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

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Abstract

To understand the importance of informal social networks for invention, I examine one of the largest involuntary disruptions of social networks in 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 largest in the first three years after the imposition of prohibition and rebounds thereafter. The effect was smaller for groups that were less likely to frequent saloons, namely women and particular ethnic groups. I present evidence that the effect was driven by the disruption of social interactions and rule out alternative explanations. I next use the prohibition experiment to document several facts. I show that the social network increases invention through exposure to ideas in addition to exposure to collaborators and that informal and formal connections are complements in the invention production function. Finally, I show that the social network exhibits path dependence in the sense that as individuals rebuilt their social networks following prohibition, they connected with new individuals and patented in new technology classes. While prohibition had only a temporary effect on the rate of invention, the fact that the post-prohibition network exposed individuals to different ideas means that prohibition had a lasting effect on the direction of inventive activity.

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1 Introduction

What is the role of informal social interactions in invention? Scholars in many different fields recognize that interpersonal communication is important for the creation of new ideas, from urban economics (Glaeser, Kallal, Scheinkman, and Schleifer (1992), Saxenian (1996), Glaeser (1999)) to economic growth (Lucas (2009), Lucas and Moll (2014), Fogli and Veldkamp (2016), Akcigit, Caicedo, Miguelez, Stantcheva, and Sterzi (2018)) to sociology (Burt (2005), Ferrary and Granovetter (2017)). But estimating the causal effects of informal social interactions on the rate and direction of inventive activity, let alone understanding the role of network structure and the response of potential innovators to shocks to the network, have proven difficult. As Breschi and Lissoni (2009, p. 442) put it, "the role of social ties as carriers of localized knowledge spillovers has been more often assumed than demonstrated."

In this paper, I answer these questions by investigating one of the largest involuntary disruptions of social networks in U.S. history: alcohol prohibition. Prior to the enactment of prohibition laws, the saloon acted as a social hub in which individuals could exchange information in an informal setting (e.g., Moore (1897), Calkins (1919), Sismondo (2011)).[1] 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 patent grants, changed following the

[1] I describe the history of saloons' social role in much more detail in Section 2 below. Scholars have noted the role of bars in bringing creative people together in more recent times (Oldenburg (1989), Florida (2002b)). Examples of inventions conceived in bars are plentiful. A large part of the modern computer industry emerged out of an informal group that met at The Oasis bar and grill (Wozniak (1984), Balin (2001), Farivar (2018)), and numerous innovations were initially sketched on bar cocktail napkins, from the first automatic electronic digital computer and MRI machines to the "Farrington B" credit card font and Discovery Channel's Shark Week (see `https://www.networkworld.com/article/2220218/ethernet-switch/napkins--where-ethernet--compaq-and-facebook-s-cool-data-center-got-their-starts.html` and `https://www.eandvgroup.com/the-cocktail-napkin-hall-of-fame/`).

prohibition-induced disruption. Prohibition in the U.S. 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 went into effect, counties that were previously wet saw a disruption of their saloon-based social networks, while the counties that were already dry did not, providing a natural control group.

The imposition of prohibition caused patenting to drop by 8-18% in the counties that wanted to remain wet relative to consistently dry counties in the same state, depending on the specification used. While patenting fell dramatically in the years immediately after prohibition went into effect, it rebounded over time, consistent with a story in which individuals gradually rebuilt their informal social networks. 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. I present further evidence that the observed effects were driven by the decline in social interactions rather than by alternative channels such as a decline in alcohol consumption, a general economic slump, or differential migration patterns.

I next use the prohibition natural experiment to shed light on several properties of the invention production function and the network formation process. First, I show that networks are important for innovation because they facilitate the exposure to new ideas, rather than simply making it easier for individuals to find collaborators. To show this, I document a decline in both solo-inventor patents and patents with multiple inventors. If networks were only useful to find collaborators, then solo-inventor patents should see no decline. Second, I document that informal and formal connections in a social network are complementary for the creation of new ideas. Prohibition disrupted only informal connections taking place in the saloon, yet patents assigned to firms, which depend at least in part on the formal connections between coworkers or between employers and employees, also exhibited a decline following prohibition. Finally, I document that the structure of the network is path dependent. When individuals rebuilt their social networks following prohibition, they did not collaborate with the same individuals as they did prior to prohibition. This is also manifested in a long-lasting change in the types of inventions individuals created, as measured by patent classes. Thus, disrupting the informal social network had permanent effects on the direction, if not the rate, of innovative activity.

In light of these findings, this paper contributes to three literatures. First, the paper contributes to the literature on the economics of innovation and technical change by showing that informal social interactions are quantitatively important for invention and documenting how information flows through a social network affect the direction of innovation and the identities of innovators. 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. Third, this study builds on the literature examining the quantitative effects of prohibition (e.g., Dills and Miron (2004) on the effect of prohibition on cirrhosis deaths, Owens (2014) 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). Like the studies on infant health, invention is particularly intriguing to study because it represents an outcome that was unintentionally affected by prohibition; policymakers and activists had no desire to alter the rate or direction of inventive activity when pushing for prohibition laws. These results moreover likely understate the effect of prohibition's disruption of the social network; while invention is a readily observable outcome, social networks are valuable for many other reasons as well (Putnam, 2000).

The prior literature has struggled to estimate the causal effect of informal social interactions on invention because of three major challenges, related to identification, size, and data. Identification poses a challenge because social network structure is endogenous (Manski, 1993). Individuals have a great deal of control over their social interactions, choosing where they live, which watering holes to frequent, and who to talk to once they get there. Size poses a challenge because invention is a relatively rare event that often occurs with a lag, and so a large and long-lasting intervention is needed to study the effect of social interactions on invention. Finally, data availability is a challenge because microdata on social interactions and social network structure is often difficult, costly, or impossible to collect. The prohibition setting allows me to overcome and circumvent each of these challenges.

To resolve the identification challenge, I exploit the fact that prohibition was imposed on counties by the state in a way that was orthogonal to the preexisting social network and to other county characteristics that might have been correlated with invention outcomes. A particular threat to identification is that counties' views towards prohibition were a manifestation of deeper social and cultural conservatism that in turn affected openness to new ideas (Bénabou, Ticchi, and Vindigni (2016), Vakili and Zhang (2018)). To overcome this concern, I leverage the political economy of the prohibition movement and, in the baseline specifications, restrict attention to counties that had consistent views towards prohibition over time. More specifically, I use a differences-in-differences framework to compare counties that were wet prior to the passage of state prohibition laws and wanted to remain wet to counties that were consistently dry and wanted to remain dry before and after the passage of state prohibition. Counties that had changing views on alcohol are omitted from the analysis. I consider a number of alternative sample specifications to ensure that underlying attitudes towards the bar remained constant across time in the treated and control counties, finding consistent results.

Prior studies that have overcome the identification challenge typically study relatively small shocks to social networks, making it difficult to study innovation as an outcome. For example, randomized trials in the development literature (e.g., Conley and Udry (2010), Banerjee, Chandrasekhar, Duflo, and Jackson (2013)) manipulate the introduction of information to a network for small numbers of villages and observe them at only a few points in time. Studies in the education literature that exploit random assignment of peers (Sacerdote (2001), Carrell, Sacerdote, and West (2013)) likewise examine relatively small numbers of students and for limited periods of time.[2] Prohibition, in contrast, represents a massive intervention, resolving the size challenge and allowing me to study innovation as an outcome. Bars and saloons were enjoyed by a large swaths of the population prior to prohibition, and prohibition laws stayed in effect for years. Prohibition laws affected large geographic areas as well: my baseline sample consists of 15 states that adopted prohibition laws between 1909 and 1919.

When existing studies do examine how changes in the social network structure affect innovative studies, they typically examine groups that are at high risk of innovation, often highly skilled scientists. For instance, Moser and San (2019) examine the inflow of scientists

[2] Carrell, Hoekstra, and Kuka (2018) is an exception in tracking the long-run effects of random exposure to peers, although it still examines a limited sample of students and does not investigate innovation as an outcome. Other studies that use deaths (Hobbs & Burke, 2017) or natural disasters (Elliott, Haney, and Sams-Abiodun (2010), Phan and Airoldi (2015), Morris and Deterding (2016)) to study social network disruption and reconstruction have similar limitations, frequently only tracking network changes for a relatively short time after the disruptive shock. They also do not examine innovation as a potential outcome.

to the U.S. following the passage of immigration quotas in the 1920s; Moser, Voena, and Waldinger (2014) examine the inflow of German Jewish scientists to the U.S. following the rise of Nazism in the 1930s; Waldinger (2010) and Waldinger (2012) examine the outflow of German scientists during the same period, Borjas and Doran (2012) and Ganguli (2015) examine 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) examine the death of scientists as a natural experiment that disrupts scientists' social networks. The prohibition natural experiment differs from these studies in that I can study innovation in the entire population, including observing individuals' entry into innovative activities.[3] In addition, prohibition is a "cleaner" setting in which to study the effects of social interactions on innovation, 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.

While I do not have detailed microdata on the informal social network–that is, I cannot observe exactly which individuals were going to bars before prohibition, who they were talking to, or what they were talking about–I do know a great deal about the types of individuals affected by prohibition, and I am able to use this information to infer a great deal about the role of informal social networks for invention. As noted above, I show that patenting dropped the most following prohibition for the groups that were most exposed to the saloon prior to prohibition. I build a simple and general model of a social network with a dynamic structure similar to Watts (2001) that captures the role of the saloon in facilitating easy informal communication between nodes in the network. I then derive a set of reduced

[3]An additional benefit of observing patenting as an outcome is that it is free of congestion. In contrast, space in academic journals, which is used as an outcome in many studies (e.g., Borjas and Doran (2012)) may be limited, and so total publications may not change much to shocks to the social network. See Moser et al. (2014) for a discussion of this point.

form predictions to test for the presence of several properties of the invention production function and the network formation process. In particular, I use data on solo-authored patents and collaborative patents to show that the social network benefits individuals by exposing them to new ideas (Hasan & Koning, 2019), in addition to likely providing the opportunity to find collaborators (Boudreau et al. (2017), Catalini (2018)). By observing how patenting within the firm changes after prohibition relative to patenting outside of firms, I show that formal and informal interactions are complements in the invention production function. Finally, by comparing collaborations in the pre-prohibition network to the post-prohibition network, I show that the network exhibits path dependence, meaning that as individuals rebuild their networks, they are connecting with new people and in new ways, in turn being exposed to different ideas than they were previously.

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 presents a simple theoretical model to illustrate the role of the bar in facilitating the exchange of information over a social network. Section 4 describes the data. Sections 5-8 present the results. Section 5 presents the baseline results and argues that they are driven by a disruption of social interactions. Section 6 documents the importance of the exposure to ideas, rather than simply exposure to collaborators, for invention. Section 7 documents that different sources of ideas are complements in the invention production function. Section 8 shows that the network structure exhibits path dependence and that this matters for the direction of invention. Section 9 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. The term "symposium" originated in ancient Greece to describe communal drinking sessions (Lynch, 2011). Pubs and taverns were the primary social gathering place in England for both the high and low classes into the late 17th century.[4] 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. After the expansion of coffeehouses, pubs were no longer the primary meeting place for intellectuals, but they were still important as the site for commoners to debate political and religious ideas. As Hailwood (2014, p. 74) writes, "[the alehouse] was an arena of vibrant political expression with greater social depth and geographical breadth than the coffeehouse would ever achieve."

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 Williamsburg's Raleigh Tavern, Winchester's Black Horse Inn, Boston's Green Dragon Tavern, and Philadelphia's City Tavern.[5] 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

[4] In one famous example, London's George's Inn was the preferred watering hole for London's literary elite over several centuries, being frequented by Chaucer, Shakespeare, and Dickens (Brown, 2014).

[5] See Sismondo (2011) and Cheever (2015) for more information on drinking patterns in colonial America.

(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 served the same social functions that taverns had in earlier eras. Of the ubiquity of the saloon across American culture in the decades immediately proceeding prohibition, George Ade writes:

> Not one-half of one per cent of the male population belonged to clubs. The church could not compete with the saloon as a social center because it was about as cheerful as a mausoleum while the place on the corner reeked with the kind of unrestrained gayety which has been in partnership with original sin since the beginning of history...The saloon gave boisterous welcome to every male adult, regardless of his private conduct, his clothes, his manners, his previous record..The saloon was the rooster-crow of the spirit of democracy (Ade, 1931, p. 99-100).

