Bar Talk: Informal Social Interactions, Alcohol Prohibition, and Invention*

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Abstract

While economists have long recognized the importance of geographic proximity and in-person social interactions for the production of innovation, little is known about what kinds of social interactions matter or how social interactions evolve over time. I evaluate the importance of informal social interactions for invention by exploiting a massive and involuntary disruption of informal social networks from U.S. history: alcohol prohibition. The enactment of state-level prohibition laws differentially treated counties depending on whether those counties were wet or dry prior to prohibition. After the imposition of state-level prohibition, previously wet counties had 8-18% fewer patents per year relative to consistently dry counties. The effect was smaller for groups that were less likely to frequent saloons, namely women and particular ethnic groups. Because prohibition laws were long-lasting, I further study how informal social networks evolve in response to the shock. The decline in patenting was largest in the first three years after the imposition of prohibition and rebounds thereafter. As individuals rebuilt their informal social networks following prohibition, they connected with new individuals and patented in new technology classes. Thus, while prohibition had only a temporary effect on the rate of invention, it had a lasting effect on the direction of inventive activity.

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1 Introduction

Economists and other social scientists have long recognized that most new ideas occur when individuals are geographically co-located (Ellison & Glaeser, 1997; Ellison, Glaeser, & Kerr, 2010; Glaeser, Kallal, Scheinkman, & Schleifer, 1992; Jacobs, 1969; Marshall, 1890), where social interactions facilitate the recombination of ideas (Lucas, 2009; Lucas & Moll, 2014; Weitzman, 1998). Much less is known about what kinds of social interactions matter for the generation of new ideas or how social interactions evolve over time. These issues are difficult to study because individuals choose with whom they interact, and hence social interactions are inherently endogenous. The few recent studies that exploit exogenous variation in social interactions (Boudreau et al., 2017; Campos, Leon, & McQuillin, 2018; Catalini, 2018; Hasan & Koning, 2019; Pennington, 2020) only study social interactions that take place among colleagues in the workplace or similar professional settings. Recent work has presented suggestive evidence that exposure to ideas in informal settings outside of the workplace plays a similarly important role in determining the direction of innovative activity (Bell, Chetty, Jaravel, Petkova, & Reenen, 2019; Hvide & Oyer, 2018), but these studies are unable to assess the causal effects of informal social interactions. In this paper, I exploit a large and long-lasting shock to informal interactions to evaluate the importance of informal social interactions for the generation of new ideas. I further show how individuals' informal social networks evolve over a several-year period in response to this shock, and the consequences of the evolving network structure for the rate and direction of innovation.

More specifically, I investigate a massive disruption of informal social networks from U.S. history: alcohol prohibition. Scholars have noted the role of bars in bringing creative people together in recent decades (Florida, 2002b; Oldenburg, 1989), and examples of inventions first articulated in bars are plentiful, from the first electronic digital computer and MRI machines to Discovery Channel's Shark Week.[1] A large part of the modern computer industry emerged out of an informal group that met at The Oasis bar and grill (Balin, 2001; Farivar, 2018; Wozniak, 1984), and several other Silicon Valley watering holes have become legendary as common meeting places for engineers during the early decades of the high tech industry.[2] In decades prior to the enactment of prohibition laws, saloons were even more important as social institutions than the bar is today, acting as informal local hubs in which a large share of the population spent a large fraction of its non-working time (e.g., Moore (1897), Calkins (1919), Sismondo (2011)).[3] With the passage of prohibition, the state took away these social hubs, disrupting the preexisting informal social network and forcing people to interact in other venues. I observe how invention, proxied by the rate and number of patents, changed following the prohibition-induced disruption.

Prohibition throughout U.S. history is a particularly useful setting to study. Before the passage of federal prohibition, states and counties could determine for themselves whether or not to allow alcohol consumption in bars. When state-level prohibition laws went into effect, counties that were previously wet saw a disruption of their saloon-based social networks, while counties within the same state that were already dry did not, providing a natural control group. I show that these two groups of counties have parallel trends in inventive activity prior to the passage of state prohibition and are balanced along observable dimensions.

[1] See, e.g., Brown (2011) or Wilke (2015).

[2] Writes Wolfe (1983): "Every year there was some place, the Wagon Wheel, Chez Yvonne, Rickey's, the Roundhouse, where...the young men and women of the semiconductor industry...would head after work to have a drink and gossip and brag and trade war stories."

[3] I describe the saloons' social role in much more detail in Section 2 below.

Moreover, I utilize the full histories of county prohibition statutes and county alcohol-related voting results to isolate counties that had persistent views towards alcohol. Hence, in the baseline sample any changes in a county's prohibition status are driven by changes in the statewide law rather than changes in underlying cultural attitudes that might directly affect innovation (Bénabou, Ticchi, & Vindigni, 2016; Vakili & Zhang, 2018).

Following the imposition of state prohibition laws, patenting dropped by 8-18% in the counties that wanted to remain wet relative to consistently dry counties in the same state. Patenting fell dramatically in the years immediately after prohibition went into effect. But as individuals reconstructed their informal social networks and found new places in which to interact, invention rebounded, returning to its prior trend within 4-6 years. While patenting in wet counties returned to its pre-prohibition level relative to the dry counties within a few years, collaboration patterns were permanently altered. Relative to inventors in dry counties, repeat inventors in wet counties were less likely to collaborate with the same individuals they had patented with prior to prohibition and were relatively more likely to collaborate with new individuals. Wet counties also saw more change in the types of inventions patented as measured by patent classes. For repeat inventors, the change in patent classes was primarily driven by inventors collaborating with new individuals. Together, these results suggest that who individuals interact with in informal settings affects the kinds of ideas they generate.

Of course, prohibition could have plausibly affected invention through many channels beyond disrupting informal social interactions. I present several pieces of evidence that suggest that disrupting interactions account for the observed decrease. First, the drop in patenting was smaller for groups that did not typically attend saloons, including women and ethnic groups that were more likely to drink in private. Second, counties that had more substitutes for the saloon (like churches, barber shops, and non-saloon restaurants) at the time prohibition went into effect had smaller drops in patenting. Finally, I directly investigate several alternative channels and show that the observed effects is not explained by a decline in alcohol consumption, a general economic slump, or differential migration patterns.

In light of these findings, this paper contributes to three literatures. First, the paper contributes to the literature on the economics of innovation by showing that informal social interactions are quantitatively important for invention. More specifically, this paper builds on a growing literature using shocks to the supply of potential innovators to estimate the importance of peers for innovative outcomes.[4] The imposition of prohibition is a particularly informative setting, since prohibition disrupted the structure of the local social network but did not alter the scale of the network or the identities of the individuals within the network. Second, the paper contributes to the large empirical literature on social networks by showing how a historical natural experiment can be used to test network properties in a reduced form way.[5] Third, this study builds on the literature examining the quantitative effects of prohibition.[6] Similar to studies of the effect of prohibition on infant health (Jacks et al.,

[4]See, for instance, Moser and San (2019) and Doran and Yoon (2019) on changes to the supply of potential inventors in the U.S. following the passage of immigration quotas in the 1920s; Moser, Voena, and Waldinger (2014) on the inflow of German Jewish scientists to the U.S. following the rise of Nazism in the 1930s; Waldinger (2010) and Waldinger (2012) on the outflow of German scientists during the same period; Borjas and Doran (2012) and Ganguli (2015) on the inflow of scientists from the former Soviet Union; and Azoulay, Graff-Zivin, and Wang (2010), Oettl (2012), and Azoulay, Fons-Rosen, and Zivin (2019) on the death of scientists as a natural experiment that disrupts scientists' peer networks.

[5]The empirical literature on social networks is too large to survey here. See Esteves and Mesevage (2019) for a review of empirical social network studies in economic history or Jackson (2008) and Bramoullé, Galeotti, and Rogers (2016) for general surveys. Attempts to draw inferences about the economics of networks without complete network data include Banerjee, Breza, Chandrasekhar, and Golub (2018), Beaman, BenYishay, Magruder, and Mobarak (2018), and Breza, Chandrasekhar, McCormick, and Pan (2019).

[6]See Dills and Miron (2004) on the effect of prohibition on cirrhosis deaths, Owens (2014) and Livingston (2016) on organized crime, Evans, Helland, Klick, and Patel (2016) on adult height, Bodenhorn (2016) on homicides, García-Jimeno (2016) on law enforcement, Hernández (2016) on firm dynamics, and Jacks, Pendakur, and Shigeoka (2016) on infant mortality.

2016), invention is particularly intriguing to study because it represents an outcome that was unintentionally affected by prohibition.

This paper is organized as follows. Section 2 describes the historical context, describing saloons' role as places of information exchange and giving an overview of the alcohol prohibition movement. Section 3 describes the data. Section 4 presents the baseline results and argues that they are driven by a disruption of social interactions. Section 5 documents the importance of the exposure to ideas, rather than simply exposure to collaborators, for invention. Section 6 shows that collaboration patterns change after the disruption of existing social networks. Section 7 briefly concludes.

2 Historical Background

2.1 Bars and Social Interactions in U.S. History

In this section, I present historical evidence that bars facilitated exposure to new people and new information throughout U.S. history. Bars, taverns, pubs, and saloons have long acted as social hubs. Pubs and taverns were the primary social gathering place in England for both the high and low classes into the late 17th century. Around that time, tea and especially coffeehouses began usurping the role of the pub for the upper classes. These new types of drinking establishments played a key role in spreading the ideas of the Scientific Enlightenment (Cowan, 2005; Mokyr, 2016). After the expansion of coffeehouses, pubs were no longer the primary meeting place for intellectuals, but they were still important as a gathering place for commoners (Hailwood, 2014).

Across the Atlantic Ocean, tea and coffeehouses never claimed the same role as social hubs for the sharing of information; instead, that role was filled by taverns and saloons. The American revolution was plotted in places like Boston's Green Dragon Tavern and Philadelphia's City Tavern (Sismondo, 2011). Because of their role in fomenting the revolution against England, taverns and saloons became known as the "nurseries of freedom". Drinking at a public house was seen as a patriotic virtue (Rorabaugh, 1979, p. 35). Thus, at a time when the upper classes in England were looking down on the pubs as wasteful distractions for the poor and uneducated, in America taverns and saloons were places frequented by rich and poor, educated and uneducated alike. The early American tavern even hosted the high-minded intellectual events that took place in coffeehouses in England; Sismondo (2011, p. 42) notes that the tavern was used "not only as a watering hole but also as a classroom and lecture hall".

