

THE EFFECT OF ALCOHOL PROHIBITION ON ILLICIT-DRUG-RELATED CRIMES*

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ABSTRACT

We evaluate the effect of alcohol access on drug-related crime and mortality using detailed information on access laws in Texas between 1978 and 1996. Counties with alcohol access have higher average levels of drug-related crimes. However, after controlling for both county and year fixed effects, we find that having local alcohol access decreases crime associated with illicit drugs. This basic finding is replicated in two alternative analyses. First, we find that prohibiting the sale of beer to persons under 21, which arguably increases the implicit price of liquor more for juveniles in wet counties than for those in dry counties, increases the fraction of drug-related arrests involving juveniles more in wet counties than in dry counties. Second, we find that after controlling for both county and year fixed effects, local alcohol access decreases mortality associated with illicit drugs. Alcohol access and illicit-drug-related outcomes appear to be substitutes.

I. INTRODUCTION

GOVERNMENT policies in the United States have long sought to regulate the consumption of goods or services that are perceived to either be or lead to behavior that is “sinful.” In puritan New England, sumptuary laws regulated extravagance in food, dress, tobacco use, and drinking. Today, the government regulates the consumption of addictive drugs, gambling, and other commodities that are seen as vaguely immoral. These policies attempt to reduce or eliminate the consumption of goods or services that may have deleterious effects on consumers as well as external effects on society. The

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effect of these regulations on consumption of the targeted good or service is relatively straightforward to predict. By effectively increasing the price, government regulations reduce consumption. Other unintended effects, however, are less certain. Raising the price of alcohol, for example, may not only reduce alcohol consumption but may also affect the market for related goods. The net effects on sinful consumption and on the resulting behaviors of interest are ambiguous.

In this paper, we evaluate an unintended effect of regulations on one of the most often studied and contentious “sinful” commodities, alcohol. In particular, we use a unique panel data set on the 254 counties in the state of Texas between 1978 and 1996 to examine the effects of alcohol regulations on illicit-drug-related arrests and deaths. In Texas, local jurisdictions have the power to regulate whether alcohol can be purchased within the county, and the state imposes age limits on consumption. For each year, we observe the county’s alcohol regulations and the county-specific rates of illicit-drug-related arrests. We also observe each county’s drug-related deaths between 1980 and 1988. Over the 19-year period in the panel, 26 counties changed to allowing local alcohol access, and the statewide legal drinking age for beer increased twice, from 18 to 19 and from 19 to 21. The empirical literature finds that these types of access restrictions reduce alcohol consumption and alcohol-related harms.¹ We exploit these discrete policy changes to consider whether there is an unintended effect on illicit-drug-related outcomes.²

¹ We expect the implicit price of alcohol to decrease significantly when a Texas county changes from dry to wet owing to the “price” associated with driving to a neighboring county to obtain alcohol, having a designated driver, obtaining a hotel room, and/or incurring the increased risk of being arrested for driving under the influence. The empirical literature confirms that these types of access regulations impact consumption. Henry J. Saffer & Michael Grossman, *Drinking Age Laws and Highway Mortality Rates: Cause and Effect*, 25 *Econ. Inquiry* 403 (1987), for example, finds that state minimum-drinking-age laws have significant effects on highway safety, while Christopher S. Carpenter, *How Do Zero Tolerance Drunk Driving Laws Work?* 23 *J. Health Econ.* 61 (2004), finds that strict laws regulating underage drinking and driving reduce binge alcohol consumption among young men by 13 percent. At the intrastate level, Russell G. Winn & David Giacomassi, *Effects of County-Level Alcohol Prohibition on Motor Vehicle Accidents*, 74 *Soc. Sci. Q.* 783 (1993), and Robert W. Brown, R. Todd Jewell, & Jerrell Richer, *Endogenous Alcohol Prohibition and Drunk Driving*, 62 *S. Econ. J.* 1043 (1996), find that counties’ alcohol access laws in Kentucky and Texas have substantial effects on highway fatalities and accidents. Also see Reagan Baughman *et al.*, *Slippery When Wet: The Effect of Local Access Laws on Highway Safety*, *J. Health Econ.* 1089 (2001); Thomas S. Dee, *State Alcohol Policies, Teen Drinking and Traffic Fatalities*, 72 *J. Pub. Econ.* 289 (1999); Christopher J. Ruhm, *Alcohol Policies and Highway Vehicle Fatalities*, 15 *J. Health Econ.* 435 (1996); David N. Figlio, *The Effect of Drinking Age Laws and Alcohol-Related Crashes: Time Series Evidence from Wisconsin*, 14 *J. Pol’y Analysis & Mgm’t* 555 (1995); and Saffer & Grossman, *supra*.

² Because no reliable price index for illicit drugs exists (*Informing America’s Policy on Illegal Drugs: What We Don’t Know Keeps Hurting Us* (Charles F. Manski, John V. Pepper, & Carol V. Petrie eds. 2001); Joel Horowitz, *Should the DEA’s STRIDE Data Be Used for Economic Analyses of Markets for Illegal Drugs?* 96 *J. Am. Stat. Assoc.* 1254 (2001)), much of the previous literature evaluates discrete changes in regulations (for example, marijuana decriminalization). Alternatively, some researchers use unrepresentative price data collected as part of Drug Enforcement Administration undercover operations.

