

The Latent Legacy of Prohibition: Resistance Culture and Modern Entrepreneurial Resilience

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Abstract

This paper examines whether early 20th century state-level prohibitions on the sale and manufacture of alcohol influenced entrepreneurial responses to the COVID-19 crisis. During the period 1880-1920, approximately 1,600 counties in the United States voluntarily went dry (i.e., they prohibited the sale and manufacture of alcohol through local referenda or local ordinances), while an almost equal number of counties were forced to comply with state-level prohibitions. I argue that being forced to become dry fostered a long-lasting culture of adaptation and informal entrepreneurship. Using county-level data from 2010–2023, I find that exposure to forced prohibition is associated with a surge in new business applications in the aftermath of the COVID-19 shock. This effect appears to be more pronounced in areas with low remote-work feasibility and stronger anti-prohibition sentiment as, for instance, measured by references to bootlegging and speakeasy in historical newspaper accounts and anti-prohibition party vote shares. My results suggest that informal entrepreneurial norms rooted in the resistance to prohibition had an enduring impact on economic adaptability and entrepreneurship.

Keywords: Prohibition, Dry Laws, Entrepreneurship, Cultural Resilience, Historical Persistence, COVID-19, Institutional Mismatch

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"The past is never dead. It's not even past."

— William Faulkner

1. INTRODUCTION

The COVID-19 pandemic triggered a surprising surge in U.S. entrepreneurship (Kuckertz et al., 2020; Chetty et al., 2020). Despite widespread economic disruption and historic job losses, new business applications rose to record levels shortly after the onset of the pandemic. This surge was, however, far from uniform: some counties experienced dramatic increases in business formation, while others showed little response. Prior work links post-COVID changes in new business formation to local governance quality, labor market composition, and pandemic-era policy responses (R. Decker and Haltiwanger, 2024; Dinlersoz et al., 2021; Fazio et al., 2021; Haltiwanger, 2022). Much less attention has been paid to deeper cultural and historical drivers. Why did some communities exhibit greater adaptive responses to the COVID-19 shock? This paper explores one answer rooted in the legacy of early 20th-century alcohol prohibition.

Before the Volstead Act of 1920, alcohol regulation in the United States was highly decentralized.¹ Some counties prohibited the sale and manufacture of alcohol through local referenda, while others were forced to comply with statewide mandates that conflicted with local sentiment—producing a patchwork of regulatory environments.

These forced-dry counties, where prohibition conflicted with local preferences, faced a distinctive institutional clash.² In response, informal economic activity expanded as alcohol moved underground, bootlegging networks and speakeasies proliferated, and residents developed informal channels to circumvent restrictions (Blocker, 2006; Okrent, 2010; Hall, 2010).³ These forms of informal adaptation went beyond short-term necessity, fostering a local culture

¹The Volstead Act, formally the National Prohibition Act of 1919, implemented the 18th Amendment by prohibiting the manufacture, sale, and transportation of intoxicating liquors nationwide. It marked the start of national prohibition in January 1920.

²The term “institutional clash” refers to conflicts between formal rules imposed by the state and informal local norms and practices. In this context, it captures the tension between top-down prohibition mandates and local opposition, which spurred the development of informal economic responses—such as bootlegging, speakeasies, and underground coordination—that later persisted as cultural norms shaping modern entrepreneurial resilience (North, 1990; Okrent, 2010).

³Speakeasies were illicit establishments that sold alcohol during prohibition, often operating in secret and requiring passwords or private invitations to enter. They became central to underground social and economic life in many communities (Okrent, 2010).

of improvisation and resistance to laws perceived as illegitimate (Levine and Reinerman, 1991; McGirr, 2016).

I hypothesize that early exposure to state-imposed prohibition, where top-down alcohol bans conflicted with local preferences, fostered durable cultural norms rooted in resistance and informal adaptation. These norms re-emerged during the COVID-19 crisis to shape entrepreneurial behavior. This perspective builds on a large literature showing that cultural traits and norms are path-dependent and can persist across generations (Acemoglu et al., 2001; Nunn, 2009; Alesina and Giuliano, 2015; Guiso et al., 2016; Dell, 2010; Naritomi et al., 2012). Entrepreneurial culture, in particular, has been shown to be persistent (Chen and Liu, 2023; Fritsch and Wyrwich, 2018; Andersson and Koster, 2011; Audretsch and Keilbach, 2004).⁴ By pushing alcohol underground, the prohibition shock created illicit economies that demanded improvisation, flexibility, and informal coordination (Blocker, 2006; Okrent, 2010). Research on informal economic activity suggests that such underground markets can function as training grounds for entrepreneurship by cultivating risk-taking, coping strategies, and adaptive capacity in environments where formal protections are absent (Webb et al., 2009; C. Williams and Nadin, 2010; Portes, 1994; Venkatesh, 2006). Experiences of resistance to illegitimate laws can also persist through collective memory, shaping later economic behavior (Puntscher et al., 2014). In this context, prohibition functioned as a formative institutional shock. In counties where statewide mandates overrode local norms, it embedded behavioral patterns that later re-emerged as adaptive responses during the COVID-19 crisis.

Using county-level data on prohibition laws (Sechrist, 2012) and modern business applications (U.S. Census Bureau, 2014–2023), I construct a continuous measure of forced exposure to prohibition: the number of years a county was forcibly dry prior to 1920. Because statewide mandates were enacted at different times but, once adopted, applied uniformly within each state, variation in forced dry years is plausibly unrelated to local entrepreneurial capacity and instead reflects exposure to institutional conflict. An event-study analysis shows that counties with longer histories of forced prohibition experienced significantly larger increases in business

⁴Evidence from Poland similarly highlights the historical roots of regional differences in entrepreneurship (Fritsch et al., 2021).

applications following COVID-19. Specifically, each additional year of forced prohibition is associated with around 2.5% post-pandemic increase in new business applications. By contrast, voluntary prohibition exposure has no effect, suggesting that it was institutional conflict rather than cultural conservatism that generated lasting impacts. The results remain consistent across a range of robustness checks and alternative specifications.

To shed light on mechanisms, I present evidence consistent with three channels. First, effects are strongest in counties with greater historical resistance to prohibition, as measured by anti-prohibition party vote shares, consistent with the importance of institutional conflict. Second, counties with more evidence of illicit activity during prohibition—measured by digitized newspaper references to bootlegging and speakeasies—show larger modern entrepreneurial responses, suggesting that illicit adaptation served as a training ground for entrepreneurship. Third, effects concentrate in counties with limited modern remote-work feasibility, where displaced workers had few alternatives during the pandemic and turned to self-employment as an adaptive response. Finally, prohibition-exposed counties were also more resilient to earlier labor market disruptions such as the China shock, pointing to a broader adaptive capacity that extends beyond COVID-19.⁵ Taken together, this evidence supports the view that prohibition cultivated a durable repertoire of adaptive behaviors that persisted across generations and re-emerged during major economic shocks such as the COVID-19 pandemic.

This study contributes to three literatures. First, it adds to work on institutional and cultural persistence by showing how localized institutional conflict can forge durable behavioral norms that shape modern economic outcomes (Acemoglu et al., 2001; Nunn, 2009; Alesina and Giuliano, 2015; Fritsch and Wyrwich, 2018; Fritsch et al., 2021; Bazzi et al., 2023; Bazzi et al., 2021). Second, it extends Andrews (2023), who shows that prohibition disrupted informal invention networks, by demonstrating that these disruptions also cultivated informal adaptation with long-run effects on entrepreneurship. Third, it offers a cultural explanation for the uneven geography of pandemic-era entrepreneurship by linking county-level variation in business formation to historical differences in adaptive capacity. While prior research

⁵China Shock refers to the surge in Chinese import competition in U.S. manufacturing regions between 1991 and 2007, as documented by Autor et al. (2013)

emphasizes contemporary factors such as policy responses, remote work feasibility, or labor market conditions (R. W. Fairlie, 2020; Haltiwanger, 2022), this paper provides the first evidence that enduring cultural repertoires, forged through resistance to institutional conflict during prohibition, helped shape modern entrepreneurial resilience.

The remainder of the paper proceeds as follows. Section 2 provides historical background on U.S. alcohol prohibition. Section 3 outlines the data sources and empirical strategy. Section 4 presents the main results and a series of robustness checks. Section 5 explores heterogeneity and mechanisms. Section 6 concludes.

2. BACKGROUND

2.1 Alcohol Prohibition in U.S. History

National prohibition began in 1920 with the Volstead Act, which banned the manufacture and sale of alcohol across the United States. By then, however, regulation had already been a source of local conflict for decades (Okrent, 2010). During the 19th century, the temperance movement portrayed alcohol as a cause of poverty and moral decline. Organizations such as the Anti-Saloon League and the Women’s Christian Temperance Union pushed for restrictions, which led to a patchwork of local and state laws well before national prohibition (McGirr, 2016). By the early 20th century, more than half of U.S. counties had experimented with prohibition in some form, often well before the federal ban (Sechrist, 2012). Statewide mandates did not arrive simultaneously; some states, such as Kansas and Georgia, enacted statewide bans by the 1880s, while others imposed them during the early 1900s.

Counties differed both in whether they adopted alcohol bans and in how those bans came into effect. Many counties went dry voluntarily through local referenda that aligned community sentiment with formal institutions. Other counties stayed wet until state legislatures enacted blanket bans. These state-level interventions were often politically driven, reflecting religious and populist coalitions at the statehouse level rather than local majorities (Pegram, 1997; Timberlake, 1963). As a result, these top-down mandates frequently overrode local sentiment, creating institutional conflict between state policy and community preference. This distinction between voluntary and forced prohibition is central. Voluntary adoption reflected temperance

preferences, while forced adoption represented an externally imposed constraint that generated institutional conflict.

This variation between voluntary and forced prohibition exemplifies a broader pattern of institutional misalignment, in which formal laws imposed by the state conflict with local informal norms and enforcement capacities (North, 1990; Greif and Laitin, 2004; Acemoglu et al., 2005; Helmke and Levitsky, 2004).⁶ When formal and informal institutions diverge, communities often construct parallel arrangements to navigate or resist state authority. In institutional theory, such environments are described as “institutional voids” that can stimulate entrepreneurial activity (Estrin and Prevezer, 2010; Mair and Martí, 2009).⁷

Research on entrepreneurship in informal settings shows that when formal rules are weak or misaligned, actors rely on informal networks, trust, and reputation to substitute for formal governance, developing adaptive and strategic responses to institutional gaps (Webb et al., 2009; Webb et al., 2014; N. Williams and Vorley, 2015). These dynamics parallel the adaptive behaviors observed under prohibition, where institutional conflict fostered informal coordination and rule-bending practices closely linked to entrepreneurial resourcefulness.⁸ In this context, variation in the timing and intensity of forced prohibition provides a natural measure of institutional misalignment, which is an ideal setting to study how communities adapt when formal authority conflicts with local norms.⁹

The bans imposed major economic and social costs. In many towns, breweries, distilleries, and saloons were among the largest local employers, and nationally more than 250,000 saloons operated by the 1910s (Okrent, 2010). Their closure eliminated not only access to alcohol but also a major source of income and employment, which caused substantial local job losses and economic disruption (Dills et al., 2005; Howard and Ornaghi, 2021). The enforcement

⁶Institutional misalignment refers to a divergence between formal rules (laws, enforcement, or regulations) and informal institutions (norms, social expectations, or networks). When this gap widens, actors often rely on informal mechanisms to substitute for or resist formal authority (Helmke and Levitsky, 2004; North, 1990).

⁷In institutional theory, such environments are described as “institutional voids” that can stimulate entrepreneurial activity (Estrin and Prevezer, 2010; Mair and Martí, 2009).

⁸N. Williams and Vorley (2015) describe such settings of formal–informal divergence as “institutional asymmetry,” where weak legitimacy of state rules compels actors to rely on informal practices for economic activity.

⁹This approach links the historical institutional conflict of prohibition to modern theories of institutional asymmetry and informal adaptation, situating prohibition as an early case of localized misalignment between formal and informal governance structures.

of these laws was uneven, and people often found ways to adapt. Pharmacies and physicians prescribed whiskey as “medicine” under legal loopholes (Burnham, 1968; Hall, 2010). Women also participated through home production, while consumers developed new drinking habits as cocktail culture emerged to mask the taste of bootleg alcohol (McGirr, 2016; Okrent, 2010). These adaptations highlight how prohibition fostered informal economic activity and resourcefulness, providing an early example of the adaptive dynamics explored here.

Scholars have also studied the direct consequences of prohibition. Dills and Miron (2003) and Dills et al. (2005) show that prohibition affected health outcomes and alcohol consumption, while Miron and Zwiebel (1991) document shifts in demand. Edwards and Howe (2015) use crop yields to infer reductions in alcohol production, and Law and Marks (2019) find mixed mortality effects with increased risks from illicit consumption. Chrystoja et al. (2020) similarly report modest declines in cirrhosis mortality and Noghanibehambari and Fletcher (2023) show a long-run impact on longevity. Owens (2011) finds prohibition raised homicide rates, underscoring its unintended consequences for crime. Beyond health and crime, Andrews (2023) highlights how prohibition disrupted informal invention networks, with lasting consequences for innovation. Other work emphasizes broader institutional and political repercussions (Timberlake, 1963; Pegram, 1997; Thornton, 1991; Okrent, 2010; McGirr, 2016; Clark, 1976; Jeffers and Kyvig, 2000; Jensen, 1971). Yet no prior research has examined whether prohibition left a lasting cultural legacy that shaped modern entrepreneurship.

2.2 The COVID-19 Entrepreneurship Surge

The onset of the COVID-19 pandemic in early 2020 triggered an unprecedented economic contraction. Employment collapsed, uncertainty spiked, and millions of workers left wage jobs (Cajner et al., 2020; Bureau of Labor Statistics, 2020). Despite the severe economic dislocation, new business applications surged in mid-2020 and remained at record levels through 2022, far above pre-pandemic trends (R. W. Fairlie, 2020; Haltiwanger, 2022). Appendix Figure A1 documents this striking break in trend, with applications far exceeding their pre-2020 baseline.

The boom, however, was not evenly distributed. Figure 1 displays substantial geographic variation in the change in business formation, measured as the difference in average log applications between the pre-pandemic (2010–2019) and post-pandemic (2020–2022) periods.

While some counties experienced dramatic increases in applications, others exhibited modest response. Standard explanations such as industry composition, local labor market conditions, and the feasibility of remote work account for part of this pattern (Dinlersoz et al., 2021; R. Decker and Haltiwanger, 2024), but they do not fully explain the divergence (Haltiwanger, 2022).

Much of the new business activity also reflected adaptation rather than pure opportunity (R. Fairlie et al., 2023; R. W. Fairlie and Fossen, 2017). In many counties, entrepreneurship substituted for disrupted wage work, with surges concentrated in sectors such as retail, transportation, and personal services, where workers could quickly pivot to self-employment (R. W. Fairlie, 2020; Barrero et al., 2021; Cajner et al., 2020). This uneven geography suggests that some communities had a greater capacity than others to adapt under crisis conditions. Understanding the roots of this adaptive capacity requires looking beyond contemporary economic factors to deeper historical legacies.

2.3 Linking History to Modern Adaptation

Historical institutional shocks like alcohol prohibition provide a lens for understanding modern economic resilience, particularly when contemporary crises echo past disruptions. A large body of research shows that cultural and institutional traits forged in earlier periods can persist across generations, shaping behavior long after the original conditions have faded (Nunn, 2009; Alesina and Giuliano, 2015; Guiso et al., 2016). When bans were imposed involuntarily before 1920, they represented sudden, top-down interventions that clashed with local norms. Communities had to rebuild informal social and economic networks as saloons closed and legal markets disappeared (Andrews, 2023; Burnham, 1968; Miron and Zwiebel, 1991).

Adaptation under prohibition required more than concealment. Bootlegging and speakeasies operated under constant threat of enforcement, compelling participants to improvise when supply chains broke down. These illicit activities fostered practices closely associated with entrepreneurship such as flexibility, innovation under constraint, and reliance on informal institutions. The literature on informality underscores this logic: underground markets can serve as training grounds for entrepreneurship, cultivating coping strategies and resourcefulness where formal protections are weak (C. Williams and Nadin, 2010; Webb et al., 2009, C. C. Williams,

2006). Prohibition thus did more than regulate alcohol; it created conditions in which communities practiced and transmitted adaptive behaviors akin to entrepreneurial skills.

The COVID-19 pandemic provides a modern parallel. Like prohibition, it was an exogenous institutional shock that disrupted formal economic structures, shuttered businesses (Barrero et al., 2021; Cajner et al., 2020), and forced communities to navigate restrictive rules (Chetty et al., 2020). Many workers pivoted to self-employment when wage jobs disappeared, and new business applications surged in precisely those sectors where adaptability mattered most (R. W. Fairlie, 2020; Haltiwanger, 2022). If prohibition cultivated a latent repertoire of informal adaptation, then COVID offered a natural test of its persistence. Counties with longer exposure to forced prohibition may have drawn on those adaptive legacies to respond more entrepreneurially. In this sense, prohibition represents a canonical case of cultural persistence—an intense but short-lived institutional conflict whose behavioral imprint remains visible in modern economic resilience. The empirical analysis below tests this prediction and explores the mechanisms through which historical legacies shaped entrepreneurial resilience.

3. EMPIRICAL STRATEGY

3.1 Data

The main explanatory variable is county-level exposure to prohibition before the Volstead Act of 1920. This measure combines the county-level prohibition database compiled by Sechrist (2012) with state prohibition enactment dates from Lewis (2008), producing an annual county–year panel covering 1880–1920. For each county and year, the dataset records whether alcohol sales were prohibited and whether the prohibition resulted from a local referendum or a statewide mandate.¹⁰

A county is coded as voluntarily dry if it adopted prohibition locally before its state imposed a ban. By contrast, a county is coded as forced dry if it remained wet until compelled by a statewide mandate that overrode local preferences. The cumulative number of forced-dry years between

¹⁰See Appendix Table A21 for the year each state enacted prohibition and whether the law was implemented through a legislative statute or statewide referendum. States adopting either channel before 1920 are coded as imposing forced prohibition on counties that had not yet adopted local bans. Because both channels compelled compliance from counties that had not adopted prohibition locally, I code them equivalently as forced dry. Results are robust to distinguishing between these channels (Appendix Figure A13).

1880 and 1920 captures the intensity of exposure to externally imposed prohibition and is used as the continuous treatment variable. Because statewide mandates were legislated independently of local conditions, variation in cumulative forced-dry years is plausibly unrelated to county-level entrepreneurial potential. For comparison, the cumulative number of voluntary dry years serves as a falsification test.¹¹ If the results reflected underlying temperance sentiment, both voluntary and forced exposure should predict modern entrepreneurship. Figure 2 illustrates the cumulative forced dry years across U.S. counties, with Appendix Figure A2 displaying voluntary dry years for reference.

Entrepreneurial activity is measured using new business applications from the Business Formation Statistics (BFS; U.S. Census Bureau, 2023c). These filings capture the intention to start a business, even if not all applications result in operational firms.¹² BFS data, drawn from administrative records, cover nearly all U.S. counties and are aggregated to the county-year level for 2014–2023. The dependent variable is the log of total business applications.

To complement this measure of entrepreneurial intent, establishment entry rates from the Business Dynamics Statistics (BDS; U.S. Census Bureau, 2023b) and nonemployer firm counts from the Nonemployer Statistics (NES; U.S. Census Bureau, 2023d) serve as indicators of realized entrepreneurial activity. To address potential confounders, modern covariates from the American Community Survey (ACS; U.S. Census Bureau, 2023a) and the Bureau of Economic Analysis (BEA; U.S. Department of Commerce, Bureau of Economic Analysis, 2023) are included to control for differences in population, income, racial and gender composition, and sectoral employment.

Figure 3 presents an initial comparison, showing the percent change in business applications relative to 2019 for counties in the top and bottom quartiles of forced prohibition exposure. The two groups evolve similarly before 2020, but a clear divergence appears afterward. Counties

¹¹Voluntary dryness is unlikely to be exogenous to local preferences and institutions. It is included solely as a falsification test, and the absence of a corresponding effect supports the interpretation that results for forced prohibition are not driven by pre-existing temperance sentiment.

¹²BFS data are based on Employer Identification Number (EIN) applications filed with the U.S. Internal Revenue Service, capturing both likely employer and nonemployer business starts. Because EIN applications are filed prior to firm formation, they measure entrepreneurial intent rather than realized entry. The series are available monthly and aggregated here to the county-year level. BFS has become the standard proxy for entrepreneurial intent (see Dinlersoz et al. (2021), Haltiwanger (2022), and R. Decker and Haltiwanger (2024)).

with the highest exposure experience a markedly larger surge in new applications. While these descriptive patterns are informative, they do not control for county characteristics or broader macroeconomic shocks. The next section introduces a formal event-study design with county and state-by-year fixed effects to isolate the causal impact of prohibition legacies on modern entrepreneurship.

