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ABSTRACT

Exploiting 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to 1939, we investigate whether the repeal of federal prohibition increased infant mortality, both in counties that repealed and in their neighboring counties. Using a binomial fixed-effects model, 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 role for cross-border policy externalities. Cumulatively, these estimates imply 26,960 infant deaths that could potentially be attributed to the repeal of federal prohibition.

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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 the history of the modern nation state. 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 "noblest 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 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 county- and year-level variation in prohibition status, 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. Alsan and Goldin, 2015; Clay, Lewis, and Severnini, 2016; Cutler and Miller, 2005; Fishback, Haines, and Kantor, 2001, 2007; Moehling and Thomasson, 2014). However, to our knowledge, this is the only study that considers the effects of the repeal of federal prohibition on infant mortality or—for that matter—any other outcome variable.

An important methodological contribution of the paper comes in explicitly recognizing the possibility of policy externalities across county borders. Thus, after 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).

A further methodological contribution 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 numbers of births are low, the 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-effect 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.

For our baseline results, we follow the spirit of the empirical approach laid out in a recent paper by 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, so that there is *a priori* less unobserved heterogeneity. 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 24.50 to 28.78% 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 back-of-the-envelope calculation suggests an excess of 26,960 infant deaths that could potentially be attributed to the repeal of federal prohibition.[3]

Our paper is broadly related to a literature which focuses on assessing 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 (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

[1] We note that while both estimates are statistically different from zero at the conventional level, 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.

a priori county-level information is likely more meaningful, and below, we also consider how such variation may result from variation in county-level alcohol consumption preferences that are themselves causal drivers of infant mortality. Likewise, our paper is related to recent work by García-Jimeno (2016) which considers the effects of federal 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 only issued in 1981. That is, the general public at the time had little knowledge of the potential negative effects of alcohol consumption during pregnancy on child development. Thus, our estimates are arguably not confounded by differences in avoidance behaviors—both avoiding conception and avoiding 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 (Barreca and Page, 2015; Carpenter and Dobkin, 2009; 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; Harding, Leibtag, and Lovenheim, 2012; Lovenheim, 2008). 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 a substantial 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, 2018; Hao and Cowen, 2017). A key insight of our paper is that infant mortality in this period was not only driven by any individual county's choice of prohibition status but also by what its neighbors' choice of prohibition status was. 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 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. It also provides a simple framework for thinking about local alcohol consumption, counties' choice of whether or not to allow for alcohol sales, and the effects of doing so. 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. Context

In this section, we lay out the historical context surrounding the rise and fall of federal prohibition, highlighting those institutional features of its repeal which are most bearing for our analysis. Then, we lay out a simple framework for how preferences for alcohol consumption at the local level likely determine both prohibition status and the size of the related treatment effects.

2.1 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 nearly $7 billion in 2015 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. Alsan and Goldin, 2015; Clay, Lewis, and Severnini, 2016; Cutler and Miller, 2005; Fishback, Haines, and Kantor, 2001, 2007; Moehling and Thomasson, 2014). 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. Mills *et al.*, 1984; and Olegård *et al.*, 1979; 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.

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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] To our knowledge, 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.

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 prohibition 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 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. We necessarily leave this task for future work, citing a lack of relevant data at the present.

2.2 A simple framework for analysis

Here, we provide a simple framework demonstrating how the distribution of the willingness-to-pay (WTP) for alcohol within a county might simultaneously determine whether or not a county goes wet and the treatment effect of that choice (please refer to Appendix A for more detail).

We motivate the framework as follows. For individuals, assume that consuming alcohol (at a potentially dangerous level) is a binary decision. They do so if their WTP for alcohol exceeds the prevailing price of alcohol. We assume that each county can be characterized by its distribution of individual WTP for alcohol and that this distribution differs by county. To simplify, we also assume that the distribution of WTP is symmetric and unimodal so that the median voter is at the top (mode) of the density function. One such distribution, shown in Figure 1a, depicts the distribution of WTP in a county where people generally have a high WTP. Under federal prohibition, alcohol sales are illegal, making it difficult to purchase and, thereby, expensive.[6] The effective price of purchasing and consuming alcohol includes the wedges associated with this difficulty and illegality and is denoted in Figure 1a by the line labeled "dry price". The area to the right of the dry price gives the fraction of the population of the county that consumes alcohol when the county's *de jure* status is dry. For this county, where generally people have a high WTP, a large fraction of people consume alcohol even though it is prohibited.

The price of purchasing and consuming alcohol is lower when a county becomes wet as the sale of alcohol may be done more easily and openly. The "wet price" is shown by the vertical line to the left of the dry price. The treatment effect of this price change is to switch people whose WTP lies between the wet price and the dry price from non-consumers into consumers of alcohol. In Figure 1a, the treatment effect of a county changing its prohibition status—that is, the average treatment effect on the treated (ATT) of becoming wet—is equal to the shaded area under the curve between the wet and dry prices. Thus, we see that for this county where the WTP is generally high, the magnitude of the treatment effect is small because many people were already consuming alcohol prior to repeal.

Consider whether or not this county will choose to become wet. Any individual whose WTP exceeds the wet price will gain from the reduction in the cost of alcohol from the dry to wet price. The WTP distribution shown in Figure 1a has the feature that its median (located at the top of the curve) is to the right of the wet price, so the majority of people would prefer that their county become wet. In a strict majority-rule setting, this county would presumably choose wet status (or, more generally, it would have a higher probability of choosing wet status). Likewise, if we consider a county where the distribution of WTP was shifted to the right, so that people liked alcohol even more, all these features would be amplified. The level of alcohol consumption under prohibition would be higher, the treatment effect of becoming wet would be smaller, and the probability of becoming wet would be higher. Thus, we have that the distribution of WTP determines both the probability of treatment and the treatment effect.

If WTP distributions—that is, preferences—vary across counties but are fixed over relatively short periods of time (as in our sample), then the inclusion of county fixed-effects would account for all the drivers of both the probability of treatment and the treatment effect.

[6] Again, Cook (2007) among others is clear that prohibition never entailed a lack of availability of alcohol in affected counties, rather it is best thought of as having raised the price of alcohol by roughly a factor of five.

Therefore, the inclusion of county fixed effects would provide us with "selection-on-observables" (or, in other words, exogeneity of treatment). Furthermore, since the distribution of WTP differs across counties, we would have heterogeneous treatment effects. Taken together, this implies that fixed-effect models could obtain unbiased estimates of the average treatment effect on the treated for those counties choosing wet status.

Now, consider a county where generally people have a low WTP as shown in Figure 1b. In this county, only a small fraction of the density lies to the right of the dry price, so that only a small fraction of people chooses to consume alcohol under federal prohibition. The treatment effect of becoming wet, given by the region under the curve between the wet and dry prices, is quite large. However, unlike the county shown in Figure 1a, the median individual in this county has a WTP lower than the wet price. That is, the majority of people in this county have nothing to gain from their county becoming wet as they are non-consumers, regardless of their county's *de jure* prohibition status. Therefore, in a strict majoritarian setting, this county would presumably choose to retain dry status (or, more generally, it would have a lower probability of becoming wet than the county depicted in Figure 1a).

Finally, consider a county that chooses to stay dry but which has a wet neighbor. Thus, it could face a change in the price of alcohol even though it did not choose to become wet itself, but instead, became dryish due to the choice of its neighbor. As in wet counties, the "dryish price" in Figure 1b is less than the dry price because alcohol may now be legally purchased in the neighboring county. But there are two frictions that make the dryish price strictly larger than the wet price: (1) the driving distance (or other transportation cost) to the neighboring county line reduces the effective magnitude of the price drop; and (2) the prospect of enforcement of local prohibition within the county likely also reduces the effective magnitude of the price drop.

In Figure 1b, the treatment effect of staying dry but having a neighbor which became wet—that is, the average treatment effect on the treated (ATT) of becoming dryish—is equal to the shaded area under the curve between the dryish and dry prices. Here, the treatment is a smaller price change than going all the way to the wet price.[7] Again, with the inclusion of county fixed effects in our empirical model, we should be able to obtain unbiased estimates of the ATT

[7] This indicates that the dryish treatment is a smaller treatment than the treatment that would obtain if a county that wished to stay dry was forced to go wet. In other words, our dryish estimate then serves as a conservative estimate of the hypothetical treatment of becoming wet in a county that would otherwise choose to stay dry.

of the dryish treatment. Finally, by comparing the size of the shaded areas between Figure 1a and Figure 1b, we see that the ATT for becoming wet could plausibly be *smaller* than the ATT for becoming dryish.

There are several lessons that we draw from this framework that will be 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 ATT of becoming dryish or wet;[8] and (3) the ATT for becoming wet could plausibly be smaller than the ATT for becoming dryish.

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 Strumpf and Oberholzer (2002) and 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 in robustness exercises below. Figure 2 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

[8] In Section 4 below, we more fully consider the potential bias induced by omitted variables that are correlated with both treatment and infant mortality as well as the means at our disposal to deal with this issue.

county. Over this period, nation-wide infant mortality rate dropped from 60.0 per thousand live births in 1934 to 50.2 per thousand in 1939.

3.2 County-level prohibition status

Ideally, we would like county-level information on alcohol consumption, particularly for pregnant women or, at least, 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 fill in the gaps (Culver and Thomas, 1940; The Distilled Spirits Council, 1935, 1941; Harrison, 1938; Thomas and Culver, 1940). 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.[9]

Figure 3 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

[9] In a larger project on the contemporaneous and long-run effects of prohibition, we have reconstructed the status of all continental US counties from 1885 to 1984.

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 as discussed in Section 2.2 above.

