APPENDIX A: LEVELS AND QUARTICS

Table 2 reports our baseline results for aggregated causes of death, using logged mortality rates as the dependent variable. In Tables A1 and A2, we consider the same specifications but instead substitute mortality rates in levels and quartic roots as the dependent variables, respectively. The quartic root preserves zeros in the data like our preferred dependent variable in which we add +1 to the count of deaths before first normalizing by city population and then taking the log. But the quartic root does allow us to avoid the admittedly arbitrary step of adding +1 to the count of deaths.

Starting with Columns 1 through 4 in both tables, we see statistically significant results are only consistently associated with acute, alcohol-related causes of death (auto accidents, homicide, other accidents, and suicide). Just as in Table 2, we also see no statistically significant results attached to either chronic, alcohol-related causes of death (cirrhosis, heart disease, and nephritis) or potentially alcohol-related causes of death (cancer, cerebral hemorrhage, cerebrospinal meningitis, influenza/pneumonia, malaria, and tuberculosis).

For non-alcohol-related causes of death (all other causes, appendicitis, diabetes mellitus, diphtheria, hernia/internal obstruction, other puerperal causes, puerperal septicemia, rheumatism/gout, scarlet fever, syphilis, typhoid/paratyphoid, and whooping cough), we see a small effect for *wet in initial year* in both Tables A1 and A2.

Tables A3 and A4 replicate the same type of exercise but do so in a consideration of the dis-aggregated acute causes of death (that is, automobile accidents, homicide, other accidents, and suicide). The same broad pattern emerges as in Table 3, namely that there are consistent, significant negative effects of repeal for homicides and other accidents but not for other acute causes of death. The implied proportionate effects are similar as well: Table A3 suggests that *wet in subsequent years* was associated with a 11.0% decline in other accidents (when evaluated at the sample mean) while Table A4 suggests that *wet in subsequent years* was associated with a 11.6% decline in the same. This compares with an equivalent figure of 10.1% reported in the main text.

Table A1: Repeal's effect on aggregated causes of death, in levels

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.034	0.091	-0.030	0.084
	(0.036)	(0.051)	(0.061)	(0.057)
	[0.93]	[1.77]	[0.49]	[1.48]
Dryish in subsequent years	**-0.186**	0.032	-0.136	0.017
	(0.078)	(0.107)	(0.128)	(0.111)
	[2.39]	[0.30]	[1.06]	[0.15]
Wet in initial year	**-0.078**	-0.033	0.003	**-0.075**
	(0.024)	(0.037)	(0.044)	**(0.036)**
	[3.24]	[0.89]	[0.06]	**[2.05]**
Wet in subsequent years	**-0.092**	-0.067	0.106	0.017
	(0.033)	(0.062)	(0.070)	(0.062)
	[2.77]	[1.07]	[1.51]	[0.28]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of mortality rates in levels, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table A2: Repeal's effect on aggregated causes of death, in quartic roots

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.006	0.008	-0.005	0.009
	(0.007)	(0.005)	(0.005)	(0.006)
	[0.93]	[1.68]	[0.92]	[1.65]
Dryish in subsequent years	**-0.027**	0.000	-0.013	0.006
	(0.013)	(0.010)	(0.010)	(0.009)
	[2.06]	[0.00]	[1.32]	[0.60]
Wet in initial year	**-0.014**	-0.003	-0.002	**-0.008**
	(0.005)	(0.003)	(0.004)	**(0.004)**
	[3.01]	[0.89]	[0.52]	**[2.21]**
Wet in subsequent years	**-0.019**	-0.006	0.005	0.001
	(0.007)	(0.006)	(0.006)	(0.006)
	[2.83]	[0.99]	[0.90]	[0.09]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of quartic root of mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table A3: Repeal's effect on acute causes of death, in levels

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	-0.001	-0.002	-0.035	0.004
	(0.017)	(0.012)	(0.032)	(0.011)
	[0.05]	[0.16]	[1.10]	[0.39]
Dryish in subsequent years	-0.046	**-0.081**	-0.067	0.008
	(0.040)	**(0.026)**	(0.044)	(0.012)
	[1.15]	**[3.15]**	[1.55]	[0.71]
Wet in initial year	-0.006	**-0.018**	**-0.058**	0.005
	(0.010)	**(0.007)**	**(0.023)**	(0.007)
	[0.61]	**[2.70]**	**[2.58]**	[0.65]
Wet in subsequent years	-0.026	-0.018	**-0.065**	0.016
	(0.017)	(0.010)	**(0.027)**	(0.011)
	[1.54]	[1.68]	**[2.36]**	[1.38]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of mortality rates in levels, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table A4: Repeal's effect on acute causes of death, in quartic roots

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	0.006	-0.016	-0.013	0.010
	(0.010)	(0.015)	(0.010)	(0.015)
	[0.63]	[1.04]	[1.34]	[0.69]
Dryish in subsequent years	-0.008	**-0.054**	-0.018	0.014
	(0.020)	**(0.026)**	(0.015)	(0.021)
	[0.40]	**[2.06]**	[1.23]	[0.68]
Wet in initial year	0.000	-0.019	**-0.021**	0.005
	(0.007)	(0.011)	**(0.008)**	(0.009)
	[0.03]	[1.78]	**[2.71]**	[0.53]
Wet in subsequent years	-0.010	**-0.039**	**-0.025**	0.018
	(0.011)	**(0.018)**	**(0.010)**	(0.013)
	[0.93]	**[2.13]**	**[2.64]**	[1.35]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of quartic root of mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

APPENDIX B: DIFFERENTIAL IMPACTS OF PROHIBITION'S REPEAL

As mentioned in the text, an attractive feature of the mortality data is that they are further broken down for cities in which the non-white population numbered at least 10,000 and/or represented at least 10% of the total population. By and large, non-white in this context can be interpreted as black in that the latter represented approximately 95% of the non-white population at the time (Gibson and Jung, 2002). However, there was significant variation in this ratio across the US, particularly in border states like California and Texas.

Here, we limit our attention to cities with overwhelmingly black populations due to inconsistencies in how non-white populations are classified in the sample period. In particular, the Mexican population of the United States went from being tabulated as non-white to white in between the 1930 and 1940 Censuses:

> Until 1930, Mexicans, the dominant Hispanic national origin group, had been classified as white. A "Mexican" race category was added in the 1930 census, following a rise in immigration that dated to the Mexican Revolution in 1910. But Mexican Americans (helped by the Mexican government) lobbied successfully to eliminate it in the 1940 census and revert to being classified as white, which gave them more legal rights and privileges. (Pew Research Center, 2015, p. 26)

Consequently, this introduces "an element of incomparability into the race-specific rates [reported in the *Vital Statistics*] for the years 1932 to 1934" (Linder and Grove, 1947, p. 103). Our work-around then comes in only considering cities both for which non-white deaths are reported as a separate category and which are located in states in which black people represented >90% of the non-white population. Thus, we collected data on causes of death for non-white people in 243 cities and matched these with black population counts for 135 cities in 1930 taken from US Bureau of the Census (1935). Of the 135 cities in this restricted sample, 121 are in the southern United States.