The social role of saloons was especially valuable for a nation with high 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 via 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 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).[6]

Moreover, 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 workers in particular professions, 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

[6]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).

"informal employment bureaus" (Powers, 1998, p. 54). Nor were drinking establishments exclusively for the workers. The German lager beer gardens, which became popular in the second half of the 19th century, were egalitarian locales where employers drank with their workers and professionals from various fields interacted (Oldenburg, 1989, p. 96-97). 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. Indeed, in 1910 the top ten most common inventor occupations included laborers, machinists, carpenters, drivers, manufacturers, and painters.[7]

The importance of these social and informational benefits of the saloon are not simply an invention of recent social historians, but were well understood by contemporaries; in addition to London (1913), see Moore (1897) and Calkins (1919).[8] 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., G. K. Chesterton remarked:

> The cinema boasts of being a substitute for the tavern, but I think it is a very bad substitute. I think so quite apart from the question about fermented liquor. Nobody enjoys cinemas more than I, but to enjoy them a man has only to look

[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.

[8] 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, the place where their philosophy of life is worked out and from which their political and social beliefs take their beginning. As an educational institution its power is very great and not to be scorned because skilled teachers are not present, for they teach themselves."

and not even to listen, and in a tavern he has to talk (Chesterton, 1922, p. 88).

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, 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).[9] 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 and 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

[9] One of the few exceptions in which prostitution appears to have been relatively commonplace was in New York after the passage of the Raines Act in 1896. The Raines Act stipulated that alcohol could only be served on Sunday in places with at least ten beds. Predictably, this led to saloons subdividing their back rooms into numerous "hotel rooms," turning otherwise legitimate saloons into brothels (Gilfoyle, 1992, p. 303-306).

movements had promoted voluntary abstinence or moderation. A new wave 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 Lewis (2008) argues, 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.

America's entry into World War I proved to be a valuable opportunity for prohibitionists. With Germans so closely associated with the brewing industry (Pabst, Schlitz, and Anheuser-Busch being a few prominent examples), it was easy to smear beer consumption as an act that aided the enemy (Okrent, 2010, p. 85-87). In the spring of 1917, President Wilson established an emergency Food Commission under the direction of Herbert Hoover which, in an effort to maximize the harvest to be shipped to Europe and feed Allied troops, dictated what grains were to be planted and what they could be used for (Paxson, 1920, p. 60-61). The actions of the Food Commission were officially sanctioned by Congress with the passage of the Lever Act on August 10, 1917 which, among other things, prohibited the production of spirits and severely limited the production of beer by reserving grains for food production. 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). Thus de facto national-level prohibition was in effect in 1917. A formal alcohol prohibition amendment to the U.S. constitution was introduced in 1917, and in 1918 an additional wartime prohibition act was passed (Tyrrell, n.d.). Contemporary sources regarded the wartime prohibition as quite effective: 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. The crowning achievement of the prohibition forces was the ratification of the 18th Amendment to the U.S. Constitution on January 16, 1919. The ensuing Volstead Act, passed on October 27, 1919, provided for federal enforcement of the prohibition amendment, stipulating that "no person shall manufacture, sell, barter, transport, import, export, deliver, or furnish any intoxicating liquor except as authorized by this act."

Once enacted, prohibition, at both the state and federal level, ended the legal operation of the saloons, indiscriminately removing their ability to prey on a possibly vulnerable working class as well as eliminating their role as social hubs. While little work examines the social impact of statewide prohibition, accounts of national prohibition give a flavor of how people responded. Anecdotally, 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."

One of the most important initial effects of prohibition was to shift drinking into the home. Consumption within the home, even if among friends, was unlikely to introduce individuals to the diversity of ideas they were exposed to in the bar. Over time, speakeasies appeared, but the transition was not immediate. Additionally, speakeasy culture was not initially conducive to exchanging and spreading ideas. Drowne (2005, p. 96) traces the origin of the word "speakeasy" to Mencken, who derives it from the Irish phrase "speak softly shop," in which drinkers literally kept their voices low to avoid detection of the authorities.[10] Such an arrangement is unlikely to be conducive to the vigorous exchange of ideas across people, particularly in the years immediately after the passage of prohibition when it was unknown how widely the prohibition laws would be flaunted.

Just how disruptive were these changes to individuals' social interactions? Hard data is difficult to come by. In Appendix B, I present results from a sample of county and city directories and shows that the vast majority, just under 90%, of addresses that housed saloons before statewide prohibition went into effect sat vacant in post-prohibition years. Thus, it

[10] Ade (1931, p. 161) explicitly notes the contrast between the speakeasy and the pre-prohibition saloon, writing "I do know that if 'speak-easy' means a place in which conversation is hushed and subdued, it bears no resemblance to the old-time saloon."

does not appear that saloons were simply able to reconstitute themselves as restaurants or other "third places" (Oldenburg, 1989) so that individuals could easily maintain their social networks. This is consistent with studies of former saloon properties following national prohibition (McGirr, 2016, p. 48). In short, it appears unlikely that the establishments that replaced the legal saloon provided a similarly lively social environment, especially right away.

3 A Simple Model of Invention in a Social Network

3.1 Invention and Co-Invention in a Social Network

Consider the set of nodes $N = \{1, 2, ..., N\}$. Define the network g as the set of links over all nodes. The distance between any two nodes $i, j \in N$ is the shortest path between i and j and is given by $d(i,j) = len(\{ia\}, \{ab\}, \{bc\}, ...\{zj\})$. If no path connects i and j, then $d(i,j) = \infty$. Trivially define $d(i,i) = 1$.

I assume that the contributions of each node j to i's invention outcome are additively separable.[11] Then, the expected number of invention produced by each node i is given by

$$f_i^{Solo}(g) = f(d(i,1), d(i,2), ..., d(i,N)) = f(d(i,j)|j \in N). \tag{1}$$

The above give expressions for inventions developed by each node i. But in many cases, inventions are created by multiple parties. The key assumption is that to co-patent, two nodes i and j must be immediately connected; that is, it must be the case that $d(i,j) = 1$.

[11] That is, conditional on its distance and perhaps individual characteristics, each node j affects i's invention in a linear fashion. This is not to say that each node i adds connections in such a linear fashion; see Section 3.3 for a discussion of the network formation decision.

Define the set $D_i^1 = \{1^1, 2^1, ..., M^1\} = \{j \in N | d(i,j) = 1\}$ to be the set of nodes that are immediately connected to i. For simplicity, I only consider inventions with two inventors, although this can easily be extended to cases with more inventors. The number of co-inventions involving each node i is given by

$$f_i^{Collab}(g) = h(D_i^1, g). \tag{2}$$

The inclusion of g as an argument reflects the fact that, for each i and $j \in D_i^1$, the potential contribution from both i and j to the collaboration may in turn depend on their overall network position, as in Equation (1).

Assume that for a given network g^B, every pair of nodes i, j can be of one of two types: $Formal$ and $Informal$. Intuitively, formal links represent connections such as those with coworkers in the same firm, researchers who coauthor with one another, or those who otherwise collaborate in an official capacity; these are the kinds of connections that are typically measured in other work. Informal links, on the other hand, capture the types of interactions that are not governed by formal or official channels, such as friends or acquaintances.[12] The $Formal$ links are a subset of the immediately connected nodes and define $D_i^{1,Formal} = \{j \in D_i^1 | \text{link between } i \text{ and } j \text{ is } Formal\}$; assume all other links are $Informal$. Equations (1) and (2) can then be rewritten as

$$f_i^{Solo}(g) = f(d(i,1), d(i,2), ..., d(i,N), D_i^{1,Formal}, D_i^{Informal}) \tag{3}$$

[12] Formal and informal connections should be conflated with strong and weak ties (Granovetter, 1973). Informal connections such as close friends may be incredibly strong ties, while individuals in the same firm may interact only infrequently.

and

$$f_i^{Collab}(g) = h(D_i^1, g, D_i^{1,Formal}, D_i^{Informal}), \tag{4}$$

indicating that inventive output may further depend on which links are formal and informal.

The total number of expected inventions in the network g is given by

$$f(g) = \sum_{i \in N} \left[f_i^{Solo}(g) + \frac{1}{2} f_i^{Collab}(g) \right]. \tag{5}$$

Obviously this expression is quite general. In Section 3.5 below, I discuss ways in which the imposition of alcohol prohibition can be used to draw some conclusions about the functional form of $f(\cdot)$.

3.2 Modeling the Bar

I next formalize the idea that bars act as social hubs in which people can easily come together and interact. To do this, I introduce a second type of node, call it B for the bar. Then the set of nodes in the modified setting is $N^B = \{1, 2, ..., N, B\}$ and the corresponding network is given by g^B. I model the idea that bars allow nodes to easily interact by defining the bar-facilitated distance between all nodes to be given by

$$d^B(i,j) = \begin{cases} d(i,j) - 1 & \text{if } B \text{ is in the path of } i, j \\ d(i,j & \text{otherwise} \end{cases} \tag{6}$$

I further assume that the bar plays no direct role in invention aside from bringing other nodes closer together. So,

$$f_B^{Solo}(g^B) \equiv f_B^{Collab}(g^B) \equiv 0 \tag{7}$$

With these definitions, I redefine total invention with the bar as

$$\begin{aligned} f(g^B) =& \sum_{i \in N} \left[f_i^{Solo,B}(g^B) + \frac{1}{2} f_i^{Collab,B}(g^B) \right] \\ =& \sum_{i \in N} \Big[f(d^B(i,1), d^B(i,2), ..., d^B(i,N), D_i^{1,Formal}, D_i^{Informal}) \\ & \qquad + \frac{1}{2} h(D_i^{1B}, g^B, D_i^{1,Formal}, D_i^{1,Informal}) \Big], \end{aligned} \tag{8}$$

where $D_i^{1B} = \{1^{1B}, 2^{1B}, ..., M^{1B}\} = \{j \in N | d^B(i,j) = 1\}$. As before, invention may depend on whether links are formal or informal. Note that formal links only occur between i and j if $d(ij) = 1$ (and *not* if $d^B(ij) = 1$), and therefore the link between i and j is of informal if B is in the path of i, j; any connection facilitated by the bar must be informal.[13]

3.3 Network Formation

Time is discrete and indexed by $t = \{1, 2, ...\}$ and g_t and g_t^B represents the network at time t, with and without the bar, respectively. At every time, a potential link ij is drawn from some probability distribution. As in Watts (2001), each node in the potential link must myopically decide whether or not to create the link based on a match-specific payoff and a

[13] Assume that a link between i and j is *Formal* if $j \in D_i^{1B,Formal}$ and $j \in D_i^{1B}$. In other words, if two nodes are linked through both formal channels and the bar, assume that they are formal. This can be relaxed without substantially altering any of the following intuition.

cost of link formation. First, consider the case in which there is no B-type node.

If i and j decide to link, i receives match-specific utility μ_{ij} and j receives μ_{ji}. Assume that μ_{ij} is drawn from some distribution and determined at time 0 for all i, j. To form a link, both nodes must pay a one-time cost that may depend on the number of immediate connections for each node in the potential link, $c(\#_{D_i^1})$, where $\#_{D_i^1}$ is the number of nodes in D_i^1.[14] Then, a link occurs if

$$\mu_{ij} - c(\#_{D_i^1}) \geq 0 \tag{9}$$

and

$$\mu_{ji} - c(\#_{D_j^1}) \geq 0. \tag{10}$$

Because the cost is paid only when the link forms, a stable network g^* trivially exists. Call the time at which the stable network forms t^*.[15]

Now consider the full set of nodes given by N^B. If the candidate link is between i and B, i gets the benefit of connecting with all nodes immediately connected to B, while only paying

[14] One could equivalently model degree-dependence in the utility term, $\mu_{ij}(\#_{D_i^1(g)})$. One could further modify this simple structure in several ways without affecting the main results. For instance, it may be desirable to make the benefit of link formation depend on each node's inventiveness, $\mu_{ij}(f_i(g), D_i^1(g))$. In addition to simplifying the analysis, I argue that having the link formation decision be independent of $f_i(g_t)$ captures the fact that individual nodes in a network are unlikely to be aware of the larger network structure, as documented by Friedkin (1983), Krackhardt (1987), Krackhardt (2014), and Banerjee, Chandrasekhar, Duflo, and Jackson (2014). In addition, invention is such a rare event that the expected marginal increase in the number of inventions resulting from a given change in network structure is likely to be negligible relative to the other costs and benefits of forming a connection. Nevertheless, the properties of the network that I highlight are still present if agents take invention outcomes into account when deciding when to form connections, although the analysis is greatly complicated.

