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Rank, Sex, Drugs, and Crime

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ABSTRACT

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In this paper we show that a student's ordinal rank in a high school cohort is an important determinant of engaging in risky behaviors. Using longitudinal data from representative US high schools, and exploiting idiosyncratic variation in the cohort composition within a school, we find a strong negative effect of a student's rank on the likelihood of smoking, drinking, having unprotected sex, and engaging in physical fights. We further provide suggestive evidence that these results are driven by status concerns and differences in career expectations.

JEL Classification: I12, I14, I21, I24

Keywords: risky behavior, ability rank, peer effects, beliefs, expectations

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1 Introduction

Risky health behaviors of adolescents, such as smoking, binge drinking, and unprotected sex, are suspected to have immediate negative impacts on educational achievements, and far-reaching consequences for a person's labor market prospects and health (Carrell et al., 2011; Cawley and Ruhm, 2011; Carpenter and Dobkin, 2009). Likewise, delinquent behavior early in life can lead to criminal careers later on. To prevent adolescents from these negative consequences, it is important to know the determinants of risky behaviors. A major determinant identified in the literature is spill-over effects from peers: the more likely one's peers are to smoke, drink, or take drugs, the more likely that person is to engage in these behaviors.

In this paper, we explore an additional channel through which peers affects risky behaviors: a student's ordinal rank in a school cohort. In some cohorts the same student is surrounded by smarter peers than in others, and, therefore, would have a different ordinal rank in different cohorts. Exploiting natural variation in the cohort composition within a school, we show that students with a low ordinal rank in their cohort are significantly more likely to engage in various risky behaviors. This effect comes on top of average peer effects, which are netted out in our empirical strategy.

We use data from the National Longitudinal Study of Adolescent Health (AddHealth), a representative panel survey of US middle and high school students which offers several key features for our analysis: multiple cohorts were sampled within each school, allowing us to apply a within-school/across-cohort design, and to observe each student's peer group in high school. Moreover, the survey contains a standardized cognitive ability test, which makes students' ability comparable across schools and cohorts. Finally, the survey includes detailed information on risky behaviors, expectations, attitudes, and feelings, helping us to explore potential mechanisms behind the reduced-form results.

We measure a student's rank as her ordinal position in the ability distribution of her school cohort. Figure 1 shows how the ordinal rank relates to risky behaviors, after controlling for school fixed effects and a student's absolute cognitive ability. The partial correlations point to a clear negative relationship between the ordinal rank and the propensity to engage in risky behaviors. To give these correlations a causal interpretation, we exploit idiosyncratic variation in cohort composition within a school and the fact that students with the same absolute ability have different ordinal ranks in different cohorts. Under the identifying assumption that being in one cohort or another is determined by a student's birth date, the variation in the ordinal rank can be considered quasi-random, such that any differences in risky behavior can be interpreted as causal.

Applying this research design, we find a large and statistically significant negative effect of a student's ordinal rank on the propensity to smoke, drink, have unprotected sex, and engage in physical fights. A one-decile increase in the ordinal rank decreases the propensity to smoke and drink regularly by 0.8 percentage points each (relative to a mean of 14%), decreases the

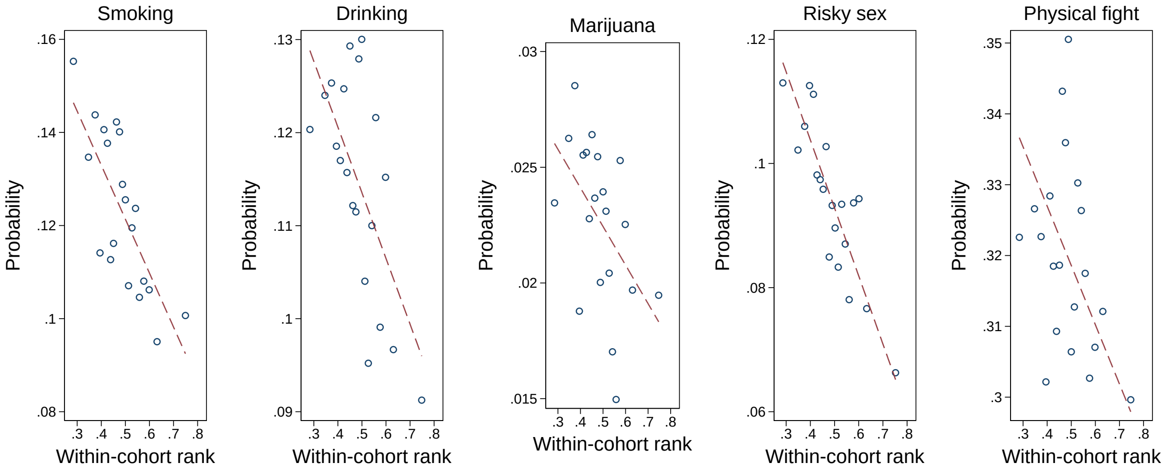

Figure 1: Partial correlations: ordinal rank and risky behavior

Notes: Bin scatters using 20 bins illustrating the relationship between likelihood of risky behavior and the ordinal rank within a school cohort (0=lowest rank, 1=higest rank), conditional on absolute ability and school fixed effects.

likelihood of having unprotected sex by 0.5 percentage points (relative to a mean of 9%) and decreases the probability of engaging in physical fights by 1 percentage point (relative to a mean of 20%). We also find a negative sign for stealing, marijuana use, and drug selling, but the effects are small and imprecisely estimated.

These findings are in line with two theoretical models. First, they can be reconciled with a human capital model, in which a student has imperfect information about her absolute level of ability. The ordinal rank provides a noisy signal about a student's actual ability, thereby distorting the trade-off between the short-run pleasure and the long-run costs of risky behaviors. A student with a high absolute ability but a low ordinal rank may incorrectly infer that she has a low absolute ability, a low expected income, and, therefore, low opportunity costs of engaging in risky behaviors. Second, the result can be explained by sorting into peer groups. Students have status concerns, and choose behaviors in order to conform with a peer group. Which peer group they choose to conform with depends on the ordinal rank in the ability distribution. Cicala et al. (2014) present such a model with two groups: "nerds," who attain a high status through high educational achievement, and "troublemakers," who attain a high status through engaging in risky behaviors. Highly ranked students have a comparative advantage in being "nerds," while students with a low rank have a comparative advantage in being "troublemakers" and thus are more likely to engage in risky behavior. The same student who has a low rank and becomes a troublemaker in one cohort, may choose to become a nerd in another cohort where she has a high rank.

We further investigate which of these mechanisms are supported by the data, which is based on extensive survey information on expectations and attitudes. We find that, conditional on absolute ability, students with a low rank have significantly lower expectations about their future educational attainment, and have a lower perceived intelligence. These results lend support to the human capital model. We also find evidence for status-seeking behavior, especially with respect to sexual intercourse. On the contrary, we find no systematic relationship between the ordinal rank and perceived support from friends, teachers, or parents, or between the rank and a student's self-esteem.

In a series of robustness checks, we carefully address several threats to identification. To fully disentangle the impact of the ordinal rank from the impact of being in a high-ability cohort, we estimate a model with school-specific cohort fixed effects, which rules out that the results may be driven by dynamic selection into schools, differences in school inputs across cohorts, or any other school-cohort-specific confounders. A further threat is reverse causality. Students may perform badly on the ability test *because* they had been drinking or consuming drugs, in which case the causality would run from risky behavior to ability. We address this concern by including contemporaneous risky behavior as a lagged dependent variable, thus estimating the impact of the ordinal rank on the *change* in risky behavior between two waves of the survey. In both cases, the results are robust to these more demanding specifications. Finally, the sampling design of the survey could introduce non-classical measurement error. Based on Monte Carlo experiments, we show that measurement error leads to a downward-bias in the estimates.

This paper provides new insights into the determinants of risky behaviors among adolescents. In particular, it complements the literature on peer effects, which has found substantial spillovers of risky behaviors (Gaviria and Raphael, 2001; Lundborg, 2006; Clark and Lohéac, 2007; Soetevent and Kooreman, 2007; Argys and Rees, 2008; Eisenberg et al., 2014).[1] Based on our identification strategy, we demonstrate that a student's ordinal rank is an additional, and equally important, channel through which a peer group affects behavior. The paper also relates to the work of Balsa et al. (2014), who identify relative material deprivation as a determinant of risky behavior: students with a higher social status within their cohort are less likely to engage in risky behavior. Our paper complements their findings by showing that, after controlling for social status, the relative ability of a student is an equally strong determinant of risky behaviors.