3.2 Estimation

To test whether prohibition legacies shaped modern entrepreneurship, I estimate event-study models that exploit county-level variation in exposure to state-imposed prohibition prior to 1920. The treatment variable is the cumulative number of years a county was forced dry, defined as years when statewide mandates prohibited alcohol despite local opposition.¹³ This measure represents exposure to prohibition as an externally imposed shock that created institutional conflict and pressures for informal adaptation.

The event-study framework traces how business applications evolved across counties with different levels of forced prohibition exposure, relative to the pre-pandemic baseline of 2019. Formally, the estimating equation is:

$$(1) \quad Y_{ct} = \mu + \sum_{j=\min}^{\max} \beta_j \text{ForcedDryYears}_c \times 1\{\text{Years to 2020}_t = j\} + \theta_c + \gamma_t + \alpha_{st} + \lambda' X_{ct} + \varepsilon_{ct},$$

where Y_{ct} is the log of business applications in county c and year t . The coefficients β_j capture the dynamic effect of prohibition exposure relative to 2019. County fixed effects (θ_c) absorb time-invariant differences across counties, year fixed effects (γ_t) account for nationwide shocks, and state-by-year fixed effects (α_{st}) flexibly control for state-level policy responses during the pandemic. The controls (X_{ct}) include population density, white share, sex ratio, per capita income and manufacturing employment share.¹⁴

Recent methodological work points out that continuous-treatment event studies can be diffi-

¹³County-level prohibition status from Sechrist (2012) is merged with state mandate dates from Lewis (2008) by matching counties to the first year their state imposed a ban. Forced-dry years sum all years between the state's mandate and 1920 for counties that had not previously adopted prohibition locally.

¹⁴Appendix Table A6 presents summary statistics for control variables in detail.

cult to interpret, because the coefficients combine heterogeneous comparisons across units with different treatment intensities (Callaway et al., 2024). To address this concern, I complement the continuous specification with a flexible binned version that groups counties by exposure intensity:

$$(2) \quad Y_{ct} = \alpha_c + \gamma_t + \alpha_{st} + \sum_q \sum_j \beta_{qj} 1\{\text{ExposureBin}_c = q\} \times 1\{t - 2020 = j\} + \varepsilon_{ct},$$

where the treatment variable is discretized into five exposure bins q (0, 1–3, 4–6, 7–14, and 15+ years of forced prohibition).¹⁵ The coefficients β_{qj} trace dynamic effects for each group relative to the baseline of zero years.¹⁶ This specification relaxes the assumption of linearity and allows the dynamic effects to vary flexibly across different levels of exposure. The specifications above allow for a dynamic comparison of counties with different levels of forced-prohibition exposure. The next section presents the main results on business applications and their evolution following the COVID-19 shock.

4. FORCED PROHIBITION AND MODERN BUSINESS APPLICATIONS

Estimates of equation (1) for modern business applications are presented in Figure 4.¹⁷ Pre-treatment β_j coefficients are statistically insignificant and exhibit no apparent trend, which is consistent with the parallel trend assumption. In the post-COVID period, the estimates display a sharp positive shift, providing evidence that counties with more years of forced prohibition experienced substantially larger increases in new business applications. The estimates imply that each additional year of forced prohibition exposure is associated with a 2-3 % increase in business applications in the post-COVID period.¹⁸ The corresponding two-period DiD estimate (i.e., estimate of β) is 0.017 (p-value <0.01). The magnitude is economically meaningful. Moving from zero to ten years of forced prohibition exposure corresponds to an increase of

¹⁵Bins were chosen to align with both substantive thresholds (short, medium, long, generational exposure) and natural breaks in the distribution of forced prohibition years across counties.

¹⁶In alternative specification, I also use low exposure as the baseline group.

¹⁷Results are similar when using log business applications per capita as the outcome (Appendix Figure A3).

¹⁸The results are robust to clustering at the state or county levels.

roughly 17% relative to baseline. For comparison, R. A. Decker and Haltiwanger (2023) report that the median U.S. county experienced a 40 log-point increase in business applications during the pandemic ($\approx 49\%$), while counties in the top quintile saw growth ranging from 61 to 289 log points ($\approx 84\%$ to over 1,200%). In this context, the 17–20% increase associated with forced prohibition exposure represents a meaningful share of the observed variation, particularly given the historical nature of the treatment and its durability over a century.

While these continuous-treatment estimates are informative, recent methodological work highlights potential challenges in interpreting continuous-treatment DiD event studies (Callaway et al., 2024). In light of these concerns, I divide counties into quartiles of forced prohibition exposure and estimate equation (2), comparing each group with the lowest-exposure counties. Figure 5 shows a clear dose-response pattern: counties in the highest quartile exhibit the largest and most persistent increases in business applications after COVID, while effects in the lower quartiles are modest. This pattern mirrors the continuous estimates but avoids concerns about functional form assumptions, reinforcing the interpretation that greater forced prohibition exposure is associated with stronger entrepreneurial responses. A simpler binary specification of equation (1), comparing counties with any forced prohibition to those with none, yields consistent results (Appendix Figure A4).

A potential concern is that the results could reflect unobserved differences in temperance sentiment. If pre-existing cultural preferences were driving the effect, then voluntary prohibition, which reflected strong local support for temperance, should also predict heightened entrepreneurial responses. As shown in Figure A6, re-estimating equation (1) using voluntary prohibition exposure produces no systematic differences across counties. Pre-trends remain aligned, further supporting the interpretation that voluntary prohibition does not predict differential trajectories in business applications. This contrast suggests that it was the experience of externally imposed prohibition and the informal adaptation it required rather than voluntary adoption that generated a lasting entrepreneurial legacy.¹⁹

¹⁹Event-study estimates from equation (1) of forced dry years remain similar when controlling for the interaction of time-to-event dummy with the voluntary dry years (see Appendix Figure A7).

4.1 Robustness

One key identification concern is that the observed effect of forced exposure might simply reflect the absence of voluntary temperance sentiment rather than the legacy of institutional conflict. In this view, counties that did not adopt prohibition voluntarily but were later forced to go dry may have had systematically different preferences or cultural traits such as weaker moral conservatism, stronger commercial alcohol interests, or broader resistance to social regulation. These underlying differences could confound the relationship between forced prohibition and modern entrepreneurial outcomes, making it unclear whether the effect stems from institutional conflict or pre-existing local attitudes. To address this, I restrict the sample to counties that experienced at least some forced prohibition. This comparison holds constant the decision not to adopt prohibition voluntarily and exploits only variation in the duration of forced exposure. Figure 6 shows similar results but with slightly larger magnitudes, confirming that the main findings hold when restricting the sample to counties that experienced some degree of forced prohibition. A simpler binary specification of equation (1), comparing counties with highly forced prohibition to those with less while excluding counties with zero years, yields consistent results (Appendix Figure A5). Figure 7 presents the corresponding binned specification from equation (2) comparing the effect of the intensity among counties that all eventually experienced forced prohibition. These patterns reinforce the interpretation that the findings reflect the intensity of externally imposed prohibition rather than differences in voluntary adoption. If the main results were driven purely by unobserved differences in cultural preferences, we would not expect to see a dose-response relationship among counties that were all reluctant adopters.

To further address concerns that the results may capture broader regional or geographic unobserved heterogeneity rather than the historical institutional conflict, I re-estimate the event-study specification in equation (1) within adjacent cross-state county pairs by controlling for border-pair fixed effects. These fixed effects absorb all time-invariant differences between neighboring counties that share geography, climate, and local markets but were subject to different pre-1920 prohibition mandates. Appendix Figure A8 reports the corresponding event-study coefficients. The pre-trends are flat, and the post-2020 coefficients turn positive, which suggest that even within shared geographic environments, the side historically forced dry experienced a sharper

entrepreneurial response to the COVID shock. The similarity in timing and magnitude to the baseline results reinforces a consistent interpretation rather than a purely regional correlation.²⁰

If the estimated effects simply reflected differences across counties with distinct demographic or economic characteristics, then allowing for flexible trajectories by these covariates should attenuate the results. To test this, I re-estimate the event-study by including interactions between covariates and event-time indicators. Figure 8 reports the estimates when including modern controls such as population density, income, education, immigration, race, and sectoral employment shares.²¹ Figure 9 repeats the exercise using historical covariates including agricultural productivity, manufacturing share in 1890, immigrant and Black population shares in 1890, ruggedness, and distance to ports or coasts.²² In both cases, pre-treatment coefficients show no systematic differences, and post-2020 effects remain stable and positive.²³ These findings indicate that the main results are not driven by differential trajectories associated with observable county characteristics.²⁴

Finally, to ensure that the results are not driven by late adopters or referendum-related exposure, two additional robustness checks are conducted. Excluding counties that became dry only under the federal Volstead Act of 1920 leaves the main results virtually unchanged (Appendix Figure A11), indicating that late holdouts do not drive the findings.²⁵ Likewise, excluding states that adopted prohibition through statewide referenda, where exposure may have reflected stronger local preferences rather than external imposition, produces similar estimates (Appendix Figure A13). The consistency of these results confirms that the main findings are not sensitive to the inclusion of late adopters or referendum-driven counties.

²⁰Standard errors are relatively large because the border-pair design relies on a limited number of independent geographic clusters (616 adjacent cross-state county pairs) and includes a rich set of fixed effects—border-pair, year, and state-by-year—which absorb most of the cross-sectional variation. The identifying variation therefore comes from highly local, within-pair differences over time, resulting in conservative inference.

²¹Modern controls are measured in 2018, prior to the pandemic. Results are similar when using time-varying controls interacted with year dummies.

²²The historical covariates are from Bazzi, S., Fiszbein, M., & Gebresilasse, M. (2020).

²³In Figure 9, I also show that adding TFE leaves the prohibition estimates virtually unchanged. TFE (‘Total Frontier Experience’) is Bazzi, S., Fiszbein, M., & Gebresilasse, M. (2020)’s measure of the number of years a county spent on the U.S. frontier between 1790 and 1890, used as a proxy for frontier persistence and individualism.

²⁴Appendix Figures A10 and A9 show covariate balance across exposure levels. The largest differences appear in agricultural productivity and Black population share, but robustness checks that flexibly control for these confirm that they do not account for the main results.

²⁵Appendix Figure A12 shows the results are similar when we exclude also those that were forced after 1917 as extra robustness checks.

5. MECHANISMS

Through what channels did forced prohibition shape modern entrepreneurial resilience? Appendix Figure A14 provides a visual summary of the hypothesized pathway. When statewide prohibition mandates overrode local preferences, they created institutional conflict that was most intense in areas with strong anti-prohibition sentiment. This conflict encouraged residents to resist and adapt informally by maintaining illegal saloons, organizing bootlegging networks, and developing alternative forms of coordination outside formal institutions. These experiences fostered flexibility, risk taking, and ingenuity that gradually became embedded in local norms of adaptation. When the COVID-19 pandemic again disrupted formal economic structures a century later, these norms resurfaced. Communities historically exposed to forced prohibition responded more entrepreneurially, drawing on the same informal capacities that had once enabled them to navigate restrictive laws. The subsections below provide evidence for each step in this sequence.

5.1 Resistance

When states imposed prohibition on counties, they set off a direct conflict between law and local preferences. This conflict mattered not only for determining which counties were dry, but also for how prohibition was experienced. Historical accounts document how residents resisted these mandates, sustaining saloons, developing bootlegging operations, and relying on informal networks despite the ban (Levine and Reinerman, 1991; Dupré 2004; Klein 2014; Hoffman 2019). Such resistance not only undermined enforcement but also reinforced distrust of state authority, nurturing a durable oppositional culture.

To quantify the intensity of this local oppositional culture, I rely on historical county-level congressional vote shares against pro-prohibition parties from Clubb et al. (2006). The share of votes cast for anti-prohibition (“wet”) parties relative to total votes serves as an indicator of resistance.²⁶ Averaging these shares across elections between 1900 and 1933 yields a stable

²⁶Following Lewinson (1928), Timberlake (1963), and Pegram (1997), pro-prohibition (“dry”) parties include the Prohibition, Progressive, American, Liberty, and Republican parties (including Republican Equal Rights). Anti-prohibition (“wet”) parties include the Democratic, Socialist, and Labor parties (including Socialist–Labor and Farmer–Labor).

county-level proxy for opposition.²⁷ Figure 10 presents event-study estimates from equation (1) separately for counties in the highest and lowest quartiles of resistance. The results show a substantially larger post-COVID increase in business applications among counties with stronger anti-prohibition sentiment. Before the pandemic, both high- and low-resistance groups followed similar trajectories, consistent with parallel pre-trends. This pattern implies that prohibition legacies were amplified by resistance: entrepreneurial resilience resurfaced most strongly where the clash between state law and local preferences had been most intense.

5.2 Informal Adaptation

Prohibition did not eliminate alcohol consumption; it pushed it underground (Miron and Zwiebel, 1991). In counties where state laws clashed with local preferences, residents adapted by running speakeasies, bootlegging liquor and building informal distribution networks (Burnham, 1968). These illicit practices required residents to develop adaptability, risk-taking, and organizational skills closely tied to entrepreneurship. If such practices persisted as cultural norms, then we should observe stronger long-run entrepreneurial responses in counties with more evidence of illicit activity during prohibition.

I construct a county-level measure of ‘illicit culture’ from digitized historical newspapers accessed via Newspapers.com, counting total references to bootlegging, speakeasies, and dry law violations between 1900 and 1940.²⁸ Appendix Figure A15 displays the geographic distribution of this measure, grouping counties by quantiles of newspaper references to the selected keywords.²⁹

Appendix Table A1 validates this measure. Counties with longer forced-prohibition exposure consistently exhibit higher newspaper references to illicit activity, even after controlling for geographic and historical characteristics such as distance to ports, agricultural productivity, and manufacturing share in 1890. The association is robust across alternative keyword sets

²⁷The resistance measure is constructed from county-level congressional election returns between 1900 and 1933 (ICPSR).

²⁸Because the selected keywords emerged with the spread of prohibition laws, newspaper mentions of them reflect contemporaneous enforcement and adaptation rather than prior cultural differences.

²⁹The baseline measure counts references to “bootlegging”, “speakeasy” and “dry law violation”. Results are robust to alternative keywords such as “moonshine” and “rum running”, which yield similar heterogeneity estimates.

and normalizations by the total number of newspapers. This pattern aligns with the idea that state-imposed prohibition directly encouraged informal adaptation.

Figure 11 plots the heterogeneous event-study results from equation (1) by historical exposure to illicit activity. Consistent with the parallel trend assumption, pre-2020 coefficients for both groups are small and statistically insignificant. After the onset of COVID-19, however, business applications rise much more sharply in counties with a stronger historical presence of bootlegging and speakeasies, with post-treatment coefficients roughly twice as large as those in low-illicit counties. This divergence indicates that the prohibition legacy was most pronounced where underground economic activity had been most prevalent. Counties that historically relied on informal adaptation during prohibition were also those that responded most dynamically through entrepreneurship during the pandemic.

5.3 Adaptation Capacity

The findings discussed above suggest that prohibition fostered a culture of informal adaptation. Here, I examine how this legacy manifested as a broader adaptive capacity during COVID-19, with effects in counties where labor market constraints left fewer alternatives.

If prohibition's legacy indeed reflected a durable capacity for adaptation, then its effects should be most pronounced where the pandemic created the greatest need for adjustment. Remote-work feasibility provides one way to capture this margin. In counties where a smaller share of jobs could shift online, displaced workers faced sharper constraints and had to improvise new livelihoods.³⁰

Figure 12 presents the heterogeneous event-study estimates by remote-work feasibility, comparing counties in the top and bottom quartiles of remote-work share. Before 2020, coefficients for both low- and high-remote counties are small and statistically indistinguishable from zero, consistent with parallel pre-trends. After the onset of COVID-19, the effects rise sharply in low-remote counties—where few jobs could transition online—while effects remain muted in high-remote counties. The post-2020 coefficients for low-remote areas are roughly

³⁰Remote-work feasibility scores are drawn from Dingel and Neiman (2020), who classify the share of tasks in each occupation that can be performed from home. I aggregate these scores to the county level using pre-pandemic industry and occupation employment distributions from the County Business Patterns (CBP), averaged over 2015–2018.

twice as large as those for high-remote ones, indicating that the prohibition legacy manifested most strongly where workers faced limited opportunities for remote employment. This pattern suggests that prohibition’s adaptive culture persisted across generations, re-emerging when modern labor markets constrained wage employment and required local improvisation through entrepreneurship.

5.4 Beyond Intentions

Business applications measure entrepreneurial intent, but not all applications become active firms. To verify that results extend to realized business activity, I examine establishment entry rates (BDS) and nonemployer establishments (NES). In Appendix Figures A16 and A17, the effects are smaller in magnitude—around 0.05 percentage points for entry rates and 0.4% for nonemployers but remain positive and statistically significant. The attenuation is consistent with the entrepreneurial funnel, in which only a fraction of applications become active firms, yet the persistence of effects across both margins indicates that prohibition legacies translated into realized ventures.

Next, I approximate the employment share of large firms (500 employees) using establishment size data from the County Business Patterns (CBP).³¹ Figure A18 shows that counties with higher levels of forced prohibition exposure experienced larger post-2020 declines in large-firm employment shares. Given that most county-level 500+ employers are in sectors with low remote feasibility (Dingel and Neiman, 2020), this pattern is consistent with a reallocation margin: workers in prohibition-exposed counties were more willing to leave inflexible large employers and start new ventures.

To further isolate the mechanism, I estimate a triple-difference (DDD) specification that interacts prohibition exposure with a post-2020 indicator and a dummy for sectors with low remote-work feasibility. This approach compares within-county changes in large-firm employment shares between high- and low-remote sectors, effectively controlling for both county-wide shocks and sector-specific trends. Appendix Table A2 from the Triple DDD results further sug-

³¹The large-firm employment share is calculated using CBP size classes. Employment in establishments with 500 employees is approximated by taking the midpoint of each class (500–999, 1,000–2,499, etc.), summing across categories, and dividing by total county employment. Results are robust to alternative midpoint assumptions. Pre-pandemic baselines are constructed using 2015–2018 CBP data.

gest that this decline is mostly coming from low remote sectors. This pattern aligns with broader labor market shifts during the “Great Resignation,” when voluntary quits surged in 2021–22 and fueled record levels of self-employment and new business formation (R. W. Fairlie, 2020; Haltiwanger, 2022).³² The decline in large-firm employment thus reinforces the interpretation that prohibition legacies influenced not only entrepreneurial entry but also the ability to adjust when traditional employment structures proved rigid.

5.5 Placebo Outcomes

To verify that the observed effects are specific to entrepreneurial activity rather than broader economic trends, I examine outcomes unlikely to be directly affected by local entrepreneurship. As placebos, I estimate event studies using (i) the share of establishments with 500 or more employees and (ii) total county employment. Very large establishments are rarely created or closed by local entrepreneurs, and aggregate employment primarily reflects macroeconomic conditions rather than small-business entry.³³ Consistent with an entrepreneurship-specific channel, coefficients from Appendix Figures A19 - A20 remain statistically indistinguishable from zero both before and after 2020. The absence of systematic effects on these outcomes supports the interpretation that the main results capture entrepreneurial responses rather than broader labor-market fluctuations.

5.6 Persistence Across Modern Shocks

The preceding analyses focused on how prohibition legacies shaped county-level entrepreneurial responses to the COVID-19 shock. If these legacies reflect a more general capacity for informal adaptation as a cultural channel, their influence should extend to other historical disruptions as well. Testing whether these effects persist across distinct shocks provides a stronger assessment of the durability of cultural adaptation. Both prohibition and the China Shock disrupted local economic structures and forced communities to adjust through informal and entrepreneurial channels. To test whether this adaptive capacity persisted beyond the pandemic, I examine how

³²The term was coined by organizational psychologist Anthony Klotz in 2021 to describe the unprecedented rise in voluntary job quits during the pandemic. Economists interpret it as a labor-market reallocation episode driven by shifting worker preferences and remote-work opportunities.

³³Large-establishment shares are constructed from County Business Patterns (CBP) size classes, while total employment is drawn from the same source.

prohibition exposure shaped local adjustment during an earlier structural disruption: the rise of Chinese import competition.