In the late 19th and early 20th centuries, saloons continued the social tradition of the tavern, with Ade (1931, p. 100) proclaiming "[t]he saloon was the rooster-crow of the spirit of democracy." The saloon was particularly important for the working class. Indeed, "only the church and the home rivaled the saloon as working-class social centers" (Rosenzweig, 1983, p. 56). The post-workday happy hour is not a recent invention: workers typically met to drink at their favorite spots after work (Rorabaugh, 1979, p. 132). Many saloons specifically catered to skilled individuals in particular occupations, and workers from different firms in the same industry would meet to talk shop, as evidenced by saloon names such as "Mechanics' Exchange" and "Stonecutters' Exchange;" saloons also frequently served as "informal employment bureaus" (Powers, 1998, p. 54). Notably, this time period is what Sokoloff and Khan (1990) and Khan (2005) refer to as "the democratization of invention":

patents tended to come not from an aristocratic elite, but from skilled workers and craftsmen, the same types of individuals likely to meet in their local saloon. In 1910, for instance, the top ten most common inventor occupations included laborers, machinists, carpenters, drivers, manufacturers, and painters.[7]

The social role of saloons was especially valuable for a nation with high occupational and geographic mobility. Okrent (2010, p. 28) writes:

> The typical saloon featured offerings besides drink and companionship, particularly in urban immigrant districts and in the similarly polygot mining and lumber settlements. In these places, where a customer's ties to a neighborhood might be new and tenuous, saloonkeepers cashed paychecks, extended credit, supplied a mailing address or a message drop for men who had not yet found a permanent home, and in some instances provided sleeping space at five cents a night. In port cities on the East Coast and the Great Lakes, the saloonkeeper was often the labor contractor for dock work. Many saloons had the only public toilets or washing facilities in the neighborhood.

Saloons also typically housed a community's first telephone (Duis, 1983, p. 121). Thus, new information often arrived first in the saloon, whether it came by person, mail, or phone. Some saloons even "doubled as lending libraries" (Sismondo, 2011, p. 169). At least one Midwestern saloon owner referred to his establishment as an "educational institution" (McGirr, 2016, p. 16). When describing the various benefits of the saloon, novelist Jack London listed its value for spreading ideas first and foremost: "Always when men came together to

[7] I construct counts of inventors by occupation using the matched patent-census data in Sarada, Andrews, and Ziebarth (2019). The most common occupations in other years during this time period are similar. These results are available upon request.

exchange ideas, to laugh and boast and dare, to relax, to forget the dull toil of tiresome nights and days, always they came together over alcohol. The saloon was the place of congregation. Men gathered to it as primitive men gathered about the fire" (London, 1913, p. 33).[8]

The importance of these social and informational benefits of the saloon are not simply a concoction of recent social historians, but were well understood by contemporaries; in addition to London (1913), see Moore (1897) and Calkins (1919).[9] Perhaps the best way to see the value of the saloon as an institution that promoted dialogue and conversation was to compare it to an emerging institution that discouraged these actions: the cinema (see Sismondo (2011, p. 206-208)). Following a visit to the U.S., Chesterton (1922, p. 88) remarked, "The cinema boasts of being a substitute for the tavern, but I think it is a very bad substitute... Nobody enjoys cinemas more than I, but to enjoy them a man has only to look and not even to listen, and in a tavern he has to talk ."

2.2 Alcohol Prohibition in U.S. History

While millions of the nation's men enjoyed the amenities provided by drinking establishments, a growing segment of society was fixated on the dark side of saloons. Okrent (2010,

[8]Jack London's life vividly illustrates both the bright and dark sides of the saloon in early 20th century America. Unable to stem his own consumption, London became an unlikely advocate for women's suffrage, famously remarking that, "The moment women get the vote in any community, the first thing they do is close the saloons. In a thousand generations to come men of themselves will not close the saloons. As well expect the morphine victims to legislate the sale of morphine out of existence" (London, 1913, p. 204).

[9]Moore (1897, p. 8) writes of the saloon-goer: "The desire to be with his fellows – the fascination which a comfortable room where men are has for him is more than he can resist; moreover the things which these men are doing are enticing to him; they are thinking, vying with each other in conversation, in story telling, debate. Nothing of general or local interest transpires which they do not "argue" out. The social stimulus is epitomized in the saloon. It is center of learning, books, papers, and lecture hall to them, the clearing house for their common intelligence... As an educational institution its power is very great and not to be scorned because skilled teachers are not present, for they teach themselves."

p. 16) stresses that some men spent the majority of their income at the bar, neglected work to drink, or spread venereal disease to their families when they "found something more than liquor lurking in saloons." Powers (1998) argues that most types of deplorable behavior in the saloons were exceedingly rare, including public drunkenness (p. 12), drinking oneself into bankruptcy (p. 52), child neglect and spousal abuse (p. 46), and prostitution (p. 31). But there can be little doubt that these saloon-borne horrors weighed heavily in the public imagination and either inspired prohibition activists or, at the very least, were used by them as propaganda. Of course, not all prohibitionists were purely altruistic. Closing the saloon was seen as a way to prevent immigrant groups, primarily Irish and German, from organizing politically (Sismondo, 2011, p. 129) and to keep alcohol out of the hands of southern blacks (Pegram, 1997; Bleakley & Owens, 2010; Okrent, 2010, p. 42-46, McGirr, 2016, p. 72-89).

Against this backdrop, an anti-alcohol movement was brewing. Temperance movements had existed in the U.S. since at least the start of the Washington Movement in 1840 (Okrent, 2010, p. 9-10), and likely several decades before that (Rorabaugh, 1979, p. 191-2), but early movements had promoted voluntary abstinence or moderation. A new round of prohibition sentiment was uncorked in the late 19th century and continued into the 1920s. Throughout this period, anti-alcohol groups, spearheaded first by the Womens Christian Temperance Union (WCTU) and then by the Anti-Saloon League (ASL), focused their attention on passing alcohol prohibition at the local level. The doctrine of the local option meant that each county determined its own liquor laws, unless the state changed the law to supersede the local decisions. By focusing on influencing local laws, the temperance forces were able to establish beachheads of dry support throughout the nation. Once prohibition forces had achieved a critical mass of anti-alcohol votes within a state, they pushed for statewide prohibition, either through legislation or, more commonly, through referendums. As Odegard (1928), K. A. Kerr (1985) and Lewis (2008) argue, state prohibition campaigns tended to be focused and directed; the groups might intensively target only a handful of communities within a state. In addition to eliminating legal alcohol sales in the affected counties, local prohibition depressed wet voter turnout in subsequent statewide referendums. Lewis (2008) suggests that this is caused by the elimination of the saloon as a site for political mobilization, but it is also the case that voting against prohibition in a state election held little appeal for voters living in already dry counties. The upshot of this strategy is that achieving prohibition at the county level had a disproportionate effect on statewide vote totals for prohibition. This means that, when statewide prohibition passed, views towards alcohol remained largely constant in most counties that maintained constant local option laws, a fact I exploit below.

The culmination of the prohibition movement was enactment of prohibition policies at the federal level. The 18th Amendment to the U.S. Constitution, which outlawed the manufacture, sale, and transportation of alcohol, was first proposed in 1917 and went into effect in 1920. But de facto federal prohibition had been in force throughout much of World War I.[10] Many contemporary sources regarded the wartime prohibition as quite effective.[11] For these reasons, it is difficult to disentangle the start of federal prohibition from the effects of

[10] Indeed, World War I marked a turning point in public opinion, with Germans so closely associated with the brewing industry (Pabst, Schlitz, and Anheuser-Busch being a few prominent examples; see, e.g. (Okrent, 2010, p. 85-87)). The establishment of the emergency Food Commission in spring 1917 and the passage of the Lever Act in August, 1917, prohibited the production of spirits and severely limited the production of beer by reserving grains for food production (Paxson, 1920, p. 60-61). In December, President Wilson signed a declaration imposing temporary prohibition on the production of alcoholic beverages (Tyrrell, n.d.). The U.S. also prevented sale of alcohol to military personnel and imposed dry zones around military bases that imposed prohibition on large swaths of the country (Mendelson, 2009, p. 244).

[11] In his analysis of prohibition, Irving Fisher dates the start of federal prohibition to 1917 (Fisher, 1927); Merz (1930) uses 1917 as the beginning of the long "dry decade;" Ade (1931, p. 77) refers to restrictions on public alcohol consumption during the war as "the grand shutdown;" and Burnham (1968, p. 59) cites a study by Warburton (1932) that finds that the greatest decline in alcohol consumption from 1910-1930 occurred between 1917 and 1919.

World War I; the imposition of state prohibition laws, at staggered times across the country, provides cleaner identification of the effects of prohibition. In Section 4.1 below, I show that all results are robust to excluding World War I years.

2.3 Consequences of Prohibition on Social Interactions

The start of prohibition, at both the state and federal level, ended the legal operation of the saloons. While it is uncertain how effective state-level prohibition laws were at stopping the flow of alcohol, "the effect [of prohibition] on the saloon...was probably greater than on drinking itself" (Rosenzweig, 1983, p. 119).[12] Accounts of national prohibition document the near-total annihilation of the saloon: McGirr (2016, p. 16) reports that "both sides [of the prohibition debate] agreed that the law almost single-handedly killed the institution of the saloon" and Welskopp (2013, p. 27) concludes that "the saloon completely vanished from the scene." To present evidence that a similar decimation also occurred following state level prohibition, in Appendix B, I present results from a sample of county and city directories and show that "official" saloons vanished from the directories following prohibition and, moreover, just under 90% of addresses that housed saloons before statewide prohibition went into effect sat vacant in post-prohibition years. Thus, it does not appear that saloons were simply able to quickly reconstitute themselves as restaurants or other "third places" (Oldenburg, 1989) to allow individuals to easily maintain their social networks. This is consistent with studies of former saloon properties following national prohibition (McGirr, 2016, p. 48).

[12]Rosenzweig (1983) describes the effects of prohibition at the city level in Worcester, MA in the late 19th century. Dills, Jacobson, and Miron (2005); Dills and Miron (2004) investigate the effect of federal prohibition on alcohol consumption and conclude that consumption fell by about 10-20%.

