SUPPLEMENT TO "THE POLITICAL ECONOMY OF MORAL CONFLICT: AN EMPIRICAL STUDY OF LEARNING AND LAW ENFORCEMENT UNDER PROHIBITION"

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This supplement provides and discusses additional empirical results and information on data sources. In Section S.1, I present descriptive statistics on the partisan support for Prohibition, and on the time-variation in Prohibition's outcomes. I also explore the importance of changes in alcohol demand, and heterogeneous effects related to World War I and state-level variation in dry legislation. Subsequently, I present evidence on the relationship between Prohibition outcomes and public opinion changes, on heterogeneity in outcomes related to the alcohol market size, and on the response of crime outcomes following the repeal of Prohibition. Finally, I discuss some evidence on the relationship between fiscal concerns and public support for Prohibition repeal. Section S.2 then describes the data sources used in the paper.

S.1. ADDITIONAL RESULTS

Partisan Support for Prohibition

TABLE S.I PRESENTS THE PARTISAN BREAK-UP of the U.S. House roll call on the 18th Amendment (Prohibition), and the 21st Amendment (repeal). It illustrates the bipartisan nature of Prohibition support, and the more lopsided support for repeal by Democrats.

Time-Varying Effects of Prohibition

Table S.II presents the main regression results from estimating equation (1) in the paper on two samples. The sample in odd-numbered columns is a balanced sample of the 66 cities for which complete data are available for the whole period 1911–1936. This is the sample used for structural estimation. The sample in even-numbered columns is an unbalanced panel excluding cities for which there are less than ten years of data for drunkenness arrests or police expenditure, or less than eight years of homicide rate data. The tables present estimates of the specification including year effects for each outcome variable. The two columns for each outcome include the coefficients on the D_τ's as a way of disaggregating the time-varying effects of Prohibition and allowing for pre-nationwide Prohibition effects.[1]

Neighboring Markets: Testing for Changes in Demand

If individuals' preferences are affected by the legal standard in place, say because they derive utility from abiding by the law, or, on the other hand, if in-

[1]For the police share and per capita police expenditure regressions, I also ran regressions including city-specific trends, which do not show any significative differences to the ones presented in Table S.II.

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TABLE S.I
U.S. HOUSE ROLL CALLS[a]

	18th Amendment (65th Congress)		21st Amendment (72nd Congress)	
	No (Wet)	Yes (Dry)	No (Dry)	Yes (Wet)
Democrats	64	140	32	179
Republicans	62	138	89	109

[a]The table decomposes the roll call votes in the U.S. House for the 18th and 21st Amendments between Democrats and Republicans. *Source*: ICPSR Congress roll calls data set.

dividuals' utility from taking an action increases when it is proscribed (a "forbidden fruit effect"), observed changes in the drunkenness arrest rate could be driven by these shocks in preferences. A way to isolate any taste shocks introduced by Prohibition is to look at the response of the alcohol market in a city which is already under Prohibition, when neighboring states' prohibitionist status changes. If drinkers in a city under Prohibition have access to neighboring markets, which is very consistent with the concern of Prohibitionists of the time, and which motivated the passage of the Webb–Kenyon act, then the closure of neighboring markets should reduce the availability of liquor in the city, without having an effect on preferences.[2] Thus, I collected information on the lengths of all state boundaries,[3] and computed, for each state, the share of state border in states under Prohibition at each point in time:[4]

$$SBP_{ct} = \frac{\sum_{j \in N_c} P_{jt} \times BorderLength_{cj}}{\sum_{j \in N_c} BorderLength_{cj}},$$

[2]The importance of cross-state-boundaries alcohol trade after Prohibition was enacted in some states but not in neighboring ones is probably best exemplified by Daniel Okrent's discussion of the huge traffic lanes along Interstate 25, connecting Toledo, OH with Detroit, MI, after Michigan was covered by statewide Prohibition in 1918. The highway was nicknamed "Avenue de Booze" (see Okrent (2010, p. 107)).

Isaac also highlights the importance of cross-state smuggling of alcohol after Tennessee started enforcing its Prohibition legislation: "The State bone-dry law, even when supplemented by the Reed amendment, or "national bone-dry law," which made it a federal crime to transport intoxicants into a dry state, did not actually stop the flow of liquor into Tennessee. During 1917 and 1918, bootleggers were adequately supplied with whiskey brought from Kentucky to Nashville and Memphis by train, automobile, farm wagon, and river boat." (Isaac (1965, p. 254).)

[3]The information on state boundary lengths was taken from Holmes (1998). There are a total of 109 boundaries between U.S. states, and 16 international boundaries.

[4]I include any international borders in the denominator, which amounts to considering Mexico and Canada as never being under Prohibition.

TABLE S.II

SHORT AND LONG-RUN EFFECTS OF PROHIBITION[a]

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st year under Prohibition	−2.290	−2.104	−7.765	−5.593	−0.0017	−0.0021	−0.032	−0.017
	(1.417)	(1.389)	(2.146)	(1.287)	(0.003)	(0.001)	(0.060)	(0.028)
2nd year under Prohibition	−1.649	−1.252	−10.838	−9.737	−0.0025	−0.0022	0.014	0.000
	(1.840)	(1.848)	(2.538)	(1.432)	(0.003)	(0.002)	(0.083)	(0.037)
3rd year under Prohibition	−1.369	−1.482	−10.947	−9.467	−0.0046	−0.0031	0.027	−0.001
	(1.958)	(1.876)	(2.995)	(1.716)	(0.004)	(0.002)	(0.096)	(0.047)
4th year under Prohibition	−3.123	−2.647	−11.721	−8.892	−0.0007	−0.0014	0.094	0.037
	(3.071)	(2.978)	(3.609)	(2.008)	(0.005)	(0.002)	(0.101)	(0.053)
5th year under Prohibition	−0.072	0.281	−9.095	−7.859	0.0025	−0.0006	0.149	0.069
	(2.439)	(2.459)	(3.844)	(2.310)	(0.005)	(0.002)	(0.101)	(0.056)
6th year under Prohibition	0.330	0.738	−8.527	−7.422	0.0043	0.0015	0.213	0.123
	(2.818)	(2.785)	(4.216)	(2.623)	(0.005)	(0.002)	(0.129)	(0.059)
7th year under Prohibition	0.117	0.731	−9.241	−7.777	0.0064	0.0038	0.243	0.151
	(2.820)	(2.746)	(4.676)	(2.601)	(0.006)	(0.002)	(0.163)	(0.063)
8th year under Prohibition	2.381	2.937	−9.238	−8.326	0.0090	0.0050	0.351	0.176
	(2.331)	(2.395)	(4.676)	(2.720)	(0.005)	(0.003)	(0.177)	(0.073)
9th year under Prohibition	2.320	3.163	−9.432	−8.083	0.0125	0.0062	0.422	0.205
	(2.628)	(2.709)	(4.996)	(2.801)	(0.006)	(0.003)	(0.196)	(0.081)
10th year under Prohibition	1.528	2.397	−9.478	−7.908	0.0122	0.0063	0.412	0.254
	(2.417)	(2.553)	(5.574)	(2.963)	(0.006)	(0.002)	(0.189)	(0.064)
11th year under Prohibition	0.841	1.853	−8.045	−7.698	0.0130	0.0065	0.456	0.259
	(2.323)	(2.505)	(5.644)	(2.995)	(0.005)	(0.002)	(0.181)	(0.059)
12th year under Prohibition	−0.488	0.904	−5.891	−7.889	0.0164	0.0082	0.438	0.262
	(1.800)	(2.005)	(6.168)	(3.349)	(0.005)	(0.002)	(0.177)	(0.060)

(*Continues*)

TABLE S.II—*Continued*

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
13th year under Prohibition	1.454	2.663	−4.241	−6.195	0.0158	0.0064	0.436	0.297
	(1.700)	(1.812)	(6.495)	(3.980)	(0.004)	(0.002)	(0.151)	(0.072)
14th year under Prohibition	0.834	2.597	−1.574	−2.151	0.0123	0.0040	0.319	0.135
	(1.377)	(1.679)	(7.353)	(3.880)	(0.004)	(0.002)	(0.147)	(0.065)
15th year under Prohibition	−2.906	−0.381	−6.240	−2.561	0.0083	0.0021	0.253	0.108
	(1.443)	(1.789)	(5.800)	(4.904)	(0.003)	(0.002)	(0.113)	(0.057)
16th year under Prohibition	−3.146	−2.034	−2.467	−6.930	0.0064	0.0025	−0.016	−0.069
	(1.675)	(1.730)	(6.333)	(2.853)	(0.003)	(0.002)	(0.120)	(0.068)
17th year under Prohibition	−4.601	−2.282	3.591	−10.118	0.0046	−0.0010	−0.187	−0.149
	(1.605)	(1.655)	(6.486)	(3.336)	(0.004)	(0.002)	(0.100)	(0.082)
18th year under Prohibition	−4.409	−3.033	1.493	−12.096	0.0008	−0.0045	−0.237	−0.033
	(1.691)	(2.128)	(6.955)	(2.976)	(0.004)	(0.004)	(0.117)	(0.074)
19th year under Prohibition	−7.033	−8.162	2.012	−8.127	0.0011	−0.0004	−0.197	−0.014
	(2.290)	(2.682)	(7.097)	(3.293)	(0.009)	(0.004)	(0.223)	(0.094)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.31	0.30	0.32	0.27	0.31	0.24	0.66	0.56
No. of cities	66	90	66	237	66	239	66	239
No. of observations	1,716	1,921	1,254	4,083	1,650	4,718	1,650	4,718

[a]The table presents the OLS coefficient estimates from models based on equation (1) in the main text. In columns 1 and 2 the dependent variable is the homicide rate. In columns 3 and 4 the dependent variable is the drunkenness arrests rate. In columns 5 and 6 the dependent variable is the city police expenditure as a fraction of total city expenditure. In columns 7 and 8 the dependent variable is the per capita city police expenditure in 1913 dollars. Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on all other dependent variables. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

where P_{jt} is an indicator variable for state j being under Prohibition at time t. N_c is the set of states neighboring city c's state (e.g., $N_{\text{SanFrancisco}} = \{Oregon, Nevada, Arizona, Mexico\}$), and $BorderLength_{cj}$ is the length in miles of the state boundary between city c's state and state j. For the pre-Constitutional Prohibition period (1911–1919), when there is variation across states in Prohibition status, I estimate models of the form

$$d_{ct} = \alpha_c + \beta_t + \delta P_{ct} + \eta SBP_{ct} + \phi P_{ct} SBP_{ct} + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}. \tag{S.1}$$

Table S.III presents the estimates of equation (S.1), for different specifications, and samples. First, the fraction of border under Prohibition should have an effect on the drunkenness arrest rate only when the city itself is under Prohibition; otherwise, the city's neighbors' Prohibition status should be irrelevant, since a free alcohol market is available. Thus, columns 1–3 in the table start presenting the estimates of a model where I include the share of border under Prohibition without an interaction with own Prohibition status. The share of state boundary under Prohibition is insignificant in the three specifications. Then columns 4–6 introduce the interaction term, and columns 7–9 additionally include time-varying controls (log of population, and time-varying state capital and South effects). The coefficient for the ϕ is negative and large in magnitude, and always highly significant, except for column 8 when looking at the smaller alternative sample. The coefficient for ϕ on column 6, for example, implies that a one pre-1920 standard deviation (0.29) increase in the fraction of state border under Prohibition implied a reduction in the drunkenness arrest rate of 1.93, which is 10% of the average pre-1920 drunkenness arrest rate in the sample. These estimates are very consistent with the idea that the sharp falls in drunkenness arrests observed were caused by a contraction in the alcohol supply available, and not due to preference shocks correlated with the introduction of Prohibition.

