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Peer Effects and their Role in Binge Drinking across American College Campuses

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Abstract

Universities have widely varying rates of heavy drinking, raising the question, “Do peers influence individual decisions to binge drink?” This study measures the effect of peer influences on binge drinking across U.S. college campuses using the 1997 and 1999 College Alcohol Studies. We find that a 1 percentage point increase in the level of peer use increases the probability of binge drinking 0.96 percentage points and frequent binge drinking 0.83 percentage points. By including both price and substance use control policies in our analyses, we provide evidence on both the direct price/policy effects on substance use behavior and the indirect price/policy effects that operate through the peer effect, referred to as the social multiplier.

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I. Introduction

College students use and abuse more substances than their same-age counterparts and are more at risk for a variety of detrimental consequences as a result. Prevalence statistics based on population surveys reveal that substance use and abuse among U.S. college students is higher than estimates for the general population of that age. The 2000 National Household Survey of Drug Abuse (NHSDA) found annual rates of alcohol use to be 61.9% among young adults between the ages of 18 and 25 (NHSDA, 2001) College students engage in heavy episodic drinking at higher rates than their same-age peers who do not attend college (O'Malley and Johnston, 2002; Bachman et al., 1984). Heavy episodic drinking is associated with a number of adverse health, educational, and social consequences, including physical injury, high-risk sexual behavior, alcohol overdose, alcohol-impaired driving, psychosocial problems, anti-social behavior, and academic difficulties (Perkins, 2002). An estimated 500,000 college students aged 18-24 suffer unintentional injuries while under the influence of alcohol and 1,400 die each year from alcohol-related unintentional injuries (Hingson et al. 2002).

Although important research has shown that select legislative policies have a direct effect in reducing drinking among college students (Chaloupka and Wechsler 1996, Czart et al. 2001), very little is known about the impact of peer influences on college student demand for these substances. Yet preliminary descriptive analysis derived from the 1999 wave of the College Alcohol Studies (CAS) data suggest that peer effects are present among college students and their decision to use addictive substances. For example, in the case of binge drinking, nearly half (43%) of students reported binge drink during the two weeks prior to taking the survey. When asked to reveal reasons for their drinking participation, one-fourth of student drinkers stated that

drinking was important to them because everyone else is/was drinking. These estimates highlight the importance of understanding the effects of peer characteristics and actions if we hope to design effective substance abuse control policies across America's college campuses. A growing literature examines both theoretically and empirically the effects of peer or neighborhood effects on individual behavior. This research extends the initial research conducted by Chaloupka and Wechsler (1996) and Czart et al. (2001) by including and evaluating the importance of the effects of peer influences on college student heavy drinking participation.

The primary data used are the 1997 and 1999 waves of the College Alcohol Study conducted by the Harvard School of Public Health (See Wechsler et al. 1998, 2000, and 2002 for descriptions.). The data are merged with additional school- and state-level control policies. In addition, campus-level measures of alcohol use as well as student body socio-demographics are constructed. Single-stage probit model estimates along with findings from two-staged instrumental variable models and school-level fixed effects models that account for the potential endogeneity of our peer measure and for the selection of college students into peer groups/colleges are presented. Formal specification tests of the exogeneity of our peer measure and robustness tests of the results are also employed and reported.

Several findings from this study carry important implications for substance use policies across American college campuses. First, these results provide evidence on the importance of peer influences on college students' substance use. Though heavy drinking may be detrimental to an individual college student, most economists argue that interventions to decrease heavy drinking rates are necessary only if negative externalities are involved. While the main externalities of heavy drinking include automobile accidents and increased crime, establishing the existence of and measuring the extent of peer effects provides an additional rationale for such

market interventions. Second, our findings provide evidence on the direct price/policy effects on youth substance use and indirect price/policy effects that operate through the peer effect, referred to as the social multiplier effect. Indeed, if peers influence individual behavior, the benefits of successful policies that reduce problem behaviors of some will have ripple effects on others. Finally, this study helps define the complex relationship between peer influences, strict control policies and substance use among America's college students.

II. Literature Review

Empirical research on peer effects tends to focus on two possible avenues: how the socioeconomic characteristics of the neighborhood or group affect individual behavior (for example see Evans, Oates and Schwab, 1992), or how the actions of the group affect individual behavior (Gaviria and Raphael 2001 and Sacerdote 2000), or both (Hanushek et al. 2000). Our interest is in the latter focal point; the impact of peers' behaviors on individual's propensity to drink heavily. Many studies focus on younger adolescents, but a growing body of research examines the behavior of college age students.

Two papers by Manski (1993, 2000) provide a thorough critique of existing empirical studies of social interactions. Manski offers a framework of three effects that may be captured by statistical analysis of social effects: endogenous, contextual, and correlated effects. The latter effect predicts that agents may behave similarly because they operate under the same, unobserved institutional constraints, and should not be considered a social effect. The first two, endogenous and contextual effects, are social, and distinguish whether the individual's behavior varies with the actions or characteristics of the group, respectively. A further econometric problem exists if social effects are present; the action of the individual influences the action of the group. This latter problem is the so-called reflection problem and peer effects estimates are then characterized by an endogeneity bias.

In order to take account of the potential endogeneity, several methods of identification are proposed including instrumental variable methods. Evans, Oates and Schwab (1992) use instrumental variables to identify contextual effects on teenagers' decisions to become pregnant and to drop out of school. Their choice of instruments has been criticized (Hanushek et al. 2000), highlighting the difficulty in choosing valid exclusion restrictions.

The instrumental variable approach is also used by Norton et al. (1998) in their study of peer effects on adolescent alcohol and tobacco use. They employ a two-stage selection correction mechanism, using parental perceptions of neighborhood characteristics as instruments for the choice of peer group and therefore peer behavior. They find little to no change in the estimates of peer influence before and after controlling for selection. Extensive tests of the overidentifying restrictions fail to reject the validity of the instruments.

Another example of instrumental variable techniques is Gaviria and Raphael (2001). In examining several outcomes including smoking and drinking among adolescents, they assume that contextual effects do not exist and, therefore, that average family socioeconomic background characteristics should affect the average group's behavior but not the individual's. Since schools and neighborhoods are highly correlated, these instruments are arguable. They find strong peer effects for drinking, church attendance, drug use, cigarette smoking, and dropping out of school. Evidence of endogeneity bias is only found for drug and alcohol use.

Powell et al. (2005) expands the youth cigarette demand literature by undertaking an examination of the determinants of smoking among high school students incorporating the importance of peer effects and allowing cigarette prices (taxes) and tobacco control policies to have a direct effect and an indirect effect (via the peer effect) on smoking behavior. To control for the potential endogeneity of the school-based peer measure they implement a two-stage

generalized least squares estimator using instrumental variables. Peer effects are found to have a significant impact on youth smoking behavior: moving a high-school student from a school where no youths smoke to a school where one quarter of the kids smoke would increase the probability that he or she smokes by about 14.5 percentage points. Overall, the results reveal that there is a strong potential for social multiplier effects with respect to any exogenous change in cigarette taxes or tobacco control policies.

Cleveland and Wiebe (2003) examine the issue of peer use similarity among adolescents using fixed and random effects to account for school/institutional effects. They find that peer-individual use was more highly correlated at schools higher overall substance use than in schools with less substance use. The effect was stronger for tobacco use than alcohol use. Their hypothesis is that it is easier for individuals with an existing propensity to use to find peers who also use. This suggests that there is an independent peer effect, in addition to the selection effect. Longitudinal data provide an additional method to separate the selection effect from a true "causal" peer effect. Maxwell (2002) uses a random named peer's substance use to examine the decision to initiate or to quit alcohol, marijuana, cigarette, chewing tobacco use, and initial sexual activity. She finds that peer's previous use influences the initiation only of cigarette and marijuana use, and peer's previous use influences both the initiation and quit decisions for alcohol and chewing tobacco. Lundborg (2006) also uses school-grade fixed effects in addition to instrumental variables with data on Swedish adolescents to wash out the selection effect from the endogenous effect. Since students are randomly assigned to classes within a school, the variation across classes in peer use within the school-grade identifies the model. As in Norton et al. (1998), Lundborg's instruments rely on the assumption of no contextual effects; they are the average background characteristics of the peers. Estimates of the marginal effects of peer binge

drinking, smoking and illicit drug use are positive and statistically significant and are strongest for binge drinking. When school-grade fixed effects are included, the magnitude of the effects decreases for binging and smoking, but remains positive and significant. When instrumental variable techniques are used to further identify the potential bias from reverse causality (the individual's behavior affects the group), the estimated marginal effects of the peer rate of use almost double, indicating that estimates that do not account for this bias are subject to considerable downward bias.