Following Autor et al. (2013), I measure commuting-zone exposure to import competition using the Bartik-style index constructed by Dorn and coauthors.³⁴ The “China shock” displaced manufacturing jobs in the 1990s and 2000s, forcing local economies to adjust through nontraditional employment and self-employment margins.

I estimate regressions of the form:

$$(3) \quad Y_{zt} = \alpha + \beta_1(ForcedDry_z \times ChinaShock_{zt}) + \beta_2 ChinaShock_{zt} + \theta_z + \gamma_t + \varepsilon_{zt},$$

where Y_{zt} is log nonemployer establishments, θ_z are commuting zone fixed effects, and γ_t are year fixed effects.

Panel A of Appendix Table A3 reports β_1 estimates from equation (3) across specifications including all NAICS sectors, excluding manufacturing, restricting to services, and separating tradable from non-tradable industries. The interaction term is positive and significant in all cases, with the largest effects in services and non-tradable sectors—precisely where informal adjustment was most feasible. Panel B confirms that the findings are robust to alternative aggregation methods for prohibition intensity.³⁵ Taken together, these results suggest that prohibition’s legacy extended beyond the COVID-19 crisis, shaping small-firm dynamism during an earlier structural shock.³⁶ The persistence of this interaction across contexts indicates that prohibition cultivated a broader adaptive capacity that re-emerges when communities confront disruptive economic change.

³⁴The index harmonizes UN Comtrade import flows to SIC87 industries and matches them to 1990 employment shares from County Business Patterns. Data are taken from Dorn’s publicly available China shock package.

³⁵Effects appear only for nonemployer establishments; Appendix Table A4 shows no significant impact on formal employer establishments, consistent with Autor et al. (2013).

³⁶Two caveats are worth noting. First, the China Shock analysis is conducted using commuting-zone-level data, resulting in a smaller sample and potential aggregation bias in the prohibition measure. Second, due to data limitations, the outcome variable captures realized business activity (nonemployer establishments) rather than entrepreneurial intent. The magnitude of the interaction effects is therefore smaller and should be interpreted as indicative rather than directly comparable to the main county-level estimates. Overall, these results should be viewed as suggestive evidence of persistence rather than as precise causal estimates.

6. CONCLUSION

This paper provides evidence that early twentieth-century prohibition forged enduring cultural legacies of adaptation and resilience. Counties with longer exposure to forced prohibition—where statewide mandates overrode local preferences—experienced significantly larger increases in business applications during the COVID-19 pandemic. By contrast, voluntary prohibition exposure had no effect, underscoring that it was institutional conflict and forced adaptation, rather than temperance sentiment, that left lasting marks.

The evidence indicates that prohibition fostered a culture of informal adaptation that persisted across generations. Mechanism analyses point to resistance politics, illicit economic activity, and limited remote-work feasibility as reinforcing channels. Additional tests show that the same legacy of adaptation re-emerged under earlier labor-market disruptions such as the China shock, suggesting that prohibition shaped not only responses to the pandemic but also broader capacities to adjust when formal institutions failed.

These findings contribute to three strands of literature. First, they advance work on historical persistence by documenting how short-lived institutional conflicts can generate enduring cultural legacies. Second, they extend research on prohibition and social networks by showing that its consequences reached beyond invention to entrepreneurship and resilience. Third, they add a cultural explanation to recent debates on the uneven geography of COVID-era entrepreneurship.

More broadly, the results demonstrate how institutional mismatches between state authority and local practice can leave durable cultural imprints, shaping economic behavior long after the original conflict has faded. Prohibition was not simply a moral crusade or a failed policy experiment; it was a formative episode that rewired local norms of adaptation and risk-taking. A century later, those norms resurfaced as entrepreneurial dynamism in the face of crisis—reminding us that the legacies of institutional conflict may shape resilience to shocks far beyond their original domain.

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- Williams, N., & Vorley, T. (2015). Institutional asymmetry: How formal and informal institutions affect entrepreneurship in bulgaria [Publisher: SAGE Publications Ltd]. *International Small Business Journal*, 33(8), 840–861. <https://doi.org/10.1177/0266242614534280>

Change in Business Applications Across U.S. Counties

Difference of Average Log Applications: 2020–2022 vs. 2010–2019

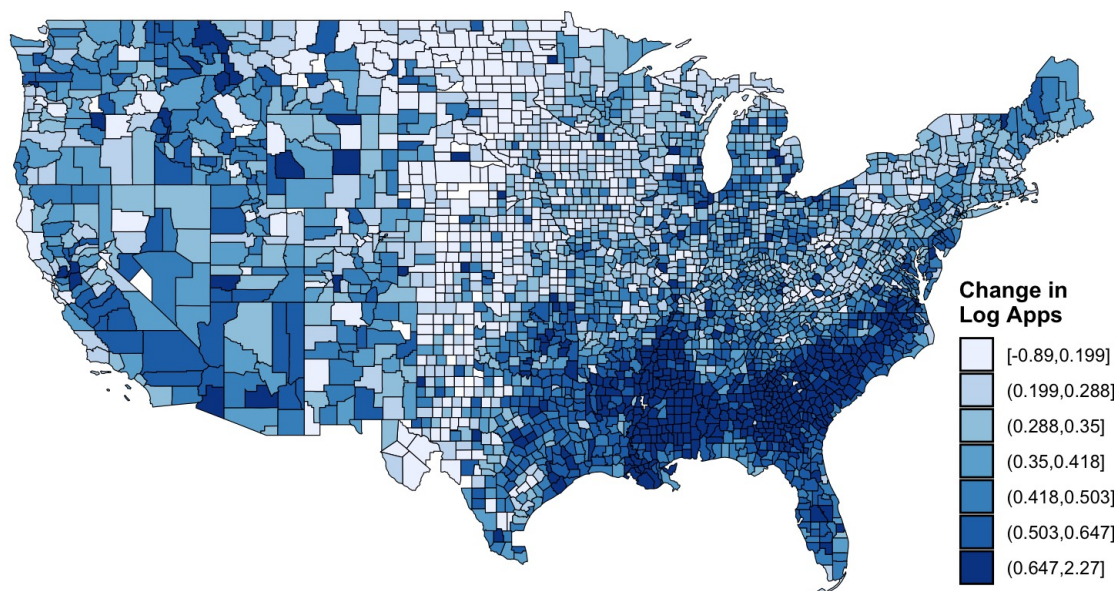


Figure 1: Change in Business Application Geographic Variation

Notes: This figure shows the change in business applications across U.S. counties, measured as the difference in average log applications between 2020–2022 and 2010–2019. Darker shading indicates larger increases. Source: Census Bureau Business Formation Statistics and population estimates.

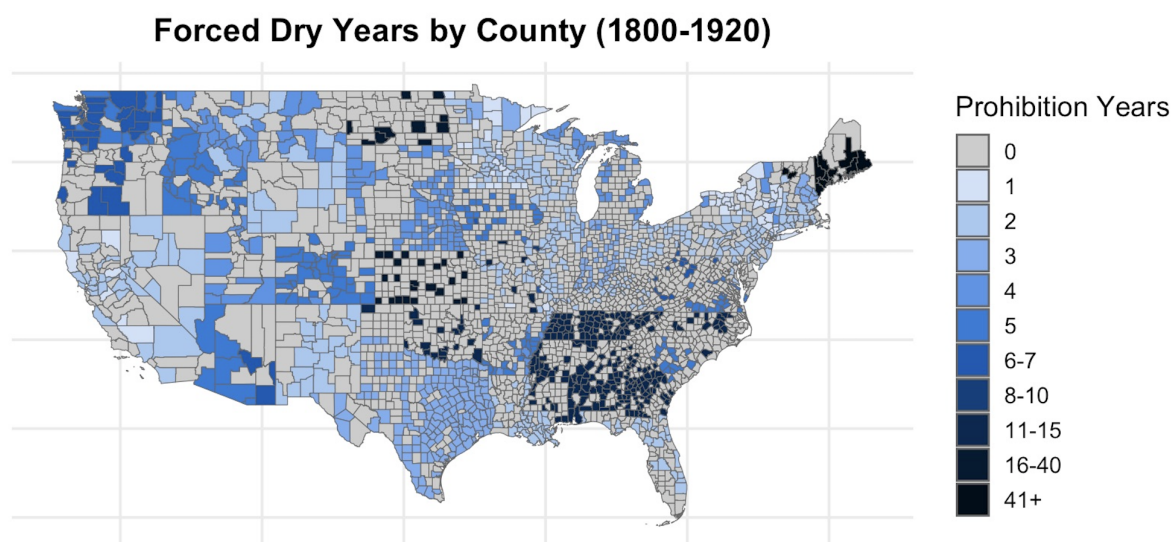


Figure 2: Forced Dry Years

Notes: This map shows the cumulative number of years each U.S. county was subject to forced prohibition between 1800 and 1920. Counties coded as forced dry are those that remained wet until compelled by statewide mandates, overriding local preferences. The treatment variable used in the analysis—Forced Dry Years—is calculated from the annual panel of county prohibition status compiled by Sechrist (2012) and state prohibition enactment dates from Lewis (2008). Counties shaded grey (0 years) were never subject to forced prohibition before 1920, while darker shades indicate longer durations of state-imposed exposure.

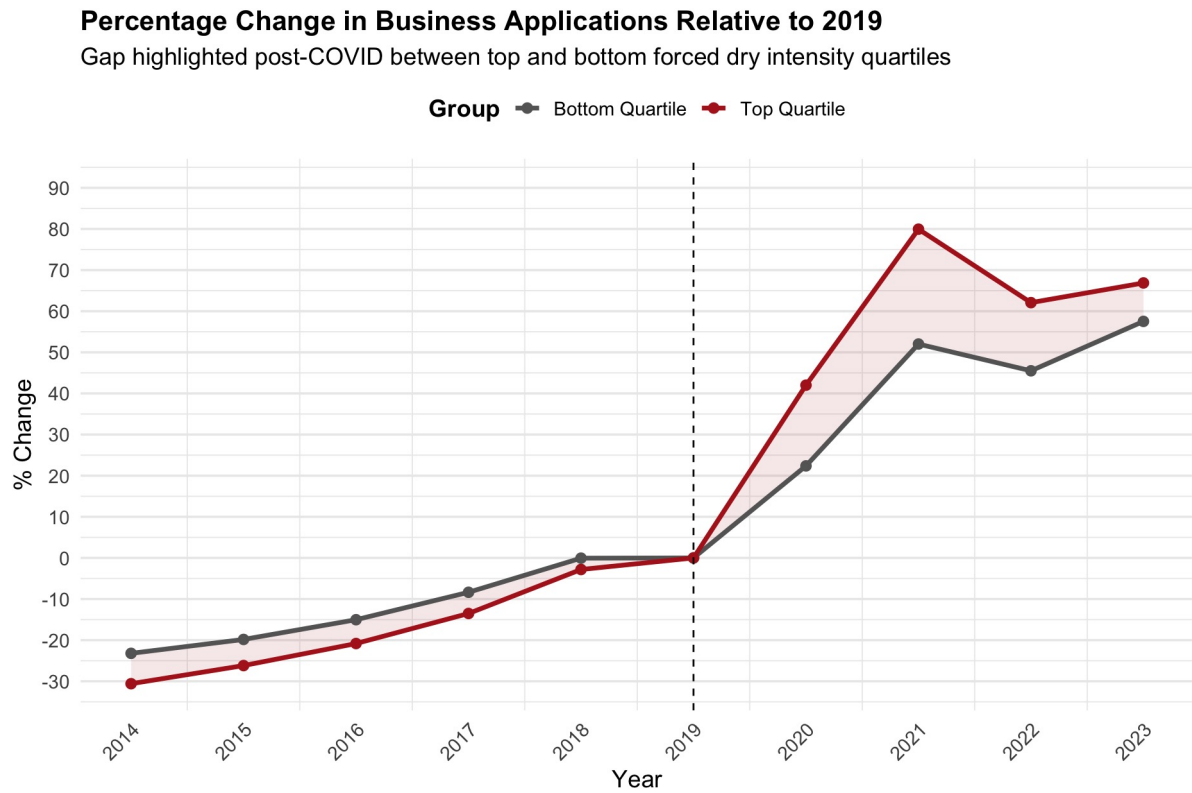


Figure 3: Change in Business Application Post-COVID

Notes: This figure plots the percent change in business applications relative to 2019 for counties in the top and bottom quartiles of forced prohibition exposure (measured as forced dry years prior to 1920). Business applications are from the U.S. Census Bureau's Business Formation Statistics (BFS), measured at the county-year level and log-transformed before aggregation.

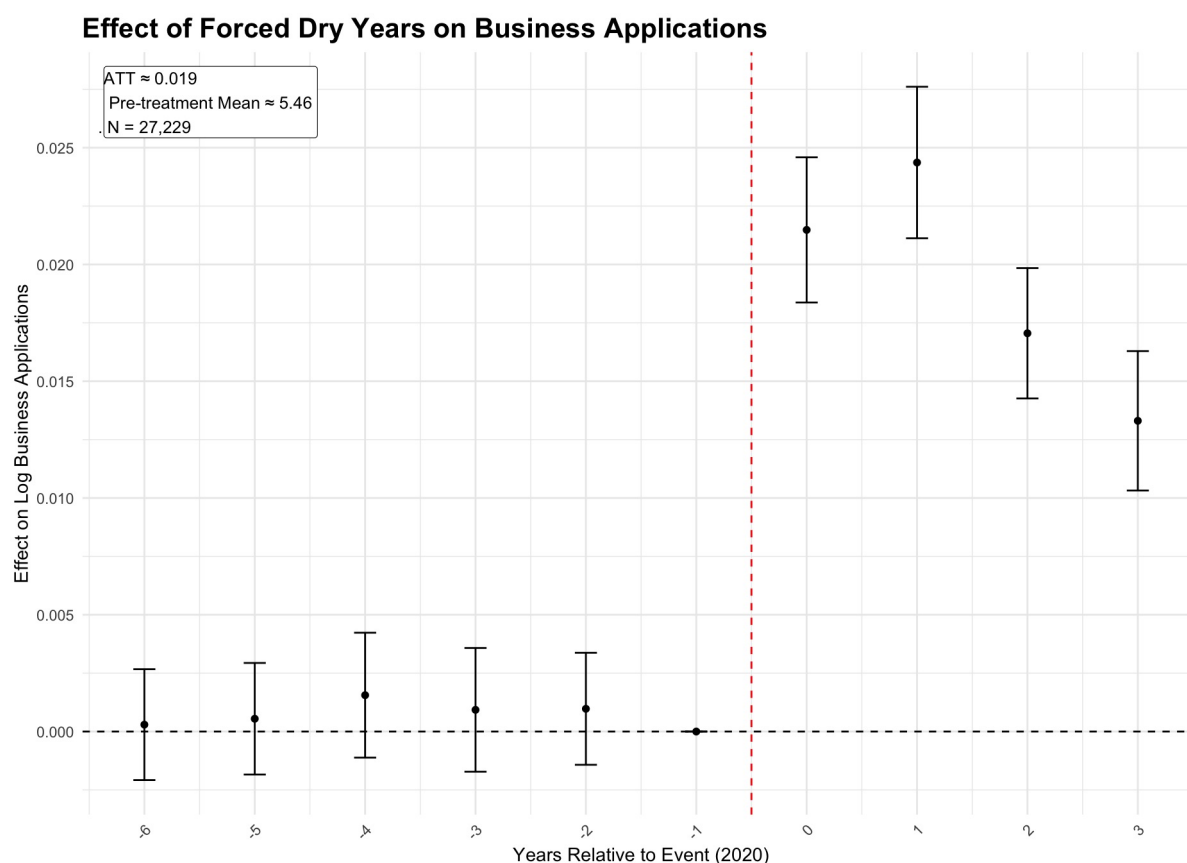


Figure 4: Effect of Forced Dry Years on Modern Business Applications

Notes: This figure reports event-study estimates of the effect of cumulative forced prohibition exposure (1880–1920) on modern business applications. The dependent variable is the log of total business applications from the U.S. Census Bureau’s Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time dummies relative to 2019, with 95% confidence intervals. All models include county fixed effects, year fixed effects, and state-by-year fixed effects. Main controls included are listed in Appendix A6. Standard errors are clustered at the county level.

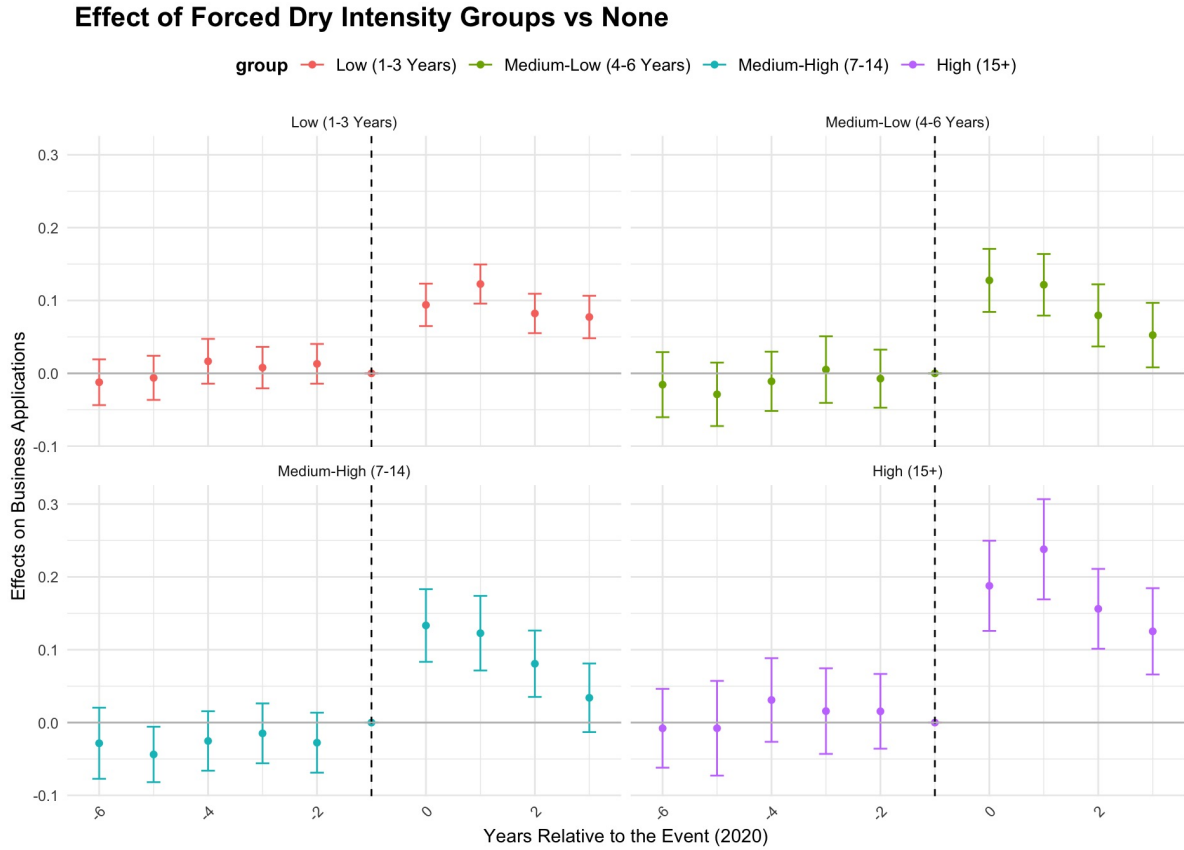


Figure 5: Effect of Forced Dry Groups on Business Applications

Notes: This figure reports estimates from equation (2), which bins counties into four groups of forced prohibition exposure—1–3 years (Low), 4–6 years (Medium-Low), 7–14 years (Medium-High), and 15+ years (High)—relative to a baseline of 0 years. The dependent variable is log business applications from the U.S. Census Bureau’s Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level.