While both state and federal laws appear to have been strongly enforced initially, after a few years people began flouting the prohibition laws with impunity. For example, Livingston (2016) argues that consumers stockpiled alcohol in the run-up to the passage of prohibition, and so the initial effects of prohibition were to shift drinking into the home. After about two years, these stockpiles were exhausted and people began turning in large numbers to outside sources of alcohol like speakeasies and bootleggers. Bader (1986) reports that in Kansas, which adopted a state prohibition amendment in 1881, enforcement was initially strict, with the years after prohibition went into effect becoming known as the "auspicious eighties." But the widespread emergence of illegal saloons after a few years gave the following decade the nickname of the "wet nineties." In Vermont, drunkenness arrests fell in 1853, the first year state prohibition was in effect, before rising in the following years; a similar pattern occurred after the imposition of federal prohibition (Krakowsi, 2016, p. 59, 100). Dills et al. (2005) observe similar dynamics when examining drunkenness arrests from a panel of cities following federal prohibition.

Unfortunately, much of the evidence on the extent to which prohibition laws were obeyed is necessarily anecdotal. But as far as one can tell, prohibition laws appear to have radically changed the social environments in which individuals interacted. It took time, likely several years, for institutions such as speakeasies to arise on a large scale to replace the pre-prohibition saloons.

3 Data and Empirical Strategy

3.1 Prohibition Data

County-level prohibition status data are from Sechrist (2012). These data list, for each U.S. county from 1801 to 1920, whether the county is wet or dry, the number of historical sources available to support the conclusion of wet or dry, and whether the entire state was dry. The fact that county-level prohibition status is available for every year allows me to determine when a particular county goes dry. See Appendix A for more details on the Sechrist (2012) data.

Of course, counties are unlikely to exogenously adopt local prohibition laws. Changes in county prohibition status are likely correlated with other county characteristics, such as changes in religious views or a changing county ethnic composition, that might be causally related to invention outcomes. To minimize concerns about county selection into prohibition, I exploit the adoption of state-level prohibition laws. Information on state prohibition laws are available from numerous sources; I rely on data from Lewis (2008) and Merz (1930).[13] When state-level prohibition laws go into effect, counties are differentially treated on the basis of their preexisting county prohibition status: counties that were already dry should have seen little effect of the state law, while counties that were wet had to shutter their saloons, disrupting informal social networks.

The same changes in public opinion that occur at the state level to drive the adoption

[13] While one can use the Sechrist (2012) data to observe the first year in which every county in a state is dry, it is still necessary to incorporate additional data sources to ensure that some counties go dry due to the imposition of state level prohibition laws rather than, for instance, every county deciding to adopt prohibition via the local option. In addition to providing the dates at which state prohibition laws go into effect, Lewis (2008) and Merz (1930) provide useful details about how state laws were adopted, in particular whether they were passed via a constitutional referendum or via statute.

of prohibition laws were also likely taking place in microcosm within counties, and hence using the adoption of state prohibition laws does not completely mitigate concerns about the endogeneity of prohibition. I further restrict attention to counties that were consistently wet or consistently dry for extended periods of time prior to the imposition of state prohibition laws.[14] The idea is that, if counties did not change their local laws despite numerous opportunities to do so, they likely had little change in the underlying social or cultural attitudes that were correlated with both invention and the desire for prohibition. The fact that groups like the Anti-Saloon League attempted to change state policies by targeting public opinion in a limited number of highly selected locations supports this argument; see the discussion in Section 2.2. In the baseline sample, I utilize data on voting in state referendums to further restrict attention to counties that voted consistently with their preexisting local laws; that is, I compare counties that were consistently wet and voted to remain wet to counties that were consistently dry and voted to remain dry.

3.2 Patent and Other County-Level Data

Data on patents is from Petralia, Balland, and Rigby (2016b).[15] This dataset is augmented with data on patent classes from Marco, Carley, Jackson, and Myers (2015) and patent

[14]Specifically, I restrict the sample to counties that were either wet or dry for five years before the enactment of state-level prohibition. Results using counties that were wet or dry for 10 or 15 years before enactment of state-level prohibition are similar although noisier due to the fact that there are fewer counties that had consistent laws for these longer stretches of time; see Appendix G.

[15]See Petralia, Balland, and Rigby (2016a) for details on how this dataset, known as the HistPat data, was constructed. Relative to other historical patent datasets, HistPat contains location information for the largest share of U.S. patents, close to the full universe. See Andrews (2019) for an in-depth analysis of the strengths and weaknesses of this dataset. One potential drawback is that the HistPat data reports the year of grant for each patent, rather than the year of filing. In ongoing work, I merge data on filing dates from the USPTO with this patent data. The use of grant dates rather than filing dates is unlikely to be a major concern in this context, since both the mean and median patent were granted the same year they were filed until the late 1910s (Berkes, 2018).

quality measures from Berkes (2018). County-level data is from the National Historical Geographic Information Series (NHGIS, Manson, Schroeder, Riper, and Ruggles (2017)). Additional supplementary datasets are described along with the results below.

3.3 Empirical Strategy

Using the sample of consistently wet and consistently dry counties as described above, I estimate the following specification:

$$\begin{aligned} Patenting_{ct} = & \beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\ & + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{1}$$

where $WetCounty_c$ is a dummy variable that equals 1 if county c was consistently wet according before the imposition of its state prohibition laws and voted to remain wet in the state referendum. $StatewideProhibition_{ct}$ is a dummy variable that equals 1 in all years t after county c's state imposes statewide prohibition. X_{ct} is a vector of county-specific time-varying covariates that are independent of the treatment. The identifying assumption is that, absent the imposition of state prohibition laws, the formerly wet and consistently dry counties would have continued to patent along parallel trends. Throughout, I cluster standard errors by state.

3.4 Descriptive Statistics

In Table 2, I predict whether a sample county is wet or dry in the last census year prior to the imposition of state prohibition on the basis of observable characteristics. Results are from a linear probability model, although results from logit or probit regressions are similar. Column 1 confirms the common conception of wet counties prior to prohibition: counties that were wet tended to have more migrants and have more males (counties with substantially more males than females tended to be low population western mining counties) and, while not individually statistically significant at conventional levels, they appear to have a larger population and be more urbanized. This common stereotype of the wet county, however, is largely picking up regional or state-by-state differences. In Column 2, I include a state fixed effect and show that most differences between wet and dry counties shrink in magnitude and are no longer statistically significant when comparing within a state.

In Appendix C, I show that the treatment, imposing state prohibition, does not statistically affect any of these observed outcome variables. Thus, including these observable characteristics in the vector X_{ct} in Equation (1) is unlikely to cause "bad control" problems in the language of Angrist and Pischke (2009) and the estimated treatment effect is unbiased, although including the observable controls does increase the precision of the estimates. I therefore include the following as control variables in all results: logged county population, the fraction of the county population living in urban areas, the fraction of county residents who are migrants from another state or country, logged manufacturing establishments, and logged manufacturing output. Appendix C also shows that estimated magnitudes and statistical inferences are insensitive to the inclusion, exclusion, or composition of X_{ct}.

Figure 1 graphically compares the formerly wet and consistently dry counties in the sample. I plot raw logged patenting (Panel a) and the raw patenting rate (Panel b) in counties that were wet and dry for extended periods of time before the imposition of state-level prohibition. Year 0 indicates the year in which a state prohibition law goes into effect.

The first thing these figures make clear is that the trends in patenting in wet and dry counties were remarkably parallel before the imposition of statewide prohibition. Patenting in the formerly wet counties decreases sharply relative to the consistently dry counties in the three years immediately following prohibition, before almost returning to the initial level in the final two years plotted. This figure provides the first evidence that prohibition caused a decline in patenting in the formerly wet counties. The difference in levels between the formerly wet and consistently dry counties is driven by a few formerly wet counties with unusually high average levels of patenting. Appendix D shows that either omitting these outlier counties or residualizing the data in a variety of ways results in formerly wet and consistently dry counties that patent at very similar levels prior to the imposition of state prohibition laws.

4 Baseline Results

Table 3 presents results from estimating Equation (1). Column 1 uses $log(Num.Patents_{ct}+1)$ as the dependent variable, Column 2 uses $arcsinh(Num.Patents_{ct}+1)$, and Column 3 uses the patenting rate, given by $\frac{Num.Patents_{ct}}{Population_{ct}}$.[16] Each group of rows estimates Equation (1) using a different subsample of county data as described in Section 3. For each group, I list the mean of the dependent variable for the wet counties, the adjusted r^2 of the regression, the number of county-year observations in the sample, and the number of individual counties in the sample.

[16] The inverse hyperbolic sine is given by $\text{arcsinh}(NumPat_{ct}) = \log(NumPat_{ct} + (NumPat_{ct}^2 + 1)^{\frac{1}{2}})$. Relative to the log transformation, it allows for the inclusion of counties with zero patents without adding an arbitrary constant to the number of patents in each county. The denominator in the patenting rate calculation is the total county population; it has not been adjusted to reflect the fact that the very young and very old are unlikely to patent.

The first group of rows presents estimates using the baseline sample of states that impose prohibition via referendum. With the baseline sample, imposing prohibition reduces patenting by about 12% in the logged specification and 16% in the inverse hyperbolic sine specification for the formerly wet counties relative to the consistently dry counties. The patenting rate declines by about 3.7 patents per one million county residents, a roughly 15% decline from the baseline of 24 patents per million residents.

The second group of rows uses all states that Sechrist (2012) identifies as becoming entirely dry, even if no state referendum was passed. The results using the Sechrist (2012) are slightly smaller than the baseline estimates although still statistically significant, with the level of patenting declining by 8-10%, or by about 2.4 patents per million population. Finally, the third group of rows restricts attention to the counties voting in the referendum that were bastions of either wet or dry sentiment. The level of patenting falls by 14-18%, with the rate of patenting falling by 8.5 patents per million population. Restricting attention to bastions of wet and dry support provides the most confidence that views towards alcohol are largely constant in the treated and control counties, but the sample size is dramatically reduced, resulting in slightly less precise estimates, although all are still statistically significant at the 5% or 10% levels.

