Rank, Sex, Drugs and Crime

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Abstract: In this paper, we show that a student's ordinal rank in the ability distribution of his/her 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 ability rank on the likelihood of smoking, drinking, having unprotected sex and engaging in physical fights. We further provide evidence that these results can be explained by sorting into peer groups and differences in career expectations. Students with a higher rank are less likely to be friends with other students who smoke and drink, while they have higher expectations towards their future educational attainment.

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 performance as well as far-reaching consequences for a person's labor market prospects and health (Carrell et al., 2011; Cawley and Ruhm, 2011; Carpenter and Dobkin, 2009). 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, whereby the more likely one's peers are to smoke, drink or take drugs, the more likely that person is to engage in such behaviors.

In this paper, we explore an additional important channel through which peers affect the engagement in risky behaviors: a student's ordinal rank in a school cohort. Depending on the cohort composition, the same student may rank highly in the ability distribution of one cohort and have a low rank in another. Our paper makes three important contributions. First, we show that between two otherwise-identical students, the student with the higher rank is significantly less likely to engage in risky behaviors, while we provide evidence that this relationship is causal. Second, we explore a wealth of channels that could explain this result, showing that it is mainly explained by selection into peer groups and the ordinal rank affecting career expectations. Third, in comprehensive simulations we show the conditions under which we can obtain a meaningful estimate of the impact of ordinal rank even if we only observe a sample of the underlying ability distribution.

We use data from the National Longitudinal Study of Adolescent Health (AddHealth), a representative panel survey of US middle- and high-school students that offers several key features for our analysis. For instance, multiple cohorts were sampled within each school, allowing us to apply a within-school/across-cohort design and 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 and allows us to rank students according to their cognitive ability. 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 ability rank as his/her ordinal position in the ability distribution of his/her school cohort. Figure 1 shows a clear negative relationship between the within-cohort rank and his/her engagement in risky behaviors after controlling for school fixed effects and a student's absolute cognitive ability. To give these correlations a causal interpretation, we exploit idiosyncratic variation in cohort composition within a school. Students with the same absolute ability face different ability distributions, thus having different ordinal ranks in different cohorts within the same school. To isolate the variation in the ordinal rank from variation in the average peer composition, we exploit the fact that the rank is assigned individually to every student in a school cohort and estimate a model with school-by-cohort fixed effects, in which the rank effect

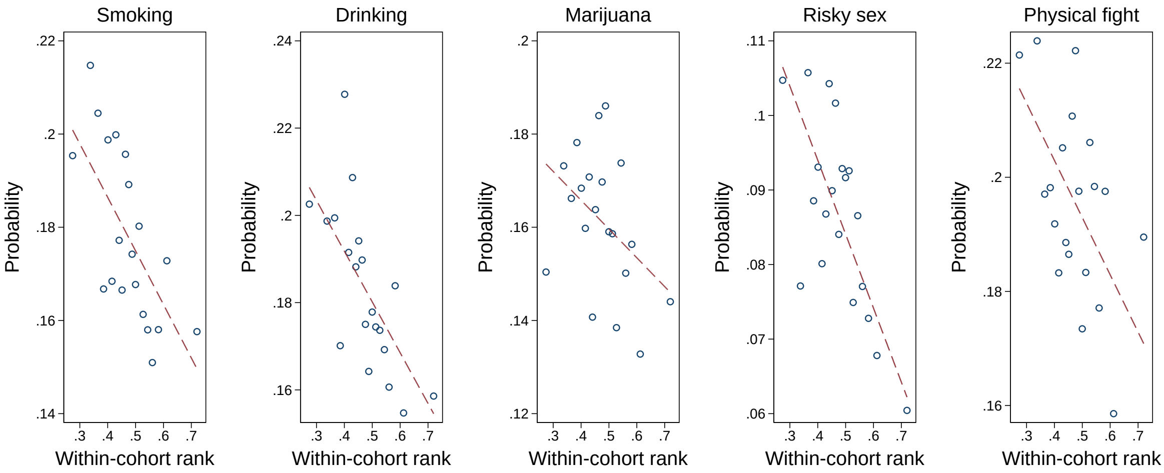

Figure 1: Partial correlations: ordinal rank and risky behavior

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=highest rank), conditional on absolute ability and school fixed effects.

is identified from differences in the variance and higher moments of the ability distribution across school cohorts. These absorb all confounding factors at the school-cohort level, such as teacher quality, differences in average ability, different shares of disruptive students or other unobserved group shocks. Under the identifying assumption that being in one cohort or another is determined by a student's birth date and the cut-off date for school entry, the variation in the ordinal rank can be considered quasi-random, giving the impact on risky behavior a causal interpretation.

Applying this research design, we find a negative effect of a student's ordinal rank on a large number of risky behaviors. The effects are large and statistically significant for smoking, drinking, risky sex and engagement in physical fights. For a given level of absolute ability, a one-decile increase in the ordinal rank — reflecting an increase of ten rank positions in a cohort of 100 students, around one within-school standard deviation in rank — reduces the probability of smoking by 1.2 percentage points (relative to a mean of 17.8%) and the probability of drinking by 1.4 percentage points (mean 18.3%). It also significantly reduces the likelihood of having sex without birth control by 0.6 percentage points (mean 8.7%) and engaging in physical fights by 1.2 percentage points (mean 19.5%). These effects are large, given that we compare students who attend the same school, have the same absolute level of ability and the same observable characteristics. The effects for marijuana use, stealing and drug selling are also negative, albeit smaller and statistically insignificant.

These findings are consistent with two theoretical models predicting a negative effect of a student's ordinal rank on risky behaviors through different channels. First, our findings can be reconciled with a human capital model in the spirit of Grossman (1972), in which students trade off the short-run pleasure against the long-run costs of risky behaviors. A high rank may signal to students a high future income, thus increasing the opportunity cost of risky behaviors. Second, our findings are in line with a model of self-selection into social categories within a school cohort or class (Akerlof and Kranton, 2002). Cicala et al. (2015) 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. Because a student with a low ordinal rank has a comparative advantage in being a 'troublemaker' relative to being a 'nerd', he/she engages more in risky behaviors than if he/she had a higher rank.

Exploiting extensive survey information on friendship formation, expectations and attitudes, we further investigate which of these theoretical mechanisms are supported by the data, finding support for both. To test whether a student's ordinal rank affects the sorting into peer groups, we analyze data on friendship networks within the school, showing that students with a higher rank are less popular and less likely to be friends with peers who smoke or drink. We also show that students with a higher rank have a higher perceived intelligence and higher expectations towards their educational career. This result provides evidence of the ordinal rank shaping expectations, which may in turn affect the engagement in risky behaviors, thus supporting the notion that the ordinal rank provides students with a noisy signal about their actual ability.

In a series of robustness checks, we carefully address several threats to identification. One issue is reverse causality, whereby students may achieve a low rank *because* they have engaged in risky behaviors. To address this concern, we estimate a value-added model and also show that risky behaviors do not predict cognitive ability measured two years later. A further issue is individual-level confounders that may be correlated with the rank but also have a direct effect on risky behaviors. In extensive simulations, we show the conditions under which these confounders bias the estimates, as well as the direction in which the bias would work. For the majority of plausible confounders such as parental pressure or intrinsic motivation, we show that our estimates are biased towards zero. Finally, because we base the ranking on a random sample of every school cohort, the rank is measured with error. In simulations, we demonstrate that this measurement error results in attenuation bias and that the bias is moderate.

This paper provides new insights into the determinants of risky behaviors among adolescents. In particular, it complements the literature on peer effects, which finds 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

[1]Besides the studies cited here, all of which 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.

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, whereby 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.

Our paper also highlights the importance of ordinal rank in high school for non-education outcomes. Recent research has found a significant impact of a student's ordinal rank in school on test scores (Azmat and Iriberri, 2010; Murphy and Weinhardt, 2014; Goulas and Megalokonomou, 2015).[2] This paper extends previous work in which we show that a student's ordinal rank significantly affects their decision to attend college (Elsner and Isphording, 2017). The present paper departs from this work in several important ways. First, we show that the ordinal rank in high school matters for a large number of non-educational outcomes, which in turn are important determinants for health and success later in life. As a second contribution, we explore sorting into peer groups as an important channel through which the ordinal rank affects risky behaviors, showing that highly-ranked students are less popular and less likely to be friends with students who themselves engage in risky behaviors. Finally, we make a methodological contribution by assessing the biases from measurement error and omitted variables inherent in the empirical analysis of the impact of ordinal rank. In extensive simulations, we demonstrate the conditions under which these biases matter for the estimation, as well as what sign and magnitude they have.

The remainder of the paper unfolds as follows. Section 2 describes the dataset and the construction of the main variables of interest. In Section 3, we present the identification strategy and discuss potential threats to identification. In Section 4, we show the main results and present a series of robustness checks, as well as summarizing the results of comprehensive simulation exercises aimed at quantifying the biases from measurement error and omitted variables. In Section 5, we explore several channels that can help to explain why a student's ordinal rank affects risky behaviors. Finally, Section 6 concludes.

2 Data and descriptive statistics

We base our empirical analysis on data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). The AddHealth data is particularly suited for our application as its main focus lies on the interaction between adolescents' education and health behavior.

[2] Moreover, as shown by Tincani (2015), rank concerns among students are an important determinant 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 to achieve a higher rank. Additional evidence on the importance of ordinal rank is given by Gill et al. (2015), who show in a laboratory experiment that the ordinal rank affects effort provision, especially at the bottom and top of the rank distribution, as well as Kuziemko et al. (2014), who use experimental and observational data to show that people exert effort to avoid being ranked in last place.

Moreover, it covers multiple cohorts within a school, allowing us to hold school characteristics constant through a within-school/across-cohort design. In the following, we describe the dataset and the sample, as well as presenting 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. In our application, we use the first two waves of the survey, the first of which was collected in 1994/95, when students were on average 16 years old, while the second wave was collected in 1996. Within each school, up to six different cohorts were initially sampled in wave I of the survey. As each cohort is observed at different grade levels in 1994/1995 (cohort 1 in grade 7, cohort 2 in grade 8, etc.), we will use the terms *grade* and *cohort* interchangeably in the following.

The AddHealth data comprises multiple samples. The *In-School* sample comprises all students of sample schools that were present on a fixed interview date. This sample provides brief information on health behaviors and educational achievement, but lacks more in-depth information on risky behaviors and skills and has not been followed over time. Therefore, for our main analysis 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 with repeated observations over time.

The in-home sample comprises a random sample of 17 boys and 17 girls drawn from every grade level of each school.[3] In addition, students from specific minorities were over-sampled (Puerto Ricans, Chinese, Cubans, high-educated blacks, twins, siblings, students with disabilities). A small number of schools was sampled completely.[4]

From 14,398 students that we observe in both wave I and II of the AddHealth, we drop observations from all schools with 20 individuals or less (55 obs.) and all grades with five students or less (340 obs.). Furthermore, we delete from the in-home survey all observations with missing information in key variables (1,545 obs.). The final sample comprises 12,536 students in 132 schools and 461 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 ability rank in a high-school cohort, which measures how a student ranks in terms of cognitive ability relative to all other students in the same

[3]Given the importance of random sampling for our analysis, we inquired with the data provider about the randomization. The sample was drawn by a NORC statistician before the survey based on a school roster. The rosters contained all of the students in the schools unless the parents requested their names to be removed. The schools were not involved in the selection of the sample. We would like to thank Joyce Tabor from the Carolina Population Center for providing us with this information. More detailed information is provided in Tourangeau and Shin (1999).