Table B1 presents the results from considering the respective aggregated mortality rates for black people, using the restricted sample. A qualitatively similar set of results emerges as in the main text. For related, potentially related, and non-related causes of black mortality, the repeal of prohibition does not seem to have left any trace. And for acute causes of death alone, we find significant effects. However, the repeal of prohibition was associated with a reduction in deaths by acute causes for *wet in subsequent years* which was roughly 3x larger than in Table 2 (and roughly 2x for *dryish in subsequent years*). At first glance then, these results seem to indicate potentially large differential impacts of prohibition's repeal on the basis of race (albeit the respective coefficients in Table B1 are not significantly different from their counterparts in Table 2).

Table B2 explores this possibility in more depth. Here, we consider white mortality rates for aggregated causes of death in the same restricted sample of 135 cities with significant black populations. The point estimates for acute causes are very close in value to those attached to black mortality in the same cities. Consequently, the respective coefficients in Table B2 are not significantly different from their counterparts in Table B1. We therefore cannot then rule out the possibility that the differential outcomes reported in Table B1 are driven by sample selection issues. In particular, 89.6% of the cities in the restricted sample are in the southern United States, so any differential outcomes on the basis of race more likely reflect underlying heterogeneity in outcomes in that particular region.

Table B3 confirms this suspicion. It returns to a consideration of total (non-white plus white) mortality by aggregated causes of death. However, it differs from Table 2 by only considering the full sample of cities less the restricted sample of cities with significant black populations used in Tables B1 and B2 (that is, n = 946 – 135 = 811). In this instance, for acute, related, potentially related, and non-related causes of total mortality alike, we see no significant effects associated with prohibition's repeal (apart from the coefficient on related causes of death for *dryish in initial year*). The point estimate for *wet in subsequent years* is, however, significantly different from both those reported in Table B1 and B2. Thus, the headline results reported in the main text may be driven by effects in a relatively small subset of cities.

Tables B4, B5, and B6 report equivalent results for dis-aggregated acute causes of death by respectively considering black mortality in the restricted sample, white mortality in the restricted sample, and total mortality in the full-less-restricted sample. The results suggest that: (1) there are no significant differences in the point estimates for the black and white populations in the restricted sample (Tables B4 and B5); and (2) there may be some reason to believe that the results for acute causes of death reported in the main text may also be driven by effects in a relatively small subset of cities (Table B6).

Table B1: Aggregated causes of death, black population, restricted sample

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.016	-0.011	-0.007	-0.003
	(0.061)	(0.035)	(0.031)	(0.027)
	[0.26]	[0.32]	[0.24]	[0.09]
Dryish in subsequent years	**-0.178**	-0.032	-0.026	-0.014
	(0.074)	(0.056)	(0.049)	(0.048)
	[2.41]	[0.57]	[0.53]	[0.29]
Wet in initial year	-0.091	-0.001	0.001	0.021
	(0.053)	(0.044)	(0.038)	(0.046)
	[1.72]	[0.03]	[0.03]	[0.45]
Wet in subsequent years	**-0.216**	-0.026	0.059	0.034
	(0.083)	(0.079)	(0.059)	(0.079)
	[2.61]	[0.33]	[1.01]	[0.44]
N of observations	540	540	540	540
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged black mortality rates, weighted by black city population. Standard errors are in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table B2: Aggregated causes of death, white population, restricted sample

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	**-0.076**	-0.026	-0.024	0.009
	(0.038)	(0.024)	(0.026)	(0.025)
	[1.99]	[1.06]	[0.92]	[0.36]
Dryish in subsequent years	**-0.130**	-0.034	-0.038	0.062
	(0.060)	(0.047)	(0.035)	(0.043)
	[2.16]	[0.72]	[1.07]	[1.46]
Wet in initial year	**-0.099**	-0.007	-0.008	-0.013
	(0.041)	(0.021)	(0.024)	(0.024)
	[2.39]	[0.33]	[0.33]	[0.53]
Wet in subsequent years	**-0.152**	-0.023	0.023	0.010
	(0.051)	(0.032)	(0.033)	(0.048)
	[3.00]	[0.72]	[0.70]	[0.20]
N of observations	540	540	540	540
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged white mortality rates, weighted by white city population. Standard errors are in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table B3: Aggregated causes of death, total population, full-less-restricted sample

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.000	**0.055**	-0.024	0.042
	(0.034)	**(0.019)**	(0.019)	(0.025)
	[0.00]	**[2.84]**	[1.21]	[1.65]
Dryish in subsequent years	-0.027	0.042	-0.051	-0.021
	(0.056)	(0.036)	(0.035)	(0.036)
	[0.47]	[1.18]	[1.48]	[0.58]
Wet in initial year	-0.020	0.015	0.001	-0.018
	(0.019)	(0.010)	(0.012)	(0.013)
	[1.07]	[1.40]	[0.08]	[1.41]
Wet in subsequent years	-0.025	0.010	0.010	0.007
	(0.030)	(0.020)	(0.021)	(0.023)
	[0.85]	[0.53]	[0.49]	[0.30]
N of observations	3,244	3,244	3,244	3,244
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table B4: Acute causes of death, black population, restricted sample

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	-0.089	0.022	-0.042	0.235
	(0.144)	(0.062)	(0.100)	(0.175)
	[0.62]	[0.36]	[0.41]	[1.35]
Dryish in subsequent years	-0.399	-0.113	-0.108	0.225
	(0.202)	(0.094)	(0.137)	(0.344)
	[1.97]	[1.20]	[0.79]	[0.65]
Wet in initial year	-0.066	-0.057	-0.134	-0.180
	(0.136)	(0.078)	(0.102)	(0.147)
	[0.48]	[0.72]	[1.31]	[1.22]
Wet in subsequent years	-0.178	-0.140	**-0.346**	0.087
	(0.185)	(0.139)	**(0.152)**	(0.227)
	[0.96]	[1.01]	**[2.27]**	[0.39]
N of observations	540	540	540	540
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged black mortality rates, weighted by black city population. Standard errors are in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table B5: Acute causes of death, white population, restricted sample

