Infant Mortality and the Repeal of Federal Prohibition*
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Abstract: Using new data on county-level variation in alcohol prohibition from 1933 to 1939, we investigate whether the repeal of federal prohibition increased infant mortality, both in counties and states that repealed and in neighboring counties. We find that repeal is associated with a 4.0% increase in infant mortality rates in counties that chose wet status via local option elections or state-wide legislation and with a 4.7% increase in neighboring dry counties, suggesting a large role for cross-border policy externalities. These estimates imply that roughly 27,000 excess infant deaths could be attributed to the repeal of federal prohibition in this period.

JEL classification: H73, I18, J1, N3
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1. Introduction

As is very well known, the United States from 1920 to 1933 embarked on one of the most ambitious policy interventions in its history. Federal prohibition laws on the production, sale, and transportation of alcohol induced massive changes in the economic and social fabric of the then 48 states. And while contemporary prohibition movements gained traction across the world, nowhere were the impulses, proclivities, and traditions of such a large population subdued for so long. Naturally, given the scale of this intervention, prohibition has alternately been described as America's most 'noble experiment' and its most ominous foray into social engineering.

Understanding the effects of federal prohibition is important with respect to a very sizeable historical literature on this topic (see Kyvig, 2000 and Okrent, 2010 among many others). And while this literature has advanced our understanding of the rise and fall of the prohibition movement as the confluence of specific political and social forces, there is surprisingly little research in quantitatively assessing the economic and social outcomes of federal prohibition in the United States. In large part, this reflects a misunderstanding of the nature of prohibition. It was not in fact a monolithic policy change with national restrictions on alcohol 'turning off' precisely in 1933. Instead, there was ample geographic and temporal heterogeneity in restrictions on alcohol after federal prohibition due to the decentralized nature of American government and the political concessions necessary to bring about repeal. In particular, the chief compromise for achieving the repeal of federal prohibition was in allowing for local option elections whereby local preferences determine whether a county, municipality, or even ward allows the sale of alcohol (Kyvig, 2000).

Exploiting a newly constructed dataset on variation in prohibition status at the county-year level, this paper asks two questions: what were the effects of the repeal of federal prohibition—and thereby, potential alcohol consumption—on infant mortality? And were there any significant externalities from the individual policy choices of counties and states on their neighbors? Our focus on infant mortality stems from the fact that it is not only a key determinant of life expectancy but also a rough indicator of population health. What is more, infant mortality is an acute, rather than a chronic, outcome of alcohol consumption, making identification a slightly easier, but still challenging task. There is a substantial literature in understanding the drivers of infant mortality in a historical context (cf. Fishback, Haines, and Kantor, 2001; Cutler and Miller, 2005; Fishback, Haines, and Kantor, 2007; Miller, 2008; Moehling and Thomasson, 2014; Clay, Lewis, and Severnini, 2016; Alsan and Goldin, 2019). However, to our knowledge, this is the only study that probes the effects of the repeal of federal prohibition on infant mortality or—for that matter—any other outcome variable.

An important methodological contribution of this paper is to take the count nature of the data seriously in our empirical model, potentially improving on existing methods (see, e.g., Cameron and Trivedi 2013a, 2013b). Nearly all of the literature in economics on the causes of infant death uses OLS to explain variation in infant mortality rates with variation in covariates (c.f., Anand and Bärnighausen, 2004; Baird, Friedman, and Schady, 2011). However, if the number of births is low, observed infant mortality rates become discrete. For example, with ten births, only infant mortality rates of [0.0, 0.1, 0.2,…., 1.0] can be observed. Generally, for low numbers of births, the distribution of these rates is bounded, discrete, and skewed and depends on the number of births. To deal with all this, we implement a binomial fixed-effects model. It has at least three advantages over the standard practice of OLS estimation: it models the discreteness of deaths given births; it can accommodate the observation of zero deaths in a county-year but cannot predict negative deaths; and it automatically accounts for heteroskedasticity induced by variation in the number of births across county-years.

A further important consideration of this paper also comes in explicitly recognizing the possibility of policy externalities across county borders. Thus, after the repeal of federal prohibition, it is not only an individual county's choice of prohibition status which matters but also the prohibition status of its neighbors. In this manner, we distinguish among counties which allow for the sale of alcohol within their borders ('wet' counties), counties which chose to continue with alcohol prohibition and found themselves with neighbors which do the same ('bone dry' counties), and—critically—counties which chose to continue with alcohol prohibition but find themselves with a wet neighbor ('dryish' counties).

For our baseline results, we follow the spirit of the empirical approach laid out in Dube, Lester, and Reich (2010). In particular, we consider triads of counties, defined as sets of three nearby counties wherein one county is bone dry, one county becomes dryish, and one county is wet. These transitions are observed within each triad in our panel of data from 1933 to 1939. Each triad can be thought of as providing an estimate of the treatment effects of dryish and wet status wherein only a nearby county is used as a control. In comparison with an estimation strategy using all US counties, our baseline results use only a subset of counties that are geographically close to a dryish county, potentially diminishing the role of unobserved heterogeneity in driving our results. Further, because variation within each triad identifies our treatment effects, we allow for the possibility that each triad follows a different time trend rather than having a common nation-wide time trend. That is, only within-triad variation over time is used to identify treatment effects.

Using this approach, we find that counties which became wet via local option elections or via state-wide legislation saw baseline infant mortality increase by 4.0%, or 2.40 additional infant deaths per 1000 live births in 1934. Allowing for potential policy externalities from neighboring counties turns out to be important as well: we find that dryish status raised baseline infant mortality by 4.7%, or 2.82 additional infant deaths per 1000 live births in 1934.[1] Putting these estimates into context, from 1934 to 1939, the nation-wide infant mortality rate for the US dropped from 60.0 per thousand live births to 50.2 per thousand in 1939, or by 16.33%. Thus, the repeal of federal prohibition can be thought of as having reversed the generalized decline in infant mortality rates in this period by roughly 25% for the treated counties in our sample.[2] Put differently, when we apply these estimates to all the counties in the US from 1934 to 1939, a rough back-of-the-envelope calculation suggests an excess of roughly 27,000 infant deaths that could be attributed to the repeal of federal prohibition.[3]

Our paper is broadly related to a literature which assesses the effects of state-level measures prior to federal prohibition on variables such as adult heights and weights (Evans *et al*., 2016), the incidence of cirrhosis (Dills and Miron, 2004), and homicide rates (Owens, 2011, 2014; Bodenhorn, 2016). However, we are alone in studying the effects of federal prohibition's repeal and do so in the context of county- as opposed to state-level variation in prohibition laws. Here, we argue that *a priori* county-level information is likely more meaningful. Likewise, our paper is related to recent work by García-Jimeno (2016) which considers the effects of federal

[1] We note that while both estimates are statistically different from zero at conventional levels, they are not statistically distinguishable from one other.
[2] These figures are simply calculated as the ratio of the 4.0 to 4.7% increase in infant mortality rates for all treated counties to the 16.33% decline in the infant mortality rate for the entire US over the same period.
[3] There were 1,113,635 live births in dryish counties from 1934 to 1939 which translates into an excess of 3,140 infant deaths in the same period. Also, there were 9,925,144 live births in wet counties from 1934 to 1939 which translates into an excess of 23,820 infant deaths in the same period. Cumulatively, the number of infant deaths which could potentially be attributed to the repeal of federal prohibition is 26,960.

prohibition on city-level crime during the period from 1920 to 1933. There, local enforcement of federal prohibition laws not only generates extra-judicial homicides and other forms of crime but also responds endogenously to perceptions of its efficacy in the immediate past. Thus, our paper shares at least one element with his work, namely an appreciation of the potential divergence between *de facto* and *de jure* prohibition status, both during and after federal prohibition.

Apart from historical interest, understanding the effects of federal prohibition is important with respect to contemporary policy issues related to alcohol and the control of illicit substances. First, this particular historical setting has unique advantages in estimating the effect of restrictions on alcohol on infant mortality. The US Surgeon General's initial warning about the risks associated with alcohol consumption during pregnancy was issued in 1981. And so, the public at the time had little definitive knowledge of the potential negative effects of alcohol consumption during pregnancy on child development (Warner and Rosett, 1975). Thus, our estimates are arguably not confounded by differences in avoidance behaviors—both avoiding conception and drinking—by mothers of different socioeconomic status (Nilsson, 2017).

Second, recent studies on the effects of alcohol restrictions have predominately focused on relatively small differences in variables such as the minimum drinking age or the availability of beer as opposed to spirits (Carpenter and Dobkin, 2009; Barreca and Page, 2015; Nilsson, 2017). However, little is known about the effects of more stark policy changes where the relative price of alcohol is more dramatically altered. We note that the scope for policy interventions is still large: although information about the risks associated with alcohol consumption during pregnancy is now widely understood in the US, over 50% of women of childbearing age drink while over 10% of women continue to drink during pregnancy (Tan *et al*., 2015).

Finally, our paper speaks to a related literature in public economics which considers differential taxation across state borders in the presence of competition in local markets and its effects on firm pass-through and, thereby, consumer prices for items like alcohol and cigarettes (cf. Doyle and Samphantharak, 2008; Lovenheim, 2008; Harding, Leibtag, and Lovenheim, 2012). This is particularly true if we conceive of prohibition and its repeal as having vitally affected the price—but not necessarily the availability—of alcohol. However, to our knowledge, few papers in this literature have addressed the issue of policy externalities, or how one location's policy choice affects outcomes in another, which this paper so strongly emphasizes. Lovenheim and Slemrod (2010) is a notable exception in that it finds that one state's reduction in the minimum legal drinking age could lead to an increase in teenage traffic fatalities in neighboring states. Likewise, Johansson, Pekkarinen, and Verho (2014) find higher rates of workplace absenteeism in Sweden after a cut in Finnish alcohol taxes. What differentiates our work in this respect is the focus on an entire country (the United States) at the lowest level of geographic aggregation possible (counties) for a more dramatic change in alcohol policy (the repeal of federal prohibition).

