



Murder and the black market: Prohibition's impact on homicide rates in American cities



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ARTICLE INFO

Article history:

Received 24 February 2015

Received in revised form

28 September 2015

Accepted 30 September 2015

Available online 9 November 2015

Keywords:

Prohibition

Homicide

Illegal Markets

ABSTRACT

The purpose of this paper is to estimate the effect of state alcohol prohibition on homicide rates using city-level data from 1911 to 1929. During this time period, urban dwellers generally opposed prohibition and did not voluntarily adopt it. Subsequently, policy changes were more exogenous. The results suggest that there are dynamic aspects of prohibition. State-level prohibition decreased homicides immediately after enforcement began, but after three years of enforcement, the law ceased having a measurable effect.

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1. Introduction

A majority of homicides committed in the United States are personal in nature. According to Federal Bureau of Investigation (FBI) data from 2012, 39% of the victims knew their murderer as a family member, significant other, close friend, neighbor, or coworker¹ with the greater part committed due to disagreements over money or romance. Murder, however, can also be considered an impersonal business practice. Established firms in the illegal drug trade can use violence as an additional barrier to new firms looking to move into the marketplace (Reuter, 1985). Recent crime statistics show that this “competitive violence” still plays a significant role in American homicide rates.

In 2012, the FBI recorded 362 homicides directly attributed to the illegal drug trade. In addition, 871 homicides were classified as gang related². Although gangs can be viewed as firms in the illegal drug trade, there is uncertainty of how many homicides were motivated by competitive violence. Currently, one significant policy

proposal, to reduce the number of homicides around the country, is to legalize prohibited drugs. In theory, legalization would diminish violence by allowing businesses to settle disputes using judges and juries rather than guns (Miron, 1999). Citizens in Colorado and Washington have already voted to legalize marijuana in order to reduce crime while raising revenue for the state. The success of these legalization initiatives reignited the national debate on the effectiveness of “the War on Drugs”. Interest groups on both sides of the drug legalization discussion have cited alcohol prohibition in the early 20th century as an important historical lesson.

I advance the alcohol prohibition literature by providing a new level of analysis: a panel data set of city-level homicide rates for 60 of the largest 68 populated cities across the United States from 1911 to 1929³. Previous papers by Miron (1999), Jensen (2000), and Owens (2011), have used state and national level data to test the impact of prohibition on homicides. City-level assessment possesses two important features absent from state- and national-level estimates. First, large cities in the sample were more resistant to prohibition than rural townships. Problems associated with selection bias and reverse causality are diminished when urban residents were struggling against the policy change. Second,

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¹ Out of 7008 explained murders, 2737 were classified as by family, friend, boyfriend, girlfriend, neighbor, employee, or employer. There were an additional 5757 unexplained murders. http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2012/crime-in-the-u.s.-2012/offenses-known-to-law-enforcement/expanded-homicide/expanded-homicide-data-table_10_murder_circumstances_by_relationship_2012.xls.

² These numbers most likely underestimate the number of murders connected to the drug trade, as a total of 4582 murders have not been classified by the FBI, with regard to motive.

³ The eight cities missing from the sample are: Oakland, California (ranked 31st in population); Birmingham, Alabama (ranked 36th); Memphis, Tennessee (ranked 41st); Dallas, Texas (ranked 42nd); Houston, Texas (ranked 46th); Des Moines, Iowa (ranked 52nd); Nashville, Tennessee (ranked 68th); and Fort Worth, Texas (ranked 64th). Homicide data for these cities is not available in the Mortality Statistics volumes.

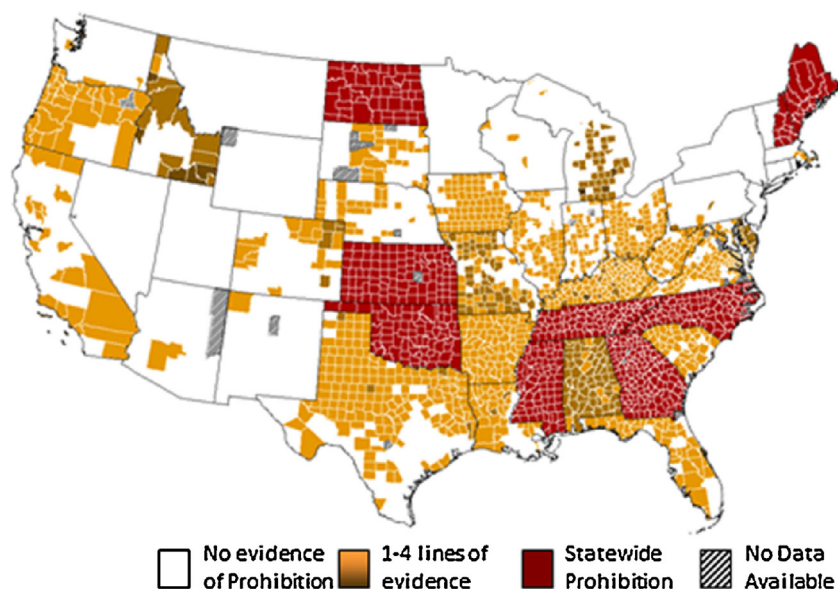


Fig. 1. Prohibition in 1911 by county. Source: Robert Sechrist's ICPSR (8343). Created by Zachary Christman (Rowan University).

state-level prohibition analysis encompasses a large geographical area without consistent policy enforcement. Counties prohibited the sale of alcohol well before state-level prohibition and states prohibited alcohol before national-level prohibition. Estimations of prohibition's impact at the state level measure the impact of alcohol prohibition on a checkered market where the alcohol is both legal and illegal. City level analysis allows for a clear market classification, as alcohol was either legal or prohibited throughout the entire city.

Another contribution of the paper is to measure the dynamic impact of prohibition laws. Estimations of the impact of the law using a single dummy variable might be biased if citizens took time to adjust to the new market conditions. There is reason to believe that there was an adjustment period after enactment of prohibition. During the time between legislative approval and actual enactment, usually a year to give time to law enforcement to prepare, citizens could have started to hoard alcohol. Measuring crime statistics in a marketplace where consumers do not demand illegal alcohol because of reserve supplies would under estimate the true cost of prohibition. I estimate the change in homicides through time using a 'years after implementation' variable.

To estimate the effect of alcohol prohibition on homicides, I performed a difference in difference fixed-effect analysis between the years of 1911–1919 and a second analysis between the years of 1911–1929. The 1911–1919 estimation evaluates the impact of the 1910s state-level prohibition laws on homicides, while the 1911–1929 estimation evaluates the impact of both state-level and national-level prohibition on homicides. The results suggest that state level prohibition reduced homicides in the large cities in the sample for the first few years after implementation.

