NBER WORKING PAPER SERIES

TEEN DRINKING AND EDUCATIONAL ATTAINMENT: EVIDENCE FROM TWO-SAMPLE INSTRUMENTAL VARIABLES (TSIV) ESTIMATES

Thomas S. Dee
William N. Evans

Working Paper 6082

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 1997

The authors wish to thank Ed Montgomery, Wallace Oates, John Shea, Judy Hellerstein and Bob Schwab for a number of helpful comments. Thomas Dee gratefully acknowledges the support of the American Education Research Association which receives funds for its AERA Grants Program from the National Science Foundation and the National Center for Education Statistics (U.S. Department of Education) under NSF Grant #RED-9452861. William Evans gratefully acknowledges the National Science Foundation which supported this research under NSF Grant #SBR-9409499. The opinions expressed here reflect those of the authors and do not necessarily reflect those of the granting agencies. This paper is part of NBER's research program in Health Economics. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

Teen Drinking and Education Attainment: Evidence
From Two-Sample Instrumental Variables (TSIV)
Estimates
Thomas S. Dee and William N. Evans
NBER Working Paper No. 6082
July 1997
JEL Nos. I12, I18, I2
Health Economics

ABSTRACT

Recent research has suggested that one of the important, life-cycle consequences of teen drinking is reduced scholastic achievement. Furthermore, it has been argued that state excise taxes on beer and minimum legal drinking ages (MLDA) are policy instruments that can have a positive impact on educational attainment. However, there is reason to question whether these results have sound empirical support. Some of the prior research has assumed that the decision to drink is made independently of schooling decisions. Furthermore, estimations that have recognized the potential simultaneity of these decisions may be poorly identified since they rely solely on the cross-state variation in beer taxes and MLDA as exogenous determinants of teen drinking.

A more convincing identification strategy would rely on the within-state variation in alcohol availability over time. To this end, we use the increases in the state MLDA during the late 70's and 80's as an exogenous source of variation in teen drinking. Using data 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. If teen drinking did reduce educational attainment, then attainment within a state should have risen after the MLDA was increased. Using data from over 1.3 million respondents who belong to the 1960-1969 birth cohorts in the 1990 Public-Use Microdata Sample (PUMS), we find that changes in the MLDA had small and statistically insignificant effects on measures of educational attainment such as high school completion, college entrance and college completion. A new technique developed by Angrist and Krueger (1992, 1995) allows us to tie these results together. Using matched cohorts from the MTF and PUMS data sets, we report two-sample instrumental variables (TSIV) estimates of the effect of teen drinking on educational attainment. These TSIV estimates are smaller than the corresponding single-equation probit estimates and are statistically insignificant, indicating that teen drinking does not have an independent effect on educational attainment.

Thomas S. Dee
School of Economics
Georgia Institute of Technology
Atlanta, GA 30332-0353

William N. Evans
Department of Economics
University of Maryland
College Park, MD 20742
and NBER
evans@econ.umd.edu

I. Introduction

The abuse of alcohol is widely believed to be 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; Yamada *et. al.*, 1993). Based, in part, on this conclusion, several authors have recommended policies which reduce alcohol availability through higher taxes (Grossman *et. al.*, 1993a, 1993b; Cook and Moore, 1994).[3] However, there is reason to question whether these recommendations have sound empirical support. Some of the prior research has assumed that the decision to drink is made independently of schooling decisions.[4] Furthermore, estimations that have recognized the potential endogeneity of these decisions may be poorly identified 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 paper uses the National Education Longitudinal Study of 1988 (NELS-88) to establish an important empirical baseline: teens who drink are less likely to complete high school and less likely to enter college. The next section presents several stylized estimations that promote the suspicion that the drinking/schooling relationship reflects correlation rather than causation. For example, if drinking has an

---

[1] The use and availability of alcohol has been linked to a number of outcomes like liver cirrhosis, traffic fatalities, fetal health, crime, earnings and marriage (NIAAA, 1993; Grossman *et. al.*, 1993b; Cook and Moore, 1994; Kenkel and Ribar, 1994).

[2] Teens also have higher rates of alcohol abuse and are involved in a disproportionate number of traffic accidents (NIAAA, 1996; Grant *et. al.*, 1991)

[3] Though all states now have a minimum legal drinking age of 21 , the impact of a lower drinking age has again become a policy issue. In March, the Louisiana Supreme Court struck down the law that barred those under 21 from buying alcohol. However, that decision has been set aside pending a request for a hearing by the U. S. Supreme Court (Bragg, 1996).

[4] However, the ambiguities inherent in schooling/health relationships have been recognized by other researchers (Kenkel, 1991).

independent effect on student achievement, drinking as a senior should be uncorrelated with test scores in 10th and 8th grades. However, using samples of NELS-88 respondents, we show that drinking as a senior is negatively related to prior test scores, even among students who did not drink as sophomores. This result suggests that students who are low academic achievers in their early teen years are more likely to drink heavily as seniors.

The likely endogeneity of teen drinking and student achievement implies that some single-equation estimation methods may overestimate the true effect of teen drinking. Recognizing this, Cook and Moore (1993) employed the cross-state variation in 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 questionable since the instruments represent not only the availability of alcohol but also the unobserved state attributes that influence teen drinking and educational attainment. Section 4 presents several reduced-form estimations which indicate that this cross-state heterogeneity may be important. Using NELS-88, the "effects" on college entrance of state policies unrelated to education are estimated. In a cross-section, several of these policies (e.g., taxes on gasoline and cigarettes, a waiting period for gun purchases, the use of capital punishment and a 65 MPH speed limit) appear to be statistically significant determinants of school persistence. Since there is no clearly unambiguous causal interpretation to many of these correlations, the usefulness of an identification strategy based on cross-state variation is in doubt.

This evidence suggests that 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 the MLDA as an exogenous determinant of teen drinking. In 1977, 30 states had an

---

[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.* 1993a, 1993b).

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 effect on teen drinking.

If teen drinking did have an independent effect on human capital accumulation, then educational attainment within a state should have risen after the MLDA was increased. We test this hypothesis using data on over 1.3 million respondents who belonged to the 1960-1969 birth cohorts in the Census Bureau's 1990 5 percent 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. Several robustness checks are presented. One of these checks consists of estimating the effect of teen exposure to an MLDA of 18 on the educational attainment of the 1950-59 birth cohorts. These older cohorts were teens during a period when MLDA were being reduced. Reduced-form estimates based on the more than 1.5 million respondents in these older cohorts also indicate that teen exposure to an MLDA of 18 had a small and statistically insignificant effect on high school completion. college entrance and college completion.

A new technique developed by Angrist and Krueger (1992, 1995) allows us to tie these results together. Using matched cohorts from the MTF and PUMS data sets, we report two-sample instrumental variables (TSIV) estimates of the effect of teen drinking on educational attainment. These TSIV estimates are smaller than the corresponding single-equation 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.

---

[6] We also discuss some evidence which supports the 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, violence and unsafe or unprotected sex. 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.

A. National Education Longitudinal Study of 1988 (NELS-88)

These estimations are based on the National Center for Education Statistics' (NCES) NELS-88 survey. This longitudinal survey reflects the NCES's most recent effort to collect "data on the factors affecting the transition from elementary school to high school and eventually to productive roles in American society." NELS-88 began with a nationally representative sample of 8th graders in 1988. The base-year sample was constructed in two stages. The first stage produced a sample of 1,052 grade schools. In general, the probability of a school's selection was proportional to its 8th grade enrollment. However, some schools (e.g. those with high minority enrollments) were oversampled. In the second stage, samples of 8th graders within those schools were included in the data collection. This selection was largely random except for some oversampling of Asian and Hispanic students. 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 10th graders in 1990 and 12th 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 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 restricted our samples to include only black, white and Hispanic respondents. We have defined college entrants as those whose highest post-secondary status in 1994 involved working towards a bachelor's degree. This construction, which focuses on obtaining a bachelor's degree as opposed to an associate's degree, is consistent with prior research (Cook and Moore, 1993).