Thus, this paper provides at least one valuable history lesson for the present-day debate on legalization of illicit substances, in particular, the recent spate of state-level legislation related to marijuana (cf. Dills, Gofford, and Miron, 2017; Hansen, Miller, and Weber, 2020; Hao and Cowen, 2020). A key insight of our paper is that infant mortality in this period was driven by not only any individual county's choice of prohibition status but also its neighbors' choice of prohibition status. That is, a county or state's choice to go wet and allow for the sale of alcohol in its borders strongly affected infant mortality in neighboring counties which chose to remain dry. It is telling that in every historical discussion on the relative merits and demerits of county-level repeal known to us, none make reference to the possibility of one county's choice affecting another. Likewise, the legal debate on the relative merits and demerits of state-level legalization of marijuana has failed to adequately address the possibility of cross-jurisdictional externalities such as the one documented in this paper.

The rest of the paper proceeds as follows. Section 2 lays out the historical context and the relationship between infant mortality and the repeal of federal prohibition. Section 3 introduces the underlying data while Section 4 introduces our empirical model. Section 5 presents our results on infant mortality for a sample of triads of nearby counties. Section 6 concludes by considering caveats to our study and avenues for future research.

2. Historical background

On a rising tide of an anti-alcohol movement led by rural Protestants and urban progressives, the US Senate proposed a constitutional amendment to affect a federal prohibition on alcohol on December 18, 1917. With the approval of 36 states by January 16, 1919, the 18th amendment was, thereby, ratified with the country becoming dry on January 17, 1920. This entailed a near-complete prohibition on the production, sale, and transportation of alcohol. But by no means did this entail the complete unavailability of alcohol as the individual consumption and possession of alcohol was not explicitly prohibited, allowing for wide differences in enforcement and legislation along these lines at the city, county, and state level. Rather, prohibition is best thought as having substantially increased the price of alcohol (Cook, 2007).

Surprisingly large effects on quantities were forthcoming. In 1934, the first year of repeal, apparent per capita alcohol consumption was 37% of its pre-prohibition peak, an effect which persisted until 1973 (LaVallee and Yi, 2011). However, initial wide-spread support for federal prohibition was eroded throughout the 1920s in the wake of concerns over the new reach of the federal government, doubts over prohibition's efficacy, and perceptions of rising criminal activity (Okrent, 2010).

Turning to its demise, the proverbial nail in the coffin for federal prohibition arose from the fiscal straits of the Great Depression. Prior to 1920, 15% of government revenues came from alcohol with the federal government alone collecting nearly $500 million in 1919, or over $7 billion in 2017 dollars (Blocker, 2006). Thus, starved of other sources of funding, various levels of government increasingly viewed the sale of alcohol as a potential source of revenue. The opening salvo in repealing federal prohibition came on March 22, 1933, when Franklin Roosevelt amended the Volstead Act (or National Prohibition Act), allowing for the resumption of low-alcohol beer consumption and production (Okrent, 2010). From there, popular and political support for prohibition quickly eroded, and the 18th Amendment was repealed on December 5, 1933, with ratification of the 21st Amendment to the US Constitution.

However, the process of repeal was decidedly—and deliberately—not uniform. The chief compromise for achieving ratification of the 21st Amendment was in allowing for local option elections to determine liquor laws deemed appropriate for local conditions (Kyvig, 2000). These elections give the electorate the right to vote on liquor control by referendum. That is, local preferences determine whether a county or municipality prohibits the sale of alcohol. At the same time, many states opted out from local option elections entirely while others allowed for referenda to be held at the state-, county-, city-, or even ward-level. The transition from prohibition was, in many instances, very rapid: by 1935, 2,120 counties became wet in some form while 991 counties stayed dry (Strumpf and Oberholzer-Gee, 2002).[4]

With respect to infant mortality, we draw on a large body of work which explores its causes in a historical context (cf. Fishback, Haines, and Kantor, 2001; Cutler and Miller, 2005; Fishback, Haines, and Kantor, 2007; Miller, 2008; Moehling and Thomasson, 2014; Clay, Lewis, and Severnini, 2016; Alsan and Goldin, 2019). However, this work has little to say about the mechanisms by which the repeal of federal prohibition could have influenced infant mortality. Instead, we turn to a substantial medical literature linking maternal alcohol consumption to both compromised infant immune systems and reduced birth weight—two key determinants of subsequent infant death (cf. Olegård *et al*., 1979; Mills *et al*., 1984; Strandberg-Larsen *et al*., 2009). Unfortunately, we lack any information of maternal alcohol consumption at the individual or aggregate level for this period.[5] Having no other more plausible prior, our proposed causal mechanism for this paper runs from the repeal of federal prohibition to potential maternal alcohol consumption and from there to infant mortality.

Here, we note a few things. First, although women were long among the most vocal proponents of prohibition, we also know that federal prohibition itself lead to more wide-spread alcohol consumption on the part of females as it served to move the place of alcohol consumption from heavily male-dominated saloons to more evenly mixed clubs, homes, and speakeasies (Rose, 1996). This has led some to characterize federal prohibition as having

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[4] More precisely, of the 2,120 counties that became wet in some form, 341 counties were of mixed status—that is, a wet county with at least one dry municipality or vice-versa. In what follows, we treat mixed counties as equivalent to wet counties as our main results are unchanged when making this distinction (results available upon request) and are omitted here for expositional purposes.

[5] Other proxies for alcohol consumption like the number of retail outlets for alcohol, retail sales of alcohol, or tax revenues from alcohol sales are not systematically available at the county level. Likewise, average birth weight and the general health of newborns was not recorded for this period.

domesticized and, thereby, feminized drinking over the period from 1910 to 1930 (Murdock, 1998). Second, another unintended consequence of federal prohibition was a dramatic change from the consumption of beer toward potentially more harmful spirits as beer with a low alcohol-to-volume ratio was also a low value-to-volume product (Warburton, 1932). Third, our argument does not hinge on potential maternal alcohol consumption for all women, rather only on potential maternal alcohol consumption for some women as it is generally thought that a small number of problem drinkers drive the contemporary results linking infant mortality to maternal alcohol consumption (Strandberg-Larsen *et al*., 2009). Finally, we readily acknowledge that other forces may have been at work such as potential paternal alcohol consumption and its effects on domestic violence, postpartum household budgets, and/or prenatal investment. Thus, our estimates may best be thought of as summary measures of the combined effects of relaxing constraints on alcohol availability arising from multiple potential channels. We necessarily leave this task for future work, citing a lack of relevant data at the present.

3. Data

Our data are drawn from three main sources: annual, county-level infant deaths and live births have been extracted from the *Vital Statistics of the United States*; annual, indicators of county-level prohibition status have been constructed from contemporary sources; and other county-level covariates are available from the US Census.

3.1 Infant mortality

Annual counts of infant deaths and live births from 1933 to 1939 for the 3,000+ counties of the continental United States are available from Fishback *et al*. (2011). The choice of a start date in 1933 is predicated by the fact that mortality statistics for Texas, with its 254 counties, only begins in this year. The choice of an end date in 1939 is predicated by the fact that the vast majority of changes in prohibition status had occurred by 1938. We also wish to avoid any confounding effects of the mobilization effort for World War II. We do, however, extend the sample into the 1940s in a robustness exercise below. Yet extending the sample further back in time would only serve to compound the problem of missing data: a 'long' event study approach to consider a balanced pre- and post-period would entail not only the loss of Texas but also the further loss of Georgia, Nevada, New Mexico, Oklahoma, and South Dakota (that is, many states with useful variation in county-level prohibition status as will be seen below). Thus, we argue for a 'short' panel with a larger cross-section for purposes of maximizing statistical power.

Figure 1 depicts infant mortality rates by prohibition status for every year and for all US counties from 1934 to 1939, weighted by the number of births in a county. Over this period, the nation-wide infant mortality rate dropped from 60.0 per thousand live births in 1934 to 50.2 per thousand in 1939 with broadly similar declines across bone dry, dryish, and wet counties. However, it is important to note that the three series depicted do not hold constant the composition of counties under the various headings and so should be interpreted with caution.

3.2 County-level prohibition status

Ideally, we would like individual-level information on alcohol consumption, particularly for pregnant women or women of child-bearing age. Of course, this type of data is not available before, especially during, or even after prohibition. Another possibility would be to rely on other legal restrictions on alcohol, yet liquor laws in the United States appear in stunningly diverse forms: among other things, individual counties and states continue to limit the maximum alcohol content of specific types of beverages sold within their borders, specify whether alcohol can be sold for off- or on-premise consumption for specific types of establishments, and/or place restrictions on the day and time of alcohol sales (so-called 'blue laws').

Instead, we rely on the sharpest distinction in prohibition status possible: dry versus wet. That is, we seek to compare outcomes for those counties for which no sales of alcohol are permitted (dry) to those for which at least some sales are permitted (wet). Again, we also make the critical distinction in between those counties which are dry and have no wet neighbors (bone dry) versus those counties which are dry and have at least one wet neighbor (dryish). Thereby, we decompose all dry counties into either bone dry or dryish counties, respectively.

To achieve this goal, we build on previous data collection efforts. Our starting point is in reconstructing the prohibition status of counties in 1935 and 1940, depicted in the maps of Strumpf and Oberholzer (2002). We then supplement these with new sources to determine the prohibition status of counties in 1934, 1936, 1937, 1938, and 1939 (The Distilled Spirits Council, 1935; Harrison, 1938; Culver and Thomas, 1940; Thomas and Culver, 1940; The Distilled Spirits Council, 1941). For a small number of counties, it was required to use LexisNexis to establish the year in which there was a change in their prohibition status. Thus, we make a significant contribution with respect to data by reconstructing the prohibition status of all continental US counties for the critical post-repeal period from 1934 to 1939.