Figure 4 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 2 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 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 unemployment rate as previous research has indicated that these variables influenced infant mortality in this period (Fishback, Haines, and Kantor, 2001, 2007).[10] 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-difference 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 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

[10] We use these variables strictly as controls. They would not make good instruments for IV regression analysis because they all plausibly have direct effects on infant mortality and so would not satisfy the exclusion restriction.

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.[11] 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. As in DLR, we achieve valid inference—that is, correct standard errors—in the presence of repeated observations of county-year data by clustering at the county level (see Dube, Lester, and Reich, 2010 and Cameron and Miller, 2015).

An advantage of only using triads comprised of geographically proximate counties is that such counties make good controls: they are likely to be similar in both their observed and unobserved characteristics. In comparison to using the full sample of all US counties, there is 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. Here, only variation within triads is used to identify the treatment effects. The basic idea is that we can use multiple observations of treated counties to aggregate

[11] We note that in 32 cases the wet partner is not an adjacent county. Although each *ever-dryish* county by definition has a wet neighboring county in their first dryish year, a non-neighboring wet county may actually have a closer county seat. Likewise, in 203 cases the dry partner is not an adjacent county. However, while the estimates become less precise due to the smaller sample size, our results are robust to excluding these triads from the sample (results available upon request).

treatment effects, imposing a common trend assumption within triads but not imposing a common trend assumption across triads.[12]

However, a disadvantage of only using triads is that we lose power in comparison to an approach that uses data from all US counties as we lose all 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. However, in Appendix C, we pursue a more traditional approach by expanding our analysis to include all US counties and incorporate only county and year fixed-effects for completeness. There, we find qualitatively similar results.

Our econometric strategy is analogous to difference-in-difference estimation. The key assumption in difference-in-difference 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 5a 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. 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 drastically 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 5a.

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.

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[12] 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.

Another means of validating our approach of using triads of nearby counties comes in considering the pre-trends for all US counties. Figure 5b 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. It is clear that, unlike in Figure 5a, the common trend assumption does not seem to hold for all 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 5a). Specifically, we report the mean of the infant mortality rate in 1933 (both in levels and logs) along with 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. 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 the alcohol prohibition status (the "treatment") influences the probability of infant death for each birth (the "response"). Thus, each birth is a Bernoulli random variable whose probability of death depends on the treatment and other covariates. However, we do not observe each birth individually. Instead, we observe the sum of births and the sum of deaths in each county-year. The econometric model corresponding to this DGP and data environment is the binomial model.[13] We, therefore, estimate the effect of treatment on the probability of infant death via maximum likelihood estimation of the binomial model.

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 estimate treatment effects on infant mortality rates by ordinary least squares, that is, to regress the (possibly logged) infant mortality rate on prohibition status and other covariates. Our approach, which takes the DGP seriously and estimates its corresponding model, 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. We first present the econometric model and then briefly discuss each of these advantages.

Our response variable is the number of infant deaths, D, in a county-year. Given the number of births, B, the infant mortality rate, I, is given by $I=D/B$. Our treatment variables are county-year level indicators of whether or not a county is itself wet, W, and of whether or not a county has at least one neighboring county that is wet, N. 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 is a bone dry county that has no wet neighboring counties and is itself not wet, thus, having $W=N=0$. 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.

For all prohibition treatment and response variables, we have panel data on counties $c=1,\ldots, C$, time periods $t=1,\ldots, T$, and triads $s=1,\ldots, S$. Our baseline model is a balanced panel of counties, years, and triads. That is, each triad has observations for each of its three counties from 1933 to 1939. 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

[13] The word "binomial" appears in the names of many distributions. To be clear, our binomial model, described formally in equations (1) and (2), is <u>not</u> a binomial logistic distribution or a negative binomial distribution.

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. For most counties, we do not have the exact date when changes in prohibition status occurred. Consequently, we set W_{cst} and N_{cst} equal to one in all the years following the change in legal status and equal to zero in the year of and all years preceding the change in legal status. We additionally include variables allowing for partial treatment effects in the year of change in legal status. The regressors W_{cst^*} and N_{cst^*} are equal to one in the year of the legal change and equal to zero in all other years. Let P_{cst} be the vector of prohibition status variables [W_{cst}, N_{cst}, W_{cst^*}, N_{cst^*}].

Assume that each birth b_{icst}, for $i=1,\ldots, B_{cst}$, in county c in triad s in time period t is an independent Bernoulli trial with a probability γ_{cst} that the birth results in an infant death. There are a total of B_{cst} births in a given county-year, and our measured outcome is the total number of infant deaths D_{cst} in that county-year. The Bernoulli structure of infant deaths implies that the probability mass function of the number of infant deaths, D_{cst}, given the number of births, B_{cst}, follows the binomial distribution, denoted $Bin(D_{cst}, B_{cst}, \gamma_{cst})$, and given by

$$Bin(D_{cst}, B_{cst}, \gamma_{cst}) = \frac{B_{cst}!}{D_{cst}!(B_{cst}-D_{cst})!}(\gamma_{cst})^{D_{cst}}(1-\gamma_{cst})^{B_{cst}-D_{cst}} \qquad [1]$$

The probability γ_{cst} is our object of interest, and it depends on our prohibition status variables, P_{cst}, and other covariates. We condition the probability on a vector of observed time-varying control variables, X_{cst}. These include both the time-invariant county-characteristics interacted with time trends and the time-varying controls reported in Table 2. Additionally, there may be unobserved characteristics of counties which could both cause a county to remain dry and influence its infant mortality. Thus, we include county-fixed effects, denoted θ_c. Standard difference-in-difference approaches would include time dummies (for all but the first year) in addition to the above county fixed effects and covariates. However, to allow for a potentially different time trend in each triad, we include triad-specific time dummies δ_{st} for each triad s in each time period t (for all but the first year).

Since probabilities lie in between zero and one, and since for infant mortality, the probability cannot be exactly equal to zero, we specify the probabilities as given by the logit function:

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

Note that since we have a full vector of county dummies θ_c, there is no intercept term inside the exponential function.

As noted above, the "industry standard" for modeling infant mortality would be to use OLS, regressing the infant-mortality rate in a county-year, I_{cst}, or its natural logarithm, $\ln(I_{cst})$, on treatment variables, county and year fixed-effects, and other time-varying control variables. The first advantage of our approach is that it directly models the discreteness of the outcome variable, infant death. In our data, the aggregate infant mortality rate is roughly 5%. Further, we observe many counties with small populations with small numbers of births and, therefore, very small numbers of infant deaths. For example, in our data, the 10th percentile of births across all county-years is 98 and the median of births is 343. Correspondingly, the 10th percentile and median of infant deaths are 4 and 18, respectively.

The discreteness of our data is very sharp when the number of births is low. Consider a county at the 10th percentile 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 center on 5, but would be truncated below by 0 and above by 98.

The second and related advantage of our approach is that it does not predict negative values for deaths and can accommodate observations with zero infant deaths. Since infant death is already a low incidence phenomenon, 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. In contrast, incorporation of zero observations is natural in the binomial model: the probability of zero infant deaths in a county-year with B_{cst} births is equal to $(1 - \gamma_{cst})^{Bcst}$. Further, these issues are also relevant to our data environment as we observe 353 county-years in the nation-wide sample with zero infant deaths.

The third advantage of using the binomial distribution for modeling infant mortality is that because it models the entire 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 in comparison to OLS when faced with grouped data (see Appendix B for more detail). The point is that higher moments of the infant death distribution are actually informative to estimation of the infant death probability.

We estimate the model by maximum likelihood. The MLE for this fixed-effect binomial model is given by

$$\max_{\alpha,\beta,\gamma,\delta_{st},\theta_c} \sum_{t=1}^{T} \sum_{c=1}^{C} \ln Bin\left(\left(D_{cst}, B_{cst}, \gamma(P_{cst}, X_{cst}; \alpha, \beta, \gamma, \delta_{st}, \theta_c)\right)\right) \qquad [3]$$

where γ is given by [2] as above. Note also that this estimator does not include the observed infant mortality rate, I_{cst}. Instead, it maximizes the likelihood of the observed data given the probability of observing each possible value of D_{cst} for a given B_{cst} when the probability of death for each birth is γ_{cst}.

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. However, the MLE defined by equation [3] for the fixed effects binomial model 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. 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 $1/S$, and in Monte Carlo experiments, Machado (2004) finds that the incidental parameters bias in the MLE is small for $T > 4$. Hahn and Newey (2004) provide an analytical bias-correction for general nonlinear fixed effects models. Estimates that incorporate these corrections are still biased, but only of order $1/T^2$ or $1/S^2$. Here, this bias correction may be written explicitly in terms of observed variables and is straightforward to compute. In the main text of this paper, we present only bias-corrected MLEs.

Consistent with Machado's observation that this bias is small, none of our bias corrections exceed 1% of the uncorrected MLE.[14]

Another consideration is the potential bias induced if we omit variables that are correlated with both the treatment and infant mortality. Such omitted variables bias is a natural worry because, as described in section 2.2, we believe that the choice to become wet is related to the county-level distribution of preferences for alcohol consumption. However, to the extent that such preferences are fixed over time, the inclusion of county fixed effects fully accounts for such preference variation. Further, to the extent that over-time 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.

Throughout our empirical work, we focus on the estimate of the parameters α which multiply the treatment regressors P_{cst}. 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, we can interpret the estimated treatment effect (α) as approximately equal to the semi-elasticity of the probability of infant death with respect to treatment.

