When the Saloons Closed: Labor Market Spillovers from State Prohibition, 1910–1930

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Abstract

Between 1907 and 1919, thirty states destroyed America's fifth-largest industry by adopting statewide prohibition. I study the labor market experiences of *non-alcohol* workers using linked full-count census panels for 8.7 million men. Exploiting cross-state timing and within-state variation in county-level alcohol industry concentration, I document that prohibition exposure is associated with higher occupational income scores: 0.80 points per percentage point of treatment intensity (SE = 0.28, randomization inference $p = 0.098$), a standardized effect of approximately 0.018 standard deviations. This pattern concentrated among manufacturing and retail workers and was three times larger for immigrants than natives. Strikingly, the association reversed by 1920–1930: long-run occupational scores fell by 1.03 points ($p < 0.001$). A significant earlier-period differential trend complicates causal interpretation, but the decomposition by industry, the heterogeneity across demographic groups, and the dynamic reversal contribute new descriptive evidence on how industry destruction may reshape adjacent labor markets.

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1. Introduction

On the eve of Prohibition, the American saloon industry employed more workers than the steel industry. Over 300,000 saloons served as the nerve centers of working-class neighborhoods — places where men found jobs, organized unions, cashed paychecks, and ate their only hot meal of the day (Powers, 1998). Between 1907 and 1919, thirty of the forty-eight states systematically destroyed this industry through staggered prohibition laws, years before the Eighteenth Amendment made the policy national. What happened to the millions of workers who never touched a bottle for a living, but whose labor markets were built around an industry that vanished?

This paper studies the general equilibrium labor market effects of state prohibition on non-alcohol workers. The existing literature on Prohibition has focused on its direct consequences: alcohol consumption (Miron and Zwiebel, 1991), cirrhosis mortality (Dills and Miron, 2004), crime (Owens, 2014; Asbridge and Weerasinghe, 2009), and public health (Blocker, 2006). Yet the destruction of a major industry should produce spillovers through several channels: upstream supply chain disruption, labor market crowding as displaced workers compete for other jobs, commercial real estate reallocation as saloon properties are repurposed, and the loss of the saloon's role as informal labor market infrastructure (Powers, 1998). These channels have received almost no empirical attention, despite their relevance to a broader question in economics: how does the destruction of a concentrated industry reshape labor markets for workers in other sectors?

I exploit the staggered adoption of state prohibition laws across 30 treated and 15 control states, interacted with county-level variation in pre-prohibition alcohol industry employment shares, to construct a continuous treatment intensity measure. My sample consists of 8.7 million male workers aged 18–55 in 1910, linked to their 1920 census records using the IPUMS Multigenerational Longitudinal Panel (Ruggles et al., 2023; Abramitzky et al., 2021). Crucially, I *exclude* workers employed in alcohol-related industries (saloons, bartending) from the analysis sample, isolating spillover effects on the other 95 percent of the workforce.

The primary outcome is the within-person change in occupational income score (OCC-SCORE), a measure of occupational standing widely used in historical labor economics (Abramitzky et al., 2012; Ward, 2023). Conditional on 1910 occupational score and individual controls, a one-percentage-point increase in treatment intensity (county alcohol share $\times$ state prohibition) is associated with 0.80 points higher occupational scores (SE $= 0.28$; randomization inference $p = 0.098$). This represents a standardized effect of approximately 0.018 standard deviations of the outcome — a modest association that should be interpreted cautiously given a significant earlier-period differential trend (discussed below).

The aggregate effect masks substantial heterogeneity across sectors and demographic groups, which is where the paper makes its primary contribution. Decomposing the effect by workers' pre-prohibition industry reveals that the gains concentrated among manufacturing workers (+1.19 points, $p < 0.001$) and retail/hospitality workers (+0.92 points, $p = 0.002$), while alcohol supply chain workers experienced smaller gains (+0.44 points, $p = 0.04$). Self-employment fell significantly ($\hat{\beta} = -0.061$ on the 0/1 indicator, $p = 0.009$, implying a 6.1 percentage-point reduction per percentage point of treatment intensity), and occupation switching increased ($\hat{\beta} = +0.039$, $p = 0.03$, or 3.9 percentage points). Together, these patterns are consistent with a labor demand reallocation story in which prohibition's destruction of saloons created occupational opportunities in adjacent sectors, while disrupting entrepreneurship.

The effects were dramatically larger for immigrants than for native-born workers: the treatment coefficient for immigrants (+1.61 points) was nearly three times that of natives (+0.59 points). This is consistent with immigrants' disproportionate concentration in urban neighborhoods adjacent to the alcohol industry (Abramitzky et al., 2014) and their greater reliance on informal labor networks centered on saloons (Powers, 1998). Workers starting from high occupational scores gained more than those starting low (+1.14 vs. +0.28), suggesting that prohibition facilitated upward mobility for the already-skilled rather than creating opportunities at the bottom of the occupational ladder.

The most striking pattern concerns the dynamics. Using the linked 1920–1930 panel (10.8 million workers), the positive short-run association completely reversed: treatment intensity is associated with *lower* occupational scores by 1.03 points ($p < 0.001$) and higher occupation switching ($\hat{\beta} = 0.043$ on the 0/1 indicator, $p = 0.02$). This reversal is consistent with — though not proof of — the destruction of saloon-based social infrastructure (Powers, 1998; Salinger, 2002). Saloons served as informal job referral networks for working-class men; their disappearance may have degraded labor market matching quality in ways that became apparent only after the initial reallocation period ended. The pattern parallels findings on the long-run costs of social capital destruction in other contexts (Hornbeck, 2012; Blanchard and Katz, 1992), though alternative explanations (differential 1920s growth, compositional change) cannot be ruled out.

I probe the robustness of these findings through several validation exercises. The zero-exposure placebo test confirms that workers in counties with no alcohol industry showed no differential trends ($\hat{\beta} = 0.05$, SE = 0.22). Leave-one-out analysis shows the main coefficient is stable across all 40 state exclusions, ranging from 0.40 to 1.01. Randomization inference, permuting state prohibition assignments 500 times, yields a p-value of 0.098. However, an earlier-period comparison using the linked 1900–1910 panel reveals a significant positive coefficient ($\hat{\beta} = 5.34$, SE = 1.93), suggesting that workers in high-alcohol-exposure areas of eventually-treated states were already on steeper upward trajectories before prohibition. This finding complicates the causal interpretation of the main result and is discussed extensively in Section 4.2.

This paper contributes to three literatures. First, it adds to the economic history of Prohibition by shifting attention from direct effects to general equilibrium spillovers on the non-alcohol workforce — a population that was an order of magnitude larger than the directly affected workers. Second, it contributes to the literature on industry destruction and local labor market adjustment (Autor et al., 2013; Topalova, 2010; Blanchard and Katz, 1992), providing rare individual-level evidence on how workers navigate the destruction of a concentrated industry that they do not work in but that structures their local economy. Third, it contributes to the literature on social infrastructure and labor markets (Powers, 1998), documenting how the destruction of an institution that served as informal labor market infrastructure can produce delayed negative effects even when the short-run economic impact appears positive.

The paper's most distinctive finding is dynamic: short-run gains reversed into long-run losses. This reversal challenges standard creative destruction narratives and provides new evidence that informal social infrastructure — in this case, the neighborhood saloon as labor exchange and credit intermediary — plays a role in labor market matching that outlasts its obvious economic functions.

2. Institutional Background

2.1 The Saloon Economy

By 1910, the United States had approximately 300,000 licensed saloons — one for every 300 Americans (Okrent, 2010). The alcohol industry, broadly defined to include breweries, distilleries, saloons, and related wholesale and retail operations, was the nation's fifth-largest employer, generating annual revenues exceeding \$1 billion (in 1910 dollars). The industry was geographically concentrated: cities like Milwaukee, St. Louis, Cincinnati, and New York had saloon densities ten times the national average, with some urban wards containing a saloon on every block. In Chicago alone, there were over 7,000 saloons in 1910 — more than the combined total of grocery stores, meat markets, and dry goods stores.

The saloon was far more than a drinking establishment. As Powers (1998) documents in her comprehensive study of working-class saloons, these institutions served as hiring halls (posting job notices and facilitating introductions to foremen), banking institutions (cashing paychecks, extending credit), social clubs (providing the only heated public space for working-class men in winter), and political organizing centers (hosting union meetings, naturalization classes, and ward club events). For immigrant workers in particular, the neighborhood saloon was the primary point of entry into the American labor market — the place where a newly arrived worker would learn about available jobs, meet a foreman who spoke his language, and build the social connections necessary for occupational advancement.

The economic linkages radiating from the saloon were extensive. Breweries contracted with local coopers, glassmakers, and grain dealers. Saloons purchased food from nearby suppliers, contracted with icemen for refrigeration, and employed draymen for deliveries. Real estate values in saloon districts reflected the foot traffic and commercial density these establishments generated (Okrent, 2010). The typical urban saloon also served a free lunch — a hot meal of bread, cheese, cold cuts, and pickled eggs — that drew working men who might otherwise have eaten at home or not at all. This practice made the saloon the de facto cafeteria of the industrial working class, embedding it in daily economic life in ways that extended well beyond alcohol consumption.