Figure 2 depicts the proportion of all US counties by prohibition status for the longer period from 1930 to 1942. We assume all counties are bone dry from 1930 to 1933 (in our results below, we partially relax this assumption by excluding those counties on the Canadian or Mexican border in 1933). By 1939, this proportion had dropped from 100% to slightly below 15%. Likewise, we observe the proportion of wet counties rising from 0% in 1933 to slightly above 70% in 1939 and the proportion of dryish counties rising from 0% to slightly above 15% in 1939. Thereafter, there is very little aggregate change in prohibition status throughout the 1940s. Given that the vast majority of changes in prohibition status occurred by 1938, we estimate our empirical model for the period from 1933 to 1939 as it represents the minimal dataset for identifying the effects of repeal. That is, adding years prior to 1933 and after 1939 adds very little by way of variation in our independent variable of interest, namely individual counties' prohibition status. This relatively short panel is also beneficial in that we believe that preferences for alcohol are unlikely to have changed very much over such a short period of time.

Figure 3 depicts the spatial distribution of prohibition status by year for all US counties from 1933 to 1939. There, it is clear that by 1935 the remaining hold-out states for prohibition were along the central axis of the US (Kansas, North Dakota, and Oklahoma) along with large parts of the Southeast (Alabama, Georgia, Mississippi, and Tennessee). This constellation changed considerably by 1937 with Alabama and North Dakota jettisoning state-wide prohibitions and allowing for local option. This along with the steady change in prohibition status at the county level for Georgia, Tennessee, and Texas in later years makes for ample variation.

3.3 Additional covariates

In determining a valid specification relating infant mortality rates to changes in prohibition status, Figure 1 suggests a potentially large role for time-invariant county characteristics. Thus, we include county fixed effects in all of our specifications. There is also a large historical literature delineating variables that shaped support for prohibition and which may be useful as further controls in explaining variation in infant mortality rates. This literature points to strong preferences for dry status before and during the period of repeal among Baptists and Methodists, the native-born, rural inhabitants, and women (Okrent, 2010). To this list, we also include information at the county level on the proportion of blacks, the number of hospital beds and medical institutions per 1000 childbearing age women, per capita New Deal spending, per capita retail sales as a proxy for income, and the unemployed-to-working age population ratio as previous research has indicated that these variables influenced infant mortality in this period (Fishback, Haines, and Kantor, 2001; 2007).[6] Table 1 provides the definition and sources of our control variables.

3.4. Sample selection

In an influential paper, Dube, Lester, and Reich (2010, hereafter DLR) propose the use of county-pairs that straddle state borders to assess the effects of changes in state-level minimum wage laws in the United States. As in much of the earlier literature, their approach exploits variation in the level of minimum wages induced by differential state legislation. However, in comparison with a standard difference-in-differences approach relying on regressions with county and time fixed effects, their approach offers two innovations. First, they use only neighboring counties as controls, arguing that these neighbor counties provide a better control than the entire collection of untreated counties. Second, because each county-pair provides identifying variation for the treatment effect, they allow for the possibility that each county-pair follows a different time trend rather than having one nation-wide time trend. This means that, in

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[6] We use these variables strictly as controls. They would not be valid as excluded instruments in IV regression analysis as they have direct effects on infant mortality and so would not satisfy the exclusion restriction.

contrast to a national level regression with time and county fixed effects, they use only within county-pair variation to identify treatment effects.

For our baseline analysis, we follow the spirit of the DLR approach by using triads of geographically proximate counties which are constructed in the following way. We first identify the 715 *ever-dryish* counties that became dryish sometime from 1934 to 1939. We retain only the 698 *ever-dryish* counties that progressed 'monotonically' from bone dry to dryish or from bone dry to dryish to wet from 1933 to 1939. This excludes counties that alternate between bone dry and dryish status, for example. These *ever-dryish* counties form the center of each triad. For each of these 698 *ever-dryish* counties, we consider the year in which that county became dryish and find the wet county whose county seat is nearest to that of the *ever-dryish* county. This county is the *wet partner* in the triad. For the *dry partner* in the triad, we again consider the year in which the *ever-dryish* county became dryish and find the bone dry county whose county seat is nearest to that of the *ever-dryish* county. Of the 698 triads so constructed, we retain the 683 triads where both the *dry partner* and the *wet partner* progress 'monotonically'. That is, the *wet partner* stays wet and the *dry partner* stays bone dry or goes from bone dry to dryish, from bone dry to dryish to wet, or from bone dry to wet during 1933 to 1939. This excludes triads with counties that alternate between bone dry and wet status, for example.

To summarize, each triad is comprised of three nearby counties: one county that is treated with dryish status in the center of the triad (*ever-dryish*); one nearby county that is treated with wet status (*wet partner*); and one nearby bone dry county that acts as the control county (*dry partner*). Our data set is constructed by observing the three members of each triad from 1933 to 1939, resulting in 14,343 observations. These triads have time-invariant members, but their members may be overlapping. That is, one county may be a member of two or more triads. For any county that shows up in k triads, we would see that county's data replicated k times. Following DLR, we adjust our inference in this context by using standard errors clustered at the county-year level.

An advantage of only using triads comprised of geographically proximate counties is that such counties make good controls: they are likely to be more similar along various dimensions. In comparison to using the full sample of all US counties, there is presumably much less heterogeneity—both observed and unobserved—when we focus only on triads of nearby counties. This intuition is borne out in an examination of the pre-trends in our triads as discussed below. A second advantage of this approach is that we can allow for triad-specific time-trends in the analysis. Hence, only variation within triads is used to identify the treatment effects. The basic idea is that we can aggregate across many treatments (that is, many triads), imposing a common trend assumption within triads but not imposing a common trend assumption across triads.[7]

One disadvantage of this approach is that the results, strictly speaking, may not be nationally representative. A further disadvantage of only using triads is that we lose power in comparison to an approach that uses data from all US counties as we do not consider those counties that are not proximate to an *ever-dryish* county. Indeed, our 683 triads cover only 1,301 counties of the 3,043 counties in the US at that time. Most of our reported estimates will be based on models following the approach wherein we use data on this subset of US counties and include county fixed effects and triad-year fixed effects. In Appendix A, we expand our analysis to include all US counties and incorporate only county and year fixed effects for completeness.

[7] As in DLR, we do not impose the restriction that group of triads (in their case, county-pairs) with overlapping members have identical time-trends across triads (county-pairs). Instead, the model allows these time trends to be the same, but does not impose it.

Even though we find qualitatively similar results, we argue for using the sample of triads of nearby counties on the basis of an analysis of pre-trends in infant mortality rates.

As our econometric strategy is analogous to difference-in-differences, the key assumption in estimation is a common-trends assumption that treated counties would have followed the same time trend as untreated counties had they not been treated. Under this 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.

Figure 4a tracks infant mortality rates (weighted by the number of births in a county) for the period from 1928 to 1933 for our sample of triads of nearby counties (n = 1,156). Here, we use only the counties where we observe vital statistics back to 1928, which leaves out, for example, Texas. We consider pre-trends back only to 1928 because the number of states reporting vital statistics drops dramatically before that. We employ the three non-exclusive categories of *ever-dryish*, *dry partner*, and *wet partner*. Thus, the composition of counties is held constant in Figure 4a. A general decline in infant mortality rates is evident throughout this period for all three county types, and the general ordering of counties by type is preserved when considering all years between 1928 and 1933: infant mortality rates are highest for *wet partner* and lowest for *dry partner* with *dryish* counties in between. In between these years, *dry partner*, *ever dryish,* and *wet partner* exhibit highly similar pre-trends. In other words, Figure 4a suggests that the common-trend assumption holds for the sample of triads of nearby counties.

Another means of validating our approach of using triads of nearby counties comes in considering the pre-trends for all available US counties. Here, the number of counties with data availability is 2,660, again due to the fact that all state returns were reported to the National Vital Statistics System only from 1933. Figure 4b tracks infant mortality rates (weighted by the number of births in a county) for the period from 1928 to 1933 as before but with one requisite adjustment to the definition of groups. *Bone dry* are dry counties which are surrounded by other dry counties throughout this period. *Ever dryish* are the counties which became dryish at any time from 1933 to 1939. *Wet* are the counties which allow for alcohol sales within their borders. Unlike in Figure 4a, the common trend assumption does not seem to hold for all available US counties.

Table 2 reports sample means for our sample of triads of nearby counties and for three non-exclusive groups: *ever-dryish* counties, their *dry partners*, and their *wet partners* (the same classification scheme as in Figure 4a). Specifically, we report the means of our proposed county-level control variables in or around 1933, all weighted by the number of births. Most of these are time-invariant, and their values from around 1930 are reported except for retail sales (a proxy for income), the number of medical institutions per capita, and the number of hospital beds per capita which are available for each county-year. Table 2 also reports the p-values for the null hypothesis that the means in the initial year are the same across the three groups in Column (4). However, what is most relevant for the underlying assumptions of the difference-in-differences framework is that the differences in covariates across years—and not differences in their means in the initial year—are balanced across treatment groups. To this end, columns (5)-(7) report differences over time by each treatment group. Likewise, column (8) reports p-values for the null hypothesis that the differences are the same across the three groups. Here, only changes in retail sales per capita are statistically significant from one another, suggesting that wet-partner counties saw larger increases in retail sales than their dry-partner and ever-dryish counterparts. At the same time, we directly control for retail sales per capita in all specifications reported below.

Finally, while it is reassuring that the common-trends assumption seems to hold for our sample of triads of nearby counties, we nonetheless include interactions of all county-level, time-invariant control variables in Table 2 with linear time trends in all specifications as in Acemoglu, Autor, and Lyle (2004) and Hoynes and Schanzenbach (2010) to control for potential differences in trends across counties which may be correlated with counties' prohibition status.

4. Econometric model

The data generating process (DGP) we have in mind is one where a county's alcohol prohibition status (the 'treatment') influences the probability of infant death (the 'response') for each birth. Each birth is a Bernoulli random variable whose probability of death depends on the treatment and other covariates, and we observe the sum of births and the sum of deaths in each county-year. This DGP and data environment is described by the binomial model.[8]

To our knowledge, ours is the first paper in the literature on infant mortality that has used the binomial model. Standard practice in this literature is to use OLS to explain variation in logged infant mortality rates with treatment status and other covariates. In contrast, the support of the binomial distribution is discrete and bounded, and its variance is a function of its probability and the number of births. This gives our approach three advantages over standard practice: (1) it models the discreteness of deaths given births; (2) it allows for the observation of zero deaths in a particular county-year and cannot predict negative deaths; and (3) it accounts for

[8] The word 'binomial' appears in the names of many distributions. To be clear, our binomial model, described formally in equations (1) and (2), is *not* a binomial logistic distribution or a negative binomial distribution.

the heteroskedasticity of the infant mortality rate induced by variation in the number of births across county-years.