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

school cohort. We construct this 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 (PPVT), which measures logical reasoning and has been shown to strongly correlate with other intelligence tests such as the Wechsler Intelligence Test or the Armed Forces Qualifying Test (AFQT) (Baker et al., 1993; Dunn and Dunn, 2007). The test is age-specific and is carried out in 87 rounds. In each round, students are shown four pictures and given a word that 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, with more difficult tasks receiving a higher weight. The Peabody test has been shown to be a feasible and successful method for assessing basic cognitive abilities in large-scale surveys, with high re-test reliability and stability of scores during childhood (Dunn and Dunn, 2007).

With all students in the in-home sample, the PPVT was carried out face-to-face with the interviewer. The results were computed after the survey day and were neither disclosed to the students nor to their teachers or parents. Therefore, students had no incentive to achieve a particular rank position and their performance is unlikely to be influenced by parental pressure, peer pressure or teaching to the test.[5]

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 cohorts with differing size, we compute a student's relative rank position by standardizing the absolute rank to the cohort size,

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

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

In our research setting, we measure the cognitive ability of a student in a given school year and rank them relative to all other students in the same school cohort in the same school year. We subsequently estimate the impact of the ordinal rank on risky behaviors under the maintained assumption that a student's cognitive ability is formed during childhood and thus predetermined during adolescence. This assumption is supported by the findings of Cunha et al. (2006) and others, showing that cognitive skills are mostly formed before the age of 10 and remain fairly stable thereafter.[6]

In each school cohort, we observe the ability distribution of a random sample of around 40 students, which we use to approximate the ability distribution of the entire school cohort. In a regression of risky behaviors on a student's rank, this approximation introduces measurement

[5]In a section on robustness checks, we will later discuss and quantify the bias if these factors affect the rank as well as risky behaviors.

[6]In a robustness check, we will later relax this assumption.

error because some students are assigned a higher rank than they would have in the population, while others are assigned a lower rank. One of the contributions of this paper is to show that even without observing the full population, one can obtain a meaningful estimate for the average impact of the ordinal rank on various outcomes as long as the sample is drawn randomly from every school cohort.

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. However, 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.[7] With such a grading scheme, two students with the same grade point average (GPA) in the same school may considerably differ with respect to ability and other characteristics. In addition, grades in AddHealth are self-reported and have many missing observations, thus making them less suitable for our analysis compared with the Peabody test.[8]

One might be concerned whether students actually know their rank, given that school cohorts are large and students do not observe their score on the ability test. However, 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 those in the same school with the same ability but with a different rank, thus providing evidence that students have an idea about their position in the ability distribution of their cohort.

2.3 Outcome variables: risky behaviors

We consider as outcomes five types of risky behaviors: smoking, (binge) drinking, marijuana consumption, risky sex and delinquent behavior (stealing, physical fights and drug selling). All dependent variables are constructed as binary indicators for different intensities of risky behavior. The information on risky behaviors is 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 further use similarly constructed indicators from wave I information to control for trends in risky behaviors in some specifications.

All behaviors are self-reported. To reduce the risk of misreporting — whether due to peer pressure, the presence of an unknown interviewer or the fear of being reported to the school

[7]See Dubey and Geanakoplos (2010) for a theory explaining 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 of grading on a curve in German primary schools.

[8]In fact, there are grades available from administrative transcripts in AddHealth. These are measured with little error, but are only available for a small subsample.

authorities — the answers to sensitive questions about drugs, sex, health behaviors or criminal activity were elicited through computer-assisted self-interviews (CASI). The questions were played to the participant via headphones and the answers were anonymously typed into a laptop without being shown to the interviewer.[9] Our main dependent variables are constructed as follows:

Smoking The indicator for smoking is based on the question "During the past 30 days, on how many days did you smoke cigarettes?". In our main analysis, we focus on an indicator for regular smoking, which is one if students report having smoked on at least 10 within the past 30 days. Further intensities considered in the appendix are whether individuals have ever smoked and whether they have smoked intensively (10 daily cigarettes on at least 10 out past 30 days).

Drinking. The indicator for drinking is based on the question "Over the past 12 months, on how many days did you drink five or more drinks in a row?". In our main analysis, we focus on regular alcohol consumption, which we define as drinking on at least 2 days per month during the last year. In the Appendix, we also consider whether individuals have ever consumed alcohol and whether they have been drunk on at least 2 days per month during the last year.

Sex. In our main results, we consider an indicator for sexual intercourse without any measure of birth control within the past 6 months, which equals one if neither the respondent nor his/her partner used contraceptives during their most recent intercourse. In the Appendix, we additionally consider any previous sexual intercourse ever and any previous sexual intercourse (with or without contraception) within the past 6 months.

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 he/she engaged in these behaviors at least once in the last 12 months. For delinquent behaviors, we only observe the incidence but not the intensity.

Descriptive statistics. Table 1 lists baseline probabilities of risky behaviors in wave II of AddHealth for different subgroups. Smoking, drinking and marijuana consumption are widespread in the observed population, with 17% of students regularly smoking and 18% regularly consuming alcohol or having recently consumed marijuana. Rates for alcohol and marijuana consumption are larger for boys than girls. Consumption appears to be largely unrelated to parental socio-economic status, although children of college-educated parents display marginally lower consumption rates There are strong racial disparities in consumption: black students are much

[9] Despite CASI reducing misreporting, there may still be misreporting systematically related to rank. In the appendix, we discuss the direction and magnitude of this bias resulting from misreporting.

less likely to smoke and drink than their white counterparts, although the difference is smaller for marijuana consumption.

About 8% of all students report having had sexual intercourse without using contraceptives within the past 6 months. This number is larger for female students (9%) compared with male students (8%) and is strongly related to parental education (14% for children of high-school dropouts compared to 6% for children of college graduates).

Average delinquency rates range from 22% for stealing to 7% for drug selling and are predominantly driven by male students. Racial disparities and differences by socio-economic background are less pronounced than with the previous categories of risky behavior.

Finally, we report the probabilities for different grade levels. Students who in grade 7 in wave I were on average 15 years old in wave II, while those in grade 12 were on average 20 years old in wave II. With the exception of delinquent behaviors, engagement in risky behaviors increases with age. The baseline probabilities for smoking, drinking, marijuana use and sex — 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

We aim 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, before discussing the identifying assumptions. We also briefly highlight some threats to identification, although we provide a more extensive discussion along with the results.[10]

3.1 Identification: basic idea

Figure 1 in the introduction has revealed a significant negative relationship between the within-cohort rank and engagement in risky behavior. A simple correlation of rank and risky behavior cannot be interpreted as 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 his/her engagement in risky behavior. Identification of a causal effect thus requires exogenous variation in a student's ordinal rank.

Our research design is based on the idea that the same student would face a different ability distribution if he/she was in a different cohort and would thus have a different ordinal ability rank in the respective cohort. Given that we can only observe each student in one school cohort, we compare students in the same school who have the same level of absolute ability but a

[10]This section draws from Elsner and Isphording (2017), where we use a similar setup 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*		
				w/o birth control	Stealing	Fights	Drug selling
All	0.18	0.18	0.16	0.09	0.23	0.20	0.07
Male	0.18	0.21	0.18	0.08	0.25	0.26	0.11
Female	0.18	0.15	0.15	0.10	0.20	0.13	0.04
Parental background:							
Less than high-school	0.18	0.17	0.16	0.14	0.23	0.22	0.08
High school	0.20	0.19	0.16	0.09	0.22	0.21	0.08
Some college	0.20	0.19	0.18	0.09	0.22	0.22	0.08
College	0.14	0.18	0.15	0.06	0.23	0.16	0.07
Race/Ethnicity:							
White	0.25	0.22	0.17	0.08	0.23	0.18	0.07
Asian	0.12	0.11	0.13	0.08	0.24	0.17	0.06
Hispanic	0.13	0.19	0.17	0.12	0.26	0.23	0.09
Black	0.05	0.11	0.14	0.10	0.20	0.22	0.08
Grade level:							
7/8	0.12	0.10	0.12	0.05	0.25	0.22	0.06
9/10	0.19	0.19	0.17	0.09	0.23	0.20	0.08
11/12	0.22	0.27	0.20	0.13	0.20	0.17	0.08

Notes: This table displays mean probabilities of risky behaviors across socio-economic groups in wave II of AddHealth. For smoking, 1 indicates having smoked on at least 10 out of last 30 days. For drinking, 1 indicates drinking on at least 2 days per month during the last year. For marijuana use, 1 indicates any marijuana consumption during the last month. "‘Sex w/o birth control"’ refers to any sexual intercourse without measures of contraception within the last 6 months. The delinquent behavior variables refer to engagement in a specific activity at least once during the last 12 months.

different ability rank due to being in different cohorts. As illustrated in Figure 2, this variation in rank may come from differences in the average cohort ability (Panel A), the variance (Panel B) or higher moments of the ability distribution.

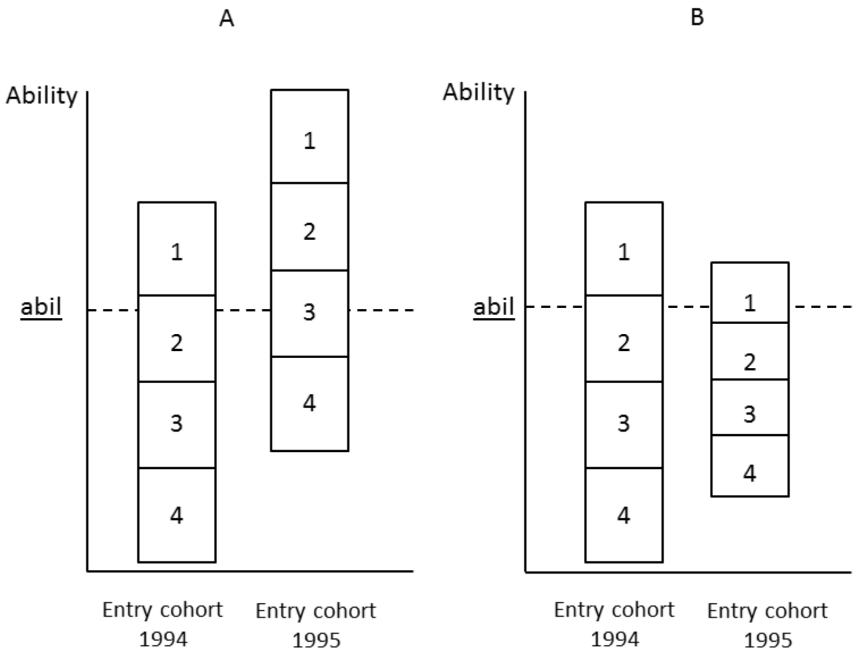

Figure 2: Variation in mean and variance of ability

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), the dispersion of the ability distribution (Panel B) or both. This graph is adapted from Elsner and Isphording (2017) and Murphy and Weinhardt (2014).