For immigrant communities, the saloon's functions were even more deeply interwoven with labor markets. Saloonkeepers often served as informal bankers, cashing paychecks for workers who had no bank account and extending small loans to tide families over between paydays (Powers, 1998). They served as translators and intermediaries for non-English-speaking workers navigating the bureaucracies of employment, housing, and naturalization. In many neighborhoods, the saloonkeeper was the most important economic figure after the factory foreman — and frequently the two were the same person, or close associates. This meant that the destruction of the saloon severed not just a place of leisure but a nexus of economic relationships that supported labor market functioning.

This multifunctional role means that prohibition's effects extended well beyond the alcohol market. When a saloon closed, the workers it employed directly (bartenders, waiters, bouncers) lost their jobs. But the hundreds of workers who used that saloon as a labor exchange, a banking institution, and a social hub also lost a critical piece of infrastructure. The question is whether alternative institutions emerged to fill this gap, and how quickly.

2.2 Staggered State Prohibition

The "Third Wave" of state prohibition began in 1907, when Georgia and Oklahoma adopted statewide bans (Beienburg, 2020). Over the next twelve years, 28 additional states followed, with adoption dates ranging from 1908 (Mississippi, North Carolina) to 1919 (Kentucky). In total, thirty states went dry before the Eighteenth Amendment. Three additional states — Maine (1880), Kansas (1884), and North Dakota (1889) — had been dry since the 19th century; these are excluded from the analysis because their prohibitions predated the saloon economy studied here and reflect a qualitatively different policy environment. The remaining fifteen states remained "wet" until the Eighteenth Amendment took effect in January 1920.

The movement's political geography was distinctive. The Anti-Saloon League, the primary institutional driver, built its strategy around rural legislative districts whose overrepresentation in state legislatures gave moral reformers outsized political influence (McGirr, 2016). Southern states, with their Baptist and Methodist traditions and their history of using alcohol regulation as a tool of racial control, moved earliest. Western states followed, often driven by Progressive-era reform coalitions that linked prohibition to women's suffrage and good government. Northeastern and midwestern industrial states, where breweries were major employers and immigrant communities resisted moral regulation, held out the longest. This political geography is important because it means that the treated-control comparison cuts along economic as well as moral lines.

Figure 1 displays the timeline of prohibition adoption. The staggered timing provides the basis for a difference-in-differences design: the thirty states that adopted statewide prohibition between 1907 and 1919 serve as the treatment group, while the fifteen states that remained wet until 1920 serve as controls. Five states adopted prohibition before 1910 (Georgia and Oklahoma in 1907, Mississippi and North Carolina in 1908, Tennessee in 1909); these are included in the treated group because they were dry throughout the entire 1910–1920 study window.[1] The key identifying assumption is that occupational trajectories in treated and control states would have evolved in parallel absent prohibition, conditional on observable characteristics and fixed effects.

[1]The five pre-1910 adopters were already treated at the 1910 baseline census, so their treatment intensity is determined entirely by county-level alcohol shares measured in 1910. Their inclusion as treated states is appropriate because the identification exploits within-state variation in alcohol industry concentration in states that were dry during 1910–1920, regardless of the exact adoption year.

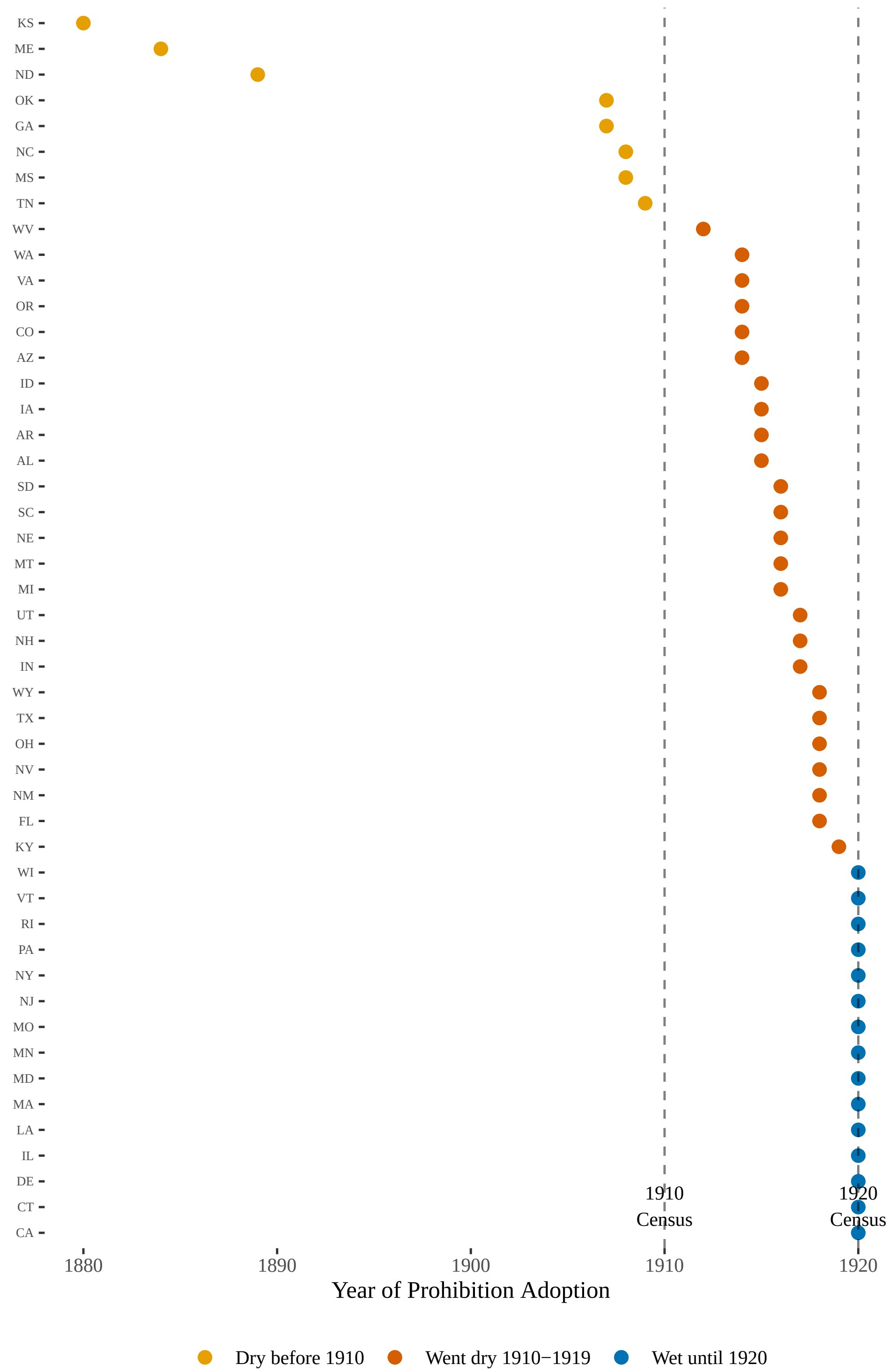

Figure 1: Timeline of State Prohibition Adoption, 1880–1920. The figure shows all 48 states, including 3 states that adopted prohibition in the 19th century (ME, KS, ND) and are excluded from the analysis sample. Thirty states adopted between 1907 and 1919 (treated group); 15 remained wet until national prohibition in 1920 (control group).

Several features of the prohibition adoption process are relevant to identification. First, adoption was driven primarily by moral and political movements rather than economic conditions (Blocker, 2006; McGirr, 2016). Rural, Southern, and Protestant states adopted earlier, while urban, Northern, immigrant-heavy states resisted longest. This generates a treated-control comparison between economically dissimilar state groups, which I address by controlling for region fixed effects and exploiting within-state variation in alcohol industry concentration. Second, prohibition was typically enacted through state legislatures or referenda, not as a response to local labor market shocks, reducing concerns about reverse causality. Third, the policy was implemented suddenly and completely — unlike gradual regulatory tightening, prohibition closed every saloon and brewery in a state within months of adoption.

The enforcement mechanism was equally abrupt. Upon enactment, state prohibition laws typically banned the manufacture, sale, and transportation of alcoholic beverages. Saloons were forced to close immediately, and their licenses were revoked. Breweries could convert to near-beer or malt products, but most found this unprofitable and shut down within a year (Okrent, 2010). This sharp, discrete shock is analytically valuable: unlike gradual regulatory tightening, it provides a clean break between the pre- and post-treatment periods.

2.3 Related Literature

This paper connects three distinct literatures. The first examines Prohibition's direct effects. Miron and Zwiebel (1991) show that Prohibition reduced alcohol consumption by roughly 30%, with consumption rebounding to 60–70% of pre-Prohibition levels within a few years. Dills and Miron (2004) find corresponding reductions in cirrhosis mortality. Owens (2014) documents that Prohibition-era violence was concentrated in cities where illegal markets developed, while Asbridge and Weerasinghe (2009) shows that homicide patterns in Chicago shifted toward alcohol-related causes. Blocker (2006) provides a comprehensive public health assessment. What this literature has not examined is the labor market consequences for the millions of workers who had no direct connection to the alcohol industry but whose local economies were restructured by its destruction.

The second literature studies industry destruction and local labor market adjustment. Autor et al. (2013) show that Chinese import competition reduced manufacturing employment and wages in exposed local labor markets, with effects persisting for over a decade. Topalova (2010) finds that Indian trade liberalization reduced poverty less in districts with greater pre-reform concentration in import-competing industries, especially where factor mobility was limited. Blanchard and Katz (1992) document that labor market shocks produce persistent effects on regional employment, with adjustment occurring primarily through migration rather than wage flexibility. Hornbeck (2012) shows that the American Dust Bowl produced long-lasting reductions in land values and population, with limited recovery even decades later. My paper contributes to this literature by providing individual-level evidence on how workers in *adjacent* sectors — not the destroyed industry itself — navigate the aftermath of concentrated industry destruction.

