Teen Drinking and Educational Attainment: Evidence from Two-Sample Instrumental Variables Estimates

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This study examines the effects of teen alcohol use and availability on educational attainment. We demonstrate that teens who faced a lower minimum legal drinking age (MLDA) were substantially more likely to drink. However, we find that changes in MLDA had small and statistically insignificant effects on educational attainment. Using matched cohorts from two data sets, we also report two-sample instrumental variables estimates of the effect of teen drinking on educational attainment. These estimates are smaller than the corresponding ordinary least squares estimates and statistically insignificant, indicating that teen drinking does not have an independent effect on educational attainment.

I. Introduction

The abuse of alcohol is widely recognized as a major social problem with important health consequences for consumers and those around them.[1] Because the habit of abusing alcohol may be developed early and have significant implications for life-cycle decisions, much of the research on alcohol consumption has focused on the behavior of teens.[2] One widely cited conclusion of this literature is that the youthful consumption of alcohol inhibits the accumulation of schooling (Mullahy and Sindelar 1989; Cook and Moore 1993, 1999; Yamada, Kendix, and Yamada 1996). Based, in part, on this conclusion, several authors have recommended policies that reduce alcohol availability through higher taxes (Grossman, Chaloupka, et al. 1993; Grossman, Sindelar, et al. 1993; Cook and Moore 1994, 1999).[3] However, there is reason to question whether these recommendations have sound empirical support. Some of the prior research, for example, has assumed that the decision to drink is made independently of schooling decisions.[4] Furthermore, as we demonstrate in this study, evaluations that have recognized the potential endogeneity of these decisions may be misspecified since they rely solely on the cross-state variation in excise taxes on beer and minimum legal drinking ages (MLDA) as exogenous determinants of teen drinking.

The first section of this article uses the National Education Longitudinal Study of 1988 (NELS-88) to establish an empirical baseline: teens who drink are less likely to complete high school and less likely to enter college. We also present results that promote the suspicion that the drinking-schooling relationship reflects correlation rather than causation. Specifically, in a sample of high school seniors who did not drink as sophomores, eighth- and tenth-grade test scores are lower among those who drank in twelfth grade, suggesting that students who are low academic achievers in their early teen years are more likely to drink heavily as seniors.

These results suggest that models of educational attainment that treat

[1] Most notably, alcohol use is strongly implicated in the leading cause of mortality among young adults: traffic fatalities (National Highway Traffic Safety Administration 2000). However, the use and availability of alcohol has been linked to other outcomes such as liver cirrhosis, smoking participation, teen childbearing, fetal health, crime, earnings, and marriage (Grossman, Sindelar, et al. 1993; Cook and Moore 1994; Kenkel and Ribar 1994; Dee 1999a, 2001; National Institute on Drug Abuse 1999).

[2] Teens also have higher rates of alcohol abuse and are involved in a disproportionate number of traffic accidents (Grant et al. 1991; National Institute on Alcohol Abuse and Alcoholism 1996).

[3] Though all states now have an MLDA of 21, the issue of whether this requirement should be changed has also been under consideration in several states (e.g., "Louisiana Stands Alone," 1996; "Va.'s Cullen Urges Look," 1997).

[4] However, the problems of establishing causality in schooling/health relationships have been recognized by other researchers (e.g., Kenkel 1991).

teen drinking as exogenous may overstate the true effect of teen drinking. Recognizing this, Cook and Moore (1993) utilized the changes in drinking generated by the cross-state variation in the MLDA and beer taxes to identify the causal effects of teen alcohol consumption on educational attainment.[5] They concluded that teen drinking significantly reduces educational attainment. Their primary evidence consisted of reduced-form estimates of the effect of beer taxes on school persistence. However, this identification strategy may be misleading since the instruments represent not only the availability of alcohol but also the unobserved state attributes that influence teen drinking and educational attainment (Dee 1999*b*).

A more convincing identification strategy would condition on unobserved state attributes and rely on the within-state variation in alcohol availability over time. To this end, we have used the increases in MLDA during the 1980s as an exogenous determinant of teen drinking. In 1977, 30 states had an MLDA of 18. By 1989, largely because of federal pressure, all states had raised their MLDA to 21. Using data on teen drinking from the 1977–92 Monitoring the Future (MTF) surveys, we demonstrate that teens who faced an MLDA of 18 were substantially more likely to drink than teens who faced a higher drinking age.[6] However, models that exploit the within-state variation in beer taxes over time suggest that this policy instrument has had no detectable effect on teen drinking (Dee 1999*b*).

If teen drinking did have an independent effect on human capital accumulation, then educational attainment within states should have risen after the MLDAs were increased. We test this hypothesis using data on over 1.3 million respondents who belonged to the 1960–69 birth cohorts in the Census Bureau's 1990 5% Public-Use Microdata Sample (PUMS). We find that teen exposure to an MLDA of 18 had small and statistically insignificant effects on indicators for high school completion, college entrance, and college persistence. Using the two-sample instrumental variables (TSIV) procedure developed by Angrist and Krueger (1992, 1995), we combine the first-stage and reduced-form results to generate estimates of the effect of teen drinking on educational attainment. These TSIV estimates are smaller than the corresponding ordinary least squares (OLS) estimates and are statistically insignificant, indicating that teen drinking does not have an independent effect on educational attainment. The final section discusses the policy implications of these results.

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[5] In fact, most of the literature addressing the policy determinants of teen drinking has relied on the cross-state variation in availability (Grossman et al. 1987; Coate and Grossman 1988; Grossman, Chaloupka, et al. 1993; Grossman, Sindelar, et al. 1993; Kenkel 1993; Cook and Moore 1994).

[6] We also discuss some evidence that supports the maintained assumption that the variation in MLDA is independent of trends in teen drinking.

II. An Empirical Baseline

The consumption of alcohol could reduce an individual's educational attainment through several mechanisms. Abusing alcohol may inhibit the ability and opportunity to learn as well as increase exposure to activities that have severe consequences such as drunk driving (see, e.g., National Highway Traffic Safety Administration 2000; Dee and Evans 2001), accidents, violence (see, e.g., Markowitz 2000), sexually transmitted diseases (see, e.g., Chesson, Harrison, and Kessler 2000), and teen childbearing (see, e.g., Dee 2001). Furthermore, available options for future schooling may be curtailed through an effect on current academic performance and through the development of a habit with negative implications for future achievement. This section sets a baseline for discussing whether these effects exist by estimating the probability that teen drinkers complete high school and go on to college. We also present some suggestive evidence that these correlations may be subject to an omitted variable bias.

These evaluations are based on the National Center for Education Statistics' (NCES) NELS-88. This survey began with a nationally representative sample of eighth graders in 1988. The base-year sample was constructed in two stages. The first stage produced a sample of 1,052 grade schools, and in the second stage a random sample of students from within these schools were selected. Nearly 25,000 students were interviewed in the base year. Follow-up interviews occurred in 1990, 1992, and 1994. The "core" sample of respondents for the follow-ups consisted of a stratified, random sample of base-year respondents. However, the sample was also "freshened" with new respondents so that nationally representative cross-sections of tenth graders in 1990 and twelfth graders in 1992 could be constructed.

We have defined high school completion and college entrance for the NELS-88 respondents by using responses to the third follow-up survey that occurred in 1994. In order to make these estimates as consistent as possible with the restrictions imposed by the other data sets we will use in later sections, we have included among high school completers those who have earned equivalency degrees.[7] Furthermore, we have, for the same reason, restricted our samples to include only black, white, and Hispanic respondents. We have also defined college entrants as those whose highest postsecondary status in 1994 involved working toward a bachelor's degree.