Another strand of literature exploits the randomization provided by roommate assignment to test for peer effects. Sacerdote (2000) measures peer effects on grade point average (GPA) and the propensity to join fraternities and sororities. With random assignment, the selection of individuals into groups can be ignored, although a reflection problem still remains when examining the effect of roommate's GPA on own GPA. To account for this problem, Sacerdote also measures the effect of the roommate's rank in high school and SAT scores on own GPA, peer effects that do not suffer from the reflection problem. He finds small, but statistically significant effects for all of these measures on freshman GPA, but the effects disappear by senior year.

Another study utilizing similar methodology is Kremer and Levy (2003). They examine the impact of freshman roommate's prior alcohol use, GPA on the subject's GPA. They find that for males, roommate's high school alcohol use lowers GPA; an effect which continues strongly into the sophomore year. In contrast, females are not affected. Roommate's high school academic performance and socio-economic background did not affect GPA. They also present some preliminary evidence that individuals were more likely to drink if their roommates drank in

high school, although they note that these results are sensitive to the specification chosen (Kremer and Levy, 2003).

Both of the above studies are confined to a very select sample of undergraduates at single colleges, and it is unclear whether they are generalizable to all college students. Further, the randomization of housing is not entirely complete; students in the Kremer and Levy (2003) study who opted into specialized types of housing or for a specific roommate were excluded from the sample; to the extent that these students differ from the randomized sample, the measured peer effects may differ. However, they are arguably cleaner tests than some of the instrumental variable methods used in other studies.

One significant trade-off in the studies reviewed above compared to ours is in specificity versus generalizability. Our measures of peer use encompass very broad conceptions of peer group, at the campus wide level, but our sample is more nearly representative of the national population of college students. Other studies may more closely model the exact peer group of an individual, but are confined to a much more limited geographic area. From a policy perspective, however, administrators are unlikely to be able to identify close peer networks, but knowing the college rate of binge drinking is a more feasible metric.

Alcohol control policy impacts have also been examined in the context of peer behaviors. Weitzman et al. (2003) found that college students who reported that they were exposed to wet environments were more likely to take up binge drinking than were their peers without similar exposures. Wet environments included friendship networks and affiliations in which binge drinking is common and endorsed. In addition, Nelson et al. (2005) found that students attending colleges in states where adult binge drinking rates were higher were more likely to be binge drinkers than students at colleges in states with lower adult binge drinking rates.

III. Methods

The main purpose of this research is to provide evidence which will inform policymakers about the existence or absence of college-level peer effects on heavy drinking behaviors. Given the Manski critique, our analyses must implement appropriate econometric models to account for the potential endogeneity of our peer effect measures. Our empirical models and the nature of the CAS data allow us to control for many of the problems outlined by Manski (1993, 2000). The literature employing instrumental variables examine teens, not college students, who are still tied to parents' location. College students, however, are more likely to be removed from their home environment, and thrown into a new group of peers. This physical detachment and change in peer groups provides the researcher with a more defensible set of instruments.

Our empirical analysis examines the effect of peer binge drinking on the probability of an individual college student's participation in binge drinking participation. Our model incorporates peer binge drinking measures into the standard economic model of demand for these substances. These models include the effects of alcohol prices, alcohol control policies, and demographic variables on the probability of binge drinking among American college students. Within this analysis, we also control for state-level and campus-level factors such as campus-based restrictions on drinking. Without these controls, the estimated peer effects may be biased upwards due to correlated effects. Our empirical estimation follows closely the methodology used by Powell et al. (2005).

The model estimates the probability of individual student binge drinking, S_{ist} (a 0-1 dichotomous indicator for binge drinking participation), given by:

$$S_{ist} = \beta_0 + \beta_1 P_{ist} + \beta_2 X_{ist} + \beta_3 E_{ist} + \beta_4 R_{ist} + \beta_5 C_{ist} + \varepsilon_{ist} \qquad (1)$$

where P_{ist} defines our campus-based peer measure for individual *i* attending college *s* at time *t* as the proportion of individuals in college *s* excluding individual *i* who binge drink, X_{ist} is a vector of personal characteristics, E_{ist} a vector of the family characteristics for the individual, R_{st} is a vector of campus characteristics, and C_{ist} is a vector containing alcohol prices and alcohol control policies.

In the empirical estimation of equation 1, two potential sources of endogeneity and bias may arise if the estimation of this equation directly assumes that P_{ist} is exogenous. First, our analyses must account for the fact that an individual student can affect the behavior of his/her peers, while at the same time the student's peers affect his/her own behavior. In this instance, there are social effects, or in Manski's parlance, "the reflection problem". Here, an individual student's error term and that of his/her peer group may be correlated. Second, another potential source of endogeneity refers to Manski's correlation effect in which case individuals may behave in the same manner as their peer group based on the fact that they have similar unobserved characteristics. In the context of our specification, college students may endogenously sort themselves across college campuses. That is, we may face an upward bias in our peer effect estimates due to the possibility that students who are more likely to engage in heavy drinking behaviors may simply be selecting into the same schools. In order to control for the potential endogeneity between our peer measure and our dependent variable of college binge drinking, this study estimates a two-staged generalized least squares model. More specifically, we implement the Amemiya Generalized Least Squares (AGLS) estimator for our models with dichotomous dependent variables. In this model, the endogenous regressor (i.e. the peer binge drinking measure) is treated as a linear function of the instruments and other exogenous variables (Newey 1987). This two-stage estimation procedure requires the inclusion of identifying variables that

are an exogenous source of variation in our peer binge drinking measure but do not directly affect individual binge drinking behavior. We also include school-level fixed-effects in the models which use sub-college groupings for the peer measures. These subgroups are gender-, housing-, greek-status-, and athletic-participation-specific measures of peer use.

In the first stage, we estimate an ordinary least squares regression of our dependent variable P_{is}, the prevalence of binge drinking among the peer group of individual i , as a function of our exogenous regressors from our estimating equation (above) and four sets of identifying regressors. In order to account for potential endogeneity of the peer measure in the estimating equation, the actual peer measure is then replaced by a predicted peer measure based on the results of the first-stage regression. Next, our second stage regression, our estimating equation above, is estimated as a probit equation.

We employ several assumptions and test the validity of instruments. One strategy is to assume that there are no contextual effects, that is, the background characteristics of an individual student's peer group do not have a direct effect on his/her binge drinking behavior. The assumption of no contextual effects is consistent with other work in this literature on adolescents (Lundborg 2006, Gaviria & Raphael 2001, and Norton et al. 1998). The assumption may even be more valid in the context of college students. College students are far less likely than high school age or younger students to have current and prolonged contact with their peers' families or to have lived in the same neighborhood. Therefore, the background characteristics of the student body, E_{is}*, at a particular institution should affect the overall rate of heavy episodic drinking, but not directly affect the individual's propensity to engage in the same behaviors. A second strategy is to use a lagged measure of the college rate of binge drinking, $P_{s,1993}$, derived from the 1993 CAS data as an instrument. The lagged level of binging cannot have a direct

effect on the current drinking of the respondent, but individuals may have selected a college partially on the basis of the drinking environment present. Students from the 1997 survey who are seniors or above in their class level would have been exposed to the 1993 mean binge rate; estimates which drop these observations do not change the marginal estimated impact of peer drinking on current behavior. The first stage equation is therefore modeled in (2) as

$$\hat{P}_{ist} = \alpha_0 + \alpha_1 E^*_{ist} + \alpha_2 P_{s,1993} + \mu_{ist} \qquad (2)$$

The assumption of no contextual effects allows us to employ the average of the peer group's measure of characteristics E^*_{ist} as identifying instruments; the * represents the school average for those characteristics, excluding the respondent. This vector of instruments (E^*_{ist}) includes the average educational attainment of the students' parents, average parental use of alcohol and the average religious affiliations of students at the school. The fourth instrument, $P_{s,1993}$, is the 1993 college mean binge rate. Because of the presence of multiple instruments, the model is overidentified. We check the robustness of the results because of this overidentification and because some of the instruments are arguable by running several sets of estimations using different subsets of these four instruments.

The inclusion of school-level fixed effects is another possible strategy to help identify endogenous regressors in certain contexts. However, fixed effects by school are not possible for our general model because we have only one observation per respondent and our independent variable of interest is very close (but not identical) to the mean value of the dependent variable of interest. Fixed effects, either using the dummy variable strategy or population average essentially allows the intercept of the regression to vary by college; this intercept captures the mean value of drinking at the school. Once the mean value of binge drinking is controlled for in this manner, the variation in our peer estimate comes solely from excluding the individual

respondent from the calculation of the mean rate of binging at the school. Specifically, it implies that non-bingers have a higher peer use value and bingers have a lower peer use value, and by construction using fixed effects would force the peer effect to be negative. Fixed effects by school is possible, however, if the peer use measure varies within the school. See the data description below for more details on the sub-groupings. We use population-average fixed effects probits. For each group-specific peer measure, we estimate the model using the pooled data and for samples split by the group type (i.e. separately for men and women etc.) to see if different groups of students respond differently to the rate of peer use.

