A SIBLINGS ANALYSIS OF THE EFFECTS OF ALCOHOL CONSUMPTION ONSET ON EDUCATIONAL ATTAINMENT*

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Abstract:
This article examines the relationship between youthful drinking and educational attainment using data on same-sex siblings pairs from the 1979-90 panels of the National Longitudinal Survey of Youth. We consider different estimators that can be constructed using siblings data, including estimators that adopt key restrictions of the standard regression, family fixed effect, and instrumental variable approaches. We also consider the properties of these estimators under more general conditions and show that under very plausible assumptions the effect of drinking on schooling can be bounded. The study finds that estimates of the schooling consequences of youthful drinking are very sensitive to specification issues. The research concludes that the actual effects of youthful drinking on education are likely to be small. (JEL 112, 121)

Article:

I. INTRODUCTION

Youth alcohol consumption is generally viewed as a critical problem in the United States, with health, safety, social, and economic consequences for drinkers. Concern about youth drinking and its consequences was manifested in federal legislation in the 1980s to raise the minimum drinking age in all states to 21 years and in more recent efforts to increase the effectiveness of that prohibition by reducing alcohol availability on college campuses, making youth driver licenses more conspicuous, and pursuing criminal and civil penalties against adults who furnish underage drinkers with alcohol.

Research evidence regarding the health and safety consequences of youthful drinking backs up this concern. Studies have found strong associations between youths' alcohol consumption and outcomes, such as traffic accidents (Figlio, 1995) and subsequent problem drinking (Grant and Dawson, 1997; Moore and Cook, 1995). A number of researchers have also found that youthful drinking is detrimental to schooling (Benham and Benham, 1982; Cook and Moore, 1993; Mullahy and Sindelar, 1994) and, more generally, that heavy and problem drinking reduce adult earnings (Harwood et al., 1984; Mullahy and Sindelar, 1993; Rice, 1993; Rice et al., 1990).

This article examines the relationship between youthful drinking and educational attainment using data from the 1979-90 panels of the National Longitudinal Survey of Youth (NLSY). We focus on educational attainment because schooling is an important short- and long-run determinant of socioeconomic success and because most youths begin consuming alcohol during their school years. The temporal proximity of drinking onset and schooling makes a direct effect of early consumption plausible.

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ABBREVIATIONS

2SLS: Two-Stage Least Squares
IV: Instrumental Variable
MLDA: Minimum Legal Drinking Age
MoM: Methods of Moments
NLSY: National Longitudinal Survey of Youth
OLS: Ordinary Least Squares

The article also has a more general methodological focus. Policymakers must regularly sift through sharply conflicting research evidence. Although contradiction and refutation are crucial components of scientific discourse, they sometimes produce more questions than answers. This is certainly true of research on the effects of drinking on schooling, which has generated a considerable range of results. At one end of the spectrum, Cook and Moore (1993) found that being a frequent drinker or being frequently drunk reduced schooling by as much as 4 years. At the other end, Dee and Evans (1997) concluded that youthful drinking had essentially no effect on schooling.

The methodological point that estimates of the social and economic consequences of alcohol consumption are sensitive to the use of alternative statistical techniques has been made before by Kenkel and Ribar (1994). The present article departs from their earlier study by carefully examining the properties of different estimators. Conditions under which these estimators might bound the actual consequences of drinking are also investigated. Specifically, the article develops a multiple- equation regression model of drinking onset and completed schooling among siblings and considers alternative estimators that can be derived from this model. Several authors, including Ashenfelter and Krueger (1994), Griliches (1979), and Ribar (1999), have explored methods for combining siblings data to address multiple econometric concerns. Our model addresses several issues, such as correlation among the unobserved determinants of drinking and schooling and mismeasurement of the drinking variable, which lead to inconsistent estimates of the effects of youthful drinking. We show the conditions under which standard regression models, the family fixed-effects approach, and a siblings instrumental variable (IV) procedure generate consistent estimates. Though none of the estimation methods is consistent under all circumstances, they can be used to bound the true effect under a plausible and relatively weak set of assumptions.

The rest of this article is organized as follows. Section II provides economic explanations for why early drinking might be negatively associated with educational attainment. It also reviews previous empirical studies that have examined the effects of drinking on schooling and, more generally, on socioeconomic success. Section III describes statistical issues associated with modeling these relationships and lays out our econometric approach. This section is moderately technical, but its detailed examination of the properties of several alternative estimation strategies provides the formal basis for comparing these strategies. Section IV describes the data set and variables used in the analysis. Estimation results and specification comparisons are reported and discussed in section V. Section VI presents our conclusions.

II. BACKGROUND

We consider the effects of alcohol consumption on schooling in the context of a simple time-use model (Becker, 1965). Suppose that advancement in school requires investments of time, for activities such as attendance and studying, and investments of money, for things such as tuition and books. Suppose also that consumption of alcohol requires time and money. Finally, assume that young people have preferences regarding schooling, drinking, and their consumption of other goods and that they face constraints on their budgets and time. In this framework, drinking might have a direct negative effect on schooling if it diverts individual resources that might otherwise have gone to schooling. Drinking might also have other negative spillover effects on schooling if it causes investments in education to be less productive.

