The Effect of Alcohol Prohibition on Illicit Drug Related Crimes: 
An Unintended Consequence of Regulation

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Abstract

This paper evaluates the effect of alcohol access on drug-related crime using detailed information on county and state level changes in access laws in Texas between 1978 and 1996. After controlling for both county and year fixed effects, we find evidence that allowing local alcohol access decreases crimes associated with illicit drugs. We also find that prohibiting the sale of beer to persons under 21 increases the fraction of drug related arrests involving juveniles more in wet counties than in dry counties. Because access decreases the implicit price for alcohol and increasing the minimum drinking age is likely to increase the implicit price of liquor more for juveniles in wet counties relative to those in dry counties, our results suggest that alcohol and illicit-drugs are substitutes.

JEL Classification: I18, H73, R50

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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 impact 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 regulation on illicit drug related arrests. In Texas, local jurisdictions have the power to regulate whether alcohol can be purchased within the county. For each year, we observe the county’s alcohol regulations and the county specific rates of illicit drug related arrests. Over the 22-year period in the panel, 26 counties changed to allowing local alcohol access and the statewide legal drinking age for beer increased twice.

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1 The empirical literature on alcohol regulations, for example, finds consistent evidence that these regulations do in fact reduce alcohol consumption and alcohol related harms. See, for example, Dee, 1999; Ruhm 1996; Baltagi, Baltagi and Griffin, 1995; Figlio, 1995; Grossman, et al., 1993; and Saffer and Grossman, 1987a and 1987b.
from 18 to 19 and from 19 to 21. We exploit these discrete policy changes to consider whether there is an unintended affect on illicit drug related behavior.\(^2\)

The few empirical studies that have considered spillover effects of alcohol regulations on illicit drug markets have drawn conflicting conclusions. Chaloupka and Laixuthai (1997) and DiNardo and Lemieux (1992) find that alcohol and marijuana are economic substitutes; Pacula (1998) finds that they are complements; Thies and Register (1993) find that they are statistically unrelated. Apparently, the empirical relationships are not robust to different specifications and different data on consumption and price.

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.\(^3\) In fact, we find evidence that accounting for unobserved county specific factors qualitatively affects the estimated parameters. Under the exogenous selection assumption, the estimates imply that local alcohol access increases drug related arrests. In contrast, with county fixed effects, the estimates suggest that alcohol access decreases drug-related arrests.

The second contribution is that we do not rely on self-reported measures of drug use, but instead focus on another outcome of interest, namely illicit drug related arrests. The previous literature

\(^2\) Because no reliable price indice for illicit drugs exists (Manski, Pepper, Petrie, 2001; Horowitz, forthcoming), much of the previous literature evaluates discrete changes in regulations (e.g., marijuana decriminalization). Alternatively, some researchers use unrepresentative price data collected as part of the Drug Enforcement Administration undercover operations.

\(^3\) The literature evaluating the direct effects of alcohol regulations finds that local regulations and prices are likely to be endogenous (Baughman et al, 2001; Strumpf and Oberholzer-Gee, 2001; Dee, 1999; Rhum, 1996; Baltagi and Griffin, 1995).
relies on self-reported survey questions on the prevalence of use during specific periods (e.g., past 30
days). Thus, inferences drawn using these prevalence measures only reveal 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 question, many of the adverse individual and
social consequences arise from misuse or overuse of drugs, as proxied for by crime.

