NBER WORKING PAPER SERIES

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Working Paper 13262
http://www.nber.org/papers/w13262

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2007

I thank participants in several informal conversations at the 2007 AEA meetings and economics department seminars at Tulane University and the Universities of Nebraska and Texas at Arlington for helpful comments. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. I1,I2

ABSTRACT

This paper estimates the impact of fraternity and sorority membership on a wide array of drinking outcomes among respondents to four Harvard College Alcohol Study surveys from 1993-2001. Identification is achieved by including proxies for specific types of unobserved heterogeneity expected to influence the relationship. These include high school and parental drinking behaviors to account for time-invariant omitted factors, and assessed importance of drinking-related activities and reasons for drinking to control for changes in preferences since starting college. Self-selection is quantitatively important. But even controlling for variables plausibly affected by fraternity membership, such as current alcohol use categorization (from abstainer to heavy drinker) and time spent socializing, fraternity membership has a large impact on drinking intensity, frequency and recency, as well as various negative drinking consequences that potentially carry negative externalities.

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1. Introduction

Fraternities and sororities are an integral part of life for many college students. In the Harvard College Alcohol Study (CAS), which sampled nationally representative groups of full-time undergraduates at four-year colleges on four occasions during 1993–2001, 16 percent of students reported belonging to a fraternity or sorority. Only one of the 125 colleges surveyed had no fraternities.[1] Within the 476 school/year cells (representing 119 schools each year), fraternity membership was 12.4 percent at the median and over 20 percent in a quarter of cases.

The activity most associated with fraternities is alcohol consumption. Fraternities are responsible for a substantial portion of campus events at which alcohol is available to students. College itself often conveys an image of fraternity parties with abundant intoxicated students, perhaps engaging in behaviors such as sex and vandalism. CAS data suggest this stereotype is not baseless. In the 30 pre-survey days, alcohol intoxication was reported by 64 percent of fraternity members but only 42 percent of non-members. Moreover, the current school year incidence of unprotected sex, property damage and driving after drinking, respectively, that respondents attributed to drinking was 13, 14 and 40 percent among students belonging to fraternities, compared with eight, eight and 26 percent among others.

From a policy standpoint, the fundamental question regarding these statistics is whether they reflect causal effects of fraternity membership. A public health argument could be made for intervening in fraternity activities that directly lead to excessive drinking. This is particularly true if alcohol use has peer effects (Kremer and Levy, 2003; Lundborg, 2006), or there is reason to believe that students make alcohol consumption decisions that are systematically shortsighted. Effects of fraternities on behaviors such as vandalism and unsafe sex would warrant regulation even under the stricter criterion that harmful externalities must be present.

[1] Henceforth, the term "fraternities" will apply to both genders, i.e. also encompasses sororities.

The obvious reason for caution in asserting that the above numbers represent a causal behavioral influence of fraternities is self-selection. Undoubtedly, students choose to join fraternities in part because of pre-existing preferences towards behaviors that membership facilitates. It would be erroneous, therefore, to assume that these unconditional prevalence differences accurately portray direct effects of fraternities. Rather, this study investigates whether fraternities are responsible for *any* of these differences. Put differently, in a counterfactual world without fraternities, would rates of drinking and related behaviors differ just as much between students who, with fraternities present, do and do not belong to them?

Presumably, the factors that confound the observed relationship between drinking and fraternity membership are those related to preferences that simultaneously influence joining fraternities and consuming alcohol. The strategy used to identify causal effects consequently takes advantage of the inclusion in the aforementioned CAS data of a large set of proxies for these preferences that can be directly entered into the regression equations. This approach parallels that of DeSimone (2007), the only related study in the economics literature. But relative to those data, CAS provides a deeper and more nuanced set of relevant covariates.

These covariates proxy for two types of omitted factors, fixed characteristics determined prior to college enrollment and time varying preferences that have since changed. Time-constant heterogeneity is absorbed by college fixed effects, which account for school selection based on prevailing drinking environments, measures of drinking behavior during senior year of high school, and parental drinking habits and attitudes. Preference shocks occurring in the meantime are captured by assessments of the importance of parties and various reasons for consuming alcohol. The most inclusive models further control for time allocated to socializing and, most restrictively, whether students classify themselves as non-, infrequent, light, moderate or heavy

drinkers. These fully saturated models arguably provide conservative estimates by controlling for behaviors plausibly affected by fraternity membership.

The richer data available from CAS allow this study to go beyond DeSimone (2007) in two additional respects. First, the sample is considerably larger, includes more schools and contains four cross sections over an eight year period rather than just one. This allows for precise estimation of smaller effect sizes and more generalizability across time and schools. Second, it examines many measures of alcohol use beyond binge drinking (i.e. having five or more drinks in a row), the only response analyzed in the earlier study, along with a wide array of drinking-related outcomes. Additional heterogeneity proxies specific to school performance and sexual behavior outcomes are included in the more extensive specifications.

The results show that self-selection into fraternities accounts for a considerable portion of the observed correlation between membership and drinking. Even the most conservative estimates, however, reveal statistically and quantitatively significant effects of fraternity involvement on drinking intensity, frequency and recency, associated outcomes like falling behind in school, and behaviors with negative external effects such as unsafe sex and vandalism.

2. Previous Studies

Most of the literature on drinking effects of fraternities is from outside of economics. Borsari and Carey (1999), for instance, offer three ways in which joining a fraternity could raise alcohol consumption. First, fraternity members might feel social pressure to drink in order to feel accepted into the group. Second, extrapolating peer drinking rates from those observed among fellow members would yield systematic overestimation, which already occurs among college students. Third, fraternity houses make alcohol easily available to members, regardless

of age, and are shielded from others who might disapprove of heavy drinking.

Supporting the statistics from the introduction, prior studies of the CAS (Chaloupka and Wechsler, 1996) and the Core Alcohol and Drug Survey (Alva, 1998; Cashin et al., 1998) found that fraternity members drink more frequently and heavily than do others. But none attempted to control for self-selection of drinkers into fraternities. For example, Sacerdote (2001) reported that high school drinkers among the Dartmouth senior classes of 1997 and 1998 were more likely than their classmates to have joined fraternities. Baer et al. (1995), Schall et al. (1992) and Wechsler et al. (1996) obtained similar findings, and also revealed that conditional on drinking in high school, students subsequently entering fraternities were more apt to have previously binge drank. One study endeavoring to account for self-selection is Lo and Globetti (1995), who found that students who had never before binge drank were three times more prone to start doing so if they joined a fraternity. Another is Sher et al. (2001), who estimated that fraternity members drank more heavily than non-members during college, conditional on previous alcohol use. Moreover, by three years after college, drinking rates no longer differed by membership status.