IV. Data

The data are the 1997 and 1999 waves of the College Alcohol Study (CAS) conducted by the Harvard School of Public Health. In 1993, the CAS survey was administered to a random sample of students attending 140 randomly selected four-year colleges and universities across the United States. For every CAS wave, administrators at each college were given specific instructions as to how to provide a random sample of undergraduates drawn from the total enrollment of full-time students. Depending on the enrollment size of each given campus, every *n*th student was drawn from the school's full-time student registry. In all survey years, questionnaires were mailed directly to students early in the spring semester to help ensure that student responses were based on a two-week period of on campus drinking experiences as opposed to drinking behavior during spring break parties. Over 200 students from each school were sent an anonymous survey to their registered school address. Of the original sample of 140 schools, 130 were retained in the 1997 wave and 128 in 1999. A total of 15,685 students responded in 1997, and 14,907 returned questionnaires in 1999. We retain data from the 1997

survey, even if the colleges are dropped from the survey universe in later waves. However, in creating our peer measures and some of the other measures, we need a sufficiently large campus sample for a more accurate picture of the college averages. Therefore, we drop respondents if there are fewer than 50 responses for the binge drinking, ease of access and other variables at the college. Respondents with missing values for any of the variables used in the regression analyses are also dropped, leaving a total sample size 24,478 from 125 colleges. Dropping the students from the smaller college samples is also responsible for the higher sample level of binging (45%) in our sample versus the entirety of the CAS data. In addition to interviewing students, the CAS surveys deans of students and other administrators at each of the participating schools. The school administrator surveys include a series of questions pertaining to campus policies toward student substance abuse and other substance use-related aspects of the campus environment. The following paragraphs describe the dependent variables of interest, peer measures and other control variables available in the CAS data. Because of survey changes, only the 1997 and 1999 data provide consistent measures of variables. We do not directly employ the 1993 data, but do use the 1993 sample to calculate the rate of binge drinking at the college, $P_{s,1993}$.

Dependent Alcohol Binge Variable: Our measure of individual binge drinking participation among college students is a gender-specific indicator of binge drinking participation. Binge drinkers include females who reported drinking four or more drinks on a single occasion two weeks prior to completing the survey or males who drank five or more drinks on a single occasion two weeks before taking the survey. A drink could include: a 12-oz (360 mL) can or bottle of beer, a 4-oz (120 mL) glass of wine, a 12 oz (360 mL) bottle or can of wine cooler, or a 1.25 oz. (37 mL) shot of hard liquor straight or in a mixed drink. The binge

drinking measure is constructed as a 0-1 dichotomous indicator of binge drinking participation that equals a value of one if a student reported any binge drinking in the past two weeks and zero otherwise. The frequent binge drinking measure is also a 0-1 dichotomous indicator of heavy binge drinking participation that is set to a value of one if a student reported binge drinking two or more times during the past two weeks and zero otherwise.

Peer Effect Measures: Our school-based peer measures for each student are constructed as the prevalence of binge drinking and frequent binge drinking at the individual student's campus in that sample academic year, excluding the given individual in the calculation. That is, for each student the prevalence of school-based peer binge drinking is the prevalence of binge drinking among the *other* respondents on their campus. This method is also used when creating the group-specific measures described below.

Defining the peer group as all students on campus might be too broad a definition. The peer whose behavior is most relevant to the respondent's may only be those with whom he/she has the most contact. Policy makers are not likely to have the resources to more finely define networks of peers within the campus; the campus-level measure is therefore relevant from a policy perspective. However, we do experiment with different definitions of peer group to form the rates of peer effect measures, which also allows us to use the variation to identify population average fixed effects probit models. Here, we divide the sample in several ways and recompute the peer effects for each subsample. The divisions include: males/females, Greeks/independents, students housed on campus/students housed off campus, and athletes/non-athletes. For example, in the split gender samples, we compute the rate of binge drinking for males (females) (excluding the respondent) and attach that variable to the male (female) respondents. We then run pooled specifications with the gender-specific peer measure. We also run estimates where we split the

sample by gender, and examine the peer effects separately for males and females. We repeat these analyses for the other divisions described above.

Control Variables: The CAS surveys collected a variety of demographic and socioeconomic data. Several potential determinants of college student drinking behaviors are constructed from these data. These include: the age and age squared of the respondent, his/her gender (Male - one for males, zero for females), year in school (Freshman - omitted, Sophomore, Junior, Senior, 5th year and beyond), race (African American, Asian, White - omitted, Native American, other), ethnicity (Hispanic), religion (None/Atheist - omitted, Catholic, Jewish, Moslem, Protestant, other), marital status (never married, married, divorced, separated, widowed), area of residence (single sex dormitory, co-ed dormitory, fraternity or sorority, other university housing, co-op or university affiliated group house, and off-campus house or apartment - omitted), sorority or fraternity membership, student income (allowance per week and wages earned per week), parental education (either mother or father attended college), regional location of college (Northeast, South, West, Midwest)[1] and the year of survey (1997 or 1999, 2001 omitted).

Price and Policy Measures

In addition to the data collected by the CAS surveys, a number of other state and/or local policy variables from a variety of external sources were merged with the CAS survey data according to state, county and city identifiers. These variables include prices, taxes, as well as various alcohol and tobacco control policies. We use the Consumer Price Index (CPI) to denote both our alcohol and cigarette prices in real 1990 dollars.

[1] We also ran state fixed effects estimates, which are not reported here but available upon request. The peer effects are generally reduced by about one-third, but remain positive and statistically significant. State policies with no time variation are dropped; the impact of other state policies are generally the same, with the exception of restrictions on pitcher sales, which becomes negative and statistically significant,

Campus Alcohol Control Policies: Drawing upon information from the CAS School Administrator surveys, this research also controls for select restrictions that a given campus has levied on alcohol use and whether or not the campus teaches alcohol use prevention. In particular**,** the administrator survey captures information on the availability and sale of alcohol on campus, as well as the presence of drinking restrictions and the degree of their enforcement. This research focuses on the effects of having 1) a college pub on campus as well as 2) the presence of an alcohol outlet within one mile of campus, and 3) whether the campus is dry. While the presence of alcohol outlets near campus has been shown to be correlated with higher levels of drinking, a number of colleges with high drinking levels have instituted a pub in the hope that this would lead to more responsible drinking)

Alcohol Price Measures: Our analysis takes advantage of two price measures available through the 1997 and 1999 waves of the CAS surveys to construct the average real college price paid per alcoholic drink and the proportion of students who pay a fixed fee for all they can drink. Students report the amount that they typically pay for a single alcoholic drink. Possible responses include: drink free, under $.50, between $.51 and $1.00, between $1.01 and $2.00, between $2.01 and $3.00, $3.01 or more and pay a set fee. Using this information, we construct the average college price as the campus mean of non-zero prices paid for a single alcoholic drink as reported by student drinkers from each campus. The proportion of students who pay a fixed fee for all they can drink is defined as the percentage of drinkers on campus who report typically paying a set fee to drink. These price measures are common to all students in the college in that year of the sample.

Because the monetary price of the drink is only part of the full cost of drinking, particularly for underage students, we also use sets of specifications which include other

measures that reflect the student perception of the availability of alcohol for underage students. These include the percent of students (not including the respondent) who answered that alcohol was easy or very easy to obtain, the percent of students who indicated there was at least one place where alcohol could be obtained without identification, and the percent of students who reported being carded at any campus or Greek event that year.

We also run sets of estimates that use peers' parent's use of alcohol as instruments, to proxy for family attitudes about drinking. The respondent's own parent's use is part of the demographic controls in the individual binging equations. These variables are mother's and father's alcohol use, defined separately for both mother and father and include: parent not present, parent abstains from alcohol - omitted, parent is an infrequent or light drinker, parent is a moderate drinker, parent is a problem/heavy drinker, and parent is a former problem drinker,. Variables that represent the percent of students who report being raised in different religious traditions are also included, to reflect another nonpecuniary variation in the costs of drinking for the student body.