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	-0.013	0.069	**-0.157**	0.016
	(0.071)	(0.175)	**(0.071)**	(0.079)
	[0.19]	[0.40]	**[2.21]**	[0.21]
Dryish in subsequent years	-0.028	**-0.353**	**-0.164**	0.001
	(0.123)	**(0.157)**	**(0.070)**	(0.076)
	[]0.23]	**[2.24]**	**[2.34]**	[0.01]
Wet in initial year	-0.053	0.060	-0.171	-0.026
	(0.053)	(0.125)	(0.090)	(0.093)
	[1.01]	[0.48]	[1.89]	[0.27]
Wet in subsequent years	-0.125	0.125	**-0.289**	0.050
	(0.092)	(0.161)	**(0.105)**	(0.174)
	[1.37]	[0.77]	**[2.75]**	[0.29]
N of observations	540	540	540	540
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged white mortality rates, weighted by white city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table B6: Acute causes of death, total population, full-less-restricted sample

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	0.060	-0.072	-0.015	0.056
	(0.053)	(0.086)	(0.053)	(0.085)
	[1.12]	[0.84]	[0.27]	[0.66]
Dryish in subsequent years	0.027	-0.123	-0.021	0.151
	(0.099)	(0.125)	(0.084)	(0.115)
	[0.27]	[0.98]	[0.25]	[1.31]
Wet in initial year	0.016	-0.049	-0.051	0.057
	(0.035)	(0.052)	(0.028)	(0.041)
	[0.46]	[0.94]	[1.83]	[1.39]
Wet in subsequent years	-0.009	-0.141	-0.052	0.084
	(0.054)	(0.090)	(0.045)	(0.067)
	[0.17]	[1.56]	[1.16]	[1.27]
N of observations	3,244	3,244	3,244	3,244
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

APPENDIX C: PRE-TRENDS FOR ACUTE MORTALITY RATES

As our econometric strategy is analogous to difference-in-difference, the key assumption in estimation is that treated counties would have followed the same time trend as untreated counties had they themselves not been treated. Under this common-trends assumption, the difference in the rates of change between treated and untreated counties equals the true treatment effect. One way to gauge the validity of this assumption is to compare the time trend before any treatments occur (that is, the pre-trend) for counties that are eventually treated with the pre-trend of counties that are never treated.

Figures C1 through C5 tracks raw mortality rates (expressed as deaths per 1,000 population) for one aggregated and four dis-aggregated causes of death in the period from 1928 to 1932 (acute, automobile accidents, homicide, other accidents, and suicide). We also only use the cities in which we observe vital statistics consistently in this period. Because of changes in data collection from year to year, our final observation count is 360 as opposed to the 946 cities used throughout in our main analysis. We employ two mutually exclusive categories, always dry and ever wet. Always dry cities are those which remain dry throughout the post-repeal period. Ever wet cities are those which allow for legal alcohol sales at some point in the post-repeal period. Thus, the composition of cities is held constant.

Starting with Figure C1, we observe a general, but moderate decline in mortality rates from acute causes of death for the two city types, and the general ordering is strongly preserved when considering the years between 1928 and 1932: acute mortality rates are highest for always-dry cities and lowest for ever-wet cities. In these years, they also exhibit highly similar pre-trends. In other words, Figure C1 suggests that the common-trend assumption holds for this restricted sample of cities, thereby validating the results reported in the text.

Similarly, we find reassuring evidence related to the common-trend assumption for homicide and other accidents in Figures C3 and C4, respectively. In both instances, always-dry cities both begin and end with higher mortality rates than ever-wet cities. Moreover, the difference between the two is roughly constant. This is reassuring, given that these were the two components of acute sources of death for which consistently significant effects were found in the text. Turning to the other two components of acute sources of death for which consistently insignificant effects were found in the text, the implications of Figures C2 and C5 for automobile accidents and suicide, respectively, are less clear. For automobile accidents, always-dry cities have consistently higher mortality rates than ever-wet cities, but the difference varies over time. Likewise, for suicide, ever-wet cities have consistently higher mortality rates than always-dry cities, but the difference narrows considerably over time.

Figure C1: Pre-trends in acute sources of urban mortality, 1928-1932

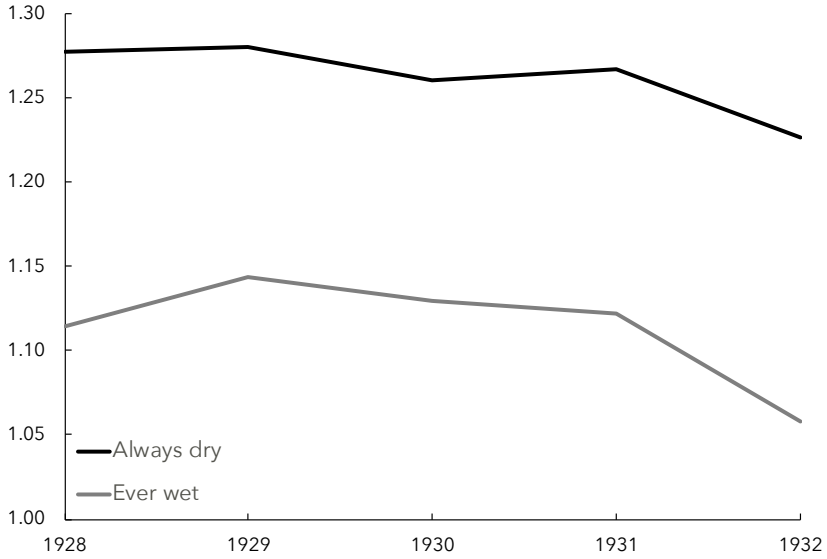

Figure C1 uses cities for which there is a consistent sample from 1928 to 1932 (n = 360). Always dry cities remain dry for the entire post-repeal period. Ever wet cities allow for legal alcohol sales at some point in the post-repeal period. Both series expressed as deaths per 1,000 population.