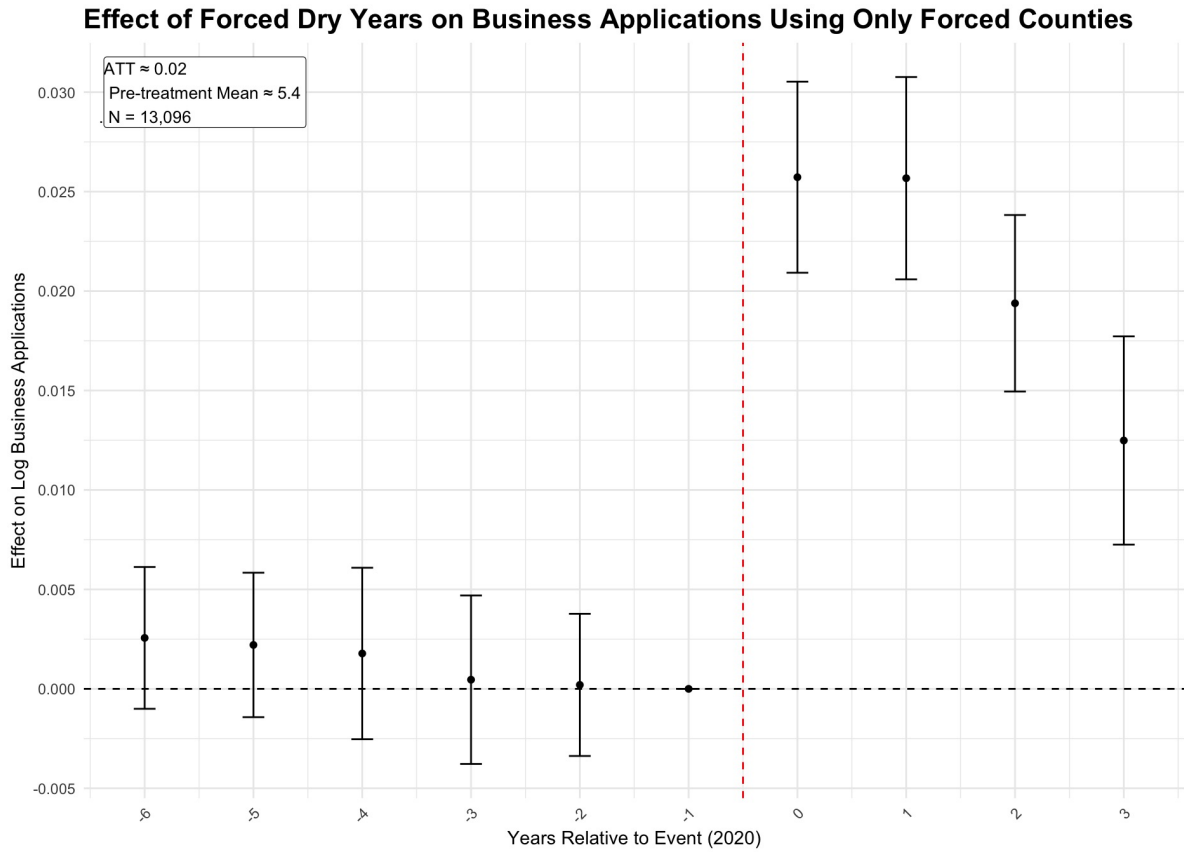


Figure 6: Effect of Forced Dry Years with Only Forced Counties

Notes: This figure reports event-study estimates from equation (1) restricting the sample to counties that were ever forced dry. The dependent variable is the log of total business applications from the U.S. Census Bureau's Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time dummies relative to 2019, with 95% confidence intervals. All models include county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the county level.

Forced Dry Groups for Only Treated

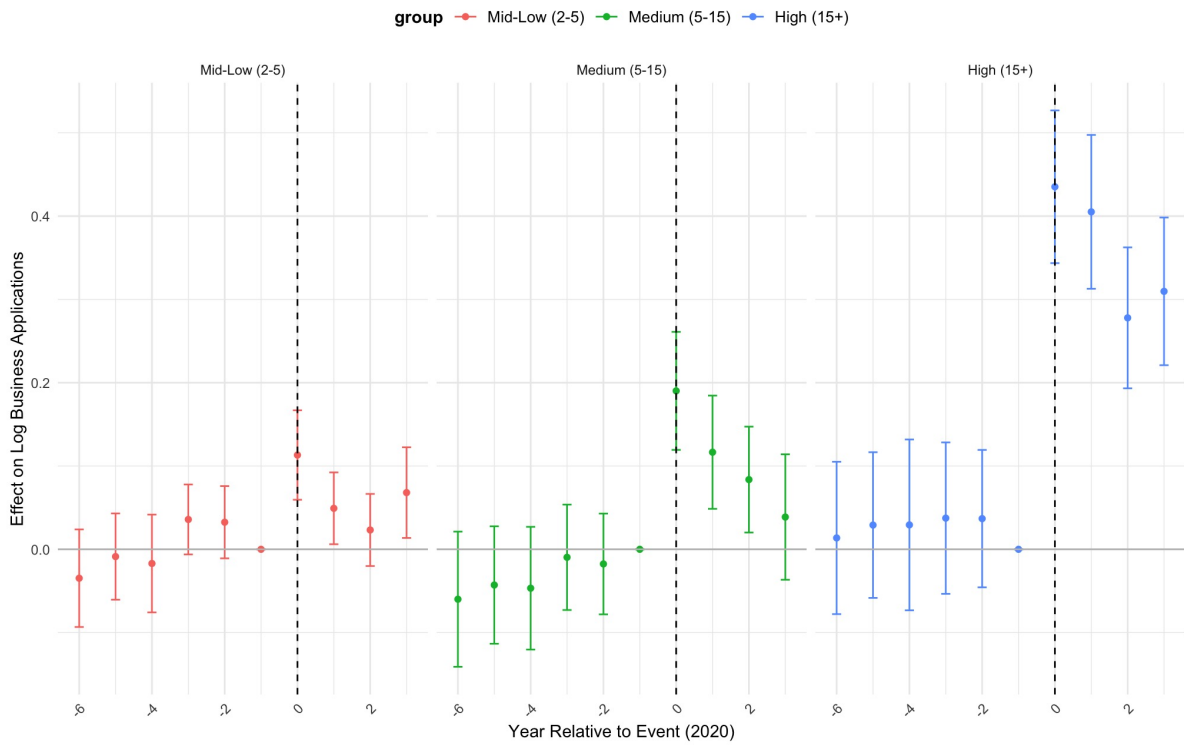


Figure 7: Effect of Forced Dry Groups on Business Applications For Only Treated

Notes: This figure reports estimates from equation (2), which bins counties into groups of forced prohibition exposure restricting the sample to counties that were ever forced dry. Counties are grouped by cumulative years of forced prohibition exposure: mid–low (2–5 years), medium (5–15 years), and high (15+ years). The coefficients present the effect of each group relative to the lowest group. The dependent variable is log business applications from the U.S. Census Bureau's Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level.

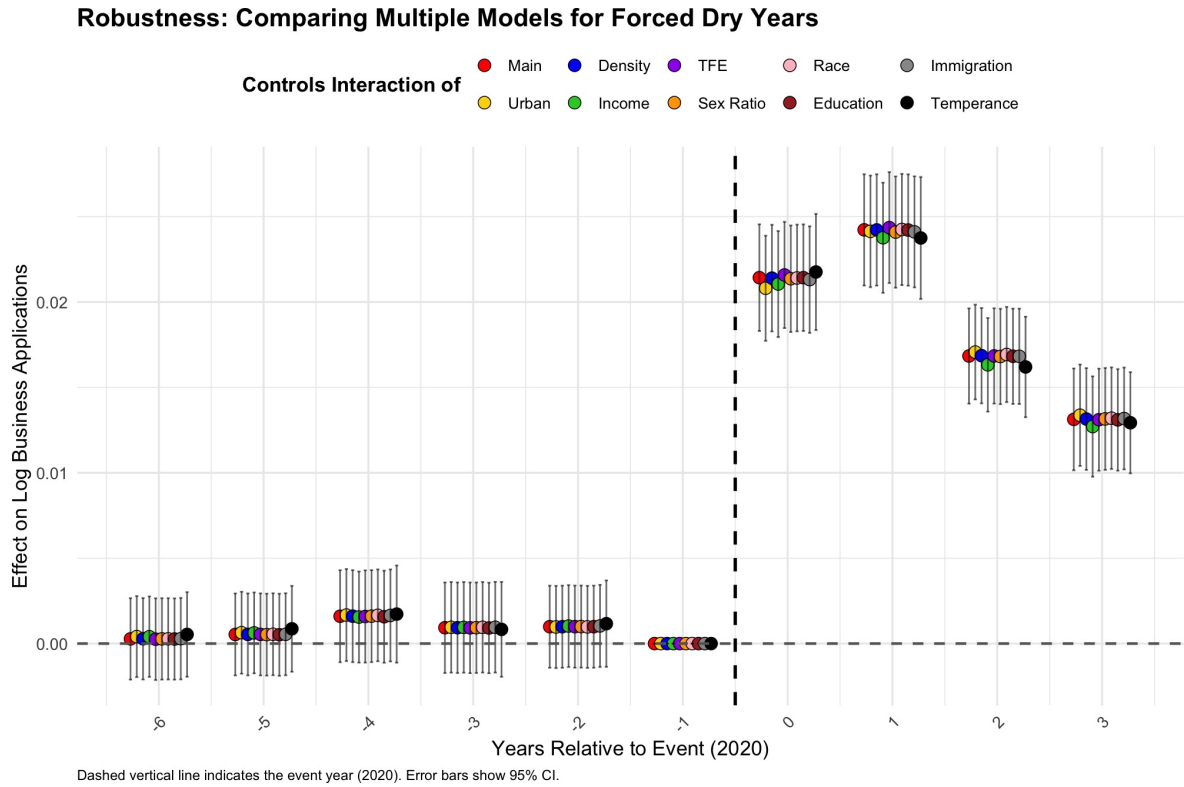


Figure 8: Robustness: Main Model Controlling for Covariate Interactions

Notes: This graph shows event-study estimates of equation (1) for forced dry years, adding interactions of event-time dummies with alternative county covariates at the pre-covid year 2018. Each color corresponds to a separate specification: population density, urban share, per capita income, education (college share), race (white share), immigration, sex ratio, temperance, and frontier experience (TFE). The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included. Results show that the positive post-2020 effects of forced prohibition exposure remain robust across all specifications.

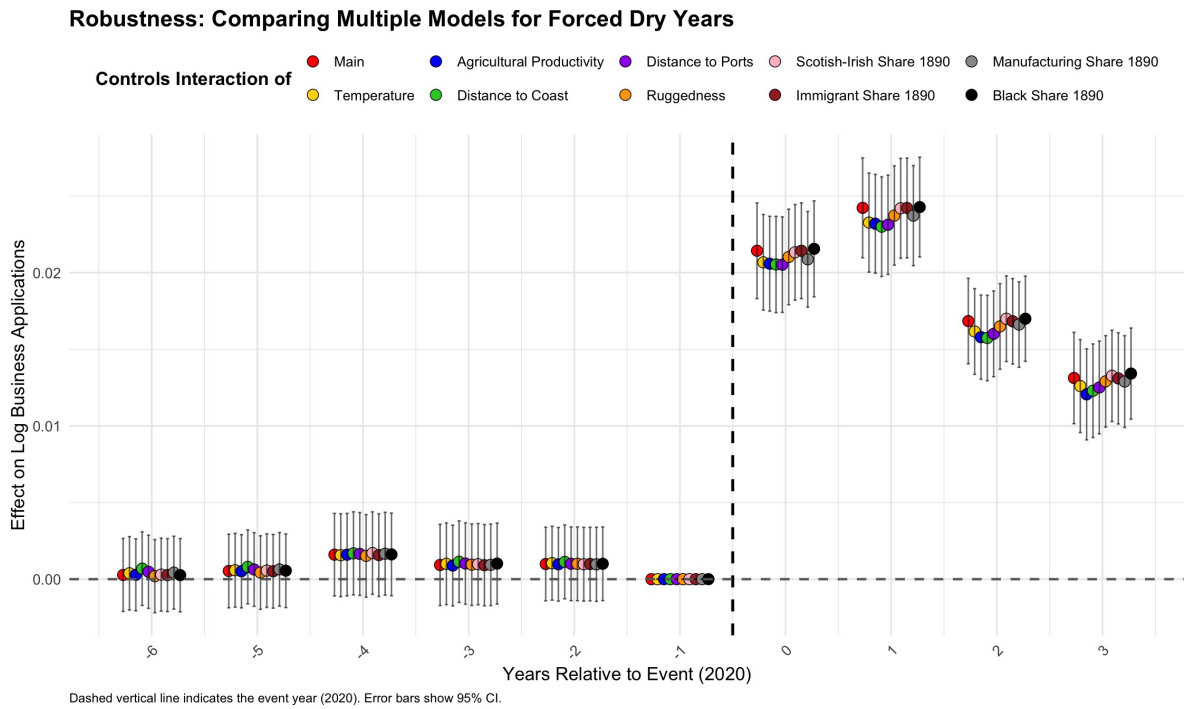


Figure 9: Robustness: Main Model Controlling for Historical Covariate Interactions

Notes: This graph shows event-study estimates of equation (1) for forced dry years, adding interactions of event-time dummies with alternative historical county covariates at. Each color corresponds to a separate specification: average temperature, agricultural productivity, distance to coast and ports, ruggedness, Scots-Irish Share, immigrant Share, manufacturing employment share and black share in 1890. These covariates are collected from the publicly available files of Bazzi, S., Fiszbein, M., & Gebresilasse, M. (2020). The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included. Results show that the positive post-2020 effects of forced prohibition exposure remain robust across all specifications.

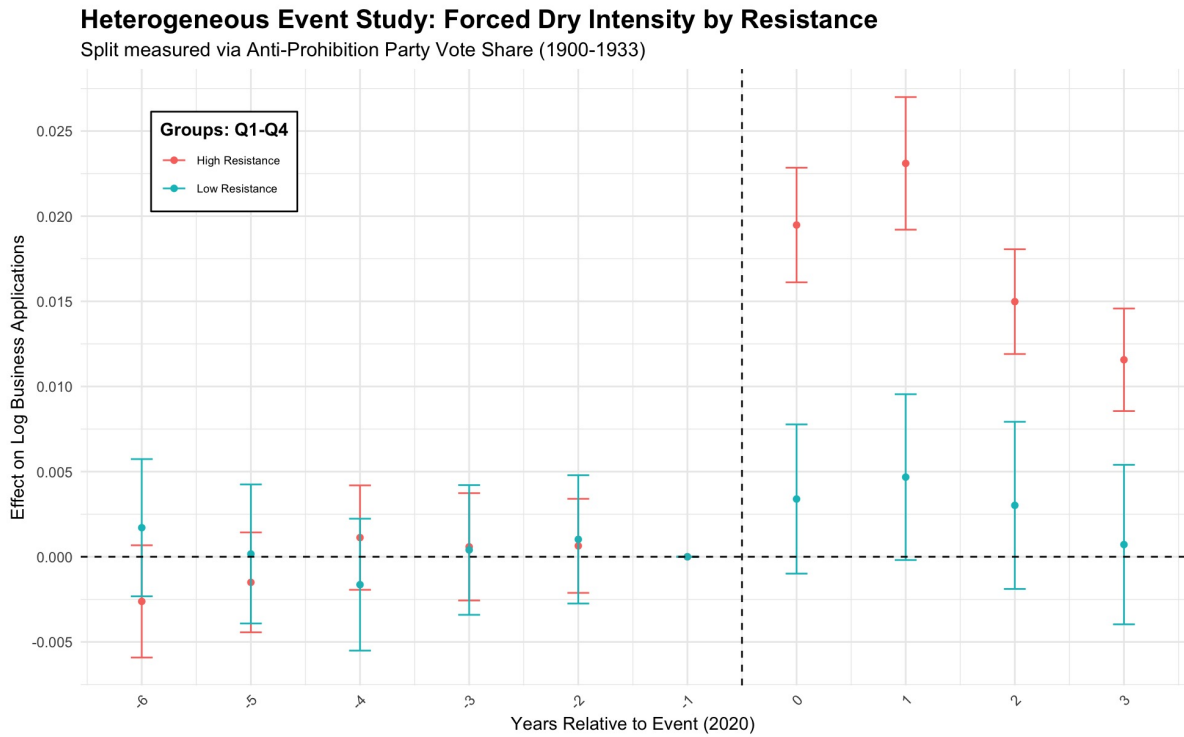


Figure 10: Event-Study Estimations by Resistance

Notes: Figure shows heterogeneous effects of forced dry years by historical resistance to prohibition. Resistance is measured using average county-level vote shares for anti-prohibition (“wet”) parties in Congressional elections, 1900–1933 (ICPSR; see Clubb et al. (2006)). Counties are split into quartiles of resistance, with high resistance defined as the top quartile of anti-prohibition vote share and low resistance as the bottom quartile. The dependent variable is log business applications from the U.S. Census Bureau’s Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level.

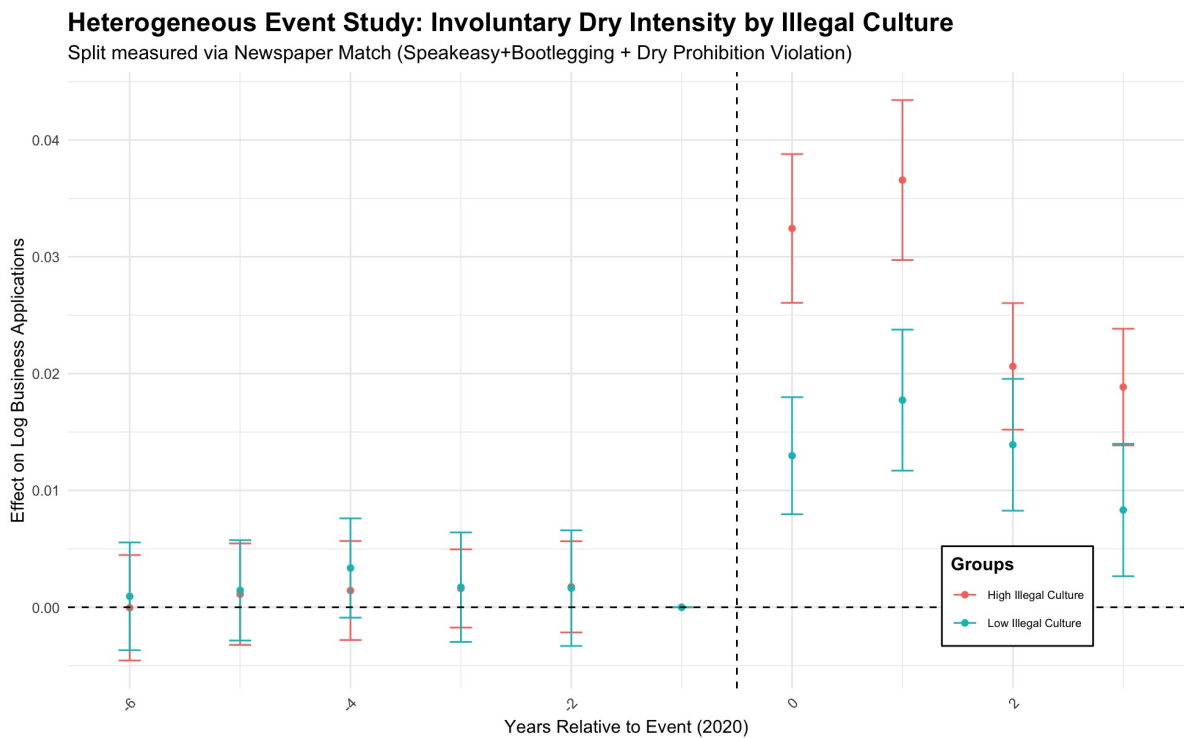


Figure 11: Event-Study Estimations by Illicit Activity History

Notes: This figure plots estimates from equation (1), splitting counties into quartiles of illicit activity mentions (bootlegging, speakeasies, dry law violations) drawn from digitized newspapers between 1900–1940. The red series shows counties in the top quartile (high illicit culture) and the blue series shows counties in the bottom quartile (low illicit culture). Counties are classified using total newspaper references aggregated from Newspapers.com. The results are shown at the 95% CI. The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included.