For the 1911–1919 estimation, prohibition was correlated with a decline of murders in the first two years after implementation, but had no measurable effect on homicides afterwards. The 1911–1929 estimate, which included states that were forced into prohibition by the Constitutional Amendment, was also correlated with a decline in homicides immediately after prohibition, but no measurable effect after two years. The results were statistically significant using robust standard errors clustered at the city level. However, homicides not attributed to alcohol prohibition rose steadily after 1920. I was not able to identify the effect of national prohibition on all states, even those already under alcohol prohibition, because of year effects absorbing all nation-wide events.

State-level prohibition estimates, however, suggests that other factors such as urbanization, gun technology, veterans returning from WWI, and changing criminal laws were key reasons for the roaring twenties crime wave.

2. The United States and alcohol prohibition

National Prohibition was the last political movement during which a majority of the United States population lived in rural areas. In 1910, before the substantial expansion of state-level prohibition laws, citizens living in towns with a population of less than 2500, made up 55% of the total population. By 1920, the year National Prohibition started, that amount shrunk to 49%. While the rural population was in decline, counties and states were busy enacting prohibition laws by attracting white, rural, evangelical Protestants (Lewis, 2008).

Figs. 1 and 2 show the expansion of state and county alcohol prohibition laws in 1911 and 1918. Counties were under prohibition before state enforcement and a majority of states were prohibiting alcohol before national prohibition. A patchwork system of state laws created spillover problems for proponents of prohibition. Temperance leaders believed that 'wet' states were undermining 'dry' states ability to restrict alcohol consumption (Merz, 1930). National prohibition was viewed as the ultimate solution.

The 18th Amendment to the United States Constitution was ratified in 1919 and prohibited the "manufacture, sale, or transportation of intoxicating liquors," leaving the *consumption* of alcohol legal. A year's delay in implementation was written into the amendment in order to allow states time to prepare for enforcement. Citizens in large cities were resistant to prohibition laws as noted by John F. Kramer, the first Federal Prohibition Commissioner, when he bemoaned that large cities were non-compliant when it came to enforcement.

[W]hen the nation as a whole adopted the principles of prohibition, it was to some extent forced upon whole states and especially upon large cities in which people had no sympathy whatsoever with the idea. In fact, they scarcely knew what the term prohibition meant. (Kramer, 1921, p. 1)

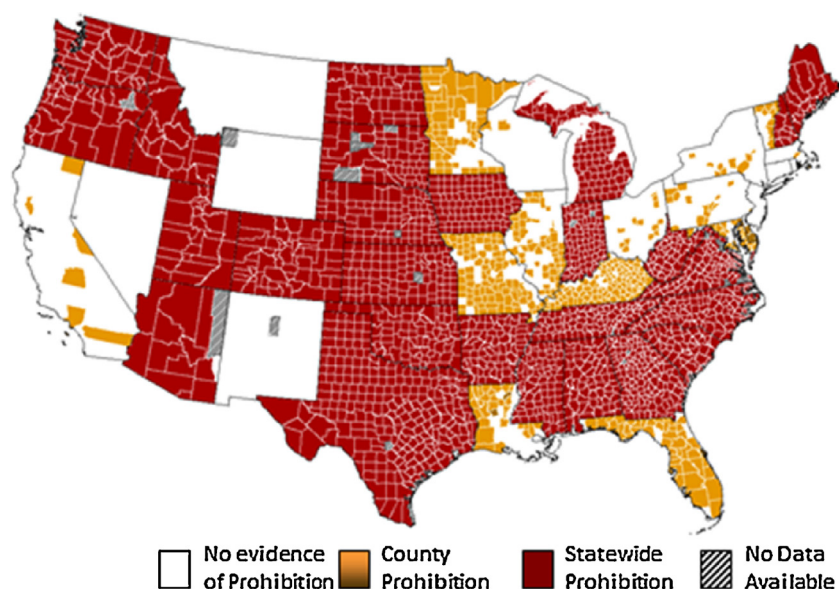


Fig. 2. Prohibition in 1918 by county. Source: Robert Sechrist's ICPSR (8343). Created by Zachary Christman (Rowan University).

When given the choice, only 2 of the 60 cities in the sample voted to voluntarily enforce prohibition⁴. The other 58 cities in the sample were forced into prohibition, either by state or national legislation.

The timing of state prohibition used for this paper differs substantially from Dills and Miron (2004) and Owens (2011). Instead of assigning the start of prohibition in the state when the state legislature first passed the law, I used the year when statewide prohibition was enforced. Table 1 details the differences between passage and enforcement in all 48 states. For 22 of the 48 states, the date of passage and enforcement do not occur in the same year. In 9 of the 22 states, the gap between passage and enforcement is greater than a year. The timing of state prohibition is important as the results from a difference in difference analysis change based on which dates are selected. If the analysis uses dates on enforcement, state prohibition is negatively correlated with homicides, while if the analysis uses dates of passage, state prohibition is not correlated with homicides. In addition to timing issues, large cities that experienced state prohibition were situated in states with different demographics than large cities that only experienced national Prohibition.

Tables 2 and 3 in the appendix compare the large cities in the sample based on whether the city was forced into prohibition by a state or the national legislature. Both Tables 2 and 3 list the percentage of the population classified as rural in the city's state and the percentage of the population that were members of the Catholic Church. The tables show that large cities in more urban, Catholic-dominated states were able to stave off state-level prohibition until much later in the 1910s. In order to test if the cities had statistically significant differences, I calculated a *t*-test of means of demographic variables listed in Table 4.

⁴ To ensure unbiased data, voting record data were collected from a variety of sources, including the Anti-Saloon League's Year Books, General Statistics of Cities, Financial Statistics of Cities, and the Year Book of Brewers. The two cities, that voluntarily enacted prohibition, were Cambridge and Fall River, both located in Massachusetts. Cambridge voted to prohibit alcohol sales in 1886 and continued to support prohibition until it reversed course voting for legalization in 1919, a year before national prohibition was enacted. Fall River voted to prohibit alcohol in 1917, but voted to legalize alcohol in 1918. The only other city claimed by the books to vote "dry" was Los Angeles, which prohibited hard liquor, while allowing 14% alcohol to be bought and sold. The city is classified as a wet city for the years 1918 and 1919 in the dataset, because prohibiting only hard liquor would not create a black market for all alcoholic goods (Anti-Saloon League Year Book, 1918).