Furthermore, even if use is an important outcome of interest, self-reported surveys of illicit
activity such as drug use invariably lead to systematic reporting errors and produce biased inferences
(Pepper, 2001; Johnston, O’Malley, and Bachman, 1998; Harrison, 1997; Harrison and Hughes, 1997).
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 discuss two different mean regressions. In Section
III, we evaluate the effects of local alcohol access laws on the illicit drug crime rate. With 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 in the county. In Section IV, we evaluate the effect of
the legal drinking age on the fraction of drug related arrests involving juveniles in wet versus dry
counties. These results provide further evidence that alcohol and drugs are substitutes. In Section V, we
conclude.
II. Data Description

The data are a panel of observations on the 254 Texas counties over the period 1978 to 1996. Table 1 displays basic descriptive statistics of the variables used in the analysis.\(^4\) For each year, we observe county specific information on the numbers of arrests per capita for traffic and possession of illicit drugs. To compare our results to those found in the existing literature that almost exclusively focuses on the complementarities between alcohol and marijuana, we distinguish between crimes associated with marijuana and other illicit drugs. Finally, to evaluate the effects of minimum age laws, we measure the fraction of all illicit drug related arrests involving juveniles. For the average Texas county, twenty nine percent of all drug-related arrests in a year involved a juvenile.

In addition to observing the arrest rates associated with illicit drugs, we also observe a number of relevant characteristics of the county. In particular, our data include annual measures of the police expenditures, population, per-capita income, religious affiliations, and percent of population under 21 years old.

Finally, the data summarize local access statutes in Texas.\(^5\) In particular, we label a county as “wet” if some alcoholic beverages can be purchased in part of the county. A county is dry if alcoholic beverages cannot legally be purchased within the county. 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.\(^6\)

Figures 1 and 2 depict the trends in drug arrests per 1,000 individuals and the fraction of drug arrests involving juveniles, respectively. Each figure displays the average arrests rate by counties that

\(^4\) See Appendix Table (1) for details on data sources.

\(^5\) Liquor law referenda can be voted on at the county, justice precinct, city, or town level. Local jurisdictions of Texas vary in terms of whether and where alcohol may be consumed (on and/or off premise) and what type of alcohol (beer and wine or all liquors) may be purchased. We only focus on whether alcohol may be purchased.
changed their status from dry to wet, for counties that were always wet, and for counties that were always dry. Clearly, drug arrests per capita are greater on average in wet counties. Drug arrests per capita 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 capita in the counties that changed status are consistently greater than in dry counties.\(^6\) The average fraction of drug arrests involving a juvenile does not vary appreciably based on the county alcohol status or whether a county changed status. However, this percent does vary across years. Close to fifty percent of all county drug-related arrests in 1978 involved juveniles. This percentage decreased gradually to less than twenty percent by 1991. After 1991, this percentage gradually increased and in 1996 over 30 percent of all county drug-related arrests involved juveniles.

### III. The Effect of Local Alcohol Prohibition on Illicit Drug Related Arrests

On average, dry counties have fewer drug related arrests per capita than counties allowing the sale of alcohol. To evaluate whether this observed relationship reflects the effects of access laws, we estimate a series of linear mean regression models that account for observed and unobserved county specific characteristics. In Section III.A, we outline the basic fixed effect model that explicitly accounts for unobserved county specific factors that may be related to both arrest rates and alcohol policy. In Section III.B, we present and discuss the estimates from a model that evaluates the effect of alcohol

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\(^6\) 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.

\(^7\) 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 the county in 1986 and 1987 resulted in the number of drug arrests per 1,000 individuals to increase from 0.54 in 1984 to 49.0 in 1987.
access on total per-capita arrests associated with illicit drugs. We begin by replicating the earlier research that effectively pools the data and then extend the analysis to exploit the panel. Finally, in Section III.C, 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.

A. Model

Formally, consider the linear model

\[ Y_{it} = \alpha_t + \beta_1 \text{WET}_{it} + \beta_2 X_{it} + \varepsilon_{it} \]  

(1)

where \( Y_{it} \) is the observed number of drug related arrests per capita (1,000) for county \( i \) in year \( t \), \( \text{WET}_{it} \) is a dummy variable that equals one if county \( i \) allows any sales of alcoholic beverages in year \( t \) and \( X_{it} \) 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, percent of residents that are Catholic, percent of residents that are Baptist, population and per capita income. The parameters \( \alpha_t, \beta_1, \) and \( \beta_2 \) are unobserved, with \( \alpha_t \) being a year fixed effect. The random variable \( \varepsilon_{it} \) measures unobserved factors influencing arrest rates.