The few empirical studies that considered spillover effects of alcohol prices and regulations on illicit-drug markets have drawn conflicting conclusions. Frank Chaloupka and Adit Laixuthai and John DiNardo and Thomas Lemieux find that alcohol and marijuana are economic substitutes; Rosalie Pacula and Jenny Williams and colleagues find that they are complements; Clifford Thies and Charles Register find that they are statistically unrelated.³ The estimated effects are not robust to different specifications and different data on consumption and price.⁴

We make two substantive contributions to this inconclusive literature. First, we exploit the panel data to explicitly account for the potentially endogenous formation of access laws. The existing research on the complementarities of alcohol and drug consumption treats both regulations and prices as exogenous. Local access laws, however, are not likely to be randomly selected.⁵ Using a series of fixed-effects models, we are able to control for unobserved county-specific factors that may influence the formation of access laws and the illicit-drug-related outcomes.

The second contribution is that we do not rely on self-reported measures

³ Frank J. Chaloupka & Adit Laixuthai, Do Youths Substitute Alcohol and Marijuana? Some Econometric Evidence, 23 E. Econ. J. 253 (1997); John DiNardo & Thomas Lemieux, Alcohol, Marijuana, and American Youth: The Unintended Consequences of Government Regulation, 20 J. Health Econ. 991 (2001); Rosalie Liccuardo Pacula, Does Increasing the Beer Tax Reduce Marijuana Consumption? 17 J. Health Econ. 557 (1998); Jenny Williams *et al.*, Alcohol and Marijuana Use among College Students: Economic Complements or Substitutes? (Working Paper No. w8401, Nat'l Bur. Econ. Res. 2001); and Clifford F. Thies & Charles A. Register, Decriminalization of Marijuana and the Demand for Alcohol, Marijuana and Cocaine, 30 Soc. Sci. J. 385 (1993).

⁴ Studies using similar consumption and price measures do not yield consistent findings. In fact, all of these studies rely on direct measures of use (not quantity consumed), and some also include indirect proxies such as traffic accidents to check the robustness of the findings. All of these analyses use alcohol and drug regulations such as minimum legal drinking ages, local access restrictions, and decriminalization of marijuana as measures of the implicit price. Some also use more direct measures such as average prices for different types of alcohol and marijuana. Furthermore, conflicting results are found both within and between data sets and researchers. DiNardo & Lemieux, *supra* note 3, discusses some of these issues in further detail. However, no one has yet to carefully evaluate the reasons for these different findings.

⁵ In fact, a growing body of literature suggests that access laws are not exogenous. A recent study, Koleman S. Strumpf & Felix Oberholzer-Gee, Endogenous Policy Decentralization Testing the Central Tenet of Economic Federalism, 110 J. Pol. Econ. 1 (2002), specifically assesses the decision of counties to adopt access laws and finds that these decisions are both strategic and associated with characteristics of the community. Steven Coate & Michael Conlin, A Group Rule—Utilitarian Approach to Voter Turnout: Theory and Evidence, 94 Am. Econ. Rev. 1476 (2004), uses the outcomes of the local Texas liquor law referenda (proposing that a dry jurisdiction become wet) to estimate alternative voting models. While the decision to have a referendum is not modeled, the study does find that Baptists and people in more urban communities are less likely to support the referendum, while single males are more likely. The recent empirical literature assessing the effect of these laws on consumption and alcohol-related harms also finds evidence of nonrandom selection (see, for example, Baughman *et al.*, *supra* note 1; Dee, *supra* note 1; Rhum, *supra* note 1; and Badi H. Baltagi & James M. Griffin, A Dynamic Demand Model for Liquor: The Case for Pooling, 77 Rev. Econ. & Stat. 545 (1995). Standard cross-section estimates that assume exogenous selection appear to substantially overstate the effects of alcohol control policies on alcohol consumption.

of drug use but instead focus on other outcomes of interest, namely, illicit-drug-related arrests and mortality. The previous literature relies on self-reported surveys of the prevalence of use during specific periods (for example, the past 30 days). Thus, inferences drawn using these prevalence measures reveal only the price elasticity of use, not the price elasticity of consumption. Although understanding the effects of alcohol regulations and prices on illicit-drug use or frequency of use is an important concern, many of the adverse individual and social consequences arise from misuse or overuse of drugs, as proxied for by crime and mortality.

Furthermore, even if use is an important outcome of interest, self-reported surveys of illicit-drug use invariably lead to systematic reporting errors and produce biased inferences.⁶ Whether self-reporting errors explain the divergent results in the existing literature is uncertain. There is consensus, however, that self-reports of drug use result in biases that are not accounted for in this literature.

After describing the data in Section II, we evaluate the effects of local alcohol access laws on the illicit-drug-related crime rate in Section III. Counties allowing access to alcohol have higher average drug-related crime rates. This association, however, appears to be spurious: after accounting for county-specific fixed effects and county-specific linear time trends, we find that local alcohol access has a substantial negative effect on drug-related arrests. Unobserved county-specific factors qualitatively affect the estimated parameters. Once we account for these factors, we find alcohol access and illicit-drug-related crimes to be substitutes.

To further assess the relationship between access and illicit-drug-related outcomes, we consider two alternative analyses. In Section IV, we evaluate the effect of the minimum legal drinking age, which presumably increases the implicit price of alcohol more for juveniles in wet counties than those in dry counties, on the fraction of drug-related arrests involving juveniles, and in Section V, we estimate the effects of local access on drug-related mortality. Both analyses provide further evidence that alcohol access reduces illicit-drug-related outcomes. In Section VI, we conclude.