The paper also stresses the importance of ordinal rank in education. Recent research has found a significant impact of a student's ordinal rank in a cohort or classroom on test scores (Azmat and Iriberri, 2010; Murphy and Weinhardt, 2014; Goulas and Megalokonomou, 2015). Moreover, as shown by Tincani (2015), rank concerns among students are an important deter-

[1]Besides the studies cited here, which all document peer effects for multiple behaviors, a wealth of studies focuses on single behaviors, for example Kremer and Levy (2008) and Fletcher (2012) on drinking, Card and Giuliano (2013) on intercourse, Krauth (2007) and Fletcher (2010) on smoking, and Lin (2014) on delinquent behavior.

minant of peer effects in student achievement. If the ability distribution of a classroom has a low variance, students have a greater incentive to work harder in order to achieve a higher rank. This paper extends previous work, where we show with the same data that a student's ordinal rank significantly affects decision to go to college (Elsner and Isphording, 2015). In the present paper, we demonstrate that the ordinal rank is equally important for non-education outcomes, as it can trigger behaviors that have long-lasting consequences for personal development. Moreover, in line with research in psychology (Marsh, 1987; Marsh et al., 2007), we find that the effect can partly be explained by academic self-concept, as well as expectations about future income. Students with a higher ordinal rank perceive themselves as more intelligent, and have higher expectations about their career, which makes engagement in risky behaviors more costly.

2 Data and descriptive statistics

We use data from AddHealth, which offers several features that are key to our analysis. Within every school, we observe multiple cohorts, which allows us to hold school characteristics constant, and implement a within-school/across-cohort design. Moreover, since one of the main aims of AddHealth is to study the interaction between education and the health behavior of adolescents, the survey includes detailed information on various types of risky behaviors. In the following, we describe the dataset and the sample, and present descriptive statistics for the main outcome variables.

2.1 The AddHealth dataset

AddHealth is a panel survey of 144 representative middle and high schools in the US. Students are followed from adolescence into adulthood in four waves. Within each school, up to six different cohorts have been sampled in wave I of the survey in 1994/1995. Each cohort is observed at a different grade level, from grade 7 to grade 12, which is why we use the terms *grade* and *cohort* interchangeably.

We use the *In-Home* sample of AddHealth, which includes comprehensive information on health conditions and behavior, family environments, cognitive ability, educational achievement and friendship relationships. In most schools, a random sample of 17 boys and 17 girls was drawn from every grade level and included in the in-home sampled. In addition, students from specific minorities were oversampled (Puerto Ricans, Chinese, Cubans, high-educated blacks, twins, siblings, students with disabilities). A small number of schools was sampled completely.[2]

We drop from the sample all schools with 20 individuals or less (109 obs.) and all grades with 5 students or less (304 obs.). Furthermore, we delete from the in-home survey all observations with missing information on one or more control variables (746 obs.), and all observations with missing information on risky behaviors in wave II (5,065 obs.). The final sample consists of

[2]The sampling design is described in greater detail in Harris et al. (2009) and Harris (2009)

12,446 students in 130 schools and 459 school-cohort combinations. Table 6 in Appendix B displays the summary statistics for the main control variables.

2.2 The ordinal rank

Our regressor of interest is a student's ordinal rank in a high school cohort. We construct the rank based on a standardized measure of cognitive ability, which is available for all students in our sample.

The in-home sample of AddHealth includes an abridged version of the Peabody Picture Vocabulary Test, which was carried out face-to-face between the interviewer and the respondent. The test is age-specific, and consists of multiple rounds. In each round, students are shown four pictures and are given a word, which they have to match to the picture that fits best. With every round, the test increases in difficulty. From this series of answers, standardized scores are computed. More difficult tasks receive a higher weight. The test scores are not made available to the participant or the interviewer. The Peabody test has been shown to be a feasible and successful method for assessing basic cognitive abilities in large-scale surveys, with high retest reliability and cross-validity with alternative intelligence tests (Dunn and Dunn, 2007).

Based on the Peabody score, we rank all students within a school cohort, assigning rank 1 to the student with the lowest score, and rank N - the total number of students in the cohort - to the student with the highest score. To ensure comparability across grades with differing size, we compute a student's relative rank position by standardizing the absolute rank to the cohort size,

$$\text{percentile rank} = \frac{\text{absolute rank} - 1}{\text{nr of students in school cohort} - 1},$$

which results in a rank measure that is bounded between 0 (the lowest scoring student) and 1 (the highest scoring student).

The main advantage of using Peabody scores as a base for the ranking is their comparability across cohorts within a school. A potential alternative metric would be grades, which are more visible to the student than cognitive ability. But grades come at the disadvantage of not being standardized within and across schools. Rather, many teachers apply *grading-on-a-curve*, i.e., they grade exams according to an a-priori determined distribution.[3] With such a grading scheme, two students with the same grade point average (GPA) in the same school may differ considerably with respect to ability and other characteristics. In addition, grades in AddHealth are self-reported and have many missing observations, making them less suitable for our analysis than the Peabody test. Nevertheless, we will later perform robustness checks with the rank variable based on GPA.

[3] See Dubey and Geanakoplos (2010) for a theory that explains the work incentives for students under grading on a curve, and the incentive for schools to implement it. Piopiunik and Schlotter (2012) provide empirical evidence for grading on a curve in German primary schools.

One might be concerned whether students actually know their rank, given that school cohorts are large and that students do not observe their score on the ability test. While the precise rank in a large peer group may not be perfectly observable, it is plausible that students know more about their relative ability in their cohort than about their absolute ability. As we will show later, students with a higher rank have a higher perceived intelligence compared to students in the same school with the same ability but with a different rank. This provides evidence that students have a relatively precise idea about their position in the ability distribution of their cohort.

2.3 Outcome variables: risky behaviors

We consider as dependent variables five types of risky behaviors: smoking, binge drinking, marijuana consumption, sex, and delinquent behavior. All dependent variables are constructed as binary indicators. For smoking, drinking, and marijuana use, we exploit information on the intensity, and construct indicators for whether student recently consumed at all, and whether they consumed regularly.

All behaviors are self-reported. To encourage honest answers, sensitive questions on drug consumption, sex, and delinquent behavior were assessed via computer-assisted audio interviews. The questions were played to the participant via headphones, and the answers were anonymously typed into a laptop.

The questions are available in wave I and II of AddHealth. Because the questions are retrospective (e.g., "During the past 30 days, on how many days did you smoke cigarettes?"), we use the answers from wave II as outcome variables, and regress them on the ordinal rank in wave I. Wave II was collected in 1996, around 18 months after wave I. We construct the variables as follows:

Smoking and marijuana use. The indicators for recent and regular smoking are based on the question "During the past 30 days, on how many days did you smoke cigarettes?". We define as *recent smokers* those who have smoked at least once during the past 30 days, and as *regular smokers* those who smoked on at least 20 out of the last 30 days. The same definitions apply to recent and regular marijuana use.

Binge drinking. The indicators for binge drinking are based on the question "Over the past 12 months, on how many days did you drink five or more drinks in a row?". *Recent drinking* includes any binge-drinking during the past 12 months. *Regular drinking* is defined as drinking 5 drinks in a row at least once a week.

Sex. With respect to sex, we consider two outcome variables: *intercourse within the past 6 months* (based on the question "In what month and year did you have sexual intercourse most recently?"), and a binary indicator for *risky intercourse*, which equals one if neither the respondent nor her partner used contraceptives during their most recent intercourse.

Delinquent behavior. Finally, we assess three categories of delinquent behavior. We construct binary indicators for stealing (including shoplifting and burglary), engagement in physical fights, and drug selling if a person reported that she engaged in these behaviors at least once in the last 12 months. For delinquent behaviors, we only observe the incidence and not the intensity.

Descriptive statistics. Table 1 lists baseline probabilities of risky behaviors in wave II of AddHealth for different subgroups. For smoking, drinking, and marijuana use, the share of regular users is considerably smaller than the share of occasional users. Around one-third of all students is sexually active, and one-third of all sexually active students recently had intercourse without protection. For all behaviors except for risky intercourse, boys show higher rates of engagement than girls. With respect to parental background, there is no noticeable gradient in most risky behaviors. An exception are students whose parents have a college degree. They are less likely to engage in most behaviors compared to students whose parents have a lower educational attainment. Risky behaviors also differ along racial and ethnic lines. Whites and Hispanics have higher rates of engagement than Asians and Blacks. Finally, we report the probabilities for different grade levels. Students in grade 7 were on average 13 in wave I, while those in grade 12 were on average 18-years-old. With the exception of delinquent behaviors, engagement in risky behaviors increases with age.

The baseline probabilities for smoking, drinking, marijuana use, and sexual activity, as well as the age gradients in these behaviors, are similar to those reported in Argys and Rees (2008), which were based on the National Longitudinal Survey of Youth 1997 (NLSY97).

3 Empirical strategy

Our aim is to estimate the causal impact of a student's ordinal rank on risky behaviors. In this section we explain how we identify the effect by exploiting differences in the ability distribution across cohorts within a school. We first describe the empirical model that allows us to isolate the identifying variation and then discuss the identifying assumptions. We also briefly point out some threats to identification, but provide a more extensive discussion along with the results.[4]

3.1 Identification

Figure 1 in the introduction has revealed a significant negative relationship between the within-cohort rank and engagement in risky behavior. This correlation cannot simply be interpreted as

[4]This section draws from Elsner and Isphording (2015) where we use a similar set-up to study the long-term impact of ordinal rank on educational attainment later in life.