The first advantage of our approach relative to OLS is that it directly models the discreteness of the outcome variable. In our data, we observe many counties with small numbers of births and, therefore, very small numbers of infant deaths. The discreteness of our data is very sharp when the number of births is low. Consider a county at the 10th percentile of births in our data, with 98 births. Given that the infant mortality rate is roughly 5% in our sample, we would expect roughly 5 infant deaths. But they would be distributed over the natural numbers {0, 1, 2,…, 98} rather than over the unbounded continuum. Further, this distribution would be asymmetric: it would be centered on 5, but it would be truncated below by 0 and above by 98.

The second, related advantage of our approach is that it does not predict negative values for deaths and can accommodate observations with zero infant deaths. Linear regression of the infant mortality rate on covariates could easily yield predicted values for the infant mortality rate of less than zero. In contrast, in the binomial model, the prediction is a probability mass function that is bounded from below at zero. Horrace and Oaxaca (2006) note that if the OLS estimator predicts below zero, as would be the case if any covariate included linearly had infinite support, then it is inconsistent. This problem is not solved, for example, by using logged infant mortality rates because in that case, county-years with zero infant deaths have to be dropped or scaled by adding an arbitrary constant to these observations. Both of these strategies induce inconsistency in the estimator whereas incorporation of observations with zero deaths is natural in the binomial model. This issue is relevant in our context: we observe 135 county-years in our estimation sample (and 353 county-years in the nation-wide sample) with zero deaths.

The third advantage of using the binomial distribution for modeling infant mortality is that because it models the binomial probability mass function of infant deaths given the number of births, it automatically takes the heteroskedasticity implied by the DGP into account. This is analogous to the efficiency gain from using weighted least squares (WLS) in comparison to OLS when faced with grouped data. In Appendix B, we compare results from OLS, WLS, and our approach. The bottom line of Appendix B is twofold: first, OLS produces substantially different estimates from WLS and our approach; second, while WLS and our approach produce similar estimates, our approach yield more precise estimates. From a practitioner's point of view, this is the main advantage of our approach over the industry-standard approach of using OLS to model logged infant mortality rates: our approach yields smaller standard errors.

Our data form a balanced panel of time periods ($t=1,\ldots, T$), triads ($s=1,\ldots, S$), and counties within triads ($c=1,2,3$). Our treatment variables are county-year level indicators of whether or not a county is itself wet, W, and of whether or not a dry county (with $W=0$) has at least one neighboring county that is wet, N.[9,10] Our source on infant mortality, the *Vital Statistics of the United States*, reports the number of infant deaths in the year after birth while births occur roughly nine months after conception. Thus, most infant deaths caused by the relaxation of alcohol prohibition would occur in the years following the change in legal status and not during that year. Furthermore, for most counties, we do not have the exact date of changes in status.

Thus, we create variables W_{cst} and N_{cst} which are equal to one in all the years following the change in legal status and equal to zero in all other years. Additionally, we create variables

[9] These are coded to be mutually exclusive by giving priority to W; for example, a county that is wet and has a wet neighbor has $W=1$ and $N=0$. The excluded category ($W=N=0$) is a bone dry county.
[10] In our sample of triads of nearby counties, all counties start in 1933 with $W=N=0$. At some point, the wet partner switches to $W=1$, and in that same year, the dryish county switches to $N=1$. The dry partner has $W=N=0$ through the year of the switch and possibly thereafter as well.

W_{cst*} and N_{cst*} which are equal to one in the year of the legal change and equal to zero in all other years. These variables pick up partial treatment effects in the year of the legal change. Let P_{cst} be the vector of prohibition status variables [W_{cst}, N_{cst}, W_{cst*}, N_{cst*}]. Let γ_{cst} be the probability that a birth results in an infant death. We observe a total of B_{cst} births and D_{cst} infant deaths in a given county-year. The probability mass function of the number of infant deaths, D_{cst}, given the number of births, B_{cst}, follows the binomial distribution:

$$Pr[D_{cst}; B_{cst}, \gamma_{cst}] = \frac{B_{cst}!}{D_{cst}!(B_{cst}-D_{cst})!}(\gamma_{cst})^{D_{cst}}(1-\gamma_{cst})^{B_{cst}-D_{cst}} \quad [1]$$

The probability γ_{cst} is our object of interest, and it depends on the prohibition status variables, P_{cst}, and other covariates. We condition the probability γ_{cst} on a vector of observed control variables, X_{cst}, which includes both the time-invariant county-characteristics interacted with time trends and the time-varying controls reported in Table 2. We additionally include county-fixed effects, denoted $F_{cst}\theta$, to account for time-invariant heterogeneity across counties. Here, F_{cst} is a vector of county fixed effects, and θ is the vector of their corresponding coefficients. F_{cst} is subscripted because it gives the county identity of each county in each triad in each year.

Standard difference-in-differences approaches would include time fixed effects in addition to the above county fixed effects and covariates. In our assessment of robustness at the end of Section 5, we report results for a standard county- and time-fixed effects difference-in-differences model where all counties are assumed to follow the same time trend (additional results along these lines are reported in Appendix A). However, in our main specification, we control for potentially different time trends in each triad by including triad-specific time fixed effects δ_{st} for each triad s in each time period t. Because we include triad-year fixed effects, we also allow for arbitrary serial correlation at the triad level.

We specify the probabilities as given by the inverse logit function of a linear index:[11]

$$\gamma_{cst} = \gamma(P_{cst}, X_{cst}, F_{cst}) = \frac{exp(P_{cst}\alpha + X_{cst}\beta + F_{cst}\theta + \delta_{st})}{1 + exp(P_{cst}\alpha + X_{cst}\beta + F_{cst}\theta + \delta_{st})} \qquad [2]$$

The first and second advantages described above result from the fact that we bound the probabilities between zero and one and embed these probabilities inside the binomial model.

The marginal effect of changing prohibition status P_{cst} on the probability of infant mortality is equal to $\alpha\gamma_{cst}(1 - \gamma_{cst})$. In the case where infant mortality rates are low (for example, they average around 5% in our sample), this is approximately equal to $\alpha\gamma_{cst}$. Thus, one may interpret α as approximately equal to the semi-elasticity of the probability of infant death with respect to changes in prohibition status.

The expected value of infant deaths for the binomial defined by equations [1] and [2] is $E[D_{cst}] = \gamma_{cst}B_{cst}$. We can, therefore, write the model as one with error terms ε_{cst} :

$$D_{cst} = \gamma_{cst}B_{cst} + \varepsilon_{cst}. \qquad [3]$$

Substituting [3] into [1] and rearranging slightly, we arrive at the probability mass function for the error term ε given D, B, and γ:

$$Pr[\varepsilon_{cst} = D_{cst} - \gamma_{cst}B_{cst}; B_{cst}, \gamma_{cst}] = \frac{B_{cst}!}{D_{cst}!(B_{cst} - D_{cst})!}(\gamma_{cst})^{D_{cst}}(1 - \gamma_{cst})^{B_{cst} - D_{cst}}. \qquad [4]$$

This error term is conditionally mean-zero by construction and has variance $V(\varepsilon_{cst}) = \gamma_{cst}(1 - \gamma_{cst})B_{cst}$. The former implies that we can form a moment condition, and the latter makes clear the heteroskedasticity implied by the model. We use the moment condition

$$E[D_{cst} - \gamma(P_{cst}, X_{cst}, F_{cst})B_{cst}]|_{P_{cst}, X_{cst}, F_{cst}} = 0, \qquad [5]$$

or, equivalently, where $I_{cst} = D_{cst}/B_{cst}$:

$$E[B_{cst}(I_{cst} - \gamma(P_{cst}, X_{cst}, F_{cst}))]|_{P_{cst}, X_{cst}, F_{cst}} = 0, \qquad [6]$$

[11] We do not include a constant term in the linear index as we have a full vector of county fixed effects.

and estimate the model by the method of moments.[12] The model is exactly identified but nonlinear in the parameters because γ is a nonlinear function of its arguments.

Equation [6] shows how the method-of-moments estimator accounts for heteroskedasticity: each observation of an infant mortality rate and regressors is weighted by the number of births B_{cst}. In contrast, nonlinear regression of the infant mortality rate on the inverse logit function of regressors has the unweighted moment condition $E[I_{cst} - \gamma(P_{cst}, X_{cst}, F_{cst})]|_{P_{cst}, X_{cst}, F_{cst}} = 0$. OLS regression of infant mortality rates on regressors further assumes that γ is linear in its arguments. Thus, the standard OLS approach has continuous predictions of deaths that may stray below zero. Furthermore, it is unweighted and so does not account for heteroskedasticity. WLS estimation accounts for heteroskedasticity, but it still yields continuous and unbounded estimates of deaths. Our approach deals with heteroskedasticity by appropriately weighting the moment condition, predicts discrete values of the outcome variable, and predicts strictly positive numbers of infant deaths.