To estimate a causal effect, we want to exploit the component of cohort-to-cohort variation in the shape of the ability distribution, which is plausibly exogenous from a student's perspective. In principle, the ability distribution differs from cohort to cohort for a variety of reasons, many of which are beyond the influence of the student or his/her parents. In most schools, whether a student is in one cohort or another is determined by a cut-off date and the student's birth date: if a student is born before the cut-off, he/she goes to school one year earlier than someone born after the cut-off. This may create idiosyncratic variation in the cohort composition if children with higher ability are born before the cut-off in some years and in other years after the cut-off. In addition, the ability distribution within a school catchment area naturally fluctuates more than it would in the entire US. If we looked at entire school entry cohorts in the US, the cohort composition would not strongly differ from one year to another due to the law of large numbers.

However, within a school catchment area, where a school entry cohort is 100-200 students, the law of large numbers may not hold, which is why the ability distribution may naturally fluctuate across cohorts.

To be sure, there may also be systematic factors explaining why the ability distribution may vary between cohorts within a school, such as selection into schools. Our identification strategy seeks to isolate the idiosyncratic variation from these systematic — and potentially confounding — factors.

The variation in the ordinal rank across cohorts within a school can be considered quasi-random under two assumptions: first, being in one cohort or another is beyond the influence of the student, his/her parents or school administrators; and second, the difference in the ability distribution across cohorts within a school is not systematic, i.e. it is not driven by unobserved factors that may also affect a student's engagement in risky behaviors. In the following, we present the empirical model and discuss the conditions under which these assumptions hold.

3.2 Empirical model

To estimate the impact of a student's ordinal rank on his/her engagement in risky behavior, we estimate versions of the following model.

$$\begin{aligned} \text{risky behavior}_{isc(t=2)} &= \gamma \text{ rank}_{isc} + f(\text{ability}_{isc}) \\ &+ \boldsymbol{X'_{isc}}\boldsymbol{\beta} + \delta_{sc} + \varepsilon_{isc}. \end{aligned} \tag{2}$$

The dependent variable is a binary indicator that equals one if student i in school s and cohort c has engaged in a risky behavior. We regress this variable on a student's ability rank in a school cohort in wave I and control for absolute cognitive ability in wave I with a fourth-order polynomial. The vector $\boldsymbol{X_{isc}}$ includes individual control variables measured in wave I, namely gender, age in months, height, race/ethnicity (white/black/Asian/Hispanic), 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) and a dummy for both parents being present in the household. The vector δ_{sc} represents a set of school and cohort fixed effects for which we choose several parameterizations. The error term ε_{isc} captures unobserved determinants of risky behaviors. To account for common shocks at the school level, we cluster the standard errors at the school level.

Our parameter of interest is γ, which relates a student's ordinal rank to his/her engagement in risky behavior. For γ to have a causal interpretation, we have to assume that the regressors are strictly exogenous, i.e. uncorrelated with the error term, formally $E(\varepsilon_{sic}|r_{isc}, a_{isc}, \boldsymbol{X_{sic}}, \delta_{sc}) = 0$. The plausibility of this assumption depends on the choice of fixed effects. We consider three versions of the model in Equation (2).

1) A two-way fixed effect model This model includes separate fixed effects for schools and cohorts, i.e. $\delta_{sc} = \rho_c + \rho_s$. It compares students with the same ability who attend the same school but are in different cohorts. The school fixed effects capture static selection into schools, i.e. the fact that students in different schools differ on average along many dimensions such as parental background or ethnicity. In a two-way fixed effect model, the parameter γ is identified through differences in all moments of the ability distribution across cohorts within a school.

Despite being a useful starting point for the empirical analysis, the separate school and cohort fixed effects do not capture any school-cohort specific confounders that may violate the strict exogeneity assumption. There are many candidates for such confounders. An important confounder is the average cognitive ability of a school cohort, which is mechanically related to a student's ordinal rank. A student with high-ability peers will *mechanically* have a lower rank than he/she would have in a school cohort with low-ability peers. Therefore, a two-way fixed effect model without any school-cohort-level controls does not allow us to disentangle the ordinal rank effect from an average peer effect. A further confounder can be dynamic selection into schools, such that the cohort quality may increase or decrease over time. Furthermore, a student's peers may differ between cohorts along many other dimensions, such as race/ethnicity, parental background or disruptive behaviors. Moreover, there are many school resources that may vary across cohorts within a school — such as teacher quality — and some school cohorts may be more exposed to health campaigns like 'Safer Sex' or 'Drink Responsibly', which may affect their engagement in risky behaviors.

2) A two-way fixed effect model with school-cohort-specific controls In this specification, we additionally include school-cohort-specific control variables, i.e. $\delta_{sc} = \rho_c + \rho_s + \boldsymbol{S'_{sc}}\boldsymbol{\phi}$. Some of the confounders mentioned above — such as average ability or parental background — may be observable or can be proxied, whereby they can thus be included as controls in the regression, summarized by $\boldsymbol{S_{sc}}$.[11] Other factors such as teacher quality or peers' disruptive behaviors are unobservable to us, thus preventing us from giving γ a causal interpretation.

3) A model with school-cohort fixed effects The strictest version of the model includes school fixed effects that vary by cohort, δ_{sc}. The school-cohort fixed effects absorb any average differences in observables and unobservables between cohorts within a school. Therefore, all confounders at the school-cohort-level — such as specific educational inputs, average school cohort characteristics or dynamic selection into schools — are absorbed by the fixed effects. Moreover, the fixed effects absorb all unobserved common shocks to a school cohort, which have been shown to bias conventional peer effects estimates (Angrist, 2014; Feld and Zölitz,

[11] In the regressions to follow, we control for the following school-cohort level variables: average cognitive ability, share of girls, mean parental education, share of first- or second-generation immigrants, share of black, Asian and Hispanic students, share of students who smoke, drink, use marijuana, engage in risky sex, steal, engage in physical fights and engage in drug crimes.

2017). This increases the plausibility that the regressors are strictly exogenous and that γ can be interpreted as causal.

It is possible to identify γ on top of school-cohort fixed effects, because the ordinal rank varies at the individual level within a school cohort. Upon first glance, it may not be intuitive where the identifying variation comes from in this model. The school-by-cohort fixed effects absorb all mean differences between *any* school cohorts in the sample. Thus, in this model we no longer compare students within a school across cohorts; rather, the fixed effects level the playing field between all school cohorts. In this specification, γ is identified from differences in the variance and higher moments of the ability distribution, as illustrated in the right panel of Figure 2.

In the empirical analysis to follow, we will present results for all three models, although the third model with school-cohort fixed effects is our preferred specification because it precludes any school-cohort-specific confounders.

3.3 Identifying variation

The aim of the fixed effect models is to isolate the variation in the ordinal rank that can be considered quasi-random. One may be concerned that there is little variation left to identify the effect in a model with school-by-cohort fixed effects. However, Table 7 in the Appendix shows that even in such a demanding specification there is considerable variation in the ordinal rank as well as the main outcomes. The most relevant variation is that of the ordinal rank conditional on absolute ability. Without any fixed effects, the standard deviation of the percentile is 0.16, which means that at a mean cohort size of 180 students, for a given level of absolute ability the ability rank would vary on average by $(0.16 \times 180 =)28.8$ absolute rank positions. Once we additionally control for separate school and cohort fixed effects, the within-school standard deviation of the rank conditional on ability slightly smaller with 0.12. In the most demanding specification with school-by-cohort fixed effects, the standard deviation in rank is 0.115.

3.4 Potential threats to identification

While the model with school-cohort fixed effects alleviates many concerns about identification, there may be more sources of bias in the estimation of γ. First, the estimates can be biased by reverse causality, as students may have a low rank *because* they engaged in risky behaviors before taking the test. A further issue is measurement error, which may bias the estimates because we do not observe the entire school cohort but rather only a random sample thereof. Moreover, there may be individual-level omitted variables that are not absorbed by the school-by-cohort fixed effects. Students may also misreport their risky behavior, which is problematic if misreporting is correlated with rank. A further problem is redshirting, which occurs if some parents send their kids to school one year later than the legal starting age. Finally, the ability test score may be a function of a student's prior rank, which would not permit a causal interpretation. In a series of robustness checks, we address all these issues and provide guidance about the likely direction and magnitude of the resulting biases. However, for the time being, we will interpret the results of the model with school-cohort fixed under the maintained assumption that the regressors are strictly exogenous and thus we will interpret γ as causal.

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 by presenting our main results using different sets of fixed effects and controls and discuss the results against potential threats to identification.

4.1 Baseline results

Table 2: Ordinal rank and risky behavior

	(1)	(2)	(3)	(4)
Smoking:				
on at least 10 days (past 30 days)	-0.101**	-0.122***	-0.123***	-0.048
mean = 17.79%	(0.040)	(0.043)	(0.045)	(0.035)
Drinking:				
on at least 2 days per month (past 12 months)	-0.114***	-0.120***	-0.139***	-0.098**
mean = 18.32%	(0.038)	(0.042)	(0.043)	(0.039)
Marijuana use:				
at least once (past month)	-0.035	-0.064	-0.074*	-0.029
mean = 16.12%	(0.038)	(0.041)	(0.043)	(0.042)
Sex:				
Intercourse w/o birth control (past 6 months)	-0.057**	-0.057**	-0.055*	-0.049
mean = 8.66%	(0.025)	(0.029)	(0.031)	(0.030)
Crime:				
Stealing at least once (past 12 months)	-0.066*	-0.055	-0.062	-0.040
mean = 22.62%	(0.040)	(0.044)	(0.045)	(0.043)
Physical fight at least once (past 12 months)	-0.124***	-0.125***	-0.136***	-0.118***
mean = 19.55%	(0.036)	(0.037)	(0.037)	(0.035)
Drug selling at least once (past 12 months)	-0.033	-0.042*	-0.052*	-0.051*
mean = 7.48%	(0.026)	(0.026)	(0.027)	(0.027)
Controls:				
Individual controls	Yes	Yes	Yes	Yes
Individual ability	Yes	Yes	Yes	Yes
School fixed effects	Yes	Yes	No	No
Cohort fixed effects	Yes	Yes	No	No
School × cohort fixed effects	No	No	Yes	Yes
Cohort mean characteristics	No	Yes	No	No
Risky behavior in wave I	No	No	No	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.

Table 2 presents 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. The coefficients can be interpreted as percentage-point changes at the mean for each percentile change in the rank position. Overall, the results reveal a very robust pattern across specifications and outcomes, confirming our hypothesis that students of higher rank less commonly engage in risky behavior. All coefficients have negative signs, although magnitude and statistical significance vary between outcome variables and specifications.

The first column displays the results of a linear model with individual controls and separate sets of school and cohort fixed effects, which control for static selection into schools and unobserved differences across schools, as well as differences between age cohorts. Identification in this specification is based on variation in the mean as well as higher moments of the ability distribution across cohorts within schools. Relying on this variation, we find a negative and statistically significant relationship between ordinal rank and the likelihood of most risky behaviors, with the exception of negative but insignificant coefficients for marijuana use and drug selling.

Relative to the baseline probabilities, these are large effects. In the case of smoking, an increase by one decile in the local ability distribution — going up ten ranks in a cohort of 100 students, or going up by one within-school standard deviation — reduces the probability of regular smoking by 1 percentage point, or by about 5.6 percent evaluated at a mean of 17.79 percent ($-0.101 * 10/17.79 \approx 0,056$). The effects are of similar magnitude for drinking (6.2 percent of the mean), risky sex (6.6 percent of the mean) and engagement in physical fights (6.3 percent of the mean).

