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Short communication

In utero and childhood exposure to alcohol and old age mortality: Evidence from the temperance movement in the US☆

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ABSTRACT

Previous research suggests the relevance of in-utero insults and early-life circumstances for a wide array of life cycle outcomes. This research note joins this strand of studies by exploring the long-run mortality effects of in-utero and early-life exposure to alcohol accessibility. In so doing, we take advantage of the prohibition movement during the early part of the twentieth century that generated quasi-natural reductions in alcohol consumption. We use Social Security Administration Death Master Files linked to the full-count 1940 census and compare the longevity of male individuals exposed to the prohibition during in-utero and early childhood (1900–1930) as a result of statewide and federal alcohol ban to those wet counties after the law change to before. The results suggest an intent-to-treat effect of 0.17 years higher longevity as a result of prohibition. A back-of-an-envelope calculation suggests a minimum treatment-on-treated effect of 1.7 years impact. Furthermore, we show that these effects are not driven by other county-level demographic and socioeconomic changes, endogenous selection of births, and preexisting trends in the outcome. Our findings contribute to the growing body of research that explores the in-utero and childhood circumstances on long-term health outcomes.

1. Introduction

In an attempt to limit or eliminate the use of intoxicating liquor, communities initiated anti-alcohol movements starting in Saratoga, New York, in 1808 (Britannica, 2021). Following prohibition reforms in Georgia and Oklahoma in 1907, the so-called Temperance Movement intensified between 1900 and 1920 through a series of statewide prohibition laws. The Anti-Saloon campaigners ultimately reached their goal of decades of efforts in 1920 when Congress passed the Volstead Act as the Eighteenth Amendment (Amendment XVIII) of the US Constitution, which imposed a national prohibition of alcohol sale and consumption. Several studies show that the Temperance Movement was a successful intervention to reduce aggregate alcohol consumption (Dills et al., 2005; Blocker, 2011, 2016; Miron and Zwiebel, 1991).

Additionally, there is a relatively large literature that documents the adverse effects of maternal alcohol consumption and alcohol availability on infants' and children's health outcomes (Barreca and Page, 2015a; Carpenter and Dobkin, 2011; Cil, 2017b; Conry, 1990; Fertig and Watson, 2009a; Lundsberg et al., 2015; Nilsson, 2017). Alcohol is known to be teratogenic to the fetus, specifically during the period of organ development. Alcohol transmits from the mother's blood to the embryo's blood and adversely affects cells' growth. The damage is dose-dependent and varies by the trimester of exposure, with the largest effects on the first trimester, the critical period of organogenesis (Nykjaer et al., 2014; Ornoy and Ergaz, 2010). The effects are observed through a wide range of adverse fetal and birth outcomes, including stillbirth (Aliyu et al., 2008), limited intrauterine growth (Fertig and Watson, 2009), preterm birth (Jaddoe et al., 2007), low birth weight (Cil, 2017), and infant mortality (Jacks et al., 2021; O'Leary et al., 2013; Wisborg et al., 2001).

These adverse effects may well go beyond short-run outcomes considering that the primary damage is to brain cell growth and the Central Nervous System (Ornoy and Ergaz, 2010). More importantly, drinking during pregnancy has long been shown to cause Fetal Alcohol

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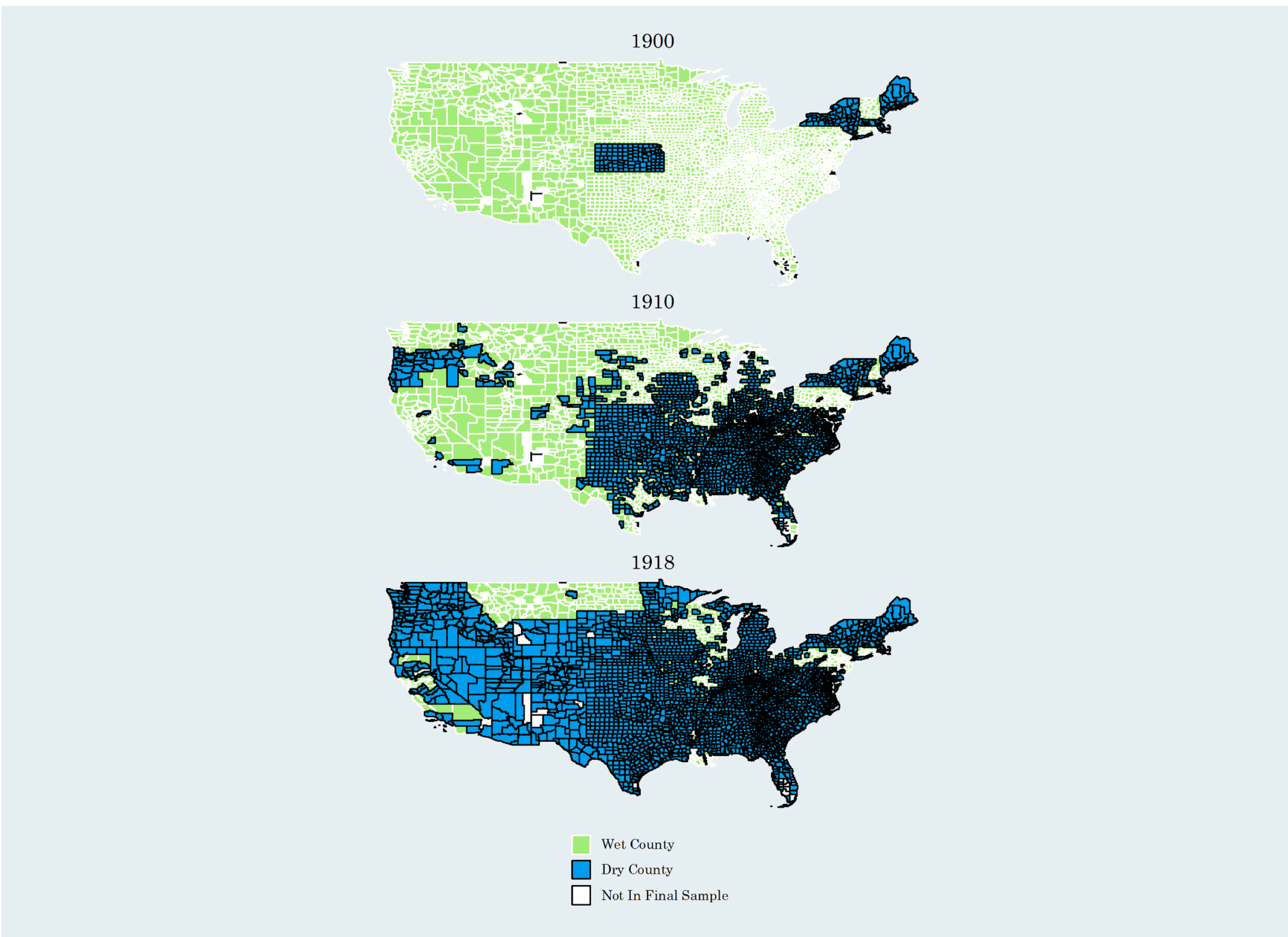

Fig. 1. Geographic Distribution of Prohibition Status across Counties and over the Years.

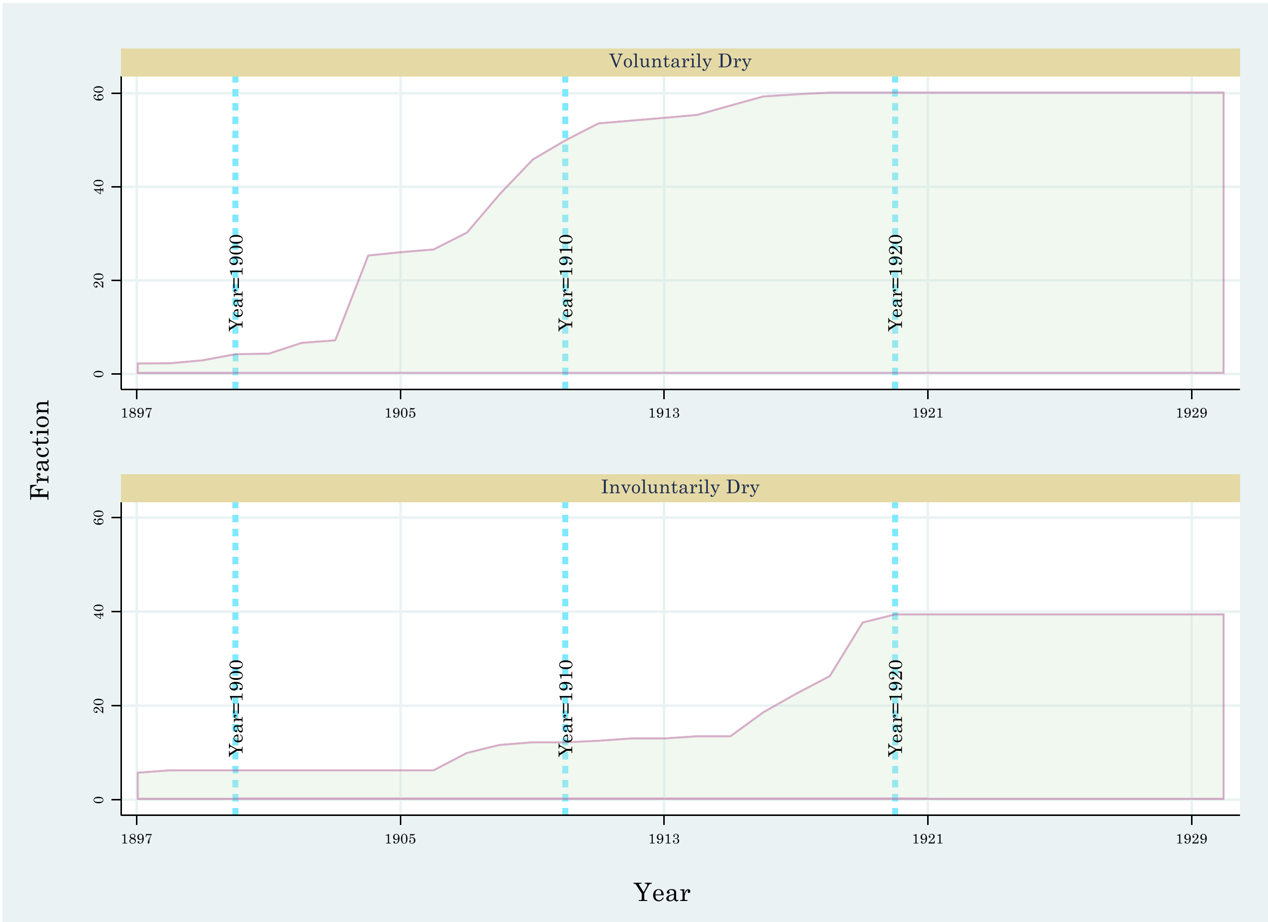

Fig. 2. Fraction of Voluntarily and Involuntarily Dry Counties over the Years.

Table 1
Balancing Test with County and Year FE.

