohort in grade 9, etc.[2]

The first wave consisted of two questionnaires: a basic *In-school* questionnaire, which was administered to all students whose schools took part in the survey, and a more comprehensive *In-home* questionnaire, which was answered by a randomly drawn subsample of students within each school. For the *In-home* sample, 17 boys and 17 girls were randomly drawn from each grade within each school. Additional students were drawn to oversample groups with certain characteristics: twins, students with disabilities, blacks from well-educated families, as well as students of Chinese, Cuban, and Puerto Rican origin.[3]

Our main sample is the *In-home* sample of the first wave, which we complement with the educational attainment information from the fourth wave. We drop from the sample all schools with 20 observations or less (109 obs.), and all grades with 5 students or less (304 obs.). Moreover, due to attrition, we drop all students for who we do not observe the educational attainment (finished high school, attended college, completed college) or other observable characteristics in wave IV (4,711 obs.). In total, this leaves us with 13,645 students in 130 schools and 432 grades.

2.2 Outcome variables: Educational attainment

We consider three outcome variables that measure different degrees of educational attainment: *completed high school*, *attended college*, *completed a 4-year college degree*. These measures are taken from wave IV of AddHealth, where respondents were asked about their highest educational attainment. The categories *attended college* and *completed a 4-year college degree* are nested; *completed a 4-year college degree* only includes students who completed at least a Bachelor's degree, while *attended college* is broader and also includes students who attended college but finished with less than a Bachelor's, or did not finish at all.

Table 1 summarizes the outcome variables for various groups. Among all students, 93% completed high school, while 67% attended college. Around half of those who attended college finished at least with a Bachelor's degree.[4]

[2] In schools that integrate high- and middle schools and that offer grades 7 to 12, all grades were sampled. In high schools that only offer grades 9-12, grades 7 and 8 were sampled from a random middle school (so-called *feeder school*) that was drawn from all surrounding middle schools that send students to the given high school. For further information on the study design and the sampling, see Harris (2009), and Harris *et al.* (2009).

[3] In 16 so-called saturated schools, all students that were present on the day of the survey were included.

[4] These numbers confirm the representativeness of the survey, as they are very close to the means in the American Community Survey (ACS): 91% have completed high school, 64% attended any type of college,

Across subgroups, the educational attainment differs considerably. In all three measures, women have a higher educational attainment than men. The data also reveal a high correlation between the educational attainment of the parents and their children. Children of college-educated parents are four times as likely to complete a college degree and ten times less likely to drop out of high school than children whose parents were high school dropouts. There is less variation in the educational attainment across ethnic groups. Hispanics and blacks have lower educational attainment than whites, but the raw differences are less than 10 percentage points. An exception are Asians, whose educational attainment is considerably higher than in all other groups.

Finally, we consider schools with different average ability and heterogeneity. Unsurprisingly, students from schools in the top half have a higher educational attainment. We also check if more heterogeneous schools are more or less conducive to educational success. If schools are homogeneous with respect to ability, for example because of tracking or because of neighborhood segregation, one would expect homogeneous schools to have different outcomes than heterogeneous schools. The raw data, however, do not support this conjecture.

2.3 Ranking students

Our regressor of interest is a student's ordinal rank in the ability distribution of a high-school cohort. To measure cognitive ability, we use the scores of a standardized Peabody Picture Vocabulary Test, of which a shortened version was included in the survey. The test works as follows: participants are asked to allocate words spoken aloud by the interviewer to a set of four pictures. The test proceeds through multiple rounds with increasing difficulty. The test is age-specific, with test scores being standardized to mean 100 within an age group. The scores are computed automatically, without being made available to the interviewer or the respondent.[5] Though measuring very basic cognitive skills, the Peabody test has been shown to have a high re-test reliability, and correlates highly with other intelligence tests for adolescents (Dunn & Dunn, 2007).

Based on the Peabody score, we rank all students within a school grade. If a grade has 100 students, the student with the highest ability is assigned rank 100, and the student with the lowest ability is assigned rank 1. Students with the same test score are assigned an equal rank. For the analysis, the absolute rank measure is problematic, because it is not comparable across grades with different sizes. Simply put, being the second best in a grade of 100 students means more than being the second best in a grade of 10 students. To make ordinal ranks fully comparable across grades, we compute a relative rank measure, which is standardized to grade size, and assigns value 1 to the student with the highest rank in a grade, and 0 to the lowest:

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and 31% completed a 4-year degree. These calculations are based on the 2007-2011 Public Use File of individuals born between 1976 and 1982 (US natives and immigrants who arrived before 1995).

[5] Further information on the Addhealth Picture Vocabulary Test is available in the AddHealth documentation at `http://www.cpc.unc.edu/projects/addhealth/data/guides`.

Table 1: Educational attainment by group

Group	*completed High school* mean	(SD)	*attended College* mean	(SD)	*completed 4-year degree* mean	(SD)	*N*
All	0.93	(0.26)	0.67	(0.47)	0.33	(0.47)	13645
Male	0.91	(0.28)	0.63	(0.48)	0.29	(0.45)	6330
Female	0.94	(0.23)	0.71	(0.45)	0.37	(0.48)	7315
Parental background:							
Less than high-school	0.81	(0.39)	0.45	(0.50)	0.13	(0.33)	1957
High school	0.91	(0.29)	0.55	(0.50)	0.19	(0.39)	3399
Some college	0.94	(0.23)	0.68	(0.47)	0.28	(0.45)	3423
College	0.98	(0.14)	0.85	(0.36)	0.54	(0.50)	4866
Race:							
White	0.94	(0.25)	0.69	(0.46)	0.36	(0.48)	7733
Asian	0.98	(0.15)	0.78	(0.42)	0.49	(0.50)	882
Hispanic	0.90	(0.30)	0.61	(0.49)	0.23	(0.42)	1961
Black	0.91	(0.28)	0.65	(0.48)	0.28	(0.45)	3069
Average school ability:							
High average ability (above median)	0.96	(0.20)	0.74	(0.44)	0.42	(0.49)	6730
Low average ability (below median)	0.90	(0.30)	0.61	(0.49)	0.24	(0.43)	6915
School heterogeneity (within-school SD in ability)							
High heterogeneity(above median)	0.92	(0.27)	0.67	(0.47)	0.32	(0.47)	7410
Low heterogeneity (below median)	0.93	(0.25)	0.68	(0.47)	0.34	(0.47)	6235