Heterogeneous Effects and Years Under Prohibition

World War I

In Table S.IV, I test whether there were heterogeneous responses of crime or the drunkenness arrest rate as a function of the extent to which different cities experienced the World War I draft. I estimate models of the form

$$y_{ct} = \alpha_c + \beta_t + \sum_{\tau=1}^{k} \delta_\tau D_{c\tau} + \sum_{\tau=1}^{k} \phi_\tau D_{c\tau} Z_c + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}. \tag{S.2}$$

In equation (S.2), Z_c is the number of males registered for the WWI draft born and living in city c in 1917 from the Civilian Draft Registration cards in Ancestry.com. I emphasize that this is not the number of men who served in

TABLE S.III

EFFECTS OF NEIGHBORING PROHIBITION ON THE ALCOHOL MARKET[a]

Dependent Variable:	Drunkenness Arrests Rate per 1,000								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Prohibition indicator	−6.123	−6.072	−6.415	−3.427	−5.464	−3.745	−3.511	−5.750	−3.932
	(1.376)	(1.927)	(1.359)	(1.864)	(2.182)	(1.859)	(2.165)	(2.652)	(2.149)
Share of border under Prohibition	−2.128	−6.257	−2.102	1.455	−5.504	1.425	0.351	−6.144	0.300
	(2.138)	(3.095)	(2.139)	(2.562)	(3.338)	(2.562)	(2.440)	(3.167)	(2.441)
Prohibition × Share of border under Prohibition				−6.778	−1.348	−6.681	−5.112	−0.046	−4.976
				(2.650)	(3.018)	(2.656)	(2.819)	(3.685)	(2.830)
Time-varying controls	No	No	No	No	No	No	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.32	0.23	0.23	0.32	0.24	0.25	0.35	0.25
No. of cities	245	66	236	245	66	236	245	66	236
No. of observations	1,876	594	1,861	1,876	594	1,861	1,876	594	1,861

[a]The table presents OLS coefficient estimates from models based on equation (S.1), on a yearly panel of U.S. cities covering the period 1911–1919 (prior to Constitutional Prohibition). The dependent variable is the drunkenness arrests rate per 1,000. Columns 1–3 include a dummy indicator for Prohibition (state-level), and the share of the city's state boundaries that border neighboring states under Prohibition. Columns 4–9 additionally include an interaction term between the dummy indicator for Prohibition and the share of the state's border under Prohibition. Columns 7–9 additionally include interactions between time and each of the covariates. Models in columns 1, 4, and 7 use all cities for which data is available. Models in columns 2, 5, and 8 use only the balanced sample of cities used for estimation of the structural model. Models in columns 3, 6, and 9 use the sample of cities for which at least ten years of data on the drunkenness arrests rate is available. All models include a constant, not reported. Time-varying controls include log population, a Border indicator, and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

TABLE S.IV

HETEROGENEOUS EFFECTS: DIFFERENCES IN WORLD WAR I CIVILIAN DRAFT REGISTRATION[a]

Dependent Variable:	Homicide Rate per 100,000				Drunkenness Arrest Rate per 1,000			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st year under Prohibition ×	−0.425	−0.404	−0.397	−0.387	0.497	0.350	0.441	0.211
log WWI civil draft registration	(0.350)	(0.344)	(0.347)	(0.337)	(0.714)	(0.290)	(0.749)	(0.305)
2nd year under Prohibition ×	−0.233	−0.128	−0.192	−0.094	−0.174	0.256	−0.157	0.109
log WWI civil draft registration	(0.260)	(0.260)	(0.250)	(0.255)	(0.813)	(0.324)	(0.891)	(0.335)
3rd year under Prohibition ×	−0.029	−0.009	−0.005	0.011	0.017	0.459	0.076	0.364
log WWI civil draft registration	(0.215)	(0.234)	(0.239)	(0.246)	(0.805)	(0.334)	(0.845)	(0.337)
4th year under Prohibition ×	−0.192	−0.163	−0.078	−0.049	0.015	0.485	0.159	0.445
log WWI civil draft registration	(0.254)	(0.245)	(0.244)	(0.235)	(0.766)	(0.333)	(0.782)	(0.332)
5th year under Prohibition ×	0.055	0.057	0.190	0.171	0.018	0.515	0.176	0.520
log WWI civil draft registration	(0.231)	(0.231)	(0.238)	(0.237)	(0.776)	(0.339)	(0.781)	(0.340)
6th year under Prohibition ×	0.163	0.173	0.264	0.243	0.223	0.406	0.291	0.401
log WWI civil draft registration	(0.319)	(0.322)	(0.304)	(0.310)	(0.758)	(0.345)	(0.765)	(0.350)
7th year under Prohibition ×	0.176	0.215	0.248	0.246	0.255	0.769	0.339	0.764
log WWI civil draft registration	(0.291)	(0.299)	(0.295)	(0.296)	(0.807)	(0.392)	(0.816)	(0.398)
8th year under Prohibition ×	0.001	0.014	0.021	0.023	0.028	0.765	0.142	0.774
log WWI civil draft registration	(0.173)	(0.174)	(0.178)	(0.178)	(0.801)	(0.386)	(0.811)	(0.390)
9th year under Prohibition ×	0.702	0.714	0.696	0.701	0.141	0.956	0.233	0.967
log WWI civil draft registration	(0.327)	(0.323)	(0.326)	(0.319)	(0.852)	(0.401)	(0.856)	(0.403)
10th year under Prohibition ×	−0.144	−0.114	−0.142	−0.119	0.136	0.593	0.203	0.610
log WWI civil draft registration	(0.230)	(0.229)	(0.230)	(0.226)	(0.844)	(0.372)	(0.837)	(0.374)
11th year under Prohibition ×	0.246	0.263	0.270	0.280	0.211	0.762	0.200	0.748
log WWI civil draft registration	(0.284)	(0.276)	(0.284)	(0.274)	(0.892)	(0.363)	(0.893)	(0.364)
12th year under Prohibition ×	0.371	0.343	0.401	0.353	−0.763	0.953	−0.753	0.901
log WWI civil draft registration	(0.214)	(0.210)	(0.204)	(0.199)	(0.731)	(0.473)	(0.706)	(0.469)

(*Continues*)

TABLE S.IV—*Continued*

Dependent Variable:	Homicide Rate per 100,000				Drunkenness Arrest Rate per 1,000			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
13th year under Prohibition × log WWI civil draft registration	0.236	0.110	0.249	0.102	−1.924	1.156	−1.588	1.048
	(0.242)	(0.213)	(0.242)	(0.216)	(1.456)	(0.631)	(1.411)	(0.675)
14th year under Prohibition × log WWI civil draft registration	0.281	0.138	0.303	0.140	−0.329	0.845	0.153	0.684
	(0.221)	(0.195)	(0.229)	(0.200)	(1.711)	(1.092)	(1.758)	(1.045)
15th year under Prohibition × log WWI civil draft registration	0.241	0.308	0.256	0.287	2.778	1.341	3.080	1.171
	(0.362)	(0.283)	(0.353)	(0.270)	(3.872)	(0.921)	(4.227)	(0.985)
16th year under Prohibition × log WWI civil draft registration	−0.515	0.125	−0.801	−0.022	2.975	2.893	3.129	2.869
	(0.581)	(0.494)	(0.499)	(0.521)	(2.487)	(0.804)	(2.454)	(0.786)
17th year under Prohibition × log WWI civil draft registration	−0.525	1.345	−0.444	1.406	1.741	3.686	0.592	3.648
	(0.835)	(0.361)	(1.011)	(0.509)	(0.595)	(1.184)	(0.987)	(1.157)
18th year under Prohibition × log WWI civil draft registration	−2.048	−0.455	−1.473	−0.114	1.581	3.155	0.273	3.209
	(1.444)	(1.392)	(1.230)	(1.318)	(0.622)	(1.263)	(1.060)	(1.219)
19th year under Prohibition × log WWI civil draft registration	1.216	3.068	2.282	3.451	1.760	1.669	0.397	1.736
	(2.110)	(2.672)	(2.642)	(2.692)	(0.631)	(1.037)	(1.089)	(1.017)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	No	No	Yes	Yes	No	No	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.29	0.29	0.32	0.31	0.30	0.26	0.32	0.28
No. of cities	66	90	66	90	66	237	66	237
No. of observations	1,716	1,921	1,716	1,921	1,254	4,083	1,254	4,083

[a]The table presents OLS coefficient estimates of the interaction terms between years under Prohibition and the log number of males who completed the Civilian Draft Registration for World War I between 1917–1918, from models based on equation (S.2), on a yearly panel of U.S. cities covering the period 1911–1936. In columns 1–4 the dependent variable is the homicide rate per 100,000, and in columns 5–8 the dependent variable is drunkenness arrests rate per 1,000. Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on the drunkenness arrests rate. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

WWI or were drafted, but rather a sample from the number of men who filled the Registration Cards, and thus is subject to measurement error. Nevertheless, filling the registration card was mandatory for men under 46. Thus, the variation I use here comes from the men who moved from their city of birth. In columns 1–4, I present results for the drunkenness arrest rate. In columns 5–8, I present results for the homicide rate. In all models, I only present the estimates of the interaction terms ϕ_τ. The first two columns for each outcome do not include year fixed effects. The last two columns for each outcome include year fixed effects. The table shows no differential short- or long-run effects of the draft potential across cities.

Dry Legislation

Can differences in pre-Prohibition alcohol-related legislation explain the trends in crime, arrests for drunkenness, and police enforcement? Prior to the adoption of state-level and nationwide Prohibition, different states had different types and numbers of dry laws. In fact, regulations over the alcohol market were in place almost everywhere. These included restrictions on selling hours, on the kinds of alcoholic beverages permitted, on the types of selling establishments allowed, and on taxation. There are two channels through which pre-Prohibition alcohol legislation might affect the evolution of outcomes during Prohibition. First, given that early on during Prohibition, collective law enforcement decisions were likely to be closely related to initial "prior" beliefs about the policy's effects, variation in the short-run effects of Prohibition might be partly explained by variation in pre-Prohibition dry legislation. The direction of an effect is not obvious a priori. On the one hand, if these laws were being successful in shrinking the alcohol market and were not affecting crime, people's priors about the introduction of federal-level Prohibition could be very optimistic; on the other hand, if the introduction of these laws was correlated with more crime, individuals might have used this information to form negative priors about nationwide Prohibition. Second, differences in dry laws could have created different initial conditions for the alcohol market at the time of Prohibition adoption. For example, heavily regulated markets might have already developed a parallel black market which could have eased the expansion of the illegal liquor trade during Prohibition.