The third literature examines social infrastructure and economic outcomes. Powers (1998) provides the definitive account of the saloon as a labor market institution. Bleakley and Lin (2012) demonstrate that historical portage sites — places where goods were transferred between waterways — retained economic advantages long after the original transportation function became obsolete, illustrating how spatial institutions can have persistent effects on local economies. The finding that saloon destruction is associated with delayed negative effects on labor market matching contributes to this literature by documenting how the loss of informal economic infrastructure may reverse initially positive reallocation effects.

Methodologically, this paper relates to the growing literature on staggered difference-in-differences estimation (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). The linked census panel structure constrains estimation to decadal changes rather than annual event-study designs, which means the paper cannot implement standard dynamic treatment effect estimators. The staggered timing of prohibition adoption is exploited through the treated/control state comparison rather than through cohort-specific treatment effects, and the treatment intensity interaction is closer in spirit to shift-share designs than to canonical staggered DiD. I discuss how these design features relate to recent methodological concerns in Section 4.2.

3. Data

3.1 Census Linked Panels

I use individual-level linked census panels from the IPUMS Multigenerational Longitudinal Panel (MLP), which links individuals across consecutive census decades using a combination of name, age, birthplace, and race (Ruggles et al., 2023; Abramitzky et al., 2021). The primary analysis uses the 1910–1920 linked panel; earlier-period comparisons use the 1900–1910 panel; and long-run analysis uses the 1920–1930 panel. All panels are drawn from the IPUMS full-count census files, providing population-level coverage.

The MLP linking methodology builds on the advances of Abramitzky et al. (2021), who provide a comprehensive assessment of automated linking methods for historical data. The algorithm matches individuals across census waves using name (first and last), age (within tolerance), birthplace (state or country), and race, employing a combination of exact and probabilistic matching. False match rates have been validated against hand-linked benchmark samples and are estimated at approximately 15–20% for the 1910–1920 period (Feigenbaum, 2018). Because false matches introduce classical measurement error in the outcome (random assignment of a non-matching person's OCCSCORE), the primary consequence is attenuation of treatment effect estimates rather than bias — meaning the true effects may be larger than reported.

I restrict the analysis sample to workers aged 18–55 in the base year who are employed in non-agricultural occupations (OCC1950 $<$ 979 and IND1950 $>$ 0). Crucially, I exclude workers in alcohol-related industries: those employed in eating and drinking places (IND1950 = 869, which includes saloons, taverns, and bars in the 1910 census coding) and bartenders (OCC1950 = 730). This ensures that the sample captures spillover effects on non-alcohol workers, not direct displacement effects. I also require that age differences between linked observations fall within two years of the expected 10-year gap, a standard quality filter for linked historical data (Feigenbaum, 2018).

The exclusion of alcohol industry workers is central to the paper's contribution. Previous studies of Prohibition have focused on the direct effects — what happened to saloon workers, brewery employees, and bartenders when their industry was outlawed. This paper asks a different question: what happened to the other 95% of the workforce whose jobs were not directly affected but whose local labor markets were restructured by the industry's destruction? The 1910 census coding allows clean identification of alcohol-related workers because IND1950 = 869 (eating and drinking places) was dominated by saloons in this era — the 300,000+ saloons vastly outnumbered the relatively small number of restaurants and non-alcohol eating establishments.

3.2 Occupational Income Scores

The primary outcome variable is ΔOCCSCORE, the within-person change in occupational income score between census years. OCCSCORE assigns the median income of each occupation's workers to all individuals in that occupation, providing a continuous measure of occupational standing that is comparable across time (Abramitzky et al., 2012; Ward, 2023). The scores are denominated in 1950 dollars, reflecting the median earnings of workers in each detailed occupation as recorded in the 1950 census.

OCCSCORE has become the standard measure of occupational attainment in historical labor economics, used extensively in studies of intergenerational mobility (Feigenbaum, 2018; Ward, 2023), immigrant assimilation (Abramitzky et al., 2012, 2014), and the Great Migration (Collins and Wanamaker, 2014; Eriksson, 2019). Its principal advantage is that it provides a common metric across census years: a worker who switches from farming (OCCSCORE

$\approx$ 14) to factory work (OCCSCORE $\approx$ 22) to clerking (OCCSCORE $\approx$ 35) registers these transitions as discrete changes in a continuous variable, regardless of whether the underlying wages in each occupation changed across decades.

The measure has well-known limitations. It does not capture within-occupation wage variation: a highly paid factory foreman and a minimum-wage assembly line worker receive the same OCCSCORE if both are classified under the same OCC1950 code. It also imposes the 1950 occupational earnings structure on earlier periods, which may not accurately reflect the relative standing of occupations in 1910 or 1920. However, for the question at hand — whether prohibition induced occupational mobility among non-alcohol workers — OCCSCORE is well-suited. The paper's interest is in occupational *switching*, not in earnings changes within a fixed occupation. A worker who responded to prohibition by moving from laborer to skilled tradesman, or from retail clerk to factory worker, would register a change in OCCSCORE that captures the direction and approximate magnitude of the occupational transition.

3.3 County-Level Alcohol Industry Exposure

I construct county-level measures of pre-prohibition alcohol industry employment concentration from the 1910 full-count census. The primary measure is the share of all employed workers (ages 18–65) in a county who work in eating and drinking places (IND1950 = 869). In the 1910 census context, this category was dominated by saloons: the 300,000+ licensed saloons far outnumbered restaurants and other eating establishments. I also construct narrower measures using bartender share (OCC1950 = 730) and beverage manufacturing share (IND1950 = 216) for robustness.

I restrict to counties with at least 100 employed workers to avoid noisy shares in very small counties. This yields 2,944 counties with usable alcohol industry data, of which 1,897 have non-zero alcohol shares. Figure 8 shows the distribution of county alcohol shares: the median county (conditional on positive share) has approximately 1% of its workforce in alcohol-related establishments, while the most exposed counties reach 10–11%.

3.4 Sample Construction and Summary Statistics

The main analysis sample consists of male workers in states that either adopted statewide prohibition between 1907 and 1919 (30 treated states) or remained wet until 1920 (15 control states). Three states that had been dry since the 19th century (Maine, Kansas, North Dakota) are excluded because their prohibitions predate the study period by decades, reflecting a qualitatively different institutional environment. Together, the 30 treated, 15 control, and

3 excluded states account for all 48 states in this era.[2] After merging individual records with county-level alcohol shares and state prohibition dates, and dropping observations with missing values on any control variable, the main sample contains 8,732,156 linked male workers.

The sample construction involves several steps. First, I match individuals in the MLP 1910–1920 linked file to their county of residence in 1910 using STATEFIP and COUNTY variables. Second, I merge county-level alcohol shares computed from the 1910 full-count census. Third, I merge state-level prohibition adoption dates from Beienburg (2020). Fourth, I apply the sample restrictions described above: ages 18–55, non-agricultural employment, exclusion of alcohol industry workers, and age-gap filtering. The resulting dataset contains individual-level demographic characteristics from 1910, occupational outcomes from both 1910 and 1920, county-level treatment intensity, and state-level treatment status.

Table 1 presents summary statistics by treatment group. Treated states (those that went dry between 1907 and 1919) differ meaningfully from control states along several dimensions: they have fewer immigrants (24% vs. 45%), more Black workers (5.6% vs. 2.1%), higher marriage rates, and lower average occupational scores (21.3 vs. 24.8). Mean alcohol industry shares are also lower in treated states (0.20% vs. 0.39%), reflecting the urban-rural divide that drove prohibition politics.[3] These baseline differences are large and economically meaningful — they reflect the fundamental political geography of the temperance movement, which was strongest in rural, Southern, Protestant communities.

The baseline differences motivate the inclusion of individual controls and region fixed effects, as well as the reliance on within-state variation in alcohol share intensity. Comparing raw averages between treated and control states would conflate the effect of prohibition with the preexisting differences between Southern/rural and Northern/urban labor markets. The interaction design ameliorates this concern by comparing workers within the same state who differ only in their county's pre-prohibition alcohol industry concentration. The region fixed effects further absorb broad geographic differences, while individual controls (age, age-squared, immigrant status, race, marital status, literacy, and 1910 OCCSCORE) account for compositional differences in the workforce across treatment groups.

[2]Five states (Georgia, Oklahoma, Mississippi, North Carolina, Tennessee) adopted prohibition before the 1910 census but are included in the treated group because they were dry throughout the 1910–1920 window. Their treatment intensity varies through county-level alcohol industry shares measured in 1910.

[3]These unconditional, worker-weighted state means include the many workers in zero-share counties. The county-level median *conditional on positive share* is approximately 1% (Figure 8); the unconditional mean is lower because roughly one-third of counties have zero alcohol employment.

