Closing Time: The Local Amenity Effects of Prohibition*

Greg Howard†
University of Illinois

Arianna Ornaghi‡
University of Warwick

August 31, 2020

Abstract

How do amenities affect local land values, production, and sorting? We study the question exploiting a large historical policy change: U.S. Alcohol Prohibition in the early 20th century. Comparing same-state early and late adopters of county dry laws in a difference-in-differences design, we find that early Prohibition adoption increased population and farm real estate values. Moreover, we find strong effects on farm productivity consistent with increased investment due to a land price channel. In equilibrium, the amenity change disproportionately attracted immigrants and African-Americans.

JEL Codes: N91, R13, E22

Keywords: migration, land values, productivity, amenities, credit

*We'd like to thank Alex Bartik, Camilo García-Jimeno, Jason Faberman, James Fenske, Jack Liebersohn, lunch participants at Warwick and Illinois, and conference participants at the European UEA Meetings, the International AREUEA Meetings, and the European Economic Association Meetings. We received funding for the project from the Centre for Competitive Advantage in the Global Economy at the University of Warwick.

†glhoward@illinois.edu

‡a.ornaghi@warwick.ac.uk

> The fact of Malden being a "no-license" city has been the cause of many people choosing it as a place of residence [...] The result of the absence of the sale of liquor is that we have the cleanest of streets, a large absence of crime, many who own their own homes, a savings bank with large deposits, and everything that goes to make us a happy and prosperous community.
>
> *George Louis Richards, Mayor of Malden, Massachusetts*
> *1908 Anti-Saloon League Handbook*

1 Introduction

Prohibition, a set of laws to restrict alcoholic beverages, was a major policy issue at the beginning of the 20th century. Efforts to limit access to alcohol culminated with the ratification of the 18th Amendment and the passage of the Volstead Act in 1919, which made the production and sale of alcoholic beverages illegal nationwide. Prohibition, however, did not originate as a federal policy: in 1919 the country was already a patchwork of dry laws at the state, county, and local levels. As the quote from Richards illustrates, local Prohibition aimed to rid communities of not only alcohol consumption but also its negative consequences.[1] For the mostly rural areas that enacted it, local Prohibition was a major amenity change.

In this paper, we investigate the local equilibrium effects of this amenity.[2] Economists have significant interest in how amenities affect economic activity and the sorting of people (e.g. Florida, 2003; Diamond, 2016). Yet most amenity changes are gradual or extremely local in nature, making the equilibrium effects at the county- or city-level hard to identify.[3] Dry laws, being an abrupt policy shock affecting the entire locality, provide a rare opportunity to explore the role of a large amenity change in a local economy.

Our empirical strategy estimates the effect of Prohibition by comparing same-state rural counties with similar preferences towards alcohol that introduced dry laws slightly earlier (1900-1909) or slightly later (1910-1919) at the beginning of the 20th century in a

[1] Or as Richards puts it, "the blighting influence of the open saloon."

[2] Prohibition is a policy, and the effects of the policy–rather than the policy itself–are more likely to directly affect location decisions. Throughout the paper, when we refer to the amenity effects of Prohibition, we are referring to the bundle of amenities that the policy changed, including but not limited to the ones Richards mentions. We discuss the existing research about the effects of Prohibition in Section 2.

[3] Consider parks. An individual park is quite local, so one can estimate the premium on house prices in the few blocks around them. This does not identify the effect of the park on house prices throughout the city because there may be substitution between houses. But at the city-level, the number of parks usually changes gradually, so is difficult to disentangle from other trends.

differences-in-differences design.[4] Since we allow for counties to be on different trends based on their initial demographics, the identification assumption is that the remaining variation in timing is as good as random. We provide supporting evidence of the identification assumption with event studies that allow us to check for parallel trends.[5]

We find that local Prohibition had significant economic effects on these rural counties. First, Prohibition increased population and land prices, consistent with it being an amenity that people value. Second, we show that counties that enacted Prohibition saw increases in labor productivity and capital investment after they became dry, consistent with agglomeration that comes through a land wealth channel. We also see an increase in banks in the areas, suggestive of more lending.[6] Third, we show counterintuitive sorting patterns: counties with local Prohibition attracted relatively more immigrants and African-Americans. Given that these groups tended to be less in favor of Prohibition, these sorting patterns seem unlikely to be driven by preferences for the amenity, but could be coming from labor market opportunities.

The causal effect of the amenity on productivity is one of our more novel results. The causal channel we propose is similar to the effect of land prices on investment in modern times (Chaney, Sraer and Thesmar, 2012; Bahaj, Foulis and Pinter, 2020): land owners become wealthier and have more access to collateral, so they can invest more in their businesses or, in this case, farms.[7] We see this in a large increase in farm equipment. In addition, looking at heterogeneity across *ex ante* measures of banking intensity, the effects are stronger in areas with more banks per capita and higher mortgage shares. This result has interesting implications for other amenities, and suggests that investing in local amenities could have productivity effects by increasing the value of land.

Prohibition has one major advantage for studying the effects of amenities: it is a large sudden change allowing for difference-in-differences estimation. We are not aware of any other sudden amenity changes that affect entire labor markets, making this a unique setting to understand the equilibrium effects of amenities.[8] But it also has a couple of

[4]A small fraction of states and counties had prohibited alcohol before this, which we drop. We additionally focus on rural areas, as Prohibition was not popular in urban areas, and was therefore unlikely to be viewed as a positive amenity. Correspondingly, few urban counties adopted Prohibition voluntarily, so there is not much variation lost by focusing on rural counties.

[5]In addition, we do not find evidence that counties that adopted Prohibition earlier were different on economic variables, once we condition on demographics.

[6]We follow Rajan and Ramcharan (2015) in using the number of banks as a good proxy for credit availability.

[7]In 1914, more than 20 percent of farm loans from commercial banks were collateralized with farmland (Rajan and Ramcharan, 2015). Mortgage credit was also extended by life insurance companies and wealthy individuals.

[8]One could argue that natural disasters, or new economic development would have large amenity effects, but they are not primarily an amenity change like Prohibition. A modern version of the Prohibition

downsides that we work to address. The first disadvantage is that it is not randomly assigned, so plausible identification is a challenge. Because of the historical and political context, we use within-state variation and dynamically control for the initial demographic characteristics that predict Prohibition adoption. We think that the remaining variation, possibly due to the timing and scarce resources of the Prohibition movement, can plausibly identify the effects of Prohibition.

The second disadvantage to our setting is that Prohibition could have had direct effects on productivity by making workers more sober.[9,10] This seems less likely than our land price channel for a few reasons. First, we find stronger effects in counties with railroads and counties that bordered wet counties. This makes sense because population and land prices would increase more in areas that are more accessible to migrants. It is inconsistent with a sobriety story because alcohol would likely be more accessible in these areas as well. Second, the share of workers in farms decreased. If the productivity increase were due only to more sober workers, we would expect farmers to hire more labor. But if the productivity increase is because of labor-substituting farm equipment, the decrease in farm-employment shares makes sense. Last, while Prohibition did shut down saloons, historians debate to what extent consumption decreased. Counties that passed Prohibition had large numbers of Protestants, many of which would have taken teetotaler pledges with their church (Okrent, 2010).[11] And for those that did drink, they were not prohibited from buying it in a neighboring county or state until the Webb-Simpson Act of 1913.[12]

Our work is directly related to the literature on the effects of alcohol prohibition, in the

shock is the legalization of marijuana (possibly positive or negative), but this is ongoing, and it may be several years before we can study the equilibrium effects. In addition, it has primarily taken place at the state level, meaning there are many fewer observations.

[9]Well-identified evidence on the relationship between alcohol and productivity is scarce. While it is generally believed that drinking is associated with decreased productivity, and that Prohibition was partly motivated by the need to have sober industrial workers, in a randomized control trial of rickshaw drivers in India, Schilbach (2019) finds no effect of decreased alcohol consumption on productivity (or other economic outcomes, other than saving).

[10]A related possibility is that Prohibition might have attracted more productive workers. However, we find that the people moving in are disproportionately groups that did not favor Prohibition (men, immigrants, and non-white people) suggesting that they are drawn by the labor market improvements, rather than the cause of the labor market improvements. Of course, there could be selection on unobserved dimensions such as proclivity to consume alcohol which go against the demographic proxies we can observe in the data.

[11]A teetotaler is a person who does not drink alcohol, and a teetotaler pledge is a promise to not drink alcohol. Such pledges are sometimes made in a group setting so members can hold one another accountable.

[12]Since some activities regarding alcohol were illegal, the local data on it is less reliable. Miron and Zwiebel (1991) shows only modest declines in national alcohol consumption per capita, and Dills and Miron (2004) finds a small effect of state-level Prohibition on cirrhosis deaths, roughly 5 percent, which is a liver disease caused by alcohol consumption. Law and Marks (2018) finds larger effects.

United States and abroad. Previous work has focused on the effect of Prohibition on alcohol consumption (Miron and Zwiebel, 1991; Dills and Miron, 2004; Law and Marks, 2018), infant mortality (Jacks, Pendakur and Shigeoka, 2017), violence (Owens, 2014), crime (Owens, 2011; Heaton, 2012), the brewing industry (Hernández, 2016), and innovation (Andrews, 2019).[13] We add to this by demonstrating that Prohibition had large equilibrium outcomes because it was valued as an amenity. There is renewed interest in this question because of recent changes to laws on marijuana (Caulkins, Kilmer and Kleiman, 2016). Indeed, Cheng, Mayer and Mayer (201) claim the legality of marijuana is a positive amenity that causes house price increases of about 6 percent.

Unlike some of these other papers, we focus only on the pre-18th-Amendment period, when Prohibition was voluntarily introduced by many localities. While the introduction and repeal of Federal Prohibition has desirable qualities for exogeneity, the former was forced upon areas that likely did not value it, and the latter was after it had generally been recognized to have negative social consequences (García-Jimeno, 2016). Therefore, only our time period is appropriate for studying positive amenity changes.

Our work also contributes to the literature on the effects of amenities on house values and other local economic outcomes. Like many in this literature, we find that amenities are associated with higher land values (Rosen, 1979; Roback, 1982, and many more since then). We then find evidence of agglomeration that results from these higher land values, which we believe is a novel result. Lastly we show sorting patterns from higher land prices that are quite different than other studies, because they have focused on amenity's effects on rents, rather than productivity (Guerrieri, Hartley and Hurst, 2013; Diamond, 2016; Autor, Palmer and Pathak, 2017; Su, 2020).[14]

The last strand of literature to which we contribute focuses on the role of credit in early 20th century farming (e.g. Pope, 1914; Fulford, 2015; Rajan and Ramcharan, 2015; Jaremski and Wheelock, 2018; Jaremski and Fishback, 2018; Carlson, Correia and Luck, 2020). Our results are consistent with this literature as credit plays an important mediating role in our land price channel.

The rest of our paper is organized as follows: in the next section, we provide some historical background that motivates our study design. Next, we present a toy model that highlights our novel agglomeration force. Then, we discuss our data and our empirical strategy. Finally, we present our results and conclude. Figures and tables are collected in

[13] We discuss the findings of these papers in Section 2.

[14] There are many differences in setting between our setting and these other studies. It is likely still true that amenities have some effect on the distribution of wages, but that effect may be different than the effects of Prohibition on farm productivity. In addition, housing has grown in cost over time, so our different results could be due to this new importance.

the back.

2 A Brief History of Prohibition

Prohibition has a long history in the United States. As shown in Figure I, which plots the share of counties that were dry in any given year from 1800 to 1920, legislation restricting access to alcoholic beverages started appearing at the local level around 1850. In the next fifty years, the share fluctuated, but never became higher than 30 percent.[15] The fortune of Prohibition changed drastically after 1900: in the short span of 20 years, dry laws went from being the exception to the rule. This process culminated in 1919, when the ratification of the 18th Amendment and the passage of the Volstead Act officially banned the production, sale, and transport of alcohol in the entire United States.

This paper focuses on the years between 1900 to 1920, the so-called Third Wave of the Temperance Movement, which correspond to the period when local and state dry laws experienced rapid expansion.[16] Most historians credit the success of the Third Wave to the Anti-Saloon League, which was founded in 1893 in Ohio, but soon developed into a national force to the point of assuming the leadership of the temperance movement by the end of the 20th Century (Okrent, 2010).

The Anti-Saloon League was rooted in local institutions, under the guidance of the national organization. Local churches were fundamental in this effort: the Anti-Saloon League managed to gain support of those religion denominations in favor of Temperance to the point that it self-described itself as "The Church in Action Against the Saloon" (Anderson, 1910). As this quote and the name suggest, while previous instances of the Temperance movement focused on abstention from drinking, the League's main focus was to restrict access to saloons, which were seen as the chief facilitator of excessive drinking.