To take a look at this question, I reviewed the available information on state-level dry legislation in the pre-18th Amendment period and constructed a variable counting the number of regulations on the alcohol market at each point in time for each state. Interestingly, although the relationship between average "wetness" of a state, as measured by μ, and the number of dry laws in place is not very strong, it is actually positive. This is likely to be the result of the equilibrium political strategies used by dry lobbies during the 1900s and 1910s. Because relatively "wet" regions were unlikely to pass Prohibition laws, the lobbies focused their efforts on passing regulatory legislation instead, which was

politically feasible.[5] States like Michigan or Minnesota (both heavily "wet") passed, especially during the 1910s, significant amounts of regulatory legislation related to alcohol. In the other extreme, radically "dry" states such as Utah and Oklahoma did not need to pass this kind of legislation because they were already under Prohibition in the first place.

Pre-Prohibition legislation is, of course, endogenous to outcomes over that period. Given that I want to explore the effects of pre-Prohibition dry legislation on outcomes during Prohibition, which might have an effect through initial beliefs (and hence, initial law enforcement choices during Prohibition), or in how they shaped the local alcohol markets (and hence, in the subsequent response of alcohol supply during Prohibition), below I briefly investigate the effect of pre-Prohibition legislation on Prohibition outcomes, conditional on local preferences, by estimating models only for Prohibition years, in which I allow for a differential effect of the number of pre-Prohibition dry laws over time under Prohibition, controlling by a time-varying effect of baseline "wetness":

$$y_{ct} = \alpha_c + \beta_t + \sum_{\tau=1}^{k} \delta_\tau D_{c\tau} + \sum_{\tau=1}^{k} \eta_\tau D_{c\tau} L_c + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}. \tag{S.3}$$

In equation (S.3), L_c is the number of dry laws in place right before the city is under Prohibition, and $\mathbf{X}_{ct}$ includes interactions of $\overline{\mu}_{c0}$ with year indicators. Because these models only look at years under Prohibition, I omit the indicator for $\tau = 1$, so the interpretation of the "years under Prohibition" indicator variables is different; coefficients must now be interpreted as relative to having experienced Prohibition for one year. The η_τ's should capture any time-varying differential effects of an extra piece of dry pre-Prohibition legislation on Prohibition outcomes. Flexibly controlling for the moral profile of the city as proxied by μ is important given that pre-Prohibition dry legislation is likely to be correlated with preferences in the city. To save space, in Table S.V, I only present results for the coefficient estimates for the η_τ's of the benchmark fixed-effects specifications. Regression results do not show any significant relationship between the amount of pre-Prohibition dry legislation and the homicide rate or the arrest rate at any time during Prohibition. There also appears to be no relation between these laws and the behavior of per capita expenditure in policing during Prohibition years. For the expenditure share, on the other hand, the interaction terms are small in magnitude but significant, suggesting up to a 1% higher police share per pre-Prohibition piece of legislation around the tenth year under Prohibition, relative to the first one (see column 6). This result is not robust to the introduction of city-specific trends, though. Overall,

[5]The data on dry legislation were mostly taken from Cherrington (1920) and Anti-Saloon League (1932). Both sources have a detailed and comprehensive compilation of dry legislation during these decades.

TABLE S.V

HETEROGENEOUS EFFECTS: DIFFERENCES IN DRY LEGISLATION[a]

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2nd year under Prohibition ×	0.098	0.166	−0.334	−0.189	0.000	0.000	−0.004	−0.004
Pre-Prohibition dry laws	(0.208)	(0.202)	(0.398)	(0.291)	(0.0003)	(0.0002)	(0.006)	(0.004)
3rd year under Prohibition ×	0.247	0.218	−0.237	0.120	−0.001	0.000	−0.008	−0.005
Pre-Prohibition dry laws	(0.200)	(0.195)	(0.409)	(0.285)	(0.000)	(0.0003)	(0.009)	(0.006)
4th year under Prohibition ×	0.452	0.469	−0.316	0.163	0.000	0.000	−0.013	−0.007
Pre-Prohibition dry laws	(0.229)	(0.229)	(0.407)	(0.282)	(0.001)	(0.0003)	(0.011)	(0.007)
5th year under Prohibition ×	0.183	0.174	−0.427	0.122	0.000	0.001	−0.018	−0.010
Pre-Prohibition dry laws	(0.289)	(0.286)	(0.435)	(0.280)	(0.001)	(0.0003)	(0.013)	(0.008)
6th year under Prohibition ×	0.319	0.289	−0.503	−0.139	0.000	0.001	−0.005	−0.005
Pre-Prohibition dry laws	(0.262)	(0.262)	(0.427)	(0.290)	(0.001)	(0.0003)	(0.019)	(0.009)
7th year under Prohibition ×	0.334	0.315	−0.431	−0.129	0.000	0.001	−0.008	−0.003
Pre-Prohibition dry laws	(0.312)	(0.304)	(0.451)	(0.296)	(0.001)	(0.0003)	(0.021)	(0.010)
8th year under Prohibition ×	0.375	0.363	−0.486	−0.010	0.001	0.001	−0.003	−0.001
Pre-Prohibition dry laws	(0.307)	(0.299)	(0.413)	(0.290)	(0.0007)	(0.0003)	(0.022)	(0.010)
9th year under Prohibition ×	0.353	0.369	−0.672	−0.054	0.001	0.001	0.005	0.001
Pre-Prohibition dry laws	(0.327)	(0.318)	(0.428)	(0.306)	(0.001)	(0.0003)	(0.025)	(0.012)
10th year under Prohibition ×	0.298	0.321	−0.559	−0.056	0.001	0.001	−0.009	0.001
Pre-Prohibition dry laws	(0.249)	(0.243)	(0.431)	(0.312)	(0.001)	(0.0004)	(0.024)	(0.013)
11th year under Prohibition ×	0.094	0.125	−0.540	−0.188	0.001	0.001	−0.010	−0.013
Pre-Prohibition dry laws	(0.251)	(0.245)	(0.418)	(0.294)	(0.001)	(0.0004)	(0.024)	(0.019)
12th year under Prohibition ×	0.194	0.149	−0.394	−0.031	0.001	0.001	−0.022	−0.001
Pre-Prohibition dry laws	(0.253)	(0.240)	(0.480)	(0.340)	(0.001)	(0.0004)	(0.030)	(0.014)

(*Continues*)

TABLE S.V—*Continued*

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
13th year under Prohibition ×	0.133	0.094	−0.079	0.188	0.001	0.001	−0.034	−0.012
Pre-Prohibition dry laws	(0.236)	(0.227)	(0.878)	(0.661)	(0.001)	(0.0004)	(0.035)	(0.019)
14th year under Prohibition ×	0.149	0.196	−1.212	0.606	0.001	0.001	−0.025	0.005
Pre-Prohibition dry laws	(0.263)	(0.250)	(1.081)	(1.197)	(0.001)	(0.0004)	(0.037)	(0.018)
15th year under Prohibition ×	−0.019	0.157	−16.505	1.956	0.002	0.002	−0.024	0.004
Pre-Prohibition dry laws	(0.242)	(0.245)	(3.514)	(2.523)	(0.0010)	(0.0005)	(0.038)	(0.021)
16th year under Prohibition ×	−0.143	0.055	−12.068	2.232	0.002	0.001	0.017	−0.002
Pre-Prohibition dry laws	(0.440)	(0.367)	(1.764)	(1.429)	(0.0011)	(0.0009)	(0.038)	(0.025)
17th year under Prohibition ×	−0.913	−0.737	5.239	−1.343	0.002	0.000	0.004	−0.053
Pre-Prohibition dry laws	(1.304)	(1.035)	(1.039)	(5.556)	(0.004)	(0.002)	(0.078)	(0.046)
18th year under Prohibition ×	1.369	1.121	4.310	0.270	0.001	−0.002	−0.027	−0.056
Pre-Prohibition dry laws	(1.404)	(1.492)	(1.077)	(4.264)	(0.004)	(0.003)	(0.072)	(0.053)
19th and more years under Prohibition ×	−4.095	−3.993	6.214	0.041	−0.009	−0.003	−0.465	−0.080
Pre Prohibition dry laws	(1.812)	(1.605)	(1.225)	(4.071)	(0.005)	(0.002)	(0.146)	(0.070)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.47	0.40	0.50	0.35	0.39	0.21	0.75	0.61
No. of cities	66	90	66	237	66	239	66	239
No. of observations	1,066	1,210	1,066	2,655	1,066	3,477	1,066	3,477

[a]The table presents OLS coefficient estimates of the interaction terms between years under Prohibition and the number of state-level dry laws in place prior to the introduction of Prohibition in the city, from models based on equation (S.3). In columns 1–2 the dependent variable is the homicide rate per 100,000. In columns 3–4 the dependent variable is the drunkenness arrests rate per 1,000. In columns 5–6 the dependent variable is the city police expenditure as a fraction of total city expenditure. In columns 7–8 the dependent variable is the per capita city police expenditure in 1913 dollars. Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on all other dependent variables. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

there seems to be no evidence that dry laws prior to Prohibition had any economically important effects on the evolution of outcomes during Prohibition years.

Women's Suffrage

Several historians have attributed some of the success of Prohibition in the United States to the significant role that the Women's Suffrage Movement played. It is undeniable that women played a prominent role in the conflict over alcohol consumption, and were of importance at least since the 1840s when the "Daughters of Temperance" were organized in New York State. Then, in the 1870s, a group of Ohio women began the "Temperance Crusade," which spread throughout the Midwest. These women would visit local saloons, and protest and pray for days until owners were forced to close. The long-term effects of the crusade are likely to have been minimal, but it constituted a major women-specific social mobilization, and was the origin of the WCTU some years later. In the twentieth century, both the Women's Suffrage Movement and the Temperance Movement were part of the Progressive-era reforms, and organizations such as the WCTU were involved in the political struggle around both issues. Although U.S.-wide women's suffrage (19th Amendment) was ratified into the Constitution in 1920, after the adoption of nationwide Prohibition (18th Amendment), authors such as Okrent (2010) argue that the Women's Suffrage Movement gave a major impulse to the Prohibition movement. The almost simultaneous ratification of the 18th and 19th Amendments makes it impossible to identify any specific effects that women's suffrage might have had during federal Prohibition years. Nonetheless, prior to the 19th Amendment, several states had already extended the franchise to women.[6] As a way to explore the importance of women's enfranchisement on Prohibition-related outcomes, I exploit the variation in women's suffrage enfranchisement prior to 1920, when both the 18th and 19th Amendments were ratified, to see if Prohibition had differential effects in cities with and without women's suffrage. If the distribution of women's preferences over Prohibition enforcement was different than men's, cities allowing women's suffrage could be under a differential trend. Thus, for the 1910–1919 period, I run regressions of the form

$$y_{ct} = \alpha_c + \beta_t + \eta W_{ct} + \sum_{\tau=1}^{k} \delta_\tau D_\tau + \sum_{\tau=1}^{k} \phi_\tau D_\tau W_{ct} + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}. \tag{S.4}$$

[6]Women's Suffrage prior to the 19th Amendment was adopted by the states as follows: Wyoming (while still a Territory) in 1869, Colorado in 1893, Utah (while still a Territory) in 1895, Idaho in 1896, Washington in 1910, Arizona (while still a Territory) in 1910, California in 1911, Kansas and Oregon in 1912, Alaska (while still a Territory) in 1913, Montana and Nevada in 1914, New York in 1917, and Michigan, Oklahoma, and South Dakota in 1918 (see Keyssar (2000)).