Notes: This table displays the share of people who completed high school, the share of people who enrolled in college, and the share of people who finished a 4-year college degree. Standard deviations are reported in parentheses. Parental background refers to the highest level of education and the highest occupational status among both parents. Average school ability is the average ability of the entire school, and above/below median refers to the school distribution, i.e. students in the "above median" group attend schools with an above-median ability-level. The school heterogeneity is measured by the within-school standard deviation of ability.

$$\text{relative rank} = \frac{\text{absolute rank} - 1}{\text{nr of students in grade} - 1}. \tag{1}$$

The Peabody score is our preferred measure for ranking students according to their ability, because the scores are comparable across grades and schools. Another suitable measure would be the grade point average (GPA). Rather than measuring relative ability, a ranking based on GPA measures relative performance. Performance, in turn, is a function of many factors besides cognitive ability, such as effort, ambition, or the choice of courses. Moreover, within many schools, GPAs are not comparable across grades because students are *graded on a curve* — that is, the same grade distribution is applied within every subject.

2.4 Summary statistics

Table 2 displays the summary statistics. Panel A summarizes the ability measures and other individual characteristics. The two columns on the right display the means for students in the bottom and top half of the within-grade ability distribution. At first glance, women and blacks are over-represented among students in the bottom half of a grade, while there is no large difference with respect to average age, and the share of Asians, Hispanics, or students with a migration background. A strong correlation appears between ability and parental education. Children of highly educated parents are more likely to have a higher rank within their grade.

Panel B summarizes the average school and grade characteristics. Schools differ considerably in terms of average ability and heterogeneity. Students in the lowest-ability school scored on average 79 on the standardized test, which is three between-school standard deviations below the mean; the highest-ability school scored 116, or 2.5 between-school standard deviations above the mean. To measure heterogeneity in ability we take the within-school standard deviation of the ability distribution. The within-school standard deviation varies between 9.2 and 20.5, and is on average twice as large as the between-school standard deviation, which is 6.5.

Depending on the school, the grade size varies greatly; in the population it ranges from 5 in the smallest grade to 645 in the largest. More relevant for our study is the actual within-grade sample size. The average grade has 40 students in the sample, which is more than the 34 students drawn at random due to oversampling of minorities and the inclusion of saturated schools. On average, 22% of a grade have been sampled.

3 Identification and Estimation Strategy

Our aim is to estimate a causal effect of a student's ability rank on educational attainment later in life. In this section, we first describe the identifying variation. We then lay out the econometric model, and discuss the identifying assumptions, as well as potential threats to identification.

3.1 Identifying variation

To estimate a causal effect, we exploit idiosyncratic variation in cohort composition within the same school over time. This variation can be due to differences in mean ability — some cohorts are on average brighter than others. It can also be due to differences in the dispersion of ability within a cohort — in some cohorts the ability is more evenly distributed than in others. The ordinal rank of a student with a given ability is affected by these differences. Figure 1 illustrates the identifying variation based on differences in mean ability for two entry cohorts in the same school, each consisting of 3 students. The 1994 entry cohort has a lower average ability, such that a student with cognitive ability $\underline{abil}$ would have the first rank. If she entered the school in 1995, when the entry cohort was stronger, she would only have the second rank, despite having the same cognitive ability.

By only using variation within schools, we can rule out that the variation in cohort compo-

Table 2: Summary Statistics of the main variables

		All		bottom 50%	top 50%
Variable	*N*	*Mean*	*SD*	*Mean*	*Mean*
A. Individual characteristics					
Ability					
Cognitive ability	13645	101.14	14.24	91.18	110.84
Ability rank	13645	0.50	0.29	0.24	0.75
Personal characteristics					
Age	13645	16.13	1.68	16.25	16.01
Female	13645	0.54	0.50	0.57	0.50
Ever repeated a grade	13645	0.20	0.40	0.28	0.13
Migration background (1st & 2nd gen.)	13645	0.15	0.36	0.16	0.14
Asian	13645	0.06	0.25	0.06	0.07
Black	13645	0.22	0.42	0.27	0.19
Hispanic ancestry	13645	0.14	0.35	0.16	0.13
Highest parental education					
Less than high-school	13645	0.14	0.35	0.19	0.10
High-school	13645	0.25	0.43	0.29	0.21
Some college	13645	0.25	0.43	0.24	0.26
College	13645	0.36	0.48	0.29	0.42
B. School and grade characteristics					
School characteristics	*N*	*Mean*	*SD*	*Min*	*Max*
Small ($<$ 401 students)	130	0.22	0.42		
Medium (401-1000 students)	130	0.47	0.50		
Large ($>$ 1000 students)	130	0.31	0.46		
Average class size	128	25.86	5.18	10.00	39.00
Mean ability	130	100.31	6.46	79.19	115.80
SD ability	130	12.89	2.29	9.24	20.48
Grade characteristics					
Grade size (population)	432	184.27	131.54	5	645
Nr students in sample	432	40.63	45.27	6	545

Notes: Panel A displays the means and standard deviations of the main variables for the whole sample, as well as the means for the students above and below the median ability of their school grade. Besides the share of Asians, all differences are statistically significant at the 1%-level. Panel B displays the average school and grade characteristics. The school characteristics have been reported by the school administrator in a separate survey. In two cases, the information on the average class size was missing.

sition driven by systematic self-selection of students into schools.In fact, there is evidence that parents strategically choose schools with their kids' rank in mind. Cullen *et al.* (2013) show that after automatic admission to the flagship state universities in Texas was granted to the top 10% of a school, parents deliberately sent their kids to lower-ability schools in order to give them a higher chance to be in the top 10%. The inclusion of school fixed effects in the empirical model

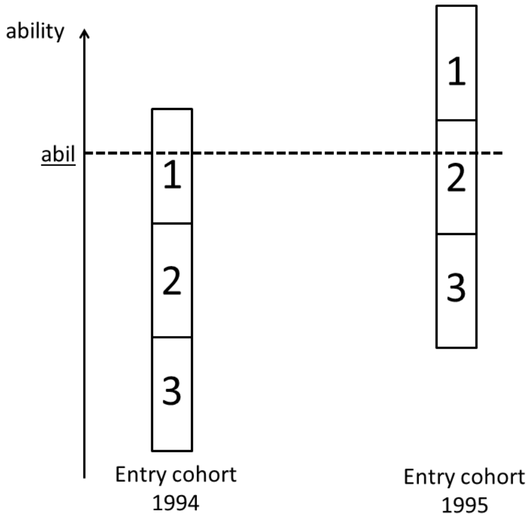

Figure 1: Identifying variation: a student with ability=abil has rank 1 in the entry cohort in 1994 but rank 2 in 1995.

will eliminate all systematic variation that is due to selection into schools.

