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ABSTRACT

This paper examines the relationship between drinking and academic performance for high school students in 2001 and 2003 Youth Risk Behavior Survey (YRBS) data. In particular, we attempt to determine the extent to which the observed negative association between alcohol use and grades reflects correlated unobserved factors rather than a true causal impact of drinking. Taking advantage of the abundant information the YRBS collects on behaviors that are potentially related with both drinking and academic performance, we estimate regressions that successively add proxies for risk and time preference, mental health and self-esteem, along with measures of other substances used. Results indicate that although estimated effects of drinking on grades are substantially reduced in magnitude when these additional covariates are included, they typically remain significantly negative. The impact on the extensive margin is over twice as large for binge drinking than for non-binge drinking, and binge drinking also has intensive margin effects that non-binge drinking does not. Drinking-related grade reductions are larger among those who are more risk averse and future-oriented. An absence of effects on outcomes with which drinking is likely associated in a non-causal way provides further support for our interpretation of the coefficient estimates as causal effects.

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I. Introduction

The consumption of alcohol has long been associated with a host of undesirable behaviors, from engaging in unsafe sex to traffic fatalities and crime. Policymakers, public health officials and parents are particularly concerned with the harmful effects of underage drinking. While drinking prevalence has declined slightly among high school students in recent years, 80 percent were estimated to have previously drunk alcohol at least once in 2003. Moreover, over the past 30 days, slightly more than half had consumed at least one drink and about a third had consumed at least five beverages in a span of a few hours on at least one occasion (CDC 2004). Considering that these statistics are for youth age 18 and younger, the question of whether drinking has potentially long-lasting consequences is a salient one.

The consequence of alcohol use on which we focus in this paper is academic performance while in high school. Any reductions in grades that result from drinking are short-term effects, in the sense that the alcohol consumption in question presumably occurred in the same academic year, or perhaps not long before that. But to the extent that high school academic performance affects the quality of the post-secondary institution into which students will be admitted or the job that students will be able to obtain after graduating from high school, and that in the extreme poor academic performance can lead to not completing high school, the impact of drinking on grades in high school will likely have far-reaching human capital impacts.

Behaviorally, drinking and educational outcomes could be associated through several mechanisms. Most commonly proposed (and observed, particularly among college students) is that time spent at events at which the primary activity is the consumption of alcohol substitutes at least partially for time that would have been spent on schoolwork, and the inebriation and hangovers that result from heavy drinking lower grades by both temporarily diminishing cognitive skills and further reducing effort. A negative association could instead result from a reverse causal relationship if students drink as a way to cope with academic failings. On the other hand, drinking and academic achievement could be positively correlated if alcohol is a normal good and students of higher socioeconomic status perform better in school, or if students celebrate academic successes by drinking. Indirect effects of alcohol use on grades are also possible through causation of other behaviors that are detrimental (e.g. sexual encounters that lead to time allocations or psychological issues that reduce academic effort) or beneficial (e.g. socializing with high-performing students).

This paper contributes to the economics literature on the relationship between alcohol use and educational outcomes by being the first to investigate the association between drinking and academic performance among high school students. Previous studies that have examined this association have done so for college rather than high school students, while other studies have concentrated on different educational outcomes such as years of schooling completed. We establish the partial correlation that drinking in various intensities and frequencies has with recent grades while holding constant other exogenous determinants of academic performance. Furthermore, as have many previous analyses of the effect of drinking on educational outcomes, we attempt to estimate the extent to which these correlations result from a causal impact of alcohol use on academic performance rather than a spurious correlation through unobserved factors that influence both drinking and grades.

Because of the specific strengths and weaknesses of the Youth Risk Behavior Survey (YRBS) data that we analyze, our method for netting out unobserved heterogeneity deviates from the commonly pursued strategy of identifying causal effects with an instrumental variables (IV) procedure. In particular, the YRBS lacks information on variables that are plausibly exogenous and related to alcohol use but not otherwise academic performance and thus would be suitable instruments for drinking in a grades equation.[1] However, the YRBS does collect abundant information on behaviors that reasonably serve as proxies for factors that are potentially correlated with both drinking and academic performance, such as preferences for risk and time, mental health and self-esteem. In addition, the YRBS reports information on the use of several illegal substances, the consumption of which may either complement or substitute for drinking and which may also directly influence grades. Our strategy for identifying the causal effect of alcohol use is thus to estimate regressions that successively add these endogenously determined variables. Not only does this isolate the impact of various sources of heterogeneity on estimates of the relationship between drinking and grades, but we further argue that the estimated drinking coefficient in the ultimate equation that includes all heterogeneous factors provides a lower bound on the causal impact of drinking.

Two other aspects of our study differ from those that make up the previous literature. First, we analyze two types of drinking, binge and non-binge, and for each type examine effects of both whether or not such drinking took place and the number of days such drinking took place. To separately investigate extensive margin effects of binge and non-binge drinking, we exclude drinkers who did not binge drink from the binge drinking regressions and binge drinkers from the non-binge drinking regressions. We then investigate intensive margin effects of each category of drinking by excluding non-drinkers from the drinking days regressions. This allows us to more precisely determine the comparative academic performance consequences of drinking of different intensities and frequencies than if we simply estimated drinking equations for the

[1] We eschew the commonly pursued strategy of using state-level alcohol policy variables as instruments. As Dee and Evans (2003) point out, within-state rather than cross-state variation in such policies must be used to credibly identify variation in drinking propensities separately from attitudes and preferences that are vary across states, and with only two years of data, the within-state policy variation available to us is insufficient to achieve such identification.

entire sample. Second, to further check that our estimated drinking effects do not still reflect unobserved heterogeneity even after adding the full set of controls for correlated factors, we estimate regressions using identical specifications for several dependent variables that drinking is correlated with but should not causally impact, including having been taught about HIV in school, the number of hours of TV viewed on an average school day, and bodyweight.

Results indicate a large negative partial correlation between alcohol use of both types and academic performance when only exogenous factors are held constant. Adding the full set of heterogeneity controls substantially reduces the magnitudes of the estimated drinking effects. The impact of binge drinking remains significantly negative on both the extensive and intensive margins, while non-binge drinking effects are considerably weaker and maintain significance only on the extensive margin. Sequential addition of subsets of variables that represent different forms of heterogeneity reveal that rates of time and risk preference are the primary sources of non-causal correlation between alcohol use and academic performance, with mental health, self-esteem and use of other substances having little additional impact. Moreover, in regressions with measures other than academic performance as dependent variables, the significance of negative drinking coefficients estimated when only exogenous factors are held constant disappears once our set of endogeneity controls is included. The results of these robustness checks strengthen a causal interpretation of the effects of drinking on grades.

The paper proceeds as follows. Section II reviews the relevant literature, section III describes the data, section IV outlines the methodology, section V presents the results and section VI concludes.

