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WET LAWS, DRINKING ESTABLISHMENTS AND VIOLENT CRIME

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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 violent crime. During this period, 86 out of 105 counties in Kansas voted to legalise the sale of alcohol to the general public for on-premises consumption. Using legalisation as an instrument, we show that a 10% increase in drinking establishments is associated with a 3–5% increase in violent crime. The estimated relationship between drinking establishments and property crime is also positive, although smaller in magnitude.

There is a strong positive correlation between local alcohol availability, as measured by the density of bars and/or other alcohol outlets, and violent crime (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 violent crime.

Even after the adoption of the Twenty-first Amendment to the US 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 1 July 1987, bars and restaurants in these 36 counties were permitted to sell beer, wine and spirits to the general public provided they derived 30% of their gross revenue from

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[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 to 1974, went so far as to raid an Amtrak train travelling through Kansas, arresting a bartender and waiter for serving alcohol to passengers (St. John, 2012*a*). 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'.

food sales (O'Connor, 1987; Robbins, 1987; St. John, 2012*b*). 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 violent crime. For instance, although there is strong evidence that consuming alcohol heightens emotional responses, impairs cognitive functioning 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 licences issued by Kansas for the period 1977–2011, we find evidence that legalising by-the-drink sales led to substantial increases in the number of drinking establishments (e.g. bars and restaurants) in operation. Using legalisation as an instrument, we show that a 10% increase in drinking establishments is associated with a 3–5% increase in violent 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 suggest that legalising by-the-drink sales to the general public leads to a 10–23% increase in violent crime and nearly a 10% increase in property crime. Because we find no evidence that crime fell in dry counties when neighbouring 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).

1. 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 US with some type of prohibition 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% 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

[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 (Chermack and Taylor, 1995; Fagan, 1993; Giancoloa, 2004).

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, legalising 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 behaviour (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 behaviour as an important component of their jobs (Gusfield *et al.*, 1984; Lee *et al.*, 2008).[3]

By-the-drink sales could also influence the demand for other substances such as marijuana and cocaine, which could, in turn, affect violent 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, 2015) but their results have been decidedly mixed.

1.1. *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; Markowitz, 2000, 2001, 2005; Markowitz and Grossman, 2000; DeSimone, 2001; Cook and Piette Durrance, 2013), Federal Prohibition (Miron, 1999; Owens, 2011), the minimum legal drinking age (Joksch and Jones, 1993; Carpenter, 2005; Carpenter and Dobkin, 2015), 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]

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

[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, 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.

[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 DC Metro expanded train services.

between establishment density in a neighbourhood (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-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 privatisation of distilled spirits sales in Seattle to estimate the relationship between liquor store density and crime. Privatisation 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 6–8% increase in crime. The effects of privatisation 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

[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 (2008*a*), Liang and Chikritzhs (2011) and Toomey *et al.* (2012). For a more thorough review of this literature, see White *et al.* (2015).

[8] Gyimah-Brempong (2001) used the number of petrol 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 (2008*b*, 2011) and White *et al.* (2015).

[10] Conlin *et al.* (2005) used county-level data from Texas for the period 1978–96 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. Several studies have estimated the relationship between local dry laws and traffic accidents (Baughman *et al.*, 2001; Blose and Holder, 1987; Gary *et al.*, 2003; Winn and Giacopassi, 1993).

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

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.

1.2. *Dry Laws and Liquor Licences in Kansas, 1977–2011*

Table 1 summarises 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 to 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 1 July 1987, by-the-drink sales to the general public became legal in 36 counties, although establishments were required to derive 30% of their gross revenue from selling food (O'Connor, 1987; Robbins, 1987; St. John, 2012*b*).[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 remain dry in 1986. Eleven of these 50 counties did not impose a food sales requirement; 39 required that establishments derive 30% 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, 1992*a*; Associated Press, 2000; Haxon, 2012).[16] However, because the process of obtaining

[12] A map of Kansas showing the year in which counties allowed by-the-drink sales is provided in Appendix A (Figure A1). If by-the-drink sales were allowed without the requirement that establishments derive 30% of their gross revenue from selling food, then the year the law went into effect is italicised.

[13] Private clubs in Kansas are classified into two types, or 'classes'. Class A clubs are owned or leased by nonprofit organisations 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 legalise by-the drink sales failed in 69 counties, the state-wide vote was 59.9% in favour and 40.1% against. Under Kansas law:

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

[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, 1992*a*; Associated Press, 2000).

Table 1

Kansas Wet Laws, 1977–2011

	Year law was passed			Year law was passed			Year law was passed	
	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 1 July 1987, establishments in these counties were allowed to sell liquor by the drink. We code these laws as equal to 0.5 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 licence 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% of gross receipts were from food sales. The remaining 24 counties allowed on-premises alcohol consumption without requiring food sales.

a new liquor licence took at least one month (Toplikar, 1992; Haxon, 2012; Scherer, 2012), we code these laws as coming into effect on 1 January of the following year.[17]

[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.

Data on liquor licences were purchased from the Kansas Division of Alcoholic Beverage Control and include information on licence type (on-*versus* off-premises), the location and name of the establishment, and the dates the licence became active and inactive.[18] This information was used to create a count of active on-premises licences 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 licences issued by the Kansas Division of Alcoholic Beverage Control. It was constructed by regressing the number of on-premises alcohol licences (per 1,000 population) in county c and year t on an indicator *Year of Law Change*$_{ct}$ 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 five lags of *Year of Law Change* were also included on the right-hand side of this regression as well as 34 year dummies. For now, we do not distinguish between counties that required 30% of gross receipts from the sale of food and those that did not; counties that never allowed by-the-drink sales were excluded from this regression.