During the first two follow-ups in 1990 and 1992, NELS-88 respondents 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 5 or more drinks in a row at least once in the last two weeks. In 1990, nearly 45 percent of NELS-88 10th graders had a drink within the last month. Over 23 percent had drunk heavily with the past two weeks. Among NELS-88 12th graders in 1992, 53.5 percent had a drink within the past month while over 28.7 percent had drunk heavily.[8] An important concern is whether such self-reported drinking data are valid. What evidence exists suggests that such self-reported drinking correlates strongly with actual drinking (Cook and Moore, 1994). Furthermore, the levels of drinking reported in the NELS-88 data are consistent with contemporaneous data from other widely used surveys. For example, in the 1992 Monitoring the Future data which are presented in Section VI, 50.2% of high school seniors report being drinkers; 26.7% report being heavy drinkers.

Using this information on self-reported teen drinking and subsequent educational attainment, we

---

[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.

[8] These means may not be nationally representative because we do not use sample weights. Our econometric work also does not use sample weights but does condition on the stratifying variables.

have estimated the relationship between teen alcohol use and schooling decisions. More specifically, using the 1990 10th 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 12th graders, we also report the "effect" of senior drinking on the probability of entering college. These estimations have conditioned on individual demographic characteristics such as age, gender, race and Hispanic ethnicity. Of the black, white and Hispanic respondents that were in 10th grade during the first follow-up in 1990, over 11,600 were included in the third follow-up in 1994. Over 10,800 black, white and Hispanic respondents who were in 12th grade in 1992 were interviewed again in 1994.[9]

B. Specifications

These estimations are based on a linear latent variable model in which the net benefits of education, E_i^*, are viewed as a function of a vector of characteristics, X_i:

$$E_i^* = X_i\beta + \epsilon_i.$$

The net benefits of a continued education are not observed. However, the decision to continue schooling is. Let the decision to continue schooling be defined by:

$$E_i = 1 \quad if\ E_i^* > 0$$

$$E_i = 0 \quad if\ E_i^* < 0$$

Assuming the error term is normally distributed and that $\Phi(.)$ is the standard normal cdf, the probability of continued schooling is:

$$Prob(E_i = 1) = Prob(E_i^* > 0) = Prob(\epsilon_i > -X_i\beta) = \Phi(X_i\beta).$$

[9] However, the final samples are somewhat smaller due to some non-response to drinking questions.

This probability provides a familiar basis for probit estimations of the determinants of educational attainment. We report estimates of the marginal effects of X_i on the probability of continued schooling that have been defined for the mean probability of continued schooling.[10]

We also report the results of estimating a linear probability model. This specification provides some evidence on whether the choice of functional form is important. Furthermore, a linear probability model facilitates the inclusion of a variety of other covariates known to be correlated with schooling decisions (Haveman and Wolfe, 1995; Hanushek, 1986.) More specifically, the estimated effect of teen drinking on educational attainment has also been conditioned on other socioeconomic covariates: fifteen indicators for the level of family income, six indicators for the composition of the respondent's family, four indicators for the level parental education and, finally, over 1,200 indicators for the school attended at the time of the follow-up interview.

C. Results

Table 1 reports the marginal effects from estimating single-equation probits for high school completion and college entrance. Students who reported drinking during their sophomore year were 4.4 percentage points less likely to complete high school and 8.5 percentage points less likely to enter college. Sophomores who drank heavily were 5.2 percentage points less likely to complete high school and 13.7 percentage points less likely to enter college. Seniors who drank were 3.7 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.

The results of estimating linear probability models for high school completion and college entrance are reported in Table 2. The linear specification generates results similar to those reported in Table 1:

---

[10] The marginal effects, defined as $\partial Prob(E=1)/\partial X = \beta\phi(X\beta)$, were calculated for $\phi(z)$ where $\Phi(z)$ equals the sample mean of the schooling outcome.

students who drank during high school were significantly less likely to complete high school or enter college. Furthermore, the magnitudes of these effects are similar to those generated by the probit estimations. These models also demonstrate that the significant correlation between teen drinking and discontinued schooling is robust to the inclusion of other important covariates. Furthermore, the magnitudes of these correlations are, for the most part, close to the estimates that only conditioned on the clearly exogenous demographic characteristics such as age, race and sex.

III. The Simultaneity of Drinking and Schooling Decisions

A central insight of the human capital model (Becker, 1964) is that the individual decision to acquire schooling is an investment in future earnings. Accordingly, the decision to acquire human capital should reflect the personal costs of schooling as well as how the expected future benefits are discounted. Other things being equal, students who find schooling unpleasant or who place little value on the benefits of 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, single-equation estimates like those presented in the previous section may overestimate the true effect of teen drinking on schooling outcomes.[11] This section presents some stylized estimations which suggest that this specification issue is important.

More specifically, some direct evidence that drinking and schooling may be jointly dependent has been constructed by exploring the timing of the teen decision to drink. If teen drinking is independent of student achievement, then the decision to drink as a senior should have no effect on achievement in the 8th and 10th grades. This hypothesis can be directly tested by estimating the "effect" of 12th grade drinking in 8th and 10th grade test-score equations. However, the power of this test is attenuated by the extent to which

[11] This specification issue could alternatively be framed as a concern over unobserved individual heterogeneity. Either formulation suggests that inferences based on single-equation estimates may falsely suggest that teen drinking reduces attainment.

12th grade drinking exhibits a serial correlation with drinking in the 8th and 10th grades. Therefore, this hypothesis was also tested among samples of students who abstained from drinking and heavy drinking as sophomores.[12]

As 8th and 10th 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 8th and 10th grade test scores for each student. Our samples of over 7,500 NELS-88 respondents consist of students who were enrolled in 10th grade in 1990, in 12th 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 estimations since dropouts would tend to have lower test scores and a higher prevalence of drinking.

The "effects" of 12th grade drinking on prior achievement are reported in Table 3. 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 8th grade test score that is 10 points lower. This significant reduction constitutes 4.8 percent of the mean test score and persists even when students who drank as sophomores are excluded from the sample. The results for more moderate 12th grade drinking and for the 10th grade test scores demonstrate a similar pattern: students who are doing poorly in school are more likely 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 which assume that teen drinking is determined independently of student achievement.

---

[12] Questions about alcohol use were not asked of the 8th grade respondents to NELS-88.

IV. State Effects in a Cross-Section: A Cautionary Tale

The policy relevance of the correlation between educational attainment and teen drinking depends critically on whether teen drinking has an independent effect on student achievement or is merely a correlate. Recognizing this, Cook and Moore (1993) used the cross-state variation in MLDA and excise taxes on beer to identify the effect of teen drinking on educational attainment.[13] They concluded that teen drinking does reduce attainment. They offer as their primary evidence reduced-form estimations which indicate that there is a positive correlation between residing in a state with a more restrictive drinking environment and subsequent educational attainment. However, 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.[14] More generally, the difficulty with state effects in cross-sectional estimations is that they represent not just variation in the policy variable but also the unobserved heterogeneity in educational attainment across states. This section will present some stylized estimations which demonstrate that the interpretation of such cross-state variation can often be problematic.

Using the NELS-88 data presented in Section II, we generated reduced-form equations similar to those in Cook and Moore (1993). 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. Through this matching, NELS-88 respondents have been linked to several state policies as of their sophomore year in high school: the state excise taxes on beer, cigarettes and gasoline and whether their state has a death penalty, a

[13] Their estimations were based on a samples of 1,904 and 753 respondents to the National Longitudinal Survey of Youth (NLSY).

[14] This issue is discussed in detail in the next section.

waiting period for gun purchases and a 65 MPH speed limit.[15] The "effect" of these state policies on college entrance has been estimated conditional on the demographic covariates as well as the indicators that reflect family income, family composition and parental education.

The results of reduced-form estimations that include these state policies are presented in Table 4.[16] The cross-state variation in beer taxes is not significantly correlated with the likelihood of entering college. However, other state policies correlate significantly with college entrance even though no plausible causal relationship necessarily exists. For example, these estimations suggest that increases in cigarette taxes reduce the probability of entering college while higher gas taxes increase the probability. Furthermore, sophomores from states with a waiting period for gun purchases 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. A 65 MPH speed limit implies a reduction of 8.8 percentage points in the likelihood of entering college. The spurious cross-sectional correlation between some state policies and the level of attainment raises doubt about this widely employed strategy for identifying the policy determinants of teen drinking and the impact of alcohol availability on youth outcomes.