The only related analysis in the economics literature is DeSimone (2007), who attempts to control for time-varying heterogeneity rather than simply pre-college drinking behavior. The key selection proxy was the number of drinking occasions over the same prior month period encompassed by binge drinking, the dependent variable. If drinking frequency is impacted by fraternity membership, this model might yield estimates that are biased towards zero. Indeed, inserting this variable considerably reduced effect sizes. However, fraternity coefficients remained large and significant, and controls for further omitted factors had little impact. The smallest estimates imply that fraternity membership raises the prevalence and frequency of binge drinking by 15–20 percent.

A weakness of that study is its lack of data on specific consequences of binge drinking that are potentially harmful. For instance, earlier research has found that college student binging is associated with lower academic performance (Kremer and Levy, 2003), injury (Hingson et al., 2003b), vandalism (Wechsler et al., 2002), drunken driving (Hingson et al., 2003b), and risky sexual activity (Hingson et al., 2003a). CAS data allow for examining the impact of fraternity membership on these outcomes, as specified by respondents to be alcohol-related.

3. Data and Empirical Strategy

To estimate drinking-related effects of fraternity membership, I estimate survey-weighted OLS regressions and adjust standard errors for arbitrary heteroskedasticity. A typical equation is

$$(1) \qquad d_i = \beta_0 + \beta_1 f + \boldsymbol{x_i}\boldsymbol{\beta_2} + \boldsymbol{y_i}\boldsymbol{\beta_3} + \boldsymbol{p_i}\boldsymbol{\beta_4} + \boldsymbol{a_i}\boldsymbol{\beta_5} + \boldsymbol{b_i}\boldsymbol{\beta_6} + u_i \, .$$

In this notation, i indexes the student, d is a drinking measure, f is an indicator of fraternity membership, the β's are parameters to be estimated, u is the error term, and remaining terms represent vectors (in bold) of explanatory variables. The β subscripts match the column numbers in tables 1–5; column 1 models include just f, column 2 models also control for $\boldsymbol{x}$, etc. The five groupings of covariates are based on the degree to which they proxy for omitted confounding factors, as explained below. Sequentially adding sets of variables to the model thus allows for exploring the impact that different types of selection have on the estimated fraternity effect.

These regressions are estimated using data from the CAS, which was administered to a nationally representative set of full-time students at four-year colleges in the springs of 1993, 1997, 1999 and 2001. Wechsler et al. (1994) provides details regarding sample selection and survey administration. Of the 195 colleges originally chosen, 140 agreed to participate in 1993, 128 of which again participated in both 1997 and 1999. The data available from these years

include only the 119 schools for which, in all three years, the student response rate was above a cutoff value of slightly under 60 percent. Data from 2001 are available for 119 of the 120 colleges sampled, 113 of which were included in the 1993–1999 data and six of which were new.

Each school chose a random sample, increasing in size with enrollment, by starting at a random point in the student registry and choosing every *r*th student. In 1993, questionnaires were mailed out in early February; 87 percent of completed surveys were returned by the end of March, with another 10 percent in April and the remainder by June. The overall student response rate from the 140 participating schools was 69 percent. Logistics were similar for the subsequent three administrations. This process yielded 54,740 total respondents. Excluding 6,804 who were not 18–24 years old (for whom exact ages are not reported and fraternity membership is substantially less prevalent), 181 for whom fraternity affiliation is not reported and 7,725 who lack complete explanatory variable information, an analysis sample of 40,030 students remains. Regression sample sizes are slightly smaller because of missing responses for dependent variables, specifics of which are discussed in the subsequent section as the results are presented.

The main explanatory variable is *f*, the fraternity membership indicator. When sample weights are (not) used, 15.6 (15.3) percent of respondents belong to fraternities. By school, the unweighted prevalence varies from zero (in one case) to nearly 53 percent, with a standard deviation of 10.1 percent. Student exposure to fraternities thus appears to vary considerably.

The first of the five equation 1 covariate sets, $\boldsymbol{x}$, consists of plausibly exogenous variables. These include indicators for gender, each year of age, race/ethnicity, religion in which the respondent was raised, parental education and survey year. Race choices are white, black, Asian, Native American and other, with Hispanic origin as a separate non-exclusive category. Religions listed are none, Catholicism, Judaism, Islam, Protestantism and other. Parental

education captures whether college was attended by only the mother, only the father, both or neither. Table 7 models further allow for the fraternity effect to vary by some of these factors.

The next covariate set, ***y***, contains variables that reflect respondent choices but are unlikely to be related to relevant unobservables. This list includes indicators for year in school, marital status, transfer status, current year residence type, health status, the number of close student friends, and school. The residual year in school category is "5th year or beyond." Marital status possibilities are never married, married, and others, which combines the divorced, separated and widowed but encompasses only 0.4 percent of respondents. Transfers are divided into those having done so before or during the current academic year. Residence types are dorm, other university housing, fraternity house and other; controlling for fraternity house residence, which holds for only 21 percent of members, eliminates this mechanism as an explanation for membership effects. Health is rated as excellent, very good, good, fair or poor. Number of friends choices are each of zero to four along with "five or more." School fixed effects control for college selection based on prevailing fraternity and drinking environments.

Remaining covariates are included specifically to address possible selection into fraternities that is correlated with drinking and some of its outcomes. The vector ***p*** accounts for factors that are fixed across time, in that its elements reflect variables determined prior to college enrollment: drinking during high school senior year along with parental drinking and family attitudes towards alcohol use while the respondent was growing up. All are again indicators constructed from categorical information. High school drinking captures the number of occasions of any drinking in a typical month and binge drinking throughout the year, and the average number of drinks per occasion. Categories are zero, 1–2, 3–5, 6–9, 10–19, 20–39 and 40 or more for occasions, and each of zero to eight and "nine or more" for drinks. Respondents

classify each parent as abstaining, formerly drinking problematically but most recently abstaining, or drinking infrequently/lightly, moderately, heavily, or problematically, with a residual category of not knowing. Family alcohol use attitudes are characterized as not approving of any, accepting light but not heavy, accepting heavy, or having disagreement.