The estimated coefficients of the five 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

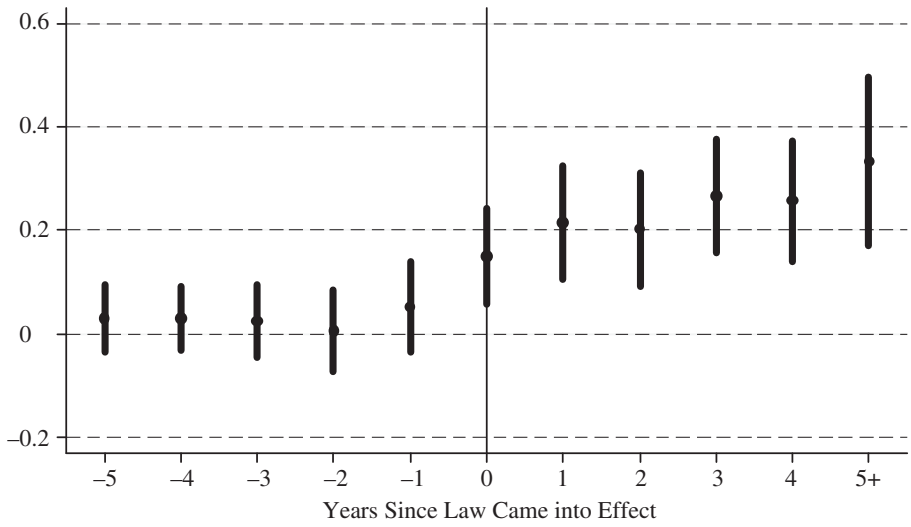

Fig. 1. *Trends in On-premises Alcohol Licences*

Notes. OLS coefficient estimates (and their 95% confidence intervals) are reported. The dependent variable is equal to the number of on-premises liquor licences per 1,000 population in county c and year t. The controls include year fixed effects and the data cover the period 1977–2011.

[18] Approximately 12% of the on-premises licences 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 licences to caterers, hotels, and public venues.

0.150 additional on-premises licences relative to the omitted period (6 or more years before the law change); two years later, on-premises licences had increased by 0.203; five years later, on-premises licences had increased by 0.333. 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 licences became active and inactive, we have information on the number of off-premises liquor licences issued by the Kansas Division of Alcoholic Beverage Control. Figure 2 shows the results of regressing the number of off-premises licences (per 1,000 population) on the indicator *Year of Law Change*, five leads of this indicator, and five lags. They suggest that neither legalisation nor its correlates were related to the number of establishments selling alcohol for off-premises consumption, perhaps because the retail liquor industry in Kansas is subject to tight controls (Byrne and Nizovtsev, 2014).[20]

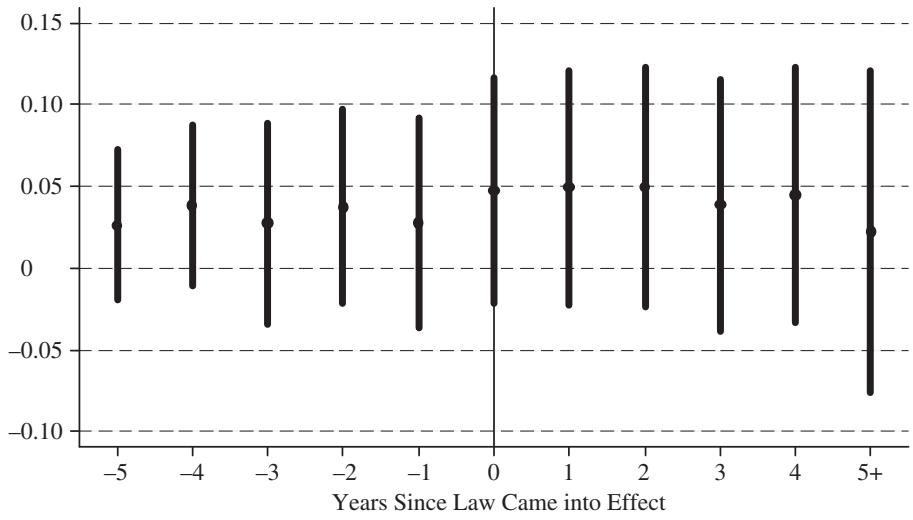

Fig. 2. *Trends in Off-premises Alcohol Licences*

Notes. OLS coefficient estimates (and their 95% confidence intervals) are reported. The dependent variable is equal to the number of off-premises liquor licences per 1,000 population in county *c* and year *t*. The controls include year fixed effects and the data cover the period 1977–2011.

[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, 1992*b*,*c*; Haxon, 2012; McKinney, 2009).

[20] Grocery and convenience stores in Kansas are banned from selling beer, liquor and wine but liquor licence fees are not prohibitively expensive. The current fee for obtaining an on-premises liquor licence is $2,000, while the fee for an off-premises licence is $500. In addition to examining whether the number of off-premises licences changed with legalisation, we used data from the US Census Bureau's County Business Patterns data set to explore what happened to employment levels at establishments licensed to sell alcohol for off-premises consumption. Figure A2 shows the results from this exercise. It suggests that neither legalisation of by-the-drink sales nor its correlates were related to employment levels at these establishments.

2. Methods

We begin our formal analysis by exploring whether economic conditions and voting patterns predict whether by-the-drink sales were legal. Specifically, we regress an indicator, *Wet Law* (equal to 1 if county c allowed by the drink sales in year t and equal to 0 otherwise), on income per capita, the unemployment rate, the ratio of Democratic to Republican (GOP) votes, an indicator for whether Sunday sales of alcohol were allowed and a set of demographic controls.[21]

Without controlling for county fixed effects, both income and the ratio of Democratic to GOP votes are positively associated with whether by-the-drinks sales were permitted (Table 3). However, when county fixed effects are included, the estimated coefficients of these variables become small and statistically insignificant.[22] Although the estimated coefficient of *Sunday Sales* is negative and significant when county fixed effects are included on the right-hand side of the regression, we are careful to include this variable as a control when estimating the effects of drinking establishments and wet laws on crime. Moreover, in a robustness check below, we show that our results are similar when focusing on the subset of counties in which Sunday sales of alcohol were not allowed.

Next, in an effort to isolate exogenous variation in the number of on-premises liquor licences, we estimate the following first-stage equation:

$$On\text{-}Premises\ Licences_{ct} = \alpha_0 + \alpha_1 Wet\ Law_{ct} + \mathbf{X}_{ct}\boldsymbol{\alpha_2} + v_c + z_t + \varepsilon_{ct}, \quad (1)$$

where the dependent variable, *On-Premises Licences*, is equal to the number of active on-premises liquor licences per 10,000 population in county c and year t.[23] The vector $\mathbf{X}$ includes county-level controls for economic conditions (income per capita and the unemployment rate), population density, demographics (% of the county population that was non-White, 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 legal. 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

[21] Descriptive statistics and variable definitions are provided in Table 2. This analysis (and subsequent analyses) drew upon data from all 105 counties in Kanas, including counties that were dry throughout the period 1977–2011. 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/). Demographic information comes from the US 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 legalisation 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).