Another model where estimated coefficients are approximately equal to the semi-elasticity of the probability of infant death with respect to treatment is the regression of the logged infant mortality rate, $\ln(I_{cst})$, on P_{cst} and other regressors. As stated before, this type of linear regression estimator is very common in the literature on infant mortality. Consequently, we present results from this type of regression for comparability with previous research in Appendix B. There, we find that while WLS estimates are similar in magnitude to our ML

[14] 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, a perhaps somewhat implausible restriction. Nonetheless, we estimated the Poisson fixed effects model and found highly similar results (results available on request).

estimates, OLS estimates are noticeably larger than ours. What is more, we find that both OLS and WLS have substantially larger standard errors than the MLE.

Finally, estimated standard errors are clustered at the county level.[15,16] These standard errors are robust both to the fact that we replicate observations of data where counties are present in multiple triads and to possible serial correlation in the dependence of infant mortality on treatment and covariates.

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 via MLE as outlined above. 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 (all columns) and the baseline covariates from Table 2, New Deal spending (columns 2 and 3), and hospital beds/medical institutions (column 3) 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

[15] DLR additionally allow for state-level clustering because all of their variation comes from state-level policy changes. Our results hold if we use state-level clustering instead (results available upon request). DLR also allow for overlapping state-border-pair clustering, again, for the fact that their policy variation is exclusively state-level. Finally, as described below, we use a binomial fixed effects model estimated by maximum likelihood. In this context, county-level clustering allows for possible correlations in the scores of the log-likelihood function (analogous to linear combinations of regressors and error terms in linear regression models) within counties across years.

[16] Clustering at the county-level means that we allow for correlations across years within counties. Technically, these are correlations between the scores of the log-likelihood function across observations for a given triad or county. They are analogous to correlations of residuals across observations within groups that are accounted for in OLS regression with clustered standard errors.

by ignoring potential cross-border policy externalities serves to mask the effects on infant mortality of both a county becoming wet and a county becoming dryish.

Accordingly, columns (4) through (6) 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). Thus, we interpret the estimate in column (6) as representing the effect of one county's (or state's) decision to go wet on its neighboring dry counties which corresponds to a 4.7% increase in infant mortality for those counties affected.

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. The results in column (6) suggests that the effect of one county going wet corresponds to a 4.0% increase in infant mortality for those counties affected. In what follows, we take our results in column (6) as our preferred specification.

As was seen before, Section 2.2 suggests why the estimated ATT for becoming wet could plausibly be smaller than the ATT for becoming dryish. At the same time, we do not emphasize this difference in magnitude as the coefficients are not different from one another in terms of statistical significance: for the results 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.

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. 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.

These results highlight two of the main arguments of this paper. First, the distinction between bone dry and dryish counties turns out to be an important one, 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 4, 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 wet counties may be thought of as rough analogs to their dryish counterparts. Column (1) of Table 4 makes this distinction for *wet* 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.

Very little changes as it relates to our previous results on dryish. However, some interesting results emerge. 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. As for *dryish county in subsequent years* and *wet county in subsequent years*, we see a high degree of comparability with our previous results. This is especially true for our preferred specification in column (3).

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 4 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.

We also extend the sample up to 1941 in column (5). 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 coefficients for and statistical significance of *dryish in subsequent years* and *wet in subsequent years* persist. At the same time, the results on *wet in subsequent years* suggest that some of the effects identified here may be transitory in nature.

We also consider the placebo effect of adding lead terms for the treatment variables in our preferred specification in column (6) 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 (7) 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. While the coefficients on *wet in subsequent years* are nearly identical to our preferred results in column (1), the coefficient on *dryish in subsequent years* is 30% lower (but still statistically significant at the 5% level).

The exclusion or inclusion of triad-year fixed effects does not affect the estimated value of the wet treatment effect. Thus, a more standard approach 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 means that the DLR innovation of allowing for heterogeneous time trends is important for the consistent estimation of the dryish effect.[17]

6. Conclusion

In considering the effects of the repeal of federal prohibition, we 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, instead offering a preferred interpretation of the data in the form of potential maternal alcohol consumption. And while there

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[17] Appendix C 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.

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, other linkages in between the availability of alcohol and infant death remain as possibilities and as 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 1a: A County with High Willingness-to-pay (WTP) for Alcohol

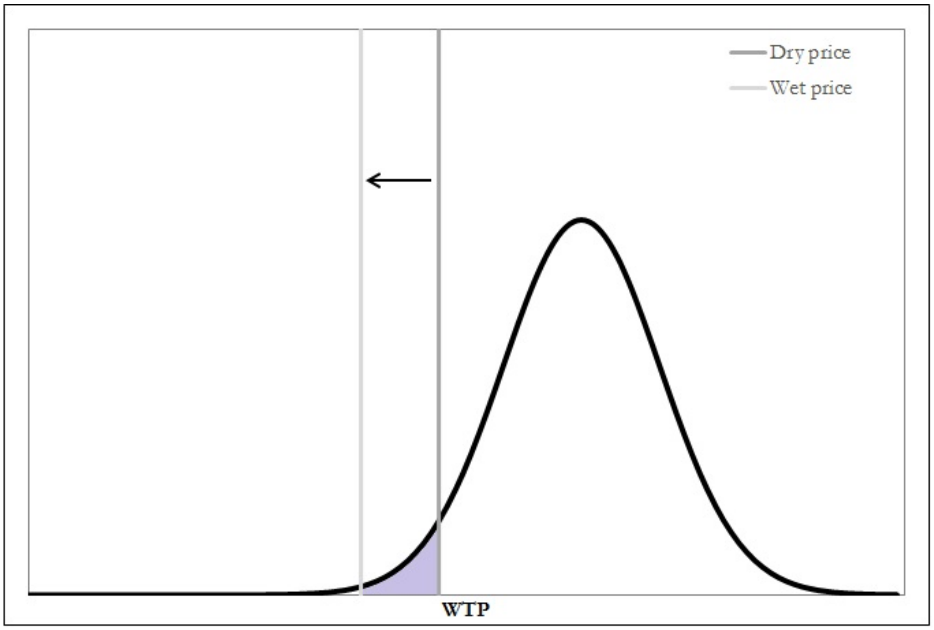
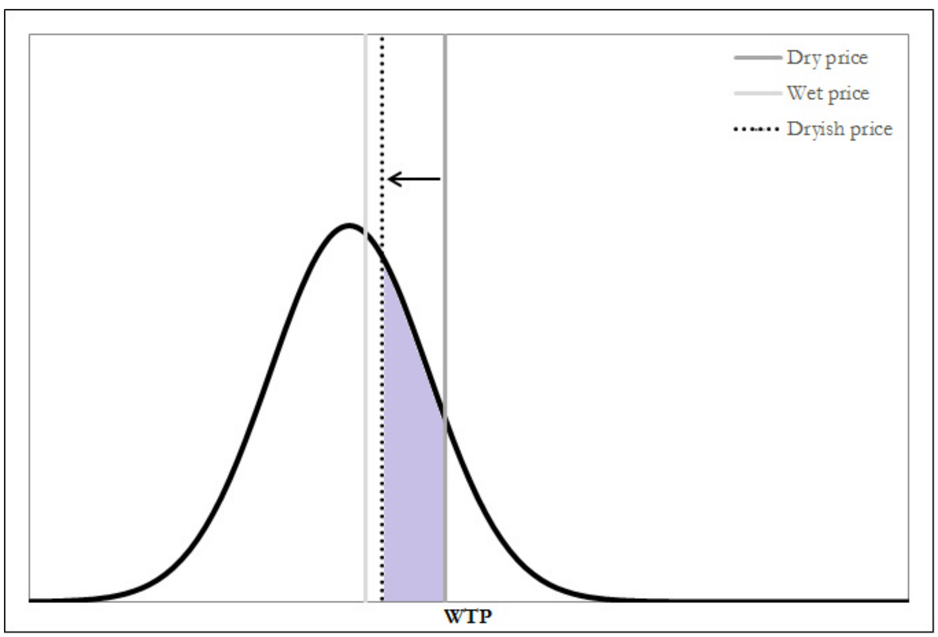
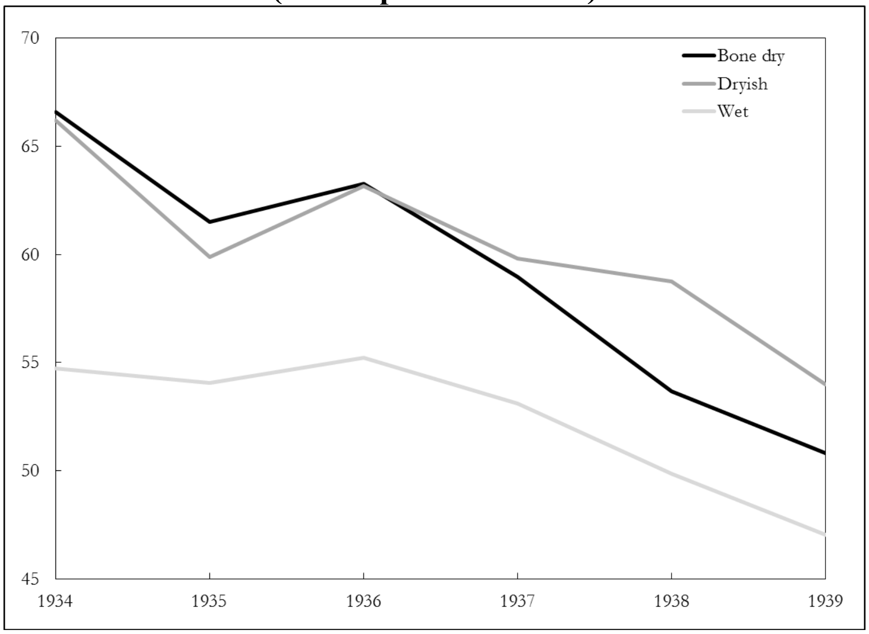