II. DATA DESCRIPTION

The data are a panel of observations on the 254 Texas counties over the period 1978–96. For each county and each year, we observe three illicit-drug-related outcomes: the numbers of arrests per 1,000 individuals for traffic

⁶ John V. Pepper, How Do Response Problems Affect Survey Measurement of Trends in Drug Use? in Manski, Pepper, & Petrie eds., *supra* note 2, at 321; Lloyd D. Johnston, Patrick M. O'Malley, & Jerald G. Bachman, 1 National Survey Results on Drug Use from the Monitoring the Future Study, 1975–1997 (1998); The Validity of Self-Reported Drug Use: Improving the Accuracy of Survey Estimates (Lana Harrison & Arthur Hughes eds. 1997); and Lana Harrison, The Validity of Self-Reported Drug Use in Survey Research: An Overview and Critique of Research Methods, in Harrison & Hughes eds., *supra*, at 17.

and possession of illicit drugs, the fraction of illicit-drug-related arrests involving juveniles, and the number of direct drug-related deaths per 1,000 individuals.⁷ In addition to these outcome variables, we observe the local access laws. Local jurisdictions in Texas can regulate if alcohol can be purchased within the county. For each year, we observe the county's alcohol regulations, and we label a county as "wet" if some alcoholic beverages can be purchased in part of the country and "dry" otherwise. In total, 53 of the 254 counties in Texas were dry at the start of the period in 1978, and 26 of these legalized some type of alcohol sales by 1996.⁸ Finally, we also observe a number of relevant characteristics of the county. In particular, our data include annual measures of police expenditures, population, per capita income, religious affiliation, and percentage of population less than 21 years old.

Table 1 displays basic descriptive statistics for the variables used in the analysis (see Appendix Table A1 for details on data sources). The first column of sample statistics provides means for the all counties across all years, while the second and third columns display the sample means for counties that changes access status and those where access regulations remained fixed over the 19-year period, respectively. Counties that changed access status have, on average, somewhat lower rates of drug-related arrests and mortality and a slightly higher fraction of arrests involving juveniles, but all of these differences are statistically insignificant. Likewise, counties that changed from dry to wet have lower average police expenditures and fractions of Catholics and higher fractions of Baptists. Although substantial, these differences are generally statistically insignificant. The one notable exception is the county population: counties that switch status over the period are substantially (and statistically significantly) smaller than those that did not.

The differences between counties that changed access status is further explored in Figure 1, which depicts the trends in drug arrests by counties that changed status from dry to wet, for counties that were always wet, and for counties that were always dry. Clearly, drug arrests per 1,000 individuals are greater on average in wet counties. Drug arrests per 1,000 individuals in dry counties and counties that changed status are similar in the early years of the data. However, beginning in the early 1980s, drug arrests per 1,000

⁷ This measure includes deaths directly linked to illicit-drug use following the International Code of Diseases, Revision 9 (National Institutes of Health, Assessing Drug Abuse within and across Communities, table B-1 (1997)). These data come from death certificates filed in the state and reported to the National Center for Health Statistics. These are all deaths for the years 1980 and 1983–88 and 50-percent samples for 1981 and 1982. After 1988, the public use versions of these data do not identify counties with fewer than 100,000 persons. We replicate our results with similar data directly from the state of Texas for 1988, 1994, 1995, and 1996 and find consistent results. These results are available upon request.

⁸ Over the period, there were 189 passing referenda, many of which legalized sales for a town or justice precinct in a county that already allowed sales elsewhere within its borders.

TABLE 1
DESCRIPTIVE STATISTICS: MEANS AND STANDARD DEVIATIONS

Variable	All Counties	Counties That Changed Status	Counties That Did Not Change Status
Outcome variables (<i>Y</i>):			
Drug-related arrests per 1,000 individuals	.369 (1.098)	.282 (.355)	.379 (1.153)
Marijuana-related arrests per 1,000 individuals ^a	.282 (1.069)	.213 (.275)	.290 (1.124)
Other illicit-drug-related arrests per 1,000 individuals	.087 (.119)	.069 (.107)	.089 (.120)
Drug arrests involving possession per 1,000 individuals	.314 (1.037)	.227 (.336)	.324 (1.089)
Drug arrests involving sales/manufacturing per 1,000 individuals	.055 (.140)	.055 (.087)	.055 (.145)
Fraction of illicit-drug-related arrests involving juveniles ^b	.290 (.184)	.291 (.167)	.286 (.228)
Fraction of marijuana-related arrests involving juveniles ^b	.314 (.193)	.314 (.206)	.314 (.192)
Fraction of other drug-related arrests involving juveniles ^b	.199 (.213)	.179 (.231)	.202 (.211)
Drug-related deaths per 1,000 individuals ^c	.0031 (.0067)	.0026 (.0067)	.0031 (.0067)
Regulations:			
Wet (= 1 if county allows the sale of any alcohol)	.75	.61	.90
Other covariates (<i>X</i>):			
Expenditures on police (\$ millions)	4.910 (25.39)	.934 (1.251)	5.363 (26.76)
Population (100,000)	648.2 (2,348)	189.7 (211.7)	700.5 (2,472)
Income per capita (\$100,000)	.128 (.044)	.131 (.040)	.127 (.045)
Percent Catholic	.239 (.244)	.113 (.131)	.254 (.250)
Percent Baptist	.439 (.190)	.517 (.142)	.430 (.193)
Fraction of population under age 21	.333 (.442)	.327 (.491)	.314 (.192)
<i>N</i>	4,826	494	4,332

^a For people over 20 years old, 54 percent of the drug arrests were for marijuana. For people under 21 years old, 69 percent of the drug arrests were for marijuana.

^b These outcome variables measure the fraction of the total number of arrests involving juveniles. When the total number of arrests equal zero, the county is dropped from the analysis.