In Column (2), we add school-cohort characteristics, notably the mean and variance of cognitive ability, as well as means of control variables (gender, race, parental education) and risky behaviors (smoking, drinking, marijuana, risky sexual intercourse and delinquent behavior) to the two-way fixed effect model. By controlling for mean risky behavior, we shut off average peer effects as a potential confounding factor. In addition, controlling for mean ability breaks the mechanical negative correlation between mean cohort ability and own rank. In this specification, the identifying variation relies on differences in the variance and higher moments of the ability distribution across cohorts within schools. The results are similar to those in Column (1), although the negative coefficient for stealing is no longer statistically significant, whereas the coefficient of drug selling is now marginally significant.

In Column (3), we estimate a model with school-by-cohort fixed effects as described in Equation (2). Compared to the specification in Column (2), this model absorbs all mean differences in unobservables across school cohorts that have not been captured by the controls included in Column (2). Although this specification is more demanding on the data compared to the specification in Column (2), the negative sign for all risky behaviors prevails and the magnitude remains almost unchanged. Nonetheless, we are more comfortable interpreting the results in Column (3) as causal because the school-cohort fixed effects absorb *any* confounder at the school-cohort level. Therefore, in the remainder of the paper we will refer the results in Column

(3) as our baseline results.

Finally, in Column (4), we address the concern that risky behavior could influence cognitive ability, based on which we construct the ordinal rank such that the causality would run from risky behavior to rank rather than the other way round. To alleviate this concern, we estimate a value-added model in which we control for the engagement in risky behavior in wave I of AddHealth. The coefficients displayed in Column (4) represent the marginal impact of an increase in the ordinal rank on risky behavior in wave II conditional on earlier risky behavior, which is why the magnitude is not directly comparable to those in Columns (1)-(3). For all risky behaviors, the negative sign prevails and the effects of the ordinal rank on drinking, physical fights and drug selling remain statistically significant. All other coefficients are negative but statistically insignificant. For smoking and risky sex, we cannot say whether the coefficient becomes statistically insignificant due to reverse causality or because these behaviors are persistent. In Table 10 in the Appendix, we provide further evidence against reverse causality by showing that risky behaviors do not predict a person's Peabody score in wave III of the survey.[12]

In sum, we consistently find that students with a higher rank are less likely to engage in risky behavior. The effects are large, given that average peer effects have been absorbed by the school-by-cohort fixed effects and that we control for a rich set of individual determinants of risky behaviors. In terms of magnitude, our results are comparable with most peer effect estimates, which lie in the range of 1.5-2 percentage-point increases in the likelihood of drinking and smoking for a 10-percentage-point increase in the share of peers who drink or smoke (Gaviria and Raphael, 2001; Lundborg, 2006; Clark and Lohéac, 2007; Fletcher, 2010), whereas in our paper a one-decile increase in the ordinal rank increases the likelihood of smoking by 1.2 and drinking by 1.4 percentage points (based on specification (3)).

Thus far, we have only displayed the results for one level of intensity for each type of risky behavior. In Table 8 in the Appendix, we present the results of the specification in Column (3) for different levels of intensity. In addition, in Table 9 we report estimates of heterogeneous effects for different parts of the local and global ability distribution, as well as by gender, race and parental education.

4.2 Robustness checks

In the previous sections, we highlighted several sources of bias for our estimates, namely measurement error, omitted variables, misreporting of risky behaviors, attrition and strategic delay in school entry. In this section, we address these issues with a series of robustness checks and simulation exercises.

[12] A negative effect of drinking and marijuana usage on cognitive skills has — among others — been shown by Meier et al. (2012), although the evidence in their paper has been challenged by Rogeberg (2013).

Table 3: Robustness checks

	Baseline (1)	Completed High-school (2)	Mean age ±0.5 years (3)	Share girls 40-60% (4)
Smoking:				
on at least 10 out of past 30 days	-0.123***	-0.113**	-0.110**	-0.121**
	(0.045)	(0.047)	(0.054)	(0.052)
Drinking:				
on at least 2 days per month past 12 months	-0.139***	-0.110**	-0.152***	-0.160***
	(0.043)	(0.050)	(0.057)	(0.051)
Marijuana use:				
at least once within past month	-0.074*	-0.071	-0.065	-0.096*
	(0.043)	(0.044)	(0.056)	(0.052)
Sex:				
Intercourse w/o birth control in past 6 months	-0.055*	-0.027	-0.045	-0.055
	(0.031)	(0.035)	(0.035)	(0.040)
Crime:				
Stealing at least once past 12 months	-0.062	-0.089*	-0.083*	-0.060
	(0.045)	(0.054)	(0.050)	(0.055)
Physical fight at least once past 12 months	-0.136***	-0.158***	-0.115**	-0.121***
	(0.037)	(0.041)	(0.048)	(0.044)
Drug selling at least once past 12 months	-0.052*	-0.071**	-0.031	-0.063**
	(0.027)	(0.028)	(0.034)	(0.031)
Sample size	12536	9407	8163	8792

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 comprises 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).

4.2.1 Measurement error

The estimates of the rank effect can potentially be biased by multiple sources of measurement error. Here, we summarize and discuss the results from extensive simulations aimed at quantifying the direction and magnitude of the bias from measurement error. A more detailed description of the simulations can be found in Appendix E.

Sampling error One source of measurement error is the fact that we only observe a student's percentile rank based on a sample of 40 out of 180 students in a school cohort on average. From this sampling ratio, critical observers might conclude that an insufficient number of students are sampled to meaningfully reconstruct the ordinal ranking and obtain an unbiased estimate. However, in Appendix E.2, we show that because students are *randomly* drawn from every school cohort, this sampling design results in classical measurement error in the rank variable, which means that the estimates are indeed biased yet lower bound to the true effect. In addition, our simulations suggest that the attenuation bias converges to a moderate level as the sampling ratio becomes smaller. Even if we only sample 40 out of 400 students at random, we under-estimate the true effect by around 25%. At the average school cohort size of 180 students, we under-estimate the true effect by around 20%. Thus, the effects shown in Section 4 are around 20% lower than the true effects would be.

Measurement error in the Peabody test score A further potential source of measurement error lies in the measurement of the Peabody test score. For example, take a student who had a bad day when taking the Peabody test. This student's test score would be lower than his/her latent cognitive ability, meaning that we would assign this student a lower rank than the rank based on his/her latent ability. Even if such deviations between the test score and a student's latent ability are not systematic — i.e. unrelated to any student characteristics — they may bias the estimate of the rank effect. As we show in Appendix E.3, the extent of this bias depends the ability measure that underlies the ranking in the data-generating process. If the rank in the data-generating process — the rank that actually matters for a student's risky behavior — is based on the latent ability whereas we compute the rank based on the test score, we show that the estimates are biased towards zero. However, if what matters to students is the rank based on the test score, then measurement error in the test score does not bias the estimates.

Gender stratification The estimates could also be biased due to 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 observe whether the effects significantly change if we only consider school cohorts with a relatively even gender balance. In Column 5 of Table 3, 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.

Over-sampling of minorities 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 assigned a higher rank on average. To test whether this source of measurement error is important, we exploit the fact that the in-home sample has precise information on who has been over-sampled and subsequently 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.

4.2.2 Omitted variable bias

A further potential source of bias is omitted variables, i.e. variables that have an effect on the ordinal rank while having a direct impact on risky behaviors. Because all school-cohort variables are absorbed by the fixed effects, omitted variables have to vary at the individual level. In our context, important candidates for omitted variables are unobserved personality traits, influences from a student's environment (parents, teachers, peers, etc.) or tracking.

We distinguish between two types of omitted variables, namely those correlated with the ordinal rank conditional on absolute ability and those affecting the ability test score, which in turn affects the observed ordinal rank. Despite being a seemingly small distinction, both types of omitted variables lead to different biases, with the second type being a more severe source of bias than the first.

The first type is a variable that is correlated with the ordinal rank *conditional on absolute ability* and has a direct impact on the outcome. For example, teachers or parents may treat a student with a low rank differently from one with a high rank, whereby this difference in treatment may directly affect risky behaviors. While such a line of argumentation would fit the definition of an omitted variable, it describes a channel rather than an omitted variable. If parents' or teachers' behavior responds to a child's rank and the rank has been assigned exogenously, then these responses are a channel through which a high rank translates into less risky behavior. A similar argument can be made about tracking: if the number of slots in a higher track is fixed, then students with a higher rank are more likely to proceed to a higher track, which may have an impact on a student's risky behavior. Yet again, students proceed to a higher track *because* they have a higher rank and thus tracking is a mediating factor rather than a confounder. In Appendix E.4.1, we provide a more detailed discussion of this type of omitted variable bias.

The second type of omitted variable is one that is correlated with a student's absolute level of ability, while having a direct influence on risky behaviors. Because the ordinal rank is based on absolute ability, a variable correlated with ability may also affect the ordinal rank, thus biasing the estimated rank effect. One example of such an omitted variable is a student's motivation. A student who is more motivated may achieve a higher test score, while a higher motivation may also lead to less engagement in risky behaviors through other channels. Similar cases can be made for other personality traits. Another example is parental pressure, which may affect a student's test score while simultaneously influencing risky behaviors.

To assess the direction and magnitude of the omitted variable bias, we conduct a Monte Carlo experiment in which we assume a data-generating process where the outcome is determined by rank, absolute ability and an omitted factor that is also correlated with ability. The analysis yields two results. First, we obtain an unbiased estimate as long as the rank in the data-generating process is based on the test score. This is the case because the direct effect of the omitted variable is absorbed by the control for absolute ability. Second, our estimates are biased towards zero if students care about the ranking in terms of latent ability while we only observe a test score that is influenced by latent ability plus an omitted variable. This type of omitted variable bias is equivalent to a classical measurement error in the rank variable. To see this, consider two students with the same latent ability but different motivation. Student A, who has a higher motivation than student B, will have a higher test score and we will assign him/her a higher rank, while in the data-generating process both have the same ability rank. The fact that we assign some students a rank that is too high and others a rank that is too low attenuates the estimate. The magnitude of this bias depends on the extent to which the omitted variable affects the ability test score. In Appendix E.4.2, we provide a detailed account of the simulations.

4.2.3 Misreporting

All measures for risky behaviors are self-reported, which is a potential source of bias in our estimates. The designers of AddHealth aimed at reducing the extent of misreporting of risky behaviors by using a so-called audio-CASI technique. Students listened to the questions through headphones and typed their answers into a computer. This method should eliminate peer pressure as a reason for misreporting, while it also avoids students having to disclose sensitive information to an unknown interviewer. Nonetheless, while this method can help to reduce misreporting, it may not fully eliminate it. For example, the descriptive statistics in Table 1 show a share of 7th and 8th graders having unprotected sex that may seem very large to some observers and could partially be the result of age-specific misreporting. In our regression, we eliminate the bias from age-specific misreporting through cohort fixed effects and controls for relative age. In fact, if misreporting is correlated with any of the control variables but not with rank, it does not bias our estimate of the rank effect.

However, misreporting can introduce a bias if it is systematically correlated with rank; for example, if highly-ranked students are more likely to under-report their engagement in risky behaviors. In Appendix G, we provide a more extensive discussion of the bias, showing that if students with a low rank over-report their risky behavior compared to students of high rank, this leads to an over-estimation of the rank effect. However, given the survey design and the application of audio-CASI, the correlation between rank and misreporting should be fairly small, such that our estimates would suffer from a small positive bias, if at all.