To put these magnitudes into perspective, compare the effect of imposing prohibition to the effect of losing a superstar academic collaborator considered in Azoulay et al. (2010). These authors find that academic scientists reduce their innovative output by roughly 8% following the unexpected death of a star collaborator, who accounts for about 2% of their collaborative relationships. I find similar magnitudes from the imposition of prohibition. The difference between these settings is that, in the case of prohibition, it is likely that a much larger share of relationships are disrupted (given the importance of the saloon as a social hub, it is plausible that far more than one in every fifty conversations a given individual had took place in the saloon), but because prohibition disrupts purely informal conversations, each conversation was probably less relevant in expectation than conversations between academic collaborators.

Figure 2 examines the dynamics of the treatment effect. I estimate

$$\begin{aligned} Patenting_{ct} =& \beta_0 + \sum_{\tau \in T} \Big[\beta_{1\tau} WetCounty_c * Time_\tau + \beta_{2\tau} Time_\tau \Big] \\ & + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{2}$$

where $\beta_{1\tau}$ are interaction terms for the wet counties in each pair of year before and after the imposition of statewide prohibition. For the years prior to the imposition of statewide prohibition, the effect is close to zero and insignificant. Consistent with the intuition from Figure 1, patenting is statistically significantly lower in the first two years for which prohibition is in effect. In the next pairs of years, the magnitude is a bit smaller although still significantly different from zero. Finally, in the following pair of years, the magnitude is even closer to zero and is statistically insignificant. I therefore cannot reject that patenting fully returns to its baseline level within five years of prohibition, although the magnitudes are also consistent with partial recovery as predicted by the model if some individuals refuse to rejoin the social network once the bars are removed. It is also important to note that, while a clear pattern is visible, the estimates for each bin of years are not statistically different from one another.

The fact that patenting falls almost immediately after the imposition of state prohibition laws, while striking, should not be surprising. As noted in Section 3, the patent examination delay was negligible for my sample period. Moreover, most of the inventors in this sample were skilled blue-collar or artisan workers, and hence most of their inventions were likely the result of tinkering rather than long-term intensive research and development activities, suggesting that the "invention lag" between a conversation and the drafting of a patent was likely quite short as well. In other historical contexts, others have documented dramatic changes in the rate and direction of patenting activity in the first year immediately following changes in laws.[17] Likewise, the timing of the rebound is consistent with anecdotal evidence on finding alternatives to legal saloons; see the discussion in Section 2.2 above.

The fact that the treatment effect exhibits a clear non-monotonicity as predicted by theory provides additional comfort that the effect is not being driven by a violation of the parallel trends assumption. It is difficult to conceive of alternative explanations for this pattern and timing that would occur in the formerly wet counties relative to the consistently dry counties.[18] In Appendix E, I present additional suggestive evidence that the observed treatment effect is not driven by differential trends by testing for the presence of several different forms of nonlinear pre-trends.

[17] See, for instance, Hanlon (2015), which documents that during the U.S. Civil War, British inventors shifted their patenting activities to take into account changes in input prices in the first year that the U.S. blockade was in effect.

[18] To be clear, the observed dynamics bolster the evidence that the observed effect is driven by the imposition of prohibition. This is not the same as arguing that the effect is driven by a disruption of social interactions. In the following sections, I present additional evidence that the disruption of social interactions played an important role.

4.1 Robustness Checks

I next probe the robustness of these results. Because the prohibition movement in the U.S. was gaining in popularity as the 1910s progressed, one concern is that estimates on the effect of statewide prohibition may be contaminated by the effects of World War I.[19] In Column 1 of Table 4, I simply drop all observations from years that occur during World War I. In Column 2, I drop observations from all states that adopted prohibition referendums during World War I. Finally, in Column 3 I drop all states that adopted prohibition referendums after 1912 and so for which World War I would overlap with the post-prohibition data. Results in Columns 1 and 2 are similar to the baseline estimates. Results in Column 3 are larger in magnitude than the baseline estimates but, due to the small number of states that adopted prohibition referendums before World War I, is very imprecisely estimated.

I briefly discuss a number of additional tests here, with the full results relegated to the appendix. First, in Appendix G, I show that the results are robust to using alternative samples, such as restricting attention to states in which the final referendum vote was close or requiring counties to be consistently wet or dry for different lengths of time to be included in the sample. In Appendix H I show that the results are robust to various alternative regression specifications. Appendix C shows that the results are robust to the inclusion or exclusion of county-specific time-variant controls. Appendix I shows that the results are not driven by the largest or smallest counties; results are similar when discarding counties of various sizes, and if anything the effect of prohibition appears to be slightly larger in magnitude for counties with a smaller population, although any differences are modest.

[19] This is especially the case if wet counties and dry counties were affected differently by the war on average. In addition, the war brought de facto national prohibition, as discussed in Section 2.2.

In Appendix J, I conduct a number of placebo tests, including examining cases in which statewide prohibition was brought to a referendum but failed to pass, the passage of statewide temperance education laws that reflected growing state prohibition sentiment but did not close the saloons, and the enactment of several other large state policies that did not directly disrupt social networks. In all cases, I find that these other policies had no differential effect in the wet counties relative to the dry counties.

4.2 Non-Saloon-Going Groups

I next show that, in line with the predictions of the model, groups that are more directly involved with the saloon prior to prohibition see larger declines in patenting.

4.2.1 Patenting by Males and Females

From the mid-19th century until the early 1920s, saloons were almost exclusively the domain of men. When they drank, women tended to do so in the privacy of the home or surrounded by close family friends (Powers, 1998, p. 27-35, 122-125; Peiss, 1986). This means that closing saloons should have little direct effect on female patenting. Using inventors' first names as in Sarada et al. (2019), I assign each patent a probability of belonging to a male or a female to get the expected number of patents for each gender. Around the time that most statewide prohibition laws in the sample were passed, females accounted for roughly 10% of all patents in the U.S. (Sarada et al., 2019).

Figure 4 plots the raw data for male and female patenting. Panel (a) plots logged patenting by males and females in the counties that were wet prior to prohibition. With the imposition of prohibition, patenting by males drops, while patenting by females is largely unaffected. One strength of this comparison is that it does not rely on the choice of counterfactual counties to compare to the formerly wet counties; instead, the figure is based on a comparison between different types of individuals within the formerly wet counties. Panel (b) plots what is essentially a triple-difference, plotting the difference in logged patenting between males and females over time in both the formerly wet and consistently dry counties. The gap in patenting between males and females shrinks in the counties that were wet prior to the imposition of prohibition, but the gap is unchanged in consistently dry counties.

Table 6 formalizes these results. Column 1 show that, if anything, the level of female patenting increased slightly following the imposition of statewide prohibition. Column 2 shows that the gap between male and female patenting shrunk by about 14% in the formerly wet counties relative to the consistently dry counties following prohibition. Columns 3 and 4 present some evidence that the fraction of all patents coming from females increased, although the magnitude is small.

4.2.2 Patenting by Saloon-Going and Non-Saloon-Going Ethnic Groups

Particular ethnic groups were also more likely to frequent saloons than others. In many cases, saloons tended to cater to particular ethnicities, and saloons for the Irish and German were especially common (Duis, 1983, 143-146). Some ethnic groups, on the other hand, were less likely to frequent public saloons: "Scandinavians, Jews, Greeks, and Italians either preferred intimate social clubs or did little drinking in public" (Duis, 2005).[20] The fact that

[20]This is not to suggest, of course, that these groups did not drink alcohol. For instance, Rorabaugh (1979, p. 239) presents estimates of cross-national per capita alcohol consumption and finds that people living in Italy drink more on a per capita basis than those living in the U.S., the U.K., or Germany in the decades surrounding U.S. prohibition. Duis (1983, p. 146-148) describes specifically Italian saloons in Chicago and Boston. But, while these groups may have consumed alcohol, their public consumption, and saloons specific to these ethnicities, were less common than for groups such as the Irish and German. To

some ethnic groups are much more connected to saloons is especially important in light of a sizable literature that shows that ethnic ties are important for invention (Foley & Kerr, 2013; S. P. Kerr & Kerr, 2018; W. R. Kerr, 2008a, 2008b).

As in the analysis with female patents, I observe how patenting changes for individuals whose last names identify them as belonging to either a saloon-going (Irish or German) or non-saloon-going (Scandinavian, Jewish, Greek, or Italian) ethnic group. At present, I match inventors' last names to databases of distinctive ethnic names; ongoing work is underway to improve the ethnic name matching.[21] Results are presented in Table 7. In Columns 1 and 2, I show that patents by inventors with distinctively saloon-going names dropped by a statistically significant 9% following prohibition, while patents by those with non-saloon-going ethnic names dropped by a statistically insignificant 1.4%. Column 3 shows that the change in the difference of levels of patenting between these two groups is statistically significant following prohibition. Finally, in Columns 4 and 5, I estimate changes in the fraction of patents by those with distinctively saloon-going names, where the denominator is all patents with an inventor whose name is ethnically identified. In both specifications, the coefficient is negative although not statistically significant.

4.3 Substitutes for the Saloon

If the drop in patenting following the imposition of statewide prohibition is driven by a disruption of the kinds of informal social interactions taking place in the saloon, then the drop should be smaller in places where alternative venues in which to interact are more

the extent Scandinavians, Jews, Greeks, and Italians interacted in saloons, the following results are a lower bound.

[21] Databases of last names come from Wikipedia categories for respective ethnic names. Ongoing work instead uses names from the decennial population censuses.

prevalent.

In Section 4.1 above and in Appendix I, I argue that the drop in patenting following the imposition of prohibition appears to be slightly smaller (although not significantly so) in counties with a larger population. This is consistent with individuals having more options to easily substitute to following the shuttering of the saloon in counties with more people.

In this section, I show that this intuition holds when more directly examining the prevalence of "substitutes for the saloon." I use decennial population census data to count how many individuals record occupations that are affiliated with substitutes for the saloon, including barbers, non-saloon restaurant workers, and religious workers and clery, in each county at the time state prohibition went into effect. Then, I estimate

$$\begin{aligned} Patenting_{ct} =& \beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\ &+ \beta_3 WetCounty_c * StatewideProhibition_{ct} * \log(Num.SaloonSubstitutes)_c \\ &+ \beta_4 StatewideProhibition_{ct} * \log(SaloonSubstitutes)_c \\ &+ X_{ct}\alpha + County_c + Year_t + \epsilon_{ct} \end{aligned} \tag{3}$$

for each proxy for saloon substitutes. In all specifications, the time-varying county-specific controls X_{ct} include controls for county population; all results are similar when using per capita measures of substitutes for the saloon instead.

