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ABSTRACT

We investigate the extent to which negative alcohol use coefficients in GPA regressions reflect unobserved heterogeneity rather than direct effects of drinking, using 2001 and 2003 Youth Risk Behavior Survey data on high school students. Results illustrate that omitted factors are quite important. Drinking coefficient magnitudes fall substantially in regressions that control for risk and time preference, mental health, self-esteem, and consumption of other substances. Moreover, the impact of binge drinking is negligible for students who are less risk averse, heavily discount the future, or use other drugs. However, effects that remain significant after accounting for unobserved heterogeneity and are relatively large for risk averse, future oriented and drug free students suggest that binge drinking might slightly worsen academic performance. Consistent with this, the relationship between grades and drinking without binging is small and insignificant on the extensive margin and positive on the intensive margin.

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I. Introduction

The consumption of alcohol has long been associated with a host of undesirable behaviors, such as unsafe sex, traffic fatalities and crime. Policymakers, public health officials and parents are particularly concerned with the harmful effects of underage drinking. Prior alcohol use was reported by 80 percent of high school students who participated in the 2003 Youth Risk Behavior Survey (YRBS). Over the previous 30 days, 48 percent of sampled students had consumed alcohol, and 29 percent had drunk at least five beverages in a span of a few hours on at least one occasion (CDC 2004). Considering that fewer than one-sixth of these students have even reached the age of 18, let alone the legal drinking age of 21, the question of whether drinking has potentially long-lasting consequences is a salient one.

This paper focuses on the relationship between alcohol use and academic performance among high school students. Any achievement reductions stemming from drinking are likely to occur in the short term, given the age of students and the avenues through which such effects are feasible. But if alcohol consumption does reduce grades in school, the human capital impact could be far-reaching. High school GPA is an important factor for college admission decisions and presumably affects the quality of job attainable by students who do not attend college. In the extreme, of course, low grades can lead to high school dropout.

Behaviorally, drinking could directly lower academic performance through several mechanisms. Time that students spend at social events at which alcohol is consumed might in part represent time that would have been spent on schoolwork. The inebriation and hangovers that result from heavy drinking at these events can lower grades by both temporarily diminishing cognitive skills and further reducing effort. Alcohol use might indirectly affect grades by prompting other behaviors that are detrimental, such as sexual encounters that lead to effort-

lessening time allocations or psychological issues.

On the other hand, unobserved personal characteristics or circumstances could simultaneously influence alcohol use and academic achievement, leading to a spurious negative correlation between the two. A negative association could instead result from a reverse causal relationship, if students drink as a mechanism to cope with academic failure. Alternately, 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 drink to celebrate academic success. Drinking might even raise school performance by providing the means to socialize 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, we attempt to provide information on whether these correlations might result from a causal impact of alcohol use on academic performance, or merely spuriously arise through unobserved factors that influence both drinking and grades.

The most commonly pursued strategy of identifying causal effects, both generally and in this literature, is to implement an instrumental variables (IV) procedure. This strategy is infeasible for us, however. The YRBS data that we analyze lack information on instrumental variables that are plausibly related to alcohol use but otherwise unrelated to academic

performance.[1] A strength of the YRBS, though, is that it collects abundant information on behaviors that could reasonably proxy for unobserved factors that are potentially correlated with both drinking and academic performance. These factors include preferences for risk, time and addictive substances, as well as mental health and self-esteem.

Our strategy for identifying various forms of unobserved heterogeneity that affect the relationship between drinking and grades, therefore, is to estimate regressions that add these proxies for omitted confounders. This isolates the impact of many commonly hypothesized sources of unobserved heterogeneity. We further argue that once all these proxy variables are held constant, many sources of bias that remained unaccounted for would result in estimates that are too positive rather than too negative. In other words, the magnitude of a negative coefficient on drinking that persists in our complete specification might be compressed rather than inflated.

We also depart from the literature by separately analyzing binge and non-binge drinking. For each type, we examine both extensive margin effects of whether or not such drinking occurred, and intensive margin effects of the number of days such drinking occurred. To investigate extensive margin effects separately for 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 for each drinking category by excluding non-drinkers from the drinking days regressions. This allows us to more precisely determine the comparative academic performance consequences of drinking with different intensities and frequencies than if we simply estimated one set of drinking equations for the entire sample.

[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. With only two years of data, the within-state policy variation available to us is insufficient to achieve such identification.

Results indicate a large negative association between alcohol use and academic performance, for both types of drinking, when only exogenous factors are held constant. Adding the full set of heterogeneity controls substantially reduces the magnitudes of the estimated drinking effects. Ultimately, the impact of binge drinking remains negative and significant for binge drinking on both the extensive and intensive margins. In contrast, effects of non-binge drinking become small and insignificant on the extensive margin, and turn positive on the intensive margin. Separate addition of subsets of proxies suggests that preferences regarding time are the most important source of unobserved heterogeneity, followed by preferences for addictive substances and risk.

We then estimate an extensive set of auxiliary regressions in different subsamples of the data. In particular, stratification on various unobserved heterogeneity proxies provides a further check on whether our estimated drinking effects might possibly reflect causality. The disappearance of binge drinking effects among respondents who highly discount the future, are less risk averse, or consume illegal drugs reiterates the importance of omitted factors. Meanwhile, significant binge drinking effects persist among respondents in the categories that are the duals of the above, i.e. the future-oriented, risk averse and drug free. This emphasizes the possibility that at least a component of the baseline coefficients might represent a causal effect of heavy drinking on academic performance.

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 the period of inebriation. 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 that heavy drinking among adolescents that leads to drunkenness will have adverse cognitive effects that deleteriously impact school performance.