4.2.4 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, allowing them to 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), 96 percent of schoolchildren in the US were enrolled at age 6 in 1968, whereas in 2005 this figure stood at 84 percent. Our regression partly accounts for the possible bias introduced by grade retention or redshirting, because we control for age in months. Nonetheless, the results could be biased if redshirted children systematically differ from those 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 most 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 significantly decline compared to the baseline results. Some coefficients — notably those of smoking and drinking — are no longer statistically significant, although 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.

4.2.5 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 the lowest-ranked students in every grade 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 across all schools, although it would not be captured if dynamic attrition systematically differs between schools. To address this problem, we restrict the sample to students who report in wave IV that they finished high school, whereby we subsequently 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 3. 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, although the coefficients of smoking and sex remain significant at the 10% level and the coefficient of engagement in physical fights is significant at the 1% level.

4.2.6 Ability influenced by prior rank

A further threat to identification could be that the ability test score based on which we compute the ordinal rank may be a function of a student's prior rank. We only observe students in wave I at the average age of 16, which means that we measure their ability a few years after they entered the school. If their ordinal rank at the time of school entry affected their measured ability, we could not interpret the estimate of γ as being causal because the prior rank would lead to omitted variable bias. In Appendix F, we provide two theoretical arguments against omitted variable bias. In addition, we provide evidence that prior rank does not affect current ability by showing that the ordinal rank in wave I does not predict the Peabody test score in wave III.

5 Exploring potential mechanisms

The results confirm our initial hypothesis that students with a lower ordinal rank more commonly engage in risky behavior. 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 his/her ability. However, if a person does not know his/her absolute ability, the ordinal rank in his/her peer group may provide him/her with an imperfect signal about his/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 a low perceived opportunity cost of risky behaviors.

A second explanation could be that the effect is driven by status concerns in combination with sorting into peer groups. This follows the idea of Akerlof and Kranton (2002), who build a model showing how students with different characteristics sort into social categories based on their social distance to each category. Moreover, in a recent paper, Cicala et al. (2015) develop a model in which a peer group is divided into two subgroups: 'nerds', students who achieve social recognition by being successful in school; and 'troublemakers', students who achieve social recognition by being disruptive in school and engaging in risky behaviors. Students sort into these groups depending on their comparative advantage within the peer group and behave as such to conform with their sub-group. In turn, the comparative advantage depends on a student's ordinal rank. The same student who tries to succeed in a class where he/she has a high rank may become a "troublemaker" in a class where he/she has a low rank.

In the following, we provide evidence in support of both mechanisms. While there may be further theories that could explain the effect, our data do not provide sufficient information to test them.

5.1 Popularity and sorting into peer groups

We first test whether we find evidence for the rank affecting sorting into peer groups as predicted in the model of Cicala et al. (2015). This model would predict that low-ranked students are more likely to be friends with students engaging in risky behaviors themselves.

To test this hypothesis, we exploit sociometric information on friendships. All students participating in AddHealth were asked to nominate up to five male and five female best friends from the school roster. Based on this information, we construct indicators of popularity and peer engagement in risky behavior.[13]

Table 4 displays the results of OLS regressions of various friendship variables on the ordinal rank as well as all control variables from Equation 2. We distinguish between in-nominations — how often a student is nominated as a friend by fellow students and the characteristics of those students — and out-nominations, namely how many fellow students a student nominates. The first row displays the effect of a student's ordinal rank on friendship nominations. A high rank significantly reduces the number of in-nominations, while having a small and statistically insignificant effect on out-nominations. This result suggests that highly-ranked students are less popular in their school. For a one within-school standard deviation increase in the ordinal rank, a student would receive 1.8 fewer friendship nominations, which is large given that the mean number of in-nominations is 4.2.[14] We also look at the average GPA as well as the likelihood of drinking and smoking among nominated friends. A student's ordinal rank has a large negative impact on the likelihood of being nominated by fellow students who smoke (smoking intensity is measured on a scale 0-6, mean = 1.07). This finding is in line with the predictions by Cicala et al. (2015), showing that highly-ranked students receive lower social recognition from other students who engage in risky behaviors themselves.

5.2 Other mechanisms

Besides sorting into peer groups, a number of alternative mechanisms can explain the negative relationship between rank and risky behaviors. We provide suggestive evidence of the importance of these mechanisms by estimating Equation 2 with proxies for each mechanism as outcome variables. The results are displayed in Table 5. Each coefficient measures the impact of the ordinal rank on the outcome displayed on the left. The symbol "**1**" represents a dummy variable that equals one if the student agreed to the statement in parentheses, and zero otherwise.

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 that of people with whom they regularly interact. 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. Put simply, students with a high rank may think that they are smarter than is actually the case, whereby they have a higher expected income and thus a higher opportunity cost of engaging in risky behavior. We first test whether students with a

[13]The friendship information was elicited in the 'in-school' sample of AddHealth, thus covering the entire student population. The same sample comprises basic information on smoking and drinking for every student. We construct the peer engagement variables based on this information, which is why the intensity of risky behaviors may not be comparable with the one used in our main estimates.

[14]To take into account the count variable character of friendship nominations, we use poisson regressions in these cases.

Table 4: Rank and Friendship Composition

	In-nominations	Out-nominations
Nominations:		
Number	-0.176*	-0.028
	(0.098)	(0.094)
GPA:		
Peer average	-0.075	-0.068
	(0.073)	(0.082)
Smoking:		
Peer intensity	-0.536***	-0.264
	(0.169)	(0.215)
Drinking:		
Peer intensity	-0.153	-0.054
	(0.125)	(0.118)
Controls:		
Individual controls	Yes	Yes
Individual ability	Yes	Yes
School × cohort fixed effects	Yes	Yes

Notes: This table displays results of separate OLS regressions of peer characteristics 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.

higher rank have a higher self-perception, based on a survey question on perceived intelligence. Indeed, higher rank is strongly related to higher self-perceptions on own ability. Going up by one decile in the rank distribution increases the probability of believing to be more intelligent than the average by 2.3 percentage points.

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 attend college and that they will have a college degree by age 30. These results confirm the predictions of a human capital model, whereby a student's ordinal rank shapes his/her expectations, thereby distorting the trade-off between the short-term pleasure and long-term costs of engaging in risky behavior.

Support from others. Besides providing a noisy signal to oneself, the ordinal rank may also provide a signal to others. For example, Kinsler et al. (2014) show that parents of young children adjust their parental support depending on the relative ability of their child in pre-school. However, not only parents but 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 his/her parents, friends or teachers care about him/her, although we find no evidence.

Table 5: Potential channels

Dependent Variable	Coefficient	SE
Self-concept		
1(I am more intelligent than the average)	0.231***	(0.056)
Grades		
GPA	0.214***	(0.070)
Expectations		
1(I want to go to college)	0.074	(0.048)
1(I will likely go to college)	0.134**	(0.054)
1(I will have a college degree by the age of 30)	0.095**	(0.045)
Support from others		
1(My parents care about me)	-0.012	(0.020)
1(My friends care about me)	-0.034	(0.045)
1(My teachers care about me)	0.006	(0.057)
Self-esteem		
1(I have a lot to be proud of)	-0.014	(0.020)
1(I like myself as I am)	0.022	(0.033)
1(I do everything just right)	0.001	(0.033)

Notes: This table displays the 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 (2)).

Self-esteem. The ability rank could affect individual self-esteem, e.g. through increasing self-confidence in one's own ability. Self-esteem has been shown to significantly 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 research 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 thus 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 that the rank affects sorting into peer groups. Students with a higher ordinal rank are less likely to be friends with those who smoke and drink, as well as being generally less popular. This is consistent with a model in which the rank determines a student's comparative advantage of being in a social category, i.e. being either a 'nerd' or a 'troublemaker' Cicala et al. (2015).

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 he/she would be in the second-best school. While our results show that a child's rank is important, it is important to highlight one caveat: our results are based on estimates *within* schools, with school inputs being held constant. Choosing a better school may result in a lower rank, although 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 policy-makers. Given that risky behaviors impose a significant cost for society, it is important to know their determinants to design interventions that prevent adolescents from engaging in them. Given that an ordinal ranking is present as soon as students slightly differ 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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Online Appendices

(Not for publication)

A Disclaimer

This research uses data from Add Health, a program project directed by Kathleen Mullan Harris and designed by J. Richard Udry, Peter S. Bearman and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill and is funded by grant P01-HD31921 from the Eunice Kennedy Shriver National Institute of Child Health and Human Development, with cooperative funding from 23 other federal agencies and foundations. Special acknowledgment is due to Ronald R. Rindfuss and Barbara Entwisle for assistance in the original design. Information on how to obtain the Add Health data files is available from the Add Health website (http://www.cpc.unc.edu/addhealth). No direct support was received from grant P01-HD31921 for this analysis.

B Summary statistics

Table 6: Sample description: Means and standard deviations

	Mean	(SD)	min	max
Cognitive ability:				
Peabody Vocabulary Test	100.47	(14.64)	13.00	139.00
Control variables:				
Age	15.84	(1.56)	11.43	20.68
Female	0.52	(0.50)	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
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	0.36	(0.48)	0.00	1.00
Number of observations	12536			

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

C Identifying variation in key variables

Table 7 displays the variation in the main variables with various sets of fixed effects. Of particular importance for the identification is the variation in the ordinal rank conditional on absolute ability, which is obtained from the residuals of a regression of the ordinal rank on absolute ability. Column (1) displays the raw standard deviations. Column (2) shows the residual variation after a transformation of separate school and cohort fixed effects. Column (3) shows the residual variation after a transformation of school-by-cohort fixed effects.

Table 7: Residual variation in key variables after fixed effect transformations

	(1) raw SD	(2) School *and* Cohort FE	(3) School $\times$ Cohort FE
Ordinal rank	0.284	0.283	0.281
Ordinal rank conditional on ability	0.161	0.120	0.115
Peabody score	14.645	13.041	12.865
Smoking (on at least 10 out of past 30 days)	0.382	0.367	0.361
Drinking (on at least 2 days per month past 12 months)	0.387	0.374	0.368
Marijuana use (at least once within past month)	0.368	0.360	0.354
Intercourse w/o birth control in past 6 months)	0.281	0.277	0.273
Stealing (at least once past 12 months)	0.418	0.411	0.405
Physical fight (at least once past 12 months)	0.397	0.392	0.387
Drug selling (at least once past 12 months)	0.263	0.260	0.256

Notes: description see text in section.

D Additional results

D.1 Varying the intensity of risky behaviors

In this section, we analyze the extent to which the ordinal rank has different effects for different intensities of risky behaviors such as the onset of risky behaviors, regular or severe engagement (e.g. regular binge drinking). Table 8 summarizes additional results for varying intensities together with the respective outcome variable definitions (delinquent behavior is omitted from this table because in this case we only observe the incidence). The outcomes that we focus on in our presentation of the main results are marked with an asterisk.

Overall, the pattern of significant negative effects of the ordinal rank on probabilities of engaging in risky health behaviors holds across intensities. The relative importance of the ordinal rank at varying intensities appears to differ by risky behavior.

For smoking and marijuana use, we observe stronger effects for onset and regular smoking, but a smaller coefficient for severe smoking and marijuana consumption. For alcohol consumption, we do not observe an effect of the ordinal rank on the onset (ever drank alcohol yes/no), but distinctive negative effects on the severity. With respect to sex, we again observe the strongest influence on having had any sexual intercourse before, with weaker influences on recent or recent risky sexual intercourse.

