Wet Laws, Drinking Establishments, and Crime

D. Mark Anderson
Departmental of Agricultural Economics and Economics
Montana State University

Benjamin Crost
Department of Agricultural and Consumer Economics
University of Illinois

Daniel I. Rees*
Center for Health and Wellbeing
Woodrow Wilson School
Princeton University

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Drawing on county-level data from Kansas for the period 1977-2011, we examine whether plausibly exogenous increases in the number of establishments licensed to sell alcohol by the drink are related to crime. During this period, 86 out of 105 counties in Kansas voted to legalize the sale of alcohol to the general public for on-premises consumption. These counties experienced substantial increases in the total number of drinking establishments (e.g., bars and restaurants). Using legalization as an instrument, this study is the first to provide estimates of the elasticity of crime with respect to drinking establishments. Specifically, we show that a 10 percent increase in drinking establishments is associated with a 3 to 4 percent increase in crime. Reduced-form estimates suggest that legalizing the sale of alcohol to the general public for on-premises consumption leads to a 10 to 12 percent increase in crime.

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*Corresponding author. Center for Health and Wellbeing, Woodrow Wilson School, Wallace Hall, Princeton University, Princeton, NJ 08544-1013. Phone: 609-258-6478. Email: direes@princeton.edu.

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1. INTRODUCTION

There is a strong positive correlation between local alcohol availability, as measured by the density of bars and/or other alcohol outlets, and crime (e.g., Scribner et al. 1999; Zhu et al. 2004; Gruenewald et al. 2006; Toomey et al. 2012). This positive correlation has been interpreted as evidence of causality but could be due to unobserved factors that simultaneously influence both variables. In an effort to break this simultaneity, we exploit changes in Kansas dry laws to examine whether plausibly exogenous increases in the number of establishments licensed to sell alcohol by the drink are related to crime.

Even after the adoption of the Twenty-first Amendment to the U.S. Constitution in 1933, the sale of alcohol for on-premises consumption was prohibited in Kansas.[1] In November of 1986, Kansas voters approved a measure allowing counties to go from "dry" to "wet." This measure garnered a majority of votes in 36 out of 105 counties. As of July 1, 1987, bars and restaurants in these 36 counties were permitted to sell beer, wine, and spirits to the general public provided they derived 30 percent of their gross revenue from food sales (O`Connor 1987; Robbins 1987; St. John 2012b). Between 1987 and 2011, 50 additional counties voted to become wet; by the end of 2011, only 19 counties still prohibited by-the-drink sales of alcohol.

Below, we argue that permitting establishments to sell alcohol by the drink could, in theory, have either a positive or negative impact on crime. For instance, although there is strong evidence that consuming alcohol heightens emotional responses, impairs cognitive functioning,

[1] The prohibition on the sale of alcohol for on-premises consumption appears to have been strictly enforced. Vern Miller, the attorney general of Kansas from 1970 through 1974, went so far as to raid an Amtrak train traveling through Kansas, arresting a bartender and waiter for serving alcohol to passengers (St. John 2012a). In response to this raid and a formal request from the attorney general's office, at least two commercial airlines (Continental and Frontier) stopped serving alcohol while flying over Kansas (United Press International 1973). In 1979, Attorney General Robert Stephen issued an opinion stating that "the Kansas Legislature has no authority to legislate regarding the sale or consumption of alcoholic liquor in the airspace above our state."

and reduces inhibitions (Boles and Miotto 2003; Carpenter and Dobkin 2011), bartenders, bouncers and servers are in a position to enforce social norms against drinking to excess and even prevent arguments from escalating into violence.[2]

Using unique data on all liquor licenses issued by Kansas for the period 1977-2011, we find evidence that legalizing by-the-drink sales led to substantial increases in the number of drinking establishments (e.g., bars and restaurants) in operation. Using legalization as an instrument, we show that a 10 percent increase in drinking establishments is associated with a 3 to 4 percent increase in crime. To our knowledge, this is the first study to provide estimates of the elasticity of crime with respect to drinking establishments.

Reduced-form estimates indicate that legalizing the sale of alcohol to the general public for on-premises consumption is associated with an increase in violent crime of 10 to 12 percent. Legalization is also associated with comparable increases in property crime. Because we find no evidence that crime fell in dry counties when neighboring counties allowed by-the-drink sales, we conclude that bars and restaurants create criminal activity as opposed to simply displacing it. Previous research on local alcohol availability and crime has not been able to distinguish between these competing hypotheses (Carpenter and Dobkin 2011).

2. BACKGROUND

Dry laws take a variety of forms. For instance, they can prohibit the sale of alcohol for on-premises consumption, prohibit any and all alcohol sales, or even prohibit the possession of alcohol. Today, there are over 200 counties in the United States with some type of prohibition

[2] Multiple reviews have concluded that alcohol is more likely to lead to psychopharmacological violence than other substances such as marijuana; experimental studies have shown that alcohol consumption can increase the amount of pain subjects are willing to inflict upon others (Fagan 1993; Chermack and Taylor 1995; Giancoloa 2004).

on alcohol sales in place (Wheeler 2012). The majority are located in the South, although a handful of counties in Kansas still prohibit by-the-drink sales to the general public, and most counties in Kansas require establishments that sell liquor for on-premises consumption to derive 30 percent of their gross revenue from food sales.

Lifting a prohibition on by-the-drink sales to the general public could increase total alcohol consumption through, in effect, allowing restaurants and bars to bundle alcohol with complementary goods and services (Guiltinan 1987; Lawless 1991). However, it could also provide an opportunity to drink in a different "social context involving a mix of circumstances, locations, [and] companions" (Lipsey et al. 1997, p. 250). In many bars and restaurants, heavy drinking is the norm; advertisements, specials, and promotions arguably lead to overindulgence (Kuo et al. 2003; Hastings et al. 2005). In other establishments, the owners, staff, and patrons actively enforce social norms against drinking to excess (Gusfield et al. 1984; Lee et al. 2008).

Regardless of its effect on total alcohol consumption, legalizing by-the-drink sales could impact crime through shifting where consumption takes place. Although previous researchers have, more often than not, argued that social interactions at bars and restaurants serve as a catalyst for violent behavior (Graham and Wells 2001, 2003; Buddie and Parks 2003; Middleton et al. 2010), it is possible that verbal arguments and minor scuffles are actually less likely to escalate into violence if they take place in public. Ethnographic studies provide evidence that bartenders and servers view the prevention and diffusion of aggressive behavior as an important component of their jobs (Gusfield et al. 1984; Lee et al. 2008).[3]

[3] Reynolds and Harris (2006) interviewed servers and managers at 21 restaurants, documenting the various tactics used to diffuse situations involving rude and potentially violent customers. Tomsen (1997), Graham et al. (2000), and Graham et al. (2005) described incidents in which bouncers and doormen clearly contributed to barroom violence, while Roberts (2007) found that violence was more likely to erupt in bars that did not employ bouncers.

By-the-drink sales could also influence the demand for other substances such as marijuana and cocaine, which could, in turn, affect crime. Consistent with the hypothesis that marijuana and alcohol are substitutes, there is evidence that marijuana participation falls sharply when individuals reach the minimum legal drinking age (Crost and Guerrero 2012), but the relationship between marijuana consumption and crime is still debated (Morris et al. 2014).[4] A number of previous studies have examined the relationship between alcohol and the use of illicit drugs other than marijuana (Petry 2001; Sumnall et al. 2004; Jofre-Bonet and Petry 2008; Conover and Scrimgeour 2013; Deza forthcoming), but their results have been decidedly mixed.

2.1. Alcohol consumption and crime

There is an extensive literature on the relationship between alcohol availability and crime.[5] For example, researchers have studied the effects of alcohol taxes (Cook and Moore 1993; Cook and Durrance 2013; Markowitz 2000, 2001, 2005; Markowitz and Grossman 2000; DeSimone 2001), Federal Prohibition (Miron 1999; Owens 2011), the minimum legal drinking age (Joksch and Jones 1993; Carpenter 2005; Carpenter and Dobkin forthcoming), underage drunk driving laws (Carpenter 2005, 2007), restrictions on weekend sales (Heaton 2012; Grönqvist and Niknami 2014), and early closing times for bars and restaurants (Chikritzhs and Stockwell 2002; Hough and Hunter 2008; Biderman et al. 2010; De Mello et al. 2013).[6]

[4] Using crime data from Los Angeles, Chang and Jacobson (2014) examined the relationship between marijuana dispensaries and crime. They found that crime increased in the immediate vicinity of dispensaries ordered to close.

[5] See Carpenter and Dobkin (2011) for an excellent review of this literature.

[6] See also Jackson and Owens (2011), who found that areas where bars were within walking distances to transit stations experienced increases in alcohol-related arrests and decreases in DUI arrests when the Washington D.C. Metro expanded train services.

A separate strand of this literature has focused on the spatial relationship between establishments that sell alcohol and crime. For example, studies conducted by ecologists, criminologists, and public health experts have examined the relationship between establishment density in a neighborhood (or county) and crime. However, many of these studies relied on cross-sectional variation in the density of liquor stores and/or bars.[7] As a consequence, their estimates of the relationship between local alcohol availability and crime may simply reflect unobserved factors such as economic conditions.[8]

Considerably fewer studies have used panel data methods, exploiting openings and closings of alcohol outlets over time.[9] While this approach offers cleaner identification than relying on cross-sectional variation, it still requires fairly strong identifying assumptions. For example, it implicitly assumes that bar, restaurant, and liquor store owners do not base their location decisions on future crime or its correlates.

To our knowledge, only three previous studies have exploited a clearly defined natural experiment to address the potentially endogenous relationship between local alcohol availability and crime. Conlin et al. (2005) and Billings (2014) used county-level data to examine the reduced-form relationship between dry laws and crime.[10] Conlin et al. (2005) found that drug-

[7] For examples of cross-sectional studies, see Scribner et al. (1995, 1999), Reid et al. (2003), Zhu et al. (2004), Britt et al. (2005), Gruenewald et al. (2006), Livingston (2008a), Liang and Chikritzhs (2011), and Toomey et al. (2012). For a more thorough review of this literature, see White et al. (forthcoming).