The key to the Anti-Saloon League success was that it was a single-issue non-partisan movement, willing to support any candidate that would vote to ban saloons. By focusing on a single issue they were able to build a broad coalition in support of specific dry can-

[15]Most of the counties were rural, where Prohibition was more popular but which tended to be less populated: the fraction of the population in dry counties was slightly lower than the fraction of dry counties. The figure does not correct for county boundary changes.

[16]The First Wave of the Temperance movement corresponds roughly to the 1850s. Twelve states passed dry legislation during the decade, but all of them with the exception of Maine repealed them during the Civil War, perhaps a sign of increased need for revenues coming from liquor licenses (Owens, 2011). The Second Wave of Prohibition (1880-1895) saw the rise of the Women's Christian Temperance Union (WCTU), which became the force behind a number of local option and state Prohibition laws. The panic of 1893, which once again enhanced the need of government revenues while weakening the WTCU, together with the difficulty of implementing a political strategy based on the Prohibition Party, put an end to the second wave of dry law expansions (Hamm, 1995)).

didates across party lines (Kerr, 1985). This was in sharp contrast to both the Prohibition Party and the WTCU, which in particular had supported numerous social reforms as part of its political project, but had failed to make substantial progress on its main reform of interest (Skocpol, 1995).

By focusing on a single-issue, the Anti-Saloon League managed to hold together disparate groups who could all get behind the objective of limiting access to the saloon. As Okrent (2010) notes, "five distinct, if occasionally overlapping, components make up this unspoken coalition: *racists*, progressives, *suffragists*, populists... and *nativists* [emphasis added]."[17] In addition, the Anti-Saloon League was politically savvy, and would focus its resources on fighting battles that had a higher likelihood of succeeding: "Study local conditions and reach after the attainable."[18] At the national level, they would push for states to allow counties to have "local options" and work from the local level up if an outright ban was unfeasible.

As one might expect from the nativist, racist, and suffragist undertones of the Temperance coalition, Prohibition was more popular among white people, natives, and women. Table I shows the coefficients from a Cox survival model exploring how county characteristics relate to adoption. Consistent with the historical narrative and existing evidence (e.g. Lewis (2008)), we find that less populous counties, counties with higher shares of whites, females, natives, and people adhering to anti-alcohol religious denominations adopted Prohibition earlier.[19,20]

The effect of Prohibition on alcohol consumption is still debated. Typically, county dry laws closed down saloons and the production of alcohol, but did not involve prohibiting the consumption of alcohol in the home. The effect of Prohibition on alcohol consumption may not have been large for two reasons: many counties in which Prohibition was popular already had high numbers of teetotalers; members at most of the churches behind the Anti-Saloon League would make vows to not drink. In addition, those that did drink were often still able to get alcohol. Until 1913 with the passage of the Webb-Simpson Act, it was legal to ship alcohol across state lines, including states in which alcohol was banned, so it was still available for those that wished to have it for private consumption.

The empirical evidence on consumption is mixed. Data on national alcohol consump-

[17] In Section 5, we discuss the effects that having populists and progressives within the coalition might have had on other policy reforms. Because these reforms take place primarily at the state-level and after 1910, we do not think they are likely to bias our results.

[18] American Issue 8 (Jan. 1990).

[19] This is consistent with Strumpf and Oberholzer-Gee (2002), who estimates these preferences after the repeal of Federal Prohibition.

[20] Conditional on demographics, economic variables do not help predict the early adoption of Prohibition, as we show in Column (3). We will revisit this when discussing the empirical specification.

tion per capita show very little movement between 1900 and 1918, suggesting the effects of local Prohibition did not have much effect on alcohol consumption before federal Prohibition (Miron and Zwiebel, 1991). Dills and Miron (2004) estimate the state prohibitions had less than one percent effect on cirrhosis from 1910-1920, and less than a five percent effect from 1920-1933. However, recent work by Law and Marks (2018) that takes into account the presence of local dry laws finds up to 30% lower deaths due to alcoholism in fully dry versus fully wet states. In line with these results, Prohibition appears to have had positive spillovers on children's health. For example, Evans, Helland, Klick and Patel (2016) find that exposure to Prohibition in utero increased educational attainment and decreased obesity, while Jacks et al. (2017) find that counties that waited to repeal Prohibition had lower infant mortality than counties that became wet immediately after 1933.

Prohibition might also have affected crime. Owens (2011) finds no overall effects of state dry laws on overall homicide rates, although in Owens (2014) the rates increased for young adults with respect to other age ranges, perhaps in line with higher market-based violence. To the extent that these studies also include the introduction of Federal Prohibition as part of their treatment, it is unclear to what extent we should expect to see the same effect for the local dry laws we are examining, where the incentives to engage in illegal markets were likely much more limited and there was likely less tension between the public and law enforcement on the matter.[21]

Alcohol taxation was a major form of revenue for the government during this time period. Counties that pass dry laws forego this revenue. Data on this is not available at the county level during this time period (Jacks et al., 2017). Moreover, this would likely bias our results downward, as higher other taxes or lower public services would lower demand to live in Prohibition counties.

3 Model

In this section, we present a brief stylized model to illustrate the economic forces that we believe are driving the empirical results we will present in this paper. The model is of a single county and captures production and the choice of where to live. It includes a somewhat novel agglomeration mechanism.[22]

[21] García-Jimeno (2016) finds that the homicide rate increased after Federal Prohibition, and that the increase was larger in cities with stronger wet constituencies. This is also consistent with the idea early supporters of Prohibition might also have seen limited effects on crime.

[22] While there is empirical evidence that higher land prices cause higher productivity and investment (Chaney et al., 2012; Bahaj et al., 2020), we are not aware of such a force ever being considered as the basis for agglomeration.

In our model, there are several types j of agents i that differ in their mobility and their amenity preferences. There is a bank that provides capital at rate r and imposes a land collateral constraint.

Agents have preferences over consumption and amenities. Utility is given by

$$c + a_j + \epsilon_{i,j}$$

where a_j is the preference for the amenity by group j and $\epsilon_{i,j}$ is an idiosynchratic preference parameter for person i, although the distribution of such parameter may depend on j. Assume that it is an i.i.d. draw from a Gumbell distribution with shape parameter $1/\mu_j$. Everyone has an outside option to live in a much bigger region outside the county with utility $\bar{u}_j + \epsilon_{k,j}$, where $\epsilon_{k,j}$ is also drawn from the same Gumbell distribution. Population of group j is therefore given by

$$N_j = \bar{N}_j (c + a_j)^{\mu_j}$$

for a constant $\bar{N}_j$. Total land demand is $L = (\sum_j N_j)$. Combining everything,

$$L = N = \left(\sum_j \bar{N}_j (c + a_j)^{\mu_j} \right) \tag{1}$$

This defines a "migration curve," an upward sloping relationship between c and L or N.

To produce goods, they combine their labor, which is supplied inelastically with 1 unit, 1 unit of land, and capital.[23] Land L is supplied elastically with elasticity σ. Their consumption is given by

$$c = Ak^{\alpha} - p - rk$$

where $\alpha < 1$. They are also subject to a capital constraint by the bank that $rk \leq \phi p$. Assume the constraint is binding, and substitute in the land supply curve:

$$c = A \left(\frac{\phi}{r} \bar{p} L^{\sigma} \right)^{\alpha} - (1 + \phi) \bar{p} L^{\sigma} \tag{2}$$

This "production curve" is non-monotonic: at low values, it is increasing in L, but at high values, it is decreasing in L. The increasing part represents an agglomeration force that comes from the banks' capital constraint.[24]

[23] The production function is a Leontieff nested within a Cobb-Douglass for tractability: $y = Ak^{\alpha} \min\{n, l\}^{1-\alpha}$, where n is labor and l is land.

[24] It is not critical that the worker owns the land or rents the capital. A landowner hiring labor at competitive rates would pay wages that fund the consumption in equation (2).

Using equations (1) and (2), we illustrate the equilibrium in Figure II. The equilibrium of this model is found at the intersection of the two lines. When the amenity value increases, the line representing equation (1) shifts to the right. If it is on the upward-sloping portion of the equation (2) line, consumption increases in response because the workers are able to invest more heavily in capital.

This model explains the mechanism through which we think our empirical results are best understood. As we will show in the data, an increase in amenities for any group raises population, land prices, capital, and output per capita. This comes through an agglomeration force based on collateral constraints of banks.

Note that the amenity does not have to be valued by every group to increase consumption of every group. So even if the amenity only matters to some of the groups, it can still increase population of all groups. If the mobility parameter μ_j is high enough for the group that does not value the amenity, they might still have more people move in in response.[25]

Finally, the model allows us to think about how the effects of an amenity change might based on local characteristics:

- We can interpret larger banking sector as a higher ϕ or a lower r. Either way, the line (2) becomes more steep in the relevant section. So the increases in population, consumption, and prices are all larger.
- We can interpret more railroads or more nearby dry counties as an increase in μ, making line (1) flatter. So a shift in the line would have a larger effect on population, consumption, and prices.[26]

The model also features a financial accelerator in response to a productivity shock. An increase in productivity A relaxes the collateral constraint and increases capital and output because people migrate in, raising prices. An implication of this accelerator is that an increase in sobriety at work might have similar comparative statics as an amenity increase. We have to rely on our auxiliary results discussed in Section 6.3 to argue in favor of our amenity channel.

4 Data

Population and Agricultural Census. County-level data are from the United States Censuses of Population 1880-1920 and of Agriculture 1880-1925. We use the official tabula-

[25] See Section 6.4 for a discussion of the higher mobility of African-Americans and immigrants.

[26] As drawn in the graph, when we change the slope of (1), an amenity shift still moves the curve downward by the same level-shift. Therefore the new equilibrium will be further up line (2).

tions of the related Census publications, which have been digitized and made available by Manson, Schroeder, Van Riper and Ruggles (2018). We use the Population Censuses to study population, share of county population living in urban areas, share male, share white, share 1st generation immigrant, and share 1st and 2nd generation immigrant. We supplement the data for outcomes that were not reported in the original tabulations calculating county level shares from a 25% sample of the census micro-data from IPUMS.[27] We use the Agricultural Censuses to look at land values per county acre, productivity, value of implements, and share of land in farms.[28,29] We adjust county-level data to fixed 1920 boundaries following Hornbeck (2010).

Census of Religious Bodies. We use data on county membership of religious organizations from the 1890 Census of Religious Bodies, also available from Manson et al. (2018), to proxy for baseline preferences for temperance. We divide the different denominations between those in favor of Prohibition (Baptist, Evangelical, Methodist/Episcopal, Mormon, Presbyterian) and against Prohibition (Catholic, Jewish, Lutheran) to define the share of population in favor of Prohibition (García-Jimeno, 2016). County boundaries are similarly normalized to 1920.

Prohibition Adoption. Data on introduction of Prohibition is from Sechrist (2012). The dataset reports information on whether a county was wet or dry yearly from 1800 to 1919, collected from state-specific historical accounts for the earlier period, and Prohibition maps published in the Anti-Saloon League after the turn of the century. If only part of a county was dry, for example if local option was introduced in a town but not in another, the county is categorized as wet: counties are only classified as dry if their entire territory is under Prohibition. However, in our normalization of county boundaries to their 1920 counterpart, we consider a county as dry if any of the parts constituting the 1920 county was dry to be as conservative as possible.

5 Empirical Strategy

The empirical strategy estimates the effect of Prohibition by comparing same state counties with similar preferences that introduced dry laws early (1900-1909) and late (1910-1919) in a differences-in-differences design. The focus of the analysis is the Third Wave of

[27] We prefer to use the official tabulations when possible as they allow us to include 1890, for which the original schedules were mostly destroyed by fire.

[28] Other studies of amenities often use house prices or rents to measure the demand to live in an area. Valuation data on housing is only available in cities and only after 1910, so we use the value of land instead.

[29] We define labor productivity as output per person. Farm output is measured as the quantity of the top five crops by the price in 1910.

Prohibition, spanning the years between 1900 and 1919. During this period, expansion of dry laws was swift, with the share of dry counties going from 20 percent to 100 percent over the short span of twenty years: counties in our treated and control group adopted alcohol restrictions only a few years apart.