Results are presented in Table 8. In Column 1, I use the number of barbers as a proxy for substitutes for the saloon. I find that 1% more barbers in a county (after controlling for population) is associated with a 0.04% less of a decrease in patenting in the formerly wet counties relative to the consistently dry counties after imposing prohibition. While this interaction term is statistically insignificant, it is economically meaningful, as a one standard deviation increase in the number of bartenders is associated with patenting declining by 5% less after prohibition. In Column 2, I proxy for substitutes for the saloon using the number of individuals who list a non-bartender restaurant-related occupation in the decennial census. 1% more restaurant workers in a county is associated with a statistically significant 0.05% smaller decline in patenting after the imposition of prohibition, or an 8% smaller drop after a one standard deviation increase in the number of restaurant workers. In Column 3, I proxy substitutes for the saloon using the number of people whose occupation is listed as "clergy" or "religious worker" in the decennial census, since in more religious areas people could more easily interact with others at local religious events. While also statistically insignificant, a 1% increase in the number of clergy is associated with a 0.03% smaller drop in patenting after prohibition, and a one standard deviation increase in the number of clergy is associated with a 3% smaller drop. Although the interaction terms are often imprecisely estimated, in all three cases greater numbers of individuals affiliated with activities that are substitutes for the saloon are associated with smaller declines in patenting following the imposition of prohibition.

4.4 Ruling Out Alternative Explanations

I argue that the observed effect is driven by a disruption of informal social interactions. To support this interpretation, in this section I show that the evidence does not support several plausible alternative interpretations.

One plausible alternative is that imposing prohibition caused a general economic down-turn. Since patenting tends to be highly pro-cyclical (Griliches, 1990), any kind of economic slump would likely be reflected in the patenting data. I control for time variant local economic and demographic variables to the extent possible.[22] Because these variables come from the decennial population censuses, they are only available at decadal frequencies, and so I interpolate for the between-census years. By construction, interpolation methods cannot capture the kind of high-frequency non-monotonicities that a temporary disruption might entail. Moreover, county-level data on economic performance metrics such as establishment counts or employment are typically not available until after the enactment of federal prohibition and so cannot be used in this study. Thus, the county-level time varying controls provide only a very crude way to control for general economic conditions.

I create an alternative measure of time-varying local economic performance by using the historical city directories described above in Section 2.2 and in Appendix B to get counts of the number of establishments in various non-saloon industries. I find no evidence that non-saloon establishments closed their doors at a faster rate in formerly wet counties relative to dry counties after the passage of statewide prohibition. These results are described in detail in Appendix B. Thus, while prohibition brought about the end of a common institution that served as a vital social hub, it did not appear to bring on more general economic disruption. Additionally, markets for technology were quite geographically integrated by the early 20th century (N. Lamoreaux, Sokoloff, & Sutthiphisal, 2013; N. R. Lamoreaux & Sokoloff, 2001), so temporary declines in local demand would be unlikely to substantially alter local inventors' incentives to patent.

An alternative explanation is that the results are driven by migration. In Appendix F,

[22] In Appendix C, I show that the results do not depend on the inclusion or exclusion of these variables.

I show that total population does not decrease in the formerly wet counties relative to the consistently dry counties following prohibition. It is possible, however, that aggregate changes in county demographics miss the effects of a policy on inventors. Highly inventive people may also particularly enjoy saloon life, and so might be particularly likely to migrate in response to the passage of prohibition. Indeed, Glaeser, Kolko, and Saiz (2001), Florida (2002a), Florida (2002b), and Vakili and Zhang (2018) suggest that one of the major benefits of social and cultural amenities is that they attract creative individuals. While it is unlikely that all creative people would pack up and leave immediately following the imposition of prohibition, making the dynamics of the observed effect difficult to square with a migration explanation, the story is nevertheless plausible. In Appendix F, I also show that the inflow of new migrants and the outflow of existing residents is not statistically different in the formerly wet counties relative to the consistently dry counties. Hence, selective migration also does not appear to be able to explain the observed results.

Other alternative explanations of the observed effects are inconsistent with the observed dynamics. For instance, the rise of organized crime or negative health effects from low quality bootleg liquor should decrease patenting monotonoically over time, while I observe a striking non-monotonicity.[23]

4.5 Alcohol or Social Interactions?

One alternative explanation that deserves special attention is the possibility that the observed change in patenting is driven by changes in the consumption of alcohol rather than

[23] As one example of this, Livingston (2016) argues that prohibition initially caused a reduction in homicides that lasted for about two years as imbibers drank down their pre-prohibition stocks of alcohol, but then homicides increased as they slowly turned to violent black markets to acquire their alcohol.

changes in social interactions. Evidence from the pharmacology and creativity literature is mixed regarding whether alcohol consumption increases creativity (Beveridge & Yorston, 1999; Hicks, Pedersen, Friedman, & McCarthy, 2011; Jarosz, Colflesh, & Wiley, 2012; Norlander, 1999; Plucker, McNeely, & Morgan, 2009).[24] Because prohibition makes public consumption illegal, it is difficult to obtain data on changes in actual alcohol consumption. I exploit two sources of variation to distinguish reductions in patenting resulting from reduced consumption and from disrupted social interactions. The key idea is that, even if drinking continued after prohibition, the illegality of consumption drove it into settings where individuals were less likely to have serendipitous social interactions. Thus, conditional on closing the saloons, there should be little difference in patenting between places where alcohol consumption could and could not continue.

First, I exploit the fact that not all state alcohol prohibition laws were the same. While all state prohibition laws outlawed saloons, some went further to eliminate drinking in other venues. In particular, some states enacted "bone dry" laws that prohibited all sale, transport, production, and consumption of alcoholic beverages. Other states allowed importation of alcohol from other states for home consumption as well as home production. Data on state prohibition laws are taken from Dills and Miron (2004, p. 301) and Merz (1930, p. 20-22).

[24]Of course, reducing alcohol consumption may also increase invention by reducing the impairment of cognitive skills. For instance, Irving Fisher, in a heroic act of extrapolation, computed that eliminating the consumption of alcohol would lead to a sufficiently large improvement in worker performance to increase the level of GDP by at least 10% (Fisher, 1927, p. 156-160). Alcohol clearly reduces self-control as well (Giancola, Josephs, Parrott, & Duke, 2010; Schilbach, 2018; Steele & Josephs, 1990), although it is unclear whether this increases or decreases the production of creative ideas.

I estimate the triple-difference regression

$$\begin{aligned} Patenting_{ct} = & \beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\ & + \beta_3 WetCounty_c * BoneDryLaw_c * StatewideProhibition_{ct} \\ & + \beta_4 BoneDryLaw_c * StatewideProhibition_{ct} \\ & + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{4}$$

where $BoneDryLaw_c$ is an indicator that is equal to one if county c is in a state that adopts a bone dry prohibition law and is zero otherwise. Results are presented in Column 1 of Table 5. The level of patenting in the wet counties relative to the dry counties, given by $\hat{\beta}_1$, declines by about 9.5%, which is slightly smaller than the baseline estimates of 12%. Moreover, the triple interaction term is not statistically different from zero and is less than half the magnitude of $\hat{\beta}_1$, indicating that the change in patenting following prohibition was not much affected by whether the prohibition law outlawed drinking outside of the saloons as well. If anything, $\hat{\beta}_3$ is positive, suggesting that the decline was slightly larger in states that were not completely bone dry. Results are nearly identical when I consider "nearly bone dry" prohibition laws in which states outlaw both saloons and importation but still allow home manufacture for home consumption; these results are available upon request.

Second, I use cirrhosis death rates as a proxy for actual alcohol consumption. Dills and Miron (2004) argue that, in spite of some concerns, the cirrhosis death rate is a good proxy for alcohol consumption even during periods of prohibition. The cirrhosis data is only available at the state level. To determine changes in county cirrhosis deaths, I therefore assume that consistently dry counties see no changes in their cirrhosis death rates and so all changes in the state rate are driven by changes in the formerly wet counties. More precisely, the change in county cirrhosis death rate is given by

$$\Delta CirrhosisDeathRate_{ct} = \begin{cases} 0 & \text{if } c \text{ is a consistently dry county} \\ \Delta CirrhosisDeathRate_{st} * \frac{Pop_c}{\sum_{w \in W} Pop_w} & \text{otherwise,} \end{cases} \tag{5}$$

where W is the set of all formerly wet counties. I then include the change in county cirrhosis death rates as an additional control variable in Equation (1). Results, presented in Column 2 of Table 5, are essentially unchanged, suggesting that the observed drop in prohibition is not driven by changes in illegal consumption of alcohol.

Taken together, these tests suggest that changes in the consumption of alcohol had at best modest effects on the patenting rate, while the large negative effects from shuttering saloons remains. This is consistent with the conclusion of qualitative researchers such as Oldenburg (1989, p. 169), who calls alcohol consumption "the junior partner in the talking/drinking synergism." Thus, while far from conclusive, these results imply that invention is one instance in which we cannot "blame it on the alcohol" (Foxx, 2008).

5 Collaborative Inventions

Many studies of social networks and innovation focus on the role of networks in facilitating collaboration between individuals (e.g., Allen (1983), Newman (2001), Nuvolari (2004), Breschi and Lissoni (2009), Crescenzi, Nathan, and Rodgríguez-Pose (2016), Mohnen

(2018)). This is not surprising, as collaboration is a readily observable outcome. But exposure to ideas may be even more important (Borjas & Doran, 2015).

To test for the importance of exposure to ideas for invention, I observe whether solo-authored patenting declines after prohibition. Results are presented in Table 9. In the first column, the dependent variable is the logged number of inventors.[25] The number of inventors in a county declines by almost the same percent as does overall patenting, suggesting that there is little change in the number of inventors on each patent. In Columns 2 and 3, I verify that $\frac{Num.Inventors_{ct}}{Num.Patents_{ct}}$ for county c at time t does not exhibit much change. Columns 4 and 5 show how the fraction of patents with more than one inventor, $\frac{Num.Patents\ with\ >1 Inventors_{ct}}{Num.Patents_{ct}}$, changes. I find that the share of patents falls by only one or two percent of all patents, with statistical significance depending on the specification. Thus, I reject the null hypothesis that social networks do not affect invention through exposing individuals to ideas.