But, within a school, where could the differences in the ability distribution across cohorts come from? As shown by Hoxby (2000a), one source of variation is the timing of births within a school year. If in some years more children are born before the age cut-off than in others, this leads to fluctuations in the cohort sizes within the same school. Along the same lines, the characteristics of parents may fluctuate from year to year. In some years, the share of children born to highly educated parents is higher than in others, the share of black or Hispanic children is higher than in others, or in some years more children with a higher innate ability are born than in others.

We will employ two identification strategies. Both exploit changes in the within-school cohort composition over time, but rely on different sources of variation. The first strategy follows Hoxby (2000b) and Bifulco *et al.* (2011), among others, and compares the outcomes of students in adjacent cohorts within the same school, as illustrated in Figure 1. We choose the first strategy for our baseline model because of its intuitive appeal. It is straightforward to think of an underlying experiment in which we compare the outcomes of students with the same cognitive ability, age, parental background, etc, and attribute the difference in their educational attainment to differences in ordinal rank. The drawback of this identification strategy is that it does not fully account for school-cohort-specific confounders, such as average peer quality. The second identification strategy is less intuitive at first sight, but rules out these confounders. We follow Murphy & Weinhardt (2014) and employ a specification with school-specific grade fixed effects. This strategy only exploits the variation within school grades; identification comes from differences in the dispersion of the ability distribution within a school over time.

3.2 Estimating equation

The following regression setup relates the educational attainment in wave IV of the survey (in 2008) to a student's ordinal rank in high school measured in wave I (in 1994/1995):

$$\begin{aligned} \text{Educ. attainment}_{ijk} &= \gamma \text{ ordinal rank}_{ijk} + g(\text{cog. ability}_{ijk}) + \boldsymbol{X'_{ijk}}\boldsymbol{\beta} \\ &+ \text{School FE}_j + \text{Grade FE}_k + \varepsilon_{ijk}. \end{aligned} \tag{2}$$

We consider the three outcome variables in separate regressions. The outcome variable of person i who attended high school j and grade k is a dummy variable that takes value one if a person has achieved a certain educational attainment — completed high school, attended any college, or completed a 4-year degree — and zero otherwise. The coefficient of interest is γ, which measures the impact of a marginal increase in the relative rank of a student within a high school cohort on educational attainment.

Given that a person's ordinal rank is determined by her cognitive ability, the ordinal rank could be seen as a mere proxy for cognitive ability, in which case γ could be interpreted as the marginal effect of cognitive ability and not of ordinal rank. To ensure that γ exclusively measures the marginal effect of ordinal rank, we control for a person's cognitive ability with a fourth-order polynomial $g(\text{cog. ability}_{ijk})$, which captures the potential non-linear relationship between ability and educational attainment.

As shown in Table 1, the outcome variables differ considerably between demographic groups. For example, men have lower educational attainment than women, blacks have lower educational attainment than whites, and children of highly educated parents have a higher educational attainment. The vector of individual control variables $\boldsymbol{X_{ijk}}$ accounts for these (pre-treatment) differences and ensures that in our regression we compare students with the same observable characteristics. The controls include a dummy for gender, dummies for race (asian, black, hispanic), a dummy for migration background (1 if a person is a first- or a second-generation migrant), dummies for the highest level of education of both parents (less than high school, high school, some college, college degree), and dummies for the highest occupational status of both parents (not working, blue collar, white collar low-skilled, white collar high-skilled).

We also control for age, which is important because age effects could confound the estimate of γ, for example if older students within a cohort are at an advantage and therefore have a higher educational attainment later. Given that we have the exact date of the interview as well as the month and year of birth, we compute the age in months, allowing for variation in age within a birth year. Finally, we include a dummy that equals one if a student has ever repeated a grade until wave I of the survey. As shown in Table 2, repeaters are concentrated in the lower half of the ability distribution of their grade. If they also have lower educational attainment, not controlling for repeaters would lead to an upward-bias in the estimate of γ.

The inclusion of separate school and grade fixed effects restricts the variance to within-schools and across-grades. The school fixed effects remove the mean differences between schools in educational attainment, cognitive ability, as well as the demographic composition of schools. The grade fixed effects remove the mean differences in all variables between the six grade levels in our sample.

Finally, ε_{ijk} is an i.i.d error term that captures all unobservable factors that affect educational attainment. Because the rank is computed at the grade-level, the error terms of all students in this grade could be serially correlated. To account for this, we cluster the standard errors at the *school* $\times$ *grade*-level.[6]

Within a given school, the identification of a causal effect rests on the assumption that being in a certain cohort is as good as random. This assumption only holds if at least two conditions are fulfilled:

1. Conditional on having chosen a specific school, neither parents nor students can manipulate the student's cohort.

2. Within a school, there is no systematic correlation between average cohort characteristics and educational attainment.

Violations to any of these conditions could introduce a systematic bias into the estimate of γ, as the ordinal rank would be correlated with the error term ($cov(\text{ordinal rank}_{ijk}, \varepsilon_{ijk}) \neq 0$). Potential violations to the first condition could be due to strategic delay of school entry (redshirting), or grade repetition. Examples for violations of the second condition are changes in the cohort quality within a school, or a direct effect of the average peer quality on outcomes. For the baseline analysis to follow, we maintain the assumption that both conditions hold. In robustness checks, we will address a large number of confounding factors, and also discuss measurement error and selective attrition as potential sources of bias.