Table 4 suggests that cities that were forced into prohibition before 1920, had a higher homicide rate, spent less on police per capita, had a significantly higher young black male population, and had fewer Catholics as a percent of the city's population than the cities that were forced into prohibition by the federal government. Fig. 2 shows that states in the South, West, and Midwest were more likely to enact prohibition than the Northeast. Northeast cities were closer together and had higher concentrations of foreigners and Catholics and more likely to resist prohibition. If the underlying response to alcohol prohibition is being driven by unobserved characteristics, then cities forced into state prohibition could produce a different estimate than cities forced into national prohibition. I conducted a robustness check to determine if cities that had prohibition forced on them before 1919 had different estimates than the full sample.

National alcohol prohibition ended on December 5, 1933 with the ratification of the 21st Amendment to the Constitution. States, counties, and cities chose between keeping prohibition laws intact, enforcing licensing procedures, and creating government-run monopolies for alcohol distribution. Reverse causality and selection bias problems exist at the city level when exiting prohibition, but not when entering into it. To minimize bias and assess the impact of prohibition causing homicides instead of homicides causing prohibition legislation, I estimate the impact of alcohol prohibition only at implementation.

3. Economic theory

A market for goods and services without legal standing is popularly referred to as a "black market." Black markets did not automatically form after enacting alcohol prohibition. Successful legislation would suppress alcohol demand and supply to sufficient extent that markets would be unable to survive. Proponents of alcohol prohibition argued that by restricting liquor sales, alcohol consumption would be reduced and if inebriated citizens were the main cause of crime, a reduction in alcohol consumption would lower crime. Miron and Zwiebel (1991), Dills and Miron (2004), Dills et al. (2005) have all found that alcohol consumption decreased immediately after prohibition was enacted, but consumption increased steadily after the first year. The tradeoff of lowering individual alcohol consumption, by using prohibition laws, is an increase in crime from businesses.

Table 1
Timing of state alcohol prohibition.

	Passage	Enforcement	Difference
Alabama	1908–1911, 1915	1915	
Arizona	1914	1915	*
Arkansas	1915	1916	*
California	1920	1920	
Colorado	1914	1916	*
Connecticut	1920	1920	
Delaware	1920	1920	
Florida	1918	1919	*
Georgia	1907	1908	*
Idaho	1915/1916	1916	
Illinois	1920	1920	
Indiana	1917	1918	*
Iowa	1915	1916	
Kansas	1880	1880	
Kentucky	1918	1920	*
Louisiana	1920	1920	
Maine	1884	1884	
Maryland	1920	1920	
Massachusetts	1920	1920	
Michigan	1916	1918	*
Minnesota	1920	1920	
Mississippi	1908	1909	*
Missouri	1920	1920	
Montana	1916	1919	*
Nebraska	1916	1917	*
Nevada	1918	1919	*
New Hampshire	1855–1903, 1917	1917	
New Jersey	1920	1920	
New Mexico	1917	1918	*
New York	1920	1920	
North Carolina	1908	1909	*
North Dakota	1889	1889	
Ohio	1918	1919	*
Oklahoma	1907	1907	
Oregon	1914	1916	*
Pennsylvania	1920	1920	
Rhode Island	1920	1920	
South Carolina	1915	1916	*
South Dakota	1916	1917	*
Tennessee	1909	1909	
Texas	1918/1919	1918	
Utah	1917/1918	1917	
Vermont	1920	1920	
Virginia	1914	1916	*
Washington	1914	1916	*
West Virginia	1912	1914	*
Wisconsin	1920	1920	
Wyoming	1918	1920	*

Sources for passage: Merz (1969), Dills and Miron (2004), Owens (2011). Sources for enforcement: Anti-Saloon League Year Book (various years).

Prior to prohibition, alcohol suppliers, when faced with a dispute over business matters, choose to resolve the dispute through the least-cost alternative of lawyers or violence (Miron, 1999). During prohibition, using the legal system to argue your case was no longer an option. Only violence or other private sector mediation methods were available to business owners. Economic theory cannot predict whether the reduction in crime from decreased alcohol consumption is of greater importance than the increase in crime from business owners.

Without a clear theoretical prediction, economists have used empirical analysis to test which effect is the most important. Historical homicide rate statistics at the national level showed dramatic increases during the 1920s, suggesting that national alcohol prohibition is positively correlated with homicides. However, unadjusted national homicide rates are misleading. States started reporting homicide rates gradually over time during the 1910s and 1920s. Eckberg (1995) adjusted for the missing data and found that the increase in homicide rates during the 1920s was less dramatic than the raw data suggested. Jensen (2000) used the corrected

Average Homicides Per 100,000 Citizens in Sample Cities Relative to the Start of Alcohol Prohibition

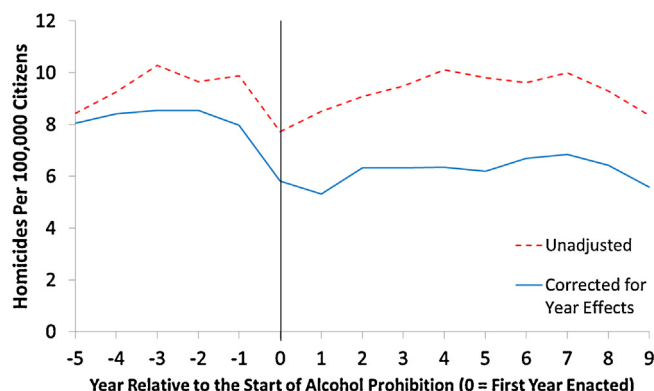


Fig. 3. Average homicides per 100,000 citizens in sample cities relative to the start of alcohol prohibition.

national level homicide rates to estimate the effect of alcohol consumption and state-level prohibition on homicides. His results found that state-level prohibition decreased alcohol consumption but increased homicides. Owens (2011) extended the literature by obtaining a panel data set of state-level homicide rates.

Owens (2011) found that while there is no measurable correlation between homicides and alcohol prohibition, urbanization and changing demographics play a key role in determining homicide rates. Although the city-level panel data set used in this paper can control for changing demographics, the results cannot take into account the movement of citizens from rural to urban centers. The migration from rural areas during the time period was mainly into small and medium cities—not large cities. The ratio of population in large cities relative to corresponding state population was relatively stable throughout the time period. To test how much migration was coming into the cities in the sample, I examined the sample city population divided by the corresponding state population in both 1929 and 1911. The mean absolute difference between the years was 3.54 percentage points with a standard deviation of 2.80 percentage points. The maximum difference was 13 percentage points in Michigan, which had Detroit’s population increase dramatically relative to the state population during the time period. The second highest was the growth of 6 percentage points in Kentucky. The differences in population ratios suggest that cities in the sample were becoming a larger part of the state population, but were not yet growing exponentially. Other nationwide events were changing homicide rates in cities.