At the same time, the moment estimator defined by equation [5] suffers from an incidental parameters problem that induces bias when one or more of the indices (in our case, *c, s,* and *t*) does not go to infinity, but rather is fixed and small (Machado, 2004). In our case, both *T* and *S*, the number of time periods and the number of counties in a triad, are fixed and small (at seven years and three counties per triad, respectively). Generally, this bias is of order *1/T* or *1/S*. In Monte Carlo experiments, Machado (2004) finds that the incidental parameters bias in the

[12] We use the one-step moment estimator that solves the sample analog of [5].

$$\sum_{c=1}^{3}\sum_{s=1}^{S}\sum_{t=1}^{T}(D_{cst} - \gamma(P_{cst}, X_{cst}, F_{cst})B_{cst})Z_{cst} = 0,$$

where Z_{cst} is the list of regressors, P_{cst}, X_{cst}, F_{cst}, and the triad-year fixed effects. The moment estimator chooses parameters to make the error terms exactly orthogonal to the regressors. We note that this is exactly equal to the first-order conditions of the likelihood function for the model, and so, it also characterises the maximum likelihood estimator (MLE). That is, the MLE equals the method-of-moments estimator in this instance.

binomial fixed effects model is small for $T > 4$. Hahn and Newey (2004) provide an analytical bias correction for general nonlinear fixed effects models. In our context, the bias correction may be written explicitly in terms of observed variables and is straightforward to compute. Estimates that incorporate these corrections are still biased, but only of order $1/T^2$ or $1/S^2$. In the main text of this paper, we present only bias-corrected estimates. Consistent with Machado's observation that this bias is small, none of our bias corrections exceed 1% of the uncorrected estimate.[13]

We cluster the standard errors at the county-year level to obtain robust inference for our setting where some county-years are observed in more than one triad. As we note above, because triad-year fixed effects are included, we allow for arbitrary serial correlation at the triad level. In Appendix B, we present alternative standard errors for our estimates that cluster at the county-level rather than the county-year level. These estimated standard errors are almost identical to those reported in the main text.[14]

Another consideration is the potential bias induced if we omit variables that are correlated with both the treatment and infant mortality. To the extent that local preferences which induce policy changes (the treatment) are fixed over time, the inclusion of county fixed effects fully accounts for such preference variation. Furthermore, to the extent that over-time

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[13] Machado (2004) shows that the common parameters α and β in the model given by equations [1] and [2] are identified: there is only one solution for those parameters given the joint distribution of births, infant deaths, and covariates implied by the DGP. She proposes a consistent estimator for this fixed-T binomial fixed-effects model. However, the complexity of her estimator rises with the factorial of the total number of deaths in a panel, analogous to Chamberlain's 1980 fixed-T logit fixed-effects estimator. In our data environment, her estimator is computationally infeasible, so we use Hahn and Newey's fixed-T bias correction instead. An alternative strategy is to use the Poisson fixed effects model where the exposure variable is births. This model does not suffer from an incidental parameters problem. This model and our model are asymptotically equivalent if the number of deaths converges to a constant as the number of births goes to infinity which *a priori* we believe to be an implausible restriction. Nonetheless, we estimated the Poisson fixed effects model and found qualitatively similar—albeit quantitatively larger—results than those reported in the text. These results are provided in Appendix B.

[14] However, the standard errors clustered at the county level are not strictly valid because equations [1] and [2] do not allow for serially correlated unobserved variables that affect the probability of infant death. Furthermore, the bias correction of Hahn and Newey is not valid in the presence of such serial correlation.

change in preferences is common across the nearby counties in each triad, the inclusion of triad-year fixed effects fully accounts for such preference variation. Finally, as noted above, we include the interaction of all county-level time-invariant county characteristics with time trends among the regressors. Since these regressors are known to influence the timing of becoming wet, their interaction with time trends should pick up a substantial fraction of any county-level, time-varying factors that are correlated with the treatment.

5. Results

We now turn to our estimates of the effects of repeal on infant mortality. We are particularly interested in the effect of one county's decision to go wet on infant mortality within its own borders and on infant mortality in neighboring counties. Thus, we relate variation in infant mortality on a county-level basis to variation in dryish and wet status along with a large set of control variables detailed in section 3.

Table 3 report our results for method-of-moments estimation of the binomial fixed effects model. Asymptotic standard errors, clustered at the county-year level, are provided in parentheses. The leftmost columns of Table 3 show estimates including only wet variables as treatment regressors. Importantly, dryish counties are here pooled with the control group. Columns (1) through (3) explain variation in infant mortality rates as a function of county and triad-year fixed effects and the covariates from Table 2 along with a variable indicating whether a particular county switched to wet status in a given year (*wet in initial year* or W_{ct*} as in section 4) and an additional variable indicating whether a particular county had previously switched to wet status (*wet in subsequent years* or W_{ct} as in section 4). Regardless of the specification used, the coefficients attached to these indicators are small in magnitude and imprecisely estimated.

However, this specification by ignoring potential cross-border policy externalities masks the effects on infant mortality of both a county becoming wet and a county becoming dryish.

Accordingly, the rightmost columns of Table 3 separate bone dry and dryish counties. What is critical here is that the control group between the two sets of columns varies: for columns (1) through (3), the control group is all dry counties—that is, both bone dry and dryish counties—while for columns (4) through (6), the control group is only bone dry counties. Here, the coefficients attached to *dryish in subsequent years* are consistently positive and statistically significant at conventional levels across all specifications in columns (4) through (6). In a related fashion, the decision to separate bone dry and dryish counties also has an important implication on the coefficient associated with wet status. In particular, it becomes larger in magnitude and statistically significant. Previously, in columns (1) through (3) our control group—that is, all dry counties—were contaminated by the presence of dryish counties. By separating out dryish counties, the coefficient on *wet in subsequent years* becomes larger relative to the new control group of bone dry counties.

Drilling down into the results presented in columns (4) through (6), our first specification in column (4) explains variation in infant mortality rates as a function of county and triad-year fixed effects and the baseline covariates from Table 2 along with the full set of variables indicating a particular county's prohibition status as detailed in section 4. The result for *wet in subsequent years* suggests that the effect of one county going wet corresponds to a 4.1% increase in infant mortality for affected counties in all years after the initial change in prohibition status. Likewise, we interpret the estimate on *dryish in subsequent years* as representing the effect of one county's (or state's) decision to go wet on neighboring dry counties. This corresponds to a

4.6% increase in infant mortality for affected counties in all years after the initial change in prohibition status.

Column (5) includes a measure of cumulative, per capita New Deal spending by county from March 1933 to June 1939. Our principle concern here is that prohibition status might be correlated with both infant mortality and New Deal spending in ways which are not apparent from casual inspection of the data. That is, perhaps New Deal spending was targeted at counties—such as those in the southern United States—with both high levels of infant mortality and subsequent changes to either dryish or wet status. Reassuringly, the results on *dryish in subsequent years* and *wet in subsequent years* are virtually identical across columns (4) and (5).

A similar concern relates to the possibility that changes in prohibition status at the county level were motivated by a desire to increase local government revenue and that enhanced revenues flowed into public health initiatives to combat a high incidence of infant mortality. Thus, column (6) further controls for measures of hospital beds and medical institutions per 1000 women of childbearing age by county to account for differential access to health care which may potentially confound our estimates. Again, there is virtually no change in our results from the inclusion of these additional controls. Therefore, in what follows, we take our results in column (6) as our preferred specification.

Importantly, we do not emphasize the differences in magnitude on *dryish in subsequent years* and *wet in subsequent years* seen throughout as the coefficients are not different from one another in terms of statistical significance. For example in column (6), the p-value of the test where the null hypothesis is equality of coefficients across *dryish in subsequent years* and *wet in subsequent years* is equal to 0.543. Thus, we fail to reject the null hypothesis that these two coefficients are equal to one another at conventional levels. Additionally, in Appendix C, we present an illustrative model of individual-level willingness-to-pay (WTP) for alcohol and county-level prohibition status. There are several lessons that we draw from this framework are important for understanding our empirical results: (1) the distribution of WTP varies across counties and determines whether or not a county goes wet as well as the treatment effect of that choice—that is, we will have heterogeneous treatment effects; (2) if the WTP distributions—or in other words, the underlying preferences of individuals—within counties are invariant over relatively short periods of time (as in our sample), then the inclusion of county fixed effects in the empirical model is sufficient to obtain unbiased estimates of the average treatment effect on the treated (ATT) of becoming dryish or wet; and (3) the ATT for becoming wet could either be larger or smaller than the ATT for becoming dryish.

In columns (4) through (6), we also see that both *dryish in initial year* and *wet in initial year* are much smaller in magnitude than their counterparts, *dryish in subsequent years* and *wet in subsequent years*. Additionally, they are not statistically significant at conventional levels. For example in column (6), the estimate for *dryish in initial year* is only 0.009 (with a p-value of 0.55) while that for *dryish in subsequent years* is 0.047 (with a p-value of <0.01). As discussed in section 4, the *Vital Statistics of the United States* reports the number of infant deaths in the year after birth while births occur roughly nine months after conception. Thus, most infant deaths caused by the relaxation of alcohol prohibition would occur in the years following the change in legal status and not during that year.

Summarizing, our estimates suggest that counties which chose wet status via local option elections or state-wide legislation saw infant mortality increase by 4.0%, or 2.40 additional infant deaths per 1000 live births in 1934. Allowing for potential policy externalities from neighboring counties turns out to be very important as well: we find that dryish status raised baseline infant mortality by 4.7%, or 2.82 additional infant deaths per 1000 live births in 1934. Putting these estimates into context, from 1934 to 1939, the nation-wide infant mortality rate for the US dropped from 60.0 per thousand live births to 50.2 per thousand in 1939, or by 16.33%. Thus, the repeal of federal prohibition can be thought of as having reversed the generalized decline in infant mortality rates in this period by roughly 25% for the treated counties in our sample.

Another way of contextualizing these results would be in terms of a nationwide count of excess infant deaths due to the repeal of federal prohibition. While caution is warranted here, we can provide a rough back-of-the-envelope calculation by extrapolating the estimates from our sample of triads of nearby counties to the national population in the following manner. There were 1,113,635 live births in dryish counties from 1934 to 1939 which translates into an excess of 3,140 infant deaths in the same period. Also, there were 9,925,144 live births in wet counties from 1934 to 1939 which translates into an excess of 23,820 infant deaths in the same period. Cumulatively then, the number of infant deaths which could potentially be attributed to the repeal of federal prohibition is 26,960.