4 Results

In this section we present the estimation results. We begin by exploring the unconditional relationship between rank and three measures of educational attainment, and gradually introduce fixed effects and control variables into the model. We further explore whether the effect differs between school types and whether it is non-linear within a cohort. While the baseline model rules out some obvious confounders, the results could still be biased due to omitted factors, measurement error, or attrition. In a series of robustness checks, we show that that these biases do not lead to dramatic changes in the results. Finally, we explore potential channels through which a student's rank affects educational attainment.

[6] We have also computed heteroskedasticity-robust standard errors, which would take into account the heteroskedastic errors that are inherent in linear probability models. In most cases, the robust standard errors are minimally smaller than the clustered standard errors reported.

4.1 Baseline results: ordinal rank and educational attainment

Table 3 displays the basic results for separate regressions of each of the three outcome variables — dummies for having completed high school, having attended college, and having completed college — on the ordinal rank of a student in her high school cohort.

Table 3: OLS regression results: the importance of rank position

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable						
Completed high school	0.133***	-0.053***	-0.002	0.054**	0.034	0.048
	(0.009)	(0.018)	(0.024)	(0.026)	(0.025)	(0.031)
Attended College	0.386***	-0.106***	0.091**	0.139***	0.103***	0.112**
	(0.014)	(0.039)	(0.039)	(0.041)	(0.038)	(0.046)
Completed 4-year degree	0.364***	-0.266***	0.073*	0.121***	0.101**	0.082*
	(0.013)	(0.044)	(0.042)	(0.042)	(0.040)	(0.048)
Controls:						
Individual ability (quartic)	No	Yes	Yes	Yes	Yes	Yes
School FE	No	No	Yes	Yes	Yes	No
Grade FE	No	No	No	Yes	Yes	No
Individual controls	No	No	No	No	Yes	Yes
School $\times$ Grade fixed effects	No	No	No	No	No	Yes
Goodness of fit:						
R^2 Completed high school	0.02	0.04	0.08	0.09	0.15	0.17
R^2 Attended College	0.06	0.10	0.15	0.15	0.23	0.25
R^2 Completed College	0.05	0.12	0.18	0.18	0.26	0.28

Note: This table displays the results of separate OLS regressions of the dependent variables *completed high school*, *attended college*, and *completed college* on the relative rank. From left to right, more controls and fixed effects are being introduced. Standard errors, clustered at the school-grade level, are displayed in parentheses, with significance levels * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The unconditional relationship in Column (1) confirms that a higher within-grade rank is associated with higher educational attainment. An increase in the relative rank by one decile, that is, the difference between the second- and the third-best student in a grade of ten students, or the difference between the second- and the fourth-best in a grade of 20, is associated with an increase in high school completion rates by 1.3 percentage points, which is 19% of the overall high school dropout rate (7%). The association with attending college and completing college is even larger. A one-decile increase in the relative ability rank increases the likelihood of going to college by 3.9 percentage points, which is 5% of the mean rate of college attendance, and increases the likelihood of completing college by 3.6 percentage points, which is more than 10% of the college completion rate in the sample.

While pointing to a strong association, the information we obtain from Column (1) is limited, because the ability rank is based on the score on the ability test, and merely is a proxy for ability due to the strong positive correlation between rank and ability. In Column (2) we control for a fourth-order polynomial in individual ability, in which case the sign of the marginal effect of ordinal rank gets reversed. Taken at face value, the results suggest that the ordinal rank negatively affects educational attainment. While this result may seem surprising, it merely reflects a mechanical correlation between rank and school quality. At a given level of ability, a student in a school with a low average ability has a higher rank than in a school with a high ability, but students in better schools have a higher educational attainment.

In Column (3) we control for unobserved heterogeneity across schools by introducing school fixed effects. Identification now only comes from within the schools. Compared to the model without fixed effects, the R^2 is considerably higher, confirming the importance of unobserved heterogeneity across schools. The coefficient for high school completion suggests that completing high school is not influenced by ordinal rank, while there is a positive association between ordinal rank and college attendance and completion.

Column (4) makes a leap towards a causal effect. While in Column (3) we estimated an average effect across all students within a school, in Column (4) we compare students with the same ability across different cohorts within the same school. We introduce grade fixed effects, which absorb the mean difference between different cohorts across the sample. If students who were in 7th grade in 1995 were on average different from those in 8th grade, this difference is accounted for in this specification. Compared to Column (3), the effects are larger and more precisely estimated. For all three outcome variables, these effects are substantial. An increase in the within-grade rank by one decile increases the likelihood of completing high school by half a percentage point, and increases college attendance and completion by 1.4 and 1.2 percentage points (2% and 3.6% of the mean), respectively.

In Column (5) we introduce individual control variables to take into account the differences in observable characteristics, and their potential effect on the outcome. There are two reasons for including control variables. First, as shown in Table 1, the outcome variables differ significantly across ethnic and parental backgrounds. Second, as indicated by the increased R^2 in Column (5), the control variables have additional explanatory power and ensure a better model fit. The inclusion of individual controls, however, has no statistically significant impact on the point estimates, which lends further credibility to our claim that cross-cohort variation in the ability distribution within the same school is quasi-random. The point estimates in Column (5) are slightly smaller than in Column (4), but the difference is not statistically significant.

Finally, we address the concern that the average grade characteristics, for example average peer ability, bias the estimates. In Column (6) we include *school* $\times$ *grade* fixed effects, taking into account school-specific average differences across grades. This specification only captures variation within a school grade. Intuitively, we compare the outcomes of students within the same grade who have a different rank, controlling for the difference in cognitive ability. The identification of γ comes from differences in the variance of the ability distribution across grades within the same school. It is reassuring that the results from this demanding specification are similar to those in the estimation with separate sets of fixed effects in Column (5). The differences between the coefficients in Columns (5) and (6) are not statistically significant.

In sum, these results clearly show that a student's rank matters for educational choices and outcomes. We find large and statistically significant differences in high-school completion rates, college attendance, and college completion of students who go to the same school but have a different rank in the ability distribution of their grade.