While multi-inventor patents fall by roughly the same percent as solo-inventor patents, the declines in collaboration that do occur appear to take place in the collaborations most likely to be facilitated by the saloon. In Appendix K, I show that small collaborations with only two inventors falls by more than collaborations featuring three or more inventors; as informal organization is difficult with many inventors, the latter are likely to be composed of individuals who know one another through some sort of formal setting. I also show that collaboration that take place across towns, counties, or states are unaffected, which is unsurprising since the saloon facilitated interactions between geographically proximate individuals.

[25]This is defined as $\sum_p Inventors_{pct}$, where $Inventors_{pct}$ is the number of inventors on patent p in county c in year t. Note that this is not the number of *unique* inventors.

6 Persistence and Sensitivity of Collaboration Patterns

Next, I further explore the sample of patents with multiple inventors to document the sensitivity of inventors' collaborations to changes in the costs of informally interacting and how long changes in collaboration patterns persist. Persistent changes to collaboration patterns resulting from shocks to the social network is equivalent to the network structure exhibiting path dependence. If there were no path dependence, then as individuals reconstruct their social networks over time, they will eventually end up with a nearly identical network to the pre-prohibition network.[26] In contrast, if the network structure does exhibit path dependence, then in the post-prohibition network individuals will be interacting with new people in new ways, and hence will be exposed to pieces of information from different parts of the network.

I build on the collaboration results from the previous section by estimating Equation (1) for variables related to the identities of co-patenting pairs and the types of inventions patented. I next investigate whether these variables return to their pre-prohibition level within five years.[27] More precisely, I estimate the following model:

$$\begin{aligned} Patenting_{ct} =& \beta_1 WetCounty_c * First3ProhibitionYears_{ct} \\ &+ \beta_2 WetCounty_c * Next3ProhibitionYears_{ct} \\ &+ \beta_3 StatewideProhibition_{ct} + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{6}$$

[26] I say "nearly identical" because there are some individuals who only participated in the pre-prohibition network because they valued the saloon, not connections to other individuals. These individuals will not be part of the new network.

[27] Compared to some of the economic history literature on long-run persistence of shocks to social networks (Nunn & Wantchekon, 2011; Voigtländer & Voth, 2012), this five-year time window is an admittedly myopic definition of "persistence."

where $First3ProhibitionYears_{ct}$ and $Next3ProhibitionYears_{ct}$ are dummy variables for the first half and second half of the studied prohibition period, respectively.

I begin by examining whether the identities of collaborators persistently change in the counties that wanted to remain wet relative to the consistently dry counties following the imposition of prohibition. Recall from Section 5 that prohibition decreased collaboration in addition to solo-authored patenting. Consider a patent issued in year T with N inventors with names ι_1, ι_2, ..., ι_N residing in county c_1, c_2, ..., c_N, respectively, and denote the inventor-residence pair by $(\iota_n, c_n) \forall n \in N$. For each such patent, I record the patent as being invented by a pre-prohibition inventor pair if for any patent issued in year $t < \min\{T, t^*\}$, where t^* is the year in which prohibition is imposed, that patent contains $\{(\iota_i, c_i), (\iota_j, c_j)\}$ for some $i, j \in N$. More informally, I check for all cases in which a common pair of inventors' names and locations appear on a patent issued before prohibition went into effect.[28] Because names may be recorded differently on different patents and the historical patent data suffers from numerous transcription and optical character recognition errors, I harmonize names to the extent possible and use fuzzy matching techniques to search for collaborative pairs. I then count the number of patents containing such a pre-prohibition inventor pair in each county and each year.[29]

Results are presented in Columns 1 and 2 in Panels (a) and (b) of Table 10. Panel (a)

[28]Note that I only check for collaborative inventor pairs in which both inventors reside in the consistent locations over time. This excludes any collaborative pairs that persist after at least one of the inventors changes location, which is desirable in this exercise as I am interested in the effect of localized informal interactions on invention. Excluding these pairs also reduces the computational burden of checking for pre-existing name pairs.

[29]These measures necessarily depend on inventors appearing multiple times in the patent record. Repeat inventors make up roughly the same share of inventors in both the formerly wet and consistently dry counties. Moreover, prohibition causes the largest decrease in inventors with no previous patents, slightly smaller declines for inventors with one previous patent, and little decline for inventors with multiple previous patents. I present these results in Appendix L.

shows that the number of patents containing a pre-prohibition inventor pair declines on average over all post-prohibition years (estimating Equation (1)) by about 6.5% in the formerly wet counties relative to the consistently dry counties after the imposition of prohibition. Pre-prohibition inventor pairs also appear to decline as a share of all patents with multiple inventors, although this difference is not precisely estimated. In Panel (b), I show that pre-prohibition inventor pairs decline by between 5% and 6% in both the first and second three-year time period following prohibition. When dividing the data in this way neither coefficient is individually statistically significant, but the magnitudes of the coefficients do not suggest that patenting by pre-prohibition inventor pairs rebound in later years.[30]

The persistence and sensitivity of collaboration patterns to prohibition is striking. When a common venue of interaction was taken away, in spite of the fact that people continue to live in the same counties and work in the same jobs, inventors still collaborated with different people at higher rates in the formerly wet counties relative to the consistently dry counties. This suggests that social interactions are highly sensitive to the locations in which individuals frequently interact, consistent with, for instance, evidence in Marmaros and Sacerdote (2006).

I next document that the direction of inventive activity also changes after prohibition. To do this, I show that patent classes change over the long-run in the counties that wanted to remain wet relative to the dry counties. I calculate the most common 2-digit USPC patent class for all patents granted in each county over the 10-6 years before the imposition of prohibition in each state (that is, for the five years before the sample period begins). I

[30]This is not the case when estimating the fraction of multiple inventor patents with a pre-prohibition inventor pair, although this could be due to changes in the denominator; these results are estimated very imprecisely.

then check whether the logged patenting for patents in this class or the fraction of patents belonging to this class change following the imposition of prohibition in the wet counties relative to the dry. Panel (a) shows that, when looking at the average of all post-prohibition years, the number of patents in the most common pre-prohibition patent classes declines by about 12% and declines as a share of all patents with identifiable classes by a small but statistically significant 0.2%. When breaking up the effect into the first and second three-year post-prohibition periods in Panel (b), I find that the number of patents in the most common pre-prohibition patent classes declines by 12-13% in the formerly wet counties relative to the consistently dry counties in both periods, with both coefficients being highly statistically significant. If anything, the most common pre-prohibition patent class declines even more as a share of all patents after more than three years have passed; there is no evidence of recovery to previous patent classes.

While inventors in the formerly wet counties change the types of inventions they patent more than the consistently dry counties following the imposition of prohibition, they do not appear to do so in a systematic way. That is, it is difficult to identify particular technologies that the formerly wet counties substitute towards or away from on average following the imposition of prohibition. I interpret this as suggestive evidence that changes in the types patents are driven by churn in social interactions rather than the fact that some technology classes become relatively easier or harder after prohibition. I present these results in more detail in Appendix M.

For comparison, in Panel (c) I estimate Equation (6) using aggregate county patenting measures as the outcome variable. In all cases, the effect of prohibition is much smaller after three or more years has passed and is statistically indistinguishable from zero except for the per capita results, which are only significant in the second three-year period at the 10% level. Thus, while the new post-prohibition network performs similarly to the pre-prohibition network in terms of aggregate patenting, the identities of connected individuals and the information they are exposed to appears to be different.

These results raise the question of whether the pre-prohibition social network was "better" than the post-prohibition network in some sense. While assessing changes in welfare is beyond the scope of this paper, it is possible to assess whether one network produced more or better inventions. The results above indicate that counties produced roughly the same number of patents post-prohibition as before. I also test whether the patents in formerly wet counties are of different quality than those in the consistently dry counties following the imposition of prohibition, using several proxies for patent quality. I find no difference in patent quality, although I caution that each of the historical patent quality measures is imperfect.

7 Closing Time

In this paper, I document that a large disruption of informal social networks causes a significant and immediate drop in patenting. Patenting rebounds over time as individuals rebuild connections in other venues, consistent with a model of dynamic social network formation. I further show that social networks are important for invention because they exposure individuals to new ideas rather than simply facilitating collaboration, that the identities of collaborators are persistently sensitive to shocks to the informal social network, and that informal and formal connections are complements in the creation of new ideas.

These results have several implications for managers and policymakers. The first lesson is obvious from the baseline results: disrupting informal social networks has large negative effects on innovation. But the second lesson is the flip side of the first: while disrupting these existing networks can have negative effects, people will form alternative networks over time. To put it slightly differently, while a given social network may be fragile, people are resilient and find ways to repair or build new networks. More generally, these results paint a more complete picture of how information flows through social networks, including demonstrating the importance of interactions that happen outside the boundaries of the firm for innovations that occurs inside the firm. Finally, these results show the importance of geographic location in determining who individuals connect with and, in turn, what they invent.