In equation (S.4), W_{ct} is an indicator variable taking the value of 1 if city c has women's suffrage in year t. Table S.VI presents results of the estimates of the ϕ_τ's from equation (S.4) for the different outcome variables, in the specifications including city fixed effects, time-varying controls, and year effects. The regressions include only up to ϕ_5, because before 1919 no city with women's suffrage in the sample had experienced more than 5 years under Prohibition. There is no evidence of a differential trend in the homicide rate in cities with women's suffrage. This is unsurprising given that the short-run effects of Prohibition on the homicide rate were very small. For the outcomes which did have large short-run changes after the introduction of Prohibition, if anything, columns 2–3 in Table S.VI show that the introduction of women's suffrage is correlated with more drunkenness arrests in the short run (after two to three years under Prohibition), but the net effect is small and insignificant quickly thereafter. This result is also not robust to the restricted sample in column 2. When looking at police enforcement in columns 4–7, the results are also very inconclusive. During years with women's suffrage, cities have slightly lower but insignificant policing, which is actually inconsistent with the idea that women's anti-Prohibitionism should translate to higher law enforcement and a smaller alcohol market after their enfranchisement. Overall, the available evidence does not suggest that alternative legislation, such as dry laws or women's suffrage, might have been driving the trends in law enforcement, crime, and arrests presented in Section 3 in the paper.

Public Opinion

I estimate fixed-effects regressions for both the county and the city samples, with two periods, $t \in \{0, 1\}$. $t = 0$ is the pre-Prohibition period, and $t = 1$ is the post-Prohibition period, for a year in which there was a liquor-related referendum. The models I estimate take the basic form

$$w_{ct} = \alpha_c + \beta t + \delta \mu_{ct} + \phi \mu_{c0} t + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}, \tag{S.5}$$

where w_{ct} is the wet vote share. In this model, the interaction term for the post period uses the initial period's "wetness," given that it is based on baseline moral preferences that law enforcement and its equilibrium effects are endogenously determined. I use two alternative proxies for wetness: the average of wet religion affiliations, the share black and foreign white, and the share of population ages 15–44, and the first principal component of these variables. $\mathbf{X}_{ct}$ is a vector of time-varying controls, including the log of population (1910 data for $t = 0$ and 1930 for $t = 1$), the urban share of the county (or of the county's city), the number of dry laws in place, the year in which the referendum took place, and indicator variables for the type of referendum (a Prohibition law, a constitutional convention election, or a constitutional amendment (omitted

TABLE S.VI

HETEROGENEOUS EFFECTS: DIFFERENCES IN WOMEN'S SUFFRAGE STATUS[a]

Dependent Variable:	Homicide Rate per 100,000	Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Women's suffrage indicator	0.117	−1.080	−2.183	0.000	0.001	0.013	0.051
	(1.024)	(1.696)	(1.132)	(0.003)	(0.002)	(0.072)	(0.046)
1st year under Prohibition × Women's suffrage	−0.734	1.410	1.311	−0.007	−0.003	−0.178	−0.085
	(1.502)	(2.812)	(1.623)	(0.003)	(0.002)	(0.092)	(0.040)
2nd year under Prohibition × Women's suffrage	−2.370	−5.484	5.409	−0.017	−0.002	−0.124	0.002
	(4.357)	(3.447)	(2.645)	(0.006)	(0.004)	(0.110)	(0.054)
3rd year under Prohibition × Women's suffrage	−1.318	−4.172	5.616	−0.021	−0.002	−0.151	−0.024
	(4.537)	(3.969)	(3.655)	(0.009)	(0.005)	(0.111)	(0.062)
4th year under Prohibition × Women's suffrage	5.552	−0.017	4.276	−0.036	−0.003	−0.223	−0.051
	(6.904)	(6.215)	(3.767)	(0.010)	(0.005)	(0.095)	(0.051)
5th year under Prohibition × Women's suffrage			3.329				
			(4.841)				
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.11	0.34	0.29	0.33	0.19	0.65	0.61
No. of cities	66	66	236	66	217	66	217
No. of observations	594	594	1,861	528	1,427	528	1,427

[a]The table presents OLS coefficient estimates from models based on equation (S.4), on a yearly panel of U.S. cities covering the period 1910–1919 (prior to Constitutional Prohibition). In column 1 the dependent variable is the homicide rate per 100,000. In columns 2–3 the dependent variable is the drunkenness arrests rate per 1,000. In columns 4–5 the dependent variable is the city police expenditure as a fraction of total city expenditure. In columns 6–7 the dependent variable is the per capita city police expenditure in 1913 dollars. All models include a dummy indicator for women's suffrage in the city's state, and interactions of this indicator with time under Prohibition. Models in columns 1, 2, 4, and 6 use only the balanced sample of cities used for estimation of the structural model. Models in columns 3, 5, and 7 use the sample of cities for which at least ten years of data on the drunkenness arrests rate is available. All models include a constant, not reported. Time-varying controls include log population, a Border indicator, and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

category)). The estimate of ϕ should capture the differential increase in the wet vote share in wetter communities.

Table S.VII presents the main results. Columns 1–6 present results for the complete sample of counties.[7] Finally, columns 7–12 present results for the sample of cities. Columns 1 and 7 first simply regress the wet vote share on a post-Prohibition period indicator. The estimated coefficient in column 1 implies that the average county experienced a 13 percentage points larger wet vote share after Prohibition (s.e. = 0.004). Column 2 then presents estimates of the main specification in equation (S.5) without additional controls. Column 3 controls for the log of population and the urban share, the year in which the referendum took place, and indicators for the type of referendum. Both the type of referendum in consideration and the year in which it took place are likely to be endogenous to the vote share, given that the timing and kind of referendum were likely to depend on the trends of public support for Prohibition; for example, a proposal for a constitutional amendment was likely to take place in states where public opinion favoring Prohibition was believed to be widespread. Thus, I do not stress the results of the models in columns 3 and 9; nevertheless, estimates are very similar to those excluding these variables.

Column 4 includes state-cross-post-Prohibition interactions, and column 5 accounts for the potential selection problem arising from the fact that a subset of wet states never held pre-Prohibition liquor referenda, by controlling for the inverse Mills ratio of the estimates of a Probit selection equation for holding a referendum.[8] Column 6 then reproduces the model in column 5 but using the

[7]For comparative purposes, I also computed estimates for analogous models restricting the sample to counties with a population larger than 30,000, which I omit to conserve space (available upon request). Estimates for the restricted sample of more populous counties are actually larger in magnitude, and imply that the differential responses observed here are not driven by a comparison of extremely dry versus extremely wet communities.

[8]A caveat in the elections data is that several states, including Louisiana, New Jersey, New York, and Pennsylvania, did not hold any liquor-related referendum in a pre-Prohibition year. This induces a potential selection bias in the estimates of equation (4) because these states never held a referendum regarding liquor precisely due to the highly anti-Prohibitionist preferences of their citizens. As a robustness check, I also estimate a selection model, by specifying a selection equation for holding a referendum (at the state level). More specifically, I assume that

$$r_{St} = \begin{cases} 1 & \text{if } t = 0 \text{ and } \boldsymbol{\eta}'\mathbf{Z}_{S0} + \upsilon_{S0} > 0, \\ 1 & \text{if } t = 1, \end{cases}$$

where r_{St} is an indicator variable for state S holding a liquor referendum, $\mathbf{Z}_{S0}$ includes the state's share of adherents to a wet religion and the share of native white individuals in 1910, and $\upsilon_{S0} \sim N(0,1)$, with $E[\varepsilon_{c0}|\upsilon_{S0}] = \rho\upsilon_{S0}$ and $E[\varepsilon_{c1}|\upsilon_{S0}] = 0$. This implies that

$$E[w_{ct}|\mu_{ct}, \mu_{c0}, \mathbf{X}_{ct}, r_{St} = 1] = \alpha_c + \beta t + \delta\mu_{ct} + \phi\mu_{c0}t + \boldsymbol{\gamma}'\mathbf{X}_{ct} + \kappa\lambda\left(\boldsymbol{\eta}'\mathbf{Z}_{S0}\right)1_{\{t=0\}},$$

where $\lambda()$ is the inverse Mills ratio. Results are reported in columns 6, 12, and 18 of Table II in the paper.

TABLE S.VII

ELECTORAL SUPPORT FOR PROHIBITION[a]

Panel A	Dependent Variable: Wet Vote Share											
	Counties Sample						Cities Sample					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post-Prohibition indicator	0.137	−0.110	0.081	−0.057	−0.109	0.158	0.245	−0.078	−0.131	0.160	0.021	0.233
	(0.004)	(0.018)	(0.022)	(0.035)	(0.017)	(0.006)	(0.013)	(0.084)	(0.123)	(0.097)	(0.094)	(0.034)
"Wetness"		0.120	0.142	0.121	0.107	−0.513		0.222	−0.257	−0.724	0.000	−0.499
		(0.090)	(0.095)	(0.078)	(0.096)	(0.082)		(0.363)	(0.370)	(0.495)	(0.413)	(0.384)
Baseline "Wetness" ×		0.697	0.597	0.481	0.735	0.079		0.664	0.590	0.426	0.474	0.040
Post-Prohibition indicator		(0.045)	(0.042)	(0.044)	(0.047)	(0.004)		(0.181)	(0.158)	(0.125)	(0.213)	(0.015)
Log of population			0.088	0.067	0.081				0.161	0.093	0.011	0.056
			(0.011)	(0.008)	(0.011)				(0.058)	(0.062)	(0.048)	(0.049)
Urban share of county			0.021	0.052	0.057				−0.183	−0.146	−0.092	−0.077
			(0.039)	(0.028)	(0.041)				(0.082)	(0.120)	(0.099)	(0.097)
Number of dry laws									−0.034	−0.002	−0.022	−0.023
									(0.011)	(0.035)	(0.008)	(0.008)
Referendum year			−0.011						0.000			
			(0.001)						(0.005)			
Referendum type:												
Prohibition law			−0.005						0.002			
			(0.008)						(0.028)			
Constitutional convention election			0.010						0.114			
			(0.010)						(0.045)			
Inverse Mills ratio × Pre-Prohibition period					0.158	0.152					−0.055	−0.044
					(0.031)	(0.031)					(0.075)	(0.073)
State cross post-Prohibition effects	No	No	No	Yes	No	No	No	No	No	Yes	No	No
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.38	0.49	0.55	0.77	0.53	0.52	0.74	0.77	0.81	0.94	0.79	0.80
No. of cross sections	1,693	1,693	1,693	1,693	1,693	1,693	258	258	258	258	258	258
No. of observations	3,386	3,386	3,386	3,386	3,386	3,386	129	129	129	129	129	129

(*Continues*)

TABLE S.VII—*Continued*

Panel B	Probit Selection Equation	
	(13)	(14)
Share of wet religions	−1.092	−1.092
	(1.277)	(1.277)
Share of non-native white	−7.297	−7.297
	(3.836)	(3.836)
Constant	3.630	3.630
	(1.605)	(1.605)
Pseudo *R*-squared	0.17	0.17
No. of observations	31	31
Log likelihood	−11.32	−11.32