Importantly, the effort was spearheaded by the political efforts of the Anti-Saloon League. The League had a strategy of investing their political and social capital in those places where they thought they could have Prohibition passed first, possibly because of already existing local temperance organizations, and then expand from there to the state and national level. This is suggestive that differences in treatment status for same-state counties with similar baseline preferences for Prohibition likely depended on idiosyncratic differences in whether a window of opportunity opened up a couple of years earlier in a county than in another.[30] Figure III shows the geographic distribution of the treatment. The figure shows that overall the West, Midwest and Northeast appear to have adopted Prohibition earlier, but there is still significant variation in adoption year within states.

Given our focus on the Third-Wave of Prohibition, we restrict the sample to counties that were wet in 1899. In addition, we restrict the sample to rural counties.[31] Our investigation is more relevant for rural than urban areas: the majority of counties that were dry were also rural, and these were areas in which Prohibition was likely to be a positive amenity. The resulting sample consists of approximately 2300 counties. While our treatment status is defined looking at the 1900-1920 period, we consider in our analysis the years that span 1890 to 1920 (1925).

The baseline specification we estimate is:

$$y_{ct} = \beta 1(adopted\ before\ 1910)_c * 1(after\ 1910)_t + \eta_{bt} + X'_{c,1890}\theta_t + \alpha_c + \alpha_{st} + \epsilon_{ct} \qquad (3)$$

where y_{ct} is outcome y for county c and year t; $1(adopted\ before\ 1910)_c$ is a dummy equal to 1 if the county introduced a dry law after 1900 but before 1910; $1(after\ 1910)_t$ is a dummy equal to 1 if the year is greater or equal to 1910; η_{bt} are dummies for which decile of the share of population belonging to religions in favor of Prohibition in 1890 the county

[30] For example, looking at the Second Wave of the Temperance Movement, García-Jimeno, Iglesias and Yildirim (2018) found that anti-saloon sentiment tended to move along railroad and telegraph networks, meaning that how closely connected you were to existing temperance movements was an important determinant. While this paper studied the earlier wave, it is likely that similar variation existed in the Third Wave, and it is the type of variation we hope is driving the adoption of Prohibition, once we condition on states and demographics.

[31] A household was considered rural if it did not live in an incorporated place of more than 2,500 people. We define a county as rural if at least 50 percent of its population lived in rural locations, as determined by the 1900 Census.

belongs to, interacted with year dummies; $X_{c,1890}$ are demographic controls according to the 1890 census (population, share urban, share male, share white, share 1st and share 1st and 2nd generation immigrant), interacted with year dummies; α_c are county fixed effects; and α_{st} are state-year fixed effects. By 1920, the whole country is treated, but we continue to look for differential effects because the effects of amenities might be long-lasting. Standard errors are clustered at the county level, but we also always report Conley standard errors allowing for spatial correlation in the errors between counties in a radius of 500km (Conley, 1999).

β estimates the relative change in the outcome after Prohibition is introduced for early versus late adopters of dry laws. The identification assumption is that rural counties that introduced dry laws early in the Third Wave of Prohibition, if not because of these laws, would have experienced similar changes in population and land values as same-state counties with similar Prohibition preferences that introduced them only after 1910. The concern is that late adopters of Prohibition are not a good control group for counties that introduced Prohibition earlier on.

Our preferred specification additionally minimizes endogeneity concerns in a number of ways. First, the inclusion of state-year fixed effects ensures that we are only using within state variation. Second, by including dummies for which decile of the share of population belonging to religions in favor of Prohibition in 1890 the county belongs to, interacted with time dummies, we are comparing counties with similar baseline preferences for Prohibition, which increases the comparability of the two groups.[32,33] Allowing counties with different baseline characteristics to be on different trends with the inclusion of demographic controls interacted with year dummies further enhances the comparability of the two groups of counties. Third, the fact that we include county fixed effects ensures that any fixed difference across the two groups is taken into account in the estimation.

However, we might still worry that places that introduced Prohibition earlier did so because they were experiencing different socio-economic changes to begin with. To take this into account, we additionally estimate the following event study specification for the

[32]Unlike our other demographic controls, we include decile dummies of Prohibition support in order to control non-linearly for it. The probability of adopting Prohibition as a function of religious support looks somewhat like a logistic function, so linear controls would not adequately control for the omitted variable. The other variables do not have such non-linear relationships to our treatment variable.

[33]We are interested in a measure of baseline support for Prohibition. This means that we cannot use another possible proxy for public support for the policy, the share of the wet vote in state-level Prohibition referendum, as it would be measured after adoption for a large fraction of our sample.

1880 to 1925 period:

$$y_{ct} = \sum_{\tau} \beta_{\tau}(adopted\ before\ 1910)_c + \eta_{bt} + \theta_t X_{c,1890} + \alpha_c + \alpha_{st} + \epsilon_{ct} \quad (4)$$

where variables are defined as before and $\tau \in \{1890, 1910, 1920, 1925\}$. In addition to showing how the treatment effect evolves over time, the 1890 coefficients provide an immediate test for pre-existing differences in the two groups with respect to the baseline year. We use this specification to provide supportive evidence for the identification assumption.[34]

Given our most interesting results are about the local economy, it is reassuring that the Cox adoption model we presented earlier (Table I) shows limited predictive power of economic variables, conditional on the demographic controls and state fixed effects. Of the five variables that we group as "economic"—the presence of a railroad, the number of banks per capita, the log of average farm value per acre, the log value of implements per person, the share of land in farms, and the log of productivity—only the log of productivity is marginally significant. Jointly, we cannot reject that all the economic values are zero, suggesting that the treated and control counties have similar economic conditions at baseline.

A related concern to identification is that some other change occurred between 1900 and 1910 that correlates with Prohibition and made counties more attractive and productive. Given that progressives were a key constituency of the temperance movement, we might be particularly concerned about Progressive Era Reforms such as Workers' Compensation and Mothers' Pension coinciding with the introduction of dry laws at the county level. A few points are worth noting in this respect. First, Workers' Compensation and Mothers' Pension were introduced at the state level and, importantly, mostly in during the 1910s (Aizer, Eli, Ferrie and Lleras-Muney, 2016; Fishback and Kantor, 2007).[35] Moreover, in most states agricultural workers were excluded from Workers' Compensation (Fishback and Kantor, 2007), thus making the reform less relevant for the rural setting we study. Because reforms at the state level would be controlled for by the state-year fixed effects and we look at effects starting from 1910, this would not bias our estimates. Second, while the Anti-Saloon League did draw alliances with other movements when

[34]Using the 1890-1920 sample gives us two pre-periods, and two post-periods. We do not go further back in time with our main specification because the advantage of having additional pre-periods trades-off against lower data quality, which comes mainly from the fact that county boundaries were significantly different in 1880 (for example, many Texas counties that give us key variation in treatment did not exist as separate entities).

[35]Workers' Compensation was introduced in earlier years in a handful of states, but was challenged in the courts.

it was convenient to do so, it was at heart a single-issue interest group (Okrent, 2010) and unaffiliated with any political party (Kerr, 1985), which means that it is unlikely that Prohibition was systematically part of large packages of law changes.

6 Results

Our results are organized into four parts. First, we show that local Prohibition was a positive amenity, attracting people and raising land prices. Second, we show that it increased farm labor productivity, and especially raised investment in equipment. Third, we show evidence consistent with the hypothesis that the increased productivity is due to increased investment from higher land values. Finally, we show that Prohibition attracted immigrants and African-Americans, a surprising result given the politics of the Temperance Movement.

6.1 Prohibition is a Positive Amenity in Rural Counties

Figure IV shows the coefficients from the event study specification (equation (2)), together with 95% confidence intervals. Population and land prices increased in counties that passed Prohibition, consistent with the interpretation that Prohibition was a positive amenity. From 1890 to 1900, there is no economically or statistically different trend for these counties in either population or land value, showing that the counties were on similar trends. Then, between 1900 and 1910, population increased by more than 5 percent and land values increased by 10 percent in counties that passed local Prohibition. For population, we show that this increase was sustained until 1920. For land values, we show that it continued to increase from 1910 to 1920, before declining from 1920 to 1925.[36]

Table II presents the estimates of our baseline specification (equation (1)), sequentially adding controls. Column (1), that includes no controls, shows positive and large effects. Including controls, especially baseline religiosity in column (2) and baseline demographics in column (3), decreases the size of coefficients but also makes our estimates more precise. Our preferred specification is reported in Column (4). The table confirms the event study results: Prohibition increased population by 6.1 percent and land prices by

[36]The event studies estimated including 1880, reported in Appendix Figure I, display the same pattern. When we estimate the event studies including 1880, we do not include controls for 1st and 2nd generation immigrant because it is sometimes unmeasured in frontier counties. These counties, largely in Texas and the Dakotas give us significant variation in treatment.

12.7 percent.[37][38] To put this number in perspective, the standard deviation of population growth rates across all rural counties from 1900-1910 was 46 percent. The five-year interstate migration rate in 1910 was over 7 percent (Molloy, Smith and Wozniak, 2011); it may be that Prohibition did not convince people to move, but rather convinced people that were already moving to consider Prohibition in their location decisions.

In Table III, we show that our results are robust to a number of potential concerns. Because Prohibition is not randomly assigned, a concern is that the result might be dependent on our specific choice of sample and control variables. In column (1) we windsorize the outcome variable to show that our results do not depend on outliers. Restricting to counties that implemented Prohibition between 1905 and 1915 to only compare counties that adopted Prohibition within a 10 years interval gives the same result (column (2)). Note that this also eliminates any counties which never voluntarily enact Prohibition. Including counties that have missing values for some of the years, or using 1900 county boundaries leaves the coefficients virtually unchanged (column (3) and column (4)). Finally, in Appendix Figure II we explore heterogeneous effects by share of the population who is rural. There is no effect of Prohibition on population and land values in urban counties, consistent with the idea that the story we are interested in exploring, that Prohibition was an amenity change that could impact land values, only holds in rural counties, which supports our sample restriction.

One concern with using a difference-in-differences strategy to study migration is that there are inevitably violations of the stable unit treatment values assumption (SUTVA), as migrants move from an untreated county to a treated county. To that extent, the point estimates should be taken with caution. But given that a gain in the relative amenity value from one area has to be a decline for other areas, the sign of the estimated coefficient is still correct. In Appendix Table II, we separately examine effects of neighboring counties who are mostly likely to be affected by violations of SUTVA, and we find limited evidence of spillovers in counties that neighbored early adopters but were not early adopters themselves, as compared to the other counties within the state.

[37] Adding one baseline control interacted with year dummies allows us to understand which controls are particularly relevant for the result. Appendix Table I shows that for population, baseline controls gradually decrease the size of the coefficient, with no single control appearing especially important. Instead, controlling for baseline population appears to be most important for land values.

[38] 12.7 percent cannot be interpreted as the value of Prohibition amenities. In later sections, we show that Prohibition also caused a productivity increase, which would amplify the effect on land prices.

6.2 Prohibition Led to Higher Farm Productivity and Investments

Next, we show that Prohibition was associated with higher labor productivity and investments. We construct a measure of output from the agricultural census, which provides consistent measures for five major crops, covering about 70 percent of total farm production. To get a real measure, we use average prices for these five crops in 1910, the first year in which the agricultural Census reports both quantities and total value.[39] We measure the farm output by multiplying the quantity of the crop by the price in 1910, and labor productivity as output per person.

Figure V shows that counties that adopted Prohibition had large increases in labor productivity, compared to the control counties, conditional on the control variables. The regression estimates from the baseline specification are reported in Table IV. Consistent with the event studies, Column (1) shows that labor productivity increased by approximately 12 percent. The effect is significant at the 1% level.

The increase in productivity is consistent with increased investment in labor saving technology. The early 20th century was a time of increased mechanization. Many of the biggest productivity gains occurred at the end of the 19th century, as farms transitioned from manpower to animal power, but new technologies such as improved plows, seed drills, and steam-powered threshers were still spreading to new farms during the time period we study (Rasmussen, 1962).[40] Importantly, the technological improvements of this time tended to be labor saving: "Such devices not only eased the burden of back-breaking labor but also reduced the number of workers and the period of employment for each task" (Atack et al., 2000).

Figure V shows that early adopters of Prohibition saw the value of implements per capita increase significantly in 1910 and 1920, with respect to late adopter. More precisely, Table IV estimates that Prohibition led to a 11.8% increase in investment in equipment in per capita terms. Instead, another input, the share of land devoted to farming did slightly increase, but it was proportionally smaller than the population increase. Consistent with increased credit demand, the number of banks in the areas increased by more than 0.039 per 1000 people.[41]

Despite the increase in farm productivity, the share of workers employed in agricul-

[39] By 1900, crop markets were increasingly national. Price gaps across space were significantly smaller than they had been during the 19th century because of the increasing ability to cheaply move crops around the country on railroads.(Harley, 1978; Atack, Bateman and Parker, 2000). Regions specialized in specific types of crops and exported them to the rest of the country.