Table 1: Risky behavior by group

Group	*Smoking*		*Drinking*		*Marijuana*		*Sex*		*Delinquent behavior*		
	Recent	Regular	at all	regular	Recent	Regular	Interc.	w/o birth control	Stealing	Fights	Drug selling
All	0.32	0.14	0.28	0.13	0.18	0.04	0.34	0.09	0.23	0.20	0.08
Male	0.33	0.15	0.31	0.16	0.19	0.06	0.33	0.08	0.26	0.26	0.12
Female	0.32	0.14	0.26	0.09	0.16	0.03	0.34	0.10	0.20	0.14	0.04
Parental background:											
Less than high-school	0.32	0.15	0.28	0.14	0.17	0.05	0.40	0.15	0.24	0.22	0.09
High school	0.35	0.17	0.29	0.14	0.17	0.04	0.35	0.09	0.23	0.22	0.09
Some college	0.35	0.17	0.31	0.13	0.19	0.05	0.37	0.09	0.23	0.22	0.08
College	0.29	0.11	0.26	0.11	0.17	0.04	0.28	0.06	0.23	0.16	0.07
Race/Ethnicity:											
White	0.40	0.20	0.34	0.16	0.19	0.05	0.32	0.08	0.23	0.18	0.08
Asian	0.24	0.09	0.21	0.09	0.14	0.03	0.22	0.08	0.24	0.17	0.06
Hispanic	0.29	0.10	0.30	0.14	0.19	0.05	0.35	0.12	0.26	0.23	0.09
Black	0.08	0.04	0.13	0.07	0.15	0.03	0.41	0.10	0.21	0.23	0.08
Grade level:											
7/8	0.27	0.09	0.17	0.07	0.13	0.03	0.17	0.05	0.25	0.22	0.06
9/10	0.34	0.16	0.30	0.13	0.19	0.05	0.34	0.09	0.24	0.20	0.09
11/12	0.36	0.18	0.38	0.19	0.21	0.06	0.50	0.13	0.20	0.17	0.09

Notes: This table displays mean probabilities of risky behaviors across socioeconomic groups in wave II of AddHealth. For smoking and marijuana use, "recent" indicates that a person has smoked during the last 30 days, and "regular" indicates that she smoked on at least 20 out of the last 30 days. For drinking, "recent" indicates that the person has been binge-drinking at least once in the last 12 months, and "regular" indicates that she has been binge-drinking at least once a week during the last 12 months. "Interc." indicates whether a person had sexual intercourse in the last 6 months. "w/o birth control" equals one if a person has not used birth control during their last intercourse. The delinquent behavior variables refer to engagement in a specific activity at least once during the last 12 months.

causal, because it could be driven by selection into schools, differences in parental background, average peer quality, or other unobserved factors that may simultaneously affect a student's rank and her engagement in risky behavior.

To identify a causal effect, we compare students in different cohorts within the same school, and exploit idiosyncratic variation in the ability distribution across cohorts. Some cohorts have a higher average ability than others, and in some cohorts the ability distribution is more compressed, while in others it is more dispersed. Thus, a student with a given level of absolute ability has a different ordinal rank in different cohorts. The intuition behind this identification strategy is illustrated in Panel A in Figure 2 for two entry cohorts in the same school. The entry cohort of 1995 has a higher average ability than the entry cohort of 1994. Consequently, a student with ability level $\underline{abil}$ would be in the second rank if she entered the school in 1994, and in the third rank if she entered in 1995.

The variation in the rank does not need to come from differences in mean ability, but can also come from differences in higher moments of the ability distribution. As shown in Panel B of Figure 2, once the variance differs between both cohorts, a given level of ability leads to a different ordinal rank.[5]

The central identifying assumption of this strategy is that being in one cohort or another is as good as random or, at least, that neither the student, nor her parents, nor school principals, nor anyone else can influence the assignment into cohorts. This assumption is plausible if the age cutoff is strictly enforced, and students born before the cutoff date enter school almost one year before those born right after. It may not hold in all cases, however, because some students have to repeat grades, and parents may strategically delay their children's school entry to allow them to mature for another year. Based on a series of robustness checks, we will later alleviate these concerns.

But where would the variation in cohort composition within a school come from, and why can it be considered idiosyncratic? A combination of at least two factors can explain the source of this variation. First, school districts are relatively small compared to the overall population of the US. In the whole of the US, the ability distribution may not fluctuate very much between cohorts due to the law of large numbers, but the fluctuations are more pronounced in smaller areas where the law of large numbers does not necessarily hold. Due to natural variation, in some years students in a school district are on average brighter than in others, or the ability distribution is more even in some years and more concentrated in others. A second factor is the cutoff date for school entry. Given that parents cannot time their children's birth exactly, the cutoff leads to fluctuations in the number of children born before and after as well as to

[5]The idea behind the identification strategy displayed in Panel A of Figure 2 follows Hoxby (2000b) and Hoxby (2000a), and has been applied in many other studies, for example, Bifulco et al. (2011) and Geay et al. (2013). Our empirical specification, however, is more similar to Ammermueller and Pischke (2009), who identify average peer effects within schools across classrooms. We use the same specification, but compare students across cohorts rather than classrooms. The identification strategy illustrated in Panel B of Figure 2 has been applied by Murphy and Weinhardt (2014) and Elsner and Isphording (2015).

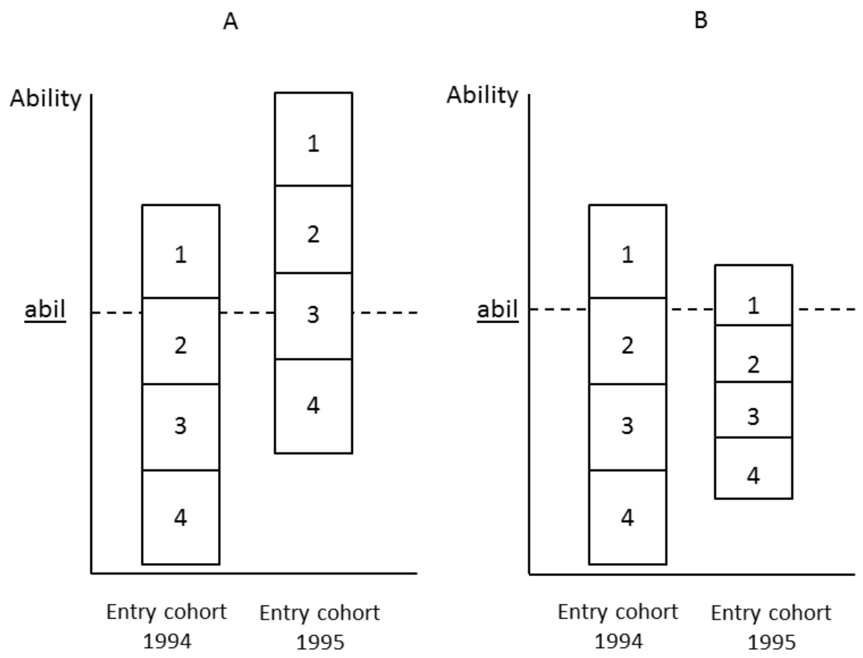

Figure 2: Variation in mean and variance of ability

Notes: This figure illustrates the sources of variation used in the identification of the rank effect. A student with a given level of ability abil has a different rank in different school cohorts if the cohorts differ in their mean ability (Panel A), or in the dispersion of the ability distribution (Panel B), or both. This graph is adapted from Elsner and Isphording (2015) and Murphy and Weinhardt (2014).

differences in their socioeconomic backgrounds.

A further important question is whether the variation in the ordinal rank across cohorts within a school is sufficient for identifying a meaningful effect. Figure 3 shows by how much the relative ability varies for a given level of absolute ability. The rank on the vertical axis is the residual of a regression of the within-cohort rank on school and cohort fixed effects. Each box plot shows for a given decile in the global ability distribution the variation of a student's ordinal rank across cohorts *within* the same school. Although the correlation is positive, it is far from perfect, which ensures that we can base our identification of the rank effect on a broad support. For example, students in the 6th percentile of the *global* distribution may end up anywhere between the 1st and the 10th decile of the *local* distribution, depending on her school and cohort.