Table 8: Different intensities of risky behaviors

	Mean	Coeff	Definition
Smoking:			
Ever	*mean = 62.45*	-0.127**	Ever smoked cigarettes yes/no
		(0.062)	
Regularly*	*mean = 17.79*	-0.123***	Smoked on at least 10 out of 30 last days
		(0.045)	
Severe	*mean = 7.92*	-0.048*	Smoked >=10 daily cigarettes on at least 10 out of 30 last days
		(0.028)	
Drinking:			
Ever	*mean = 64.78*	-0.065	Ever drank alcohol yes/no
		(0.053)	
Regularly*	*mean = 18.32*	-0.139***	Drank on at least 2/3 days per month during last year
		(0.043)	
Severe	*mean = 11.05*	-0.100***	Got seriously drunk at least 2/3 days per month during last year
		(0.033)	
Marijuana:			
Ever	*mean = 34.86*	-0.090*	Ever used Marijuana yes/no
		(0.054)	
Recently*	*mean = 16.12*	-0.074*	Used marijuana during past 30 days
		(0.043)	
Severe	*mean = 7.47*	-0.054*	Used marijuana at least 2 times during past 30 days
		(0.029)	
Sex:			
Ever	*mean = 48.00*	-0.145***	Had sexual intercourse before yes/no
		(0.055)	
Recently	*mean = 33.00*	-0.102*	Had sexual intercourse past 6 months
		(0.058)	
Recently w/o birth control*	*mean = 8.66*	-0.055*	Had sexual intercourse without birth control past 6 months
		(0.031)	

Notes: This table displays results of separate OLS regressions of different definitions of binary variables indicating the engagement in risky behavior on the percentile rank. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ – Standard errors, clustered at the school level, are reported in parentheses. – The regression specification is similar to Table 2, Column (3) including school-specific cohort fixed effects.

D.2 Heterogeneous effects

In this section, we analyze heterogeneous effects of the ordinal rank on risky behaviors across various groups. The results are summarized in Table 9. Each set of coefficients (labeled as *main* and *int*) displays results from a single regression as in Equation 2 including an interaction of the rank variable with a respective group indicator.

Overall, we do not find a coherent pattern. In most cases, the interactions are imprecisely estimated, preventing us from identifying heterogeneous effects.

In Column (1), we analyze a potential non-linearity in the rank effect between students of high and low rank. Interaction effects between rank and an indicator for having a rank above the median are small and insignificant. Therefore, our results do not support any non-linear rank effect on risky behaviors, which is in line with findings by Murphy and Weinhardt (2014), who find a virtually linear effect of a student's rank on test scores.

We additionally analyze whether students in high-ability schools are more responsive to their ranking (Column (2)). We do not find any significant heterogeneity with the exception of alcohol consumption, for which the effect is significantly stronger in low-ability schools.

Gender differences (Column (3)) in the rank effect emerge for marijuana use, risky sex and stealing. In these cases, female risky behavior appears to react more strongly to the ordinal rank than that of male students.

We further analyze heterogeneity by family background (parental college degree, Column (4)) and race (Column (5)), finding some differences. In the case of alcohol consumption, children of college-educated parents are more affected by their rank than children of parents with less than a college degree. Non-white students are less affected than white students. The same pattern emerges for stealing. In the case of engagement in physical fights, both children of parents with college degree and non-white students are more likely affected by their rank status.

Table 9: Heterogeneous effects

	(1) Rank > .5		(2) High abil. school		(3) Female		(4) Parents: College		(5) non-white	
	main	int	main	int	main	int	main	int	main	int
Smoking:										
Regularly	-0.099*	-0.018	-0.087*	-0.048	-0.106**	-0.032	-0.110**	-0.035	-0.139**	0.075
	(0.054)	(0.023)	(0.047)	(0.030)	(0.046)	(0.021)	(0.049)	(0.029)	(0.043)	(0.025)
Drinking:										
Regularly	-0.130**	-0.007	-0.184***	0.059**	-0.128***	-0.022	-0.114***	-0.067***	-0.144***	0.023***
	(0.057)	(0.025)	(0.046)	(0.025)	(0.046)	(0.024)	(0.044)	(0.024)	(0.044)	(0.024)
Marijuana use:										
Recently	-0.113**	0.029	-0.084*	0.013	-0.052	-0.042*	-0.066	-0.021	-0.075	0.002
	(0.055)	(0.023)	(0.045)	(0.028)	(0.045)	(0.022)	(0.043)	(0.027)	(0.043)	(0.027)
Sex:										
Risky intercourse	-0.048	-0.005	-0.037	-0.024	-0.034	-0.040**	-0.058*	0.008	-0.047*	-0.036
	(0.043)	(0.019)	(0.035)	(0.021)	(0.031)	(0.018)	(0.033)	(0.021)	(0.032)	(0.021)
Crime:										
Stealing	-0.044	-0.014	-0.075	0.017	-0.037	-0.047*	-0.039	-0.061**	-0.070	0.040**
	(0.058)	(0.026)	(0.049)	(0.028)	(0.050)	(0.026)	(0.048)	(0.028)	(0.046)	(0.031)
Physical fight	-0.125**	-0.008	-0.118***	-0.024	-0.117***	-0.036	-0.113***	-0.060**	-0.119***	-0.083**
	(0.051)	(0.022)	(0.038)	(0.026)	(0.042)	(0.028)	(0.038)	(0.030)	(0.039)	(0.027)
Drug selling	-0.054	0.002	-0.069**	0.022	-0.047	-0.009	-0.044	-0.021	-0.051	-0.003
	(0.035)	(0.014)	(0.030)	(0.018)	(0.031)	(0.020)	(0.027)	(0.020)	(0.027)	(0.016)

Notes: This table displays results of OLS regressions of binary indicators indicating engagement in risky health behavior on the ordinal rank in a cohort and its interaction with group membership indicators. Columns *main* display main effects, *int* display coefficients of interactions. – * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. – Standard errors, clustered at the school level, are reported in parentheses. – The regression specification is similar to Table 2, Column (3) including school-specific cohort fixed effects.

E Measurement error and omitted variable bias

E.1 Basic Monte Carlo experiment

The estimates presented in Section 4 are potentially biased due to measurement error in the rank variable or due to measurement error in the Peabody test scores that we use to construct the ordinal rank. Moreover, there could be unobservable factors that affect the ordinal rank as well as having a direct effect on the outcome. To shed light on the direction and magnitude of the bias arising from measurement error and omitted variables, we carry out an extensive series of Monte Carlo experiments.
In all simulations, we assume the following data-generating process (DGP):

$$y = -0.1r - 0.6a + \delta \tag{3}$$

where r is a student's ability rank, a is his/her ability and δ is a school-cohort specific intercept. For simplicity we drop all subscripts.

E.2 Bias from sampling error in the rank variable

In the first Monte Carlo experiment, we quantify the bias from sampling error in the rank variable. Throughout our analysis, we compute the ability rank within a school cohort based on a random sample of 40 out of 180 students on average. Because we do not observe the full population, we assign some students a rank that is higher than their true rank in the population and others a rank that is lower. Given that the sample was drawn at random from the population, the average sampling error – i.e. the difference between the sample rank and the population rank – is zero, although the standard deviation of the sampling error is greater than zero. Therefore, the error from random sampling can be seen as a classical measurement error of a regressor, which attenuates the estimates.

To quantify the degree of attenuation bias, we conduct a Monte Carlo experiment with ten sets of 1,000 replications. In each replication, we construct a population of schools that have the same features as those in AddHealth. We draw from each school cohort a random sample of 40 students and estimate the model in Equation (3). In each set of replications, we assume a different size of the underlying school cohort. In the first set, the population school cohort size is 40, such that we sample the entire school cohort. In subsequent sets, we increase the population school cohort size in steps of 40 up to 400 students, in which case we only sample 10% of every school cohort.

In each replication, we construct a dataset of 500 school cohorts. To account for heterogeneity in the mean and variance of ability across school cohorts, we draw the ability distribution in two steps. We first draw for each school cohort the mean ability from a normal distribution $\delta \sim N(mean = 101, sd = 7)$, and the standard deviation of ability with $\sigma \sim N(mean = 12, sd = 2.5)$, and in a second step draw the ability of each student in a school cohort from the normal distribution $a \sim N(\delta, \sigma)$. Based on the ability distribution of every school cohort, we compute a student's rank in the population as well as the outcome y using Equation (3). Finally, we draw from each population school cohort a random sample of 40 students, compute the ordinal rank based on this sample, estimate the model

$$y = \gamma r + \beta a + \delta + \varepsilon \tag{4}$$

1,000 times. To assess the bias from measurement error, we compare the average estimated $\hat{\gamma}$

from these 1,000 replications to the true effect $\gamma = -0.1$.

Figure 3 plots the estimated coefficients as a function of the school cohort size N in the underlying population. Overall, we can observe that the sampling error in the ordinal rank biases the estimates towards zero, while the bias peters out as the sampling ratio becomes smaller. At $N = 40$, when we sample the entire cohort there is no bias because the sample rank equals the population rank. At $N = 400$, when the sampling ratio is 10%, the estimated rank effect is smaller but still amounts to $\hat{\gamma} = -0.077$. The vertical dashed line represents the average population cohort size of 180 students. In the average school in AddHealth, we would under-estimate the effect of ordinal rank on risky behaviors by 20%.

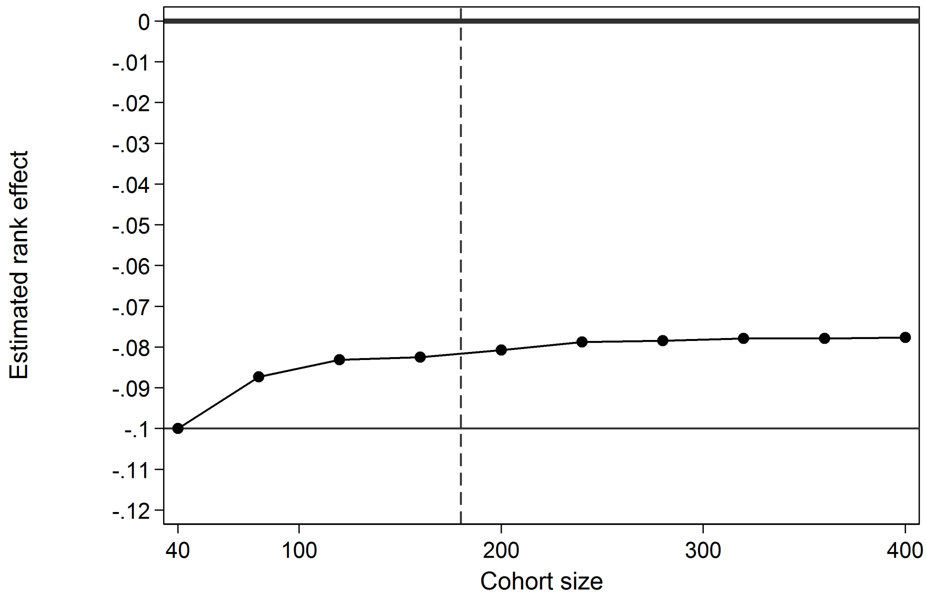

Figure 3: Measurement error due to stratified random sampling

Notes: This figure plots the simulated effect of rank on risky behaviors as a function of the school cohort size in the underlying population. Each point represents the average estimate from 1,000 replications. The assumed true effect is $\gamma = -0.1$. The dashed vertical line represents the average school cohort size in AddHealth of 180 students.