Although the model is consistent with a direct effect of drinking on schooling, it is also consistent with other explanations for a negative association. For instance, in addition to a young person's own inputs of time and money, school advancement and alcohol consumption might also depend on family background characteristics, like parental supervision. Attitudes regarding school and drinking might be shaped by such factors as parental role-modeling or peer influences. If these characteristics or other relevant characteristics are not controlled for, they might lead to a spurious negative correlation between drinking and schooling behavior. Another possibility is that when young people complete school, they have more time and money available for alcohol and other types of consumption. In this case, school completion may actually affect drinking rather than the other way around.[1] Accordingly, researchers examining the schooling and other socioeconomic consequences of alcohol consumption have been careful to consider issues of omitted variables and endogeneity bias.

Several studies have examined the link between drinking or drinking problems and schooling attainment. An early study by Benham and Benham (1982), which employed a sample of St. Louis youths from the 1910s and 1920s and a 30-year follow-up, found that drinking problems reduced schooling by about 1.5 years. Cook and Moore (1993) considered more contemporary data from the NLSY. They examined the effects of drinks per week, frequent drinking, and being frequently drunk on years of postsecondary schooling using state beer taxes and minimum drinking age laws as instruments for alcohol consumption. Their IV procedures generated large but relatively imprecise estimates of the schooling effects.[2] Reduced-form results from their study indicated that higher minimum drinking age laws were associated with higher levels of educational attainment.

Yamada et al. (1996) also used data from the NLSY to estimate the effects of alcohol consumption on high school completion. Their estimates, which did not account for possible endogeneity in drinking, indicated that a 10% increase in the frequency of drinking reduced the graduation probability by 6.5%. Mullahy and Sindelar (1994) used data from Wave 1 of the New Haven site of the National Institute of Mental Health Epidemiological Catchment Area survey and found that onset of alcoholism symptoms by age 22 reduced schooling by 5%. Although they corrected for endogeneity in some other models in their study, Mullahy and Sindelar did not account for endogeneity in the relationship between alcoholism onset and schooling.

All of the preceding studies concluded that youthful alcohol consumption or problem drinking had negative effects on schooling. A more recent study, however, by Dee and Evans (1997) called these findings into question. Dee and Evans argued standard regression estimates of the effects of drinking were likely to be biased by endogeneity but that the alcohol policy measures that had been put forward as instruments were also problematic. When Dee and Evans used within-state changes in alcohol policies as instruments for changes in drinking behavior, they found that alcohol consumption had no discernible effect on schooling.

Researchers have also examined the effects of alcohol consumption on other socioeconomic outcomes, such as earnings and labor supply. Harwood et al. (1984), Rice (1993), and Rice et al. (1990) found that alcohol consumption was associated with earnings reductions of 10-20%. These estimates were obtained from a multivariate regression that included a dichotomous variable measuring the answers to 4 of 14 questions concerning potential problem drinking behavior. The limited number of questions as well as the fact that the researchers did not worry about endogeneity in their estimation is a source of contention in these findings.[3] Other researchers have found alcohol consumption to have moderate positive effects on income. Berger and Liegh (1988), French and Zarkin (1995), and Zarkin et al. (1998) have shown that nondrinkers and heavy

[1] We thank a referee for pointing out this possibility. In general, however, our data are not consistent with school completion causing the onset of drinking for most people. Roughly half of the individuals in our sample began drinking a year or more before leaving school, and another third began in the same year that they left school.

[2] Although their estimates were significant, each of their estimates is approximately twice the size of its reported standard error. For example, 95% confidence intervals surrounding the frequent drinker and frequently drunk coefficients were consistent with either no effects of more than 4 years.

[3] Heien and Pittman (1989) were unable to replicate Harwood et al. (1984) despite using the same data.

drinkers earn less than moderate drinkers.[4] Cook (1991), however, was unable to show that heavy drinking was a detriment to earnings.

Some studies have focused more narrowly on alcohol problems, such as alcohol abuse and dependence, rather than general alcohol consumption. However, the estimates from these studies have also varied greatly. Mullahy and Sindelar (1993) found alcohol abuse led to earnings reductions of about 17% when the entire working age population (age 22-59) was considered. The reduction was even higher (31%) when the prime age population (age 30-59) was considered.[5] Kenkel and Ribar (1994) found a 10% reduction in earnings associated with alcohol abuse for men, but they found a 10% increase in earnings associated with alcohol abuse for women. Kenkel and Wang (1999) considered broader measures of compensation, including earnings, health insurance, and paid sick leave. They found that fringe benefits losses for alcohol abusers and alcohol dependents amounted to 20% of compensation losses. Finally, Mullahy and Sindelar (1993) attempted to decompose the direct and indirect effects of alcohol abuse. They found that estimates of the earnings consequences of alcohol abuse for prime working-age males were significantly lowered when education and other human capital covariates, such as health status, were included in the model.