	White	Black	Hispanic	Family Size	Occupational Prestigious Score	Occupational Income Score	Share of Literate People	Female Labor Force Participation Rate	Share of Farmers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Involuntarily Dry (ID)	-0.00698	0.00752	-0.00001	0.00913	0.15075	0.05397	-0.00029	0.01642	0.00446
	(0.00552)	(0.00533)	(0.0014)	(0.03262)	(0.11876)	(0.11857)	(0.00266)	(0.01002)	(0.00351)
Voluntarily Dry (VD)	0.00541	-0.00552	-0.00017	-0.0709 * *	0.29937 * *	-0.07426	0.01276 * **	-0.02958 * *	0.01605 * **
	(0.00566)	(0.00553)	(0.00142)	(0.02844)	(0.12356)	(0.12008)	(0.00297)	(0.01228)	(0.00327)
Observations	11,734	11,734	11,734	11,734	11,734	11,734	11,734	11,734	11,734
R-squared	0.98546	0.98676	0.96484	0.93412	0.87382	0.97535	0.91401	0.87924	0.97434
Mean DV	0.895	0.101	0.010	4.893	33.184	22.861	0.932	0.205	0.301
%Change ID	-0.780	7.450	-0.116	0.187	0.454	0.236	-0.032	8.009	1.483
%Change VD	0.605	-5.461	-1.705	-1.449	0.902	-0.325	1.369	-14.429	5.333
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. The panel is constructed from county-year characteristics of decennial censuses over the years 1900–1930 extracted from Ruggles et al. (2020). All Regressions are weighted using county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Syndrome (FAS). FAS is associated with a wide range of learning, communication, and physical conditions with no established cure (CDC, 2021; Jones and Smith, 1973). Evans et al. (2016b) investigate the effects of changes in alcohol prohibition laws across states and over the years 1904–1923 during the Temperance Movement on later life obesity, height, and education. They use a subsample of World War II enlistees and show that those who were exposed to alcohol prohibition during pregnancy and early-life are more educated and less likely to be obese. They do not find any significant effect on an individual's height.

While the literature on short-run and medium-run effects is relatively large, the literature on longer-run outcomes is limited. Specifically, very few studies have explored the effect of in-utero and early-life alcohol

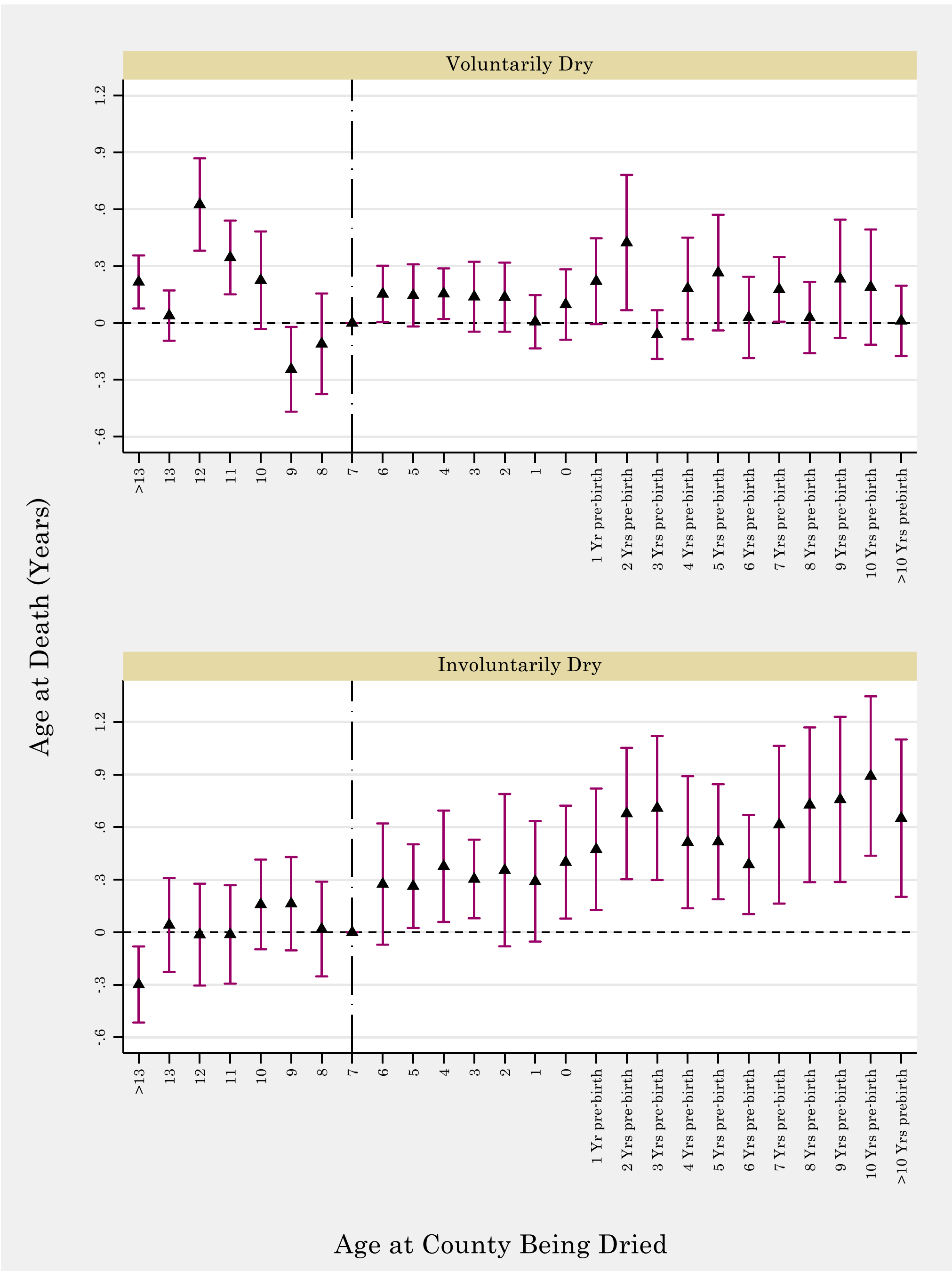

Fig. 3. Event Study Results for Voluntarily and Involuntarily Dry Counties. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects, birth year fixed effects, individual covariates, parental controls, and county controls. Individual controls include dummies for race and ethnicity. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.

Table 2
The Effects of Alcohol Prohibition Laws at Year of Birth on Old Age Longevity.

	Outcome: Age at Death (Years)			
	(1)	(2)	(3)	(4)
Share Exposure to Involuntarily Dry	0.17746 * *	0.17397 * *	0.15922 * *	0.16514 * **
	(0.08436)	(0.07517)	(0.07077)	(0.06375)
Share Exposure to Voluntarily Dry	0.08007	0.08239	0.07776	0.12253 *
	(0.08652)	(0.08128)	(0.0756)	(0.06483)
Observations	2585202	2585202	2585202	2585202
R-squared	0.28648	0.28668	0.28716	0.28719
Mean DV	73.409	73.409	73.409	73.409
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Parental Controls	No	No	Yes	Yes
County-by-Birth-year Controls	No	No	No	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. County-by-Year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include the father's socioeconomic index dummies and the mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

exposure, specifically due to the Temperance Movement, on old-age mortality. This paper aims to fill this gap.

We explore this long-term relationship using Social Security Administration death records to male individuals between 1975 and 2005 linked to the full-count 1940 census. We then use full-count historical censuses to search for individuals' county-of-residence during childhood. We employ the prohibition status of the county of birth/childhood during the period 1900–1930 as the intent-to-treat of alcohol exposure. Our identification strategy compares the old-age longevity of individuals who were exposed to prohibition laws during their early-life and childhood to those who were not. To tackle the endogenous selection of counties into dry status,[2] we disentangle prohibition status into two types: voluntarily and involuntarily prohibition counties. Involuntarily dry counties are counties that are enforced to be dry due to a state-level or a federal policy change while voluntarily dry counties chose to become dry through local referenda or legal changes applying only to the county. Therefore, by focusing on involuntarily dry-status changes, we leverage the enactment of policies across counties and over-time in a staggered adoption difference-in-difference setting. Our results indicate sizeable and statistically significant intent-to-treat effects of prohibition on longevity. Conditional on county and birth year fixed effects and a wide range of covariates, full exposure to prohibition is associated with 0.17 years higher age-at-death. A series of balancing tests are consistent with the claims that these effects are not driven by changes in other county-level health determinants and the selection of births based on observables. In addition, an event-study analysis shows that the effects are primarily concentrated in exposures during early childhood and suggest larger impacts for in-utero exposures to prohibition.

This study adds to the literature in two ways. First, the literature on in-utero and infancy alcohol exposure and later-life health and longevity is limited (Evans et al., 2016; Xuan and Egon, 2016). This paper is the first to study the long-term effects of in-utero prohibition on longevity. Second, this study adds to our understanding of the longer-term costs of alcohol exposure during pregnancy. Understanding these costs is important as the share of women who drink during pregnancy has increased from 9.2% to 11.3% from 2011 to 2018 (Denny et al., 2020). The associated problems, such as Fetal Alcohol Syndrome (FAS), impose an average lifetime cost of about $2 million per person (Lupton et al., 2004). Given the relatively high estimates of children diagnosed with FAS,[3] it is essential to understand a broader set of costs associated with drinking during pregnancy.

2. Data and Method

Our primary data source is the Death Master Files (DMF) of the Social Security Administration death records linked to the full-count 1940 US census extracted from Goldstein et al. (2021). It covers deaths that occurred to male individuals between the years 1975–2005.[4] The DMF-census-linked data provides information on age at death, date of birth, and parental and individual characteristics. Since our main focus is on in-utero and childhood exposure, we need information on county of birth or residence during childhood. To infer this variable, we search each individual in historical censuses 1910–1930. In so doing, we use historical census linking rules provided by the Census Linking Project (Abramitzky et al., 2020). We focus on county information in the first decennial census that each individual appears and use it as the county of birth/childhood.

We link this data with county-level prohibition status between 1900 and 1920 extracted from Sechrist (2012) based on county and year of birth. We extend this data by adding ten additional years to cover the post-federal-prohibition period. Therefore, our prohibition database covers the years 1900–1930. We construct two measures of prohibition status based on the information on county-year dry/wet status. First, an indicator of voluntarily dry (hereafter VD) that equals one if a county is dry in a wet state. Second, an indicator of involuntarily dry (hereafter ID) that equals one if the county has been wet in the previous periods and becomes dry only after statewide/federal dry status is imposed on all counties within the state/nation. For post-1919 years, we impute dry status based on two rules.[5] First, if the county is voluntarily dry in 1919, it is voluntarily dry for all years afterward. Second, if the county is involuntarily dry or is wet in 1919, it is considered involuntarily dry afterward. Therefore, the VD reveals the self-selection of counties into the prohibition while ID points to state/federal induced reforms that affect the prohibition status of a county.[6]

The linked DMF-census-prohibition data covers birth cohorts of 1900–1930 and includes 2585,202 observations.[7] Fig. 1 shows the geographic distribution of prohibition status across counties in three exemplary years before the federal ban. Fig. 2 illustrates the over-time changes in dry counties by type (VD and ID) as a fraction of all US counties.

The econometric method compares the average longevity of dry versus wet counties after the prohibition status change to before, conditional on covariates and fixed effects. We implement this comparison using ordinary-least-square formulations as follows:

[2] In this context, a dry county is an area where the production, sale, and consumption of alcoholic beverages is prohibited.