[22] 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 legalising by-the-drink sales.

[23] There is a one-to-one correspondence between the number of active on-premises licences in county c and year t and the number of establishments permitted to sell alcohol by-the-drink. *On-Premises Licences* is equal to 0 for 392 of the 3,352 county-year observations.

Table 2

Descriptive Statistics

	Full sample	*Wet Law* = 1	*Wet Law* = 0	
	Mean	Mean	Mean	
	(SD)	(SD)	(SD)	Description
Violent crime	3.81	4.28	3.05	Violent crimes in county *c* and year *t* per
	(2.97)	(2.94)	(2.86)	1,000 population
Property crime	40.8	42.0	38.8	Property crimes in county *c* and year *t*
	(21.6)	(20.6)	(23.0)	per 1,000 population
On-Premises Licences	0.617	0.813	0.295	Active on-premises alcohol licences per
	(0.334)	(0.223)	(0.216)	1,000 population in county *c* and year *t*
Income	26.0	28.5	21.9	Real income per capita ($1,000)
	(7.02)	(7.21)	(4.24)	
Unemployment	4.92	5.05	4.70	County unemployment rate
	(1.79)	(1.74)	(1.85)	
Democratic to GOP	0.919	0.954	0.862	Ratio of democratic to GOP votes in
	(0.579)	(0.632)	(0.474)	presidential and gubernatorial elections
Population Density	0.322	0.398	0.199	Population per square mile (1,000)
	(0.369)	(0.390)	(0.292)	
% Non-White	0.087	0.103	0.060	% of the county population that was
	(0.076)	(0.077)	(0.065)	non-white
% Adult Male	0.356	0.360	0.350	% of the county population that was
	(0.021)	(0.021)	(0.021)	male and 18+ years of age
% 21 and Over	0.687	0.692	0.678	% of the county population that was
	(0.026)	(0.023)	(0.029)	21+ years of age
Sunday Sales	0.189	0.299	0.009	=1 if Sunday sales of alcohol for
	(0.391)	(0.458)	(0.094)	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 US 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.

and Social Research.[24] The (second-stage) relationship between violent crime and on-premises liquor licences is given by the following equation:

$$\textit{Violent Crime}_{ct} = \beta_0 + \beta_1 \textit{On-Premises Licences}_{ct} + \mathbf{X}_{ct}\boldsymbol{\beta_2} + v_c + z_t + \varepsilon_{ct}, \qquad (2)$$

where *Violent Crime* is equal to the violent crime rate in county *c* and year *t*, and *On-Premises Licences* is instrumented using (1). The vector **X** is composed of the

[24] For multiple-offence events, only the most serious crime is reported. Crime data at the county level were unavailable from the UCR for the period 1993–9. 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.

Table 3

County Characteristics and Wet Laws

	Wet Law	*Wet Law*
Income	0.016***	0.002
	(0.005)	(0.005)
Unemployment	−0.000	−0.014
	(0.012)	(0.009)
Democratic to GOP	0.071**	−0.029
	(0.033)	(0.029)
Sunday sales	−0.054	−0.129***
	(0.033)	(0.034)
Mean of *wet law*	0.629	0.629
N	3,675	3,675
R^2	0.721	0.840
Year FEs	Yes	Yes
County demographics	Yes	Yes
County FEs	No	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. 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 the county demographics is provided in Table 2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

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 the effect of an additional establishment licensed to sell alcohol to the general public for on-premises consumption (i.e. bars and restaurants) on violent crime.[25]

The instrumental variables strategy outlined above is based on the assumption that the adoption of wet laws affected violent crime only through the number of establishments with on-premises liquor licences. It is possible, however, that allowing by-the-drink sales impacted violent crime through the number of off-premises licences 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 licences 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

[25] Many private clubs were converted into restaurants or bars after voters allowed by-the-drink sales to the general public (Buckner, 1992*b*; Haxon, 2012; McKinney, 2009; Toplikar, 1992). 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% 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).

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 of going from dry to wet on violent crime. Below, we present estimates of the following reduced-form equation:

$$Violent\ Crime_{ct} = \pi_0 + \pi_1 Wet\ Law_{ct} + \mathbf{X}_{ct}\boldsymbol{\pi_2} + v_c + z_t + \varepsilon_{ct}, \quad (3)$$

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]

3. First-stage Estimates

Estimates of the first-stage relationship between *Wet Law* and on-premises liquor licences are reported in Table 4 and 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 0.177 increase in the number of active on-premises licences per 1,000 population (relative to a pre-treatment mean of 0.295). 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 0.143

Table 4

Wet Laws and On-Premises Alcohol Licences, 1977–2011

	On-Premises Licences	*On-Premises Licences*
Wet Law	0.177***	0.143***
	(0.025)	(0.022)
Pre-treatment mean of *On-premises Licences*	0.295	0.295
N	3,352	3,352
R^2	0.893	0.942
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

Notes. *Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to the number of active on-premises alcohol licences per 1,000 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.

[26] Unfortunately, we do not have access to data on alcohol consumption at the county level. Figure A3 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 neighbouring states, suggesting that it was not caused by changes in Kansas wet laws.

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

increase in the number of on-premises licences. Both of these estimates are statistically significant at conventional levels and meet the Staiger and Stock (1997) criterion.[28]

Estimates of the relationship between allowing by-the-drink sales and off-premises liquor licences are presented in Table A1. 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.

4. Violent Crime

4.1. *Instrumental Variables Estimates*

The first three columns of Table 5 present OLS estimates of the relationship between on-premises liquor licences and violent crime. Without the covariates on the right-hand side of the model, issuing an on-premises liquor licence is associated with 0.199 additional violent crimes, although this estimate is not statistically significant at conventional levels. When the covariates listed in Table 2 and county-specific linear trends are included, issuing an on-premises liquor licence is associated with 1.24 additional violent crimes.