[8] Gyimah-Brempong (2001) used the number of gas stations in a census tract and median rent as instrumental variables to account for the endogeneity of alcohol outlets in the cross section. However, it is unlikely that these instruments satisfy the exclusion restriction.

[9] Examples of panel studies include Gruenewald and Remer (2006), Teh (2007), Livingston (2008b, 2011), and White et al. (forthcoming).

[10] Conlin et al. (2005) used county-level data from Texas for the period 1978-1996 to examine the reduced-form relationship between dry laws and drug-related arrests. Specifically, Conlin et al. (2005) examined marijuana-related arrests, "other" illicit-drug-related arrests, drug arrests involving possession, and drug arrests involving sales/manufacturing. Billings (2014) used county-level data for the period 1994-2006 from Alabama, Kentucky, North Carolina, Tennessee, and Texas to examine the reduced-form relationship between dry laws and total arrests.

related arrests fell when strict prohibitions on the sale of alcohol were lifted, but did not examine the impact of dry laws on other types of crime; Billings (2014) found that total arrests increased when strict prohibitions on the sale of alcohol were lifted. Neither Conlin et al. (2005) nor Billings (2014) distinguished between counties that allowed only retail sales of alcohol and those that allowed both retail and by-the-drink sales.[11]

Chamberlain (2014) exploited the 2012 privatization of distilled spirits sales in Seattle to estimate the relationship between liquor store density and crime. Privatization led to a sharp expansion in local availability as large grocery and drug stores began to stock distilled spirits. He found that a one-mile reduction in the distance to the nearest liquor store was associated with a 6 to 8 percent increase in crime. The effects of privatization on violent and drug crimes were persistent, while the effects on shoplifting and other non-violent crimes were short-lived.

Like Conlin et al. (2005), Billings (2014), and Chamberlain (2014), our empirical strategy relies on a unique natural experiment. However, our focus is on establishments licensed to sell alcohol for on-premises consumption (e.g., bars and restaurants) as opposed to retail stores that sell alcohol for off-premises consumption. By exploiting the gradual relaxation of Kansas dry laws at the county level over the period 1977-2011, we are able to isolate arguably exogenous changes in the number of establishments licensed to serve alcohol for on-premises consumption.

Several studies have estimated the relationship between local dry laws and traffic accidents (Blose and Holder 1987; Winn and Giacopassi 1993; Baughman et al. 2001; Gary et al. 2003).

[11] Conlin et al. (2005) and Billing (2014) defined dry counties as those in which both retail alcohol sales and by-the-drinks sales were prohibited.

2.2. Dry laws and liquor licenses in Kansas, 1977-2011

Table 1 summarizes changes to the wet/dry status of Kansas counties for the period under study.[12] These data were obtained through correspondence with the Kansas Division of Alcoholic Beverage Control. From 1977-1986, by-the-drink sales to the general public (including beer with an alcohol content of greater than 3.2%) were prohibited throughout Kansas. Private clubs were exempted from this prohibition, but becoming a member required paying a one-time $10 fee and a 10-day wait (Stites 1985; Robbins 1986).[13] These membership requirements, coupled with stringent licensing and record-keeping requirements, appear to have limited the number of private clubs selling liquor by the drink (Stites 1985).

On July 1, 1987, by-the-drink sales to the general public became legal in 36 counties, although establishments were required to derive 30 percent of their gross revenue from selling food (O`Connor 1987; Robbins 1987; St. John 2012b).[14] Between 1987 and 2011, 13 of the original 36 counties removed the food sales requirement. During this same period, voters approved by-the-drink sales to the general public in 50 of the 69 counties that had opted to

[12] A map of Kansas showing the year in which counties allowed by-the-drink sales is provided in the appendix (Appendix Figure 1). If by-the drink sales were allowed without the requirement that establishments derive 30 percent of their gross revenue from selling food, then the year the law went into effect is italicized.

[13] Private clubs in Kansas are classified into two types, or "classes." Class A clubs are owned or leased by nonprofit organizations such the American Legion or the Elks Lodge, while Class B clubs are operated for profit.

[14] These 36 counties were among the most populous in Kansas. Although the measure to legalize by-the drink sales failed in 69 counties, the state-wide vote was 59.9 percent in favor and 40.1 percent against. Under Kansas law,

> A license for a drinking establishment shall allow the licensee to offer for sale, sell and serve alcoholic liquor for consumption on the licensed premises which may be open to the public…, but only if such premises are located in a county where the qualified electors of the county: (1) (A) Approved, by a majority vote of those voting thereon, the proposition to amend section 10 of article 15 of the constitution of the state of Kansas at the general election in November 1986; or (B) have approved a proposition to allow sales of alcoholic liquor by the individual drink in public places within the county at an election… (K.S.A. 2012 Supp. 41-2642).

As of 2013, 5 small cities in Kansas prohibited the retail sales of alcohol: Moundridge, Parkerfield, Hesston, North Newton, and Nickerson (Kansas Department of Revenue 2013).

remain dry in 1986. Eleven of these 50 counties did not impose a food sales requirement; 39 required that establishments derive 30 percent of their gross receipts from food sales. Counties that remained dry throughout the period under study are denoted with an asterisk in Table 1.[15]

Votes to allow by-the-drink sales or remove the food sales requirement took place in November and were officially implemented within a few days (Buckner 1992a; Associated Press 2000; Haxon 2012).[16] However, because the process of obtaining a new liquor license took at least one month (Toplikar 1992; Haxon 2012; Scherer 2012), we code these laws as coming into effect on January 1st of the following year.[17]

Data on liquor licenses were purchased from the Kansas Division of Alcoholic Beverage Control and include information on license type (on- versus off-premises), the location and name of the establishment that purchased the license, and the dates the license became active and inactive.[18] This information was used to create a count of active on-premises licenses by county and year. Figure 1 provides evidence that allowing by-the-drink sales to the general public resulted in a sharp increase in the total number of active on-premises licenses issued by the Kansas Division of Alcoholic Beverage Control. It was constructed by regressing the natural log

[15] A brief history of Kansas liquor laws is available from the Kansas Legislative Research Department (2003). By the end of 2013, only 13 counties in Kansas prohibited on-premises sales of alcohol.

[16] Kansas state law requires that by-the-drink votes must be held during the November general elections (Buckner 1992a; Associated Press 2000).

[17] Holder and Blose (1985) found that the availability of alcohol for on-premises consumption increased gradually over a two-year period after counties in North Carolina approved by-the-drink sales.

[18] Approximately 12 percent of the on-premises licenses issued between 1977 and 2011 had missing inactive dates that could not be determined. When an inactive date was missing and could not be determined, we assigned an inactive date based on the average time-to-closure of establishments in the data (5 years). The results reported below are not sensitive to either dropping establishments with missing inactive dates or assuming an establishment with a missing inactive date remained open through 2011. In addition to bars, private clubs, and restaurants, the Kansas Division of Alcoholic Beverage Control issues on-premises liquor licenses to caterers, hotels, and public venues.

of on-premises alcohol licenses (per 10,000 population) on an indicator *Year of Law Change*, equal to one the year in which by-the-drink sales to the general public were permitted (and equal to zero otherwise). Five leads and 5 lags of *Year of Law Change* were also included on the right-hand side of this regression as well as 104 county dummies and 34 year dummies. For now, we do not distinguish between counties that required 30 percent of gross receipts from the sale of food and those that did not.

The estimated coefficients of the 5 leads are, without exception, small and statistically indistinguishable from zero. The first year in effect (year "0" on the horizontal axis), permitting by-the-drink sales to the general public is associated with a 19.6 percent increase in on-premises licenses relative to the omitted period (6 or more years before the law change); two years later, on-premises licenses had increased by 22.3 percent; 5 years later, on-premises licenses had increased by 36.1 percent. This pattern of results is consistent with the hypothesis that membership and record-keeping requirements limited the number of private clubs operating in dry counties.[19]

In addition to observing when on-premises licenses became active and inactive, we have information on the number of off-premises liquor licenses issued by the Kansas Division of Alcoholic Beverage Control. Figure 2 shows the results of regressing off-premises licenses on the indicator *Year of Law Change*, 5 leads of this indicator, and 5 lags. They suggest that neither legalization nor its correlates were related to the number of establishments selling alcohol for

[19] Because private clubs could only serve alcohol to members, and because becoming a member required a one-time $10 fee and a 10-day wait, tourists and visitors were effectively barred from consuming alcohol at a bar or restaurant in dry counties. Under Kansas law, club owners are also required to screen applicants for "good moral character" and maintain a list of all members along with their addresses. The sale of memberships must be conducted in person on club premises. Many private club owners view these restrictions as onerous and club owners often spearheaded efforts to lift the ban on by-the-drink sales (Buckner 1992b; Buckner 1992c; McKinney 2009; Haxon 2012).

off-premises consumption, perhaps because the retail liquor industry in Kansas is subject to relatively tight controls (Byrne and Nizovtsev 2013).[20]

3. METHODS

We begin by using within-county changes in wet/dry status to isolate exogenous variation in the number of active on-premises liquor licenses. Specifically, we estimate the following first-stage equation:

$$(1) \qquad \ln(\textit{On-Premises Licenses}_{ct}) = \alpha_0 + \alpha_1 \textit{Wet Law}_{ct} + \boldsymbol{X}_{ct}\alpha_2 + \nu_c + z_t + \varepsilon_{ct},$$

where the dependent variable, *On-Premises Licenses*, is equal to the number of active on-premises liquor licenses per 10,000 population in county *c* and year *t*.[21] The indicator *Wet Law* is equal to one if county *c* allowed by-the-drink sales to the general public in year *t* (and is equal to zero otherwise). Counties that allowed by-the-drink sales are considered wet regardless of whether they required establishments to derive 30 of their gross receipts from food sales.