[15] All of the below results hold if instead the cost must be paid each period and so nodes must decide whether to maintain existing links. This admits the possibility of cycles (Watts, 2001) and so complicates the intuition.

the cost to connect once. More formally, i receives $\mu_{iB}^B = \mu_{iB} + \sum_{j \in D_B^1/D_i^1} \mu_{ij}$, where D_B^1/D_i^1 is the set of immediate connections of B that were not already immediate connections of i, and i must pay a cost $c(D_i^1 + 1)$. Note that this cost depends on D_i^1 and not on D_i^{1B}; i does not need to pay a cost for the additional nodes for which B facilitates the connection. Allow the bar to face a different cost than other nodes and assume that $\mu_{Bi} > 0, c(D_B^1) \; \forall \; i$, so that B always wants to connect with other nodes. Intuitively, interpret this as each individual connecting with B provides a stream of revenue to the bar that more than offsets the cost; for simplicity ignore the possibility that the bar may become congested, although this complication does not affect the main intuition of the model. Finally, to ensure that at least one node wants to connect with the bar even in the absence of the bar's connection benefit, assume that there exists at least one i such that $\mu_{iB} \geq c(N)$. Refer to the stable network with B-type nodes g^{B*}.

3.4 Modeling Prohibition

I model prohibition as an event that removes any B-type nodes, which also eliminates all of B's immediate connections. This increases the distance between any pair of nodes i, j such that B was in the path of i and j.

To see the effects of prohibition, consider a stable network at some time $t \geq t^*$. Without loss of generality, call the time at which prohibition is imposed $t^* + 1$. Then for all $i, j \in N$, $d_{t^*}(i,j) \geq d_{t^*+1}(i,j)$. Since all formal links are between nodes that are immediately connected, $D_i^{1,Formal} \subset D_i^1 \; \forall i$, prohibition only increases the distance between nodes that are informally linked.

As nodes form connections following prohibition, eventually a new stable network exists. Call this network g^{**} and the time at which it forms t^{**}.

3.5 Identifying Network Properties

The imposition of prohibition changes the network structure. Even in cases in which this structure is unobservable (that is, when it is impossible to observe links between individual nodes), by observing how aggregate invention (given by $f(g_t)$) changes from t^* to t^*+1 to t^{**}, it is possible to infer a great deal about how information moves through social networks to produce inventions.

If invention falls following prohibition, that is, if $f(g_{t^*+1}) < f(g^B_{t^*})$, then one immediate implication is that the structure of the social network has an effect on invention outcomes.[16] In other words, I can reject the hypothesis that $f_i(g) = \delta_i \; \forall i$. In fact the imposition of prohibition reveals more than that. Because distance increases only for informal links, if invention falls following prohibition, then it must be that $\frac{\partial f_i}{d(i,j)} < 0$ for some i and j such that $j \in D^{1,Informal}_{i,t^*}$. Or, in other words, a decline in invention following prohibition implies that informal interactions matter for invention.

Figure 1 illustrates the behavior of the social network at t^* (Panel (a)), t^*+1, (Panel (b)), and t^{**} (Panel (c)). Clearly the B-type nodes, represented as the large blue node, is an important focal node for the network under this parameterization. Removing B and all of its immediate connections results in a much sparser network, with the distance between many nodes increases and no path existing between a number of nodes that were formerly

[16] This assumes, of course, that prohibition has an immediate effect on invention only through disrupting the social network. I argue this in Section 5 below.

connected. Over time, nodes form connections until a new stable network is reached. In the new stable network, numerous dyadic connections replace a single connection that occurred through B. Intuitively, and loosely speaking, one could say that $g_{t^{**}}$ more closely resembles a complete graph whereas g_{t^*} more closely resembled a star graph (Jackson, 2008; Jackson & Wolinsky, 1996), with the bar serving as the central node. Moreover, there are several nodes for which it was worthwhile to connect to B in g^* but for whom it is not worthwhile to pay the cost for numerous dyadic connections. Figure 2 plots the total expected number of inventions in the network over time under particular functional form assumptions for $f(g)$, with the most important assumption being that $\frac{\partial f_i}{d(i,j)} < 0 \ \forall \ i, j \in N$. The fact that total inventions at t^{**} are less than those at t^* reflects the fact that, without B, the new stable network may still be less dense than the previous stable network.

Prohibition also allows for the examination of three additional properties that are worth exploring in more detail. The first two are properties of the invention production function $f(g)$, while the third is a property of the network formation process.

3.5.1 Exposure to Ideas and Exposure to Collaborators

As Equation (5) makes clear, the social network may affect invention both by exposing individuals to collaborators necessary to execute ideas, as well as to ideas that aid in the conception of an idea. Borjas and Doran (2015) draw out this distinction as well, identifying knowledge spillovers in "idea space" and "collaboration space." If solo-authored patenting falls following the imposition of prohibition, then the hypothesis that social networks operate exclusively through providing exposure to potential collaborators is rejected. More formally, if $f_i^{Solo}(g^{**} - f_i^{Solo}(g^{B*}) \neq 0$, then $\frac{\partial f_i^{Solo}}{d(i,j)} \neq 0$ for at least some $i, j \in N$.

3.5.2 Complementarities between Nodes

The invention production functions given by Equations (5) and (8) are flexible enough to encompass different assumptions about complementarity or substitutability between different nodes in the network for the production of inventions. Complementarity occurs if there are increasing returns: the number of inventions an individual creates increase at an increasing rate with proximity to the other nodes in the network, with an "O-ring" invention production function (Kremer, 1993) being an extreme case. Substitutability is exemplified by the "co-author" model of Jackson and Wolinsky (1996) in which individuals must divide a fixed amount of time among all connections, and so the marginal value of decreasing the distance to a given node decreases with the proximity to other nodes. Of course, the contributions of nodes may be independent of one another, in which case the contribution of each node is additively separable.

The invention production function for each i can be rewritten to emphasize the possibly distinct role of informal and formal connections as

$$f_i(g) = f(h^F(D_i^{1,Formal}), h^I(D_i^{Informal})). \tag{11}$$

The derivative of this invention production function with respect to the distance to informal nodes $j \in D_i^I$ is given by

$$\frac{\partial f_i(g)}{\partial d(i,j)} = \frac{\partial f}{\partial h^{Formal}} \frac{\partial h^{Formal}}{\partial d(i,j)} + \frac{\partial f}{\partial h^{Informal}} \frac{\partial h^{Informal}}{\partial d(i,j)}. \tag{12}$$

Assuming $\frac{\partial f}{\partial h^F} > 0$, so that more ideas through formal channels increases total invention, the sign on $\frac{\partial h^F}{\partial d(i,j)}$ reveals whether formal and informal connections are complements, substitutes, or independent of one another. Recall that prohibition changes only the distance to informal connections, $d(i,j)$ for $j \in D_i^I$. Then if $\sum_{i \in N} h^F(D_i^{1,Formal}, g^{**}) < h^F(D_i^{1,Formal}, g^{B*})$, informal and formal connections must be complements in the invention production function.

3.5.3 Network Path Dependency

A stable network g_1^* is said to exhibit path dependence if, for the same set of nodes N with the same μ_{ij} $\forall$ $i,j \in N$, there exists a second stable network g_2^* such that $g_2^* \neq g_1^*$. In other words, path dependence means that if two networks are formed from the same fundamentals, with the only difference being that candidate connections may be drawn in a different order, these networks may be different.

Given the network formation process laid out in Section 3.3 above, a simple sufficient condition for the absence of path dependence is that $\frac{c(\#_{D_i^1})}{\#_{D_i^1}} \leq 0$. In contrast, in many cases it is plausible to expect $\frac{c(\#_{D_i^1})}{\#_{D_i^1}} > 0$, in which cases the network may exhibit path dependence. This captures the idea, documented in Miritello, Lara, Cebrian, and Moro (2013), that individuals possess network capacity and so adding additional connections becomes increasingly difficult or costly. Alternative modeling assumption may also naturally lead to path dependence. For instance, if nodes take the benefits of creating inventive output into account when deciding to form links, then sufficiently strong substitutability across nodes, as in the Jackson and Wolinsky (1996) connections model mentioned above, also generates path dependence since whether or not it is worthwhile to connect to a node depends on the number of existing connections at the time the potential link is drawn.

Prohibition provides a unique opportunity to examine the stable networks at multiple points in time, g^{B*} and g^{**}. If collaborations among inventors are different across these two networks, that is if there exists an i, j such that $d(i, j, t^{*B} = 1)$ but $d(i, j, t^{**} \neq 1)$ or $d(i, j, t^{*B} \neq 1)$ but $d(i, j, t^{**} = 1)$, then we can conclude that the social network exhibits path dependence. This in turn suggests that prohibition, or indeed any disruption of the existing social network, may have long-lasting effects.

While each of these properties follows almost immediately from the imposition of prohibition given the network setup, I restate them here as a lemma to summarize:

Lemma 1. *If $f(g_{t^*+1}) < f(g^B_{t^*})$, then $\frac{\partial f_i}{d(i,j)} < 0$ for some i and j such that $j \in D^{1,Informal}_{i,t^*}$. Furthermore,*

1. *if $f^{Solo}_i(g^{**} - f^{Solo}_i(g^{B*}) \neq 0$, then $\frac{\partial f^{Solo}_i}{d(i,j)} \neq 0$ for at least some $i, j \in N$*

2. *if $\sum_{i \in N} h^F(D^{1,Formal}_i, g^{**}) < h^F(D^{1,Formal}_i, g^{B*})$, then $\frac{\partial h^F}{\partial d(i,j)} < 0$ for some i, j such that $j \in D^{Informal}_i$*

3. *if $\exists i, j$ such that $d(i, j, t^* = 1)$ but $d(i, j, t^{**} \neq 1)$ or $d(i, j, t^* \neq 1)$ but $d(i, j, t^{**} = 1)$, then g^* exhibits path dependence.*

4 Data and Empirical Strategy

4.1 Patent and Other County-Level Data

Data on patents is from Petralia, Balland, and Rigby (2016b).[17] This dataset is augmented with data on patent classes from Marco, Carley, Jackson, and Myers (2015). The use of

[17] See Petralia, Balland, and Rigby (2016a) for details on how the dataset was constructed. Also see Andrews (2019) for the strengths and weaknesses of this patent data.

patent data as a proxy for invention, while imperfect in many ways (see Griliches (1990) and Nagaoka, Motohashi, and Goto (2010) for a discussion), is attractive because it provides the name of individual innovators. By linking patent records across time or using these names to infer inventor characteristics, it is possible to learn a great deal about how the identity of inventors changes after prohibition, and hence to test the model's predictions on patenting by different types of individuals.

County-level data is from the National Historical Geographic Information Series (NHGIS, Manson, Schroeder, Riper, and Ruggles (2017)). The NHGIS data comes from federal census records, which are only available at decadal frequencies, but the patent data is available for all years. The use of yearly data is valuable, as the model suggests that short-term dynamics are interesting. I therefore linearly interpolate NHGIS data for between-census years. Results are not sensitive to different interpolation methods. Additional supplementary datasets are described along with the results below.