During the first two follow-ups in 1990 and 1992, NELS-88 respon-

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[7] The 1990 PUMS employs a similar definition of high school completion. There is some evidence that this construction is inappropriate since equivalency degrees may be poor substitutes for graduating on time (Cameron and Heckman 1993). The correlation of drinking with finishing high school on time is somewhat stronger than that suggested by this construction.

dents were asked about the frequency and the quantity of their alcohol consumption. As in other empirical research on teen drinking, we have defined a drinker as a teen who reports having had at least one drink in the last month. A heavy drinker reports having had five or more drinks in a row at least once in the last 2 weeks. In 1990, 42% of NELS-88 tenth graders had a drink within the last month, and 23.6% had drunk heavily within the past 2 weeks. Among NELS-88 twelfth graders in 1992, 52.1% had a drink within the past month, while about 28.8% had drunk heavily.[8] While there is concern that such self-reported consumption may understate actual use, the levels of drinking reported in the NELS-88 data are quite consistent with the contemporaneous data from other widely used surveys.[9] Furthermore, validation studies based on longitudinal surveys like MTF indicate that adult recalls about teen substance use are highly consistent with the responses as teens, which suggests the reliability of the self-reported data (e.g. O'Malley, Bachman, and Johnston 1983). There is also evidence that youths are less likely to underreport substance use in school-based surveys such as NELS-88 and MTF than in other household-based surveys (e.g. Gfroerer, Wright, and Kopstein 1997).

Using this information on self-reported teen drinking and subsequent educational attainment, we generated stylized evidence on the partial correlations between teen alcohol use and schooling decisions. More specifically, using the 1990 tenth graders, we report the "effects" of sophomore drinking on the probability of completing high school and on the probability of entering college. Using the 1992 twelfth graders, we also report the "effect" of senior drinking on the probability of entering college. Table 1 reports the estimated coefficients on the drinking variables in linear probability models where the outcomes of interest are the discrete variables high school completion and college entrance.[10] The first two columns report estimates for high school completion models for the sophomore class, while the next four columns report estimates from college entrance models for the sophomore and senior classes, respectively. As we move down the table, we include additional controls for family income, family structure, and parental education, plus school fixed-effects.[11] These

[8] These means may not be nationally representative because we do not use sample weights. Our econometric work also does not use sample weights.

[9] For example, in the 1992 MTF data that are presented later, 50.2% of high school seniors report being drinkers; 26.7% report being heavy drinkers.

[10] We report linear probability estimates instead of probit models to hold constant the estimation procedure when we compare OLS and two-sample instrumental variable estimates. In the latter models, we estimate linear regression models in order to construct the instrumental variables estimates. The results of probit models, however, are quite similar to those reported in table 1.

[11] These parent-reported family traits were represented in an unrestrictive manner by 15 dummy variables for family income, six for family structure, and four for parental education.

Table 1
Linear Probability Estimates of Teen Drinking on Educational Attainment, NELS-88, Parameter Estimates and Standard Errors

| Covariates | High School Completion | | College Entrance | | | |
| | | | Sophomore Year | | Senior Year | |
	Drinker (1)	Heavy Drinker (2)	Drinker (3)	Heavy Drinker (4)	Drinker (5)	Heavy Drinker (6)
Indicators for age, race, and gender	−.034	−.056	−.074	−.127	−.030	−.076
	(.0048)	(.0063)	(.0096)	(.0101)	(.010)	(.0108)
Previous model and indicators for family income and composition	−.033	−.054	−.072	−.1156	−.043	−.079
	(.0048)	(.0062)	(.0092)	(.0097)	(.0097)	(.0103)
Previous model and indicators for parental education	−.033	−.053	−.067	−.1032	−.045	−.070
	(.0047)	(.0062)	(.0090)	(.0094)	(.0094)	(.0099)
Previous model and school fixed effects	−.035	−.051	−.078	−.114	−.063	−.088
	(.0050)	(.0067)	(.0097)	(.0101)	(.0103)	(.0110)
Observations	9,946	10,833	9,913	10,849	9,439	9,895
Sample mean of education variable	.941	.941	.367	.363	.397	.396
Sample mean of drinking variable	.420	.236	.419	.236	.521	.288

NOTE.—NELS-88 = National Education Longitudinal Study of 1988. Heteroscedastic-consistent standard errors are reported in parentheses. Drinker is a discrete variable that equals one for students who report having had a drink in the past month. A heavy drinker is someone who reports having had five or more drinks in a row within the past 2 weeks.

results indicate that students who reported drinking during their soph-
omore year were about 3.5 percentage points less likely to complete high
school and 7.8 percentage points less likely to enter college. Sophomores
who drank heavily were over 5 percentage points less likely to complete
high school and roughly 11 percentage points less likely to enter college.
Seniors who drank were 3–6 percentage points less likely to enter college;
heavy drinkers were 7–9 percentage points less likely to enter college. All
of these estimated effects are statistically significant.

There is, however, reason to be concerned about whether the results
in table 1 represent a causal relationship. A central insight of the human
capital model (Becker 1964) is that the individual decision to acquire
schooling is an investment in future earnings potential. Accordingly, the
decision to acquire human capital should reflect the personal costs of
schooling as well as the discounted expected future benefits. Other things
being equal, students who find schooling unpleasant or who place little
value on the future earnings are more likely to drop out. Similarly, such
students may be more likely to engage in behaviors that might inhibit
their education or have adverse health consequences later in life. Because
these decisions are made simultaneously, OLS estimates like those pre-
sented in the previous section may overestimate the true effect of teen
drinking on schooling outcomes.[12]

We can construct some direct evidence on whether drinking and school-
ing are jointly dependent by exploring the timing of the teen decision to
drink. If teen drinking were truly independent of student achievement,
then the decision to drink as a senior should be unrelated to prior achieve-
ment in the eighth and tenth grades. This hypothesis can be directly tested
with the NELS-88 data by estimating the "effect" of twelfth-grade drink-
ing in eighth- and tenth-grade test-score equations. However, the power
of this test is attenuated by the extent to which twelfth-grade drinking
is serially correlated with drinking that occurred in the eighth and tenth
grades. Therefore, this hypothesis was also tested among samples of stu-
dents who abstained from drinking and heavy drinking as sophomores.[13]

As eighth and tenth graders, NELS-88 respondents took tests in four
subject areas: reading, mathematics, science, and history. The standardized
scores on these four tests have been aggregated into eighth- and tenth-

[12] This specification issue could alternatively be framed as a concern over unob-
served individual heterogeneity. Either formulation suggests that inferences based
on OLS estimates may falsely suggest that teen drinking reduces attainment. It
should be noted that it is also possible that the results in table 1 understate the
true effect of teen drinking on schooling. For example, this could occur if teen
drinking and dropping out of school constitute substitutable forms of teen
rebellion.
[13] Questions about alcohol use were not asked of the eighth-grade respondents
to NELS-88.

Table 2
OLS Estimates: The "Effect" of Twelfth-Grade Drinking on Eighth- and Tenth-Grade Test Scores, NELS-88

Sample Selection by Sophomore Drinking	Mean Test Score	Model 1		Model 2		No. of Obs.
		Drinker	Heavy Drinker	Drinker	Heavy Drinker	
Eighth grade–senior panel:						
All students	211	−2.7	−9.7	−3.9	−9.3	7,317
		(.8)	(.8)	(.8)	(.8)	
Not a drinker	212	−1.9	−10.5	−2.8	−10.0	4,237
		(1.0)	(1.3)	(1.1)	(1.4)	
Not a heavy drinker	214	−.5	−8.6	−2.1	−8.5	5,645
		(.9)	(1.0)	(.9)	(1.0)	
Sophomore–senior panel:						
All students	210	−3.2	−10.1	−4.6	−9.8	7,466
		(.7)	(.8)	(.8)	(.8)	
Not a drinker	212	−2.1	−10.0	−2.3	−9.7	4,334
		(1.0)	(1.3)	(1.1)	(1.4)	
Not a heavy drinker	213	−.7	−8.1	−2.0	−8.2	5,749
		(.8)	(1.0)	(.9)	(1.0)	

NOTE.—NELS-88 = National Education Longitudinal Study of 1988. Heteroscedastic-consistent standard errors are reported in parentheses. Model 1 includes indicators for race, gender, ethnicity, and year of birth. Model 2 adds to model 1 school fixed effects and indicators for family composition, parental education, and family income.

grade test scores for each student.[14] Our samples of over 7,500 NELS-88 respondents consist of students who were enrolled in tenth grade in 1990, in twelfth grade in 1992, who answered both drinking questions in 1990 and 1992, and who took all four tests in either the base year or the first follow-up. Dropouts were omitted because their pattern of alcohol consumption is likely to differ greatly from that of enrolled students. This selection is not likely to generate any problematic bias for these stylized evaluations since dropouts would tend to have lower test scores and a higher prevalence of drinking.