V. Identifying Causal Effects

The estimations presented above have raised the concern that, because drinking and schooling may be jointly dependent, the OLS and single-equation probit estimates presented in Section II could be biased. Generating unbiased estimates of the effect of teen drinking on educational attainment requires an exogenous source of variation in teen drinking. However, the results from Section IV suggest that prior identification

---

[15] The data on the level of beer taxes has been drawn from DISCUS (1996) 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 from the Statistical Abstract of the United States. Data on waiting periods for gun purchases are from the National Survey of State Laws.

[16] Again, the basis for the probit specification is a linear latent variable model and the marginal effects are defined for the mean value of the dependent variable.

strategies which rely solely on cross-state variation may not satisfy the exclusion restriction. A more appropriate model would be one that utilizes within-state variation in alcohol availability over time. Such a model would allow us to purge permanent differences across states in drinking and educational attainment with state fixed effects. There are two policy instruments that we can potentially use as exogenous sources of variation in alcohol availability: state excise taxes on beer and the MLDA. Because beer is the drink of choice for most teens, the within-state changes in beer taxes may provide the necessary variation in alcohol consumption to identify an instrumental variables model. However, as we illustrate in the next section, once we rely on within-state variation, excise taxes on beer appear to have no statistically significant impact on teen alcohol consumption.

Nonetheless, we find that the within-state variation in teen exposure to MLDA is an independent and significant determinant of all levels of teen drinking. Therefore, we use the increases in MLDA that began in 1977 to identify the effect of teen alcohol use on educational attainment. In 1977, 30 states had an MLDA of 18. By 1989, all states had raised their MLDA to 21. In the next section, we demonstrate that teen exposure to an MLDA of 18 was strongly correlated with the level of both casual and heavy teen drinking. However, the validity of this identification strategy also requires that the movement away from an MLDA of 18 was independent of teen drinking (Besley and Case, 1994; Meyer, 1995). One suggestive indication that the changes in state MLDA were exogenous is that they were largely compelled by the Federal government over a relatively brief period.[17] Nonetheless, in the next section, we also discuss some empirical evidence which indicates that the timing of MLDA changes was independent of state-specific trends in teen drinking.

A. Two-Sample Instrumental Variables (TSIV) Estimates

Unfortunately, a traditional instrumental variables (IV) estimator that employed an identification

[17] Under the Danforth-Lautenberg Act (PL98-363) which was signed on July 17, 1984, the Secretary of Transportation was required to withhold some Federal highway funds from states that did not enact an MLDA of 21.

strategy based on within-state variation would require that we have pooled cross-sections that contain data on both teen drinking and subsequent schooling decisions. Because this would imply either following cohorts of teens through their early 20's or surveying individuals in their 20's and asking retrospective questions about teen drinking, no large-scale nationally representative survey known to us 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.

To illustrate the two-sample instrumental variables procedure (TSIV), consider the simple structural equation of interest

$$Y=X\beta + \epsilon$$

where Y is an (n x 1) vector, X is an (n x k) matrix, β is a (k x 1) vector of unknown parameters and ϵ is an (n x 1) vector of mean-zero random errors. When a regressor in X is not independent of ϵ, OLS estimates of this equation will generate biased estimates of β. However, given an (n x k) matrix of valid instruments Z, the instrumental variables (IV) estimate of β,

$$\beta_{IV} = (Z'X)^{-1}Z'Y$$

produces unbiased estimates.[18] The traditional applications of IV estimation have utilized one data set that contains information on Y, X and Z.

The TSIV procedure is motivated by the observation that this traditional IV estimate consists of two distinct components: the moment matrices, $(Z'X)^{-1}$ and $Z'Y$. This implies that one data set with information on X and Z could be matched to another data set with cohort-specific information on Y and the same Z in order to generate unbiased estimates of β. Operationally, the TSIV approach can work like a standard two-

[18] Throughout this discussion, we assume that Z satisfies the conventional IV requirements that plim $(Z'\epsilon/n) = 0$ and that plim $(Z'X/n)$ is a finite, nonsingular matrix. The first-stage and reduced-form estimations presented later provide strong evidence that both assumptions are valid for our models.

stage procedure. Suppose, for example, one data set contained the (n_1 x k) matrices X_1 and Z_1. A regression of X_1 on Z_1 would generate an estimate of the first-stage coefficients, Ω:

$$X_1 = Z_1\Omega + v$$

A matching of these first-stage results to a second data set which contained the (n_2 x 1) matrix Y_2 and the (n_2 x k) matrix Z_2 could then be used to form cross-sample fitted values for the potentially endogenous regressors:

$$\hat{X}_{21} = Z_2\hat{\Omega} = Z_2(Z_1'Z_1)^{-1}Z_1'X_1$$

A regression of Y_2 on these fitted values would imply a consistent TSIV estimate of β:

$$\beta_{TSIV} = (\hat{X}_{21}'\hat{X}_{21})^{-1}\hat{X}_{21}'Y_2$$

The asymptotic properties of this estimator are derived in Angrist and Krueger (1992).

It is straightforward to show that this TSIV estimator can also be expressed in terms of the consistently estimated first-stage and reduced-form parameters:

$$\beta_{TSIV} = [(Z_1'Z_1)^{-1}Z_1'X_1]^{-1}[(Z_2'Z_2)^{-1}Z_2'Y_2].$$

The first bracketed term contains the inverse of the first-stage estimates; the second contains the reduced-form estimates. If Y, X and Z all came from the same data set, this expression for the TSIV estimate of β would equal the traditional IV estimator, $(Z'X)^{-1}Z'Y$. However, we will demonstrate that such an indirect least squares (ILS) interpretation of the TSIV estimator is useful because it provides an important benchmark for evaluating the plausibility of reduced-form estimates.

B. Specifications

Our structural equation of educational attainment is defined by:

$$E_{ist} = W_{ist}\,\Pi + D_{ist}\,\gamma + u_s + v_t + \epsilon_{ist}$$

where 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 effects, and ϵ_{ist} is a mean-zero random error. The potentially endogenous covariate of interest is an indicator for teen drinking, D_{ist}. As we noted above, the primary instrument 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.[19] However, the TSIV procedure only requires 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 exposure, is based on pooled cross-sections from the Monitoring the Future (MTF) surveys. Our second data set, which has information on educational attainment and teen MLDA exposure, is based on the Census Bureau's 5 percent 1990 Public-Use Microdata Sample (PUMS). These data sets are described in more detail in the next two sections.

The TSIV estimate of γ is fully implied by the reduced-form estimations 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:

$$D_{ist} = W_{ist}\,\Pi_1 + M_{st}\,\gamma_1 + u_{1s} + v_{1t} + \epsilon_{1ist}.$$

From the PUMS data set, we can obtain an estimate of the reduced-form relationship between educational

[19] 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.

attainment and the instrumental variable by estimating the equation:

$$E_{ist} = W_{ist}\Pi_2 + M_{st}\gamma_2 + u_{2s} + v_{2t} + \epsilon_{2ist}.$$

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:

$$\hat{\gamma}_{TSIV} = \hat{\gamma}_2 / \hat{\gamma}_1$$

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.