^c Only 1980–88. *N* = 2,277.

individuals in the counties that changed status are consistently greater than in dry counties.⁹

Figures 2 and 3 display the analogous time series for the two other illicit-drug-related outcomes, namely, the fraction of arrests involving juveniles (Figure 2) and illicit-drug-related mortality per 1,000 individuals (Figure 3). These figures do not reveal clear differences between the different types of counties. Figure 2 shows that the average fraction of drug arrests involving juveniles does not noticeably differ on the basis of a county's alcohol status

⁹ The dramatic spike in the average number of drug arrests for counties with wet status in 1986 and 1987 is attributable to Kenedy County. Kenedy has a population of approximately 4,000, and the large number of drug arrests in 1986 and 1987 caused the number of drug arrests per 1,000 individuals to increase from .54 in 1984 to 49.0 in 1987. In specification tests available from the authors, we find that the results do not vary when we exclude Kenedy County.

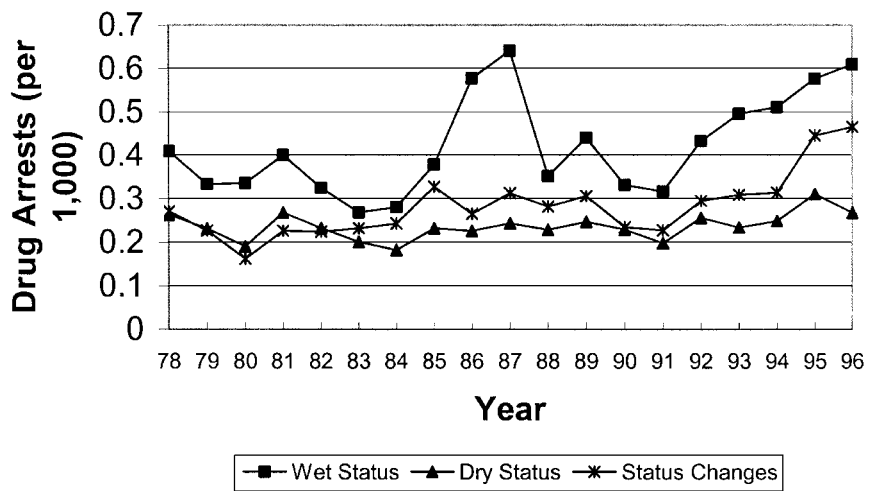


FIGURE 1.—Trends in total drug arrests by county alcohol access policy

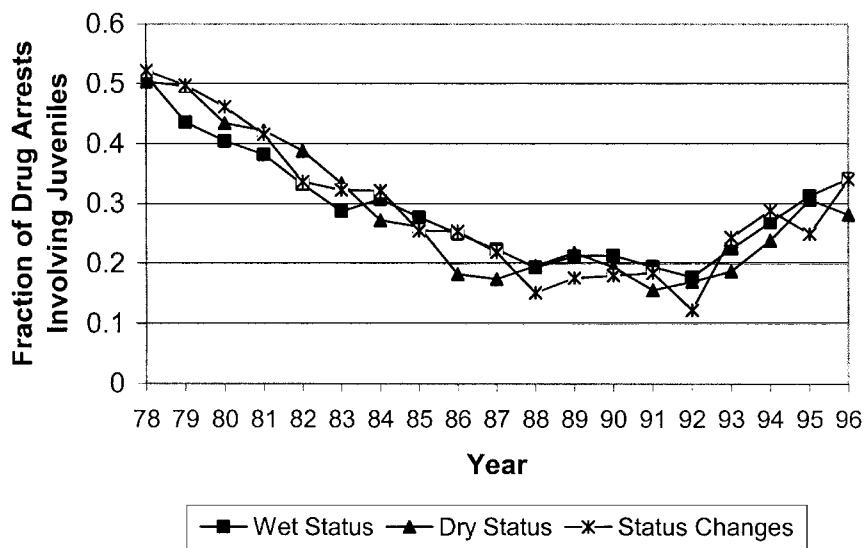


FIGURE 2.—Trends in juvenile drug arrests by county alcohol access policy

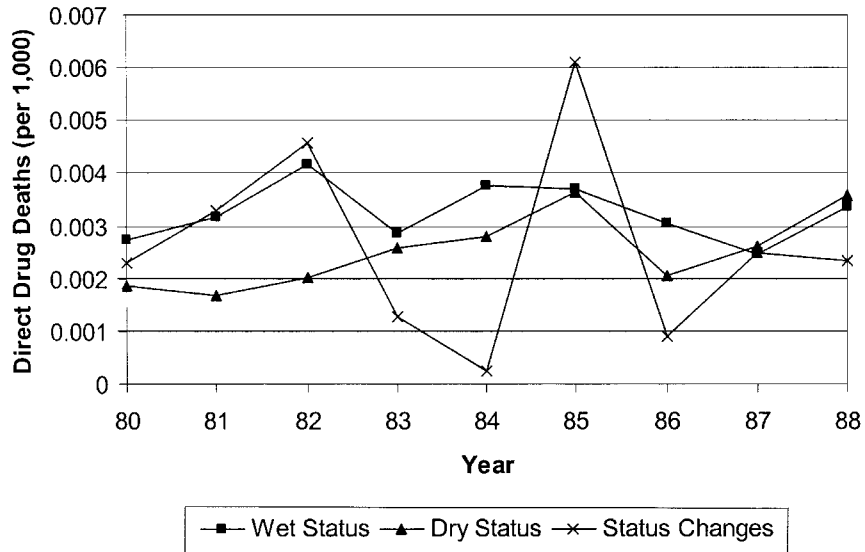


FIGURE 3.—Trends in drug-related mortality by county alcohol access policy

or whether a county changed status. Figure 3 shows no distinct pattern in the average county mortality trends for counties that changed status relative to those that did not.