Of course, many students drink more heavily in college than in high school, while others might cut back on alcohol use in college in order to survive academically. The vector ***a*** thus in part consists of indicators that reflect current attitudes towards drinking. In particular, students are asked about the importance of participating in parties and of drinking to get drunk, have a good time with friends, reward hard work, and feel more comfortable with the opposite sex. Choices are very important, important, somewhat important or not at all important. The estimated fraternity effect in these models is hence conditioned on a set of drinking preference measures and a key mechanism, parties, through which fraternities might influence alcohol use.

Nonetheless, these preferences are sufficiently specific to prospectively not encompass all relevant omitted factors. More generally, the main drawback to the proxy variable approach is that it is impossible to know whether all pertinent unobservables have been identified. One last vector, ***b***, is therefore added as a stronger control for current drinking preferences. Most notably, ***b*** contains a set of indicators for whether respondents consider themselves abstainers, infrequent drinkers, light drinkers, moderate drinkers or heavy/problem drinkers. In addition, ***b*** includes an estimate of the mean daily hours spent socializing with friends in the past 30 days.[2]

The key attribute separating the latter two sets of proxies is that those in ***b***, to a much greater degree than ***a***, are quite possibly outcomes of fraternity membership. Specifying alcohol

[2] Like the subsequently introduced study time estimate, this is constructed from a categorical variable with choices of 0, 1, 2, 3, 4, and at least 5 hours. The latter is top coded at 6.4 under the assumption of normality and rounding to the nearest tenth. In 1993, the top category is subdivided into choices of 5, 6, 7, and 8 or more hours, with the latter similarly topcoded at 9 hours.

use extent and socializing time as determinants, instead of responses, of being in a fraternity imposes a strong constraint on the variation in drinking that fraternities can possibly affect. For instance, this strategy treats student drinking category as predetermined, even though fraternities could easily influence movements within categories. The argument is similar for socializing time, changes in which might affect drinking. The idea is to "over-control" for the variation in fraternity outcomes as a way to offset unavoidable uncertainty regarding specific unobservables that prevail. Optimistically, this generates an arguably causal fraternity effect. Less ambitiously, the resulting estimate identifies a distinctive selection process that must operate to generate the remaining observed correlation between fraternities and drinking if no causal pathway exists.

A weakness of the strategy outlined thus far is that the proxies are related solely to drinking behavior, while some dependent variables reflect behaviors for which drinking is an acknowledged factor but not a defined component. Two such behaviors relate to school performance and sexual activity. Both ***a*** and ***b*** therefore include determinants of these. The ***a*** vector contains indicators for the importance of participating in academic work (specified as above) and whether respondents are very satisfied, somewhat satisfied, somewhat dissatisfied or very dissatisfied with the education they have received. One last element is an indicator for whether respondents have had sex in the past 30 days. Meanwhile, the ***b*** vector controls for mean daily hours spent studying outside of class in the past 30 days, as well as current school year GPA converted to a four point scale from responses of A, A–, B+, B, B–, C+ C, C– or D. Also included are indicators for having two or more sex partners in the past 30 days, and for whether the respondent or sex partner never, rarely, sometimes or always uses a condom during sex. As before, the distinction between variables in ***a*** and ***b*** is that the former are less likely to be directly influenced by fraternity membership, meaning the latter are much stronger controls.

4. Results

Tables 1–5 display the main results. Each cell represents a different regression of the dependent variable in the row heading. The "N/Mean" column gives the sample size and mean for each dependent variable. As the last five rows document, successive columns control for additional explanatory factors in concordance with the equation 1 notation. Columns 5 and 6 might be taken as rough bounds on the fraternity effect. In particular, the column 6 estimate is prospectively conservative because its set of regressors includes potential outcomes of fraternity membership that are closely related to the dependent variables. Moreover, measurement error that either is random or stems from systematic underreporting of drinking-related outcomes by fraternity members will bias the fraternity coefficient towards zero. Table 6 shows estimates for various sample and methodological permutations using the specification in column 6 of tables 1–5, while table 7 does the same for models in which interaction terms have been added.

a. Binge Drinking

Table 1 shows results for past two week binge drinking. Two variables are examined, an indicator of any binging and log binge occasions, with the latter sample including only bingers. Each is constructed in three different ways, corresponding to the three panels, depending on whether a threshold of five or four drinks is used. The third panel mimics the current NIAAA standard of five drinks for men and four for women (e.g. www.hsph.harvard.edu/cas), who typically weigh less and thus have a lower alcohol tolerance. The original information on having five or more drinks consecutively is converted to numerical form using midpoints (and a maximum of 14, equating occasions to days) from categories of 0, 1, 2, 3–5, 6–9, and 10 or more. The four drink version is formed from analogous information on having exactly four drinks, with the two numerical measures summed and then constrained to be no greater than 14.

The discussion focuses on the top panel, which yields the smallest coefficients; estimates in the lower panels are quite similar. The 43 percent of respondents who report binge drinking average just over 1.5 episodes per week. Column 1 shows that, unconditionally, fraternity membership is associated with a nearly 60 percent higher probability of binge drinking and a 15 percent increase in binge occasions. These estimates are very highly statistically significant.

Three central observations stem from columns 2–6. First, a substantial component of the column 1 effect is attributable to selection. Coefficients in both equations fall steadily as additional controls are inserted. Ultimately, the estimated fraternity effect decreases by over 75 percent for any binging and nearly half for occasions. The former is plausibly more highly correlated with student characteristics than the latter, which is already selected on binging.

Second, the sets of proxies added in columns 4–6 specifically to control for unobserved heterogeneity have large impacts. The lone exception is the column 4 pre-college drinking and attitudinal variables in the binge occasions equation. This result contradicts the expectation that the fraternity coefficient in part reflects fraternity participation by students who previously drank heavily and would continue to do so regardless of their fraternity involvement. In contrast, these factors reduce the effect on any binging by almost 30 percent. A selection mechanism similar to that just outlined, but involving whether or not to binge drink, is thus quite relevant.

Adding time-varying attitudinal factors in column 5 lowers the fraternity coefficient by over 40 percent in the any binging equation and 25 percent in the occasions equation. These coefficients imply that fraternity membership increases binge drinking by 19 and 10 percent on the extensive and intensive margins, respectively. Although it was earlier argued that these estimates might constitute upper bounds on the fraternity effect, they are prospectively more accurate than those in column 6 if fraternities also influence the drinking importance ratings

added in column 5. Moreover, the semi-elasticity of 19 percent is virtually identical to that from DeSimone (2007), which controlled for strong heterogeneity proxies such as past month drinking days, years since first drink, and three measures of situational alcohol use.