C. Sample Size

Another important specification issue concerns the appropriate sample size that will allow us to construct meaningful inferences about the relationship between 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 single-equation 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 3.1 percentage points. The single-equation estimates in Table 1 indicated that heavy drinkers in 10th grade were 5.2 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 0.16 percentage points (.052*.031). Using heavy drinking in the 12th grade, if the single-equation estimate were true, we would expect exposure to an MLDA of 18 to reduce the probability of college entrance by 0.24 percentage points (.079*.031). A meaningful and unbiased 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 simple, bivariate regression model:

$$y_i = \alpha + \beta d_i + \epsilon_i$$

where y_i is an indicator that equals 1 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

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

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 single-equation estimation of this simple model generates unbiased estimates. Given these assumptions, it must be that a reduced-form regression of y on z would generate the following estimate:

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

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

$$\left|\frac{\hat{\beta}_{RF}}{\hat{\sigma}_{RF}}\right| > 1.96$$

where $\hat{\sigma}_{RF}$ 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. By definition, σ^2_{RF} 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 re-write σ^2_{RF} as $[2(\hat{p}_0-\hat{p}_0^2)-.0024(1.0024-2\hat{p}_0)]/n$. In order for the estimate of $(\hat{p}_1 - \hat{p}_0) =$

-.0024 to be statistically significant, it must be the case that $.0024/[[2(\hat{p}_0-\hat{p}_0^2) - .0024(1.0024 - 2\hat{p}_0)]/n]^{1/2} \geq 1.96$. 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 percent, we can set $\hat{p}_0 = 0.48$. This calculation then implies that we would need over 300,000 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. How many observations would we need to identify the reduced-form relationship between high school completion and a teen MLDA of 18? Given a high school completion rate of 90 percent and a reduced-form effect of .0016 that is implied by single-equation estimation, we should expect to need nearly 300,000 observations in each of the treatment and control groups. The reduced-form estimates we present in Section VII are based on samples of over 1.3 million individuals. The simple calculations presented here suggest that those 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 suggestion that an identification strategy based on cross-state heterogeneity can be problematic.[20] 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.

---

[20] Some caveats are appropriate because Cook and Moore's (1993) models are overidentified.

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

This section presents new estimates of the policy determinants of teen drinking that are based on pooled cross-sections from the 1977 to 1992 Monitoring The Future (MTF) surveys. A novel feature of these estimations is that they can condition on the unobserved state attributes that might bias the traditional cross-sectional estimates of the effect of state policies.

A. 1977-1992 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 a 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 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.[21] Since estimations with these data replicate prior results, this modest aggregation does not appear to generate any bias. Furthermore, using a similar construction with the other data sets used in this research has not demonstrated any sensitivity in the results.

This data set contains three frequently employed measures of teen drinking. 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 5 or more drinks in a row in the last 2 weeks. Additionally, these unique data set identifies "moderate drinkers": those who report having had 10 or more drinks within the past month. The trends in these measures of teen drinking are illustrated in Figure 1. Each level of teen drinking has been characterized by a slow but steady decline over the 1977-92 period. However, the rates at which teens use and abuse alcohol are still among the highest of any segment of society (Grant *et. al.*, 1991).

B. Alcohol Availability

The policy variables of interest in this literature have been those that affect the availability of alcohol: the MLDA and taxes. Information on the history of alcohol taxation and MLDA in the states has been taken from two publications of the Distilled Spirits Industry Council (DISCUS, 1996a, 1996b). Since some changes in MLDA occurred mid-year, 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 excise taxes on the drink of choice among teens, beer.[22] The beer taxes have been defined per gallon of beer and, where relevant, refer to the tax on beer greater than 3.2 percent alcohol by volume or sold in

[21] Cells with fewer than 5 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.

[22] Some research has used price data. However, there is evidence that tax increases on alcohol are completely passed on to consumers in the form of higher prices (Grossman *et. al.* 1993b; Cook, 1981). Furthermore, changes in the tax provide an independent source of variation in the price of alcohol.

cases. The nominal taxes have been converted into real terms using the Consumer Price Index (1982-1984 = 1). However, the results to be presented are robust to other reasonable constructions of the tax data.

Each of these policy instruments has exhibited considerable variation over this period. In 1977, 30 states had an MLDA of 18. As illustrated in Figure 2, nearly 60 percent of the MTF respondents resided in these states. By the end of 1988, all states had raised their MLDA to 21. The data in Figure 2 also demonstrate that the real costs of the nominal excise taxes on beer were declining over much of this period. The one change in this national trend was driven by a 1991 increase in the Federal tax on beer. Though inflation has generally eroded the real cost of nominal beer taxes nationally, there has still been considerable variation within states. For example, of the 44 states represented within our MTF data set, 18 have changed their beer taxes over periods in which students were interviewed. Many of these changes were large in percentage terms.

C. Results

The estimations we report here are based on weighted ordinary least-squares (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.[23] All of these models condition on the survey cohorts and the available demographic information. In Table 5, we present the results of estimating the determinants of heavy drinking by teens. The coefficients on the demographic covariates indicate consistently that older, white males are more likely to drink heavily. The first two models in Table 5 parallel the prior literature by estimating the effect of the cross-state variation in alcohol policy on heavy drinking. Model (1) indicates that an MLDA of 18 implies higher levels of heavy drinking.[24] Model (2) replicates the traditional result that

---

[23] This weighted linear specification utilizes the data as we received them. However, as the estimations in Section VIII demonstrate, these results are robust to other specifications.

[24] The size of this effect is consistent with prior estimates. However, the strong correlation between an MLDA of 20 and heavy drinking can only be understood as spurious since it is not robust to the inclusion of state effects.

students in states with high beer taxes are significantly less likely to drink heavily. The implied elasticity of heavy teen drinking with respect to the beer tax is -0.13 (-.092*.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).

The impact of including state effects in these regressions is addressed in Models (3) through (6). The estimates in Models (4) and (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 heavy teen drinking. Furthermore, Model (3) indicates that students facing an MLDA of 19 or 20 were no more likely to drink heavily than students facing an MLDA of 21.[25] However, the movement away from an MLDA of 18 did have a significant impact. Model (6) indicates that students who faced an MLDA of 18 were 3.1 percentage points more likely to drink heavily. The estimates reported in Table 6 replicate this exercise for other levels of teen drinking. The pattern of these results is consistent with those reported in Table 5. Estimations that do not condition on unobserved state attributes generate the standard result that the cross-state variation in beer taxes correlates negatively with teen drinking. However, the within-state variation in beer taxes has small and insignificant correlations with all three levels of teen drinking.

The lack of a correlation between the within-state variation in beer taxes and teen drinking is further evidence on the difficulties of an identification strategy based on cross-state variation. One important concern with this novel result is that it might be driven by a collinearity between the state effects and the beer taxes. As a check of this possibility, we have replicated the estimations in Table 6 using only those respondents in the 18 states that exhibited within-state variation in their beer taxes. The results of those estimations, which were based on the responses of 122,584 students, were similar to those reported in Table

[25] This result may not be surprising since an MLDA of 18 makes alcohol available to some high school age students. This result suggests that the usual approach of ignoring the grandfathering of some MLDA changes is not problematic for modeling drinking among high school students.

6. Nonetheless, the fact that beer taxes have no statistically signifcant impact on teen drinking may appear to some to be completely inconsistent with most prior research. However, 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. No prior study has used the within-state variation in beer taxes as the identifying assumption.

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.[26] Two points about this body of research are worth noting. First, in a related paper, Dee and Evans (1997) show that the tax effect in teen auto fatality models is not robust to the inclusion of state-specific time trends. 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 too large to be believed. For example, Saffer and Grossman (1987) estimate that the elasticity of traffic fatalities for 18-20 year olds with respect to beer tax is -0.27, but since beer taxes represent only 13 percent of retail price, this implies a price elasticity of about -2.0 (-0.27/.13). 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 -4.

This evidence indicates that the variation in beer taxes does 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 estimations reported in Table 6 indicate that an MLDA of 18 had a significant impact on all levels of teen drinking. Students who faced an MLDA of 18 were 3.5 percentage points more likely to be drinkers; 2.2 percentage points more likely to be moderate drinkers and 3.1 percentage points more likely to drink heavily. An important concern

[26] For a review and recent example of this research, see Ruhm (1995).

with these estimations is whether they identify the independent effect of MLDA changes on teen drinking. If the timing of a state's MLDA change was also a response to a change in teen drinking, the quality of our identification strategy is in doubt (Besley and Case, 1994; Meyer, 1995).

However, there is some suggestive evidence to buttress the standard 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 period before the dramatic MLDA changes (Figure 1). This suggests that the movement away from an MLDA of 18 was not a response to increases in teen drinking. More formally, the first-stage coefficients reported in Table 6 are largely robust to the inclusion of both linear and quadratic state-specific trend variables. This indicates that the state-specific variation in teen drinking was not correlated with the timing of a state's MLDA change.[27] Also, the results presented in Table 6 indicate that the correlation between an MLDA of 18 and each level of teen drinking is roughly the same regardless of whether state 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. Similarly, the timing of MLDA changes should also be independent of state-specific trends in educational attainment. Because our structural equation is not overidentified, we cannot test the orthogonality assumption formally. However, the reduced-form estimates in the next section provide some direct evidence that this assumption is not problematic.