Two previous studies have attempted to estimate the causal impact of drinking on academic performance. Both did so for students in college rather than high school, using instrumental variables regressions in data from the Harvard College Alcohol Study. Wolaver (2002) showed that binge drinking and intoxication decrease grades both directly, through cognitive impairment, and indirectly by reducing study hours. Heavy drinking lowers the probability of an "A" average by between 12 and 18 percent, with most of this effect occurring directly rather than indirectly. In contrast, Williams et al. (2003) estimated that the direct effect of drinking on GPA is positive, but outweighed by a negative indirect effect.

The remaining economics literature on the relationship between alcohol use and educational outcomes has focused on educational attainment rather than academic performance. Two studies using single equation methods concluded that drinking reduces subsequent schooling. 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. In data from the National Longitudinal Survey of Youth (NLSY), Yamada et al. (1996) found that a 10 percent increase in the frequency of drinking reduced the probability of graduating from high school by 6.5 percent. As these estimates are from models that do not control for the prospective endogeneity of drinking, however, they cannot necessarily be interpreted as causal effects.

Similar conclusions are obtained by several additional studies that use IV regression to

investigate whether drinking causally reduces schooling. Cook and Moore (1993) showed in NLSY data that heavy drinking in high school lowers years of post-secondary schooling completed and the likelihood of graduating from college. Koch & McGeary (2005), also in the NLSY, found that alcohol initiation before age 14 decreases the probability of timely high school completion by 7–22 percent. Using NLSY Young Adult data, Chatterji & DeSimone (2005) estimated that binge or frequent drinking among 15–16 year old students lowers the probability of having graduated or being enrolled in high school four years later by 11 percent.

Contradictory IV evidence comes from Dee and Evans (2003), who found in Monitoring the Future data that drinking is unrelated to educational attainment. Likewise, after examining NLSY data on same-sex sibling pairs, Koch and Ribar (2001) concluded from estimates based on OLS, family fixed effects and IV approaches that effects of drinking on schooling are likely to be small. These disparate findings 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**),

$$G = \beta_0 + \beta_1 D + \mathbf{X}\boldsymbol{\beta_2} + \varepsilon, \quad (1)$$

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 preferences for risk and time, mental health, and self-esteem, potentially influence both drinking and academic performance. In addition, the use of other addictive substances might be related to that of alcohol. Other drugs might be complements that are consumed at the same time because of the environments in which drinking occurs or the interaction of pharmacological

effects, or as substitute producers of intoxication-like experiences. Like alcohol, though, their use is liable to both affect academic performance and be endogenous in the GPA equation.

In equation 1, these unobservable factors, including those underlying the use of addictive substances which simultaneously determine the use of alcohol and other drugs, are omitted and hence 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.

Our data do not contain feasible instruments with which to identify causal effects of alcohol use. In lieu of estimating an IV model, therefore, our methodology for treating this omitted variable problem is to add to equation 1 sets of proxies for each of the unobserved factors identified above, including measures reflecting the use of other substances. We specifically estimate the regression

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

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.

When estimating equations 1 and 2 and their variants, we use YRBS sampling weights and adjust standard errors for heteroskedasticity of unknown form. Most regressions use OLS, although for some robustness checks we use alternate methods because the dependent variable is limited in some way.

Our primary interest lies in the estimate of the parameter β_1 in equation 2. Ideally, we would be able to assert that β_1 reflects the causal impact of alcohol use on academic

performance.[2] However, this goal is too ambitious, as it is impossible to verify that our proxies for omitted factors control for all types of unobservable heterogeneity that affect the relationship between drinking and grades. For example, equation 2 still leaves out variables such as ability, peer and sibling influence, and parental education, work status and expectations, any of which might affect both drinking and grades. Though the additional equation 2 covariates might well be correlated with these additional factors, none are included explicitly to proxy for them.

Our more realistic goal, consequently, is to gauge the extent to which an assortment of commonly hypothesized and empirically representable forms of unobserved heterogeneity affects the relationship between drinking and grades. A large decline in the negative coefficient on alcohol use when moving from equation 1 to 2 would serve as evidence that single equation estimates are likely to be biased. In the extreme, elimination of this negative coefficient would imply the absence of a causal effect. In contrast, the persistence of a negative drinking effect that is statistically and practically significant would suggest the possibility that drinking does directly lower grades. This is particularly true if the negative effect holds up using various samples stratified according to values of the unobserved heterogeneity proxies, as discussed further below.

Furthermore, the change in the estimate of β_1 when a set of proxies for a specific omitted factor is added yields information on the importance of that factor to the relationship between drinking and grades, with larger changes indicating greater influence. For this reason, we estimate a series of seven regressions for each of four alcohol use measures that we study. We first estimate equation 1, i.e. equation 2 while imposing the constraint that $\boldsymbol{\beta}_k = \mathbf{0}$, k = 3, …, 7.

[2] A drawback of our approach is that it does not address the possibility of reverse causality, i.e. low grades directly leading to alcohol consumption. To the extent that this occurs, our estimate of β_1 is still larger in magnitude than the true causal effect of drinking on academic performance. However, in two stage least squares regressions of binge drinking on GPA, instrumented by average GPA of students in the same state, the GPA coefficient is extremely small in magnitude and highly insignificant.

Then we estimate five additional regressions, each corresponding to relaxing this constraint for exactly one of the coefficient vectors in equation 2 by adding the corresponding set of variables to the right hand side. The final regression estimates the complete, unconstrained version of equation 2, allowing for an assessment of the overall impact of the forms of unobserved heterogeneity for which we proxy.

In the first regression, which simply controls for drinking and a parsimonious set of exogenous explanatory factors, we expect the drinking coefficient to be negative, significant and large. However, it is likely that at least some of this negative correlation between alcohol use and grades is spurious. Thus, the magnitude and significance of the drinking coefficient is predicted to diminish upon inserting at least some sets of variables that proxy for unobserved heterogeneity. For instance, 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. Consequently, omitting these unobserved factors could make the drinking coefficient more negative than is any true underlying causal effect.