[a]The table presents OLS regression results of models based on equation (S.5), on a two-periods panel. Period 0 uses pre-Prohibition era information, while period 1 uses late-Prohibition era information. In Panel A the dependent variable is the fraction of the vote share in alcohol-related referenda supporting an anti-Prohibitionist ballot proposal (the wet vote share). Columns 1–6 report results using a sample of U.S. counties. Columns 7–12 report results using a sample of U.S. cities. Columns 5, 6, 11, and 12 report results controlling for selection using the inverse Mills ratio from a Probit selection (into having a pre-Prohibition era liquor referendum) equation at the state level. Models in columns 1–5 and 7–11 use the "wetness" proxy defined in the text as the average of the fraction of the population in either of Orthodox, Jewish, Lutheran, Catholic, or Other ascriptions from the 1916 Census of Religions, the fraction of the non-native white population, and the fraction of the population ages 15–44. Models in columns 6 and 12 use an alternative "wetness" proxy computed as the principal component of the same three variables. Panel B reports the estimates from the Selection equation (see footnote 8), which includes an indicator for having experienced an Alcohol-related referendum in the pre-Prohibition era, the state-level share of adherents to any "wet" religion as defined in the main text (Orthodox, Jewish, Lutheran, Catholic, Other) from the 1916 Census of Religions, and the share of non-native white individuals in the state, from the 1910 Population Census. Models in Panel A include a constant, not reported. All standard errors are robust to arbitrary heteroskedasticity and clustered at the county or city level.

principal component wetness proxy instead of the average proxy showing very similar results. In all regressions, I run a completely balanced panel. The estimates of the selection equation are shown in Panel B. If anything, the size of ϕ, the estimated differential effect of having a larger wet constituency, increases when accounting for selection.

The estimate of ϕ from column 4 implies that a county with a one standard deviation higher average baseline wetness would differentially increase its wet vote share by 6 percentage points ($0.062 = 0.48 \times 0.13$). The interaction terms are very precisely estimated across specifications, and the regression results suggest that most of the increase in support for anti-Prohibitionism occurred through the differentially larger growth in wet support of "morally" wet communities. The magnitude and significance of the estimates for the city sample are very close to those of the county sample, as can be seen in columns 7–12.

Heterogeneity in Moral Views During Prohibition

Similarly to the empirical strategy in equation (1) in the paper, I regress each of the outcome variables y_{ct} on the years-under-Prohibition indicators, and their interaction with the initial value of the "wetness" measure.[9] As a benchmark for comparison, I ran analogous regressions using only the Constitutional Prohibition indicator. The models I estimate take the form

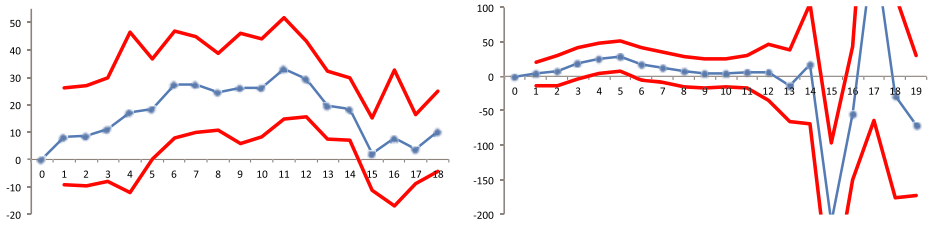
$$y_{ct} = \alpha_c + \beta_t + \sum_{\tau=1}^{k} \delta_\tau D_{c\tau} + \sum_{\tau=1}^{k} \phi_\tau D_{c\tau} \bar{\mu}_c + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}, \tag{S.6}$$

where $\bar{\mu}_c$ is a wetness proxy (either the average of demographics or the principal component measure). Interest lies in the differential evolution of outcomes over time under Prohibition, captured by the estimates of the ϕ_τ's in equation (S.6). These measure how the different outcome variables changed differentially over the years under Prohibition, between cities with varying "moral" distributions (relative to a city with zero "wet" population).

For ease of exposition, Panel A in Figure S.1 graphs the estimated ϕ_τ's using the average wetness proxy from the models in Table S.VIIIA. Estimates are very similar in magnitude across alternative samples. The figure shows an increasing differential gap in the homicide rate during the first years under Prohibition, which subsequently closes over time, for cities with relatively "wetter" constituencies. This happened especially during the years in which the homi-

[9]I take 1911 as the baseline value for μ_{ct}. For cities without religious distribution data before that year, I use the earliest year available (1916 in most cases). As a robustness check, I ran identical regressions using the 1911 data on the somewhat reduced sample of cities without data before 1916, and results varied only marginally (available upon request).

Panel A: Homicide rate (per 100,000) (left) and drunkenness arrest rate (per 1,000) (right).



Panel B: Police expenditure share (left) and per capita police expenditure (right).

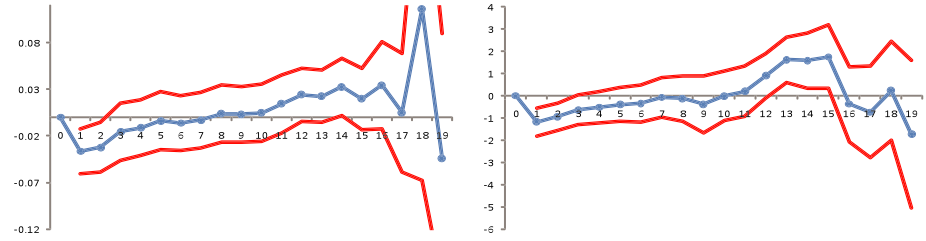


FIGURE S.1.—ϕ_τ's from equation (S.6).

cide rate was high. Because the differential increases in crime followed the same time pattern of overall crime during Prohibition, this suggests that a large fraction of the increase in criminality occurred in cities with wetter constituencies. Differential changes in the drunkenness arrest rate, which can be seen in the right panel of Figure S.1, appear to be small and significantly different from zero only in a few of the years under Prohibition when the alcohol supply was likely experiencing its fastest recovery.

Panel B plots the estimated ϕ_τ's for the police share and per capita police equations. Both show a similar pattern: cities with "wetter" constituencies increased police expenditure differentially less during early Prohibition years, but this gap closes over time, and for later Prohibition years, wetter cities have differentially higher spending in police. The relatively tighter law enforcement in drier cities during the early Prohibition years is consistent with their constituencies having relatively optimistic beliefs about its effects, making them more willing to repress the alcohol market, and expecting little response of crime. But criminality was increasing relatively more in wetter cities, and their alcohol markets were bouncing back faster.

This suggests that criminality was very sensitive to the size of the potential alcohol market, requiring higher levels of crime enforcement in wetter cities, despite their preferences for a more lenient enforcement of the Prohibition laws. Indeed, Panel B in Figure S.1 shows that changes in police expenditure were

TABLE S.VIIIA

HETEROGENEOUS EFFECTS: DIFFERENCES IN MORAL VIEWS[a]

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st year under Prohibition × Wetness	−13.58	−13.16	−10.99	7.39	−0.097	−0.031	−3.304	−1.022
	(14.73)	(14.30)	(21.37)	(8.72)	(0.028)	(0.012)	(0.855)	(0.304)
2nd year under Prohibition × Wetness	−2.103	1.406	−13.65	12.24	−0.100	−0.023	−3.478	−0.719
	(11.10)	(9.48)	(22.11)	(11.70)	(0.032)	(0.013)	(0.943)	(0.294)
3rd year under Prohibition × Wetness	−5.83	−3.87	−5.76	19.05	−0.103	−0.002	−3.400	−0.259
	(9.23)	(8.49)	(23.05)	(12.40)	(0.037)	(0.014)	(1.068)	(0.327)
4th year under Prohibition × Wetness	9.867	8.950	3.567	23.34	−0.106	−0.003	−3.237	−0.235
	(8.692)	(8.489)	(20.37)	(11.90)	(0.035)	(0.012)	(1.027)	(0.307)
5th year under Prohibition × Wetness	8.545	11.05	−0.761	25.51	−0.107	0.005	−3.158	−0.081
	(7.449)	(7.657)	(20.32)	(11.65)	(0.034)	(0.012)	(1.023)	(0.330)
6th year under Prohibition × Wetness	17.97	21.92	6.486	14.69	−0.097	0.001	−2.775	−0.140
	(9.142)	(9.429)	(20.53)	(12.51)	(0.028)	(0.013)	(0.923)	(0.387)
7th year under Prohibition × Wetness	28.18	26.30	0.257	9.445	−0.093	0.004	−2.144	0.144
	(11.930)	(7.865)	(21.48)	(11.56)	(0.030)	(0.013)	(0.961)	(0.379)
8th year under Prohibition × Wetness	20.58	23.79	−7.061	3.815	−0.071	0.011	−2.143	0.206
	(7.737)	(7.566)	(22.30)	(11.49)	(0.026)	(0.013)	(0.827)	(0.451)
9th year under Prohibition × Wetness	32.92	32.46	−11.510	0.992	−0.055	0.013	−1.240	−0.011
	(11.015)	(9.942)	(22.90)	(11.36)	(0.024)	(0.013)	(0.879)	(0.552)
10th year under Prohibition × Wetness	22.07	22.24	−3.044	1.578	−0.059	0.015	−1.520	0.309
	(9.146)	(8.214)	(22.70)	(11.15)	(0.022)	(0.013)	(0.795)	(0.475)
11th year under Prohibition × Wetness	29.03	29.36	−4.601	2.361	−0.036	0.019	−0.843	0.300
	(10.50)	(9.27)	(23.65)	(13.08)	(0.020)	(0.013)	(0.780)	(0.491)
12th year under Prohibition × Wetness	27.21	28.25	28.04	−1.229	0.036	0.027	1.273	0.883
	(7.488)	(6.853)	(28.15)	(22.00)	(0.020)	(0.012)	(0.759)	(0.438)

(*Continues*)

TABLE S.VIIIA—*Continued*

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
13th year under Prohibition × Wetness	17.28	15.74	18.81	−21.00	0.014	0.027	1.741	1.469
	(7.821)	(6.388)	(39.12)	(28.34)	(0.018)	(0.012)	(0.929)	(0.471)
14th year under Prohibition × Wetness	19.82	15.65	28.86	6.424	0.009	0.030	2.353	1.196
	(6.940)	(6.432)	(83.42)	(43.46)	(0.018)	(0.012)	(0.985)	(0.528)
15th year under Prohibition × Wetness	−1.986	0.901	−110.50	−214.43	0.003	0.013	2.622	1.112
	(9.616)	(7.330)	(74.04)	(47.45)	(0.023)	(0.013)	(1.022)	(0.644)
16th year under Prohibition × Wetness	17.98	6.842	−4.00	−46.92	0.008	0.019	1.382	−0.295
	(15.128)	(13.627)	(66.96)	(46.32)	(0.039)	(0.015)	(1.597)	(0.459)
17th year under Prohibition × Wetness	−4.708	4.993		172.81	0.027	0.035	0.217	−0.102
	(8.743)	(7.385)		(135.19)	(0.019)	(0.012)	(0.401)	(0.284)
18th year under Prohibition × Wetness	8.58	11.88		−22.21	0.011	0.044	0.295	0.575
	(7.886)	(7.348)		(72.68)	(0.025)	(0.017)	(0.538)	(0.260)
19th year under Prohibition × Wetness	7.646	14.11		−64.03	−0.014	0.032	0.296	1.065
	(9.716)	(10.67)		(50.04)	(0.040)	(0.017)	(1.003)	(0.351)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.34	0.33	0.32	0.27	0.36	0.25	0.69	0.57
No. of cities	66	90	66	237	66	239	66	239
No. of observations	1,716	1,921	1,254	4,083	1,650	4,913	1,650	4,913