4.2 Heterogeneous effects

While the regression results in Table 3 show a positive impact of high school rank position on educational attainment, the strength of this impact differs along the ranking and across school types. The first three rows in Figure 2 displays the results for different school types, which we obtained by re-estimating Equation 3 on split samples. The three classifications are given in the survey, and are the only available measure of school size. As shown in the first row, the effects differ considerably by school size. For college attendance and completion, the effects are mainly driven by large and medium-sized schools, while the effect for high-school completion is larger in smaller schools. Finding larger effects in larger schools is evidence against measurement error in the rank variable due to the random sampling. Because the same number of students from a grade was sampled regardless of the school size, the measurement error, and therefore the attenuation bias should be greater in larger schools. While not an ironclad proof for the absence of measurement error, the results in the first row suggest that measurement error should not be too important.

We also test whether the effects are different in high- and low-ability schools, as well as in more and less heterogeneous schools. The second row of Figure 2 displays separate effects for schools with an average ability above and below the median: the effect is the same regardless whether the school has a high or a low average ability. Similarly, we consider schools with a high and low variance in ability. In segregated neighborhoods, we would expect a greater homogeneity within schools, and the ordinal rank could be more important in more or less segregated schools. However, we find no difference in the effect between high- and low-variance schools.

Finally, we consider non-linear effects within a grade. According to the linear effect in Table 3 going from rank 60 to rank 50 would make the same difference as going from rank 10 to rank 1. This can hardly be the case. While we lack the statistical power to test for non-linearities along the entire ranking, we provide evidence for a non-linear effect based on quintiles of the within-grade ability distribution. We estimate a model similar to Equation (3), but replace the relative rank with dummy variables for the quintiles 2-5. The lowest quintile is the base category. As shown in the fourth row of Figure 2, the effect is virtually zero in the bottom half of the within-grade distribution. From the third quintile onwards, the effect is positive, and the relationship between rank and educational outcomes looks linear.

4.3 Discussion and robustness checks

Thus far, we have interpreted the rank effect on educational attainment as causal, given that the individual controls as well as the fixed effects rule out many confounding factors, and based on the assumption that being in one school cohort or another is exogenous to the student. In

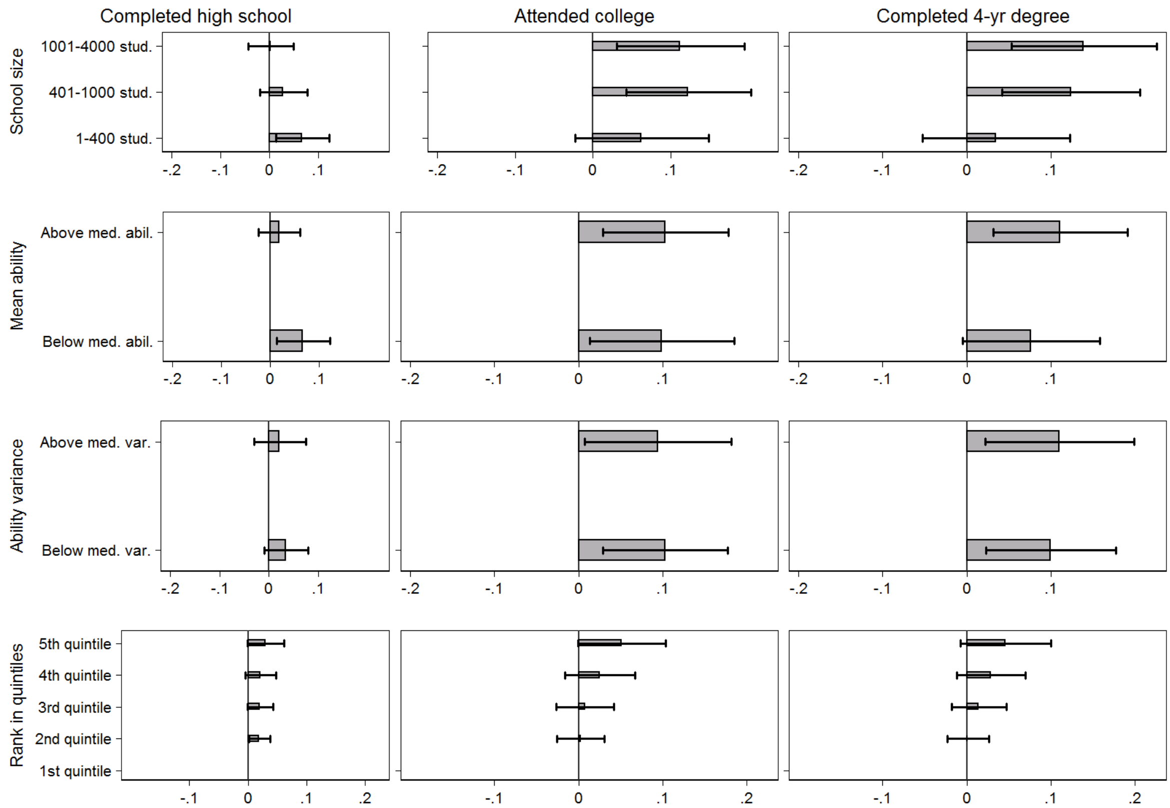

Figure 2: OLS results: heterogeneous effects across school types and grades

Note: The first three rows display the marginal effects of rank on educational attainment for different school types. The bottom panel displays the point estimates for quintile dummies of the within-grade ability distribution, with the lowest quintile as the base category.

this section we discuss various sources of bias and demonstrate that the results are robust to numerous specification checks. Table 7 displays the results. The baseline results in row 1) serve as a benchmark.