IV. Data

We analyze data from the YRBS (CDC 2002, 2004), 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 collected only in 2001 and 2003, we utilize data from just those two survey years.

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 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 prevalent than non-response from students.[3]

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.[4] Our main dependent variable, which allows for straightforward parameter interpretation, is a numerical GPA measure. Using a four-point scale, the five grade categories are assigned values of 3.75, 3, 2, 1 and 0.25, i.e. the midpoints of each interval with extremes of 4 and 0. Any random measurement error stemming from grades being

[3] 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 absent students drink more heavily and perform more poorly in school than do others, and drinking directly reduces grades (possibly through absenteeism), this selection process will bias the estimated drinking effect towards zero.
[4] Cassady (2001) finds that self-reported GPA values are "remarkably similar to official records," making them "highly reliable" and "sufficiently adequate for research use."

bunched into a small number of categories will simply compress the magnitudes of the parameter estimates. To check that this strategy does not drive our results, though, we also conduct analyses on two binary indicators, one for mostly As and another for mostly Cs, Ds or Fs, using probit models, and also examine the original categorical variable using both ordered probit and interval regression methods.

We construct four alcohol use variables that capture drinking in the 30 days prior to the interview.[5] 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, both of which enter the regressions in logged form and are constructed by assigning midpoints to categorical responses.[6] Binge drinking days is analyzed in a sample that includes only binge drinkers, and non-binge drinking days is analyzed in a sample that includes only drinkers who did not binge. Specifying the variables and samples in this manner clarifies any differences in effects of drinking of varying intensities and frequencies compared with

[5] The YRBS does not collect information about alcohol use in the past 12 months, which is the time period to which the GPA variable pertains. Although the literal effect of drinking in the past 30 days on grades over the past year is by definition limited, we assume that past 30 day drinking is representative of drinking behavior over the preceding year. The statistics cited in the introduction showed that 60 percent of previous drinkers in the 2003 YRBS reported consuming alcohol in the past 30 days. In the 2002 National Survey on Drug Use and Health, 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, these examples show 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.

[6] 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).

analyzing samples of all respondents. In the latter scenario, for instance, the impact 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, with "other" omitted), urban and suburban residence (rural omitted), and a complete set of interactions between an indicator for the 2001 survey year and indicators for each sampled state except one. Also included in **X** is self-reported height, as a proxy for early childhood parental inputs (e.g. Schultz 2002).[7]

The proxies for risk preference contained in the vector **R** are measures of the frequency with which respondents use seatbelts when in a motor vehicle and 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. Similarly, Dickie and Gerking (1996) found that willingness to pay for sunscreen increases with perceived risks from sunlight exposure. For each of these two 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 in the past 30 days when smoking occurred. The latter two measures are constructed by

[7] The state/year indicator interactions partially control for school quality. This is true substantively because of the relationship between state-specific preferences towards education (i.e. funding) and school quality, and logistically because the average number of schools sampled in each YRBS state is only seven. If school quality is inversely correlated with both drinking and grades, imperfectly controlling for its effects will bias our estimated drinking effects towards zero.

assigning midpoints to categorical responses.[8] Cigarette use might directly impact academic performance. But cigarette use tends to relax smokers and deleterious long-term effects of smoking are unlikely among high school students. In all likelihood, therefore, 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 strongly related to rates of time preference.

The vector of mental health proxies **M** includes five variables reflecting depressed mood and suicidal behavior over the past 12 months. Four of these are symptoms of clinical depression: 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 a suicide attempt, and making a plan to attempt suicide, along with the number of times suicide was attempted. The fifth, an indicator that a suicide attempt necessitated medical treatment, is assumed to be correlated with the seriousness of the attempt and thus the severity of any depression that occurred. Controlling for depression symptoms and severity is important because falling grades are considered 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** self-reported bodyweight, four indicators for self-categorized bodyweight relative to the ideal (very or slightly under- or overweight, with "about the right weight" omitted), three indicators representing trying to lose, gain or maintain the same weight (with "not trying to do anything" omitted), and an integer from zero to three representing the number of sports teams played on during the past 12 months. Being overweight could have direct deleterious impacts on academic performance. Similarly,

[8] 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.

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 management skills. We interpret these variables and the weight-related indicators as self-esteem proxies, however. This is supported by research among psychologists 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), including boys who consider themselves underweight (Konstanski et al. 2004), but higher among adolescents who participate in sports, particularly on teams (Marsh and Kleitman 2003).[9]

Rather than attempt to specify proxies for unobservables that jointly determine the use of alcohol and other substances, we directly enter measures reflecting the consumption of various other 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 ever used. All are constructed by assigning midpoints and top codes to categorical measures.[10] Evidence from Chatterji (2006) that consumption of marijuana and cocaine in 10th and 12th grade reduces years of schooling completed suggests that the use of these drugs, and perhaps other substances listed above, is associated with diminished academic performance. If so, a decline in the estimated negative effect of drinking on grades when these variables are introduced into the regression implies that alcohol and these other substances are net complements, while the opposite would imply that they are net substitutes.

[9] 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. Participation in sports might also proxy for attachment to school, which is associated with good academic performance.

[10] 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).

Table 1 displays summary statistics, calculated using YRBS sampling weights, for the academic performance and drinking variables. Each row pertains to a separate sample that corresponds to one particular measure of alcohol use. After deleting all observations in which any analysis variables were missing, there remained 8,315 respondents who did not drink in the previous 30 days, 4,762 who binge drank at least once during that time, and 2,818 who drank at least once but did not binge drink. This yields the sample sizes in the first column, pertaining to combined samples of students who did not drink along with those who binge drank (first row) or drank without binging (third row), and samples of only students who binge drank (second row) or drank without binging (fourth row). Overall, then, nearly 48 percent of students consumed alcohol at least once in the previous 30 days.