Figure C2: Pre-trends in deaths due to automobile accidents, 1928-1932

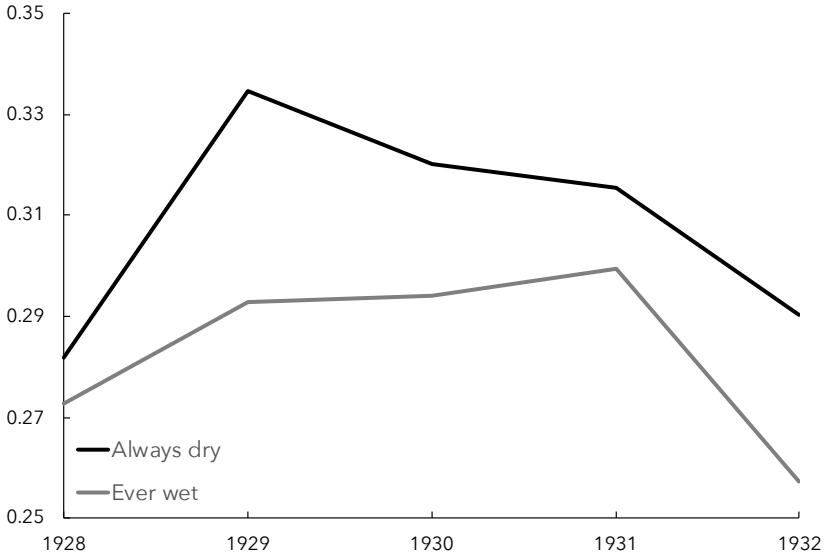

Figure C2 uses cities for which there is a consistent sample from 1928 to 1932 (n = 360). Always dry cities remain dry for the entire post-repeal period. Ever wet cities allow for legal alcohol sales at some point in the post-repeal period. Both series expressed as deaths per 1,000 population.

Figure C3: Pre-trends in deaths due to homicide, 1928-1932

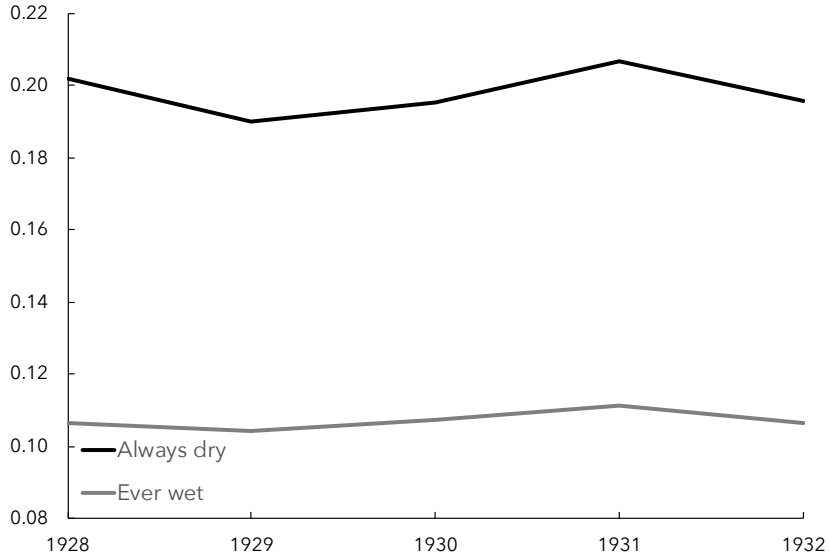

Figure C3 uses cities for which there is a consistent sample from 1928 to 1932 (n = 360). Always dry cities remain dry for the entire post-repeal period. Ever wet cities allow for legal alcohol sales at some point in the post-repeal period. Both series expressed as deaths per 1,000 population.

Figure C4: Pre-trends in deaths due to other accidents, 1928-1932

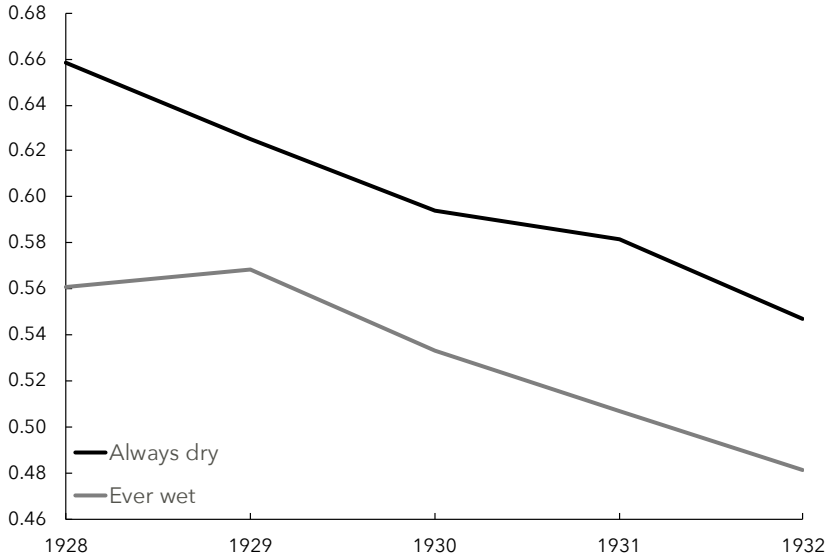

Figure C4 uses cities for which there is a consistent sample from 1928 to 1932 (n = 360). Always dry cities remain dry for the entire post-repeal period. Ever wet cities allow for legal alcohol sales at some point in the post-repeal period. Both series expressed as deaths per 1,000 population.

Figure C5: Pre-trends in deaths due to suicide, 1928-1932

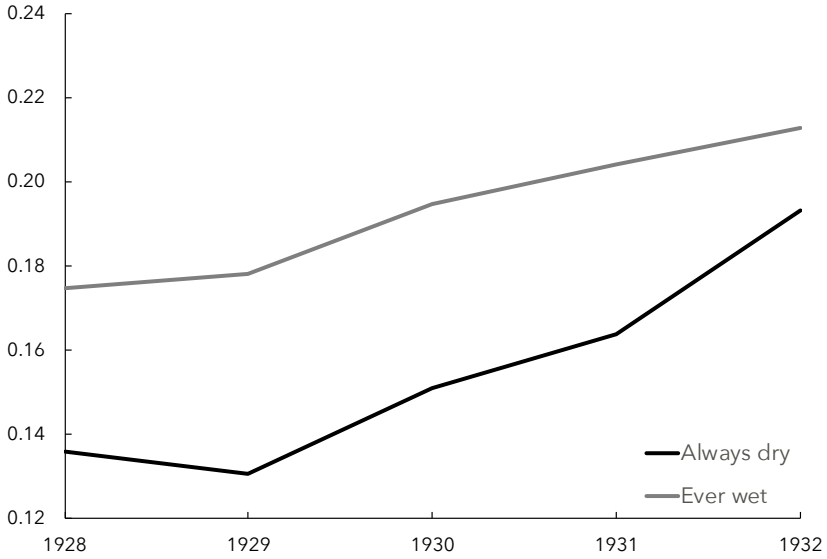

Figure C5 uses cities for which there is a consistent sample from 1928 to 1932 (n = 360). Always dry cities remain dry for the entire post-repeal period. Ever wet cities allow for legal alcohol sales at some point in the post-repeal period. Both series expressed as deaths per 1,000 population.