Nonetheless, the column 6 estimates are important as a stricter test of causality. The third and most important table 1 observation, therefore, is that these are statistically significant and quantitatively meaningful. They imply that fraternity membership raises binge likelihood by 14 percent and occasions by eight percent. This is despite conditioning on not only time allocated to socializing, but also five alcohol use categories that, by definition, almost certainly respond to fraternity membership if it indeed affects binge drinking. This strongly suggests that joining a fraternity increases binging. The caveat remains that it is impossible to know whether the proxies address all applicable omitted factors. An added point, though, is that the coefficient decline from columns 5 to 6, particularly for any binging (30 percent), is smaller proportionately than that from columns 4 to 5. Given the blunt nature of the column 6 covariates, this provides further confidence that the column 5 covariates indeed capture relevant unobservables.

Remaining tables show a similar pattern. Fraternity coefficients are initially quite large, fall considerably when personal characteristics and time-constant selection controls are introduced in columns 2–4, but tend to remain significant when time-varying preference proxies are added in column 5 and even in the highly constrained specifications of column 6.

b. Drinking Intensity and Frequency

Table 2 details results pertaining to four past month intensity and frequency measures: getting drunk, occasions of intoxication and any drinking, and average drinks consumed on drinking occasions. The latter three are in log form and are analyzed only for those reporting the activity. The occasions measures are constructed, using midpoints, from categories of 1–2, 3–5,

6–9, 10–19, 20–39 and 40 or more. The two highest categories are collapsed to 30, imposing the condition that occasions cannot exceed days; only 3.3 percent of drinkers and 0.9 percent of students reporting intoxication are affected. Average drinks is reported as integers from one to nine (or more), with the top category coded as nine and including 5.5 percent of drinkers.

In the column 6 models, fraternity membership raises the probability of intoxication by 7.8 percent, intoxication occasions by 7.0 percent, average drinks by 6.0 percent and drinking occasions by 6.4 percent. All four effects are all very highly significant. The intoxication results confirm that the table 1 estimates reflect an impact on drunkenness. Those for average drinks do the same for the intensive drinking margin, as opposed to simply the extensive margin at the four or five drink thresholds. These implications underscore the relevance of DeSimone (2007), which studies only binge drinking. Moreover, the responsiveness of drinking occasions to fraternity membership suggests that the estimates in DeSimone (2007) might be conservative, as they stem from models that control for drinking frequency.

It is not obvious why the effect on any intoxication is little more than half of that on any binging. The mean is slightly higher for intoxication, so underreporting is not an explanation. The longer time period encompassed by the intoxication measure could be a reason, as could inaccuracy, when defining binge drinking, in using a fixed threshold number of drinks to proxy intoxication. Yet, intensive margin effects for intoxication and binging are similar in size.

c. Drinking Recency

Table 3 outlines effects on whether any alcohol was consumed over the past week, month, year and lifetime. These estimates serve as another indirect test of causation, taking the time frame in which drinking is measured to reflect actual drinking periodicity. Even if selection of drinkers into fraternities is paramount, membership might well induce drinking on a weekly

basis that might otherwise have not occurred as often. However, monthly drinking would be more common even without fraternities and hence should be less affected. Impacts on yearly or lifetime drinking, which would be widespread regardless of fraternities, should be small.

This is exactly the observed pattern. Coefficients shrink as the period over which drinking is measured lengthens and, in column 6, are significant only for weekly and monthly consumption. The estimated semi-elasticity declines from 6.9 percent for the past week to 3.2 percent for the past month, 0.5 percent for the past year and 0.3 percent for ever drinking. Even in terms of the proportion of non-drinkers, which falls as the period widens, the weekly and monthly semi-elasticities of 6.7 and 7.2 percent, respectively, are three times as large as those of 2.2 and 2.3 percent for annual and lifetime drinking.

These specifications also further test the utility of the selection controls. Assuming fraternity membership truly has little bearing on lifetime drinking, the high significance of the corresponding column 5 estimate is unreasonable. Yet, the column 5 coefficients are quantitatively small for both past year and lifetime drinking, implying semi-elasticities of 2.1 and 1.4 percent, compared with 12.2 percent for weekly and 6.2 percent for monthly alcohol use.

d. Direct Effects of Drinking

Table 4 explores effects of fraternities on five dependent variables that are direct outcomes of drinking: indicators for driving after drinking or having at least five drinks, and whether drinking has caused hangovers, memory loss, or the need for medical treatment because of an overdose. Drinking and driving is measured over the past month. The driving after binge drinking coefficient becomes insignificant in column 5, and disappears almost entirely in column 6. The large column 1–3 estimates thus appear to be manifestations of selection into fraternities by students prone to binge drink and drive. In contrast, the semi-elasticity for driving after any

drinking, which is initially smaller than that for driving after binging, remains highly significant in column 5 and marginally significant in column 6, with respective values of 9.4 and 4.2 percent. Recent adoption of 0.08 blood alcohol content DUI laws imply that drinking can impair some drivers even when the number of drinks consumed is insufficient to be classified as binge drinking. Though still merely suggestive, this consequently constitutes the strongest evidence yet that fraternity membership has negative external drinking-related effects.

Remaining table 4 outcomes are three distinct manifestations of heavy drinking over the current school year. Compared with tables 1 and 2, one might expect smaller effects because of the longer time frame, but larger effects because each condition requires a high level of intoxication, increasingly so moving down the table. In all three cases, coefficients maintain high significance in column 6. Indeed, progressing from hangovers to memory loss to medical treatment, semi-elasticities increase from 9.6 to 22.5 to roughly 67 percent (.0029/.0043) in column 6, while selection appears decreasingly relevant. Notably, the overdose coefficient actually increases slightly, from .0027 to .0029, from columns 1 to 6. As in tables 1 and 3, selection becomes less important as the type of drinking analyzed becomes more extreme. Assuming that students who overdose do not incur full medical treatment costs, such treatment reflects a tangible external cost of fraternity membership. As do some of the table 5 estimates, this further validates the usefulness of the results from DeSimone (2007).

e. Additional Outcomes of Drinking

Table 5 studies indicators for whether nine additional drinking outcomes took place during the current school year. Although not potentially unavoidable consequences of drinking at a particular intensity (or choosing to drive after doing so), these outcomes are attributed to drinking by respondents in the same manner as the latter three dependent variables in table 4.