The 2SLS estimates, which are reported in columns (4) through (6) of Table 5, are quite a bit larger than the OLS estimates. Specifically, issuing an on-premises liquor licence is associated with 3.64–5.00 additional violent crimes. There are at least two

Table 5

On-premises Licences and Violent Crime, 1977–2011

	OLS	OLS	OLS	2SLS	2SLS	2SLS
	Violent Crime	*Violent Crime*	*Violent Crime*	*Violent Crime*	*Violent Crime*	*Violent Crime*
On-Premises Licences	0.199	0.853*	1.24*	3.64**	4.31**	5.00***
	(0.466)	(0.500)	(0.667)	(1.55)	(1.77)	(1.87)
Pre-treatment mean of *Violent Crime*	3.05	3.05	3.05	3.05	3.05	3.05
N	3,352	3,352	3,352	3,352	3,352	3,352
R^2	0.730	0.758	0.847	0.709	0.740	0.836
F-test of instrument	–	–	–	47.9	51.9	41.9
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	No	Yes	Yes
County linear trends	No	No	Yes	No	No	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to 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.

[28] The test of whether the coefficient of *Wet Law* is equal to zero yields an F-statistic of 51.9 when county-specific linear trends are not included as controls. The F-statistic is 41.9 when county-specific linear trends are included.

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 licences were issued to private clubs once by-the-drink sales were permitted, the 2SLS estimates in Table 5 should be thought of as representing the effect of a new establishment licensed to sell alcohol to the general public for on-premises consumption (i.e. bars and restaurants).

The 2SLS estimates reported in Table 5 can be used to calculate elasticities of violent crime with respect to the number of drinking establishments. For instance, if we use the 2SLS estimate from the model without covariates or county-specific linear trends on the right-hand side, 3.64 violent crimes represent a 119% increase relative to the pre-treatment mean of 3.05. Given that a new on-premises liquor licence represents a 339% increase relative to the pre-treatment mean of 0.295, a 10% increase in the number of drinking establishments is associated with a 3.5% increase in violent crime. If we use the 2SLS estimate from the fully specified model, a 10% increase in the number of drinking establishments is associated with a 4.8% increase in violent crime.

Table 6 shows 2SLS estimates disaggregated by type of violent crime. The sample sizes in Table 6 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 1993–9 and the *Kansas Statistical Abstract*, our alternative source of data, does not report violent crimes disaggregated by type. Opening a new drinking establishment is associated with an

Table 6

On-premises Licences and Violent Crime by Crime Type, 1977–2011

	Violent crime	Murder	Rape	Robbery	Assault
On-Premises Licences	5.38**	0.044	0.421**	2.53**	2.39
	(2.19)	(0.049)	(0.170)	(1.12)	(1.63)
Pre-treatment mean of dependent variable	3.12	0.049	0.257	0.760	2.05
N	2,932	2,932	2,932	2,932	2,932
R^2	0.833	0.528	0.744	0.838	0.790
F-test of instrument	42.6	42.6	42.6	42.6	42.6
Year FEs	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate 2SLS regression. The dependent variable is equal to the relevant crime rate in county *c* and year *t*. The years 1993–9 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 Table A2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

additional 0.421 rapes, 2.53 robberies and 2.39 assaults. The estimates for rape and robbery are statistically significant, while the estimate for assault is statistically insignificant at conventional levels (p-value = 0.143). The relationship between on-premises liquor licences and murders is small and statistically insignificant.[29]

4.2. *Reduced-form Estimates*

Table 7 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. The baseline estimate reported in the first column of Table 7 suggests that allowing by-the-drink sales is associated with a 0.721 increase in the violent crime rate, or a 23.6% increase relative to the pre-treatment mean of 3.05. When covariates are added, the estimate of π_1 increases to 0.761, but the fully specified model produces a slightly smaller estimate: allowing by-the-drink sales is associated with a 0.714 increase in the violent crime rate, or a 23.4% increase relative to the pre-treatment mean.

The remaining columns in Table 7 present estimates of (3) augmented with leads and lags of the wet law indicator. Consistent with the parallel trends assumption, there is little evidence that the violent crime rate increased in the years leading up to legalisation. At Year 0, estimates of the relationship between allowing by-the-drink sales and violent crime are small and statistically insignificant; after two years, these estimates are positive and significant at conventional levels; and after 3 years, allowing by-the-drink sales is associated with roughly one additional violent crime per 1,000 population. However, after 5 years the estimates become smaller, suggesting that the effect of legalising by-the-drink sales could eventually dissipate.

We report reduced-form estimates disaggregated by type of violent crime in Table 8. The results are qualitatively similar to those in Table 6 and suggest that allowing by-the-drink sales is associated with an additional 0.065 rapes, 0.393 robberies and 0.372 assaults. The estimates for rape and robbery are statistically significant at the 5% level, while the estimate for assault is not statistically significant at conventional levels (p-value = 0.150). The estimated relationship between wet laws and murder is small and statistically insignificant.

We report the results of various robustness checks in Table 9. 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 legalised 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% level only twice. The mean of the placebo

[29] This last result is consistent with those of previous studies. For instance, Carpenter and Dobkin (2015) found that turning 21 years of age led to more robberies and assaults but was essentially unrelated to murders and rapes; Corman and Mocan (2015) 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% of inmates incarcerated for assault were drinking at the time of the offence, over 40% of inmates incarcerated for rape were drinking at the time of the offence, and approximately 25% of inmates incarcerated for robbery were drinking at the time of the offence (Billings, 2014). Means of crime by type are provided in Table A2.