4.2 Prohibition Data

Sechrist (2012) forms the basis for the data on whether each county is wet or dry in a particular year. This data lists, 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. While the Sechrist (2012) data reports when all counties within a state have gone dry, it does not tell why counties became dry; it could be the case that every county decided for itself to prohibit saloons on the basis of the local option. I therefore supplement the Sechrist (2012) data with information on the passage of prohibition laws at the state-level. In particular, I restrict attention to the states that passed statewide prohibition laws by referendum identified in Lewis (2008) and Merz (1930).[18] For a handful of states, Lewis (2008) and Merz (1930) record different dates in which states pass prohibition referendums than does Sechrist (2012). This suggests that naively using the Sechrist (2012) data either introduces measurement error if prohibition dates are misrecorded or endogeneity bias if all counties in a state are deciding to change their local laws prior to the passage of a state referendum. See Appendix A for more details on dataset construction. In Appendix D, I compare wet and dry counties along observable dimensions both cross sectionally and in the years before they passed statewide prohibition.

In the analyses that follow, I take several additional steps to minimize endogeneity concerns and identify the causal effect of prohibition on invention. Because many counties switched from wet to dry before the imposition of statewide prohibition, it may be the case that the prohibition movement was also gaining strength in wet counties, and so any difference between the wet and dry counties may be due to an underlying shift in the attitudes of wet counties, even if they themselves did not adopt county prohibition. I minimize this concern by restricting the sample to only counties that have been consistently wet or dry for an extended period of time before the enactment of statewide prohibition.[19] The counties that were consistently wet or dry likely saw the least within-county change in the run-up to statewide prohibition. As I show below, trends in patenting across consistently wet and dry counties were parallel before imposition of prohibition, so different levels of patenting in

[18] I thank Michael Lewis for generously providing the raw data upon which his calculations are based.

[19] Specifically, I restrict the sample to counties that were either wet or dry for 5 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 were wet or dry for these longer stretches of time; see Appendix E.

wet counties will be absorbed by county fixed effects. I use the set of consistently wet and consistently dry counties in the states that passed statewide prohibition referendums as my baseline sample. I drop any states that did not have at least one consistently wet and at least one consistently dry county prior to the imposition of state prohibition.

One benefit of the passage of state prohibition by referendum is that it is often possible to see how individual counties voted. Even if a particular county never adopted prohibition at the county level, the views of its population may be changing over time. So if a county's vote in a statewide referendum is close, this may reflect the fact that a county is becoming, for example, more religious or culturally conservative; this conservatism may have an effect on invention independently of its effect on prohibition. As a robustness exercise, I further refine the data to include only counties that voted solidly to remain either wet or dry, using county voting results provided by Lewis (2008); I refer to these counties as "bastions of wet or dry support," respectively. I consider a county to be a bastion of wet sentiment if wet votes outnumber dry votes by two-to-one or more, and vice versa for bastions of dry sentiment. The results are not sensitive to alternative cutoff values. I also present results using the Sechrist (2012) data for all states as an additional robustness test; additional samples used for robustness are discussed in Appendix E.Table 1 lists the number of wet and dry counties in each sample state for each of the three samples described here.

4.3 Empirical Strategy

After constructing "treated" and "control" counties as described above, I estimate a simple differences-in-differences specification,

$$\begin{aligned} Patenting_{ct} = &\beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\ &+ X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{13}$$

where $WetCounty_c$ is a dummy variable that equals 1 if county c was wet according to the definition of each subsample of the data used. $StatewideProhibition_{ct}$ 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. I control for logged county population, the fraction of the county living in urban areas, and the fraction of the population living in a state other than their state of birth in all regressions unless otherwise noted, as these variables may be correlated with both prohibition status and overall levels of patenting. I show in Appendix G that the results are not sensitive to the inclusion, exclusion, or composition of X_{ct}. Because counties are considered wet or dry if they have been so consistently for at least five years, in the baseline specification I only include data starting five years before each state imposes prohibition. I likewise only include data for the first five full years that state prohibition is in effect (plus the zeroth year in which prohibition was imposed) to minimize concerns about subsequent policies that could have differentially affected wet and dry counties. Thus the baseline results study the short run effects of disrupting the social network. I relax these restrictions in Appendix E. Throughout, standard errors are clustered at the county level.

5 Baseline Results

In this section, I analyze how the aggregate level and rate of patenting change following the imposition of prohibition at the state-level. Before presenting regression results, in Panels (a) and (b) of Figure 3 I plot raw logged patenting and the raw patenting rate in counties that were wet and dry for extended periods of time before the imposition of state-level prohibition. In Panels (c) and (d), I residualize the patenting data by controlling for year effects and the covariates mentioned above. The year variable on the x-axis has been normalized to 0 for the year in which statewide prohibition is enacted. 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, although due to the smaller number of wet counties in the sample the wet county trend is slightly more volatile. Residualizing the patenting data decreases the level difference between the two trends. In all four plots, patenting in the formerly wet counties decreases sharply relative to the consistently dry counties in the three years immediately following prohibition. This decrease halts and almost returns to its initial level in the final two years plotted.

To quantify and test this relationship more formally, Table 2 shows results of estimating Equation (13). 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 $\frac{Num.Patents_{ct}}{Population_{ct}}$.[20] Each group of rows estimates Equation (13) using a different subsample of

[20]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

county data as described in Section 4. 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.

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 patenting rate results should be interpreted with caution: since county populations are only available every ten years, I linearly interpolate population in the between-census years, and thus cannot capture, for instance, non-monotonicities in the population size between censuses. Therefore, for the future analysis, I used logged outcome variables in most specifications.

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

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.

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.

Figure 4 examines the dynamics of the effect more rigorously. I estimate

$$\begin{aligned} Patenting_{ct} =& \beta_0 + \sum_{\tau \in T} \Big[\beta_{1\tau} WetCounty_c * Time_\tau + \beta_{2\tau} Time_\tau \Big] \\ & + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{14}$$

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 model described in Section 3 and the intuition from Figure 3, patenting is statistically significantly lower in the first two years for which prohibition is in effect. In the next two pairs of years, the magnitude is closer to zero and the statistical significance of the magnitude depends on the specification. 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. I discuss the dynamics in a bit more detail in Section 8.

5.1 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.[21] In Column 1 of Table 3, 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 E, 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 F I show that the results are robust to various alternative regression specifications.

In Appendix H, I conduct a number of placebo tests, including examining cases in which statewide prohibition was brought to a referendum but failed to pass and the enactment of several other large state policies that did not directly disrupt social networks. In all cases, I find null effects.

[21] 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.

5.2 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.

5.2.1 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 6 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. Panel (b) plots what is essentially a triple-difference, plotting the difference in logged patenting between males and females over time in both the previously 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 5 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.

5.2.2 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).[22] The fact that 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 (W. R. Kerr (2008a), W. R. Kerr (2008b), Foley and Kerr (2013), S. P. Kerr and Kerr (2018)).

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

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

to improve the ethnic name matching.[23] Results are presented in Table 6. 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.

5.3 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 downturn. 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.[24] 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

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

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

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.

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

5.4 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 changes in social interactions. Evidence from the pharmacology and creativity literature is mixed regarding whether alcohol consumption increases creativity (Norlander (1999), Beveridge and Yorston (1999), Plucker, McNeely, and Morgan (2009), Hicks, Pedersen, Friedman, and McCarthy (2011), Jarosz, Colflesh, and Wiley (2012)).[25] 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

[25] 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 (Steele and Josephs (1990), Giancola, Josephs, Parrott, and Duke (2010), Schilbach (2018)), although it is unclear whether this increases or decreases the production of creative ideas.

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). I estimate the triple-difference regression

$$\begin{aligned}
Patenting_{ct} = &\beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\
&+ \beta_3 WetCounty_c * BoneDryLaw_c * StatewideProhibition_{ct} \\
&+ \beta_4 BoneDryLaw_c * StatewideProhibition_{ct} \\
&+ X_{ct}\alpha + County_c + Year_t + \epsilon_{ct},
\end{aligned} \tag{15}$$

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 4. 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. I therefore estimate whether patenting in wet counties declines relative to dry counties by a larger amount in states with higher cirrhosis death rates, with the identifying assumption that any change in cirrhosis death rates is driven by changes in consumption in the formerly wet counties. I estimate

$$\begin{aligned} Patenting_{ct} =& \beta_1 WetCounty_c * StatewideProhibition_{ct} + \beta_2 StatewideProhibition_{ct} \\ &+ \beta_3 WetCounty_c * Cirrhosis_{ct} * StatewideProhibition_{ct} \\ &+ \beta_4 Cirrhosis_{ct} * StatewideProhibition_{ct} + \beta_5 Cirrhosis_{ct} \\ &+ X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}, \end{aligned} \tag{16}$$

where $Cirrhosis_{ct}$ is the number of cirrhosis deaths per 100,000 capita in county c's state in year t. Results are presented in Column 3 of Table 4. After controlling for cirrhosis, the estimate for $\hat{\beta}_1$ is negative and similar in magnitude to the baseline specifications, although not statistically significant. Patenting in wet counties falls more relative to dry counties after imposing prohibition in states with higher cirrhosis rates (as estimated by $\hat{\beta}_3$), suggesting that if anything, continued alcohol consumption without the social interactions in saloons led to fewer patents, although this coefficient is also not statistically significant.

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. While far from conclusive, these results imply that invention is one instance in which we cannot "blame it on the alcohol" (Foxx, 2008).

6 Collaborative Inventions

I next test for the predictions of part 1 of Lemma 1. Many studies of social networks and innovation focus on the role of networks in facilitating collaboration between individuals (e.g., Allen (1983), Newman (2001), Breschi and Lissoni (2009), Crescenzi, Nathan, and Rodgríguez-Pose (2016), Mohnen (2018), Catalini (2018)). This is not surprising, as collaboration is a readily observable outcome. But exposure to ideas may be even more important. The model in Section 3 explicitly accounts for both of these possibilities.

Indeed, it is theoretically ambiguous whether prohibition should have a larger effect on networks' role in facilitating exposure to collaborators or exposure to ideas. Prohibition reduces the number of immediate connections, reducing the chances to collaborate. But these immediate connections also reduce the number of ideas to which an individual is exposed, and prohibition also makes it more difficult to access ideas possessed by friends of friends, their friends, etc.

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 7. In the first column, the dependent variable is the logged number of inventors.[26] The number of inventors in a county declines by almost the same percent as does overall patenting, suggesting that

[26]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.

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 I, I show that small collaborations with only two inventors falls by more than collaborations featuring three or more inventors; as informal organization is difficult, 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.

7 Complements in the Invention Production Function

Next, I test the prediction of part 2 of Lemma 1. If patents that are the result of formal connections decline following the imposition of prohibition, then informal and formal connections must be complements in the invention production function. The reason for this is that prohibition also increases in the distance between informal connections. Formal connections are unchanged: an individual's coworkers prior to prohibition are still their coworkers after prohibition (especially in light of the results in Section 5.3 that prohibition does not appear to change underlying economic conditions).

While this logic is straightforward, operationalizing it requires determining which patents are the result of formal connections. To do this, I observe changes in patents that are assigned to firms in formerly wet counties relative to consistently dry counties following the imposition of prohibition.. By the start of the twentieth century, most states had adopted some variation of the shop right doctrine in which firms retained ownership over inventions created by their employees (Fisk (1998), Fisk (2009)). Moreover, the decades before national prohibition saw large R&D firms that specialized in invention begin to emerge, replacing the largely independent inventor (Nicholas (2010), Nicholas (2011)). Thus, a patent that is assigned to a firm is strong evidence that a patent was invented at least in part as a result of formal connections between an employer and employee or between coworkers.

Results are presented in Table 8. Column 1 shows that assigned patents declined in previously wet counties by about 8%. Non-assigned patents, shown in Column 2, declined by about 9%. This suggests that invention declined more when the formal social network was not in place, although the difference in magnitudes is small, consistent with substantial contagion between the informal and formal connections in the network. Indeed, Column 3 shows that the difference in the decline between the assigned and non-assigned patents is statistically indistinguishable from zero. Future work will continue to improve the measurement of patent assignment and further investigate these results, so these results should be considered as preliminary and taken with a rim of salt.

These findings on changes in assigned patents are also important because much of the prior research on social connections and innovation has followed individuals in the workplace (Song, Almeida, and Wu (2003), Singh (2005), Burt (2005), Agrawal, Cockburn, and McHale

(2006), Catalini (2018)). Prohibition therefore provides a unique opportunity to investigate the role of distant and informal connections for within-firm outcomes, an understudied and important issue for managers.