The top two rows are outcomes related to academic achievement. Despite controlling for the importance of schoolwork and educational satisfaction (each added in column 5) along with hours spent studying and GPA, both column 6 coefficients are highly significant. Semi-elasticities of 29.2 percent for missing a class and 10.2 percent for falling behind suggest the potential for increased drinking from fraternity involvement to reduce school performance.

The next two outcomes are risky sexual behaviors that could impose costs on sex partners and others through pregnancy and infection with sexually transmitted diseases such as HIV. Unplanned sex is presumably more likely than planned sex to be later regretted by one or both partners, and less likely to involve condom use. Even controlling for whether the respondent had sex at all and with multiple partners in the last 30 days, the column 6 estimate is highly significant with a semi-elasticity of 13.5 percent. Column 6 further implies that fraternity membership raises the probability of unprotected sex by 9.1 percent, but the associated *t* statistic is only 1.5. However, restricting the sample to unmarried students (and lowering the sample size by 1,272) raises the *t* statistic to 1.7 while leaving the coefficient unchanged (and slightly increases the unplanned sex coefficient). Furthermore, the column 5 estimate, which is still conditioned on any past 30 day sex, is highly significant with a semi-elasticity of 14.8 percent.

The last five rows show results for additional outcomes that, to varying degrees, also carry harmful external effects. In column 6, fraternity member alcohol use is unrelated with police trouble, but raises the likelihood of arguing with friends by 8.7 percent, vandalism by 9.7 percent, injury by 13.9 percent and doing something later regretted by 12.3 percent.

One emergent theme is that, in contrast to tables 1–4, fraternity membership has larger impacts on more common behaviors. Effects have the least significance and smallest semi-elasticities for the most extreme outcomes (police trouble, vandalism and unprotected sex), but

are very highly significant and large for the most prevalent outcomes (missing class and regret).

f. Alternative Methods and Samples

For all 28 dependent variables, table 6 shows results from fully-specified models, analogous to column 6 of tables 1–5 (which column 1 restates), for two alternative methods and two alternative samples. Column 2 uses unweighted OLS. Column 3 estimates binary models with probit regressions and remaining models with interval regressions, which maximize likelihood functions that account for interval dependent variables similarly to Tobit regressions. In both cases, marginal effects averaged across respondents are displayed. Column 4 omits observations in which any of the binary dependent variables are missing, and thus are uniform (for binary models) but smaller than previously. Column 5 includes respondents of all ages.

Estimates vary little across columns, providing confidence that the results are not driven by sample weights, the binary and interval nature of the dependent variables, missing data patterns or omitting students of non-traditional ages. Conclusions are only affected in two cases. First, the unprotected sex effect is insignificant only in the original specification; it is significant at 10 percent using the new samples, and at 5 percent using the new methods. As when married respondents were omitted, the coefficient does not change much, but standard errors are smaller. Second, the property damage effect falls by two-thirds and becomes insignificant in the probit regression. However, it maintains significance when sample weights are omitted, and increases in size while becoming significant at 5 percent with the two alternative samples.

g. Interaction Effects

Finally, table 7 presents specifications that include interactions terms. In columns 1–3, an extra term representing the interaction between the fraternity indicator and a demographic characteristic is added to the fully saturated model. Only the interaction term results are shown.

Fraternity effects rarely differ by gender (column 1), race/ethnicity (column 2) or age (column 3). Sororities are more likely than (male) fraternities to spur past month drinking and less likely to prompt unplanned sex or property damage, but otherwise have similar effects. Relative to white non-Hispanic fraternity members, Hispanics and those of other races binge more often because they consume more drinks per occasion, and are more prone to past month drinking, but are less likely to miss a class. Underage fraternity drinkers have fewer beverages per occasion but are more apt to miss a class and use alcohol in the past month, year and lifetime, and also drink more often. The drinking frequency results possibly explain a major difference between these estimates and those from DeSimone (2007), which finds that holding frequency constant, fraternity effects on binge drinking stem entirely from underage students. The results are consistent if fraternities increase underage binging purely by raising drinking frequency.

Columns 4 and 5, which contain coefficients from the same regressions, examine social interaction effects by adding two variables, the fraction of school/year students that are fraternity members (column 4) and its interaction with membership (column 5), to the original column 6 models. The fraternity proportion variable is based on the sample of 54,410 that prevailed before imposing inclusion restrictions. Results must be interpreted cautiously because the sample proportion of students in fraternities is a noisy measure of the true value, and the source of its within-school variation is unclear. Nonetheless, variation across schools is substantial (as in the analysis sample, the standard deviation is 10 percent, with a minimum and maximum of zero and 53 percent), and within-school temporal variation is sufficient to identify relatively small effects.

In column 4, an increased fraternity presence is associated with higher probabilities of binging, hangovers, memory loss, and falling behind in school, along with more drinks per occasion. These are among the less socially harmful effects that would be expected if greater

fraternity membership leads to more parties, including a positive impact on any intoxication that is similar in size to that on binging but significant only at 20 percent. This suggests that a more alcohol-conducive atmosphere both induces some students to drink excessively who might not have otherwise, and pushes students who already binge (and miss classes) to drink more intensely and experience the concordant consequences, including falling behind academically.

Moreover, the column 5 interaction term is significant at 10 percent in six out of 24 cases (counting the binge drinking measures only once), and is always positive in the 12 regressions in which the *t* statistic is above unity. A one standard deviation increase in the proportion of students in fraternities raises the membership effect on binge drinking incidence by 2.5 percent, intoxication occasions by 2.4 percent, drinking occasions by 2.0 percent, past week drinking by 3.2 percent, unplanned sex by 6.6 percent and vandalism by 8.3 percent. It thus appears that greater fraternity penetration potentially both increases heavy drinking among non-members and exacerbates the effect of membership on intense drinking and its outcomes.

5. Conclusion

This study has presented evidence suggesting a causal effect of fraternity membership on heavy drinking and its consequences. Fraternity membership coefficients are significant and large in regressions that condition on a wide array of proxies for omitted sources of spurious correlation arising from students who join fraternities also being more likely to use alcohol. These include sets of time-constant measures (school fixed effects along with previous and family drinking), time-varying measures (attitudes towards partying and various reasons for drinking), and potential outcomes of fraternity membership (drinking category and time spent socializing). Fraternities affect drinking intensity, frequency and recency, as well as additional

outcomes of drinking that are potentially harmful to the drinker and, often, other individuals.