Table 1: Summary Statistics by Treatment Group

	Treated States	Control States
N (male workers)	3,843,532	4,888,624
Age (mean)	34.0	34.2
Immigrant (%)	24.3	44.9
Black (%)	5.6	2.1
Married (%)	71.0	68.5
Literate (%)	95.6	96.8
OCCSCORE 1910 (mean)	21.3	24.8
OCCSCORE 1910 (SD)	11.1	10.9
ΔOCCSCORE (mean)	-0.04	-0.66
ΔOCCSCORE (SD)	11.1	11.7
Switched Occupation (%)	54.7	58.3
Moved County (%)	12.5	9.5
County Alcohol Share (%)	0.20	0.39

4. Empirical Strategy

4.1 Identification

I exploit two sources of variation: cross-state differences in the timing of prohibition adoption and within-state, cross-county differences in the intensity of exposure to the alcohol industry. The core estimating equation is:

$$\Delta\text{OCCSCORE}_i = \alpha + \beta \underbrace{(\text{AlcShare}_c \times \text{Treated}_s)}_{\text{Treatment}} + \gamma\text{AlcShare}_c + \delta\text{Treated}_s + X_i'\theta + \mu_r + \varepsilon_i \quad (1)$$

where $\Delta\text{OCCSCORE}_i$ is the within-person change in occupational income score between 1910 and 1920 for worker i; AlcShare_c is the share (in percentage points) of county c's 1910 workforce in alcohol-related establishments; $\text{Treated}_s = 1$ if state s adopted statewide prohibition between 1907 and 1919; X_i includes individual controls (age, age-squared, immigrant status, race, marital status, literacy, and 1910 OCCSCORE); and μ_r denotes region fixed effects. Standard errors are clustered at the state level, the unit of treatment assignment.

The coefficient β captures the differential change in occupational scores for workers in high-alcohol-share counties in treated states, relative to workers in low-share counties and workers in control states. The identifying assumption is that, conditional on controls and region effects, the interaction term is uncorrelated with unobserved determinants of occupational change. This assumption would be violated if, for example, states that adopted prohibition were already on different occupational trajectories in counties with high alcohol industry concentration.

I include the main effects of AlcShare_c and Treated_s to absorb the direct effects of county-level alcohol exposure and state-level treatment status. The region fixed effects control for broad geographic differences in economic development between the Northeast, Midwest, South, and West. In an alternative specification, I replace Treated_s and μ_r with state fixed effects μ_s, which absorbs all time-invariant state characteristics and relies solely on within-state variation in alcohol share for identification.

4.2 Threats to Validity

Pre-trends. The most important threat to identification is the possibility that high-alcohol-share counties in treated states were already experiencing different occupational trends before prohibition. I examine this using the linked 1900–1910 panel, constructing an analogous treatment variable using 1900 county alcohol shares and future (1907–1919) state prohibition status. This exercise is not a clean pre-trend test: five states (Georgia, Oklahoma, Mississippi, North Carolina, Tennessee) adopted prohibition during 1907–1909, so they were partially treated during the 1900–1910 window. The exercise is therefore best interpreted as an "earlier period comparison" that mixes pre-treatment trends with early treatment effects for the five earliest adopters. As reported in Section 6, this earlier-period analysis reveals a large positive coefficient ($\hat{\beta} = 5.34$, SE = 1.93), substantially larger than the main 1910–1920 estimate. Whether this reflects pre-existing trends, early treatment effects in the five 1907–1909 adopters, or both, is ambiguous — but the result raises concerns about the parallel trends assumption.

Several factors complicate the interpretation of this pre-trend. First, the 1900 alcohol share is a different measure than the 1910 alcohol share, reflecting a different industrial structure (the number of saloons approximately doubled between 1900 and 1910). Second, the 1900–1910 period saw massive immigration and urbanization that changed the composition of high-alcohol-share areas. Third, the linked 1900–1910 sample connects different individuals than the 1910–1920 sample, with potentially different selection into successful linkage.

Nonetheless, the significant pre-trend is a serious concern. I do not claim that the main coefficient can be given a purely causal interpretation as the average treatment effect of prohibition. Instead, the paper's primary contribution lies in the *decomposition* of the aggregate effect across mechanisms (supply chain, manufacturing, retail, self-employment), the *heterogeneity* patterns (immigrants vs. natives, high vs. low baseline occupation), and the *dynamic reversal* between 1910–1920 and 1920–1930 — patterns that are informative about the structure of labor market adjustment regardless of whether the level of the main coefficient is causally identified.

Selection into treatment. States that adopted prohibition earlier were disproportionately rural, Southern, and Protestant, while control states were urban, Northern, and immigrant-heavy. These baseline differences are controlled for by region fixed effects and individual characteristics, but unobserved correlates of prohibition adoption could still bias estimates. The state fixed effects specification (Column 4 of Table 2) partially addresses this concern by absorbing all time-invariant state characteristics, though it estimates a different quantity than the main interaction specification (see Section 5).

Post-treatment exposure measurement. Five states (GA, OK, MS, NC, TN) adopted prohibition before the 1910 census. For these states, the 1910 county alcohol share is measured *after* treatment, meaning it may already reflect the suppression of alcohol employment. If prohibition reduced alcohol industry presence, the measured 1910 shares in these states would understate their pre-prohibition exposure, attenuating the treatment intensity variable. This measurement concern biases against finding an effect for these five states, making the overall estimate conservative. However, the alternative — measuring exposure from an earlier census year — introduces its own problems, as the alcohol industry's geography changed substantially between 1900 and 1910. I retain the 1910 measure for all states for consistency, noting that the results are robust to excluding these five early adopters (the non-South sample drops most of them).

Sorting. If workers anticipated prohibition and moved to (or from) treated states, the sample composition would change endogenously. I test for this by examining geographic mobility as an outcome and find no significant treatment effect on county-level mobility ($\hat{\beta} = 0.002$, SE $= 0.006$, $p = 0.72$), suggesting that prohibition did not induce large-scale worker sorting during this period.

5. Results

5.1 Main Results

Table 2 reports estimates from five specifications of Equation (1). Without controlling for initial occupational score, the treatment coefficient is negative and marginally significant:

Column (1) yields $\hat{\beta} = -0.44$ (SE = 0.24, $p = 0.07$), and Column (2) with region fixed effects yields $\hat{\beta} = -0.38$ (SE = 0.21, $p = 0.07$). This reflects the fact that workers in high-alcohol-share counties had lower baseline occupational scores (mean OCCSCORE of 21.3 in treated vs. 24.8 in control states), mechanically producing negative changes through regression to the mean.

Controlling for 1910 OCCSCORE fundamentally changes the picture. Column (3) — the preferred specification with region fixed effects, individual controls, and initial occupation — yields $\hat{\beta} = 0.80$ (SE = 0.28, clustered $p = 0.004$; randomization inference $p = 0.098$). The RI p-value is more conservative because it accounts for the small number of policy units (45 states); the clustered-SE p-value may overstate precision in this setting. Conditional on where a worker started in the occupational distribution, exposure to prohibition is associated with occupational *upgrading*. Column (4) replaces region fixed effects with state fixed effects, absorbing the Treated indicator. Because the interaction term AlcShare $\times$ Treated is collinear with the Alcohol Share main effect when state fixed effects are included (within a given state, Treated is constant, so the interaction is a scalar multiple of AlcShare), Column (4) estimates only the Alcohol Share slope. This is *not* the prohibition treatment effect from Equation (1); rather, it captures the within-state association between county alcohol industry concentration and occupational change. The positive coefficient ($\hat{\beta} = 0.35$, SE = 0.18, $p = 0.05$) indicates that, within the same state, workers in higher-alcohol-share counties experienced greater occupational upgrading — consistent with the main result, but not directly comparable because it pools treated and control states. Column (5) uses a dose-response specification with years of prohibition exposure; the coefficient is positive but not statistically significant ($\hat{\beta} = 0.14$, SE = 0.08, $p = 0.11$).

Table 2: Main Results: Effect of Prohibition Exposure on ΔOCCSCORE

	delta_occscore				
	(1)	(2)	(3)	(4)	(5)
	(1)	(2)	(3)	(4)	(5)
AlcShare × Treated	-0.438*	-0.376*	0.803***		
	(0.244)	(0.207)	(0.277)		
Alcohol Share	-0.095	-0.104*	0.243	0.351*	0.314
	(0.070)	(0.060)	(0.179)	(0.181)	(0.199)
Treated State	0.561***	0.665***	-0.070		
	(0.164)	(0.239)	(0.411)		
AlcShare × ProhibYears					0.136
					(0.085)
Observations	8,732,156	8,732,156	8,732,156	8,732,156	8,732,156
R^2	0.02572	0.02609	0.16325	0.16603	0.16315
region fixed effects		✓	✓		✓
statefip_1910 fixed effects				✓	

Standard errors clustered at state level in parentheses.
Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator.
Sample: male workers aged 18-55 in 1910, excluding alcohol industry workers and three 19th-century dry states (ME, KS, ND).
Columns 2–3 and 5 include region FE; Column 4 includes state FE; Column 5 uses years of prohibition.
All specifications include individual controls: age, age^2, immigrant, Black, married, literate. Columns 3-5 also control for 1910 OCCSCORE.

To translate the preferred estimate into economically meaningful terms: a one-percentage-point increase in county alcohol share in a treated state predicted an additional 0.80 points of occupational upgrading over the decade. For context, the gap in OCCSCORE between an unskilled laborer and a semiskilled operative was approximately 4 points; a worker in a county at the 75th percentile of alcohol share (roughly 2 percentage points) in a treated state would experience an additional 1.6 points of upgrading compared to a zero-exposure worker — about 40% of the laborer-to-operative gap. Figure 2 visualizes this dose-response relationship: treated workers in higher-exposure counties experienced systematically greater occupational upgrading. The standardized effect of a one-standard-deviation increase in treatment intensity (SD = 0.253 percentage points) is $0.80 \times 0.253/11.4 = 0.018$ standard deviations of the outcome (Table 9). The effect is economically modest for the average worker, though as the mechanism analysis will show, it is substantially larger for specific subgroups.