[3] Reports among school-aged children suggest a rate of between 6 and 9 per 1000 children (May et al., 2009, 2014).

[4] In ↱Appendix D, we show that the effects are larger (and significant) among males compared with females when we use an alternative data source. We should also note that linking death records and the 1940 census is primarily based on name commonalities and it is more comprehensive among males, as females usually change their names over time.

[5] The federal prohibition ban came into effect under the Volstead Act that was enacted in January 1920.

[6] This categorization leaves us with three sets of counties prior to 1920: voluntarily dry, involuntarily dry, and wet. From 1920 onwards, there are only voluntarily dry and involuntarily dry. However, prior to 1920, a small fraction of counties that became voluntarily dry, switch back to being wet. In the main analyses, VD takes a value of 0 in the years they are wet and 1 for the years they are dry. However, in ↱Appendix H, we show the robustness of the results to excluding these switch-back-to-wet counties.

[7] Summary statistics are reported in ↱Appendix A.

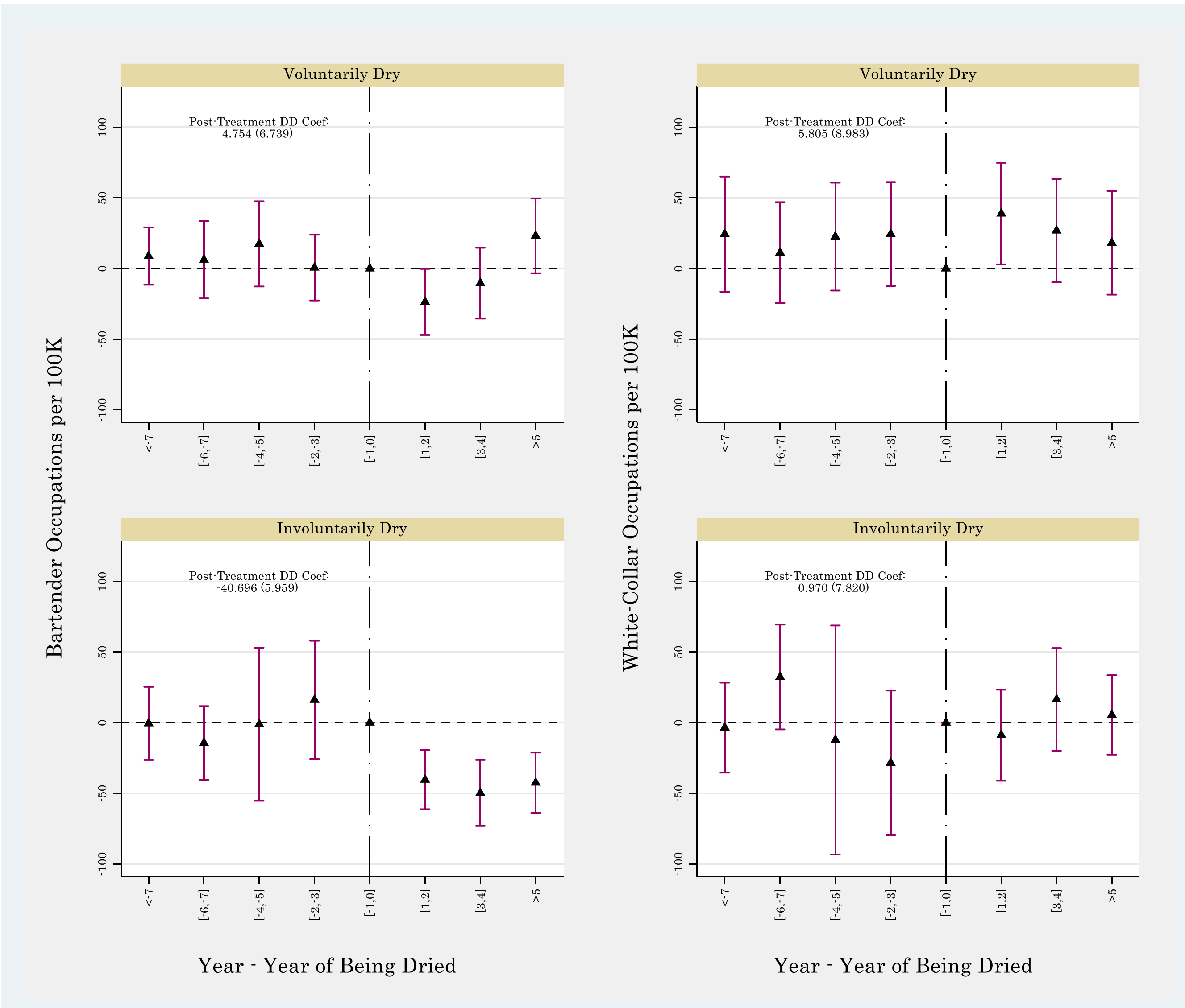

Fig. 4. Event-Study Analysis for the Effects of Prohibition on County-Level Bartenders per 100 K Population. Notes. Point estimates and 95% confidence intervals are illustrated. The panel is constructed from county-year characteristics of decennial censuses over the years 1900–1930 extracted from Ruggles et al. (2020). Standard errors are clustered at the county level. All regressions include county fixed effects, and birth year fixed effects, and county controls. County-by-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. All regressions are weighted by county population.

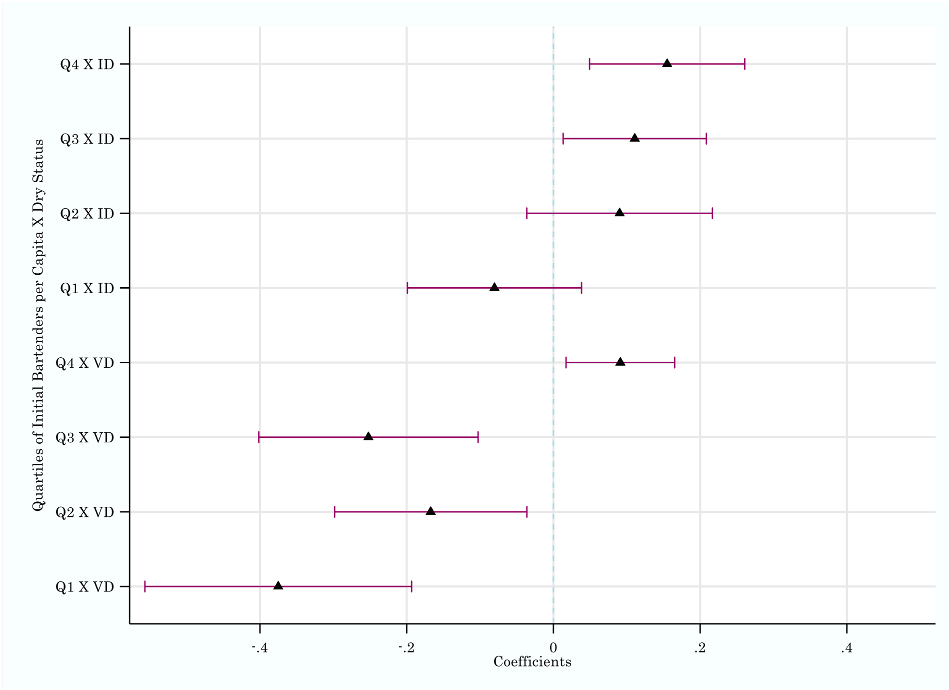

Fig. 5. Heterogeneous Effects of Prohibition on Age at Death across Counties at Different Quartiles of Initial Bartender per 100 K. Notes. The figure represents the results of regressing age at death on the interaction of ID and VD with quartiles of bartenders per capita at the year the county becomes dry. ID and VD stand for "Involuntarily Dry" and "Voluntarily Dry", respectively. Each Qz represents quartile z of initial (the year that the county becomes dry) bartenders per capita. Point estimates and 90% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects, birth year fixed effects, individual covariates, parental controls, and county controls. Individual controls include dummies for race and ethnicity. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.

$$DA_{icb} = \beta_0 + \beta_1 Share\_ID_{cb} + \beta_2 Share\_VD_{cb} + \Gamma X_i + \Lambda Z_{cb} + \eta_b + \zeta_c + \varepsilon_{icb} \quad (1)$$

Where DA is the age at death (in years) of individual i in county c who is born in year b. Matrix X includes individual race/ethnicity dummies, father's socioeconomic index dummies, and maternal education dummies (and missing indicators for missing values). In Z, we include county-level demographic and socioeconomic characteristics. These covariates are extracted from the 1900–1930 decennial censuses and interpolated for inter-decennial years. Birth-year and county fixed effects are represented by η and ζ, respectively. The parameters $Share\_ID$ and $Share\_VD$ represent the share of years between ages 0–7 that an individual's county has been ID and VD, respectively.[8] All regressions are weighted by the county population. Standard errors are clustered on county.

3. Results

3.1. Endogeneity Concern

We posit that among counties that become involuntarily dry as the statewide/federal prohibition status change, the effects are arguably orthogonal to other confounders as they were forced to follow the state/federal mandate rather than choosing/voting to be dry in earlier years. However, prohibition could be accompanied by other demographic and economic changes that influence infants' health outcomes which in turn affect their old-age longevity. To address this concern, we regress a series of demographic and socioeconomic outcomes on ID and VD in a county-by-decennial-year panel that also include county and year fixed effects. The results, reported in Table 1, point to quite small and insignificant correlations of ID. However, consistent with the endogeneity of VD, we do find associations between VD and family size, occupational score, the proportion literate, and female labor force participation. Moreover, we complement this analysis by evaluating the changes in sociodemographic characteristics of the final sample as a result of exposure to prohibition. In so doing, we regress individuals and family characteristics on the share of childhood exposure to ID and VD, conditional on county and birth-year fixed effects. The results are reported and discussed in ↱Appendix B. The estimated effects do not point to an

Table ↱A-1
- Summary Statistics.

	Mean	SD	Min	Max
Individual Characteristics:				
Age at Death (years)	74.592	9.538	44	105
White	0.957	0.203	0	1
Hispanic	0.01	0.1	0	1
Birth Year	1915.293	8.221	1900	1930
Death Year	1990.403	8.584	1975	2005
Involuntarily Dry	0.334	0.472	0	1
Voluntarily Dry	0.31	0.462	0	1
Father's SEI Low	0.214	0.41	0	1
Father's SEI High	0.205	0.404	0	1
Father's SEI Missing	0.581	0.493	0	1
Mother's Education<HS	0.365	0.481	0	1
Mother's Education=HS	0.116	0.32	0	1
Mother's Education Missing	0.496	0.5	0	1
Family Size	3.357	2.711	0	11
County Characteristics:				
Occupation: White-Collar Workers	2.788	0.918	0	90.174
Occupation: Farmers	24.485	22.05	0	97.75
Occupation: Other	72.548	21.438	1.275	100
Literate	93.845	6.691	0	100
Married	59.12	3.882	18.75	77.807
Occupational Income Score	22.498	4.099	10.214	29.717
Age 0–4	11.253	2.289	1.395	24.392
Age 5–10	12.992	2.386	1.329	22.348
Age 11–18	15.67	2.297	1.076	23.029
Age 19–25	12.84	1.419	4.239	40.779
Age 26–55	37.365	5.225	22.24	63.547
Age> 55	9.88	3.076	0	27.745
Population	3.079	6.369	0	39.864
Family Size	4.946	0.608	1.973	8.051
Observations	2585202			

[8] We choose ages 0–7 based on the event study results that suggest the effects are primarily concentrated in early childhood (see section ↱3.1). However, in ↱Appendix J, we show that the effects are considerably larger for in-utero exposure (age 0) compared with exposure at ages 1–7.