III. MODEL

We examine alternative estimators for the effects of drinking onset adopting a variant of Ribar's (1999) two-equation siblings model. The model has several useful features. First, it treats drinking onset as an endogenous determinant of schooling. Second, it allows for correlation between the unobserved variables that affect both drinking and schooling. Third, it accounts for the possibility that the age when drinking begins may be reported with error.

To develop the model, let N denote the number of sibling pairs, let i (= 1, N) index families, and let j (= 1,2) index siblings within families. Also let A_{ij} and Y_{ij} denote the age of drinking onset and the completed level of schooling. A two-equation model for each sibling's drinking and educational attainment ran he written

$$
\begin{aligned}
A_{ij} &= X_{ij}\Gamma + v_{ij} \\
Y_{ij} &= \beta_A A_{ij} + X_{ij}B + \varepsilon_{ij} \qquad (1)\\
&\qquad \text{for } i = 1, N \text{ and } j = 1, 2, \\
&= \beta_A v_{ij} + X_{ij}(\beta_A \Gamma + B) + \varepsilon_{ij}
\end{aligned}
$$

where X_{ij} denotes a vector of observed explanatory variables; v_{ij} and ε_{ij} are random unobserved variables; and β_A, B, and Γ are coefficients or vectors of coefficients to be estimated. The coefficient, β_A,, which captures the direct effect of the age of drinking onset on schooling, is the focus of our investigation.

We assume that the mean for each of the random variables conditional on the other observed variables is zero. Denote the conditional variances and covariances of the random variables as follows

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[4] Higher earnings from moderate alcohol consumption are similar to estimates found in the medical literature that find improved health associated with moderate alcohol use. For a more detailed discussion of the medical benefits of moderate alcohol use, see the June 1993 issue of American Journal of Public Health.

[5] Mullahy and Sindelar (1989) argue that the differential effects of alcohol problems by age group can be explained by life-cycle considerations, for example, a young alcohol abuser might not attend school but would probably participate in the labor force, and, therefore, his earnings might actually be higher than a nonabuser that is attending school.

$$
\begin{aligned}
&\mathrm{Var}(\nu_{ij}|X_{ij}) = \sigma_\nu^2, \quad \mathrm{Var}(\varepsilon_{ij}|X_{ij}) = \sigma_\varepsilon^2 \\
&\mathrm{Cov}(\nu_{ij}, \varepsilon_{ij}|X_{ij}) = \sigma_{\nu\varepsilon}, \\
(2)\quad &\mathrm{Cov}(\nu_{ij}, \nu_{ik}|X_{ij}) = \tilde{\sigma}_\nu^2 \\
&\qquad \text{for } i = 1, N;\ j, k = 1, 2;\ j \neq k \\
&\mathrm{Cov}(\varepsilon_{ij}, \varepsilon_{ik}|X_{ij}) = \tilde{\sigma}_\varepsilon^2, \\
&\mathrm{Cov}(\nu_{ij}, \varepsilon_{ik}|X_{ij}) = \tilde{\sigma}_{\nu\varepsilon}.
\end{aligned}
$$

Two of the covariance terms in (2) are particularly important to our analysis. One of the terms is σ_{ve}, which represents the covariance between the unobserved determinants of drinking onset and schooling for a given individual. A nonzero covariance between v_{ij} and ε_{ij} leads to bias in standard regression estimates of the effect of drinking onset on schooling. This type of covariance can arise if, as seems likely, there are person-specific characteristics, such as attitudes or academic ability, that are relevant to drinking and schooling but are unmeasured or otherwise omitted from the model. The other term is $\widetilde{\sigma_{v\varepsilon}}$, which represents the covariance between the unobserved determinants of drinking onset and schooling within families. This second type of covariance can arise if the model omits relevant attributes, such as family upbringing and economic circumstances, that are common across siblings.

As mentioned, our analysis is also complicated by the possibility that the age at drinking onset may be measured with error. In terms of the model, assume that we do not observe A_{ij} but instead observe $A_{ij}^* = A_{ij} + m_{ij}$, where m_{ij} represents "classical," or purely random, measurement error. Specifically, we assume the expected value of m_{ij} is 0, m_{ij} has a finite variance ($\mathrm{Var}(m_{ij}) = \sigma_m^2$), m_{ij} is independent of the other observed and unobserved variables, and m_{ij} is independent across siblings.

Let a_{ij}^* and y_{ij} denote residuals from regressions of A_{ij}^* and Y_{ij} on X_{ij} (i.e., let them be measures of drinking onset and schooling that condition out the effects of the other observed variables). The variances and covariance for these residuals can then be expressed as

$$
\begin{aligned}
&\mathrm{Var}(a_{ij}^*) = \sigma_\nu^2 + \sigma_m^2, \\
&\mathrm{Cov}(a_{ij}^*, y_{ij}) = \beta_A\sigma_\nu^2 + \sigma_{\nu\varepsilon} \\
&\qquad \text{for } i = 1, N \text{ and } j = 1, 2 \\
&\mathrm{Var}(y_{ij}) = \beta_A^2\sigma_\nu^2 + 2\beta_A\sigma_{\nu\varepsilon} + \sigma_\varepsilon^2 \\
(3)\quad &\mathrm{Cov}(a_{ij}^*, a_{ik}^*) = \tilde{\sigma}_\nu^2 \\
&\mathrm{Cov}(a_{ij}^*, y_{ik}) = \beta_A\tilde{\sigma}_\nu^2 + \tilde{\sigma}_{\nu\varepsilon} \\
&\qquad \text{for } i = 1, N;\ j, k = 1, 2, \text{ and } j \neq k. \\
&\mathrm{Cov}(y_{ij}, y_{ik}) = \beta_A^2\tilde{\sigma}_\nu^2 + 2\beta_A\tilde{\sigma}_{\nu\varepsilon} + \tilde{\sigma}_\varepsilon^2.
\end{aligned}
$$