The second column shows that in weighted terms, these numbers translate to alcohol use prevalence rates of nearly 37 percent in the binge drinking sample, from which drinkers who did not binge are excluded, and 24 percent in the sample of non-bingers. On average, alcohol was consumed one day per week by binge drinkers, but only slightly more than half as often by drinkers who did not binge. The median average grade is "mostly B's," with non-drinkers performing better than drinkers and binge drinkers performing worse than drinkers who did not binge. The same holds when academic performance is evaluated by the percentage of students receiving mostly A's or mostly C's, D's or F's. Although not shown, a similar pattern prevails for many of our unobserved heterogeneity proxies: the use of seatbelts, sunscreen, cigarettes and other substances is more prevalent among non-drinkers and for drinkers who did not binge compared to binge drinkers, as are depression symptoms.

V. Results

a. Effects of specific omitted characteristics

Table 2 takes an initial look at the impact of each category of unobserved heterogeneity proxies on the estimated relationship between academic performance and drinking. Each cell pertains to a different regression of GPA on the drinking measure listed in the row heading, with corresponding samples defined as in table 1. Coefficients are listed on top, with absolute heteroskedasticity-adjusted *t* statistics beneath. As indicated in the bottom five rows, the first column provides baseline estimates for regressions in which only exogenous factors are held constant, while columns 2–6 each control for the baseline exogenous variables along with exactly one set of omitted variable proxies. The column 2 models include the seatbelt and sunscreen use indicators, column 3 holds constant cigarette use, column 4 inserts the depression measures, column 5 adds the bodyweight-related and sports teams variables, and column 6 controls for other substances used. This allows us to examine the separate influence of each type of omitted factor based on the change in the drinking coefficient from its baseline value.

As expected, the column 1 estimates reveal that when only the few exogenous factors observed in the YRBS are held constant, the association between drinking and GPA is negative. Particularly noteworthy is that the coefficients on binge drinking are much larger than are those on drinking without binging. The average GPA decrement from using alcohol, compared with abstaining, is .40 points for binge drinking (first row) but only .17 points from drinking without binging (third row). The difference across drinking intensity categories is proportionately much larger for days of alcohol use among drinkers. Doubling the number of days on which alcohol is consumed lowers GPA by approximately .14 points for binge drinking (second row), but only .03 points for drinking without binging (fourth row). Moreover, the latter does not approach

statistical significance, while the other estimates are very highly significant.

The column 1 estimates for non-binge drinking appear unlikely to fully reflect a causal effect. Primarily, this is because drinking without binging, i.e. drunkenness, would have little scope to adversely affect cognitive functioning. This leaves spending time in drinking-related activities in lieu of studying as the main avenue through which non-binge drinking could directly impact academic performance. Under that scenario, however, time constraints should be tighter as more days are spent drinking. Instead, additional days of drinking without binging are not statistically related with GPA. Combined with the significant negative extensive margin effect of any non-binge drinking, this suggests that moderate drinkers possess unobserved characteristics that are associated with poor academic performance.

The column 1 binge drinking estimates, meanwhile, indicate an effect on grades that increases with drinking frequency, which would be expected if the impact of binge drinking is causal. The observed diminishing marginal effect with additional days of drinking also seems plausible.[11] Assuming the average extensive margin effect of binge drinking of –.40 points corresponds to the mean of 4.24 binge drinking days, the first day of binge drinking each month would reduce GPA by .20 points [i.e. $-.40 - (-.139 \times \log 4.24)$], while the fifth through seventh days of binge drinking per month lowers GPA by another .07 points beyond the first four. The GPA penalty for students who report binge drinking on 25 of the previous 30 days, which is the maximum value in the data, is .65 points relative to abstainers.

The results in columns 2–6 suggest that the column 1 coefficients at least partially represent spurious correlations between drinking and academic performance. For all four forms of drinking, coefficients are reduced in magnitude (and sometimes become positive in the case of

[11] Students have the ability to choose when to drink, i.e. primarily on weekends and not immediately before exams. This suggests that unless drinking encompasses school nights, it might not impact grades even if it has short-term cognitive effects. However, models specifying drinking days in log form fit the data best.

non-binge drinking days) when individual sets of unobserved heterogeneity proxies are inserted into the equation. The only exception to this is the self-esteem measures in column 5, i.e. weight and sports teams, which have little impact on the estimated drinking effects.

The most important types of omitted factors appear to be time preference, i.e. cigarette smoking (column 3), and the use of other substances (column 6). Controlling for smoking reduces the coefficient on alcohol use by around 65 percent for any binge drinking, 50 percent for days of binge drinking, and 70 percent for any drinking without binging. Meanwhile, holding constant the consumption of other substances lowers the effect of alcohol use by about 40 percent for any binging, 50 percent for binge days and 25 percent for any non-binge drinking. Thus, drinkers are more likely to smoke cigarettes and use other drugs, and these behaviors are strongly associated with relatively poor academic performance. Not surprisingly, the impact of other drugs besides cigarettes is relatively weaker for drinkers who do not binge, and stronger among more frequent binge drinkers, i.e. the prevalence of illegal drug use is higher among frequent bingers than among moderate drinkers and non-drinkers.

It is especially notable that on the extensive margin, i.e. for any drinking of either type, cigarette smoking is a much more important confounder of the relationship between alcohol use and grades than is the use of other drugs. In contrast, for days of binge drinking, the impacts of smoking and other substances are equally important. This is consistent with (though clearly does not confirm) the hypothesis that in this context, cigarette smoking behavior largely reflects time preference whereas the use of other drugs is liable to have a direct impact on grades. In particular, cigarette smoking seems unlikely to have the means to causally reduce academic performance, while drinking and smoking status appear to identify lower-performing students in ways that are highly correlated.