Alcohol Control Policies: With each wave of the CAS, the Harvard School of Public Health also collected information describing the local and state alcohol control laws as they pertain to each college campus. In this analysis, state-level law indicators are set equal to a value of one if the state mandates restrictions on happy hours, for presence of restrictions of pitcher sales of beer, if blood alcohol of .08 is illegal per se, if there are zero tolerance laws for driving while intoxicated, if possession of fake identification is illegal, if it is illegal to sell to minors, if it is illegal for minors to attempt to buy alcohol, if it is illegal for minors to attempt to consume for minors, if open containers are prohibited, required keg registration, warning signs must be posted regarding illegality of sales to minors, if billboard ads are prohibited. Local control

policies included in the regressions are if the minimum age to sell/serve is less than 21, if open containers are prohibited, if responsible server training is required, if alcohol delivery to homes is prohibited, if warning signs must be posted regarding illegality of sales to minors, and if billboard ads are prohibited.

Table 1 contains the definitions, means (and where appropriate, standard deviations) for selected variables of interest. Forty-five percent of our college sample engages in binge drinking and 23 percent are frequent binge drinkers. Alcohol is quite inexpensive, with drinkers reporting a real average price of just $1.31 per drink (the median price is $1.27). While only 4 percent of students reported paying a set fee to drink unlimited amounts of alcohol, alcohol is widely available. Twenty-seven percent of students have access to a pub on campus, and 73% of students are at colleges with a bar within a mile of campus, whereas only 5% of students are on a dry campus. The mean percent of students at the college who report that alcohol is easy or very easy to obtain is 79%, 39% of underage student drinkers reported obtaining alcohol without identification, and only 7% of student drinkers have ever been carded on campus or at Greek events. Only 4% of students are in states with restrictions on pitcher sales, but 52% of students are in states with restrictions on happy hours.

V. Results and Discussion

In examining the importance of peer influences on heavy college drinking behavior we consider binge drinking and frequent binge drinking with the results presented in tables 2A and 2B, respectively. We present the peer effect probit model (Model 1) results in the first column, the peer effect from the preferred specification of the AGLS model (Model 2) results in column two, and probit model results that omit peer effects in column three (Model 3). The results are the marginal effects of the variables, rather than the raw coefficient estimates; for dummy

variables, the marginal effects are calculated as the change from zero to one, for other variables they are evaluated at the mean of the variable. Table 3 presents the results from the first stage regressions of our preferred specifications (using only the 1993 lagged college binge rate as an instrument) for binge drinking and frequent binge drinking. Table 4 presents the results using different definitions of peer networks and includes both instrumental variables (AGLS) and fixed effects results. The results show both pooled estimates which substitute the group-specific rate of binging/frequent binging for the college-year rate and for split sample results. Table 5 shows robustness checks for the three alternative instrument sets and for various restrictions on the sample. We also show results which include a control for the respondent's high school alcohol use and ones which omit the college price data. Both the measured peer effect and two specification tests are included in this table.

Peer Effects

Examining the point estimates of the marginal effects, in Tables 2A and 2B, in the majority of specifications, our peer measures in the drinking models shows that for binge drinking and frequent binge drinking, the reported marginal effects are somewhat larger in the AGLS models than in the naïve probit models (which treat the peer alcohol use as exogenous). For example, the exogenous probit model in table 2A predicts that for every 1 percentage point increase in the level of binge drinking at the school, the probability that an individual binges will increase by 0.744 percentage points, whereas in the AGLS model, the prediction increases to 0.959 percentage points. Similar changes occur in the frequent binging models, shown in table 2B, where the predicted marginal effect increases from 0.605 to 0.825 percentage points.

Overall Effect of Peer Behavior on Individual Propensity to Binge

The marginal effects reported in the results are assessed at the mean peer rate of binging, but, as the probit model is nonlinear in the dependent variables, it is interesting to look at the effect on the entire distribution of peer binging. Figures 1A and B simulates the impact of peer binging and peer frequent binging on the individual's predicted probability of binging as the rate of peer binging ranges from 0 to 100% of other students at the college. Estimates from both the single equation probit and the second stage AGLS-corrected estimates are shown. Even if 100% of one's peers are binge or frequent binge drinkers, the maximum predicted probability that the individual will also binge / frequently binge is less 85 and 90% respectively. One should note that the naïve estimates show a lower marginal impact at these extremely high rates of peer alcohol use. Additionally, at the very high simulated rates of peer binging, the marginal effects are out-of-sample estimates and should be viewed with caution. Nonetheless, the relationships shown here are consistent with simulations using agent-based modeling which predict that introducing even one drinker into a population of susceptible individuals could influence the entire group to drink under the right circumstances (Gorman et al. 2006).

The first stage regression results using only the 1993 college-specific level of binging as an instrument are shown in Table 3. Under the null hypothesis of the Smith-Blundell exogeneity test, the model is appropriately specified with all explanatory variables as exogenous. Under the alternative hypothesis, the suspected endogenous variable is expressed as a linear projection of a set of instruments. The residuals from the first-stage regression are added to the model and, under the null hypothesis, they should have no explanatory power (Smith and Blundell 1986). For both binging and frequent binging, the tests reject exogeneity of peer alcohol use, and the 1993 rate of binge drinking at the college appears to be a strong instrument. It is positively associated with the contemporaneous peer rate of binging and is strongly statistically significant.

Variations on Definition of Peer Group

The above results are all reported for the base measure of peer effects. We also conducted analyses under different definitions of peer networks by defining gender-, Greek-, housing-, and athlete/non-athlete- specific measures of the peer group. Because rates of use differ, the mean binge and frequent binge rate for each subgroup is shown in Table 4A. The regression results are shown in table 4B. The first set of results use the preferred AGLS specification (identified by only the 1993 college rate of binging) and are shown in the first column of results; the second set used fixed effects by school to control for endogenous sorting into schools and are shown in the second results column.[2] For each variation, we run estimates of the pooled data, followed by estimates which split the sample into each sub-group to test whether there are important differences in the way different groups respond to their peers. Under all definitions of peer networks, rates of frequent and episodic peer drinking have a positive and large impact on the probability of individual heavy episodic drinking. These predictions hold in the estimates which pool the data as well as those which split the data by broad peer networks.

The split sample results allow peer effects to differ by gender, Greek status, housing status and athletic status respectively. One should note, as shown in Table 4A, that rates of binging and frequent binging vary by group. Half of males engage in binge drinking compared to 41% of females; 65% of Greek students compared to 41% of independent students, and 54% of athletes compared to 43% of non-varsity athletes. The rates among students living on and off campus are more similar, however, 46% compared to 43%. The differences for the frequent binging behavior also vary. The gender differences get smaller; men are more likely to binge

[2] The sample size for these results is 23,509. As discussed above, to create valid measures of the average level of binge drinking among peers, we need a sufficient number of the relevant peer group at the college. We dropped

frequently, but only by 4 percentage points (25% compared to 21%). The difference between Greek and independent students and athletes/non-athletes remains large: an astounding 39% of Greeks frequently binge, compared to 20% rate for Independents and 29% for varsity athletes compared to 21% for non-athletes. The difference in frequent binging rates by housing status remains small (24% compared to 21%). The magnitude of peer drinking calculated at the subgroup mean is generally larger for males than for females, Greeks versus independents, on-campus students relative to off-campus students, and for athletes relative to non-athletes, although this trend is weaker for some of the group splits between the AGLS and the fixed effects results and may differ depending on whether the outcome measure is binging or frequent binging. These are all consistent with a priori expectations based on the relative proximity of students.

Finally, allowing variation in the definition of peer group within colleges allows fixed effects estimation, shown in the second column of results. When fixed effects are employed, the magnitude of the impact of peer use on individual propensity is reduced compared to the instrumental variable results, although the estimates are uniformly positive and statistically significant. Again, the relationship is stronger for binging than for frequent binging.

Price and Policy Effects

Examining the influence of prices on binge and frequent binge drinking based on the probit model without peer effects (column 3 in Tables 2A and 2B), we find that the real college price significantly reduces both excessive drinking behaviors, while access to a flat rate for all you can drink (zero marginal cost) significantly increases the likelihood of binge and frequent

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from the sample respondents at colleges where the number of male (or female, or athletes/non-athlete etc…) responses were less than 25.

binge drinking. However, once we account for peer effects the direct effect of prices becomes insignificant and switches sign for both drinking measures. And, again, a strong price effect is observed in the peer drinking equations as seen in Table 3, which suggests a strong indirect impact of prices on binge drinking through the peer effect, although this estimate is likely to be biased towards zero due to reverse causality. Because our dependent variable represents potentially a large part of the market, one cannot assume that the price measures are exogenous to the rates of peer drinking. A higher rate of binge drinking at the college represents a shift in demand, putting positive pressure on prices. Exogenous price movements, due to supply shifts, will tend to reduce the quantity demanded of alcohol and therefore the rates of binge drinking at the college. We estimate that a one dollar increase in the average price for alcoholic beverages will decrease the average college rate of binge and frequent binge drinking by about 8.3 and 5.6 percentage points, respectively.