Table ↱B-1
- Balancing Test with County and Year FE.

	Outcomes:								
	White	Black	Hispanic	Father's SEI below Median	Father's SEI above Median	Father's SEI Missing	Mother's Education less than High School	Mother's Education High School	Mother's Education Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Share Exposure to Involuntarily Dry	0.00469 (0.00956)	-0.00423 (0.0092)	-0.00694 * (0.00366)	0.00164 (0.01836)	0.00731 (0.02144)	-0.00895 (0.01317)	0.01292 (0.01316)	-0.01472 (0.00937)	0.00097 (0.00841)
Share Exposure to Voluntarily Dry	-0.00021 (0.01169)	0.00049 (0.01147)	-0.00415 (0.00402)	-0.00606 (0.02905)	0.01899 (0.03439)	-0.01292 (0.01411)	-0.00861 (0.00568)	0.0082 (0.00826)	0.00167 (0.00659)
Observations	2585202	2585202	2585202	2585202	2585202	2585202	2585202	2585202	2585202
R-squared	0.06294	0.06602	0.08354	0.08364	0.21194	0.37564	0.20528	0.08413	0.41144
Mean DV	0.979	0.019	0.009	0.150	0.362	0.488	0.463	0.133	0.379
%Change ID	0.479	-22.255	-77.086	1.094	2.019	-1.834	2.790	-11.065	0.256
%Change VD	-0.021	2.591	-46.068	-4.043	5.245	-2.648	-1.861	6.163	0.440
90% Confidence Interval of ID	[− 0.011 0.020]	[− 0.019 0.011]	[− 0.013 − 0.001]	[− 0.029 0.032]	[− 0.028 0.043]	[− 0.031 0.013]	[− 0.009 0.035]	[− 0.030 0.001]	[− 0.013 0.015]
90% Confidence Interval of VD	[− 0.019 0.019]	[− 0.018 0.019]	[− 0.011 0.002]	[− 0.054 0.042]	[− 0.038 0.076]	[− 0.036 0.010]	[− 0.018 0.001]	[− 0.005 0.022]	[− 0.009 0.013]
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. All regressions include county fixed effects and birth year fixed effects All regressions are weighted by county population

endogenous change in the sample's socioeconomic and sociodemographic features due to exposure to prohibition. This test lends to the claim that endogenous birth and survival into adulthood do not confound the estimated effects of the main results.[9]

3.2. Event Study Analysis

While the literature usually points to the impacts of in-utero alcohol exposure for later-life outcomes, the effects could also appear for other ages at exposure. For instance, alcohol accessibility could redirect a portion of family income toward alcohol consumption and substitute for resources that could be made available for children. Studies suggest that family income and resources during childhood are associated with later-life health and mortality (Almond et al., 2018; Currie, 2009). Moreover, it could limit the time spent on children by parents, which is an important input for cognitive and educational outcomes (Baker and Milligan, 2016). To empirically explore the effects of prohibition across different ages, we implement an event-study analysis in which the event time is the child's age at prohibition status change. Specifically, we implement regressions similar to Eq. 1 in which the primary variables of interest are a series of dummies indicating age-at-prohibition, conditional on a full set of fixed effects and controls. We remove the coefficient of age-at-exposure of seven to compare other coefficients with the effects on this age group. The results are reported in two panels of Fig. 3 for voluntarily (top-panel) and involuntarily (bottom-panel) prohibition status change. Considering the self-selection of voluntarily dry counties and the observed associations of VD with other county-level features in Table 1, it is not surprising that the results fail to provide a consistent pattern of effects across age groups (top panel). Specifically, we observe some positive coefficients for age-at-exposure of 10–13 but no effects for many in-utero cohorts (age-at-exposure of less than zero).

For the involuntarily dry status counties, we do not observe a significant effect for age-at-exposures of 7–13 years old. The effects start to rise for age-at-exposure of 6-and-below. We also observe another sharp jump in the coefficients for age-at-exposure of zero and those individuals who were in-utero at the time of prohibition. The effects also become statistically significant for the in-utero exposures.

Table ↱C-1
- Heterogeneity of the Results.

	Outcome: Age at Death (Years)		
	(1)	(2)	(3)
ID× White	0.58653 * (0.32731)		
VD× White	0.04905 (0.32934)		
White	0.30431 (0.44076)		
ID× Low Educated Mother<HS		0.2822 * ** (0.03817)	
VD× Low Educated Mother<HS		0.3037 * ** (0.04405)	
Low Educated Mother<HS		-1.03248 * ** (0.0923)	
ID× Low SEI Father			0.23885 * * (0.11229)
VD× Low SEI Father			0.30782 * ** (0.09035)
Low SEI Father			-0.30749 * * (0.13379)
ID	-0.41511 (0.32799)	0.10455 (0.06417)	0.15308 * * (0.06401)
VD	0.07324 (0.3733)	0.05851 (0.0634)	0.1102 * (0.06625)
Observations	2585202	2585202	2585202
R-squared	0.28719	0.28721	0.28719

Notes. Standard errors, clustered at the county level, are in parentheses. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

[9] In ↱Appendix I, we use full-count decennial censuses, implement event-studies with county and year fixed effects, and show that there is no discernible and significant evidence of a change in counties' sociodemographic composition following the prohibition status change, suggesting that endogenous migration is not a concern.

Table D-1
- Sensitivity of the Results to Coverage Years of Death Data.

	Outcome: Age at Death (Years), Samples:					
	DMF (Males Only, 1975–2005)	DMF (Males Only, 1975–1987)	DMF (Males Only, 1988–2005)	Numident (Males, 1988–2005)	Numident (Females, 1988–2005)	Numident (Both Genders, 1988–2005)
	(1)	(2)	(3)	(4)	(5)	(6)
Share Exposure to Involuntarily Dry	0.15897 * **	0.05558 * *	0.08278 * **	0.1233 * **	0.02334	0.04883 * **
	(0.04104)	(0.02822)	(0.0231)	(0.02754)	(0.02885)	(0.01838)
Share Exposure to Voluntarily Dry	0.25712 * **	0.1156 *	0.00317	0.09864 * *	0.00945	0.02951
	(0.09749)	(0.06424)	(0.02305)	(0.04161)	(0.05389)	(0.03815)
Nonwhite	-0.74107 * **	-0.13413 * **	-0.30888 * **	-0.33349 * **	-0.43782 * **	-0.3645 * **
	(0.07666)	(0.01873)	(0.03755)	(0.02801)	(0.03423)	(0.01944)
Hispanic	0.56584 * **	0.12914 * *	0.26108 * **	0.23291 * **	0.18177 * *	0.19409 * **
	(0.08987)	(0.05438)	(0.07965)	(0.06614)	(0.08456)	(0.06046)
Observations	5757176	2285585	3471590	3923744	3609493	7533237
R-squared	0.29393	0.82087	0.61089	0.60272	0.51233	0.57413
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Parental Controls	Yes	Yes	Yes	Yes	Yes	Yes
County-by-Birth-year Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Table E-1
- Robustness of the Main Results to Alternative Models.

	Baseline from column 5 Table 2	Adding Region-by-Birth-Cohort FE	Adding County-by-Race FE	Adding County-by-Parental Covariates FE	Adding Birth-State by 1940-State FE	Clustering SE at State Level	Outcome: Log of Death Age	Outcome: Dummy for Death Age> 75	Outcome: Dummy for Death Age> 80
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Share Exposure to Involuntarily Dry	0.16514 * **	0.1333 * *	0.16432 * *	0.21308 * **	0.17186 * *	0.16514 * **	0.00175 * *	0.01053 * **	0.01242 * **
	(0.06375)	(0.05357)	(0.06456)	(0.07121)	(0.06678)	(0.05396)	(0.00079)	(0.00397)	(0.00447)
Share Exposure to Voluntarily Dry	0.12253 *	0.0454	0.1391 * *	0.11147	0.1258 *	0.12253 *	0.00124	0.00935 * *	0.01118 *
	(0.06483)	(0.05468)	(0.06922)	(0.08081)	(0.06866)	(0.06958)	(0.00077)	(0.00401)	(0.00604)
Observations	2585202	2585202	2584903	2585199	2585073	2585202	2585202	2585202	
R-squared	0.28719	0.28742	0.28733	0.28741	0.28784	0.28719	0.28061	0.1853	0.1839
Mean DV	73.409	73.409	73.409	73.409	73.409	73.409	4.287	0.434	0.277
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. Except for column 3, standard errors, clustered at the county level, are in parentheses. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

3.3. Main results

The main results are reported in Table 2. We start with a parsimonious model and add more covariates across consecutive columns. In the most parametrized specification, full exposure to prohibition during ages 0–7 in ID and VD counties is associated with a 0.17 and 0.12 years increase in longevity during old ages, respectively. These effects are equivalent to 16.6% and 12.4% of the white-nonwhite gap in the outcome, respectively.[10]

3.4. First-Stage Effects

The lack of granular data at the state or county level on alcohol production and consumption during the early decades of the 20th century makes it difficult to directly gauge the impacts of the prohibition on alcohol use. However, to indirectly explore the magnitude of first-stage effects on alcohol consumption, we examine how the county-level concentration of bartenders varies over the years.[11] In so doing, we implement an event-study analysis in a county-by-decennial-years panel where the event-time is years since the prohibition status change. The results, reported in the left panels of Fig. 4, suggest a drop in bartenders

[10] This number is calculated using the marginal effects of ID and VD relative to the marginal effects on a *nonwhite* dummy in the regression of column 4 of Table 2. The coefficient (not reported in this table) implies a white-nonwhite difference of, on average, 0.99 years (se=0.13).

[11] We use information on individuals' occupation in decennial censuses to extract a county-by-decennial-census measure of bartenders per capita.

Table ↱F-1
– Exploring Mechanisms: Effects of Prohibition on Socioeconomic Outcomes.

	Outcomes:		
	Years of Schooling	Socioeconomic Index	Occupational Income Score
	(1)	(2)	(3)
Share Exposure to Involuntarily Dry	0.16167 * *	1.55056 * **	0.72333 * **
	(0.06804)	(0.4251)	(0.15963)
Share Exposure to Voluntarily Dry	0.14212 * **	1.05186 * **	0.47553 * **
	(0.05233)	(0.32324)	(0.15548)
Observations	1914290	1703696	1732856
R-squared	0.04946	0.10031	0.1093
Mean DV	10.754	34.804	26.639
%Change ID	1.503	4.455	2.715
%Change VD	1.322	3.022	1.785
County FE	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes
County-by-Birth-year Controls	Yes	Yes	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. The sample is restricted to birth cohorts of 1900–1922. The outcomes are from the 1940 census. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

Table ↱H-1
- The Effects of Alcohol Prohibition Laws at Year of Birth on Old Age Longevity Dropping Reverting Counties.