8 The Path Dependence of Network Structure

Finally, I test the prediction of part 3 of Lemma 1. The presence of path dependence in a social network is important to ascertain because it determines whether state-level prohibition laws, or indeed any disruptions of social networks, had persistent effects. If there were no path dependence, then as individuals reconstruct their social networks over time, they will eventually end up with an nearly identical network to the pre-prohibition network.[27] 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.

To test for the presence of path dependence, I begin by estimating Equation (13) for variables related to the types of inventions patented and the identities of co-patenting pairs. I next investigate whether these variables return to their pre-prohibition level within five

[27] 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; see Figure 1.

years.[28] More precisely, I estimate the following model:

$$\begin{aligned}Patenting_{ct} =&\beta_1 WetCounty_c * First3ProhibitionYears_{ct}\\&+\beta_2 WetCounty_c * Next3ProhibitionYears_{ct}\\&+\beta_3 StatewideProhibition_{ct} + X_{ct}\alpha + County_c + Year_t + \epsilon_{ct},\end{aligned} \tag{17}$$

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 6 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.[29] Because names may be recorded differently on different patents and the historical patent data suffers

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

[29] 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.

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.

Results are presented in Columns 1 and 2 in Panels (a) and (b) of Table 9. Panel (a) shows that the number of patents containing a pre-prohibition inventor pair declines on average over all post-prohibition years (estimating Equation (13)) 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]

I next document that the path dependence of network structure is also reflected in the direction of inventive activity. 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 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

[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.

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.

For comparison, in Panel (c) I estimate Equation (17) 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. This is consistent with prior results that show that social connections are caused by the locations in which an individual interact (e.g., Marmaros and Sacerdote (2006)). I extend this earlier literature by showing that the venues in which individuals interact also affects the direction, if not the rate, of invention.

9 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 informal and formal connections are complements in the creation of new ideas, and that the structure of social networks exhibit path dependence.

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 network is fragile, people are resilient in staying networked. More generally, these results paint a more complete picture of how information flows through networks, including demonstrating the importance of interactions that happen outside the boundaries of the firm for innovation 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.

Several additional analyses are on tap, including exploring the heterogeneity of the treatment effect across counties with different pre-existing network structures and examining additional episodes of prohibition.

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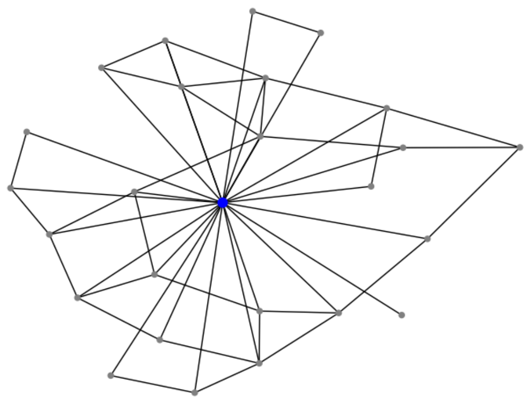
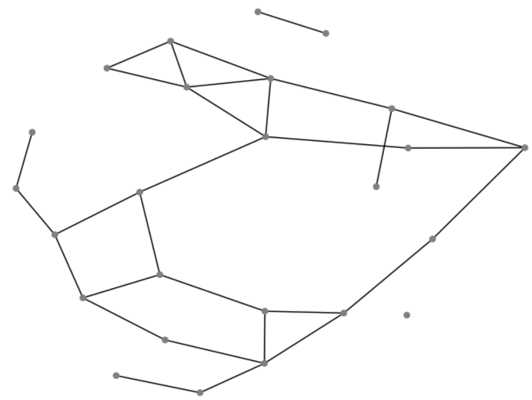
Graphs

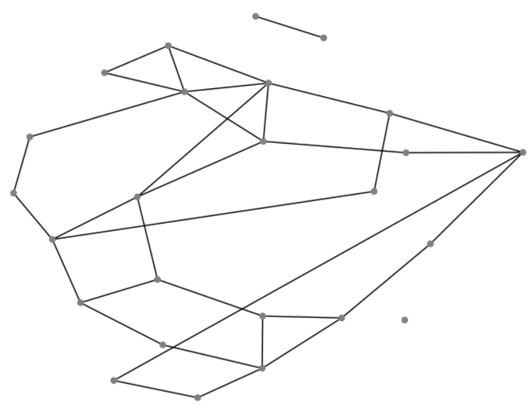
Figure 1: An Example Social Network



(a) Stable Network with B at Time t^*_{-1}



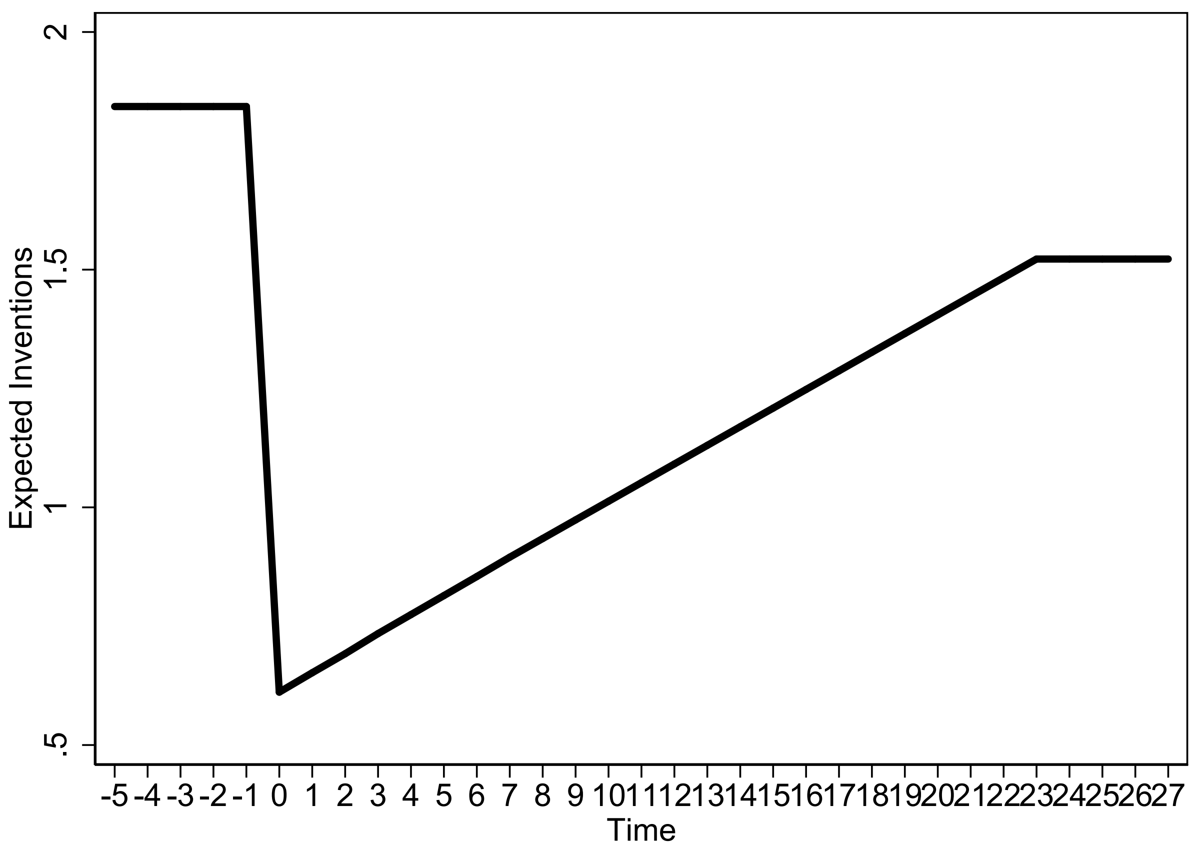
(b) Network After Removing B at Time t^*_0



(c) Stable Network without B at Time t^{**}

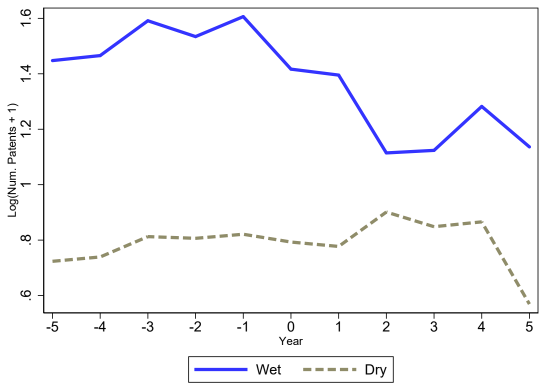
Notes: An example social network with 25 nodes of type A and one node of type B.

Figure 2: Aggregate Invention in a Sample Social Network

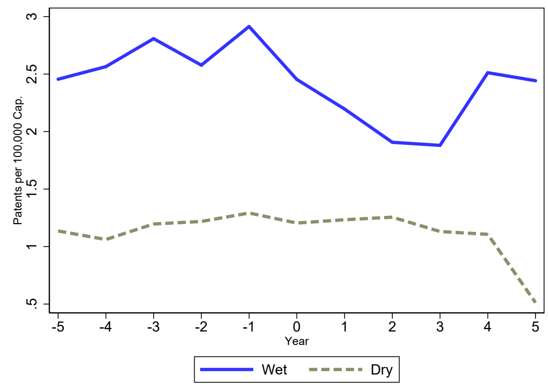


Notes: The total number of expected inventions from the sample network presented in Figure 1 over time.

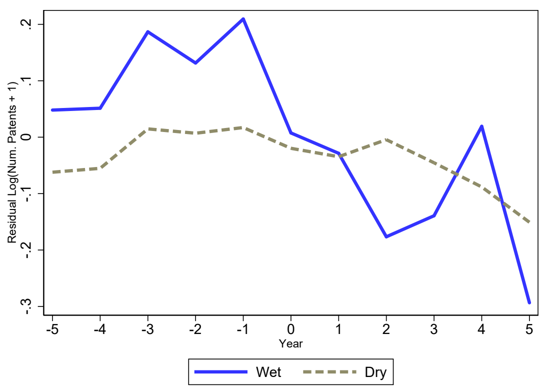
Figure 3: Patenting in Wet and Dry Counties



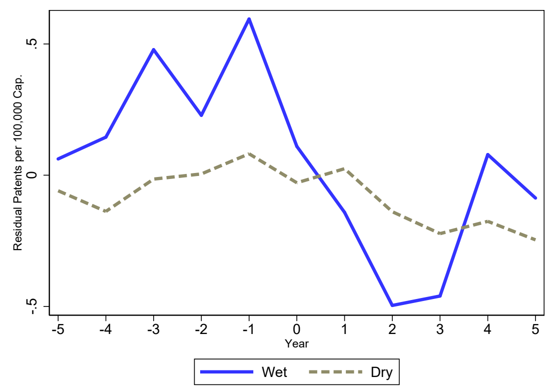
(a) log(Num. Pat + 1), Raw Data



(b) Patents per Capita, Raw Data



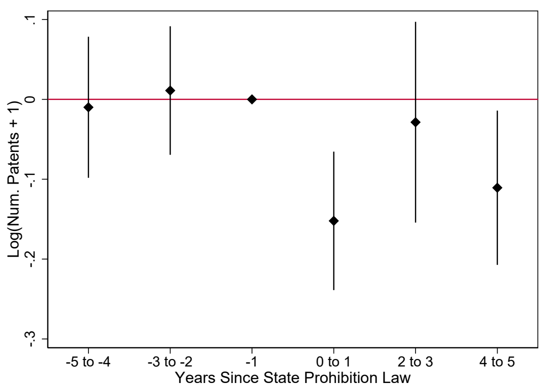
(c) log(Num. Pat + 1), Residualized Data



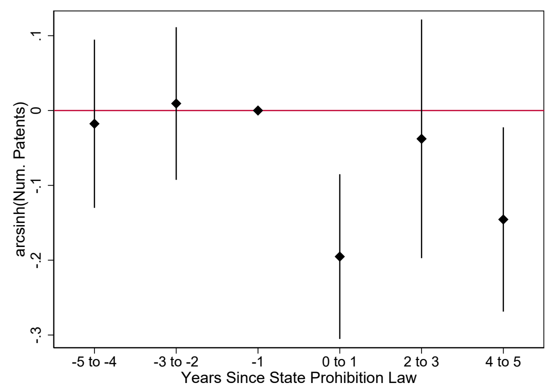
(d) Patents per Capita, Residualized Data

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. Panels (a) and (c) use $log(Num.Patents + 1)$ as the dependent variable. Panels (b) and (d) use $\frac{Num.Patents}{TotalPop.}$ as the dependent variable. Panels (a) and (b) plot raw data, while Panels (c) and (d) plot data residualized by controlling for year effects, logged total county population, the fraction of the county population living in urban areas, and the fraction of the county population born out of state.