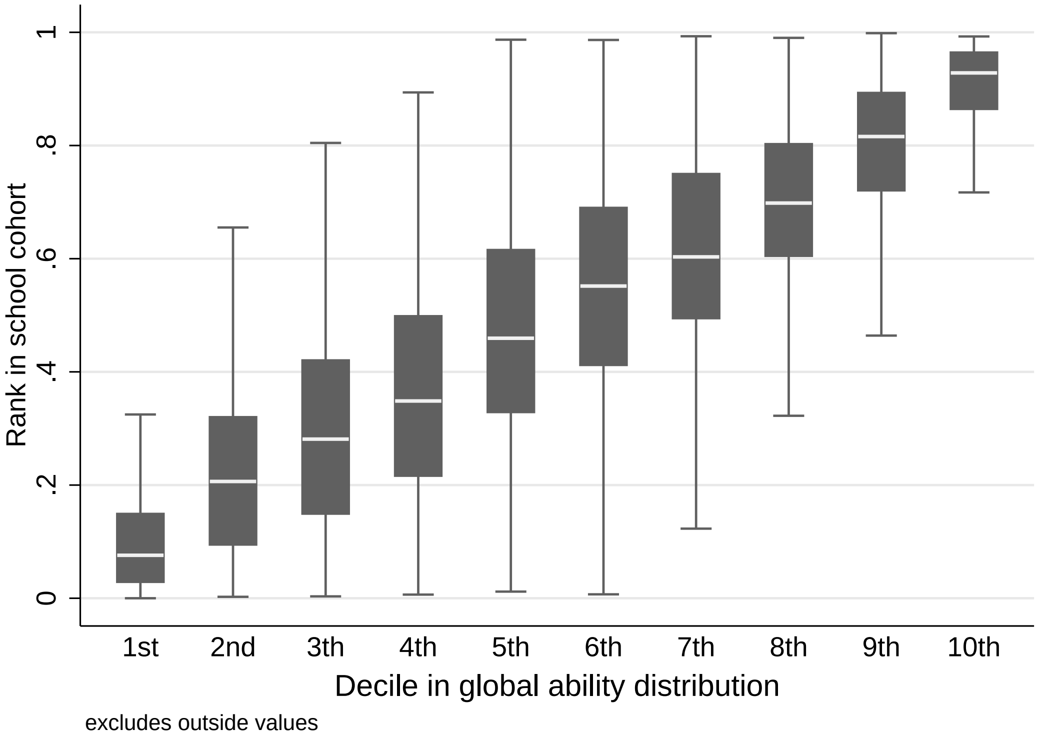

Figure 3: Global vs local rank

Notes: This figure shows to what extent the rank in a school cohort varies *within* a school, for a given decile in the global ability distribution. The boxes summarize the (75th − 25th perc.) interquartile range with the median indicated by white line and whiskers including the 1.5 × (75th − 25th perc.) range.

3.2 Basic estimating equation

As a baseline specification, we estimate a linear probability model with separate sets of school and cohort fixed effects,[6]

$$\begin{aligned} \text{risky behavior}_{ijk(t=2)} &= \gamma \text{ percentile rank}_{ijk(t=1)} + f(\text{individual ability}_{ijk(t=1)}) \\ &+ \boldsymbol{X'_{ijk(t=1)}\beta} + \text{School FE}_j + \text{Cohort FE}_k + \epsilon_{ijk}, \end{aligned} \tag{1}$$

which relates the risky behavior in wave II of student i in grade k in school j to her within-cohort percentile rank in wave I. We flexibly control for absolute ability with a fourth-order polynomial in the Peabody score, $f(\text{individual ability}_{ijk(t=1)})$. The parameter of interest, γ, is identified from differences in the mean and higher moments of the ability distribution across cohorts within a school.

[6]A linear probability model allows us to isolate the identifying variation through a large number of fixed effects, which is an advantage over probit or logit models. We believe that this advantage weighs more than the cost of obtaining predicted probabilities smaller than zero and greater than one.

To ensure that we compare individuals with the same observable characteristics, we include a rich set of individual control variables, $\boldsymbol{X_{ijk(t=1)}}$. These are: gender, age in months, height, race/ethnicity (white/black/asian/hispanic), migration background (a dummy that equals 1 if a person is a first or second generation migrant), indicators for highest parental education (less than high-school, high-school, some college), highest parental occupational status (not working/blue collar/white-collar low skilled/white-collar high skilled), a dummy for both parents being present in the household, and a repeater dummy that equals one if a student has ever repeated a grade up until wave I. To account for heterogeneous family backgrounds with respect to health behaviors, we also include dummies for whether alcohol and cigarettes are easily available in the household.

The separate school and cohort fixed effects isolate the variation within schools and across cohorts. ϵ_{ijk} as an i.i.d. error term. Because the rank is a function of the average ability in a school cohort, and outcomes may be correlated across cohorts within the school, we cluster the standard errors at the school-level.[7]

3.3 Accounting for school-cohort-specific confounders

While intuitive, the identification strategy shown in Panel A of Figure 2 cannot account for confounders at the school-cohort level, for example, the average peer ability, the share of disruptive students, or grade-specific health campaigns (e.g., "Drink responsibly!", "Safe sex!"). Another counfounder could be dynamic selection into schools: schools may be getting better or worse over time, which would not be captured by the school fixed effects. To eliminate these confounders, we apply a more demanding identification strategy based on a model with school-specific cohort fixed effects

$$\begin{aligned} \text{risky behavior}_{ijk(t=2)} &= \gamma \text{ percentile rank}_{ijk(t=1)} + f(\text{individual ability}_{ijk(t=1)}) \\ &+ \boldsymbol{X'_{ijk(t=1)}}\boldsymbol{\beta} + \text{School-cohort FE}_{kj} + \epsilon_{ijk}. \end{aligned} \tag{2}$$

The fixed effects absorb all mean differences between cohorts within a school, as well as school-specific time trends. Most importantly, they net out average peer effects, i.e. the direct impact of the average cohort ability, or the share of smokers and drinkers in a cohort, on individual outcomes. The parameter of interest, γ is now identified through differences in higher moments (variance, skewness, kurtosis) of the ability distribution between cohorts within the same school, as illustrated in Panel B of Figure 2.

Although the fixed effects in Equation 2 eliminate many potential confounders, there are further threats to identification. The estimate of γ may be biased due to reverse causality, if

[7] One could also make a case for clustering standard errors at the school-cohort level, because the rank variable is assigned at this level. We estimated the main models with standard errors clustered at this level, and obtained slightly smaller standard errors. The outputs are available on request from the authors.

risky behavior affects the rank rather than the other way round. Furthermore, the rank may be measured with error due to the sample design. Also, selective attrition of students with a low ability could bias the results. We will address all these points in a series of robustness checks, but for the time being we work with the maintained assumption that the assignment of the ordinal rank is quasi-random conditional on the control variables and fixed effects in Equation (2).

4 The impact of ordinal rank on risky behaviors

In this section we present estimates for the impact of ordinal rank on engagement in risky behaviors. We begin with a parsimonious specification, and gradually add control variables and fixed effects. Going further, we consider heterogeneous effects by gender, race/ethnicity, within-cohort rank, and school heterogeneity. Finally, we perform a series of robustness checks.

4.1 Baseline results

Table 2 summarizes the results of OLS regressions of risky behavior in wave II on the within-cohort percentile rank in wave I. Each entry displays the result of a separate regression, and represents the marginal effect of rank on the risky behaviors indicated on the left, conditional on the controls in the respective column. Overall, the results confirm our hypothesis that students of higher rank engage less in risky behavior. All coefficients in Table 2 have a negative sign, although the magnitude and statistical significance varies between outcome variables and specifications.

The first column shows a strong negative association between the ordinal rank and risky behavior. However, we cannot interpret the meaning of these coefficients as causal, because the relationship can be influenced by many confounding factors at the school and cohort level.

To control for endogenous school choice and unobservable influences at the school level, as well as differences between age cohorts, we introduce school and cohort fixed effects in Column (2). Identification is now based on variation within schools across cohorts. In this specification, we compare students in the same school with the same absolute ability but a different percentile rank. Relying on this within-school variation, we find a large and statistically significant impact of ordinal rank on the likelihood of smoking, drinking, intercourse, and engaging in physical fights, but not for marijuana consumption, stealing, and drug selling.

In Column (3) we include individual control variables, which has a minor influence on the estimates. Only the effects on occasional smoking and sexual intercourse drop in magnitude and lose their statistical significance. The small differences in the point estimates with and without individual controls make us confident that the assignment of students to cohorts within a school is as good as random.

Finally, we address potentially confounding influences at the school-cohort level. In Column

Table 2: Ordinal rank and risky behavior

	(1)	(2)	(3)	(4)
Smoking:				
Recently	-0.263***	-0.106**	-0.064	-0.098*
	(0.084)	(0.046)	(0.045)	(0.051)
Regularly	-0.216***	-0.119***	-0.087**	-0.090**
	(0.062)	(0.036)	(0.036)	(0.041)
Drinking:				
Recently	-0.303***	-0.151***	-0.117***	-0.110**
	(0.075)	(0.043)	(0.043)	(0.051)
Regularly	-0.143***	-0.083***	-0.062**	-0.069**
	(0.040)	(0.031)	(0.030)	(0.033)
Marijuana use:				
Recently	-0.080*	-0.039	-0.017	-0.040
	(0.044)	(0.038)	(0.037)	(0.043)
Regularly	-0.025	-0.024	-0.015	-0.017
	(0.018)	(0.020)	(0.020)	(0.023)
Sex:				
Recent intercourse	-0.095*	-0.128***	-0.095**	-0.096*
	(0.055)	(0.042)	(0.041)	(0.053)
Risky intercourse	-0.012	-0.068***	-0.058**	-0.062**
	(0.026)	(0.024)	(0.024)	(0.029)
Crime:				
Stealing	-0.020	-0.062	-0.062	-0.067
	(0.035)	(0.040)	(0.040)	(0.045)
Physical fight	0.054*	-0.121***	-0.113***	-0.108***
	(0.028)	(0.036)	(0.034)	(0.036)
Drug selling	-0.031	-0.040	-0.033	-0.051*
	(0.021)	(0.027)	(0.027)	(0.028)
Controls:				
Individual ability	Yes	Yes	Yes	Yes
School fixed effects	No	Yes	Yes	No
Grade fixed effects	No	Yes	Yes	No
School × Grade fixed effects	No	No	No	Yes
Individual controls	No	No	Yes	Yes

Notes: This table displays results of separate OLS regressions of binary variables indicating the engagement in risky behavior on the percentile rank, as well as the controls and fixed effects in the respective columns. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the school level, are reported in parentheses.