The observed inverse relationship between drinking and grades also appears to reflect in part the influence of risk aversion (column 2) and mental health (column 4), as measured by the use of seatbelts and sunscreen and the presence of depression symptoms, respectively. As hypothesized, drinkers are less risk averse and mentally healthy than are non-drinkers, and both risk aversion and mental health is positively correlated with academic performance.

b. The overall impact of unobserved heterogeneity

The next step in the process of evaluating causality is to estimate GPA equations that simultaneously include all the unobserved heterogeneity proxies. Table 3 shows the corresponding results. Column 1 serves as our first test of whether drinking might directly effect academic performance: the coefficients can be compared to those in the corresponding rows of table 2, column 1 to gauge the total impact of the omitted factors for which we have realizations.

These comparisons show, as was previewed in the rest of table 2, that the vast majority of the baseline negative correlation between alcohol use and academic performance merely reflected common omitted factors. Adding our measures of risk and time preference, depression, self esteem, and drug use reduces the coefficient on alcohol consumption by roughly 80 percent for any binge drinking, 70 percent for days of binge drinking and 85 percent for any non-binge drinking. For days of non-binge drinking, the coefficient becomes positive and marginally statistically significant while doubling in size. Even without further judging the merits of the table 3 estimates, we can surmise that single equation estimates of the relationship between drinking and grades are subject to severe bias from unobserved heterogeneity.

On the other hand, for several reasons, the estimates in column 1 of table 3 do not eliminate the possibility that at least a small part of the baseline relationship does in fact

represent a direct harmful GPA impact of drinking. First, both binge drinking measures in column 1 of table 3 are negative and statistically significant. On average, engaging in binge drinking is linked to a .08 point reduction in GPA. Moreover, doubling the number of days on which binge drinking takes place lowers GPA by .05 points.

Second, using the same simulation logic as before, the gradient of the relationship, which is smaller than earlier documented for the baseline model but similar in shape, is conceivable. An initial monthly binge drinking episode brings about a .02 point GPA decline. An additional three binge events in the month reduces GPA by another .06 points, and three further days of binging lowers GPA by .03 more points. The maximum observed binging days of 25 is associated with a GPA that is .16 points less than that of abstainers. These effects are small, but non-trivial relative to the average GPA of 2.88 in the combined sample of binge drinkers and non-drinkers.

Third, the extensive margin impact of drinking without binging is very small, i.e. one-third of the size of that for binge drinking, and statistically insignificant. Again, there is little reason to expect moderate drinking to have a detrimental effect on school performance.

Fourth, additional non-binge drinking experiences have a beneficial effect of similar size to the adverse effect of additional binge drinking experiences. Doubling the number of days on which moderate drinking occurs is associated with a .06 point increase in GPA. This is consistent with each of the several explanations to which observed positive wage effects of moderate drinking are commonly attributed (Chatterji and DeSimone 2006). Namely, moderate drinking could increase school productivity by providing relaxation, improving health (though beneficial health effects are more likely for adults than adolescents) or enhancing social connections. Alternatively, moderate drinkers might have social skills that also translate to

better academic performance, or could come from more affluent backgrounds and thus have more access to alcohol as well as advantageous educational outcomes.

To this point, therefore, the evidence on the potential causality of alcohol use with respect to academic performance is mixed. Unobserved heterogeneity almost certainly plagues estimates of the baseline relationship between drinking and grades. Yet, some of that initial relationship could represent a direct adverse impact of binge drinking on GPA.

The remainder of Table 3 simply constitutes a check on whether our results are unique to the idiosyncratic specification of the dependent variable. Columns 2–5 depict results from equations that have the same right hand side variables as column 1, but use different measures of the GPA variable and, in accordance, methods to estimate the regressions. In column 2, interval regression is used. This avoids having to use midpoints of the grade categories, as only the category bounds are specified (e.g. "mostly A's" reflects a GPA between 3.5 and 4.0, "mostly B's" reflects a GPA between 2.5 and 3.5). The coefficients, which are interpreted as in OLS, and associated statistical significance levels are nearly identical to those from column 1.

Column 3 estimates are obtained from an ordered probit model. This imposes even less structure on the GPA measure than does the interval model, as it simply recognizes that there are five ranked grade categories. The magnitudes of the coefficients are not directly comparable to those in the first two columns, but the results are qualitatively similar in terms of coefficient signs, relative sizes and significance levels.

The last two columns of table 3 represent probit models in which the dependent variable is a binary indicator of "mostly A's" (column 4) or "mostly C's, D's or F's" (column 5). The associated marginal effects, listed in italics, are similar to those obtained using the ordered probit (which are not shown). The separate probits, though, allow for distinct *t* statistics to be easily

computed for the corresponding grade categories. Also, collapsing the lower three grade categories is appealing, as the proportion of students reporting grades of A and C or lower is roughly equivalent. Again, the implications of the results are similar to those displayed in the first three columns. Furthermore, drinking appears to lower overall academic performance more by reducing the propensity of high grades than by increasing the propensity of low grades.

c. Variation in drinking effects across stratified samples

The final two tables show estimated drinking coefficients in samples stratified by exogenous characteristics (table 4) and proxies for unobserved heterogeneity (table 5). They return to specifications analogous to column 1 of table 3, in which OLS is used to estimate the numerical GPA equation while controlling for all endogenous right hand side variables. Given the insignificant coefficients for drinking without binging in table 3, tables 4 and 5 show estimates only for the two binge drinking samples.

Table 4 reveals that estimated binge drinking effects vary considerably by gender, race, grade level and population density. The GPA discrepancy between drinkers and non-drinkers is the same for each gender, but the negative gradient for additional binge episodes that would be expected to accompany causation is present only for females. While both extensive and intensive margin effects are significant for whites and non-whites alike, the former are almost four times as large for non-whites. The coefficient for any binging shrinks as students progress through high school, and is significant only for 9^{th} and 10^{th} graders, while additional days are associated with lower achievement only for 9^{th} and 12^{th} graders. Drinkers perform worse than non-drinkers in all population density categories, but additional binge events are harmful only for rural residents.

