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Pinka Chatterji
Jeff DeSimone

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ABSTRACT

This paper estimates the effect of binge and frequent drinking by adolescents on subsequent high school dropout using data from the National Longitudinal Survey of Youth 1979 Young Adults. We estimate an instrumental variables model with an indicator of any past month alcohol use, which is by definition correlated with heavy drinking but should have minimal additional impact on educational outcomes, as the identifying instrument, and also control for a rich set of potentially confounding variables, including maternal characteristics and dropout risk factors measured before and during adolescence. In comparison, OLS provides conservative estimates of the causal impact of heavy drinking on dropping out, implying that binge or frequent drinking among 15 −16 year old students lowers the probability of having graduated or being enrolled in high school four years later by at least 11 percent. Overidentification tests using two measures of maternal youthful alcohol use as additional instruments support our identification strategy.

Pinka Chatterji
Center for Multicultural Mental Health Research
Cambridge Health Alliance
Harvard Medical School
120 Beacon Street, 4th Floor
Somerville, MA 02143
and NBER
pchatterji@charesearch.org

Jeff DeSimone
Department of Economics
College of Business Administration
University of South Florida
4202 East Fowler Avenue, BSN 3403
Tampa, FL 33620-5500
and NBER
jdesimon@coba.usf.edu

Introduction

Despite its illegality, alcohol is extensively used by teenagers. In the 2004 Monitoring the Future (MTF) study, a national survey of adolescent substance use, 48 percent of high school seniors reported consuming alcohol in the past month, and 29 percent reported binge drinking (having at least five drinks in a single episode) in the past two weeks. Drinking prevalence was only slightly lower among 2004 MTF 10th graders: 35 percent used alcohol in the past month and 22 percent binge drank in the past two weeks. Even among 2004 MTF 8th graders, almost one in five students reported past month alcohol use (Johnston et al. 2005).

The primary motivation for alcohol control policies targeted at youth, such as zero tolerance laws, restrictions on advertising, and minimum legal drinking ages, is to lessen the harmful social impact of teenage alcohol use. While such policies appear to have reduced drinking among youth (e.g. Chaloupka et al. 2002), it is unclear whether any health or social benefits will ensue. In particular, if alcohol use does not cause the adverse outcomes with which it is associated, even successful alcohol policies cannot prevent these outcomes. For example, while alcohol use is highly correlated with suicidal behaviors and risky sexual practices, causal relationships are difficult to establish (Chatterji et al. 2004, Grossman et al. 2002, Rashad & Kaestner 2004, Rees et al. 2001).

This study provides evidence on the causal nature of the relationship between heavy drinking among middle and high school students and subsequent high school dropout, an important human capital outcome. Alcohol use has a strong inverse association with educational success: adolescent alcohol users receive lower grades and are more likely to report academic difficulties and not graduate from high school than their non-using peers (Cook & Moore 1993, DeSimone & Wolaver 2005, Ellickson et al. 2003, Yamada et al. 1996). This association,

however, may be partially or entirely incidental. Adolescent drinkers typically have characteristics that predispose them to poor educational outcomes, such as behavioral and family problems, parental substance use, minimal parental monitoring, and low attachment to school (Borawski et al. 2003, Diego et al. 2003, Ellickson et al. 2001, Maney et al. 2002, Sale et al. 2003, Silberg et al. 2003). Empirical models may not sufficiently control for these factors because they are difficult to measure. The effects of heavy drinking on educational attainment estimated by standard regression procedures therefore may suffer from omitted variable bias and not reflect causal impacts.

We analyze data from National Longitudinal Survey of Youth 1979 Young Adults (NLSY79-YA). As children of the youngest, and thus relatively disadvantaged, NLSY79 mothers, NLSY79-YA are at elevated risk for school dropout, making them a particularly appropriate group to study. Furthermore, the combination of NLSY79-YA and NLSY79 mothers offers an unusually rich data source that contains detailed information not only on youth alcohol use and educational attainment, but also on socioeconomic background, maternal alcohol use, and risk factors for adolescent drinking and dropout. These measures include many potentially confounding correlates of alcohol use and educational attainment, facilitating the estimation of the causal impact of heavy drinking on subsequent high school dropout.

Specifically, we estimate the effects of both binge drinking and frequent drinking, the latter defined as having 14 or more drinks in the previous month (as explained further below), on the likelihood that students who were in seventh through twelfth grade at age 15–16 had not either graduated from or remained enrolled in high school as of the interview that occurred four years later. We begin by estimating ordinary least squares (OLS) regression models that control for a basic set of covariates that are standard in the literature, a set of maternal characteristics

including educational attainment and current alcohol use, and several less plausibly exogenous respondent characteristics including current depressive symptoms and early behavior problems and reading ability. Results imply that heavy drinking reduces the probability of graduating or remaining in school by 11–13 percent.

To assess the legitimacy of interpreting these OLS estimates as causal effects, we then estimate instrumental variables (IV) models that specify an indicator of any past month alcohol use as the sole identifying instrument. Our logic for this choice is that a correlation between any drinking and heavy drinking exists by definition, while drinking that is not intense or frequent presumably has no mechanism by which to impact educational outcomes. Two stage least squares (2SLS) estimation reveals that not only are the first stage relationships between any drinking and both binge and frequent drinking indeed quite strong, but moreover the second stage binge and frequent drinking effects are somewhat larger in magnitude than those from OLS, suggesting that the latter provides conservative causal effect estimates.

Formal overidentification tests from an alternative specification that includes two measures of youthful maternal drinking as additional instruments provide empirical evidence that the indicator of any past month alcohol use can be validly excluded from the educational attainment equation. Finally, estimated effects that are qualitatively and often quantitatively similar persist upon various modifications to the sample inclusion criteria. These findings suggest that limiting adolescent drinking may have important human capital benefits, at least for children of relatively young and disadvantaged mothers.