Our interest is in learning \( \beta_1 \), the effect of allowing the sale of alcohol within a county, on drug related crime. Arguably, however, the unobserved factors, \( \varepsilon_{it} \), influencing arrests are not independent of unobserved factors associated with the county specific local access laws, \( \text{WET}_{it} \), 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. While some of these factors may be observed, others certainly are not. In this case, the observed correlation between regulations and outcomes would be spurious.

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

$$E[\varepsilon_{it}| WET, X] = C_i + T_{it},$$

Equations (1) and (2) reveal a mean regression that includes a time fixed effect, $\alpha_t$, a county fixed effect, $C_i$, and a county specific linear time trend, $T_{it}$. Thus, the model explicitly accounts for unobserved time and county specific factors that might jointly influence local access laws and the arrest rates.

The existing literature on the complementarities in consumption of alcohol and other drugs assumes that price variables are exogenous. In this case, the conditional expectation in Equation (2) does not vary across county: unobserved factors influencing crime are independent of the local regulations. To provide a comparison to the previous literature, we present estimates from this restricted model as well as estimates from the unrestricted model specified in Equations (1) and (2).

We use least-squares estimation to derive consistent estimates of the parameters. We also present robust standard errors that allow for arbitrary heteroscedasticity and correlation within counties over time.
B. Aggregated Outcome: All Illicit Drug Related Arrests

Table 2 presents the estimated effect of allowing local alcohol access on drug related crime along with the associated standard error. 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.\(^8\)

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, 0.07 more illicit drug related arrests per capita (1,000) than counties without local access.

Once we account for county fixed effects, however, this observed association appears spurious. Rather than having a positive impact on arrests, the estimates from the fixed effects models imply that local access decreases illicit drug related arrests. The Model 2 estimates imply a negative but statistically insignificant effect while the Model 3 estimate of \(-0.16\) is both substantial and statistically significant at the 10 percent significance level. This estimate is particularly large in light of the fact that the average drug related crime rate is only 0.37. Apparently, local access decreases the expected number of drug related arrests, suggesting that alcohol and drugs are substitutes in consumption.

\(^8\) Other county characteristics generally have the expected effect. For example, an additional one million dollars in policy expenditures is associated with 0.001 more arrests per capita, on average. These results are available from the authors.
C. **Disaggregated Outcomes: Arrests by Drug and by Offense**

The estimates from the fixed effects models presented in Table 2 suggest that regulations and thus prices are not random. This finding represents an important contribution to the existing literature that has maintained the exogenous selection assumption. Arguably, the models from this literature are not identified.

While accounting for the potential endogeniety 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. Suppose, for example, that the police change their behavior in response to local access policies. Although changes in the level of police expenditures are observed, changes in the distribution of spending and in the methods of surveillance are unobserved. Thus, the estimated parameters may reflect variation in enforcement patterns, rather than access laws.

As a check on the robustness of our results, we evaluate whether the basic findings are consistent across different outcome measures. In particular, we disaggregate illicit drug arrests into violations associated with different types of drugs, marijuana versus other illicit drugs, and different types of offenses, traffic versus possession. Presumably, changes in the distribution of police resources or other nonlinear variation in county factors have different impacts on each of these disaggregated measures. Suppose, for example, that alcohol access leads to increases in surveillance of highways and bars. Holding expenditures fixed, these activities could arguably increase the number of arrests for drug possession and decrease the number of arrests for drug sales and manufacturing.
Table 3 displays the coefficient estimates for the four different outcome measures under the Model 3 assumptions. 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.