The vector ***X*** includes county-level controls for economic conditions (income per capita and the unemployment rate), population density, demographics (percent of the county population that was nonwhite, adult male, and 21 years of age and older), the ratio of Democratic to GOP votes in presidential and gubernatorial elections, and whether Sunday sales of alcohol were

[20] Grocery and convenience stores in Kansas are banned from selling beer, liquor and wine, but liquor license fees are not prohibitively expensive. The current fee for obtaining an on-premises liquor license is $2,000, while the fee for an off-premises license is $500.

[21] There is a one-to-one correspondence between the number of active on-premises licenses in county *c* and year *t* and the number of establishments permitted to sell alcohol by-the-drink. Because the variable *On-Premises Licenses* is equal to 0 for 392 of the 3,352 county-year observations, we followed Wooldridge (2013, pp. 193-194) by adding 1 to *On-Premises Licenses* before taking its log.

legal.[22] Descriptive statistics and variable definitions are provided in Table 2. County and year fixed effects are represented by v_c and z_t, respectively.

Yearly crime data for the period 1977-2011 come from the FBI's Uniform Crime Reports (UCR) and were made available by the Interuniversity Consortium for Political and Social Research.[23] The (second-stage) relationship between violent crime and on-premises liquor licenses is given by the following equation:

$$\ln(Crime_{ct}) = \beta_0 + \beta_1 \ln(On\text{-}Premises\ Licenses_{ct}) + \boldsymbol{X_{ct}}\beta_2 + v_c + z_t + \varepsilon_{ct}, \quad (2)$$

where *Crime* is equal to the violent crime rate in county *c* and year *t*, and the natural log of *On-Premises Licenses* is instrumented using equation (1).[24] The vector $\boldsymbol{X}$ is composed of the observable time-varying determinants of crime listed above; county and year fixed effects are represented by v_c and z_t, respectively. Estimates are weighted by county population and standard errors are corrected for clustering at the county level (Bertrand et al. 2004). Because so few private clubs opened after counties went wet, β_1 can be thought of as the elasticity of crime with

[22] Data on income per capita, the unemployment rate, and the Democratic to GOP voting ratio come from the University of Kansas's Institute for Policy and Social Research on-line data archive (http://ipsr.ku.edu/ksdata/). Population data come from the U.S. Census. The Kansas Department of Revenue provided the authors with a list of municipalities that currently allow Sunday sales of alcohol for off-premises consumption. For these municipalities, a date of legalization was acquired by searching municipal codes, local newspapers, and town council minutes, or contacting municipal clerks directly. The Sunday sales indicator is equal to one if Sunday sales for off-premises consumption were allowed in any municipality in county *c* and year *t* (and is equal to zero otherwise). Consistent with previous research (Heaton 2012), we found that Sunday sales were negatively related to county crime rates. However, these estimates were generally statistically insignificant.

[23] For multiple-offense events, only the most serious crime is reported. Crime data at the county level were unavailable from the UCR for the period 1993-1999. As a substitute, we turned to the *Kansas Statistical Abstract*, from which we obtained violent crime counts by county for the years 1993, 1994, 1997, and 1998, leaving three years of missing violent crime data. Unfortunately, the *Kansas Statistical Abstract* does not report violent crimes by crime type. As a consequence, estimates by type of crime are based on panels in which the years 1993 through 1999 are missing. Descriptive statistics for violent crimes by type are provided in Appendix Table 2.

[24] Because *Violent Crime* is equal to 0 for 310 of the 3,352 county-year observations, we added 1 to the violent crime rate before taking its natural log (Wooldridge 2013, pp. 193-194). Following Carpenter and Dobkin (2009) and Gelber (2014), we also experimented with adding .5 to *Violent Crime* and to *On-Premises Licenses* before taking natural logs. This exercise produced similar results to those reported below.

respect to establishments licensed to sell alcohol to the general public for on-premises consumption (i.e., bars and restaurants).[25]

The instrumental variables strategy outlined above is based on the assumption that the adoption of wet laws affected crime only through the number of establishments with on-premises liquor licenses. It is possible, however, that allowing by-the-drink sales impacted crime through the number of off-premises licenses issued by the Kansas Division of Alcoholic Beverage Control, which would be a violation of the exclusion restriction. Below, we present estimates of the relationship between wet laws and active off-premises liquor licenses in county *c* and year *t*. Consistent with the trends shown in Figure 2, they suggest that allowing by-the-drink sales had no effect on the number of liquor retailers in operation.

It is also possible that changes in wet/dry status had a direct effect on the demand for alcohol at the county level, perhaps by removing or lessening the social stigma attached to binge drinking.[26] If this were the case, then estimates of β_1 would be biased, but a reduced-form approach could still be used to estimate the overall effect on violent crime of going from dry to wet. Below, we present estimates of the following reduced-form equation:

$$(3) \qquad \ln(Crime_{ct}) = \pi_0 + \pi_1 Wet\ Law_{ct} + \boldsymbol{X_{ct}}\pi_2 + \nu_c + z_t + \varepsilon_{ct},$$

[25] Many private clubs converted to restaurants or bars after voters allowed by-the-drink sales to the general public (Buckner 1992b; Toplikar 1992; McKinney 2009; Haxon 2012). At least one club became a restaurant shortly after voters allowed by-the-drink sales but converted back to its original status because it had trouble meeting the requirement that 30 percent of receipts be derived from the sale of food (Toplikar 1992). In 1986 there were approximately 550 private clubs operating in Kansas (or 2.30 per 10,000 population); by 2011, this number had fallen to approximately 420 (or 1.50 per 10,000 population).

[26] Unfortunately, we do not have access to data on alcohol consumption at the county level. Appendix Figure 2 shows state-level trends in total alcohol consumption for the period 1977-2011. After July 1, 1987, when Kansas counties had the option of allowing on-premises consumption, there is a modest upward trend in total alcohol consumption. However, this trend is also evident in neighboring states, suggesting that it was not caused by changes in Kansas wet laws.

where the variables are defined as above. Under the parallel-trends assumption, the estimate of π_1 represents the relationship between allowing by-the-drinks sales and the violent crime rate. Again, estimates are weighted by county population and standard errors are corrected for clustering at the county level (Bertrand et al. 2004).[27]

4. FIRST-STAGE ESTIMATES

Estimates of the first-stage relationship between *Wet Law* and on-premises liquor licenses are reported in Table 3. They are consistent with the trends shown in Figure 1. Without controlling for county-specific linear time trends, allowing by-the-drink sales to the general public is associated with a 32.8 percent increase in the number of active on-premises licenses ($e^{.284} - 1 = .328$). When county-specific linear time trends are included on the right-hand side, this estimate decreases slightly in magnitude: allowing by-the-drink sales is associated with a 28.5 percent increase in on-premises licenses ($e^{.251} - 1 = .285$). Both of these estimates are statistically significant at conventional levels and easily meet the Staiger and Stock (1997) criterion.[28]

Estimates of the relationship between allowing by-the-drink sales and off-premises liquor licenses are presented in Appendix Table 1. These estimates are consistent with the trends shown in Figure 2. Specifically, the coefficient of the wet law indicator is small and statistically insignificant with or without controlling for county-specific linear trends.

[27] Table 2 presents summary statistics of the variables used in the statistical analyses. On average, there were 8.13 active on-premises licenses per 10,000 population in wet counties. In contrast, there were only 2.95 on-premises licenses per 10,000 population in dry counties. Wet counties also experienced higher rates of violent crime and property crime as compared to dry counties.

[28] The test of whether the coefficient of *Wet Law* is equal to zero yields an F-statistic of 43.6 when county-specific linear trends are not included as controls. The F-statistic is 62.9 when county-specific linear trends are included.

5. VIOLENT CRIME

5.1. Instrumental variables estimates

Table 4 presents estimates of the relationship between on-premises liquor licenses and violent crime. The first two columns present OLS estimates of this relationship while the last two columns present two stage least squares (2SLS) estimates. The OLS estimates suggest that a 10 percent increase in drinking establishments leads to a 0.6 to 1.1 percent increase in violent crime. The 2SLS estimates are quite a bit larger. Specifically, a 10 percent increase in drinking establishments is associated with a 3.7 to 4.1 percent increase in violent crime.

There are at least two plausible explanations for why the 2SLS estimates are larger than the OLS estimates. First, it is possible that drinking establishments opened because their owners anticipated that economic conditions would improve and crime rates would go down. Instrumenting using *Wet Law* avoids this potential source of endogeneity. Alternatively, it is possible that, on average, opening a bar or restaurant has a larger effect on crime than opening a private club, perhaps because private clubs attract less rowdy patrons or because Kansas state law requires that applicants be "screened by the club for good moral character." Because so few new licenses were issued to private clubs once by-the-drink sales were permitted, the 2SLS estimates in Table 4 should be thought of as the representing the elasticity of violent crime with respect to establishments licensed to sell alcohol to the general public for on-premises consumption (i.e., bars and restaurants).

Table 5 shows 2SLS estimates disaggregated by type of violent crime. The sample sizes in Table 5 are smaller than in previous tables (2,932 versus 3,352) because crime data at the county level were unavailable from the UCR for the period 1993-1999 and the *Kansas Statistical Abstract*, our alternative source of data, does not report violent crimes disaggregated by type.