Adolescent Alcohol Use and Human Capital Outcomes

Conceptually, numerous causal pathways exist through which alcohol use could potentially interfere with the human capital accumulation that occurs during adolescence. Among heavier users, drinking might impair cognition, learning and memory (e.g. Ziegler et al. 2005), take time away from studying and class attendance (Powell, Williams & Wechsler, 2004; Williams, Powell & Wechsler, 2003; Wolaver 2002), diminish academic reputation among teachers and peers, and lower attachment to school and motivation. Through any of these avenues, heavy drinking would be expected to decrease the marginal benefit of investing in additional schooling.

Previous research has found that heavy drinking during youth is strongly associated with adverse educational outcomes (Ellickson, Tucker & Klein, 2003; Cook & Moore, 1993; Yamada, Kendix & Yamada 1996; Mullahy & Sindelar 1989) but offers only limited evidence regarding whether this association represents a causal relationship, given that alcohol use may be correlated with unmeasured factors that also detract from educational attainment. Empirical methods that address this issue have commonly identified the educational attainment equation using state-level alcohol policies, which are expected to influence adolescent alcohol use without directly affecting educational attainment. Cook and Moore (1993), for example, use this approach to estimate the effect that drinking on at least two days in the past week has on additional years of schooling in NLSY79 data on high school seniors. They report large detrimental effects of alcohol use: drinkers finish 2.3 fewer years of college than do non-drinkers.

This identification strategy has been called into question for several reasons. State policies tend to be only weakly correlated with alcohol use, at least by the standards required to have confidence in IV estimates. Bound, Jaeger and Baker (1995), Bollen, Guilkey and Mroz

(1995), Nelson and Startz (1990) and Staiger and Stock (1997) demonstrate that a low first stage *F* statistic for the joint significance of the identifying instruments could lead to IV estimates being more biased than corresponding OLS estimates. Rashad and Kaestner (2004) show that using alcohol policies as instruments may be problematic whenever equations modeling alcohol use and consequences of alcohol use are estimated jointly. Dee (1999) similarly argues that cross-state alcohol policy variation is endogenous, in the sense that it is associated with unobserved state sentiments that are correlated with both drinking and educational attainment. In sum, state alcohol policies lack credibility as identifying instruments both because they are typically poor predictors of adolescent drinking, and when state-level fixed effects cannot be included to capture time invariant state sentiments that are correlated with policy enactment.

Because of these problems, two recent related studies apply alternative empirical approaches. Koch and Ribar (2001) estimate the relationship between the age of alcohol use initiation and years of schooling completed by age 25 in NLSY79. They estimate family fixed effects models using NLSY79 siblings, and instrumental variables models using the sibling's initiation age as an identifying instrument. Their results indicate that on average delaying alcohol use initiation by one year increases educational attainment by 0.22 years. Dee and Evans (2003) use a two-sample IV (TSIV) strategy in which they use data first from the 1977–1992 MTF surveys to estimate the impact of minimum legal drinking ages on teen alcohol use, and then from the 1990 Census to construct simulated drinking variables based on the MTF equations and estimate their effects on schooling. The results contradict most existing research, in that none of the drinking measures they examine have statistically significant effects on high school graduation, college entrance or college completion.

Methods

Our estimates of the effect that heavy drinking has on subsequently dropping out of high school before graduating come from the equation

$$D = \beta_0 + \beta_1 A + \mathbf{X}\boldsymbol{\beta}_2 + \mathbf{M}\boldsymbol{\beta}_3 + \mathbf{R}\boldsymbol{\beta}_4 + \varepsilon, \quad (1)$$

where D represents dropout, A denotes binge or frequent alcohol use, ε is the error term, and **X**, **M** and **R** are vectors of presumably exogenous variables, maternal characteristics and student-level risk factors, respectively, that potentially influence educational attainment. The inclusion of elements in **M** and **R** that are also correlated with drinking eliminates the bias that would have resulted if these measures had otherwise been subsumed in ε. But if factors such as orientation towards the future relative to the present, risk aversion and self-esteem affect both dropout and drinking and are still not held constant in equation (1) because of their inherently unobservable nature, despite the presence of **M** and **R**, the OLS estimate of the causal effect β_1 remains inconsistent. Specifically, the true causal effect will be smaller in magnitude than the OLS coefficient implies.

Previous studies such as Dee and Evans (2003) and DeSimone and Wolaver (2005) have focused on this source of bias, i.e. unobserved heterogeneity, in their arguments that alternative methods explicitly dealing with the endogeneity of alcohol use are necessary to obtain a consistent estimate of the causal effect of heavy drinking on educational outcomes. An alternative potential source of bias that could result in OLS instead underestimating the causal effect is measurement error. If our drinking variables are misreported or misspecify the relevant drinking behavior, the OLS estimate of β_1 will be biased downward in magnitude as long as these errors in measurement are random.