IV. The Effect of the Minimum Drinking Age on the Fraction of Juvenile Arrests

Next we turn to an alternative way of identifying the effect of a change in alcohol prices on drug outcomes. Certain alcohol regulations target juveniles. In particular, minimum drinking age laws 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 (i.e., 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 from 1987 through the present, the minimum drinking age is 21. Certainly these legal drinking age laws have differential effects on the implicit price of alcohol for juveniles and adults.\(^9\) In particular, increasing the drinking age increases the implicit price of liquor for juveniles but not for adults.\(^10\) In addition, 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. Increasing the drinking age to 21 years old prohibits juveniles in wet counties from purchasing beer. In contrast, juveniles in dry counties cannot purchase beer from

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\(^{9}\) Many studies have found that juveniles are more sensitive to prices and regulation than adults (Becker et al., 1994; Grossman et al., 1993; Chaloupka, 1991) and attribute this differential effect to peer influences and the addictive properties of alcohol and illicit drugs.

\(^{10}\) The effect of local access on relative prices is less certain. Allowing local alcohol access is likely to change the price of alcohol differently 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.
local distributors regardless of the drinking age, and therefore, increasing the minimum drinking age is not likely to affect their implicit price of beer as much as 18 to 20 year olds in wet counties.

To evaluate the effects of minimum drinking age laws on illicit drug related crimes, we consider the differential affect of these policies on the fraction of all drug related arrests involving juveniles in wet versus dry counties.\footnote{The impact of the minimum drinking age on the fraction of drug related arrests involving juveniles in wet or dry 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 relative to dry counties.} 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 (years 1982 through 1986) and the second equals one if the minimum drinking age is 21 (years 1987 through 1996). 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 fixed and linear county specific time trends. Unobserved nonlinear changes, such as variation in the allocation of police resources, may bias the results. Of course, by evaluating the ratio rather than the level of juvenile arrests, nonlinear changes will only bias the estimators if unobserved factors differentially effect juveniles and adults in wet counties differently than in dry counties. However, if the minimum drinking age affects how police target drug use by adults in a similar manner as drug use by juveniles or in the same way for wet and dry counties, the model is identified.
Table 4 presents the estimated effect of legal drinking age restrictions on the fraction of drug related arrests involving juveniles. Increasing the legal drinking age increases the fraction of drug related arrests involving juveniles more in wet counties than in dry counties. In Model 2, for instance, the estimated effect of increasing the drinking age for beer from 18 to 19 and from 18 to 21 is 0.043 and 0.055 greater in wet counties, respectively. Because the average fraction of drug related arrests attributed to juveniles is 0.29, this increase is substantial. Furthermore, these estimates are statistically significant at the five percent level. Although the magnitudes of these effects decrease when we include county time trends, the qualitative findings remain. Likewise, the last column in Table 4 indicates that this differential effect is similar for the fraction of marijuana related arrests involving juveniles as for all drug related arrests. Assuming 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 4 confirm our earlier findings that illicit drugs and alcohol are substitutes.

V. Conclusion

Our empirical analysis evaluates an unintended consequence of alcohol prohibitions by exploring the effects of alcohol access on illicit drug related arrests. This research makes two contributions to the existing literature. First, we focus on an outcome of interest to policy makers, public health officials, and social scientist, namely crime. While previous studies have primarily focused on the elasticity of use or frequency of use, we study the relationship between regulation and deviant behaviors. Second, using a unique panel data set of counties in the state of Texas, we explicitly account for the endogeneity of regulation and, therefore, price. We find strong evidence that failure to account for county specific factors results in biased estimates. By not accounting for the endogeneity of price and/or regulations,
earlier cross sectional studies may have reported substantially biased estimates of the complementarities in sinful consumption.