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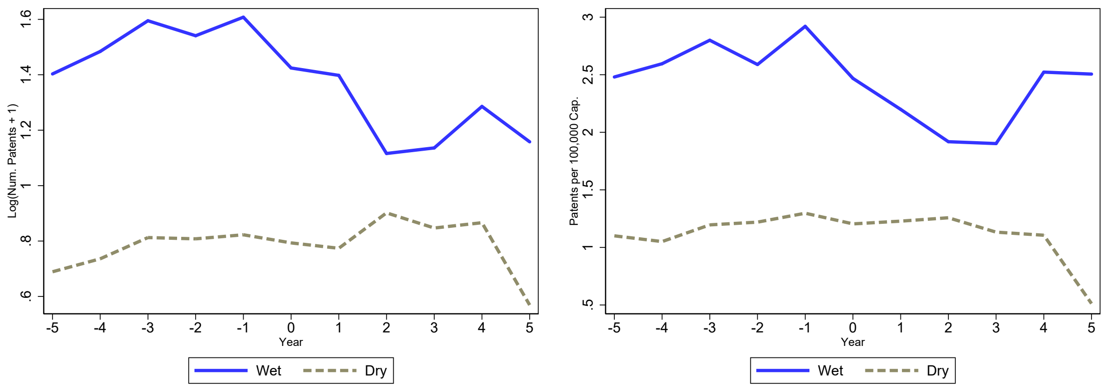
Graphs

Figure 1: Patenting in Formerly Wet and Consistently Dry Counties

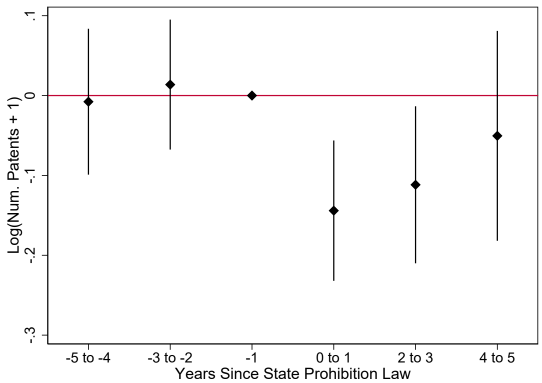


(a) *log*(Num. Pat + 1), Raw Data

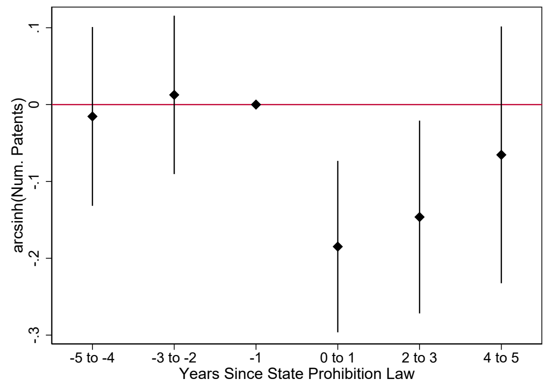
(b) Patents per Capita, Raw Data

Notes: Mean patenting in wet (blue solid line) and dry (brown dashed line) counties. Counties are listed as wet if they have been wet for at least 5 years before the enactment of state-level prohibition, and vice versa for the dry counties. The x-axis shows the number of years since the enactment of state-level prohibition. The year in which state-level prohibition is enacted is normalized to year 0. Everything left of year 0 shows pre-prohibition means; everything to the right shows post-prohibition means. The y-axis plots the dependent variable. Panel (a) uses $log(Num.Patents + 1)$ as the dependent variable. Panel (b) uses $\frac{Num.Patents}{TotalPop.}$ as the dependent variable.

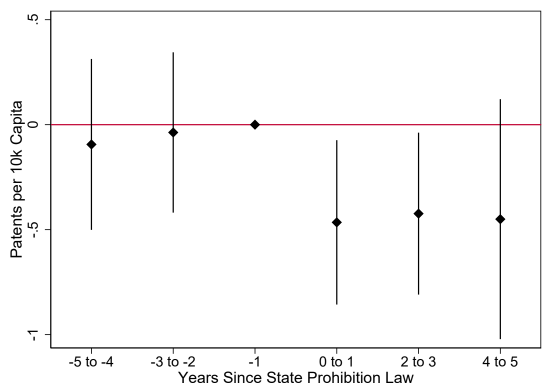
Figure 2: Difference by Time Since Prohibition



(a) *log*(Num. Pat + 1)



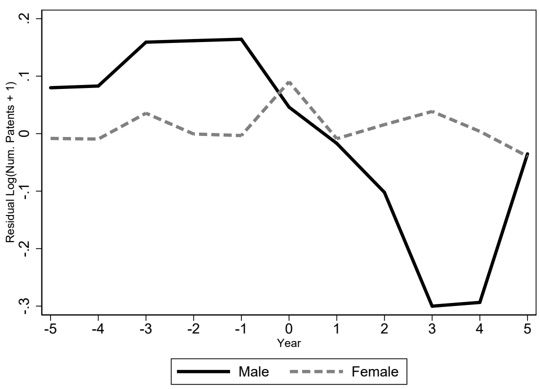
(b) *arcsinh*(Num. Patents)



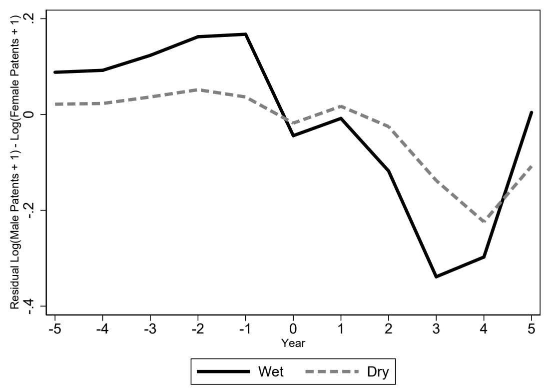
(c) Patents per Capita

Notes: Estimates of an interaction term for wet counties times a time dummy for each pair of year before and after the imposition of statewide prohibition. The x-axis shows the number of years since the enactment of state-level prohibition. The year in which state-level prohibition is enacted is normalized to year 0. Everything left of year 0 shows pre-prohibition means; everything to the right shows post-prohibition means. The y-axis plots the dependent variable. Panel (a) uses $log(Num.Patents + 1)$ as the dependent variable. Panel (b) uses $arcsinh(Num.Patents)$ as the dependent variable. Panel (c) uses $\frac{Num.Patents}{TotalPop.}$ as the dependent variable. The red horizontal line indicates the level of patenting in the base year, year 0.

Figure 3: Patenting by Females and Males



(a) Females vs. Males in Wet Counties



(b) Females - Males in Wet vs. Dry Counties

Figure 4: *Notes:* Patenting by females versus males. The x-axis shows the number of years since the enactment of state-level prohibition. The year in which state-level prohibition is enacted is normalized to year 0. Everything left of year 0 shows pre-prohibition means; everything to the right shows post-prohibition means. The y-axis plots the dependent variable. Panel (a) uses $\log(Num.Patents + 1)$ as the dependent variable and plots it separately for males and females in the previously wet counties. Panel (b) uses $\log(Num.MalePatents + 1) - \log(Num.FemalePatents + 1)$ as the dependent variable and plots it separately for previously wet and consistently dry counties.

Tables

Table 1: Dates of the Start of Prohibition and Sample Sizes in Each State

Prohibition Year	State	Referendum States		All Sechrist (2012) Data		Bastions of Wet or Dry Support	
		# Wet Counties	# Dry Counties	# Wet Counties	# Dry Counties	# Wet Counties	# Dry Counties
1852	Massachusetts			2	11		
1908	Georgia			22	3		
1909	Tennessee			4	62		
1909	North Carolina	3	57	3	57		
1909	Mississippi			5	72		
1914	Oregon	2	2	2	2		
1915	Arizona	11	1	11	1		
1916	Idaho	11	9	11	9	2	9
1916	Virginia	12	59	12	59		
1916	Washington	32	4	32	4		
1916	South Carolina	6	29	6	29		
1916	Colorado	40	13	40	13	14	9
1916	Iowa	11	66	11	68		
1917	Nebraska	55	20	55	20	5	20
1917	Michigan	31	29	31	29	1	24
1917	South Dakota	42	7	43	10	4	5
1918	Indiana			16	25		
1918	Texas			57	165		
1919	Florida	2	38	2	38	2	12
1919	Ohio	44	12	44	12		
1919	Kentucky	9	101	9	101	5	45

The years when each state adopted statewide prohibition between 1852 and 1919, along with the number of wet and dry counties in each state in each sample of the data.

Table 2: Joint Tests for Balance between Formerly Wet and Consistently Dry Counties

	Wet County	Wet County
log(Total Pop)	0.051	0.026
	(0.039)	(0.040)
Frac. Urban	0.040	0.026
	(0.031)	(0.037)
Frac. Interstate Migrant	0.678***	0.215
	(0.077)	(0.336)
Frac. Male	2.076***	1.996
	(0.588)	(1.244)
log(Manuf. Establishments)	0.070*	0.080
	(0.036)	(0.050)
log(Manuf. Output)	0.016	0.026**
	(0.014)	(0.010)
State Fixed Effects	No	Yes
F-stat	63.75	5.90

Joint tests for balance of covariates between the formerly wet and consistently dry counties in the baseline sample. In all columns, a linear probability model is estimating and the dependent variable is a dummy variable equal to one if a county is wet in the last census prior to the imposition of state prohibition. Column 2 includes state fixed effects.

Table 3: Baseline Results

	log(Patents + 1)	arcsinh(Patents)	Pat. per 100k Cap.
		Referendum States	
Wet County * Statewide Prohibition	-0.125***	-0.157***	-0.369***
	(0.031)	(0.039)	(0.103)
Statewide Prohibition	-0.015	-0.026	0.115
	(0.029)	(0.037)	(0.090)
Mean of Dep. Var.	1.415	1.765	2.443
Adj. R-Squared	0.808	0.786	0.505
Cnty-Year Obs.	6,548	6,548	6,548
# Counties	726	726	726
		All Sechrist (2012) Data	
Wet County * Statewide Prohibition	-0.078***	-0.099***	-0.233***
	(0.024)	(0.031)	(0.078)
Statewide Prohibition	-0.006	-0.014	0.094
	(0.023)	(0.029)	(0.074)
Mean of Dep. Var.	1.363	1.699	2.150
Adj. R-Squared	0.794	0.771	0.490
Cnty-Year Obs.	10,433	10,433	10,433
# Counties	1,151	1,151	1,151
		Bastions of Wet or Dry Support	
Wet County * Statewide Prohibition	-0.138*	-0.177*	-0.848**
	(0.078)	(0.100)	(0.379)
Statewide Prohibition	0.002	-0.002	0.466*
	(0.058)	(0.075)	(0.260)
Mean of Dep. Var.	1.162	1.444	2.934
Adj. R-Squared	0.786	0.770	0.392
Cnty-Year Obs.	1,194	1,194	1,194
# Counties	142	142	142

Notes: Baseline regression results. Column 1 uses $\log(Num.Patents+1)$ as the dependent variable. Column 2 uses $arcsinh(Patents)$ as the dependent variable. Column 3 uses $\frac{Num.Patents}{TotalPop.}$ as the dependent variable. Each group of rows shows results using a different subsample of the data. The first group of rows shows results from the baseline sample, which uses all counties that were wet or dry for at least five years before their state passed a prohibition referendum. The second group of rows uses all counties that were wet or dry for at least five years before Sechrist (2012) identifies the start of state prohibition. The third group of rows further restricts the baseline sample by only keeping counties that are identified as bastions of wet or dry sentiment based on their voting in the prohibition referendum. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 4: Excluding World War I