Because the statistical and economic significance of these price decreases when the school level of binge and frequent binge drinking is added to the models (comparing the results in column 3 with those of the first two columns in table 2) and because it remains significant in the first-stage results (shown in table 3), the results indicate that the price effects operate indirectly through a social multiplier effect. Since the marginal effect estimates of the peer effects are statistically significant and in most cases close to 1, the indirect effects remain fairly potent, despite working through the filter of their impact on the overall level of heavy episodic drinking on campus.

An analogous analysis applies to the fraction of students who pay a fixed fee for alcohol. Again, the bias in the coefficient estimate of exogenous change is also towards zero. This variable is also positively associated with individual drinking in model 3, but not in the AGLS

models. However, again, in the first stage regression results reported in Table 3 the relationship between more students paying a fixed fee for alcohol and the level heavy episodic drinking and frequent heavy episodic drinking is positive, as expected, but is not statistically significant. Because of the potential bias in these estimates, the price effects will actually *under*estimate the magnitude of exogenous price changes. Therefore, the estimates of the policy effects of price changes are lower bounds on the true impacts of exogenous changes in price on rates of heavy episodic drinking.

Various non-pecuniary measures of the price of alcohol predict the rates of binge drinking and frequent binge drinking generally as expected. Ease of access to alcohol for underage drinkers, as measured by the percent of students who reported that alcohol was easy to obtain, is positively related to the probability of binging and frequent binging in table 2, although the statistical significance disappears in the AGLS model and becomes smaller in magnitude, while the fraction of drinkers obtaining alcohol without ID is statistically significant only when peer effects are omitted. Again, the indirect "price" effect of these variables is more important than the direct price effect. These ease of access variables perform better as determinants of the peer rates of drinking in the first stage of the model as shown in table 3. In particular, every 1 percentage point reduction the fraction of students at the college reporting that alcohol is easy or very easy to obtain by could reduce the level of binge drinking at the college by about a third of a percentage point. The percent of students who are ever carded on campus is positively related to the peer rates of drinking; this effect seems to be reflecting bias due to reverse causality, rather than a measured enforcement effect., This variable is negatively related to individual drinking for underage students in the binge models, although, perhaps for the same reason, the estimates are not statistically significant.

The only policy variable that is consistently and statistically significantly associated with reduction in the peer rate of binging or frequent binging is the legal control against attempted alcohol purchase by minors. The lack of a statistically significant measured impact of other policies on use is likely to be due to endogeneity bias itself. States and localities with higher perceived problems with binge drinking may be more likely to implement various policy measures in response, producing a positive bias in the coefficient estimates which may counteract any negative causal impact the policies have on use. This possible endogeneity may also bias estimates of the impact of other characteristics on individual binging; however, excluding these variables does not alter the positive, statistically significant association of peer rates of binge drinking on individual propensity to consume.

Sensitivity Analysis

Instrumental variables analyses were also conducted using various combinations of the average peer family characteristics as described in the methods section. These analyses failed the tests of exogeneity and did not meet the F statistic threshold for strong instruments, therefore, the preceeding discussion was limited to the AGLS results which use only the previous college binge rate to identify the model. However, as shown in the first sets of results in table 5, other sets of instruments produce similar peer effects estimates; that is, all of the estimates are positive, statistically significant, and generally, of the same magnitude.

The main analysis did not control for whether the respondent reported binge drinking in high school. Previous use may proxy for other unmeasured personal characteristics which may influence choice of peer group and current drinking. Under the rational addiction model, however, previous use may be endogenous to current use, and would introduce bias into the

results. As a further robustness check, we also show the peer effect when high school use is included, for both the IV and naïve models. Including high school use reduces the marginal predicted effect of peer binging and frequently binging on the individual propensity, but the estimates remain positive and statistically significant.

The main results relied almost exclusively on estimates that used the 1993 college binge rate to identify the endogenous peer effect. This instrument is valid under the assumption that the 1993 rate does not directly influence current drinking; for a seniors and above in the 1997 wave, this assumption may be violated since these students are likely to have been freshmen or sophomores at the college in 1993. To test the validity of the lagged college rate, we drop seniors; the effects are again very close to the estimates discussed previously.

The assumption of no contextual effects could be weaker for commuter campuses, where students are more likely to be living at home and directly connected to their peer's families. To test for the sensitivity of our results, we analyze the whole sample, and for subsets of the sample which include only non-commuter campuses and only students housed on-campus. Variable subsets of the peer family and religion characteristics in the first stage regressions yield F-statistics of less than 10, a heuristic cut-off for "weak" instruments (see Stock & Staiger 1997 for a discussion of the possible biases introduced from weak identifiers), but estimates which employ the 1993 binge rate perform well on statistical tests of validity. All estimates produce similar qualitative results for the estimated impact of peer drinking on respondent drinking in sign, statistical significance, and rough magnitude of results.

Finally, the price variable used is based on the average price reported at the college. However, the price reported is drawn from a biased sample- drinkers. The last robustness check

drops this variable from the analysis; the peer effect remains positive and statistically significant, and of the same magnitude as the base set of results.

VI. Conclusions

This paper has offered new evidence on the determinants of college drinking behaviors by providing estimates of the impact of peer influences. The drinking models jointly examine the importance of alcohol prices, alcohol state-level control policies, and peer influences in each of the respective substance use decisions. The key findings are that peer effects play a significant role in both college binge drinking decisions. We find that a 1 percentage point increase in the level of peer use increases the probability of binge drinking by between 0.74 and 0.96 percentage points and frequent binge drinking by between 0.61 and 0.83 percentage points. The empirical results also showed that alcohol prices and policies can significantly impact college drinking behaviors indirectly via the peer effect. The strength of these effects may partially explain the strong persistence in the rate of binge drinking at each college over time. However, few of the specific policies as examined here were consistently found to reduce heavy episodic drinking on college campuses based on either the first or second stage results. It is likely that the policy effects are operating largely through the price of alcohol reported by students at the colleges. The measures of alcohol restrictions used here are very crude and the results suggest that simply looking at several very specific laws or ordinances may miss other means of sale of large amounts of alcohol at low prices. These results reveal a strong potential for social multiplier effects with respect to exogenous changes in the price of alcohol. Future work will examine the importance of perceptions of student drinking relative to the actual rates of drinking to produce further insights into the possible efficacy of social norming campaigns.

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Table 1: Selected Variable Means and Standard Deviations

Variable Definition	Mean	Std Dev.
1 if binge drink, 0 otherwise	0.45	0.14
1 if frequently binge drink, 0 otherwise	0.23	0.10
"Price" Variables (all exclude respondent)		
Average real price paid by drinkers at college	1.31	0.22
Fraction of drinkers paying fixed fee at college	0.04	0.05
Fraction of drinkers at college reporting alcohol easy to obtain	0.79	0.06
Fraction of drinkers at college obtaining alcohol without identification	0.39	0.13
Fraction of drinkers at college who report never being carded	0.07	0.03
Alcohol Control Policies & Availability Measures		
Alcohol outlet present within mile of campus	0.73	0.44
College pub on campus	0.27	0.44
Dry campus	0.05	0.23
State Level Policies		
State restricts/prohibits happy hours	0.52	0.50
State restricts pitcher sales	0.04	0.19
State 08 Blood alcohol of .08 is illegal per se	0.33	0.47
State law has zero tolerance for driving while intoxicated	0.91	0.28
Illegal to posses fake id in state	0.85	0.35
Illegal to sell to minors in state	0.98	0.14
Illegal in state to attempt to buy alcohol	0.82	0.38
Illegal in state to attempt to consume for minors	0.73	0.44
Open container restrictions in state	0.56	0.50
State requires keg registration	0.28	0.45
Warning signs posted regarding illegality of sales to minors	0.43	0.50
Billboard ads are prohibited in state	0.29	0.45
Local Policies		
Minimum age to sell/serve less than 21	0.73	0.44
Open container restrictions	0.96	0.20
Responsible server training required	0.29	0.46
Alcohol delivery to homes prohibited	0.20	0.40
Warning signs posted regarding illegality of sales to minors	0.24	0.43
Billboard ads are prohibited in locality	0.24	0.43
Selected Personal Characteristics		
Male	0.39	0.49
Age of student	20.82	2.09
Greek	0.14	0.35
Real wage income ($ per week)	34.13	34.39
Real income from allowance ($ per week)	19.11	27.97

Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys.