Our regression analysis in Sections III–V will control for unobserved county fixed effects. In this framework, the parameters are identified by differences in the county-specific growth rates, rather than level differences, of illicit-drug-related outcomes. Table 2 displays the average annual growth rates in the three outcome variables for counties that switched status and those that did not. The only substantial difference is in the annual growth rates of drug-related mortality, but the large standard errors imply that these are not significantly different. The average growth rates in the arrest rate measures are neither economically nor statistically different between counties that change from dry to wet and counties that never change. Apparently, changes in the growth rates of the illicit-drug-related outcome measures are similar, or at least statistically indistinguishable, in counties that change status and those that do not.

III. THE EFFECT OF LOCAL ALCOHOL PROHIBITION ON ILLICIT-DRUG-RELATED ARRESTS

On average, dry counties have fewer drug-related arrests per 1,000 individuals than counties allowing the sale of alcohol. To evaluate whether this observed relationship reflects the effects of access laws, we estimate a series

TABLE 2
ANNUAL GROWTH RATES IN ILLICIT-DRUG-RELATED OUTCOMES

Outcome Variables	All Counties	Counties That Changed Status	Counties That Did Not Change Status
Drug-related arrests per 1,000 individuals	.278 (1.566)	.292 (1.214)	.276 (1.602)
Fraction of illicit-drug-related arrests involving juveniles ^a	.095 (.826)	.126 (.902)	.092 (.817)
Drug-related deaths per 1,000 individuals ^b	-.262 (.846)	-.731 (.594)	-.244 (.849)

NOTE.—Standard errors are in parentheses. The growth rates cannot be calculated when the base year observation is zero.

^a These outcome variables measure the fraction of the total number of arrests involving juveniles. When the total arrests are zero, the county is dropped from the analysis.

^b Only 1980–88.

of linear mean regression models that account for observed and unobserved county-specific characteristics. In Section IIIA, we outline the basic fixed-effects model that explicitly accounts for unobserved county-specific factors that may be related to both drug arrest rates and alcohol policy. In Section IIIB, we present and discuss the estimates from a model that evaluates the effect of alcohol access on total illicit-drug-related arrests per 1,000 individuals. We begin by replicating the earlier research that effectively pools the data and then extend the analysis to exploit the panel. Finally, we evaluate whether the basic findings are robust to different outcome measures. In particular, we distinguish between marijuana and other drugs and between arrests for possession and arrests for trafficking. Section IIIC concludes by discussing the implications of these findings.

A. Model

Formally, consider the linear model

$$Y_{it} = \alpha_t + \beta_1 \text{WET}_{it} + \beta_2 \mathbf{X}_{it} + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the observed number of drug-related arrests per 1,000 individuals for county i in year t , WET_{it} is a dummy variable that equals one if county i allows any sales of alcoholic beverages in year t , and \mathbf{X}_{it} is the observed vector of other county i characteristics in year t that are likely to influence the number of arrests. These include police expenditures, percentage of Catholic residents, percentage of Baptist residents, population, and per capita income. The parameters α_t , β_1 , and β_2 are unobserved, with α_t being a year fixed effect. The random variable ε_{it} measures unobserved factors influencing arrest rates.

Our interest is in learning β_1 , the effect of allowing the sale of alcohol within a county on drug-related crime. The existing literature on the com-

TABLE 3
ESTIMATED EFFECT OF ALCOHOL ACCESS ON CRIMES PER 1,000 INDIVIDUALS
ASSOCIATED WITH ALL DRUGS

	Model 1	Model 2	Model 3
County status wet	.07 (.04)	-.05 (.06)	-.16 (.09)
R^2	.02	.22	.26
County fixed effects	No	Yes	Yes
County time trend	No	No	Yes

NOTE.—Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity and correlation within counties over time. The covariates included in the regression are police expenditures, percentage of Catholic residents, percentage of Baptist residents, population, and per capita income. All models include year fixed effects. $N = 4,826$.

plementarities in consumption of alcohol and other drugs assumes that price variables are exogenous. In this case, the conditional expectation of the residual, ε_{it} , is independent of the access status.

Arguably, however, the unobserved factors, ε_{it} , influencing arrests are not independent of unobserved factors associated with the county-specific local access laws, WET_{it} , our proxy for the price of alcohol. For instance, addictions, emotional status, religious convictions, economic status, and human capital characteristics of the individuals in a county may all influence the counties' regulations and crime rates. If some of these confounders are unobserved, the correlation between regulations and outcomes would be spurious.

To account for this identification problem, we allow for the expectation of the unobserved factors to vary across county and time as follows:

$$E[\varepsilon_{it} | WET, X] = C_i + T_i t. \quad (2)$$

Equations (1) and (2) reveal a mean regression that includes a time fixed effect, α_t , a county fixed effect, C_i , and a county-specific linear time trend, T_i . Thus, the model explicitly accounts for unobserved time- and county-specific factors that might jointly influence local access laws and the arrest rates.

We use least-squares estimation to derive consistent estimates of the parameters. We also present robust standard errors that allow for arbitrary heteroskedasticity and correlation within counties over time.

B. Results

Table 3 presents the estimated effect of allowing local alcohol access on drug-related crime. The table presents estimates from three specifications—without county effects, with county fixed effects, and with both county fixed effects and county-specific linear time trends.¹⁰

¹⁰ Other county characteristics generally have the expected effect. For example, an additional \$1 million in police expenditures is associated with .001 more arrests per 1,000 individuals on average. These results are available from the authors.