How does these figures compare to other estimates in the literature for the same period? By our reckoning, the increase in infant mortality due to the repeal of federal prohibition fully counteracted the decline in infant mortality from 1908 to 1933 which was associated with the establishment of county health departments charged with improving both primary health services and sanitation considered in Hoehn-Velasco (2018). It also more than matched the decline in infant mortality which was associated with the introduction of the Rural Electrification Administration from 1935 to 1940 considered in Kitchens and Fishback (2013). In cumulative terms, the figure of 23,820 excess infant deaths for the period of 1934 to 1939 alone exceeds the implied count of excess infant deaths associated with the historical expansion of coal-fired electricity generation in the United States from 1938 to 1962 consider in Clay, Lewis, and Severnini (2016). Thus, the repeal of federal prohibition seems to have had sizeable effects on infant mortality in both absolute and relative terms.

These results also highlight two of the main arguments of this paper. First, the distinction between bone dry and dryish counties turns out to be critical, both in terms of estimation and interpretation. This finding suggests that cross-border policy externalities are likely important, both in contemporary and historical settings. Second, for whatever benefits the repeal of federal prohibition conferred in terms of consumer welfare, diminished expenditure on law enforcement, and/or freedom of choice, it also came at the cost of increasing baseline infant mortality rates in both dryish and wet counties. Naturally, there were other associated costs which remain unexplored in this paper, but which should be added to any reckoning of repeal's legacy.

In what follows, we subject these results to a series of robustness checks. Critically, the results in Table 3 on the causal effects of repeal appear to be robust across all specifications considered.

What about the role of unobservables in driving the estimated effects of wet status?

Naturally, there may still be lingering concerns over the exogeneity of wet status. Even though we have included county fixed effects and a battery of county-level controls interacted with time trends in columns (1) through (6) of Table 3, the possibility remains that other time-varying unobservables are driving both a county's incidence of infant death and its prohibition status. To this end, we make a distinction in between those counties which went wet through state legislation (*wet state*) and those which went wet through local option (*wet county*). The reason for doing so is that the former changes in prohibition status are arguably more exogenous than the latter from the perspective of individual counties. That is, a county's inhabitants could have strong preferences for remaining dry but find themselves residing in a state with strong preferences for becoming wet. Thus, such counties in wet states may be thought of as rough analogs to their dryish counterparts.

Table 4 presents results which separate counties with wet status into two bins, *wet county* and *wet state*, while no changes are made to *dryish*. Column (1) makes this distinction with our baseline controls. Column (2) incorporates per capita New Deal spending by county. Column (3) does the same but controls for differential access to hospital beds and medical institutions. Once again, there is a very high degree of comparability across these three specifications. As one would expect, we see no change as it relates to our previous results on *dryish*. However, some interesting results emerge for counties which went wet through state legislation. In particular, the coefficients for *wet state in subsequent years* are, for the most part, statistically significant and virtually indistinguishable in magnitude from *dryish in subsequent years*. In our preferred specification (column 3), the point estimate on *dryish in subsequent years* is 0.047 while that on *wet state in subsequent years* is 0.045.

For counties which went wet through local option, the coefficient for *wet county in subsequent years* is 0.039 in column 6. Thus, it is economically meaningful and statistically significant at the 5% level. And while this estimate is slightly smaller than those for *dryish in subsequent years* as well as *wet state in subsequent years* in the same specification, it is not statistically distinguishable from these other estimates. Thus, counties that opted for wet status through local option have very similar outcomes as those that went wet through statewide legislation. To the extent that the timing of transition to wet status for the latter set of counties is more exogenous than for the former set of counties, this result suggests that our strategy of using difference-in-differences in the context of triads of nearby counties is sufficient to deal with the endogeneity of the timing of changes in prohibition status. Finally, we again find it reassuring that the coefficients for both *wet county in initial year* and *wet state in initial year* are neither large in magnitude nor statistically significant.

Are the estimated effects robust to other specifications?

Table 5 incorporates other specifications to further establish the robustness of the dryish and wet effects. Specifically, column (1) replicates our preferred results from column (6) in Table 3 for ease of comparison, and further checks on this specification are incorporated in turn. First, linear trends interacted with our county controls may be obscuring important variation on a year-by-year basis which may be correlated with our dryish and wet indicator variables. Therefore, it may be appropriate to incorporate more flexibility into our specification and include our county controls interacted with year fixed effects. However, if anything, the results of this exercise reported in column (2) suggest the contrary: the coefficients on *dryish in subsequent years* and *wet in subsequent years* remain virtually unchanged.

Column (3) excludes counties which border Canada or Mexico to account for potential cross-board smuggling with materially the same results. Similar results are obtained when we exclude all coastal counties. Visual inspection of Figure 3 reveals why: relatively few of the geographically proximate counties in our sample of triads are affected by these restrictions.

In our preferred specification, we are unable to control for county-level, time-varying unobservables through the use of county and triad-year fixed effects. Of course, county-year fixed effects are infeasible, but county and triad-year fixed effects along with state-year fixed effects are not. Column (4) reports the results of this regression. For the most part, it mirrors the results for column (1). The only exception in this regard is the reduction in the size of the coefficient for *wet in subsequent years*. Although the coefficient on *wet in subsequent years* becomes statistically insignificant, we do not put much interpretive weight on this result as the category of wet includes both counties which opt for wet status and counties within states which opt for wet status. By including state trends (or state-year fixed effects), we thereby eliminate any variation coming from wet states. Instead, we emphasize that the coefficient on *dryish in subsequent years* is remarkably robust even after conditioning on a very large set of controls. Consequently, the results in column (4) are encouraging in that they control for any number of unobserved contemporaneous changes to government programs, legislative enactments, and local economic conditions that vary at the state-year level such as the hygiene campaigns associated with the women's suffrage movement (Miller, 2008).

Another potential concern relates to conditional convergence in infant mortality across time. In other words, counties with a high incidence of infant mortality at the beginning of the 1930s might have experienced more dramatic declines in the same for reasons which were unrelated to repeal such as the diffusion of best practices in neonatal health. To account for this possibility, column (5) directly incorporates counties' infant mortality rate in 1933 as a control. As with many of our other controls, the initial infant mortality rate is interacted with a linear time trend. Apart from some marginal and statistically insignificant differences in the values of the coefficients, the results are the same.

We also extend the sample up to 1941 in column (6). Previously, we argued for a terminal date of 1939, given that there is little variation in treatment status after that date. Here, we consider a terminal date of 1941 to extend the sample but avoid any effects that World War II and its associated mobilization effort might have had on infant mortality. The coefficient values for and statistical significance of *dryish in subsequent years* and *wet in subsequent years* persist.

We also consider the placebo effect of adding lead terms for the treatment variables in our preferred specification in column (7) of Table 5. That is, if a county becomes dryish in 1935, we assign a value of one for a new indicator variable for this county in 1934. For both dryish and wet status, no lead terms are individually or jointly statistically significant in column (6), suggesting that we are not picking up the residual effects of unobserved county characteristics in our preferred specification.

A final specification using our triad-based sample of nearby counties considers the importance of the triad-year fixed effects. We report in column (8) estimates from a model where the triad-year fixed effects δ_{st} are constrained to be identical for all triads (so that $\delta_{st} = \delta_t$). This is equivalent to a model with just county and year fixed effects. The coefficient on *wet in subsequent years*, 0.039, is nearly identical to our preferred results in column (1), 0.040. At the same time, the coefficient on *dryish in subsequent years*, 0.033, is now 30% lower (albeit still statistically significant at the 5% level).

Thus, the exclusion or inclusion of triad-year fixed effects does not affect the estimated value of the wet treatment effect, and the strategy of including county and year fixed effects would not be very misleading for estimation of the wet effect. However, the estimated value of the dryish treatment effect does depend on whether or not triad-year fixed effects are included. This implies that the DLR innovation of allowing for heterogeneous time trends is important for consistent estimation of the dryish effect. Appendix A considers an extended robustness exercise incorporating all US counties and their spatial distribution into the empirical model. This approach necessitates the use of county and time year fixed effects. All of the results presented there are qualitatively similar to our baseline results in Table 3 and quantitatively similar to the results in column (7) of Table 5. However, they come with the caveats discussed here.

Finally, Appendix D addresses concerns related to selection driving some of our results. In particular, these relate to concerns that: (1) individuals may have migrated to counties in response to the respective maintenance or repeal of prohibition at the local level; (2) changes in prohibition status at the local level may have induced changes in underlying fertility patterns or the incidence of stillbirth; and (3) low SES mothers (that is, those with a potentially higher risk of infant death) may have experienced more unplanned pregnancies in response to the increased availability of alcohol. A separate consideration of: (1) county-level measures of net migration in 1940 finds no systematic relationship in between changes in prohibition status and county-level changes in population (after accounting for births and deaths); (2) county-level counts of annual live births finds no systematic relationship between changes in prohibition status and county-level fertility or stillbirths; and (3) household-level counts of the cumulative number of surviving children born in the immediate post-repeal period reveals that the number of years a county was wet by 1940 is associated with slightly fewer surviving children, a finding consistent with our infant mortality results. Furthermore, we find that this effect is larger for lower SES households. However, as these results are purely cross-sectional in nature, they cannot account for time-varying heterogeneity. We consequently think of these results as being supportive of our baseline results, but not definitive in their own right.

6. Conclusion

In considering the effects of the repeal of federal prohibition, we use new data on county-level variation in alcohol prohibition from 1933 to 1939 and find robust evidence that relaxing restrictions on alcohol sales lead to increases in infant mortality. Critical in establishing this result is recognizing that it is not only an individual county's choice of prohibition status which matters but also the prohibition status of its neighbors. Thus, our strongest set of results—both in the estimated magnitude of the effect and in the number of specifications for which it holds—relates to dry counties being 'treated' with wet neighbors. Clearly, this type of policy externality is important not only in the context of assessing the repeal of federal prohibition but also in the context of current policy debates related to states potentially legalizing other illicit substances.