Because IV models with properly specified instruments are immune to bias resulting

from unobserved heterogeneity and measurement error in the drinking variables, we estimate IV models to generate consistent estimates of the causal effect of heavy drinking on dropping out, and use these estimates to assess whether our OLS estimates reflect causal effects. Our IV models are estimated using 2SLS. The first stage is an equation that explains heavy drinking in terms of observed factors **X**, **M** and **R**, and one or more instruments **Z** that affect heavy drinking but have no further impact on dropping out,

$$A = \alpha_0 + \mathbf{Z}\boldsymbol{\alpha}_1 + \mathbf{X}\boldsymbol{\alpha}_2 + \mathbf{M}\boldsymbol{\alpha}_3 + \mathbf{R}\boldsymbol{\alpha}_4 + u\,. \tag{2}$$

The second stage is equation (1) above, using the fitted values from equation (2) in place of the observed binge or frequent drinking measures and adjusting standard errors accordingly. We further adjust standard errors for both heteroskedasticity of arbitrary form, which is particularly relevant because both drinking and dropout are measured as binary indicators, and correlation between observations from siblings, because our 1,107 respondents come from only 912 different mothers.[1]

Our identification strategy specifies a binary indicator of any past month alcohol use as the sole excluded instrument. This is notable in that the nature of the association between the identifying instrument and the endogenous variable is definitional rather than statistical. By definition, past month drinking is correlated with the relatively intense and frequent types of drinking that we hypothesize will affect educational attainment, since one cannot drink heavily without drinking at all. As a result, standard errors in the dropout equation are small enough to allow for more precise inferences. Any uncertainty regarding the appropriateness of this

[1] Two points are relevant here. First, although generalized method of moments is in principle more efficient than 2SLS when regression errors are heteroskedastic, in our application the two procedures produce identical results to any reasonable level of precision, so we utilize 2SLS for expository convenience. Second, Angrist (2001) and Wooldridge (2002, pg. 622) argue that researchers can, and in many cases should, use 2SLS even when the endogenous and outcome variables are binary, rather than methods specifically designed to address the binary nature of these variables.

identification strategy would therefore involve the exogeneity of the identifying instrument with respect to the error term in the dropout equation. Specifically, our strategy presumes that non-binge and infrequent drinking will not reduce educational attainment separately from its influence on binge and frequent drinking.

Our conceptual argument for this exclusion restriction relies on the premise that there is no mechanism by which drinking can impact educational attainment unless it is either intense in some episodes or occurs frequently. For instance, Wolaver (2002) and Williams et al. (2003) argued that drinking can reduce academic performance either directly by diminishing cognitive skills, or indirectly because drinking occurs during time that would otherwise have been allocated towards studying. But drinking that is not intense, i.e. does not result in drunkenness and consequent hangovers or addiction, cannot directly affect cognitive skills. And infrequent drinking simply does not occupy enough time to meaningfully alter time constraints.

Our exclusion restriction is possibly not as intuitively compelling as that made by previous researchers who use state-level alcohol policies to identify equations that estimate outcomes of alcohol use. However, such policies are correlated with unobserved state-level determinants of educational attainment when state fixed effects are not included (e.g. Dee and Evans 2003), but have weak predictive power for drinking when state effects are included.[2] Additionally, even when state fixed effects are included, using state policies for identification fails to take advantage of variation in heavy drinking that occurs within the same state and time period. In contrast, our identification strategy acknowledges the importance of the first-stage explanatory power of the instrument in generating IV estimates that are consistent and sufficiently precise to test the hypothesis of interest.

[2] Inclusion of state fixed effects is particularly problematic in our sample, which is relatively small in size and has minimal time variation.

Moreover, we empirically assess the merit of our exogeneity argument by estimating an additional specification in which two indicator variables serve as auxiliary instruments that overidentify the dropout equation: (1) maternal drinking on a weekly basis before the mother was 16 years old (maternal adolescent drinking) and (2) past month maternal binge drinking measured in 1982, when the mother was between 17 and 25 years old (maternal young adult drinking). Maternal adolescent and young adult drinking are likely linked to respondent drinking through genetic factors such as preferences for alcohol. Also, mothers who were binge or frequent drinkers as youths may find it difficult to prohibit their children from doing the same. These measures should not directly affect educational attainment, however, given that the **M** vector includes various maternal attributes, such as educational attainment and current alcohol use, which will mediate any correlation between previous maternal drinking and respondent educational outcomes. Because the dropout equation is overidentified in these models, we can conduct formal overidentification tests to confirm the validity of our primary exclusion restriction. The results of these tests also address any concern that unobservable factors drive both differences in educational outcomes between drinkers and non-drinkers and the correlation between any alcohol use and heavy drinking.

The National Longitudinal Survey of Youth 1979 – Young Adults

NLSY79 Young Adults and Mothers

Our data come from the 2002 release of the National Longitudinal Survey of Youth 1979 Young Adults (NLSY79-YA) and the NLSY79, an annual nationally representative survey initiated in 1979 with a sample of 12,686 individuals aged 14–21. NLSY79 respondents provided extensive information on labor market participation, education, fertility, substance use

and family background. Beginning in 1986, children of female NLSY79 respondents (CNLSY79) were assessed biennially in a variety of areas relevant for child development. As these children began to enter late adolescence, there was interest in obtaining information from them on the same topics as were covered by NLSY79. Thus, beginning in 1994, CNLSY79 respondents who would be at least 15 years old by the end of each survey year formed a new cohort, the NLSY79-YA.

NLSY79-YA offers some of the richest data available to social scientists: longitudinal information on participants from birth to young adulthood combined with detailed information on their mothers' experiences beginning in adolescence. Respondents biennially complete a computer-assisted survey instrument, administered either in person (in most cases) or by telephone. The survey parallels the NLSY79 survey and focuses on transitions into adulthood. The 2002 survey had 4,648 respondents aged 15 to 32. These are the children of the youngest NLSY79 mothers: at the time of the respondents' births, all mothers were under age 27 and 41 percent were teenagers.

We use information from the 1994, 1996, 1998, 2000 and 2002 NLSY79-YA surveys, linked to earlier assessments obtained during the 1988, 1990, 1992 and 1994 CNLSY79 surveys and maternal characteristics from the 1982, 1992, 1994, 1996 and 1998 NLSY79 surveys. The analysis sample consists of 1,107 NLSY79-YA respondents who were 19–24 years old in 2002 and had information available on alcohol use at age 15–16 (from the 1994, 1996 and 1998 surveys) and educational attainment at age 19–20 (i.e. from the survey administered four years later).[3] We further limit our sample to respondents who were in grades 7–12 at age 15–16: 68 respondents who dropped out before the age 15–16 interview are excluded.