Figure 1b: A County with Low Willingness-to-pay (WTP) for Alcohol



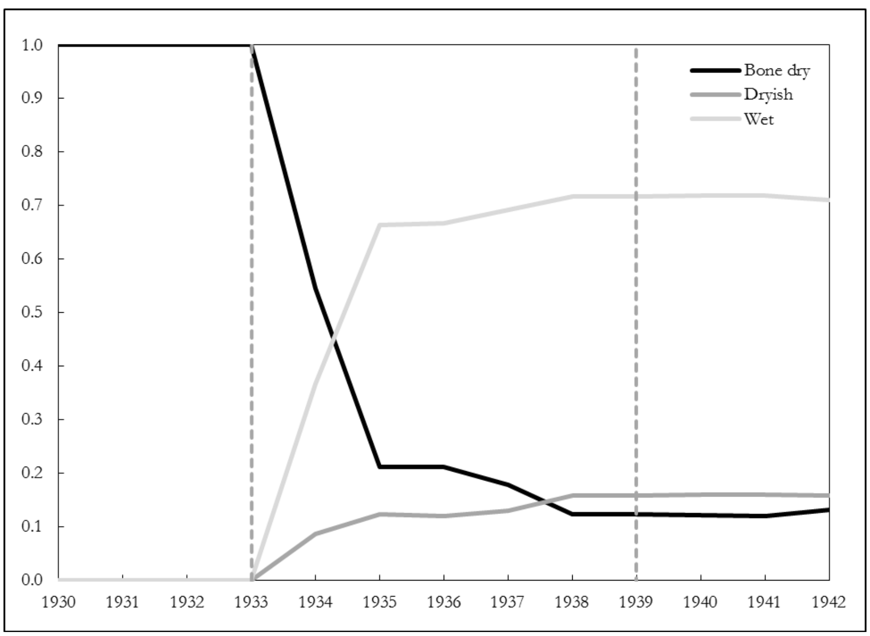
Notes: We assume that each county can be characterized by its distribution of individuals' willingness-to-pay (WTP) for alcohol and that this distribution differs by county. To simplify, let the distribution of WTP be symmetric and unimodal so that the median voter is at the top (mode) of the density function. For individuals, assume that consuming alcohol (at a potentially dangerous level) is a binary decision. They do so if their willingness-to-pay for alcohol exceeds its price. The effective price of purchasing and consuming alcohol while the county is variously dry, dryish, or wet are respectively denoted by "dry price", "dryish price", and "wet price".

Figure 2: Infant Mortality Rates by Prohibition Status for All US Counties, 1934–1939 (deaths per 1000 births)



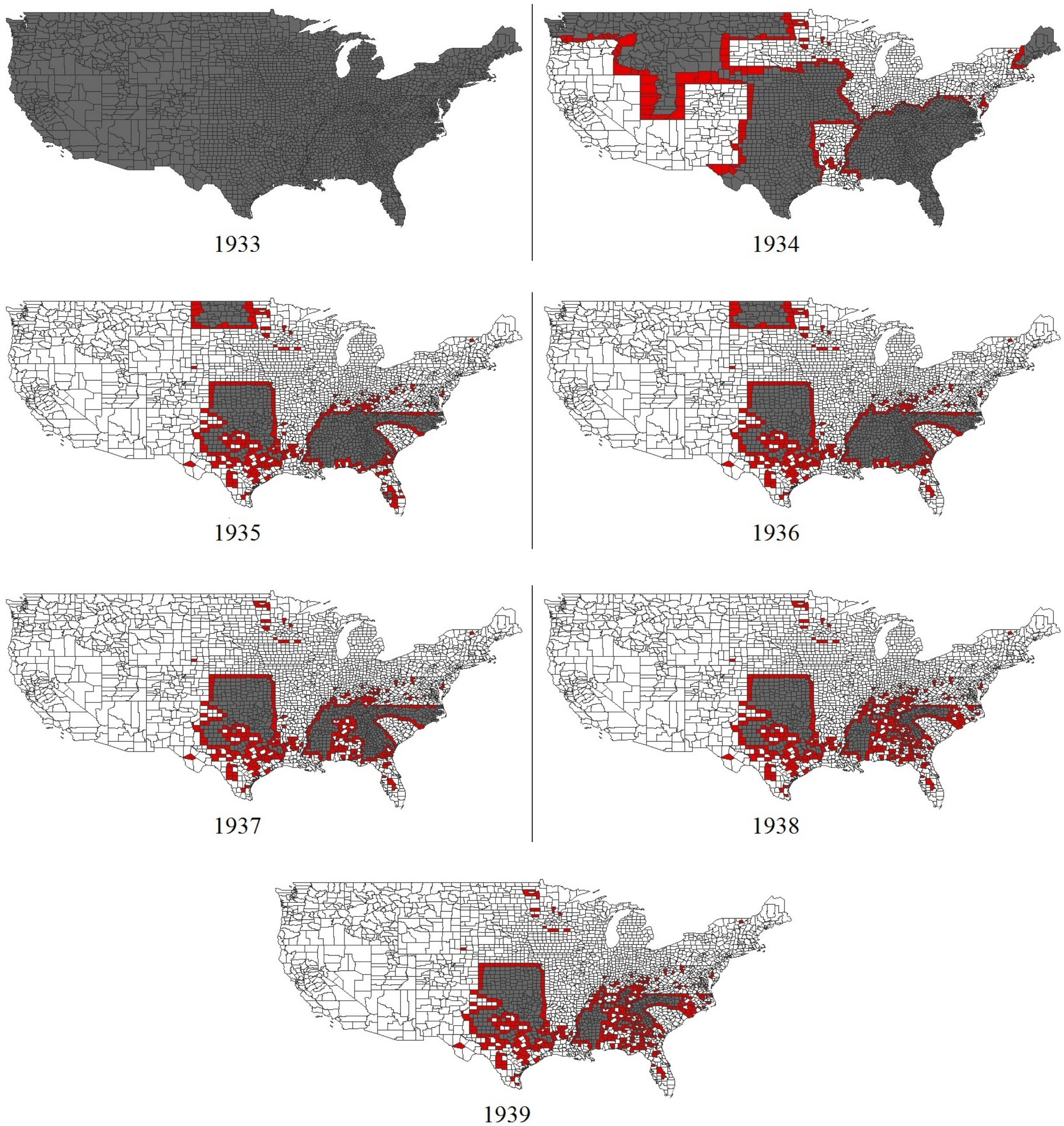
Notes: Figure 2 uses all 3,043 US counties. The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. The number of births for each county are used as weights. *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders.

Figure 3: Proportion of All US Counties by Prohibition Status, 1930–1942



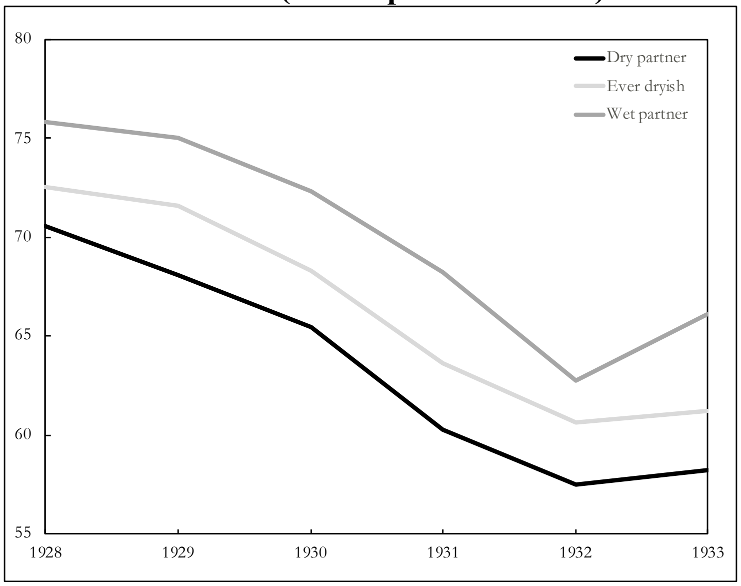
Notes: Figure 3 uses all 3,043 US counties. *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders. The figure treats every county as bone dry prior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

Figure 4: Spatial Distribution of All US Counties by Prohibition Status



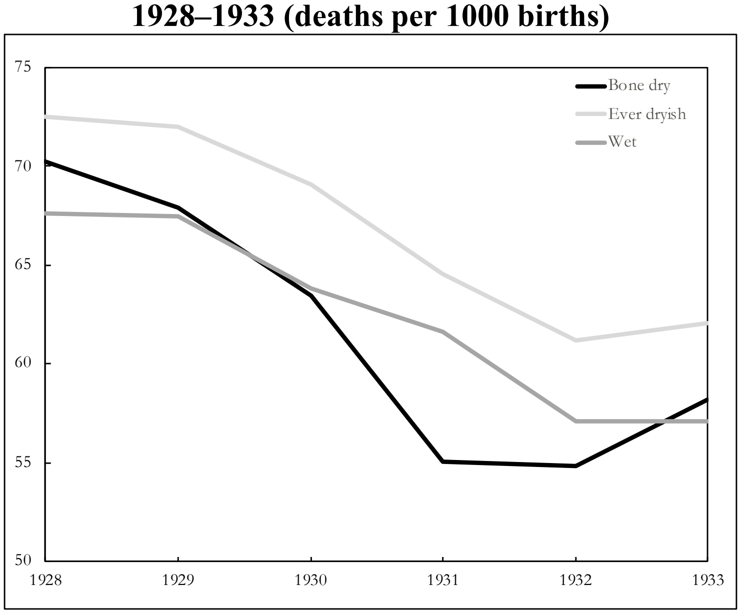
Notes: Figure 4 uses all 3,043 US counties. The counties in dark gray, red, and white correspond to bone dry, dryish, and wet counties, respectively. *Bone dry* are dry counties surrounded by other dry counties. *Dryish* counties are dry counties with at least one wet neighbor. *Wet* counties are counties which allow for alcohol sales within their borders.