Fig. 3 aligns homicide rates based on the beginning of prohibition for the city regardless of the calendar year prohibition was enacted. The figure includes 58 cities, instead of the full 60, to keep the number of cities constant throughout the fifteen years displayed. Homicides consistently dip during the first year of alcohol prohibition, a consistent feature of the all prohibition data. However, after the first year, homicides start increasing. When controlling for year effects, the decrease in homicides from alcohol prohibition has a longer lasting impact. Unfortunately, year effects also obfuscate any delayed influence of National Prohibition on crime rates. It is difficult to determine what caused the explanatory power of year effects during the 1920s as societal changes were numerous and difficult to disentangle.

4. Analytical framework

I follow the difference in difference estimation technique used by Wolfers (2006), who asserted that a single dummy variable is

Table 2
Implementation of state prohibition in sample cities.

City	State	Start of prohibition for the city	% of the state that was urban in 1910	% of state religious members that were Catholic in 1916
Akron	Ohio	1919	55.9	36.8
Atlanta	Georgia	1908	20.6	1.5
Cincinnati	Ohio	1919	55.9	36.8
Cleveland	Ohio	1919	55.9	36.8
Columbus	Ohio	1919	55.9	36.8
Dayton	Ohio	1919	55.9	36.8
Dallas	Texas	1918	24.1	22.6
Denver	Colorado	1916	50.7	40.7
Detroit	Michigan	1918	47.2	48.4
Grand Rapids	Michigan	1918	47.2	48.4
Houston	Texas	1918	24.1	22.6
Indianapolis	Indiana	1918	42.4	23.1
Kansas City	Kansas	1880	29.2	21.1
Memphis	Tennessee	1909	20.2	2.7
Nashville	Tennessee	1909	20.2	2.7
Norfolk	Virginia	1916	23.1	3.9
Omaha	Nebraska	1917	20.1	30.7
Portland	Oregon	1916	45.6	27.7
Richmond	Virginia	1916	23.1	3.9
Salt Lake City	Utah	1917	46.3	3.6
San Antonio	Texas	1918	24.1	22.6
Seattle	Washington	1916	53.0	34.3
Spokane	Washington	1916	53.0	34.3
Toledo	Ohio	1919	55.9	36.8
Washington	DC	1917	100	31.3
Youngstown	Ohio	1919	55.9	36.8

Sources: *Anti-Saloon League Year Book* (various years), *Religious Bodies* (1916), *Thirteenth Census of the United States Taken in the Year 1910: Volume I*. Virginia passed prohibition legislation on September 12th 1914, but it was not implemented until November 1st, 1916 (*Anti-Saloon Year book*, 1915, pp. 263). Oregon passed prohibition legislation on November 3rd, 1914, but it was not implemented until January 1st, 1916 (*Anti-Saloon Year book*, 1915, pp. 248). Washington passed prohibition legislation on November 3, 1914, but it was not implemented until January 1st, 1916 (*Anti-Saloon Year book*, 1916, pp.265). Colorado passed prohibition legislation on November 3rd, 1914, but it was not implemented until January 1st, 1916 (*Anti-Saloon Year book*, 1916, pp. 195). Michigan passed prohibition legislation on November 7th, 1916, but it was not implemented until May 1st, 1918 (*Anti-Saloon League Year Book*, 1917, pp. 150). Indiana passed prohibition legislation on 1917, but it was not implemented until April 1st, 1918 (*Anti-Saloon Year book*, 1917, pp. 116). Nebraska passed prohibition legislation on November 7th, 1916, but it was not implemented until May 1, 1917 (*Anti-Saloon Year book*, 1917, pp. 174). Ohio passed prohibition legislation on November 5, 1918, but it was not implemented until May 27, 1919 (*Anti-Saloon Year book*, 1919, pp. 138).

unable to identify the impact of a policy shock in a difference in difference estimation technique. The problem occurs if citizens do not have an immediate and constant response to the policy change. For prohibition, there is evidence that consumers and producers alike were unable to instantly snap into a long run behavior. Consumers' hoarded alcohol and black markets needed time to develop. *Wolfers (2006)* suggested that if policy shocks have different short run and long run effects, a measure of how long the policy has been in effect should replace the standard dummy policy variable in the estimation. The estimation uses this measurement as well as state-specific time trends to accurately take into account differences between control and treatment states.

The estimation is as follow:

$$\begin{aligned}
 \text{Murder}_{c,t} = & \sum_{k \geq 1} \beta_k \text{prohibition has been in effect for } k \text{ periods}_{c,t} \\
 & + \sum_{c \geq 1} \text{City Fixed Effects}_c \\
 & + \sum_{t \geq 1} \text{Times Fixed Effects}_t + \theta \text{ Demographic Controls}_{c,t} \\
 & + \sum_s \text{State}_s * \text{Time}_t + \sum_s \text{State}_s * \text{Time}_t^2 + \varepsilon_{c,t}
 \end{aligned} \quad (1)$$

The dependent variable for the analysis is Murder, the number of homicides per 100,000 citizens as recorded by the Census Bureau for city c in year t . Prohibition is a dummy variable that records a value of one if a city was under prohibition k periods after implementation of the law. The coefficient of interest is the sum of the β coefficients, the change in the number of homicides per 100,000 citizens attributed to alcohol prohibition through time. To account for unobserved time-invariant variables that could influence homicide rates, I used a fixed-effects estimation technique.

The regression controls for city-level fixed effects, year effects, and observable city-level control variables, X . City level control variables included real per-capita police expenditures in 2012 US dollars; real per-capita total charity, hospital, and prison expenditures in 2012 US dollars; the percentage of white males ages 15–24; the percentage of foreign-born males ages 15–24; the percentage of black males ages 15–24; the percentage of Catholic citizens; the percentage of Jewish citizens; the percentage of illiterate citizens over the age of 10; the number of state executions occurring during the previous year; the population over the age of 10 living within a 30 mi radius of the city divided by city population; and the population in a dry county within a 30 mi radius of the city divided by city population⁵. Estimates for the percent of white males, black males, foreign-born males, and illiterate citizens were linearly interpolated between the census years. Catholic and Jewish citizens living in the city were also linearly interpolated between the Religious Bodies publications in 1906, 1916, 1926, and 1936.

Estimates of populations living within a 30 mi radius of the city were constructed because neighboring counties could influence variables within the sample city, resulting in imprecise estimates. *Fig. 4* shows the range of spillover effects in the sample, by city. In addition, a state-specific linear trend and a state-specific quadratic time trend was included to control for difference cross state trends in homicides. The disturbance term for the estimation, ε , is for each city/year observation. Summary statistics for all variables included in the regression are given in *Table 3*.

⁵ The years 1914, 1920, and 1922, were missing for both Police expenditures and Charity, Hospital, and Prison expenditures. Since these city expenditure amounts followed smooth growth patterns throughout the series, I used a linear interpolation for missing years.