Despite the reduction in sample size, 2SLS estimates of the relationship between drinking establishments and the overall violent crime rate are similar to those reported in Table 4. The regressions with county-specific linear time trends suggest that a 10 percent increase in the number of active on-premises liquor licenses leads to a 1.4 percent increase in rapes, a 4.6 percent increase in robberies, and a 2.9 percent increase in assaults. The estimates for rape and robbery are statistically significant at conventional levels, while the estimate for assault is statistically insignificant when the county-specific linear time trends are included on the right-hand side of the estimating equation (p-value = .164). The estimated relationship between on-premises liquor licenses and murders is relatively small and statistically insignificant.[29]

5.2. Reduced-form estimates

Table 6 presents reduced-form estimates of the relationship between wet/dry status and violent crime. The advantage of the reduced-form approach is that it does not rely on excluding the wet law indicator from a second-stage equation. Baseline estimates in the first two columns of Table 6 suggest that allowing by-the-drink sales resulted in a 10.7 to 11.0 percent increase in the violent crime rate.[30]

The remaining columns in Table 6 present estimates of equation (3) augmented with leads and lags of the wet law indicator. Consistent with the parallel trends assumption, there is

[29] This last result is consistent with those of previous studies. For instance, Carpenter and Dobkin (forthcoming) found that turning 21 years of age led to more robberies and assaults, but was essentially unrelated to murders and rapes; Corman and Mocan (2013) found that alcohol sales were positively related to assaults and rapes, but were unrelated to murders and robberies. Data from the Uniform Crime Report's *Survey of Inmates* suggests that over 60 percent of inmates incarcerated for assault were drinking at the time of the offense, over 40 percent of inmates incarcerated for rape were drinking at the time of the offense, and approximately 25 percent of inmates incarcerated for robbery were drinking at the time of the offense (Billings 2014).

[30] The reduced-form results were robust to using the wild cluster bootstrap method to calculate standard errors and t-statistics (Cameron et al. 2008).

little evidence that the violent crime rate increased in the years leading up to legalization. At Year 0, estimates of relationship between allowing by-the-drink sales and violent crime are small and statistically insignificant; after two years, these estimates are consistently positive and significant at conventional levels; and after 3 years, allowing by-the-drink sales is associated with a 17.7 to 22.6 percent increase in violent crime. However, after 5 years these estimates become smaller and lose statistical significance, suggesting that the effect of legalizing by-the-drink sales could eventually dissipate.

We report reduced-form estimates disaggregated by type of violent crime in Table 7. The results are qualitatively similar to those in Table 5. The regressions with county-specific time trends suggest that allowing by-the-drink sales is associated with a 4.0 percent increase in rapes, a 13.8 percent increase in robberies, and an 8.5 percent increase in assaults. The estimates for rape and robbery are statistically significant at the 5 percent level, while the estimate for assault is not statistically significant at conventional levels (p-value = .185). The estimated relationship between wet laws and murder is relatively small and statistically insignificant.

We report the results of various robustness checks in Table 8. The estimate in the first column comes from a series of regressions (i.e., trials) in which placebo *Wet Law* indicators were randomly assigned. Because 86 counties in Kansas legalized by-the-drink sales during the period 1986-2011, 86 placebo indicators were assigned per trial. The estimated coefficient of the placebo indicator was positive in 51 out of 100 trials, and positive and significant at the 5 percent level only three times. The mean of the placebo coefficients is equal to -.006. This exercise illustrates that our results cannot be easily reproduced by randomly generating the variable of interest.

Lott and Whitley (2003) noted that rural counties with relatively small populations in the UCR underreport crime at higher rates than do larger counties. In columns (2) and (3) we restrict our attention to counties with a population greater than 5,000 and 10,000 residents, respectively. The estimate in column (2) is similar to those reported in Table 6, while the estimate in column (3) is slightly larger in magnitude. Specifically, when the sample is restricted to counties with a population greater than 10,000, allowing by-the-drink sales is associated with a 12.9 percent increase in the violent crime rate.

Next, we control for county-specific quadratic time trends. The estimate, reported in column (4), is positive, larger in magnitude than our baseline estimates, and statistically significant at conventional levels. Specifically, allowing by-the-drink sales is associated with a 15.6 percent increase in the violent crime rate.

The crack epidemic began in 1986, shortly before by-the-drink sales became legal in 36 Kansas counties (Cooper 2002; Fryer et al. 2013). In an effort to account for the effect of the crack epidemic on violent crime in Kansas City (and, to a much lesser extent, its effect on violent crime in Topeka and Wichita), we include a control based on the crack index developed by Fryer et al. (2013).[31] The results are reported in column (5) of Table 8. Controlling for the crack epidemic does not have an appreciable impact on our estimate of π_1. Specifically, allowing by-the-drink sales is associated with a 12.6 percent increase in the violent crime rate.[32]

[31] The Fryer et al. (2013) crack index is at the city level and was calculated using data on cocaine arrests and seizures, emergency room visits involving cocaine, and newspaper reports that mentioned crack. It is available for Kansas City (Johnson County and Wyandotte County), Topeka (Shawnee County) and Wichita (Sedgwick County) for the years 1985, 1989, 1993, 1997, and 2000. We used linear interpolation to calculate values of the index for the years 1986-1988, 1990-1992, 1994-1996, 1998-1999, and 2001-2003. Our crack epidemic control takes on the value of 0 for all other counties and years.

[32] In an effort to explore whether the crack epidemic reached smaller Kansas cities, we examined cocaine arrests outside of the 4 most populous counties in Kansas (Johnson, Sedgwick, Shawnee and Wyandotte) for the period 1985-1992. There was a steady increase in cocaine arrests after 1986. This increase, however, was not pronounced

In column (6) of Table 8, we regress the raw crime rate (as opposed to its natural log) on the wet law indicator; in column (7) we estimate the relationship between by-the-drink sales and violent crime using a negative binomial regression model. These modifications produce larger estimates than those reported in the first two columns of Table 6. Specifically, allowing by-the-drink sales is associated with a .714 increase in the violent crime rate, or a 23.4 percent increase relative to the pre-treatment mean of 3.05. When violent crime is modeled as a count process, allowing by-the-drink sales is associated with a 12.4 ($e^{.117} - 1 = .124$) percent increase.[33]

Changes in policing effort in response to the legalization of by-the-drink sales represent a potential source of omitted variable bias. If police departments and sheriffs' offices hired extra officers after legalization, then the true relationship between *Wet Law* and violent crime could, in fact, be larger than the estimates of π_1 reported above. To explore this issue, we regressed sworn officers per capita on *Wet Law* and the controls used in previous analyses.[34] The estimates are reported in Table 9. Allowing by-the-drink sales is associated with a (statistically insignificant) 3.1 percent decrease in the number of officers employed by sheriffs' offices and a (statistically insignificant) 3.3 percent decrease in the number of officers employed by police departments.

in counties that voted to legalize by-the-drink sales in 1986 as compared to counties that voted to legalize by-the-drinks sales after 1986.

[33] Allowing by-the-drink sales is associated with a 15.1 percent increase in violent crime when county-specific linear time trends are excluded from the negative binomial model. The negative binomial model was used instead of the Poisson because we found strong evidence of overdispersion in the data. Greene (2007) noted that the negative binomial model with fixed effects may suffer from the incidental parameters problem. However, in a simulation study, Allison and Waterman (2002) found little evidence of the incidental parameters problem when estimating an unconditional negative binomial regression with fixed effects. On average, there were 93.6 violent crimes committed per county-year.

[34] Law enforcement employment data are from the annual report *Crime in the United States*, published by the Federal Bureau of Investigation. Because these data are available only for the period 1995-2011, the sample size in Table 9 is reduced to approximately 1,700.

As a final robustness test, we examine whether economic conditions or broad changes in social mores, as measured by voting patterns in congressional and gubernatorial elections, can predict the legal status of by-the-drink sales (Table 10). Specifically, we regress the wet law indicator on income per capita, the unemployment rate, the ratio of Democratic to GOP votes, and the full set of controls. Point estimates from this exercise are small and statistically insignificant, suggesting that the post-legalization increase in violent crime documented in Tables 6-8 was not driven by economic conditions or the adoption of more liberal attitudes with regard to issues aside from the on-premises consumption of alcohol.[35]

5.3. Distinguishing between wet laws based on whether food sales were required

By the end of 2011, 24 out of the 86 wet counties in Kansas did not require food sales. The remaining 62 wet counties required that establishments derive 30 percent of their gross receipts from food sales (Table 1). Up to this point in the analysis, we have not distinguished between wet counties based on whether they had a food sales requirement.

In Table 11, we report estimates of equation (3) in which *Wet Law* is replaced by two mutually exclusive indicators: *Wet Law with Food Sales 30% Gross* is equal to one if county *c* required establishments that served alcohol to derive 30 percent of their gross revenue from food sales (and is equal to zero otherwise); *Wet Law with Food Sales Not Required* is equal to one if county *c* did not require these establishments to derive 30 percent of their gross revenue from food sales (and is equal to zero otherwise).

The estimates in Table 11 provide little evidence that the food sales requirement dampened the effect of by-the-drink sales on violent crime: without controlling for county-

[35] We also explored this issue using a discrete-time hazard model. The results provided little evidence that voting patterns were related to the likelihood of legalizing by-the-drink sales.

specific linear time trends, the estimated coefficient of *Wet Laws with Food Sales 30% Gross* is .101, while the estimated coefficient of *Wet Laws with Food Sales Not Required* is .119. We cannot, however, reject the hypothesis that these estimates are equal. When we control for county-specific linear trends, the estimated coefficient of *Wet Laws with Food Sales Not Required* is actually smaller than the estimated coefficient of *Wet Laws with Food Sales 30% Gross*, but again the difference is not statistically significant.[36]

5.4. Spillovers across county borders

The issue of displacement is not typically addressed by studies on local alcohol availability and crime (Carpenter and Dobkin 2011).[37] In the context of the present study, the positive relationship between allowing by-the-drink sales and violent crime could, in theory, reflect a net increase in criminal activity; alternatively, it could be the case that violence-prone residents of neighboring counties drove across the border after establishments began selling alcohol to the general public for on-premises consumption.

In an effort to distinguish between these hypotheses, we included an additional variable on the right-hand side of equation (3) equal to the number of wet counties bordering county *c* in year *t*.[38] The results are reported in the first column of Table 12. When this additional variable

[36] This pattern of results could reflect the fact that many establishments required to derive 30% of their sales from food essentially operate as bars during the nighttime and early morning hours. For example, Johnny's Tavern in Johnson County serves drinks until 2:00AM and has a happy hour on Sunday through Thursday from 10:00PM to 2:00AM. Idle Hour, a "dive" bar in Harper County with a dance floor, serves drinks until 2:00AM and has weekly karaoke nights. Even Hays House, a historic site and family-style restaurant in Morris County, serves drinks until 2:00AM.