The "effects" of twelfth-grade drinking on prior achievement are reported in table 2. Drinking as a high school senior is always associated with lower levels of prior achievement, even when other determinants of student achievement are included as covariates. For example, drinking heavily as a senior implies an eighth-grade test score that is roughly 10 points lower. This statistically significant reduction constitutes nearly 5% of the mean test score and persists even when students who drank as sophomores are excluded from the sample. The results for more moderate twelfth-grade drinking and for the tenth-grade test scores demonstrate a similar pattern: students who are doing poorly in school are more likely

[14] The tenth-grade test score data should be interpreted with caution since student performance on the eighth-grade test influenced the difficulty level of the later NELS-88 tests.

to drink later in high school. The timing and correlation of these decisions raises serious doubts about the frequently cited conclusion that teen drinking causes students to do poorly in school and about the appropriateness of empirical designs that assume that teen drinking is determined independently of student achievement.

III. Identifying Causal Effects

Although teens who drink are less likely to finish high school or start college, the evaluations from table 2 have raised the critical concern that teen drinking may proxy for some omitted factors in the educational attainment equations. Generating unbiased estimates of the effect of teen drinking on educational attainment requires an exogenous source of variation in teen drinking. Following the work of Cook and Moore (1993), we exploit the variation in teen alcohol use generated by relevant state policies. There are two policy instruments that we can potentially use in this manner: the MLDAs and state excise taxes on beer.

In 1977, 30 states had an MLDA of 18. By 1989, all states had raised their MLDA to 21. The rapid change in state policy was precipitated in part by passage of the Danforth-Lautenberg Act (PL98-363), signed on July 17, 1984, which required the secretary of transportation to withhold some federal highway funds from states that did not enact an MLDA of 21. In contrast to previous work, we examine the impact of the MLDA on drinking using a within-group estimator where we effectively compare the difference in drinking among high school seniors before and after hikes in the MLDA to the contemporaneous changes among teens in states with no reform. The need for a within-group model is strongly suggested by the tremendous persistence in teen drinking and high school graduation rates across states over time. If states with low graduation rates had low MLDAs, a model that only examines cross-state variation would overstate the reduced-form effect of alcohol availability and educational attainment. These potential correlations can be eliminated from the data by using panels of repeated cross-sections that allow us to include a compete set of state and year fixed effects in the model. However, the validity of this identification strategy also requires that the within-state movement away from an MLDA of 18 was independent of teen drinking (Besley and Case 1994; Meyer 1995). The fact that changes in the MLDA were often compelled by federal law suggests that these changes were exogenous. Additionally, in the next section, we also discuss some empirical evidence that indicates that the timing of MLDA changes was independent of state-specific trends in teen drinking. Because beer is the drink of choice for most teens, within-state changes in beer taxes may also provide the necessary variation in alcohol consumption to identify the impact of drinking on educational attainment. However, as we illus-

trate in the next section, once we restrict our attention to models with state fixed effects, excise taxes on beer appear to have no statistically significant impact on teen alcohol consumption (Dee 1999*b*).

Our use of within-state variation in alcohol availability differs from the work of Cook and Moore (1993) who used the cross-state variation in MLDA and excise taxes on beer to identify the effect of teen drinking on educational attainment. Their primary evidence of a link between drinking and attainment are the results from reduced-form evaluations that indicate there is a positive correlation between residing in a state with a more restrictive drinking environment and subsequent educational attainment. There is reason to be concerned that an identification strategy based on the cross-state variation in such policies may be a poor one. One immediate source of concern is that the magnitudes of the reported effects are implausibly large.[15] More generally, the potential difficulty with state effects in conventional cross-sectional evaluations is that they may be biased by unobserved and state-specific determinants of risky youth behaviors and alcohol availability (e.g., cultural sentiment). To illustrate this concern, we used the cross-sectional NELS-88 data presented in the previous section to estimate reduced-form models for educational attainment similar to those in Cook and Moore (1993). However, instead of including state laws concerning alcohol availability, we added indicators for state laws that should have no impact on educational attainment.[16] These laws include the state excise taxes on cigarettes and gasoline, whether the state had a death penalty, a waiting period for gun purchases and a 65-mile-per-hour (MPH) speed limit.[17] We estimated the "effect" of these state policies on college entrance in probit models that included demographic covariates as well as the indicators that reflect family income, family composition, and parental education. In all cases, the state policies correlated significantly with college entrance even though no plausible causal relationship necessarily exists. For example, our evaluations suggest that increases in cigarette taxes reduce the probability of entering college, while higher gas taxes increase the probability (Dee and Evans 1997). Furthermore, sophomores from states with a waiting period for gun pur-

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[15] This criticism is discussed in detail below.

[16] For confidentiality reasons, the public-use version of the NELS-88 data set does not contain state codes. However, through an agreement with the Department of Education, we were able to match NELS-88 respondents to the state in which they attended school in 1990.

[17] The data on the level of beer taxes has been drawn from the Distilled Spirits Industry Council of the United States (DISCUS 1996*a*) and has been converted to real terms using the CPI (1982–84 = 1). Data on gas and cigarette taxes and the death penalty are from the 1990–91 *Book of the States*. Data on speed limits are from the *Statistical Abstract of the United States*. Data on waiting periods for gun purchases are from the *National Survey of State Laws*.

chases are 4.2 percentage points more likely to enter college, students from states with a death penalty are 5.4 percentage points less likely to enter college, and a 65-MPH speed limit implies a reduction of 8.8 percentage points in the likelihood of entering college. All of these estimates are statistically significant. The spurious cross-sectional correlation between some state policies and the level of attainment raises doubt about this widely employed identification strategy.

This study exploits the within-state variation in alcohol availability to identify the effects of teen drinking on educational attainment. However, a traditional instrumental variables (IV) estimator that adopts this approach would require that we have repeated cross-sections that contain data both on teens' drinking and their subsequent schooling decisions. Because this would imply either following cohorts of teens through their early 20s or surveying individuals in their 20s and asking retrospective questions about teen drinking, no large-scale nationally representative survey has all the necessary information. To circumvent the lack of data, we have relied on a new technique pioneered by Angrist and Krueger (1992, 1995) that will allow us to generate instrumental variables estimates using the cohort-specific information in two data sets.

A. Specifications

To illustrate how TSIV estimates are generated, consider the following structural equation of educational attainment:

$$E_{ist} = W_{ist}\pi + D_{ist}\gamma + u_s + v_t + \varepsilon_{ist}, \tag{1}$$

where is E_{ist} is an indicator for the education obtained by person i from state s and birth cohort t; W_{ist} is a vector of exogenous individual characteristics; u_s and v_t are state and cohort fixed effects; and ε_{ist} is a mean-zero random error. The potentially endogenous covariate of interest is an indicator for teen drinking, D_{ist}. The instrumental variable for D_{ist} will be an indicator, M_{st}, for whether a teen in a particular state and year cohort was exposed to an MLDA of 18. Therefore, the model is exactly identified.

Most data sets that measure teen drinking do not follow these individuals over time and record their ultimate level of education. As a result, we typically do not have E, D, and M in the same data set.[18] However, the TSIV procedure requires only one data set with data on E and M and a second data set with data on D and M for the same cohorts. Our first-stage data set, which has information on teen drinking and MLDA ex-

[18] If all the necessary data were available in one data set, the appropriate specification would be a bivariate probit. However, there is evidence that linear IV estimation is a viable alternative to the bivariate probit model (Angrist 1991). Furthermore, probit estimation of the first-stage and reduced-form equations generate results similar to those reported for these linear specifications.

posure, is based on pooled cross-sections from the MTF surveys. Our second data set, which has information on educational attainment and teen MLDA exposure, is based on the Census Bureau's 5% 1990 PUMS. These data sets are described in more detail in the next two sections.