Local alcohol access appears to decrease the prevalence of crimes associated with illicit drug consumption. Because local access decreases the implicit price for alcohol, this suggests that alcohol and illicit drugs are substitutes. The effects of the minimum drinking age laws are somewhat more complex. 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 relative to dry counties, these results also suggest that alcohol and drugs are substitutes. Apparently, regulations on sinful activities lead to important unintended and possibly counteracting consequences for other deviant behaviors.
REFERENCES


### Table 1: Descriptive Statistics

<table>
<thead>
<tr>
<th>VARIABLE</th>
<th>MEAN</th>
<th>STANDARD DEVIATION</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Outcome Variables (Y)</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Drug Related Arrests Per Capita (1000)</td>
<td>0.369</td>
<td>1.098</td>
</tr>
<tr>
<td>Marijuana Related Arrests Per Capita (1000)</td>
<td>0.282</td>
<td>1.070</td>
</tr>
<tr>
<td>Other Illicit Drug Related Arrests Per Capita(1000)</td>
<td>0.087</td>
<td>0.119</td>
</tr>
<tr>
<td>Drug Arrests Involving Possession Per Capita (1000)</td>
<td>0.055</td>
<td>0.140</td>
</tr>
<tr>
<td>Drug Arrests Involving Sales/Manu. Per Capita (1000)</td>
<td>0.314</td>
<td>1.037</td>
</tr>
<tr>
<td>Fraction of Illicit Drug Related Arrests Involving Juveniles^2</td>
<td>0.290</td>
<td>0.184</td>
</tr>
<tr>
<td>Fraction of Marijuana Related Arrests Involving Juveniles^2</td>
<td>0.314</td>
<td>0.193</td>
</tr>
<tr>
<td>Fraction of Other Drug Related Arrests Involving Juveniles^2</td>
<td>0.199</td>
<td>0.213</td>
</tr>
<tr>
<td><strong>Regulations</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wet (=1 if county allows the sale of any alcohol)</td>
<td>0.752</td>
<td>0.432</td>
</tr>
<tr>
<td><strong>Other Covariates (X)</strong></td>
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<td></td>
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<tr>
<td>Expenditures on Police</td>
<td>4.910</td>
<td>25.387</td>
</tr>
<tr>
<td>Population</td>
<td>648.2</td>
<td>2348</td>
</tr>
<tr>
<td>Income per capita</td>
<td>0.128</td>
<td>0.044</td>
</tr>
<tr>
<td>Percent Catholic</td>
<td>0.239</td>
<td>0.244</td>
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<tr>
<td>Percent Baptists</td>
<td>0.439</td>
<td>0.190</td>
</tr>
<tr>
<td>Fraction of Population Under the Age of 20</td>
<td>0.333</td>
<td>0.044</td>
</tr>
</tbody>
</table>

Notes:

1. For people over 20 years old, 54% of the drug arrests were for marijuana. For people under 21 years old, 69% of the drug arrests were for marijuana.

2. 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.

3. Police expenditures are in millions of dollars. Population and per capita income are measured in units of 100,000.
### Table 2:
**The Estimated Effect of Alcohol Access on Crimes Per Capita (1000) Associated With Drugs**

N = 4826

<table>
<thead>
<tr>
<th>Illicit Drugs</th>
<th>Model 1 All Drugs</th>
<th>Model 2 All Drugs</th>
<th>Model 3 All Drugs</th>
</tr>
</thead>
<tbody>
<tr>
<td>County Status Wet</td>
<td>0.07 (0.04)</td>
<td>-0.05 (0.06)</td>
<td>-0.16 (0.09)</td>
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<tr>
<td>R²</td>
<td>0.02</td>
<td>0.22</td>
<td>0.26</td>
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<tr>
<td>Year Fixed Effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
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<tr>
<td>County Fixed Effects</td>
<td>NO</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>County Time Trend</td>
<td>NO</td>
<td>NO</td>
<td>YES</td>
</tr>
</tbody>
</table>

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, percent of residents that are Catholic, percent of residents that are Baptist, population and per capita income.
Table 3:
The Estimated Effect of Alcohol Access on Crimes Per Capita (1000) Associated With Drugs