Table 7

Reduced-form Relationship between Wet Laws and Violent Crime, 1977–2011

	Violent Crime	*Violent Crime*	*Violent Crime*	*Violent Crime*	*Violent Crime*
Wet Law	0.721**	0.761**	0.714**	–	–
	(0.292)	(0.306)	(0.281)		
5 *Years before Wet Law*	–	–	–	–	−0.261
					(0.169)
4 *Years before Wet Law*	–	–	–	–	−0.362**
					(0.177)
3 *Years before Wet Law*	–	–	–	–	−0.072
					(0.223)
2 *Years before Wet Law*	–	–	–	–	0.154
					(0.261)
1 *Year before Wet Law*	–	–	–	–	0.424
					(0.342)
Year of Law Change	–	–	–	0.199	0.184
				(0.250)	(0.339)
1 *Year after Wet Law*	–	–	–	0.412	0.389
				(0.270)	(0.361)
2 *Years after Wet Law*	–	–	–	0.794**	0.762*
				(0.319)	(0.405)
3 *Years after Wet Law*	–	–	–	1.05**	1.03*
				(0.455)	(0.548)
4 *Years after Wet Law*	–	–	–	1.10***	1.09**
				(0.410)	(0.493)
5+ *Years after Wet Law*	–	–	–	0.625*	0.606*
				(0.326)	(0.341)
Pre-treatment mean of *Violent Crime*	3.05	3.05	3.05	3.05	3.05
N	3,352	3,352	3,352	3,352	3,352
R^2	0.732	0.759	0.847	0.848	0.848
Year FEs	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	Yes	Yes
County linear trends	No	No	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to 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.

coefficients is equal to −0.022. 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 7, while the estimate in column (3) is slightly larger in magnitude. When the sample is limited to counties with a population over 10,000, allowing by-the-drink sales is associated with a 0.895 increase in the violent crime rate.

Next, we restrict the analysis to counties that prohibited the sale of alcohol for off-premises consumption on Sundays. The estimate of π_1, reported in column (4), is positive, larger in magnitude than our baseline estimates and statistically significant at

Table 8

Reduced-form Relationship between Wet Laws and Violent Crime by Crime Type, 1977–2011

	Violent crime	Murder	Rape	Robbery	Assault
Wet Law	0.838**	0.007	0.065**	0.393**	0.372
	(0.345)	(0.008)	(0.029)	(0.186)	(0.257)
Pre-treatment mean of dependent variable	3.12	0.049	0.257	0.760	2.05
N	2,932	2,932	2,932	2,932	2.932
R^2	0.848	0.530	0.753	0.865	0.797
Year FEs	Yes	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to the relevant crime rate in county *c* and year *t*. The years 1993–9 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 Table A2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

conventional levels. Specifically, allowing by-the-drink sales is associated with a 0.810 increase in the violent crime rate. Likewise, controlling for county-specific quadratic time trends produces a relatively large, positive and significant estimate of π_1.

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 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).[30] The results are reported in column (6) of Table 9. 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 0.769 increase in the violent crime rate.[31]

In column (7) of Table 9, we regress the natural log of the crime rate on the wet law indicator and the full set of controls; in column (8) we estimate the relationship between by-the-drink sales and violent crime using a negative binomial regression model.[32] These modifications produce smaller estimates than those reported in the

[30] 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 1986–8, 1990–2, 1994–6, 1998–9 and 2001–3. Our crack epidemic control takes on the value of 0 for all other counties and years.

[31] In an effort to explore whether the crack epidemic reached smaller Kansas cities, we examined cocaine arrests outside of the four most populous counties in Kansas (Johnson, Sedgwick, Shawnee and Wyandotte) for the period 1985–92. There was a steady increase in cocaine arrests after 1986. This increase, however, was not pronounced in counties that voted to legalise by-the-drink sales in 1986 as compared to counties that voted to legalise by-the-drinks sales after 1986.

[32] 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. 193–4). Following Carpenter and Dobkin (2009) and Gelber (2014), we also experimented with adding 0.5 to *Violent Crime* and to *On-premises licences* before taking natural logs. This exercise produced similar results to those reported.

Table 9

Robustness Checks, Violent Crime

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Placebo *Wet Law*	Restrict to counties with population > 5,000	Restrict to counties with population > 10,000	Restrict sample based on *Sunday Sales* = 0	Control for county quadratic trends	Control for crack epidemic	Dependent variable = ln (*Violent Crime*)	Negative binomial
Average placebo *Wet Law* estimate	−0.022	–	–	–	–	–	–	–
Wet Law	–	0.741**	0.895**	0.810***	0.926**	0.769**	0.102**	0.117*
		(0.300)	(0.387)	(0.275)	(0.449)	(0.327)	(0.048)	(0.065)
N	3,352	2,299	1,366	3,141	3,352	3,352	3,352	3,352
Number of trials	100	–	–	–	–	–		–
Placebo coefficient > 0	51	–	–	–	–	–		–
Placebo coefficient > 0 and significant at 5% level	2	–	–	–	–	–		–
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	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Unless otherwise noted, each column represents the results from a separate OLS regression. In columns (1)–(6) the dependent variable is equal to the violent crime rate in county *c* and year *t*. In column (7) the dependent variable is equal to the natural log of the violent crime rate in county *c* and year *t*. In column (8) 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 10

Wet Laws and Sworn Officer Employment

	Officers employed by Sheriffs' offices	Officers employed by police depts.
Wet Law	−0.033	−0.037
	(0.047)	(0.045)
Pre-treatment mean of the number of sworn officers per 1,000 population	1.02	0.969
N	1,731	1,711
R^2	0.866	0.811
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. 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 to 2011. The dependent variable is equal to the number of sworn officers per 1,000 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.

first three columns of Table 7. Specifically, if we take the natural log of the dependent variable, allowing by-the-drink sales is associated with a 10.7% increase in the violent crime rate ($e^{0.102} - 1 = 0.107$). When the dependent variable is modelled as a count process, allowing by-the-drink sales is associated with a 12.4% increase in the violent crime rate ($e^{1.117} - 1 = 0.124$).[33]

Changes in policing effort in response to the legalisation of by-the-drink sales represent a potential source of omitted variable bias. If police departments and sheriffs' offices hired extra officers after legalisation, then the estimates reported above could be biased downwards due to a deterrence effect. On the other hand, if the extra officers facilitated the reporting of crime, then the estimates could be biased upwards.

To explore this issue, we regressed sworn officers per capita on *Wet Law* and the controls used above.[34] The estimates are reported in Table 10. Allowing by-the-drink sales is associated with a (statistically insignificant) 0.033 decrease in the number of officers employed by sheriffs' offices and a (statistically insignificant) 0.037 decrease in the number of officers employed by police departments. Finally, it should be noted

[33] Allowing by-the-drink sales is associated with a 15.1% 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 10 is reduced to approximately 1,700.