VII. Reduced-Form Estimates

The estimations 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 estimations presented in this section address this question by estimating the

---

[27] 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.

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 V we demonstrated that, because the true 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 percent Public-Use Microdata Sample (PUMS) to estimate the impact on educational attainment of teen exposure to an MLDA of 18.[28] The 1990 PUMS consists of the more than 12 million individual respondents who received the long-form questionnaire in the most recent Census enumeration. Geographically, this sampling was based on Public Use Microdata Areas (PUMA) that are essentially counties or county equivalents. Within a PUMA, respondents were selected on a housing unit by housing unit basis. All states are represented in this data set.

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 1977-1986 period when MLDA 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 have been defined as those respondents who have completed "some college" or earned a bachelor's degree.[29] 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

[28] We have replicated the high school completion results to be reported here with a sample of 19-21 year-olds from the 1981-1992 October Current Population (CPS) survey. However, that data set only consisted of 67,361 respondents.

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

to include those who are still enrolled in a college program in addition to those who have earned a bachelor's degree.[30] The trends in these attainment measures are illustrated in Figure 3.

The members of this PUMS sample were matched the 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 the 11th grade are included. The final sample consists of 1,376,762 respondents. Matching respondents to their teen MLDA by state of birth does not appear to be problematic. Tabulations from the 1980 PUMS indicate that nearly 80 percent of teens resided in their state of birth. Furthermore, there is no reason to believe that the pattern of inter-state childhood mobility for those who advanced past the 10th grade was correlated with changes in MLDA. A robustness check discussed in the appendix offers further evidence that this construction is not problematic.

B. Results

The effect of teen exposure to a relaxed drinking environment on educational attainment has been estimated using linear probability models. However, similar results emerge from estimations 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 7. 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 7 include cohort effects but not state effects. These estimates indicate that, even in the absence of controls for 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-state variation in teen exposure to a relaxed drinking environment is positively and significantly correlated

---

[30] 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.

with both high school completion and college persistence.[31]

The next three models in Table 7 condition on unobserved state heterogeneity. Using the restricted and unrestricted R-squared from these estimations, it is straightforward to show that the state effects are highly significant determinants of educational attainment.[32] However, these models imply that the within-state variation in the MLDA over time has had small and statistically insignificant 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 reduced educational attainment. And that effect is small (less than one-tenth of a percentage point) and statistically insignificant.

C. Robustness Checks

The estimates presented in Table 7 provide important evidence on the existence and the plausible magnitude of a relationship between alcohol availability and educational attainment. Therefore, it is essential to ask whether these results are robust to several important specification issues. For example, the reduced-form estimates indicated that there was an insignificant but negative correlation between a relaxed drinking environment and completing high school. If the standard error on the MLDA variable were heteroscedastic, it might be that this effect was actually statistically significant. However, this does not appear to be the case. We have generated consistent standard errors (White, 1980) for the high school completion model. The consistent standard error on the MLDA variable implies a t-statistic (1.23) that is the same as that reported in Table 7.

We have constructed another important check of these reduced-form results by exploring the

---

[31] However, estimations with these data can replicate Cook and Moore's (1993) 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.

[32] For example, using the high school completion estimations, 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.

relationship between an earlier episode of MLDA variation and educational attainment. More specifically, we have replicated the estimations in Table 7 with the 1950-59 birth cohorts in PUMS. Several of these cohorts who were 17 between 1967 and 1976 were exposed to reductions in state MLDA. The construction of this PUMS sample was similar to that of the younger PUMS cohorts. An added feature of working with these 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, which are discussed in the appendix, indicate that the effect of teen exposure to a relaxed drinking environment on subsequent educational attainment was also small and statistically insignificant.

The "completed spells" problem is driven by the possibility that some of the younger PUMS respondents may not have finished entering and completing college.[33] The existence of a state-specific cohort effect on educational attainment could be confounded with the timing of within-state changes in MLDA by the reduced-form estimations presented in Table 7. The reduced-form estimations with the older PUMS cohorts suggest that a "completed spells" problem is not generating any bias in the results reported in Table 7. Nonetheless, the appendix also presents a second check which consists of replicating the reduced-form estimations with samples that exclude the younger cohorts. These estimations generate results similar to those in Table 7: teen exposure to an MLDA of 18 had small and insignificant effects on all three measures of educational attainment.

A more obvious specification issue concerns omitted variable bias. These estimations have included only those demographic covariates that are inarguably exogenous. A great deal of research (Haveman and Wolfe, 1995; Hanushek, 1986) 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

[33] Angrist and Evans (1996) discuss this issue in more detail.

attributes correlates with the timing of MLDA changes, it is unlikely that the omission of these attributes generates any bias in the parameter of interest. Nonetheless, estimations are presented in the appendix which condition on the within-state variation in these attributes. Data on these family characteristics were constructed for households with teenage children enrolled in school using the October Current Population Survey (CPS). Because local macroeconomic conditions might also affect schooling decisions, the state unemployment rate at age 17 has also been included as a covariate (Duncan, 1965). The inclusion of these covariates as well as race- and gender-specific cohort effects does not substantively alter the results presented in Table 7.

Other important robustness 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, a check was constructed by forming a weighted MLDA that reflects the pattern of teen mobility over the period in question. Using the 1980 5 Percent PUMS, the probabilities a teen born in a particular state resided in a particular state were constructed. These were 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. Estimations with the mobility-adjusted MLDA variable are also consistent with the results presented in Table 7.

Estimations 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 towards zero). 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 7. As an additional robustness check, we have matched the five birth cohorts who were 21 to 25 at the time of 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 estimation with this sample suggests 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 percent were exposed to an MLDA of 18. Among those who were 17 in 1986, only 5 percent were exposed to an MLDA of 18.

VIII. Two-Sample Instrumental Variables (TSIV) Estimates

First-stage estimates indicated that exposure to an MLDA of 18 had a significant impact on all levels of teen drinking. However, the reduced-form estimates presented in the previous section indicated that alcohol availability had small and insignificant effects on educational attainment. 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 the OLS and single-equation probit estimates presented in Tables 1 and 2.

The TSIV estimates reported here have been based on a cross-sample matching of the 1977-1986 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.[34] 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 163,189 individual-level records. Because some of the MTF cells were empty, the number of PUMS respondents has fallen to 1,261,831. As discussed in Section V, 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

---

[34] Since the MTF respondents are not all 17 years old, there are some caveats associated with this matching. However, because the resulting TSIV estimates are consistent with the pattern established by the reduced-form estimates, this concern may be overdrawn.

MTF data and the teen MLDA exposure of respondents in both data sets. Then, consistent second-stage estimates were produced by regressing the educational outcomes of the PUMS respondents on these cross-sample fitted values.

The results of these estimations are reported in Table 8. The first panel of Table 8 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 drinking as a teen. The second panel of Table 8 reports the effect of an MLDA of 18 on each measure of attainment. These estimates 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 8 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.[35] This suggests that the covariance between teen drinking and discontinued schooling, identified in Tables 1 and 2, 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 2.6 percentage points less likely to complete high school; heavy drinkers are 3.0 percentage points less likely and frequent drinkers are 4.2 percentage points less likely. But these effects are smaller than the corresponding single-equation probit estimates 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

[35] 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 estimations. Because the first-stage estimates are precise, the TSIV t-statistics approximate those in the reduced-form estimations.

equal in magnitude to the estimates in Table 1. For example, if the TSIV estimate, like the estimate in Table 1, found that drinking reduced the probability of completing high school by 4.4 percentage points, the t-statistic would have been 1.95. Similarly, if the TSIV estimate had found that heavy drinking reduced the probability of high school completion by 5.2 percentage points, the t-statistic would have been 1.99. 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 could have identified it.