Table 5 engages in seven separate exercises in which the main sample is divided along the lines of a specific behavior representing an omitted characteristic: seatbelt use, sunscreen use, cigarette smoking, depression symptoms, weight goals, sports team participation, and consumption of a substance other than alcohol and cigarettes. A discernable pattern emerges. For students with values of the proxy variable that are associated with lower achievement, binge drinking generally has little effect on school performance. In contrast, drinking effects are large, on both the extensive and intensive margins, for those with values of the proxy variable that are correlated with higher achievement.

The top panel, i.e. first two rows, of table 5 show that binge drinking effects are small and insignificant for students who wear seatbelts no more often than sometimes, but are 3–4 times larger and highly significant for those who wear seatbelts most or all of the time. By this account, alcohol use lowers grades substantially for those who are risk averse but not for those who are not. The next panel reveals that results are less stark for our other risk preference proxy, as drinking effects on the extensive margin are similar regardless of whether students wear sunscreen at least sometimes or no more often than rarely. Yet, on the intensive margin, the alcohol coefficient is particularly large for the former group, but an order of magnitude smaller and insignificant for the latter.

In the third panel, the pattern across three groups defined by smoking behavior is analogous. Among current smokers, who we expect more highly discount the future than do the other two sets of students, binge drinkers have significantly higher GPAs than do non-drinkers, and additional binge drinking episodes have no impact. For students who never smoked, who we predict to most take the future into account, any binge drinking and further days of binging have considerable harmful effects on academic performance that are as large or larger than for any

other subsample in table 5. Alcohol use is deleterious for the middle group of students who previously smoked but do not currently do so, but coefficients are somewhat smaller than for non-smokers.

Perhaps not surprisingly, given the strong influence in table 2 of accounting for the use of other drugs, the comparison in the bottom panel between students who use at least one other substance besides alcohol and cigarettes and those who do not is similar. Drinking coefficients are twice as large for the latter group, although additional drinking days enters significantly for illegal drug users as well.

For the two categories of omitted factors that had the least influence on the baseline relationship between drinking and GPA, depression and self-esteem, the variation in coefficients across subgroups is not as systematic. For instance, drinking effects are much stronger for students who were sad but had no suicidal thoughts than for those who considered, plan or attempted suicide, but are weaker for students who exhibited no depression symptoms. Also, alcohol lowers grades more so for students who are trying to lose weight or think they are overweight, and who did not play on a sports team, than for other students. Still, in all three cases, there is strong evidence that the effect of alcohol use depends on student type.

d. Discussion: unobserved heterogeneity or causation?

Tables 4 and 5 bolster the already strong case for the importance of unobserved heterogeneity. A better question, at this point, might be: is the entire baseline effect of drinking on grades the result of spurious correlation, or can we interpret the evidence as suggesting that at least some small part of the negative association represents causality? The evidence most damaging to the argument in favor of a causal effect is that, for all sample stratifications in tables

4 and 5, the alcohol use coefficient for at least one of the subsamples is much smaller and less significant in at least one of the two binge drinking regressions. One might argue that the diminutions of cognitive skills or study time that drinking must induce if it indeed directly affects academic performance should not depend on attributes such as preferences for risk, time and drugs.

On the other hand, we also observe negative and significant coefficients for at least one subgroup in each of the sample stratifications in tables 4 and 5. With that in mind, it could be precisely because of the particular characteristics that are unequal across the stratified categories that negative effects of binging in specific subsamples might in fact reflect causal impacts. Specifically, the strategy of introducing the omitted factor proxies in tables 2 and 3 is motivated by the suspicion, which the results confirm, that binge drinking is associated with poorer grades in part because students who drink are, on average, worse students regardless of their drinking habits. Indeed, in table 5, with the exception of the samples divided by sports participation and depression symptoms (for those without suicidal thoughts or attempts), it is the lower-achieving samples in which we find the weaker drinking effects. For instance, in the third panel of table 5, the average sample-weighted GPA is 2.53 among current smokers, 2.82 among non-smokers who previously smoked, and 3.13 among lifetime non-smokers. This is also true across genders and population density categories and for 12^{th} graders v. others.

A possible implication of the significant results generally occurring among students with higher GPAs is that the scope for alcohol use to influence grades in school exists only for students who are applying the cognitive skills and using the study time that drinking would reduce. Poorer performers might not experience academic harm from drinking because the effort these students allocate towards school is already minimal. However, the ability and effort that

better performers allocate towards schoolwork can be adversely impacted by heavy drinking.

Many of the remaining differences in estimates across subgroups could be related to alcohol consumption representing a more extreme behavior for groups exhibiting larger drinking coefficients (dividing column 3 by column 1 yields the unweighted drinking prevalence for the sample in the corresponding row). This is relevant for stratification by race and grade level in table 4, as well as by sports participation in table 5. Further inspection of the data, not shown here, reveals that highly frequent binge drinking, which could represent clinically defined alcohol dependence or abuse, is more prevalent among non-whites, 9th graders and students not on a sports team. In particular, binge drinking on at least 20 of the previous 30 days is reported by 3.7 percent of non-whites, 2.7 percent of sports non-participants and 2.3 percent of 9th graders, compared with 1.4 percent of whites, 1.3 percent of sports participants and 1.7 percent of 10th and 11th graders. Very heavy drinking could thus be driving the results for these groups.

Finally, it is important to note that our inability to control for several factors could act to mask causal effects that are in fact present. First, binge drinking is likely to be measured with error. Random measurement error from not having exact frequency information, along with inevitable recall error among students who often engaged in binge drinking, will bias the coefficients on days of binge drinking towards zero. Further, if lower achievers are more likely to underreport binge drinking, perhaps to avoid the perception of a link between the two, effects of both binge drinking variables will be underestimated. Second, socioeconomic status is not observed. Higher family income is known to be correlated with better academic performance, and is also thought by many researchers to increase alcohol use by shifting out the budget constraint sufficiently to impart positive bias in estimated wage effects of drinking (e.g. Cook and Peters 2005). Third, teen drinkers might have social skills that also translate to better grades.