Table 11

Distinguishing between Wet Laws With and Without the 30% Food Sales Requirement

	Violent Crime	*Violent Crime*
Wet Law with Food Sales 30% Gross	0.686**	0.695**
	(0.300)	(0.281)
Wet Law with Food Sales not Required	1.26**	0.875*
	(0.607)	(0.509)
N	3,352	3,352
R^2	0.760	0.847
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to 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.

that, if instead of hiring extra officers, police departments redirected their efforts (for instance, by asking officers to drive past establishments serving alcohol), this could also lead to biased estimates. Again, the direction of the bias is, *a priori*, ambiguous.

4.3. *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% 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 (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% 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% of their gross revenue from food sales (and is equal to zero otherwise).[35]

The estimates in Table 11 provide some evidence that the food sales requirement dampened the effect of by-the-drink sales on violent crime: without controlling for county-specific linear time trends, the estimated coefficient of *Wet Laws with Food Sales 30% Gross* is 0.686, while the estimated coefficient of *Wet Laws with Food Sales Not Required* is 1.26. 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*

[35] In results not reported, we examined the first-stage relationship between *On-Premises Licences* and these two mutually exclusive indicators. Wet laws with the food sales requirement are associated with a 47% increase in on-premises licences relative to the mean rate in dry counties, while wet laws without the food sales requirement are associated with a 60% increase. We cannot, however, reject the hypothesis that these effect sizes are equal.

Sales Not is 0.695, while the estimated coefficient of *Wet Laws with Food Sales Not Required* is 0.875. Again, we cannot reject the hypothesis that these estimates are equal.[36]

4.4. *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 this 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 neighbouring 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 (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 is included on the right-hand side of (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 neighbouring county legalised by-the-drink sales to the general public.

Including mutually exclusive indicators for having one wet county as a neighbour or having two or more wet counties as neighbours 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 travelled across the border and committed violent crimes that would have otherwise been committed nearer home.[39]

In columns (5) and (6) of Table 12, we distinguish between neighbouring 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% of their gross revenue

[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 to 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.

[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. Although not shown, we explored whether the relationship between violent crime and *Number of Wet Counties on the Border* differed by the population of the wet counties on the border. We found no evidence that violent crime was displaced when more populous neighbouring counties legalised by-the-drink sales to the general public.

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)
	Violent Crime	*Violent Crime*	*Violent Crime*	*Violent Crime*	*Violent Crime*	*Violent Crime*
Wet Law	0.711**	0.735**	–	–	0.692**	–
	(0.327)	(0.328)			(0.314)	
Number of Wet Counties on Border	0.003	–	0.048	–	–	–
	(0.143)		(0.118)			
One Wet County on Border	–	0.281	–	−0.075	–	–
		(0.629)		(0.343)		
Two or more Wet Counties on Border	–	−0.001	–	0.055	–	–
		(0.712)		(0.302)		
Number of Wet Counties on Border with Food Sales Requirement	–	–	–	–	−0.004	0.047
					(0.147)	(0.108)
Number of Wet Counties on Border without Food Sales Requirement	–	–	–	–	0.307	0.055
					(0.326)	(0.277)
N	3,352	3,352	607	607	3,352	607
R^2	0.847	0.847	0.591	0.591	0.848	0.591
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

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to 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.

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 (3) provides little evidence that crime went down when residents had the option of crossing county lines to buy liquor by the drink.

5. Property 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. For instance, Grönqvist and Niknami (2014) found that allowing Swedish liquor stores to stay open on Saturdays led to more property crimes, a result that is consistent with the argument that individuals under the influence of alcohol are at greater risk of victimisation (Carpenter and Dobkin, 2011, p. 295).[40] Lifting prohibitions on alcohol sales could also encourage individuals to 'place themselves in situations where they are at greater risk of becoming victims' (Grönqvist and Niknami, 2014, p. 80).

In Table 13 we explore the relationship between legalising 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 relationship between drinking establishments and property crime; the third and fourth columns present reduced-form estimates.

If we do not control for county-specific linear trends, the estimated relationship between drinking establishments and property crime, although positive, is not statistically significant (p-value = 0.143). If taken at face value, it suggests that a 10% increase in the number of drinking establishments would lead to a 1.2% increase in property crime. According to the fully specified 2SLS model, issuing an on-premises liquor licence is associated with 26.6 additional property crimes; a 10% increase in the number of drinking establishments is associated with a 2.0% increase in property crime. Counties that allowed by-the-drink sales experienced, on average, 3.81 additional property crimes per year, or a 9.8% increase relative to the pre-treatment mean.[42]

In Table 14, we explore the relationship between by-the-drink sales and property crimes by type. Controlling for county-specific linear trends, legalisation is associated with 3.40 additional larcenies and 0.859 additional motor vehicle thefts; the reduced-form estimate for burglary, although positive, is not statistically significant at conventional levels.[43]

[40] See also Carpenter (2005, 2007), who 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 (2015) found a positive relationship between alcohol sales and larcenies in New York City.

[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] When the dependent variable is modelled as a count process, allowing by-the-drink sales is associated with a 9.5% increase in the property crime rate.

[43] Table A3 shows 2SLS estimates of the relationship between on-premises licences and property crime by type. Controlling for county-specific linear time trends, issuing a new on-premises liquor licence is associated with 21.9 additional larcenies and 5.52 additional motor vehicle thefts.

Table 13

On-premises Consumption of Alcohol and Property Crime, 1977–2011

	2SLS	2SLS	Reduced-form	Reduced-form
	Property Crime	*Property Crime*	*Property Crime*	*Property Crime*
On-Premises Licences	15.2	26.6**	–	–
	(10.4)	(11.5)		
Wet Law	–	–	2.68	3.81**
			(1.93)	(1.83)
Pre-treatment mean of *Property Crime*	38.8	38.8	38.8	38.8
N	3,352	3,352	3,352	3,352
R^2	0.835	0.869	0.839	0.872
F-test of instrument	51.9	41.9	–	–
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
County linear trends	No	Yes	No	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate regression. The dependent variable is equal to 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

	Property crime	Burglary	Larceny	Motor vehicle theft
Wet Law	5.13**	0.874	3.40***	0.859**
	(2.00)	(0.759)	(1.14)	(0.395)
Pre-treatment mean of dependent variable	39.6	11.4	26.1	2.11
N	2,932	2,932	2,932	2,932
R^2	0.869	0.883	0.861	0.752
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to the relevant crime rate in county *c* and year *t*. The years 1993–9 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 Table A2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

6. 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 legalising 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 licences, we find that the elasticity of violent crime with respect to drinking establishments is between 0.35 and 0.48. Instrumental variables estimates of the elasticity of property crime with respect to drinking establishments are between and 0.12 and 0.20. To our knowledge, no previous study has produced estimates of the elasticity of crime with respect to drinking establishments.