[40] Our time period is around the invention, but just before the adoption of the tractor (Rasmussen, 1962; Manuelli and Seshadri, 2014).

[41] We are using the number of banks per capita as a rough measure of the lending in the county. See Rajan and Ramcharan (2015).

ture decreased (Table V column (1)). This is driven entirely by the share of farm laborers (column (2) and (3)). The result is consistent with the idea that farm investment is labor-substituting; the addition of equipment on farms allows farms to replace workers.

The net effect on the labor market is ambiguous given the increase in productivity, but it being due to labor-substituting capital. However, note that the decrease in the share of farm laborers is small compared to the population increase, so the total number of farm laborers would still be increasing. In addition, the share of employed workers falls by less, and is not statistically significant (column (4)). This would suggest the labor market boost of increased farm productivity spills over into other sectors, possibly including manufacturing (column (5)).[42]

6.3 The Land Price Channel

Why did Prohibition have such a large effect on productivity? Our preferred explanation is that the higher land values allowed increased borrowing and investment in capital. This investment raised labor productivity, and further increased the population inflows and land values. In the previous sections, we already presented some evidence towards this channel: the disproportionate increase in equipment is suggestive of lower capital costs.[43] Here, we show that the effects of Prohibition on land prices, population, and productivity were stronger in areas of the country where these channels would be more likely to be operative. In addition, we use these interactions to help us to distinguish whether the land price channel had any effect beyond any direct effects of Prohibition on productivity.

Our results are presented in Figure VI. To run these regressions, we use the same baseline specification, but estimate a separate coefficient for different areas. We also include a dummy control for being in that area.[44] We begin by investigating heterogeneous effects by access to lending, which we proxy by whether a county is above or below the state median of share of farms mortgaged in 1890, as reported on the Agricultural Census, and the

[42] In the heterogeneous effects section, we see that more people move into banking-intensive areas, suggestive that the labor-market effects are also an attractive feature, in addition to the direct effects of the amenity.

[43] The next section's results on sorting are also suggestive of this channel.

[44] A more demanding specification would be to split the sample based on whether or not the county is in that category. This would be equivalent to interacting the category with all of our controls. With more data, we would prefer such a specification, but there is a variance trade-off. Using such a specification changes the point-estimates slightly in most cases and the standard errors increase, sometimes by as much as a factor of two. Specifically, for our share-mortgaged and banks per capita regressions, for the effect on farm values and productivity, the point estimates reverse order: i.e. there is a higher estimated effect in places with fewer mortgages. Those estimates should be taken with a grain of salt.

number of banks per capita in 1890 from Jaremski and Fishback (2018).[45] The early 20th Century was a time when farmers were becoming increasingly reliant on borrowing. Often, mortgages were taken out in order to invest in the mechanization of the farm (Pope, 1914). The farm mortgage market was regionally segregated, leading to differences in mortgage rates, and presumably mortgage lending, that was independent of default risk (Snowden, 1987; Eichengreen, 1987).

The results appear to be stronger in places where there is more lending, and therefore an easier way for the land price channel to matter. For population, there seems to be little difference, though the point estimate is slightly higher in the more intensive lending counties. For land value, the effects are definitely stronger in areas with more mortgages, but there does not seem to be a major difference across the number of banks. The results on productivity are the most clear, in that for both, Prohibition causes a bigger increase in productivity in counties with more banks or mortgages. For counties with fewer banks and mortgages, the effect is not statistically significantly different from zero. Overall, we think this is suggestive that the land price channel helps explain why Prohibition had large productivity effects.

Next, we look for a different type of heterogeneity, based on how much we expect location demand to change. Specifically, counties on railroads were more connected, and so were able to attract migrants when local amenities improved. Similarly, places surrounded by wet counties were likely going to be destinations for people that wanted to move into dry counties, more so than places that are already surrounded by dry counties.[46] Importantly, we also expect that these might be correlated to the availability of alcohol within that county. If we thought Prohibition had a direct effect on productivity, we would expect to see that in places where alcohol is still available in neighboring counties or on a railroad, that the effects on productivity would be smaller. Therefore, this interaction should tell us more than just whether Prohibition had amenity value, it will also help us distinguish between two stories about why it might increase productivity.

The results do confirm our interpretation that Prohibition was a positive amenity. Both population and land prices went up more in both railroad-connected counties and counties with relatively more wet borders. They also indicate that the productivity improvements were likely due to the land price channel rather than any direct productivity effects of less drinking, although it is hard to draw strong conclusions from the heterogeneous

[45]These variables are surprisingly only weakly correlated (0.1), so we show both. The differences in mortgage availability are at least partly driven by differences in the local supply of mortgages (Snowden, 1987; Eichengreen, 1987).

[46]These two variables are actually slightly negatively correlated (-.07) and have no more than a .13 correlation with either lending variable we used above.

effects on railroads because of the large standard errors. For the dry border share, there is a much stronger effect in places with more wet borders, which is a natural implication of the land price story because land prices went up more, but the opposite of what a direct effect from less drinking story would suggest.

6.4 Prohibition Attracted Men, Immigrants, and Non-White People

In section 6.1, we showed that local Prohibition attracted people. Here, we investigate *which* people. There are two reasons to do this: first, because it is inherently interesting which groups of people benefit from amenity changes, and second, because it provides additional evidence on the land price channel.

Some historical context is helpful to understand the intended beneficiaries. Closing the saloon was made the chief goal of the movement not only because it would diminish temptation, but also because it would thwart the ability of immigrant groups to organize (Sismondo, 2011) and decrease access to alcohol of Southern blacks (see, among others, McGirr (2015)). According to *the Montgomery Advertiser* in 1929, "In Alabama, it [was] hard to tell where the Anti-Saloon League ends and the Klan begins" (as cited in Ball, 1996). In short, Prohibition was supported by whites, native-born, and women.

Yet given our previous results on economic effects, it is possible that Prohibition attracted individuals whose preferences might have been not directly aligned with the policy itself, but who responded to the potentially higher wages. In addition, these migrants would likely only be attracted by external increases in productivity, like our land price channel, rather than benefits from sobriety.[47] Given that minorities and immigrants were especially mobile, they are particularly likely to be responsive to the changes induced by Prohibition. For example, the timing of Prohibition slightly overlaps with the First Great Migration (Boustan, 2009; Derenoncourt, 2019), a time when African-Americans would be particularly mobile.

Our event study results are shown in Figure VII, while Table VI reports the estimates from the baseline specification (equation (1)). The share of men slightly increased in counties that passed Prohibition, but the effect is not statistically significant in the main specification. The share minority increased instead by 1.3 percentage points in early adopting counties after they became dry, with respect to late adopters. Similarly, the share of first-generation immigrants increased by 0.4 percentage points and the share of first- and second-generation immigrants increased by 0.06 percentage points, although the coeffi-

[47] One could argue that workers know they can benefit from increased sobriety, but lack self-control to impose it on themselves. However, this argument would rely even more heavily on the unavailability of alcohol and would make the results on neighboring dry counties and railroads even harder to explain.

cient is statistically significant only at the 10% level. Immigrants who–according to the historical narrative–were particularly opposed to Prohibition, i.e. Germans, Irish, and Italians, accounted for most of the gain.[48,49] The event studies for minorities and first-generation immigrants do have border-line significant pre-trends, which raises concerns about whether we identify the causal effect. Compared to our other results on farm values, population, and productivity, these estimates should be taken with some caution.[50]

While the magnitudes of these changes are small, the particularly interesting part is the signs. Prohibition's supporters tended to be female, white, and native, so it is unlikely that these results are due to the heterogeneous preferences for the amenity. Rather, these groups are also most likely to benefit from increased farm labor productivity. It supports the land price channel because increased productivity due to sobriety would have been available to them in any location, but increased productivity due to higher investment would be specific to a location.

The sorting of workers might also have amplified the effects on productivity, as the groups moving in may have had higher farm productivity. However, attracting these groups was probably not the initial cause of the increase productivity, as these groups valued the amenity less. So while we think this sorting may have amplified the channels discussed previously, we still think the evidence points to the land price channel as the initial cause.[51]

The magnitudes of our sorting results do not imply that women, natives, and whites were moving out of areas that imposed Prohibition. In fact, combining this with our results on population would imply that the number of people of all groups rose because of Prohibition. But we wish to stress the fact that these were disproportionately men, immigrants, and non-whites.

[48]Germans were especially represented in the brewing industry, and thus had economic interests against Prohibition (Okrent, 2010). As opposed to most Protestant denominations, Catholicism did not emphasize temperance and abstinence. More generally, the saloon played an important social role for immigrants who were new to the county, to the point that Okrent (2010) states that according to census reports around 80% of saloons were owned by foreign-borns.

[49]Event studies including 1880 for the sorting outcomes are reported in Appendix Figure V. Early adopters appear to be different than later adopters in 1880 along a number of demographic dimensions. This is driven by very small counties (counties with fewer than 250 people) experiencing large demographic shifts between 1880 and 1890. In fact, comparing 1890 and 1900, early and late adopters appear to be comparable. We therefore do not think that this threatens our identification.

[50]Given the magnitudes, we do not worry that this signals a threat to the identification of our other outcomes.

[51]We cannot rule out sorting of workers on unobservables, e.g. sobriety, as the initial cause, but the fact that the sorting on observables goes the opposite way is suggestive that it is unlikely.

7 Conclusion

In this paper, we investigated the equilibrium effects of a prominent amenity, the U.S. Prohibition. Our results are consistent with the interpretation of Prohibition as an amenity: land prices and population increased, and did so more in areas that were connected via railroad or that were surrounded by counties without the amenity. Prohibition increased productivity, and we present evidence that this is due to a land price channel. We also show evidence that sorting occurred in a counterintuitive way: the groups that most preferred Prohibition actually decreased as a share of the population.

The various causal relationships between sorting, productivity, land prices, and amenities has been the subject of much study (e.g. Glaeser, Kolko and Saiz, 2001; Florida, 2003). Here, we are able to document a new channel, that positive amenities cause higher land prices that cause higher productivity that cause a higher share of low-skilled workers. To our knowledge, this has not been documented previously. The existing literature has focused on the effects of amenities affecting sorting either directly or through land prices, and usually finds sorting toward high-skilled workers (Diamond, 2016). Our paper shows that in at least one setting, amenities encouraged sorting towards low-skilled groups. More generally, we have shown that it is important to pay attention to the land price channel on productivity when considering the sorting effects of amenities.

References

Aizer, Anna, Eli, Shari, Ferrie, Joseph and Lleras-Muney, Adriana. 2016. 'The Long-Run Impact of Cash Transfers to Poor Families', *American Economic Review* 106(4), 935–71.

Anderson, William H. 1910, *The Church in Action Against the Saloon*, American Issue Publishing Company.

Andrews, Mike. 2019, Bar Talk: Informal Social Interactions, Alcohol Prohibition, and Invention. Working Paper.

Atack, Jeremy, Bateman, Fred and Parker, William N. 2000. 'The Farm, the Farmer, and the Market', *The Cambridge Economic History of the United States* 2, 245–84.

Autor, David, Palmer, Christopher J and Pathak, Parag A. 2017, Gentrification and the Amenity Value of Crime Reductions: Evidence from Rent Deregulation. NBER Working Paper # 23914.

Bahaj, Saleem A, Foulis, Angus and Pinter, Gabor. 2020. 'Home Values and Firm Behaviour', 110(7), 2225–2270.

Ball, Howard. 1996, *Hugo L. Black: Cold Steel Warrior*, Oxford University Press.

Boustan, Leah. 2009. 'Competition in the Promised Land: Black Migration and Racial Wage Convergence in the North, 1940-1970', *The Journal of Economic History* 69(3), 755–782.

Carlson, Mark A, Correia, Sergio and Luck, Stephan. 2020. 'The Effects of Banking Competition on Growth and Financial Stability: Evidence from the National Banking Era', *SSRN # 3202489* .

Caulkins, Jonathan P, Kilmer, Beau and Kleiman, Mark AR. 2016, *Marijuana Legalization: What Everyone Needs to Know*, Oxford University Press.

Chaney, Thomas, Sraer, David and Thesmar, David. 2012. 'The Collateral Channel: How Real Estate Shocks Affect Corporate Investment', *American Economic Review* 102(6), 2381–2409.

Cheng, Cheng, Mayer, Walter J and Mayer, Yanling. 201. 'The Effect of Legalizing Retail Marijuana on Housing Values: Evidence from Colorado', *Economic Inquiry* 56(3), 1585–1601.