Table 4: Robustness checks

	Dependent variable		
	Completed high school	*Attended college*	*Completed 4-year degree*
Regressor: relative ability rank			
1) Baseline estimates	0.034	0.103***	0.101**
	(0.025)	(0.038)	(0.040)
2) Control for conscientiousness and neuroticism	0.033	0.092**	0.096**
	(0.025)	(0.038)	(0.040)
3) Keep if age = 0.4 ± mean age	0.006	0.088*	0.115**
	(0.026)	(0.051)	(0.054)
4) Control for average cohort ability	0.054*	0.126***	0.081*
	(0.030)	(0.045)	(0.047)
5) Keep grades with female share 40-60%	0.047*	0.129***	0.130***
	(0.027)	(0.049)	(0.048)
6) Control for gpa	0.022	0.075**	0.066*
	(0.024)	(0.037)	(0.038)
7) Relative rank within gender group	0.029	0.119***	0.080***
	(0.019)	(0.031)	(0.031)

Note: This table displays estimation results for Equation (3). In Panel B the ordinal ability rank has been replaced by the gpa rank, and the controls for ability have been replaced by a control for gpa. Standard errors, clustered at the school-grade level, are displayed in parentheses, with significance levels * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Does the rank measure reflect non-cognitive skills? One concern with the Peabody test score as a measure for cognitive ability is that it could in part reflect personality traits. Peabody is not a high-stakes test, and students had no particular incentive to achieve a high test score. Therefore, one would expect more conscientious students to put more effort into the test and achieve a higher score. Exploiting information on conscientiousness and neuroticism in the first wave of AddHealth, we carry out two tests to assess whether our rank measure in part reflects non-cognitive skills. We first regress the relative rank on both personality measures, controlling for individual characteristics, as well as school and grade fixed effects. If the rank measure reflected non-cognitive skills, we would expect statistically significant coefficients for both non-cognitive skills. As shown in Appendix B, the coefficients are close to zero and statistically insignificant. In Column 2) of Table 7, we also include both measures as endogenous control into the regression and find no significant difference in the estimates compared to the baseline specification.

Are the results affected by strategic delay of school entry? The central identifying assumption is that being in a cohort is as good as random. This assumption holds if a student's birthday determines in what year she enters the school. However, as shown by Deming & Dynarski (2008), academic redshirting — delaying school entry to allow their children to mature for another year — is widespread in the US. In Column 3) of Table 7, restrict the sample to age bands of 0.4 years around the mean age of an entire cohort. Students who were redshirted, or students who repeated a grade, would not be in this sample. The results show that strategic delay is not a threat to our identification.

Are the baseline results affected by average peer quality? Much of the peer effects literature shows that the average peer ability has a positive impact on individual student outcomes (Angrist, 2014). In our baseline model in Equation (3), average peer ability — and in fact any other school-grade-specific characteristic — would be an omitted variable, and bias the estimate of γ. By using a more demanding specification with *school* $\times$ *grade* fixed effects, we can net out all confounders at the grade-level within a school, and we have shown in Column 6) of Table 3 that the results do not change dramatically. However, including *school* $\times$ *grade* fixed effects takes out a lot of variation, and identifies the effect only from the differences in the variance of ability across cohorts within a school. In Table 7 Row 4) we present the results of a more straightforward specification, by including average peer ability as an additional control in Equation (3). The results are similar to those from a model with *school* $\times$ *grade* fixed effects. The estimate for college attendance is higher than in the other models, but the difference in coefficients is not statistically significant.

Is grade the relevant peer group? We were also interested whether students in the same grade are the relevant comparison group, or whether more narrowly defined peer groups matter more. One such group are students of the same sex in the same grade. It may matter more if a girl is the best among all girls, than it matters if she is the best among everyone in the grade. In Row 7) of Table 7, we replace the relative rank in a school grade with the relative rank within a gender group within a grade. The results are almost identical as in the baseline specification.

Measurement error One potential source of bias is measurement error in our rank measure. Measurement error could arise from the over-sampling of minorities. If minorities are in the bottom half of the within-grade ability distribution, and if they are over-sampled compared to white Americans, then white Americans would be assigned a higher relative rank than under random sampling. The survey design offers an opportunity to assess the size of the measurement error through the sequencing of the sampling. First, a random sample was drawn and labeled as the *core sample*, and second, additional students were drawn from given minorities. Hence, we observe for each student in the sample the rank with and without over-sampling. The correlation in the relative ranks in both samples is 0.9867, which indicates that measurement error from over-sampling is negligible.

A further source of measurement error is the gender stratification. Within each school grade, equal numbers of boys and girls were drawn, unless the school is a single-sex school. This sampling could introduce measurement error in the rank measure, if the gender distribution within a grade is skewed. Consider a grade of 100 students, of which 20 are female. If we draw 17 male and 17 female students, then we would sample 85% of all females, but only 21% of all males in a grade. To assess the extent to which stratified sampling affects the estimates, we re-estimate the baseline model, but exclude grades in which one gender group has a share greater than 60%. The remaining sample consists of 9,747 observations. We compute the population gender distribution for each school grade from the in-home sample of AddHealth, which covers all students in a grade, with the exception of students who were absent on the survey date. The results in Row 4) suggest that stratified sampling indeed introduces measurement error and biases the estimates towards zero: the effects are around 20% larger for the restricted sample compared to the sample with all grades.

Finally, measurement error could arise from the random sampling of students within grades. Apart from a small number of saturated schools, around 25% of all students in a school grade were drawn at random. Suppose that in a given grade students from the lower end of the ability distribution are over-represented in the sample. Based on the sample we would ascribe to some students a higher rank than they actually have in the population. But due to the random sampling of students within a grade, this measurement error should be standard, and lead to a downward-bias in the results. The heterogeneous effects in Figure 2 give us some idea about the size of the bias resulting from measurement error. Given that the same number of students is drawn regardless of the grade size, we would expect the measurement error to be larger, and the estimates to be smaller in larger schools. While this is no proof, the evidence goes against a large measurement error, as we find smaller effects in smaller schools.

Selective attrition An additional source of bias is selective attrition. With 25%, AddHealth has a low attrition rate between waves I and IV, but this attrition may not be random. For example, if students with a lower rank have higher attrition rates, we would over-estimate the effect. We address this concern in a robustness check in Appendix B, in which we estimate Equation 3 with the attrition status as the dependent variable, and find no evidence for a systematic relationship between rank and attrition.