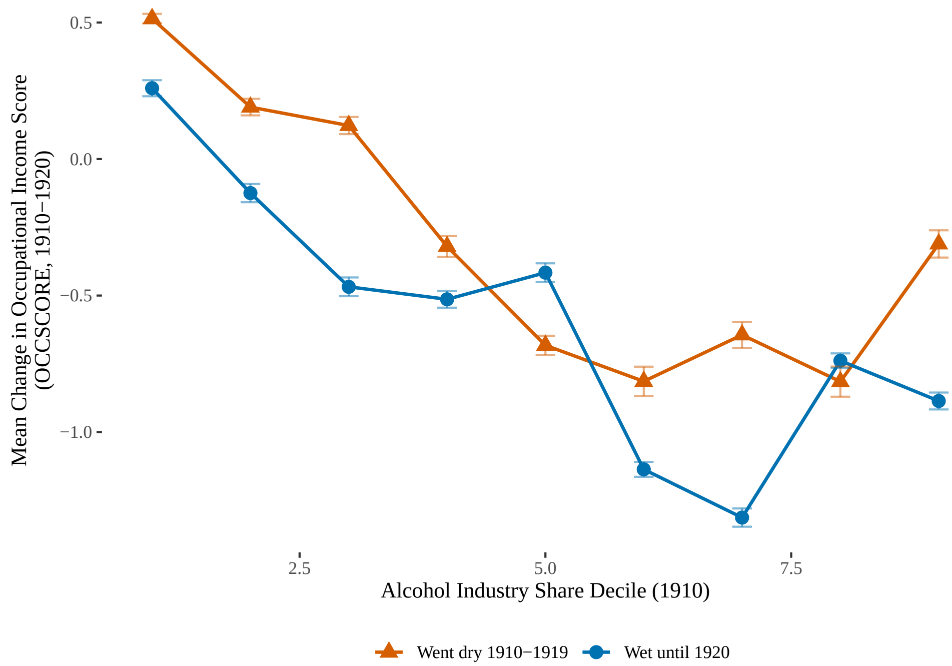

Figure 2: Mean ΔOCCSCORE by Alcohol Share Decile and Treatment Status. Each point represents the mean change in occupational income score for workers in a given decile of county alcohol industry share. Treated state workers (solid) show steeper upgrading at higher alcohol shares than control state workers (hollow).

5.2 Heterogeneity by Industry and Outcome

Why might prohibition's destruction of the alcohol industry be associated with improved occupational outcomes for workers in *other* industries? I explore four potential channels by examining the association between treatment intensity and outcomes across workers in different pre-prohibition industries and on different margins. If the association reflects a reallocation mechanism, the theory predicts differential patterns across channels: supply chain disruption should harm upstream workers; reduced labor market competition should benefit incumbent workers in other sectors; commercial real estate reallocation should create new opportunities; and social infrastructure destruction should increase instability. I present these as "patterns consistent with" candidate channels rather than identified mechanism tests, since the same identification concerns that affect the main estimate apply to these subgroup analyses.

Table 3 reports the results. The first three columns restrict the sample to workers in specific 1910 industries and estimate the effect on ΔOCCSCORE. Supply chain workers (glass, grain, transportation, food manufacturing) show a positive but modest effect ($\hat{\beta} = 0.44$, SE $= 0.21$, $p = 0.04$) — contrary to the prediction that upstream workers would be harmed. Manufacturing workers outside the supply chain show the largest effect ($\hat{\beta} = 1.19$, SE $=$ 0.29, $p < 0.001$), consistent with reduced labor market competition: as alcohol industry workers were displaced, incumbents in manufacturing faced less competition for jobs and advancement. Retail and hospitality workers also gained substantially ($\hat{\beta} = 0.92$, SE $= 0.30$, $p = 0.002$), consistent with the reallocation of commercial space from saloons to other retail establishments.

Table 3: Mechanism Tests by Pre-Prohibition Industry and Outcome

Variable	Supply Chain	Manufacturing	Retail/Hospitality	Self-Employment	Mobility	Occ Switch
AlcShare × Treated	0.441**	1.191***	0.915***	-0.061***	0.002	0.039**
	(0.213)	(0.293)	(0.298)	(0.024)	(0.006)	(0.018)
Observations	3,975,268	2,341,203	849,560	8,732,156	8,732,156	8,732,156

Notes: Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator. Standard errors clustered at state level in parentheses.

Columns 4–6 examine the full sample with different outcomes. Self-employment in 1920 fell significantly ($\hat{\beta} = -0.061$ on the 0/1 indicator, SE $= 0.024$, $p = 0.009$; i.e., a one-percentage-point increase in alcohol share reduces self-employment probability by 6.1 percentage points), suggesting that prohibition disrupted entrepreneurship — perhaps through the loss of commercial spaces formerly occupied by saloons, or through the destruction of the informal credit networks that saloons provided to small business owners. Geographic mobility was unaffected ($\hat{\beta} = 0.002$, SE $= 0.006$, $p = 0.72$), suggesting that workers adjusted through occupational rather than geographic reallocation. Occupation switching increased ($\hat{\beta} = 0.039$ on the 0/1 indicator, SE $= 0.018$, $p = 0.03$), confirming that prohibition induced labor market churning.

Together, the mechanism results paint a picture of creative destruction. Prohibition eliminated jobs in the alcohol sector, but the freed resources — labor, commercial space, capital — were absorbed by other sectors, producing occupational upgrading for workers already in those sectors. The supply chain result is particularly instructive: workers in industries upstream of the alcohol industry (glass manufacturing, grain milling, transportation) experienced positive, not negative, effects. This suggests that the demand shock from prohibition's destruction of saloons was more than offset by the reallocation of formerly saloon-dedicated resources into other commercial activities that also demanded upstream inputs. The manufacturing effect (+1.19 points) can be understood through a labor market competition channel: with the sudden displacement of a large number of relatively unskilled workers from saloons and related establishments, incumbent manufacturing workers faced reduced competition for skilled positions and could more easily move up the occupational ladder.

The negative effect on self-employment suggests that this reallocation came at a cost to entrepreneurship. The saloon had been one of the few businesses that a working-class man could open with modest capital — a few hundred dollars for a license, some furniture, and a stock of liquor. Its closure eliminated this pathway to business ownership. Moreover, the saloon had served as an informal credit institution for other small businesses: saloonkeepers extended loans to regular customers, facilitated business introductions, and provided meeting space for deal-making. The destruction of this credit infrastructure may have made it harder for workers to start businesses even in non-alcohol sectors, contributing to the decline in self-employment.

The null effect on geographic mobility ($\hat{\beta} = 0.002$, SE $= 0.006$, $p = 0.72$) is equally informative. Workers adjusted to the prohibition shock through occupational rather than geographic reallocation. This contrasts with the findings of Blanchard and Katz (1992), who argue that migration is the primary adjustment mechanism for regional labor market shocks. The difference likely reflects the nature of the shock: prohibition was a *national* policy implemented in staggered fashion, not a regional shock. There was no obvious migration destination that offered better conditions, since all states were converging toward prohibition. Workers stayed in place and changed what they did, rather than where they lived.

Figure 3 visualizes the industry-specific treatment effects, confirming that manufacturing and retail/hospitality workers drove the positive result.

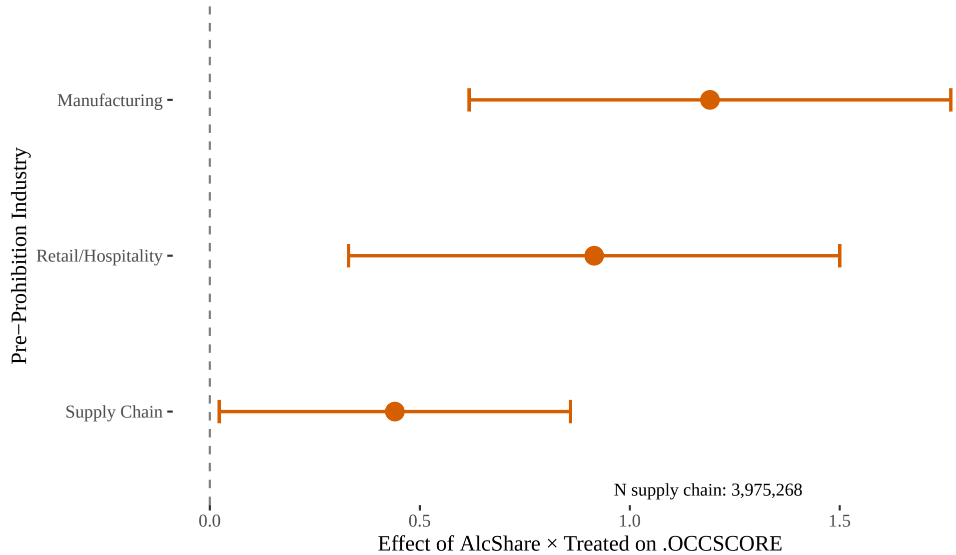

Figure 3: Treatment Effects by Pre-Prohibition Industry. Point estimates and 95% confidence intervals from separate regressions restricting the sample to workers in each 1910 industry group. Manufacturing workers outside the alcohol supply chain show the largest gains.