APPENDIX D: RESULTS FOR ALL OTHER DISAGGREGATED CAUSES OF DEATH

In the text, we closely consider the four dis-aggregated acute causes of death, motivated by the results in Tables 2 which detects consistently signed and significant effects for their summation.

In the interests of space, the text thereby neglects a separate consideration:
(i) of the three dis-aggregated chronic, alcohol-related causes of death;
(ii) of the six potentially alcohol-related causes of death; and
(iii) of the 12 non-alcohol-related causes of death.

Here, we remedy this situation and consider each of the 21 other dis-aggregated causes of death in turn. In sum, these results strongly support our decision to focus on acute causes of death.

For chronic, alcohol-related causes, there is exactly one statistically significant coefficient (that attached to *dryish in initial year* for heart disease) across the four parameters of interest and the three dependent variables considered (that is, one out of 12 coefficients of interest). These results are displayed in Table D1.

For potentially alcohol-related causes, there are exactly zero statistically significant coefficients across the four parameters of interest and the six dependent variables considered (that is, zero out of 24 coefficients of interest). These results are displayed in Table D2 below.

And for non-alcohol-related causes, there are five statistically significant coefficients across the four parameters of interest and the 12 dependent variables considered (that is, five out of 48 coefficients of interest). These results are displayed in Tables D3 and D4 below.

In sum, six statistically significant – but potentially spurious – coefficients out of 84 coefficients of interest (or 7.14%) is roughly to be expected when using a 5% level of significance.

Table D1: Repeal's effect on related causes of death

	(1)	(2)	(3)
	Cirrhosis	Heart disease	Nephritis
Dryish in initial year	0.031	**0.035**	-0.022
	(0.072)	**(0.018)**	(0.038)
	[0.43]	**[1.97]**	[0.58]
Dryish in subsequent years	-0.213	0.002	0.004
	(0.129)	(0.034)	(0.043)
	[1.65]	[0.06]	[0.10]
Wet in initial year	0.033	-0.014	0.008
	(0.050)	(0.012)	(0.022)
	[0.66]	[1.23]	[0.34]
Wet in subsequent years	0.001	-0.028	0.024
	(0.077)	(0.019)	(0.034)
	[0.01]	[1.44]	[0.70]
N of observations	3,784	3,784	3,784
City & year fixed effects	X	X	X
County controls with linear trends	X	X	X
State linear trends	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table D2: Repeal's effect on potentially related causes of death

	(1)	(2)	(3)	(4)	(5)	(6)
	Cancer	Cerebral hemorrhage	Cerebrospinal meningitis	Influenza/ pneumonia	Malaria	Tuberculosis
Dryish in initial year	-0.036	0.019	0.131	-0.030	0.005	-0.065
	(0.021)	(0.029)	(0.104)	(0.036)	(0.094)	(0.044)
	[1.74]	[0.66]	[1.26]	[0.81]	[0.06]	[1.47]
Dryish in subsequent years	0.001	-0.072	-0.123	-0.064	0.048	-0.000
	(0.040)	(0.053)	(0.195)	(0.053)	(0.145)	(0.054)
	[0.01]	[1.37]	[0.63]	[1.20]	[0.33]	[0.01]
Wet in initial year	-0.002	-0.033	0.113	-0.041	-0.025	0.015
	(0.014)	(0.020)	(0.087)	(0.022)	(0.059)	(0.024)
	[0.15]	[1.68]	[1.30]	[1.86]	[0.43]	[0.64]
Wet in subsequent years	0.003	-0.038	0.110	0.018	-0.069	0.014
	(0.024)	(0.030)	(0.138)	(0.034)	(0.064)	(0.039)
	[0.12]	[1.25]	[0.80]	[0.55]	[1.08]	[0.37]
N of observations	3,784	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X	X
County controls with linear trends	X	X	X	X	X	X
State linear trends	X	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table D3: Repeal's effect on non-related causes of death, part 1

	(1)	(2)	(3)	(4)	(5)	(6)
	All other causes	Appendicitis	Diabetes mellitus	Diphtheria	Hernia/ Internal obstruction	Other puerperal causes
Dryish in initial year	0.028	0.063	-0.036	0.180	0.015	-0.043
	(0.020)	(0.062)	(0.061)	(0.114)	(0.063)	(0.071)
	[1.38]	[1.01]	[0.58]	[1.58]	[0.24]	[0.60]
Dryish in subsequent years	0.012	0.068	0.062	0.166	-0.010	-0.164
	(0.029)	(0.076)	(0.061)	(0.170)	(0.069)	(0.101)
	[0.40]	[0.88]	[1.02]	[0.97]	[0.14]	[1.62]
Wet in initial year	-0.006	**-0.074**	-0.055	0.016	-0.036	-0.065
	(0.014)	**(0.030)**	(0.031)	(0.071)	(0.033)	(0.045)
	[0.41]	**[2.47]**	[1.76]	[0.22]	[1.11]	[1.44]
Wet in subsequent years	0.020	**-0.093**	-0.006	-0.069	0.029	-0.011
	(0.023)	**(0.046)**	(0.046)	(0.128)	(0.057)	(0.074)
	[0.86]	**[2.04]**	[0.12]	[0.54]	[0.51]	[0.15]
N of observations	3,784	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X	X
County controls with linear trends	X	X	X	X	X	X
State linear trends	X	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table D4: Repeal's effect on non-related causes of death, part 2

	(7)	(8)	(9)	(10)	(11)	(12)
	Puerperal septicemia	Rheumatism/ gout	Scarlet fever	Syphilis	Typhoid/ paratyphoid	Whooping cough
Dryish in initial year	-0.007	-0.049	-0.032	-0.030	0.102	-0.136
	(0.085)	(0.099)	(0.122)	(0.064)	(0.090)	(0.132)
	[0.09]	[0.49]	[0.26]	[0.47]	[1.14]	[1.03]
Dryish in subsequent years	**0.233**	-0.085	0.293	-0.080	-0.029	0.137
	(0.115)	(0.167)	(0.166)	(0.091)	(0.100)	(0.189)
	[2.03]	[0.51]	[1.77]	[0.88]	[0.29]	[0.73]
Wet in initial year	-0.047	-0.064	0.009	-0.032	-0.057	**-0.522**
	(0.057)	(0.069)	(0.091)	(0.045)	(0.096)	**(0.094)**
	[0.83]	[0.92]	[0.10]	[0.70]	[0.60]	**[5.56]**
Wet in subsequent years	-0.035	-0.063	0.047	-0.019	-0.065	**-0.506**
	(0.093)	(0.097)	(0.152)	(0.071)	(0.138)	**(0.152)**
	[0.38]	[0.65]	[0.31]	[0.27]	[0.47]	**[3.33]**
N of observations	3,784	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X	X
County controls with linear trends	X	X	X	X	X	X
State linear trends	X	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

APPENDIX E: ROBUSTNESS, VARIATIONS ON CITY AND STATE EFFECTS

In the text, Tables 6, 7, and 8 collate the results from various robustness exercises for acute causes of death, homicide, and other accidents, respectively. Here, we explore the full set of robustness results related to variations on the state fixed effects used as controls. In particular, we consider the substitution of state-by-year fixed effects and city linear trends for state linear trends, in turn.