[a]The table presents OLS coefficient estimates of the interaction terms between years under Prohibition and the "wetness" proxy defined in the text as the average of the fraction of the population in either of Orthodox, Jewish, Lutheran, Catholic, or Other ascriptions from the 1916 Census of Religions, the fraction of the non-native white population, and the fraction of the population ages 15–44, from models based on equation (S.6). In columns 1 and 2 the dependent variable is the homicide rate. In columns 3 and 4 the dependent variable is the drunkenness arrests rate. In columns 5 and 6 the dependent variable is the city police expenditure as a fraction of total city expenditure. In columns 7 and 8 the dependent variable is the per capita city police expenditure in 1913 dollars. Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on all other dependent variables. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

differentially higher in wetter cities during the later years under Prohibition. These were years in which cities were, overall, reducing police expenditure, so the figure implies that wet cities were unable to reduce law enforcement as fast.[10] The estimates for these models are presented in Table S.VIIIA, and analogous estimates using the alternative wetness proxy based on the first principal component of the wet demographics and religious affiliations are presented in Table S.VIIIB for completeness. These present very similar patterns to those described above. I report the estimates of the interaction terms ϕ_τ only, to save space.

If tightening Prohibition enforcement drove illegal producers towards a more intensive use of violence, why did police enforcement fall more slowly in wet cities in the later years under Prohibition, if these were the ones most unwilling to enforce it? I suggest the answer is the impossibility to separate overall crime enforcement and the enforcement of restrictions over a specific market, when the legal standard prescribes full Prohibition. The prohibited market itself becomes a major source of criminality, so that combatting crime also indirectly tightens the alcohol market. Under Prohibition, the ability to specifically target crime without restricting the alcohol market was limited, especially for policing activities. Thus, Prohibition in wet cities not only had adverse effects over crime, but also was costly because for a given level of police expenditure, it would lead to a larger response of crime relative to a city with a smaller alcohol market. This predicts larger shifts in preferences over Prohibition in these communities.

Prohibition Repeal

The repeal of the 18th Amendment itself also allows for the exploration of differential trends in criminality between cities with varying moral profiles. Here I exploit the repeal of nationwide Prohibition in December 1933 with the ratification of the 21st Amendment, to provide some additional evidence of the response of crime to Prohibition, and its stronger effects in communities with larger alcohol markets. I take advantage of the availability of more detailed crime data for the 1930–1936 period, taken from the Uniform Crime Reports (UCR) complied by the FBI starting in 1930. The UCR reports, for a large number of cities, the total number of offenses known to the authorities (which include any of the following: murder, rape, robbery, assault, burglary, larceny, and auto theft), and an independent measure of reported murders. Thus, I compare crime outcomes in the 1930–1933 period with the 1934–1936 period, allowing for differential behavior after repeal, as cities vary in their moral preference distribution, as proxied by μ. Indeed, simple summary statis-

[10]As a placebo test for the results on police expenditure, I ran analogous models using the expenditure in fire. I do not include the results here to save space, but no discernible differences appear between cities with different moral profiles.

TABLE S.VIIIB

HETEROGENEOUS EFFECTS: DIFFERENCES IN MORAL VIEWS[a]

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st year under Prohibition × Wetness	−1.094	−1.013	−1.439	0.729	−0.006	−0.001	−0.258	−0.081
	(1.162)	(1.130)	(1.816)	(0.712)	(0.002)	(0.001)	(0.073)	(0.025)
2nd year under Prohibition × Wetness	−0.119	0.147	−1.603	1.002	−0.006	−0.001	−0.261	−0.057
	(0.789)	(0.701)	(1.933)	(1.076)	(0.003)	(0.001)	(0.082)	(0.025)
3rd year under Prohibition × Wetness	−0.600	−0.571	−0.697	1.440	−0.006	0.000	−0.246	−0.027
	(0.555)	(0.519)	(1.969)	(1.128)	(0.003)	(0.001)	(0.090)	(0.027)
4th year under Prohibition × Wetness	0.609	0.544	−0.052	1.995	−0.006	0.001	−0.230	−0.009
	(0.638)	(0.635)	(1.824)	(1.074)	(0.002)	(0.001)	(0.085)	(0.027)
5th year under Prohibition × Wetness	0.362	0.653	−0.205	2.351	−0.006	0.002	−0.220	0.007
	(0.615)	(0.634)	(1.781)	(1.054)	(0.002)	(0.001)	(0.082)	(0.029)
6th year under Prohibition × Wetness	1.210	1.627	0.075	1.445	−0.006	0.001	−0.195	0.010
	(0.799)	(0.847)	(1.836)	(1.124)	(0.002)	(0.001)	(0.072)	(0.032)
7th year under Prohibition × Wetness	1.776	2.045	−0.503	1.007	−0.006	0.001	−0.114	0.036
	(0.895)	(0.827)	(1.949)	(0.941)	(0.002)	(0.001)	(0.076)	(0.035)
8th year under Prohibition × Wetness	1.280	1.676	−0.589	0.633	−0.004	0.002	−0.129	0.029
	(0.616)	(0.627)	(2.007)	(0.935)	(0.002)	(0.001)	(0.065)	(0.040)
9th year under Prohibition × Wetness	2.511	2.539	−1.168	0.235	−0.003	0.002	−0.048	0.026
	(0.898)	(0.822)	(2.058)	(0.963)	(0.002)	(0.001)	(0.076)	(0.046)
10th year under Prohibition × Wetness	1.615	1.695	−0.916	0.051	−0.003	0.002	−0.080	0.059
	(0.683)	(0.619)	(2.030)	(0.957)	(0.002)	(0.001)	(0.066)	(0.040)
11th year under Prohibition × Wetness	2.232	2.312	−0.749	0.016	−0.002	0.002	−0.036	0.058
	(0.780)	(0.694)	(2.088)	(1.079)	(0.002)	(0.001)	(0.065)	(0.046)
12th year under Prohibition × Wetness	1.983	1.964	4.980	0.137	0.004	0.003	0.164	0.121
	(0.674)	(0.595)	(2.394)	(1.851)	(0.002)	(0.001)	(0.061)	(0.040)

(*Continues*)

TABLE S.VIIIB—*Continued*

Dependent Variable:	Homicide Rate per 100,000		Drunkenness Arrest Rate per 1,000		Police Expenditure Share		Per capita Police Expenditure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
13th year under Prohibition × Wetness	1.244	1.169	6.002	−0.921	0.002	0.003	0.182	0.170
	(0.679)	(0.567)	(2.631)	(2.535)	(0.002)	(0.001)	(0.076)	(0.040)
14th year under Prohibition × Wetness	1.430	1.106	15.545	−0.132	0.001	0.003	0.236	0.152
	(0.564)	(0.516)	(5.390)	(3.097)	(0.002)	(0.001)	(0.084)	(0.051)
15th year under Prohibition × Wetness	−0.354	−0.021	−8.830	−14.15	0.000	0.001	0.224	0.156
	(0.724)	(0.557)	(6.330)	(2.931)	(0.002)	(0.001)	(0.090)	(0.059)
16th year under Prohibition × Wetness	1.164	0.441	1.081	−3.472	−0.002	0.001	0.171	−0.043
	(1.596)	(1.173)	(6.084)	(3.231)	(0.004)	(0.002)	(0.156)	(0.069)
17th year under Prohibition × Wetness	−6.129	0.906		7.056	−0.013	−0.002	−0.099	−0.130
	(3.208)	(2.264)		(9.060)	(0.005)	(0.003)	(0.167)	(0.081)
18th year under Prohibition × Wetness	1.392	3.739		−1.573	−0.011	0.004	−0.245	−0.038
	(2.132)	(1.954)		(5.304)	(0.006)	(0.005)	(0.222)	(0.077)
19th year under Prohibition × Wetness	5.507	−0.978		−4.252	−0.044	0.002	−1.030	−0.126
	(4.682)	(3.259)		(3.675)	(0.005)	(0.005)	(0.287)	(0.129)
Time-varying controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.33	0.32	0.33	0.27	0.34	0.25	0.69	0.57
No. of cities	66	90	66	237	66	239	66	239
No. of observations	1,716	1,921	1,254	4,083	1,650	4,913	1,650	4,193

[a]The table presents OLS coefficient estimates of the interaction terms between years under Prohibition and the "wetness" proxy computed as the principal component of the fraction of the population in either of Orthodox, Jewish, Lutheran, Catholic, or Other ascriptions from the 1916 Census of Religions, the fraction of the non-native white population, and the fraction of the population ages 15–44, from models based on equation (S.6). In columns 1 and 2 the dependent variable is the homicide rate. In columns 3 and 4 the dependent variable is the drunkenness arrests rate. In columns 5 and 6 the dependent variable is the city police expenditure as a fraction of total city expenditure. In columns 7 and 8 the dependent variable is the per capita city police expenditure in 1913 dollars. Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on all other dependent variables. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

tics show that offenses and murders were both lower in the post-18th Amendment years.[11]

Thus, I look exclusively at the period 1930–1936, and run regressions for UCR offenses and murders, and for the homicide rate:

$$y_{ct} = \alpha_c + \beta_t + \delta CP_t + \phi CP_t \overline{\mu}_c + \boldsymbol{\gamma}' \mathbf{X}_{ct} + \varepsilon_{ct}. \tag{S.7}$$

In equation (S.7), CP_t is an indicator variable for Constitutional Prohibition. Regression results are reported in Table S.IX. Columns 1–4 look at the homicide rate. The coefficient on the interaction is always large, highly significant, and robust to the introduction of state-cross-year effects, suggesting that the fall in crime was larger in wetter cities. Take, for example, column 2. The estimates imply that for the city with mean "wetness" of 0.49, repeal was associated with a fall in the annual homicide rate of $4.6 = (0.49 \times 23.28) - 6.76$. Even in the driest city, with $\mu = 0.3$, the estimated fall in the homicide rate is $0.21 = (0.3 \times 23.28) - 6.76$. Columns 5–8 then present analogous results for the UCR number of murders per 100,000. The pattern is very similar to the one for the homicide rate, although standard errors increase somewhat, and the magnitude of the effect is smaller for the larger sample of cities covered. Nonetheless, for the sample for which homicide rates are available, results are very similar. The large standard errors for the sample in columns 6 and 8 are due to the larger number of smaller cities included, in which reported murders were very small or close to zero, and present very little variation. Finally, columns 9–12 present results for offenses per 1,000. Interestingly, a pattern very similar to the one for homicides and arrests emerges, but this time, the effect is statistically significant especially in the larger sample including cities of smaller sizes. From column 12, for example, it follows that repeal in the city with average "wetness" implied a fall in total offenses of $3.85 = (0.49 \times 6.53) + 0.669$ per 1,000 population, which is 43% of this variable's standard deviation of 8.65. As the results suggest, while the reduction in criminality in larger cities was associated especially with a lower homicide rate, looking at a larger sample including smaller cities, repeal was associated with lower levels of other types of crime.