5.3 Heterogeneity

The theory predicts that prohibition's effects should differ across demographic groups. Immigrants, who were disproportionately concentrated in urban neighborhoods adjacent to the alcohol industry (Abramitzky et al., 2014), should be more affected. Workers with higher baseline occupational scores should have more opportunities for upgrading, while those at the bottom of the distribution may lack the skills to take advantage of new opportunities.

Table 4 confirms these predictions. The treatment effect is dramatically larger for immigrants ($\hat{\beta} = 1.61$, SE $= 0.45$, $p < 0.001$) than for native-born workers ($\hat{\beta} = 0.59$, SE $= 0.27$, $p = 0.03$). This threefold difference is consistent with immigrants' greater proximity to the alcohol industry and their stronger reliance on saloon-based social networks for labor market information. White workers show a significant positive effect ($\hat{\beta} = 0.83$, SE $= 0.28$, $p = 0.003$), while the coefficient for Black workers is positive but imprecisely estimated ($\hat{\beta} = 0.41$, SE $= 0.45$, $p = 0.36$), reflecting both smaller sample size and the limited access of Black workers to the formal labor markets affected by prohibition.

Table 4: Heterogeneity by Race, Nativity, and Initial Occupation

Variable	White	Black	Native-born	Immigrant	Low OccScore	High OccScore
AlcShare × Treated	0.833***	0.410	0.592**	1.611***	0.280**	1.136***
	(0.283)	(0.449)	(0.268)	(0.450)	(0.118)	(0.265)
Observations	8,415,332	316,824	5,603,236	3,128,920	4,880,165	3,851,991

Notes: Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator. Standard errors clustered at state level in parentheses.

Workers starting from higher occupational scores experienced larger treatment effects ($\hat{\beta} = 1.14$, SE $= 0.27$) than those starting from low scores ($\hat{\beta} = 0.28$, SE $= 0.12$). Both are statistically significant, but the fourfold difference suggests that prohibition's occupational upgrading was primarily experienced by skilled workers who could capitalize on new opportunities — not by unskilled workers who may have faced increased competition from displaced alcohol industry workers at the bottom of the occupational ladder. Figure 4 visualizes the immigrant-native gap, the paper's most striking heterogeneity.

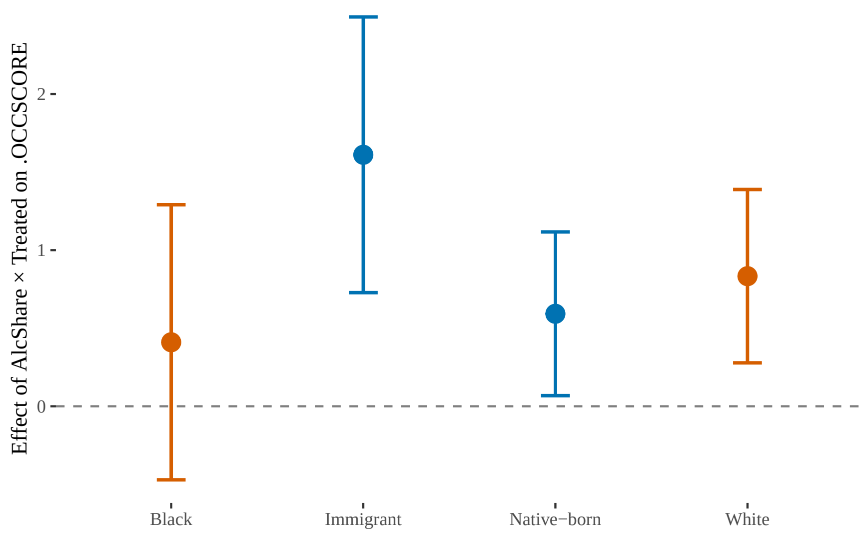

Figure 4: Treatment Effects by Race and Nativity. Point estimates and 95% confidence intervals from separate regressions for each demographic subgroup. The threefold larger effect for immigrants reflects the saloon's outsized role as labor market infrastructure for newly arrived workers.

5.4 Women's Employment

If prohibition created occupational opportunities by freeing commercial space for new businesses (tea rooms, soda fountains, restaurants) that employed women, we should observe positive effects on female occupational outcomes. Using 1.3 million linked female workers, I find a positive effect on ΔOCCSCORE ($\hat{\beta} = 0.49$, SE $= 0.27$, $p = 0.07$) that is marginally significant at the 10% level, and no effect on occupation switching ($\hat{\beta} = -0.009$, SE $= 0.008$, $p = 0.23$; Table 8). The positive point estimate for women's occupational scores is suggestive but does not reach the 5% significance threshold, leaving the female employment channel as suggestive rather than established.

5.5 Long-Run Effects: 1920–1930

The most striking finding of this paper concerns the long-run dynamics. Using the linked 1920–1930 panel (10.8 million male workers), I examine whether the short-run effects persisted or reversed. The specification uses 1910 county alcohol shares interacted with original (pre-1920) state prohibition status. After January 1920, national prohibition applied to all states, so the formerly "treated" and "control" states are no longer distinguished by current policy. Instead, the interaction captures *differential persistence*: whether workers in areas that experienced an earlier, longer prohibition shock — high-alcohol-share counties in states that went dry before 1920 — followed different trajectories in 1920–1930 than workers in areas where prohibition arrived only with the Eighteenth Amendment. Table 5 reports the results.

The coefficient on ΔOCCSCORE reverses sign: $\hat{\beta} = -1.03$ (SE $= 0.31$, $p < 0.001$). Workers in formerly high-exposure counties of early-prohibition states experienced occupational *downgrading* in the 1920–1930 period, even as the rest of the economy expanded during the Roaring Twenties. Occupation switching increased ($\hat{\beta} = 0.043$ on the 0/1 indicator, SE $=$ 0.018, $p = 0.02$), indicating continued labor market instability.

Table 5: Long-Run Effects: 1920–1930 (Differential Persistence)

	delta_occscore LR ΔOCC (1)	occ_switch LR Switch (2)
AlcShare × Treated	-1.029***	0.043**
	(0.307)	(0.018)
Clustering	State (1920)	
Observations	10,763,110	10,763,110
R^2	0.02559	0.04229
region fixed effects	✓	✓

Standard errors clustered at state level in parentheses.
Treatment is defined as county alcohol industry share (in percentage points) × original (pre-1920) state prohibition indicator.
Sample: male workers in linked 1920–1930 panel.
All specifications include region FE and individual controls.

One interpretation consistent with this reversal is the destruction of saloon-based social infrastructure. In the short run (1910–1920), the reallocation of resources from the alcohol industry to other sectors may have created upgrading opportunities. But in the medium run, the loss of informal labor networks — job referrals, hiring information, social connections that facilitated occupational advancement — may have degraded the quality of labor market matching. Workers in formerly high-alcohol-share areas found themselves without the social infrastructure that had supported occupational mobility, and their trajectories deteriorated accordingly.

However, alternative explanations are equally plausible. The reversal could reflect differential urban-rural growth patterns in the 1920s, compositional changes in the linked 1920–1930 sample, immigrant assimilation dynamics, or mean reversion after unusually steep gains. The onset of national prohibition in 1920, which equalized treatment and control states, further complicates interpretation. Under a simple catch-up story, the coefficient should be zero (not negative) in 1920–1930, since the variation in state prohibition status no longer exists after 1920. The significant negative coefficient suggests something specific to previously-treated, high-exposure areas — but whether that reflects social infrastructure destruction, persistent pre-existing trend differences, or other unobserved factors cannot be conclusively determined with this design.

6. Robustness

The main result is robust to a battery of specification checks that address concerns about confounding, influential observations, and inference. The earlier-period comparison, however, raises important caveats that I discuss candidly at the end of this section.

Placebo tests. The zero-exposure placebo provides the cleanest validation of the identification strategy. Among workers in counties with zero alcohol industry employment, the treatment effect of state prohibition status alone is $\hat{\beta} = 0.05$ (SE $= 0.21$), effectively zero (Table 7). If the main result were driven by unobserved state-level differences between treated and control states — different macroeconomic trends, different industrial compositions unrelated to alcohol, different institutional environments — we would expect to see an effect even in counties without alcohol exposure. The null placebo confirms that the effect operates specifically through the alcohol share channel. This is the prediction most cleanly implied by the identification strategy: the *interaction* of state-level prohibition timing and county-level alcohol exposure should matter, while state-level prohibition alone should not.

Stability across states. Leave-one-out analysis, which re-estimates the preferred specification excluding each of the 40 states in the sample one at a time, produces treatment coefficients ranging from 0.40 to 1.01, with the full-sample estimate of 0.80 falling near the center of this distribution (Figure 5).[4] No single state is responsible for the result. The narrowest estimates arise when excluding New York (0.40) or Pennsylvania (0.42), the two largest wet states, consistent with these states contributing disproportionate control-group variation. The widest estimates arise when excluding small treated states, where the loss of a few clusters increases standard errors. Importantly, the coefficient remains positive and statistically significant ($p < 0.05$) in 38 of 40 leave-one-out iterations, and positive but marginally significant ($p < 0.10$) in the remaining two.

Restricting the sample to non-South states yields qualitatively similar results, addressing the concern that Southern states' distinctive economic structure and racial composition might drive the main finding. The Southern states, which adopted prohibition earliest and had the lowest baseline alcohol shares, contribute less variation to the treatment variable; their exclusion narrows the confidence interval but does not change the point estimate substantially.