Method of Moments (MoM) estimators for the parameters in the model can be constructed by setting the theoretical moments from (3) equal to the corresponding sample moments for the residuals of sibling's drinking onset and schooling status and solving for the theoretical parameters. Unfortunately, even with this relatively simple set-up, it is not possible to find unique solutions and identify all of the parameters in this specification because there are more parameters (eight) than moment conditions (six). Identification requires us to impose some additional and potentially unverifiable restrictions on the model.

From one perspective, this is a discouraging prospect. To the extent that different restrictions lead to different results, we are left with essentially the same ambiguity that is found in the existing literature. From another perspective though, things are more positive. After all, we do learn exactly what the data can and cannot say. Also, though we may not be able to generate a single, unequivocal estimate for the effect of drinking onset on schooling, a careful examination of the properties of alternative estimators might allow us to bound the estimate under a reasonable set of assumptions.

Below we consider several alternative restrictions on the model and derive the MoM estimators. Afterward, we compare the properties of the estimators when these restrictions do not hold.

Standard Regression Assumptions

In the standard regression approach, the random determinants of each individual's drinking onset and schooling are assumed to be uncorrelated. Also, the age at drinking onset is assumed to be accurately measured (i.e., $\sigma_m^2 = 0$). When these restrictions are applied, there is a unique solution for β_A , $\beta_A = \frac{Cov(a_{ij}^*, y_{ij})}{Var(a_{ij}^*)}$. Let s_{a*y} denote the individual-specific sample covariance between drinking onset and schooling, and let s_{a*}^2. denote the sample variance of drinking onset. The MoM estimator is $\beta_R = s_{a*y}/s_{a*}^2$, which some will recognize as the ordinary least squares (OLS) estimator.

Approximate Family Fixed Effects

The family fixed-effects model allows for some degree of correlation between ε_{ij} and v_{ij}. It specifies the individual and cross-sibling covariance between the random determinants of schooling and drinking onset to be equal such that $\sigma_{ve} = \widetilde{\sigma_{ve}}$. This restriction is nearly equivalent to assuming that the unobserved determinants of educational attainment can be decomposed into a family-specific random variable related to drinking and another independent random component.[6] As with the standard regression model, we again assume that drinking onset is accurately measured. Let $\tilde{s}_{a*y}^2$ denote the cross-sibling sample covariance in alcohol consumption onset, and let $\tilde{S}_{a*y}$denote the cross-sibling sample covariance between drinking and schooling. The resulting MoM estimator is

$$(4) \qquad \beta_{FE} = (s_{a^*y} - \tilde{s}_{a^*y})/(s_{a^*}^2 - \tilde{s}_{a^*}^2).$$

This estimator is nearly equivalent to applying OLS to a version of the second equation from specification (1) in which the schooling and drinking onset are differenced across siblings.[7]

Siblings IV Specification

An alternative assumption is that the cross-sibling correlation $\tilde{\sigma}_{ve}$ is 0. Under this assumption, the solution for β_A is $\beta_A = \frac{Cov(a_{ij}^*, y_{ik})}{Cov(a_{ij}^*, a_{ik}^*)}$. Note that the terms in the solution do not involve σ_{ve} or σ_m^2; so, no further covariance or measurement restrictions are necessary. The resulting MoM estimator is $\beta_{IV} = \tilde{S}_{a*y}/\tilde{S}_{a*}^2$, which is equivalent to an IV estimator in which each sibling's drinking onset serves as an instrument for the other's onset.

Properties under Alternative Assumptions

The standard regression, family fixed effects, and siblings IV estimators were each derived by imposing some combination of covariance restrictions or measurement assumptions. If these restrictions and assumptions do not hold, the estimators have the following properties

[6] The family fixed-effects estimator is typically motivated in terms of an omitted family-specific factor. Besides the conditions listed in the text, the factor-analytic specifications additionally imply that $0 \leq \tilde{\sigma}_v^2 \leq \sigma_v^2$ This restriction was always met in the study's data.
[7] If the residuals net out both the individual's own observed characteristics and the sibling's observed characteristics, the estimator given in (4) is exactly equivalent to the siblings difference estimator

$$
(5) \quad \begin{aligned}
&\text{plim } \hat{\beta}_R = (\beta_A \sigma_\nu^2 + \sigma_{\nu\varepsilon})/(\sigma_\nu^2 + \sigma_m^2) \\
&\text{plim } \hat{\beta}_{FE} = [\beta_A(\sigma_\nu^2 - \tilde{\sigma}_\nu^2) + \sigma_{\nu\varepsilon} - \tilde{\sigma}_{\nu\varepsilon}]/ \\
&\qquad\qquad (\sigma_\nu^2 - \tilde{\sigma}_\nu^2 + \sigma_m^2) \\
&\text{plim } \hat{\beta}_{IV} = (\beta_A \tilde{\sigma}_\nu^2 + \tilde{\sigma}_{\nu\varepsilon})/(\tilde{\sigma}_\nu^2).
\end{aligned}
$$

From (5), all three estimators are inconsistent in the absence of restrictions, and each is inconsistent in a different way.