[37] Chamberlain (2014) is one of the few studies to address this issue. He found evidence that reductions in the distance to the nearest liquor store created violent and drug-related crimes. In contrast, increases in shoplifting and non-violent crimes were found to be, at least in part, due to displacement.

[38] Wet counties in Colorado, Nebraska, Missouri and Oklahoma that were on the Kansas state border were included in this count. Four counties in Oklahoma (Beaver, Cimarron, Grant, and Harper) were coded as dry.

is included on the right-hand side of equation (3), the estimated coefficient of *Wet Law* is essentially unchanged. More importantly, the estimated relationship between violent crime and the number of wet counties bordering county *c* in year *t* is small and statistically insignificant, suggesting that crime was not displaced (i.e., shifted across county lines) when a neighboring county legalized by-the-drink sales to the general public.

Including mutually exclusive indicators for having one wet county as a neighbor or having two or more wet counties as neighbors produces qualitatively similar estimates to those reported in column (1). Likewise, when the sample is restricted to counties that remained dry throughout the period 1977-2011, we find little evidence to suggest that residents of these counties traveled across the border and committed violent crimes that would have otherwise been committed nearer to home.[39]

In columns (5) and (6) of Table 12, we distinguish between neighboring wet counties based on whether they had a food sales requirement. Specifically, we included two new variables in the model: the first is equal the number of wet counties sharing a border with county *c* that required establishments to derive 30 percent of their gross revenue from food sales; the second is equal to the number of wet counties sharing a border with county *c* that did not require establishments to sell food. Including these new variables on the right-hand side of equation (3) provides little evidence that crime went down when residents had the option of crossing county lines to buy liquor by the drink.

[39] The 19 counties in Kansas that remained dry throughout the period under study are denoted with an asterisk in Table 1. These counties were typically less populous than the counties with which they shared a border and experienced lower levels of violent crime. During the period under study, always-dry counties had a mean population of 5,443 and experienced an average of 1.15 violent crimes per year, while Kansas counties on their border had a mean population of 15,025 and experienced an average of 2.01 violent crimes per year.

6. PROPERY CRIME

Our focus has, thus far, been on the relationship between wet laws and violent crime. Researchers, however, have also found evidence of a positive association between the availability of alcohol and property crime.[40] In Table 13 we explore the relationship between legalizing by-the-drink sales on property crimes, defined as burglaries, larcenies, and motor vehicle thefts.[41] The first two columns of Table 13 present 2SLS estimates of the elasticity of property crime with respect to drinking establishments; the third and fourth columns present reduced-form estimates of the relationship between allowing by-the-drink sales and property crime.

These estimates are similar in magnitude to the violent crime estimates presented above. Specifically, a 10 percent increase in on-premises liquor licenses is associated with a 3.3 to 4.1 percent increase in property crime; counties that allowed by-the-drink sales experienced, on average, a 9.7 to 10.7 percent increase in property crime.[42]

In Table 14, we explore the relationship between allowing by-the-drink sales and property crimes by type. Without controlling for county-specific linear trends, the legalization of by-the-drink sales is associated with a 13.9 percent increase in motor vehicle thefts; the reduced-form estimates for burglary and larceny, although positive, are not statistically significant at

[40] Carpenter (2005, 2007) found that the adoption of stricter underage drunk driving laws was positively related to nuisance and property crimes (but unrelated to violent crimes). Using alcohol taxes and the minimum legal drinking age as instruments, Corman and Mocan (2013) found a positive relationship between alcohol sales and larcenies in New York City. Grönqvist and Niknami (2014) found that allowing liquor stores to stay open on Saturday in Sweden led to more property crimes.

[41] Our estimates for violent crime, particularly those that could reflect domestic violence (i.e., assault and rape), could be inflated if wet laws impacted which crimes were actually reported. A benefit of focusing on property crimes is that this issue is less likely to be a concern.

[42] Appendix Table 3 reports robustness checks. When property crime is modeled as a count process, allowing by-the-drink sales is associated with a 9.5 percent ($e^{.091} - 1 = .095$) percent increase in the number of property crimes reported in county *c* and year *t*.

conventional levels. Controlling for county-specific linear trends, the legalization of by-the-drink sales is associated with a 14.6 percent increase in larcenies and a 19.7 percent increase in motor vehicle thefts; the reduced-form estimate for burglary, although positive, is not statistically significant.[43]

7. CONCLUSION

While a large number of studies have found a positive association between local alcohol availability and crime, these studies have generally treated the location decisions of bar and liquor store owners as exogenous. In contrast, we exploit a unique natural experiment to address the potential endogeneity of local alcohol availability. Specifically, using county-level data from Kansas for the period 1977-2011, we examine whether arguably exogenous increases in the number of establishments licensed to sell alcohol by the drink are related to crime. During this period, 86 out of 105 counties voted to go from "dry" to "wet" by legalizing the sale of alcohol to the general public for on-premises consumption.

Using changes in wet/dry status at the county level to instrument for the number of active on-premises liquor licenses, we find that the elasticity of violent crime with respect to drinking establishments is between .37 and .41. Instrumental variables estimates of the elasticity of property crime with respect to drinking establishments are between .33 and .41. To our knowledge, no previous study has produced estimates of the elasticity of crime with respect to drinking establishments.

[43] Appendix Table 4 shows 2SLS estimates of the relationship between on-premises licenses and property crime by crime type. Controlling for county-specific linear time trends, a 10 percent increase in on-premises liquor licenses is associated with a 4.8 percent increase in larcenies and a 6.4 percent increase in motor vehicle thefts. The issue of displacement is revisited in Appendix Table 5. The results provide little evidence that property crime was shifted across county lines when a neighboring county legalized by-the-drink sales to the general public.

Our instrumental variables estimates are based on the assumption that changes in the wet/dry status of counties influence crime only through the number of establishments with on-premises liquor licenses, which may have been violated in practice. For instance, it is possible that these changes could have had a direct effect on the overall demand for both on- and off-premises alcohol consumption, perhaps by lessening the social stigma attached to drinking. Reduced-form estimates, which are not based on the assumption that wet laws worked only through drinking establishments, suggest that allowing by-the-drink sales is associated with a 10.7 to 11.0 percent increase in violent crime and a 9.7 to 10.7 percent increase in property crime.[44] Because we find no evidence that crime fell in dry counties when their neighbors allowed by-the-drink sales, we conclude that bars and restaurants create criminal activity as opposed to simply encouraging violence-prone residents to cross county lines.

There are at least three potential mechanisms that could explain the positive relationship between allowing by-the-drink sales and crime. First, it could be due to increased consumption of alcohol, which has been linked to crime by an extensive literature. Second, the increase in crime may be the result of a shift in alcohol consumption from private homes to public venues such as bars and restaurants. This latter explanation is consistent with previous evidence that alcohol consumption in public serves as a catalyst for violent behavior (Graham and Wells 2001, 2003; Buddie and Parks 2003; Middleton et al. 2010). Finally, it could be due to more late-night foot traffic around bars and restaurants, increasing the supply of potential victims. While we cannot distinguish between these mechanisms, our results provide evidence that restrictions on local alcohol availability can play an important role in crime prevention.

[44] Interestingly, these estimates are comparable to those from the casinos and crime literature. Specifically, Evans and Topoleski (2002) found that county-level violent crime, auto theft, and larceny rates went up by roughly 10 percent after tribal casinos were allowed to operate. Grinols and Mustard (2006) found that property and violent crime rates were roughly 9 percent and 13 percent higher, respectively, in counties with casinos.