(4), we estimate Equation (2), i.e., a model with school-by-cohort fixed effects, which absorb all mean differences across cohorts within a school. Identification now hinges on differences in the variance of ability across cohorts within a school. While this specification absorbs a great deal of variation, the negative sign for all risky behaviors prevails and the magnitude remains almost unchanged. The small difference between the results in Columns (3) and (4) suggests that school-cohort-specific confounders have no major influence on the result, and that the results in Column (3) can be viewed as causal.

In sum, we consistently find that students with a higher rank are less likely to engage in risky behavior. Comparing the coefficients to the baseline probabilities reported in Table 1, the effects are sizeable. Even in Column (4), where we eliminate many potential confounders but also absorb substantial variation, we find that individuals with a higher rank have significantly lower rates of smoking and binge drinking, are more careful in their sexual behavior, and a lower incidence of delinquent behavior. An increase in the within-cohort rank by one decile decreases the probability of smoking regularly by 0.9 percentage points (baseline probability 14%), the probability of binge drinking by 0.69 percentage points (baseline probability 13%), and the probability of having unprotected sex by 0.62 percentage points (baseline probability 9%). The effects on delinquent behavior are equally strong. A one-decile increase in the rank decreases the likelihood of engaging in physical fights by 1.08 percentage points (baseline probability 20%). The remaining coefficients have the expected negative sign, but are small and statistically insignificant.

These effects are large given that average peer effects have been absorbed by the school-by-cohort fixed effects, and that we control for many individual determinants of risky behaviors. Most studies on average peer effects find a 2-percentage-point increase in the likelihood of drinking and a 1.5-percentage-point increase in the likelihood of smoking for a 10-percentage-point increase in the peers who drink or smoke (Gaviria and Raphael, 2001; Lundborg, 2006; Clark and Lohéac, 2007; Fletcher, 2010). Thus, the effect of a one-decile increase in the ordinal rank on smoking and drinking corresponds to a 6-percentage-point increase in the share of peers who smoke, and a 3.5-percentage-point increase in the share of peers who binge drink.

4.2 Heterogeneous effects

After having uncovered large and robust average effects of ordinal rank on smoking, drinking, sex, and delinquent behavior, we now analyze how these effects differ between groups of students, as well as between schools with different degrees of heterogeneity. The results, based on Equation 2 and split samples, are displayed in Table 3.

We first consider differences between boys and girls, and find that the ordinal rank is a much stronger determinant of risky behaviors for girls than for boys. Point estimates are, in general, larger for girls than for boys, with the exception of marijuana use. Potential reasons might be differences in the costs of engaging in risky behavior between boys and girls: girls bear the burden of unwanted pregnancies, potentially face a higher social stigma for engaging in risky behavior, or run a greater risk of being sexually abused after consuming alcohol.

We also find significant differences in the effect for Whites and Non-Whites. The effects are strong and in most cases statistically significant for Non-Whites, while they are small and statistically insignificant for Whites.[8] As with boys and girls, the different effects for Whites and non-Whites can potentially be explained by different opportunity costs of engaging in risky behavior, or, as shown experimentally by Collado et al. (2015), differences in risk preferences across racial and ethnic groups.

[8]This finding is in line with Watt (2004), who uses the National Household Survey of Drug Abuse in 2001, and shows that Black and Hispanic youth have lower rates of alcohol abuse, and that the difference to White students can largely be explained by differences in risk preferences.

Table 3: Heterogeneous effects

	Gender		Race/ethnicity		Within-cohort rank		Within-school variance in ability	
	female	male	white	non-white	high	low	high	low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Smoking:								
Recently	-0.135**	-0.083	-0.007	-0.172**	-0.268***	-0.094	-0.129*	-0.026
	(0.066)	(0.078)	(0.081)	(0.072)	(0.094)	(0.118)	(0.071)	(0.076)
Regularly	-0.092*	-0.090	-0.039	-0.076*	-0.180**	0.025	-0.065	-0.043
	(0.055)	(0.056)	(0.067)	(0.043)	(0.075)	(0.087)	(0.053)	(0.063)
Drinking:								
Recently	-0.188***	-0.052	-0.065	-0.141**	-0.251***	-0.146	-0.131*	-0.064
	(0.068)	(0.069)	(0.075)	(0.066)	(0.094)	(0.098)	(0.075)	(0.071)
Regularly	-0.072	-0.086	-0.091	-0.056	-0.053	-0.060	-0.134***	-0.022
	(0.049)	(0.054)	(0.058)	(0.040)	(0.074)	(0.079)	(0.044)	(0.054)
Marijuana use:								
Recently	-0.036	-0.099	0.066	-0.159**	-0.228***	-0.090	-0.084	0.008
	(0.057)	(0.071)	(0.060)	(0.062)	(0.084)	(0.080)	(0.058)	(0.066)
Regularly	-0.011	-0.036	-0.006	-0.028	-0.076	-0.021	-0.038	-0.011
	(0.027)	(0.039)	(0.035)	(0.030)	(0.047)	(0.041)	(0.039)	(0.033)
Sex:								
Recent intercourse	-0.092	-0.069	-0.032	-0.088	-0.297***	-0.179*	-0.029	-0.076
	(0.076)	(0.068)	(0.073)	(0.077)	(0.080)	(0.097)	(0.060)	(0.091)
Risky intercourse	-0.102**	-0.010	-0.022	-0.134***	-0.090*	-0.105	-0.037	-0.054
	(0.044)	(0.038)	(0.042)	(0.050)	(0.050)	(0.070)	(0.039)	(0.044)
Crime:								
Stealing	-0.136**	0.020	-0.056	-0.096*	-0.149	-0.011	-0.127*	-0.015
	(0.069)	(0.070)	(0.069)	(0.058)	(0.091)	(0.103)	(0.065)	(0.073)
Physical fight	-0.124***	-0.113	-0.055	-0.222***	-0.068	-0.176*	-0.157***	-0.066
	(0.047)	(0.071)	(0.056)	(0.058)	(0.075)	(0.098)	(0.051)	(0.055)
Drug selling	-0.039	-0.075	-0.022	-0.094**	-0.130**	-0.056	-0.098**	-0.004
	(0.027)	(0.051)	(0.044)	(0.047)	(0.065)	(0.059)	(0.040)	(0.042)

Notes: This table displays results of separate OLS regressions of binary variables indicating engagement in risky health behavior on the ordinal rank in a cohort. In each column, the sample includes only the group indicated. – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. – Standard errors, clustered at the school level, are reported in parentheses. – The regression includes the same individual controls as the baseline regression, and school-specific cohort fixed effects (Equation (2)).

In a further step, we look into heterogeneous effects *within* a school cohort, i.e., whether the effect is larger in the top half or the bottom half of a cohort. As Columns (5) and (6) show, the effects are substantially larger at the top than at the bottom. One potential explanation for this difference could be that the ordinal rank is generally more important at the top. Presumably, in a cohort of 100 students, it matters more to be at the 10th instead of the 20th rank, rather than at the 70th instead of the 80th. Moreover, at the top of the ranking, the effect seems to be mainly at the extensive margin. Students with a higher rank who are in the top half of their cohort are less likely to smoke, drink, and consume marijuana *at all*, and they are more likely to be sexually abstinent.

Finally, we investigate whether the rank effect depends on the variance of ability within a school. On theoretical grounds, there are good reasons to believe that this is the case. One theory, put forward by Tincani (2015), would predict a higher rank effect in schools with a low variance. If students derive utility from a high rank, they have a greater incentive to exert effort in classes with a low variance, because a less additional effort is needed to climb one position higher in the ranking. An alternative explanation would be that students are more aware of their rank in a heterogeneous school, and the rank provides a more precise signal about a student's own ability. Therefore, the rank effect would be stronger in heterogeneous schools. We consider as heterogeneous schools those with a standard deviation above the median of all the schools in the sample. The results, displayed in Columns (7) and (8) of Table 3, support the latter explanation. By and large, we find larger, and in most cases statistically significant, effects for heterogeneous schools.

4.3 Robustness checks and extensions

In the previous sections, we pointed out several sources of bias for our estimates: reverse causality, attrition, strategic delay in school entry, and measurement error. In a series of robustness checks, we now address these issues. We also carry out a series of robustness checks in which we replace the rank based on ability with the rank based on GPA.

4.3.1 Addressing reverse causality

A potential threat to identification could be reverse causality, which could occur if students perform badly on the ability test *because* they had been drinking or taking drugs. In Equations (1) and (2) we partly account for reverse causality by regressing the outcome in wave II on the ordinal rank in wave I. Clearly, behavior in wave II cannot have an influence on ability in wave I, but reverse causality may remain an issue since many risky behaviors are addictive, and, therefore, path-dependent: a person who smoked in wave I is likely to smoke in wave II.