[3] NLSY79-YA respondents who were age 25 or older in 2002 were 15–16 years old before the NLSY79-YA surveys were initiated, while those younger than 19 years old in 2002 are too young to assess school dropout.

Educational Attainment and Alcohol Use Measures

Because our sample represents three separate NLSY79-YA cohorts, we measure most relevant characteristics at the same age for all respondents. In particular, our drinking variables capture past month alcohol use as of the interview at which respondents were age 15 or 16, and our educational attainment variable corresponds to the interview that occurred four years subsequently, by which respondents were age 19 or 20. The latter is specifically a binary indicator that is coded to one if the respondent both had not graduated from high school and was not enrolled in school at the age 19–20 interview, and otherwise equals zero. We do not categorize respondents who are still in school but have yet to graduate as "dropouts" because 7th and 8th graders are not expected to finish high school within four years.

As only drunkenness or frequent drinking is expected to be a detriment to educational attainment, the two binary alcohol use indicators that we consider are binge drinking, defined as consuming at least five drinks in a single episode, and frequent drinking, defined as consuming at least 14 alcoholic beverages over the month and based on responses to the questions "On how many days did you drink during the past 30 days?" and "On average, how many drinks did you consume on the days you drank in the past 30 days?" The threshold of 14 is chosen to capture roughly the most frequent-drinking one-third of sample past month alcohol users; while almost 10 percent of drinkers consumed exactly 12 beverages, no respondents reported having 13 drinks and no single number of drinks above 14 applies for more than three percent of drinkers.[4]

Explanatory Variables

A variety of potential determinants of educational attainment are important to include as control variables because of their possible correlation with binge and frequent drinking during

[4] Results were similar when slightly higher drink thresholds were used to categorize frequent drinking.

adolescence. We begin by estimating models that include **X**, a basic set of variables that are plausibly exogenous from the adolescent's perspective. These include indicators for being African-American, Latino, female, and each of ages 20–24 (age 19 omitted) and in each of grades 8–12 (7^{th} grade omitted), living in the central, western and southern U.S. (northeast omitted) and in an urban location, and all four quartiles of total net previous year family income (with family income not observed as the omitted category), along with the log of the number of other household members age 17 and younger. All but the race and gender indicators are from the same survey year as the alcohol use variable, when the respondent is 15–16 years old.

Next we estimate an expanded model that also includes the maternal characteristics **M** in order to gauge their role as potential confounders. These include indicators for each age from 31 to 41 years old, with the omitted category including a total of seven observations in which age was 29, 30 or not reported; having graduated from high school, attended college, and graduated from college, defined as mutually exclusive categories with not having completed high school as the baseline; being employed and unemployed, with out of the labor force as the omitted category; currently being married, the father residing in the household, alcohol use when pregnant with the respondent, and having binge drank at least once in the past month in 1994. All variables besides the two maternal drinking indicators are measured when the youth is age 15–16. Maternal binge drinking might be correlated with teen drinking and dropout because of both genetic and environmental factors (Cloniger et al. 1981, Hrubec & Omenn 1981, Chassin et al. 1991). We take this variable from the 1994 NLSY, when our sample respondents were 10–16 years old, because this is the closest available match to our standard baseline of age 15–16.

Previous research suggests that three of the most important individual-level risk factors for adolescent alcohol use are childhood impulsive and aggressive behavior, poor early academic

achievement, and psychiatric disorders such as conduct disorder, attention deficit hyperactivity disorder (ADHD), anxiety and depression (Pulkkinen & Pitkanen 1994, Costa et al. 1999, Caspi et al. 1996, Brook et al. 1992, Rydelius et al. 1981, Brook et al. 1986, Block et al. 1988, Boyle & Offord 1991, Kushner & Sher 1993, Deykin et al. 1987, Deykin et al. 1992). All three might also impact educational attainment directly. In previous studies, these variables generally have been omitted, thus remaining a potential source of unobserved heterogeneity. However, NLSY79-YA includes measures of all three of these quantities, enabling estimation of our "full" model that also includes the vector **R** of risk factors for alcohol use and school dropout. We add **R** separately from **M** because these variables are potentially endogenous: its inclusion might reduce bias from unobserved heterogeneity, but alternatively might increase bias if its elements are determined simultaneously with educational attainment.

Our measures of early behavior problems and school achievement are based on the Behavior Problems Index (BPI) and the Peabody Picture Vocabulary Test (PPVT), respectively, both of which were administered when respondents were age 10 or 11. The BPI consists of 28 items describing a particular type of behavior pertaining to sociability, anxiety, depression, headstrongness, hyperactivity, immaturity, dependency and social withdrawal (Peterson & Zill, 1986). Mothers respond "often" or "sometimes true," which are assigned a value of one, or "not true," which receives a value of zero. Scores are summed and then normed based on data from the 1981 National Health Interview Survey. Because young children with high percentile scores on behavior problem scales are more likely than others to subsequently have persistent behavior problems, DSM III diagnoses of externalizing disorders, and mental health services use (Achenbach et al. 1995, Campbell & Ewing 1990, McGee et. al. 1991, Gortmaker et. al. 1990), we include a binary indicator for whether the gender-specific normed BPI score was at or above

the 90th percentile. The PPVT is considered to be a highly valid and reliable measure of scholastic aptitude (Campbell, Bell & Keith 2001). Children listen to an interviewer state a word, and then are asked to choose which of four pictures best represents its meaning. Increasingly difficult questions are asked until the child incorrectly responds to six of eight consecutive questions. We control for the normed percentile PPVT score.