The estimated effect of local alcohol access on drug-related arrests is highly sensitive to whether we account for unobserved county-specific effects. Model 1 confirms our findings from Figure 1 that counties with local alcohol access have higher drug-related criminal activity. Counties with local access to alcohol sales have, on average, .07 more illicit-drug-related arrests per 1,000 individuals than counties without local access. Once we account for county fixed effects, however, this observed association appears spurious. Instead of having a positive impact on the number of arrests, the estimates from the fixed-effects models imply that local access decreases the number of illicit-drug-related arrests. The model 2 estimate implies a negative ($-.05$) but statistically insignificant effect of local alcohol access on illicit-drug arrests. The model 3 estimate of $-.16$ is statistically significant at the 10 percent level. Apparently, local access decreases the expected number of drug-related arrests, which suggests that alcohol and drugs are substitutes in consumption.¹¹ Moreover, these estimated effects are substantial. Consider the change in drug-related arrests resulting from all counties going from dry to wet. Using the point estimates from models 2 and 3, this simple policy simulation reveals that providing local alcohol access in all counties would reduce annual illicit-drug-related arrests by between 9 and 30 percent.¹²

To further explore these results, we evaluate whether the basic findings are consistent across different drug-related arrests measures. In particular, we disaggregate illicit-drug arrests into violations associated with different types of drugs, that is, marijuana versus other illicit drugs, and different types of offenses, that is, traffic versus possession. Table 4 displays the coefficient estimates for the four different outcome measures under the model 3 as-

¹¹ Running the same regressions with alcohol arrests as the dependent variable reveals that access has a positive effect on alcohol-related arrests. The point estimate on access is .21 in model 2 (with county fixed effects) and .01 in model 3 (with county time trend), although both estimates are statistically insignificant at the 5 percent level. While this finding is consistent with the basic substitution hypothesis, arrests are not likely to be a good proxy for alcohol consumption in response to changes in alcohol access laws. Alcohol-related arrests consist of three different offenses: open liquor, public intoxication, and driving under the influence. By allowing individuals to buy liquor within the county, local access may actually decrease the total distance individuals under the influence are driving even if consumption increases. In fact, Baughman *et al.*, *supra* note 1, finds that allowing the sale of beer and wine decreased a county's alcohol-related accidents. Likewise, allowing liquor stores and bars in a county may also decrease the number of open-liquor violations and public intoxication arrests. Instead of drinking in a park or some other public location, individuals may now have the option to drink at a bar.

¹² Since this study is the first to assess the effects of access on drug-related arrests, it is difficult to compare the specific outcomes with the results from the existing literature. However, the basic findings appear to be generally consistent with the estimates reported in the previous literature that finds evidence of substitution. In particular, Chaloupka & Laixuthai, *supra* note 3, finds that decriminalizing marijuana reduces the number of frequent drinkers by 11 percent, the number of nonfatal traffic accidents by 7.5 percent, and the number of fatal accidents by around 6 percent. DiNardo & Lemieux, *supra* note 3, in reduced-form regressions, finds that increasing the minimum drinking age from 18 to 21 increases the prevalence of marijuana consumption by around 2.4 percent.

TABLE 4
ESTIMATED EFFECT OF ALCOHOL ACCESS ON CRIMES PER 1,000 INDIVIDUALS
ASSOCIATED WITH DRUGS (Model 3)

	Marijuana	Hard Drugs	Possession	Sales/Manufacturing
County status wet	-.10 (.07)	-.06 (.02)	-.15 (.08)	-.017 (.011)
R^2	.24	.59	.26	.17

NOTE.—Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity and correlation within counties over time. The covariates included in the regression are police expenditures, percentage of Catholic residents, percentage of Baptist residents, population, and per capita income. All measures include year and county fixed effects and county time trends. $N = 4,826$.

sumptions. In each case, local access decreases the expected number of illicit-drug-related arrests. The fact that we find a negative coefficient for all four outcome measures suggests that the basic qualitative finding is robust. Alcohol access appears to decrease illicit-drug-related arrests.

Furthermore, the relative magnitudes of the disaggregated coefficients are consistent with what might be expected if illicit drugs and alcohol are substitutes. Arrests for hard drugs are less sensitive to local liquor access than arrests for marijuana, a drug that arguably lends itself more to experimentation and is more likely to be used as a substitute for alcohol than are hard drugs. Arrests for sales and manufacturing are substantially less sensitive than arrests for possession, which is likely more closely tied to consumption within the county than is sales and manufacturing.

C. Discussion

The estimates from the fixed-effects models presented in Table 3 suggest that alcohol access regulations are not random. This finding represents an important contribution to the existing literature that has maintained the exogenous-selection assumption.

While accounting for the potential endogeneity of local access laws is an important innovation to the literature, the parameters of interest may still not be identified. The models we estimate account for unobserved county-specific factors that are fixed or vary linearly over time. It may be, however, that unobserved county-specific factors associated with access laws vary nonlinearly over time. The most likely candidate for a confounding explanation is that the police change their behavior in response to local access policies. Although changes in the level of police expenditures are observed and controlled for, changes in the distribution of spending and in the methods of surveillance are unobserved. Thus, even after accounting for county-specific unobserved effects, the estimated parameters may reflect variation in enforcement patterns rather than access laws. Unfortunately, this level of detail on police effort is not available in the data.

The fact that the basic qualitative finding holds for different types of illicit

drugs and different types of offenses provides modest evidence that the results are not spurious. Presumably, changes in the distribution of police resources or other nonlinear variation in county factors would have different effects on each of the disaggregated measures of illicit-drug arrests. Yet, in all four cases we find that the effect of access on illicit-drug-related arrests is negative.