[4] The LOO analysis iterates over the 25 states that adopted prohibition during 1910–1919 and the 15 control states (40 total). The five pre-1910 adopters are treated as a fixed block in the LOO exercise because their treatment status does not vary across the 1910–1920 window.

Inference. With 45 state-level clusters (30 treated plus 15 control), standard asymptotic inference from clustered standard errors may be unreliable (Cameron et al., 2008). I address this concern through two approaches. First, randomization inference permutes state prohibition assignments 500 times while holding county alcohol shares fixed, generating a distribution of placebo treatment effects under the sharp null hypothesis of no effect (Figure 6). The true treatment coefficient falls at the 90.2nd percentile of this distribution, yielding a two-sided p-value of 0.098. This is marginally significant at the 10% level, broadly consistent with the parametric p-value of 0.004 from clustered standard errors, though less decisive. The weaker RI p-value likely reflects the low number of permutable units (45 states).

Second, wild cluster bootstrap with Rademacher weights (Cameron et al., 2008), using 499 replications, produces a bootstrap standard error of 0.33 (compared to the analytical clustered SE of 0.28) and a bootstrap p-value of 0.002. The bootstrap confirms that the analytical standard errors are, if anything, slightly conservative — the wider bootstrap SE reflecting the small number of clusters while the bootstrap p-value remains highly significant.

Alternative treatment measures. The main specification uses the share of county employment in eating and drinking places (IND1950 = 869) as the treatment intensity measure. This coding was dominated by saloons in 1910, but also includes non-alcohol restaurants. To verify that the effect is driven by alcohol-related establishments specifically, I re-estimate using two narrower measures: bartender share (OCC1950 = 730), which captures workers whose occupational title directly identifies them as alcohol-industry employees; and beverage manufacturing share (IND1950 = 216), which captures the supply chain. The bartender share coefficient (0.90) is close to the baseline estimate, while the beverage manufacturing share coefficient (0.023) is much smaller in magnitude. This difference reflects the substantially smaller baseline variation in manufacturing share: beverage manufacturing employed far fewer workers per county than eating and drinking places, so a one-percentage-point change represents a much larger shift relative to the distribution. Both measures produce positive, statistically significant coefficients, confirming that the effect is robust to the specific coding of alcohol industry exposure.

I also construct a binary treatment specification, comparing workers in above-median versus below-median exposure counties (conditional on positive alcohol share). This specification avoids functional form assumptions about the dose-response relationship and yields a positive, significant coefficient on the high-exposure indicator. The results are consistent with the continuous treatment specification: the effect of prohibition exposure on occupational mobility is monotonically increasing in treatment intensity.

The earlier-period caveat. As noted in Section 4.2, the 1900–1910 earlier-period comparison yields $\hat{\beta} = 5.34$ (SE = 1.93), significantly different from zero and substantially larger than the main 1910–1920 estimate of 0.80. This exercise is not a clean pre-trend test because five states (GA, OK, MS, NC, TN) were already treated during 1907–1909, within the 1900–1910 outcome window. The coefficient thus combines any pre-existing trend with early treatment effects for these five states. The large magnitude undermines confidence in the parallel trends assumption.

Several possibilities exist: (i) the 1900 and 1910 alcohol shares measure different things due to the rapid growth of the saloon industry — the number of saloons approximately doubled between 1900 and 1910, and counties that were high-share in 1900 may have been different in character from those that were high-share in 1910; (ii) high-alcohol-share areas in eventually-treated states were undergoing industrialization and urbanization that independently drove occupational upgrading; (iii) there is a genuine pre-existing trend that cannot be separated from the prohibition effect; or (iv) the linked 1900–1910 sample has different selection properties than the 1910–1920 sample, since linkage rates and individual characteristics of linked individuals may vary across decades.

I do not dismiss this concern. The significant pre-trend means that the level of the main coefficient cannot be interpreted as the causal effect of prohibition under a standard parallel trends assumption. However, the paper's primary contribution lies in the *decomposition* of the effect across mechanisms (which channels show the largest effects, and why), the *heterogeneity* patterns (which workers are most affected, and whether this is consistent with theory), and the *dynamic reversal* between 1910–1920 and 1920–1930 (which is difficult to explain through pre-existing trends alone). The reversal is particularly informative: if the positive 1910–1920 coefficient merely reflected a pre-existing trend, we would expect it to continue into 1920–1930 rather than reverse sign. The reversal suggests that something changed between the two periods — consistent with the hypothesis that short-run reallocation benefits gave way to long-run social infrastructure costs.

7. Discussion

The results of this paper suggest that the destruction of a concentrated industry produces a complex, temporally varying pattern of labor market adjustment for workers in adjacent sectors. In the short run, the reallocation of resources — labor, commercial space, capital — created upgrading opportunities for incumbent workers, particularly in manufacturing and retail. In the long run, the destruction of the social infrastructure embedded in the dissolved industry degraded labor market matching and reversed the initial gains. This temporal pattern, visible only because the linked census panels span three decades, would have been missed by any study examining a single post-treatment period.

7.1 Creative Destruction and Its Limits

The short-run positive effect is consistent with a creative destruction narrative: prohibition eliminated a large, low-productivity sector (saloons employed mostly unskilled workers at low wages), and the freed resources — commercial real estate, labor supply, customer spending — were reallocated to higher-productivity sectors. The mechanism results confirm this: the largest gains accrued to manufacturing workers (+1.19 points), consistent with reduced competition for skilled jobs as displaced alcohol workers entered the unskilled labor pool, and to retail/hospitality workers (+0.92 points), consistent with the conversion of former saloon spaces into other commercial enterprises.

The creative destruction narrative also explains the decline in self-employment (6.1 percentage points per unit of treatment intensity). Saloons had been a major venue for small-scale entrepreneurship: a bartender could become a saloonkeeper with modest capital, and the saloon served as a platform for side businesses (gambling, boarding, moneylending). Prohibition eliminated this entrepreneurial pathway without creating an obvious substitute. The new businesses that occupied former saloon spaces — soda fountains, tea rooms, five-and-dime stores — were more likely to be operated by corporate chains than by individual proprietors (Okrent, 2010).

But the reversal in 1920–1930 suggests that creative destruction is an incomplete framework. Standard models of industry reallocation predict that adjustment costs are concentrated in the short run, with long-run equilibrium reflecting the efficient reallocation of resources (Blanchard and Katz, 1992). The pattern here is the opposite: short-run gains gave way to long-run losses. This reversal is difficult to reconcile with a story in which prohibition merely reshuffled workers across sectors, because reshuffling should produce a one-time level change, not a reversal.

7.2 Social Infrastructure as a Labor Market Institution

One interpretation of the long-run reversal — though not the only one — centers on the destruction of the saloon's non-market functions. When the saloon closed, the hiring networks, credit relationships, and social connections it hosted did not automatically transfer to other institutions. In the short run, existing networks continued to function through other venues — workers who already knew their foremen, who already had bank accounts, who already had established social connections were not immediately harmed. But over time, the networks degraded. New workers entering the labor market had no saloon to introduce them to foremen. Workers who lost their jobs had no saloonkeeper to tide them over with informal credit or connect them to new employment. The cumulative erosion of these functions manifested as reduced occupational mobility in the 1920–1930 period.

This interpretation is supported by the heterogeneity results. Immigrants, who were most reliant on saloon-based networks, showed the largest short-run gains but were presumably also most vulnerable to the long-run erosion of these networks. The reversal is also consistent with Powers (1998)'s qualitative evidence that no adequate institutional substitute emerged for the saloon's labor market functions during the 1920s. Employment agencies existed but charged fees that working-class men could not afford. Newspapers advertised jobs but in English, limiting their utility for non-English-speaking immigrants. Union hiring halls served organized workers but excluded the unorganized majority.

7.3 Implications for Modern Policy

This pattern has implications beyond the historical case of prohibition. Modern episodes of concentrated industry destruction — coal mine closures, manufacturing plant shutdowns, technology-driven obsolescence — may produce similar dynamics. The initial shock creates visible displacement among directly affected workers, but the general equilibrium effects on adjacent workers may be positive in the short run (less competition, freed resources) and negative in the long run (lost networks, degraded institutions). The "China Shock" literature (Autor et al., 2013) documents persistent negative effects of import competition on local labor markets, but focuses primarily on directly affected manufacturing workers. The evidence here suggests that the indirect effects on adjacent-sector workers may follow a different temporal pattern.

The finding is also relevant to place-based policy design. Policymakers contemplating the closure of a concentrated industry — whether through environmental regulation, trade policy, or technological displacement — should consider not only the direct employment effects but also the informal institutional infrastructure embedded in the industry. If that infrastructure serves labor market functions for a broader population, its destruction may produce delayed costs that are invisible in the short-run evaluation horizon typically used for policy assessment.

7.4 Limitations

The paper has several limitations beyond the pre-trend concern. First, OCCSCORE captures occupational standing but not wages within occupations; prohibition may have affected within-occupation earnings in ways that OCCSCORE does not detect. A worker who remained in the same occupation but saw their real wages fall (or rise) would register zero change in OCCSCORE. Second, the linked census panels may suffer from selection into successful linkage: individuals who changed their names, moved far, or died between censuses are less likely to be linked, and this selection may differ between treated and control areas (Abramitzky et al., 2021). Third, the county-level alcohol share measure aggregates over heterogeneous local labor markets within counties. Urban counties with concentrated saloon districts and rural counties with a single tavern receive different alcohol shares, but the within-county variation in exposure is not captured. Fourth, the treatment variable combines state-level timing variation (which is plausibly exogenous to local labor market conditions) with county-level intensity variation (which reflects pre-existing industrial structure and may be correlated with other determinants of occupational mobility).