The first step to calculating the TSIV estimate of γ is to fit the models for alcohol use with the MTF data and to use those parameter estimates to predict the drinking behavior of the contemporaneous PUMS respondents. Then, the TSIV estimate of γ is generated by a regression of the educational outcomes of the PUMS respondents on the cross-sample fitted value for their alcohol use (Dee and Evans 1997). However, since this model is just identified, the TSIV estimate of γ is also fully implied by the reduced-form evaluations with these two data sets. More specifically, from the MTF data set, we can obtain an estimate of the first-stage relationship between teen drinking and alcohol availability by estimating the equation:

$$D_{ist} = W_{ist}\pi_1 + M_{ist}\gamma_{1t} + u_{1s} + v_{1t} + \varepsilon_{1ist}. \qquad (2)$$

From the PUMS data set, we can obtain an estimate of the reduced-form relationship between educational attainment and the instrumental variable by estimating the equation:

$$E_{ist} = W_{ist}\pi_2 + M_{st}\gamma_2 + u_{2s} + v_{2t} + \varepsilon_{2ist}. \qquad (3)$$

Because our model is exactly identified, it is straightforward to show that the TSIV estimate of γ is equivalent to the ratio of the reduced-form and first-stage estimates:

$$\hat{\gamma}_{TSIV} = \hat{\gamma}_2/\hat{\gamma}_1. \qquad (4)$$

This expression for the TSIV estimate of γ proves useful for evaluating the plausibility of prior estimates of the effect of teen drinking on educational attainment and for placing bounds on the possible impact of state alcohol policies on schooling decisions.

B. Sample Size

Another important specification issue concerns the appropriate sample size that will allow us to construct meaningful inferences about the relationships among teen drinking, a state's MLDA, and educational attainment within that state. In order to address this question, it is useful to identify the likely magnitude of the reduced-form relationship between a teen MLDA of 18 and attainment if it were the case that the OLS estimates of the effect of drinking on attainment were unbiased. Consider the case of heavy teen drinking. In the next section, we will demonstrate that teen exposure to an MLDA of 18 increased heavy drinking among students by a statistically significant 3.2 percentage points. The OLS es-

timates in table 1 indicated that heavy drinkers in tenth grade were 5.6 percentage points less likely to complete high school. If that were the true effect, we would expect teen exposure to an MLDA of 18 to reduce the probability of completing high school by only 0.18 percentage points (.056 × .032). Using heavy drinking in the twelfth grade, if the OLS estimate were true, we would expect exposure to an MLDA of 18 to reduce the probability of college entrance by 0.24 percentage points (.076 × .032). The precise estimation of such small effects is likely to require a large data set.

To illustrate more carefully the necessity of having a large sample, consider the following, bivariate regression model:

$$y_i = \alpha + \beta d_i + \varepsilon_i, \tag{5}$$

where y_i is an indicator that equals one if a student entered college and d_i is an indicator for whether the student was a heavy drinker in high school. Let z_i denote the binary instrument for the respondent's teen exposure to an MLDA of 18. The IV or Wald (1940) estimate of β in this equation is

$$\hat{\beta}_{IV} = \frac{(\bar{y}|z_i = 1) - (\bar{y}|z_i = 0)}{(\bar{d}|z_i = 1) - (\bar{d}|z_i = 0)}, \tag{6}$$

where $(\bar{y}|z_i = 1)$ is the mean of y_i for those observations with $z_i = 1$ and other terms are similarly defined. The numerator and denominator capture the reduced-form relationships between y_i and z_i and between d_i and z_i. Consider the case where OLS estimates of this simple model generate unbiased estimates. Given these assumptions, it must be that a reduced-form regression of y on z would generate the following estimate:

$$\hat{\beta}_{RF} = (\bar{y}\,|\,z_i = 1) - (\bar{y}\,|\,z_i = 0) = -.0024. \tag{7}$$

This reduced-form estimate will only be statistically significant if:

$$|\frac{\hat{\beta}_{RF}}{\hat{\sigma}_{RF}}| \geq 1.96, \tag{8}$$

where σ_{RF} is the standard error of β_{RF}. Under the assumptions we have made, we can solve this expression for the minimum number of observations that would be necessary to make such an inference about the reduced-form relationship. Let $\hat{p}_1 = (\bar{y}|z_i = 1)$ and $\hat{p}_0 = (\bar{y}|z_i = 0)$, and suppose that there are n observations in both the treatment ($z_i = 1$) and the control ($z_i = 0$) groups. Therefore, σ_{RF}^2 approximately equals $[\hat{p}_1(1 - \hat{p}_1) + \hat{p}_0(1 - \hat{p}_0)]/n$. Since $\hat{p}_1 = \hat{p}_0 - .0024$, we can rewrite σ_{RF}^2 as $[2(\hat{p}_0 - \hat{p}_0^2) - .0024(1.0024 - 2\hat{p}_o)]/n$. For equation (8) to be true, it must be the case that $.0024/\{[2(\hat{p}_0 - \hat{p}_0^2) - .0024(1.0024 - 2\hat{p}_o)]/n\}^{1/2} \geq 1.96$.

Solving for n, we have that $n \geq (1.96/.0024)^2[2(\hat{p}_0 - \hat{p}_0^2) - .0024(1.0024 - 2\hat{p}_0)]$. Given a college entrance rate of 48% among the PUMS respondents, we can set $\hat{p}_0 = 0.48$. This calculation then implies that we would need over 330,000 observations in the treatment group and an equal number in the control group to generate a statistically significant reduced-form relationship between college entrance and a teen MLDA of 18. The reduced-form estimates we present in Section V are based on samples of over 1.3 million individuals, and the calculations presented here suggest that our samples are large enough to provide a fair test of the hypothesis that alcohol availability has influenced educational attainment.

The specification issues discussed in this section also provide a framework for evaluating the plausibility of Cook and Moore's (1993) widely cited conclusion that teen exposure to an MLDA of 20 or 21 increased the probability of completing college by 4.2 percentage points. One source of concern is that the estimate is implausibly large. If we were to make the very generous assumption that the same change in MLDA reduced drinking by 5 percentage points, the implied effect of teen drinking on college completion, using the Wald estimate, would be 84 percentage points (.042/.05). The implausibility of this implied IV estimate is another indication that an identification strategy based on cross-state heterogeneity can be problematic. Furthermore, because these estimates are based on fewer than 2,000 observations, the statistical significance of this estimate may be driven solely by its unusually large magnitude.

IV. The First-Stage: The Impact of MLDA on Teen Drinking

This section presents estimates of the policy determinants of teen drinking that are based on pooled cross-sections from the 1977–92 MTF surveys.

A. 1977–92 Monitoring the Future

The widely used MTF surveys, which have been organized by the Survey Research Center at the University of Michigan, were designed to identify changes in important youth behaviors and attitudes. In the spring of each year, a nationally representative cross-section of high school seniors have been asked about their drug and alcohol use. These samples have been constructed in three stages. The first stage consisted of selecting geographic areas. The basis for these selections were the primary sampling units (PSU) developed by the Survey Research Center for nationwide interviews. In the second stage, high schools within each PSU were chosen. The probability of selection for a school was proportional to the size of its senior class. In the final stage, up to 400 seniors in a selected school are included in the data collection. In small schools, all seniors were

usually interviewed. In larger schools, a sample of seniors was randomly selected. Each yearly survey has consisted of at least 15,000 respondents from roughly 130 schools.

For confidentiality reasons, the public-use MTF data do not identify the state in which the selected school is located. In order to match the teen drinking behavior reported in the MTF surveys to state alcohol policies, we have reached a special agreement with the Survey Research Center. As a condition of this agreement, we could only match MTF respondents to their states by accepting some limitations on the available demographic covariates. The data set we received identified the proportion of respondents satisfying three drinking definitions within a given state, year, race, and age cell and the number of observations within that cell. More specifically, responses with a given state and year were defined by gender, age (i.e., above or below the age of 18), and race/ethnicity (i.e., white non-Hispanics or not). This data set contains 3,941 cells representing the responses of 255,560 students in 44 states.[19] Since evaluations with these data replicate prior results, this modest aggregation does not appear to generate any bias.