This paper also documents that these developments occurred in an environment when the general trajectory of infant mortality rates was distinctly downward. From 1934 to 1939, the nation-wide infant mortality rate for the US dropped from 60.0 per thousand live births to 50.2 per thousand in 1939, or by 16.33%. In our sample of triads of nearby counties, we estimate that dryish status was associated with a 4.7% increase in infant mortality rates. We also estimate that wet status was associated with a 4.0% increase in infant mortality rates. Thus, the repeal of federal prohibition can be thought of as having reversed the generalized decline in infant mortality rates in this period by 24.50 to 28.78% for the treated counties in our sample. Cumulating across all counties and all years, as in Section 5, our results imply 26,960 excess infant deaths that could be attributed to the repeal of federal prohibition in 1933.

Admittedly, we have been relatively silent on mechanisms given the constraints of historical data and instead offer a preferred interpretation of the data in the form of potential maternal alcohol consumption. And while there is an established medical literature which suggests a link from maternal alcohol consumption to infant mortality via both compromised immune systems and low birth-weights, we have very little by way of corroborating evidence in support of this hypothesis. Thus, our estimates may best be thought of as summary measures of the combined effects of relaxing constraints on alcohol availability arising from multiple potential channels. Unpacking our preferred interpretation and other linkages in between the availability of alcohol and infant death remains an area for future work.

Further avenues for future work come in assessing the effects of repeal on other contemporaneous outcomes, such as adult morbidity and mortality, violent crime, and worker productivity. Similar work could also exploit the variation in prohibition laws at the county level which predated federal prohibition in 1920 and which has been neglected in the literature. More ambitiously, we hope to explore the long-run effects of prohibition by considering how changes in potential maternal alcohol consumption induced by prohibition laws affected children born in these periods throughout their lives. Thus, we will correlate the ample geographic and temporal heterogeneity in restrictions on alcohol, both before and after federal prohibition, with long-term outcomes such as educational attainment, occupational status, and wages. Taken together, such work will—at last—allow a final tab for prohibition in all of its forms to be drawn.

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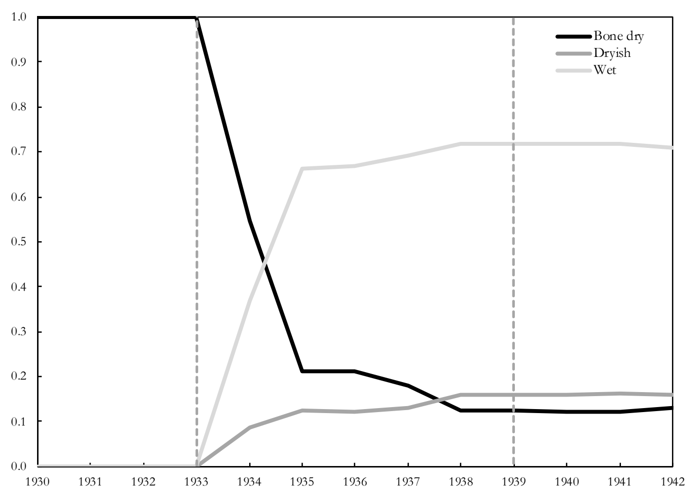
Figure 1: Infant mortality rates by prohibition status for all US counties, 1934–1939 (deaths per 1000 births)

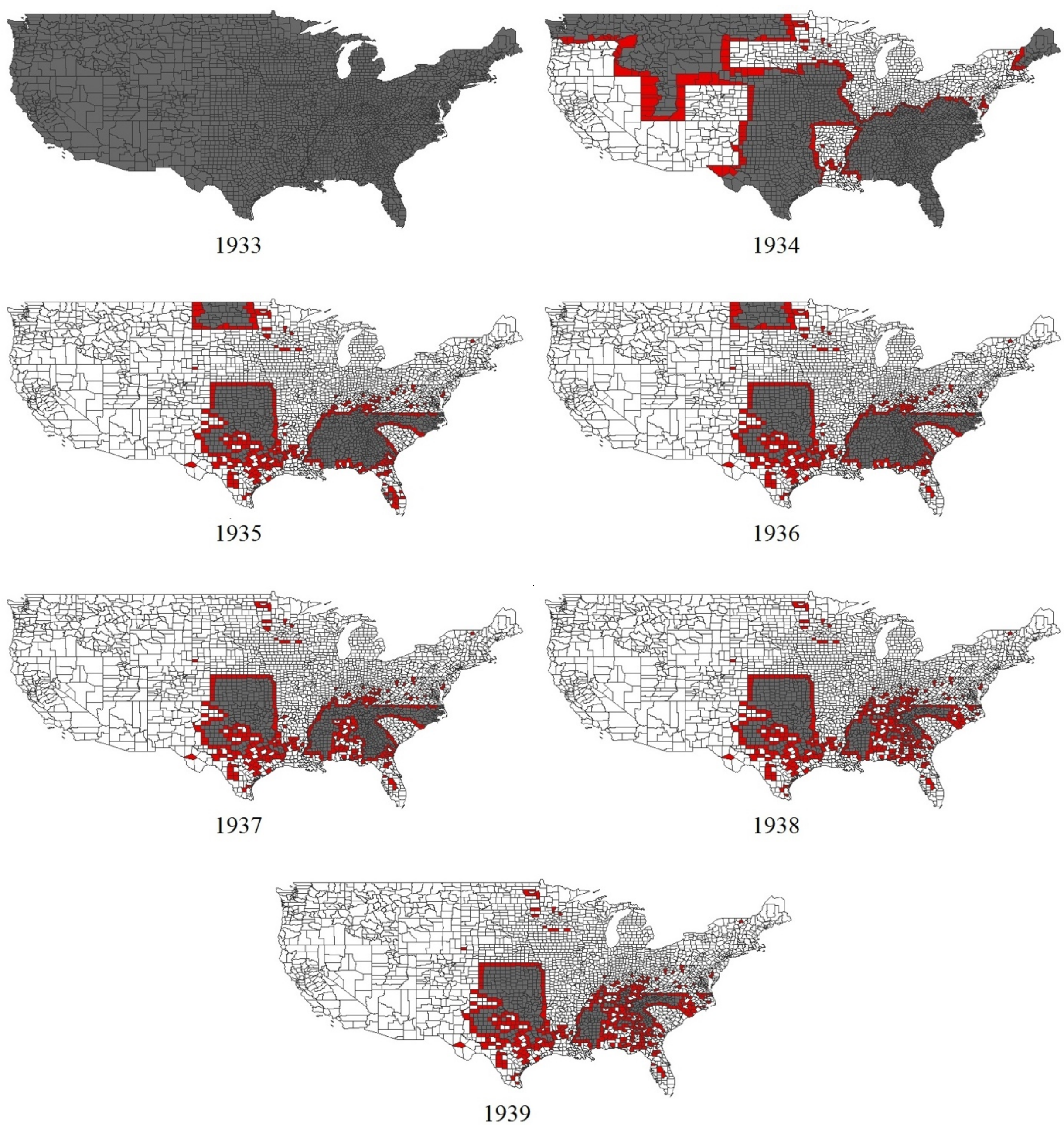
Notes: Figure 1 uses all US counties (n = 3,043). The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. Here, the composition of counties by category is time-varying. The number of births for each county are used as weights. *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders.

Figure 2: Proportion of all US counties by prohibition status, 1930–1942



Notes: Figure 2 uses all US counties (n = 3,043). *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders. The figure treats every county as bone dry prior to 1934. The two vertical dashed lines correspond to the beginning (1933) and end (1939) of our sample period.

Figure 3: Spatial distribution of all US counties by prohibition status



Notes: Figure 3 uses all US counties (n = 3,043). The counties in dark gray, red, and white correspond to bone dry, dryish, and wet counties, respectively. *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders.

Figure 4a: Pre-trends in infant mortality rates for sample of triads of nearby counties, 1928–1933 (deaths per 1000 births)

Notes: Figure 4a uses our sample of triads of nearby counties (n = 1,156). The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. Here, the composition of counties by category is time invariant. The number of births for each county are used as weights. *Dry partner* counties are the nearest bone dry counties to their dryish counterparts and generally stayed bone dry during our sample period from 1933 to 1939 (although some did transition to dryish and wet status). *Ever dryish* are the counties which became dryish at any time from 1933 to 1939 and which could be matched with bone dry and wet counterparts. *Wet partner* counties are the nearest wet counties to their dryish counterparts and stayed wet during our sample period from 1933 to 1939.

Figure 4b: Pre-trends in infant mortality rates for all available US counties, 1928–1933 (deaths per 1000 births)

Notes: Figure 4b uses all available US counties (n = 2,660). The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. Here, the composition of counties by category is time invariant. The number of births for each county are used as weights. *Bone dry* are dry counties which are surrounded by other dry counties throughout this period. *Ever dryish* are the counties which became dryish at any time from 1933 to 1939. *Wet* counties are counties which allow for alcohol sales within their borders at any time from 1933 to 1939.

Table 1: Variable definitions and data sources

Variable name	Definition	Source
Retail sales	Retail sales per capita, linearly interpolated between 1933, 1935, and 1939 (*time varying*)	Fishback *et al.* (2011)
% Baptist/Methodist	Number of Baptists and Methodists in 1926 divided by total population in 1930	1926 Census of Religious Bodies
% black	Number of blacks divided by total population in 1930	1930 Census – State and County I
% female	Number of females divided by total population in 1930	1930 Census – State and County I
% immigrant	Number of foreign born divided by total population in 1930	1930 Census – State and County I
% urban	Number of urban residents divided by total population in 1930	1930 Census – State and County I
Unemployment	Number of unemployed divided by population aged 15-64 in 1930	1930 Census – State and County I
New Deal spending	Cumulative New Deal spending from March 1933 through June 1939 divided by total population in 1930	Fishback *et al.* (2011)
Hospital beds	Hospital beds per 1000 women aged 15-44 (*time varying*)	Fishback *et al.* (2011)
Institutions	Medical institutions per 1000 women aged 15-44 (*time varying*)	Fishback *et al.* (2011)

Sources: Fishback, P.V., W. Troesken, T. Kollmann, M. Haines, P. Rhode, and M. Thomasson (2011). ‘Information and the impact of climate and weather on mortality rates during the Great Depression’, in (G. Libecap and R. Steckel, eds.), *The Economics of Climate Change*, pp. 131-168, Chicago: University of Chicago Press; Gardner, J. and W. Cohen (1992). ‘Demographic characteristics of the population of the United States, 1930-1950: county-level’, Ann Arbor: ICPSR; US Department of Commerce, Bureau of the Census (1980). *Censuses of Religious Bodies, 1906-1936*. Ann Arbor: ICPSR.