IX. Conclusions

Previous research has concluded that teen drinking has an independent effect on educational attainment and that reductions in teen alcohol availability can therefore improve student outcomes. However, the estimations presented in this paper 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 were valid. 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 variation 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 between 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 did have an independent effect on attainment and that every state, adopting the recent opinion of the Louisiana Supreme Court, were to lower their MLDA to 18. The estimates presented here suggest that heavy drinking among high school seniors would rise by 3.1 percentage points. Since 12th graders who drink heavily are 7.9 percentage points less likely to enter college, an MLDA of 18 would only reduce the likelihood of entering college by 0.24 percentage points (.031*.079). 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.

REFERENCES

Angrist, Joshua D., "Instrumental Variables Estimation of Average Treatment Effects in Econometrics and Epidemiology," National Bureau of Economic Research, Technical Working Paper No. 115, November 1991.

Angrist, Joshua D. and William N. Evans, "Schooling and Labor Market Consequences of the 1970 State Abortion Reforms," National Bureau of Economic Research, Working Paper No. 5406, January 1996.

Angrist, Joshua D. and Alan B. Krueger, "The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples," *Journal of the American Statistical Association*, LXXXVII (1992), 328-36.

Angrist, Joshua D. and Alan B. Krueger, "Split-Sample Instrumental Variables Estimates of the Return to Schooling," *Journal of Business and Economic Statistics*, XIII (1995), 328-36.

Becker, Gary. *Human Capital* (New York: National Bureau of Economic Research, 1964).

Besley, Timothy and Anne Case, "Unnatural Experiments: Estimating the Incidence of Endogenous Policies," National Bureau of Economic Research, Working Paper No. 4956, December, 1994.

Bragg, Rick. "Louisiana Stands Alone on Drinking at 18," *New York Times*, March 23, 1996.

Cameron, Stephen V. and James J. Heckman, "The Nonequivalence of High School Equivalents," *Journal of Labor Economics*, XI (1993), 1-47.

Card, David, and Alan B. Krueger, "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States," *Journal of Political Economy*, C (1992), 1-40.

Cook, Philip J. "The Effect of Liquor Taxes on Drinking, Cirrhosis and Auto Fatalities," In *Alcohol and Public Policy: Beyond the Shadow of Prohibition* (Washington, D.C.: National Academy Press, 1981).

Cook, Philip J. and Michael J. Moore, "Drinking and Schooling," *Journal of Health Economics*, VII (1993), 411-429.

____, "This Tax's For You: The Case for Higher Beer Taxes," *National Tax Journal*, XLVII (1994), 559-573.

Dee, Thomas S. and William N. Evans, "Beer Taxes, Teen Drinking and Traffic Fatalities," Department of Economics Working Paper, University of Maryland, 1997.

Distilled Spirits Council of the United States, Inc. "History of Beverage Alcohol Tax Changes," DISCUS Office of Strategic and Policy Analysis, 1996.

_____, "Minimum Purchase Age By State and Beverage, 1933-Present," DISCUS Office of Strategic and

Policy Analysis, 1996.

Duncan, B. "Dropouts and the unemployed," *Journal of Political Economy*, LXXIII (1965): 121-134.

Evans, William N. and Edward Montgomery. "Education and Smoking: Where There's Smoke, There's an Instrument," Department of Economics Working Paper. University of Maryland. 1995.

Fuchs, Victor and Philip Farrell. "Schooling and Health: The Cigarette Connection," *Journal of Health Economics*, I (1982), 217-30.

Grant, Bridget F., Thomas C. Hartford, Patricia Chou, Roger Pickering, Deborah A. Dawson, Frederick S. Stinson and John Noble, "Prevalence of DSM-III-R Alcohol Abuse and Dependence," *Alcohol Health and Research World*, XV (1991), 91-96.

Grossman, Michael, Frank J. Chaloupka, Henry Saffer and Adit Lauxuthai. "Effects of Alcohol Price Policy on Youth," National Bureau of Economics Research. Working Paper No. 4385, June 1993.

Grossman, Michael, Jody L. Sindelar, John Mullahy, and Richard Anderson. "Policy Watch: Alcohol and Cigarette Taxes," *Journal of Economic Perspectives*, VII (1993), 211-22.

Hanushek, Eric A. "The Economics of Schooling: Production and Efficiency in Public Schools," *Journal of Economic Literature*, XXIV (1986), 1141-77.

Haveman, Robert and Barbara Wolfe, "The Determinants of Children's Attainment: A Review of Methods and Findings," *Journal of Economic Literature*, XXXIII (1995), 1829-1878.

Kenkel, Donald S. and David C. Ribar, "Alcohol Consumption and Young Adult's Socioeconomic Status," *Brookings Papers: Microeconomics*, (Washington D.C: 1994), 118-175.

Kenkel, Donald S. "Health Behavior, Health Knowledge and Schooling," *Journal of Political Economy*, XCIX (1991), 287-305.

Leung, Siu Fai and Charles E. Phelps. "My Kingdom for a Drink...?: A Review of the Price Sensitivity of Demand for Alcoholic Beverages," *Economics and the Prevention of Alcohol-Related Problems*, Research Monograph No. 25, National Institute on Alcohol Abuse and Alcoholism, 1993.

Meyer, Bruce D. "Natural and Quasi-Experiments in Economics," *Journal of Business and Economic Statistics*, XIII (1995), 151-161.

Mullahy, John and Jody L. Sindelar, "Life-Cycle Effects of Alcoholism on Education, Earnings and Income," *Inquiry*, XXVI (1989), 272-282.

National Institute on Alcohol Abuse and Alcoholism, "Alcohol Research and Public Health Policy," Alcohol Alert No. 20, 1993.

______, "Drinking and Driving," Alcohol Alert No. 32, 1996.

Ruhm, Christopher, "Alcohol Policies and Highway Vehicle Fatalities," National Bureau of Economic

Research, Working Paper No. 5195, July 1995.

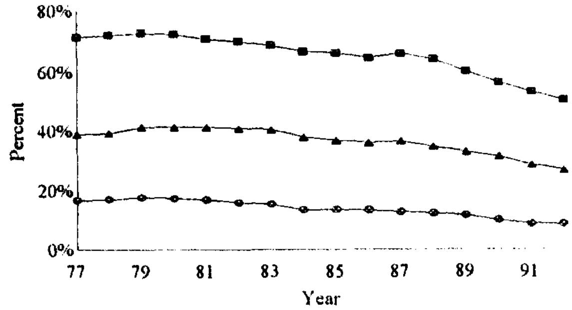
Saffer, Henry and Michael Grossman, "Beer Taxes, the Legal Drinking Age, and Youth Motor Vehicle Fatalities," *Journal of Legal Studies*, XVI (1987), 351-74.

Yamada, Tetsuji, Michael Kendix and Tadashi Yamada. "The Impact of Alcohol Consumption and Marijuana Use on High School Graduation," National Bureau of Economic Research, Working Paper No. 4497, October 1993.

Wald, Abraham. "The Fitting of Straight Lines if Both Variables are Subject to Error," *Annals of Mathematical Statistics*, XI, 284-300.

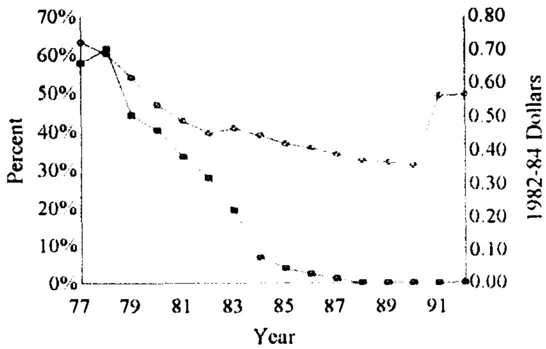
White, Halbert. "A Heteroscedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroscedasticity," *Econometrica*, LVIII (1980), 817-838.