This data set contains three distinct measures of teen drinking participation. As in the NELS-88 data, a drinker is a respondent who reports having had a drink in the last month. A heavy drinker has had five or more drinks in a row in the last 2 weeks. Additionally, this unique data set identifies "moderate drinkers": those who report having had 10 or more drinks within the past month. Each level of teen drinking has been characterized by a slow but steady decline over the 1977–92 period. However, this trend has reversed in recent years and the rates at which teens use and abuse alcohol are still among the highest of any segment of society (Grant et al. 1991; Johnston et al. 1999).

B. Alcohol Availability

The policy variables of interest in this literature have been those that affect the availability of alcohol: the MLDAs and taxes. Information on the history of alcohol taxation and MLDA in the states has been taken from two publications of DISCUS (1996*a*, 1996*b*). Since some changes in MLDA occurred midyear, the MLDA for a state in a given year is considered to be the one in effect for the largest proportion of that year. Like much of the prior research, we also focused on federal and state

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[19] Cells with fewer than five respondents were deleted by the Survey Research Center. The public use surveys over the 1977–92 period consisted of 271,012 respondents. Not all of the 44 states in this data set are represented in each survey year.

excise taxes on the drink of choice among teens, beer.[20] The beer taxes have been defined per gallon of beer and, where relevant, refer to the tax on beer greater than 3.2% alcohol by volume or sold in cases. The nominal taxes have been converted into real terms using the Consumer Price Index (1982–84 = 1).

Each of these policy instruments has exhibited considerable variation over this period. In 1977, 30 states had an MLDA of 18, and nearly 60% of the MTF respondents resided in these states. By the end of 1988, all states had raised their MLDA to 21. However, over this period, the amount of beer tax variation that can be understood as unique within-state innovations is actually quite limited. In general, the real value of beer taxes was declining over much of this period due to the shared effects of price inflation. The one exception to this time-series trend was the 1991 increase in the federal tax on beer. The relatively stable cross-state differences in beer taxes are quite large and have been exploited in prior studies (Grossman, Coate, and Arluck 1987; Coate and Grossman 1988; Grossman, Chaloupka, et al. 1993; Grossman, Sindelar, et al. 1993; Kenkel 1993; Cook and Moore 1994). However, an important concern raised here and elsewhere (Dee 1999*b*) is that this cross-sectional beer tax variation may be correlated with the unobserved state-specific determinants of youth behaviors. The unique within-state variation in beer taxes that is available to us is driven by state changes in their nominal excise taxes on beer. Of the 44 states represented within our MTF data set, 19 changed their beer taxes over periods in which students were interviewed. While this tax variation is relatively limited, Dee (1999*b*) shows that there is enough variation to demonstrate that the within-panel tax elasticities are smaller and statistically distinguishable from the conventional cross-sectional elasticities.

C. Results

The evaluations we report here are based on weighted OLS regressions in which the dependent variable is the proportion of students within a cell who satisfy a drinking definition and the weight is the number of observations per cell.[21] All of these models condition on the available demographic information and fixed effects for each of the annual survey cohorts. We present some results based on the full 1977–92 MTF data set in order to capture all of the relatively limited within-state variation in

[20] Some of the earlier research used price data. However, changes in the tax provide an independent source of variation in the price of alcohol and are less subject to measurement error. There is evidence that tax increases on alcohol are completely passed on to consumers in the form of higher prices (Cook 1981).

[21] This weighted linear specification utilizes the data as we received them and provides a plausible correction for heteroscedasticity. However, as the evaluations in Sec. V demonstrate, these results are quite robust to other specifications.

Table 3
Weighted Least Squares Estimates of the Determinants of Alcohol Use by Teens, Monitoring the Future, 1977–92: Parameter Estimates and Standard Errors

Independent Variable	With Cohort Fixed Effects				With State and Cohort Fixed Effects			
	1977–92 (1)	1977–92 (2)	1977–86 (3)	1977–86 (4)	1977–92 (5)	1977–92 (6)	1977–86 (7)	1977–86 (8)
Drinking:								
MLDA 18	.073	.041	.040	.034	.035	.035	.054	.038
	(.005)	(.005)	(.006)	(.005)	(.006)	(.006)	(.009)	(.006)
MLDA 19	.027	.009	.009	. . .	−.001	−.001	.019	. . .
	(.005)	(.005)	(.006)		(.006)	(.006)	(.008)	
MLDA 20	.072	.056	.056	. . .	.005	.005	.012	. . .
	(.010)	(.010)	(.010)		(.011)	(.011)	(.013)	
Real beer tax	−.184	. . .	. . .	. . .	−.001	. . .	. . .	. . .
	(.010)				(.030)			
Moderate drinking:								
MLDA 18	.033	.026	.026	.025	.020	.021	.035	.023
	(.003)	(.003)	(.003)	(.003)	(.004)	(.004)	(.006)	(.004)
MLDA 19	.004	−.001	−.0002	. . .	−.002	−.002	.015	. . .
	(.003)	(.003)	(.003)		(.004)	(.004)	(.005)	
MLDA 20	.021	.017	.016	. . .	−.005	−.005	−.0004	. . .
	(.006)	(.006)	(.006)		(.007)	(.007)	(.009)	
Real beer tax	−.044	. . .	. . .	. . .	.010	. . .	. . .	. . .
	(.006)				(.019)			
Heavy drinking:								
MLDA 18	.049	.028	.027	.025	.026	.027	.042	.032
	(.005)	(.005)	(.005)	(.005)	(.006)	(.006)	(.009)	(.006)
MLDA 19	.008	−.004	−.005	. . .	−.009	−.008	.011	. . .
	(.005)	(.005)	(.005)		(.006)	(.006)	(.008)	
MLDA 20	.064	.053	.051	. . .	.004	.004	.008	. . .
	(.009)	(.009)	(.009)		(.011)	(.011)	(.012)	
Real beer tax	−.124	. . .	. . .	. . .	.037	. . .	. . .	. . .
	(.009)				(.028)			

NOTE.—Drinking sample mean = .657; moderate drinking sample mean = .138; heavy drinking sample mean = .367. Standard errors are reported in parentheses. All models include binary indicators for gender, age, and race. The 1977–92 evaluations are based on 3,941 cells representing the responses of 255,560 Monitoring the Future (MTF) respondents. The 1977–86 evaluations are based on 2,511 cells representing the responses of 163,177 MTF respondents. The weight is the number of respondents per cell.

beer taxes. However, we also report results based only on the 1977–86 MTF data set, which has drinking data for those cohorts whose subsequent educational outcomes we observe in the 1990 PUMS data. In table 3, we present the key results from estimating the determinants of teen participation in each of the three drinking measures. The top panel has the results for drinking participation; the middle and bottom panels report the results for moderate and heavy drinking, respectively. The unreported coefficients on the demographic covariates generally indicate that older, white males are more likely to drink alcohol.

The first four models in table 3 parallel the prior literature by estimating the effects of the cross-state variation in alcohol policies on each drinking measure. These models uniformly indicate that exposure to an MLDA of 18 implies a significantly higher prevalence of each teen drinking behavior. Furthermore, model 1 also replicates the traditional result that students in states with high beer taxes are substantially less likely to drink. The implied tax elasticities of these teen drinking measures are quite large. For example, the implied elasticity of heavy teen drinking with respect to the beer tax is

−0.17 (−.124 × [.50/.367]). The magnitude of this elasticity is consistent with the findings of other research that has used the cross-state variation in taxes (Leung and Phelps 1993). However, one suggestive indication that these cross-sectional results should be viewed with some suspicion is the evidence that exposure to an MLDA of 20 was also associated with a significantly higher prevalence of these drinking behaviors.

We provide more definitive evidence on the relevance of these specification concerns in the last four models, which introduce state fixed effects that control for the unobserved, state-specific determinants of these drinking behaviors. The estimates from model 5 demonstrate that the frequently cited correlation between the cross-state variation in beer taxes and teen drinking does not appear to be robust. The within-state variation in beer taxes exhibits a small and statistically insignificant correlation with each teen drinking measure. Furthermore, model 3 indicates that students facing an MLDA of 19 or 20 were, in general, no more likely to consume alcohol than students facing an MLDA of 21.[22] However, the movement away from an MLDA of 18 did have a significant impact. In particular, model 8 indicates that students who faced an MLDA of 18 were 3.8 percentage points more likely to drink, 2.3 percentage points more likely to drink moderately, and 3.2 percentage points more likely to drink heavily.