Table 3
Implementation of national prohibition in sample cities.

City	State	Start of prohibition for the city	% of the state that was urban in 1910	% of state religious members that were Catholic in 1916
Albany	New York	1920	78.8	63.6
Baltimore	Maryland	1920	50.8	36.4
Boston	Massachusetts	1920	92.8	71.3
Bridgeport	Connecticut	1920	89.7	66.8
Buffalo	New York	1920	78.8	63.6
Cambridge*	Massachusetts	1886	92.8	71.3
Camden	New Jersey	1920	75.2	59.1
Chicago	Illinois	1920	61.7	46.4
Fall River	Massachusetts	1920	92.8	71.3
Hartford	Connecticut	1920	89.7	66.8
Jersey City	New Jersey	1920	75.2	59.1
Kansas City	Missouri	1920	42.5	32.5
Los Angeles	California	1920	61.8	56.4
Louisville	Kentucky	1920	24.3	16.6
Lowell	Massachusetts	1920	92.8	71.3
Milwaukee	Wisconsin	1920	43.0	51.2
Minneapolis	Minnesota	1920	41.0	44.6
New Bedford	Massachusetts	1920	92.8	71.3
New Haven	Connecticut	1920	89.7	66.8
New Orleans	Louisiana	1920	30.0	59.1
New York	New York	1920	78.8	63.6
Newark	New Jersey	1920	75.2	59.1
Paterson	New Jersey	1920	75.2	59.1
Philadelphia	Pennsylvania	1920	60.4	44.5
Pittsburgh	Pennsylvania	1920	60.4	44.5
Providence	Rhode Island	1920	96.7	75.9
Reading	Pennsylvania	1920	60.4	44.5
Rochester	New York	1920	78.8	63.6
San Francisco	California	1920	61.8	56.4
Scranton	Pennsylvania	1920	60.4	44.5
Springfield	Massachusetts	1920	92.8	71.3
St. Louis	Missouri	1920	42.5	32.5
St. Paul	Minnesota	1920	41.0	44.6
Syracuse	New York	1920	78.8	63.6
Trenton	New Jersey	1920	75.2	59.1
Wilmington	Delaware	1920	48.0	34.9
Worcester	Massachusetts	1920	92.8	71.3
Yonkers	New York	1920	78.8	63.6

Sources: *Anti-Saloon League Year Books* (various years), *Religious Bodies*, (1916), *Thirteenth Census of the United States Taken in the Year 1910: Volume I*.

* Cambridge votes to prohibit alcohol voluntarily.

Theory cannot predict the sign of the variable of interest β_k . A statistically significant estimated coefficient of $\beta_k > 0$ indicates that homicides rates on average increased for the k th year after the implementation of Prohibition. An insignificant estimated coefficient suggests that Prohibition does not affect homicide rates k periods after implementation. A statistically significant estimated coefficient of $\beta_k < 0$ suggests Prohibition reduced homicides in the k th year after implementation. The estimate was

run twice using a shorter time period, between 1911 and 1919, and a longer time period, between 1911 and 1929, to different between possible policy differences between state and national laws.

First, I restricted the years in the sample from 1911 to 1919, to account for the nine observable years without national Prohibition. Restricting the sample before national Prohibition allows for an estimation of the impact that state temperance laws had

Table 4
Means and standard deviations by timing of prohibition.

	Cities that went dry before 1920	Cities that went dry during 1920	<i>p</i> Value for difference in means
Murders per 100,000 citizens	12.35 (7.68)	7.25 (6.24)	0.01
City population	263,797 (218,983)	545,635 (1011,983)	0.10
Real police spending per capita in 2012 USD	34.67 (11.04)	47.79 (13.72)	0.00
Real charity, prison, and hospital spending per capita in 2012 USD	17.25 (17.57)	28.75 (16.44)	0.02
Percent of city population that is a white male between the ages of 15–24	0.071 (0.011)	0.069 (0.009)	0.40
Percent of city population that is a black male between the ages of 15–24	0.009 (0.010)	0.004 (0.004)	0.03
Percent of city population that is a foreign male between the ages of 15–24	0.009 (0.006)	0.013 (0.006)	0.01
Percent of city population that is Catholic	0.17 (0.10)	0.34 (0.12)	0.00
Percent of city population that is Jewish	0.02 (0.01)	0.04 (0.03)	0.00
Percent of city population that is illiterate	0.03 (0.02)	0.04 (0.02)	0.03
No. of state executions in the previous years	3.09 (2.73)	5.02 (4.92)	0.06
Population living within a 30 mi radius divided by city population	2.63 (4.94)	8.66 (2.13)	0.01
Population in a dry county living within a 30 mi Radius divided by city population	1.72 (2.89)	4.89 (6.97)	0.02

Notes: 1140 observations for 60 cities over 19 years. There were 23 cities that were dry before 1920 and 37 cities that became dry in 1920. Between standard deviation in parentheses. Sources: *Mortality Statistics* (various years), *Financial Statistics of Cities* (various years), *Thirteenth Census of the United States Taken in the Year 1910: Volume I*, *A National Survey of Conditions Under Prohibition* (1926).

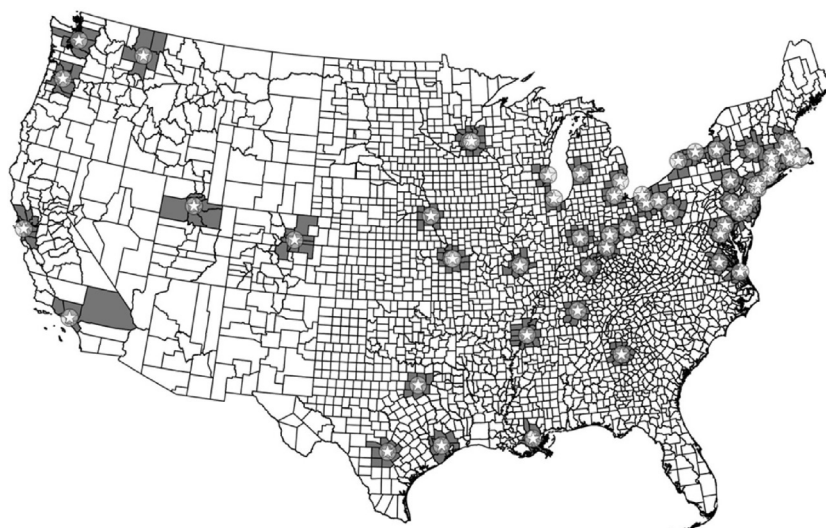


Fig. 4. Counties within 30 mi of a city in the sample.

on homicides. State prohibition laws were most likely less effective than the national Prohibition laws due to spillover effects from 'wet' states. After running the pre-national prohibition sample, I reran the estimation again with the years 1911 through 1929, or 19 observable periods. The expanded sample takes into account the impact of all prohibition legislation on homicides.