To eliminate reverse causality, we estimate a value-added model by including risky behavior in wave I as an additional regressor.[9] The coefficient γ now measures the impact of the ordinal rank on the *change* in a given risky behavior. For example, it measures to what extent a student with a higher rank is more or less likely to switch from smoking to not smoking, or vice versa. The results are displayed in Column 2 of Table 4. For drinking and smoking, the coefficients of the ordinal rank are now small and statistically insignificant, such that we cannot fully exclude reverse causality as a confounder. For smoking, however, it is difficult to imagine why heavy consumption of cigarettes may directly affect cognitive ability. The absence of an effect could rather indicate that the ordinal rank does not affect the change in smoking behavior. The results for sex and delinquent behavior, by contrast, hold up to this test; the effects remain large and statistically significant.

[9] Our choice of specification is limited here. We do not have information on ability and rank in wave II. Though cognitive skills are assessed again in wave III, the sampling no longer follows a within-grade stratified sampling, and we cannot compute the within-grade rank by cognitive skills in wave III. Therefore, we cannot estimate a model in first differences.

Table 4: Robustness checks

	Baseline (1)	Value-added (2)	Completed High-school (3)	Mean age ±0.5 years (4)	Share girls 40-60% (5)
Smoking:					
Recently	-0.098*	-0.017	-0.103*	-0.079	-0.124**
	(0.051)	(0.045)	(0.058)	(0.062)	(0.058)
Regularly	-0.090**	-0.023	-0.084*	-0.070	-0.086*
	(0.041)	(0.032)	(0.044)	(0.050)	(0.045)
Drinking:					
Recently	-0.110**	-0.075	-0.081	-0.106	-0.144**
	(0.051)	(0.047)	(0.057)	(0.070)	(0.060)
Regularly	-0.069**	-0.042	-0.048	-0.048	-0.081**
	(0.033)	(0.032)	(0.042)	(0.045)	(0.040)
Marijuana use:					
Recently	-0.040	0.003	-0.023	-0.014	-0.058
	(0.043)	(0.042)	(0.045)	(0.059)	(0.054)
Regularly	-0.017	-0.011	-0.018	-0.028	-0.012
	(0.023)	(0.023)	(0.027)	(0.029)	(0.025)
Sex:					
Recent intercourse	-0.096*	-0.080*	-0.064	-0.132**	-0.080
	(0.053)	(0.045)	(0.062)	(0.057)	(0.068)
Risky intercourse	-0.062**	-0.061**	-0.041	-0.072**	-0.062
	(0.029)	(0.029)	(0.033)	(0.036)	(0.039)
Crime:					
Stealing	-0.067	-0.038	-0.064	-0.109**	-0.033
	(0.045)	(0.043)	(0.054)	(0.052)	(0.056)
Physical fight	-0.108***	-0.091***	-0.122***	-0.108**	-0.081*
	(0.036)	(0.033)	(0.039)	(0.046)	(0.045)
Drug selling	-0.051*	-0.047*	-0.062**	-0.037	-0.052
	(0.028)	(0.027)	(0.030)	(0.036)	(0.032)
Sample size	12523	12446	9431	8061	8802

Notes: This table displays results of separate OLS regressions of binary variables indicating engagement in risky behavior on the ordinal rank. – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. – Robust standard errors, clustered at the school-level, are reported in parentheses. – All estimations include the same controls and fixed effects as in Equation (2). The value-added models presented in Column 2 further include a contemporaneous risky behavior. In Column 3 the sample consists of all students who eventually completed high school. The sample in Column 4 only includes students born within 6 months before or after the cohort average. In Column 5 the sample only includes school cohorts with a relatively even gender balance (between 40-60 and 60-40).

4.3.2 Attrition

The baseline estimates are potentially biased by selective attrition. Between wave I and wave II, we lose 5,000 observations, which represents 28% of the sample. Attrition introduces a bias if it is selective: if low-ranked students are more likely to attrit from the sample, we would expect a downward-bias in the results, whereas if higher-ranked students are more likely to attrit, we would expect an upward-bias. To test whether selective attrition introduces a bias, we regress an attrition dummy on the ordinal rank, a quartic in absolute ability, individual controls, as well as school and cohort fixed effects. There is no evidence of systematic attrition. The coefficient of the ordinal rank is positive, but statistically insignificant (coefficient 0.047, standard error 0.034, t-statistic 1.38).

A more subtle form of attrition could occur because we observe every cohort at a different grade level. If in every grade the lowest-ranked students drop out, then grade 12 represents a much more positive selection of students than grade 7. This difference would be captured by cohort fixed effects if it was the same in all schools, but it would not be captured if dynamic attrition differs systematically between schools. To address this problem, we restrict the sample to students who report in wave IV that they finished high school, and then we compute the rank based on this selected group, such that the rank measure is not contaminated by dynamic attrition. The results are displayed in Column 3 of Table 4. For most behaviors, the magnitude of the effect is the same as in the baseline (Equation (2)). The statistical significance is lower for most coefficients, yet the coefficients of smoking and sex are still significant at the 10%-level, and the coefficient of engagement in physical fights is significant at the 1%-level.

4.3.3 Strategic delay of school entry

The central identifying assumption is that, conditional on school choice, being in one cohort or another is as good as random, which is the case if students and parents cannot influence the assignment into cohorts. This assumption may be violated if students have to repeat a grade, or if parents strategically delay their children's school entry to let their children mature for one more year. Strategic delay of school entry, also called "redshirting,", has become more common over time in the US. As shown by Deming and Dynarski (2008), in 1968, 96 percent of schoolchildren in the US were enrolled at age 6, whereas in 2005, this figure stood at 84 percent. Our regression accounts in part for the bias that could be introduced by grade retention, or redshirting, because we include a repeater dummy, and we control for age in months. Still, the results could be biased if redshirted kids differ systematically from kids whose school entry was entirely determined by their birth date and the cut-off date of their school. To alleviate this concern, we restrict the sample to students who are at maximum 6 months older or younger than the cohort average. Because redshirted students would be more than 6 months older than the cohort average, they are excluded from the sample. The magnitude of the coefficients does not drop significantly compared to the baseline results. Some coefficients, notably those of smoking and drinking, are no longer statistically significant, but given that the point estimates are similar, the higher standard errors seem to be due to a smaller sample size rather than a bias in the estimates.

4.3.4 Measurement error

The estimates can potentially be biased by multiple sources of measurement error in the rank variable. One source of measurement error is the random sampling within school cohorts. Rather than observing the full population of a school cohort, we only observe a random sample of 34 students plus oversampled minorities, and, therefore, we cannot compute the precise ordinal rank. Due to the sampling, some students are assigned a higher rank than they actually have, while others are assigned a lower rank. However, because the sampling within a school cohort is random, it introduces a classical measurement error, which attenuates the estimates. In Appendix C we conduct a Monte-Carlo experiment, which confirms this notion and quantifies the attenuation bias.

A further source of measurement error could be the gender stratification within school cohorts. From each school cohort, equal numbers of boys and girls were sampled, regardless of the underlying gender distribution in the population. Given that we observe the population gender distribution in the in-school sample of AddHealth, we can see whether the effects change significantly if we only look at school cohorts with a relatively even gender balance. In Column 5 of Table 4, we only keep cohorts with a gender balance between 40-60 and 60-40. The results are very similar to the baseline, indicating the absence of measurement error due to gender stratification.

Finally, measurement error could arise from the over-sampling of minorities. If minorities systematically have a lower rank but are over-sampled, then non-minority students would be, on average, assigned a higher rank. To test whether this source of measurement error is important, we exploit that the in-school sample has precise information on who has been over-sampled, and then compute the rank purely based on the random sample. The correlation between the rank with and without over-sampling is almost perfect ($\rho = 0.987$), which means that we can safely assume that over-sampling does not introduce measurement error.

4.3.5 Rank based on ability vs. rank based on GPA

We choose to calculate the ordinal rank based on the standardized ability test, because it makes students comparable across cohorts within a school, allowing us to apply the within-school/across-cohort design outlined in Section 3.1. However, the ability test score comes at the disadvantage that students do not know their test scores, and, thus, can only guess their own ordinal rank. The ability rank is not as salient as the rank based on GPA, which students often know from their end-of-year reports. GPA, on the other hand, has two important drawbacks. First, in the survey it is self-reported and has substantial missing information. Second, if exams are not standardized and independently graded, teachers may apply *grading-on-a-curve*, in which case the GPA itself is a ranking. In that case, grades are not comparable across cohorts within the same school, which invalidates our identification strategy. In Row 2) of Table 5, we provide evidence for grading on a curve. Without grading on a curve, students with a higher absolute ability should receive higher grades, while relative ability should not matter, and, thus, the coefficient of the ordinal rank should not be significantly different from zero. Yet the estimated coefficient is positive and statistically significant, suggesting that a one-percentage-point increase in a student's rank increases her GPA by 0.16 points (on a scale from 1 to 4, with 4 being the best).