Table 2A: Base Model Specification, Binge Drinking Models

Variable	Exogenous Probit			AGLS			Probit, No Peer Effects		
	Marg. Effects	Std. Error		Marg. Effects	Std. Error		Marg. Effects	Std. Error	
Peer Binge	0.744	0.050	*	0.959	0.081	*			
Real Price	0.009	0.025		0.037	0.026		-0.088	0.024	*
% of drinkers pay fixed fee	0.017	0.085		-0.065	0.088		0.310	0.082	*
% drinkers: alcohol easy to obtain	0.226	0.097	*	0.029	0.114		0.876	0.086	*
% drinkers obtain w/o ID	0.005	0.045		-0.040	0.047		0.158	0.043	*
% of drinkers at college ever carded	-0.060	0.142		-0.117	0.143		0.150	0.140	
College Pub	0.003	0.008		0.005	0.009		-0.003	0.008	
Presence of bar w/in mile of campus	0.023	0.031		0.014	0.032		0.050	0.031	
Dry campus	0.012	0.018		0.008	0.018		0.023	0.018	
State Policy Variables									
Restrict Happy hours	-0.008	0.010		-0.003	0.010		-0.025	0.010	*
Restrict Pitcher sales	-0.018	0.019		-0.023	0.019		0.006	0.019	
.08 Blood alcohol illegal	-0.018	0.010	†	-0.013	0.010		-0.033	0.010	*
Zero tolerance, DWI	0.027	0.014	†	0.029	0.014	‡	0.023	0.014	†
Illegal to posses fake ID	0.012	0.011		0.010	0.011		0.021	0.011	†
Illegal to sell to minors	0.072	0.029	*	0.063	0.030	‡	0.106	0.028	*
Illegal to attempt to buy	-0.017	0.011		-0.005	0.011		-0.058	0.011	*
Illegal to attempt to consume	0.009	0.010		0.009	0.010		0.007	0.010	
Open Container restrictions	0.005	0.011		0.010	0.011		-0.012	0.010	
Required keg registration	0.019	0.012		0.009	0.012		0.051	0.011	*
Warning signs posted, sales to minors illegal	0.005	0.010		0.007	0.010		-0.004	0.010	
Billboard ads prohibited	0.003	0.009		-0.004	0.009		0.027	0.009	
Local Policy Variables									
Minimum age to sell/serve less than 21	-0.008	0.010		-0.008	0.010		-0.009	0.010	
Open Container restrictions	-0.019	0.020		-0.022	0.020		-0.006	0.020	
Responsible server training required	0.002	0.010		-0.003	0.010		0.018	0.010	†
Alcohol delivery prohibited	0.016	0.013		0.012	0.013		0.027	0.013	‡
Warning signs posted, sales to minors illegal	0.000	0.009		0.004	0.009		-0.010	0.009	
Billboard ads prohibited	0.005	0.013		0.009	0.013		-0.012	0.013	
Log likelihood	-15123.41			-15418.00			-154236.58		

Source: Author's calculations using 1997& 1999 Harvard College Alcohol Surveys, N = 25,478

*, ‡, † statistically significant at the 1%, 5%, and 10% level. Regressions also include survey year, region, gender, age, age squared, race, class year, religion, marital status, housing controls, Greek status, real wage income, own parental alcohol use, other income, size of college, and controls for public/private, traditionally all female and traditionally African American college. NOTE TO REFEREEs: A sample of the full specification results for the AGLS models are included in the technical appendix.

Table 2B: Base Model Specification, Frequent Binge Drinking Models

Variable	Exogenous Probit			AGLS			Probit, No Peer Effects		
	Marg. Effects	Std. Error		Marg. Effects	Std. Error		Marg. Effects	Std. Error	
Peer Binge	0.605	0.043	*	0.825	0.079	*			
Real Price	-0.018	0.019		0.001	0.020		-0.071	0.019	*
% of drinkers pay fixed fee	0.009	0.062		-0.053	0.065		0.205	0.060	*
% drinkers: alcohol easy to obtain	0.161	0.073	‡	0.040	0.081		0.513	0.068	*
% drinkers obtain w/o ID	0.016	0.034		-0.013	0.035		0.093	0.034	*
% of drinkers at college ever carded	-0.006	0.109		-0.048	0.110		0.142	0.108	
College Pub	0.006	0.006		0.006	0.006		0.003	0.006	
Presence of bar w/in mile of campus	0.013	0.023		0.003	0.024		0.039	0.021	†
Dry campus	-0.004	0.013		-0.006	0.013		0.007	0.014	
State Policy Variables									
Restrict Happy hours	0.001	0.007		0.005	0.007		-0.011	0.007	
Restrict Pitcher sales	-0.023	0.014		-0.026	0.014	†	-0.013	0.014	
08 Blood alcohol illegal	-0.002	0.008		0.002	0.008		-0.011	0.008	
Zero tolerance, DWI	0.016	0.010		0.014	0.010		0.021	0.010	‡
Illegal to posses fake id	-0.002	0.009		-0.006	0.009		0.011	0.009	
Illegal to sell to minors	0.053	0.020	‡	0.045	0.021	‡	0.086	0.016	*
Illegal to attempt to buy	-0.010	0.008		0.001	0.009		-0.045	0.008	*
Illegal to attempt to consume	-0.001	0.008		-0.001	0.008		-0.001	0.008	
Open Container restrictions	-0.002	0.008		0.003	0.008		-0.014	0.008	†
Required keg registration	0.006	0.009		0.000	0.009		0.025	0.009	*
Warning signs posted, sales to minors illegal	0.000	0.007		0.004	0.008		-0.007	0.007	
Billboard ads prohibited	0.004	0.007		-0.004	0.007		0.029	0.007	*
Local Policy Variables									
Minimum age to sell/serve less than 21	-0.007	0.008		-0.007	0.008		-0.007	0.008	
Open Container restrictions	0.000	0.016		-0.003	0.016		0.013	0.015	
Responsible server training required	0.004	0.007		-0.001	0.007		0.013	0.007	†
Alcohol delivery prohibited	0.004	0.010		0.002	0.010		0.011	0.010	
Warning signs posted, sales to minors illegal	-0.005	0.007		-0.002	0.007		-0.016	0.006	‡
Billboard ads prohibited	0.013	0.010		0.015	0.010		0.005	0.010	
Log likelihood	-11697.73			-11736.32			-11795.92		

Source: Author's calculations using 1997& 1999 Harvard College Alcohol Surveys, N = 25,478

*, ‡, † statistically significant at the 1%, 5%, and 10% level. Regressions also include survey year, region, gender, age, age squared, race, class year, religion, marital status, housing controls, Greek status, real wage income, respondent's self-reported parental alcohol use, other income, size of college, and controls for public/private, traditionally all female and traditionally African American college. NOTE TO REFEREEs: A sample of the full specification results for the AGLS models are included in the technical appendix.

Table 3A: First Stage Regression Results, Determinates of Rates of Binging, Frequent Binging

	Binging			Frequent Binging		
	Marginal Effect	**Standard Error**		**Marginal Effect**	**Standard Error**	
1993 College Binge Rate	0.575	0.056	*	0.454	0.057	*
Real Price	-0.083	0.031	*	-0.056	0.028	‡
% of drinkers pay fixed fee	0.138	0.150		0.106	0.142	
% drinkers: alcohol easy to obtain	0.413	0.107	*	0.156	0.103	
% drinkers obtain w/o ID	0.126	0.058	*	0.065	0.051	
% of drinkers at college ever carded	0.275	0.182		0.217	0.149	
College Pub	-0.012	0.010		-0.006	0.010	
Presence of bar w/in mile of campus	0.056	0.034		0.056	0.029	†
Dry campus	0.016	0.026		0.012	0.021	
State Policy Variables						
Restrict Happy hours	-0.013	0.013		-0.009	0.011	
Restrict Pitcher sales	-0.003	0.022		-0.011	0.015	
08 Blood alcohol illegal	-0.012	0.013		-0.007	0.011	
Zero tolerance, DWI	-0.008	0.017		0.007	0.013	
Illegal to posses fake id	0.003	0.014		0.013	0.011	
Illegal to sell to minors	0.032	0.031		0.044	0.026	†
Illegal to attempt to buy	-0.036	0.015	*	-0.037	0.013	*
Illegal to attempt to consume	-0.012	0.013		-0.009	0.012	
Open Container restrictions	-0.008	0.012		-0.009	0.010	
Required keg registration	0.021	0.017		0.009	0.016	
Warning signs posted, sales to minors illegal	-0.013	0.011		-0.014	0.009	
Billboard ads prohibited	0.017	0.011		0.025	0.011	‡
Local Policy Variables						
Minimum age to sell/serve less than 21	-0.004	0.012		-0.003	0.010	
Open Container restrictions	0.037	0.015	*	0.037	0.015	‡
Responsible server training required	0.021	0.013		0.017	0.011	
Alcohol delivery prohibited	0.002	0.016		-0.002	0.014	
Warning signs posted, sales to minors illegal	-0.011	0.012		-0.016	0.010	†
Billboard ads prohibited	-0.015	0.019		-0.005	0.015	
Smith-Blundell test of exogeneity	11.11	Pvalue= 0.00086	*	11.39	P value = .0074	*
F-test on instrument	1030.95	Pr>F=0.00	*	630.42	Pr>F-0.00	*
R-squared	.8593			0.8112		

Source: Authors' calculations from 1997-1999 Harvard College Alcohol Surveys. N=25,478

*, ‡, † Statistically significant at the 1%, 5%, and 10% levels. Standard errors corrected for clustering at the college level.