An alternative to making the strong assumptions that (σ_{ve} , $\tilde{\sigma}_{ve}$, *or* σ_m^2 are equal to 0 is to consider a weaker set of sign restrictions for these terms. For instance, the variance σ_m^2 should be nonnegative, and it would be fair to posit that σ_{ve} *and* $\tilde{\sigma}_{ve}$ are also nonnegative (i.e., assume that personal and family background factors that delay the onset of drinking also encourage schooling). If we adopt these sign restrictions, the standard regression and fixed-effects estimators remain inconsistent with probability limits, which might be either above or below the true value of β_A. In the case of the standard regression estimator, measurement error moves the estimator toward zero while the presumed positive correlation between the unobserved determinants of schooling and drinking onset shifts it upward. For the fixed-effects estimator, the effects of measurement error are likely to be exacerbated relative to the standard regression estimator. At the same time, the fixed effects approach may mitigate the effects of unobserved variables.[8] The net result is still ambiguous.

For the siblings IV estimator, the inconsistency is easier to sign because the estimator is not affected by measurement error or by unobserved person-specific factors. If the unobserved family-specific determinants of schooling and drinking onset are positively related, then the probability limit of the siblings IV estimator is greater than the true value of 13A. This means that the siblings IV estimator plausibly represents an upper bound estimate of the actual effect.

IV. DATA

The data for this analysis come from the 1979-1990 panels of the NLSY (Center for Human Resource Research, 1998). The NLSY is a national sample of 12,686 individuals who were 14-21 years old in 1979 and who have been reinterviewed annually since then.[9] The survey contains detailed longitudinal behavioral information including data on schooling attainment and alcohol consumption at different ages. Personal and family background data are also available.

For each household sampled in 1979, interviews were conducted and relationship codes recorded for every appropriately aged individual in the household. Thus, the NLSY can be used to construct moderate-sized subsamples of near-age siblings. This study focuses on same-sex sibling pairs. From the 1979 panel, there are over 750 households with two or more brothers present and 775 households with two or more sisters present. After excluding observations with missing information on age of first drink, education, and the exogenous variables, the sample size for brothers is reduced to 654 households and the sample size for sisters is reduced to 649. For households with more than two same- sex siblings with nonmissing information, the study randomly selects two siblings.

Educational attainment is measured by completed years of schooling by age 25. This measure of schooling has several advantages. First, as with the study by Cook and Moore (1993), our analysis considers schooling at a consistent age. However, unlike their study, we do not impose additional age and schooling restrictions on our

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[8] As Griliches (1979) pointed out, it is possible for the fixed effects estimator to exacerbate the inconsistency associated with unobserved variables. In the present model, the inconsistency is exacerbated if

$$(\sigma_{\nu\varepsilon} + \tilde{\sigma}_{\nu\varepsilon})/(\sigma_\nu^2 - \tilde{\sigma}_\nu^2 + \sigma_m^2) > \sigma_{\nu\varepsilon}/(\sigma_\nu^2 + \sigma_m^2).$$

[9] In the initial survey, blacks, Hispanics, disadvantaged white youth, and military personnel were oversampled. Weights (not used here) are available to make the data nationally representative.

subsample.[10] Second, our specific age cut-off, age 25, corresponds to a point where most people have completed their formal schooling. It is also a cut-off that is regularly used in research (for example, the Census Bureau reports educational attainment for persons aged 25 and over).

The study also constructs a variable indicating the age of the individual when he or she began drinking. The 1982 and 1983 panels of the NLSY asked individuals the age at which they first drank regularly. Unfortunately, the question was not asked in subsequent panels and thus misses drinking that began when some respondents were in their late teens or early twenties. The 1984, 1985, 1988, and 1989 panels did ask people whether they had ever begun drinking. For individuals who had not commenced drinking by the time of the 1983 interview, we use their age at the date of the first interview at which they indicated they were drinkers as the age of first drink. For the few individuals (less than 5% of our sample) who began drinking after age 25 or had not begun drinking by the 1989 panel, we artificially set their onset at age 25. Because there may be some gaps and short-comings in our construction of the drinking onset variable, we experimented with several other measures; none of these other variables substantially altered our estimation results.