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 licences, 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 10–23% increase in violent crime and a nearly 10% increase in property crime.[44] Because we find no evidence that crime fell in dry counties when their neighbours 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.

Although the welfare gains attributable to drinking establishments are difficult to gauge, their cost to society can be calculated by combining the 2SLS estimates reported in Tables 5–13 with cost-of-crime estimates published by McCollister *et al.* (2010).[45] We find that the additional crimes from opening a new drinking establishment cost society a total of $595,799 per year. Most of this cost ($421,918) is due to violent crimes such as assaults and robberies, but the cost of property crime is not trivial. For instance, additional larcenies from opening a new drinking establishment cost $78,528, and additional motor vehicle thefts cost $65,764.

Finally, 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 evidence that alcohol consumption in public serves as a catalyst for violent behaviour (Graham and Wells, 2001, 2003; Buddie and Parks,

[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% after tribal casinos were allowed to operate. Grinols and Mustard (2006) found that property and violent crime rates were roughly 9% and 13% higher, respectively, in counties with casinos.

[45] These estimates include direct victim costs, indirect victim costs (e.g. psychological distress), criminal justice system costs and crime career costs. See McCollister *et al.* (2010) for the numerous sources used to generate their cost estimates.

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.[46]

While we cannot distinguish between these mechanisms, our results provide evidence that restricting the availability of on-premises licences can play an important role in crime prevention. Dry counties are becoming increasingly rare in the US (Wheeler, 2012) but other types of restrictions as to where drinking establishments can locate are still commonplace. For instance, in Massachusetts municipalities are only allowed to issue one on-premises liquor licence per 2,000 residents; in New Jersey the cap is one on-premises liquor licence per 3,000 residents; and in Utah the cap is one on-premises licence per 4,925 residents (Saksa, 2014). These types of regulations have been ignored by researchers interested in local alcohol availability and crime but their effects could be profound and deserve more attention.

[46] Within the context of bystanders in the immediate vicinity of medical marijuana dispensaries and restaurants, Chang and Jacobson (2014) found that 'eyes upon the street' may actually serve to deter crime.