Ability vs. GPA We have based our rank measure on a standardized ability test for two reasons. First, it allows for comparisons across schools and grades. A student who scored 100 in one school has on average the same ability as a student who scored 100 in another. The same holds for students with the same score in different school cohorts — the variation we are exploiting. Second, almost every student in the in-home sample of AddHealth took part in the test. A further, and arguably more salient metric to rank students, is GPA. AddHealth contains information on grades in English, math, history, and science, from which a GPA can be computed. Because grades are self-reported, and because of many missing observations, grades are noisier than Peabody scores. We assess how important GPA is in explaining the results, by including it as an additional endogenous regressor. If the ability rank was merely reflecting GPA, then the coefficient of the ability rank should be small and statistically insignificant. As shown in Row 6) of of Table 7, including GPA reduces the coefficients by around one third, but they remain large and statistically significant. GPA can be seen as one channel, through which the ability rank affects outcomes, but we can exclude that GPA is a mere proxy for the ability rank.

4.4 Potential channels

Table 5: Regression results: rank position and intermediate outcomes, wave I

Dependent Variable	Coefficient	SE
Self-concept		
1(I am more intelligent than the average)	0.090*	(0.046)
Expectations		
1(I want to go to college)	0.028	(0.040)
1(I will likely go to college)	0.082*	(0.042)
1(I will have a college degree by the age of 30)	0.106**	(0.043)
Intrinsic factors		
1(I was often hopeful last week)	0.110***	(0.042)
1(I was often happy last week)	-0.002	(0.040)
1(I was often depressed last week)	0.029	(0.029)
1(I was often fearful last week)	0.022	(0.018)
Effort		
1(I was absent at school without excuse)	-0.115***	(0.043)
Support from others		
1(I feel that teachers care about me)	0.085*	(0.049)
1(I feel that parents care about me)	0.003	(0.031)
1(I feel that friends care about me)	-0.003	(0.017)

Note: This table displays the results for separate OLS regressions of the outcomes listed in the first column on relative ability rank within a school grade. Each outcome is a dummy variable with value 1 if an event occurred often or was very likely, and zero otherwise. All regressions include school fixed effects, grade fixed effects, and control for individual ability, age, minority dummies, and parental characteristics. Standard errors, clustered at the school-grade level, are displayed in parentheses, with significance levels * p<0.10, ** p<0.05, *** p<0.01.

The baseline results show a significant causal impact of ordinal rank in high school on human capital investment later in life. The question remains which mechanisms can explain this reduced-form relationship. Here we present theoretical arguments for four mechanisms, and use the rich survey information provided in AddHealth to analyze which of these mechanisms dominates. We run two sets of regressions. First, we explore to what extent rank affects a

Table 6: Regressions with mediators as endogenous controls

	Dependent variable		
	Completed high school	Attended college	Completed 4-year degree
Baseline estimates	0.034	0.103***	0.101**
	(0.025)	(0.038)	(0.040)
Including endogenous controls:			
With self-concept	0.031	0.091**	0.089**
	(0.025)	(0.038)	(0.039)
With expectations	0.030	0.084**	0.082**
	(0.025)	(0.037)	(0.038)
With intrinsic factors and effort	0.025	0.079**	0.083**
	(0.025)	(0.038)	(0.040)
With support from parents, teachers, friends	0.031	0.095**	0.094**
	(0.025)	(0.038)	(0.039)
With all variables	0.015	0.053	0.048
	(0.024)	(0.036)	(0.037)

Note: The results in this table show by how much the point estimates of the relative rank are attenuated when mediating variables are included. The table displays OLS estimates for the baseline model in Equation 3, including the variables outlined in Table 5 as additional regressors. Standard errors, clustered at the school-grade level, are displayed in parentheses, with significance levels * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

number of mediating variables. We re-estimate the model in Equation (3), using as dependent variable a dummy that equals one if the student strongly agrees to a given statement, and zero otherwise. Table 5 displays the results of this exercise. Second, we analyze the strength of these variables in explaining the impact of rank on later outcomes, by including the mediating variables as additional regressors in the baseline model. If a channel matters for the causal pathway from rank to later outcomes, the coefficient for the relative rank should be smaller once a proxy for the channel is included.

Rank as a noisy signal of own ability One mechanism could be that the rank provides students with a noisy signal about their own ability. There is ample evidence that students in general have imperfect knowledge about their actual ability (Jensen, 2010; Zafar, 2011; Stinebrickner & Stinebrickner, 2012; Bobba & Frisancho, 2014). The ordinal rank can be one of the reasons for this imperfect knowledge. Students compare themselves to their immediate peers, and use their *local rank* to infer their *global rank* in the overall ability distribution. A student who believes that she has a low overall ability because she compares herself to better peers, may invest less in her human capital.

We first assess whether students with a higher rank have a higher perceived ability, as suggested in the psychology literature (Marsh, 1987). As shown in the first panel of Table 5, this is indeed the case. In wave I of the survey, students were asked if they think that they are more intelligent than the average. Conditional on absolute ability, students with a 10 percentage points higher rank are 0.9 percentage points more likely to consider themselves more intelligent than the average. Table 5 shows that self-concept can explain part of the overall effect of rank on later outcomes. The coefficients for the college variables are around 10 percent lower once self-concept is included.

Furthermore, we proxy for expected returns to education with various measures for career expectations. In wave I, students were asked they want to go to college, whether they will likely go to college, and whether they expect to have a college degree at the age of 30.

We find strong support that rank affects expected returns to education. As shown in the second panel of Table 5, students with a higher rank have a higher probability of stating that they will likely go to college, and they are more likely to think that they will have a college degree by age 30. Remarkably, the impact of rank on college expectations at age 16 is equally large as the impact of rank on actual college outcomes more than 10 years later. Also, as shown in Table 6, including expectations attenuates the baseline estimate by around 20%.

Intrinsic factors and effort The effect could also be explained by intrinsic factors. As suggested by the literature on relative comparisons and effort provision, a higher rank may give students a greater motivation, make them more self-confident, and ultimately induce them to exert more effort in their studies (Clark *et al.*, 2010; Azmat & Iriberri, 2010).

The third panel in Table 5 displays the results on intrinsic factors, exploiting questions from a survey module on mental distress. Students with a higher rank are significantly more optimistic, while we find no effect of rank on happiness, depression, or fearfulness. To proxy for effort, we use self-reported information on school absences, and construct a dummy that equals one if the student has been absent without excuse at least once in the last school year. We find that students with a higher rank are significantly less likely to be absent without excuse, which indicates that they take their studies more seriously and put more effort into it. In Table 6 we include the intrinsic factors and the absence dummy into the baseline regression. The coefficients are around 20% smaller.