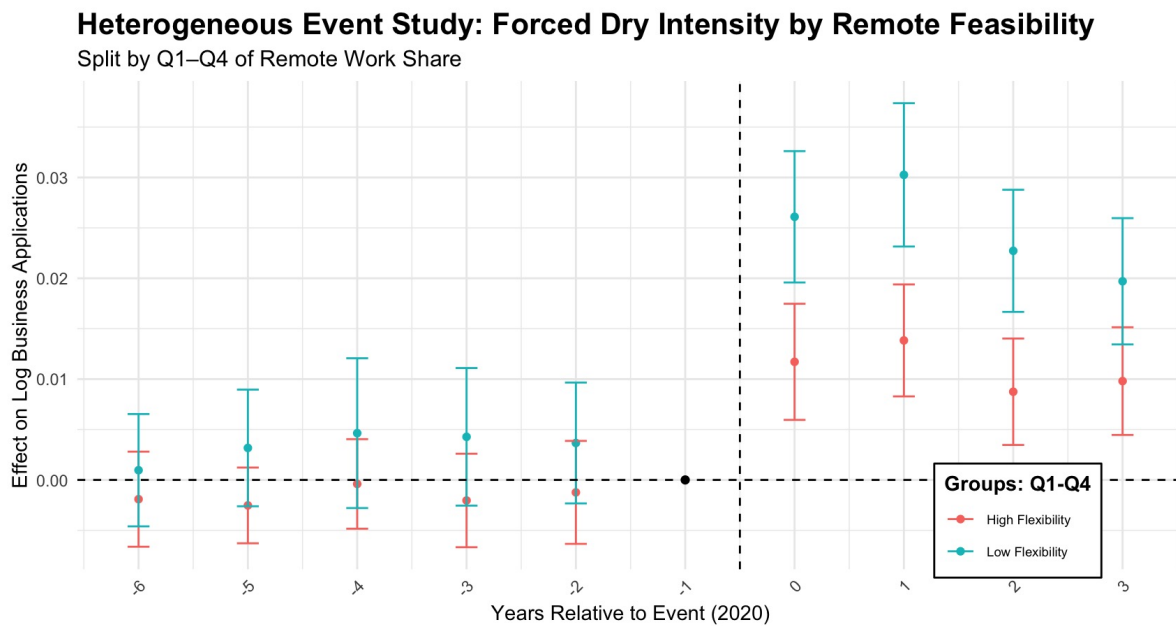
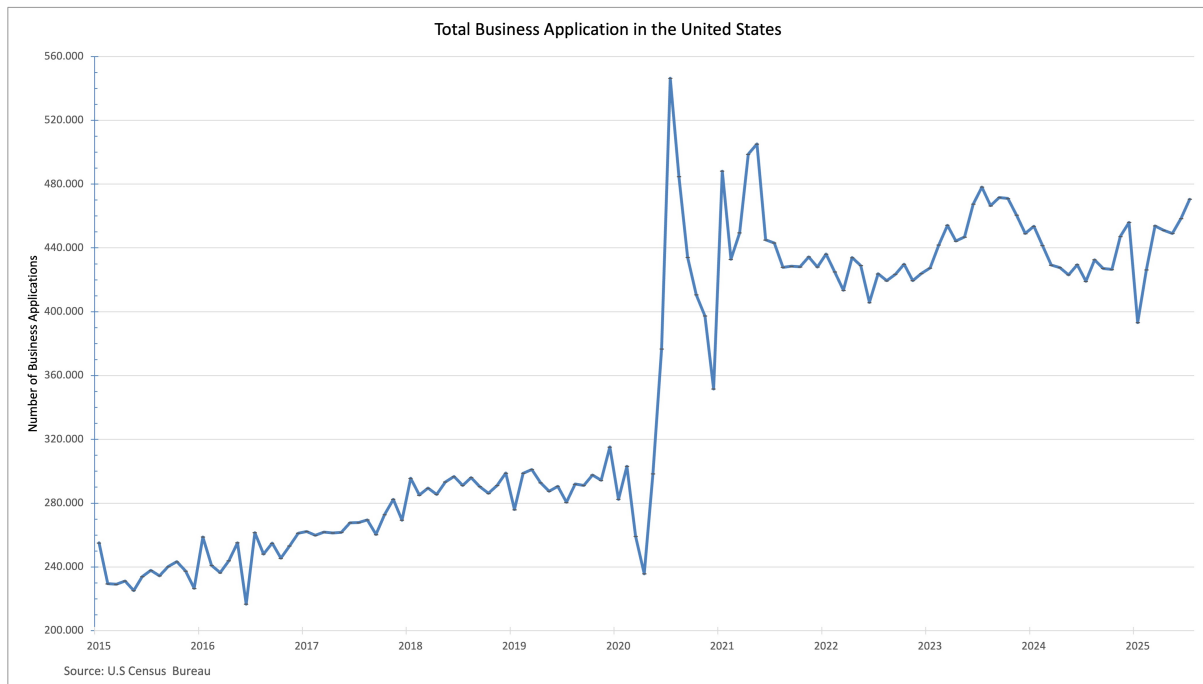


Figure 12: Event-Study Estimations by Remote Work Feasibility

Notes: This figure plots estimates from equation (1), splitting counties into quartiles of remote-work feasibility. Remote-work feasibility scores are drawn from Dingel and Neiman (2020), who classify the share of tasks in each occupation that can be performed from home. Scores are aggregated to the county level using pre-pandemic industry and occupation employment distributions from the County Business Patterns (2015–2018). The red series shows counties in the top quartile (high flexibility) and the blue series shows counties in the bottom quartile (low flexibility). The results are shown at the 95% CI. The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included. The standard errors are clustered at county level.

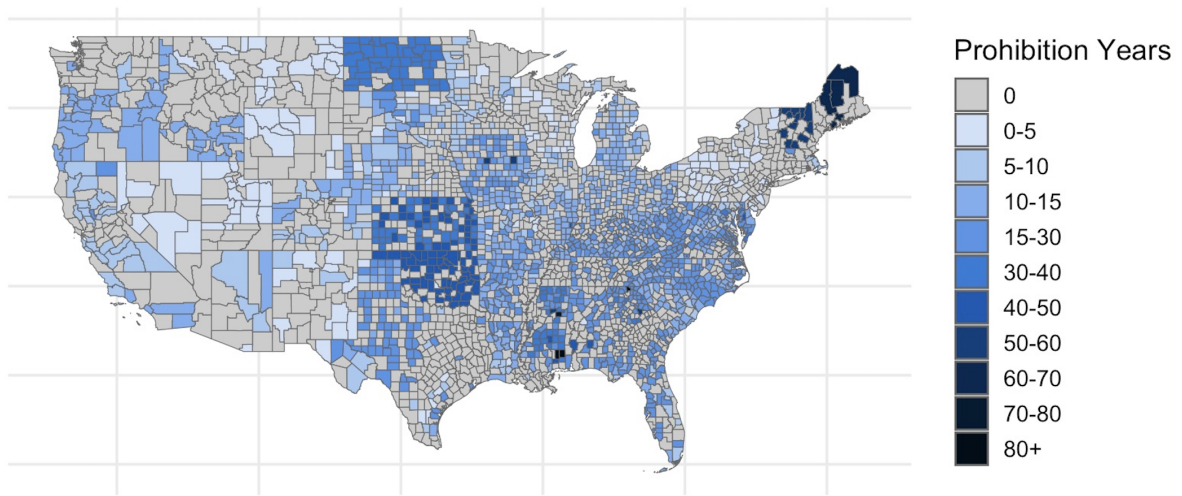
Appendix



Appendix Figure A1: Business Applications in the US between 2015-2025

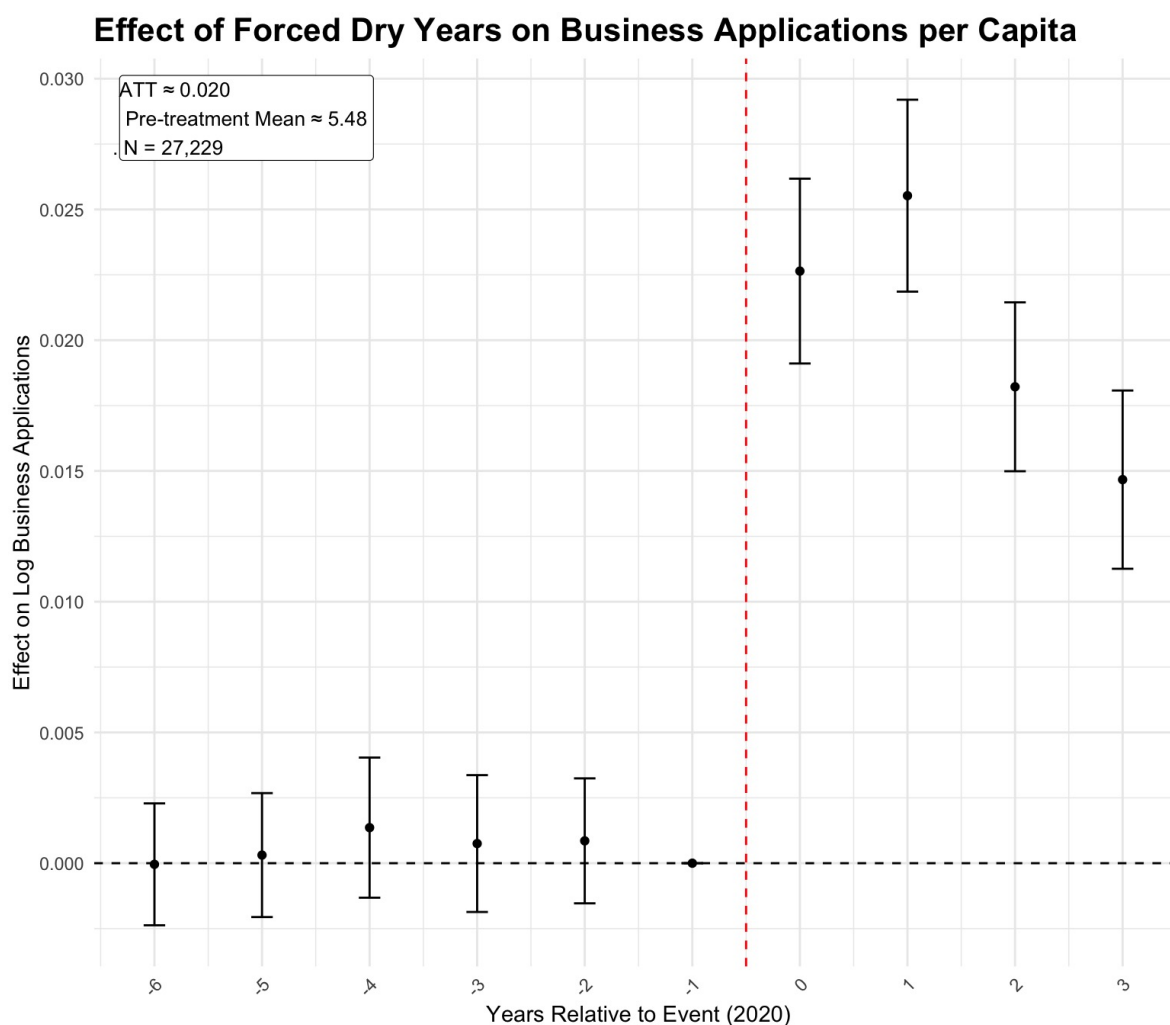
Notes: This figure shows monthly counts of business applications in the United States, as reported in the U.S. Census Bureau's Business Formation Statistics (BFS). The data cover all U.S. counties and include both likely employer and nonemployer applications.

Voluntary Dry Years by County (1800-1920)



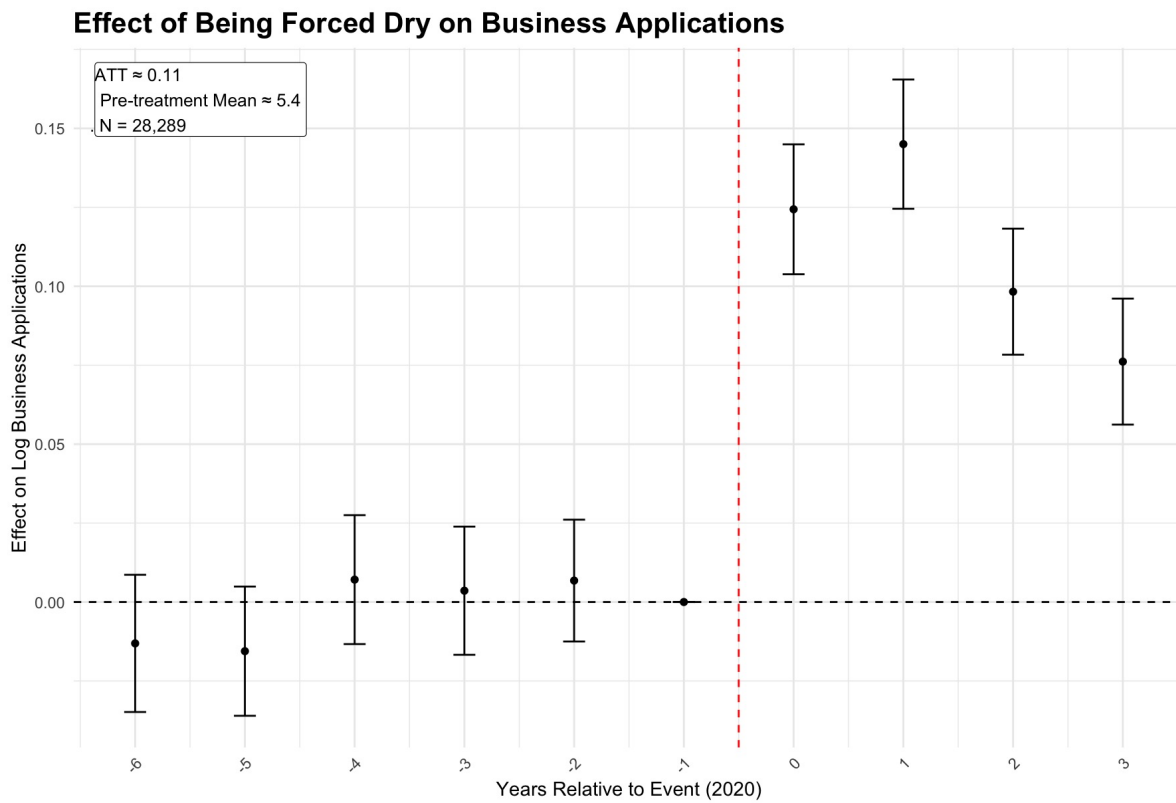
Appendix Figure A2: Voluntary Dry Years

Notes: This map shows the cumulative number of years each U.S. county was under voluntary prohibition between 1800 and 1920. Voluntary prohibition refers to counties that adopted local bans on alcohol through referenda or ordinances, reflecting alignment with temperance preferences rather than statewide mandates. The measure is constructed from Sechrist (2012), summing all years of locally chosen prohibition prior to national prohibition in 1920. Counties shaded grey (0 years) never adopted voluntary prohibition, while darker shades indicate longer durations of locally imposed bans. This measure is used in the analysis as a falsification test: if results were driven by underlying temperance sentiment, voluntary dry exposure would predict modern entrepreneurship as well.



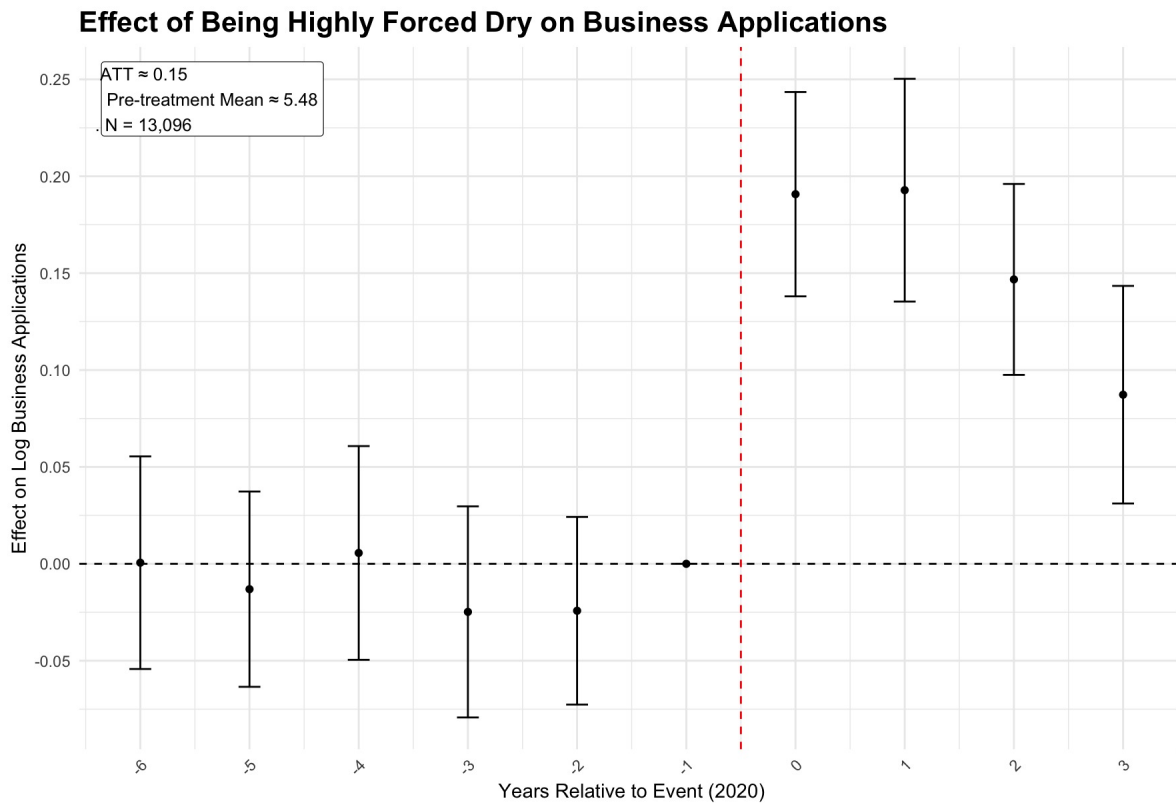
Appendix Figure A3: Effect of Forced Dry Years on Log Business Applications per Capita

Notes: This figure reports event-study estimates of the effect of cumulative forced prohibition exposure (1880–1920) on modern business applications. The dependent variable is the log of total business applications per capita from the U.S. Census Bureau’s Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time dummies relative to 2019, with 95% confidence intervals. All models include county fixed effects, year fixed effects, and state-by-year fixed effects. The main controls included are listed in Appendix A6. Standard errors are clustered at the county level.



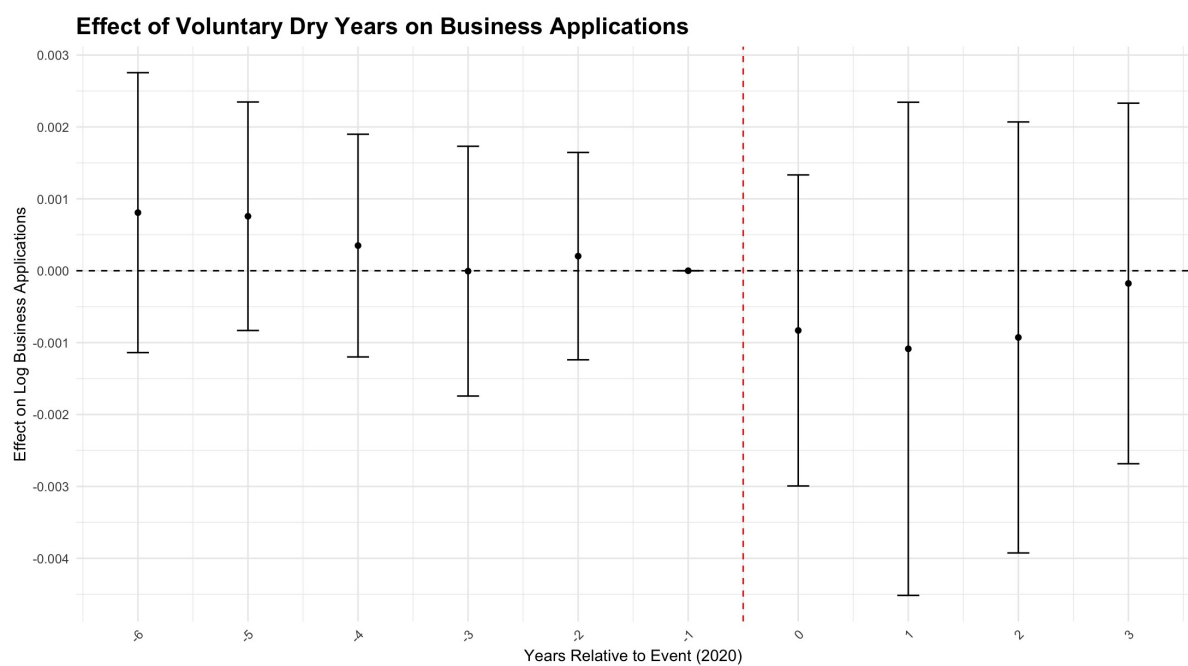
Appendix Figure A4: Effect of Being Forced Dry on Business Applications

Notes: This figure reports event-study estimates from equation (1), where the treatment is defined as a binary indicator for whether a county experienced any years of forced prohibition prior to 1920. The dependent variable is log business applications from the U.S. Census Bureau's Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level.