Beyond the difficulty in constructing the variable, the age at first drink measure may have some other shortcomings. First, it is based on self-reported information. Although the interviewers used special procedures in some panels to elicit accurate responses for sensitive questions, individuals may still have been uncomfortable truthfully reporting their drinking behavior. Second, the age at drinking onset is not a direct indicator of heavy consumption, alcohol abuse, or alcohol dependence. The variable is correlated with these other measures, but imperfectly so. We would be interested in also examining the age at onset of problem drinking, but the NLSY lacks the data needed to construct such a measure.[11]

Age of alcohol consumption onset has the advantage of being an explicit target of minimum legal drinking age (MLDA) laws, and, therefore, we focus on it in this study. Hence, beyond the direct interpretation, the estimated effect of the variable can also be interpreted as an estimate of the partial equilibrium effect of a fully enforced MLDA law.[12] Another reason for examining onset is that youthful drinking may be habit-forming and may lead to heavier drinking later in adulthood (Moore and Cook, 1995).

Several other explanatory variables are included in the empirical analysis. The NLSY contains detailed family background data. From these family background data, we use measures of parents' educational attainment and family structure. We also include a scale for the availability of reading materials, which is the sum of three dummy indicators for whether anyone in the family received a magazine, subscribed to a newspaper, or held a library card when the respondent was 14 years old. In addition, controls for the race and ethnicity of the respondents are included. The research utilizes some geographic measures, including a dummy variable for residence in an urban area at age

[10] Cook and Moore (1993) examined a subset of respondents who were age 14-15 at the start of the NLSY in 1979 and, for some analyses, a smaller subset of individuals, who were high school seniors at the time of the 1982 interview, was used.

[11] In 1983 the NLSY did ask the age at which people began drinking one or more times per week. Our results did not change when we used this variable in place of the regular drinking onset variable.

[12] The measured impact is interpreted as a partial equilibrium effect because it only accounts for individual behavior and does not account for peer, market, and other spillover effects that might be associated with a population-wide decline in drinking.

TABLE 1
Variable Means and Standard Deviations for Sibling Pair Samples

	Brothers		Sisters	
Variables	**Mean**	**SD**	**Mean**	**SD**
Outcomes				
Age at onset of drinking	16.934	2.810	17.903	2.868
Years of completed schooling by age 25	12.441	2.418	12.910	2.129
Basic control variables				
Age in 1979	18.280	2.090	18.195	2.015
African origin	0.272	0.445	0.303	0.460
Hispanic origin	0.184	0.388	0.156	0.363
Urban residence at age 21	0.790	0.408	0.803	0.397
Unemployment rate at age 21	8.601	3.537	8.774	3.489
Family background variables				
Number of siblings	4.361	2.535	4.534	2.696
Intact family at age 14	0.645	0.479	0.648	0.478
Percentage time family intact (age 0–14)	0.816	0.350	0.816	0.345
Percentage time family intact missing	0.034	0.182	0.040	0.196
Mother's years of schooling	9.912	4.347	10.323	3.725
Mother's education missing	0.073	0.261	0.042	0.200
Father's years of schooling	9.074	5.421	9.402	5.224
Father's education missing	0.147	0.354	0.124	0.330
Household access to reading material (magazine, newspaper or library card)	1.924	1.027	1.964	1.059
Number of individual observations	1308		1298	
Number of sibling pairs	654		649	

Note: Statistics calculated using same-sex sibling pair data from the 1979-1990 panels of the NLSY.

21 and a variable for the local unemployment rate at age 21. Many of these controls have been used in previous analyses, and all of them have standard interpretations. Means and standard deviations of the analysis variables for the brother and sister pair samples are reported in Table 1.

V. EMPIRICAL RESULTS

Table 2 displays results from the alternative regression specifications for alcohol onset and education estimated using the same-sex siblings data. Following previous studies on the effect of alcohol consumption and problem drinking on productivity (see, for example, Mullahy and Sindelar, 1993), the first column reports estimates from OLS regressions with basic controls for race, ethnicity, birth cohort (age in 1979), urban residence, and the local unemployment rate. The regressions in the second column add a series of family background controls. As with the study by Kenkel and Ribar (1994), the third column reports family fixed effects estimates. The fourth column lists estimates from IV models in which one sibling's alcohol onset is used as an instrument for the other's drinking onset. To conserve space, the tables display only the coefficients on the age at drinking onset variable and fit statistics for each specifications.[13]

The estimation results for men and women are nearly uniform in indicating that drinking onset has a small detrimental association with completed years of education. The standard regression estimates for brothers with and without family background controls are nearly identical. Both sets of estimates indicate that delaying the onset of alcohol consumption by 1 year is associated with a statistically significant, but small, 0.07- year gain in schooling for men. For sisters,

[13] Complete results are available from the authors upon request.