Conley, Timothy G. 1999. 'GMM Estimation with Cross Sectional Dependence', *Journal of Econometrics* 92(1), 1–45.

Derenoncourt, Ellora. 2019, Can You Move to Opportunity? Evidence from the Great Migration. Working Paper.

Diamond, Rebecca. 2016. 'The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980-2000', *American Economic Review* 106(3), 479–524.

Dills, Angela K and Miron, Jeffrey A. 2004. 'Alcohol Prohibition and Cirrhosis', *American Law and Economics Review* 6(2), 285–318.

Eichengreen, Barry. 1987. 'Agricultural Mortgages in the Populist Era: Reply to Snowden', *The Journal of Economic History* 47(3), 757–760.

Evans, Mary F, Helland, Eric, Klick, Jonathan and Patel, Ashwin. 2016. 'The Developmental Effect of State Alcohol Prohibitions at the Turn of the Twentieth Century', *Economic Inquiry* 54(2), 762–777.

Fishback, Price V and Kantor, Shawn Everett. 2007, *A Prelude to the Welfare State: The Origins of Workers' Compensation*, University of Chicago Press.

Florida, Richard. 2003. 'Cities and the Creative Class', *City & Community* 2(1), 3–19.

Fulford, Scott L. 2015. 'How Important are Banks for Development? National Banks in the United States, 1870–1900', *Review of Economics and Statistics* 97(5), 921–938.

García-Jimeno, Camilo. 2016. 'The Political Economy of Moral Conflict: An Empirical Study of Learning and Law Enforcement under Prohibition', *Econometrica* 84(2), 511–570.

García-Jimeno, Camilo, Iglesias, Angel and Yildirim, Pinar. 2018, Women, Rails and Telegraphs: An Empirical Study of Information Diffusion and Collective Action. NBER Working Paper # 24495.

Glaeser, Edward L, Kolko, Jed and Saiz, Albert. 2001. 'Consumer City', *Journal of Economic Geography* 1(1), 27–50.

Guerrieri, Veronica, Hartley, Daniel and Hurst, Erik. 2013. 'Endogenous Gentrification and Housing Price Dynamics', *Journal of Public Economics* 100, 45–60.

Hamm, Richard F. 1995, *Shaping the Eighteenth Amendment: Temperance Reform, Legal Culture, and the Polity, 1880-1920*, University of North Carolina Press.

Harley, C Knick. 1978. 'Western Settlement and the Price of Wheat, 1872–1913', *The Journal of Economic History* 38(4), 865–878.

Heaton, Paul. 2012. 'Sunday Liquor Laws and Crime', *Journal of Public Economics* 96(1), 42–52.

Hernández, Carlos Eduardo. 2016, Adaptation and Survival in the Brewing Industry during Prohibition. Working Paper.

Hornbeck, Richard. 2010. 'Barbed Wire: Property Rights and Agricultural Development', *The Quarterly Journal of Economics* 125(2), 767–810.

Jacks, David S, Pendakur, Krishna and Shigeoka, Hitoshi. 2017 , Infant Mortality and the Repeal of Federal Prohibition, NBER Working Paper # 23372.

Jaremski, Matthew and Fishback, Price V. 2018. 'Did Inequality in Farm Sizes Lead to Suppression of Banking and Credit in the Late Nineteenth Century?', *The Journal of Economic History* 78(1), 155–195.

Jaremski, Matthew S and Wheelock, David C. 2018, Banking on the Boom, Tripped by the Bust: Banks and the World War I Agricultural Price Shock. NBER Working Paper # 25159.

Kerr, Kathel Austin. 1985, *Organized for Prohibition: A New History of the Anti-Saloon League*, Yale University Press.

Law, Marc T and Marks, Mindy. 2018, Did Early Twentieth Century Alcohol Prohibition Matter? SSRN # 3276349.

Lewis, Michael. 2008. 'Access to Saloons, Wet Voter Turnout, and Statewide Prohibition Referenda, 1907–1919', *Social Science History* 32(3), 373–404.

Manson, Steven, Schroeder, Jonathan, Van Riper, David and Ruggles, Steven. 2018, 'IPUMS National Historical Geographic Information System: Version 13.0 [Database]'.

Manuelli, Rodolfo E and Seshadri, Ananth. 2014. 'Frictionless Technology Diffusion: The Case of Tractors', *American Economic Review* 104(4), 1368–91.

McGirr, Lisa. 2015, *The War on Alcohol: Prohibition and the Rise of the American State*, WW Norton & Company.

Miron, Jeffrey A and Zwiebel, Jeffrey. 1991. 'Alcohol Consumption During Prohibition', *American Economic Review* 81(2), 242.

Molloy, Raven, Smith, Christopher L and Wozniak, Abigail. 2011. 'Internal Migration in the United States', *Journal of Economic Perspectives* 25(3), 173–96.

Okrent, Daniel. 2010, *Last Call: The Rise and Fall of Prohibition*, Simon and Schuster.

Owens, Emily G. 2011. 'Are Underground Markets Really More Violent? Evidence from Early 20th Century America', *American Law and Economics Review* 13(1), 1–44.

Owens, Emily G. 2014. 'The American Temperance Movement and Market-Based Violence', *American Law and Economics Review* 16(2), 433–472.

Pope, Jesse E. 1914. 'Agricultural Credit in the United States', *The Quarterly Journal of Economics* 28(4), 701–746.

Rajan, Raghuram and Ramcharan, Rodney. 2015. 'The Anatomy of a Credit Crisis: The Boom and Bust in Farm Land Prices in the United States in the 1920s', *American Economic Review* 105(4), 1439–77.

Rasmussen, Wayne D. 1962. 'The Impact of Technological Change on American Agriculture, 1862–1962', *The Journal of Economic History* 22(4), 578–591.

Roback, Jennifer. 1982. 'Wages, Rents, and the Quality of Life', *Journal of Political Economy* 90(6), 1257–1278.

Rosen, Sherwin. 1979. 'Wage-Based Indexes of Urban Quality of Life', *Current Issues in Urban Economics* pp. 74–104.

Schilbach, Frank. 2019. 'Alcohol and Self-Control: A Field Experiment in India', *American Economic Review* 109(4), 1290–1322.

Sechrist, Robert P. 2012. 'Prohibition Movement in the United States, 1801-1920. ICPSR08343-v2', *Ann Arbor, MI: Inter-university Consortium for Political and Social Research (distributor)* pp. 10–26.

Sismondo, Christine. 2011, *America Walks into a Bar: A Spirited History of Taverns and Saloons, Speakeasies and Grog Shops*, Oxford University Press.

Skocpol, Theda. 1995, *Protecting Soldiers and Mothers: The Political Origins of Social Policy in United States*, Harvard University Press.

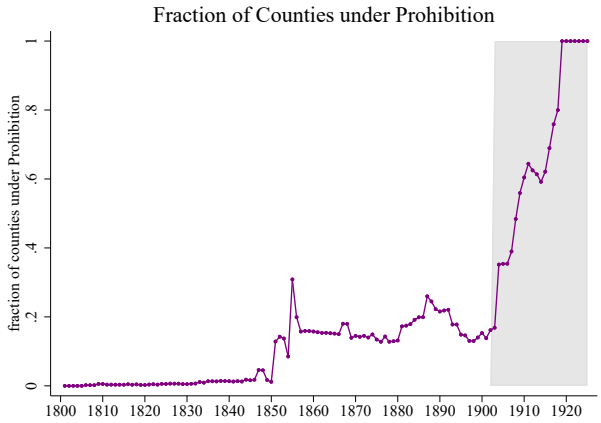
Snowden, Kenneth A. 1987. 'Mortgage Rates and American Capital Market Development in the Late Nineteenth Century', *The Journal of Economic History* 47(3), 671–691.

Strumpf, Koleman S and Oberholzer-Gee, Felix. 2002. 'Endogenous Policy Decentralization: Testing the Central Tenet of Economic Federalism', *Journal of Political Economy* 110(1), 1–36.

Su, Yichen. 2020, The Rising Value of Time and the Origin of Urban Gentrification. SSRN # 3216013.

Figures

Figure I: Share of Dry Counties 1800-1925



Notes: The graph shows the share of counties under a dry law for each year 1800-1925. The three shaded areas refer to the three Waves of Prohibition.

Figure II: Migration (Blue) and Production (Magenta) Curves, with an Amenity Change (Dashed)

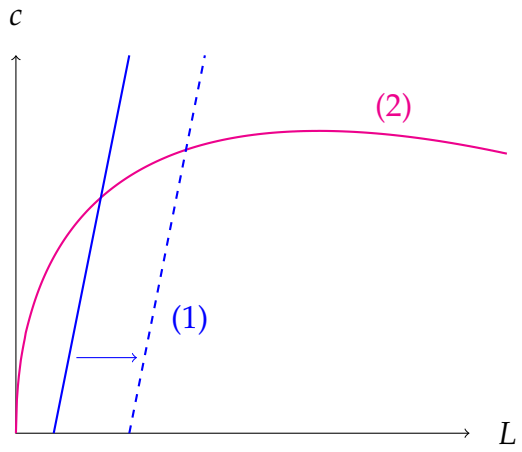
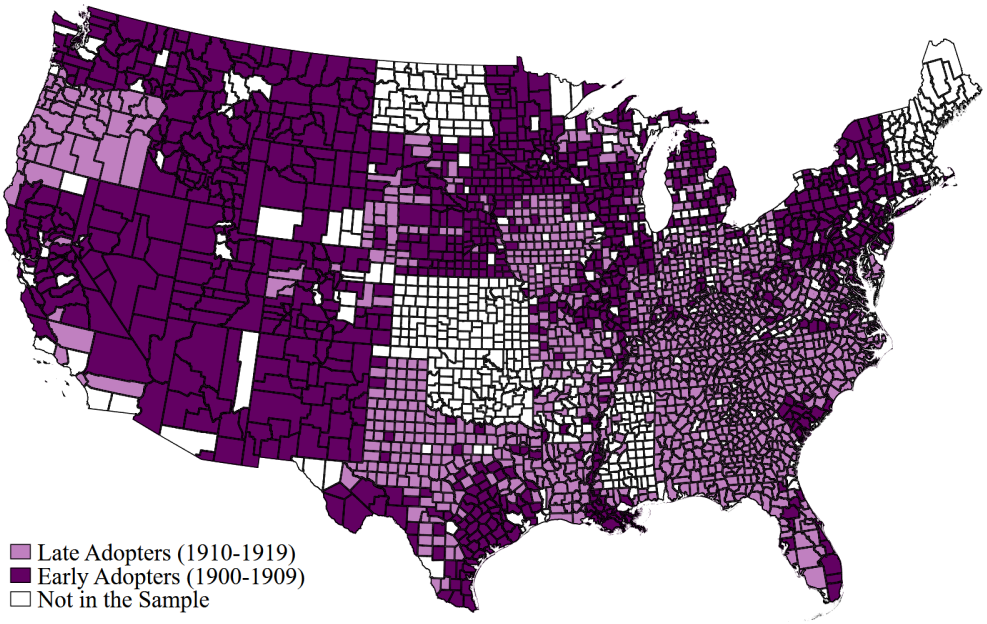
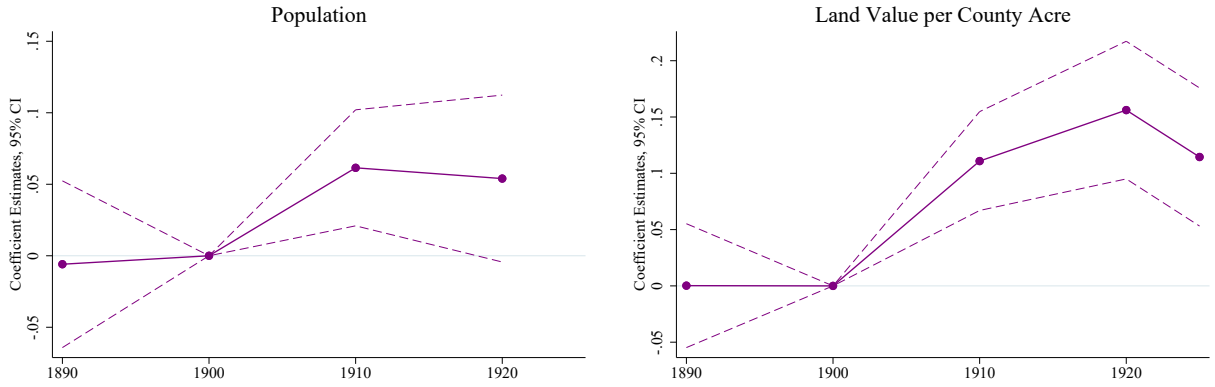