The lack of a correlation between the within-state variation in beer taxes and teen drinking is straightforward evidence of the limitations of conventional identification strategies based on cross-state variation. However, one important concern with this unorthodox result is that it might simply be driven by the collinearity between the state effects and the beer taxes. As a check of this possibility, we have replicated the results from table 3 using only those respondents in the 19 states that exhibited within-state variation in their beer taxes. The results of those evaluations, which were based on the responses of 133,854 students, were quite similar.[23] Nonetheless, the fact that beer taxes have no statistically significant impact on teen drinking may appear to some to be completely inconsistent with most prior research. However, almost all prior demand estimates based on individual-level data for teens have relied solely on the cross-state variation in taxes to identify the parameters of interest. Recent studies that employ the within-state variation in beer taxes as the identifying

[22] The one exception is that an MLDA of 19 had a weakly significant effect on drinking participation. The irrelevance of higher MLDA is not entirely surprising in this context since only an MLDA of 18 makes alcohol available to some high school age students.

[23] Dee (1999*b*) presents additional evidence that the collinearity between beer taxes and state fixed effects is not problematic. For example, even in the models that include state fixed effects, the standard error on the estimated tax responsiveness of teen drinking is still small enough to reject the conventional estimate.

assumption generate results similar to those reported here (Dinardo and Lemieux 1996; Dee 1999*b*).

There are, however, numerous papers that examined the link between beer taxes and highway traffic fatalities using panel data and state fixed effects. In the majority of these papers, the authors find that beer taxes reduce alcohol-related traffic fatalities.[24] Two points about this body of research are worth noting. First, in a related paper, Dee (1999*b*) shows that the tax effect in teen auto fatality models is not robust to the inclusion of state-specific time trends.[25] Including state-specific time trends may be particularly important in this context since a large portion of the within-state changes in tax rates is due solely to inflation rather than changes in the nominal tax rates. Interestingly, the effect of the MLDA in these models is quite robust across specifications. Second, the estimates in these traffic studies are implausibly large. For example, Saffer and Grossman (1987) estimate that the elasticity of traffic fatalities for 18–20-year-olds with respect to beer tax is −0.17, but since beer taxes represent roughly 10% of the retail price, this implies a price elasticity of about −1.7 (−0.17/ .10). Since alcohol is a factor in about half of traffic safety fatalities, this suggests that the elasticity of alcohol-sensitive traffic fatalities with respect to price is about −3.4. If these conventional estimates are accurate, a mere 3% increase in the price of beer would reduce youth fatalities by more than the move to an MLDA of 21 has (Dee 1999*b*).

The empirical models presented here indicate that beer taxes do not provide a plausible instrument for teen drinking. However, the movement away from an MLDA of 18 does appear to provide a valid source of exogenous variation for identifying the welfare consequences of teen drinking. The evaluations reported in table 3 indicate that an MLDA of 18 had a large and statistically significant impact on all levels of teen drinking. An important concern with these evaluations is whether they identify the independent effect of MLDA changes on teen drinking. If the timing of a state's MLDA change were also a response to a change in teen drinking, the quality of our identification strategy would be in doubt (Besley and Case 1994; Meyer 1995). However, in addition to the available anecdotal evidence, there is some suggestive empirical evidence to buttress the assumption that the variation in state MLDA was independent of teen drinking. The national trends in all levels of teen drinking were quite stable in the period before the dramatic MLDA changes. This suggests that the movement away from an MLDA of 18 was not a response to increases in

[24] For an overview of the literature on youth traffic safety, see Dee and Evans (2001).

[25] The implausibility of the conventional tax results is more convincingly indicated by a counterfactual based on comparing models of nighttime fatalities to models of daytime fatalities that have substantially lower rates of alcohol involvement (Dee 1999*b*; Dee and Evans 2001).

teen drinking. More formally, the first-stage coefficients reported in table 3 are largely robust to the inclusion of both linear and quadratic state-specific trend variables, implying that the state-specific variation in teen drinking was not correlated with the timing of a state's MLDA change.[26] Also, the results presented in table 3 indicate that the correlation between an MLDA of 18 and each level of teen drinking is roughly the same regardless of whether state fixed effects are included. The weak relevance of the considerable cross-state heterogeneity in this instance suggests that within-state heterogeneity is unlikely to be problematic.

V. Reduced-Form and Two-Sample Instrumental Variables Estimates

The evaluations in the previous section demonstrated that the timing of a state's movement away from an MLDA of 18 had a significant impact on all levels of teen drinking. It follows that if teen drinking had an independent effect on schooling decisions, then changes in MLDA should have also had an effect on educational attainment. The evaluations presented in this section address this question directly by estimating the effect of teen exposure to an MLDA of 18 on high school completion, college entrance, and college completion.

A. 1990 Public-Use Microdata Sample

In Section III we demonstrated that, because the effect of teen exposure to an MLDA of 18 may be quite small, a precise estimate of its effect is likely to require a large number of observations. Therefore, we have used data from the Census Bureau's 1990 5% PUMS to estimate the impact on educational attainment of teen exposure to an MLDA of 18.[27] The 1990 PUMS consists of the more than 12 million individual respondents who received the long-form questionnaire in that census enumeration.

Our PUMS sample consists of white, black, and Hispanic respondents from the 1960–69 birth cohorts. These respondents ranged in age from 21 to 30 at the time of the 1990 interview. Their MLDA exposure at age 17 occurred during the 1977–86 period when many state MLDAs were being increased. Educational attainment for these respondents has been defined by binary indicators for high school completion, college entrance, and college persistence. In the PUMS data set, high school completion includes those who have earned equivalency degrees. College entrants

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[26] We have also regressed an indicator for an MLDA of 18 on state effects, cohort effects, and the level of teen drinking lagged by 2 and 3 years. The lack of a partial correlation between lagged teen drinking and a state's MLDA status is further evidence that the instrument is exogenous.

[27] We have replicated the high school completion results to be reported here with a sample of 19–21-year-olds from the 1981–92 October Current Population Survey (CPS). However, that data set only consisted of 67,361 respondents.

have been defined as those respondents who have completed "some college" or earned a bachelor's degree.[28] Because some of the younger respondents in this sample have not had a chance to have a "completed spell" of college completion, college persistence has been defined to include those who are still enrolled in a college program in addition to those who have earned a bachelor's degree.[29]

The members of this PUMS sample were matched to the MLDA in their state of birth when they were 17. Because some respondents dropped out of school before their MLDA exposure at age 17, only students who attained at least the eleventh grade are included. This construction allows the PUMS sample to roughly parallel the contemporaneous MTF sample whose first-stage assignments and drinking behavior we observe. However, it should be noted that these data limitations imply that we cannot speak directly to the drinking and education relationship for earlier dropouts. The final PUMS sample consists of 1,376,762 respondents. Matching these respondents to their teen MLDA by their states of birth does not appear to be problematic. Tabulations from the 1980 PUMS indicate that nearly 80% of teens resided in their state of birth. Furthermore, there is no reason to believe that the pattern of interstate childhood mobility for those who advanced past the tenth grade was correlated with changes in MLDA. A specification check based on constructing mobility-adjusted measures of teen MLDA exposure offers further evidence that this construction is not misleading (Dee and Evans 1997).

B. Reduced-Form Estimates

We estimated the effects of teen exposure to a relaxed drinking environment on educational attainment using linear probability models and heteroscedastic-consistent standard errors. However, similar results emerged from evaluations based on probit and logit specifications. The results of estimating the impact of an MLDA of 18 on high school completion, college entrance, and college persistence are reported in table 4. These models consistently demonstrate that non-Hispanic whites are significantly more likely to continue their schooling while males are less likely. The first three models reported in table 4 include cohort fixed effects but not state fixed effects. These estimates indicate that, even in the absence of controls for unobserved state heterogeneity, there is no evidence that teen exposure to an MLDA of 18 reduced subsequent educational attainment. In fact, these specifications indicate that the cross-

[28] This definition excludes those who earn an associate's degree. However, this construction does not substantively alter the pattern of the results.