Table 4A: Split Sample Means

	Binge Rate	Frequent Binge Rate
Males	0.50	0.25
Females	0.41	0.21
Greeks	0.65	0.39
Independents	0.41	0.20
On Campus	0.46	0.24
Off Campus	0.43	0.21
Varsity Athletes	0.54	0.43
Non-Athletes	0.29	0.21

Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys,

Table 4B: Robustness Checks: Marginal Impacts for Within College Network Peer Effects

	AGLS Model			Fixed Effects by School		
	Marginal Effect	**Standard Error**		**Marginal Effect**	**Standard Error**	
	Gender-specific peer measure					
Binge	0.987	0.0847	*	0.697	0.0418	*
Frequent Binging	0.848	0.0777	*	0.563	0.0391	*
Split sample: Males (N=9,182)						
Binge	1.121	0.162	*	0.849	0.0513	*
Frequent Binging	0.966	0.141	*	0.696	0.0553	*
Split Sample: Females (N=14,327)						
Binge	0.915	0.0978	*	0.772	0.0356	*
Frequent Binging	0.790	0.0917	*	0.625	0.033	*
	Greek-status Specific Peer Measure					
Binge	0.994	0.084	*	0.763	0.0366	*
Frequent Binging	0.852	0.079	*	0.588	0.036	*
Split sample: Non-Greeks (N=20,777)						
Binge	0.996	0.0939	*	0.799	0.0301	*
Frequent Binging	0.815	0.0843	*	0.663	0.0246	*
Split sample: Greeks (N=2,731)						
Binge	1.030	0.314	*	0.693	0.0590	*
Frequent Binging	1.071	0.276	*	0.544	0.0804	*
	Housing-status (On/Off campus) Specific Peer Measure					
Binge	1.010	0.0868	*	0.545	0.0390	*
Frequent Binging	0.840	0.0779	*	0.330	0.0355	*
Split Sample: Off Campus (N=12,254)						
Binge	1.089	0.156	*	0.659	.0436	*
Frequent Binging	0.811	0.140	*	0.532	0.0329	*
Split Sample: On Campus (N=11,255)						
Binge	0.969	0.107	*	0.833	0.0484	*
Frequent Binging	0.877	0.0987	*	0.761	0.0354	*
	Varsity Athletic Participation Specific Peer Measure					
Binge	1.001	0.0857	*	0.599	0.0338	*
Frequent Binging	0.858	0.787	*	0.303	0.0344	*
Split Sample: Non-Athletes (N=20,806)						
Binge	0.975	0.761	*	0.746	0.0407	*
Frequent Binging	0.811	0.0688	*	0.604	0.0388	*
Split Sample: Athletes (N=2,905)						
Binge	0.899	0.278	*	0.762	0.0563	*
Frequent Binging	0.966	0.322	*	0.695	0.0740	*

Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys, AGLS model specification.
Time invariant, school specific covariates omitted from these specifications.
*, †, ‡ Statistically significant at the 1%, 5%, and 10% level.

Table 5: Robustness Checks: Specification Changes

Model	Binging: Marginal Effect (Standard Error)	Binging: Specification Tests[a]	Frequent Binging: Marginal Effect (Standard Error)	Frequent Binging: Specification Tests[a]
Base set of AGLS results (tables 2A and 2B)	0.959* (0.081)		0.825* (0.079)	
Variations in Instrument Sets				
Instruments = peer religious affiliation	0.905* (0.133)	Test 1: 1.69 P-value= 0.19 Test 2: 5.23	0.683* (0.132)	Test 1: 0.388, P-value= 0.53 Test 2: 3.08
Instruments = mean peer parental use indicators	0.934* (0.092)	Test 1: 6.66 P-value= 0.010* Test 2: 8.34	0.798* (0.116)	Test 1: 3.55 P-value= 0.06† Test 2: 4.64
Instruments = mean peer parental use, religious affiliation, & 1993 College rate	0.964* (0.068)	Test 1: 24.17, P-value= 0.000* Test 2: 9.37	0.784* (0.068)	Test 1: 12.23, P-value= .005* Test 2: 6.76
Additional Control Variable				
Include respondent high school binge indicator, treating peer rate as exogenous	0.595* (0.052)	NA	0.447* (0.042)	NA
Include respondent high school binge indicator, [b]	0.782* (0.085)	Test 1: 7.68, P-value=0.006* Test 2: 104.2	0.615* (0.077)	Test 1: 6.82, P-value= .009* Test 2: 63.36
Variations in Sample Restrictions				
Dropping commuter colleges[b]	0.900* (0.091)	Test 1: 5.97, P-value= 0.015* Test 2: 11635.7	0.824* (0.087)	Test 1: 6.95, P-value= 0.008* Test 2: 10115.4
Dropping students housed off campus[b]	1.030* (0.111)	Test 1: 3.29, P-value= 0.07† Test 2: 93.25	0.912* (0.109)	Test 1: 5.74, P-value= 0.017‡ Test 2: 50.69
Dropping 1997 senior students[b]	0.946* (0.088)	Test 1: 5.75, P-value= 0.002* Test 2: 105.2	0.853* (0.0859)	Test 1: 9.24, P-value= 0.006* Test 2: 63.62
Omitting in Control Variable				
Omitting college price variable	0.939* (0.0769)	Test 1: 11.12, P-value= 0.001* Test 2: 101.7	0.822* (0.0749)	Test 1: 11.55, P-value= 0.001* Test 2: 66.65

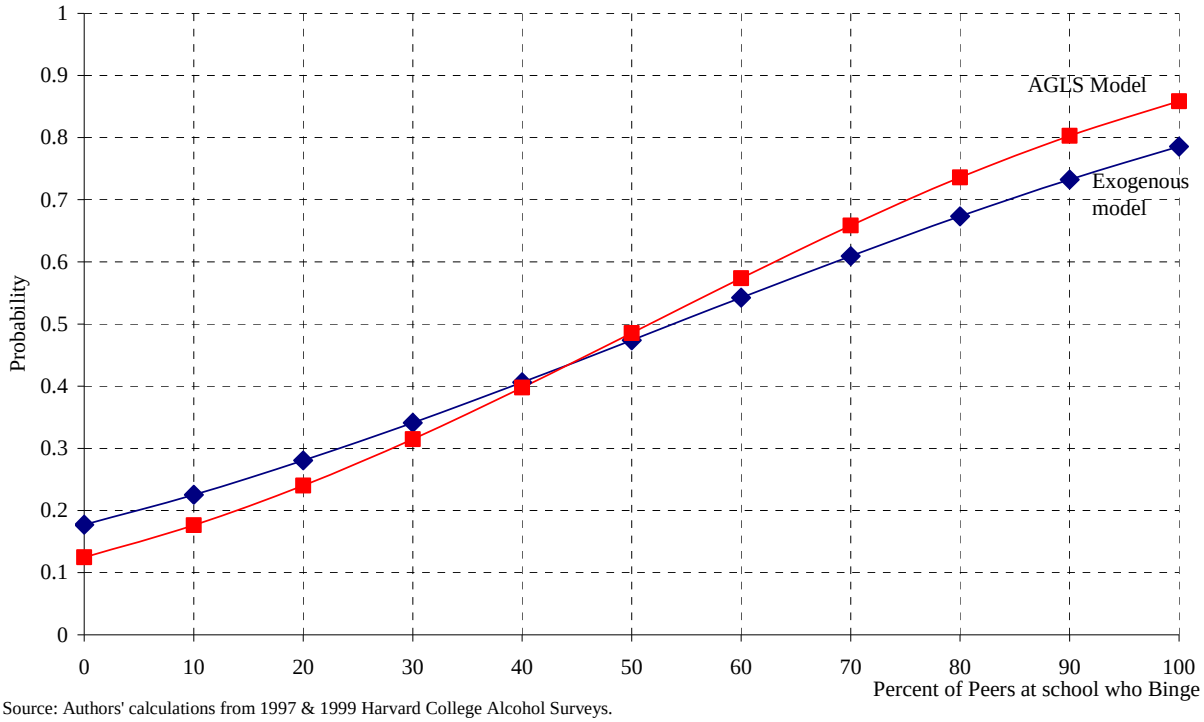
Source: Author's calculations using 1997 & 1999 Harvard College Alcohol Surveys

*, ‡, † statistically significant at the 1%, 5%, and 10% level. Regressions also include survey year, region, gender, age, age squared, race, class year, religion, marital status, housing controls, Greek status, real wage income, own parental alcohol use, other income, size of college, and controls for public/private, traditionally all female and traditionally African American college.

a. Test 1= Smith-Blundell exogeneity test statistic. Test 2= F-test of instrument set in first stage regression.