Previous research has documented several potential benefits of fraternities, including identification with a group (Hunt and Rentz, 1994), greater connection to current and future campus life (De Los Reyes and Rich, 2003) manifested in higher alumni giving rates (Harrison et al., 1995), and lifelong friendships (Sacerdote, 2001) that facilitate career networking (Marmaros and Sacerdote, 2002). This limits the desirability of reducing fraternity presence on campus. However, the observed effects on drinking behaviors that convey harmful external impacts might warrant policies attempting to limit heavy drinking by fraternity members. Efforts showing the potential to be effective include educational interventions for first-year members (Larimer et al., 2001), mandated educational sessions after alcohol-related medical treatment or disciplinary infractions (Barnett et al., 2006), and social norms marketing campaigns (DeJong et al., 2006).

One particularly valuable short-term benefit that fraternities might bring is the retention of members who might otherwise drop out. Anecdotally, in schools requiring a minimum GPA to join a fraternity, students who fail to attain that GPA are disproportionately likely to drop out, even when in good academic standing. Another possible source of retention is camaraderie with brothers and sisters that represents an opportunity cost of dropout. Without current means to address this question, it is left for future research.

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Table 1: Binge Drinking (past two weeks)

	N/Mean	(1)	(2)	(3)	(4)	(5)	(6)
Drinks = 5							
Any	39,895	.252	.231	.203	.145	.084	.059
	.432	(33.7)	(31.9)	(26.9)	(21.0)	(13.0)	(10.1)
Log(times)	16,992	.153	.150	.139	.133	.100	.078
	.852	(10.3)	(10.2)	(9.1)	(9.4)	(7.6)	(6.5)
Drinks = 4							
Any	39,829	.256	.234	.207	.148	.084	.058
	.486	(35.4)	(33.4)	(28.1)	(21.7)	(13.2)	(10.2)
Log(times)	19,180	.181	.182	.170	.159	.120	.094
	1.129	(11.8)	(12.0)	(10.9)	(11.0)	(8.7)	(7.5)
Drinks = 5/4 (men/women)							
Any	39,861	.257	.236	.207	.148	.085	.059
	.468	(34.8)	(32.6)	(27.6)	(21.4)	(13.2)	(10.2)
Log(times)	18,596	.159	.164	.153	.146	.109	.085
	.974	(10.8)	(11.1)	(10.0)	(10.3)	(8.1)	(7.0)
Models include:							
Exogenous characteristics		No	Yes	Yes	Yes	Yes	Yes
Endogenous characteristics		No	No	Yes	Yes	Yes	Yes
High school & family drinking		No	No	No	Yes	Yes	Yes
Drinking/academic ratings, sex		No	No	No	No	Yes	Yes
Other preference proxies		No	No	No	No	No	Yes

Drinks are the threshold number consumed in a row to constitute binge drinking. Regressions are estimated by OLS using sample weights. Heteroskedasticity-robust absolute *t* statistics are in parentheses. Exogenous characteristics include indicators for gender, age, race/ethnicity, parental education and survey year. Endogenous characteristics include indicators for class standing, marital status, transfer status, living arrangements, health status, number of friends, and school. High school drinking represents senior year and includes indicators for frequency of any and binge drinking and average number of drinks, while family drinking includes indicators for parental drinking and family drinking approval. Ratings include indicators for satisfaction with education and the importance of parties, academic work, and various reasons for drinking, while sex indicates intercourse in the past 30 days. Other preference proxies include past year GPA and hours spent studying and socializing in the past 30 days, along with indicators for current alcohol use category, condom use frequency and sex with multiple partners in the past 30 days.

Table 2: Drinking Intensity and Frequency (past 30 days)

	N/Mean	(1)	(2)	(3)	(4)	(5)	(6)
Was intoxicated	39,667	.223	.199	.174	.122	.057	.035
	.450	(28.9)	(26.6)	(22.4)	(16.7)	(8.8)	(5.7)
Log(times intoxicated)	17,863	.173	.166	.146	.132	.103	.070
	1.049	(10.5)	(10.2)	(8.7)	(8.4)	(7.2)	(5.2)
Log(average drinks)	27,949	.178	.174	.153	.125	.081	.060
	1.218	(17.6)	(18)	(15)	(13.4)	(9.3)	(7.4)
Log(times drank)	26,445	.271	.242	.217	.181	.109	.064
	1.497	(17.3)	(15.9)	(13.9)	(12.8)	(8.4)	(5.5)
Models include:							
Exogenous characteristics		No	Yes	Yes	Yes	Yes	Yes
Endogenous characteristics		No	No	Yes	Yes	Yes	Yes
High school & family drinking		No	No	No	Yes	Yes	Yes
Drinking/academic ratings, sex		No	No	No	No	Yes	Yes
Other preference proxies		No	No	No	No	No	Yes

Regressions are estimated by OLS using sample weights. Heteroskedasticity-robust absolute *t* statistics are in parentheses. See the table 1 footnote for variables included in each specification.

Table 3: Drinking Recency

	N/Mean	(1)	(2)	(3)	(4)	(5)	(6)
Drank past week	39,716	.220	.194	.171	.119	.060	.034
	.490	(29.6)	(26.8)	(22.8)	(16.4)	(8.7)	(5.2)
Drank past month	39,716	.196	.171	.148	.097	.043	.022
	.693	(34.0)	(30.1)	(24.7)	(17.1)	(8.0)	(5.0)
Drank past year	39,716	.128	.113	.093	.052	.017	.004
	.821	(28.2)	(24.9)	(19.8)	(12)	(4.1)	(1.3)
Drank ever	39,716	.096	.085	.070	.036	.012	.003
	.870	(25.3)	(22.3)	(17.2)	(9.5)	(3.3)	(1.0)
Models include:							
Exogenous characteristics		No	Yes	Yes	Yes	Yes	Yes
Endogenous characteristics		No	No	Yes	Yes	Yes	Yes
High school & family drinking		No	No	No	Yes	Yes	Yes
Drinking/academic ratings, sex		No	No	No	No	Yes	Yes
Other preference proxies		No	No	No	No	No	Yes

Regressions are estimated by OLS using sample weights. Heteroskedasticity-robust absolute *t* statistics are in parentheses. See the table 1 footnote for variables included in each specification.