	No WWI Years	No States with WWI Proh	No States with WWI Years
Wet County * Statewide Prohibition	-0.101**	-0.115***	-0.238
	(0.041)	(0.036)	(0.208)
Statewide Prohibition	-0.036	-0.040	0.000***
	(0.038)	(0.047)	(0.000)
Mean of Dep. Var.	1.453	1.248	0.477
Adj. R-Squared	0.806	0.767	0.464
Cnty-Year Obs.	4,540	5,138	632
# Counties	725	525	58

Notes: Robustness checks of the baseline results that exclude data during World War I. The dependent variable in all columns is $\log(Num.Patents + 1)$. Column 1 drops all World War I years. Column 2 excludes all states that enacted prohibition during World War I. Column 3 excludes all states for which a post-prohibition year occurred during World War I. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 5: Social Interactions vs. Alcohol Consumption

	Bone Dry Laws	Cirrhosis Deaths per 100k
Wet County * Statewide Prohibition	-0.095**	-0.092***
	(0.044)	(0.034)
Statewide Prohibition	-0.009	-0.013
	(0.047)	(0.032)
Wet County * Bone Dry Law * Statewide Prohibition	0.045	
	(0.084)	
Bone Dry Law * Statewide Prohibition	-0.111	
	(0.070)	
Δ Cirrhosis Deaths per 100k Capita		-40.369
		(115.640)
Mean of Dep. Var.	1.415	1.415
Adj. R-Squared	0.769	0.832
Cnty-Year Obs.	5,148	5,148
# Counties	526	526

Notes: Results that check whether the decline in patenting can be explained by reductions in consumption of alcohol. The dependent variable for all columns is $\log(Num.Patents + 1)$. $BoneDryLaw$ is a dummy variable equal to one if a state enacts a bone dry prohibition law. $\Delta CirrhosisDeathRate$ is the change number of county cirrhosis deaths per 100,000 capita. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 6: Female Patenting

	log(Female Pat. + 1)	log(Male)-log(Female)	Frac. Female Pat.	Frac. Female Pat.
Wet County * Statewide Prohibition	0.030**	-0.141***	0.010*	0.013**
	(0.014)	(0.030)	(0.006)	(0.006)
Statewide Prohibition	0.003	0.069**	-0.008	-0.007
	(0.012)	(0.029)	(0.006)	(0.006)
Zero Patents				0.961***
				(0.003)
Mean of Dep. Var.	0.185	0.991	0.038	0.251
Adj. R-Squared	0.621	0.742	0.074	0.953
Cnty-Year Obs.	6,548	6,548	6,548	6,548
# Counties	726	726	726	726

Notes: Results on patents by female inventors. Column 1 uses $\log(Num.FemalePatents + 1)$ as the dependent variable. Column 2 uses $\log(Num.MalePatents+1) - \log(Num.FemalePatents+1)$ as the dependent variable. Columns 3 and 4 use $\frac{Num.FemalePatents}{Num.Patents}$ as the dependent variable. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 7: Patenting by Inventors of Different Ethnicities

	log(Saloon Eth. Pat. + 1)	log(Non-Saloon Eth. Pat. + 1)	log(Saloon Pat) - log(Non-Saloon Pat)	Frac. Saloon Ethnicity Pat.	Frac. Saloon Ethnicity Pat.
Wet County * Statewide Prohibition	-0.090**	-0.014	-0.076*	-0.069	-0.020
	(0.036)	(0.022)	(0.040)	(0.064)	(0.018)
Statewide Prohibition	0.149***	0.028	0.120***	0.099	0.029
	(0.046)	(0.027)	(0.046)	(0.061)	(0.020)
Zero Inventors with Ethnic Names					0.198***
					(0.015)
Mean of Dep. Var.	0.385	0.131	0.254	0.802	0.925
Adj. R-Squared	0.552	0.395	0.298	0.202	0.217
Cnty-Year Obs.	3,156	3,156	3,156	872	3,156
# Counties	594	594	594	240	594

Notes: Results on patents by different ethnic groups. Column 1 uses $\log(Num.PatentsBySaloonEthnicities + 1)$ as the dependent variable. Column 2 uses $\log(Num.PatentsByNonSaloonEthnicities + 1)$ as the dependent variable. Column 3 uses $\log(Num.PatentsBySaloonEthnicities + 1) - \log(Num.PatentsByNonSaloonEthnicities + 1)$ as the dependent variable. Columns 4 and 5 use $\frac{Num.PatentsBySaloonEthnicities}{Num.Patents}$ as the dependent variable. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 8: Substitutes for the Saloon

	Pop.	Barbers	Restaurant Workers
Wet County * Statewide Prohibition	-0.242***	-0.311***	-0.213*
	(0.081)	(0.077)	(0.107)
Statewide Prohibition	0.014	0.042	0.020
	(0.072)	(0.066)	(0.085)
Wet County * Statewide Prohibition * log(Barber + 1)	0.037		
	(0.022)		
Statewide Prohibition * log(Barber + 1)	-0.013		
	(0.020)		
Wet County * Statewide Prohibition * log(Rest. Workers + 1)		0.051**	
		(0.019)	
Statewide Prohibition * log(Rest. Workers + 1)		-0.022	
		(0.017)	
Wet County * Statewide Prohibition * log(Clergy + 1)			0.029
			(0.032)
Statewide Prohibition * log(Clergy + 1)			-0.013
			(0.028)
Mean of Dep. Var.	1.419	1.419	1.419
Adj. R-Squared	0.805	0.805	0.805
Cnty-Year Obs.	6,473	6,473	6,473
# Counties	757	757	757

Notes: Results on the mitigation of the treatment effect when substitutes for the saloon are readily available. The dependent variable in all columns is $\log(Num.Patents + 1)$. Column 1 interacts the treatment effect with $\log(Num.Barbers + 1)$ in the last decennial census prior to state prohibition. Column 2 interacts the treatment effect with $\log(Num.RestaurantWorkers + 1)$ in the last census prior to state prohibition. Column 3 interacts the treatment effect with $\log(Num.Clergy + 1)$ in the last census prior to state prohibition. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 9: Collaborative Patents

	log(Num. Inventors + 1)	Inventors per Pat	Inventors per Pat	Frac. Pat with Multiple Inventors	Frac. Pat Multiple Inventors
Wet County * Statewide Prohibition	-0.139***	-0.022*	-0.016*	-0.020***	-0.014*
	(0.032)	(0.013)	(0.009)	(0.008)	(0.008)
Statewide Prohibition	-0.010	0.008	0.011	0.005	0.008
	(0.031)	(0.015)	(0.010)	(0.008)	(0.008)
Zero Patents			-0.082***		0.925***
			(0.007)		(0.006)
Mean of Dep. Var.	1.457	1.080	1.063	0.058	0.271
Adj. R-Squared	0.799	0.061	0.059	0.036	0.894
Cnty-Year Obs.	6,548	4,463	6,548	6,548	6,548
# Counties	726	659	726	726	726

Notes: Results on collaborative patent. Column 1 uses $\log(Num.Inventors + 1)$ as the dependent variable. Columns 2 and 3 use $\frac{Num.Inventors}{Num.Patents}$ as the dependent variable. Columns 4 and 5 use $\frac{Num.Patentswith{>}1Inventor}{Num.Patents}$ as the dependent variable. Columns 5 and 6 use $\frac{Num.Patentswith2Inventors}{Num.Patents}$ as the dependent variable. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 10: Persistent Effects of Prohibition

(a)

	log(Prev. Inventor Pair + 1)	Frac. with Prev. Inventor Pair	Log(Lead Pat. Class + 1)	Frac. Lead Pat. Class
Wet County * Statewide Prohibition	-0.112***	-0.354	-0.123***	-0.002**
	(0.040)	(0.329)	(0.027)	(0.001)
Statewide Prohibition	0.171***	0.438	0.074***	0.001
	(0.051)	(0.582)	(0.024)	(0.001)
Mean of Dep. Var.	0.329	2.278	0.328	0.092
Adj. R-Squared	0.683	0.654	0.584	0.997
Cnty-Year Obs.	3,156	650	6,548	6,548
# Counties	594	191	726	726

(b)

	Log(Lead Pat. Class + 1)	Pat. Class	log(Prev. Inventor Pair + 1)	Prev. Inventor Pair
Wet County * Statewide Prohibition (First 3 Years)	-0.113**	-0.117	-0.125***	-0.002*
	(0.050)	(0.492)	(0.028)	(0.001)
Wet County * Statewide Prohibition (Next 3 Years)	-0.090*	-0.199	-0.122***	-0.003**
	(0.049)	(0.469)	(0.031)	(0.001)
Statewide Prohibition (First 3 Years)	0.194***	0.203	0.081***	0.000
	(0.059)	(0.699)	(0.028)	(0.001)
Statewide Prohibition (Next 3 Years)	0.271***	1.037	0.096**	-0.002
	(0.097)	(1.540)	(0.044)	(0.002)
Mean of Dep. Var.	0.353	2.679	0.328	0.092
Adj. R-Squared	0.685	0.647	0.584	0.997
Cnty-Year Obs.	3,156	650	6,548	6,548
# Counties	594	191	726	726

(c)

	log(Patents + 1)	arcsinh(Patents)	Pat. per 100k Cap.
Wet County * Statewide Prohibition (First 3 Years)	-0.149***	-0.188***	-0.397***
	(0.032)	(0.041)	(0.103)
Wet County * Statewide Prohibition (Next 3 Years)	-0.062	-0.078	-0.282*
	(0.044)	(0.056)	(0.163)
Statewide Prohibition (First 3 Years)	-0.010	-0.020	0.081
	(0.031)	(0.040)	(0.097)
Statewide Prohibition (Next 3 Years)	-0.063	-0.087	-0.101
	(0.051)	(0.065)	(0.150)
Mean of Dep. Var.	1.415	1.765	2.443
Adj. R-Squared	0.808	0.786	0.505
Cnty-Year Obs.	6,548	6,548	6,548
# Counties	726	726	726

Notes: Results on the persistence of the effects of prohibition. $StatewideProhibition$ is a dummy variable equal to one in years after a state has enacted statewide prohibition. $WetCounty * StatewideProhibition$ is an interaction term equal to one for counties that were previously wet for at least five years before a state enacts statewide prohibition. Standard errors clustered by state and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$