II. Literature Review

Clinical studies have found that heavy drinking impairs brain functioning. Nordby et al. (1999) reviewed evidence that alcohol intoxication reduces recall for a period extending beyond when sobriety is regained. Both Deas et al. (2000) and DeBellis et al. (2000) linked alcohol disorders to reductions in brain functioning among adolescents. Wuethrich (2001) showed evidence from studies of magnetic resonance imaging that sustained alcohol abuse destroys brain matter. These findings suggest the likelihood of a deleterious impact of chronic heavy drinking on academic performance in high school, although it is unclear from these studies whether regular moderate or less regular heavy drinking among teenagers will have adverse cognitive effects that are serious enough to significantly lower grades.

Two previous studies have attempted to estimate the causal impact of drinking on academic performance, but both did so for students in college rather than high school, using data from the College Alcohol Study. Instrumenting for drinking with religiosity, parental alcohol use, family attitude about alcohol use, and measures of the nonpecuniary price of alcohol such as the fraction of drinkers at the school that obtained alcohol without identification, Wolaver (2002) showed that binge drinking and intoxication decrease grades both directly and indirectly by reducing study hours and lowered the probability of an "A" average by between 12 and 18 percent overall. Williams et al. (2003) agreed that alcohol consumption negatively affects grades, but found that this occurred primarily through an indirect effect on study hours rather than a direct effect.

The remaining economics literature on the relationship between alcohol use and educational outcomes has focused on years of schooling completed rather than academic performance. Cook and Moore (1993) showed that heavy drinking in high school reduces years of post-secondary schooling completed and the likelihood of graduating from college in reduced form and instrumental variable (IV) models using National Longitudinal Survey of Youth (NLSY) data. Mullahy and Sindelar (1994) found that the onset of alcoholism symptoms by age 22 reduced schooling by five percent in data on males from Wave 1 of the New Haven site of the Epidemiological Catchment Area survey. Yamada et al. (1996) also used NLSY data and found that a 10 percent increase in the frequency of drinking reduced the probability of graduating from high school by 6.5 percent. Of these, only Cook and Moore (1993) control for the potential endogeneity of drinking in their educational outcome equations, using the state beer tax and minimum drinking age as instruments. However, Dee and Evans (2003) find no effects on educational attainment in an IV model that using within-state variation in these two policy variables to identify drinking, and criticize Cook and Moore (1993) for using only cross-state variation in these measures because other state-level factors that are correlated with drinking may also influence educational attainment. Koch and Ribar (2001) examine data on same-sex sibling pairs from the 1979–1990 NLSY panels, finding that estimates of the educational attainment consequences of drinking vary across standard regression, family fixed effects and IV approaches and concluding that the actual effects are likely to be small. These disparate estimates of the effects of alcohol use on schooling highlight the need for further investigation of the impact of adolescent drinking on educational outcomes.

III. Methodology

Consider a regression of GPA (G) on alcohol use (D) and other exogenous variables (**X**),

(1) $$G = \beta_0 + \beta_1 D + \mathbf{X}\boldsymbol{\beta}_2 + \varepsilon,$$

where β_0, β_1 and $\boldsymbol{\beta}_2$ are parameters to be estimated and ε is the error term. By definition, the vector **X** can only include factors that are observable. However, many unobservable factors, such as rates of risk and time preference, mental health and self-esteem, potentially influence both drinking and academic performance. In addition, the use of other substances might be related to that of alcohol, either as complements that tend to be consumed at the same time because of the environments in which drinking occurs or the interaction of pharmacological effects with alcohol intoxication, or as substitute producers of intoxication-like effects, but are also endogenous choice variables that likely affect grades. In equation (1), all of these unobservable factors, including those that simultaneously determine the use of alcohol and other substances, would be omitted and therefore subsumed by the error term ε. The result is omitted variable bias, i.e. correlation between ε and D, the drinking variable, that biases β_1, the estimated effect of drinking on academic performance.

As explained previously, our data do not include appropriate instrumental variables with which to identify causal effects of alcohol use. In lieu of estimating an IV model, therefore, our methodology for treating this endogeneity problem is to literally address the omitted variable issue by adding sets of proxies for each of the unobserved factors identified above, including measures representing use of various types of substances. In particular, we estimate the regression

$$G = \beta_0 + \beta_1 D + \mathbf{X\beta_2} + \mathbf{R\beta_3} + \mathbf{T\beta_4} + \mathbf{M\beta_5} + \mathbf{E\beta_2} + \mathbf{S\beta_7} + \varepsilon, \tag{2}$$

where **R**, **T**, **M**, **E** and **S** denote vectors of variables that proxy for risk preference, time preference, mental health, self-esteem and (unobservable determinants of) substance use, respectively.

The richness of these additional data that we use to proxy for unobserved heterogeneity allows us to obtain alternative estimates to those prospectively generated by IV. Specifically, to the extent that these proxies control for all types of unobservable heterogeneity in the relationship between academic performance and alcohol use, the parameter β_1 reflects the causal impact of alcohol use on academic performance. Consequently, obtaining an estimate of β_1 in equation (2) is our primary interest in this study. We estimate this and all other regressions, as described below, by ordinary least squares, with standard errors adjusted for state-level clustering and heteroskedasticity of unknown form.

A straightforward implication of our methodology is that the change in the estimate of β_1 when a set of proxies for a specific unobservable factor is added yields information on how problematic omission of that particular unobservable would be. While a large change implies that the unobservable in question is an important component of the heterogeneity that complicates estimation of the effect of drinking on academic performance, a negligible change suggests that the given unobservable is inconsequential. For each pair of academic performance and alcohol use measures that we study, we thus estimate a series of six regressions, starting with equation (1) and sequentially adding an additional vector of proxies in equation (2) (i.e. successively relaxing the constraint that $\boldsymbol{\beta_k = 0}$, k = 3, …, 7 for one parameter vector at a time) until the final regression estimates the complete version of (2). This yields information on both the nature of the relationship between drinking and academic performance and, ultimately, the degree to which simple correlations between these two variables misrepresent causal impacts of the former on the latter.

The first regression estimates equation (1), e.g. simply includes the drinking variable and exogenous explanatory factors. We expect the drinking coefficient in this baseline model to be negative, significant and large, and that the magnitude and significance of this coefficient will steadily diminish as sets of proxies for unobserved heterogeneity are included. The unobserved factors for which we attempt to control are likely to bring about a spurious negative relationship between drinking and academic performance. Risk-averse students might fear the consequences of both not studying and of consuming alcohol. Students who heavily discount the future might ignore potential longer-term negative effects of both drinking and not studying. Depressed students might neglect schoolwork and drink to self-medicate. Students lacking self-esteem might underestimate the benefits of allocating additional time towards schoolwork and drink to fit in with peers. Finally, the use of other substances might be complementary with that of alcohol and cause academic performance diminution separate from any detrimental effects of alcohol. Thus, bias from omission of these unobserved factors is predicted to make the drinking coefficient more negative than is the actual causal effect.