Figure III: Treatment Status Map



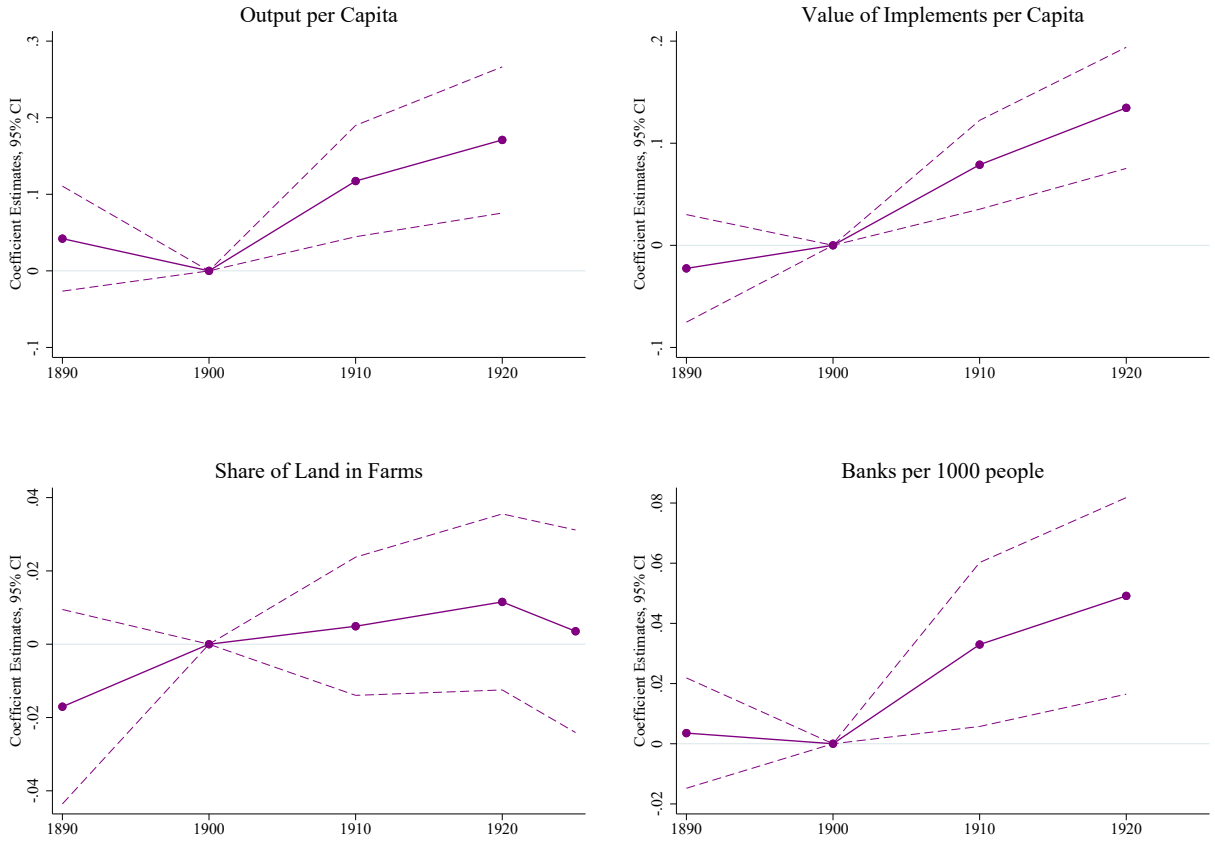
Notes: The map shows the treatment status of counties across the United States. Early adopters (treatment group) are counties that introduced Prohibition from 1900 to 1909; late adopters (control group) are counties that introduced Prohibition from 1910 to 1919. We exclude from the sample counties for which there no information about year of introduction of Prohibition (54 counties), counties which adopted before 1900 (412 counties), and urban counties (219 counties). The final sample includes 2,381 counties. Prohibition adoption data is from Sechrist (2012).

Figure IV: Prohibition Counties have Higher Population and Land Values



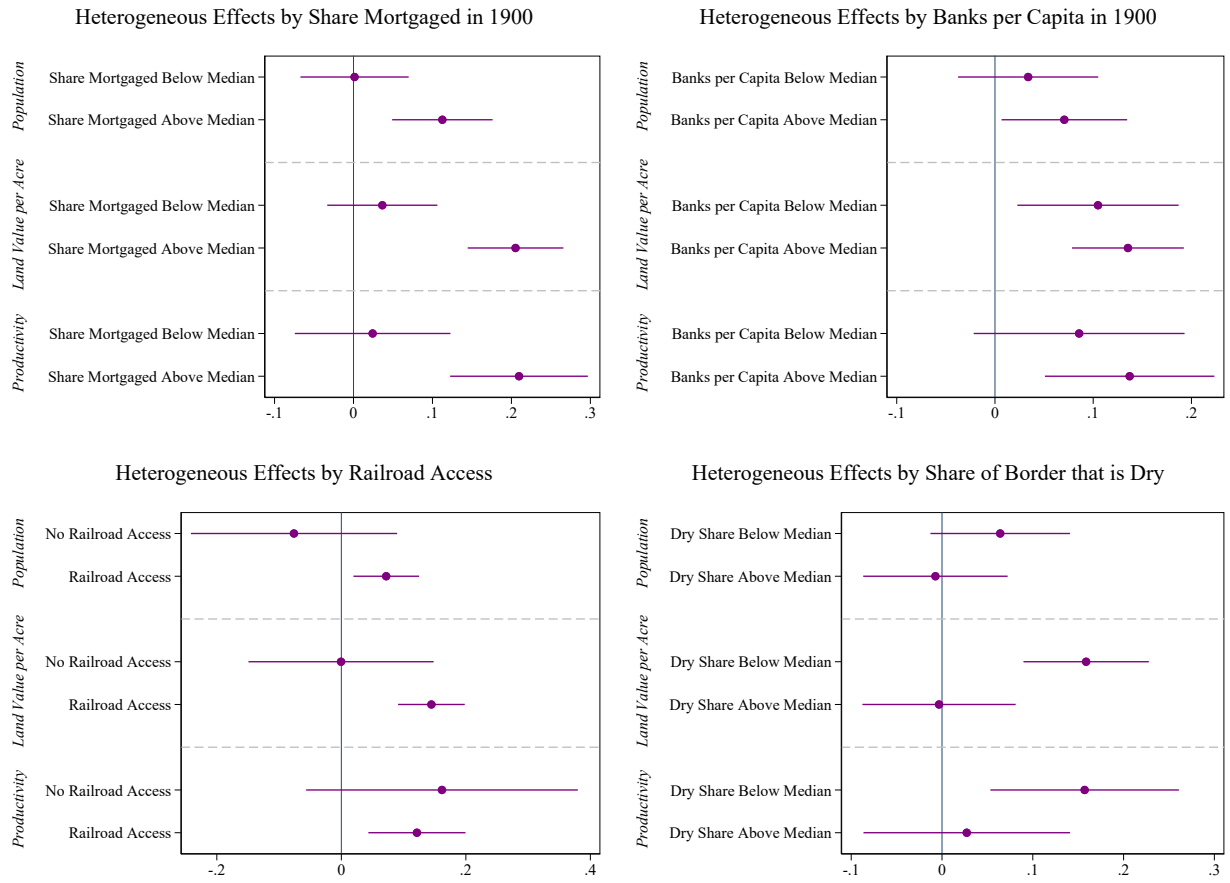
Notes: The graphs show the effect of Prohibition on population and land values per county acre by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level.

Figure V: Prohibition Counties have Higher Productivity



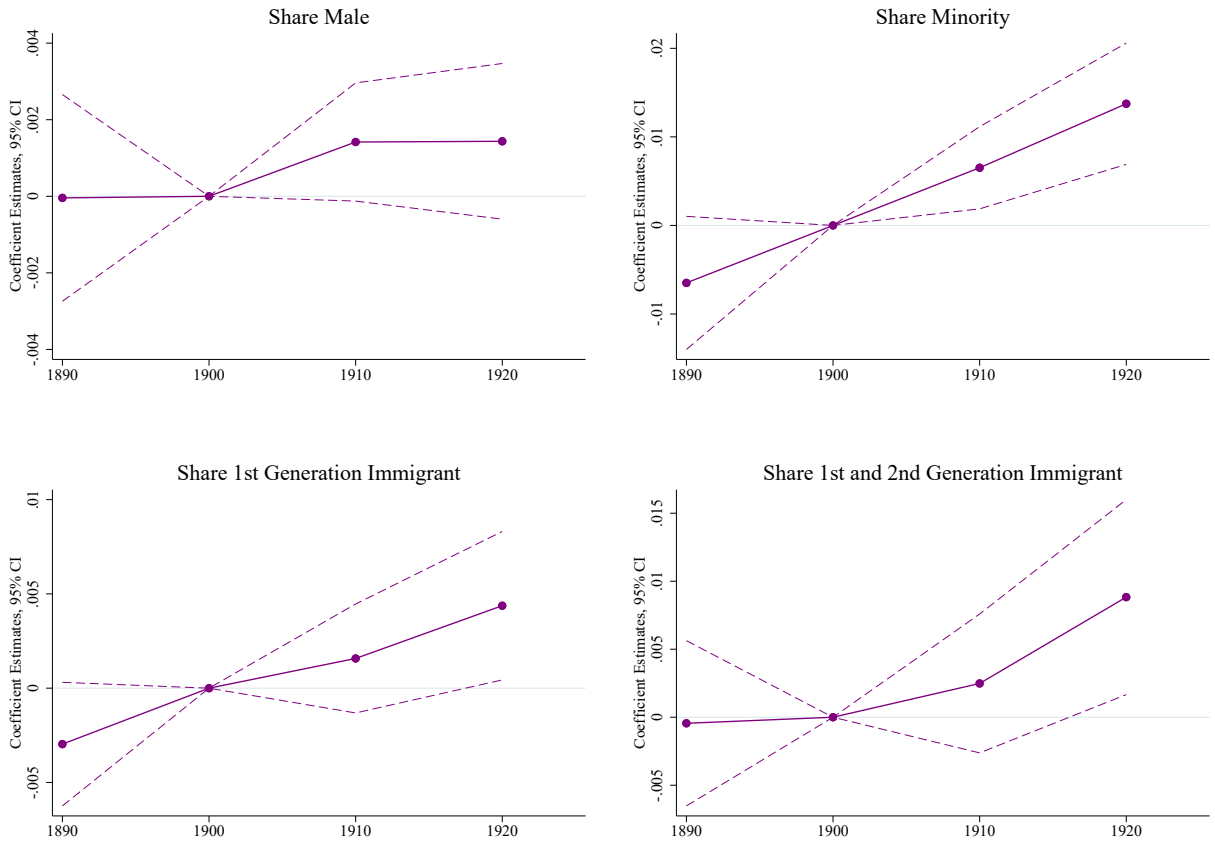
Notes: The graphs show the effect of Prohibition on productivity, investments, and banks per capita by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). Productivity is defined as log output for the five major crops times 1910 prices per capita. Implements per person are also defined in log. Productivity, implements per capital and share of land in farms are from the Census of Agriculture 1890-1925. Banks per capita are from Jaremski and Fishback (2019). We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level.

Figure VI: Heterogeneous Effects by Mortgage Share in 1890, Banks per Capita, Railroad Access, and Share of Border that is also Dry



Notes: the graphs show heterogeneous effects by mortgage share in 1890, number of banks per capita, railroad access, and share of border that is also dry for population, land value per county acre, and productivity. The coefficients are estimated in an interacted specifications. We regress the outcome on an indicator variable for being an early adopter interacted with an indicator variable dummy for the post period and with dummies for the group of interest, dummies for the group of interest interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects. Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1900 and interacted with year dummies. Population regressions do not control for baseline population. The points are the point estimates β with 95% confidence intervals. Standard errors are clustered at the county level.

Figure VII: Effect of Prohibition on Sorting



Notes: The graphs show the effect of Prohibition on sorting by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). All outcomes are from the Population Census 1890-1920. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. In regressions including one of the outcomes, we omit the respective control. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level.