Figure 1: Teen Drinking By Year
1977-1992 MTF



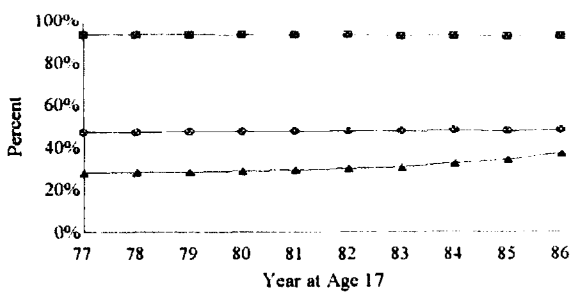
-■- Drinking -○- Moderate Drinking -▲- Heavy Drinking

Figure 2: Alcohol Policy By Year
1977-1992 MTF



-■- MLDA of 18 -◆- Real Beer Tax

Figure 3: Educational Attainment By
Year at Age 17, 1990 PUMS



-■- High School Completion -○- College Entrance
-▲- College Persistence

Table 1

Single-Equation Probits: The Marginal Effect of Teen Drinking on Educational Attainment, NELS-88[a]

Teen Drinking	Mean	Dependent Variables: High School Completion		Dependent Variables: College Entrance		Number of Observations
Sophomore Year						
Drinker	.445	-.044 (8.7)	--	-.085 (8.6)	--	10,077
Heavy Drinker	.236	--	-.052 (10.3)	--	-.137 (11.9)	10,851
Senior Year						
Drinker	.535	--	--	-.037 (3.7)	--	9,601
Heavy Drinker	.287	--	--	--	-.079 (7.1)	9,983

[a] Absolute values of t-statistics are reported in parentheses. All models include indicators for age, race and gender. The marginal effects are defined for the mean values of the dependent variables.

Table 2

Linear Probability Models: The Marginal Effect of Teen Drinking on Educational Attainment, NELS-88[a]

	High School Completion		College Entrance			
	Sophomore Year				Senior Year	
Covariates	*Drinker*	*Heavy Drinker*	*Drinker*	*Heavy Drinker*	*Drinker*	*Heavy Drinker*
Indicators for Age, Race and Gender	-.040 (8.6)	-.056 (10.7)	-.082 (8.6)	-.126 (11.8)	-.036 (3.6)	-.076 (7.0)
Previous Model and Indicators for Family Income and Composition	-.039 (8.4)	-.054 (10.4)	-.079 (8.6)	-.116 (11.2)	-.051 (5.4)	-.081 (7.8)
Previous Model and Indicators for Parental Education	-.038 (8.4)	-.053 (10.2)	-.072 (8.2)	-.103 (10.4)	-.050 (5.4)	-.074 (7.3)
Previous Model and Indicators for School Attended	-.042 (8.4)	-.050 (9.1)	-.084 (9.1)	-.114 (11.0)	-.066 (6.7)	-.087 (8.1)

[a] Absolute values of t-statistics are reported in parentheses.

Table 3

OLS Estimates: The "Effect" of 12th Grade Drinking on 8th and 10th Grade Test Scores, NELS-88[a]

Sample Selection by Sophomore Drinking	Mean Test Score	Model (1) Drinker	Model (1) *Heavy Drinker*	Model (2) Drinker	Model (2) *Heavy Drinker*	Number of Observations
8th Grade-Senior Panel						
All Students	210	-3.4	-10.0	-4.9	-10.0	7,586
		(4.4)	(12.2)	(6.4)	(12.3)	
Not a Drinker	212	-2.3	-10.0	-3.0	-10.0	4,210
		(2.3)	(7.5)	(2.8)	(7.2)	
Not a Heavy Drinker	212	-1.3	-8.8	-3.0	-9.4	5,736
		(1.5)	(8.4)	(3.4)	(9.0)	
Sophomore-Senior Panel						
All Students	210	-4.1	-10.5	-5.4	-10.2	7,639
		(5.5)	(13.1)	(7.3)	(12.9)	
Not a Drinker	213	-2.3	-9.6	-2.9	-9.7	4,254
		(2.4)	(7.4)	(2.8)	(7.2)	
Not a Heavy Drinker	213	-1.3	-8.2	-2.5	-8.2	5,773
		(1.6)	(8.1)	(3.0)	(8.1)	

[a] Absolute values of t-statistics 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.

Table 4

Reduced-Form Probits: The Marginal Effects of State Policies on College Entrance in a Cross-Section, NELS-88[a]

State Attribute	Mean	Marginal Effect
State Excise Tax on Beer Per Gallon	$.15	-.022 (0.6)
State Excise Tax on Cigarettes Per Pack of 20	$.17	-.219 (3.1)
State Excise Tax on Gasoline Per Gallon	$.11	.472 (3.4)
Waiting Period for Gun Purchases	.41	.042 (4.2)
Death Penalty	.81	-.054 (4.4)
65 MPH Speed Limit	.80	-.088 (7.2)

[a] Absolute values of t-statistics are reported in parentheses. The marginal effects are defined for the mean value of the dependent variable. These estimations are based on a sample of 11,147 observations. All models include ndicators for sex, race and ethnicity, year of birth, family composition, parents' education and family income.

Table 5

OLS Estimates of the Determinants
of Heavy Drinking by Teens:
Monitoring the Future, 1977-92[a]

Independent Variables	Mean	With Cohort Effects		With State and Cohort Effects			
		(1)	(2)	(3)	(4)	(5)	(6)
MLDA of 18	.196	.028 (5.83)	--	.027 (4.52)	--	.030 (5.83)	.031 (6.17)
MLDA of 19	.139	-.004 (0.86)	--	-.008 (1.51)	--	--	--
MLDA of 20	.029	.053 (5.81)	--	.004 (0.36)	--	--	--
Real Federal and State Excise Tax on Beer	$.50	--	-.092 (10.26)	--	.067 (2.38)	.036 (1.26)	--
White Non-Hispanic	.767	.180 (51.52)	.177 (50.92)	.168 (52.65)	.169 (52.48)	.168 (52.64)	.168 (52.64)
Less than 18	.534	-.026 (8.85)	-.027 (9.16)	-.025 (9.62)	-.025 (9.59)	-.025 (9.62)	-.025 (9.61)
Male	.507	.177 (59.88)	.177 (60.06)	.176 (68.11)	.177 (67.84)	.176 (68.11)	.177 (68.12)
R-Squared		.656	.659	.739	.737	.739	.739

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 3,941 observations which are based on 255,560 respondents. The estimations and means were weighted by the number of observations per state/cohort/age/race/sex cell.

Table 6

OLS Estimates of the Policy Determinants of Teen Drinking: Monitoring the Future, 1977-1992[a]

		With Cohort Effects		With State and Cohort Effects	
Dependent Variable	Mean	MLDA of 18	Beer Tax	MLDA of 18	Beer Tax
Drinker	.657	.059 (11.69)	-.170 (16.67)	.036 (6.49)	-.001 (0.04)
		.034 (6.93)	--	.035 (6.60)	--
Moderate Drinker	.138	.031 (10.21)	-.042 (6.85)	.022 (6.23)	.009 (0.48)
		.025 (8.54)	--	.022 (6.44)	--
Heavy Drinker	.367	.042 (9.18)	-.117 (12.58)	.030 (5.83)	.036 (1.26)
		.025 (5.67)	--	.031 (6.17)	--

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 3941 observations which are based 255,560 respondents. The estimations and means were weighted by the number of observations per state/cohort/age/race/sex cell. All models include age, race and sex effects.

Table 7

Reduced-Form Linear Probability Models: The Determinants of Educational Attainment, 1990 PUMS, Ages 21-30[a]

Covariates	With Cohort Effects			With State and Cohort Effects		
	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
	(2.24)	(0.75)	(8.93)	(1.23)	(1.27)	(0.67)
White Non-Hispanic	.08597	.09702	.12377	.07937	.10118	.12288
	(135.09)	(78.73)	(109.34)	(119.83)	(79.06)	(104.53)
Male	-.01750	-.02631	-.01353	-.01763	-.02665	-.01385
	(39.87)	(30.97)	(17.34)	(40.23)	(31.48)	(17.81)
R-Squared	.014259	.005133	.012235	.017327	.012578	.019623

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 1,376,762 observations.