Figure 5a: Pre-trends in Infant Mortality Rates for Sample of Triads of Nearby Counties, 1928–1933 (deaths per 1000 births)



Notes: Figure 5a uses our sample of triads of nearby counties. The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. 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 5b: Pre-trends in Infant Mortality Rates for All US Counties, 1928–1933 (deaths per 1000 births)



Notes: Figure 5b uses all US counties. The infant mortality rate is the number of infant deaths within a year from live birth per 1000 births. 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.

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 rate	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)
Medical institutions	Number of medical institutions divided by total population in thousands (*time varying*)	Fishback *et al.* (2011)
Hospital beds	Hospital beds 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 *The Economics of Climate Change* (Ed.s G. Libecap and R. Steckel). Chicago: University of Chicago Press, 131-168; Gardner, J. and W. Cohen (1992), "Demographic Characteristics of the Population of the United States, 1930-1950: County-Level." Ann Arbor: Inter-university Consortium for Political and Social Research, http://doi.org/10.3886/ICPSR00020.v1; US Department of Commerce, Bureau of the Census (1980), *Censuses of Religious Bodies, 1906-1936*. Ann Arbor: Inter-university Consortium for Political and Social Research, http://doi.org/10.3886/ICPSR00008.v1

Table 2: Baseline Sample County Characteristics by Treatment Group for Sample of Triads of Nearby Counties

	All	Mean by treatment group		
		Dry partner	Ever dryish	Wet partner
	(1)	(2)	(3)	(4)
Infant mortality rate in 1933	62.54	58.75	62.05	66.08
	[20.73]	[18.08]	[22.17]	[19.99]
Log (infant mortality rate) in 1933	4.08	4.02	4.07	4.14
	[0.35]	[0.34]	[0.36]	[0.33]
Retail sales in 1933	364.85	308.65	325.27	435.43
	[206.05]	[188.92]	[186.57]	[221.14]
% Baptist/Methodist in 1926	18.97	16.67	20.21	20.04
	[19.86]	[19.99]	[20.53]	[19.19]
% black	33.21	22.84	29.32	42.77
	[31.74]	[24.80]	[31.48]	[32.75]
% female	3.12	2.39	2.62	3.74
	[4.99]	[4.30]	[4.46]	[5.54]
% immigrant	24.28	25.87	25.94	22.77
	[15.34]	[15.52]	[15.93]	[14.28]
% urban	49.55	49.24	49.48	49.86
	[1.62]	[1.53]	[1.66]	[1.56]
Unemployment rate	2.10	1.72	1.87	2.48
	[1.55]	[1.35]	[1.53]	[1.55]
New Deal spending	115.77	116.87	108.74	123.22
	[72.85]	[78.33]	[68.89]	[73.52]
Medical institutions	51.15	46.26	45.23	59.44
	[60.63]	[49.80]	[47.77]	[75.15]
Hospital beds	9.70	6.77	9.55	11.14
	[15.54]	[8.60]	[17.60]	[14.56]
Number of counties	1,301	327	683	378

Notes: Here, we use our sample of triads of nearby counties. Column (1) reports means across all counties within triads while columns (2)–(4) report means by each treatment group with standard deviations in brackets. The number of births in each county is used as weights. Unless otherwise mentioned, values of each variable above come from 1930. *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. Finally, the sum of observations in the last three columns (N = 1,388) does not equal the value for the first column (N = 1,301) as a few *ever dryish* counties serve as a dry or wet partner in other triads.

Table 3: The Effect of Repeal on Infant Mortality Rates

	Without dryish			With dryish		
	(1)	(2)	(3)	(4)	(5)	(6)
Dryish in initial year				0.008	0.009	0.009
				(0.016)	(0.016)	(0.016)
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)	(0.017)	(0.017)	(0.017)
Wet in subsequent years	0.011	0.009	0.009	0.041**	0.040**	0.040**
	(0.012)	(0.011)	(0.011)	(0.018)	(0.018)	(0.018)
N	14,337	14,337	14,337	14,337	14,337	14,337
N of county	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/ Medical institutions			X			X

Notes: All estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004). Standard errors clustered at the county level are reported in parentheses. The sample size is 14,337 (683 triads of counties for each year from 1933 to 1939 excluding six observations with no births in the year). 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 1 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 *medical institutions* is the number of medical institutions per 1000 people 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
	(0.014)	(0.014)	(0.014)
Dryish in subsequent years	0.046***	0.046***	0.047***
	(0.014)	(0.014)	(0.014)
Wet state in initial year	0.017	0.017	0.018
	(0.035)	(0.035)	(0.035)
Wet state in subsequent years	0.046*	0.045*	0.045*
	(0.026)	(0.027)	(0.027)
Wet county in initial year	0.013	0.012	0.012
	(0.014)	(0.014)	(0.014)
Wet county in subsequent years	0.041***	0.039***	0.039***
	(0.015)	(0.015)	(0.015)
N	14,337	14,337	14,337
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/Medical institutions			X

Notes: All estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. The sample size is 14,337 (683 triads of counties for each year from 1933 to 1939 excluding six observations with no births in the year). County controls are the variables reported in Table 1 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 *medical institutions* is the number of medical institutions per 1000 people 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 Rates, Additional Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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 fixed effects	(1) with extended sample to 1941	(1) with lead terms	County and year fixed effects
Dryish in initial year	0.009	0.006	0.008	0.004	0.007	0.005	0.020
	(0.016)	(0.014)	(0.014)	(0.017)	(0.014)	(0.017)	(0.013)
Dryish in subsequent years	0.047***	0.047***	0.049***	0.040**	0.042***	0.044***	0.033**
	(0.016)	(0.014)	(0.014)	(0.017)	(0.013)	(0.016)	(0.014)
Wet in initial year	0.013	0.015	0.010	0.016	0.008	0.003	0.017
	(0.017)	(0.014)	(0.014)	(0.021)	(0.014)	(0.017)	(0.013)
Wet in subsequent years	0.040**	0.040***	0.038***	0.014	0.031**	0.029*	0.039***
	(0.018)	(0.015)	(0.015)	(0.025)	(0.014)	(0.017)	(0.015)
Dryish one year before (t-1)						-0.001	
						(0.017)	
Wet one year before (t-1)						-0.020	
						(0.015)	
N	14,337	14,337	13,984	14,337	18,422	14,337	9,098
N of triads	683	683	666	683	683	683	-
County & triad-year FEs	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					
State-year FEs				X			
County & year fixed effects							X

Notes: All estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county 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 13,984. 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 excluding nine observations with no births in the year). County controls are the variables reported in Table 1 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. *Medical institutions* is the number of medical institutions per 1000 people in a county while *hospital beds* is the number of hospital beds 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

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Appendix A: An Illustrative Model of Alcohol Consumption

Suppose that individuals have a willingness to pay for alcohol, v, drawn from a county-specific (time-invariant) distribution, $F_c(v)$. Suppose that the cost of buying alcohol under prohibition (that is, in a dry county) is d and the cost when buying without prohibition restrictions (that is, in a wet county) is w and that $d > w$. Suppose that each individual faces a binary choice of whether or not to buy alcohol based on whether their v exceeds the cost of purchase. Then, the purchase rate is $1- F_c(d)$ in a dry county and $1- F_c(w)$ in a wet county. The causal effect of ending prohibition on alcohol purchases is thus $1- F_c(w)-(1- F_c(d))= F_c(d)- F_c(w)$.

Suppose that individuals prefer to change the law to end prohibition if for some cutoff $k \leq w$, their willingness to pay satisfies $v > k$. This cutoff k is assumed to be less than or equal to w because individuals who value alcohol at least enough to buy it when it is legal will be interested in ending prohibition. Additionally, some individuals who would not buy it might value freedom of choice as a generic good or might value the reduced criminal activity associated with ending prohibition. Then, under majority rule, prohibition will end in those counties where $1-F_c(k) > 0.5$, or, equivalently, where $F_c(k) < 0.5$. Thus, the distribution F_c determines both whether or not a county will end prohibition *and* the causal effect on alcohol consumption. This implies that we have both heterogeneous treatment effects and correlated heterogeneity determining which counties are treated.

Since the only heterogeneity we allow for in this simple model is time-invariant, the inclusion of county fixed effects is enough to ensure that treatment is exogenous to the regressors. This implies that we can obtain an unbiased estimate of the average treatment effect on the treated (ATT) for wet counties. However, since the model tells us that treatment effects are heterogeneous, the ATT for wet counties does not in general equal the average treatment effect (ATE) for the population.

Individuals who have a willingness to pay greater than or equal to d are *drinkers*, because they will buy alcohol whether or not it is prohibited. Individuals who have willingness to pay less than or equal to zero are *abstainers*, because they will not buy alcohol at any price. The normal distribution implies that all counties have a nonzero fraction of abstainers who do not value alcohol consumption and a nonzero fraction of drinkers who highly value alcohol consumption. Neither abstainers nor drinkers change their binary alcohol purchase decision in response to changes in legal prohibition.