N = 4826

<table>
<thead>
<tr>
<th>Illicit Drugs</th>
<th>Model 3 Marijuana</th>
<th>Model 3 Hard Drugs</th>
<th>Model 3 Possession</th>
<th>Model 3 Sales/Manu.</th>
</tr>
</thead>
<tbody>
<tr>
<td>County Status Wet</td>
<td>-0.10 (0.07)</td>
<td>-0.06 (0.02)</td>
<td>-0.15 (0.08)</td>
<td>-0.017 (0.011)</td>
</tr>
<tr>
<td>( R^2 )</td>
<td>0.24</td>
<td>0.59</td>
<td>0.26</td>
<td>0.17</td>
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<tr>
<td>Year Fixed Effects</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
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<tr>
<td>County Fixed Effects</td>
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<td>YES</td>
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<tr>
<td>County Time Trend</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
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</tr>
</tbody>
</table>

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, percent of residents that are Catholic, percent of residents that are Baptist, population and per capita income.
Table 4:  
The Estimated Effect of Alcohol Access on 
The Fraction of Drug Related Arrests that Involve Juveniles 
(Persons under age of 21)

N = 4826

<table>
<thead>
<tr>
<th>Drug</th>
<th>Model 1 All Drugs</th>
<th>Model 2 All Drugs</th>
<th>Model 3 All Drugs</th>
<th>Model 3 Marijuana</th>
</tr>
</thead>
<tbody>
<tr>
<td>County Status Wet</td>
<td>-0.034 (0.018)</td>
<td>-0.037 (0.024)</td>
<td>0.024 (0.043)</td>
<td>0.018 (0.043)</td>
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<tr>
<td>County Status Wet x 19</td>
<td>0.040 (0.022)</td>
<td>0.043 (0.023)</td>
<td>0.025 (0.026)</td>
<td>0.031 (0.027)</td>
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<tr>
<td>County Status Wet x 21</td>
<td>0.055 (0.022)</td>
<td>0.055 (0.023)</td>
<td>0.017 (0.035)</td>
<td>0.038 (0.037)</td>
</tr>
<tr>
<td>(R^2)</td>
<td>0.28</td>
<td>0.38</td>
<td>0.44</td>
<td>0.42</td>
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</table>

Year Fixed Effects: YES          
County Fixed Effects: NO          
County Time Trend: NO

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, percent of residents that are Catholic, percent of residents that are Baptist, population and per capita income.
Source: Authors’ calculations from Texas Alcoholic Beverage Commission and Texas Department of Public Safety (see Appendix).
<table>
<thead>
<tr>
<th>Variable</th>
<th>Frequency</th>
<th>Year</th>
<th>Source</th>
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<tr>
<td>Drug Related Arrests</td>
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<td>Texas Department of Public Safety</td>
</tr>
<tr>
<td>Liquor Law Status</td>
<td>Annual</td>
<td>1978-1996</td>
<td>Texas Alcoholic Beverage Commission</td>
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<td>Results of all alcohol policy referenda</td>
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<td>1978-1996</td>
<td>Texas Alcoholic Beverage Commission</td>
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<tr>
<td>Police Expenditures*</td>
<td>Every 5 years</td>
<td>1978-1992</td>
<td>Texas Department of Public Safety</td>
</tr>
<tr>
<td>Religious Participation (Catholic, Baptist)*</td>
<td>Every 10 years</td>
<td>1970, 1980, 1990</td>
<td>Churches and Church Membership in the U.S., Glenmary Research Center</td>
</tr>
<tr>
<td>Population</td>
<td>Annual</td>
<td>1978-1996</td>
<td>Bureau of Economic Analysis, Regional Economic Info. System</td>
</tr>
<tr>
<td>Per Capita Income</td>
<td>Annual</td>
<td>1978-1996</td>
<td>Bureau of Economic Analysis, Regional Economic Info. System</td>
</tr>
</tbody>
</table>

* Note: For variables not available at the annual level, we have filled in missing values, assuming a constant rate of growth across years.