IV. Data

We analyze data from the YRBS, a survey of U.S. high school students administered during the spring of every odd-numbered year starting in 1991. Because information on grades in school was not collected until 2001, we utilize data from only the 2001 and 2003 waves.

The YRBS focuses on risky behaviors established during youth that result in significant health and social problems during both youth and adulthood (CDC 2004). Each wave employs a three-stage cluster sample design to produce a nationally representative sample of students in grades 9–12. First, 57 primary sampling units (PSUs), which consist of large counties, sub-areas of very large counties, or groups of small, adjacent counties, are chosen from a set of about 1,260. Second, slightly less than 200 schools located in these PSUs are chosen. Selection of both PSUs and schools occurs with probability proportional to school enrollment size, with schools containing large numbers of black and Hispanic students oversampled. Third, one or two classes of a required subject are selected at each school. Participation is anonymous and voluntary, with questionnaires self-administered in classrooms during regular class periods and local parental permission procedures followed. Overall response rates are around two-thirds, with non-response from schools being slightly more likely than non-response from students.[2]

Our academic performance measures are constructed from a question asking students whether they would describe their grades during the past 12 months as mostly As, mostly Bs, mostly Cs, mostly Ds or mostly Fs.[3] We construct two binary dependent variables from their responses: indicators for mostly As and for mostly Cs, mostly Ds or mostly Fs, with the former relevant for drinking impacts at the high end of the grade distribution and the latter relevant for impacts at the low end. To simultaneously consider drinking effects at both ends of the grade distribution and allow for more straightforward parameter interpretation, we also construct a GPA variable. Using a four-point scale, the five grade categories are assigned values of 3.7, 2.85, 2, 1.15 and 0.3. This algorithm imposes symmetry and incorporates the expectation that, e.g., any non-B grades for those selecting "mostly Bs" will on average be worse than B (with the opposite true for "mostly Ds" and the analogous relationships holding by definition for "mostly As" and "mostly Fs"). Relative to assigning integers from 4 to 0, this procedure generates conservative marginal effects. As long as the measurement error resulting from grades being bunched into a small number of categories is random, it will simply further reduce the magnitudes of the estimated effects.

We construct four alcohol use variables that capture drinking in the 30 days prior to the

[2] Because the YRBS is administered to students during class sessions, our estimates do not pick up potential effects of drinking on school dropout. Moreover, students with high absenteeism rates are less likely to be sampled. If such students are more likely to drink and perform poorly in school because of preferences for risky and present-oriented activities, rather than perform poorly in school because of drinking, the results in Table 3 suggest that estimated effects of drinking would be smaller if these students were not omitted.
[3] Cassady (2001) finds that self-reported GPA values are "remarkably similar to official records" and therefore are "highly reliable" and "sufficiently adequate for research use."

interview.[4] To begin, we divide the sample into three distinct groups: those who binge drank, defined as consuming five or more alcoholic beverages within a few hours, on at least one occasion; those who drank but did not binge drink; and those who did not drink. Accordingly, we create binary indicators of binge and non-binge drinking, and examine the former in a sample that omits respondents who drank but did not binge drink, and the latter in a sample that omits binge drinkers. The other two drinking variables are the number of days the respondent drank, and binge drank, in the past 30 days, both of which enter the regressions in logged form and are constructed by assigning midpoints to categorical responses.[5] Binge drinking days is analyzed in a sample that includes only binge drinkers, and non-binge drinking days is analyzed in a sample that excludes both non-drinkers and binge drinkers. Specifying the variables and samples in this manner clarifies any differences in effects of drinking of varying intensities and frequencies compared with examining all drinking measures in samples of all respondents, in which case, for instance, impacts of "any drinking" might be driven by binge drinkers.

Exogenous binary components of the vector **X** that appears in all regressions include indicators for being female, each age from 15–18 (age 14 omitted), each grade from 10–12 (grade nine omitted), three races/ethnicities (white, black and Hispanic), urban and suburban residence (rural omitted), the first survey year and each sampled state except one. Non-binary variables included in **X** are real state per capita income, the average monthly state unemployment

[4] The YRBS does not collect information about alcohol use in the past 12 months, which is the time period encompassed by the academic performance question. Although the literal effect of past 30 day drinking on grades is presumably limited, we assume that drinking in the past 30 days is representative of drinking behavior over the previous year. In the 2002 National Survey on Drug Use and Health, for instance, only 30 percent of 14–18 year olds who abstained in the past month reported any past year drinking, and average past year drinking days was 65 for past month drinkers but only 19 for past month abstainers (SAMHSA 2004). On the other hand, this shows that our group of nondrinkers almost certainly contains some students who drink infrequently or have recently quit drinking, and similarly our drinking groups likely includes some students who only recently began drinking. Both types of miscategorizations will bias our estimates towards zero.

[5] The categories for binge drinking days are 0, 1, 2, 3–5, 6–9, 10–19 and 20–30 days, while those for having at least one drink of alcohol are 0, 1–2, 3–5, 6–9, 10–19, 20–29 and all 30 days. For these and other analogously constructed measures, as described below, results are invariant to assigning different values within the given ranges (or any sensible top-coding values for variables with open-ended highest categories).

rate over the previous 12 months, height (which could proxy for early childhood parental inputs) and days of physical education classes in an average week (which might be correlated with academic program rigor).

The proxies for risk preference contained in the vector **R** are measures of frequency of using seatbelts and using sunscreen with an SPF of 15 or higher when outside for more than one hour on a sunny day. Neither should have a substantive effect on grades, but both are presumably correlated with risk preferences. Leigh (1990) and Hersch and Pickton (1995) showed that seatbelt users are more risk averse, while Dickie and Gerking (1996) found that willingness to pay for sunscreen increases with perceived risks from sunlight exposure. For each of these two protective behaviors, indicators of never, rarely, sometimes and mostly using are included in **R**, with always using as the omitted category.

Our time preference proxies, as represented by the vector **T**, are indicators of any previous smoking and ever smoking daily for 30 consecutive days, along with estimates of the number of days smoked during the past 30 days and the number of cigarettes smoked per day on days when smoking occurred in the past 30 days, with the latter two measures constructed by assigning midpoints to categorical responses.[6] Cigarette use might directly impact academic performance, but as cigarette use tends to have a relaxing effect on smokers and deleterious long-term effects of smoking are unlikely among this age group, in all likelihood any direct effect is small and perhaps even positive for current smoking and negligible for previous smoking. Meanwhile, Evans and Montgomery (1994) and Fuchs (1982) showed that smoking behavior at early ages is highly related to rates of time preference.