In the remaining sections of the paper, we further explore the relationship between access and illicit-drug-related outcomes using two alternative analyses that are arguably even less sensitive to unobserved confounders. In the first case, we effectively introduce a control group, namely, adults, to account for unobserved factors that vary within a county across years, and in the second, we consider an outcome, namely, illicit-drug-related mortality, that is likely to be independent of the unobserved factors that may influence arrest rates.

IV. THE EFFECT OF THE MINIMUM DRINKING AGE ON THE FRACTION OF JUVENILE ARRESTS

Certain alcohol regulations target juveniles. In particular, laws instituting a minimum drinking age restrict alcohol sales and consumption to persons above some age threshold. Throughout the panel, the state of Texas maintained a legal drinking age of 21 for hard liquor and wine (that is, beverages with greater than 4 percent alcohol). For beer, however, there were three distinct regimes: through 1981, the minimum drinking age was 18; from 1982 through 1986, the minimum drinking age was 19; and since 1987, the minimum drinking age has been 21.

A legal drinking age of 21 years old prohibits juveniles in wet counties from purchasing beer. Thus, in wet counties, laws regulating the legal drinking age have differential effects on the implicit price of alcohol for juveniles and adults.¹³ Increasing the drinking age increases the implicit price of liquor for juveniles but not for adults.¹⁴ In contrast, juveniles in dry counties cannot purchase beer from local distributors regardless of the drinking age. Thus, increasing the drinking age is likely to increase the implicit price of liquor relatively more for juveniles in wet counties than for juveniles in dry counties.

To evaluate the effects of minimum-drinking-age laws on illicit-drug-related crimes, we consider the differential effect of these policies on the fraction of all drug-related arrests involving juveniles in wet versus dry

¹³ Many studies have found that juveniles are more sensitive to prices and regulation than adults (Gary S. Becker, Michael Grossman, & Kevin M. Murphy, *An Empirical Analysis of Cigarette Addiction*, 84 *Am. Econ. Rev.* 396 (1994); and Frank J. Chaloupka, *Rational Addictive Behavior and Cigarette Smoking*, 99 *J. Pol. Econ.* 722 (1991)) and attribute this differential effect to peer influences and the addictive properties of alcohol and illicit drugs.

¹⁴ The effect of local access on relative prices is less certain. Allowing local alcohol access is likely to change the price of alcohol for juveniles and adults. Whether the decrease in the implicit price of alcohol resulting from local access is greater for adults or juveniles is not obvious.

TABLE 5
ESTIMATED EFFECT OF ALCOHOL ACCESS ON THE FRACTION OF DRUG-RELATED ARRESTS
INVOLVING JUVENILES (under Age 21)

	ALL DRUGS			MODEL 3, MARIJUANA
	Model 1	Model 2	Model 3	
County status wet	-.034 (.018)	-.037 (.024)	.024 (.043)	.018 (.043)
County status wet × 19	.040 (.022)	.043 (.023)	.025 (.026)	.031 (.027)
County status wet × 21	.055 (.022)	.055 (.023)	.017 (.035)	.038 (.037)
R ²	.28	.38	.44	.42
County fixed effects	No	Yes	Yes	Yes
County time trend	No	No	Yes	Yes

NOTE.—Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity and correlation within counties over time. The covariates included in the regression are police expenditures, percentage of Catholic residents, percentage of Baptist residents, population, the fraction of a county's population under the age of 21, and per capita income. All models include year fixed effects. $N = 4,826$.

counties.¹⁵ In particular, we interact the wet status variable with two indicator variables pertaining to the legal drinking age for beer: the first equals one if only people under the age of 19 are prohibited from drinking beer (1982–86), and the second equals one if the minimum drinking age is 21 (1987–96). We also add a covariate measuring the fraction of a county's population under the age of 21. Otherwise, by including time fixed effects, county fixed effects (model 2), and linear county-specific time trends (model 3), we replicate the model described in equations (1) and (2).

Before turning to the results, it is important to discuss the conditions under which this model is identified. As before, the model accounts for relevant observed factors (see Table 1) as well as for county fixed effects and linear county-specific time trends. Moreover, in this framework, adults effectively serve as a comparison group to net out county-year factors related to illicit-drug-related crimes and local access.¹⁶ By evaluating the ratio rather than the level of juvenile arrests, unobserved nonlinear changes in policing (or other factors) will bias the estimators only if these factors have different effects on juveniles and adults and on wet counties and dry counties. This seems unlikely. As long as the minimum drinking age affects how police target drug use by adults in a manner similar to that by juveniles or in the same way for wet and dry counties, the model is identified.

Table 5 presents the estimated effect of drinking-age restrictions on the fraction of drug-related arrests involving juveniles. The legal drinking age increases the fraction of drug-related arrests involving juveniles more in wet

¹⁵ The effect of the minimum drinking age on the fraction of drug-related arrests involving juveniles in wet counties is not separately identified from the year fixed effects. Rather, all we can identify is the effect of the minimum drinking age in wet or dry counties.

¹⁶ Carpenter, *supra* note 1, uses a similar identification strategy to assess the effects on consumption of alcohol regulations targeted toward juveniles.

counties than in dry counties. In model 2, for instance, the estimated effects of increasing the drinking age for beer from 18 to 19 and from 18 to 21 are .043 and .055 greater in wet counties, respectively. The model 3 estimates, which include county-specific linear time trends, also imply a positive effect of the minimum drinking age on the fraction of illicit-drug arrests involving juveniles in wet counties, although the point estimates are somewhat smaller and less precise than those from model 2. Because the average fraction of drug-related arrests attributed to juveniles is .29, these estimated effects are substantial. If we assume that increasing the minimum drinking age increases the implicit price of liquor more for juveniles in wet counties than in dry counties, the results in Table 5 confirm our earlier findings that illicit drugs and alcohol are substitutes.