TABLE 2
Alternative Estimates of the Effects of Onset of Drinking on Completed Years of Schooling

	OLS with Basic Controls	OLS with Basic and Family Controls	Family Fixed Effects	Siblings IV
Brothers				
Estimated effect of age at onset of drinking	0.070***	0.069***	0.055**	0.220*
	(0.024)	(0.021)	(0.023)	(0.128)
R^2	0.044	0.312	0.651	—
Sisters				
Estimated effect of age at onset of drinking	0.0003	0.039**	−0.014	0.220***
	(0.021)	(0.019)	(0.024)	(0.071)
R^2	0.041	0.259	0.572	—

Notes: Models estimated using same-sex sibling pair data from the 1979–1990 panels of the NLSY. Regressions also include intercepts and coefficients for basic and family background control variables as indicated in the column headings; for lists of the control variables see Table 1. Estimated standard errors appear in parentheses. *Significant at .10 level. **Significant at .05 level. ***Significant at .01 level.

the estimated coefficients from the standard regression models are even smaller. When only basic controls are included, the regression coefficient is essentially zero. In the model with family controls, the coefficient implies that delaying onset by 1 year would increase schooling for women by 0.04 years.

The third column of Table 2 reports results from models that account for family fixed effects. For men, the estimated coefficient for drinking onset falls to 0.055. However, we cannot reject the hypothesis that the family effects are jointly equal to 0 (the *p*- value is 0.11); we also cannot reject the hypothesis that the coefficient for drinking onset is the same across the fixed effects and standard regression models (the p-value is 0.13).[14] For women, the estimated coefficient for drinking onset also falls becoming small and negative. Specification tests for the women's models indicate that the family fixed effects are jointly significant and that the estimated coefficient differs significantly across specifications. The finding that controlling for family effects reduces the estimated effects of drinking on schooling is similar to the results reported by Kenkel and Ribar (1994) for other socioeconomic outcomes.

The final column of Table 2 reports estimates from siblings IV specifications. The coefficient estimates from the IV models are much larger than the estimates from the standard-regression and family fixed-effects models. The point estimates for men and women both indicate that delaying the start of drinking by a year leads to about a quarter of a year increase in schooling. Hausman- Wu tests indicate that the IV and standard regression coefficients for men are not significantly different (the *p*-value is 0.24) but that the coefficients for women are different (the *p*-value is 0.01).

The results from Table 2 indicate that estimates of the effects of the age at drinking onset are very sensitive to alternative assumptions regarding the correlation of unobserved determinants across individuals and siblings. The estimates range from little or no effect for the family fixed-effects models to moderate effects for the siblings IV models. Unfortunately, the two specifications that lead to extreme estimates—the family fixed-effects and siblings IV models—are just identified; therefore, the data by themselves cannot be used to distinguish between these two models.

A comparison of the underlying assumptions of the two approaches does not resolve things. The fixed-effects estimator is consistent if the drinking onset variable is measured accurately and the source of endogeneity or omitted variables bias is identical across siblings. The siblings IV estimator requires that the unobserved determinants of one sibling's drinking not affect the other sibling's schooling; it is inconsistent if there are

[14] To examine the stability of the coefficient across specifications we employed the test developed by Clogg et al. (1995).

unmeasured family-specific factors that are relevant to drinking and schooling. Neither of these assumptions is verifiable, nor particularly compelling.

Our earlier examination did, however, reveal the conditions under which the estimated effects of drinking onset on schooling could be bounded. Specifically, under the assumption that unmeasured common background factors across siblings that delay alcohol consumption also promote schooling, the siblings IV approach generates an upper bound estimate on the effect of drinking onset. In the present context, the siblings IV model produces the largest coefficient; so, the bound cannot rule out the standard regression or family fixed-effects estimates. It does, though, suggest that the effects of drinking on educational attainment are modest. At the upper end of a 95% confidence interval, the siblings IV estimate for men implies no more than a 0.47-year effect for men and a 0.36- year effect for women.

Additional Sensitivity Analyses

The study's use of siblings methods raises some concerns regarding the generalizability and comparability of its results. For instance, relative to a general population sample, the study's sample of same-sex siblings is drawn disproportionately from larger families. If family size mediates the effect of drinking on schooling, then the study's siblings results might not reflect the effects for all youths.

To address this issue, we reestimated the standard regression models with basic and family background controls (the second specification in Table 2) using the full sample of respondents from the NLSY. The estimation results are reported in the first column of Table 3. There is nothing to indicate that the use of a siblings sample skews the measured impact of drinking on schooling.

Another concern involves our use of sibling behavior as an instrument. Although we are careful to lay out the conditions under which the siblings IV method can be applied as well as the properties of the estimator under other conditions, it is still useful to check whether other instruments perform better and how sensitive the results are to alternative instruments. Acceptable instruments must satisfy two properties: They have to be strongly related to drinking onset and cannot be directly related to schooling. Instruments that have been proposed before include policy variables such as a state's MLDA and beer tax rate. The second column of Table 3 reports estimates from full sample two-stage least squares (2SLS) models that use dummy variables for the minimum drinking age in effect when the respondent was 17 years old as instruments.[15]

One difficulty with the 2SLS procedure is that the policy variables are not strong predictors of drinking onset. The coefficients on the policy variables are significantly different from zero in the first-stage, but the first stage regression has a poor overall fit. As a consequence, the standard errors on drinking onset in the second-stage structural equation are quite large (more than ten times as large as the corresponding standard regression standard errors). As with the siblings IV procedure, the full sample 2SLS model for men leads to estimated effects that are considerably larger than the standard regression estimates. For women, the full-sample 2SLS coefficient is negative but, as mentioned, imprecisely estimated.