The vector of mental health proxies **M** includes five variables reflecting behavior over

[6] The categories for days smoked are 0, 1–2, 3–5, 6–9, 10–19, 20–29 and all 30, while those for cigarettes smoked per day are less than 1, 1, 2–5, 6–10, 11–20, and more than 20, with a value of 30 assigned for the last category.

the past 12 months. Four of these, indicators of feeling so sad or hopeless almost every day for two weeks or more in a row that some usual activities were stopped, seriously considering attempting suicide, making a plan to attempt suicide and the number of times suicide was attempted, are symptoms of clinical depression (or closely related, e.g. attempting suicide). The fifth, an indicator that a suicide attempt necessitated medical treatment, might indicate the seriousness of the attempt and thus the severity of any depression that occurred, which might also be a function of the number of suicide attempts. Meanwhile, falling grades are one of the main signs of depression among adolescents, between four and eight percent of whom are thought to suffer from the condition (HIP 2004).

To proxy for self-esteem, we include in the vector **E** indicators regarding self-described bodyweight (very or slightly under- or overweight, with "about the right weight" omitted) and what respondents are trying to do about their weight (lose, gain or stay the same, with "not trying to do anything" omitted), along with an integer from zero to three representing the number of sports teams played on during the past 12 months. Alternately, being overweight and trying to lose weight could have a direct deleterious impact on academic performance, and allocating more time to sports teams could lower grades by reducing time available for schoolwork or increase grades through social relationships established on teams or by improving time utilization skills. Our interpretation of these variables as self-esteem proxies, however, is supported by research from psychology finding that self-esteem is lower among adolescents who consider themselves overweight (Klaczynski et al. 2004) or are dissatisfied with their bodyweight (Frost and McKelvie 2004), which includes boys who consider themselves underweight (Konstanski et al. 2004), but higher among adolescents who participate in sports, particularly on teams (Marsh and Kleitman 2003).[7]

Rather than attempt to specify proxies for unobservables that jointly determine the use of alcohol and other substances, we directly enter measures of the consumption of various drugs into the vector **S**. These include the number of times marijuana, cocaine and inhalants were each used ever and in the past 30 days and the number of times heroin, methamphetamines, steroids (without a doctor's prescription), ecstacy and hallucinogenic drugs were used ever, again constructed by assigning midpoints and top codes to categorical measures.[8] If the use of these substances is associated with diminished academic performance, as suggested by evidence from Chatterji (2003) that consumption of marijuana and cocaine in 10th and 12th grade reduces years of schooling completed, a decline (increase) in the estimated magnitude of the negative effect of drinking on grades when these variables are introduced into the regression implies that alcohol and the two illegal drugs are on net complementary (substitute) goods.

Finally, to maximize sample sizes, for each variable except the indicators of gender, age, grade and race, missing values are recoded to zero and an indicator that the value of the corresponding variable is missing is included. Values for the aforementioned four variables are rarely missing and serious data problems seem possible for those cases, so corresponding observations were excluded. States of residence, and thus the state-level income and unemployment measures, are always observed.

Table 1 displays summary statistics for each of the four drinking variable samples. In weighted terms, 46 percent of students consumed alcohol in the past 30 days, and roughly 60

[7] Our results for these variables, including a large positive association between the number of sports teams and academic performance, are consistent with this interpretation. Marsh and Kleitman (2003) also estimate a positive relationship between sports participation and grades among high school students.

[8] Categories for all substance use variables include 0, 1–2, 3–9, 10–19 and 20–39. Lifetime marijuana use also provided choices of 40–99 and 100 or more, the latter of which we coded as 150, while remaining substance use variables simply contained a single category for 40 or more, to which we assigned a value of 69.5 (i.e. the midpoint of the 40–99 category for lifetime marijuana use).

percent of these students binge drank at least once during that time. On average, binge drinkers did so on slightly more than four of the past 30 days, while non-binge drinkers drank on between two and three of the past 30 days. The median academic performance category is "mostly Bs," with grades higher among non-drinkers than drinkers and among non-binge drinkers than binge drinkers. A similar pattern is evident for many of our unobserved heterogeneity proxies: the prevalence of seatbelt and sunscreen use, smoking, depression symptoms, and the use of other substances are each lower among non-drinkers and non-binge drinkers.

V. Results

a. Main results

Table 2 shows the main estimation results, with italicized entries in the third rows of cells in column 6 representing estimated elasticities or semi-elasticities (for the binary measures) evaluated at the weighted academic performance variable means. Each entry pertains to a different regression of the academic performance variable listed in the panel heading on the drinking measure listed in the row heading. The first column provides estimates for regressions in which only exogenous factors are held constant. The column 2 regressions also include the seatbelt and sunscreen use indicators on the right hand side, column 3 models also controls for cigarette use, column 4 adds the depression measures, column 5 includes the bodyweight-related and sports teams variables, and column 6 adds the controls for other substances used.

The column 1 estimates uniformly indicate a negative association between drinking and grades, and except for non-binge drinking days are also highly statistically significant. As anticipated, the column 6 estimates are substantially smaller than are those in column 1: magnitudes of significant coefficients fall by 60–75 percent. Thus, failure to adequately control for various factors that are correlated positively with alcohol use and negatively with academic performance produces upward bias in the magnitudes of estimated effects of drinking on grades.

Preferences regarding time and, to a lesser extent, risk appear to be responsible for most of this bias. The column 2 estimates are 20–30 percent smaller in magnitude than are those in column 1. The change is even larger in moving to column 3, estimates in which are 50–60 percent smaller than are those in column 2. Depression proxies have a minimal impact, particularly in absolute terms, and estimates increase in magnitude when self-esteem measures are included.[9] Controlling for the use of other substances has little effect on estimated drinking coefficients at the top end of the grade distribution but an impact at the lower end that is similar in proportion to that of adding the risk preference measures, although the absolute effect is much smaller. The relative importance of time and risk preference is notable in that their proxies, unlike those for the other forms of unobserved heterogeneity, are extremely unlikely to directly impact academic performance.

The column 6 estimates, if not pure causal effects, should at least be free of endogeneity from correlations with risk and time preferences, symptoms of depression, self-esteem and the use of other substances. The estimates for the two binary drinking measures imply that alcohol use, even in moderation, impairs academic performance throughout the grade distribution. As expected, binge drinking is more detrimental than non-binge drinking.[10] Binge drinkers are 21 percent less likely to earn A's, 16 percent more likely to receive grades below B, and have GPAs that are 0.1 point, or four percent, lower compared with non-drinkers. Meanwhile, non-binge drinkers are seven percent less likely to earn A's, nine percent more likely to receive grades

[9] Disaggregation of the separate effects of self-assessed weight, weight goals and sports team variables reveals that the latter is responsible for most of the increase in magnitude. Indeed, both GPA and the probability of binge drinking increase with the number of sports teams on which the student played.

[10] Although not shown, the difference between the binge and non-binge drinking coefficients in column 6 is statistically significant at the five percent level for all three academic performance measures.

below B, and have GPAs that are two percent (0.05 points) lower than are non-drinkers.