Suppose that F_c is normal with unit variance and heterogeneous medians, m_c, which vary across counties. Counties whose county-specific median, m_c, is low would have nearly all abstainers and counties whose median is high would have nearly all drinkers. A county with mostly drinkers would have a very high m_c so that both w and d would be very far below the median. Consequently, such a county would go wet, but the causal effect on alcohol purchase will be small. Such counties have high fractions of the population purchasing alcohol regardless of its prohibition status.

A county with mostly abstainers would have a very low median so that have both w and d would be very far above the median. Such a county would stay dry, but if it went wet, it would also have a very small causal effect on alcohol consumption.

Counties with m_c just below w might go either way on staying dry, but those that become wet would tend to have a large causal effect because the density of a normal is highest at the median. The observed ATT will, thus, depend on the distribution of m_c across counties. If the empirical distribution of m_c across counties is right-skewed, for example, the ATT could be quite small because the counties full of drinkers would dominate the counties that ended prohibition.

Now consider an intermediate treatment: having a wet neighbor but remaining dry (that is, dryish in the language of the main text). This reduces the cost of purchase from d to n, where $w < n < d$ gives the cost of buying alcohol from a neighboring county, which is less than the cost of buying it locally under prohibition, but more than the cost of buying it in a wet county. The source of this price friction is at least twofold: (1) travel distance between one's residence and the county line of the nearest wet neighbor and (2) potential local enforcement of prohibition restrictions in the home county.

The treatment effect for dryish is then $F_c(n)$- $F_c(w)$. Conditional on the distribution of willingness-to-pay in the country, this treatment effect is strictly smaller than that of transitioning to wet status, because we integrate a smaller range of the willingness-to-pay distribution. Further, only a dry county can be treated with this intermediate treatment. For these two reasons, we argue that the treatment effect of a dry county having a wet neighbor provides a *conservative* (lower-bound) estimate of the ATT for a dry county becoming wet. This is an interesting object for two reasons: (1) we can say something about the effect of treatment on agents that never choose treatment and (2) if we take our estimated ATT for wet as unbiased, then we can combine the estimates to get a lower-bound estimate of the ATE for the population.

The above arguments suggest that in this simple model, the treatment effect is non-monotonic in the county-specific preference parameters. If treatment is exogenous to the regressors (as in the fixed-effects estimate), then we can recover the ATT for wet counties. However, if treatment is endogenous, perhaps due to time-varying county-specific unobserved preference shocks, then one would typically appeal to instrumental variables to correct for that endogeneity. However, because the treatment effect is non-monotonic, standard instrumental variables approaches will not work.

We can also express this intuition more formally as a heterogenous treatment effects model. Let Y_{ct} be the alcohol purchase rate (binary for individuals, but a rate for a county-year). The above model implies that

$$Y_{ct} = T_{ct} * G_c + X_{ct} * B + \theta_c + \delta_t + e_{ct},$$

where T_{ct} is the treatment status (wet=1, dry=0) for county c in period t, X_{ct} are covariates, θ_c are county fixed effects, δ_t are year fixed effects, and e_{ct} are error terms. The important feature here is that the treatment effects G_c are heterogeneous across counties, so this a random coefficients model. Furthermore, there is an underlying parameter governing county-specific preferences for consuming alcohol, m_c, that determines both T_{ct} and G_c. Under the model above, T_{ct} is monotonically increasing in m_c. Further, for m_c high enough, G is monotonically decreasing in m_c.

Given the restriction that variation in alcohol preferences (m_c) is time-invariant, then the only variable that treatment depends on is county. If we include county fixed effects as regressors, then we satisfy the selection-on-observables condition that guarantees that the estimated value of the coefficient on *wet* is an unbiased estimate of the average treatment effect on the treated (ATT) for counties that go wet.

Now assume that alcohol preferences vary over both time and county in ways uncorrelated with the observed covariates (so that we have m_{ct}). This implies

$$Y_{ct} = T_{ct} * G_{ct} + X_{ct} * B + \theta_c + \delta_t + e_{ct},$$

because m_{ct} causes variation in the treatment effect (G). Furthermore, assume that counties that go wet have m_{ct} high enough that the treatment effect (G) is monotonically decreasing in m_{ct}. In this case, including the county fixed effects does not deliver selection-on-observables, and the estimated coefficient on *wet* is a biased estimate of the ATT for counties that choose wet status. However, because we know that T_{ct} increases in m_{ct} and G_{ct} decreases in m_{ct}, we can sign the bias: the estimated coefficient is a downward-biased estimate of the ATT for wet status.

Note that the channel for this form of endogeneity does not run through the additive error term (e_{ct}). Instead, the issue arises all through the fact that a single unobserved variable (alcohol preferences) drives both the response of alcohol consumption to a change in prohibition status and the probability of opting for a change in prohibition status.

Appendix B: A Comparison of Results under MLE and OLS

A very common specification in much of the literature is to regress the (logged) infant mortality rate on covariates of interest. Thus, we compare our MLE with unweighted and weighted OLS where the weight is the number of births in a county-year.

We begin with the observation that for small numbers of births B_{ct}, the binomial distribution of infants deaths D_{ct} is skewed and discrete, but as B_{ct} gets large, the distribution of infant deaths becomes more symmetric and smooth. As $B_{ct} \rightarrow \infty$, the distribution of D_{ct} is approximately normal by the de Moivre–Laplace theorem and is approximately distributed as $D_{ct} \sim N(B_{ct}\gamma_{ct}, B_{ct}\gamma_{ct}(1-\gamma_{ct}))$. Consequently, the infant mortality rate, $I_{ct} = D_{ct} / B_{ct}$, is approximately asymptotically distributed as $I_{ct} \sim N(\gamma_{ct}, \gamma_{ct}(1-\gamma_{ct})/B_{ct})$, leading to the widespread use of least squares estimates regressing the infant mortality rate on covariates. The limiting distribution of the binomial suggests that if the observed numbers of deaths are large, then least squares estimators are acceptable, but they should use weights B_{ct} to gain efficiency. That is, estimation should be by weighted least squares, given the dependence of the variance $\gamma_{ct}(1-\gamma_{ct})/B_{ct}$ on the observed numbers of births. Furthermore, robust standard errors should be used to deal with the fact that the variance also depends on the unobserved observation-specific γ_{ct}.

Following the same notation from the fixed effects binomial model in the main text, we estimate the following OLS equation:

$$\log(D_{ct}/B_{ct}) = \beta_0 + \beta_1 W_{ct} + \beta_2 N_{ct} + \beta_3 W_{ct*} + \beta_4 N_{ct*} + \gamma X'_{ct} + \theta_c + \delta_t + \varepsilon_{ct}$$

where D_{ct} and B_{ct} are the number of infant deaths and the number of births at county c in time t. W_{ct} and N_{ct} are equal to one in all the years following the change in prohibition status and equal to zero in the year of—and all years preceding—the change in prohibition status for wet and neighboring counties, respectively. Similarly, W_{ct*} and N_{ct*} are equal to one in the year of the status change and equal to 0 in all other years to allow for partial treatment effects in the year of change in prohibition status. The control group is then the set of bone dry counties, dry counties without any wet neighbors. X_{ct} is a set of the time-varying county characteristics which is the same as in our baseline estimate found in column (6) of Table 4. λ_c and θ_t are county and time fixed effects, respectively. Our coefficients of interests are β_2 and also potentially β_1.

This specification presents another issue which arises with OLS: observations with no infant deaths must be dropped or scaled before taking the log. Following the convention in the literature (e.g., Carpenter and Dobkin, 2009), we add 0.5 to the value of infant deaths in order to retain the 342 observations that have no reported infant deaths. Unweighted OLS estimates do not change if instead we add 0.1.

For ease of comparison, columns (1) through (3) in Table B1 replicates the MLE estimates from columns (4) through (6) in Table 3. Columns (4) through (6) present the results of an equivalent regression of the logged infant mortality rate on the same covariates estimated via unweighted OLS. Comparing our preferred results in column (3) to those in column (6), we see that the estimated effect of *dryish in subsequent years* in column (6) is nearly 50% larger than that in column (3). Likewise, we see a 33% larger estimated effect associated with *wet in subsequent* *years* in column (6) as opposed to column (3), but now with a lack of statistical significance. This suggests that unweighted OLS may be problematic in that it may be placing undue weight on unrepresentative observations, e.g., particularly small counties.

Table B1: MLE versus Unweighted OLS Estimates

	MLE			Unweighted OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Dryish in initial year	0.008	0.009	0.009	0.014	0.014	0.013
	(0.016)	(0.016)	(0.016)	(0.027)	(0.027)	(0.027)
Dryish in subsequent years	0.046***	0.046***	0.047***	0.071**	0.070**	0.069**
	(0.016)	(0.016)	(0.016)	(0.028)	(0.028)	(0.028)
Wet in initial year	0.014	0.013	0.013	0.026	0.025	0.025
	(0.017)	(0.017)	(0.017)	(0.026)	(0.026)	(0.026)
Wet in subsequent years	0.041**	0.040**	0.040**	0.054	0.054	0.053
	(0.018)	(0.018)	(0.018)	(0.033)	(0.033)	(0.033)
R-squared	N/A	N/A	N/A	0.66	0.66	0.66
N	14,337	14,337	14,337	14,337	14,337	14,337
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/Medical institutions			X			X

Notes: Estimates in columns (1)–(3) are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. Columns (4)–(6) report the estimates from unweighted OLS with standard errors clustered at the county level in parentheses. The outcome in these columns is the logged infant mortality rate where we add 0.5 deaths to the observations with no infant deaths before taking the log. Changing this number to 0.1 has no effects on our estimates. The sample size is 14,337 (683 triads of counties for each year from 1933 to 1939 excluding six observations with no births in the year). See Table 1 for the definition of each control variable. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table B2 again compares our preferred set of estimates from MLE to those of an equivalent regression of the logged infant mortality rate on the same covariates, estimated this time via OLS weighted by the number of births in a county. While the results are quite similar in magnitude, there is a distinct decline in precision as we move to columns (4) through (6) with the associated standard errors increasing by roughly 35%. This suggests that even weighted OLS is not the ideal estimation technique for our sample.