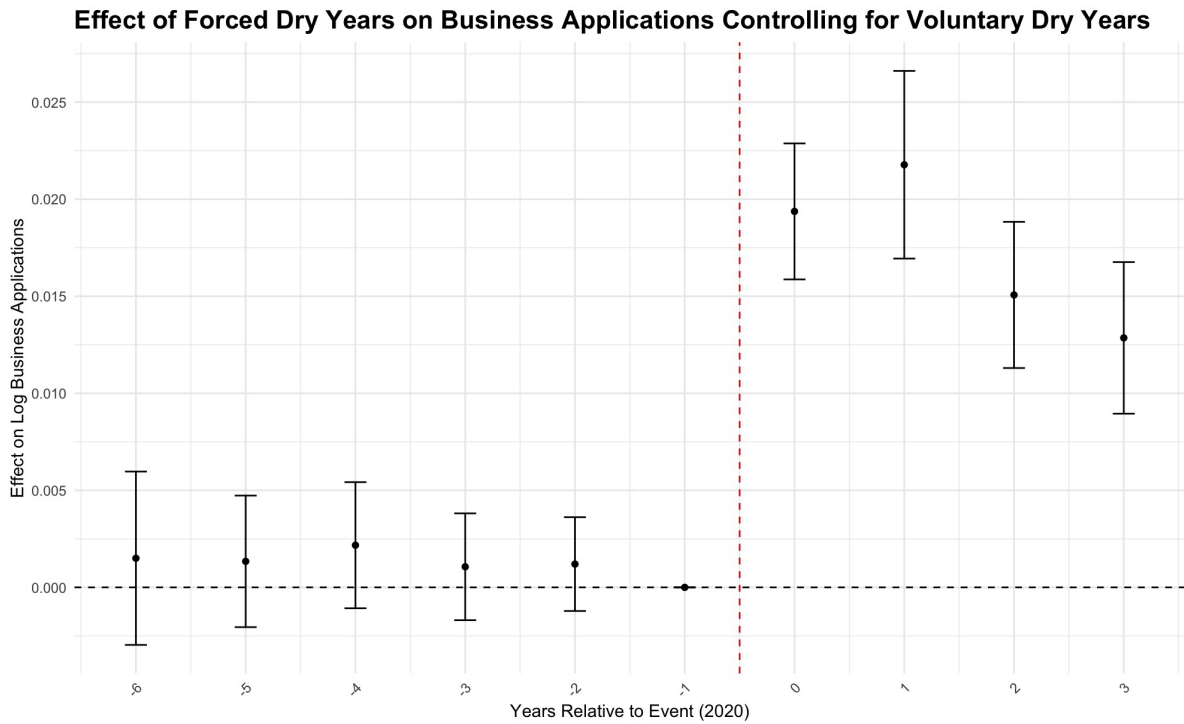
Appendix Figure A5: Effect of Being Highly Forced Dry on Business Applications (Only Forced Counties)

Notes: This figure reports event-study estimates from equation (1), where the treatment is defined as a binary indicator for whether a county experienced at more than 6 years (highest quartile) of forced prohibition prior to 1920. The dependent variable is log business applications from the U.S. Census Bureau's Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level.



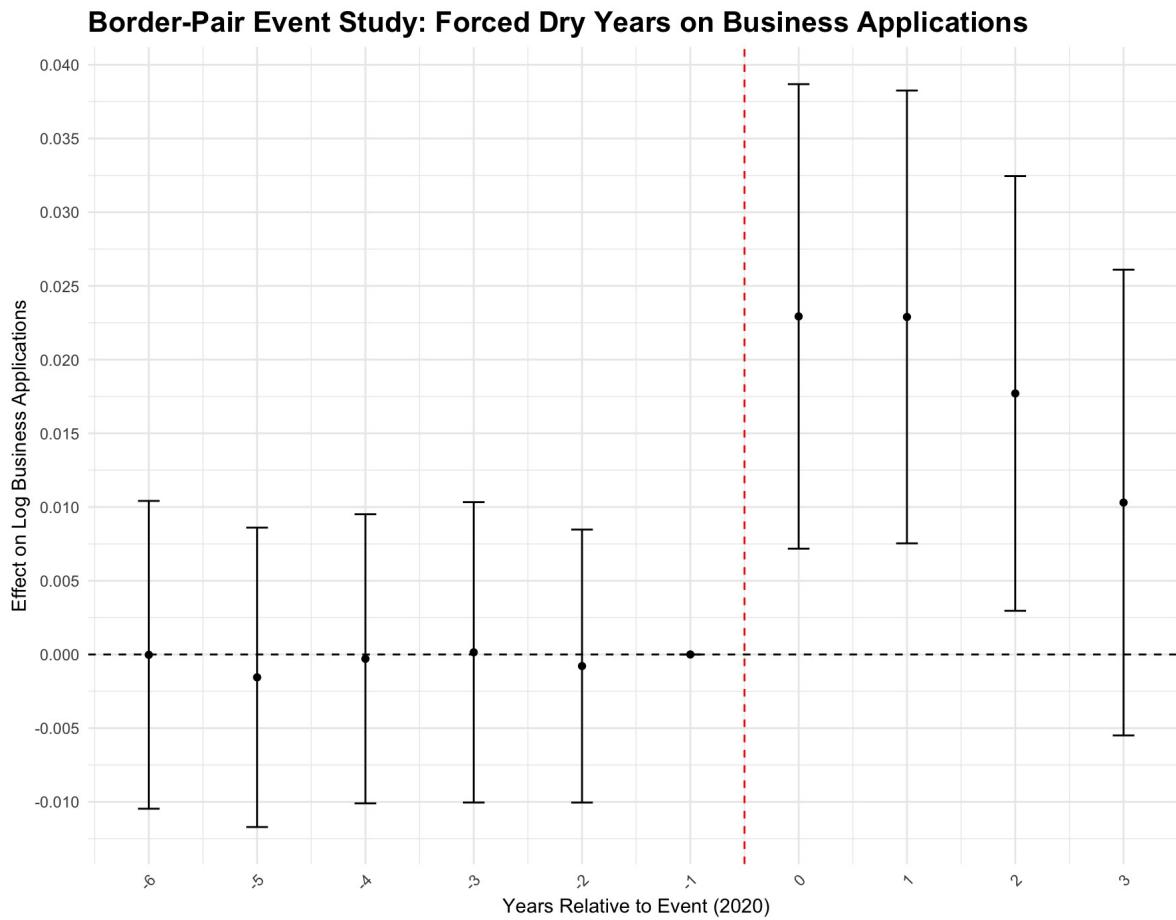
Appendix Figure A6: Effect of Voluntary Dry Years on Modern Business Applications

Notes: This figure reports event-study estimates from equation (1), replacing forced prohibition exposure with the cumulative number of voluntary dry years prior to 1920. The dependent variable is log business applications from the U.S. Census Bureau's Business Formation Statistics (2014–2023). Models include county, year, and state-by-year fixed effects, with standard errors clustered at the county level level.



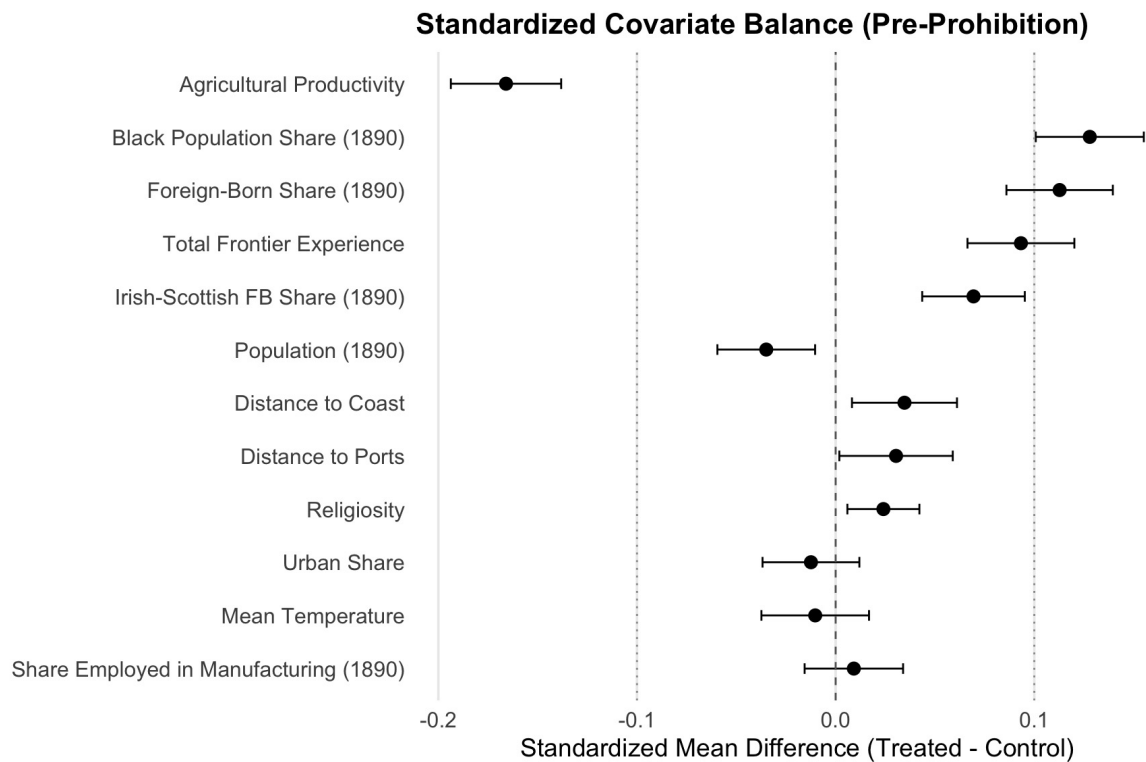
Appendix Figure A7: Effect of Forced Dry Years Controlling for Voluntary Dry Years

Notes: This figure reports event-study estimates from equation (1) of the effect of cumulative forced prohibition exposure (1880–1920) on modern business applications controlling for the interaction of time-to-event dummies with voluntary dry years. The dependent variable is the log of total business applications from the U.S. Census Bureau’s Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time dummies relative to 2019, with 95% confidence intervals. All models include county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the county level.



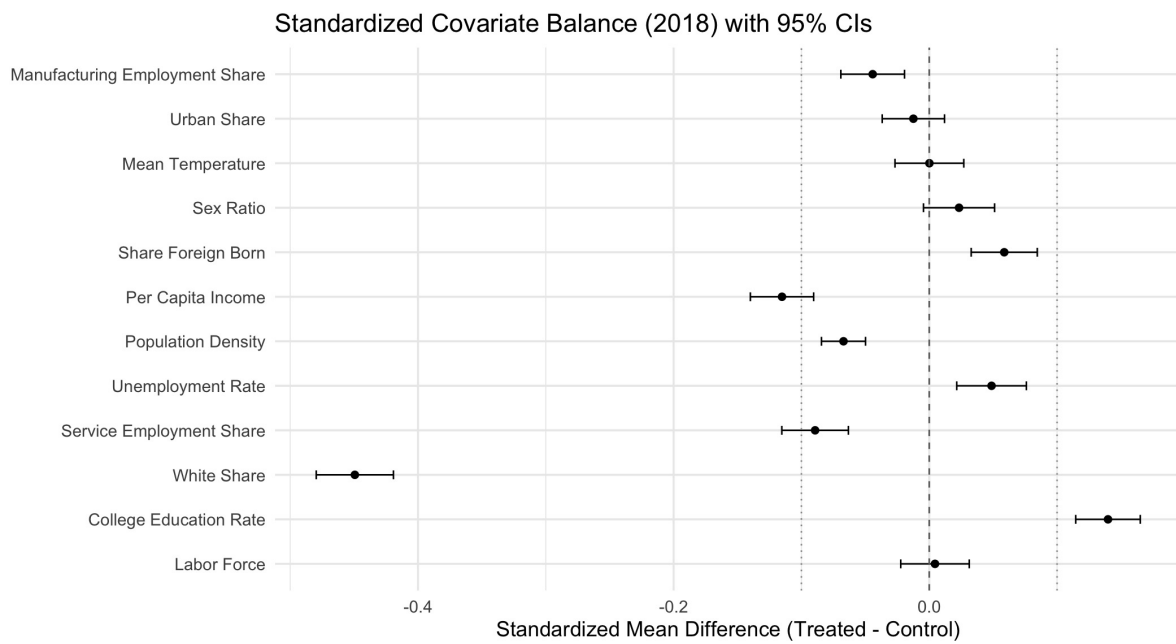
Appendix Figure A8: Effect of Forced Dry Years Comparing Neighbor Counties

Notes: This figure reports event-study estimates from equation (1) of the effect of cumulative forced prohibition exposure (1880–1920) on modern business applications, estimated within adjacent cross-state county pairs. The dependent variable is the log of total business applications from the U.S. Census Bureau’s Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time indicators relative to 2019, with 95% confidence intervals. The specification includes border-pair, year, and state-by-year fixed effects. Standard errors are clustered by county.



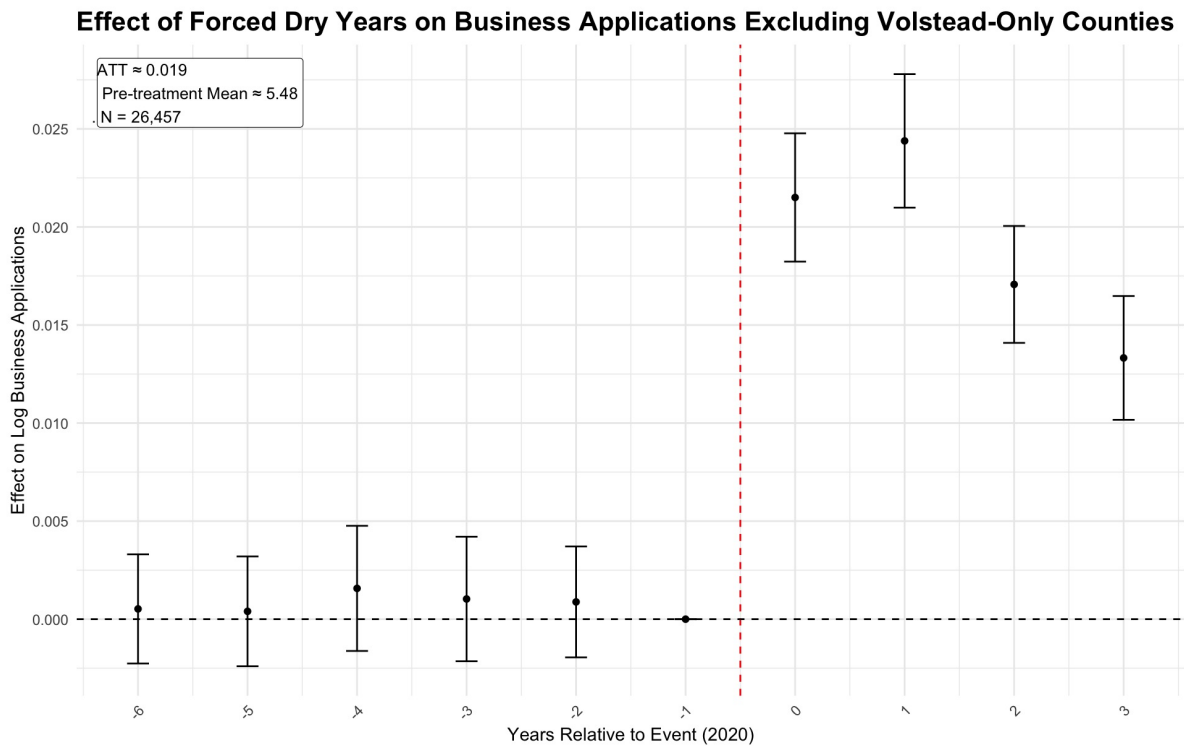
Appendix Figure A9: Balance Plot for Pre-Prohibition Era Covariates

Notes: This figure presents standardized mean differences in pre-prohibition covariates between counties with any forced dry years and those never forced. Covariates include agricultural productivity, population, urban share, mean temperature, religiosity, distance to ports/coast, manufacturing share, frontier experience, and demographic composition in 1890 (foreign-born, Black, Irish-Scots). Dashed lines mark ± 0.1 , a common threshold for balance. The figure shows that most covariates are well balanced, with only agricultural productivity and Black share showing modest imbalance, addressed in robustness checks.



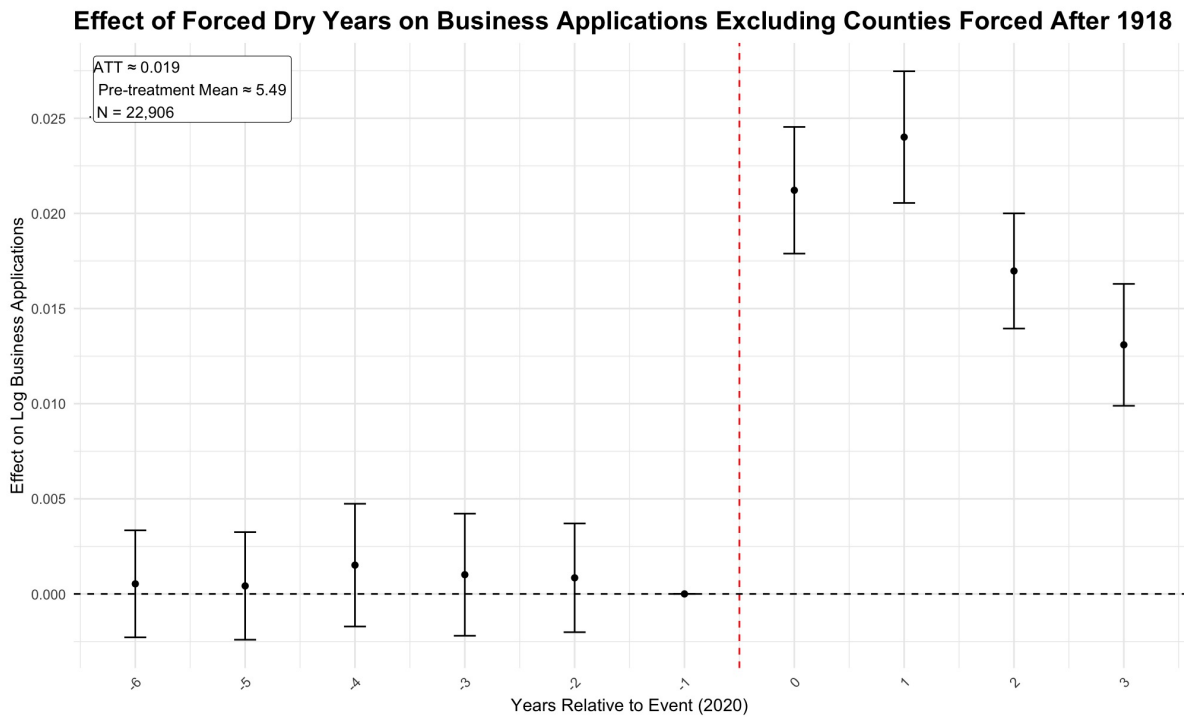
Appendix Figure A10: Balance Plot for Pre-COVID Covariates

Notes: This figure shows standardized mean differences in 2018 county-level covariates between counties with any forced dry years and those never forced. Covariates are drawn from the American Community Survey (ACS) and Bureau of Economic Analysis (BEA), including manufacturing and service employment shares, unemployment rate, labor force size, urban share, population density, per capita income, college education, racial composition, foreign-born share, sex ratio, and mean temperature. Dashed vertical lines at ± 0.1 mark common thresholds for imbalance. The largest gaps appear in racial composition (White share) and education, but robustness checks with flexible controls confirm that these differences do not drive the main results.