Tables

Table I: Correlates of Early Adoption

Dependent Variable	Probability Dry		
	(1)	(2)	(3)
Population	-0.179***	-0.164***	-0.168***
	(0.028)	(0.031)	(0.041)
Share Urban	-0.967***	-0.431**	-0.402**
	(0.191)	(0.183)	(0.187)
Share Male	-7.150***	-5.049***	-4.439***
	(0.912)	(0.944)	(1.042)
Share White	0.321***	0.898***	0.920***
	(0.107)	(0.136)	(0.149)
Share 1st and 2nd Generation Immigrant	-2.760***	-5.255***	-5.337***
	(0.385)	(0.465)	(0.471)
Share 1st Generation Immigrant	4.435***	7.907***	8.299***
	(1.002)	(0.970)	(1.000)
Share in Favor	1.285***	0.703***	0.688***
	(0.093)	(0.106)	(0.116)
Farm Value			-0.036
			(0.043)
Implements per Capita			-0.078
			(0.054)
Share of Land in Farms			0.253
			(0.174)
Productivity			0.058**
			(0.027)
Railroad			-0.074
			(0.058)
Banks per capita			-0.027
			(0.144)
Observations	2279	2279	2266
State FE	yes	yes	yes

Notes: The table shows the coefficients of a Cox hazard model for when counties become dry. The explanatory variables are county characteristics in 1900. Standard errors are robust. *** p<0.01, ** p<0.05, * p<0.1.

Table II: Effect of Prohibition on Population and Land Values

	(1)	(2)	(3)	(4)
Panel A: Effects on Population				
Early Adopter * After 1910	0.151***	0.108***	0.059*	0.061**
	(0.037)	(0.030)	(0.033)	(0.029)
	[0.043]	[0.031]	[0.024]	[0.025]
N	9488	9260	9488	9260
Clusters	2372	2315	2372	2315
Panel B: Effects on Land Values per Country Acre				
Early Adopter * After 1910	0.226***	0.231***	0.121***	0.127***
	(0.036)	(0.033)	(0.028)	(0.027)
	[0.040]	[0.034]	[0.026]	[0.026]
N	11635	11505	11635	11505
Clusters	2327	2301	2327	2301
County FE	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes
Controls for Baseline Religiosity		yes		yes
Controls for Baseline Demographics			yes	yes

Notes: The table shows the effect of Prohibition on population and land values per county acre. Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table III: Robustness Checks

	(1)	(2)	(3)	(4)
Robustness Check	Windsor. Outcome	1905 - 1915 Adopters	Includes Unbalanced Counties	1900 County Boundaries
Panel A: Effects on Population				
Early Adopter * After 1910	0.062**	0.054*	0.061**	0.058**
	(0.028)	(0.033)	(0.029)	(0.024)
	[0.025]	[0.022]	[0.025]	[0.023]
N	9260	3752	9260	8684
Clusters	2315	938	2315	2171
Panel B: Effects on Land Values per Country Acre				
Early Adopter * After 1910	0.120***	0.103***	0.125***	0.152***
	(0.027)	(0.035)	(0.027)	(0.027)
	[0.026]	[0.028]	[0.026]	[0.027]
N	11505	4665	11553	10810
Clusters	2301	933	2314	2162
County FE	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes
Controls for Baseline Religiosity	yes	yes	yes	yes
Controls for Baseline Demographics	yes	yes	yes	yes

Notes: The table shows robustness of the effect of Prohibition on population and land values per county acre. Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Column (1) shows that the main results are robust to winsorizing the outcomes at the 95% level. Column (2) restricts the sample to counties that adopted Prohibition between 1905 and 1915. Column (3) includes counties for which we have missing values in certain years. Finally, Column (4) uses 1900 county boundaries. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table IV: Effect of Prohibition on Productivity and Investments

	(1)	(2)	(3)	(4)
Dependent Variable	Productivity	Implements per Capita	Share of Land in Farms	Banks per Capita
Early Adopter * After 1910	0.123***	0.118***	0.015	0.039***
	(0.040)	(0.025)	(0.010)	(0.013)
	[0.037]	[0.027]	[0.008]	[0.010]
N	9116	9208	5305	9236
Clusters	2279	2302	1061	2309
Outcome Mean	3.482	2.749	0.646	0.272
County FE	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes
Controls for Baseline Religiosity	yes	yes	yes	yes
Controls for Baseline Demographics	yes	yes	yes	yes

Notes: The table shows the effect of Prohibition on productivity, investments, and banks per capita. Productivity is defined as log output for the five major crops times 1910 prices per capita. Implements per person are also defined in log. Productivity, implements per capital and share of land in farms are from the Census of Agriculture 1890-1925. Banks data are from Jaremski and Fishback (2018). We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table V: Effect of Prohibition on Employment Shares

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Share of Males 15-60 in Agriculture	Share of Males 15-60 who are Farmers	Share of Males 15-60 who are Farm Laborers	Share of Males 15-60 who are Employed	Share of Males 15-60 in Mfg
Early Adopter * After 1910	-0.010*	-0.000	-0.010***	-0.004	0.004
	(0.006)	(0.004)	(0.004)	(0.004)	(0.003)
	[0.005]	[0.004]	[0.004]	[0.004]	[0.002]
N	6930	6930	6930	6930	6930
Clusters	2310	2310	2310	2310	2310
Outcome Mean	0.452	0.317	0.133	0.831	0.057
County FE	yes	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes	yes
Controls for Baseline Religiosity	yes	yes	yes	yes	yes
Controls for Baseline Demographics	yes	yes	yes	yes	yes

Notes: The table shows the effect of Prohibition on employment shares. All outcomes are calculated from a 25% sample of the Population Census 1890-1920. Banks data are from Jaremski and Fishback (2018). We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

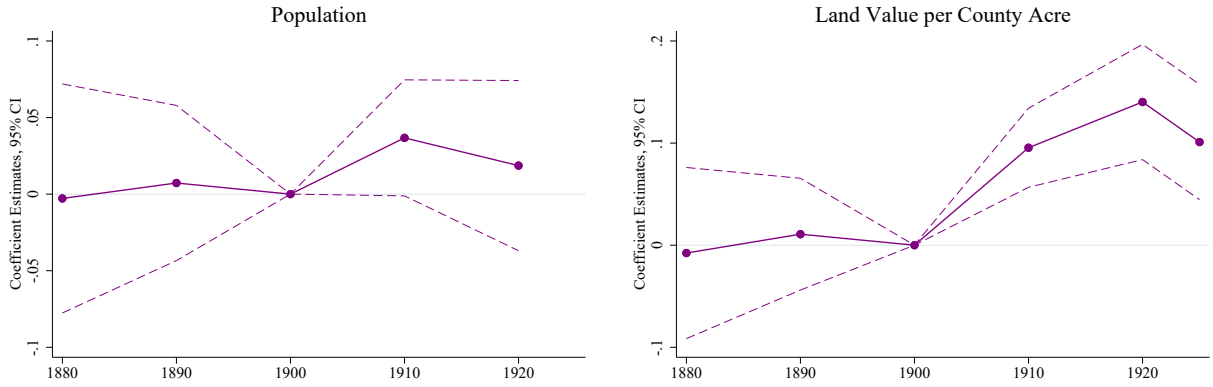
Table VI: Effect of Prohibition on Sorting

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Share Male	Share Minority	Share 1st Gen Imm	Share 1st and 2nd Gen Imm	Share German, Irish, Italian Imm
Early Adopter * After 1910	0.001	0.013***	0.004**	0.006*	0.008***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)
	[0.001]	[0.004]	[0.002]	[0.003]	[0.003]
N	9260	9260	9260	9260	6930
Clusters	2315	2315	2315	2315	2310
Outcome Mean	0.526	0.143	0.079	0.217	0.089
County FE	yes	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes	yes
Controls for Baseline Religiosity	yes	yes	yes	yes	yes
Controls for Baseline Demographics	yes	yes	yes	yes	yes

Notes: The table shows the effect of Prohibition on sorting. All outcomes are from the Population Census 1890-1920, with the exception of share German, Irish and Italian immigrants that is calculated from a 25% sample of the Population Census 1880-1920. We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Column (1) excludes share male, Column (2) excludes share white, and Columns (3)-(5) exclude share 1st and 2nd generation immigrant and share 1st generation immigrant from the baseline controls. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

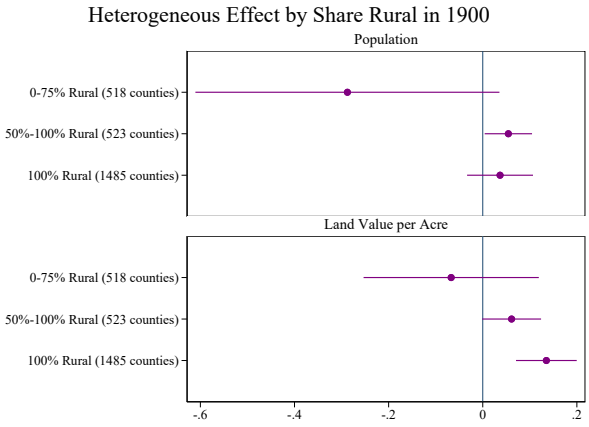
Appendix

Appendix Figure I: Prohibition Counties have Higher Population and Land Values



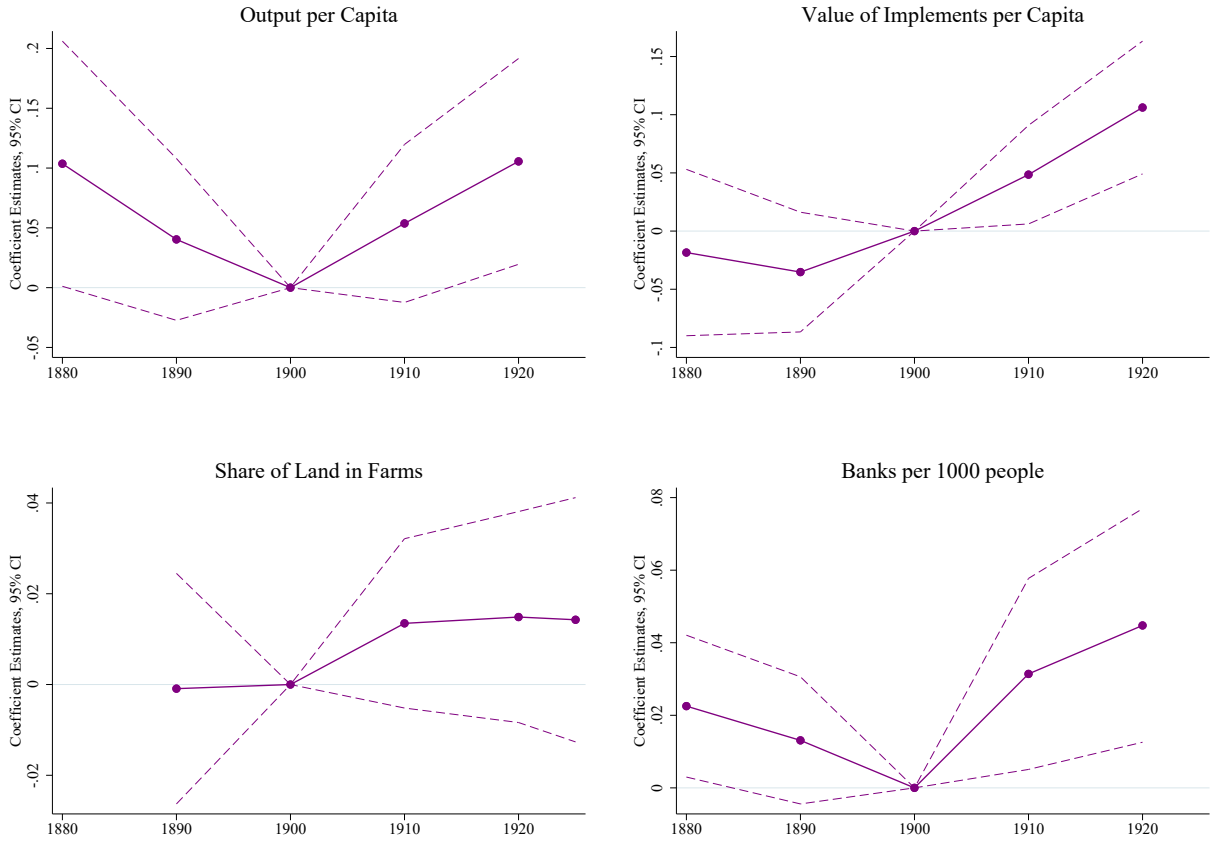
Notes: The graphs show the effect of Prohibition on population and land values per county acre by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). Both outcomes are defined in logs. Population is from the Population Census 1880-1920, and land values are from the Census of Agriculture 1880-1925. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, and share male, all measured in 1880. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level. 1880 data features substantial county boundary changes leading to some large changes in population and demographics from 1880 to 1890, so while we show this for robustness, our preferred specifications focus on the 1890-1925 time period.