Appendix A. Additional Figures and Results

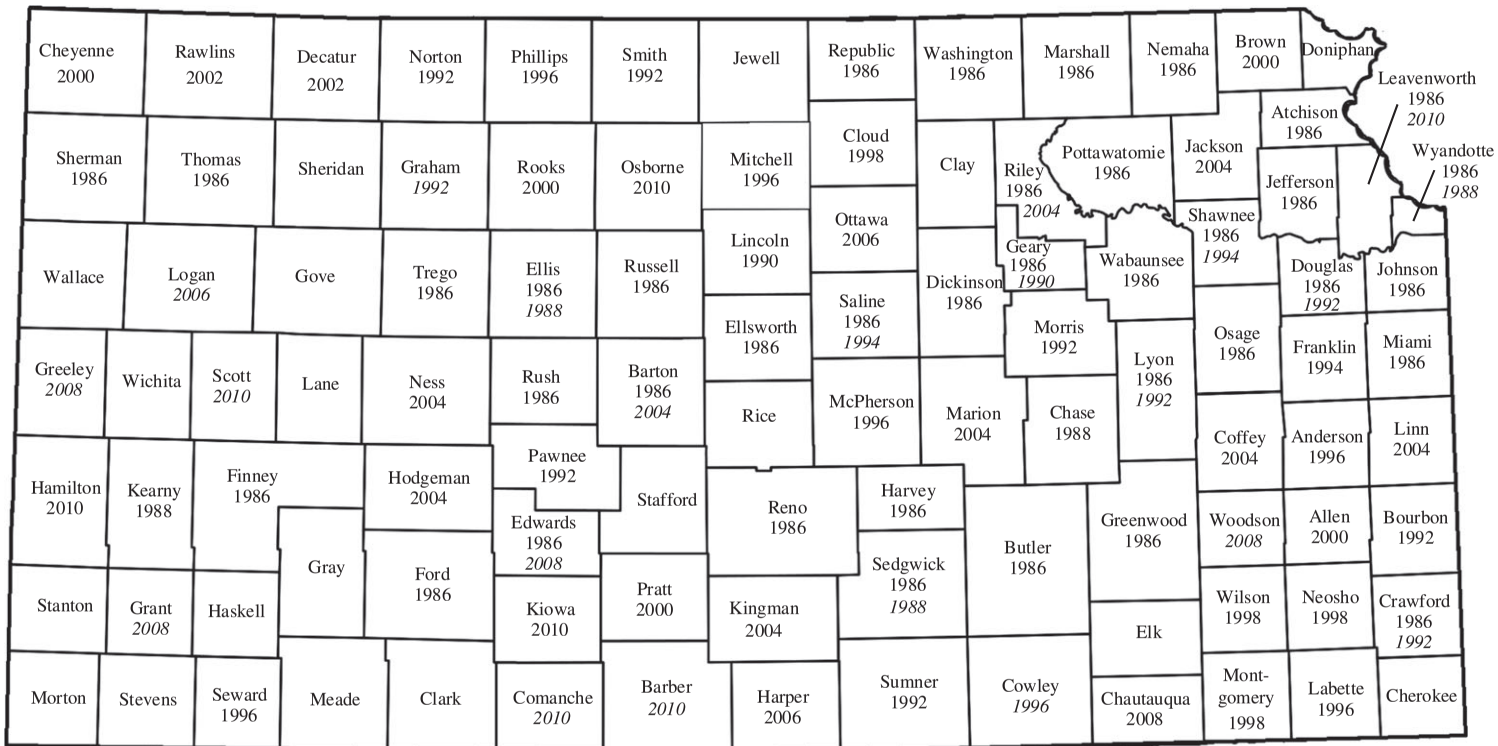

Fig. A1. *Kansas Wet Laws, 1977–2011*

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

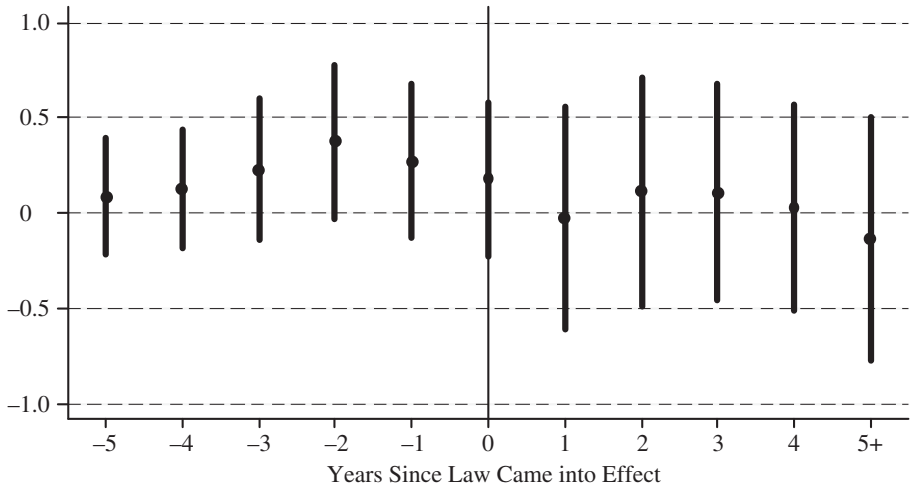

Fig. A2. *Trends in Employment Levels at Off-premises Liquor Establishments*
Notes. OLS coefficient estimates (and their 95% confidence intervals) are reported. The dependent variable is equal to the number of employees at off-premises liquor establishments per 1,000 population in county c and year *t*. The controls include year fixed effects and the data cover the period 1977–2011.

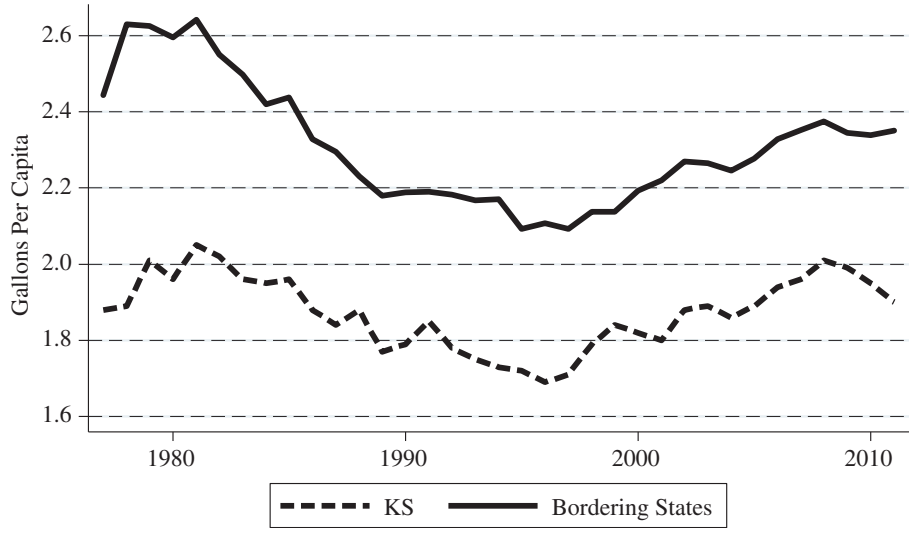

Fig. A3. *Alcohol Consumption in Kansas and Bordering States, 1977–2011*
Note. Data on alcohol consumption are from the National Institute on Alcohol Abuse and Alcoholism.

Table A1

Wet Laws and Off-Premises Alcohol Licences, 1977–2011

	Off-Premises Licences	*Off-Premises Licences*
Wet Law	−0.001	0.020
	(0.017)	(0.016)
Pre-treatment mean of *Off-Premises Licences*	0.185	0.185
N	3,352	3,352
R^2	0.744	0.823
Year FEs	Yes	Yes
County FEs	Yes	Yes
Covariates	Yes	Yes
County linear trends	No	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate OLS regression. The dependent variable is equal to the number of active off-premises alcohol licences per 1,000 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 A2

Descriptive Statistics for Violent and Property Crime by Type

	Full sample	*Wet Law* = 1	*Wet Law* = 0
	Mean	Mean	Mean
	(SD)	(SD)	(SD)
Violent crime	3.73	4.16	3.12
	(2.89)	(2.80)	(2.92)
Murder	0.046	0.044	0.049
	(0.060)	(0.059)	(0.062)
Rape	0.349	0.413	0.257
	(0.252)	(0.231)	(0.252)
Robbery	0.811	0.847	0.760
	(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 1993–9. Means are weighted by county population and standard deviations are shown in parentheses. All variables are reported as rates per 1,000 population.

Table A3

On-premises Licences and Property Crime by Crime Type, 1977–2011

	Property crime	Burglary	Larceny	Motor vehicle theft
On-premises licences	33.0***	5.62	21.9***	5.52**
	(12.4)	(4.45)	(7.71)	(2.37)
Pre-treatment mean of dependent variable	39.6	11.4	26.1	2.11
N	2,932	2,932	2,932	2,932
R^2	0.862	0.884	0.852	0.736
F-test of instrument	42.6	42.6	42.6	42.6
Year FEs	Yes	Yes	Yes	Yes
County FEs	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes
County linear trends	Yes	Yes	Yes	Yes

Notes. * Statistically significant at 10% level; ** at 5% level; *** at 1% level. Each column represents the results from a separate 2SLS regression. The dependent variable is equal to the relevant crime rate in county *c* and year *t*. The years 1993–9 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 Table A2. Regressions are weighted by county population and standard errors are corrected for clustering at the county level.

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Additional Supporting Information may be found in the online version of this article:

Data S1.

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