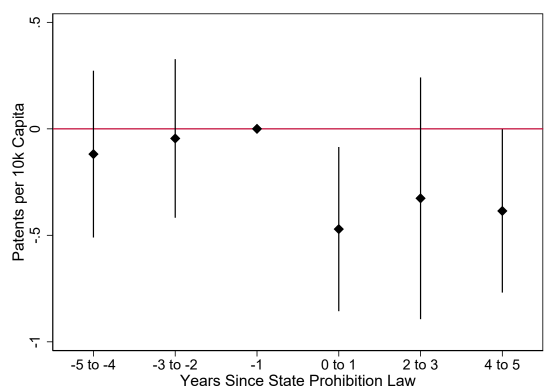
Figure 4: Difference by Time Since Prohibition



(a) log(Num. Pat + 1)



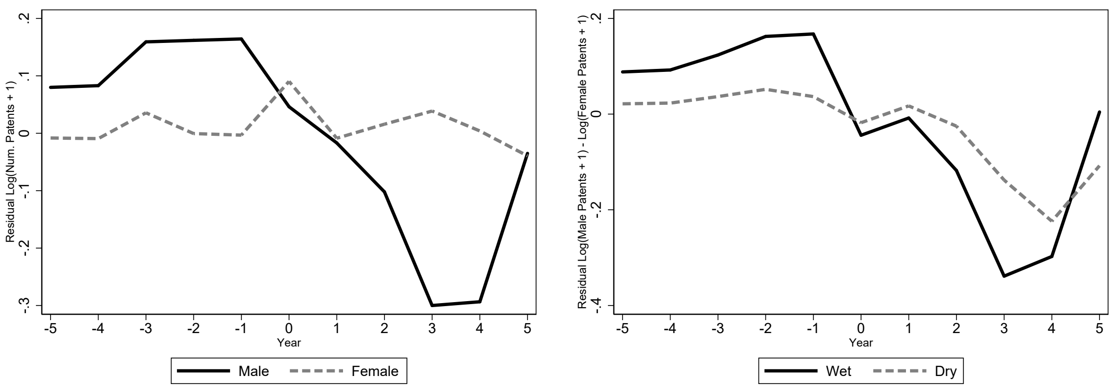
(b) $arcsinh$(Num. Patents)



(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 5: Patenting by Females and Males



(a) Females vs. Males in Wet Counties

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

Figure 6: *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: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 3: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 4: Social Interactions vs. Alcohol Consumption

	Bone Dry Laws	Cirrhosis Deaths per 100k
Wet County * Statewide Prohibition	-0.095**	-0.078
	(0.044)	(0.113)
Statewide Prohibition	-0.009	0.069
	(0.047)	(0.086)
Wet County * Bone Dry Law * Statewide Prohibition	0.045	
	(0.084)	
Bone Dry Law * Statewide Prohibition	-0.111	
	(0.070)	
Wet County * Cirrhosis Death Rate * Statewide Prohibition		0.007
		(0.015)
Cirrhosis Death Rate * Statewide Prohibition		-0.012
		(0.012)
Cirrhosis Death Rate		0.037***
		(0.010)
Mean of Dep. Var.	1.415	1.415
Adj. R-Squared	0.769	0.866
Cnty-Year Obs.	5,148	3,587
# Counties	526	504

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. $CirrhosisDeathRate$ is the number of cirrhosis deaths in a given state 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 5: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 6: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 7: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 8: Assigned Patents

	log(Assigned Pat. + 1)	log(Non-Assigned Pat. + 1)	log(Non-Assigned)-log(Assigned)
Wet County * Statewide Prohibition	-0.084***	-0.088***	-0.003
	(0.022)	(0.030)	(0.036)
Statewide Prohibition	0.028	-0.024	-0.052
	(0.022)	(0.028)	(0.035)
Mean of Dep. Var.	0.515	1.289	0.774
Adj. R-Squared	0.808	0.779	0.415
Cnty-Year Obs.	6,548	6,548	6,548
# Counties	726	726	726

Notes: Results on patent assignments. Column 1 uses $\log(Num.AssignedPatents + 1)$ as the dependent variable. Column 2 uses $\log(Num.Non-AssignedPatents + 1)$ as the dependent variable. Column 3 uses $\frac{Num.AssignedPatents}{Num.Patents}$ 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 9: 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

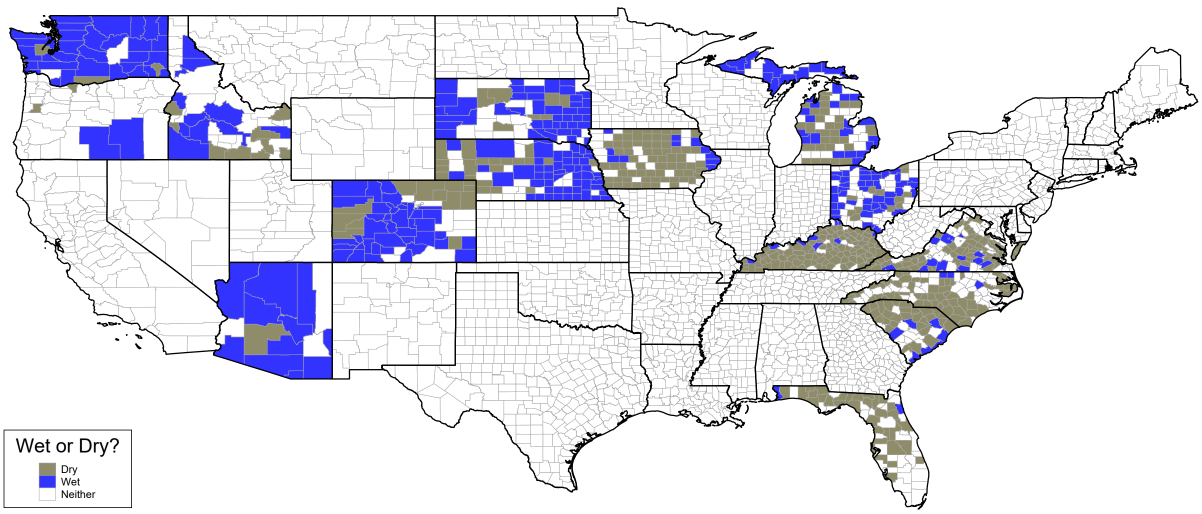
A Construction of Prohibition Data

As described in Section 4 above, data from Sechrist (2012) is used to determine if each state is wet or dry in each year from 1801 to 1920. The Sechrist (2012) data also indicates if an entire state is dry. One drawback of this dataset is that it does not indicate how a state adopted prohibition. In particular, the possibility exists that a state can be recorded as dry in the Sechrist (2012) data because all counties within a state have voted to prohibit alcohol at the county level. If this were the case, these states should be excluded from the analysis because the previously dry counties did not have prohibition imposed on them by the state. I therefore supplement the Sechrist (2012) with data from Lewis (2008) and Merz (1930) which, in addition to providing a check on the Sechrist (2012) prohibition dates, also provides data on how each state adopted prohibition.

The Lewis (2008) and Merz (1930) data do not contain any of the states that imposed statewide prohibition before 1907 (Delaware, Illinois, Kansas, Maine, Massachusetts, New York, North Dakota, Rhode Island, and Vermont), so these states are therefore removed from the baseline sample. In addition, the Lewis (2008) data contains four states that are not in the Sechrist (2012) data: Montana, New Mexico, and Wyoming. Because I rely on the Sechrist (2012) data to determine if a particular county within these states was wet or dry in particular years, I also exclude these states from the analysis. Finally, even when a state appears in both datasets, occasionally each will list a different year in which statewide prohibition was enacted. I used the dates in Sechrist (2012); otherwise, either the entire state would be listed as dry prior to the imposition of statewide prohibition, or else some states would be recorded as wet even after statewide prohibition. Results are not sensitive to dropping these states.

Figures 7 shows the geographic location of wet and dry counties in the baseline subsamples used throughout the paper. Map for the other subsamples are available upon request.

Figure 7: Map of Statewide Prohibition: Baseline Sample



Notes: Map of all wet and dry counties in the baseline sample, which consists of all counties that were either wet or dry for at least five years prior to the imposition of statewide prohibition by referendum.

B County and City Directory Results

It is difficult to believe that prohibition has a large effect on social networks if, for example, saloons respond to prohibition by simply converting to become restaurants. It is likewise difficult to believe that observed effects are driven by disruptions of social interactions if prohibition induced large negative economic outcomes across a wide range of industries. Little data exists to empirically test for these issues.

To overcome this, I consult historical county and city directories that list the names, industries, and addresses of all businesses within particular cities or counties. The directories are primarily from a repository at United States Online Historical Directories,[31] although they are supplemented by directories from Hathitrust and the Smithsonian collection.[32] Table 10 lists the counties for which directory data are available. To be useful, directories must be available in the years before and after state prohibition. I find such directories for a total of twelve counties, with 2-15 years collected for each county. In some cases, a directory covers only the major cities in a county. This is true, for instance, in the case of Fort Collins in Larimer County, CO, or Indianapolis in Marion County, IN. When a directory covers an entire county, I list the city as "na" in Table 10.

For the subset of directories for which I transcribe addresses of all saloons in the pre-prohibition years (indicated in the last column of Table 10, I locate these same addresses in the first city directory available after prohibition went into effect. If the address is not found, I record it as vacant.[33] Panel (a) of Figure 9 shows that almost 90% of addresses that housed a saloon in pre-prohibition years is vacant after prohibition. In Panel (b), I plot the distribution of industries for those addresses that are occupied in the post-prohibition directory. The majority of these addresses are converted to restaurants, although I stress that this represents a very small share of the pre-prohibition saloon addresses. About a quarter become other types of food-related businesses such as grocers, bakers, or butchers that are less likely to serve as hubs of social networks. A trivial share become hotels. "Other" includes all other types of business. While this includes some types of businesses that may also serve as effective locations to share information, such as billiard halls or barber shops, these types of establishments are exceedingly rare; most are retail stores (e.g., clothes, auto parts, farm

[31] `https://sites.google.com/site/onlinedirectorysite/Home/usa/`, accessed June 21, 2019.

[32] I thank Priyanka Panjwani for assistance in collecting and transcribing these data.

[33] "Vacant" means that no business occupies a saloon's address in the post-prohibition city directory; I record an address as vacant even if it is used as a private residence.

implements) or services (e.g., real estate, carpenters). The city directories also reinforce the fact that prohibition was indeed effective at shuttering the saloons: no establishments are identified as saloons or other types of alcohol-based businesses in the post-prohibition directories.