	Outcome: Age at Death (Years)			
	(1)	(2)	(3)	(4)
Share Exposure to Involuntarily Dry	0.17288 * *	0.16877 * *	0.15351 * *	0.15834 * *
	(0.08504)	(0.07549)	(0.07088)	(0.0624)
Share Exposure to Voluntarily Dry	0.08115	0.08303	0.07757	0.12479 *
	(0.08756)	(0.08194)	(0.07627)	(0.06485)
Observations	2566832	2566832	2566832	2566832
R-squared	0.28665	0.28686	0.28733	0.28736
Mean DV	73.403	73.403	73.403	73.403
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Parental Controls	No	No	Yes	Yes
County-by-Birth-year Controls	No	No	No	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. County-by-Year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include the father's socioeconomic index dummies and the mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

per capita in involuntarily dry counties. The fact that we do not observe a change for voluntarily dry counties after prohibition is consistent with their self-sorting. As a placebo check, we also look at the evolution of white-collar occupations as they should not change by changes in dry status. The right panels of Fig. 4 satisfy our a-prior expectations and add to the validity of the results of the left panels.

3.5. Heterogeneity analysis

As we show in ↱Appendix C, the results reveal substantial heterogeneity. The effects are primarily concentrated among whites, non-Hispanics, and individuals with lower maternal education and family socioeconomic status. Moreover, if the prohibition is the main driver of the effects, one would expect a higher treatment effect in counties that absorb a higher dosage of the treatment, i.e., counties with higher initial bartenders per capita. We interact ID and VD with indicators of quartiles of bartenders per capita in the year of prohibition. The results, reported in Fig. 5, confirm this and suggest higher effects in counties with a higher concentration of bartenders.

Table ↱J-1
- Disentangling the Effects of In-Utero Exposure and Exposure at Ages 1–7.

	Outcome: Age at Death (Years)			
	(1)	(2)	(3)	(4)
In-Utero Exposure to Involuntarily Dry	0.21204 * *	0.20804 * **	0.19362 * **	0.21322 * **
	(0.0901)	(0.0799)	(0.0747)	(0.06441)
In-Utero Exposure to Voluntarily Dry	0.11255 * *	0.1072 * *	0.10434 * *	0.13182 * **
	(0.05613)	(0.05231)	(0.04749)	(0.04962)
Share Exposure Ages 1–7 to Involuntarily Dry	0.11759	0.11831	0.11471	0.171 * *
	(0.09265)	(0.08761)	(0.08275)	(0.07179)
Share Exposure Ages 1–7 to Voluntarily Dry	0.087 *	0.08467 *	0.0808 *	0.11003 * **
	(0.04935)	(0.04665)	(0.0433)	(0.03881)
Observations	2585202	2585202	2585202	2585202
R-squared	0.28649	0.28669	0.28716	0.28719
Mean DV	73.409	73.409	73.409	73.409
County FE	Yes	Yes	Yes	Yes
Birth Year FE	Yes	Yes	Yes	Yes
Parental Controls	No	No	Yes	Yes
County-by-Birth-year Controls	No	No	No	Yes

Notes. Standard errors, clustered at the county level, are in parentheses. County-by-Year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include the father's socioeconomic index dummies and the mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.
* ** $p < 0.01$, * * $p < 0.05$, * $p < 0.1$

3.6. Additional analyses

We show that the results remain significant but become smaller when we look at alternative death data with a shorter (and later) death window (specifically 1988–2005), suggesting that the mortality effects are larger in younger ages (↱Appendix D). We also employ an alternative data source and show that the effects are larger among males compared with females (↱Appendix D). As further checks, we show that the results are robust to alternative specifications, standard-errors clustering, and nonlinearities in the longevity (↱Appendix E). The information in the 1940 census suggests that improvements in socioeconomic status and occupational income score are potential channels of impact (↱Appendix F).

In addition, several studies document that in cases of policy analysis with staggered adoption, OLS-produced difference-in-difference estimations can be biased (de de de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Given the staggered nature of the roll-out of the policy in our analysis, we check for the robustness of OLS using an alternative method developed by Sun and Abraham (2021) and de de de Chaisemartin and D'Haultfœuille (2020). The results, reported in ↱Appendix G, suggest that our estimations are not driven by contaminations of OLS in a two-way fixed effect framework.

4. Conclusion

A century of temperance movement intensified rapidly between the

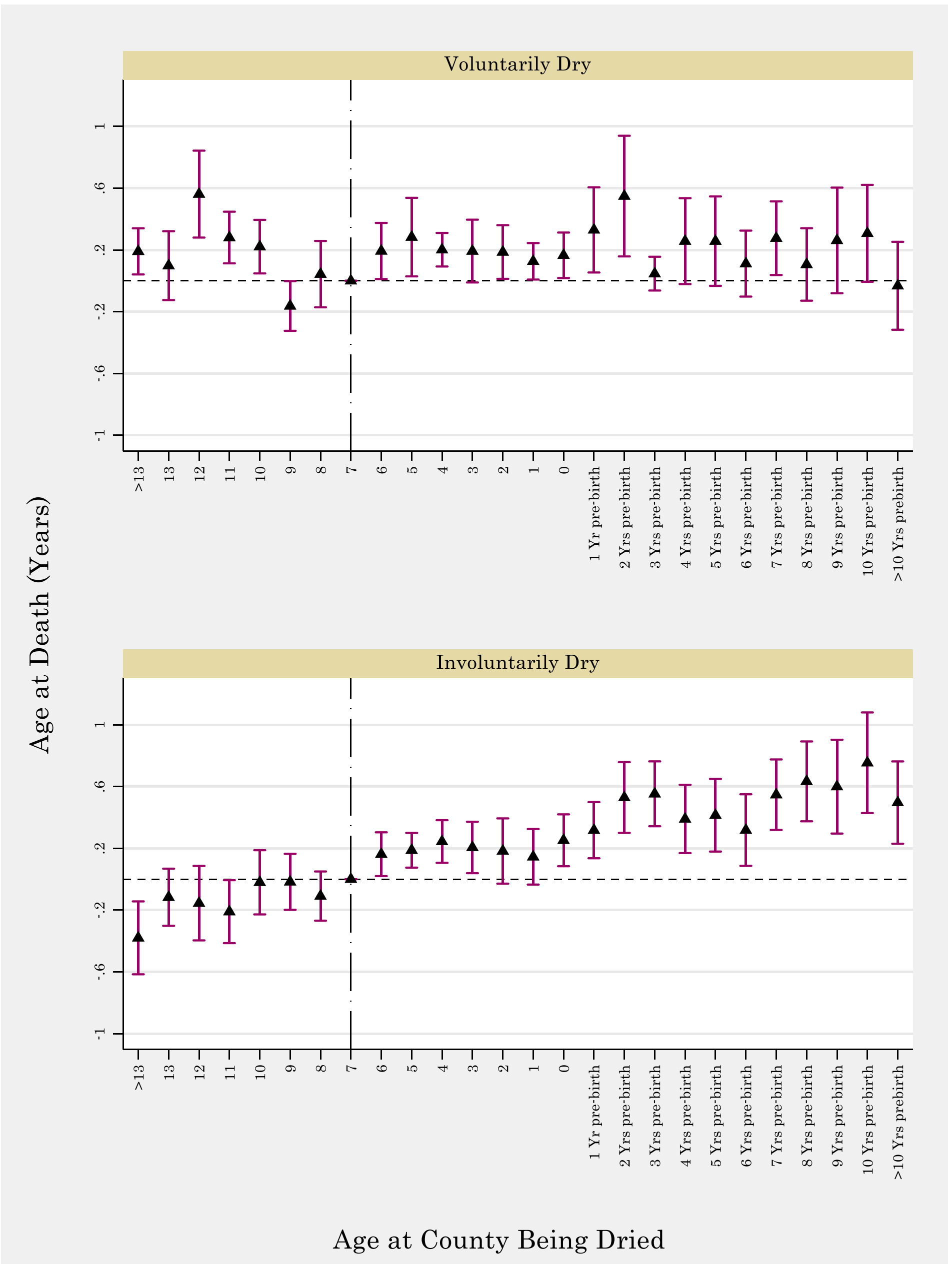

Fig. G-1. - Event Study Results for Voluntarily and Involuntarily Dry Counties Using Sun and Abraham (2021) Estimation Strategy. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects, birth year fixed effects, individual covariates, parental controls, and county controls. Individual controls include dummies for race and ethnicity. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.

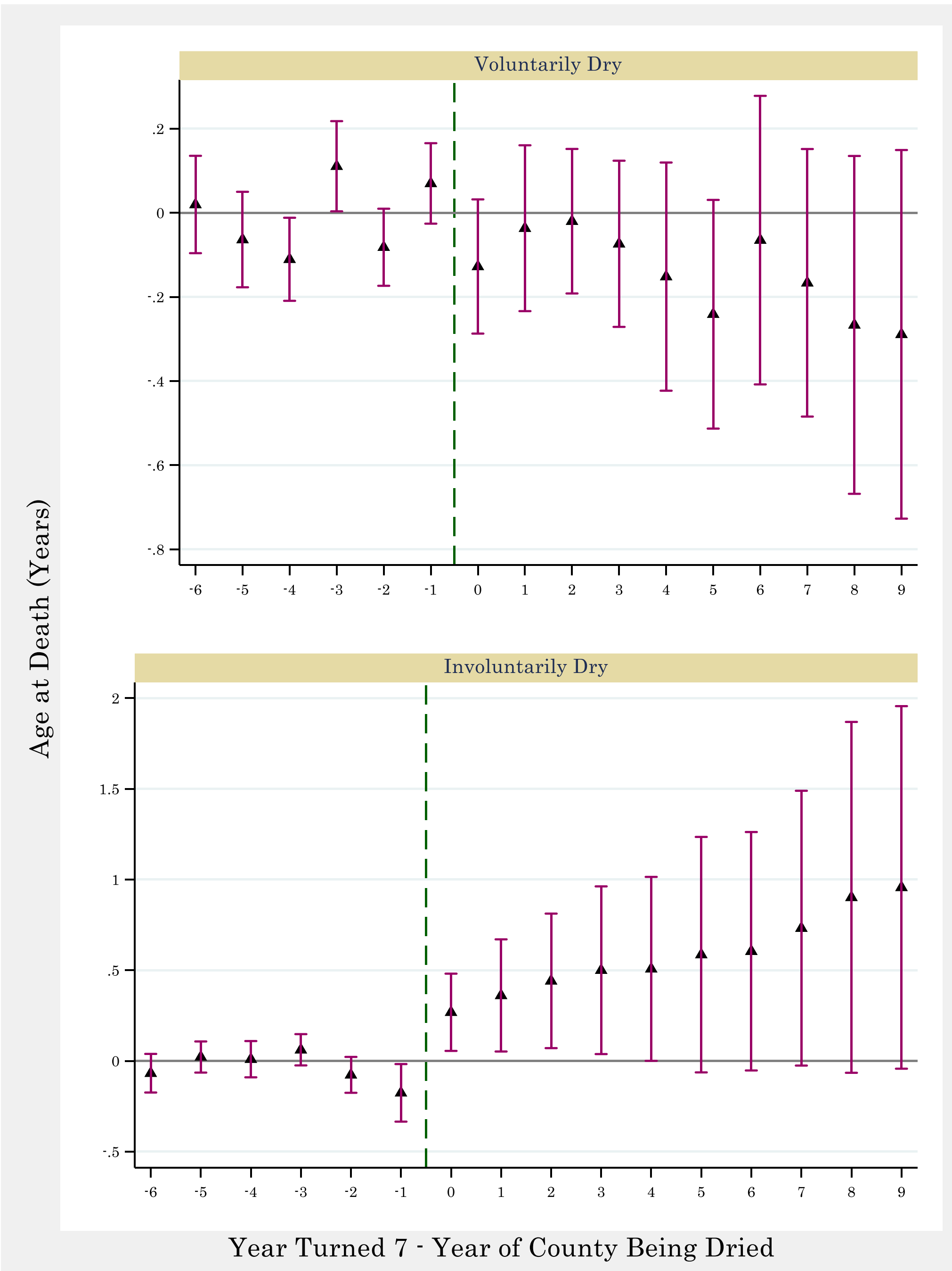

Fig. G-2. - Event Study Results for Voluntarily and Involuntarily Dry Counties Using de de de Chaisemartin and D'Haultfœuille (2020) Estimation Strategy. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects, birth year fixed effects, individual covariates, parental controls, and county controls. Individual controls include dummies for race and ethnicity. County-by-birth-year controls include share of workers (males aged 25–55) in different occupations, share of literate people, share of people in different age groups, share of married people, population, average family size, and average male occupational income score. Parental controls include father's socioeconomic index dummies and mother's education dummies (and missing indicators for missing values). All regressions are weighted by county population.