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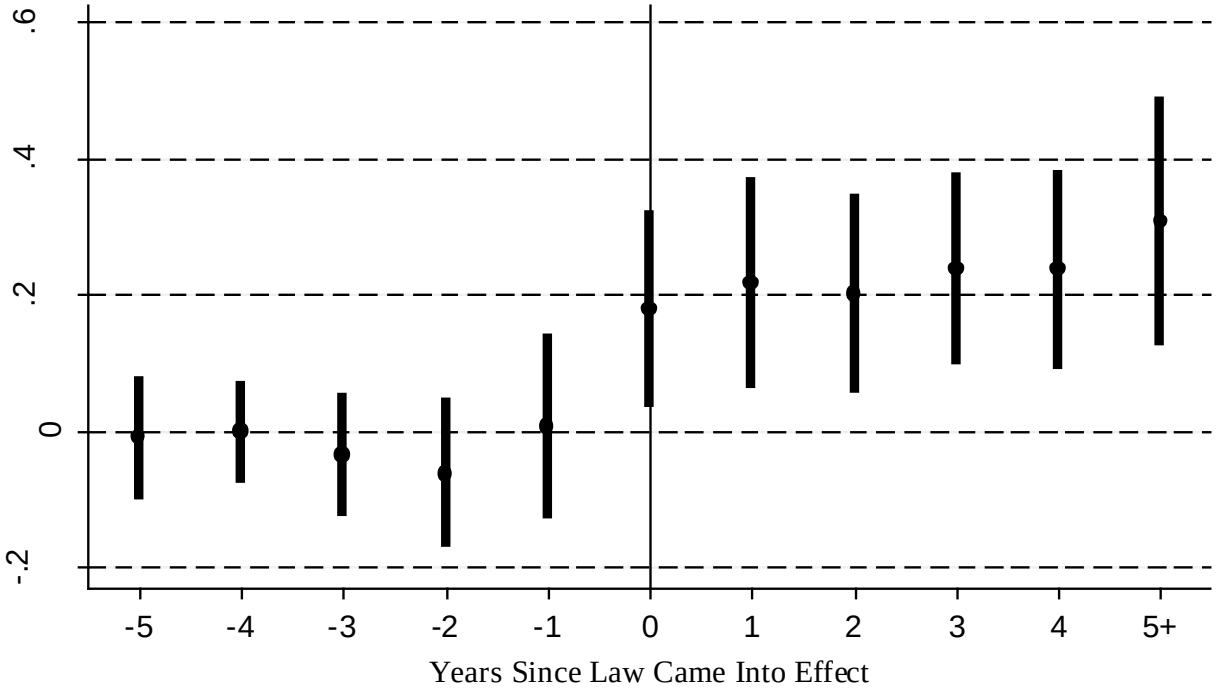
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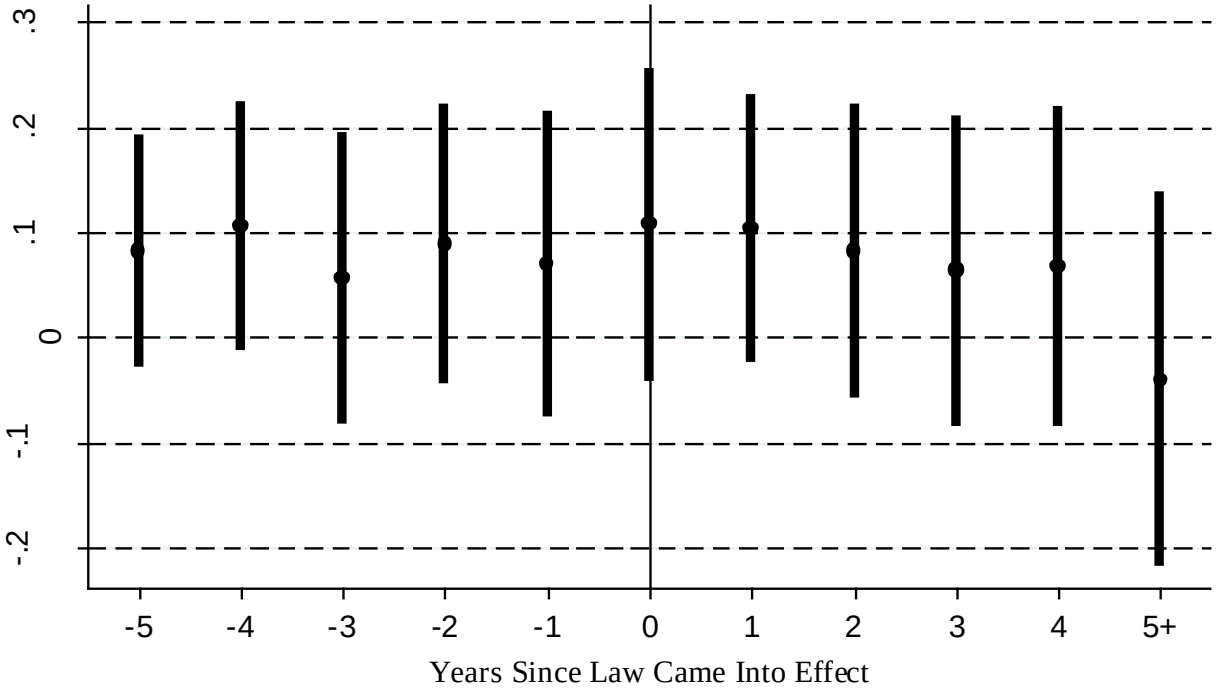
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Figure 1. Trends in On-Premises Alcohol Licenses



Notes: OLS coefficient estimates (and their 95% confidence intervals) are reported. The dependent variable is equal to the natural log of the number of on-premises liquor licenses per 10,000 population in county *c* and year *t*. The controls include county and year fixed effects and the data cover the period 1977-2011.

Figure 2. Trends in Off-Premises Alcohol Licenses



Notes: OLS coefficient estimates (and their 95% confidence intervals) are reported. The dependent variable is equal to the natural log of the number of off-premises liquor licenses per 10,000 population in county *c* and year *t*. The controls include county and year fixed effects and the data cover the period 1977-2011.

Table 1. Kansas Wet Laws, 1977-2011

	Year Law Went Into Effect			Year Law Went Into Effect			Year Law Went Into Effect	
	Food Sales 30% Gross	Food Sales Not Required		Food Sales 30% Gross	Food Sales Not Required		Food Sales 30% Gross	Food Sales Not Required
Allen	2000		Greeley		2008	Osborne	2010	
Anderson	1996		Greenwood	1986		Ottawa	2006	
Atchison	1986		Hamilton	2010		Pawnee	1992	
Barber		2010	Harper	1996		Phillips	1996	
Barton	1986	2004	Harvey	1996		Pottawatomie	1986	
Bourbon	1992		Haskell*			Pratt	2000	
Brown		2000	Hodgeman	2004		Rawlins	2002	
Butler	1986		Jackson	2004		Reno	1986	
Chase	1988		Jefferson	1986		Republic	1986	
Chautauqua	2008		Jewell*			Rice*		
Cherokee*			Johnson	1986		Riley	1986	2004
Cheyenne	2000		Kearny	1988		Rooks	2000	
Clark*			Kingman	2004		Rush	1986	
Clay*			Kiowa	2010		Russell	1986	
Cloud	1998		Labette	1996		Saline	1986	1994
Coffey	2004		Lane*			Scott		2010
Comanche		2010	Leavenworth	1986	2010	Sedgwick	1986	1988
Cowley		1996	Lincoln	1990		Seward	1996	
Crawford	1986	1992	Linn	2004		Shawnee	1986	1994
Decatur	2002		Logan		2006	Sheridan*		
Dickinson	1986		Lyon	1986	1992	Sherman	1986	
Doniphan*			McPherson	1996		Smith	1992	
Douglas	1986	1992	Marion	2004		Stafford*		
Edwards	1986	2008	Marshall	1986		Stanton*		
Elk*			Meade*			Stevens*		
Ellis	1986	1988	Miami	1986		Sumner	1992	
Ellsworth	1986		Mitchell	1996		Thomas	1986	
Finney	1986		Montgomery		1998	Trego	1986	
Ford	1986		Morris	1992		Wabaunsee	1986	
Franklin	1994		Morton*			Wallace*		
Geary	1986	1990	Nemaha	1986		Washington	1986	
Gove*			Neosho	1998		Wichita*		
Graham		1992	Ness	2004		Wilson	1998	
Grant		2008	Norton	1992		Woodson		2008
Gray*			Osage	1986		Wyandotte	1986	1988

Notes: Based on information provided by the Kansas Division of Alcoholic Beverage Control. In 1986, voters approved a measure allowing counties to go from "dry" to "wet." This measure garnered a majority of votes in 36 counties and, as of July 1, 1987, establishments in these counties were allowed to sell liquor by the drink. We code these laws as equal to .5 in 1987 and equal to one for the years thereafter. Subsequent votes to allow by-the-drink sales or remove the food sales requirement took place in November and were officially implemented within a few days. Because the process of obtaining a new liquor license took at least one month, we code these laws as coming into effect on January 1st of the following year. As of 2011, 19 counties in Kansas prohibited on-premises alcohol consumption. These counties are denoted with an asterisk. Sixty-two counties allowed establishments to sell alcohol for on-premises consumption provided that at least 30 percent of gross receipts were from food sales. The remaining 24 counties allowed on-premises alcohol consumption without requiring food sales.

Table 2. Descriptive Statistics

	Full sample Mean (SD)	*Wet Law* = 1 Mean (SD)	*Wet Law* = 0 Mean (SD)	Description
Violent Crime	3.81 (2.97)	4.28 (2.94)	3.05 (2.86)	Violent crimes in county *c* and year *t* per 1,000 population
Property Crime	40.8 (21.6)	42.0 (20.6)	38.8 (23.0)	Property crimes in county *c* and year *t* per 1,000 population
On-Premises Licenses	6.17 (3.34)	8.13 (2.23)	2.95 (2.16)	Active on-premises alcohol licenses per 10,000 population in county *c* and year *t*
Income	26.0 (7.02)	28.5 (7.21)	21.9 (4.24)	Real income per capita ($1,000)
Unemployment	4.92 (1.79)	5.05 (1.74)	4.70 (1.85)	County unemployment rate
Democratic to GOP	.919 (.579)	.954 (.632)	.862 (.474)	Ratio of Democratic to GOP votes in presidential and gubernatorial elections
Population Density	322.2 (368.8)	397.7 (390.1)	198.7 (291.9)	Population per square mile
Percent Nonwhite	.087 (.076)	.103 (.077)	.060 (.065)	Percent of the county population that was nonwhite
Percent Adult Male	.356 (.021)	.360 (.021)	.350 (.021)	Percent of the county population that was male and 18+ years of age
Percent 21 and Over	.687 (.026)	.692 (.023)	.678 (.029)	Percent of the county population that was 21+ years of age
Sunday Sales	.189 (.391)	.299 (.458)	.009 (.094)	= 1 if Sunday sales of alcohol for off-premises consumption were legal anywhere within the county, = 0 otherwise
N	3,352	1,291	2,061	

Notes: Crime data come from the Uniform Crime Reports and the *Kansas Statistical Abstract*. Data on income per capita, the unemployment rate, and the Democratic to GOP voting ratio come from the Institute for Policy and Social Research at the University of Kansas (http://ipsr.ku.edu/ksdata/). Population data come from the U.S. Census. The Kansas Department of Revenue provided the authors with a list of municipalities that currently allow Sunday sales of alcohol for off-premises consumption. Effective dates for Sunday sales were acquired by searching municipal codes, local newspapers, and town council minutes, or contacting municipal clerks directly. The years 1995, 1996, and 1999 are excluded because of missing crime data. Means are weighted by county population and standard deviations are shown in parentheses.