Table E1 introduces state-by-year fixed effects into our baseline specification for the four aggregated mortality rates (acute, related, potentially related, and non-related) for the total population of cities. For acute causes, *dryish in subsequent years* and *wet in subsequent years* both register as negative and of a similar magnitude as the coefficients reported in the main text. And while the former is statistically significant, the latter is not. Again, question how much interpretive weight to place on the non-significance of the results for *wet in subsequent years* in particular, as the category of wet includes both counties which opt for wet status and counties within states which opt for wet status. None of the other aggregated mortality rates seem to have been materially affected by this substitution. Table E2 introduces state-by-year fixed effects into our baseline specification for the four acute causes of death (automobile accidents, homicide, other accidents, and suicide) for the total population of cities. In this case, the results for homicide and other accidents are consistent in sign and size relative to the baseline specification, but are marginally statistically insignificant.

Finally, Tables E3 and E4 report the results from the introduction of city linear trends (instead of state linear trends) into our baseline specification for the four aggregated mortality rates and the four acute causes of death, respectively. We exercise extreme caution in interpreting these results and only present them for the sake of completeness. We note that, in general, this particular specification gives rise to problems in estimation. The high number of parameters to be estimated (nearly 2,000), the high degree of collinearity among them, and the large number of clusters leads to potentially singular VCV matrices and instability in the value of point estimates depending on which program and/or routine is used. Thus, we choose to heavily discount these results but are reassured by the close correspondence in between the magnitude of the effects for *wet in subsequent years* reported here and in the main text.

Table E1: Repeal's effect on aggregated causes of death, state-by-year fixed effects

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.027	0.026	-0.023	0.033
	(0.033)	(0.017)	(0.020)	(0.020)
	[0.80]	[1.50]	[1.13]	[1.67]
Dryish in subsequent years	**-0.116**	-0.000	-0.052	0.021
	(0.053)	(0.031)	(0.032)	(0.026)
	[2.19]	[0.01]	[1.60]	[0.80]
Wet in initial year	-0.041	0.007	-0.045	0.006
	(0.060)	(0.050)	(0.041)	(0.046)
	[0.69]	[0.14]	[1.11]	[0.12]
Wet in subsequent years	-0.123	0.010	-0.035	0.002
	(0.069)	(0.048)	(0.047)	(0.047)
	[1.78]	[0.22]	[0.74]	[0.05]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State-year fixed effects	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table E2: Repeal's effect on acute causes of death, state-by-year fixed effects

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	-0.036	0.006	-0.029	0.013
	(0.047)	(0.061)	(0.046)	(0.068)
	[0.77]	[0.10]	[0.62]	[0.19]
Dryish in subsequent years	-0.079	**-0.205**	-0.097	-0.003
	(0.096)	**(0.091)**	(0.069)	(0.084)
	[0.82]	**[2.26]**	[1.41]	[0.03]
Wet in initial year	0.055	-0.076	-0.066	-0.028
	(0.109)	(0.152)	(0.102)	(0.157)
	[0.50]	[0.50]	[0.65]	[0.18]
Wet in subsequent years	-0.074	-0.237	-0.161	-0.093
	(0.126)	(0.153)	(0.107)	(0.123)
	[0.59]	[1.55]	[1.50]	[0.75]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State-year fixed effects	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table E3: Repeal's effect on aggregated causes of death, city linear trends

	(1)	(2)	(3)	(4)
	Acute	Related	Potentially related	Non-related
Dryish in initial year	-0.005	0.020	-0.020	0.020
	(0.031)	(0.015)	(0.019)	(0.025)
	[0.15]	[1.35]	[1.06]	[0.83]
Dryish in subsequent years	-0.020	-0.006	-0.050	-0.018
	(0.075)	(0.048)	(0.037)	(0.056)
	[0.26]	[0.12]	[1.34]	[0.32]
Wet in initial year	**-0.046**	-0.010	-0.005	-0.025
	(0.021)	(0.012)	(0.013)	(0.013)
	[2.14]	[0.84]	[0.38]	[1.89]
Wet in subsequent years	**-0.061**	-0.017	0.017	-0.001
	(0.031)	(0.020)	(0.020)	(0.024)
	[1.99]	[0.82]	[0.85]	[0.97]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
City linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table E4: Repeal's effect on acute causes of death, city linear trends

	(1)	(2)	(3)	(4)
	Automobile accidents	Homicide	Other accidents	Suicide
Dryish in initial year	0.024	-0.023	-0.048	0.104
	(0.056)	(0.084)	(0.054)	(0.077)
	[0.42]	[0.28]	[0.89]	[1.36]
Dryish in subsequent years	-0.003	-0.144	-0.056	0.305
	(0.132)	(0.152)	(0.142)	(0.196)
	[0.02]	[0.95]	[0.39]	[1.56]
Wet in initial year	-0.003	-0.065	**-0.081**	0.038
	(0.033)	(0.051)	**(0.040)**	(0.044)
	[0.08]	[1.26]	**[2.06]**	[0.87]
Wet in subsequent years	-0.047	-0.143	**-0.099**	0.090
	(0.055)	(0.089)	**(0.050)**	(0.072)
	[0.85]	[1.60]	**[1.97]**	[1.25]
N of observations	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
City linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

APPENDIX F: ROBUSTNESS, VARIATIONS ON THRESHOLD FOR DRYISH

In the text, Tables 6, 7, and 8 collate the results from various robustness exercises for acute causes of death, homicide, and other accidents, respectively. Here, we explore the full set of robustness results related to variations in how the threshold for dryish is defined. In particular, we compare the results from our baseline specification where dryish cities are dry cities within 30 km from legal alcohol to those in which this distance is 10, 50, 70, or 90 km.