While saloons predictably disappear following prohibition, interpreting the effect of prohibition is difficult if formerly wet counties also see a drop in many different types of businesses. To verify that this is not the case, I count the number of businesses in a number of industries for city directories in both formerly wet and formerly dry counties for a number of years both before and after prohibition (since I do not record addresses for all of these establishments, I collect this data for a much larger set of directories).[34] I count the number of establishments in any industry related to automobiles, all barbers, all bowling alleys, and all dentists. Changes in the number of establishments in these industries are likely orthogonal to saloon-specific shocks, but will still capture overall changes in the economic outlook. For each industry, I estimate a simple differences-in-differences model very similar to the baseline specification in Equation (13):

$$\begin{aligned}\log(Num.Establishments_{ct} + 1) =& \beta_1 WetCounty_c * StatewideProhibition_{ct} \\ &+ \beta_2 StatewideProhibition_{ct} \\ &+ X_{ct}\alpha + County_c + Year_t + \epsilon_{ct}.\end{aligned}$$

Results are presented in Table 11. Due to the relatively small number of counties, none of the

[34]To ensure that the sample of counties for which city directories are available is not in some ways exceptional, I also verify that patenting declines in these counties after imposing prohibition, as it does in the baseline sample. While the relatively small number of directories means these results are imprecisely estimated, they are qualitatively similar to the baseline results. These results are available upon request.

results are consistently estimated, but there is certainly no consistent pattern of prohibition reducing the number of establishments by more in formerly wet counties relative to formerly dry counties. Results are similar using other transformations of the dependent variable. In Column 5, I combine results from all four industries, estimating

$$\begin{aligned}\log(Num.Establishments_{ict} + 1) = &\beta_1 WetCounty_c * StatewideProhibition_{ct} \\ &+ \beta_2 StatewideProhibition_{ct} \\ &+ X_{ct}\alpha + County_c + Year_t + Industry_i \\ &+ County_c * Industry_i + Year_t * Industry_i + \epsilon_{ct},\end{aligned}$$

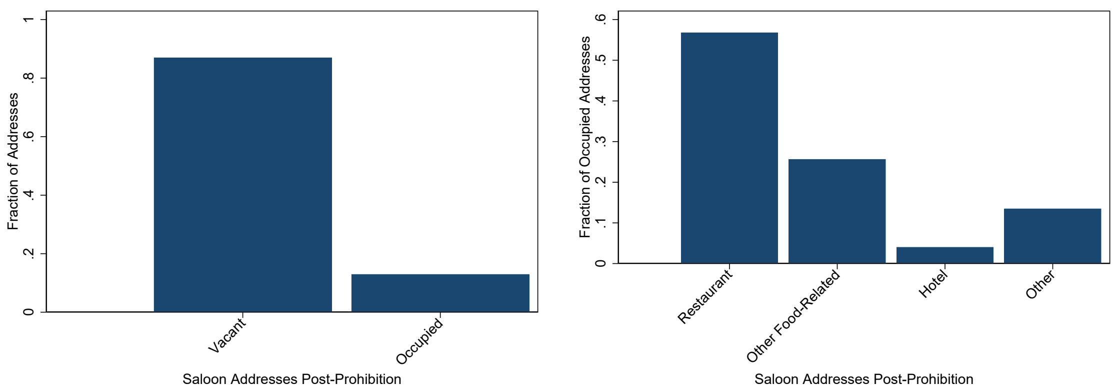
where i indexes the industry. Here, I find that prohibition has zero effect on the number of establishments. Thus, the negative effects of prohibition appear to be restricted to saloons.

Table 10: List of County and City Directories

	State	County	City	# Directories	First Directory	Last Directory	Year State Prohibition	Have Saloon Addresses?
1	Colorado	ELPASO	na	15	1905	1922	1916	Yes
2	Colorado	LARIMER	FORTCOLLINS	5	1909	1922	1916	
3	Indiana	LAPORTE	MICHIGANCITY	3	1905	1925	1918	
4	Indiana	MARION	INDIANAPOLIS	9	1914	1922	1918	Yes
5	Indiana	MONROE	BLOOMINGTON	5	1909	1925	1918	
6	Michigan	BRANCH	COLDWATER	2	1912	1919	1917	
7	Michigan	GENESEE	na	2	1915	1922	1917	
8	Michigan	IONIA	na	2	1915	1917	1917	
9	Michigan	JACKSON	JACKSON	5	1909	1922	1917	
10	Michigan	KENT	GRANDRAPIDS	4	1911	1922	1917	
11	Michigan	SAGINAW	na	3	1909	1921	1917	Yes
12	Michigan	WASHTENAW	ANNARBOR	14	1909	1922	1917	

Notes: All states, counties, and, if applicable, cities for which directory information is available along with the total number of directories consulted for each city/county, the first and last directory consulted, and whether or not saloon addresses were collected from the directory.

Figure 8: Saloon Addresses in the Post-Prohibition Directory



(a) Vacant vs. Occupied

(b) Occupied by Industry Type

Figure 9: *Notes:* Each category is a possible use of a saloon's address in the first post-prohibition directory. Panel (a) indicates the share of addresses that are vacant or occupied by some kind of business. Panel (b) breaks down the share of occupied addresses by type of business.

Table 11: Number of Establishments in Different Industries

	Auto	Barber	Bowling	Dentist	All
Wet County * Statewide Prohibition	0.244	-0.324	0.0770	0.0451	0.0105
	(0.358)	(0.227)	(0.141)	(0.162)	(0.156)
Statewide Prohibition	-0.136	0.125	-0.286	-0.0620	-0.0899
	(0.391)	(0.310)	(0.264)	(0.250)	(0.183)
N	66	66	66	66	264
Adj. R-Sqr.	0.939	0.981	0.698	0.982	0.972
Controls	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Industry FE	No	No	No	No	Yes
County*Industry FE	No	No	No	No	Yes
Year*Industry FE	No	No	No	No	Yes

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Differences-in-differences results on the number of establishments in wet and dry counties before and after the imposition of statewide prohibition. Column 1 shows results for auto-related establishments. Column 2 shows results for barbers. Column 3 shows results for bowling alleys. Column 4 shows results for dentists. Column 5 shows results for all four industries. The dependent variable for all columns is $\log(Num.Establishments + 1)$. $StatewideProhibition$ 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

C Population Changes and Migration following Prohibition

To examine differential changes in population in the formerly wet and consistently dry counties following the imposition of statewide prohibition, I use population data from the decennial population censuses. I estimate versions of Equation (13) using population-related measures as the outcome variable and keeping data only from the census years immediately before and after the imposition of prohibition. Column 1 of Table 12 shows that aggregate population does not change significantly differently in the formerly wet relative to the consistently dry counties following the imposition of prohibition.

It is possible that, while net population change is similar in both types of counties, the composition of the population changes in formerly wet relative to consistently dry counties. To test for this, I examine whether outflows and inflows of population are the same size in both types of counties. To determine who leave and enters a county, I largely follow Ferrie (1996) to link individuals across decennial censuses, fuzzy matching on first and last name and then keeping individuals with consistent birth years, birthplaces, and parents' birthplaces. I record any individual that is not matched to the same location in the previous decennial census to be migrant into the county, and any individuals who do not match to the next decennial census to be migrants out of the county. In Column 2 of Table 12, I show that population inflows are also not significantly different in the formerly wet relative to the consistently dry counties. Furthermore, the estimated coefficients are small in magnitude relative to the baseline results on patenting.

Even if the number of people who migrate to formerly wet or consistently dry counties is similar, these flows may mask differences in the types of people who are moving. In Column 3, I show that there is no statistically significant difference in the average logged occupational education score.[35] In Column 4, I show that this is also true when examining the inflows. Hence, prohibition does not appear to affect the average amount of skill possessed by individuals in each county.

Table 12: Population and Migration Results

	log(Pop)	log(Inflows)	Education Score	Education Score Inflow
	Referendum States			
Wet County * Statewide Prohibition	-0.008	-0.006	-0.010	0.025
	(0.031)	(0.032)	(0.016)	(0.022)
Mean of Dep. Var.	7.494	7.039	6.265	6.304
Adj. R-Squared	0.943	0.942	0.337	0.320
Cnty-Year Obs.	3,608	3,608	3,424	3,418
# Counties	1,717	1,717	1,712	1,709

Notes: Results on population and migration. Column 1 uses $\log(Population)$ as the dependent variable. Column 2 uses $log(PopulationInflows)$ as the dependent variable. Column 3 uses $log(PopulationOutflows)$ as the dependent variable. Results are from the census years before and after 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

D Balance Checks and Cross Sectional Correlations

I first compare wet and dry counties in the cross section at three different points in time using the same patenting variables as in Table 2. For all of the specifications, the estimated magnitude of patenting in wet counties relative to dry is positive, and for all except two results in 1915, the difference is statistically significant. This is prima facie evidence that prohibition

[35] The education score records the average number of years of education for individuals with a given occupation in the 1950 census.

reduces patenting, although obviously counties can select into adopting prohibition laws and so these results cannot be interpreted as causal.

I next compare wet counties to dry counties in the census year prior to the start of state prohibition. Results are presented in Table 14, which shows the results of t-tests between wet and dry counties in the last census year before the enactment of statewide prohibition. Differences between wet and dry counties are not statistically significant. The fact that consistently wet and consistently dry counties look fairly similar before the imposition of state prohibition raises the possibility that the large differences in Table 13 could be driven by selection of states that were adopted prohibition laws; for instance, states with few high-patenting counties may have been more likely to adopt prohibition, creating the illusion that wet counties have many more patents. The results of Table 14 suggest that the within-county comparisons that exploit the timing of state prohibition laws is more likely to deliver credible results.

E Results with Other Samples of the Data

Section 4 describes several different data samples that are used in the analysis to ensure both that the observed results are widespread across the country and also to ensure that the treatment is exogenous to the sample counties. In this section, I present results with several additional samples of the data and show that the baseline results are robust to these as well.

In Table 15, I vary the number of years over which a county must be consistently wet or dry to be included in the sample. In the first group of results, I include all counties that were wet or dry in the year before the imposition of statewide prohibition. In the second group,

Table 13: Comparison of Wet and Dry Counties in Various Years

	Pat. per Cap.	log(Patents + 1)	arcsinh(Patents)
		1885	
Wet County	1.420***	0.537***	0.683***
	(0.331)	(0.021)	(0.026)
Mean of Dep. Var.	1.426	0.656	0.825
Adj. R-Squared	-0.001	0.001	0.001
# Counties	1,330	1,426	1,426
		1900	
Wet County	1.385***	0.695***	0.883***
	(0.356)	(0.022)	(0.027)
Mean of Dep. Var.	1.547	0.981	1.228
Adj. R-Squared	-0.001	0.001	0.001
# Counties	1,667	1,675	1,675
		1915	
Wet County	1.447***	0.447	0.556
	(0.068)	(0.340)	(0.427)
Mean of Dep. Var.	2.627	1.442	1.794
Adj. R-Squared	-0.001	0.000	0.000
# Counties	1,265	1,320	1,320

Notes: Results of regressing patenting on Wet, a dummy variable that takes the value of 1 if a county is recorded as wet in a particular year according to Sechrist (2012). Column 1 uses $\frac{Num.Patents}{TotalPop.}$ as the dependent variable. Column 2 uses $\log(Num.Patents+1)$ as the dependent variable. Column 3 uses $arcsinh(Patents)$ as the dependent variable. Each group of rows shows results from a particular calendar year: 1900, 1905, 1910, and 1915, respectively. Counties are dropped when an entire state is recorded as dry. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 14: Compare Wet and Dry Counties: Economic and Demographic Dimensions

	Wet	Dry	Wet - Dry
log(Pat. + 1)	0.389	0.530	-0.141
	(0.705)	(0.621)	(0.288)
log(Total Pop.)	9.326	9.227	0.100
	(1.447)	(0.514)	(0.591)
Frac. Urban	0.093	0.057	0.037
	(0.319)	(0.127)	(0.143)
Frac. Male	0.535	0.553	-0.019
	(0.052)	(0.051)	(0.021)
Frac. Interstate Migrants	0.409	0.431	-0.023
	(0.298)	(0.236)	(0.122)
log(Manuf. Employment)	4.471	5.455	-0.984
	(2.232)	(1.775)	(1.119)
log(Value Manuf. Output)	11.017	12.643	-1.626
	(4.265)	(1.583)	(2.135)

Notes: T-tests comparing the means of the wet counties to the means of the dry counties in the last census year before the adoption of each state's prohibition referendum. The first column lists the mean and standard deviation of wet counties. The second column lists the mean and standard deviation of the dry counties. The third column lists the difference in the mean between the wet and dry counties and the standard error of the difference. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

I include all counties that were wet or dry for at least four years. Using a four year cutoff substantially increases the sample size relative to the baseline sample because many counties vote to change their local prohibition laws in county-level referendums that occur during the four-year electoral cycle; for example, many Georgia counties changed their local laws four years before the passage of statewide prohibition. In the third group, I include all counties that were wet or dry for at least ten years. In the fourth group, I include all counties that were wet or dry for at least 15 years. While the sample size shrinks as I require counties to maintain their wet or dry status for longer periods of time, the estimated coefficients are qualitatively consistent across samples.