Table 3. First-Stage: Wet Laws and On-Premises Alcohol Licenses, 1977-2011

	ln(*On-Premises Licenses*)	ln(*On-Premises Licenses*)
Wet Law	.284***	.251***
	(.043)	(.032)
N	3,352	3,352
R^2	.909	.947
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the number of active on-premises alcohol licenses per 10,000 population in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 4. On-Premises Licenses and Violent Crime, 1977-2011

	OLS ln(*Violent Crime*)	OLS ln(*Violent Crime*)	2SLS ln(*Violent Crime*)	2SLS ln(*Violent Crime*)
ln(*On-Premises Licenses*)	.062	.110**	.365**	.405**
	(.043)	(.054)	(.183)	(.179)
N	3,352	3,352	3,352	3,352
R^2	.767	.820	.758	.815
F-test of instrument	...	...	43.6	62.9
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 5. On-Premises Licenses and Violent Crime by Crime Type, 1977-2011

	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Murder*)	ln(*Murder*)	ln(*Rape*)	ln(*Rape*)	ln(*Robbery*)	ln(*Robbery*)	ln(*Assault*)	ln(*Assault*)
ln(*On-Premises Licenses*)	.360*	.432**	.009	.022	.138**	.139**	.302**	.459**	.312*	.290
	(.192)	(.203)	(.017)	(.024)	(.067)	(.063)	(.137)	(.194)	(.184)	(.208)
N	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932
R^2	.750	.812	.515	.536	.677	.751	.862	.877	.648	.771
F-test of instrument	41.8	63.4	41.8	63.4	41.8	63.4	41.8	63.4	41.8	63.4
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate 2SLS regression. The dependent variable is equal to the natural log of the relevant crime rate in county *c* and year *t*. The years 1993-1999 are excluded because of missing crime data. A list of covariates is provided in Table 2 and means of violent crimes by type are provided in Appendix Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 6. Reduced-form Relationship between Wet Laws and Violent Crime, 1977-2011

	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)
Wet Law	.104**	.102**	…	…	…	…
	(.051)	(.048)				
7 Years before Wet Law	…	…	…	…	…	.064*
						(.035)
6 Years before Wet Law	…	…	…	…	.053	.067
					(.038)	(.043)
5 Years before Wet Law	…	…	…	-.052	-.042	-.026
				(.041)	(.045)	(.051)
4 Years before Wet Law	…	…	…	-.063	-.052	-.037
				(.047)	(.050)	(.055)
3 Years before Wet Law	…	…	…	-.014	-.004	.014
				(.050)	(.053)	(.058)
2 Years before Wet Law	…	…	…	.026	.038	.053
				(.051)	(.055)	(.060)
1 Year before Wet Law	…	…	…	.076	.086	.108
				(.057)	(.061)	(.069)
Year of Law Change	…	…	.003	-.000	.014	.032
			(.044)	(.058)	(.063)	(.068)
1 Year after Wet Law	…	…	.077	.072	.084	.104
			(.054)	(.070)	(.074)	(.080)
2 Years after Wet Law	…	…	.135**	.129*	.142*	.160*
			(.059)	(.075)	(.080)	(.086)
3 Years after Wet Law	…	…	.167**	.163*	.175*	.204**
			(.070)	(.088)	(.091)	(.101)
4 Years after Wet Law	…	…	.156**	.153*	.173*	.195**
			(.065)	(.082)	(.088)	(.095)
5+ Years after Wet Law			.062	.057	.074	.102
			(.060)	(.070)	(.072)	(.079)
N	3,352	3,352	3,352	3,352	3,352	3,352
R^2	.768	.820	.821	.821	.822	.822
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	No	Yes	Yes	Yes	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 7. Reduced-form Relationship between Wet Laws and Violent Crime by Crime Type, 1977-2011

	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Murder*)	ln(*Murder*)	ln(*Rape*)	ln(*Rape*)	ln(*Robbery*)	ln(*Robbery*)	ln(*Assault*)	ln(*Assault*)
Wet Law	.112*	.122**	.003	.006	.043**	.039**	.094**	.129**	.097*	.082
	(.058)	(.060)	(.005)	(.007)	(.020)	(.019)	(.043)	(.057)	(.055)	(.061)
N	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2.932	2,932
R^2	.760	.819	.515	.539	.695	.757	.873	.891	.659	.775
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the relevant crime rate in county *c* and year *t*. The years 1993-1999 are excluded because of missing crime data. A list of covariates is provided in Table 2 and means of violent crimes by type are provided in Appendix Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 8. Robustness Checks, Violent Crime

	(1) Placebo *Wet Law*	(2) Restrict to counties with population > 5,000	(3) Restrict to counties with population > 10,000	(4) Control for county-specific quadratic time trends	(5) Control for crack epidemic	(6) Dependent variable = *Violent Crime*	(7) Negative binomial
Average placebo *Wet Law* estimate	-.006	...	...	...	...	...	...
Wet Law	...	.098*	.121*	.145**	.119**	.714**	.117*
		(.051)	(.066)	(.072)	(.058)	(.281)	(.065)
N	3,352	2,299	1,366	3,352	3,352	3,352	3,352
Number of trials	100	...	...	...	...	...	...
Placebo coefficient > 0	51	...	...	...	...	...	...
Placebo coefficient > 0 and significant at 5% level	3	...	...	...	...	...	...
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Unless otherwise noted, each column represents the results from a separate OLS regression. In columns (1) through (5) the dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. In column (6) the dependent variable is equal to the crime rate in county *c* and year *t*. In column (7) the dependent variable is measured as a count and county population is added as a control. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 9. Wet Laws and Sworn Officer Employment

	ln(*Officers Employed by Sheriffs' Offices*)	ln(*Officers Employed by Police Depts.*)
Wet Law	-.031	-.032
	(.059)	(.046)
Mean of the number of sworn officers per 10,000 population	7.18	14.0
N	1,731	1,711
R^2	.912	.878
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. Law enforcement employment data come from the annual publication *Crime in the United States* and are available for the period 1995 through 2011. The dependent variable is equal to the natural log of the number of sworn officers per 10,000 population in county *c* and year *t*. A list of covariates is provided in Table 2. In the police employment regression, we also controlled for the number of police departments reporting data in a county-year. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 10. Economic Conditions, the Democratic to GOP Voting Ratio, and Wet Laws

	Wet Law	*Wet Law*
Income	.002	.007
	(.005)	(.005)
Unemployment	-.014	-.005
	(.009)	(.009)
Democratic to GOP	-.029	-.044
	(.029)	(.035)
Mean of *Wet Law*	.629	.629
N	3,675	3,675
R^2	.840	.876
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to one if county *c* allowed by-the-drink sales to the general public in year *t* (and is equal to zero otherwise). A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 11. Distinguishing between Wet Laws with and without the 30 Percent Food Sales Requirement

	ln(*Violent Crime*)	ln(*Violent Crime*)
Wet Law with Food Sales 30% Gross	.101**	.104**
	(.049)	(.047)
Wet Law with Food Sales Not Required	.119	.078
	(.098)	(.074)
N	3,352	3,352
R^2	.768	.820
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 12. Did Wet Laws Create or Displace Violent Crime? Adding Bordering Wet Counties to the Model

	Full sample		Always-dry counties		Full sample	Always-dry counties
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)	ln(*Violent Crime*)
Wet Law	.108*	.101*	...	...	.105*	…
	(.059)	(.052)			(.058)	
Number of Wet Counties on Border	-.006	…	.012	…	…	…
	(.024)		(.054)			
One Wet County on Border	…	.043	…	-.075	…	…
		(.095)		(.173)		
Two or More Wet Counties on Border	…	.018	…	.008	…	…
		(.090)		(.137)		
Number of Wet Counties on Border with Food Sales Requirement	…	…	…	…	-.007	.017
					(.025)	(.048)
Number of Wet Counties on Border without Food Sales Requirement	…	…	…	…	.037	-.019
					(.046)	(.125)
N	3,352	3,352	607	607	3,352	607
R^2	.820	.820	.608	.609	.821	.608
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Table 13. On-Premises Consumption of Alcohol and Property Crime, 1977-2011

	2SLS ln(*Property Crime*)	2SLS ln(*Property Crime*)	Reduced-form ln(*Property Crime*)	Reduced-form ln(*Property Crime*)
ln(*On-Premises Licenses*)	.328*	.408**	...	...
	(.192)	(.201)		
Wet Law	...	...	.093*	.102*
			(.054)	(.052)
N	3,352	3,352	3,352	3,352
R^2	.761	.800	.768	.806
F-test of instrument	43.6	62.9	...	...
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate regression. The dependent variable is equal to the natural log of the property crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

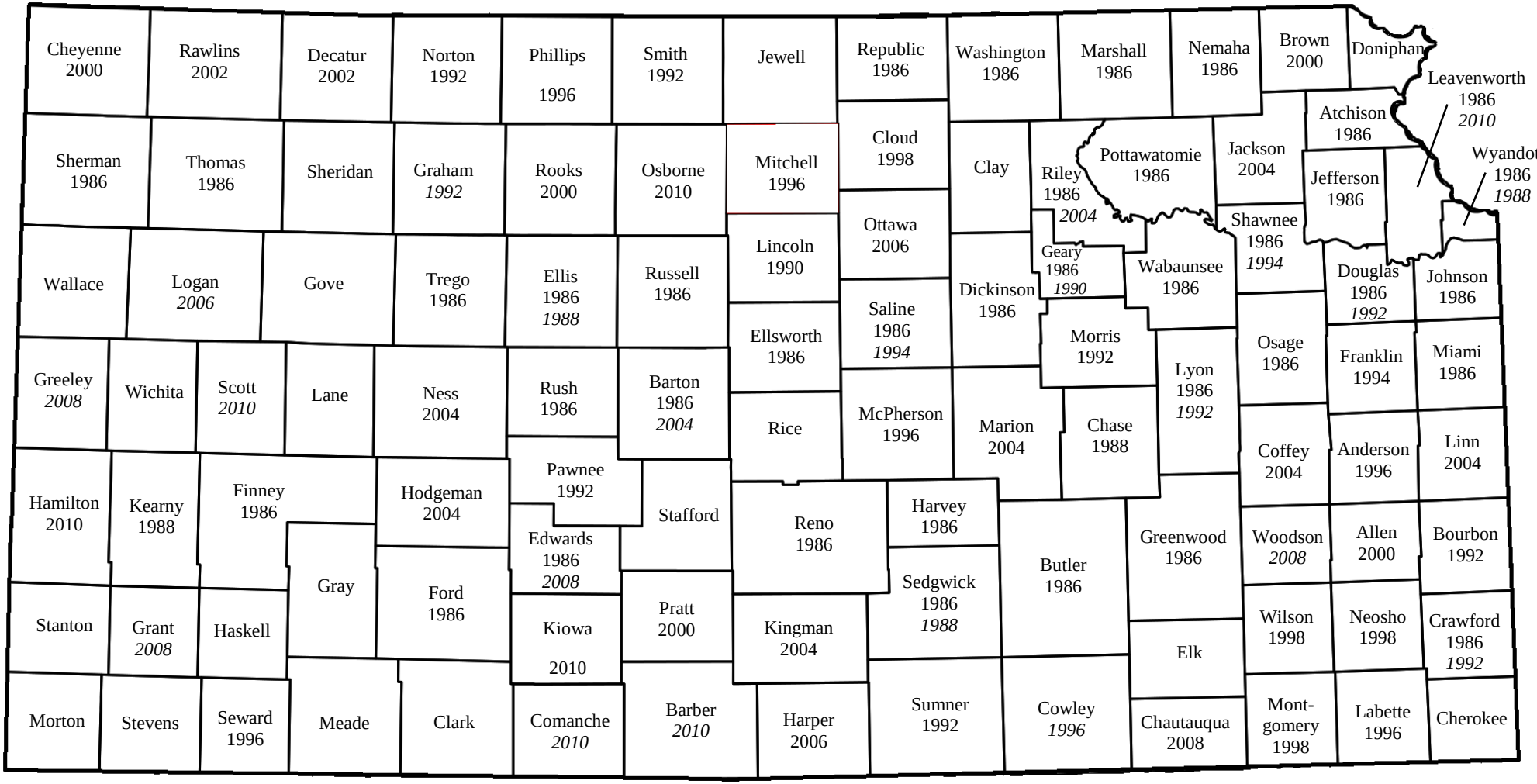
Table 14. Reduced-form Relationship between Wet Laws and Property Crime by Crime Type, 1977-2011