For instance, Chatterji and DeSimone (2006) find an otherwise unexplained positive association between binge drinking in 10th grade and the wage ten years later.

By definition, it is impossible to determine if additional omitted variables affect the relationship between drinking and grades. Academic ability is positively associated with school performance, but could be related in either direction, or not at all, with binge drinking. Reciprocally, factors such as parental drinking, parental monitoring and family structure are understood to be correlated with both teen alcohol use and poor academic performance, but such links might be adequately accounted for by our unobserved heterogeneity proxies.

Finally, comparison of table 2, column 3 with table 3, column 1 shows that time preference, as represented by the four cigarette smoking measures, accounts for about three-quarters of the total impact on the estimated binge drinking coefficients of the potentially confounding factors for which we control. This means that the use of other drugs, which has a strong individual effect on the binge drinking estimates, and risk aversion and depression, which have non-negligible effects, do not have much further influence once time preference is held constant. Meanwhile, the self-esteem measures have little separate effect. This might indicate that few further omitted factors are relevant to the relationship between drinking and grades. In sum, unobserved heterogeneity is undoubtedly quite important and most certainly cannot be ignored, but the possibility remains that binge drinking has a small negative causal impact on academic performance.

VI. Conclusion

This study attempts to extend a short literature on the potential effects of drinking on academic achievement, and in particular is the first of which we are aware to do so using data on

high school students. It focuses on gauging the impact of unobserved heterogeneity on the relationship between past month alcohol use and previous year grades among respondents to the 2001 and 2003 YRBS. Specifically, we ran regressions that added proxies for risk and time preference, mental health and self-esteem along with measures of other substances used.

Results show strong evidence that a large component of the negative association between drinking and school performance represents the influence of omitted variables. Namely, baseline effects of binge drinking are reduced by factors of five and three on the extensive and intensive margin, respectively, while effects of drinking without binging are either eliminated or changed in sign. In addition, for several subgroups defined by demographic characteristics or behaviors thought to be related to potentially confounding characteristics, binge drinking is uncorrelated with achievement. Time preference, measured by cigarette smoking behavior, appears to be the main source of spurious correlation. Consumption of other drugs, risk aversion and depression symptoms are further sources of bias, while self-esteem does not seem to be relevant.

However, in the full sample, binge drinking continues to enter GPA equations negatively and significantly, conveying reductions of .08 points from any binging and .05 points when the number of binge episodes is doubled. Moreover, in stratified samples, alcohol-related grade decrements are as large as .19 points from binge participation and .12 points from binge drinking on twice as many days. Given several sources of bias against finding harmful drinking effects, and that our remaining unobserved heterogeneity proxies have little impact once cigarette smoking (i.e. time preference) is held constant, the possibility that binge drinking does indeed harm academic performance cannot be dismissed.

Our inability to draw a conclusive inference about causality limits the implications of our results for policies that are able to reduce drinking among high school students. We can make

two statements with confidence. First, it is quite possible that drinking that does not involve binging has no detrimental impact on academic performance. Consequently, increased achievement should not necessarily be expected to accompany actions that limit moderate alcohol use among adolescents. Second, caution must be used when interpreting estimated binge drinking effects generated by methods that do not attempt to control for the endogeneity of alcohol use in academic performance equations. Such methods may lead to overestimates of the potential GPA impact of effective binge drinking reduction programs. Yet, because improved grades lead to better post-schooling earnings opportunities, and our analysis leaves open the prospect that reducing or eliminating binge drinking can lead to small GPA improvements, policymakers should not completely discount the possibility that decreasing binge drinking among students could improve their socioeconomic status in the long run.

In the extreme, binge drinking is simply a behavior that identifies poor academic performers. Even in this case, our results are interesting because they highlight the types of students for which heavy drinking is associated with lower grades. This generally implies a focus on groups who either achieve more highly or are less likely to binge, including females, non-whites, freshmen and sophomores, non-smokers, drug non-users and seatbelt wearers, although the latter three characteristics might not be easily identifiable. It also calls for research on why binge drinking more adversely affects grades for these students.

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Table 1: Means of drinking & academic performance variables

Variable	Sample size	Mean	GPA (4 point)	GPA = A	GPA = C or below
Binge drank past 30 days	13,077	.368	2.88 (0.81)	.310	.285
Days binge drank past 30 days	4,762	4.24 (4.70)	2.64 (0.86)	.197	.394
Drank but did not binge in past 30 days	11,133	.238	2.98 (0.76)	.353	.239
Days drinking for non-bingers in past 30 days	2,818	2.39 (2.24)	2.85 (0.80)	.281	.290

YRBS sampling weights are used. Parentheses contain standard deviations, reported only for non-binary variables.

Table 2: Unobserved heterogeneity and the effect of drinking on GPA

	Sample size	(1)	(2)	(3)	(4)	(5)	(6)
Any past 30 day binge drinking	13,077	–.401 (20.1)	–.296 (14.5)	–.145 (6.28)	–.344 (17.0)	–.398 (20.4)	–.250 (11.4)
Log (days binged in past 30 days)	4,762	–.139 (7.64)	–.103 (5.68)	–.071 (3.78)	–.121 (6.66)	–.141 (7.95)	–.069 (3.74)
Any past 30 day non-binge drinking	11,133	–.169 (7.62)	–.131 (6.13)	–.049 (2.17)	–.139 (6.28)	–.168 (7.79)	–.122 (5.48)
Log (days of non-binge drinking in past 30 days)	2,818	–.025 (0.71)	–.006 (0.17)	.030 (0.89)	–.021 (0.59)	–.013 (0.36)	.021 (0.59)
Includes measures of:							
Risk preference		No	Yes	No	No	No	No
Time preference		No	No	Yes	No	No	No
Depression		No	No	No	Yes	No	No
Self-esteem		No	No	No	No	Yes	No
Substance use		No	No	No	No	No	Yes

Coefficients are from OLS regressions of GPA on the drinking variable in the left column, the exogenous variables described in the text, and, in columns 2–6, the set of unobserved heterogeneity proxies corresponding to the row among the bottom five in which Yes appears. Drinkers who do not binge are excluded from the binge drinking regressions and binge drinkers are excluded from the non-binge drinking regressions. Parentheses contain absolute values of heteroskedasticity-adjusted *t* statistics.