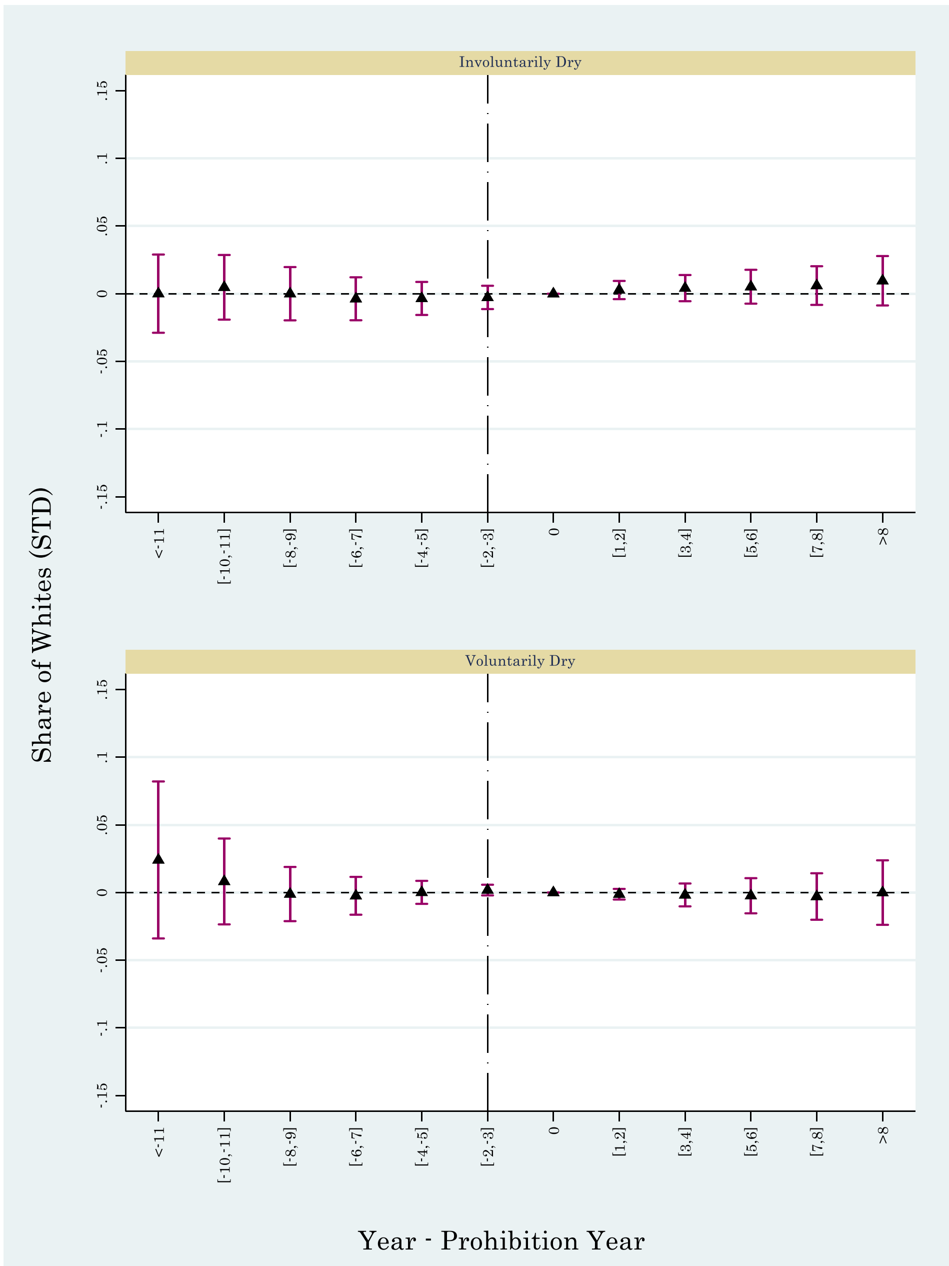

Fig. ↱I-1. - Event Study Results for Changes in County Population Characteristics. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. All regressions are weighted by county population.

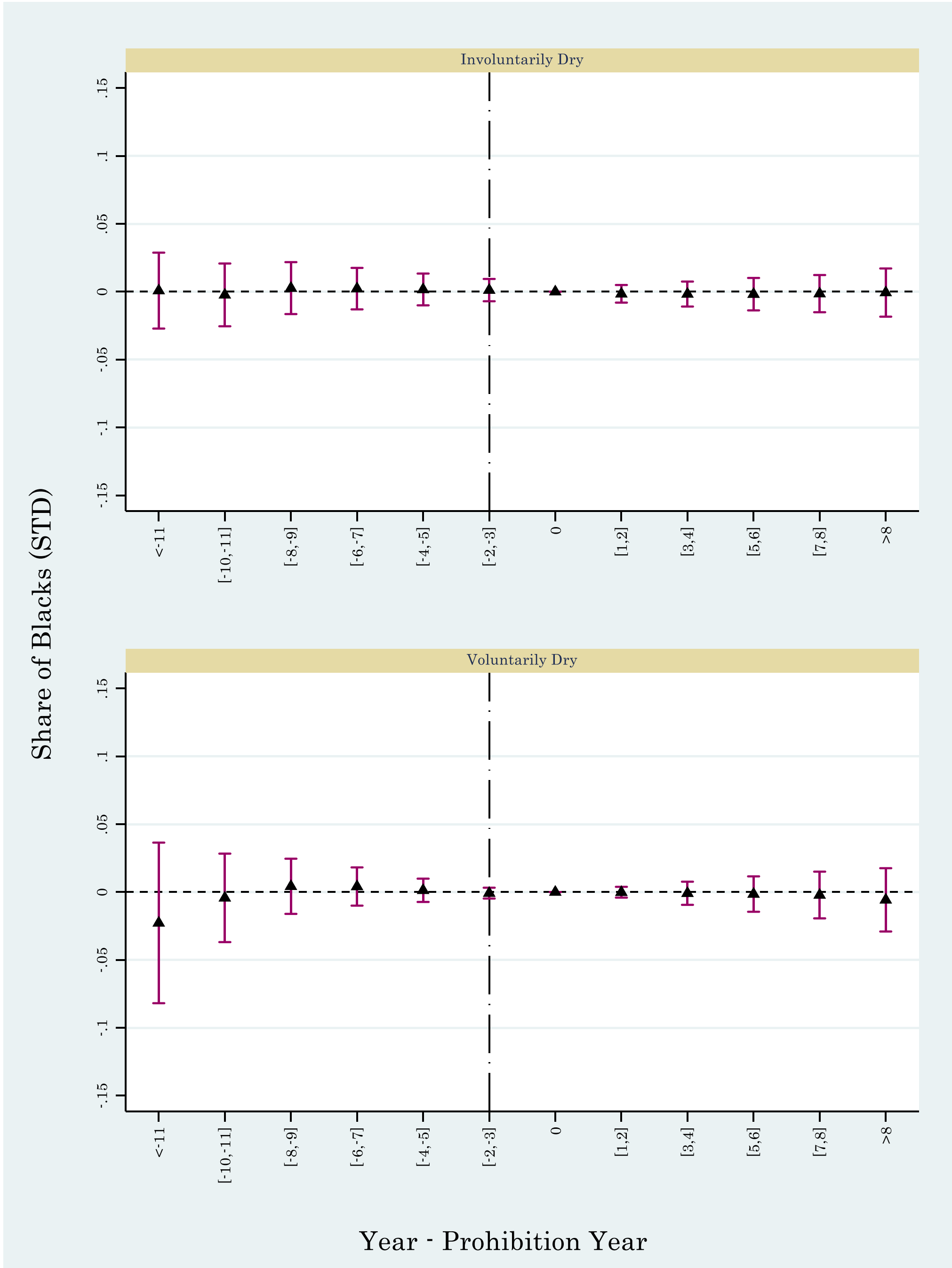

Fig. I-2. - Event Study Results for Changes in County Population Characteristics. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. All regressions are weighted by county population.

years 1900–1920. Recent studies suggest that lowering alcohol availability due to prohibition reduced mortality, decreased drug-related crime, and improved child health (Evans et al., 2016; Jacks et al., 2021; Law and Marks, 2020). We added to this literature by documenting the long-run health benefits of the prohibition on the longevity of children who experienced the prohibition during in-utero and early-life. We showed that being born in a county that was forced to be dry as a result of statewide/federal alcohol prohibition is associated with roughly 0.17 additional years of longevity during old age.

We note that these effects are only intent-to-treat impacts across all populations, regardless of whether or not their mother drank during pregnancy. Given a lower bound of previous studies on the share of women with an alcohol problem (10%) during the late 19th and early 20th century (Lender, 2009; Murdock, 2002), assuming that women who used to drink also drank during pregnancy (which attenuates the treatment-on-treated effects), and assuming no illegal consumption after prohibition (which again attenuates the treatment-on-treated effects), a back of the envelope calculation suggests a potential treatment-on-treated effect of 1.7 additional life-years.

We should also note that these effects are observed for cohorts born in the early 20th century in the US. Although the developmental stage of many developing economies is comparable to those of the US in early decades of the 20th century, one may exercise caution in generalizing the findings in other geographic regions, cohorts, and cultures.