Despite these drawbacks, we report the results for the rank based on GPA in Table 7 in

Appendix D. When we apply the baseline regression from Equation 2 and additionally control for GPA (Column (2)), we can see that GPA only has a small mediating role in explaining the outcome, highlighting that relative ability is an important determinant of risky behavior regardless of a student's GPA. When we replace the ability rank by a GPA rank in Column (3), we obtain much larger coefficients that are statistically significant for all outcomes. It is difficult, however, to give these results a causal interpretation because differences in the GPA rank may be due to many factors besides ability, for example, motivation, effort, or diligence, which we do not observe and, thus, cannot condition on. Once we control for the absolute level of GPA in Columns (4) and (5), we find a small and statistically insignificant coefficient for the GPA rank in most cases, which is indirect evidence for *grading-on-a-curve*,. Two factors can explain the absence of a larger effect: first, GPA is not comparable across grade levels within the same school. And second, with *grading-on-a-curve*, the GPA itself is a rank, leaving little variation for an additional rank variable based on GPA.

5 Exploring potential mechanisms

The results confirm our initial hypothesis that students with a lower ordinal rank engage more in risky behavior. As mentioned in the introduction, this finding can be reconciled with at least two theoretical models. In a standard human capital model, such as Becker (1962) or Grossman (1972), a person chooses the optimal amount of risky behavior by trading off the short run gains, i.e., the pleasure from smoking, drinking, or sex, against the long-run costs of these behaviors, i.e., health problems, lower income, or unwanted childbearing. A person with a higher ability has a higher expected income, and, consequently, a higher opportunity cost of engaging in risky behaviors. The standard model implicitly assumes that a person knows her ability. If, however, a person does not know her absolute ability, the ordinal rank in her peer group may provide her with an imperfect signal about her absolute ability. A person who is actually smart but happens to have a low ordinal rank may choose to engage more in risky behavior because the low rank conveys low expected earnings and low perceived opportunity cost of risky behaviors.

A second explanation could be attributed to status concerns in combination with sorting into peer groups. Cicala et al. (2014) present such a model in which a peer group is divided into two subgroups: "nerds," which are students who achieve social recognition by being successful in school, and "troublemakers," which are students who achieve social recognition by being disruptive in school, and by engaging in risky behaviors. Students sort into these groups depending on their comparative advantage within the peer group and behave so as to conform with their subgroup. The comparative advantage, in turn, depends on a student's ordinal rank. The same student who tries to succeed in a class where she has a high rank may become a "troublemaker" in a class where she has a low rank.

Both mechanisms are not mutually exclusive, and we provide suggestive evidence that both are at play. To do so, we regress proxy variables for a particular mechanism on all the right-hand-side variables of Equation (2). Table 5 displays the results. Each coefficient measures the impact of the ordinal rank on the outcome displayed on the left. The symbol "**1**" indicates a dummy variable that equals one if the student agreed to the statement in parentheses, and zero otherwise.

Table 5: Mediating factors

Dependent Variable	Coefficient	SE
Self-concept		
1(I am more intelligent than the average)	0.101	(0.069)
Grades		
GPA	0.164^{***}	(0.063)
Expectations		
1(I want to go to college)	0.064	(0.043)
1(I will likely go to college)	0.137^{***}	(0.047)
1(I will have a college degree by the age of 30)	0.105^{**}	(0.042)
Sexual intercourse will affect ...		
1(my friend's respect)	-0.057^{*}	(0.032)
1(my loneliness)	-0.046	(0.037)
1(my attractiveness)	0.001	(0.023)
Support from others		
1(My parents care about me)	-0.000	(0.017)
1(My friends care about me)	-0.028	(0.038)
1(My teachers care about me)	0.036	(0.049)
Mental distress		
1(I felt disliked last week)	-0.016	(0.022)
1(I felt fearful last week)	0.006	(0.019)
1(I felt depressed last week)	0.008	(0.029)
School integration		
1(I feel I am part of the school)	0.003	(0.046)
1(I am happy at school)	0.005	(0.049)
1(I feel treated fairly at school)	0.025	(0.044)
Self-esteem		
1(I have a lot to be proud of)	-0.006	(0.016)
1(I like myself as I am)	0.023	(0.029)
1(I do everything just right)	0.009	(0.032)

Notes: This table displays results of separate OLS regressions of binary variables of potential mediating mechanisms on the ordinal rank. All estimations include the same controls and fixed effects as in Equation (2). – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. – Standard errors, clustered at the school level, are reported in parentheses. – Controls and fixed effects are the same as in Equation (1)).

Distorted beliefs. While students may not know their absolute level of cognitive ability, they most likely have some idea of how their ability compares to the ability of people they regularly interact with. Therefore, the ordinal rank can provide students with a signal about their actual ability. Two students with the same absolute ability may assess their ability differently if they have different ordinal ranks. Simply put, students with a high rank may think that they are smarter than they actually are, in which case they have a higher expected income, and, thus, a higher opportunity cost of engaging in risky behavior. We first test whether students with a higher rank have a higher self-perception, based on a survey question on perceived intelligence. While not statistically significant, we find a positive relationship between the ordinal rank and self-perception.

We further test whether students with a higher rank have higher expectations about their future. Wave I of AddHealth includes various questions about career expectations. As shown in Table 5, students of higher rank are significantly more likely to expect that they will go to college, and will have a college degree by age 30. These results confirm the predictions of a human capital model: a student's ordinal rank shapes her expectations, thereby distorting the trade-off between the short-term pleasure and long-term costs of engaging in risky behavior.

Besides providing a noisy signal to oneself, the ordinal rank may also provide a signal to others. Kinsler et al. (2014), for example, show that parents of young children adjust their parental support depending on the relative ability of their child in pre-school. But not only parents, also friends and teachers might base their support on the ability rank of a student. We test this channel using questions on whether the student thinks that her parents, friends, or teachers care about her, but we find no evidence.

Status concerns. Besides leading to immediate benefits through consumption, risky behaviors might also be used as a device to gain social reputation (Fryer Jr. et al., 2012). We explore this potential channel by examining the effect of the ability rank on subjective answers on questions related to perceived integration within the school: whether students feel close to other people, whether they feel as part of the school, and whether they feel safe in school. In none of these cases do we find any correlation with the ordinal rank.

We further explore the reputation channel by looking at questions on the motivations of students behind engaging in sexual intercourse. We find that students with a low rank gain greater respect among friends from sexual intercourse, which gives evidence that the rank effect can in part be explained by status-seeking behavior.

Mental stress and self-esteem. We analyze two further mechanisms. Low ranked students might engage in risky behaviors and use its short term benefits to compensate for negative feelings and mental stress. Wood et al. (2012) demonstrate such a relationship between rank and levels of mental distress. Using subjective information on whether individuals feel disliked, fearful, or depressed, we find no evidence for such compensating behavior.

Finally, the ability rank could affect individual self-esteem, which has been shown to affect adolescent risky sexual behavior (Favara, 2013). We assess the effect of a student's ordinal rank on three items of a common self-esteem questionnaire, again finding no significant relationship between the ordinal rank and these indicators.

6 Conclusion

In this paper, we show that a student's ordinal rank in a high school cohort is an important determinant of risky behaviors. Using data from AddHealth, and applying a within-school/across-cohort design, we show that highly ranked students are significantly less likely to smoke, drink, have unprotected sex, and engage in physical fights. These effects are robust to controlling for average peer effects, dynamic selection into schools, and school-cohort specific unobserved factors.

Based on rich survey information, we show that these effects can be reconciled with two theoretical models. We find that students of higher rank have significantly higher career expectations and, therefore, lower perceived opportunity costs of risky behavior. This result is in line with a human capital model in which the ordinal rank provides students with an imperfect signal about their actual ability, thereby influencing the trade-off between the long-run costs and short-run gains from risky behaviors. We also find evidence for an additional explanation arising out of status concerns. If students gain social status either by being successful or by engaging in risky behaviors, engagement in risky behaviors helps low-ranked students to compensate for their lack of social status.

These results highlight the importance of a student's ordinal rank in high school as a determinant for outcomes later in life. Parents should be concerned by these findings because they can have an important influence on the ordinal rank of their child via their school choice. Our results suggest that choosing the best possible school is not always optimal, because a child with a low rank in the best school may be more inclined to engage in risky behavior than she would be in the second-best school. While our results show that a child's rank is important, one caveat is in order: our results are based on estimates *within* schools, with school inputs being held constant. Choosing a better school may result in a lower rank, but the costs of a low rank may be outweighed by the benefits of better teachers and a better learning environment.

Our results also provide insights for policymakers. Given that risky behaviors impose a significant cost for society, it is important to know their determinants in order to design interventions that prevent adolescents from engaging in them. Given that an ordinal ranking is present as soon as students differ slightly in their ability, it is not possible to prevent students from engaging in risky behaviors by having particularly homogeneous or heterogeneous classrooms. A more effective measure would be to specifically target low-ranked students and inform them about the long-run consequences of risky behaviors.