V. THE EFFECT OF LOCAL ALCOHOL PROHIBITION ON DRUG-RELATED DEATHS

Finally, we estimate the relationship between access laws and a different outcome measure: drug-related mortality. Arguably, our analysis of mortality rates provides a conservative test of the basic relationship between access laws and illicit-drug-related outcomes. On the one hand, unobserved variables that may change in response to the adoption of local access laws (such as policing) would seem to have little if any impact on mortality rates. On the other hand, one might not expect to find any noticeable effect of access laws (or any other policy, for that matter) on illicit-drug-related mortality. After all, drug-related mortality is a rare event that may not be closely tied to changes in consumption caused by access and even so may be difficult to detect in these aggregated data.¹⁷ Thus, while the estimated relationships are not likely to be biased by confounding variables, one might not observe any relationship at all.

To evaluate the effect of alcohol access laws on drug-related mortality, we again estimate the models described in equations (1) and (2), where the dependent variable is now the natural logarithm of drug-related deaths per 1,000 individuals.¹⁸ Table 6 displays the estimated results, which are largely consistent with those found for crime. The estimates in model 1 reveal that wet counties have higher average drug-related death rates. As before, how-

¹⁷ During the 9-year period over which we observed county-level mortality data, the illicit-drug mortality rate per 1,000 individuals was .0031 (see Table 1), which reflects just over 4,000 illicit-drug-related deaths. Forty-two counties had no recorded illicit-drug-related deaths. In most counties in most years, there are few if any illicit-drug-related deaths. In some counties and some years, however, the number of illicit-drug-related deaths can exceed 100. Seven counties had over 150 illicit-drug-related deaths during the 9-year period, with Dallas, Harris, and Tarrant Counties having over 500.

¹⁸ We add one to the number of each county's drug-related deaths to address the fact that numerous counties have no drug-related deaths in a year. The results are not sensitive to adding a different small number to the dependent variable.

TABLE 6
ESTIMATED EFFECT OF ALCOHOL ACCESS ON DIRECT DRUG-RELATED
DEATHS PER 1,000 INDIVIDUALS

	Model 1	Model 2	Model 3
County status wet	.024 (.054)	-.137 (.079)	-.039 (.131)
R^2	.67	.86	.88
County fixed effects	No	Yes	Yes
County time trend	No	No	Yes

NOTE.—Standard errors are in parentheses. The standard errors are robust to arbitrary heteroskedasticity and correlation within counties over time. The covariates included in the regression are the natural log of police expenditures, the percentage of Catholic residents, the percentage of Baptist residents, the natural log of population, and the natural log of per capita income. All models include year fixed effects. $N = 2,277$.

ever, once we account for county fixed effects, this observed association appears spurious. Instead of having a positive impact, the estimated fixed-effects models imply that allowing local alcohol access decreases drug-related mortality. In model 2, switching from dry to wet status is estimated to lower drug-related mortality by approximately 14 percent and is statistically significant at the 10 percent level. Including county trends in model 3 gives the same qualitative result, although the point estimate of $-.039$ is smaller and statistically insignificant. Thus, this conservative assessment of the effects of access on illicit-drug-related outcomes confirms our earlier finding. Estimates from both fixed-effects models suggest that decreasing the cost of alcohol reduces mortality related to drug use.

VI. CONCLUSION

Our empirical analysis evaluates an unintended consequence of alcohol prohibitions by exploring the effects of alcohol access on illicit-drug-related arrests and mortality. Using a unique panel data set of counties in the state of Texas, we explicitly account for the endogeneity of regulation and, therefore, price using county fixed-effects models. We find strong evidence that failure to account for county-specific factors results in biased estimates.

Once these factors are accounted for, three distinct analyses reveal the same basic conclusion: alcohol access reduces illicit-drug-related outcomes. First, we find that allowing local alcohol access appears to decrease the prevalence of crimes associated with illicit-drug consumption. Second, we find that prohibiting the sale of beer to persons under 19 and under 21 increases the fraction of drug-related arrests attributed to juveniles more in wet counties than in dry counties. Assuming that increasing the minimum drinking age increases the implicit price of liquor more for juveniles in wet counties than in dry counties, these results also suggest that alcohol and drugs are substitutes. Finally, our last piece of evidence uses drug-related deaths as an alternative measure of drug use. Changing an alcohol access policy to

allow the sale of alcohol decreases drug-related mortality. Apparently, regulations on sinful activities lead to important unintended and possibly counteracting consequences for other deviant behaviors.

APPENDIX

TABLE A1
VARIABLES

Variable	Frequency of Sample	Years	Source
Drug-related arrests	Annual	1978–96	Texas Department of Public Safety
Direct drug-related deaths	Annual	1980–88	U.S. Department of Health and Human Services, National Center for Health Statistics, Multiple Cause of Death (1980–88)
Liquor law status	Annual	1978–96	Texas Alcoholic Beverage Commission
Results of all alcohol policy referenda	Date of referenda	1978–96	Texas Alcoholic Beverage Commission
Police expenditures	Every 5 years	1978–92	Texas Department of Public Safety
Religious participation (Catholic, Baptist)	Every 10 years	1970, 1980, 1990	<i>Churches and Church Membership in the United States</i> (1972, 1982, 1992)
Population	Annual	1978–96	U.S. Department of Commerce, Bureau of Economic Analysis, Regional Economic Information System
Per capita income	Annual	1978–96	U.S. Department of Commerce, Bureau of Economic Analysis, Regional Economic Information System

NOTE.—For variables not available at the annual level (except direct drug-related deaths), we filled in missing values, assuming a constant rate of growth across years.

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