[29] Again, this definition is not problematic. Similar results are obtained using cohorts with completed spells and defining college completion as having earned a bachelor's degree.

Table 4
Reduced-Form Linear Probability Models: The Determinants of Educational Attainment, 1990 PUMS, Ages 21–30

	With Cohort Fixed Effects			With State and Cohort Fixed Effects		
Covariates	High School Completion	College Entrance	College Persistence	High School Completion	College Entrance	College Persistence
MLDA of 18 at age 17	.00115	.00074	.00812	−.00099	.00198	.00095
	(.00051)	(.00099)	(.00090)	(.00081)	(.00156)	(.00144)
White non-Hispanic	.08597	.09702	.12377	.07937	.10118	.12288
	(.00084)	(.00121)	(.00102)	(.00086)	(.00127)	(.00107)
Male	−.01750	−.02631	−.01353	−.01763	−.02665	−.01385
	(.00044)	(.00085)	(.00078)	(.00044)	(.00085)	(.00078)
R-squared	.0143	.0051	.0122	.0173	.0126	.0196

NOTE.—Heteroscedastic-consistent standard errors are reported in parentheses. This data set contains 1,376,762 observations.

state variation in teen exposure to a relaxed drinking environment is positively and significantly correlated with both high school completion and college persistence.[30]

The next three models in table 4 condition on unobserved state-specific determinants by introducing state fixed effects. Using the restricted and unrestricted R-squared from these evaluations, it is straightforward to show that the state fixed effects are statistically significant determinants of educational attainment.[31] However, these models imply that the within-state variation in the MLDA over time has had small and statistically imprecise effects on all three measures of attainment. Only the coefficient in the high school completion model has the negative sign that would be expected if one believed alcohol availability and teen drinking actually reduced educational attainment. And that effect is relatively small (less than one-tenth of a percentage point).

C. Two-Sample Instrumental Variables Estimates

First-stage estimates indicate that exposure to an MLDA of 18 had a significant impact on all levels of teen drinking (table 3). However, the

[30] However, evaluations with these data can replicate Cook and Moore's (1993) NLSY-based result that teen residence in a state with a high MLDA and college completion are positively correlated. Conditional on other covariates, PUMS respondents who were 17 in 1981 and 1982 in a state with an MLDA of 20 or 21 were more likely to complete college.

[31] For example, using the high school completion evaluations, the test value for an F-statistic is $[(.017327 − .014259)/50]/[(1 − .017327)/1,376,700] = 86$, which exceeds the standard critical values for an F-statistic. The hypothesis that the state effects have zero coefficients is rejected. The state effects are jointly significant in the other models as well.

reduced-form estimates presented above indicate that alcohol availability had small and insignificant effects on educational attainment (table 4). The TSIV procedure developed by Angrist and Krueger (1992, 1995) will allow us to tie these results together by generating unbiased estimates of the effect of teen drinking on educational attainment that can be compared to some of the OLS estimates presented in table 1.

The TSIV estimates reported here have been based on a cross-sample matching of the 1977–86 MTF surveys and the PUMS respondents who were 17 over the same period. More specifically, these data sets have been matched by state, year, race, and gender indicators.[32] For purposes of this procedure, the MTF data on teen drinking behavior within the state/year/race/sex cells were converted back to their original status as 163,177 individual-level records. Because some of the MTF cells were empty, the number of PUMS respondents has fallen to 1,319,806. As discussed in Section III, the first step in the construction of the TSIV estimates was to form cross-sample fitted values for teen drinking using first-stage coefficients based on the MTF data and the teen MLDA exposure of respondents in both data sets. Then, consistent second-stage estimates can be produced by regressing the educational outcomes of the PUMS respondents on these cross-sample fitted values.

The results of these evaluations are reported in table 5. The first panel of table 5 reports the effect of an MLDA of 18 on each measure of teen drinking. These estimates are consistent with the results reported earlier: exposure to an MLDA of 18 implies significantly higher probability of participation in each teen drinking measure. The second panel of table 5 reports the effect of an MLDA of 18 on each measure of attainment. These estimates also replicate the results discussed in the previous section: teen exposure to an MLDA of 18 had small and statistically insignificant effects on educational attainment. The final panel of table 5 contains the TSIV estimates of the effect of teen drinking on educational attainment. Note that each TSIV estimate is equivalent to the ratio of the reduced-form and first-stage coefficients. The effects of teen drinking on college entrance and on college completion are positive and statistically insignificant.[33] This suggests that the covariance between teen drinking and

[32] Since the MTF respondents are not all 17 years old, there are some caveats associated with this matching. However, the resulting TSIV estimates are consistent with the patterns established by the reduced-form estimates.

[33] Standard errors were computed under the assumption of zero covariance between the first-stage and reduced-form estimates using a linear Taylor series approximation. Using these assumptions, it is straightforward to show that the t-statistic for the TSIV estimates is a function of the t-statistics in the first-stage and reduced-form evaluations. Because the first-stage estimates are precise, the TSIV t-statistics approximate those in the reduced-form evaluations.

Table 5
TSIV Estimates of the Effect of Teen Drinking on Educational Attainment, 1977–86

					TSIV Estimates		
					Endogenous Covariate		
First-Stage Estimates, 1977–86 MTF		Reduced-Form Estimates, 1990 PUMS					
Dependent Variable	Effect of an MLDA of 18	Dependent Variable	Effect of an MLDA of 18	Dependent Variable	Drinker	Moderate Drinker	Heavy Drinker
Drinker	.03813 (.00405)	High school completion	−.00078 (.00083)	High school completion	−.021 (.022)	−.034 (.035)	−.024 (.026)
Moderate drinker	.02334 (.00334)	College entrance	.00183 (.00159)	College entrance	.048 (.042)	.078 (.068)	.057 (.049)
Heavy drinker	.03215 (.00435)	College persistence	.00134 (.00146)	College persistence	.035 (.038)	.057 (.063)	.042 (.046)
Number of observations	163,177	Number of observations	1,319,806	Number of observations	1,319,806	1,319,806	1,319,806

Note.—TSIV = two-sample instrumental variables; MTF = Monitoring the Future; PUMS = Public-Use Microdata Sample; MLDA = minimum legal drinking age. Heteroscedastic-consistent standard errors are reported in parentheses. All models include state, cohort, race, and sex fixed effects.

discontinued schooling, identified in table 1 and in prior studies, does not represent a causal effect.

However, the TSIV estimates suggest that teen drinking may reduce the probability of completing high school. For example, they indicate that drinkers are 2.1 percentage points less likely to complete high school, moderate drinkers are 3.4 percentage points less likely, and heavy drinkers are 2.4 percentage points less likely. These effects are smaller than the corresponding OLS in table 1, and each is statistically insignificant. Do these results imply that the estimates in table 1 overstate the true effect or that there simply was insufficient power in the instrumental variable to identify the true effect? One way to address this question is to ask whether the TSIV estimates would have been significant if they were equal in magnitude to the estimates in table 1. For example, if the TSIV estimate, like the estimate in table 1, found that heavy drinking reduced the probability of high school completion by 5.2 percentage points, the t-statistic would have had an absolute value of 2. These simple calculations suggest that if teen drinking did have a significant effect on high school completion, the TSIV procedure and an identification strategy based on the within-state variation in MLDA would have had sufficient power to distinguish it. However, the imprecision of the reduced-form and TSIV estimates does qualify the high school completion results somewhat. In particular, the 95% confidence interval for the TSIV estimate of this effect does include the marginal effect reported in table 1. However, it should be noted that these evaluations also have less relevance for high school completion since the MTF data only allow us to identify the first-stage drinking responses of high school seniors.