5. Results

The results indicate that state level prohibition reduced homicides immediately after enactment. Table 5 shows the results from the shorter time period estimation. Column 1 gives the estimation using only city and year effects. The results suggest that in the first year of state prohibition, states that enacted

Table 5
Dynamic effects of adopting state prohibition, 1911–1919 (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

Specification	(1) Basic	(2) City controls	(3) State-specific linear trends	(4) State-specific quadratic trends
First year	−2.49 (0.86)	−2.84 (0.76)	−8.28 (4.43)	−8.28 (4.43)
Year 2	−4.68 (1.46)	−4.96 (1.39)	−11.48 (2.06)	−11.48 (2.06)
Year 3	−1.16 (2.79)	−1.55 (2.28)	−5.06 (1.48)	−5.06 (1.48)
≥ Year 4	0.31 (4.68)	0.27 (4.21)	−2.47 (5.40)	−2.47 (5.40)
Per capita police spending		0.02 (0.05)	0.03 (0.06)	0.03 (0.06)
Per capita charity, prison, and hospital spending		−0.006 (0.04)	−0.02 (0.04)	−0.02 (0.04)
% of white males ages 15–24		126 (94)	191 (128)	191 (128)
% of black males ages 15–24		669 (514)	613 (744)	613 (744)
% of foreign males age 15–24		172 (204)	110 (138)	110 (138)
% Catholic		29 (16)	17 (21)	17 (21)
% Jewish		−36 (29)	−16 (30)	−16 (30)
% Illiterate		−208 (207)	17 (128)	17 (128)
No. of state executions in the previous year		−0.025 (0.075)	−0.67 (0.53)	−0.67 (0.53)
Population living within 30 mi divided by city population		−0.26 (0.53)	0.03 (0.49)	0.03 (0.49)
Dry population living within 30 mi divided by city population		−0.25 (0.12)	−0.26 (0.16)	−0.26 (0.16)
Controls				
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State × time	No	No	Yes	Yes
State × time ²	No	No	No	Yes
R ²	0.05	0.18	0.01	0.03

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

Table 6

Dynamic effects of adopting state and national prohibition, 1911–1929 (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

Specification	(1) Basic	(2) City controls	(3) State-specific linear trends	(4) State-specific quadratic trends
First year	−0.84 (0.69)	−1.21 (0.62)	−7.23 (6.62)	−7.23 (6.62)
Year 2	−1.16 (0.74)	−1.56 (0.79)	−7.95 (6.63)	−7.95 (6.63)
Year 3	0.48 (1.40)	0.04 (1.35)	−9.44 (6.53)	−9.44 (6.53)
≥Year 4	−0.01 (1.72)	−0.44 (1.64)	−3.91 (6.52)	−3.91 (6.52)
Per capita police spending		−0.03 (0.03)	0.004 (0.04)	0.004 (0.04)
Per capita charity, prison, and hospital spending		0.004 (0.02)	0.001 (0.03)	0.001 (0.03)
% of white males ages 15–24		37 (20)	66 (35)	66 (35)
% of black males ages 15–24		565 (289)	631 (599)	631 (599)
% of foreign males age 15–24		142 (82)	86 (98)	86 (98)
% Catholic		31 (8)	22 (10)	22 (10)
% Jewish		11 (8)	9 (12)	9 (12)
% Illiterate		−142 (85)	22 (81)	22 (81)
No. of state executions in the previous year		0.003 (0.045)	−0.06 (0.09)	−0.06 (0.09)
Population living within 30 mi divided by city population		−0.17 (0.21)	−0.16 (0.22)	−0.16 (0.22)
Dry population living within 30 mi divided by city population		0.002 (0.02)	0.009 (0.02)	0.009 (0.02)
Controls				
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State × time	No	No	Yes	Yes
State × time ²	No	^a No	No	Yes
R ²	0.01	0.11	0.12	0.08

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

^a State level data on the number of people living under prohibition might not be effective at communicating black markets if land area and connections to other black markets are important.

prohibition reduced homicides by 3.62 homicides per 100,000 citizens. The sample mean over all cities in all years was, 9.2 homicides per 100,000 citizens suggesting that state level prohibition had an immediate and observable impact. In year two, state prohibition and homicides were also negatively correlated. Both estimates were statistically significant using standard errors

clustered at the city-level. After the second year, the estimation suggests that prohibition was not correlated with a measurable difference in homicides in cities with prohibition compared to cities without prohibition. Column 2 adds demographic information into the estimation increasing the R -squared substantially. State prohibition has a statistically significant impact, decreasing

Table 7

Dynamic effects of adopting state prohibition, 1911–1919 only cities under prohibition before 1919 in Sample (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

Specification	(1) Basic	(2) City controls	(3) State-specific linear trends	(4) State-specific quadratic trends
First year	−4.82 (0.89)	−4.43 (1.15)	−7.84 (3.81)	−7.84 (3.81)
Year 2	−7.68 (1.74)	−6.47 (2.14)	−7.75 (2.97)	−7.75 (2.97)
Year 3	−4.53 (3.02)	−2.61 (2.06)	−5.51 (2.43)	−5.51 (2.43)
≥Year 4	−3.21 (5.30)	−0.67 (3.85)	2.37 (2.13)	2.37 (2.13)
Per capita police spending		−0.07 (0.11)	0.39 (0.30)	0.39 (0.30)
Per capita charity, prison, and hospital spending		0.06 (0.09)	0.07 (0.17)	0.07 (0.17)
% of white males ages 15–24		1154 (428)	465 (718)	465 (718)
% of black males ages 15–24		1646 (562)	4915 (790)	4915 (790)
% of foreign males age 15–24		−127 (286)	981 (610)	981 (610)

Table 7 (Continued)

Specification	(1) Basic	(2) City controls	(3) State-specific linear trends	(4) State-specific quadratic trends
% Catholic		16 (35)	35 (52)	35 (52)
% Jewish		92 (163)	137 (273)	137 (273)
% Illiterate		–207 (313)	–272 (385)	–272 (385)
No. of state executions in the previous year		0.03 (0.15)	– 0.93 (0.42)	– 0.93 (0.42)
Population living within 30 mi divided by city population		5.25 (2.27)	1.56 (4.23)	1.56 (4.23)
Dry population living within 30 mi divided by city population		– 2.51 (0.77)	0.42 (1.81)	0.42 (1.81)
Controls				
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State × time	No	No	Yes	Yes
State × time ²	No	No	No	Yes
R ²	0.17	0.01	0.15	0.15

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

homicides immediately after implementation similar to Column 1.