Table B2: MLE versus Weighted OLS Estimates

	MLE			Weighted OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Dryish in initial year	0.008	0.009	0.009	0.008	0.009	0.008
	(0.016)	(0.016)	(0.016)	(0.020)	(0.020)	(0.020)
Dryish in subsequent years	0.046***	0.046***	0.047***	0.049**	0.050**	0.049**
	(0.016)	(0.016)	(0.016)	(0.022)	(0.022)	(0.022)
Wet in initial year	0.014	0.013	0.013	0.009	0.008	0.008
	(0.017)	(0.017)	(0.017)	(0.020)	(0.020)	(0.020)
Wet in subsequent years	0.041**	0.040**	0.040**	0.037	0.036	0.036
	(0.018)	(0.018)	(0.018)	(0.024)	(0.024)	(0.024)
R-squared	N/A	N/A	N/A	0.75	0.75	0.75
N	14,337	14,337	14,337	14,337	14,337	14,337
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/Medical institutions			X			X

Notes: Estimates in columns (1)–(3) are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. Columns (4)–(6) report the estimates from weighted OLS with standard errors clustered at the county level in parentheses. The outcome in these columns is the logged infant mortality rate where we add 0.5 deaths to the observations with no infant deaths before taking the log. Changing this number to 0.1 has no effects on our estimates. The sample size is 14,337 (683 triads of counties for each year from 1933 to 1939 excluding six observations with no births in the year). See Table 1 for the definition of each control variable. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Appendix C: Incorporating All US Counties and their Spatial Distribution into the Empirical Model

As an extended robustness exercise, we can more fully exploit the underlying dataset collected for this project which tracks the prohibition status of all counties in the then 48 states and which is depicted in Figure 4. In so doing, we necessarily have to consider a specification which moves away from the use of triad-year fixed effects but which preserves much of our concern with controlling for as much variation in unobservables as possible. One candidate in this vein would be a standard panel specification with county and year fixed effects as reported in Column (7) in Table 5 for our sample of triads of nearby 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 for all 3,043 counties in the then 48 states.

Table C1 reports the results of this specification with county and time fixed effects for all US counties via binomial fixed effect maximum likelihood estimation. Here, the triad-year fixed effects δ_{st} are constrained to be identical for all triads (so that $\delta_{st} = \delta_t$), and we use a sample of all US counties (with no multiply-observed counties) rather than the sample of triads of nearby counties. In Table C1, we see qualitatively similar results to those in Table 3, but with somewhat smaller magnitudes attached to *dryish in subsequent years* and *wet in subsequent years*. The results in column (3) suggest that the effect of one county's (or state's) decision to go wet on its neighboring dry counties corresponds to a 2.3% increase in infant mortality for those counties affected and the effect of one county going wet corresponds to a 2.0% increase in infant mortality for those counties affected.

While both estimates are smaller than our preferred estimates in the previous section, we should again view these results with a considerable caution. Here, in addition to the stricter common trend assumption as mentioned above, using the full sample of all US counties introduced much more heterogeneity—both observed and unobserved. Thus, while we report these estimates for the sake of the completeness, our preferred estimates come from the specification that exploit only within triad-year variation using the triads of nearby counties (Table 3).

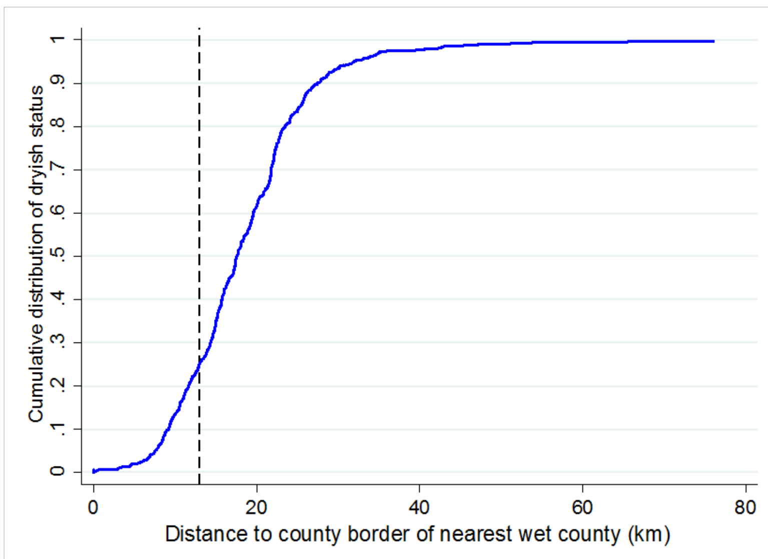
Table C1: The Effect of Repeal on Infant Mortality Rates, Nation-wide Sample

	(1)	(2)	(3)
Dryish in initial year	0.029**	0.029**	0.029**
	(0.012)	(0.012)	(0.012)
Dryish in subsequent years	0.024**	0.023*	0.023*
	(0.012)	(0.012)	(0.012)
Wet in initial year	0.006	0.006	0.006
	(0.008)	(0.008)	(0.008)
Wet in subsequent years	0.020**	0.020**	0.020**
	(0.009)	(0.009)	(0.009)
N	21,291	21,291	21,291
N of county	3,043	3,043	3,043
County & year FEs	X	X	X
County controls	X	X	X
New Deal spending		X	X
Hospital beds/Medical institutions			X

Notes: All estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. The sample size is 21,291 (3,043 counties for each year from 1933 to 1939 excluding 10 observations with no births in the year). County controls are the variables reported in Table 1 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 *medical institutions* is the number of medical institutions per 1000 people in a county (both of which are time-varying). Significance levels: *** p<0.01, ** p<0.05, * p<0.10

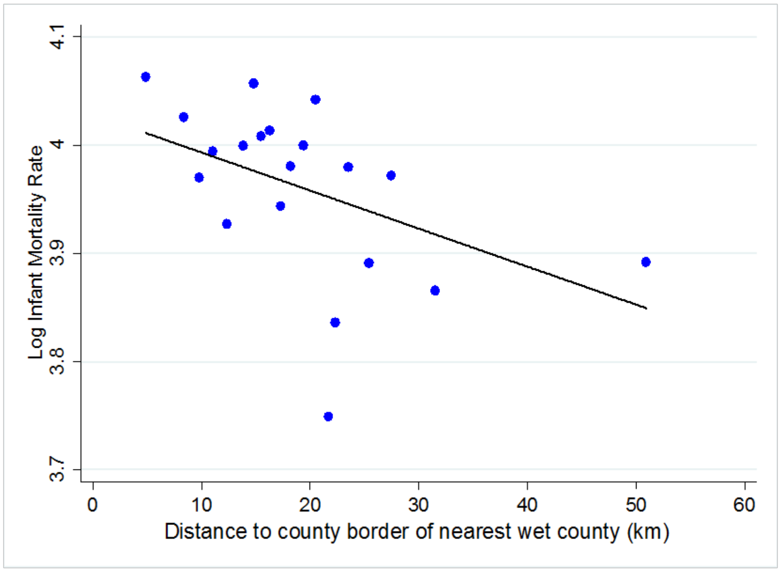
In the results presented in Table C1, our main explanatory variable of interest is dryish, an indicator variable for whether a dry county has at least one wet neighbor. One of the primary reasons for using this particular specification is to increase the statistical power of our estimation. However, dryish counties may be heterogeneous. In particular, the spatial distribution of counties might need to be taken into account. That is, what might matter is not only whether a neighboring county chooses wet status, but also how far away that county is. For instance, counties in the eastern and western halves of the United States have widely divergent sizes: New York County comprises 59 square kilometers while San Bernardino County comprises 52,072 square kilometers, implying that their respective neighbors could be very near or very far. Figure C1 below lays out the cumulative distribution of dryish county seats by distance to the border of the nearest wet county. The sample is comprised of the 700+ counties which are dryish at some point in between 1934 and 1939. The horizontal line corresponds to the first quartile of 13.0 kilometers, suggesting dryish counties were, in the main, close to their wet neighbors (for presentation purposes, we limit the value of the *x*-axis to 80 kilometers while the maximum value observed in sample is 125.8 kilometers).

Figure C1: Cumulative Distribution of Dryish Counties by Distance to Nearest Wet County



As a first step, we can consider the unconditional relationship between distance to the nearest wet county and infant mortality rates as in Figure C2. More specifically, the raw correlation between the distance to the county border of the nearest wet county and the logged infant mortality rate is plotted. The sample is comprised of the 700 counties which are dryish at some point in between 1934 and 1939 (N = 2,546). We use the command “binscatter” in Stata (Stepner, 2016) to generate an equal-sized binned scatterplot. The solid line is the line fitted by OLS when we assign the distance in the contemporaneous year for the initial year when the county becomes dryish and the distance from the previous year for the subsequent years, in order to be consistent with our main specification. Thus, there is some indication of a negative relationship in between the two variables.