One might argue that effects on the extensive margin without accompanying intensive margin effects, even with our plethora of controls for unobserved heterogeneity, would merely be identifying a characteristic that differentiates high and low achievers. Put differently, if binge drinking is on average bad in a causal manner, then effects should worsen as binge drinking occurs more often. The significance of the estimated binge drinking days effects in column 6 thus provide further support for our assertion that our estimates have a causal component. Doubling the number of binge drinking days reduces the probability of A grades by seven percent, the likelihood of grades above C by four percent, and GPA by just over one percent.[11]

The implied combined extensive and intensive margin effects of binge drinking are sensible. For instance, halving binge drinking days from the average, four days, to two days decreases the reduction in probability of A grades attributable to binge drinking from 21 to 14 percent, and further halving to one day lowers the marginal reduction to 7 percent, while doubling from four to eight days increases the reduction to 28 percent. The interpretation of the overall impact of binge drinking is therefore straightforward: binge drinking harms academic performance, with increases in binge drinking frequency bringing about additional but decreasing marginal reductions in grades.

In contrast, effects of non-binge drinking days are uniformly insignificant. Why non-binge drinking would lower grades on the extensive but not intensive margin is unclear. It may be simply that even moderate drinking hurts academic performance because it is often associated

[11] If the primary impact of drinking on grades is through study effort, or if cognitive effects of drinking are relatively short term, the ability of students to choose when to drink (i.e. perhaps primarily on weekends and not immediately before exams) suggests that an exponential model might be more appropriate for drinking days: unless drinking occurs on sufficient days that it must encompass school nights, it potentially does not impact grades. However, models specifying drinking days in log form fit the data best among those in which a continuous measure of drinking days is used. Also, results are quite similar when a binary indicator of at least two (or at least three) drinking days is specified.

with socializing and consequently both involves time that would otherwise be devoted to schoolwork and leads to other grade-reducing activities, but our data on non-binge drinking days are not rich enough to identify small intensive margin effects. In particular, 98 percent of non-binge drinkers are in the lower three of the six drinking days categories, compared with only 77 percent of binge drinkers.[12]

b. Results for subgroups of non-missing data & varying risk and time preference

The first column of Table 3 shows the impact of omitting all observations for which any variables are unobserved, which cuts sample sizes by roughly one-third. This does not change any of the qualitative conclusions regarding binge drinking, though intensive margin effects and the extensive margin effect on the probability of A grades are increased while remaining extensive margin effects are reduced. However, the impact of these omissions is important for the non-binge drinking indicator, the decline in coefficient sizes of which, particularly for the lower end of the grade distribution and GPA, render them statistically insignificant. This makes the conclusion that non-binge drinking has a small but nonzero effect on grades rather tenuous.

The remainder of Table 3 contains three pairs of columns, each of which divides respondents from the original samples into two groups based on a single risk or time preference dimension while holding constant all other unobserved heterogeneity proxies. This is another strategy for filtering out the two factors that caused the majority of spurious negative correlation in the relationship between drinking and grades in Table 2. Our expectation is that the academic performance of risk averse and future-oriented students should be more affected by alcohol use

[12] Results are similar when the sample is stratified by gender or grade, with the main differences being that binge drinking days coefficients are larger for females than males, and all coefficients besides those for non-binge drinking days are substantially larger for ninth graders than for those in higher grades (though binge drinking effects are significantly negative for all grades).

than that of more risk-seeking and present-oriented students, for whom part of negative association between drinking and grades is explained by risk and time preferences.

This is precisely what we find. The column 2 sample includes the roughly 40 percent of respondents who used seatbelts at least mostly and sunscreen at least rarely, while the column 3 sample contains remaining respondents. At the top of the grade distribution, effects of binge drinking and the non-binge drinking indicator are significant and several times as large for the risk-averse sample in column 2, with column 3 effects are insignificant except in the case of any binge drinking. At the bottom end, extensive margin effects are actually larger for the less risk-averse sample, but the intensive margin impact is five times smaller and insignificant. Overall, binge drinking has a larger effect on GPAs for risk-averse students.

Results are similar for earning A grades in columns 4 and 5, with the latter sample including the approximately 25 percent of respondents who ever smoked daily for 30 days and the former sample containing remaining respondents. Unlike the risk preference samples, the estimates for grades C through F and GPA parallel those for A's: other than non-binge drinking days, the effect of drinking is larger and only significant for future-oriented students.

Columns 6 and 7 contain the most extreme dichotomy between effects in samples with different risk/time preferences: column 6 reports estimates for the roughly 80 percent of students who did not smoke in the past 30 days and column 7 shows estimates for remaining students. Results for non-smokers are similar to those in column 6 of Table 2, with effects that are larger, particularly for binge drinking days. In contrast, no type of drinking significantly diminishes grades for smokers. Dividing the sample according to current smoking status hence provides a clear example of how rates of time preference can confounds the attempt to establish causation between drinking and grades. Reciprocally, controlling for current smoking removes a substantial spurious component of the negative association between drinking and grades, with resulting estimates for non-smokers (i.e. those who are less present-oriented) providing further evidence of a detrimental causal impact of alcohol use on academic performance.

c. Sensitivity analysis: does drinking affect outcomes that should not be influenced?

To further examine whether our estimated drinking coefficients reflect causal effects, we estimate regressions with specifications that are identical to those underlying columns 1 and 6 of Table 2, except that we replace academic performance with dependent variables that might be correlated with drinking but should not be causally influenced. If including our unobserved heterogeneity proxies indeed adequately controls for endogeneity, any significance in the drinking coefficients from these models should vanish when these proxies are added. Finding appropriate variables for this exercise is a non-trivial matter, however, because many behaviors on which the YRBS collects information, such as fighting and sexual activity, are potentially determined by drinking. We ultimately specify three such measures: an indicator of whether the student was taught about HIV or AIDS in school, the number of hours of TV the student watched on an average school day, and bodyweight (in kilograms).

Column 1 shows a significant negative correlation between binge drinking and learning about HIV/AIDS in school when only exogenous covariates are held constant, indicating that binge drinkers come from schools with less comprehensive health education programs or less effective teachers or were less likely to attend or pay attention in health education classes. This correlation becomes insignificant in column 2 when the heterogeneity proxies are added. In column 3, binge drinking is positively related to TV hours, which could arise from the higher future discount rates that drinkers evidently possess. This relationship disappears for binge drinking days and becomes negative for any binge drinking when the heterogeneity controls are introduced in column 4, which is a more likely direction for any causal effect stemming from substitution of time at social gatherings where binge drinking takes place for time watching TV. Finally, in columns 5 and 6, the magnitudes of positive associations between drinking and bodyweight are reduced by two-thirds, and rendered insignificant in the case of non-binge drinking days, when unobserved heterogeneity is taken into account.

In sum, even when various sources of endogeneity are accounted for, alcohol use, particularly binge drinking, is associated with poorer academic performance. Risk and time preferences are more important sources of unobserved heterogeneity than self-esteem, depression and factors associated with using other substances. Estimates of large negative impacts among risk averse and future-oriented students combined with small or insignificant impacts among those who are more risk-seeking and present-oriented, along with negative effects on outcomes that should not be causally influenced by drinking that disappear when unobserved heterogeneity is controlled for, further suggest that our findings represent causal effects of alcohol use.