The demographic coefficients were not statistically significant in the estimation; however, the signs of the coefficients were in agreement with previous papers. Police spending was positively correlated with homicides, while charity spending was negatively correlated with murders. This corresponds to the signs reported by Miron (1999), Fishback et al. (2010), and Owens (2011).

Coefficients on the males between the ages of 15 and 24, percent of the city that was Catholic, and percent of the city that was foreign were all positive. These positive correlations are in agreement with the positive correlation found by Owens (2011) at the state level. There is a minor deterrent effect from an additional state execution. Spillover effects are negative meaning that increasing the number of people living in a neighboring dry county decreases the number of homicides. Adding in state specific linear trends increases the

Table 8

Dynamic effects of adopting state and national prohibition, 1911–1929 cities that adopted prohibition before 1919 (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

Specification:	(1) Basic	(2) City controls	(3) State-specific linear trends	(4) State-specific quadratic trends
First year	– 2.74 (1.00)	– 4.27 (1.22)	– 14.79 (2.37)	– 14.79 (2.37)
Year 2	– 3.29 (1.09)	– 5.09 (1.58)	– 19.46 (4.52)	– 19.46 (4.52)
Year 3	–0.97 (1.44)	–2.79 (1.58)	– 29.11 (4.77)	– 29.11 (4.77)
≥ Year 4	–2.09 (2.45)	–3.98 (2.48)	– 16.44 (5.80)	– 16.44 (5.80)
Per capita police spending		–0.06 (0.07)	–0.09 (0.13)	–0.09 (0.13)
Per capita charity, prison, and hospital spending		0.05 (0.04)	–0.04 (0.10)	–0.04 (0.10)
% of white males ages 15–24		37 (74)	– 200 (76)	– 200 (76)
% of black males ages 15–24		971 (440)	3406 (321)	3406 (321)
% of foreign males age 15–24		144 (231)	505 (106)	505 (106)
% Catholic		47 (19)	34 (32)	34 (32)
% Jewish		110 (45)	183 (41)	183 (41)
% Illiterate		– 346 (159)	274 (204)	274 (204)
No. of state executions in the previous year		0.10 (0.15)	–0.85 (0.49)	–0.85 (0.49)
Population living within 30 mi divided by city population		–0.82 (1.08)	– 2.20 (0.72)	– 2.20 (0.72)
Dry population living within 30 mi divided by city population		0.38 (0.19)	0.15 (0.49)	0.15 (0.49)
Controls				
City FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
State × time	No	No	Yes	Yes
State × time ²	No	No	No	Yes
R ²	0.02	0.17	0.44	0.44

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

Table 9
Effects of enacting prohibition, 1911–1919 (dummy variable specification) (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

	Passage (1)	Enforcement (2)	Passage (3)	Enforcement (4)	Passage (5)	Enforcement (6)
Prohibition in the city	0.08 (1.23)	−2.59 (1.36)	0.08 (1.06)	−2.96 (1.09)	−2.05 (0.74)	−4.76 (0.86)
Per capita police spending			−0.002 (0.05)	0.01 (0.05)	0.03 (0.06)	0.03 (0.08)
Per capita charity, prison, and hospital spending			0.004 (0.04)	−0.002 (0.039)	−0.02 (0.04)	−0.02 (0.06)
% of white males ages 15–24			162 (87)	118 (86)	191 (128)	191 (128)
% of black males ages 15–24			646 (508)	659 (492)	613 (743)	613 (743)
% of foreign males age 15–24			77 (199)	146 (203)	110 (138)	110 (138)
% Catholic			28 (17)	28 (16)	16 (21)	16 (21)
% Jewish			−17 (29)	−40 (28)	−15 (30)	−15 (30)
% Illiterate			−167 (209)	−194 (213)	18 (121)	17 (128)
No. of state executions in the previous year			−0.01 (0.08)	0.001 (0.089)	−0.04 (0.09)	−0.04 (0.09)
Population living within 30 mi divided by city population			−0.27 (0.52)	−0.28 (0.54)	0.03 (0.49)	0.03 (0.49)
Dry population living within 30 mi divided by city population			−0.21 (0.14)	−0.26 (0.12)	−0.26 (0.16)	−0.26 (0.16)
City fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
State linear time trends?	No	No	No	No	Yes	Yes
R squared	0.03	0.03	0.18	0.13	0.07	0.16
Groups	60	60	60	60	60	60
Time periods	9	9	9	9	9	9
Observations	540	540	540	540	540	540

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

magnitude of the coefficient measurements but adds little to the estimation.

Column 3 shows the results from the estimation when adding in state specific linear trends. Coefficient estimates of prohibition on homicides are statistically significant and negatively correlated for the second and third year. The larger estimates suggest colinearity problems between the years under prohibition variables and the city effects, year effects, and state specific linear time trends. Column 4 confirms that adding additional information with state specific quadratic trends does not improve the specification. All coefficients and standard error estimates are the same between Column 3 and Column 4. Both estimations have small R-squared, suggesting that the predictive power of the estimation still is not powerful. The problem with state specific time trends does not improve, even when adding an additional ten years to the sample.

Table 6 gives the results from all cities from 1911 to 1929. Since the time period was extended, both state and national Prohibition are now accounted for in the estimation. The results are similar to the state-level prohibition estimate with an immediate negative correlation between the enactment of prohibition and homicides. However, the estimates are less negative than in the smaller time period sample. City controls, again, help explain fluctuations in homicide rates significantly better than the basic model. The estimates from Column 2 are barely statistically insignificant at the 5% level for both the first and second year of prohibition. Coefficient estimates become less negative the longer national Prohibition has been in place. Adding in state-specific linear and quadratic trends causes the coefficient estimates and standard errors of the estimation to increase significantly.

I conducted three robustness checks on the estimation. First, the estimation was re-run using only the nineteen cities that were forced into state prohibition. If the 23 cities acted fundamentally different than the whole sample, the estimates could be biased.

Second, to compare the results from cities to those of states in Owens (2011), I constructed a non-dynamic analysis. For the analysis, state prohibition was coded as a dummy variable that equaled one if prohibition was being enforced and zero if the city was not under prohibition. Third, since the year prohibition was passed and enacted differed, I redid the dummy variable estimation using the timing detailed in Owens (2011). The results from the robustness check suggest that the negative correlation findings from previous estimations are strong; however, the timing of prohibition changes the estimation results.