Figure C2: Distance to Nearest Wet County and Infant Mortality Rate



We now formalize this relationship by incorporating distance to the nearest wet county into our empirical model and replicating the specifications of Table C1. Here, we make a distinction between dryish counties which are near and dryish counties which are far from neighboring wet counties. Ideally, we would like to split this distance to the nearest wet county into multiple bins in order to estimate the differential effects by distance in a flexible way (e.g., Lovenheim and Slemrod, 2010). However, since the number of dryish counties is not very large, we will unfortunately lack power in that type of specification. We instead choose to impose a degree of parameterization to increase the statistical power. In this case, we would like our distance measure to satisfy the following two conditions: (1) its value should approach one as the distance to the nearest wet county approaches zero and (2) its value should approach zero as the distance to the nearest wet county increases.

One natural starting point might be the inverse of the distance to the nearest wet county. However, its value approaches infinity as the distance approaches zero. Slightly modifying this measure, we define an inverse-distance measure as follows:

$$\text{Inverse distance} = \begin{cases} 1 & \text{if } \mathrm{d} \le \mathrm{L} \\ L/d & \text{if } \mathrm{d} > \mathrm{L} \end{cases}$$

where d is the distance to the nearest wet county and the L is a threshold value. In this way, our *inverse distance* measure is bounded between 0 and 1. To be consistent with our main specification, we assign the distance (d) in the contemporaneous year for the initial year and assign the distance from the previous year for the subsequent years. In terms of the distance measure (d), we consider both the distance from a county seat to the border of the nearest wet county (our preferred choice) and the distance from a county seat to the county seat of the nearest wet county. In terms of the threshold distance (L), we take the 25th percentile of distance to the nearest wet county among dryish county as our default. We also report the results using the 5th and 10th percentiles. Thus, our default L, the 25th percentile value for the distance to the nearest wet county border, is 13.0 kilometers as shown in Figure C1 while that for the distance to the nearest wet county seat is 28.6 kilometers.

To follow our main specification, we also separately include wet county/state in the initial year and wet county/state in subsequent years. Thus, the control group is bone-dry. In this way, we exploit the heterogeneity among dryish counties where the variation comes from the distance to the nearest wet county. However, we choose to not take any of these specifications as our baseline because: (1) there is a relative lack of power when we incorporate this heterogeneity in distance; (2) the inverse distance measure likely suffers from measurement error, primarily for the fact that we do not know the distribution of population within the counties and, thereby, cannot calculate population-weighted distances to the nearest wet county; and (3) it is unclear what functional form any inverse distance measure should take as the decay rate of a variable like dryish is unknown.

With these caveats in mind, Table C2 presents the results of our preferred specification incorporating distance to the border of the nearest wet county. Table C3 presents the results of a very similar specification which incorporates distance to the nearest wet county seat while Table C4 presents the results for both measures of distance, but with varying threshold values (L).

Consider column (1) of Table C2. Again, we have statistically significant effects estimated for both dryish counties in subsequent years along with *wet counties in subsequent years*. Naturally though, the interpretation of the coefficients is slightly different. Here, the value of 0.028 for *inverse distance in subsequent years* suggests that dryish counties which were 13 kilometers or less away from a wet border experienced a 2.8% rise in infant mortality while dryish counties which were more than 13 kilometers from a wet border experienced less than a 2.8% rise in infant mortality. For instance, moving from the 25th percentile of distance (13.0 kilometers) to the 75th percentile of distance (22.3 kilometers) entailed a decline in the estimated effect of being dryish from 2.8% to 1.8%.

The remaining columns of Table C2 suggest that materially the same results arise across the use of different controls. Table C3 speaks to a broad equivalence of results when using the distance separating dryish county seats from wet county seats as opposed to wet county borders, although the results for wet county borders generally have greater power and for *inverse distance in subsequent years* are now statistically insignificant. Finally, Table C4 suggests that our distance-related results are relatively robust, regardless of the choice of the threshold value. Cumulatively, the tables speak to the fact that geography was an important factor in mediating our results for the nationwide sample.

Table C2: The Effect of Repeal on Infant Mortality Rates,
Distance from Dryish County Seats to Nearest Wet County Borders

	(1)	(2)	(3)
Inverse distance in initial year	0.041***	0.040***	0.040***
	(0.015)	(0.015)	(0.015)
Inverse distance in sub. years	0.028*	0.027*	0.027*
	(0.015)	(0.015)	(0.015)
Wet in initial year	0.006	0.006	0.006
	(0.008)	(0.008)	(0.008)
Wet in subsequent years	0.020**	0.020**	0.020**
	(0.009)	(0.009)	(0.009)
N	21,291	21,291	21,291
N of county	3,043	3,043	3,043
County & year FEs	X	X	X
County controls	X	X	X
New Deal spending		X	X
Hospital beds/Medical institutions			X

Notes: Estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. The sample size is 21,291 (3,043 counties for each year from 1933 to 1939 excluding 10 observations with no births in the year). *Inverse distance* takes a value of one if the distance from a dryish county seat to the nearest wet county border (d) is less than or equal to the threshold (L) and takes the value of L/d if the distance is greater than the threshold (L). Therefore, *inverse distance* is bounded between 0 and 1. To be consistent with our main specification, we assign the distance (d) in the contemporaneous year for the initial year and assign the distance from the previous year for the subsequent years. Here, we take the 25th percentile of distance to the nearest wet county border which is 13.0 kilometers (as shown in Figure C1). Column (1) includes our baseline covariates. County controls in column (2) are the variables reported in Table 1 interacted with a linear trend, except for *retail sales* which is time-varying. *New Deal spending* in column (2) is the cumulative amount of county-level New Deal spending per capita interacted with a linear trend. *Medical institutions* in column (3) is the number of medical institutions per 1000 people in a county while *hospital beds* is the number of hospital beds 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 C3: The Effect of Repeal on Infant Mortality Rates, Distance from Dryish County Seats to Nearest Wet County Seats

	(1)	(2)	(3)
Inverse distance in initial year	0.041***	0.041***	0.041***
	(0.015)	(0.015)	(0.015)
Inverse distance in sub. years	0.024	0.024	0.024
	(0.015)	(0.015)	(0.015)
Wet in initial year	0.005	0.006	0.006
	(0.008)	(0.008)	(0.008)
Wet in subsequent years	0.019**	0.019**	0.019**
	(0.009)	(0.009)	(0.009)
N	21,291	21,291	21,291
N of county	3,043	3,043	3,043
County & year FEs	X	X	X
County controls	X	X	X
New Deal spending		X	X
Hospital beds/Medical institutions			X

Notes: Estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. The sample size is 21,291 (3,043 counties for each year from 1933 to 1939 excluding 10 observations with no births in the year). *Inverse distance* takes a value of one if the distance from a dryish county seat to the nearest wet county border (d) is less than or equal to the threshold (L) and takes the value of L/d if the distance is greater than the threshold (L). Therefore, *inverse distance* is bounded between 0 and 1. To be consistent with our main specification, we assign the distance (d) in the contemporaneous year for the initial year and assign the distance from the previous year for the subsequent years. Here, we take the 25th percentile of distance to the nearest wet county seat which is 28.6 kilometers. Column (1) includes our baseline covariates. County controls are the variables reported in Table 1 interacted with a linear trend, except for *retail sales* which is time-varying. *New Deal spending* in column (2) is the cumulative amount of county-level New Deal spending per capita interacted with a linear trend. *Medical institutions* in column (3) is the number of medical institutions per 1000 people in a county while *hospital beds* is the number of hospital beds 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 C4: The Effect of Repeal on Infant Mortality Rates,
Distance from Dryish County Seats to Nearest Wet Counties Using Different Thresholds

	Nearest wet county borders			Nearest wet county seats		
Percentile of threshold (*L*)	5th	10th	25th	5th	10th	25th
Threshold (*L*) in km	7.5	9.2	13.0	16.0	20.9	28.6
	(1)	(2)	(3)	(4)	(5)	(6)
Inverse distance in initial year	0.055***	0.048***	0.040***	0.062***	0.051***	0.041***
	(0.021)	(0.018)	(0.015)	(0.020)	(0.018)	(0.015)
Inverse distance in sub. years	0.038*	0.031	0.027*	0.024	0.026	0.024
	(0.022)	(0.019)	(0.015)	(0.022)	(0.019)	(0.015)
Wet in initial year	0.006	0.006	0.006	0.005	0.006	0.006
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Wet in subsequent years	0.019**	0.019**	0.020**	0.017*	0.019**	0.019**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
N	21,291	21,291	21,291	21,291	21,291	21,291
N of county	3,043	3,043	3,043	3,043	3,043	3,043
County & year FEs	X	X	X	X	X	X
County controls	X	X	X	X	X	X
New Deal spending	X	X	X	X	X	X
Hospital beds/ Medical institutions	X	X	X	X	X	X

Notes: Estimates are from binomial fixed effect maximum likelihood estimation (MLE), bias-corrected following Hahn and Newey (2004), and consistent for slowly increasing T. Standard errors clustered at the county level are reported in parentheses. The sample size is 21,291 (3,043 counties for each year from 1933 to 1939 excluding 10 observations with no births in the year). *Inverse distance* takes a value of one if the distance from a dryish county seat to the nearest wet county border (d) is less than or equal to the threshold (L) and takes the value of L/d if the distance is greater than the threshold (L). Therefore, *inverse distance* is bounded between 0 and 1. To be consistent with our main specification, we assign the distance (d) in the contemporaneous year for the initial year and assign the distance from the previous year for the subsequent years. For columns (1) through (3), we change the threshold (L) to the 5th, 10th, and 25th percentile of distance to nearest wet county border, respectively. Thus, column (3) replicates column (1) of Table C1. Columns (4)–(6) instead use the distance to nearest wet county seat with column (6) replicating column (1) in Table C2. County controls are the variables reported in columns (1) and (3) of Table 2 interacted with a linear trend, except for *retail sales* which is time-varying. Significance levels: *** p<0.01, ** p<0.05, * p<0.10