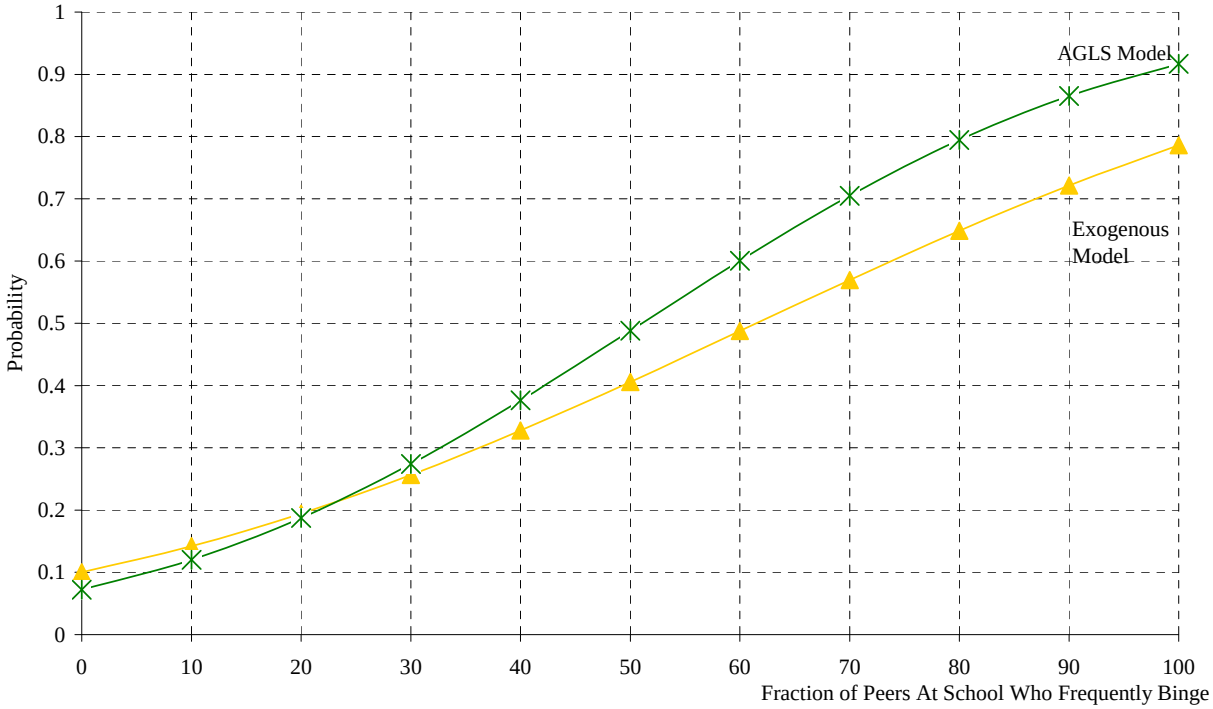
b. Uses only 1993 college level of binging as instrument.

Figure 1A: Predicted Probability Individual Binges as Peer Behavior Changes



Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys.

Figure 1B: Predicted Probability, Individual Frequently Binges As Peer Behavior Changes



Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys.

Technical Appendix Table 1: Full Specification Results, Binge drinking AGLS model (Marginal effects for other control variables not shown in Table2A)

Variable	Marginal Effect	Standard Error	
Male	0.035	0.005	*
Age	0.172	0.034	*
Age Squared	-0.004	0.001	*
African American	-0.115	0.009	*
Asian	-0.101	0.008	*
Native American	0.013	0.034	
Other Race	-0.053	0.011	*
Hispanic	0.010	0.014	
Sophomore	-0.027	0.008	*
Junior	-0.038	0.010	*
Senior	-0.046	0.012	*
More than 4th Year Senior	-0.028	0.014	‡
Catholic religious background	0.044	0.008	*
Jewish religious background	-0.019	0.014	
Moslem religious background	-0.077	0.027	†
Protestant religious background	-0.017	0.008	†
Other religious background	0.009	0.011	
Married	-0.141	0.007	*
Divorced	-0.080	0.017	*
Separated	-0.055	0.035	
Widowed	0.012	0.097	
Same sex dorm	-0.048	0.008	*
Coeducational dorm	-0.024	0.007	*
Other university housing	-0.008	0.015	
Live in fraternity/sorority house	0.051	0.017	*
Other social housing	-0.062	0.011	*
In Greek System	0.133	0.009	*
Real wage income	0.000	0.000	
Real other income	0.001	0.000	*
Parents have college education	0.028	0.007	*
Traditionally all female college	0.042	0.021	†
Traditionally African American college	0.019	0.042	
Commuter college	0.010	0.012	
Small private college	0.033	0.012	*
Large private college	-0.008	0.009	
Small public college	0.016	0.009	‡
South	0.011	0.010	
West	-0.007	0.014	
Midwest	-0.005	0.010	
1997 sample	-0.015	0.006	†
Father former drinker	0.059	0.020	*
Father infrequent drinker	0.021	0.008	*

Variable	Marginal Effect	Standard Error	
Father moderate drinker	0.066	0.010	*
Father heavy/problem drinker	0.044	0.011	*
Mother former drinker	-0.024	0.026	
Mother infrequent drinker	0.024	0.006	*
Mother moderate drinker	0.067	0.011	*
Mother heavy/problem drinker	0.051	0.020	*

Source: Authors' calculations from 1997 & 1999 Harvard College Alcohol Surveys.
*, †, ‡ Statistically significant at the 1%, 5%, and 10% level.

Technical Appendix Table 2: Full Specification Results, Frequent Binge drinking AGLS model (Other control variables not shown in Table2B)

Variable	Marginal Effect	Standard Error	
Male	0.089	0.007	*
Age	0.331	0.043	*
Age Squared	-0.008	0.001	*
African American	-0.236	0.013	*
Asian	-0.173	0.012	*
Native American	0.033	0.042	
Other Race	-0.074	0.015	*
Hispanic	0.030	0.017	‡
Sophomore	-0.044	0.012	*
Junior	-0.034	0.015	†
Senior	-0.051	0.016	*
More than 4th Year Senior	-0.043	0.019	†
Catholic religious background	0.053	0.011	*
Jewish religious background	-0.013	0.020	
Moslem religious background	-0.137	0.037	*
Protestant religious background	-0.039	0.011	*
Other religious background	0.003	0.013	
Married	-0.244	0.012	*
Divorced	-0.110	0.025	*
Separated	-0.063	0.050	
Widowed	-0.039	0.120	
Same sex dorm	-0.058	0.012	*
Coeducational dorm	-0.001	0.010	
Other university housing	-0.020	0.020	
Live in fraternity/sorority house	0.130	0.025	*
Other social housing	-0.092	0.016	*
In Greek System	0.192	0.011	*
Real wage income	0.000	0.000	*
Real other income	0.001	0.000	*
Parents have college education	0.036	0.009	*
Traditionally all female college	0.059	0.023	†
Traditionally African American college	0.187	0.040	*
Commuter college	0.030	0.014	†
Small private college	0.039	0.015	*
Large private college	-0.019	0.012	
Small public college	0.032	0.012	*
South	0.023	0.014	‡
West	-0.019	0.020	
Midwest	0.000	0.014	
1997 sample	-0.014	0.008	‡
Father former drinker	0.097	0.023	*
Father infrequent drinker	0.044	0.010	*

Variable	Marginal Effect	Standard Error	
Father moderate drinker	0.091	0.011	*
Father heavy/problem drinker	0.061	0.013	*
Mother former drinker	-0.023	0.036	
Mother infrequent drinker	0.041	0.008	*
Mother moderate drinker	0.115	0.013	*
Mother heavy/problem drinker	0.074	0.023	*

Source: Authors' calculations from 1997, 1999, and 2001 Harvard College Alcohol Surveys
Uses same specification as AGLS model5.
*, †, ‡ Statistically significant at the 1%, 5%, and 10% level.

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