VI. Conclusion

This study has examined the relationship between alcohol consumption and academic performance for high school students in 2001 and 2003 YRBS data. Specifically, we ran regressions that successively added proxies for risk and time preference, mental health and self-esteem along with measures of other substances used, in an attempt to estimate the extent to which the observed negative association between drinking and grades reflects omitted unobservable factors rather than a true causal impact of drinking. Results indicate that although estimated effects of drinking on grades are substantially reduced in magnitude when these additional covariates are included, they typically remain significantly negative. The impact on the extensive margin is over twice as large for binge drinking than for non-binge drinking, and binge drinking also has intensive margin effects that non-binge drinking does not. Grade reductions inflicted by drinking vary positively with risk aversion and future orientation. An absence of effects on outcomes with which drinking is likely associated in a non-causal way provide further support for the interpretation of the coefficient estimates as causal effects.

Still, two important limitations prevent the causal effect interpretation of our estimates from being definitive. First, by definition it is impossible to determine if our proxies completely account for unobserved heterogeneity in the relationship between drinking and grades. For instance, we did not explicitly defined proxies to control for unobserved characteristics like inherent rebelliousness and proclivity for illegal activity. However, it seems likely that many of our proxies, particularly the measures of substance use, would be correlated with these and other unobserved factors that create a spurious negative correlation between alcohol use and academic performance. Moreover, the negligible impacts on the estimated drinking coefficients of adding the proxies for depression, self-esteem and unobserved tastes for substance use, all of which were expected to be important sources of heterogeneity, suggests that few further omitted factors are relevant to the relationship between drinking and grades. In that sense, the estimates in column 6 of Table 2 might plausibly be interpreted as causal effects.

The second notable limitation is that we are unable control for individual or familial socioeconomic status. Although student height arguably reflects parental inputs (which is consistent with its estimated large positive association with academic performance), the inputs that affect height are those available the first few years after birth rather than during high school, though familial resources during these two periods might be correlated. And, though the inclusion of state income potentially eliminates bias from omission of personal income, YRBS samples are not necessarily representative within states and other familial resources that might affect academic performance are not observed. This limitation implies that our estimates might in fact provide lower causal effect bounds (in absolute terms): if alcohol is a normal good, a positive reverse causal effect of academic performance on alcohol consumption seems probable, since students with higher socioeconomic status will both have more money to spend on alcohol and perform better in school.[13]

A related reason that our regression coefficients might be biased towards zero (i.e. in a positive direction) is that several studies have observed that moderate drinking, i.e. one or two drinks per day, is associated with productivity increases (e.g. French & Zarkin 1995). This might even affect our binge drinking effect estimates, to the extent that the moderate drinking threshold represents an average consumption flow and the positive wage effects of moderate drinking reflect the accumulation of "social capital" obtained through drinking at social events (Peters and Stringham 2004). If so, students with better social skills might both have better grades and drink more, and this relationship might even encompass occasional binge drinking.

The large magnitudes of the estimated drinking effects, particularly for binge drinking, yields implications for the analysis of policies that reduce the prevalence of alcohol consumption among high school students. Identifying such policies is important, to the extent that high school academic performance is a crucial determinant of future earnings through its influence on the ability to progress through high school and on the quality of post-secondary educational opportunities that are available. Our results show that we cannot assume the effects of such

[13] In principle, the bias from omitting socioeconomic status controls could operate in the opposite direction, i.e. students with better-educated parents might be more aware of the long-run risks of drinking or less willing to break underage drinking laws. But it seems likely that our proxies for unobserved heterogeneity would account for most socioeconomic status-related sources of bias that work in this direction.

policies on academic performance will be nearly as large as simple partial correlations like those shown in column 1 of Table 2, particularly because unobserved heterogeneity from time and risk preferences inflate estimated impacts of drinking on grades. Notwithstanding, we can predict that policies that reduce binge drinking among high school students will significantly improve their academic performance and thus their post-schooling earning prospects.

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Table 1: Summary statistics

Drinking measure:	Past 30 day binge drinking		Days binged in past 30 days		Any past 30 day drinking		Days drank in past 30 days	
	n = 20,855		*n* = 7,386		*n* = 17,862		*n* = 4,393	
	Mean = .356		Mean = 4.33		Mean = .234		Mean = 2.48	
			S.D. = 4.92				S.D. = 2.46	
Variable	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Academic performance								
Mostly As	.291		.188		.327		.257	
Mostly Cs, Ds or Fs	.305		.420		.260		.326	
GPA	2.77	.783	2.54	.818	2.86	.746	2.72	
Exogenous variables								
Female	.496		.469		.523		.561	
Age 15	.253		.215		.271		.259	
Age 16	.262		.258		.263		.258	
Age 17	.239		.288		.218		.235	
Age 18	.133		.172		.119		.145	
Grade 10	.262		.250		.265		.255	
Grade 11	.238		.259		.226		.226	
Grade 12	.214		.273		.195		.242	
White	.668		.744		.625		.619	
Black	.119		.057		.156		.166	
Hispanic	.136		.138		.137		.146	
Height (meters)	1.70	.106	1.71	.106	1.69	.106	1.69	.104
Height & weight missing	.059		.050		.061		.052	
Urban residence	.281		.250		.300		.305	
Suburban residence	.545		.557		.543		.558	
Metropolitan status missing	.002		.001		.002		.001	
Phys. ed. classes per week	2.13	2.20	2.13	2.20	2.15	2.19	2.19	2.18
Phys. ed. classes missing	.083		.079		.083		.075	
Real per capita income (100s)	28662	3820	28442	3821	28843	3835	29038	3898
Past year unemployment rate	4.90	1.14	4.85	1.13	4.92	1.14	4.89	1.13
Year = 2001	.477		.493		.473	.499	.487	
Risk preference								
Never uses seatbelt	.053		.084		.035		.036	
Rarely uses seatbelt	.093		.152		.067		.089	
Sometimes uses seatbelt	.152		.208		.129		.156	
Most of time uses seatbelt	.287		.287		.296		.327	
Seat belt use missing	.006		.006		.007		.009	
Never uses sunscreen	.325		.358		.314		.337	
Rarely uses sunscreen	.296		.327		.288		.318	
Sometimes uses sunscreen	.230		.196		.241		.212	
Most of time uses sunscreen	.111		.090		.119		.107	
Sunscreen use missing	.125		.101		.135		.123	
Time preference								
Any lifetime smoking	.582		.884		.480		.693	
Any lifetime smoking missing	.020		.015		.021		.014	
Ever smoked daily for 30 days	.175		.374		.082		.151	
Ever smoked daily missing	.044		.024		.053		.045	
Days smoked past 30 days	4.09	9.34	9.90	12.5	1.58	5.95	3.55	8.47
Days smoked missing	.035		.058		.027		.044	
Daily cigarettes past 30 days	1.37	4.00	3.42	5.92	.440	1.94	.988	2.80
Daily cigarettes missing	.034		.055		.027		.044	