Fiscal Pressure and the Demise of Prohibition

Finally, in Table S.X, I present results discussed in Section 3 of the paper, testing the "fiscal pressure hypothesis." With this purpose, I collected data on city cost payments and revenues for 1928–1931. I computed two alternative proxies of "fiscal pressure": the difference between per capita revenues and cost payments, and the per capita cost payments per dollar of revenue. I then run models for the wet support in alcohol-related referenda on these measures

[11] Average murders per 100,000 are 8.57 (s.e. = 10.3) in the 1930–1933 period, and 6.53 (s.e. = 8.8) in 1934–1936, with a t-statistic for the difference in means of 4.62. For offenses per 1,000, the 1930–1933 mean is 16.26 (s.e. = 8.6), while the 1934–1936 mean is 15.6 (s.e. = 8.6), with a t-statistic of 1.64, significant at the 5% level.

TABLE S.IX

REPEAL OF THE 18TH AMENDMENT AND CRIME FALL[a]

Dependent Variable:	Homicide Rate per 100,000				Murders per 100,000				All Offences per 100,000			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constitutional Prohibition	−5.910	−6.767	−6.008	−4.350	6.268	−0.436	9.144	3.230	0.458	−3.945	−4.122	0.669
(1930–1933)	(4.181)	(3.738)	(5.394)	(3.527)	(5.791)	(1.996)	(10.493)	(9.065)	(4.569)	(1.836)	(7.454)	(2.530)
Constitutional Prohibition ×	20.118	23.280	22.133	18.465	4.036	3.880	15.549	3.375	3.045	5.318	12.215	6.529
Wetness	(7.423)	(6.646)	(11.927)	(7.799)	(10.926)	(3.640)	(23.203)	(5.479)	(8.529)	(3.211)	(16.484)	(3.875)
Time-varying controls	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
Year effects	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No
City effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
R-squared	0.45	0.35	0.71	0.62	0.35	0.08	0.61	0.25	0.12	0.07	0.48	0.23
No. of cities	66	93	66	93	66	324	66	324	66	324	66	324
No. of observations	462	651	462	651	417	1,938	417	1,938	414	1,943	414	1,943

[a]The table presents OLS regression results of models based on equation (S.7), on a yearly panel of U.S. cities covering the period 1930–1936. In columns 1–4 the dependent variable is the homicide rate per 100,000, in columns 5–8 the dependent variable is the number of murders per 100,000 from the FBI's Uniform Crime Reports, and in columns 9–12 the dependent variable is the number of total offences per 100,000 from the FBI's Uniform Crime Reports. The explanatory variables are a dummy indicator for the years under Constitutional Prohibition (1930–1933), and an interaction between the Prohibition period dummy and "wetness" as defined in the text (fraction of the population in either of Orthodox, Jewish, Lutheran, Catholic, or Other ascriptions from the 1916 Census of Religions). Odd columns present results using the balanced sample of cities used for structural estimation. Even columns use the sample of all cities with at least eight years of data for the homicide rate, or at least ten years of data on all other dependent variables. All models include a constant, not reported. Time-varying controls include log population, a Border indicator and a State-capital indicator. Standard errors are robust to arbitrary heteroskedasticity and clustered at the city level.

TABLE S.X

ELECTORAL SUPPORT FOR PROHIBITION: TESTING THE FISCAL PRESSURE HYPOTHESIS[a]

Panel A	Dependent Variable: Late Prohibition Wet Vote Share							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Prohibition wet vote share	0.5768	0.3883	0.5597	0.3885	0.6199	0.3944	0.6109	0.3911
	(0.079)	(0.086)	(0.077)	(0.085)	(0.071)	(0.083)	(0.071)	(0.082)
Per capita revenues minus cost payments, 1928	0.000	0.000						
	(0.001)	(0.001)						
Per capita cost payments per dollar of revenue, 1928			0.132	0.032				
			(0.104)	(0.103)				
Per capita revenues minus cost payments, 1931					−0.0007	−0.0008		
					(0.001)	(0.001)		
Per capita cost payments per dollar of revenue, 1931							0.143	0.063
							(0.103)	(0.092)
"Wetness"		0.719		0.714		0.736		0.741
		(0.181)		(0.180)		(0.177)		(0.176)
Log of population		0.005		0.005		0.007		0.005
		(0.009)		(0.009)		(0.009)		(0.009)
Urban share of county		0.033		0.032		0.061		0.061
		(0.064)		(0.066)		(0.066)		(0.067)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.652	0.749	0.658	0.749	0.646	0.765	0.651	0.765
No. of observations	122	122	122	122	127	127	127	127

(*Continues*)

TABLE S.X—*Continued*

Panel B	Dependent Variable: Change in Wet Vote Share (Late vs. Early Prohibition)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Per capita revenues minus cost payments, 1928	0.000	0.000								
	(0.001)	(0.001)								
Per capita cost payments per dollar of revenue, 1928			0.018	0.030						
			(0.120)	(0.125)						
Per capita revenues minus cost payments, 1931					−0.0011	−0.0016				
					(0.001)	(0.002)				
Per capita cost payments per dollar of revenue, 1931							0.106	0.116		
							(0.127)	(0.127)		
Change in per capita cost payments per dollar of revenue, 1928–1931									0.073	0.077
									(0.116)	(0.121)
Change in "wetness" (late vs. early Prohibition)		−0.899		−0.887		−0.901		−0.944		−0.890
		(0.551)		(0.539)		(0.514)		(0.509)		(0.542)
Log of population		0.011		0.013		0.016		0.013		0.011
		(0.011)		(0.010)		(0.010)		(0.010)		(0.010)
Urban share of county		0.038		0.027		0.070		0.071		0.045
		(0.081)		(0.082)		(0.076)		(0.076)		(0.080)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.740	0.756	0.739	0.756	0.736	0.762	0.737	0.761	0.741	0.758
No. of observations	122	122	122	122	127	127	127	127	122	122

[a]The table presents OLS regression results for a cross section of cities. In Panel A the dependent variable is the vote share against Prohibition in a referendum during the late years of Prohibition. The explanatory variables are the per capita city revenues net of cost payments, or the ratio of cost payments to revenues in either 1928 or 1931. In Panel B the dependent variable is the difference between the vote shares against Prohibition in a referendum during the late years of Prohibition, and before national Prohibition. The explanatory variables are the per capita city revenues net of cost payments, the ratio of per capita cost payments to revenues in either 1928 or 1931, and the change in the ratio of per capita cost payments to revenues between 1928 and 1931. All models in even columns include the fraction of the population in any of the wet religious ascriptions (Orthodox, Jewish, Lutheran, Catholic, and Other from the 1916 Census of Religions), the log of population in 1930, and the urban share of the city's county. All models include state fixed effects and a constant, not reported. All standard errors are robust. City finance data are from the Financial Statistics of Cities.

of fiscal pressure. In Panel A, I estimate a model for the vote share against Prohibition in the late Prohibition period on the fiscal pressure proxies, controlling for the wet vote share in the pre-Prohibition period. Columns 1–4 look at fiscal pressure in 1928, before the Great Depression had begun. There is no correlation between the balance sheets of cities and the wet support. In columns 5–8, I look at fiscal pressure in 1931, in the midst of the Great Depression. Once again, I find no correlation between the fiscal condition of cities and their late Prohibition wet support.

In Panel B, I estimate models similar to those in Panel A, but in first differences. The dependent variable in this case is the change in wet support between the late and early Prohibition referenda. In even columns, I control for the change in "wetness" using the average proxy described above. Columns 1–4 use the 1928 fiscal pressure measure, and columns 5–8 use the 1931 fiscal pressure measure. I do not find any significant correlation for the pre-Depression or Depression years. Finally, in columns 9–10, the explanatory variable is the change in fiscal pressure between 1928 and 1931. I am also unable to find a correlation with the change in wet support in this case.

S.2. DATA SOURCES

Most of the information available for the study of Prohibition is available at the city level, so I focused on constructing a yearly panel data set of cities, covering the 1910s, 1920s, and early 1930s. The data collected come from a wide array of sources. The first source of information is the collection of original documents from the *National Commission on Law Observance and Enforcement*, most commonly known as the Wickersham Commission after the name of its Chair Commissioner, Attorney General George Wickersham. It was appointed in the Spring of 1929 by President Hoover, with the specific purpose of "studying exhaustively the entire problem of the enforcement of our laws and the improvement of our judicial system, including the special problem and abuses growing out of the Prohibition laws" (Wickersham-Commission (1928–1931)). It was, of course, appointed as a response to the growing concerns about the effects Prohibition was having throughout the country, and the public discontent over the policy's effects. The Commission produced a series of reports on the different aspects of Prohibition, after directly collecting data and evidence across the country, and issued its main findings in 1931. Harvard's Law School Library currently holds the collection of documents from the Commission, including the originals of much of the summarized data in the published reports, in addition to several other unpublished information. The detailed city-by-city "Prohibition Survey" reports, compiled directly by commissioners traveling to the cities and collecting information about the recent evolution of criminality, and the "Cost of Crime" state-level folders, providing detailed data on local law enforcement activity, contain the most valuable information from the Wickersham papers.

TABLE S.XI

CITIES IN THE STRUCTURAL ESTIMATION SAMPLE

City	State	City	State	City	State
Akron	OH	Indianapolis	IN	Portland	OR
Albany	NY	Jersey City	NJ	Providence	RI
Atlanta	GA	Kansas City	KS	Reading	PA
Baltimore	MD	Kansas City	MO	Richmond	VA
Birmingham	AL	Los Angeles	CA	Rochester	NY
Boston	MA	Louisville	KY	Saint Louis	MO
Bridgeport	CT	Lowell	MA	Saint Paul	MN
Buffalo	NY	Memphis	TN	Salt Lake City	UT
Cambridge	MA	Milwaukee	WI	San Antonio	TX
Camden	NJ	Minneapolis	MN	San Francisco	CA
Chicago	IL	Nashville	TN	Scranton	PA
Cincinnati	OH	New Bedford	MA	Seattle	WA
Cleveland	OH	New Haven	CT	Spokane	WA
Columbus	OH	New Orleans	LA	Springfield	MA
Dallas	TX	New York	NY	Syracuse	NY
Dayton	OH	Newark	NJ	Toledo	OH
Denver	CO	Norfolk	VA	Trenton	NJ
Detroit	MI	Oakland	CA	Washington	DC
Fall River	MA	Omaha	NE	Wilmington	DE
Grand Rapids	MI	Paterson	NJ	Worcester	MA
Hartford	CT	Philadelphia	PA	Yonkers	NY
Houston	TX	Pittsburgh	PA	Youngstown	OH

Crime Data

Criminality became the main source of concern about Prohibition for the public. The homicide rate is the variable for which most comprehensive information is available, and one for which measurement error is likely to be very limited. Thus, I collected information from the *Mortality Statistics* published yearly by the Bureau of the Census, reporting the number of non-traffic-related homicides for a sample of U.S. cities. I complemented this information with the homicide data reported in the Wickersham Commission documents, finally putting together yearly data for the period 1911–1936 and a sample of up to 93 cities. Data on drunkenness arrests, on the other hand, are very detailed and cover a total of 573 cities for the period 1910–1929.[12] Finally, the Federal Bureau of Investigation began compiling and publishing its *Uniform*

[12]The data on drunkenness arrests contained in the Wickersham Commission papers appear to have been originally compiled by the World League. Dills, Jacobs, and Miron (2005) used this source, covering a shorter time period, together with an alternative source compiled independently by the Moderation League. Both series appear to be highly correlated, so I restrict attention to the World League data, which cover the whole 1911–1929 period.