Table F1 introduces these other thresholds for dryish into our baseline specification for acute causes of death in the total population of cities. Column 1 of Table F1 corresponds with Column 2 of Table 2 (our benchmark specification). Naturally, there is a very high degree of correspondence among all of the results for *wet in subsequent years* in Table F1 as, regardless of what threshold is used, the number of wet cities is large and the number of dryish cities is small. For *dryish in subsequent years*, there is no distinguishable difference across Columns 1, 2, and 3. The effect continues in relative magnitude but not statistical significance once the threshold reaches 70 km. The effect is roughly halved and again statistically insignificant once the threshold reaches 90 km.

Table F2 introduces these other threshold for dryish into our baseline specification for homicide in the total population of cities. Column 1 of Table F2 corresponds with Column 2 of Table 4 (our benchmark specification). With respect to *wet in subsequent years* in Table F2, the magnitude of the effect remains virtually the same across specifications in Columns 2 through 5 but becomes marginally statistically insignificant as the threshold increases in size from 30 km. For *dryish in subsequent years*, the magnitude of the effect diminishes once we reach 90 km in Columns 2 through 5, and in only one other specification is statistical significance achieved. Thus, there is some evidence speaking to the robustness of *wet in subsequent years* for homicide as it relates to other thresholds for dryish, but our benchmark estimate on dryish seems somewhat fragile.

Table F3 introduces these other threshold for dryish into our baseline specification for other accidents in the total population of cities. Column 1 of Table F3 corresponds with Column 3 of Table 4 (our benchmark specification). With respect to *wet in subsequent years* in Table F3, the magnitude of the effect remains virtually the same across specifications and maintains statistical significance in Columns 2 through 5. For *dryish in subsequent years*, the magnitude of the effect remains roughly the same in Columns 2 through 5, skirts the margins of statistical significance in Columns 3 through 5, only becoming statistically significant with a threshold of 10 km in Column 2. Thus, there is some evidence speaking to the robustness of *wet in subsequent years* for other accidents as it relates to other thresholds for dryish, but again, our benchmark estimate on dryish seems somewhat fragile. In sum, these and other results lead us to down weight *dryish in subsequent years* as a headline result for the paper.

Table F1: Repeal's effect on acute causes of death, other thresholds for dryish

	(1)	(2)	(3)	(4)	(5)
	Acute (Dryish <30 km)	Acute (Dryish <10 km)	Acute (Dryish <50 km)	Acute (Dryish <70 km)	Acute (Dryish <90 km)
Dryish in initial year	-0.023	0.005	-0.029	-0.022	-0.020
	(0.026)	(0.027)	(0.024)	(0.023)	(0.022)
	[0.90]	[0.20]	[1.20]	[0.99]	[0.90]
Dryish in subsequent years	-0.089	**-0.100**	**-0.084**	-0.067	-0.040
	(0.046)	**(0.047)**	**(0.037)**	(0.039)	(0.039)
	[1.92]	**[2.11]**	**[2.23]**	[1.71]	[1.02]
Wet in initial year	**-0.049**	**-0.043**	**-0.051**	**-0.049**	**-0.050**
	(0.018)	**(0.018)**	**(0.018)**	**(0.018)**	**(0.018)**
	[2.76]	**[2.40]**	**[2.85]**	**[2.73]**	**[2.71]**
Wet in subsequent years	**-0.066**	**-0.058**	**-0.068**	**-0.066**	**-0.066**
	(0.025)	**(0.025)**	**(0.025)**	**(0.025)**	**(0.026)**
	[2.62]	**[2.30]**	**[2.70]**	**[2.60]**	**[2.57]**
N of observations	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X
County controls with linear trends	X	X	X	X	X
State linear trends	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table F2: Repeal's effect on homicide, other thresholds for dryish

	(1)	(2)	(3)	(4)	(5)
	Homicide (Dryish <30 km)	Homicide (Dryish <10 km)	Homicide (Dryish <50 km)	Homicide (Dryish <70 km)	Homicide (Dryish <90 km)
Dryish in initial year	-0.014	0.014	0.020	0.016	0.013
	(0.058)	(0.063)	(0.051)	(0.049)	(0.048)
	[0.24]	[0.23]	[0.40]	[0.33]	[0.28]
Dryish in subsequent years	**-0.191**	-0.105	**-0.141**	-0.117	-0.109
	(0.078)	(0.092)	**(0.068)**	(0.075)	(0.075)
	[2.44]	[1.13]	**[2.08]**	[1.56]	[1.46]
Wet in initial year	-0.065	-0.059	-0.057	-0.056	-0.056
	(0.042)	(0.041)	(0.042)	(0.043)	(0.043)
	[1.55]	[1.42]	[1.34]	[1.30]	[1.28]
Wet in subsequent years	**-0.147**	**-0.137**	-0.138	-0.136	-0.136
	(0.073)	**(0.072)**	(0.073)	(0.074)	(0.074)
	[2.03]	**[1.96]**	[1.88]	[1.85]	[1.83]
N of observations	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X
County controls with linear trends	X	X	X	X	X
State linear trends	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table F3: Repeal's effect on other accidents, other thresholds for dryish

	(1)	(2)	(3)	(4)	(5)
	Other accidents (Dryish <30 km)	Other accidents (Dryish <10 km)	Other accidents (Dryish <50 km)	Other accidents (Dryish <70 km)	Other accidents (Dryish <90 km)
Dryish in initial year	-0.057	-0.032	**-0.086**	**-0.081**	**-0.070**
	(0.042)	(0.046)	**(0.038)**	**(0.034)**	**(0.033)**
	[1.36]	[0.69]	**[2.24]**	**[2.35]**	**[2.12]**
Dryish in subsequent years	-0.076	**-0.122**	-0.096	-0.102	-0.089
	(0.065)	**(0.059)**	(0.060)	(0.056)	(0.052)
	[1.16]	**[2.05]**	[1.59]	[1.83]	[1.72]
Wet in initial year	**-0.082**	**-0.075**	**-0.091**	**-0.092**	**-0.091**
	(0.033)	**(0.032)**	**(0.033)**	**(0.033)**	**(0.034)**
	[2.54]	**[2.33]**	**[2.75]**	**[2.75]**	**[2.67]**
Wet in subsequent years	**-0.101**	**-0.092**	**-0.112**	**-0.114**	**-0.114**
	(0.041)	**(0.041)**	**(0.042)**	**(0.042)**	**(0.043)**
	[2.46]	**[2.27]**	**[2.68]**	**[2.70]**	**[2.64]**
N of observations	3,784	3,784	3,784	3,784	3,784
City & year fixed effects	X	X	X	X	X
County controls with linear trends	X	X	X	X	X
State linear trends	X	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

APPENDIX G: ROBUSTNESS, VARIATIONS ON SAMPLE

In the text, Tables 6, 7, and 8 collate the results from various robustness exercises for acute causes of death, homicide, and other accidents, respectively. Here, we explore the full set of robustness results related to variations on the sample used for estimation.