	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Burglary*)	ln(*Burglary*)	ln(*Larceny*)	ln(*Larceny*)	ln(*Motor Vehicle Theft*)	ln(*Motor Vehicle Theft*)
Wet Law	.118*	.148**	.085	.107	.093	.136**	.130**	.180**
	(.061)	(.066)	(.061)	(.069)	(.058)	(.062)	(.058)	(.073)
N	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932
R^2	.757	.799	.760	.802	.773	.818	.764	.793
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the relevant crime rate in county *c* and year *t*. The years 1993-1999 are excluded because of missing crime data. A list of covariates is provided in Table 2 and means of property crimes by type are provided in Appendix Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

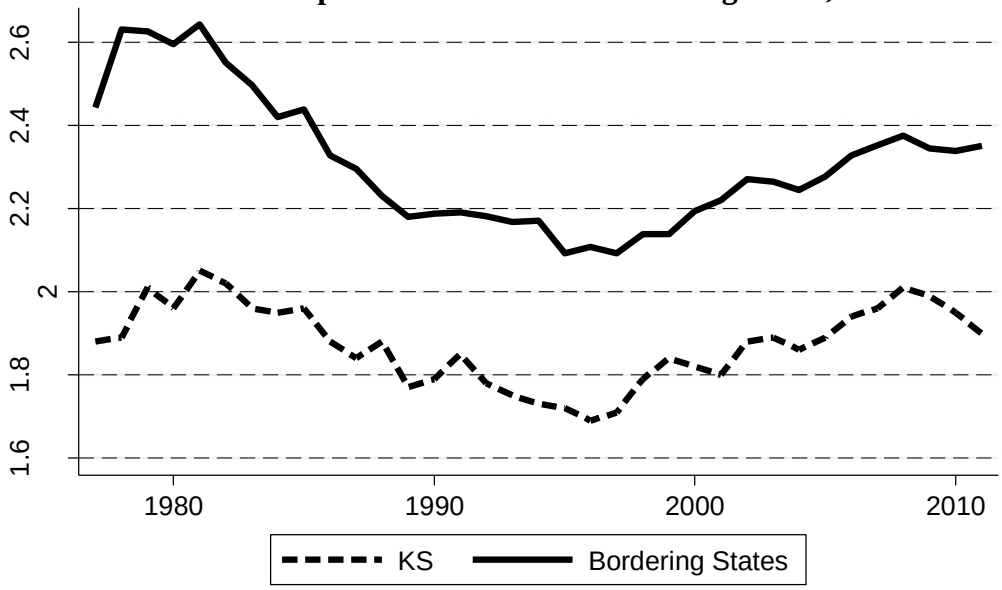
Appendix Figure 1. Kansas Wet Laws, 1977-2011



Notes: The year below each county name denotes when on-premises alcohol consumption was voted into law. Italicized print indicates that on-premises consumption was allowed without requiring food sales.

Appendix Figure 2

Alcohol Consumption in Kansas and Bordering States, 1977-2011



Note: Data on alcohol consumption are from the National Institute on Alcohol Abuse and Alcoholism.

Appendix Table 1. Wet Laws and Off-Premises Alcohol Licenses, 1977-2011

	ln(*Off-Premises Licenses*)	ln(*Off-Premises Licenses*)
Wet Law	-.011	.036
	(.053)	(.046)
Mean of *Off-Premises Licenses*	2.45	2.45
N	3,352	3,352
R^2	.763	.842
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the number of active off-premises alcohol licenses per 10,000 population in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Appendix Table 2. Descriptive Statistics for Violent and Property Crime by Type

	Full sample Mean (SD)	*Wet Law* = 1 Mean (SD)	*Wet Law* = 0 Mean (SD)
Violent Crime	3.73	4.16	3.12
	(2.89)	(2.80)	(2.92)
Murder	.046	.044	.049
	(.060)	(.059)	(.062)
Rape	.349	.413	.257
	(.252)	(.231)	(.252)
Robbery	.811	.847	.760
	(1.05)	(1.04)	(1.06)
Assault	2.53	2.86	2.05
	(1.80)	(1.76)	(1.75)
Property Crime	39.9	40.2	39.6
	(21.2)	(19.9)	(23.1)
Burglary	9.80	8.69	11.4
	(6.52)	(5.54)	(7.43)
Larceny	27.6	28.6	26.1
	(13.9)	(13.1)	(14.8)
Motor Vehicle Theft	2.57	2.89	2.11
	(2.53)	(2.86)	(1.87)
N	2,932	1,089	1,843

Notes: Information on crimes by type is unavailable for the years 1993-1999. Means are weighted by county population and standard deviations are shown in parentheses. All variables are reported as rates per 1,000 population.

Appendix Table 3. Robustness Checks, Property Crime

	(1) Placebo *Wet Law*	(2) Restrict to counties with population > 5,000	(3) Restrict to counties with population > 10,000	(4) Control for county-specific quadratic time trends	(5) Control for crack epidemic	(6) Dependent variable = *Property Crime*	(7) Negative binomial
Average placebo *Wet Law* estimate	-.002	...	...	...	...	...	...
Wet Law	...	.070	.055	.161**	.127*	3.81**	.091**
		(.052)	(.068)	(.075)	(.066)	(1.83)	(.043)
N	3,352	2,299	1,366	3,352	3,352	3,352	3,352
Number of trials	100	...	...	...	...	...	...
Placebo coefficient > 0	46	...	...	...	...	...	...
Placebo coefficient > 0 and significant at 5% level	1	...	...	...	...	...	...
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Unless otherwise noted, each column represents the results from a separate OLS regression. In columns (1) through (5) the dependent variable is equal to the natural log of the property crime rate in county *c* and year *t*. In column (6) the dependent variable is equal to the crime rate in county *c* and year *t*. In column (7) the dependent variable is measured as a count and county population is added as a control. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Appendix Table 4. On-Premises Licenses and Property Crime by Crime Type, 1977-2011

	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Burglary*)	ln(*Burglary*)	ln(*Larceny*)	ln(*Larceny*)	ln(*Motor Vehicle Theft*)	ln(*Motor Vehicle Theft*)
ln(*On-Premises Licenses*)	.378*	.525**	.274	.380	.300	.482**	.419**	.640**
	(.201)	(.233)	(.193)	(.235)	(.191)	(.223)	(.189)	(.249)
N	2,932	2,932	2,932	2,932	2,932	2,932	2,932	2,932
R^2	.748	.790	.756	.799	.767	.809	.746	.771
F-test of instrument	41.8	63.4	41.8	63.4	41.8	63.4	41.8	63.4
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes	No	Yes	No	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate 2SLS regression. The dependent variable is equal to the natural log of the relevant crime rate in county *c* and year *t*. The years 1993-1999 are excluded because of missing crime data. A list of covariates is provided in Table 2 and means of property crimes by type are provided in Appendix Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

Appendix Table 5. Did Wet Laws Create or Displace Property Crime? Adding Bordering Wet Counties to the Model

	Full sample		Always-dry counties		Full sample	Always-dry counties
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Property Crime*)	ln(*Property Crime*)
Wet Law	.101	.112**	...	...	.100	…
	(.061)	(.055)			(.060)	
Number of Wet Counties on Border	.001	…	-.019	…	…	…
	(.020)		(.076)			
One Wet County on Border	…	.068	…	-.159	…	…
		(.061)		(.243)		
Two or More Wet Counties on Border	…	-.024	…	.036	…	…
		(.057)		(.221)		
Number of Wet Counties on Border with Food Sales Requirement	…	…	…	…	.000	.015
					(.020)	(.057)
Number of Wet Counties on Border without Food Sales Requirement	…	…	…	…	.028	-.241
					(.046)	(.191)
N	3,352	3,352	607	607	3,352	607
R^2	.806	.806	.676	.678	.806	.681
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes	Yes

*Statistically significant at 10% level; ** at 5% level; *** at 1% level.

Notes: Each column represents the results from a separate OLS regression. The dependent variable is equal to the natural log of the property crime rate in county *c* and year *t*. The years 1995, 1996, and 1999 are excluded because of missing crime data. A list of covariates is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.