Our psychological disorder variable is a measure of adolescent depressive symptoms at age 15–16 using items from the Center for Epidemiologic Studies Depression Scale (CES-D), which is widely used and has demonstrated excellent psychometric properties (Crockett et al. 2005, Radloff 1977, Husaini et al. 1980). The NLSY79-YA includes seven of 20 items from the CES-D scale: poor appetite, trouble keeping mind on tasks, depressed, everything took extra effort, restless sleep, sad and could not get going. Responses are on a four-point Likert scale, with higher values corresponding to higher frequencies of the item in the past week. Because the full 20-item scale was not included, we use an indicator of whether the sum of the responses falls at or above the 90th percentile of the sample to represent an elevated level of depressive symptoms.

Instruments

As described previously, our primary identifying instrument is an indicator of any past month alcohol use at age 15–16. To overidentify the model, we also estimate specifications that include two additional instruments, binary indicators for whether the mother of the respondent: (1) had at least one binge drinking episode in the past month in 1982, when she was age 17–25, and (2) had initiated weekly use of alcohol before age 16. Maternal alcohol use data come from responses to 1982 NLSY79 questions regarding the number of alcohol binges in the past 30 days

and the age at which drinking first occurred on at least a weekly basis.

Descriptive Statistics

Table 1 displays variable means.[5] The first column pertains to the full sample, the second and third columns split the sample into binge drinkers and others, and the fourth and fifth columns divide the sample into frequent drinkers and others. Drinking status generally varies with exogenous covariates in the predicted directions, with income (i.e. a nonlinear effect), maternal employment status, paternal presence in the home, and PPVT score constituting the few exceptions and differences across drinking status being small in those cases.

Just over a sixth of sample respondents are classified as dropouts. Slightly less than 10 percent of the sample binge drank at least once during the previous month, and about six percent are categorized as frequent drinkers.[6] For both binge and frequent drinking, dropout prevalence is 11 percentage points higher for drinkers than for non-drinkers. But binge and frequent drinking are not synonymous: while all but two of the 65 frequent drinkers are also binge drinkers, 40 percent of binge drinkers consumed 12 or fewer beverages in the past month, which implies that they had two or fewer binge drinking episodes and otherwise drank infrequently.

Although our primary instrument, having consumed any alcohol in the past month, is by definition correlated with our measures of heavy drinking, roughly half of past month drinkers did not binge drink, and two-thirds are not frequent drinkers. Our auxiliary instruments also have high simple correlations with our heavy drinking variables. Having a mother who drank

[5] Because all but two variables are binary indicators, we do not show standard deviations or ranges. In the full sample, PPVT ranges from 0 to 99 with a standard deviation of 25.9, while the number of other household members under age 18 ranges from 0 to 12 with a standard deviation of 1.59.

[6] These rates are somewhat lower than national rates of adolescent alcohol use in the mid-1990s, when respondents were 15–16 years old. For example, among 1996 MTF 10th graders, 26 percent of whites, 12 percent of African-Americans and 30 percent of Latinos binge drank in the past 2 weeks. Among 1996 MTF 8th graders, these rates were 15 percent for whites, 10 percent for African-Americans and 21 percent for Latinos. It is important to note that 36 percent of our sample is African-American, the racial group with the lowest binge drinking prevalence.

weekly by age 16 is more than twice as likely among both binge and frequent drinkers. Similarly, the prevalence of mothers who binge drank in the month prior to the 1982 interview, which corresponds to between one year before and four years after the birth years of respondents, is about twice as high for binge drinkers and 50 percent higher for frequent drinkers.

Results

Table 2 displays the main results of the analysis. The first three columns provide binge-drinking results for our basic, expanded and full set of explanatory variables, respectively, while the latter three columns do the same for frequent drinking. Maternal characteristics are excluded from the basic model and risk factors for drinking and dropout are included only in the full model. The top row shows that regardless of whether maternal characteristics and risk factors are held constant, more than half of past month drinkers have at least one binge drinking episode and almost a third of drinkers consume at least 14 drinks. The next row indicates that the extremely large *t* statistics for any alcohol use translate to *F* statistics of around 220 for binge drinking and nearly 100 for frequent drinking, which are considerably higher than the threshold of 10 sometimes used to judge the adequacy of instrument strength (Staiger and Stock 1997).

OLS estimates of the effects of binge and frequent drinking on dropping out, all of which are significant at the five percent level, are listed in the following row. For both drinking measures, the estimated effect falls as additional explanatory variables are included in the regression. In particular, moving from the basic to expanded model reduces the binge drinking coefficient by 26 percent and the frequent drinking coefficient by 13 percent, and moving from the expanded to full model lowers the coefficients by another six to seven percent, producing overall declines in magnitude of 31 percent for binge drinking and 19 percent for frequent drinking. This provides some assurance that the unique aspects of our data, namely the maternal

characteristics and dropout risk factors, do in fact capture some of the unobserved heterogeneity that potentially plagues OLS estimates of the causal effect of drinking on educational attainment. In the full model, which we prefer because it yields the most conservative estimates, binge and frequent drinking increase the probability of dropping out by 9–11 percentage points, which at the mean dropout proportion implies an 11–13 percent reduction in the probability that 15–16 year old students have graduated or are still in school four years later.

2SLS estimates, which identify binge and frequent drinking using just the indicator of any past month alcohol use and are also highly significant in all cases, appear in the subsequent row. The pattern of declining coefficient magnitudes as maternal characteristics and dropout risk factors are added parallels that for OLS. Notably, the 2SLS estimates are larger in magnitude than those from OLS, by roughly 50 percent for binge drinking and 100 percent for frequent drinking, contradicting the expectation that they would be smaller because of unobserved heterogeneity. The exogeneity *t* statistics, however, reveal that there is no statistical difference between OLS and 2SLS in most specifications, including the full models that we prefer.