Appendix Figure II: Heterogeneous Effects by Share of County that is Rural



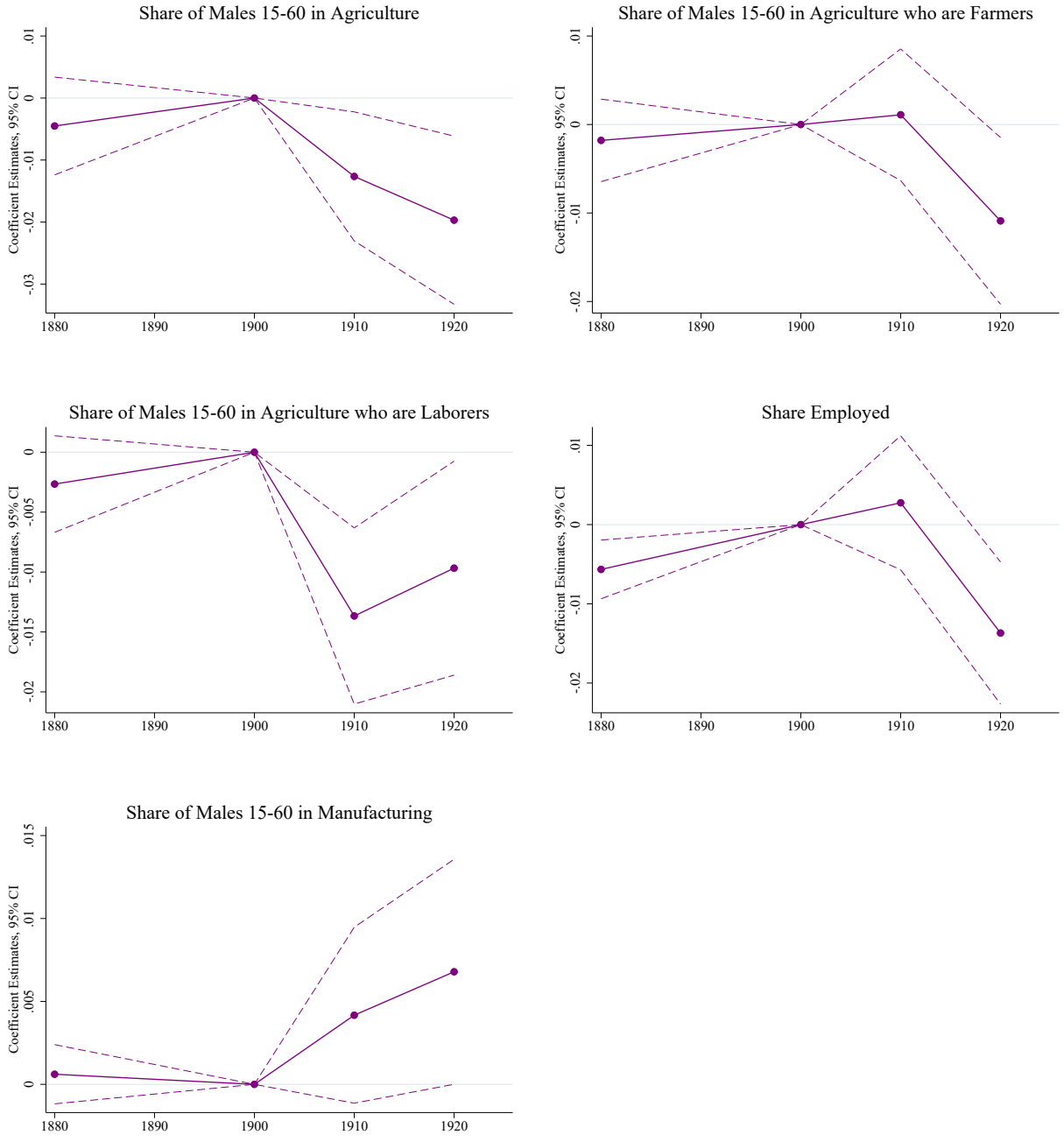
Notes: The graph shows heterogeneous effects of Prohibition on population and land values per county acre by share of county that is rural. The coefficients are estimated in separate regressions. Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. All outcomes are from the Population Census 1890-1920. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level.

Appendix Figure III: Prohibition Counties have Higher Productivity



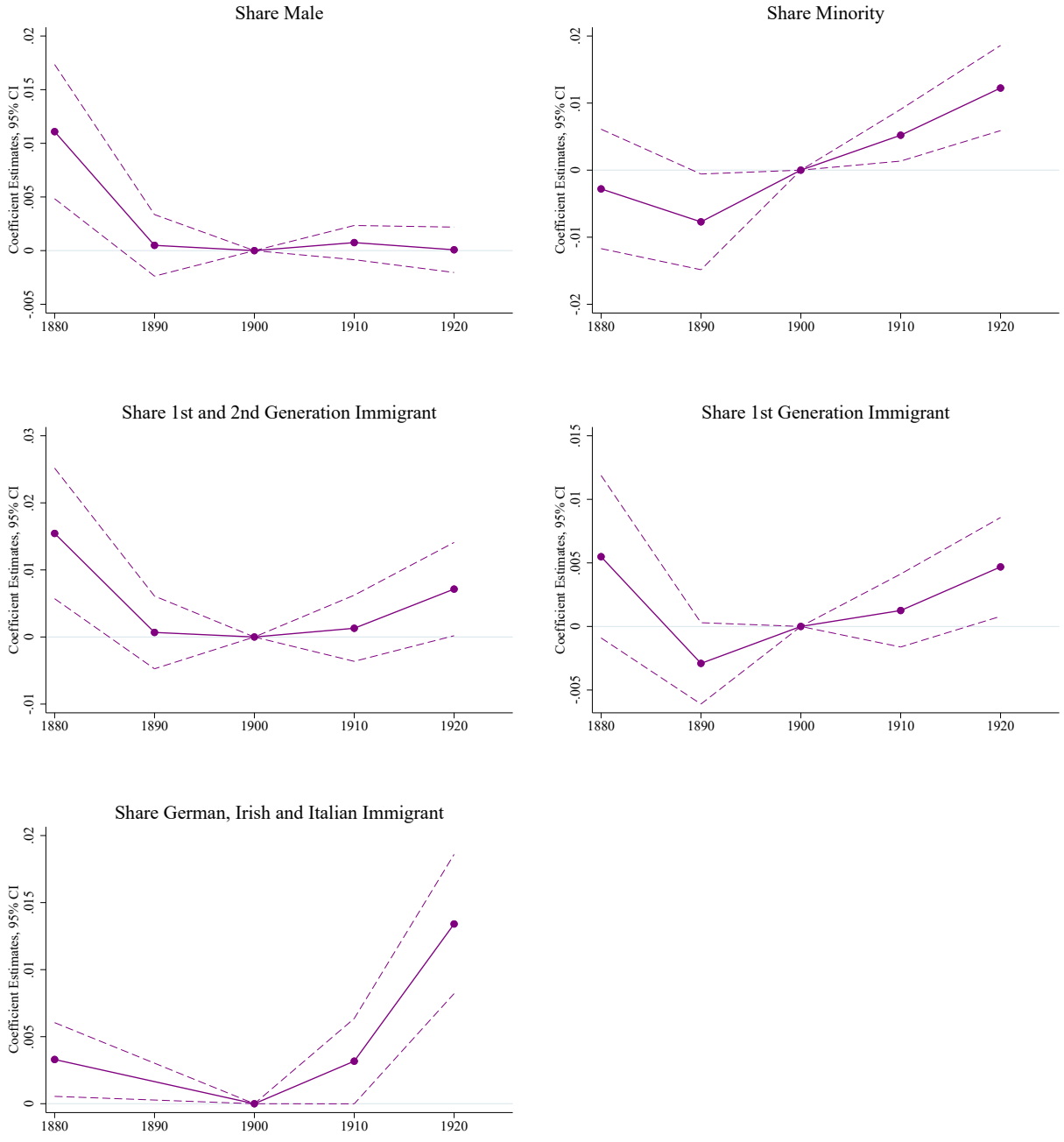
Notes: The graphs show the effect of Prohibition on productivity, investments, and banks per capita by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). Productivity is defined as log output for the five major crops times 1910 prices per capita. Implements per person are also defined in log. Productivity, implements per capital and share of land in farms are from the Census of Agriculture 1880-1925. Banks per capita are from Jaremski and Fishback (2019). We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, and share male, all measured in 1880. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level. 1880 data features substantial county boundary changes leading to some large changes in population and demographics from 1880 to 1890, so while we show this for robustness, our preferred specifications focus on the 1890-1925 time period.

Appendix Figure IV: Effect of Prohibition on Employment Shares



Notes: The graphs show the effect of Prohibition on employment shares by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). All outcomes are calculated from a 25% sample of the Population Census 1880-1920. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, and share male, all measured in 1880. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level. 1880 data features substantial county boundary changes leading to some large changes in population and demographics from 1880 to 1890, so while we show this for robustness, our preferred specifications focus on the 1890-1925 time period.

Appendix Figure V: Effect of Prohibition on Sorting



Notes: The graphs show the effect of Prohibition on sorting by year. Each graph shows the coefficients β with 95% confidence intervals from event study specification (equation (2)). All outcomes are from the Population Census 1880-1920, with the exception of share German, Irish and Italian immigrants that is calculated from a 25% sample of the Population Census 1880-1920. We regress the outcome on an indicator variable for being an early adopted interacted with year dummies, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (2)). Baseline controls are population, share urban, share white, and share male, all measured in 1880. In regressions including one of the outcomes, we omit the respective control. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors are clustered at the county level. 1880 data features substantial county boundary changes leading to some large changes in population and demographics from 1880 to 1890, so while we show this for robustness, our preferred specifications focus on the 1890-1925 time period.

Appendix Table I: Effect of Prohibition on Population and Land Values, Adding One control at the Time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Additionally Controlling For	No Additional Controls	Population	Share Urban	Share Male	Share White	Share 1st Gen Imm	Share 1st and 2nd Gen Imm
			Panel A: Effects on Population				
Early Adopter * After 1910	0.108***	-	0.110***	0.090***	0.069**	0.056*	0.061**
	(0.030)	-	(0.030)	(0.029)	(0.029)	(0.030)	(0.029)
	[0.031]	-	[0.031]	[0.026]	[0.025]	[0.024]	[0.025]
N	9260	-	9260	9260	9260	9260	9260
Clusters	2315	-	2315	2315	2315	2315	2315
			Panel B: Effects on Land Values per Country Acre				
Early Adopter * After 1910	0.231***	0.129***	0.135***	0.137***	0.138***	0.142***	0.127***
	(0.033)	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)	(0.027)
	[0.034]	[0.027]	[0.026]	[0.026]	[0.026]	[0.026]	[0.026]
N	11505	11505	11505	11505	11505	11505	11505
Clusters	2301	2301	2301	2301	2301	2301	2301
County FE	yes	yes	yes	yes	yes	yes	yes
State-Year FE	yes	yes	yes	yes	yes	yes	yes
Controls for Baseline Religiosity	yes	yes	yes	yes	yes	yes	yes

Notes: The table shows the effect of Prohibition on population and land values per county acre, adding one set of baseline controls at the time. Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). The baseline controls we include incrementally are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Appendix Table II: Effect of Prohibition on Population and Land Values, Spillovers

	(1)
Panel A: Effects on Population	
Early Adopter * After 1910	0.061**
	(0.029)
	[0.025]
Neighbor Early Adopter * After 1910	-0.043
	(0.130)
	[0.086]
N	9260
Clusters	2315
Panel B: Effects on Land Values per Country Acre	
Early Adopter * After 1910	0.127***
	(0.027)
	[0.026]
Neighbor Early Adopter * After 1910	0.023
	(0.133)
	[0.165]
N	11505
Clusters	2301
County FE	yes
State-Year FE	yes
Controls for Baseline Religiosity	yes
Controls for Baseline Demographics	yes

Notes: The table shows the effect of Prohibition on population and land values per county acre on treated and neighboring counties. Both outcomes are defined in logs. Population is from the Population Census 1890-1920, and land values are from the Census of Agriculture 1890-1925. We regress the outcome on an indicator variable for being an early adopted interacted with an indicator variable for the post period, an indicator variable for being the neighbor of an early adopted interacted with an indicator variable for the post period, deciles of the share of population belonging to religions in favor of Prohibition in 1890 interacted with year dummies, baseline controls interacted with year dummies, county fixed effects and state-year fixed effects (equation (1)). Baseline controls are population, share urban, share white, share male, share 1st and 2nd generation immigrant and share 1st generation immigrant, all measured in 1890. Population regressions do not control for baseline population. The sample excludes urban counties and counties that adopted Prohibition before 1899. All regressions are estimated by OLS. Standard errors clustered at the county level are in parentheses and Conley standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1.