Table G1 introduces these other samples into our baseline specification for acute causes of death in the total population of cities. Column 1 of Table G1 corresponds with Column 2 of Table 2 (our benchmark specification). Column 2 of Table G1 expands the number of cities to include those with more than 400,000 inhabitants (n = 17) but retains the sample period from 1933 to 1936. Column 3 of Table G1 retains the original sample of cities but expands the sample period to 1939. Column 4 of Table G1 relaxes both of these constraints. For *wet in subsequent years*, there is no statistically distinguishable difference across Columns 1, 2, and 3. However, by relaxing both constraints on the sample as in Column 4, the coefficient's magnitude nearly doubles (albeit it is not significantly different from that of Column 1). For *dryish in subsequent years*, the effect appreciably declines in magnitude from Column 2 to Column 4 and is never statistically significant.

Table G2 introduces these other samples into our baseline specification for homicide in the total population of cities. Column 1 of Table G2 corresponds with Column 2 of Table 4 (our benchmark specification) while Columns 2, 3, and 4 respectively expand the number of cities, expand the sample period to 1939, and relax both of these constraints. With respect to *wet in subsequent years* in Table G2, there is no statistically distinguishable difference across Columns 1, 2, 3, and 4. For *dryish in subsequent years*, the effect persists in relative magnitude in Column 2 and is statistically significant. Turning to the results in Column 3 and 4, the coefficient is much smaller in magnitude and statistically insignificant. Cumulatively, this suggests that if there really is an effect associated with *dryish in subsequent years* than it is limited in its temporal scope.

Table G3 introduces these other samples into our baseline specification for other accidents in the total population of cities. Column 1 of Table G3 corresponds with Column 3 of Table 4 (our benchmark specification) while Columns 2, 3, and 4 respectively expand the number of cities, expand the sample period to 1939, and relax both of these constraints. With respect to *wet in subsequent years* in Table G2, there is no statistically distinguishable difference across Columns 1, 2, 3, and 4 (albeit the results in Column 2 are marginally statistically insignificant). For *dryish in subsequent years*, the effects dramatically decline in value in Columns 2 through 4. They also tend towards gross statistical insignificance. Again, these and other results lead us to down weight *dryish in subsequent years* as a headline result for the paper.

Table G1: Repeal's effect on acute causes of death, extensions of sample

	(1)	(2)	(3)	(4)
	Acute (<1937, <400K)	Acute (<1937, all cities)	Acute (all years, <400K)	Acute (all cities, all years)
Dryish in initial year	-0.023	0.020	-0.027	0.006
	(0.026)	(0.043)	(0.028)	(0.038)
	[0.90]	[0.46]	[0.96]	[0.15]
Dryish in subsequent years	-0.089	-0.061	-0.026	-0.021
	(0.046)	(0.048)	(0.032)	(0.034)
	[1.92]	[1.26]	[0.82]	[0.61]
Wet in initial year	**-0.049**	**-0.062**	**-0.057**	**-0.072**
	(0.018)	**(0.017)**	**(0.015)**	**(0.004)**
	[2.76]	**[3.58]**	**[3.68]**	**[4.55]**
Wet in subsequent years	**-0.066**	**-0.065**	**-0.102**	**-0.126**
	(0.025)	**(0.030)**	**(0.019)**	**(0.022)**
	[2.62]	**[2.14]**	**[5.31]**	**[5.69]**
N of observations	3,784	3,852	6,622	6,741
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table G2: Repeal's effect on homicide, extensions of sample

	(1)	(2)	(3)	(4)
	Homicide (<1937, <400K)	Homicide (<1937, all cities)	Homicide (all years, <400K)	Homicide (all cities, all years)
Dryish in initial year	-0.014	-0.019	-0.025	-0.045
	(0.058)	(0.046)	(0.054)	(0.046)
	[0.24]	[0.40]	[0.47]	[0.98]
Dryish in subsequent years	**-0.191**	**-0.196**	-0.033	-0.054
	(0.078)	**(0.075)**	(0.048)	(0.046)
	[2.44]	**[2.62]**	[0.69]	[1.16]
Wet in initial year	-0.065	**-0.100**	-0.047	**-0.096**
	(0.042)	**(0.042)**	(0.029)	**(0.030)**
	[1.55]	**[2.39]**	[1.62]	**[3.22]**
Wet in subsequent years	**-0.147**	**-0.143**	**-0.107**	**-0.145**
	(0.073)	**(0.065)**	**(0.041)**	**(0.038)**
	[2.03]	**[2.20]**	**[2.61]**	**[3.76]**
N of observations	3,784	3,852	6,622	6,741
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.

Table G3: Repeal's effect on other accidents, extensions of sample

	(1)	(2)	(3)	(4)
	Other accidents (<1937, <400K)	Other accidents (<1937, all cities)	Other accidents (all years, <400K)	Other accidents (all cities, all years)
Dryish in initial year	-0.057	0.037	-0.060	0.024
	(0.042)	(0.085)	(0.040)	(0.073)
	[1.36]	[0.44]	[1.50]	[0.33]
Dryish in subsequent years	-0.076	-0.015	-0.010	0.014
	(0.065)	(0.077)	(0.051)	(0.053)
	[1.16]	[0.20]	[0.19]	[0.27]
Wet in initial year	**-0.082**	**-0.087**	**-0.073**	**-0.075**
	(0.033)	**(0.032)**	**(0.025)**	**(0.024)**
	[2.54]	**[2.75]**	**[2.95]**	**[3.13]**
Wet in subsequent years	**-0.101**	-0.089	**-0.111**	**-0.110**
	(0.041)	(0.050)	**(0.025)**	**(0.026)**
	[2.46]	[1.79]	**[4.38]**	**[4.24]**
N of observations	3,784	3,852	6,622	6,741
City & year fixed effects	X	X	X	X
County controls with linear trends	X	X	X	X
State linear trends	X	X	X	X

Regression of logged mortality rates, weighted by city population. Standard errors in parentheses and clustered at the city level; t-statistics reported below standard errors in brackets. Figures in bold are significant at the 5% level.