Table 4: Direct Effects of Drinking

	N/Mean	(1)	(2)	(3)	(4)	(5)	(6)
Past 30 days							
Drove after drinking	39,941	.138	.121	.098	.059	.027	.012
	.286	(18.4)	(16.5)	(13.2)	(8.3)	(3.8)	(1.8)
Drove after 5 drinks	39,915	.069	.063	.050	.027	.008	.001
	.110	(12.2)	(11.2)	(8.8)	(4.9)	(1.5)	(0.1)
Current school year							
Had hangover	39,756	.230	.207	.183	.123	.067	.050
	.519	(31.5)	(28.8)	(24.1)	(17.8)	(10.3)	(8.2)
Forgot actions	39,745	.162	.154	.140	.106	.065	.053
	.236	(22.2)	(21.3)	(18.8)	(14.7)	(9.5)	(8.0)
Treated for overdose	39,942	.003	.003	.004	.003	.003	.003
	.004	(2.2)	(2.6)	(3.2)	(2.6)	(2.3)	(2.2)
Models include:							
Exogenous characteristics		No	Yes	Yes	Yes	Yes	Yes
Endogenous characteristics		No	No	Yes	Yes	Yes	Yes
High school & family drinking		No	No	No	Yes	Yes	Yes
Drinking/academic ratings, sex		No	No	No	No	Yes	Yes
Other preference proxies		No	No	No	No	No	Yes

Regressions are estimated by OLS using sample weights. Heteroskedasticity-robust absolute *t* statistics are in parentheses. See the table 1 footnote for variables included in each specification.

Table 5: Additional Outcomes of Drinking (current school year)

	N/Mean	(1)	(2)	(3)	(4)	(5)	(6)
Missed a class	39,748	.204	.194	.173	.132	.091	.076
	.260	(27.2)	(25.9)	(22.9)	(18.4)	(13.4)	(11.5)
Fell behind in school	39,741	.111	.105	.096	.067	.031	.020
	.197	(16.3)	(15.4)	(13.8)	(9.9)	(4.9)	(3.2)
Had unplanned sex	39,749	.115	.109	.102	.074	.035	.025
	.185	(16.5)	(15.7)	(14.3)	(10.7)	(5.2)	(3.8)
Had unprotected sex	39,751	.054	.051	.047	.031	.013	.008
	.088	(10.9)	(10.2)	(9.1)	(6.2)	(2.5)	(1.5)
Argued with friends	39,740	.104	.098	.086	.058	.026	.017
	.195	(15.2)	(14.4)	(12.4)	(8.5)	(3.9)	(2.6)
Damaged property	39,937	.059	.054	.049	.033	.014	.009
	.093	(10.7)	(10.3)	(9.2)	(6.3)	(2.8)	(1.8)
In trouble with police	39,944	.021	.020	.019	.009	.000	–.003
	.051	(5.4)	(5.2)	(4.6)	(2.3)	(0.1)	(0.8)
Was injured	39,937	.064	.062	.056	.038	.019	.014
	.101	(11.9)	(11.5)	(10.2)	(7.1)	(3.6)	(2.7)
Regretted some action	39,746	.166	.155	.139	.099	.050	.038
	.308	(21.7)	(20.3)	(17.8)	(13.3)	(6.9)	(5.4)
Models include:							
Exogenous characteristics		No	Yes	Yes	Yes	Yes	Yes
Endogenous characteristics		No	No	Yes	Yes	Yes	Yes
High school & family drinking		No	No	No	Yes	Yes	Yes
Drinking/academic ratings, sex		No	No	No	No	Yes	Yes
Other preference proxies		No	No	No	No	No	Yes

Regressions are estimated by OLS using sample weights. Heteroskedasticity-robust absolute *t* statistics are in parentheses. See the table 1 footnote for variables included in each specification.

Table 6: Alternative Methods or Samples

	Baseline estimates (tables 1–5) (1)	Unweighted OLS (2)	Probit & interval models (3)	Omits all w/ missing dep. var. (4)	Includes all ages (5)
Binged (5 drinks)	.059 (10.1)	.052 (10.0)	.054 (10.2)	.059 (9.8)	.058 (10.3)
Log(times 5 drinks)	.078 (6.5)	.070 (6.3)	.078 (6.6)	.072 (5.8)	.078 (6.6)
Binged (4 drinks)	.058 (10.2)	.051 (10.1)	.056 (10.4)	.058 (9.9)	.057 (10.4)
Log(times 4 drinks)	.094 (7.5)	.088 (7.7)	.094 (7.6)	.090 (7.1)	.093 (7.6)
Binged (5/4 drinks)	.059 (10.2)	.053 (10.2)	.056 (10.3)	.059 (9.9)	.059 (10.4)
Log(times 5/4 drinks)	.085 (7.0)	.078 (7.0)	.085 (7.1)	.079 (6.4)	.084 (7.1)
Was intoxicated	.035 (5.7)	.035 (6.4)	.031 (5.6)	.034 (5.5)	.036 (6.1)
Log(times intoxicated)	.070 (5.2)	.072 (5.8)	.071 (5.3)	.069 (5.1)	.068 (5.1)
Log(average drinks)	.060 (7.4)	.056 (7.6)	.064 (7.3)	.060 (7.2)	.055 (6.9)
Log(times drank)	.064 (5.5)	.065 (6.3)	.063 (5.5)	.063 (5.3)	.065 (5.7)
Drank past week	.034 (5.2)	.034 (6.1)	.032 (5.1)	.035 (5.2)	.035 (5.5)
Drank past month	.022 (5.0)	.021 (5.5)	.027 (5.3)	.020 (4.7)	.022 (5.3)
Drank past year	.004 (1.3)	.004 (1.6)	.009 (1.7)	.004 (1.3)	.005 (1.6)
Drank ever	.003 (1.0)	.003 (1.2)	.005 (0.7)	.003 (1.2)	.005 (1.7)
Drove after drinking	.012 (1.8)	.011 (1.7)	.013 (2.2)	.013 (1.8)	.012 (1.8)
Drove after 5 drinks	.001 (0.1)	–.001 (0.3)	.002 (0.6)	.001 (0.2)	.000 (0.0)
Had hangover	.050 (8.2)	.049 (9.0)	.049 (8.3)	.050 (8.1)	.049 (8.3)
Forgot actions	.053 (8.0)	.049 (8.1)	.041 (7.8)	.053 (7.8)	.054 (8.5)
Treated for overdose	.003 (2.2)	.003 (2.4)	.003 (3.0)	.002 (1.9)	.003 (2.1)
Missed a class	.076 (11.5)	.073 (12.1)	.059 (11.4)	.076 (11.3)	.073 (11.5)
Fell behind in school	.020 (3.2)	.023 (3.9)	.017 (3.4)	.019 (2.9)	.019 (3.2)
Had unplanned sex	.025 (3.8)	.020 (3.5)	.021 (4.0)	.025 (3.7)	.025 (4.0)
Had unprotected sex	.008 (1.5)	.011 (2.5)	.007 (2.0)	.009 (1.8)	.007 (1.6)
Argued with friends	.017 (2.6)	.019 (3.2)	.014 (2.6)	.017 (2.5)	.018 (2.9)
Damaged property	.009 (1.8)	.007 (1.7)	.003 (1.0)	.011 (2.1)	.011 (2.2)
In trouble with police	–.003 (0.8)	–.002 (0.6)	–.002 (0.7)	–.004 (0.9)	–.002 (0.5)
Was injured	.014 (2.7)	.012 (2.5)	.011 (2.7)	.013 (2.5)	.013 (2.7)
Regretted some action	.038 (5.4)	.031 (4.9)	.031 (5.2)	.037 (5.1)	.038 (5.6)