To check if the results were robust to city selection problems, I restricted the sample to only cities that were forced into prohibition by state legislatures. Table 7 shows the estimation results from the reduced sample for the years 1911 to 1919. The results are similar to Table 5 except that the coefficients are more negative. The restricted sample also produces similar results for the extended time period of 1911 to 1929 detailed in Table 8. The results from the full time period by the restricted sample are in contrast to entire sample results that were less statistically significant as detailed in Table 6. Both restricted samples produce the same dynamic result, with homicides being suppressed immediately after enactment of prohibition. Both estimations also suffer from large coefficients and standard errors in the state specific linear time trends and quadratic time trend models. In addition to selection problems, I also conducted a robustness check to ensure the Spanish Flu of 1918 was not confounding the results.

The Spanish Flu caused a great loss of life and most likely reduced crime in 1918 just as states were entering into prohibition. The effect of the Spanish Flu on crime could also have been dynamic with deaths of young citizens reducing the size of the pool of potential criminals in the future. However, the results from the estimation suggest the Spanish Flu had little impact on reduction of homicides correlated with prohibition. Eliminating data from

Table 10

Effects of enacting prohibition, 1911–1929 (dummy variable specification) (Dependent variable: annual homicides per 100,000 persons (Mean = 9.2)).

	Passage (1)	Enforcement (2)	Passage (3)	Enforcement (4)	Passage (5)	Enforcement (6)
Prohibition in the city	0.04 (1.06)	−1.72 (1.08)	−0.37 (0.93)	−2.36 (1.01)	−8.78 (8.05)	−8.78 (8.05)
Per capita police spending			−0.02 (0.03)	−0.02 (0.03)	0.007 (0.035)	0.007 (0.035)
Per capita charity, prison, and hospital spending			0.004 (0.019)	0.009 (0.020)	−0.002 (0.024)	−0.002 (0.024)
% of white males ages 15–24			37 (20)	35 (20)	64 (35)	64 (35)
% of black males ages 15–24			570 (285)	630 (274)	741 (684)	741 (684)
% of foreign males age 15–24			141 (81)	139 (77)	60 (100)	60 (100)
% Catholic			31 (7.6)	27 (7.4)	14 (9)	14 (9)
% Jewish			10 (7)	10 (7)	6 (12)	6 (12)
% Illiterate			−142 (79)	−142 (79)	−0.49 (79)	−0.49 (79)
No. of state executions in the previous year			0.003 (0.044)	0.003 (0.044)	0.28 (0.22)	0.28 (0.22)
Population living within 30 mi divided by city population			−0.10 (0.20)	−0.10 (0.20)	−0.09 (0.22)	−0.09 (0.22)
Dry population living within 30 mi divided by city population			0.003 (0.018)	0.003 (0.018)	−0.0002 (0.020)	−0.0002 (0.020)
City fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
State linear time trends?	No	No	No	No	Yes	Yes
R squared	0.01	0.01	0.11	0.17	0.05	0.31
Groups	60	60	60	60	60	60
Time periods	19	19	19	19	19	19
Observations	1140	1140	1140	1140	1140	1140

Notes: Robust standard errors clustered by city are in parentheses. Coefficients in bold are significant at the $p < 0.05$ level.

the 1918 created comparable results to Tables 5 and 6 with state prohibition still having a negative effect on homicide rates⁶.

There are two testable hypotheses for the statistically significant negative correlation found in this paper compared to Owens (2011) statistically insignificant result. First, Owens (2011) used a dummy variable to measure prohibition instead of Wolfers (2006) dynamic framework potentially altering the outcome. Second, Owens (2011) used the passage of prohibition legislation instead of enforcement. Tables 9 and 10 show the results from the estimation using a standard dummy variable. Table 9 details the results from the 1911–1919 estimation, while Table 10 shows the results from the 1911–1929 estimation. Each specification was run twice using both passage and enforcement timing. I found that the disagreement between Owens (2011) and the results in the paper are attributable to the disparity between the timing of passage and enforcement, rather than the dynamic versus static measurement of prohibition.

For both tables, Columns 1, 3, and 5 show the results of conducting the estimation using the timing from Owens (2011) and a single dummy variable to indicate whether the city was under prohibition. The comparable columns for the estimates are 2, 4, and 6 which show the results using a single dummy variable for state prohibition and the start of enforcement as the year when state prohibition began in the city. The coefficients in the enforcement columns have a similar magnitude to the results from the dynamic measurement. This indicates that while a dynamic estimation shows that reduction in homicides is brief, a static analysis would produce similar results. However, in every specification the coefficient of interest using the enforcement timing produces more negative results than the coefficient of interest using passage. The statistically significant results in Columns 2, 4, and 6 combined with the lack of statistically

significant results in Columns 1, 3, and 5 suggests that the estimation is not robust to changes in the timing of prohibition. Interesting the specifications both produced the same effect if the longer time period with state linear trends were used. Which results are more indicative of how prohibition affects homicides? If citizens did not change their behavior until after prohibition was enforced – rather than when a law was voted on – then the negative correlation results are the more accurate measurement of the effects of prohibition on homicides. If citizens change their behavior immediately after passing a law, then the timing of the approval vote is a more accurate measurement of state prohibition.

6. Conclusion

Alcohol prohibition has repeatedly been linked to increased violence by popular culture. Newspapers, during the 1920s, attributed the crime wave sweeping the nation as a direct result of Prohibition. Panic over the perceived increase in crime from national Prohibition culminated in a watershed moment for United States law enforcement. Nine years after the start of Prohibition, President Hoover appointed the National Commission on Law Observance and Enforcement to begin tackling the perceived lawlessness of the nation. Identifying the original causes of the crime waves that instigated the forming of the commission during the 1920s has remained elusive for scholars.

I present evidence that alcohol prohibition was not the cause of increased homicide rates during the 1910s and 1920s. Indeed, I estimate that state prohibition was actually a dynamic event that initially decreased homicides in the short run, while having no effect on homicides after two years. The estimation benefited from the United States federal system whereby cities are compelled to enforce laws implemented by a higher level of government. Cities were forced into banning alcohol by state legislatures during the

⁶ Regression results without the Spanish Flu year are available upon request.

1910s and by the national government in 1920. Variations in the timing of imposed prohibition allow a difference-in-difference estimation technique used in the paper.

Homicides are only one dimension of the costs of alcohol prohibition. Prohibition could have varying effects on less serious crimes such as robbery, larceny, burglary, assault, embezzlement, prostitution, and gambling. One of the lasting legacies of the National Commission of Law Observance was implementation of a consolidated report of all crimes from city police departments across the country. The FBI was placed in charge of the Uniform Crime Reporting (UCR) program that has continuously reported crime statistics in cities since 1930. Unfortunately, crime statistics before 1930 do not exist in a consolidated format. Data for future research will need to be painstakingly collected city by city in order to provide scholars with a better understanding of the effect alcohol prohibition had on other crimes, not only homicides.

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