Behavioral responses from teachers, parents, and friends A further potential channel is behavioral responses from a student's environment. As shown by Pop-Eleches & Urquiola (2013), teachers and parents are responsive to a student's relative position within their school. They compare marginal students who just made it into a high-quality school to those who didn't, and find parents provide less effort when their child attends a better school. Moreover, teachers could have a preference for and give support to students with a higher rank.

In the fifth panel of Table 5, we show the effect of relative rank on perceived support from teachers, parents, and friends. The ordinal rank has indeed a positive impact on the perceived support from teachers. Students with a higher rank are more likely to feel that their teachers care about them. The effects on the perceived support from parents and friends, in contrast, are small and statistically insignificant. Moreover, when we include the support variables into the main regression, the coefficient of the relative rank does not decrease by a large amount, indicating that the support from the environment plays a minor role in explaining the result.

Selective college admissions Finally, the effect could be driven by college admissions policies. One such policy is affirmative action, that is, colleges give preferential access to students with specific characteristics, for example women, blacks, or hispanics, or to students from poorer families. While affirmative action has been shown to significantly distort the sorting into colleges (Arcidiacono, 2005), it should not explain our results, because we control for many characteristics that define the minorities targeted by affirmative action.

However, following prominent lawsuits in the mid-1990s, affirmative action has been abandoned by many state colleges in the US. California, Texas, and Florida introduced ten-percent plans instead, granting automatic access to flagship state universities to students in the top 10 percent of their high-school cohort. Daugherty *et al.* (2014) for Texas and Arcidiacono *et al.* (2014) for California give evidence that the introduction of these plans changed the composition of students at flagship state colleges. In Texas, attendance and completion rates at flagship colleges increased, but more so for students from high-ability high schools, while in California the college attendance rates of blacks increased, but they went to lower-ranked colleges. Even though the first wave of AddHealth was collected 3 years before these plans were introduced, they could still affect the younger cohorts, and give our results a mechanical interpretation.

Besides these plans that specifically apply to students with a given rank, the effect of rank on college outcomes can more generally be driven by selective college admission policies. Students typically apply for college with their 11th-grade results, which often state the percentile of a student in the GPA distribution of her grade. If college admission officers have this information, and if GPA rank is positively correlated with the ability rank, then our result could reflect a pure mechanical effect: colleges only admit students with a higher rank, which is why we observe higher college attendance rates for highly ranked students. While we do not have direct information on the type of college students apply or are admitted to, in Section 4.3 we provide evidence that college admissions can — if at all — only in part explain the results.

5 Conclusion

This paper shows that a student's rank in the ability distribution within a high school cohort is an important determinant for educational attainment later in life. If Jack and Jim have the same ability, but Jim is the brightest student in his grade, while Jack ranks in the middle of his grade, our results predict that Jim is more likely to get a college degree and to complete high school than Jack. This effect runs counter to most of the literature on peer effects, which finds that being exposed to high-achieving peers has a positive effect on educational attainment.

The results should concern parents and policymakers alike. Parents could derive from this study that it is better to send their child to a school in which he or she has a higher rank, that is, it is better to send a child to a school with lower-ability peers. However, our results reflect local effects, which we obtained by comparing students within the same school but in different cohorts. If parents chose schools based on their children's rank, such a behavior would be problematic, however, because the positive effect of having a higher rank could be compensated by a lower peer quality, and generally a lower school quality. Moreover, if all parents choose schools according to their children's rank, this would result in a difficult choice problem, and the the general equilibrium outcomes would be far from clear.

Policymakers should be concerned as well, because the results suggest that the selection into schools and the transition into college leads to a low-level equilibrium: if parents try to send their kids to the best possible schools, and if a child's rank within the school is important for educational attainment, potentially fewer students complete college than would be optimal given their absolute ability. For a government, this underinvestment in human capital is not optimal. Given that the ordinal rank depends on the mean ability as well as on the ability distribution within a class, it is difficult to think of an effective algorithm that changes the ability composition of schools in order to encourage more investment in human capital.

A potentially more efficient policy would be to give more support to students at lower ranks of the ability distribution, in order to compensate for the negative impact of their rank. Especially for students with a low rank in high-ability schools — small fishes in big ponds — providing them with information on their absolute ability could be an inexpensive and effective way to increase their educational attainment. Recent experimental studies by Azmat & Iriberri (2010), Tran & Zeckhauser (2012), Hastings & Weinstein (2008), Bettinger *et al.* (2012), and Wiswall & Zafar (2015) have shown that students are indeed responsive to these type of interventions.

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A Disclaimer

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B Robustness checks

Table 7: Robustness checks

	Dependent variable	
	Relative rank	*Attrition dummy*
Neuroticism	-0.001	
	(0.001)	
Conscientiousness	0.001	
	(0.001)	
Relative rank		-0.010
		(0.036)

Note: This table displays estimation results for Equation (3). In Panel B the ordinal ability rank has been replaced by the gpa rank, and the controls for ability have been replaced by a control for gpa. Standard errors, clustered at the school-grade level, are displayed in parentheses, with significance levels * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

In this section, we show that the results are robust to various specification changes. Panel A in Table 7 presents the results for the baseline model with different regressors of interest or additional regressors. The first row displays the baseline results for comparison.

Peabody test scores and non-cognitive skills One concern with using the Peabody test scores as ability measures is that Peabody is a low-stakes test in which students have little incentive to do well. In light of this, more conscientious students may perform better on the test simply because they take the test more seriously. In Row 2), we include measures for conscientiousness and neutoticism, measured in wave I, as additional regressors. If they were significant confounders, their inclusion should fundamentally change the results. As can be seen in Row 2), this is not the case.

Selective attrition In Section **??**, we explained that the results could be biased due to selective attrition, for example if we are less likely to observe lower-ranked students in wave IV. To assess whether selective attrition is an issue for our analysis, we re-estimate the baseline model on the full sample of wave I, and use as outcome variable an attrition dummy that equals one if the person is *not* in the sample in wave IV. As shown in Panel B of Table 7, selective attrition is unrelated to rank and should not lead to a systematic bias in our estimates.