Table 8

TSIV Estimates of the Effect of Teen Drinking on Educational Attainment, 1977-1986[a]

First-Stage Estimates, 1977-86 MTF		Reduced-Form Estimates, 1990 PUMS		TSIV Estimates			
					Endogenous Covariate		
Dependent Variable	Effect of an MLDA of 18	Dependent Variable	Effect of an MLDA of 18	Dependent Variable	*Drinker*	*Moderate Drinker*	*Heavy Drinker*
Drinker	.0379 (9.26)	*High School Completion*	-.00097 (1.15)	*High School Completion*	-.026 (1.15)	-.042 (1.15)	-.030 (1.15)
Moderate Drinker	.0233 (7.05)	*College Entrance*	.00196 (1.21)	*College Entrance*	.052 (1.21)	.084 (1.21)	.061 (1.21)
Heavy Drinker	.0323 (7.43)	*College Persistence*	.00133 (0.89)	*College Persistence*	.035 (0.89)	.057 (0.89)	.041 (0.89)
Number of Observations	163,189	Number of Observations	1,261,831	Number of Observations	1,261,831	1,261,831	1,261,831

[a] Absolute values of t-statistics are reported in parentheses. All models include state, cohort, race and sex effects.

Appendix: Robustness Checks

A. Completed Spells

Further evidence on the educational impact of alcohol availability as well as on the completed-spells issue has been constructed by looking at the PUMS cohorts aged 31 to 40 in 1990. These cohorts who were 17 from 1967 to 1976 faced a natural experiment similar to the one faced by the younger cohorts. In 1967, 12 states had an MLDA of 18; by 1976, 30 states did. This movement paralleled the decrease in the legal voting age and the popular sentiment that those old enough to fight in the military should be allowed to drink. These older cohorts were matched to the MLDA at age 17 in their state of birth. Educational attainment was defined as before except that college completers included only those who had a bachelor's degree. Reduced-form estimations with this data set which consists of 1,520,645 observations are reported in Table A1. In the absence of state effects, these estimations generate the impression that teen exposure to an MLDA of 18 significantly reduced the probability of entering college. However, the within-state variation in MLDA had no effect on any measure of educational attainment for these older cohorts. Since these estimations are based on different cohorts, some caveats are appropriate. Nonetheless, these estimations suggest that the incomplete educational spells are not driving the reported reduced-form results and they provide further evidence on the impact of alcohol availability on student outcomes.

The estimations reported in Table A2 replicate the reduced-form estimation with samples that exclude some of the younger cohorts who are most likely to have incomplete spells. There is no evidence that focusing on older cohorts generates substantively different results.

B. Omitted Variables Bias

State unemployment rates were identified by various editions of the Statistical Abstract of the United States. Information on the socioeconomic characteristics of households with teens in school (ie: marital status, family income and parental education) was constructed using cross-sections from the 1977-1986 October CPS. First, CPS households with children who were between 14 and 17 and who were enrolled in school were identified. A cardinal measure of family income was constructed for each household by taking the mean of family income for the indicated income range. Because these income data are top-coated, the measure of family income used in these estimations is the median of the cardinal household numbers within a given state and survey year. Like the tax data, these measures have been deflated by the CPI.

Because detailed relationship codes were not available for all of these survey years, parental education in each household with a teen in school is measured by the highest education level reported by any adult over age 25. Within a given state and survey year, these data were used to construct three measures of parental education: the proportion who were high school dropouts, proportion who were high school completers and the proportion with some college. Two measures of marital status were constructed for the salient households based on the adult identified as the "principal person of household" or "reference person" within each household. One measure identifies the proportion of households within a given state and year where the reference adult was never married. The second identifies the proportion of households within a given state and year where the reference adult is separated, divorced or widowed. The trends in all of these family characteristics track the national trends in other published data.

The impact of including these covariates as well as gender-specific and race-specific cohort effects in the reduced-form estimations is presented in Table A3. These estimations demonstrate that the results presented in the paper are robust to the inclusion of other covariates. Teen exposure to an MLDA of 18 had a small and insignificant effect on all levels of educational attainment.

C. Match By State of Birth

Using state of birth and state of residence information on 15 to 18 year old respondents to the 1980 PUMS, a measure of the probability that someone born in state s resided as a teen in state j, p_{sj}, was constructed. This pattern of childhood mobility across states was combined with indicators for an MLDA of 18 across states to create a weighted variable reflecting teen exposure to an MLDA of 18. More specifically, for a particular PUMS cohort born in state s, exposure to an MLDA of 18 was formed as the probability-weighted sum of MLDA indicators for all states when they were 17:

$$\hat{M}_s = \sum_{j=1}^{51} p_{sj}(M_j)$$

Among PUMS respondents, the MLDA exposure measured by this weighted variable which is bound on the unit interval has roughly the same mean as the unweighted exposure. The results of using this weighted MLDA measure in reduced-form estimations are reported in Table A4. The use of the weighted measure does not substantively alter the results based on the state-of-birth match.

Table A1

Reduced-Form Linear Probability Models: The Determinants of Educational Attainment, 1990 PUMS, Ages 31-40[a]

	With Cohort Effects			With State and Cohort Effects		
Covariates	High School Completion	College Entrance	College Completion	High School Completion	College Entrance	College Completion
MLDA of 18 at Age 17	-.00028 (0.74)	-.00906 (10.68)	.01507 (20.11)	.00111 (1.42)	.00005 (0.03)	.00185 (1.23)
White Non-Hispanic	.08376 (145.37)	.09229 (72.69)	.12259 (109.40)	.07524 (124.19)	.09082 (68.30)	.11764 (100.18)
Male	-.00816 (22.25)	.02421 (29.94)	.02733 (38.29)	-.00831 (22.67)	.02405 (29.90)	.02721 (38.31)
R-Squared	.014519	.006861	.011417	.017402	.017535	.021212

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 1,520,645 observations.

Table A2

Reduced-Form Linear Probability Models: The Effect of an MLDA of 18 at Age 17 on Educational Attainment, 1990 PUMS[a]

	Dependent Variables			
Sample	High School Completion	College Entrance	College Persistence	Number of Observations
Ages 21-30	-.00099 (1.23)	.00198 (1.27)	.00095 (0.67)	1,376,762
Ages 22-30	-.00063 (0.74)	.00203 (1.22)	.00099 (0.65)	1,253,326
Ages 23-30	-.00047 (0.51)	.00225 (1.24)	-.00017 (0.10)	1,135,638
Ages 24-30	-.00012 (0.12)	.00052 (0.25)	-.00171 (0.92)	1,013,545
Ages 25-30	-.00009 (0.07)	.00377 (1.53)	.00074 (0.33)	888,275

[a] Absolute values of t-statistics are reported in parentheses. All models include state, cohort, race and sex effects.

Table A3

Reduced-Form Linear Probability Models: The Effect of an MLDA of 18 at Age 17 on Educational Attainment, 1990 PUMS, Ages 21-30[a]

	Dependent Variables		
Covariates	High School Completion	College Entrance	College Persistence
State, Cohort, Race and Sex Effects	-.00099 (1.23)	.00198 (1.27)	.00095 (0.67)
Previous Model and Median Family Income	-.00100 (1.24)	.00192 (1.23)	.00092 (0.64)
Previous Model and Indicators for Marital Status and Parental Education	-.00083 (1.03)	.00216 (1.38)	.00067 (0.46)
Previous Model and the State Unemployment Rate	-.00119 (1.45)	.00085 (0.54)	.00061 (0.42)
Previous Model and (Race x Cohort) Effects and (Sex x Cohort) Effects	-.00129 (1.57)	.00023 (0.14)	.00025 (0.17)

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 1,376,762 observations.

Table A4

Reduced-Form Linear Probability Models: The Determinants of Educational Attainment, 1990 PUMS, Ages 21-30[a]

Covariates	With Cohort Effects			With State and Cohort Effects		
	High School Completion	College Entrance	College Persistence	High School Completion	College Entrance	College Persistence
Weighted MLDA of 18 at Age 17	.00073 (1.12)	-.00136 (1.08)	.00906 (7.83)	-.00147 (1.46)	.00323 (1.66)	.00200 (1.12)
White Non-Hispanic	.08592 (134.99)	.09684 (78.57)	.12369 (109.25)	.07937 (119.83)	.10118 (79.05)	.12288 (104.53)
Male	-.01750 (39.88)	-.02632 (30.97)	-.01353 (17.34)	-.01763 (40.23)	-.02665 (31.48)	-.01385 (17.81)
R-Squared	.014256	.005134	.012222	.017327	.012579	.019623

[a] Absolute values of t-statistics are reported in parentheses. This data set contains 1,376,762 observations.