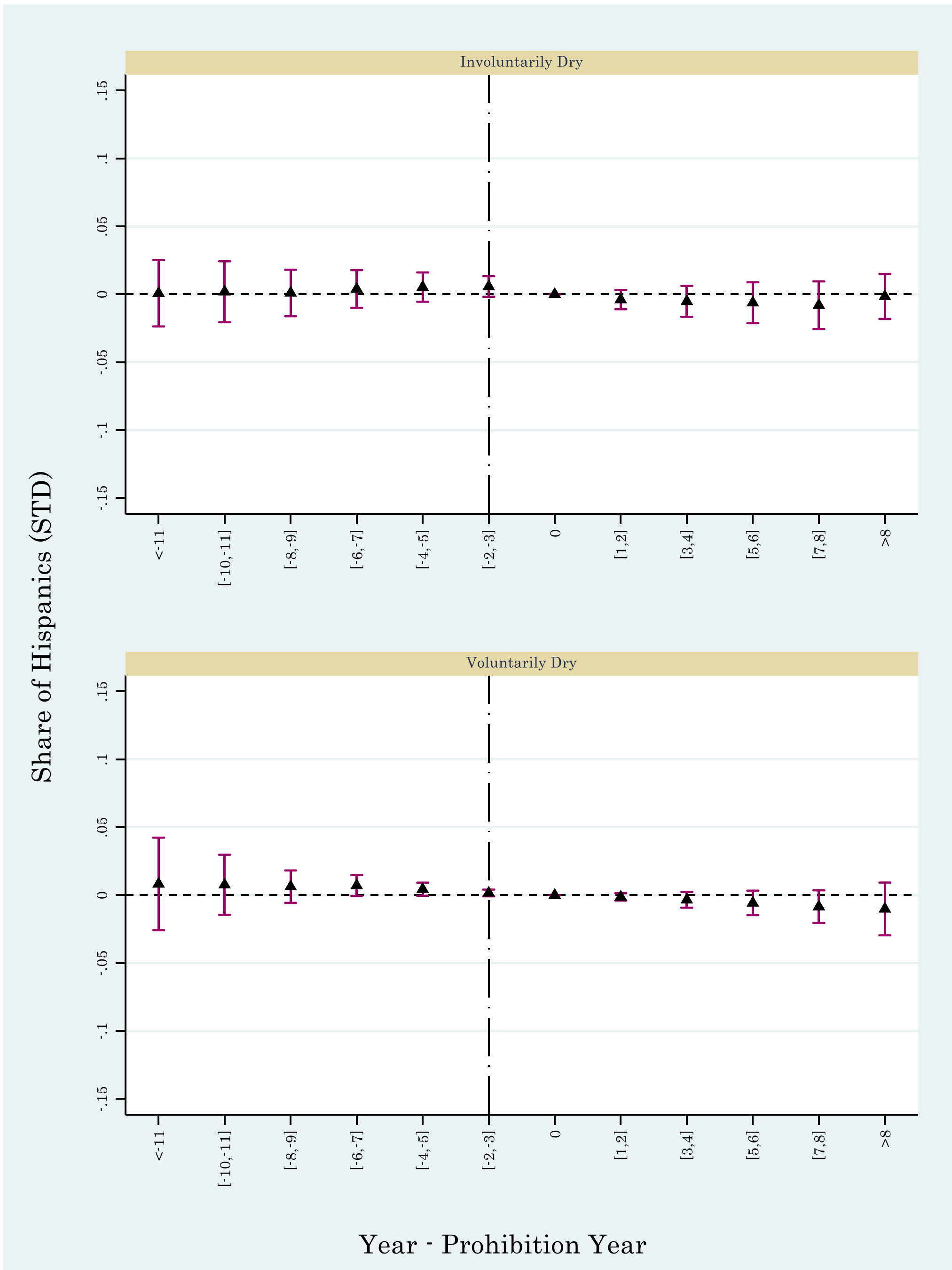

Fig. ↱I-3. - Event Study Results for Changes in County Population Characteristics. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. All regressions are weighted by county population.

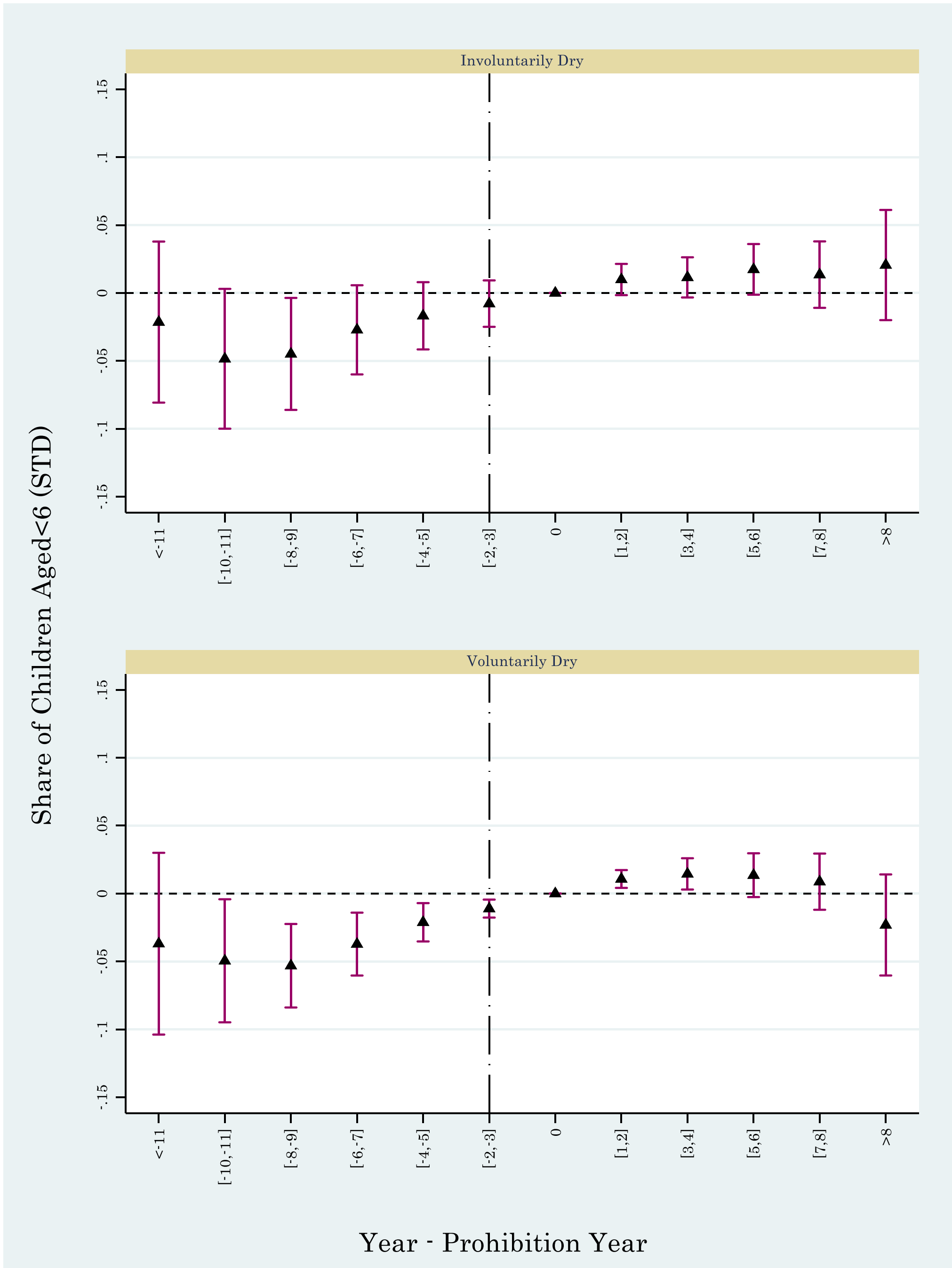

Fig. ↱I-4. - Event Study Results for Changes in County Population Characteristics. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. All regressions are weighted by county population.

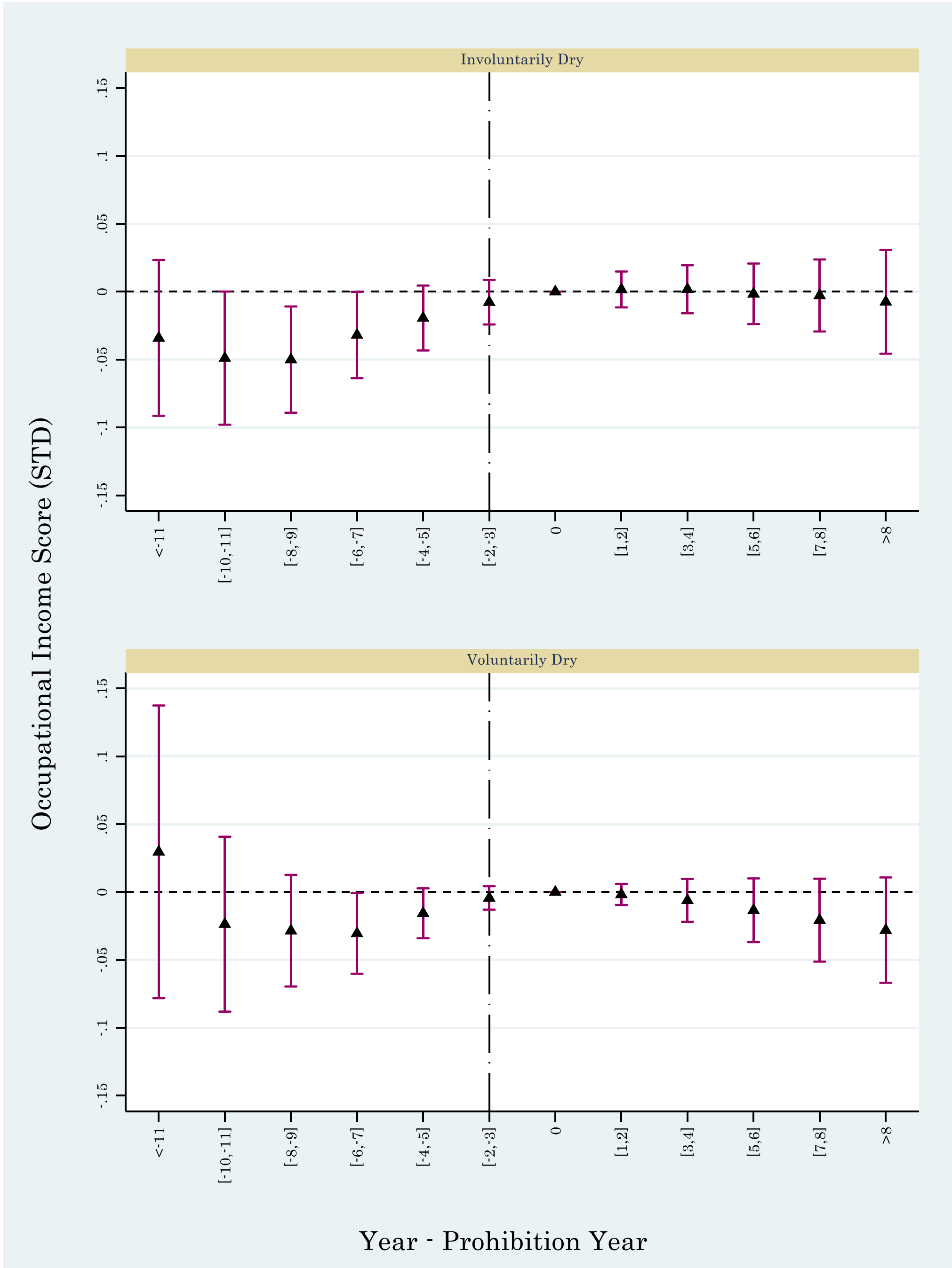

Fig. ↱I-5. - Event Study Results for Changes in County Characteristics. Notes. Point estimates and 95% confidence intervals are illustrated. Standard errors are clustered at the county level. All regressions include county fixed effects and year fixed effects. All regressions are weighted by county population.