TABLE S.XII

FEDERAL AND STATE-LEVEL PROHIBITION ENFORCEMENT STATISTICS[a]

		1923–1924	1925–1926	1927–1928	1929–1930	1931–1932
Midwest	Seized distilleries, stills, and still worms	8,098	9,171	10,207	11,369	5,095
	Seized fermenters	6,830	7,525	48,748	81,178	8,714
	Seized spirits	1,050,017	610,440	472,922	475,840	505,713
	Seized malt, wine, cider, mash, and pomace	3,675,499	7,034,847	11,206,588	15,029,002	15,915,534
	Seized autos and boats	1,095	1,873	2,949	3,069	4,154
	Killed or injured officers	14	15	38	48	4
	Federal arrests	–	24,150	28,185	31,755	25,528
	State arrests	–	8,335	7,500	6,227	7,460
Northeast	Seized distilleries, stills, and still worms	4,191	3,456	11,136	6,960	4,511
	Seized fermenters	2,506	3,411	50,079	53,973	9,264
	Seized spirits	206,411	767,086	1,142,467	929,877	1,753,629
	Seized malt, wine, cider, mash, and pomace	1,143,955	6,334,026	11,811,643	13,930,121	21,233,629
	Seized autos and boats	1,082	4,078	4,333	3,334	4,115
	Killed or injured officers	4	24	26	40	3
	Federal arrests	–	35,316	46,396	29,657	47,585
	State arrests	–	3,610	3,828	3,708	2,848

(*Continues*)

TABLE S.XII—*Continued*

		1923–1924	1925–1926	1927–1928	1929–1930	1931–1932
South	Seized distilleries, stills, and still worms	34,087	48,038	43,327	43,273	31,671
	Seized fermenters	179,280	238,528	267,605	301,521	8,513
	Seized spirits	284,888	732,713	647,875	799,950	842,375
	Seized malt, wine, cider, mash, and pomace	11,933,042	22,026,530	30,428,757	34,753,824	29,953,542
	Seized autos and boats	2,703	4,979	5,290	7,058	9,054
	Killed or injured officers	18	42	62	122	4
	Federal arrests	–	42,673	49,498	53,300	51,976
	State arrests	–	11,778	11,001	12,811	7,737
West	Seized distilleries, stills, and still worms	4,691	7,089	6,334	4,956	2,141
	Seized fermenters	4,855	14,463	21,291	18,711	1,745
	Seized spirits	115,377	230,132	228,029	269,567	283,214
	Seized malt, wine, cider, mash, and pomace	1,171,349	3,019,286	3,952,164	7,374,756	5,242,851
	Seized autos and boats	1,138	1,421	1,774	2,040	3,009
	Killed or injured officers	18	20	31	17	3
	Federal arrests	–	17,007	14,486	16,845	9,943
	State arrests	–	4,718	6,828	6,640	9,262

[a]The table presents aggregate federal enforcement outcomes statistics throughout the Prohibition Era. Amounts of alcohol are in gallons. The Northeast region includes the states of ME, NH, VT, MA, RI, CT, NY, PA, and NJ. The Midwest region includes the states of ND, SD, NE, KS, MN, IA, MO, WI, IL, IN, MI, and OH. The South region includes the states of DE, MD, DC, VA, WV, KY, NC, TN, SC, GA, AL, MS, FL, AR, LA, OK, and TX. The West region includes the states of WA, OR, CA, ID, MT, WY, CO, UT, NV, AZ, and NM. *Source*: U.S. Bureau of Prohibition, Statistics Concerning Intoxicating Liquors, and Wickersham Commission papers.

Crime Report (UCR) in 1930, which contains yearly city-level data on murders and other offenses reported to the authorities.

Law Enforcement Data

Law enforcement is hard to measure because it depends on the discretion of the enforcer, and thus, is partly unobservable. Moreover, measuring law enforcement through its outcomes is problematic; an increase in liquor stills seized, for example, could be explained by an increase in Prohibition enforcement on a constant level of illegal alcohol production, or by a reduced level of law enforcement which allows for illegal production to increase. Because a great deal of Prohibition enforcement, and all of local crime enforcement, was decided and implemented at the city level, I focused on collecting data on city public finances, and specifically, on police expenditure. I use the *Financial Statistics of Cities* published yearly by the Bureau of the Census, which report disaggregated data on city public finances for cities with populations above 30,000 (around 250 cities), and obtain data on total city public expenditure and investment, police expenditure and investment, and all protection expenditure and investment (all protection includes police, fire, and other expenditure), for the period 1911–1936. I computed 1913-constant prices expenditure data by using the U.S.-wide CPI as of June of each year as the deflator.[13]

The Wickersham Commission papers also contain other data on total arrests, unfortunately available only during the 1910s and in 1929. Data on a set of other Prohibition enforcement outcomes are available only at the state level from the U.S. Bureau of Prohibition for the years 1923–1932, such as the number of still and liquor seizures, arrests of alcohol producers, and casualties caused by Prohibition enforcement agencies (U.S. Bureau of Prohibition (Several Years), Wickersham-Commission (1928–1931)). This information aggregates Prohibition enforcement operations from both federal and local authorities in most cases. I collected data on criminal judicial prosecutions from the Attorney General Annual Reports, which are available at the Judicial District level only, for the years 1915–1936.

Demographic and Religious Data

City- and county-level data on demographic characteristics are taken from the decennial population censuses. I focus on the age distribution, the ethnicity distribution,[14] and total population, from the 1910–1940 Censuses. Given the

[13]Data for the years 1914 and 1920 are unavailable. For the balanced panel estimations below, I use the interpolated values (1913–1915 average for 1914, and 1919–1921 average for 1920) for these two years.

[14]I focus on the distribution of the population between native white, foreign white, and black individuals.

strong relationship between religiosity and attitudes towards the liquor problem, I use religious ascription data from the decennial Censuses of Religions (1906, 1916, 1926, and 1936), to capture heterogeneity in moral views about Prohibition. I aggregated religious ascriptions in the following nine groups, directly from their names: Baptist, Eastern Orthodox, Evangelical, Jewish, Mormon, Lutheran, Methodist/Episcopal, Catholic, Presbyterian, and other. The consensus amongst historians is that Baptist, Evangelical, Mormon, Methodist, Episcopal, and Presbyterian communities held the strongest views in favor of Prohibition, while Catholic, Orthodox, Jewish, and Lutheran communities had much more favorable positions regarding alcohol consumption (see Foster (2002), Lewis (NA), Szymansky (2003)). I refer to the former as "dry" and to the latter as "wet" religions. I then computed the share in each religion directly as the number of adherents divided by the total number of adherents to any religion in the city (or county).

For the first four decades of the twentieth century, data on the distribution of religious ascriptions are available from the 1906, 1916, 1926, and 1936 decennial Censuses of Religions. The Censuses have comprehensive information about the number of adherents to each of the different faiths or churches in the United States.

Public Opinion Data

Most of the data come from the state official rosters or "blue books," which states publish on an annual or biannual basis. The information for some of the states was found in the state archives, and for a few other referenda not reported in official sources, I took the data from local newspapers. A second major source of electoral data on the Prohibition issue are the election returns for the 21st Amendment Constitutional Convention elections, also found in the state rosters and some state archives.

To measure public opinion about Prohibition, I collected electoral returns data on referenda on alcohol-related issues for the different states, taking place during the 1900s–1930s. These referenda were usually ballot measures proposed to the citizens to approve or repeal liquor laws, or amend the state constitutions. In states where local option was in place, county- or city-level referenda had the purpose of allowing or forbidding the sale of alcohol. When submitting the 21st Amendment to the states, the U.S. Congress determined that Constitutional Conventions should be elected in the different states to decide the issue, and candidates should run in either a dry or a wet slate (Brown (1935)). All of the referenda returns allow me to directly compute the fraction of (anti-Prohibitionist) wet vote, which I use as a proxy of wet support.[15] Al-

[15]The main caveat here is that turnout rates might differ systematically between Prohibitionist and anti-Prohibitionist voters, not reflecting the true distribution of political preferences in the community. For an empirical model of turnout on alcohol-related referenda, see Coate and Conlin (2004).

most all of the electoral returns data are available at the county level, except for referenda in the states of Connecticut and Massachusetts, for which city-level data were reported. Overall, I have referenda election returns for 2,083 counties.

Structural Estimation Data

The sample includes cities from all over the United States, and although the range of population sizes in this sample of cities goes from 51,000 to 5.6 million (1920 numbers), admittedly this is a sample of urban communities. Of course, this is mainly due to the availability for the homicide rate data, which were reported on a population basis and for cities only. It is important to stress that the results should be seen as the effects of Prohibition in the most urbanized parts of the American society.

Section 6 mentioned that in spite of being a dynamic model, Maximum Likelihood estimation was not subject to an initial conditions problem. The careful reader might have noticed that this requires the sample to cover years under no Prohibition and under Prohibition, while a few states were already under Prohibition before 1911. Given the timing of the adoption of Prohibition across states (see Figure 3), and the data availability, for Nashville and Memphis in Tennessee, Atlanta in Georgia, and Kansas City in Kansas, the sample covers Prohibition years exclusively. These three states officially adopted Prohibition in 1909, 1908, and 1880, respectively. Nevertheless, following the historical account on Prohibition in Tennessee, I code the cities in this state as being under Prohibition only starting in 1914. As mentioned in footnote 3, the governor of Tennessee decided not to enforce the constitutional amendment enacted in 1909, and Prohibition only was enforced after the new Republican governor took office.[16]

Although for Atlanta, GA, and Kansas City, KS, the drunkenness arrest data also show a fall only in 1917 (when war-time Prohibition was adopted), suggesting little actual law enforcement of the state Prohibition laws (Atlanta's arrests fall from 18.4 to 12.2 between 1916 and 1917), there is no clear evidence that the laws were actually not being enforced. Instead of specifying a distribution for the unobserved homicide rate prior to 1911 for these three cities, I estimate the structural model assuming they enter Prohibition in 1917, and check the robustness of the model to excluding them from the estimation altogether.

[16]The fact that Prohibition did not take place in Tennessee before 1914 can be corroborated directly by looking at the drunkenness arrests data. For example, this variable falls from 17.5 to 8.9 per 1,000 people between 1913 and 1914 in Knoxville. Hilary House, Nashville's mayor at the time, even explicitly "acknowledged before the world that the state-wide Prohibition law is violated in Nashville…with knowledge and consent of the great majority of the people." (Isaac (1965, p. 174).)

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