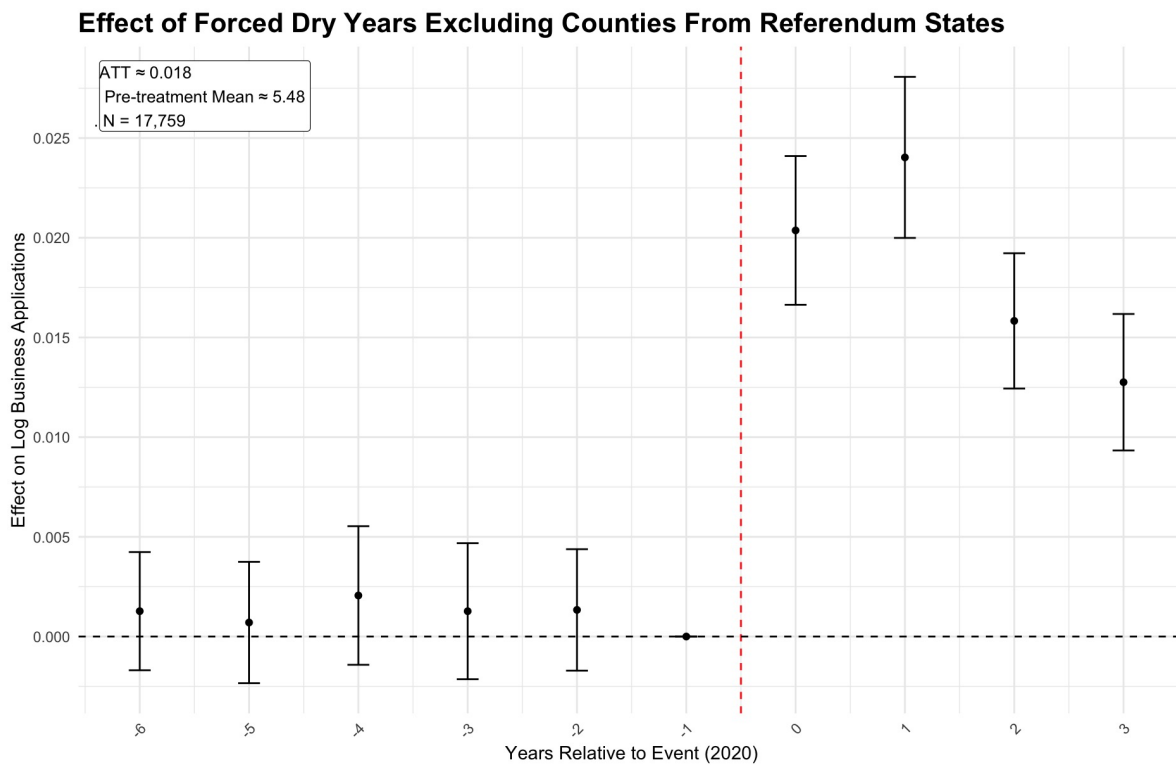
Appendix Figure A11: Robustness: Main Model Excluding Volstead-Only Counties

Notes: Event-study estimates of the effect of forced dry years on log business applications, excluding counties that only became dry under the federal Volstead Act of 1920. The specification follows equation (1) with county, year, and state-by-year fixed effects. The standard errors are clustered at the county level. Pre-treatment coefficients are flat and indistinguishable from zero with 95% confidence intervals, while post-2020 effects remain positive and significant. Results indicate that the main findings are not driven by late holdouts that were only forced dry at national prohibition.



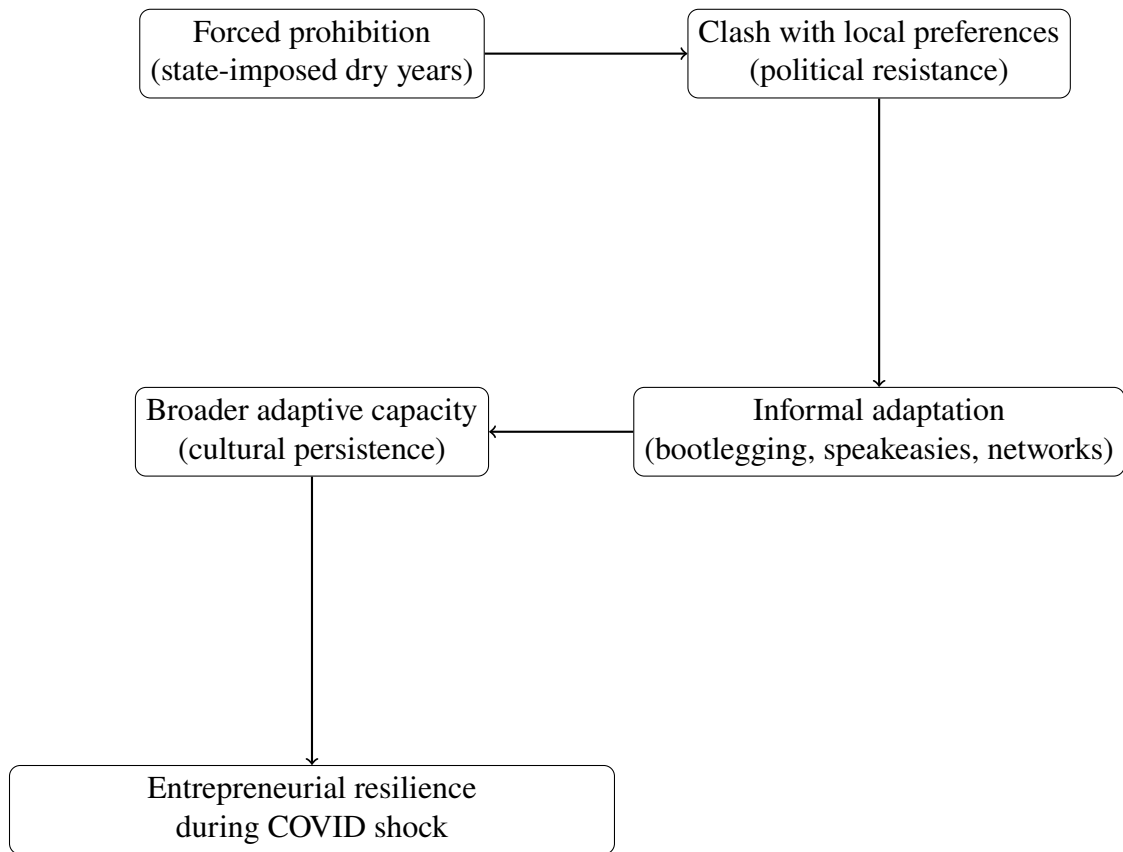
Appendix Figure A12: Robustness: Main Model Excluding Counties Forced After 1918 – Late Adopters

Notes: Event-study estimates of the effect of forced dry years on log business applications, excluding counties that were forced dry only after 1918. This restriction removes late holdouts that might differ systematically. The specification follows equation (1) with county, year, and state-by-year fixed effects. The standard errors are clustered at the county level. Pre-treatment coefficients are flat and indistinguishable from zero with 95% confidence intervals, while post-2020 effects remain positive and significant.



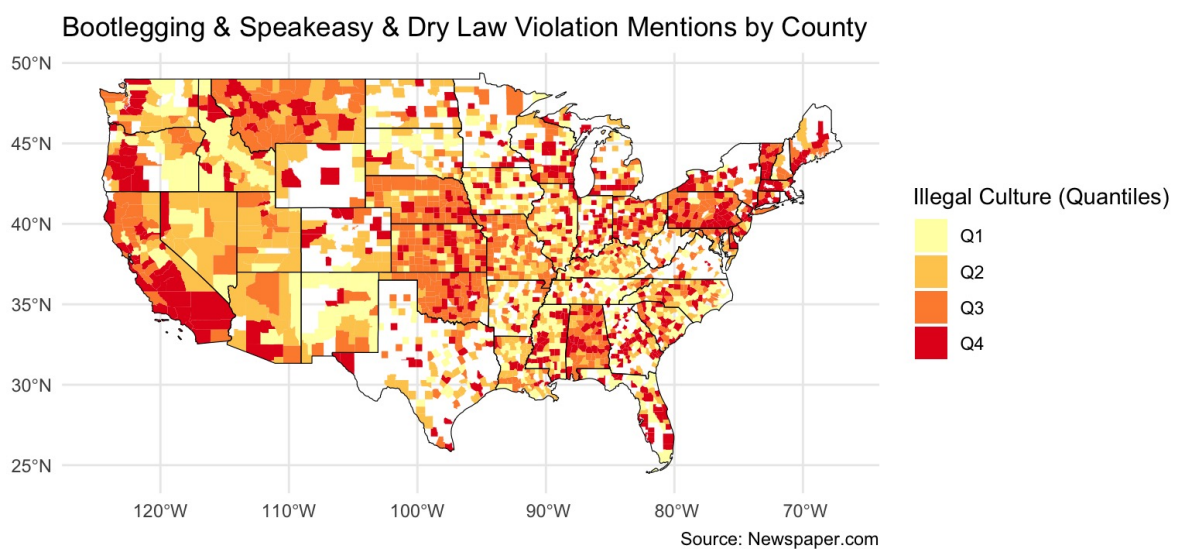
Appendix Figure A13: Event-Study Estimations Excluding State Bans with Referendum

Notes: This figure reports event-study estimates from equation (1) excluding the counties that became dry by state referendum. The dependent variable is the log of total business applications from the U.S. Census Bureau's Business Formation Statistics (BFS), 2014–2023. Coefficients plot interactions between forced dry years and event-time dummies relative to 2019, with 95% confidence intervals. All models include county fixed effects, year fixed effects, and state-by-year fixed effects. Standard errors are clustered at the county level.



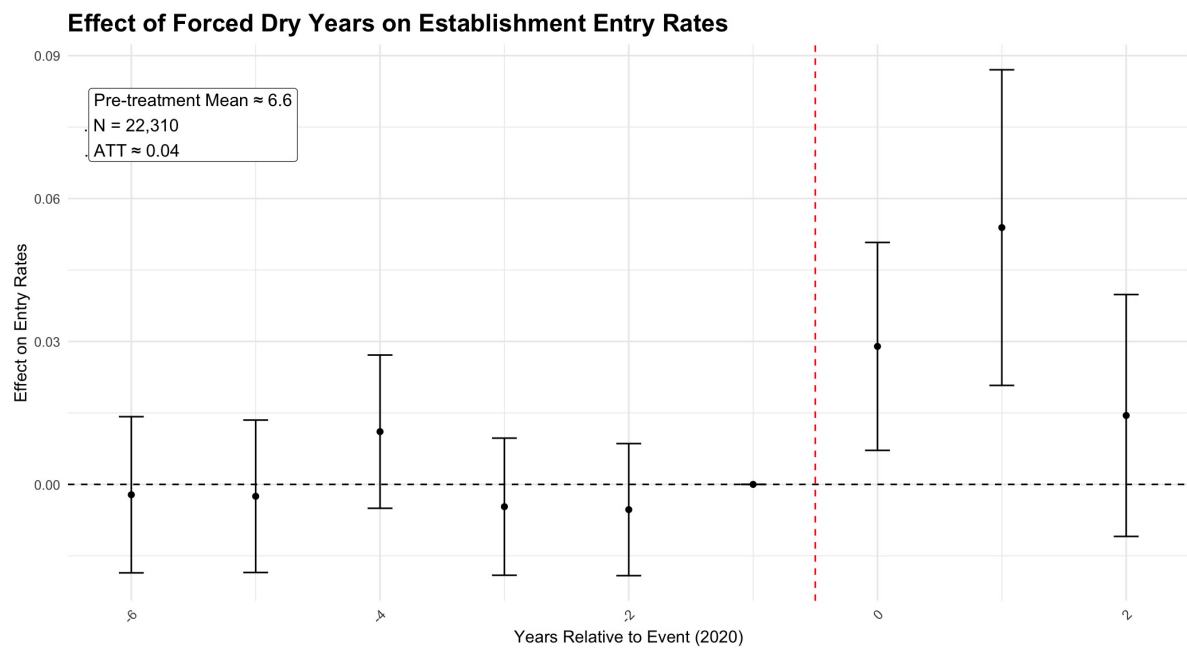
Appendix Figure A14: Conceptual pathway from forced prohibition to entrepreneurial resilience.

Notes: This figure presents a schematic pathway linking forced prohibition to entrepreneurial resilience. Forced prohibition (measured as state-imposed dry years) created institutional clashes with local preferences, captured by political resistance (vote shares for anti-prohibition parties), and encouraged informal adaptation (bootlegging, speakeasies, and networks, measured via newspaper references). These mechanisms contributed to broader adaptive capacity (cultural persistence), which later shaped entrepreneurial resilience during the COVID-19 shock. The diagram summarizes the mechanisms tested empirically in Section 5.



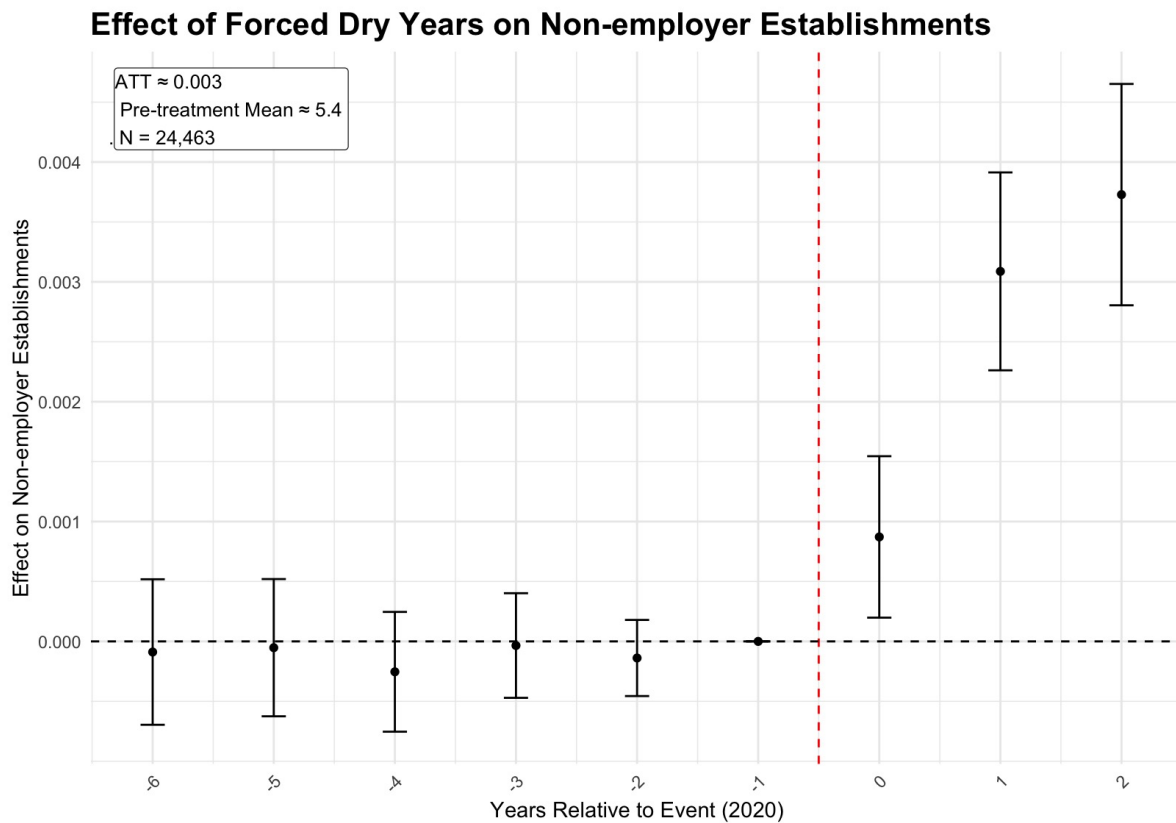
Appendix Figure A15: Informal Adaptation by Newspaper References

Notes: This map shows the county-level distribution of newspaper mentions of “bootlegging,” “speakeasy,” and “dry law violation” between 1900 and 1940, drawn from digitized archives on Newspapers.com. Counties are grouped into quantiles (Q1–Q4) of total mentions, with darker shading indicating higher intensity of illicit activity. This measure serves as a proxy for the prevalence of informal adaptation to prohibition laws.



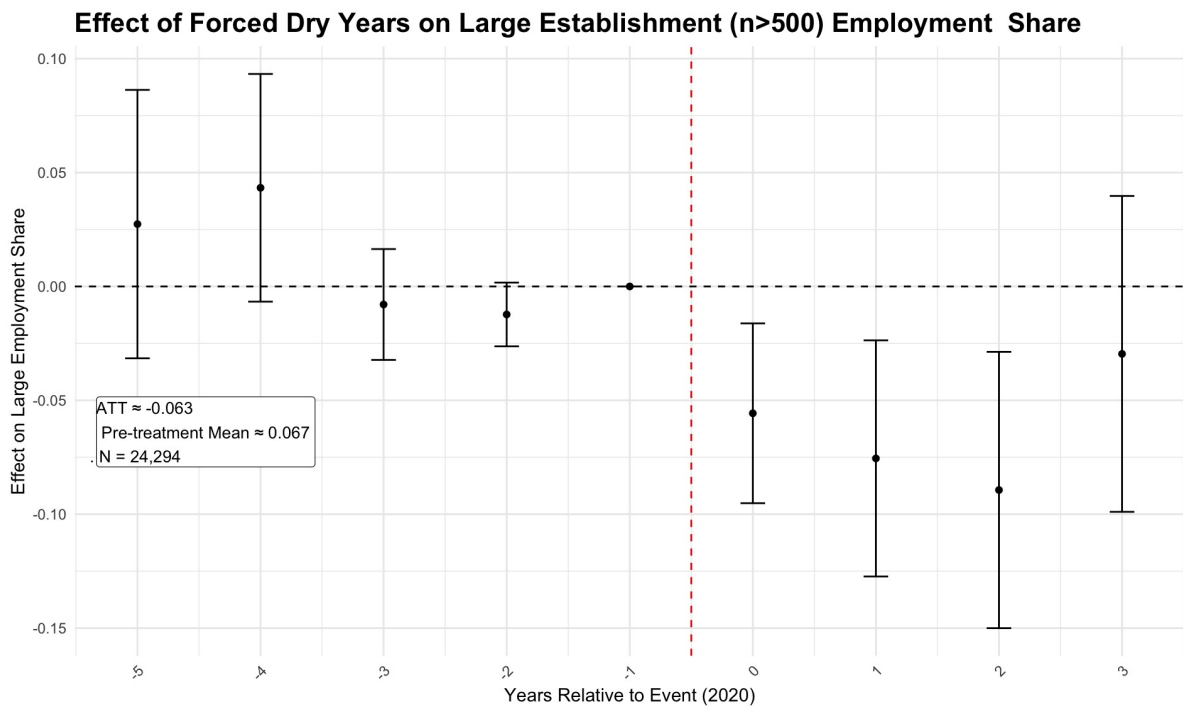
Appendix Figure A16: Event-Study Estimations on Establishments Entry Rates

Notes: This figure plots estimates from equation (1) with the dependent variable equal to establishment entry rates (BDS, 2014–2020). The treatment variable is cumulative years of forced prohibition (1880–1920). County, year, and state–year fixed effects are included. Error bars show 95% confidence intervals. The standard errors are clustered at the county level.



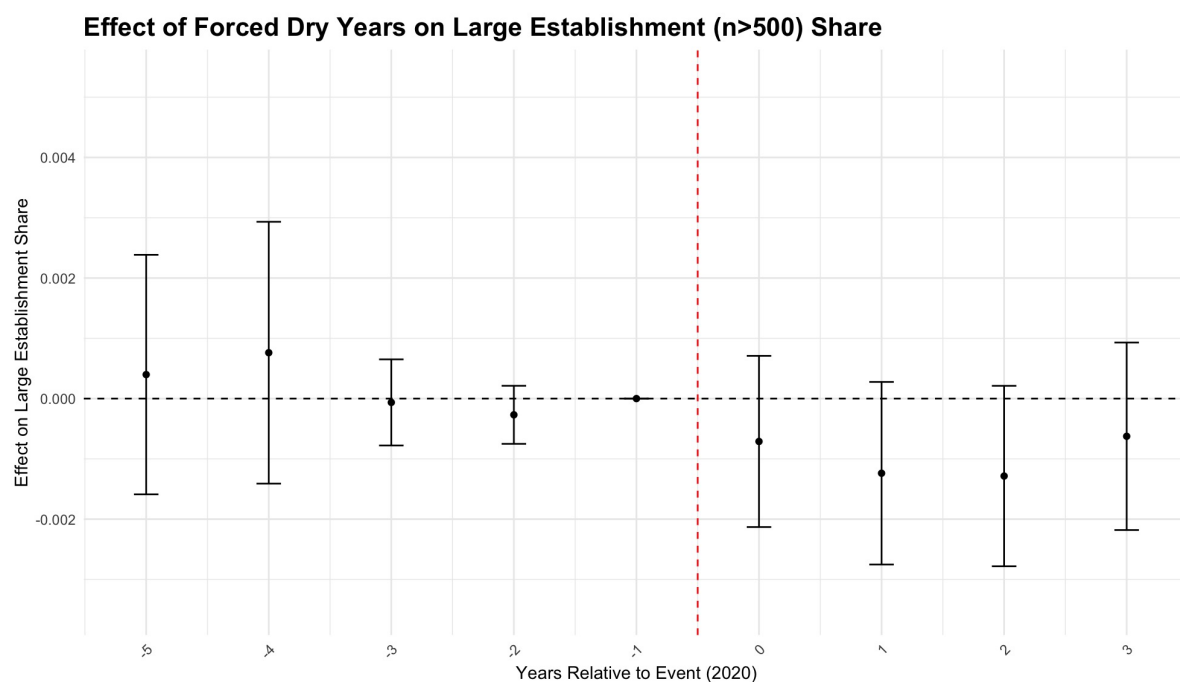
Appendix Figure A17: Event-Study Estimations on Nonemployer Establishments

Notes: This graph shows event-study estimates of equation (1) for forced dry years from equation (1) using log nonemployer establishments (U.S. Census Bureau, NES 2014–2021) as the dependent variable. County, year, and state–year fixed effects are included. Standard errors are clustered at the county level. Error bars show 95% confidence intervals.