The results in table 5 make a compelling case that the lower educational attainment of teenagers who drink may in the end simply reflect correlation rather than causation. In cases where a direct comparison is possible, the TSIV estimates are smaller than the corresponding OLS values. More specifically, the only two groups of OLS and TSIV estimates that we can directly compare are the effects of drinking and heavy drinking in the twelfth grade on college entrance.[34] Although the TSIV estimates of these effects in table 5 are statistically insignificant, these estimates are still sufficiently precise to reject the null hypothesis that the TSIV and OLS estimates are equal. Looking at the final rows of table 1, drinking is estimated to decrease college entrance by 6.3 percentage points. The 95% confidence interval for this estimate is $(-0.083, -0.043)$. In comparison, the confidence interval for the corresponding estimate in table 5 is $(-0.034, 0.130)$. Likewise, OLS estimates suggest that twelfth-grade heavy drinking decreases the chance of enrolling in college by 8.8 percentage

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[34] Because of the sample selection rules, the results in table 1 for high school completion use tenth-grade drinking as the covariate of interest.

points with a 95% confidence interval of $(-0.110, 0.066)$. The confidence interval for this parameter in the TSIV models is $(-0.041, 0.155)$.

In this instance, the statistical insignificance of the basic reduced-form relationship between the MLDA 18 and educational attainment is also informative. As we mentioned above, some authors have suggested that education outcomes can be improved by changing the policies concerning alcohol availability. For example, Cook and Moore (1994, p. 568) state that "increasing the tax on beer and other alcoholic beverages can be justified by the potential benefits associated with reduced violent crime and traffic accidents and improved school performance." Our results suggest that these gains will be incredibly small, if they exist at all. Using the results from table 4, and constructing a 95% confidence interval around the estimates for the MLDA of 18 coefficient, we find that raising an MLDA from 18 to 21 would increase the probability of graduation by at most 0.3 percentage points and increase college entrance rates by at most 0.1 percentage points. Because we cannot even detect a basic first-stage relationship between changing taxes and teen drinking, there is no evidence to suggest that higher beer taxes can be used to improve educational outcomes.

D. Specification Checks

The estimates presented in tables 4 and 5 provide novel evidence on the relationship between alcohol policies, alcohol use, and educational attainment. We have evaluated the robustness of these results by examining the reduced-form relationship between teen MLDA exposure and subsequent educational attainment in more detail. For example, we have constructed one important check of these reduced-form results by exploring the relationship between an earlier episode of MLDA variation and educational attainment (Dee and Evans 1997). More specifically, we have replicated the evaluations in table 4 with the 1950–59 birth cohorts in the 1990 PUMS. Several of these cohorts who were 17 between 1967 and 1976 were exposed to reductions in state MLDA as teens. The construction of this PUMS sample was similar to that of the younger PUMS cohorts. An added feature of working with these earlier cohorts is that, since they were between 31 and 40 at the time of the 1990 interview, they had largely completed their spells of schooling. Therefore, we have defined college completion for these cohorts as simply having a bachelor's degree. Reduced-form estimates with these cohorts indicate that the effect of teen exposure to a relaxed drinking environment on subsequent educational attainment was also small and statistically insignificant.

Another specification concern involves the fact that some of the younger 1990 PUMS respondents may not have finished entering and

completing college when interviewed.[35] This "completed spells" problem could confound the reduced-form evaluations presented in table 4 if state and cohort-specific patterns of schooling completion are related to the timing of the within-state changes in MLDA. The reduced-form evaluations with the older PUMS cohorts suggest that the patterns of incomplete spells are not generating any bias in the results reported in table 4. Nonetheless, we also replicated the reduced-form evaluations with samples that incrementally exclude the younger cohorts (Dee and Evans 1997). These evaluations generate results similar to those in table 4: teen exposure to an MLDA of 18 had small and insignificant effects on all three measures of educational attainment.

A related specification issue concerns omitted variable biases. These evaluations have included only those demographic covariates that are unarguably exogenous. A great deal of research (Hanushek 1986; Haveman and Wolfe 1995) has indicated that other teen characteristics like family structure, family income, and parental education are strong correlates of student achievement. Unfortunately, such data are unavailable in the PUMS. However, since there is no reason to believe that within-state trends in these attributes correlate with the timing of MLDA changes, it is unlikely that the omission of these attributes generates any bias in the parameter of interest. Nonetheless, we also evaluated specifications that introduced controls for the within-state variation in these attributes (Dee and Evans 1997). Data on these family characteristics were constructed for households with teenage children enrolled in school using the October CPS from these years. Because local macroeconomic conditions might also affect schooling decisions (Duncan 1965), the state unemployment rate at age 17 has also been included as a covariate. The inclusion of these covariates as well as race- and gender-specific cohort effects did not substantively alter the results presented in table 4.

Other important specification checks concern the appropriateness of matching respondents by their state of birth to an MLDA of 18 at age 17. Since most teens reside in their state of birth and because there is no reason to believe that childhood mobility is correlated with the timing of MLDA changes, this approach should not be problematic. Nonetheless, we addressed this concern by forming a weighted MLDA that reflects the pattern of teen mobility observed over the period in question. Using the 1980 5% PUMS, the probabilities a teen born in a particular state resided in a particular state were constructed. These were then used to adjust the teen exposure to an MLDA of 18. Card and Krueger (1992) employed a similar adjustment in their research on school quality and expenditures. Evaluations with the mobility-adjusted MLDA variable are also consistent with the results presented in table 4 (Dee and Evans 1997).

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[35] Angrist and Evans (1999) discuss this issue in more detail.

Evaluations with the mobility-adjusted MLDA variable also suggest that the general measurement error in teen MLDA exposure introduced by the state-of-birth match is not problematic. The presence of measurement error implies that the coefficient on the MLDA variable may be attenuated (i.e., biased toward zero). It is also important to note that such an occurrence could only be an important issue for the high school completion model. If the coefficients in the college entrance or persistence models were attenuated, it would only mean that the true coefficients were even more positive. However, attenuation in the high school completion model could mean that the true effect of an MLDA of 18 was more negative than that reported in table 4. As an additional specification check, we have matched the five birth cohorts who were 21–25 at the time of the 1990 interview to their MLDA exposure at age 17 using their reported state of residence in 1985 rather than their state of birth. The reduced-form evaluations with this sample suggest that a teen MLDA of 18 had a positive and statistically insignificant effect on high school completion. This sample consisted of over 620,000 respondents and had considerable within-state variation in MLDA. Among those who were 17 in 1982, nearly 34% were exposed to an MLDA of 18. Among those who were 17 in 1986, only 5% were exposed to an MLDA of 18.

VI. Conclusions

Though alcohol use is illegal for teens in the United States, its abusive consumption is surprisingly common, with roughly a third of high school seniors self-identifying as heavy drinkers. This abusive drinking can promote a broad variety of risks to the welfare and development of teens, perhaps most notably, traffic-related accidents and fatalities (Dee and Evans 2001). However, previous research has suggested that reduced educational attainment also represents an important consequence of teen drinking and that reductions in teen alcohol availability can therefore improve student outcomes. The evaluations presented in this article have raised two concerns about those conclusions. One is that the correlation between student outcomes and teen drinking may not reflect a causal relationship. The second concern is that the frequently employed identification strategy based on the cross-state variation in alcohol control policies may not be appropriate. The evidence presented here suggests that, to some extent, both concerns are empirically relevant. For example, estimates of the policy determinants of teen drinking demonstrated that, though the cross-state variation in beer taxes correlates with teen drinking, the within-state variation does not. Therefore, frequent recommendations for increased beer taxes appear to be based on what may only be a spurious correlation generated by unobserved state heterogeneity. However, the within-state increases in MLDA, which significantly affected all levels of

teen drinking, provided a source of exogenous variation for identifying the true effect of teen alcohol consumption on educational attainment. The TSIV estimates based on this instrument suggest that teen drinking has not had an independent effect on any level of educational attainment.

By focusing on the magnitudes of the links among alcohol policy, teen drinking, and educational attainment, this identification strategy has also underscored the fact that alcohol control policies could at best be a fairly weak policy lever for improving the levels of schooling among youth. For example, suppose that teen drinking actually did have an independent effect on attainment and that every state were to lower their MLDA to 18. The estimates presented here suggest that heavy drinking among high school seniors would rise by 3.2 percentage points. Since twelfth graders who drink heavily are at most 8.8 percentage points less likely to enter college, an MLDA of 18 would only reduce the likelihood of entering college by 0.28 percentage points (.032 × .088). Other policy interventions with larger and more direct links to the schooling decisions made by teens should be able to promote a greater improvement in the accumulation of human capital.

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