Highly depressed students are seven percentage points more likely to drop out, while a one standard deviation increase of 26 points in the PPVT score reduces the dropout probability by four percentage points. The former effect is significant at the 10 percent level, while the latter is significant at one percent. Despite the large coefficients on these variables, the estimated drinking effects fall only minimally when they are added to the model along with the youth behavioral problems indicator, and are still highly significant.

It seems reasonable that frequent drinking has a slightly larger effect on dropout propensity than binge drinking, in that while 97 percent of frequent drinkers are binge drinkers, 40 percent of binge drinkers are not frequent drinkers. The negative effect of frequent drinking

evidently increases the dropout rate of all non-binge drinkers relative to that of just infrequent drinkers and abstainers, thus dampening the estimated impact of binge drinking.

A possible explanation for the 2SLS effect being larger than the OLS effect is random measurement error in the binge and frequent drinking variables. Both drinking measures could be misreported in a nonsystematic way because of recall error regarding the timing of drinking or quantity of drinks consumed. And both variables are only crude measures of the form of alcohol use that would presumably impact educational outcomes, in that neither fully captures the intensity of heavy drinking episodes or the frequency with which drinking events take place. Even our frequent drinking indicator, which for practical purposes encompasses binge drinking, is imprecise because both the number of days on which drinking occurs and the amount of alcohol necessary to produce intoxication could vary across respondents.

A more problematic alternative reason for the relative magnitudes of the OLS and 2SLS estimates would be if the identifying instrument, the any past month alcohol use indicator, is not truly exogenous with respect to high school dropout even conditional on binge and frequent drinking. Table 3 empirically examines this possibility by displaying results of models in which the two measures of youthful maternal drinking, indicators of whether the mother initiated weekly drinking by age 16 and binge drank at least once in the past month in 1982, serve as additional excluded instruments in the 2SLS model. This allows us to overidentify the dropout equation and thereby conduct formal tests for the hypothesis that past month alcohol use and the two youthful maternal drinking indicators are jointly validly excluded.

The first and second columns, and fourth and fifth columns, juxtapose estimates from the basic and full models for binge and frequent drinking, respectively. The first row, in comparison to the analogous row of table 2, shows that including the additional instruments has no tangible

effect on the magnitude or standard error of the first stage any alcohol use coefficient. As evident from the next two rows, when our primary instrument is included the auxiliary instruments have minimal first stage strength, though maternal binge drinking is a significant predictor of binge drinking and maternal weekly drinking is a significant determinant of frequent drinking in the full model. The subsequent row indicates that *F* statistics for the identifying instruments, though reduced in size by almost two-thirds, are still very large, and the following row reveals that with our original instrument retaining the vast majority of the identifying power, the 2SLS estimates change little when the auxiliary instruments are added.

Hansen's *J* statistic, i.e. the minimized value of the corresponding generalized method of moments objective function multiplied by the sample size, is reported in the last row, with *p*-values given in brackets beneath. Although this overidentification test is comfortably passed even in the basic model, moving to the full model raises the *p*-values from around .5 to .9 as the 2SLS estimates fall in size by over one-quarter. This provides further evidence that the maternal characteristics and dropout risk factors that are included in the full but not basic model account for some of the unobserved heterogeneity that inflates estimates of the impact of drinking on educational attainment. Moreover, the very high *J* statistic *p*-values in the full models offer assurance that our main identifying instrument does not influence high school dropout beyond effects of binge or frequent drinking and is therefore appropriately excluded from the dropout equation.

The third and sixth columns of table 3 show estimates from 2SLS models in which the any past month alcohol use indicator is omitted, leaving the two youthful maternal drinking indicators as the only identifying instruments. Both variables are more highly correlated with binge and frequent drinking than when past month drinking is included, but their joint *F* statistics

are substantially lower and well below the Staiger and Stock (1997) threshold of 10. The consequence is very large second stage standard errors. Importantly, however, the 2SLS coefficients are somewhat larger in magnitude, and overidentification tests are still passed but with slightly lower *p* values. Combined with the overidentification statistics in the second and fifth columns, these results provide strong confirmation that our identification strategy is valid.

Table 4 explores the sensitivity of the estimated full model drinking effects to various sample modifications. For convenience, the first row restates the main results from table 2. The next two rows more specifically examine high school dropout by omitting 7th and 8th graders, who would not be expected to have graduated four years later, and 12th graders, who are already close to graduation.[7] In the second row, simply altering the sample in this way has little impact on the results. When we further change the dependent variable to reflect only high school graduation, so that students still in high school are now reported as "dropouts," the OLS effect diminishes is size, implying that abstaining from binge or frequent drinking is a reason that students who have not yet graduated but are still in school have not dropped out (rather than drinking being a reason that these students are still in school rather than having graduated), but the change in magnitude is not dramatic and the 2SLS effect is slightly larger. The following two rows indicate that binge drinking is problematic for Latinos and blacks while frequent drinking is not, with the opposite being true for others (mostly whites). Subsequent rows show that drinking effects are larger for males, non-urban residents, and students with mothers who are less educated and unmarried. All in all, though, the relative stability of the estimates across a variety of subsamples offers further evidence that our results are not spurious.

[7] Only one of 31 sample 12th graders is classified as a dropout.