E.3 Bias from measurement error in the test score

A further potential source of bias is measurement error in the Peabody test score. Rather than measuring the pure ability a, the measured ability $\tilde{a}$ also includes an error component z,

$$\tilde{a} = a + \phi z, \tag{5}$$

with ϕ governing the strength of the measurement error. This error can occur either because the ability test score was computed wrongly or due to factors that affect a student's test performance yet are unrelated to rank.

E.3.1 Measurement error in the test score but not in the rank

As long as z is pure measurement error with mean zero unrelated to a student's ordinal rank, it will not affect our estimates of γ, because we control for the measured absolute ability $\tilde{a}$ in all regressions. To verify this, we conduct a Monte Carlo experiment in which we assume that z is drawn from a standard normal distribution with mean zero and variance one. For simplicity, we also draw the latent ability a from a standard normal distribution $a \sim N(0, 1)$. We compute the percentile rank based on the measured ability using Equation (1) and assume that this ranking based on measured ability is the true rank in the data-generating process, i.e. the rank that affects student's engagement in risky behavior. We run 11 sets of 1,000 replications each, varying the measurement error ϕ in steps of 0.05 from 0 to 1. Each replication is based on a dataset of 500 simulated school cohorts with 40 students per school cohort. As shown in Figure 4, as long as the measurement error in the ability variable does not carry over to the ordinal ranking the estimates are unbiased.

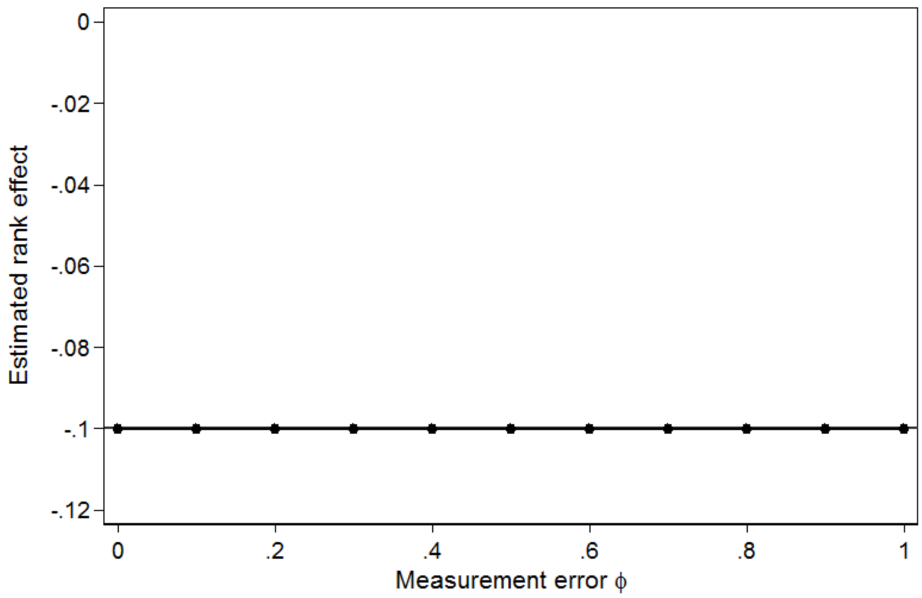

Figure 4: Simulations: measurement error in the Peabody test score

Notes: This figure plots the simulated effect of rank on risky behaviors as a function of the measurement error in the ability test score. Each point represents the average estimate from 1,000 replications. The assumed true effect is $\gamma = -0.1$.

E.3.2 Measurement error in the test score carries over to rank

In a next step, we analyze the bias if the ability is measured with error and the measurement error carries over to the ordinal rank. This would be the case if a student's decisions are affected by a ranking based on latent ability a, while we observe the ability with a measurement error and base the ability rank on the mismeasured variable $\tilde{a}$.

Let $h(a)$ be the ability distribution of student i's school cohort and $f(a_i, h(a)) \rightarrow [0, 1]$ be the assignment function that maps student i's absolute ability into an ability rank that ranges between zero and one. We now assume that the ranking that enters the DGP is based on $r = f(a_i, h(a))$, whereas we observe a ranking based on measured ability $\tilde{r} = f(\tilde{a}_i, h(\tilde{a}))$. Deriving this bias analytically is difficult, because the first step of the rank assignment turns the continuous ability distribution into a discrete ranking.[15]

Therefore, we assess the bias using Monte Carlo simulations. As before, each replication is based on 500 school cohorts, each with 40 students. Latent ability and the measurement error are independently drawn from a standard normal distribution and we compute the rank in the DGP based on the latent ability, whereas we estimate the model using the rank based on measured ability. We run 20 sets of simulations, varying the measurement error from 0 to 1 in steps of 0.05. Each set of simulations comprises 1,000 replications.

Figure 5 plots the estimated effect of ordinal rank as a function of the measurement error ϕ. It shows that this measurement error biases the results towards zero. This suggests that our estimates are a lower bound to the true effect.

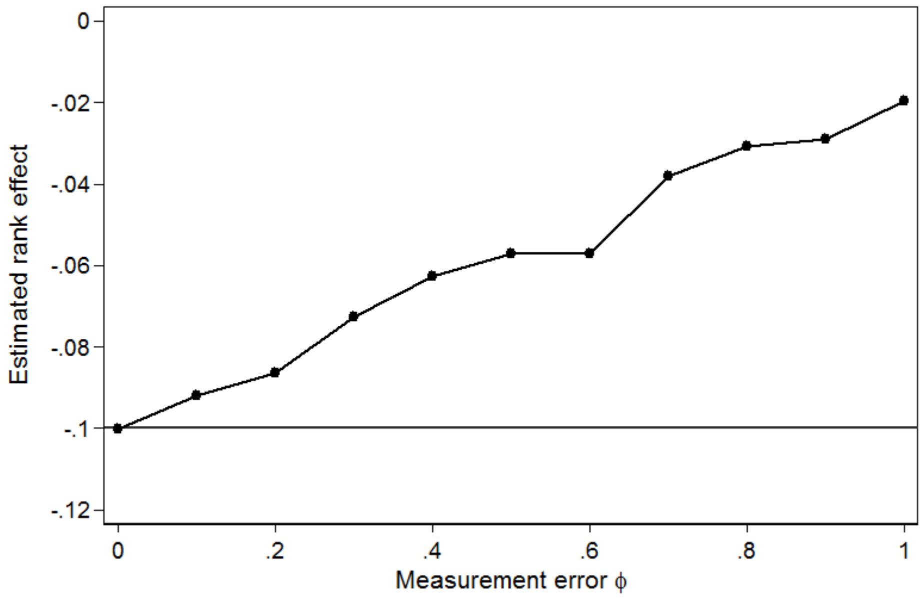

Figure 5: Simulations: measurement error in the Peabody test score that carries over to measurement error in the ordinal rank

Notes: This figure plots the simulated effect of rank on risky behaviors as a function of the measurement error in the ability test score. As opposed to Figure 4, the measurement error in the ability variable affects the ordinal ranking. Each point represents the average estimate from 1,000 replications. The assumed true effect is $\gamma = -0.1$.

[15]The extent to which measurement error in the ability variable carries over into the rank variable depends on the shape of the measured ability distribution $h(\tilde{a})$ and the size of the measurement error z. Take two students with latent abilities $a_2 > a_1$, but $z_1 > z_2$. Based on latent abilities, student 2 ranks higher than student 1. If z_1 is sufficiently large compared to z_2, then student 1 may rank higher than student 2 based on measured ability, $\tilde{a_1} > \tilde{a_2}$.

E.4 Assessing omitted variable bias

A further potential source of bias could be omitted variables that have a direct effect on the outcome while being correlated with the rank and/or the Peabody test score. Examples of omitted variables are parental pressure, peer pressure, a student's intrinsic motivation or other personality traits. We assess two channels through which omitted variable bias can occur: one if the omitted variable is correlated with the ordinal rank conditional on absolute ability and one if the omitted variable affects measured ability, which in turn affects the ordinal rank. We assume the true model to be

$$y = \gamma r + \beta a + \rho z + \delta + \varepsilon, \tag{6}$$

where ε is an i.i.d error term that is uncorrelated with the regressors. The variable z is unobservable and has an impact on the outcome. For the simulations, we assume that z has a negative impact on the outcome, $\rho < 0$.[16]

E.4.1 Omitted variable correlated with rank

A first source of omitted variable bias could be a confounding factor that is correlated with the ordinal rank conditional on absolute ability and school-cohort fixed effects, i.e. $cov(z, r|a, \delta) \neq 0$. So far, we assumed that the assignment of the ordinal rank is quasi-random conditional on a and δ, allowing us to estimate a causal reduced-form relationship between ordinal rank and risky behaviors. This relationship can be the result of multiple channels. Many variables that come mind as being correlated with rank and having a direct effect on the outcomes represent these channels. For example, parents or teachers may support a child with a low rank more than a child with a high rank, whereby this support may directly affect the likelihood of engaging in risky behavior. However, given that parents or teachers respond *because* a child has been assigned a low rank, this response is not a confounder and does not lead to omitted variable bias.

Theoretically, there could exist omitted factors that affect a student's ordinal rank *conditional on his/her absolute ability*, but we find it difficult thinking of a factor that could plausibly classify as such. By contrast, a more likely — and perhaps more severe — source of omitted variable bias is unobserved variables that simultaneously affect the outcome and the ability test score and thus are also correlated with the ordinal rank. We will discuss these in the next section.

E.4.2 Omitted variable correlated with test score

In this section, we discuss two sources of bias whereby an omitted variable z directly affects the outcome as in Equation (6), while at the same time affecting the measured ability test score $\tilde{a}$ as in Equation (5). There are several examples for an omitted variable z that could fulfill these criteria:

- Personality traits. For instance, a student with a higher intrinsic motivation may achieve a higher test score, while motivation may also affect a student's engagement in risky behaviors. Similar cases can be made for more conscientious students, students with more grit, students who are less neurotic, etc.

[16] This would hold for many but not all potential confounders. For example, students who receive more support from their parents may be less likely to engage in risky behavior.

- Parental or peer pressure. Parents may put pressure on a student to achieve a high test score, whereby this pressure may be correlated with other parental inputs that may directly affect the likelihood of children engaging in risky behaviors. Similarly, peers may influence a student's test performance and have a direct effect on risky behaviors.

While we argue that the setting of the Peabody test — a low-stakes test, with the results not being communicated to students, teachers or parents — makes it unlikely that parental pressure or peer pressure plays an important role, we cannot fully eliminate any of these factors as a source of bias. Therefore, we assess the direction and magnitude of the omitted variable bias for various strengths of the direct effect on the outcome, ρ, and the indirect effect through measured ability, ϕ.