Table 1: Summary statistics (continued)

Drinking measure:	Past 30 day binge drinking		Days binged in past 30 days		Any past 30 day drinking		Days drank in past 30 days	
Variable	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Mental health (past 12 months)								
Sad for 2 weeks	.268		.366		.240		.325	
Sad for 2 weeks missing	.005		.007		.004		.004	
Considered suicide attempt	.169		.250		.142		.202	
Considered attempt missing	.002		.003		.002		.001	
Planned suicide attempt	.137		.202		.115		.161	
Planned attempt missing	.010		.007		.010		.006	
Suicide attempts	.194	.948	.349	1.30	.124	.699	.183	.813
Suicide attempts missing	.095		.081		.100		.090	
Injured in suicide attempt	.023		.044		.012		.017	
Injured in attempt missing	.098		.085		.102		.095	
Self-esteem								
Very underweight	.020		.018		.020		.016	
Slightly underweight	.139		.137		.136		.118	
About the right weight	.550		.550		.549		.546	
Slightly overweight	.251		.257		.256		.283	
Self-described weight missing	.011		.010		.011		.009	
Trying to lose weight	.450		.476		.448		.490	
Trying to gain weight	.170		.182		.164		.165	
Trying to stay same weight	.189		.175		.194		.185	
Weight goals missing	.008		.008		.009		.009	
Sports teams past 12 months	1.06	1.11	1.07	1.11	1.05	1.11	1.03	1.11
Sports teams missing	.031		.020		.035		.024	
Substance use								
Times marijuana ever	21.6	46.8	50.4	61.8	8.80	29.7	18.8	41.8
Times marijuana ever missing	.011		.013		.010		.009	
Times marijuana past 30 days	4.70	15.0	11.8	22.0	1.46	8.27	3.62	12.7
Times marij. past 30 missing	.014		014		.012		.009	
Times cocaine ever	1.48	8.46	3.96	13.5	.213	2.90	.511	4.47
Times cocaine ever missing	.005		.008		.003		.005	
Times cocaine past 30 days	.533	5.03	1.46	8.24	.031	1.05	.069	1.22
Times cocaine past 30 missing	.011		.009		.012		.011	
Times inhalant ever	1.53	8.22	3.45	12.4	.616	4.76	1.14	6.46
Times inhalant ever missing	.012		.009		.013		.012	
Times inhalant past 30 days	.540	5.31	1.40	8.59	.091	1.70	.174	2.16
Times inhalant past 30 missing	.010		.011		.011		.013	
Times heroin ever	.536	5.47	1.41	8.80	.067	1.91	.106	2.39
Times heroin missing	.005		.004		.006		.007	
Times methamphetamine ever	1.49	8.55	3.76	13.3	.334	3.87	.638	5.27
Times methamph. missing	.002		.002		.003		.004	
Times steroid ever	.871	6.69	1.89	9.71	.308	3.88	.311	3.25
Times steroid missing	.004		.002		.004		.003	
Times ecstacy ever	1.26	7.58	3.20	11.9	.227	2.82	.418	3.57
Times ecstacy missing	.069		.055		.077		.078	
Times hallucinogen ever	1.56	8.39	3.91	13.1	.302	3.36	.663	4.98
Times hallucinogen missing	.143		.117		.154		.141	

YRBS sampling weights are used. Standard deviations are reported only for non-binary variables. Means pertain to non-missing values, which are coded to zero for all variables except the female, age, grade and race indicators. Bodyweight is missing only when height is missing so no corresponding missing value indicator is included. Omitted groups for categorical explanatory variables are age 14, grade 9, other race/ethnicity, rural residence, always uses seat belt, always uses sunscreen, very overweight and “not trying to do anything” about weight. Income and unemployment rate pertain to the state of residence.

Table 2: Effect of drinking on typical past year grades

	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Probability of mostly As					
Any past 30 day binge drinking	-.1740	-.1425	-.0625	-.0569	-.0635	-.0598
	(.0097)	(.0084)	(.0089)	(.0088)	(.0082)	(.0084)
						-.205
Log (days binge drank in past 30 days)	-.0315	-.0232	-.0108	-.0100	-.0142	-.0129
	(.0053)	(.0049)	(.0062)	(.0061)	(.0057)	(.0061)
						-.069
Any past 30 day non-binge drinking	-.0874	-.0743	-.0259	-.0215	-.0234	-.0238
	(.0133)	(.0130)	(.0115)	(.0112)	(.0115)	(.0117)
						-.073
Log (days of non-binge drinking in past 30 days)	-.0019	.0038	.0220	.0229	.0236	.0200
	(.0197)	(.0208)	(.0202)	(.0214)	(.0210)	(.0185)
						.078
	(b) Probability of mostly Cs, Ds or Fs					
Any past 30 day binge drinking	.1949	.1515	.0641	.0570	.0652	.0498
	(.0125)	(.0105)	(.0105)	(.0101)	(.0094)	(.0101)
						.163
Log (days binge drank in past 30 days)	.0582	.0412	.0192	.0180	.0235	.0175
	(.0081)	(.0089)	(.0087)	(.0085)	(.0086)	(.0093)
						.042
Any past 30 day non-binge drinking	.0894	.0749	.0326	.0269	.0293	.0238
	(.0097)	(.0107)	(.0116)	(.0120)	(.0113)	(.0107)
						.092
Log (days of non-binge drinking in past 30 days)	.0355	.0247	.0059	.0052	.0061	.0002
	(.0204)	(.0217)	(.0215)	(.0224)	(.0221)	(.0226)
						.001
	(c) Grade point average					
Any past 30 day binge drinking	-.3942	-.3115	-.1312	-.1160	-.1316	-.1035
	(.0198)	(.0157)	(.0162)	(.0161)	(.0144)	(.0146)
						-.037
Log (days binge drank in past 30 days)	-.1165	-.0830	-.0394	-.0363	-.0476	-.0333
	(.0135)	(.0128)	(.0135)	(.0128)	(.0126)	(.0142)
						-.013
Any past 30 day non-binge drinking	-.1830	-.1546	-.0618	-.0503	-.0547	-.0467
	(.0192)	(.0194)	(.0197)	(.0196)	(.0185)	(.0175)
						-.016
Log (days of non-binge drinking in past 30 days)	-.0407	-.0226	.0180	.0194	.0183	.0225
	(.0309)	(.0347)	(.0329)	(.0339)	(.0324)	(.0315)
						.008
Includes risk preference measures	No	Yes	Yes	Yes	Yes	Yes
Includes time preference measures	No	No	Yes	Yes	Yes	Yes
Includes depression measures	No	No	No	Yes	Yes	Yes
Includes self-esteem measures	No	No	No	No	Yes	Yes
Includes substance use measures	No	No	No	No	No	Yes

Coefficients are from OLS regressions of the academic performance measure in the panel heading on the drinking variable in the left column along with the indicated variables in the bottom five rows and the exogenous variables described in the text, with non-binge drinkers excluded from binge drinking regressions and binge drinkers excluded from the non-binge drinking regressions. Parentheses contain standard errors adjusted for heteroskedasticity and state-level clustering. Italicized entries in column (6) are semi-elasticities for any drinking and elasticities for drinking days, both calculated by dividing the coefficient by the weighted sample mean of the corresponding academic performance variable. Sample sizes are 20,855 for any binge drinking, 7,386 for binge drinking days, 17,862 for any non-binge drinking and 4,393 for non-binge drinking days.