Discussion

Previous studies have had difficulty in inferring whether the well-documented negative correlation between adolescent alcohol use and educational attainment reflects a causal relationship, with researchers facing two challenges: a lack of data on some important risk factors for alcohol use, and a reliance on state-level policies as identifying instruments even though they have only weak explanatory power for adolescent drinking. Our study takes advantage of an unusually rich data set to assess the evidence on whether teen drinking causally increases the likelihood of high school dropout. We estimate OLS dropout equations that include controls for maternal characteristics and dropout risk factors that are likely to be related with alcohol use, as well as 2SLS models using an indicator of any past month alcohol use as the identifying instrument. Besides establishing that any drinking is an excellent predictor of binge and frequent drinking, we make both conceptual and empirical cases that its exclusion from the dropout equation is legitimate.

The OLS results imply that past month binge and frequent drinking at age 15–16 bring about 11–13 percent declines in the likelihood of still being enrolled in or having completed high school four years later. 2SLS estimates are even larger in magnitude, regardless of whether two indicators of youthful maternal drinking are specified as auxiliary or sole identifying instruments. Estimated effects are reasonably insensitive to changes in sample composition.

We conservatively interpret our findings as indicating that OLS estimates represent causal effects of binge and frequent drinking on high school dropout. A caveat to this conclusion is that it applies specifically to relatively disadvantaged teenagers who are already at elevated risk of school dropout. But our results are notable in that the 2SLS estimates are larger in magnitude, rather than smaller, compared with those from OLS, as well as in finding significant

effects in a comparatively small sample size of just over 1,000.

These findings are consistent with those of Mullahy & Sindelar (1989, 1994), who suggest that the onset of alcoholism before age 19 may have important indirect effects on adult labor market outcomes through reductions in schooling. Given that a very large body of literature shows no evidence that alcohol use directly detracts from labor market outcomes (Cook & Moore 2002), drinking may impact labor market productivity primarily through reductions in educational attainment. If so, alcohol policies that reduce the prevalence of teenage drinking might increase human capital accumulation and ultimately labor market outcomes. Future research should address this issue by linking measures of alcohol policies directly to data on educational attainment, and investigating long-run labor market effects of adolescent drinking.

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Table 1: Variable means

	Full	Binge drinker?		Frequent drinker?	
	Sample	Yes	No	Yes	No
Sample size	1,107	105	1,002	65	1,042
Dropped out by age 19-20	.173	.276	.162	.277	.166
Binge drinker past month	.095	–	–	.969	.040
Frequent drinker past month	.059	.600	.002	–	–
Any alcohol use past month	.182	–	.097	–	.131
Mother drank weekly by age 16	.127	.258	.113	.279	.117
Mother binge drank past month in 1982	.179	.316	.165	.264	.174
8th grade	.067	.038	.070	.031	.069
9th grade	.291	.324	.287	.308	.290
10th grade	.392	.352	.396	.400	.392
11th grade	.213	.257	.209	.231	.212
12th grade	.028	.029	.028	.031	.028
20 years old in 2002	.191	.114	.199	.108	.196
21 years old in 2002	.182	.171	.183	.154	.183
22 years old in 2002	.199	.267	.194	.246	.196
23 years old in 2002	.112	.181	.105	.200	.107
24 years old in 2002	.076	.095	.074	.077	.076
Latino	.196	.257	.190	.292	.190
Black	.355	.143	.377	.138	.369
Female	.518	.505	.519	.477	.520
Midwest	.223	.276	.218	.246	.222
South	.476	.419	.482	.431	.478
West	.184	.190	.184	.200	.183
Urban	.709	.654	.715	.657	.712
Lowest income quartile (< $17,000)	.202	.171	.206	.169	.204
2nd lowest income quartile (< $33,000)	.203	.295	.194	.292	.198
2nd highest income quartile (< $56,000)	.201	.229	.199	.277	.197
Highest income quartile	.198	.181	.200	.169	.200
Other HH members age 17 and under	4.18	4.03	4.20	3.92	4.20
Father at home (age 10–11)	.521	.514	.522	.520	.521
Mother graduated from high school	.399	.423	.396	.477	.394
Mother entered college	.248	.183	.255	.200	.251
Mother graduated from college	.052	.020	.055	.031	.053
Mother is married	.563	.543	.565	.569	.563
Mother is employed	.691	.714	.689	.754	.687
Mother is unemployed	.069	.029	.073	.046	.070
Mother binge drank past month in 1994	.196	.343	.180	.292	.190
Mother drank weekly during pregnancy	.048	.067	.046	.046	.048

Table 1: Variable means (continued)

	Full Sample	Binge drinker?		Frequent drinker?	
		Yes	No	Yes	No
Sample size	1,107	105	1,002	65	1,042
Mother age 31	.014	.000	.016	.000	.015
Mother age 32	.038	.057	.036	.031	.038
Mother age 33	.083	.114	.080	.077	.083
Mother age 34	.120	.143	.118	.138	.119
Mother age 35	.156	.181	.154	.185	.155
Mother age 36	.154	.152	.154	.154	.154
Mother age 37	.139	.190	.134	.246	.132
Mother age 38	.117	.038	.125	.031	.122
Mother age 39	.081	.095	.080	.092	.081
Mother age 40	.062	.029	.066	.046	.063
Mother age 41	.029	.000	.032	.000	.031
CES-D depression score: 90th percentile	.115	.162	.110	.185	.110
Behavior Problems Index: 90th percentile	.220	.257	.216	.308	.214
Peabody Picture Vocabulary Test score	36.1	36.5	36.0	38.6	35.9

Unless otherwise specified, variables correspond to the interview that occurred when the respondent was 15 or 16 years old. All variables other than PPVT and the number of other household members age 17 and under are binary indicators. In the full sample, PPVT ranges from 0 to 99 with a standard deviation of 25.9, while the number of other household members under the age of 18 ranges from 0 to 12 with a standard deviation of 1.59. Omitted categories for indicators constructed from categorical measures are 7th grade, 19 years old in 2002, non-black and non-Latino, northeast, income not reported, mother age 29 or 30 or age of mother not reported, mother did not graduate from high school, and mother is not in the labor force.