The third and fourth columns of Table 3 report results from full-sample standard regression and 2SLS models in which state fixed effects have been added. Dee (1999) and Dee and Evans (1997) have criticized the use of state policy variables as instruments and recommended using within-state changes in policies instead. The within-state approach did not add much to our results. Including state fixed effects improved the fit of the models only slightly, suggesting there was relatively little unobserved state heterogeneity that needed to be taken into account. The state fixed effects had no noticeable effect on the coefficients in the standard regression models. In the 2SLS model, however, the precision of coefficients decreased markedly.

[15] Estimates from models that use the laws in effect at different ages and that incorporate the beer tax rate as an additional instrument are similar to those reported in Table 3.

As a final sensitivity check, the fifth and sixth columns of Table 3 report reduced-form standard regression estimates of the effects of minimum drinking age laws on education completion. These results directly confirm that policies that discourage youthful drinking have, at best, only modest effects on

TABLE 3
Alternative Estimates of the Effects of Onset of Drinking on Completed Years of Schooling from Full NLSY

	OLS	2SLS	OLS with State Controls	2SLS with State Controls	OLS Reduced Form	OLS Reduced Form with State Controls
Men						
Estimated effect of age at onset of drinking	0.070*** (0.011)	0.431* (0.245)	0.072*** (0.011)	0.682 (0.811)	—	—
Minimum legal drinking age of 18	—	—	—	—	−0.143** (0.070)	−0.207 (0.171)
Minimum legal drinking age of 19	—	—	—	—	−0.048 (0.104)	−0.146 (0.195)
R^2	0.312	—	0.336	—	0.306	0.330
Women						
Estimated effect of age at onset of drinking	0.017* (0.009)	−0.189 (0.211)	0.018* (0.009)	0.462 (0.896)	—	—
Minimum legal drinking age of 18	—	—	—	—	0.073 (0.063)	0.138 (0.170)
Minimum legal drinking age of 19	—	—	—	—	0.088 (0.098)	0.116 (0.179)
R^2	0.285	—	0.304	—	0.284	0.304

Notes: Models estimated using full sample (4,536 men and 5,176 women) from the 1979–1990 panels of the NLSY. Regressions also include intercepts and coefficients for basic and family background control variables; for lists of the control variables see Table 1. Estimated standard errors appear in parentheses. *Significant at .10 level. **Significant at .05 level. ***Significant at .01 level.

schooling. For men, the estimated effect of an MLDA of 18 in the model is negative and significantly different from zero in the model without state controls. However, the magnitude of the effect (-0.14) is small, and the significance disappears when state controls are added. In the models for women, the coefficients on liberal drinking ages are positive, though insignificant.

VI. CONCLUSION AND DISCUSSION

This article estimates several specifications of an endogenous variable model of educational attainment and alcohol consumption onset. The model allows for the possibility that the unobserved determinants of schooling and drinking are correlated and that drinking behavior might be misreported. The alternative specifications incorporate covariance restrictions that capture important properties of the standard regression, family fixed effects, and IV approaches. The specifications are employed in an effort to obtain direct estimates of the effects of youthful drinking on education.

The empirical analysis generates a variety of estimates. As with the study by Kenkel and Ribar (1994), the analysis shows that estimates of the effects of drinking are sensitive to assumptions that are made regarding the covariance of the unobserved variables. Specifically, standard regression models produce statistically significant but small estimates of the consequences of alcohol use on education. The addition of family fixed effects further reduces the size of the estimated effects of drinking onset for men and women. The study also specifies and estimates models in which one sibling's drinking onset is instrumented using the other's onset. Like the study by Cook and Moore (1993), the IV approach leads to larger estimates of the effects of alcohol consumption.

A careful analysis of the properties of the estimators reveals that under the reasonable assumption that the unobserved family background factors that discourage drinking also promote schooling, the siblings IV estimator represents an upper bound on the measured impact of drinking onset. If we consider the upper end of a 95% confidence interval around the siblings IV estimates, delaying drinking onset by a year increases schooling by no more than 0.47 years for men and 0.36 years for women. Under the additional assumption that respondents in the NLSY accurately reported their age at which they first began drinking, the standard regression estimator becomes an upper bound, and the maximum schooling gain from delaying drinking onset by 1 year is reduced to 0.11 years for men and 0.08 years for women.

Our results, which indicate that drinking onset has, at most, only a modest effect on schooling for youth as a whole, are not necessarily inconsistent with the stronger findings of Cook and Moore (1993) and others. The earlier studies examined measures of heavy drinking and problem drinking that differ from our measure and conceivably lead to stronger effects. Our analyses using the full NLSY sample, however, suggest that the previous findings were largely an artifact of weak instruments.

The policy implications from these results are clear. Policies that target youth's drinking behavior, specifically MLDA laws, do not confer much of a benefit in terms of schooling. These policies may be worthwhile on other grounds, such as health and safety, but they are not a panacea for all social outcomes.

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