We make an important distinction between two DGPs. In one DGP, we assume that students know the measured test score $\tilde{a}$ and the corresponding rank $\tilde{r}(\tilde{a})$, and both variables determine the outcome in Equation (6). In the second case, we assume that students know their latent ability a and the corresponding rank $r(a)$, whereas we estimate our model based on the measured test score $\tilde{a}$ and the corresponding rank $\tilde{r}(\tilde{a})$. In both cases we run simulations for three parameter values for the indirect effect $\phi \in \{0, 0.25, 0.5\}$, and for 20 parameter values of the direct effect $\rho \in [-1, 0]$. For each (ϕ, ρ)-pair, we run 10,000 replications, assuming the direct effect of rank on the outcome to be -0.1 and the effect of ability on rank to be -0.6, as before. It should be noted that the ranges of ϕ and ρ cover fairly extreme cases. For example, a direct effect of $\rho = -0.5$ means that the direct effect of z on the outcome is five times as large as the effect of rank on the outcome.

Case 1) Students base rank on measured ability score In the first case, we assume that both the absolute ability and the ability rank in the DGP are based on the measured test score, i.e. $a = \tilde{a}$, and $r = \tilde{r}(\tilde{a})$. Figure 6 displays the simulation results. The values of the direct effect are displayed on the horizontal axis. The vertical axis shows the estimated rank effect based on 10,000 simulations. As can be seen, an omitted variable that affects both the test score and the outcome does not bias the estimated rank effect as long as the measured test score and the corresponding rank are what matters for students' outcomes. There is no bias because the entire direct effect of z on the outcome is absorbed by the control for measured ability.

Case 2) Students base rank on correct ability score Matters are different when students base their decisions on their latent ability and the corresponding ability rank, whereas we compute the rank based on observed ability. Therefore, the ordinal rank in the DGP differs from the rank used in the estimation. In Figure 7, we show that this setting indeed leads to a bias in the estimates. The estimates are biased towards zero, such that we under-estimate the true effect. The magnitude of the bias depends on the indirect effect. At $\phi = 0$, there is no omitted variable bias, because the omitted variable is uncorrelated with the regressors. If the indirect effect is greater than zero, the estimated rank effect is smaller in absolute value than the true effect. The simulation results become more volatile at very large negative values of ρ, although there is a clear pattern showing that the bias becomes larger the higher the indirect effect, whereas the bias does not increase in ρ. The result that the bias becomes larger with higher ϕ is consistent with the simulations shown in Figure 5, where we interpreted z to be a measurement error. As the simulations show, the same bias occurs if we see z as an omitted variable.

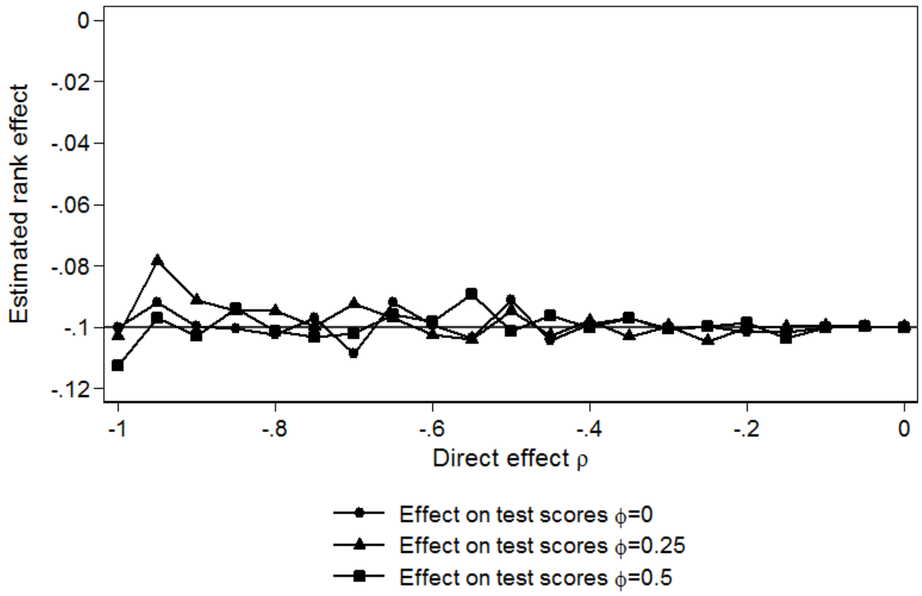

Figure 6: Simulations: omitted variable bias when students base their decisions on the measured ability score and the corresponding rank

Notes: This figure plots the simulated effect of rank on risky behaviors as a function of the direct effect of the omitted variable on the outcome, ρ. The three lines represent distinct values for $\phi\{0, 0.25, 0.5\}$. Each point represents the average estimate from 10,000 replications. The assumed true effect is $\gamma = -0.1$.

F Potential influence of prior rank and reverse causality

One potential threat to identification is that the Peabody score is a function of a student's rank in earlier years, which could cause omitted variable bias. There are at least two arguments against this type of omitted variable bias. First, even if the prior rank affects the ability score, it is unclear that this would affect the *ordinal* ability ranking at the time we measure cognitive ability. Suppose students that had we measure ability in $t = 1$ and suppose that a student with a higher rank in $t = 0$ will have a higher ability score in $t = 1$ than an otherwise-identical student with a lower rank in $t = 0$. Even if the cardinal difference in absolute ability between the two students is greater in $t = 1$ than in $t = 0$, the ordinal difference remains the same. A second argument — discussed in Section 2.2 — is that cognitive ability is formed during childhood and remains stable during adolescence.

To provide further evidence against omitted variable bias, we estimate the model in Equation (2) with school-by-cohort fixed effects, using as dependent variable the Peabody score in wave III of AddHealth. As shown in Column (1) of Table 10, the ordinal rank is not statistically significantly related to the ability score in wave III.

In Columns (2)-(7) of the same table, we provide evidence against reverse causality by regressing the ability score in wave III on risky behaviors in wave I, controlling for a quartic in absolute ability, individual-level controls and school-by-cohort fixed effects. All behaviors predict a lower ability score, but these effects are very small (smokers score on average 0.29 points lower than non-smokers, relative to a mean of 100) and statistically insignificant.

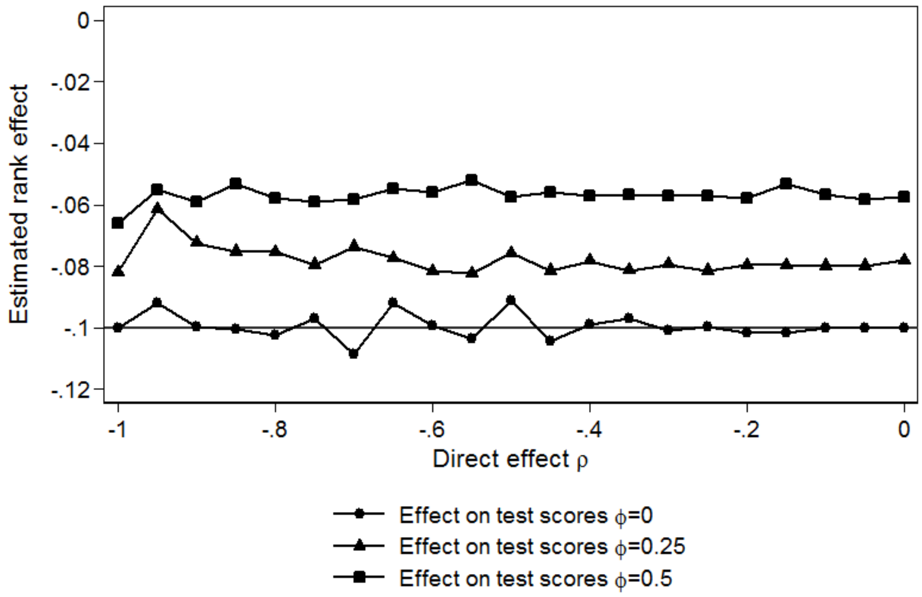

Figure 7: Simulations: omitted variable bias when students base their decisions on their latent ability and the corresponding rank

Notes: This figure plots the simulated effect of rank on risky behaviors as a function of the direct effect of the omitted variable on the outcome, ρ. The three lines represent distinct values for $\phi\{0, 0.25, 0.5\}$. Each point represents the average estimate from 10,000 replications. The assumed true effect is $\gamma = -0.1$.

Table 10: Robustness check: prior rank and reverse causality

	Ability	Moderate behavior			Severe Behavior		
	Rank Wave 1	Drinking	Smoking	Marijuana	Drinking	Smoking	Marijuana
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
AH PVT Wave III	1.422	-0.497	-0.290	-0.019	-0.308	-0.754	-0.244
	(0.914)	(0.904)	(0.908)	(0.912)	(0.907)	(0.906)	(0.917)
Number of observations	12,686	12,613	12,686	12,558	12,613	12,686	12,558

Notes: This table displays the results from a regression of the Peabody test score in wave III on the variables listed in the column headings. The regression controls for a quartic in absolute ability, individual characteristics and school-by-cohort fixed effects.

G Misreporting

A potential source of bias is the misreporting of risky behaviors once it is systematically related to a student's ability rank.[17] For example, more highly-ranked students may face a greater stigma in admitting to smoke or drink and thus they may under-report their engagement. In the following, we assess under what conditions misreporting leads to an over- or under-estimation of the true effect. For this purpose, we view misreporting as a non-classical measurement error in the dependent variable. For simplicity, we consider risky behavior as a continuous variable.

Suppose that the true engagement in risky behavior is y^* and assume the true model to be

$$y^* = \beta_0 + \beta_1 r + \beta_2 a + \delta + \varepsilon, \tag{7}$$

with r being a person's ordinal rank and a being his/her cognitive ability. However, due to misreporting, we observe a risky behavior $y = y^* + \eta$, that partly reflects the true risky behavior and partly misreporting η. Suppose further that misreporting is correlated with rank conditional on own ability and school-cohort fixed effects,

$$\eta = \gamma_0 + \gamma_1 r + \gamma_2 a + \delta + \rho, \tag{8}$$

with ρ being an i.i.d error term. It can be shown that γ_1 is the bias from misreporting in the estimation of Equation (7) with non-classical measurement error in the dependent variable, i.e.[18]

$$\hat{\beta}_1 - \beta_1 = \gamma_1. \tag{9}$$

From this result, we can determine the sign of the bias in the presence of misreporting. Assume that the true effect of ordinal rank on risky behaviors is negative, i.e. $\beta_1 < 0$. If highly-ranked students are more likely to under-report their risky behavior, i.e. if $\gamma_1 < 0$, then we would obtain a positive bias, i.e. in absolute value the estimated coefficient would be larger than the true effect, $|\hat{\beta}_1| > |\beta_1|$. If in turn highly-ranked students are more likely to over-report their risky behavior, then we would under-estimate the true effect.

To assess the magnitude of the bias, it is helpful to consider the strength of the partial correlation of the rank with the misreporting, γ_1, relative to the true effect. If γ_1 is negative and in absolute value is 10% of the true effect β_1, then we would over-estimate the true effect by 10%. If the partial correlation γ_1 as large as the true effect β_1, then our estimated effect would be twice as large as the true effect. While we do not know the true extent of misreporting, its correlation with rank should not be too high, given that students gave the information on risky behaviors without their peers being present and the information was elicited in computer-assisted self-interviews, which gave students little incentive to misreport their behaviors.

[17] Misreporting could also depend on a student's ability, but this would not be a problem because we control for the absolute level of ability in all regressions.

[18] This follows from $\hat{\beta}_1 = \frac{cov(r,y|a,\delta)}{Var(r|a,\delta)} = \frac{cov(r,y^*|a,\delta)}{Var(r|a,\delta)} + \frac{cov(r,\eta|a,\delta)}{Var(r|a,\delta)} = \beta_1 + \gamma_1$