Table 2: Effects of frequent and binge drinking on subsequent dropout

Drinking variable:	Binge drinker			Frequent drinker		
Explanatory variable set:	Basic	Expand	Full	Basic	Expand	Full
First stage drinking equation						
Any alcohol use	.520	.513	.515	.323	.324	.324
	(.035)	(.035)	(.035)	(.032)	(.033)	(.033)
F statistic (instrument)	224	220	222	99.7	98.7	98.7
Second stage drinking effect						
OLS	.136	.101	.094	.134	.116	.109
	(.043)	(.044)	(.044)	(.053)	(.052)	(.051)
2SLS	.198	.156	.146	.318	.246	.233
	(.058)	(.059)	(.058)	(.097)	(.094)	(.093)
Exogeneity *t*-statistic	1.54	1.41	1.38	2.26	1.66	1.60
2SLS dropout equation coefficients						
CES-D depression score:			.074			.071
90^{th} percentile			(.038)			(.038)
Behavior Problems Index:			.022			.017
90^{th} percentile			(.027)			(.027)
Peabody Picture Vocabulary			–.0015			–.0016
Test score			(.0004)			(.0004)

The first stage equation is an OLS regression of the drinking variable in the corresponding column heading on an indicator for any past month alcohol use and the explanatory variables listed in Table 1 starting with the grade indicators (and including the last four variables listed above for the expanded set). The second stage equation is the regression of having dropped out without finishing high school on the corresponding drinking variable and the aforementioned explanatory variables. The exogeneity statistic is the difference in the IV and OLS coefficients divided by the square root of the difference in the IV and OLS coefficient variances. Parentheses contain standard errors that are robust to arbitrary forms of heteroskedasticity and allow observations from siblings to be correlated.

Table 3: Dropout effects using alternative instrument sets

Drinking variable:	Binge drinker			Frequent drinker		
Any alcohol use included as IV:	Yes	Yes	No	Yes	Yes	No
Explanatory variable set:	Basic	Full	Full	Basic	Full	Full
First stage drinking equation						
Any alcohol use	.514	.512		.319	.320	
	(.035)	(.035)		(.032)	(.033)	
Mother drank weekly as an adolescent	.023	.026	.083	.033	.046	.081
	(.023)	(.024)	(.034)	(.023)	(.024)	(.029)
Mother binge drank as a young adult	.043	.035	.048	.004	.004	.012
	(.019)	(.020)	(.028)	(.018)	(.019)	(.021)
F statistic (instruments)	79.2	77.9	4.61	33.9	34.1	4.11
Second stage drinking effect						
2SLS	.202	.147	.222	.328	.236	.331
	(.058)	(.058)	(.359)	(.097)	(.092)	(.451)
Overidentification test statistic	1.55	.297	.254	1.31	.097	.050
	[.460]	[.862]	[.614]	[.519]	[.953]	[.824]

The first stage equation is an OLS regression of the drinking variable in the corresponding column heading on the instruments listed in the first three rows (but excluding any alcohol use in the second and fourth columns) and the explanatory variables listed in Table 1 starting with the grade indicators. The second stage equation is the two stage least squares regression of having dropped out without finishing high school on the corresponding drinking variable and the aforementioned explanatory variables. The overidentification statistic is Hansen's *J* statistic, i.e. the minimized value of the corresponding method of moments objective function multiplied by the sample size, with p-values reported in brackets beneath. Parentheses contain standard errors that are robust to arbitrary forms of heteroskedasticity and allow observations from siblings to be correlated.

Table 4: Dropout effects using various subsamples and specifications

Drinking variable:	Binge drinker		Frequent drinker		Sample
Estimation method:	OLS	2SLS	OLS	2SLS	size
Full sample	.094	.146	.109	.233	1,107
	(.044)	(.058)	(.051)	(.093)	
9^{th} – 11^{th} graders	.091	.142	.104	.225	992
	(.045)	(.059)	(.054)	(.095)	
9^{th} – 11^{th} graders	–.076	–.159	–.069	–.251	992
(Dep. var. = HS graduation)	(.044)	(.064)	(.054)	(.102)	
Latinos and blacks	.176	.188	.077	.279	610
	(.073)	(.096)	(.080)	(.151)	
Non-Latino, non-black	.040	.123	.139	.206	497
	(.057)	(.070)	(.071)	(.115)	
Females	.065	.111	.036	.188	573
	(.060)	(.079)	(.068)	(.137)	
Males	.132	.183	.173	.277	534
	(.060)	(.079)	(.076)	(.121)	
Urban residents	.077	.119	.088	.189	780
	(.056)	(.073)	(.063)	(.116)	
Non-urban residents	.157	.244	.198	.384	320
	(.076)	(.099)	(.095)	(.157)	
Mom is high school graduate	.080	.161	.097	.222	768
	(.050)	(.071)	(.061)	(.098)	
Mom is not high school graduate	.132	.172	.195	.362	331
	(.089)	(.100)	(.108)	(.217)	
Mom is married	.098	.122	.076	.188	621
	(.057)	(.073)	(.066)	(.113)	
Mom is non-married	.132	.232	.160	.374	482
	(.066)	(.091)	(.078)	(.151)	

Entries represent coefficients of the drinking variable in the column heading in least squares regressions of having dropped out without finishing high school that also include the explanatory variables listed in Table 1 starting with the grade indicators, with the sample defined as indicated in the row heading. 2SLS estimates are from two stage least squares regressions identified by an indicator of any past month alcohol use. Parentheses contain standard errors that are robust to arbitrary forms of heteroskedasticity and allow observations from siblings to be correlated.