Specifications correspond to column 6 of tables 1–5; these estimates are shown in column 1. Column 3 shows average marginal effects. The column 4 sample size for binary dependent variable regressions is 38,441; log model samples are accordingly smaller than in other specifications. In column 5, the model includes indicators for ages younger than 18 and older than 24, and samples contain roughly 5,000 (1,000–3,000) additional observations for binary (log) dependent variable models. Heteroskedasticity-robust absolute *t* statistics are in parentheses.

Table 7: Interaction Terms

	Female	Nonwhite and/or Hispanic	Age < 21	School/year proportion in fraternity	
				Variable by itself	Interacted w/ frat. membership
	(1)	(2)	(3)	(4)	(5)
Binged (5 drinks)	–.009 (0.8)	.000 (0.0)	–.009 (0.8)	.102 (1.7)	.113 (2.0)
Log(times 5 drinks)	–.009 (0.4)	.081 (2.3)	.029 (1.3)	–.091 (0.6)	.088 (0.7)
Binged (4 drinks)	.001 (0.1)	.016 (1.0)	–.016 (1.6)	.117 (1.9)	.139 (2.5)
Log(times 4 drinks)	–.011 (0.5)	.053 (1.4)	.025 (1.1)	–.218 (1.4)	.105 (0.8)
Binged (5/4 drinks)	.000 (0.0)	.008 (0.5)	–.006 (0.5)	.117 (1.9)	.118 (2.1)
Log(times 5/4 drinks)	.007 (0.3)	.063 (1.8)	.020 (0.9)	–.087 (0.6)	.071 (0.6)
Was intoxicated	.016 (1.4)	–.010 (0.5)	.003 (0.3)	.086 (1.3)	.052 (0.9)
Log(times intoxicated)	–.006 (0.3)	–.013 (0.3)	.032 (1.3)	–.055 (0.3)	.241 (1.8)
Log(average drinks)	–.020 (1.3)	.043 (1.8)	–.029 (2.0)	.168 (1.7)	.044 (0.5)
Log(times drank)	–.015 (0.7)	–.002 (0.0)	.042 (2.0)	.047 (0.3)	.204 (1.7)
Drank past week	.007 (0.6)	–.001 (0.0)	.011 (0.9)	–.008 (0.1)	.156 (2.3)
Drank past month	.014 (1.8)	.026 (1.9)	.020 (2.6)	.024 (0.4)	.021 (0.5)
Drank past year	.001 (0.2)	.010 (0.9)	.015 (2.9)	.020 (0.5)	–.018 (0.6)
Drank ever	.005 (0.9)	.005 (0.6)	.014 (2.8)	.016 (0.4)	–.009 (0.3)
Drove after drinking	–.011 (0.8)	.004 (0.2)	–.019 (1.5)	–.011 (0.2)	.098 (1.4)
Drove after 5 drinks	–.008 (0.8)	.013 (0.9)	–.012 (1.2)	.016 (0.3)	.052 (1.0)
Had hangover	.007 (0.6)	–.008 (0.4)	.005 (0.4)	.144 (2.2)	–.059 (1.0)
Forgot actions	.004 (0.3)	–.015 (0.9)	.001 (0.1)	.136 (2.1)	.046 (0.7)
Treated for overdose	–.002 (0.9)	.002 (0.4)	–.002 (0.6)	–.005 (0.4)	–.009 (0.9)
Missed a class	.013 (1.0)	–.034 (2.1)	.038 (3.1)	.023 (0.3)	.078 (1.2)
Fell behind in school	.019 (1.6)	.016 (1.0)	.018 (1.5)	.129 (2.0)	.045 (0.7)
Had unplanned sex	–.051 (4.0)	.001 (0.0)	.001 (0.1)	–.003 (0.0)	.123 (1.8)
Had unprotected sex	.013 (1.4)	–.008 (0.6)	–.013 (1.3)	.011 (0.2)	.068 (1.3)
Argued with friends	–.005 (0.4)	–.018 (1.1)	.000 (0.0)	–.033 (0.5)	.087 (1.3)
Damaged property	–.066 (6.6)	.013 (0.9)	–.008 (0.9)	.013 (0.3)	.077 (1.6)
In trouble with police	–.005 (0.7)	.009 (0.8)	–.001 (0.1)	–.059 (1.3)	.018 (0.5)
Was injured	–.010 (1.0)	–.004 (0.3)	.012 (1.2)	–.002 (0.0)	.064 (1.2)
Regretted some action	–.003 (0.2)	.004 (0.2)	.008 (0.6)	.013 (0.2)	.083 (1.1)

Specifications correspond to column 6 of tables 1–5, but also include the interaction between fraternity membership and the variable listed in the column heading. The specification in columns 4–5 also includes the variable "school/year proportion in fraternity" by itself, estimates for which are shown in column 4. Heteroskedasticity-robust absolute *t* statistics are in parentheses.