F Alternative Specifications

Table 16 shows results with a number of additional regression specifications. Column 1 shows results when $Num.Patents$ is the dependent variable. Column 2 uses $log(Num.Patents_{ist})$ as the dependent variable, where $log(Num.Patents_{ist}) \equiv 0$ when $Num.Patents_{ist} = 0$, and I include a dummy variable for county-years in which there are zero patents. Column 3 uses $log(Num.Patents_{ist} + 0.001)$ as the dependent variable. In Column 4, I drop all dry counties so that the results are identified only off of the timing of prohibition among counties that were wet before prohibition. In all cases, results are similar to the baseline estimates, although they are smaller in magnitude (when converted to a percentage change relative to the mean of the dependent variable) in Column 1 and larger in magnitude in Column 3.

Table 15: Results with Alternative Time Cutoffs to be Considered Consistently Wet or Dry

	Pat. per Cap.	log(Patents + 1)	Num. Patents
	Wet or Dry for at least 1 Year		
Wet County * Statewide Prohibition	-0.210*	-0.080**	-0.061
	(0.112)	(0.031)	(0.264)
Statewide Prohibition	0.006	0.012	-0.082
	(0.091)	(0.030)	(0.238)
Fixed Effects	Yes	Yes	Yes
Mean of Dep. Var.	2.016	1.166	9.408
Adj. R-Squared	0.540	0.816	0.986
Cnty-Year Obs.	4,848	4,848	4,848
# Counties	1,616	1,616	1,616
	Wet or Dry for at least 4 Years		
Wet County * Statewide Prohibition	-0.308***	-0.100***	-0.648**
	(0.075)	(0.024)	(0.321)
Statewide Prohibition	0.088	-0.011	0.492**
	(0.067)	(0.021)	(0.222)
Fixed Effects	Yes	Yes	Yes
Mean of Dep. Var.	2.159	1.365	11.964
Adj. R-Squared	0.508	0.798	0.975
Cnty-Year Obs.	10,213	10,213	10,213
# Counties	1,287	1,287	1,287
	Wet or Dry for at least 10 Years		
Wet County * Statewide Prohibition	-0.288***	-0.133***	-0.641*
	(0.073)	(0.035)	(0.387)
Statewide Prohibition	0.054	0.008	0.135
	(0.062)	(0.026)	(0.216)
Fixed Effects	Yes	Yes	Yes
Mean of Dep. Var.	1.620	1.284	14.322
Adj. R-Squared	0.504	0.788	0.975
Cnty-Year Obs.	8,203	8,203	8,203
# Counties	581	581	581
	Wet or Dry for at least 15 Years		
Wet County * Statewide Prohibition	-0.113	-0.038	-0.261
	(0.084)	(0.044)	(0.666)
Statewide Prohibition	0.034	0.022	-1.005
	(0.061)	(0.035)	(0.741)
Fixed Effects	Yes	Yes	Yes
Mean of Dep. Var.	1.525	1.227	16.679
Adj. R-Squared	0.670	0.845	0.966
Cnty-Year Obs.	4,645	4,645	4,645
# Counties	247	247	247

Notes: Baseline regression results using alternative samples of wet and dry counties. Column 1 uses $\frac{Num.Patents}{TotalPop.}$ as the dependent variable. Column 2 uses $\log(Num.Patents + 1)$ as the dependent variable. Column 3 uses $Num.Patents$ 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. Each group of rows shows results using a different subsample of the data.Group 1 uses all counties that were wet or dry for at least 1 year before statewide prohibition. Group 2 uses all counties that were wet or dry for at least 10 year before statewide prohibition. Group 3 uses all counties that were wet or dry for at least 15 year before statewide prohibition. Standard errors clustered by county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

Table 16: Results with Alternative Regressions Specifications

	Num. Patents	Alt. log Pat.	log(Pat. + 0.001)	Wet Only log(Pat. + 1)
Wet County * Statewide Prohibition	-0.253	-0.069**	-0.631***	
	(0.657)	(0.028)	(0.166)	
Statewide Prohibition	0.154	0.029	-0.310*	-0.091**
	(0.427)	(0.027)	(0.172)	(0.038)
Zero Patents		-0.402***		
		(0.015)		
Fixed Effects	Yes	Yes	Yes	Yes
Mean of Dep. Var.	13.117	1.180	-0.287	1.418
Adj. R-Squared	0.973	0.845	0.514	0.858
Cnty-Year Obs.	6,538	6,538	6,538	2,667
# Counties	725	725	725	294

Notes: Baseline regression results using alternative regression specifications. The dependent variable in Column 1 is $Num.Patents_{ct}$). The dependent variable in Column 2 is $log(Num.Patents_{ct})$, which is set equal to 0 when $Num.Patents_{ct} = 0$, and a dummy variable is included for counties with zero patents in a given year. The dependent variable in Column 3 is $log(Num.Patents_{ct} + 0.001)$. The dependent variable in Column 4 is $\log(Num.Patents_{ct} + 1)$ and the specification includes only the consistently wet counties. $StatewideProhibition$ 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

G County-Level Controls

In this section, I present the effects of the county time-varying controls included in all regressions which are omitted above for readability. Results are presented in Table 17. Column 1 includes no controls. Columns 2-4 include one control each. Column 5 repeats the baseline regression specification, but the coefficients on the control variables are displayed. Across all columns, the estimated decline in logged patenting in formerly wet counties relative to dry counties after imposing statewide prohibition is remarkably consistent regardless of the inclusion of controls.

Table 17: Controlling for Demographic and Economic Variables

	None	log(Total Pop.)	Frac. Urban	Frac Interstate Migrants	All
Wet County * Statewide Prohibition	-0.113***	-0.120***	-0.123***	-0.114***	-0.123***
	(0.030)	(0.030)	(0.030)	(0.031)	(0.031)
Statewide Prohibition	-0.009	-0.009	-0.015	-0.010	-0.015
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
log(Total Pop.)		0.448***			0.420***
		(0.108)			(0.119)
Frac. Urban			-0.020		-0.031*
			(0.033)		(0.017)
Frac. Interstate Migrants				0.499	-0.090
				(0.395)	(0.438)
Fixed Effects	Yes	Yes	Yes	Yes	Yes
Mean of Dep. Var.	1.418	1.418	1.418	1.418	1.418
Adj. R-Squared	0.806	0.808	0.807	0.807	0.807
Cnty-Year Obs.	6,825	6,683	6,538	6,683	6,538
# Counties	755	741	725	741	725

Notes: Results that control for changes in demographic and economic variables. The dependent variable for all columns is $\log(Num.Patents + 1)$. Column 1 includes a control for $\log(TotalPop.)$. Column 2 includes a control for $\frac{Pop.LivinginUrbanAreas}{TotalPop.}$. Column 3 includes a control for $\frac{Pop.BorninAnotherState}{TotalPop.}$. $StatewideProhibition$ 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

H Placebo Tests

As a first placebo test, I use the data provided by Lewis (2008) to examine changes in patenting in consistently wet and dry counties in states that voted on prohibition referendum but ultimately voted these laws down. These votes occurred in Florida in 1910, Ohio in 1913, Vermont in 1916, Iowa in 1917, California in 1918, and Missouri in 1918. I present results in Column 1 of Table 18. The threat of prohibition laws does not appear to have any effect in the places for which the referendums did not pass.

It is possible that patenting decreased not from the disruption of social interactions that take place in saloons, but because of a more general reaction to a major policy change. In Columns 2 and 3 of Table 18, I verify that other large scale Progressive-era policy changes did not lead to a comparable fall in patenting. In Column 2 I use women's suffrage as a placebo policy change, using data from Lott and Kenny (1999) to date the beginning of legal women suffrage in each state. Women's suffrage appeared to increase patenting in wet counties relative to the dries, although this result is not consistent across other subsamples and the dynamics do not follow a similar pattern to those in Figures 3 and 4; figures of the dynamics are available upon request. In Column 3, I use the introduction of state compulsory schooling laws as the placebo policy, using data from Umbeck (1960); data on the passage of Progressive-era compulsory schooling laws have also been used by Acemoglu and Angrist (2000), Goldin and Katz (2003), and Stephens and Yang (2014). These laws have a small and insignificant positive effect. From these tests, I tentatively conclude that large scale social changes in general appear to have little effect on patents; only changes that affect people's social interactions cause a decline in patenting.

Table 18: Placebo Tests

	Placebo Prohibition	Womens Suffrage	Compulsory Schooling
Wet County * Statewide Policy	0.009	0.228***	0.021
	(0.061)	(0.085)	(0.041)
Statewide Policy	0.007	-0.246***	-0.086**
	(0.052)	(0.092)	(0.038)
Mean of Dep. Var.	2.205	1.255	0.476
Adj. R-Squared	0.912	0.843	0.705
Cnty-Year Obs.	1,698	3,721	5,439
# Counties	186	779	756

Notes: Results that check for other plausible explanations for the decline in patenting. The dependent variable for all columns is $\log(Num.Patents+1)$. Column 1 uses as the treatment state prohibition referendums that were voted on but did not pass. Column 2 shows the results using the passage of state-level women's suffrage as the treatment instead of the imposition of prohibition. Column 3 shows the results using the introduction of state compulsory schooling laws as the treatment. $StatewidePolicy$ is a dummy variable equal to one in years after a state has voted down a prohibition referendum in Column 1 or has enacted either women's suffrage or a compulsory schooling, for Columns 2 and 3 respectively. $WetCounty{*}StatewidePolicy$ is an interaction term equal to one for counties that were previously wet for at least five years before the imposition of statewide prohibition in the years after a state has enacted the statewide policy. Standard errors clustered by county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$

I Additional Results on Collaboration

In Columns 1 and 2 of Table 19, I repeat the analysis for collaborative patents from Column 5 of Table 7 but use the share of patents with small teams as the dependent variable. I define a patent to be the result of a small team if it has two inventors; results are similar with alternative definitions of small teams. The idea here is that small teams are more likely to form as a result of informal interactions that occur in saloons. Large teams are likely to form as a result of informal interactions and likely require some sort of formal coordination. The share of patents by small teams falls by about four percentage points, about twice as large as the overall decline in the share of patents with multiple inventors.

In Columns 2-4 I show that the number of patents for which inventors live in different towns, different counties, and different states, respectively, does not change following the imposition of prohibition. More precisely, I estimate the change in the number of patents with at least two inventors in which one inventor lives in either a formerly wet or consistently dry county and another inventor lives in either another town (regardless of county or state), another county (regardless of state), or another state in the formerly wet counties relative to the consistently dry counties.

Table 19: Collaborative Patents

	Frac. Pat by Small Team	Frac. Pat by Small Team	log(Inter-Town Collab. + 1)	log(Inter-County Collab. + 1)	log(Inter-State Collab. + 1)
	Referendum States				
Wet County * Statewide Prohibition	-0.033***	-0.023***	0.019	0.020	0.016
	(0.013)	(0.009)	(0.049)	(0.049)	(0.049)
Statewide Prohibition	0.020	0.008	0.015	0.011	0.015
	(0.016)	(0.010)	(0.043)	(0.043)	(0.043)
Zero Patents		0.883***			
		(0.005)			
Mean of Dep. Var.	0.079	0.275	0.352	0.349	0.351
Adj. R-Squared	0.092	0.885	0.620	0.619	0.619
Cnty-Year Obs.	4,463	6,538	2,480	2,480	2,480
# Counties	658	725	572	572	572

Notes: Additional results on collaborative patent. Columns 1 and 2 use $\frac{Num.Patentswith2Inventors}{Num.Patents}$ as the dependent variable. Columns 3 uses $\log(Num.PatentswithInventorsLivinginDifferentTowns)$ as the dependent variable, Column 4 uses $\log(Num.PatentswithInventorsLivinginDifferentCounties)$, and Column 5 uses $\log(Num.PatentswithInventorsLivinginDifferentStates)$. $StatewideProhibition$ 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 county and shown in parentheses. Stars indicate statistical significance: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$