Table 3: Effects of drinking on grades in various specifications

Outcome		GPA	GPA (5 levels)	GPA (5 levels)	Pr(A)	Pr(C or below)
Method	Sample size	OLS	Interval	Ordered Probit	Probit	Probit
		(1)	(2)	(3)	(4)	(5)
Any past 30 day binge drinking	13,077	–.082	–.085	–.152	–.062	.033
		(3.57)	(3.65)	(4.37)	(5.11)	(2.54)
		–.028			*–.200*	*.116*
Log (days binged in past 30 days)	4,762	–.045	–.047	–.076	–.024	.020
		(2.46)	(2.52)	(2.90)	(2.82)	(1.94)
		–.017			*–.122*	*.051*
Any past 30 day non-binge drinking	11,133	–.026	–.027	–.049	–.021	.006
		(1.23)	(1.24)	(1.40)	(1.58)	(0.52)
		–.009			*–.060*	*.025*
Log (days of non-binge drinking in past 30 days)	2,818	.055	.058	.093	.031	–.014
		(1.69)	(1.81)	(1.79)	(1.73)	(0.76)
		.019			*.110*	*–.048*

Coefficients are from regressions of the academic performance measure in the top row on the drinking variable in the left column along with the full sets of exogenous variables and unobserved heterogeneity proxies described in the text. Columns 4–5 show average marginal effects across observations. Drinkers who do not binge are excluded from binge drinking regressions and binge drinkers are excluded from the non-binge drinking regressions. Parentheses contain absolute values of heteroskedasticity-adjusted *t* statistics. Italicized entries 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 measure.

Table 4: Effects of drinking on GPA in samples stratified by exogenous characteristics

Drinking measure	Any binge		Log (binge days)	
	Sample size	Coefficient (*t* statistic)	Sample size	Coefficient (*t* statistic)
	(1)	(2)	(3)	(4)
Females	6,742	–.093	2,251	–.077
		(2.96)		(3.03)
Males	6,335	–.091	2,511	–.022
		(2.73)		(0.90)
Non-white	6,108	–.193	1,902	–.063
		(5.11)		(2.05)
White	6,969	–.054	2,860	–.047
		(1.93)		(2.16)
9th grade	3,025	–.172	794	–.070
		(3.39)		(1.66)
10th grade	3,318	–.091	1,111	.004
		(1.96)		(0.10)
11th grade	3,347	–.044	1,327	–.006
		(1.14)		(0.17)
12th grade	3,387	–.018	1,530	–.057
		(0.46)		(2.20)
Urban	4,084	–.117	1,401	–.042
		(2.87)		(1.20)
Suburban	7,059	–.063	2,630	–.033
		(2.04)		(1.41)
Rural	1,934	–.138	731	–.099
		(2.37)		(2.38)

Coefficients are from regressions of GPA on the drinking variable in the top row along with the full sets of exogenous variables and unobserved heterogeneity proxies, using the sample described in the left column. Drinkers who do not binge are excluded from all regressions. Parentheses contain heteroskedasticity-adjusted *t* statistics.

Table 5: Effects of drinking on GPA in samples stratified by proxies for unobservables

Drinking measure	Any binge		Log (binge days)	
	Sample size	Coefficient (*t* statistic)	Sample size	Coefficient (*t* statistic)
	(1)	(2)	(3)	(4)
Wears seatbelt sometimes, rarely or never	3,827	–.032 (0.73)	2,005	–.021 (0.73)
Wears seatbelt most of time or always	9,250	–.102 (3.88)	2,757	–.075 (3.15)
Wears sunscreen rarely or never	8,524	–.078 (2.66)	3,317	–.013 (0.61)
Wears sunscreen sometimes, most of time or always	4,553	–.098 (2.68)	1,445	–.124 (3.92)
Current smoker	3,222	.110 (2.20)	2,633	–.003 (0.11)
Current non-smoker but smoked previously in lifetime	4,481	–.064 (1.96)	1,562	–.087 (2.61)
Lifetime non-smoker	5,374	–.187 (4.40)	567	–.120 (2.14)
Considered, planned or attempted suicide	2,452	–.005 (0.09)	1,263	–.050 (1.39)
Sad for 2 weeks but no suicide thoughts or attempts	1,923	–.159 (2.81)	830	–.117 (2.85)
Has no depression symptoms	8,702	–.079 (2.80)	2,665	–.015 (0.65)
Not trying to lose/maintain weight and does not think is overweight	4,335	–.043 (1.13)	1,562	.002 (0.06)
Trying to lose or maintain weight or thinks is overweight	8,742	–.106 (3.71)	3,200	–.067 (3.17)
Did not play on a sports team	5,864	–.094 (2.39)	2,067	–.056 (1.91)
Played on at least one sports team	7,213	–.083 (2.91)	2,695	–.029 (1.22)
Used at least one illegal drug	5,955	–.027 (0.85)	3,825	–.037 (1.79)
Used no illegal drugs	7,122	–.061 (1.74)	937	–.072 (1.78)

Coefficients are from regressions of GPA on the drinking variable in the top row along with the full sets of exogenous variables and unobserved heterogeneity proxies, using the sample described in the left column. Drinkers who do not binge are excluded from all regressions. Parentheses contain heteroskedasticity-adjusted *t* statistics.