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A Disclaimer

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B Summary statistics

Table 6: Sample description: Means and standard deviations

	Mean	(SD)	min	max
Cognitive ability:				
Peabody Vocabulary Test	100.63	(14.45)	13.00	139.00
Control variables:				
Age	15.85	(1.56)	11.43	20.68
Female	0.51	(0.50)	0.00	1.00
Migration background (1st & 2nd gen.)	0.16	(0.36)	0.00	1.00
White	0.56	(0.50)	0.00	1.00
Asian	0.07	(0.25)	0.00	1.00
Black	0.22	(0.42)	0.00	1.00
Hispanic ancestry	0.15	(0.36)	0.00	1.00
Both parents present	0.46	(0.50)	0.00	1.00
Parental education: high school dropout	0.14	(0.35)	0.00	1.00
Parental education: high school	0.25	(0.43)	0.00	1.00
Parental education: some college	0.24	(0.43)	0.00	1.00
Parental education: college	0.37	(0.48)	0.00	1.00
Alcohol easily available	0.29	(0.45)	0.00	1.00
Cigarettes easily available	0.30	(0.46)	0.00	1.00
Grade retention	0.21	(0.41)	0.00	1.00
Number of observations	12523			

Notes: This table summarizes mean, standard deviation and range of the control variables for all individuals whose risky behaviors are observed in wave II. Data source is the “In-Home” sample of AddHealth.

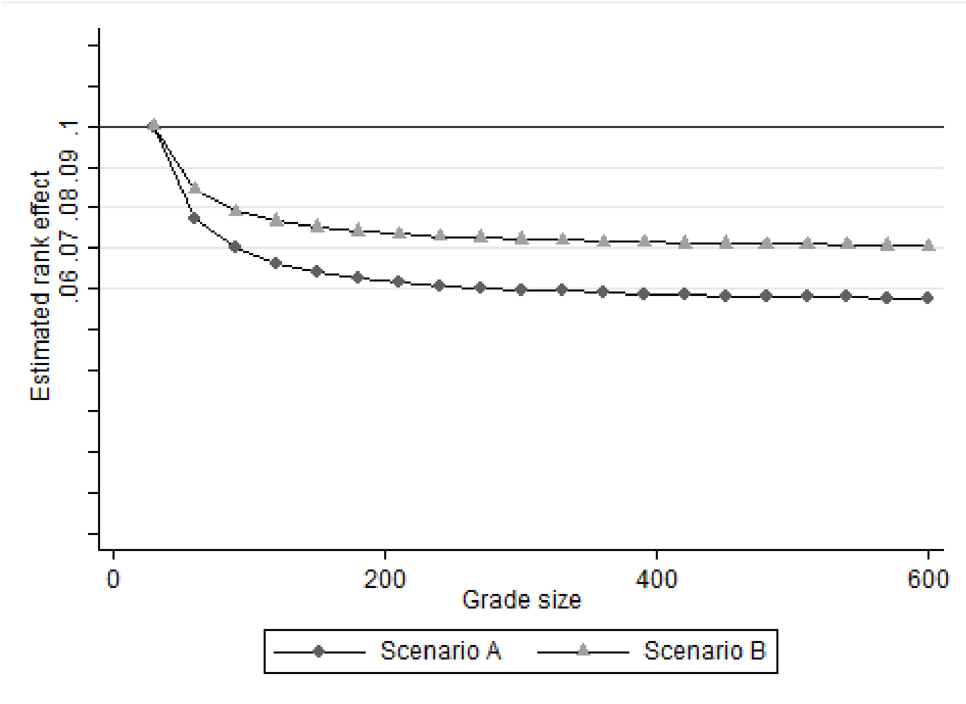

Figure 4: Measurement error due to stratified random sampling

C Quantifying measurement error

C.1 Basic Monte Carlo experiment

The estimates presented in Section 4 are potentially biased due to measurement error in the rank variable. The rank is measured with error because we observe from each school cohort a random sample rather than the full population. Based on the observed ability distribution, some students are assigned a higher rank than they would have in the population, while others are assigned a lower rank. Because the sampling is random within a school cohort, it introduces classical measurement error, which leads to attenuation bias: the estimated effects are lower than the true effects.

To quantify the size of the attenuation bias, we carry out a series of Monte Carlo simulations. We assume the true data-generating process to be

$$y_{ijk} = 0.1 \text{ rank}_{ijk} + 0.6 \text{ abil}_{ijk} + \delta_{jk}, \tag{3}$$

where δ_{jk} is a school-cohort specific intercept.

In each experiment, we run 500 replications of the following procedure:

1. We generate a dataset with 100 school-cohort combinations ("grades"), with $n \geq 30$ stu-

dents per school cohort;

2. We model student's ability to match a distribution $abil_{ink} \sim N(101, 14)$, where mean and standard deviation are chosen to correspond to AddHealth characteristics;
3. We compute the percentile rank as in Section 2.2, and compute the outcome based on Equation (3);
4. We randomly draw 30 students from each school cohort, and compute the observed rank within this random sample;
5. We estimate the model of Equation (2) including school-cohort fixed effects, and obtain an estimate for γ.

We begin with a school cohort size of $n = 30$, in which case our sample consists of everyone in the population. We then perform the same experiment with 500 replications for various cohort sizes, up to $n = 300$. Scenario A in Figure 4 displays the estimated $\hat{\gamma}$ for various cohort sizes. When the sample equals the population, we obtain the true effect $\gamma = 0.1$, whereas the size of the estimates decreases with increasing school cohort size and then seemingly levels off at $\hat{\gamma} = 0.06$.

C.2 Experiment with heterogeneous ability distributions

In the previous experiment, the ability of all students has been drawn from the same distribution, regardless of the school. However, schools in our dataset differ greatly in terms of average ability, and variance in ability. In a further experiment, we account for this heterogeneity, and generate student's ability in two steps. First, for each school-cohort combination we draw the mean of ability $\overline{abil}_{jk} \sim N(101, 7)$ (i.e., a grade-fixed average ability), the standard deviation $\sigma = sd(\overline{abil}_{jk}) \sim N(12, 2.5)$ (i.e., a grade-fixed variance component), and finally model an individual student's ability according to $abil_{ijk} \sim N(\overline{abil}_{jk}, \sigma^2_{jk})$. Again, parameters are chosen to represent AddHealth characteristics. Otherwise, the experiment is the same as in the previous section.

The simulation results are shown for different cohort sizes in Scenario B in Figure 4. When we account for heterogeneity in the ability distribution across schools, the attenuation bias is smaller than without heterogeneity. The estimated effect seems to level off at $\hat{\gamma} = 0.07$, in which case we would under-estimate the effect by around 30%.

The attenuation bias is smaller with heterogeneity because the average within-cohort variance is now larger than in a model without heterogeneity. A smaller variance means that fewer students are concentrated between any two points of the support of the ability distribution, thus making a smaller error in assigning the rank based on the sample.

Conditional on this setup, our simulations indicate that the measurement error in the rank variable, induced by within-grade random sampling, leads to an under-estimation of the true effect by about 30%.

D Robustness check: ability rank vs. GPA rank

Table 7: Ability rank vs. GPA rank

Rank based on:	Ability (1)	Ability (2)	GPA (3)	GPA (4)	GPA (5)
Smoking:					
Recently	-0.098*	-0.075	-0.275***	-0.048	-0.045
	(0.051)	(0.051)	(0.019)	(0.037)	(0.038)
Regularly	-0.090**	-0.073*	-0.197***	-0.001	0.001
	(0.041)	(0.041)	(0.017)	(0.029)	(0.028)
Drinking:					
Recently	-0.110**	-0.096*	-0.178***	-0.077**	-0.074**
	(0.051)	(0.051)	(0.015)	(0.035)	(0.035)
Regularly	-0.069**	-0.061*	-0.108***	-0.043**	-0.037*
	(0.033)	(0.034)	(0.012)	(0.021)	(0.022)
Marijuana use:					
Recently	-0.040	-0.024	-0.174***	0.015	0.011
	(0.043)	(0.043)	(0.016)	(0.028)	(0.028)
Regularly	-0.017	-0.011	-0.061***	0.048***	0.045***
	(0.023)	(0.023)	(0.009)	(0.016)	(0.016)
Sex:					
Recent intercourse	-0.096*	-0.080	-0.186***	-0.065**	-0.050
	(0.053)	(0.052)	(0.017)	(0.032)	(0.032)
Risky intercourse	-0.062**	-0.055*	-0.083***	-0.008	-0.003
	(0.029)	(0.029)	(0.012)	(0.023)	(0.023)
Crime:					
Stealing	-0.067	-0.055	-0.136***	-0.021	-0.017
	(0.045)	(0.045)	(0.016)	(0.029)	(0.029)
Physical fight	-0.108***	-0.097***	-0.138***	-0.012	-0.011
	(0.036)	(0.036)	(0.016)	(0.032)	(0.032)
Drug selling	-0.051*	-0.043	-0.085***	0.031	0.030
	(0.028)	(0.028)	(0.010)	(0.022)	(0.022)
Controls:					
Individual ability	Yes	Yes	Yes	No	Yes
GPA	No	Yes	No	Yes	Yes

Notes: This table displays results of separate OLS regressions of binary variables indicating engagement in risky behavior on the ordinal rank, which is computed based on either ability or GPA, as indicated in the first row.. – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. – Robust standard errors, clustered at the school-level, are reported in parentheses. – All estimations include the same individual controls as in Equation (2), as well as school-by-cohort fixed effects. Column (1) represents the baseline result from Table 2.