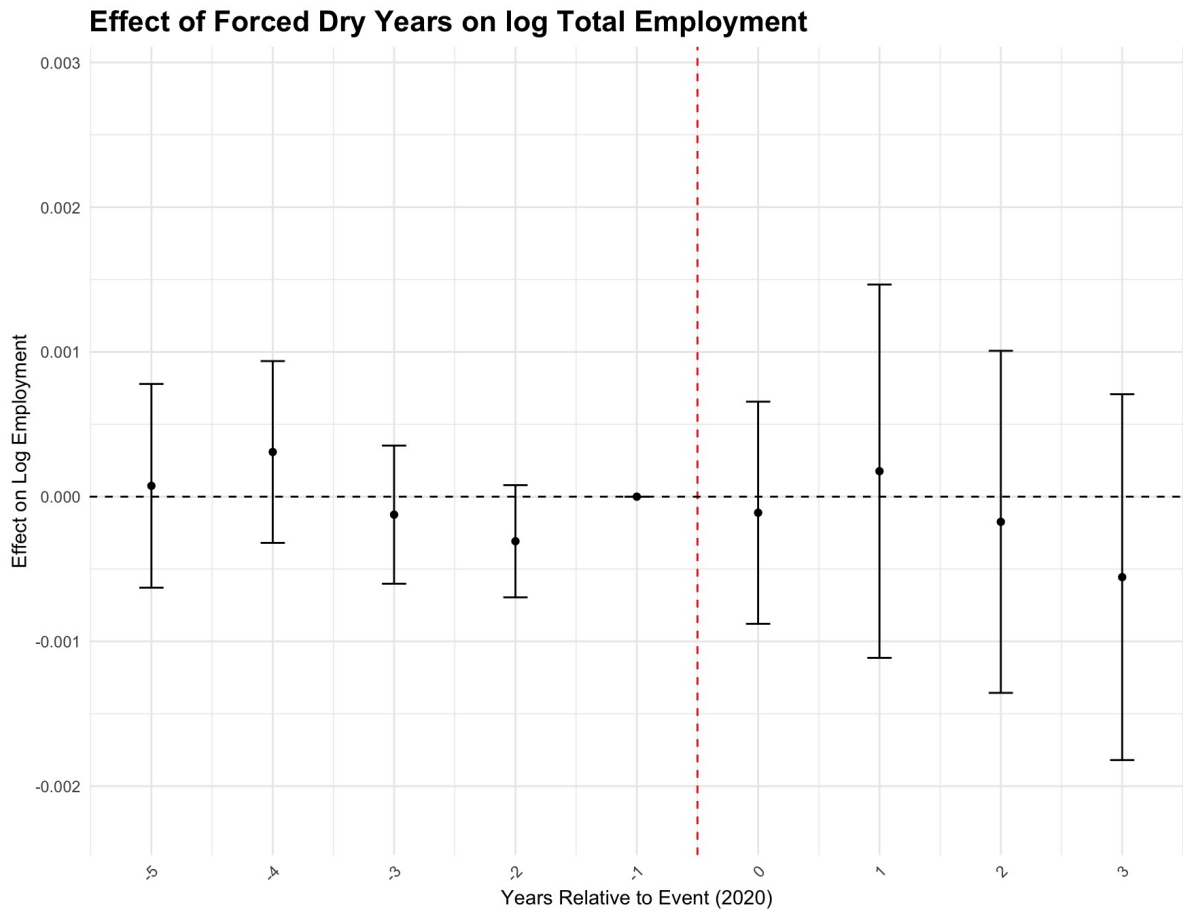
Appendix Figure A18: Event-Study Estimations on Large Establishments Employment Share

Notes: This figure plots estimates from equation (1), using the share of county employment in establishments with 500 or more employees as the dependent variable. Because County Business Patterns (CBP) does not directly report employment, the measure is approximated by combining establishment size class counts with midpoint employment values. The results are shown at the 95% CI. The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included.



Appendix Figure A19: Event-Study Estimations on Large Establishment Share

Notes: This figure plots event-study estimates from equation (1), where the dependent variable is the county share of establishments with 500+ employees from County Business Patterns (CBP). The treatment variable is cumulative years of forced prohibition (1880–1920). County, year, and state–year fixed effects are included. Error bars show 95% confidence intervals. The standard errors are clustered at the county level.



Appendix Figure A20: Event-Study Estimations on Employment

Notes: This figure plots event-study estimates from equation (1), where the dependent variable is log total county employment (QWI, 2014–2023). Because aggregate employment is primarily driven by broader macroeconomic conditions and not local entrepreneurial activity, this outcome serves as a placebo. Estimates are expressed relative to 2019, with the vertical dashed line marking the onset of the COVID-19 shock (2020). The results are shown at the 95% CI. The dependent variable is log business applications (BFS, 2014–2023). County, year, and state–year fixed effects included. The standard errors are clustered at county level.

State-Level Prohibition Enactment

State	Year	Statute	Ref.	Fed.	State	Year	Statute	Ref.	Fed.
Alabama	1907	✓			Missouri	1920	✓		✓
Alaska	1920	✓		✓	Montana	1916		✓	
Arizona	1914		✓		Nebraska	1916		✓	
Arkansas	1916		✓		Nevada	1918		✓	
California	1920	✓		✓	New Hampshire	1917	✓		
Colorado	1914		✓		New Jersey	1920	✓		✓
Connecticut	1920	✓		✓	New Mexico	1917		✓	
Delaware	1920	✓		✓	New York	1920	✓		✓
DC	1920	✓		✓	North Carolina	1908		✓	
Florida	1918		✓		North Dakota	1920	✓		✓
Georgia	1907	✓			Ohio	1918		✓	
Hawaii	1920	✓		✓	Oklahoma	1907		✓	
Idaho	1916		✓		Oregon	1914		✓	
Illinois	1920	✓		✓	Pennsylvania	1920	✓		✓
Indiana	1917	✓			Rhode Island	1920	✓		✓
Iowa	1915	✓			South Carolina	1915		✓	
Kansas	1881	✓			South Dakota	1916		✓	
Kentucky	1919		✓		Tennessee	1909	✓		
Louisiana	1920	✓		✓	Texas	1918	✓		
Maine	1920	✓		✓	Utah	1918		✓	
Maryland	1920	✓		✓	Vermont	1920	✓		✓
Massachusetts	1920	✓		✓	Virginia	1914		✓	
Michigan	1916		✓		Washington	1914		✓	
Minnesota	1920	✓		✓	West Virginia	1912		✓	
Mississippi	1908	✓			Wisconsin	1920	✓		✓
					Wyoming	1918		✓	

Appendix Figure A21: Statewide Prohibition Enactment

Notes: This Table summarizes the dates when statewide prohibition laws were enacted and indicates whether adoption occurred through state legislature, referendum or Federal law based on Sechrist (2018).

Appendix Table A1: Simple Regressions for Illicit Activity

	Dependent Variables		
	Log Total Matches S-B-D	Log Matches per Newspaper S-B-D	Log Matches per Newspaper S-B-D-R-M
	(1)	(2)	(3)
Forced Dry Years	0.0402*** (0.0116)	0.0582*** (0.0128)	0.0549*** (0.0122)
Number of Counties	2,121	752	653
Mean Dep. Var	6.254	4.903	4.984
Regional FE	Yes	Yes	Yes
Geo/Agro Controls	Yes	Yes	Yes

Notes: This table reports OLS regression estimates where the outcomes are listed at the top of each column. Column (1) uses the total number of newspaper article matches for the keywords "speakeasy," "bootlegging," and "dry law violation." Column (2) scales the same match count by the total number of newspapers published in the county. Column (3) extends the keyword list to include "rum-running" and "moonshine." All regressions include census division fixed effects. Time-invariant geographic and historical controls include county area; distance to the nearest coast and port; average historical temperature; population density; total frontier experience; manufacturing employment share in 1890; Black share; immigrant share in 1890; sex ratio; and potential agricultural yield. Standard errors are clustered at the county level. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

	(1)	(2)
Prohibition Intensity \times Post-2020 \times Low-Remote	-0.078*** (0.017)	-0.070*** (0.017)
Prohibition Intensity \times Post-2020 \times Other Sectors	-0.016 (0.017)	-0.007 (0.018)
County controls	No	Yes
County FE	Yes	Yes
Year FE	Yes	Yes
State \times Year FE	Yes	Yes
Observations	51,610	48,500
Number of counties	2,889	2,889

Notes: This table reports triple-difference estimates of the effect of prohibition intensity on the share of county employment in large firms with ≥ 500 employees, distinguishing between low- and high-remote feasibility sectors. The dependent variable is expressed in percentage points. Employment shares are approximated from County Business Patterns (CBP) establishment size classes, since CBP does not directly report employment; midpoint values of size bins are used to impute county-level employment. “Low-Remote” and “Other Sectors” are defined using Dingel and Neiman (2020)’s remote-work feasibility index, aggregated to the county level. Column (1) includes only fixed effects (county, year, and state–year). Column (2) additionally controls for county-level covariates (population, density, income, race, sex ratio, manufacturing and service employment shares). Standard errors are clustered at the county level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2: Triple-Difference Estimates of Large-Firm Employment Shares

Panel A: Industry Restrictions

Dependent Variable:	Log Nonemployer Establishments				
Model:	All NAICS	Excluding Manufacturing	Services Only	Non-Tradable	Tradable
Forced Dry \times China Shock	0.0125*** (0.0038)	0.0137*** (0.0040)	0.0136*** (0.0040)	0.0142*** (0.0040)	0.0096* (0.0044)
Year & CZ FE	Yes	Yes	Yes	Yes	Yes
Observations	5,588	5,588	5,588	5,588	5,588
Mean Dep Var	8.78	8.48	8.21	8.24	5.82

Panel B: Alternative Prohibition Intensity

Dependent Variable:	Log Nonemployer Establishments		
Model:	Simple Avg.	Pop-Weighted Avg.	Share Forced
Forced Dry \times China Shock	0.0133*** (0.0045)	0.0125*** (0.0038)	
Share Forced \times China Shock			0.1452* (0.0688)
Year & CZ FE	Yes	Yes	Yes
Observations	7,942	5,588	7,634
Mean Dep Var	8.78	8.78	8.78

Notes: This table reports the interaction effects of prohibition exposure and China Shock exposure on the number of nonemployer firms at the commuting zone (CZ) by year level. Nonemployer firms are measured from the Nonemployer Statistics (NES) at the 2-digit NAICS level. Panel A presents robustness to alternative industry samples: all NAICS sectors, excluding agriculture and manufacturing, services only, and splits into tradable vs. non-tradable industries. Panel B presents alternative measures of prohibition exposure: simple county averages, 1900 Census population-weighted averages, and the share of counties with any forced dry years. Prohibition exposure is defined as the average number of years counties were under forced prohibition between 1880–1920, aggregated to the CZ. China Shock exposure is a Bartik-style measure following Autor et al. (2013), constructed by interacting national industry-level Chinese import growth with CZ industry employment shares in 1990. All regressions include CZ and year fixed effects. Standard errors are clustered at the CZ level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A3: Effects of Prohibition Exposure and China Shock on Nonemployer Firms

Dependent Variables: Model:	Log Establishments (1)	Log Est. Entry (2)	Log Job Creation (3)
Forced Dry × China Shock	0.0001 (0.0002)	-0.0004 (0.0002)	-0.00003 (0.0002)
CZ & Year FE	Yes	Yes	Yes
Observations	37,808	37,507	37,806
Mean Dep. Var	6.32	4.17	6.88
R ²	0.52	0.37	0.21

Notes: This table reports OLS estimates at the commuting zone (CZ) by year level. The dependent variables are the log number of employer establishments, the log establishment entry rate, and the log job creation rate, drawn from the U.S. Census Bureau's Business Dynamics Statistics (BDS). The key regressor is the interaction between forced prohibition exposure and China Shock exposure. Prohibition exposure is measured as the average number of years counties were under forced prohibition between 1880–1920, aggregated to the CZ. China Shock exposure is a Bartik-style predicted import penetration from China, constructed using national industry-level Chinese import growth and 1990 CZ employment shares provided by Autor et al. (2013). All specifications include CZ and year fixed effects. Standard errors, clustered at the CZ level, are reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A4: Employer Establishments

Dependent Variable: Model:		Log Business Applications					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Variables</i>							
4 Years Before the COVID Shock	0.0016 (0.0016)	0.0016 (0.0022)	0.0071 (0.0104)	0.0022 (0.0016)	0.0016 (0.0016)	0.0015 (0.0016)	0.0021 (0.0018)
3 Years Before the COVID Shock	0.0010 (0.0016)	0.0004 (0.0022)	0.0036 (0.0103)	0.0011 (0.0014)	0.0010 (0.0016)	0.0010 (0.0016)	0.0013 (0.0017)
2 Years Before the COVID Shock	0.0009 (0.0014)	0.0002 (0.0018)	0.0068 (0.0098)	0.0012 (0.0012)	0.0009 (0.0014)	0.0008 (0.0015)	0.0013 (0.0016)
COVID Year 2020	0.0215*** (0.0017)	0.0254*** (0.0024)	0.1244*** (0.0105)	0.0194*** (0.0017)	0.0215*** (0.0017)	0.0212*** (0.0017)	0.0204*** (0.0019)
1 Year After the COVID Shock	0.0244*** (0.0017)	0.0254*** (0.0026)	0.1450*** (0.0104)	0.0218*** (0.0024)	0.0244*** (0.0017)	0.0240*** (0.0018)	0.0240*** (0.0021)
2 Years After the COVID Shock	0.0171*** (0.0015)	0.0192*** (0.0022)	0.0983*** (0.0102)	0.0151*** (0.0019)	0.0171*** (0.0015)	0.0170*** (0.0015)	0.0158*** (0.0017)
3 Years After the COVID Shock	0.0133*** (0.0016)	0.0124*** (0.0027)	0.0761*** (0.0102)	0.0129*** (0.0019)	0.0133*** (0.0016)	0.0131*** (0.0016)	0.0127*** (0.0017)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27,119	13,096	28,289	26,071	26,457	22,906	17,759
Pre-Treatment Mean	5.4	5.4	5.4	5.4	5.4	5.4	5.4

Notes: This table reports the event-study coefficients from equation (1) used to produce the main figures in the paper. The dependent variable is the log of business applications from the U.S. Census Bureau's Business Formation Statistics (BFS). The key regressor is the interaction between forced prohibition exposure and the years before and after the COVID shock. Prohibition exposure is measured as the average number of years counties were under forced prohibition between 1880–1920. Columns (1)–(7) report alternative specifications: (1) main specification; (2) sample restricted to counties with any forced exposure; (3) binary treatment defined as 1{forced dry intensity > 0}; (4) controls for voluntary dry years; (5) excludes “Volstead-only” counties (dry only due to federal prohibition, no pre-1919 state/local law); (6) excludes counties first forced after 1918; and (7) excludes referendum states. All models include county, year, and state-by-year fixed effects and controls mentioned in the main specification. Standard errors are clustered at the county level and reported in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A5: Coefficients from the Event-Studies

Variable	Binary Treatment			Quartiles of Forced Dry		Description	Source
	Full Sample	Control (mean (sd))	Treated (mean (sd))	Control (mean (sd))	Treated (mean (sd))		
Business Applications	985 (4111)	821 (3808)	1578 (6190)	701 (3088)	883 (3343)	Number of business applications for Employer Identification Numbers (EINs) likely corresponding to new business formations.	U.S. Census Business Formation Statistics
Establishments Entry Rate	9.51 (2.70)	9.86 (2.96)	10.09 (2.83)	9.41 (2.71)	9.84 (2.64)	Share of establishments that are new entrants in a county–year.	U.S. Census Business Dynamics Statistics
<i>Main Controls</i>							
Population Density	255.61 (1727)	153.17 (640)	351.97 (2589)	152.80 (644)	160.57 (455)	Annual resident population per square mile.	U.S. Census Bureau, Population Estimates Program
White Population Share (%)	0.84 (0.16)	0.86 (0.14)	0.83 (0.16)	0.87 (0.14)	0.82 (0.18)	Percent White.	U.S. Census Bureau, PEP
Manufacturing Share	0.11 (0.10)	0.12 (0.10)	0.11 (0.09)	0.12 (0.10)	0.11 (0.09)	Share of employees in manufacturing sector.	Quarterly Census of Employment and Wages
Service Sector Share	0.55 (0.13)	0.54 (0.12)	0.56 (0.12)	0.55 (0.12)	0.54 (0.13)	Share of employees in service sector.	Quarterly Census of Employment and Wages
Per Capita Income	419.07 (119.56)	451.61 (153.69)	455.72 (141.20)	414.80 (119.20)	406.32 (103.63)	Per capita personal income.	BEA — Regional Economic Accounts
<i>Robustness Controls</i>							
Unemployment Rate	4.93 (1.98)	5.15 (2.32)	5.34 (2.40)	4.84 (1.91)	4.86 (2.04)	Annual average unemployment rate.	BLS, Local Area Unemployment Statistics
Sex Ratio	1.001 (0.08)	0.99 (0.07)	0.99 (0.08)	0.99 (0.07)	1.01 (0.08)	Female-to-male ratio.	U.S. Census Bureau, PEP
Education	30.91 (5.26)	30.93 (5.37)	30.91 (5.11)	30.89 (5.37)	31.11 (4.97)	% adults with a bachelor’s degree or higher.	USDA, Economic Research Service
Total Frontier Experience	1.45 (1.25)	1.43 (1.22)	1.46 (1.28)	1.43 (1.22)	1.53 (1.29)	Number of decades (1790–1890) county was frontier.	Bazzi, Fiszbein, Gebresilasse (2020), <i>Econometrica</i>
Immigrant Share	11.12 (13.08)	9.68 (12.47)	12.90 (13.62)	9.71 (12.52)	10.70 (13.04)	Share of population foreign-born in 1890.	Bazzi, Fiszbein, Gebresilasse (2020)
Temperature	55.16 (8.35)	55.29 (7.83)	55.65 (8.89)	54.91 (7.85)	56.71 (8.83)	Annual average air temperature by county.	NOAA, NORA
Urban	0.05 (0.14)	0.045 (0.12)	0.076 (0.16)	0.045 (0.124)	0.052 (0.123)	Percent living in urban places in 1890.	U.S. Census (Haines 2010); Bazzi et al. (2020)
Distance to Coast	0.37 (0.27)	0.38 (0.26)	0.35 (0.26)	0.38 (0.27)	0.36 (0.26)	Min. great-circle distance from county centroid to nearest U.S. coastline (miles).	Derived from USGS coastline; Bazzi et al. (2020)
Distance to Ports	5.88 (1.06)	5.87 (1.08)	5.89 (1.12)	5.87 (1.02)	5.75 (1.20)	Distance to nearest port of entry (km).	Bazzi et al. (2020)
Agricultural Productivity	0.46 (0.18)	0.47 (0.18)	0.44 (0.18)	0.47 (0.18)	0.43 (0.20)	Average agricultural yield potential (soil, slope, climate).	Bazzi et al. (2020)
Ruggedness	0.06 (0.07)	0.06 (0.07)	0.06 (0.08)	0.06 (0.07)	0.06 (0.08)	Average Terrain Ruggedness Index (TRI).	Bazzi et al. (2020)
Manufacturing (%)–(1890)	2.65 (4.03)	2.05 (3.17)	3.33 (4.17)	2.06 (3.18)	2.51 (3.75)	Share of employment in manufacturing in 1890.	Aggregated by Bazzi et al. (2020)
Scots–Irish (%)	1.50 (2.05)	1.21 (1.66)	1.83 (2.36)	1.22 (1.67)	1.29 (1.71)	Share of population with Scots–Irish ancestry in 1890.	Bazzi et al. (2020)

Notes: This table reports summary statistics and data sources for the main variables used in the analysis. Means and standard deviations (in parentheses) are shown for the full sample, and separately for counties classified as treated or control under both the binary and quartile definitions of forced dry exposure. The binary treatment equals one for counties with any exposure to involuntary prohibition (forced dry > 0). The quartile treatment equals one for counties in the top quartile of the forced dry exposure distribution (forced dry > Q3). Main controls correspond to the covariates included in the baseline specification (1), while historical and geographic controls are added in the robustness analyses.

Appendix Table A6: Summary Statistics by Treatment Status and Treatment Intensity