CRediT authorship contribution statement

Both authors (Jason Fletcher and Hamid Noghanibehambari) contributed equally to the production of the paper.

Data Availability

Data will be made available on request.

Appenix A

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Appendix Tables ↱A-1 provides summary statistics of the final sample used in the text. On average, the age at death is 74.6 years, with a minimum of 44 and a maximum of 105. The average ID and VD are 0.33 and 0.31, respectively. In regressions of the main text and the event-study of Fig. 3, we include all parental characteristics and county covariates listed in this table.

Appendix B

The identification strategy of Eq. 1 rests on the assumption that spike drops in alcohol consumption in counties that become dry as the statewide or federal changes in prohibition status are uncorrelated with other determinants and trends in their newborns' health outcomes. As mentioned in section ↱3.1, we rely on the plausibly exogenous status change in counties that become involuntarily dry as they were forced to oblige by the state mandate rather than choosing/voting to be dry in earlier years. One potential confounder is the selection of births or survival of the fetus based on observable characteristics. We explore this source of endogeneity by regressing average DMF characteristics and parental characteristics on primary explanatory variables of Eq. 1, conditional on fixed effects and trends. The results are reported in Appendix Table ↱B-1. There is no evidence that prohibition status change affects the structure of the sample based on race, maternal education, and father's socioeconomic index. The effects are small in magnitude (and relative to the mean) and mostly statistically insignificant. Overall, we fail to find strong associations suggesting that changes in county characteristics and selection of births could confound the estimates.

This balancing test is a dual test for potential issues caused by the selection of births as well as migration issues as we use the county of residence observed in decennial censuses as a proxy for county of birth and childhood. Migration causes a problem if there are characteristics in migrants that lead them to migrate and those characteristics are also correlated with the prohibition status of counties. If there are indeed certain features that drive the migration as a result of prohibition status change, we should observe a consistent correlation between observable characteristics and ID and VD. The results of Appendix Table ↱B-1 do not offer such evidence.

Appendex C

As an additional analysis, we explore the heterogeneity of the effects based on race/ethnicity, county features, and parental characteristics. In so doing, we interact with ID and VD in Eq. 1 a dummy for being white, a dummy for mother's education less than high school, and a dummy for fathers' socioeconomic index (SEI) being below the median. The results are reported in Appendix Table ↱C-1. As the double-interaction terms suggest, the effects are larger among whites, children of lower maternal education, and children of lower SEI fathers.

Appendix D

The DMF data covers deaths of male individuals that occurred over the years 1975–2005. To see the robustness of the results to include females as well as other death windows, we use Numident death records from the Social Security Administration extracted from Goldstein et al. (2021). The Numident is also linked to the 1940 census and covers deaths to both females and males but covers a shorter death window (1988–2005).

As we discussed in section ↱2, to infer county of birth/childhood, we employ cross-census linking rules. Since women usually change their name after marriage, these linking rules only provide cross-census match for male individuals. Therefore, we are unable to use the same method for the Numident death records. Instead, we use the county-of-residence in 1940 as the proxy for county of birth/childhood. As discussed in ↱Appendix B, we do not find evidence of endogenous migration and selective survival to adulthood specifically for ID prohibition counties. Therefore, we believe that migration is unlikely to bias the prohibition-longevity relationships. However, to check to what extent this proxy choice may affect the estimates, we use DMF data and use the 1940-county as the county of birth/childhood and replicate the main results. The results are reported in column 1 of Appendix Table ↱D-1. We observe very similar coefficients as those of column 4 of Table 2. In column 2, we restrict the sample to DMF deaths that occurred between the years 1975–1987. For comparison purposes, we also report the results using DMF data for the period covered by Numident, i.e., years 1988–2005 (column 3).

Next, we employ Numident death records, use 1940-county as the county of birth/childhood, and implement the same sample selection and empirical method as the main results. In columns 4, 5, and 6, we show the results using Numident data for males, females, and both genders, respectively. The marginal effect of column 4 (Numident males, 1988–2005) is about 50% larger than column 3 (DMF males, 1988–2005). We do not find a significant impact on female longevity. The estimated marginal effect is considerably smaller in size and statistically insignificant (column 5).

There are two conclusions from the results of Appendix Table ↱D-1. First, the DMF results of the paper are gender-specific. The effects are almost exclusively driven by males. The second conclusion from a comparison of columns 1 and 4–6 is that the effects are considerably larger when we include earlier deaths. This fact implies that the mortality effects of alcohol exposure in-utero may appear in younger ages. This finding is consistent with reports of previous studies. For instance, Xuan and Egon (2016) use data on inpatients, outpatients, or practitioner claims in Alberta, Canada between the years 2003–2012 linked with death registry data to show that the life expectancy of people diagnosed with FASD is 42% below the population average. They estimate an average life expectancy of about 34 years, which suggests that mortality consequences appear in relatively younger ages.

Appenix E

Appendix Table ↱E-1 explores the robustness of the results across alterative specifications. We start by replicating the full specification of column 4 of Table 2. In column 2, we add to the column 1 model a series of region-of-birth-by-birth-cohort dummies. The estimated effect of ID is comparable to that of column 1. However, the effect of VD becomes quite small and insignificant.

In column 3, we allow the fixed characteristics of counties to vary flexibly by individuals' race. The effects are virtually unchanged relative to column 1. In column 4, we interact county fixed effects with maternal education dummies and paternal socioeconomic status dummies. The ID effect rise by about 23% and the VD effect decreases by about 8%.

In column 5, we identify the effects in groups of individuals based on their birth-state and 1940-state to mitigate the confounding influence of migration. Specifically, we add birth-state by state-of-residence in 1940 fixed effects. The marginal effects are almost identical to those of column 1.

In column 6, we show the results for clustering the standard errors at the state level. The standard errors become smaller for ID status and statistical significance does not change. For VD status coefficient, the standard errors inflate and the effect becomes statistically insignificant.

In column 7, we replace the outcome with the log of age at death. The marginal effect of ID status remain statistically significant. We continue to explore the nonlinearities in the outcome by replacing the outcome with a dummy that equals one if age at death is larger than 65 and 75 years. The results are reported in columns 8 and 9. The effects appear to be stronger in older ages. With respect to the mean of the outcomes, the ID effects suggest increases in the probability of living beyond 75 and 80 years by about 2.4% and 4.5%, respectively.

Appendex F

While we primarily rely on the literature on maternal alcohol consumption and life-course outcomes as the mechanism channels of impact (Barreca and Page, 2015; Cil, 2017; Evans et al., 2016; Fertig and Watson, 2009; Jacks et al., 2021; Jaddoe et al., 2007; Leon et al., 2007; O'Leary et al., 2013), we also explore the effects on some of the mediatory outcomes in Appendix Table F-1. In this table, we remove observations below 18 years old as we focus on education and labor market outcomes. The larger effects of ID versus VD are consistent with the reduced-form effects of the main results. However, as we discussed in section 3.1, the association of county demographic-socioeconomic characteristics (Table 1) with VD status makes the interpretation of its coefficients difficult. This is expected as the voluntarily dry counties vote/sort/select themselves into being dry and this self-selection is likely correlated with other health-determinant factors. Therefore, we rely on the coefficients of ID.

Column 1 suggests that a full exposure to ID prohibition is associated with 0.16 additional years of schooling. We can use previous literature to evaluate to what extent this effect can explain the observed reduced-form effects. Halpern-Manners et al. (2020) employ Numident death records of Social Security Administration linked with the 1940 census and implement twin strategy to explore the education-longevity relationship. They find that each additional year of schooling is associated with 0.34 additional years of life during old ages. Therefore, based on the effects of column 1 of Appendix Table F-1, a full exposure to ID prohibition is associated with 0.05 years higher longevity. This is about 33% of the observed reduced from effect of Table 2.

Moreover, columns 2 and 3 suggest significant improvements in socioeconomic index and occupational income score. A full exposure to ID prohibition status of the county is associated with a 1.5 and 0.7 units increase in the socioeconomic index and occupational income score, equivalent to 4.5% and 2.7% rise from the mean of the outcomes, respectively.

Appendix G

In the main text, we used ordinary least square regressions. In this appendix, we explore the robustness of the results to using OLS-produced effects and to check for potential bias resulted from using OLS (Baker et al., 2022; Callaway and Sant'Anna, 2021; de de de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). The results of Sun and Abraham (2021) estimation strategy are reported in Appendix Figure G-1. We observe a similar pattern of effects as those produced by OLS in Fig. 3. In Appendix Figure G-2, we employ the method developed by de de de Chaisemartin and D'Haultfœuille (2020) to replicate the event-study results. Similar to the event-study of the paper, we evaluate all effects with respect to those aged 7 at the time of prohibition status change. Therefore, the event-time is year the individual turned 7 minus the prohibition year. Event-times of 7-and-above refer to in-utero exposures. Event-times of − 1-and-below refer to age-at-exposure of 8-and-above. For the ID prohibition status (bottom-panel), we observe a similar pattern as the OLS results of Fig. 3. The effects are small and insignificant for age-at-exposure of 7-and-above. The effects start to rise in magnitude and become statistically significant for age-at-exposure of below 7. Moreover, we observe a small jump in the effects for in-utero exposure (i.e., event-time of 7–9).

Appendix H

During post-1900 years, most counties that become dry (voluntarily or involuntarily) remain a dry county until the federal ban of 1920. However, in the final sample of the paper, about 0.7% of observations (18,370 individuals) are born in a county that, after becoming dry, switch back to become a wet county. In Appendix Table H-1, we drop these individuals and replicate the main results of Table 2. We observe almost identical coefficients across models.

Appendix I

One concern in the main results is the selective migration of people after a prohibition reform. The migration may confound our estimates if it changes the characteristics of counties in a way that are correlated with both health and prohibition status. For instance, if more white people move out of counties that joined the prohibition movement, the results may underestimate the true effects as whites have, on average, higher longevity than non-whites. In Appendix B, we show that in the final sample, there is no significant difference in the characteristics of individuals and the dry status of counties. In this appendix, we empirically explore this concern using full-count decennial censuses 1900–1930. We build a county-year panel data and implement event-study regressions that include county and year fixed effects. We regress county characteristics on indicators of voluntarily and involuntarily dry status. The results are reported in Appendix Figure I-1 through Appendix Figure I-5. We do not observe consistent and discernible change in county population composition and socioeconomic measures after adoption of prohibition, suggesting that selective migration is not likely to confound our estimates.

Appendix J

In the main analysis of the paper, we focus on exposure between ages 0 (in-utero) until 7. This choice is made based on the event-study which suggests discernible effects among those below age 7. In this appendix, we disentangle in-utero exposure effects from early-childhood exposure effects. Instead of *Share_ID* and *Share_VD* variables in Eq. 1, we use two dummy variables indicating in-utero exposure to the dry status and one variable that captures the share of exposure between ages 1–7. The results are reported in Appendix Table J-1. We observe considerably larger effects for in-utero exposures. For instance, among involuntarily dry counties, we find an increase of 0.21 and 0.13 for in-utero and age 1–7 exposures, respectively.

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