Table 3: Effects of drinking in non-missing & risk/time preference-based subgroups

	Non-missing only	Seat belts mostly & any sunscreen?		Ever smoked daily for 30 days?		Smoked during past 30 days?	
		Yes	No	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(a) Probability of mostly As							
Any past 30 day binge drinking	-.0699 (.0102)	-.0876 (.0161)	-.0293 (.0128)	-.0568 (.0095)	-.0336 (.0173)	-.0712 (.0116)	.0150 (.0143)
Log (days binge drank in past 30 days)	-.0258 (.0075)	-.0290 (.0124)	-.0074 (.0073)	-.0169 (.0084)	.0020 (.0076)	-.0382 (.0116)	.0072 (.0082)
Any past 30 day non-binge drinking	-.0221 (.0141)	-.0440 (.0159)	-.0068 (.0119)	-.0220 (.0120)	-.0106 (.0215)	-.0250 (.0148)	.0050 (.0146)
Log (days of non-binge drinking in past 30 days)	.0288 (.0234)	.0541 (.0346)	-.0067 (.0166)	.0236 (.0224)	.0281 (.0197)	.0220 (.0273)	.0025 (.0186)
(b) Probability of mostly Cs, Ds or Fs							
Any past 30 day binge drinking	.0311 (.0141)	.0384 (.0142)	.0559 (.0118)	.0508 (.0101)	.0243 (.0215)	.0649 (.0123)	-.0250 (.0159)
Log (days binge drank in past 30 days)	.0184 (.0118)	.0416 (.0147)	.0082 (.0105)	.0229 (.0131)	.0061 (.0128)	.0427 (.0148)	.0012 (.0103)
Any past 30 day non-binge drinking	.0067 (.0145)	.0152 (.0159)	.0316 (.0155)	.0202 (.0100)	.0182 (.0254)	.0292 (.0119)	-.0212 (.0219)
Log (days of non-binge drinking in past 30 days)	-.0107 (.0221)	.0160 (.0266)	-.0149 (.0245)	-.0055 (.0234)	.0132 (.0497)	-.0056 (.0224)	.0143 (.0405)
(c) Grade point average							
Any past 30 day binge drinking	-.0855 (.0211)	-.1194 (.0209)	-.0847 (.0210)	-.1004 (.0162)	-.0564 (.0304)	-.1329 (.0187)	.0409 (.0275)
Log (days binge drank in past 30 days)	-.0441 (.0169)	-.0674 (.0215)	-.0255 (.0171)	-.0394 (.0172)	-.0131 (.0241)	-.0847 (.0184)	.0014 (.0176)
Any past 30 day non-binge drinking	-.0271 (.0252)	-.0476 (.0252)	-.0457 (.0192)	-.0401 (.0154)	-.0319 (.0442)	-.0563 (.0191)	.0340 (.0382)
Log (days of non-binge drinking in past 30 days)	.0509 (.0340)	.0406 (.0404)	.0137 (.0346)	.0256 (.0339)	.0306 (.0651)	.0237 (.0331)	.0059 (.0603)

Coefficients are from OLS regressions of the academic performance measure in the panel heading on the drinking variable in the left column along with the exogenous variables and unobserved heterogeneity proxies described in the text, with non-binge drinkers excluded from binge drinking regressions and binge drinkers excluded from the non-binge drinking regressions. Parentheses contain standard errors adjusted for heteroskedasticity and state-level clustering. Column (1) sample sizes are 13,077 for any binge drinking, 4,762 for binge drinking days, 11,133 for any non-binge drinking and 2,818 for non-binge drinking days. Column (2) and (3), (4) and (5), and (6) and (7) each split the original sample along the dimensions indicated in the heading for the corresponding column pair, with all observations with any missing values included in columns (3), (5) and (7). In columns (2) and (3), respective sample sizes are 8,361 and 12,494 for any binge drinking, 2,508 and 4,878 for binge drinking days, 7,591 and 10,271 for any non-binge drinking, and 1,738 and 2,655 for non-binge drinking days. In columns (4) and (5), respective sample sizes are 15,331 and 5,524 for any binge drinking, 3,110 and 4,276 for binge drinking days, 15,391 and 2,471 for any non-binge drinking, and 3,170 and 1,223 for non-binge drinking days. In columns (6) and (7), respective sample sizes are 16,893 and 3,962 for any binge drinking, 4,808 and 2,578 for binge drinking days, 15,729 and 2,133 for any non-binge drinking, and 3,644 and 749 for non-binge drinking days.

Table 4: Effects of drinking on potentially correlated variables that should not be causally affected

	Taught about HIV or AIDS in school		Average TV hours on school day		Bodyweight (kilograms)	
	(1)	(2)	(3)	(4)	(5)	(6)
Any past 30 day binge drinking	-.0166	-.0072	.0930	-.0978	.1727	.0650
	(.0074)	(.0094)	(.0561)	(.0612)	(.1864)	(.2004)
	n = 21,610		*n* = 21,547		*n* = 22,020	
Log (days binge drank in past 30 days)	-.0224	-.0042	.1049	-.0016	-.0221	-.0012
	(.0081)	(.0065)	(.0441)	(.0325)	(.1839)	(.1592)
	n = 7,672		*n* = 7,664		*n* = 7,813	
Any past 30 day non-binge drinking	.0104	.0077	.0855	-.0110	.4040	-.1227
	(.0063)	(.0062)	(.0719)	(.0719)	(.3031)	(.2059)
	n = 18,469		*n* = 18,403		*n* = 18,822	
Log (days of non-binge drinking in past 30 days)	-.0142	-.0038	.0539	.0659	.9992	.3294
	(.0103)	(.0089)	(.0845)	(.0867)	(.3800)	(.3114)
	n = 4,531		*n* = 4,520		*n* = 4,615	
Includes proxies for unobserved heterogeneity?	No	Yes	No	Yes	No	Yes

Coefficients are from OLS regressions of the variable in the column heading on the drinking variable in the left column along with the exogenous variables described in the text and, in the even-numbered columns, the proxies for unobserved heterogeneity, with non-binge drinkers excluded from binge drinking regressions and binge drinkers excluded from the non-binge drinking regressions. Parentheses contain standard errors adjusted for heteroskedasticity and state-level clustering. Sample sizes are indicated below each pair of entries corresponding to the same dependent and drinking variables.