Table 2: Baseline sample county characteristics by treatment group for sample of triads of nearby counties

	Mean				Difference			
	Dry partner	Ever dryish	Wet partner	p-value	Dry partner	Ever dryish	Wet partner	p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Retail sales pc	256.61	271.02	323.51	0.000	148.55	148.88	192.69	0.000
	[152.10]	[151.18]	[174.06]		[118.48]	[117.56]	[140.09]	
% Baptist/Methodist	26.95	24.87	21.76	0.001	-5.36	-5.19	-4.29	0.266
	[15.84]	[16.83]	[14.65]		[9.19]	[8.54]	[7.90]	
% black	15.84	17.43	17.54	0.594	-0.71	-0.76	-0.85	0.796
	[19.52]	[21.19]	[20.52]		[1.98]	[1.88]	[2.63]	
% urban	12.99	14.46	21.85	0.000	2.17	2.04	3.16	0.204
	[18.41]	[21.20]	[25.84]		[6.72]	[6.70]	[9.20]	
% immigrant	2.01	2.31	2.69	0.143	-0.44	-0.35	-0.47	0.586
	[3.94]	[4.19]	[4.39]		[1.14]	[2.06]	[2.12]	
% female	48.84	48.56	48.70	0.086	0.24	0.31	0.40	0.221
	[1.60]	[2.07]	[2.23]		[0.86]	[1.07]	[1.05]	
Unemployment ratio	1.31	1.41	1.79	0.000	3.62	3.58	3.20	0.175
	[1.16]	[1.25]	[1.44]		[2.71]	[3.05]	[2.86]	
New Deal spending pc	131.77	136.3	144.64	0.459	-	-	-	-
	[108.32]	[119.39]	[122.84]					
Hospital beds pc	34.49	41.73	52.45	0.012	1.97	-0.97	-0.75	0.803
	[52.99]	[64.58]	[76.53]		[44.12]	[43.90]	[45.65]	
Institutions pc	4.08	6.08	8.76	0.005	0.76	0.69	-0.15	0.591
	[7.03]	[16.12]	[30.26]		[4.26]	[6.35]	[12.77]	
Observations	683	683	683		683	683	683	

Notes: Here, we use our sample of triads of nearby counties. Columns (1)–(3) report means by each treatment group with standard deviations in brackets. Column (4) reports p-values for the null hypothesis that the means are the same across the three groups. Similarly, columns (5)–(7) report differences over time by each treatment group with standard deviations in brackets. Column (8) reports p-values for the null hypothesis that the differences are the same across the three groups. For time-varying control variables (retail sales per capita, hospital beds per capita, medical institutions per capita), values in means come from 1933 and values in differences are from 1933 to 1939. For other variables derived from the Census (% black, % urban, % immigrant, % female, the unemployment ratio), values in means come from 1930 and values in difference are from 1930 to 1940. For % Baptist/Methodist, values in means come from the 1926 Census of Religious Bodies and values in difference are from 1926 to 1936. The values in difference for New Deal spending per capita is not reported as it is reported as the average from 1933 to 1939. Dry partner counties are the nearest bone dry counties to their dryish counterparts and generally stayed bone dry during our sample period from 1933 to 1939 (although some did transition to dryish and wet status). Ever dryish are the counties which became dryish at any time from 1933 to 1939 and which could be matched with bone dry and wet counterparts. Wet partner counties are the nearest wet counties to their dryish counterparts and stayed wet during our sample period from 1933 to 1939.

Table 3: The effect of repeal on infant mortality

	Without dryish			With dryish		
	(1)	(2)	(3)	(4)	(5)	(6)
Dryish in initial year				0.008	0.009	0.009
				(0.015)	(0.015)	(0.015)
Dryish in subsequent years				0.046***	0.046***	0.047***
				(0.016)	(0.016)	(0.016)
Wet in initial year	0.004	0.003	0.003	0.014	0.013	0.013
	(0.012)	(0.012)	(0.012)	(0.016)	(0.016)	(0.016)
Wet in subsequent years	0.011	0.009	0.009	0.041**	0.040**	0.040**
	(0.011)	(0.011)	(0.012)	(0.017)	(0.017)	(0.017)
N	14,343	14,343	14,343	14,343	14,343	14,343
N of triads	683	683	683	683	683	683
County & triad-year FEs	X	X	X	X	X	X
County controls	X	X	X	X	X	X
New Deal spending		X	X		X	X
Hospital beds/Institutions			X			X

Notes: All results are from method-of-moments estimation of the binomial fixed-effects model, bias-corrected following Hahn and Newey (2004). Standard errors clustered at the county-year level are reported in parentheses. The sample size is 14,343 (683 triads of counties for each year from 1933 to 1939). Columns (1)–(3) do not distinguish dryish from dry while columns (4)–(6) separate dryish counties from dry counties. Critically, the control group between the two sets of columns varies: for columns (1)–(3), the control group is all dry counties while for columns (4)–(6), the control group is only bone dry counties. County controls are the variables reported in Table 2 interacted with a linear trend, except for retail sales which is time-varying. New Deal spending is the cumulative amount of county-level New Deal spending per capita interacted with a linear trend. Hospital beds is the number of hospital beds per 1000 women aged 15–44 in a county while institutions is the number of medical institutions per 1000 women aged 15–44 in a county (both of which are time-varying). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 4: Wet counties versus wet states

	With wet state/county		
	(1)	(2)	(3)
Dryish in initial year	0.008	0.009	0.009
	(0.015)	(0.015)	(0.016)
Dryish in subsequent years	0.046***	0.046***	0.047***
	(0.016)	(0.016)	(0.016)
Wet state in initial year	0.017	0.017	0.018
	(0.032)	(0.032)	(0.032)
Wet state in subsequent years	0.046*	0.045*	0.045*
	(0.025)	(0.025)	(0.025)
Wet county in initial year	0.013	0.012	0.012
	(0.016)	(0.017)	(0.017)
Wet county in subsequent years	0.041**	0.039**	0.039**
	(0.018)	(0.019)	(0.018)
N	14,343	14,343	14,343
N of triads	683	683	683
County & triad-year FEs	X	X	X
County controls	X	X	X
New Deal spending		X	X
Hospital beds/Institutions			X

Notes: All results are from method-of-moments estimation of the binomial fixed-effects model, bias-corrected following Hahn and Newey (2004). Standard errors clustered at the county-year level are reported in parentheses. The sample size is 14,343 (683 triads of counties for each year from 1933 to 1939). County controls are the variables reported in Table 2 interacted with a linear trend, except for retail sales which is time-varying. New Deal spending is the cumulative amount of county-level New Deal spending per capita interacted with a linear trend. Hospital beds is the number of hospital beds per 1000 women aged 15–44 in a county while institutions is the number of medical institutions per 1000 women aged 15–44 in a county (both of which are time-varying). In columns (1) through (3), wet is divided into those counties which went wet through state legislation (wet state) and those counties which went wet through local option (wet county). The former includes: Arizona, California, Indiana, Nevada, and South Dakota in 1934; Delaware, Idaho, Iowa, Montana, South Carolina, Utah, and Wyoming in 1935; and North Dakota in 1937. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 5: The effect of repeal on infant mortality, additional specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Preferred (Col. 6, Table 3)	(1) with controls interacted with year FEs	(1) w/o counties bordering Canada or Mexico	(1) with state-by-year FEs	(1) with initial infant mortality rate	(1) with extended sample to 1941	(1) with lead terms	County and year FEs
Dryish in initial year	0.009	0.006	0.008	0.004	0.007	0.007	0.005	0.020*
	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)	(0.016)	(0.018)	(0.012)
Dryish in subsequent years	0.047***	0.047***	0.049***	0.040**	0.040**	0.042***	0.044**	0.033***
	(0.016)	(0.016)	(0.016)	(0.017)	(0.016)	(0.015)	(0.019)	(0.013)
Wet in initial year	0.013	0.015	0.01	0.016	0.012	0.008	0.003	0.017
	(0.015)	(0.016)	(0.016)	(0.021)	(0.016)	(0.016)	(0.019)	(0.013)
Wet in subsequent years	0.040**	0.040**	0.038**	0.014	0.039**	0.031*	0.029	0.039***
	(0.017)	(0.017)	(0.017)	(0.023)	(0.017)	(0.016)	(0.020)	(0.013)
Dryish one year before (t-1)							-0.001	
							(0.017)	
Wet one year before (t-1)							-0.020	
							(0.017)	
N	14,343	14,343	13,986	14,343	14,343	18,441	14,343	9,107
N of triads	683	683	666	683	683	683	683	-
County & triad-year FEs	X	X	X	X	X	X	X	
All county controls with linear trends	X		X	X		X	X	X
All county controls with year FEs		X			X			
State-year FEs				X				
County & year FEs								X

Notes: All results are from method-of-moments estimation of the binomial fixed-effects model, bias-corrected following Hahn and Newey (2004). Standard errors clustered at the county-year level are reported in parentheses. Column (1) replicates our baseline estimates from Column (6) of Table 3. Column (2) includes other county controls interacted with year fixed effects rather than a linear trend. Column (3) excludes those counties which border Canada or Mexico.

The resulting sample size is 13,986. Column (4) includes state by year fixed effects. Column (5) extends the sample to 1941. Column (6) adds lead treatment variables for dryish and wet. Column (7) includes county and year fixed effects instead of county and triad-year fixed effects, exploiting the within-county variation in prohibition status over time. The resulting sample size is 9,098 (1301 counties for each year from 1933 to 1939). County controls are the variables reported in Table 2 interacted with a linear trend, except for retail sales which is time-varying. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0$