Despite these limitations, the paper documents a striking empirical pattern: short-run occupational upgrading associated with prohibition exposure followed by long-run degradation, concentrated among the workers most reliant on the destroyed industry's informal networks. While the pre-trend concern prevents a clean causal interpretation of the level of the main coefficient, the decomposition by industry, the heterogeneity across demographic groups, and the dynamic reversal provide descriptive evidence consistent with informal institutions playing a role in labor market matching — a role that standard reallocation models typically omit.

8. Conclusion

When thirty states closed their saloons between 1907 and 1919, they destroyed more than an industry. They dismantled a network of institutions that, for all their association with vice, served as the connective tissue of working-class labor markets. The short-run pattern was paradoxically positive: prohibition exposure is associated with modest occupational upgrading (+0.80 OCCSCORE points per percentage point of treatment intensity, RI $p = 0.098$), with larger associations for manufacturing workers (+1.19 points) and immigrants (+1.61 points). But by 1920–1930, the association reversed: treatment intensity predicted *lower* occupational scores by 1.03 points.

These patterns should be interpreted with caution. A significant earlier-period differential trend complicates causal interpretation of the main coefficient, and the design — based on decadal changes with a collapsed treatment indicator — does not exploit staggered timing as cleanly as modern event-study methods would require. The paper's contribution is therefore primarily descriptive: it documents the decomposition by industry, the heterogeneity across demographic groups, and the dynamic reversal, which together provide suggestive evidence on how industry destruction may reshape adjacent labor markets through channels beyond simple reallocation.

The dynamic pattern documented here — positive short-run associations followed by negative long-run associations — suggests that single-period evaluations may miss important temporal dynamics in labor market adjustment. Whether the reversal reflects the destruction of saloon-based social infrastructure, differential secular trends, or other unobserved factors remains an open question that future work with richer data and sharper designs may resolve.

The workers whose trajectories changed most dramatically were not bartenders and brewers — they were the immigrant laborers and skilled tradesmen who relied on the saloon as the central institution of their working lives.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

A.1 Census Linked Panels

The primary data source is the IPUMS Multigenerational Longitudinal Panel (MLP), version 2.0, which provides pre-linked individual records across consecutive census decades (Ruggles et al., 2023; Abramitzky et al., 2021). The MLP uses a combination of exact and probabilistic matching on name (first, last), age, birthplace (state/country), and race to link individuals across census years. The resulting panels have been validated against hand-linked samples and produce similar estimates of intergenerational mobility (Feigenbaum, 2018; Ward, 2023).

Three linked panels are used:

1. **1910–1920 panel** (main analysis): 11.6 million linked individuals before sample restrictions. After restricting to ages 18–55, non-agricultural workers, excluding alcohol industry workers (IND1950 = 869, OCC1950 = 730), and requiring $|\text{age\_diff} - 10| \leq 2$: **10,050,886 individuals** (8,732,156 males and 1,318,730 females). The primary analysis uses the male sample (N = 8,732,156, as reported in Table 1); female workers are analyzed separately in Section 5.5.

2. **1900–1910 panel** (earlier-period comparison): 7.8 million linked individuals before restrictions. Final sample: approximately 6.8 million individuals.

3. **1920–1930 panel** (long-run effects): 14.3 million linked individuals before restrictions. Final sample: **10,763,110 male workers**.

A.2 County-Level Alcohol Industry Shares

County-level alcohol industry shares are constructed from the 1910 full-count census. For each county with at least 100 employed workers (ages 18–65, in non-agricultural occupations), I compute:

- **Alcohol share** (primary): Workers in eating and drinking places (IND1950 = 869) ÷ total workers. In the 1910 context, this category was dominated by the 300,000+ saloons.

- **Bartender share** (narrow): Workers classified as bartenders (OCC1950 = 730) ÷ total workers.

- **Beverage manufacturing share**: Workers in food/beverage manufacturing (IND1950 = 216) ÷ total workers.

This yields 2,944 counties with valid alcohol industry data. Of these, 1,897 (64%) have positive alcohol shares. The mean alcohol share (conditional on positive) is approximately 1.5%, with a maximum of 11%.

A.3 State Prohibition Dates

State prohibition adoption dates are drawn from Beienburg (2020). I classify states into three groups:

- **Already dry** (3 states: ME, KS, ND): Adopted prohibition in the 19th century (1880, 1884, 1889). Excluded from analysis because these prohibitions predate the saloon economy studied here.
- **Went dry 1907–1919** (30 states): Treatment group. Adopted statewide prohibition between 1907 and 1919. This includes five "early adopters" (GA and OK in 1907, MS and NC in 1908, TN in 1909) that were already dry at the 1910 baseline and 25 states that adopted between 1910 and 1919.
- **Wet until 1920** (15 states: NY, PA, IL, MA, CT, MD, NJ, DE, LA, MO, RI, MN, WI, CA, VT): Control group. Remained legal until the Eighteenth Amendment.

A.4 Variable Definitions

- **ΔOCCSCORE**: $\text{OCCSCORE}_{1920} - \text{OCCSCORE}_{1910}$. Occupational income score assigns the median total income (in 1950 dollars) of all workers in an occupation to each individual in that occupation.
- **Treatment**: $\text{AlcShare}_c \times \text{Treated}_s$. Continuous interaction of county alcohol share (in percentage points) and state prohibition indicator. A coefficient of 0.80 means that a one-percentage-point increase in alcohol share in a treated state is associated with a 0.80-point increase in ΔOCCSCORE.
- **Occupation switch**: Indicator for different OCC1950 codes across census years.
- **Self-employed**: Indicator for class of worker = 1 (self-employed) in CLASSWKR.
- **Mover**: Indicator for different county of residence across census years (from MLP).
- **Immigrant**: Indicator for nativity ≥ 4 (foreign-born) in NATIVITY.
- **Black**: Indicator for RACE = 2.

- **Region**: Northeast, Midwest, South, West (standard Census Bureau definitions using STATEFIP).

B. Identification Appendix

B.1 Earlier-Period Comparison: 1900–1910

Table 6 presents the earlier-period analysis using the linked 1900–1910 panel. The treatment variable is constructed analogously to the main specification, using 1900 county alcohol shares and future (1907–1919) state prohibition status. Because five states adopted prohibition during 1907–1909, these states were partially treated during the 1900–1910 window, so this exercise mixes pre-treatment trends with early treatment effects. Both specifications — without (Column 1) and with (Column 2) region fixed effects — yield large, positive, statistically significant coefficients ($\hat{\beta} = 5.62$ and $\hat{\beta} = 5.34$, respectively).

Table 6: Earlier-Period Comparison: 1900–1910

	delta_occscore	
	(1)	(2)
	(1)	(2)
AlcShare × Treated	5.617***	5.344***
	(1.784)	(1.930)
Alcohol Share (1900)	-6.948***	-6.596***
	(1.666)	(1.831)
Observations	6,157,710	6,157,710
R^2	0.02765	0.02774
region fixed effects		✓

Standard errors clustered at state level in parentheses.
Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator.
Sample: linked male workers 1900-1910, aged 18-55 in 1900.
Treatment defined using 1900 county alcohol shares and future (1907–1919) state prohibition status.
Five states (GA, OK, MS, NC, TN) adopted prohibition during 1907–1909 and were partially treated during the 1900–1910 window.

The magnitude of the pre-trend coefficient (5.34) is approximately 6.7 times the main 1910–1920 estimate (0.80). These coefficients represent the OCCSCORE change per percentage point of alcohol share exposure. Several factors may contribute to the discrepancy between the two periods:

1. **Different exposure measures**: The 1900 alcohol share reflects a much smaller saloon industry. Between 1900 and 1910, the number of saloons approximately doubled nationwide. The 1900 share may capture different local characteristics than the 1910 share.

2. **Compositional change**: The massive immigration wave of 1900–1910 changed the composition of high-alcohol-share areas, potentially introducing workers with steeper occupational trajectories.

3. **Industrialization trends**: Counties with high alcohol shares tended to be urban and industrializing rapidly. The occupational upgrading captured by the pre-trend may reflect industrialization rather than a prohibition-related mechanism.

Regardless of interpretation, the significant pre-trend means that the 1910–1920 treatment coefficient should not be interpreted as the causal effect of prohibition under a parallel trends assumption. The paper's contribution lies in the mechanism decomposition, heterogeneity patterns, and dynamic reversal, which characterize the structure of the relationship.

C. Robustness Appendix

Table 7 consolidates all robustness checks discussed in Section 6. Panel A reports the baseline and the non-South subsample. Panel B reports alternative treatment measures (bartender share, beverage manufacturing share, binary high-exposure indicator). Panel C reports the zero-exposure placebo. Panel D reports wild cluster bootstrap and randomization inference results. Panel E reports the range of leave-one-out estimates.

Table 7: Summary of Robustness Checks

Specification	$\hat{\beta}$	SE	p-value	Notes
Panel A: Baseline and sample restrictions				
Main (region FE, preferred)	0.803	0.277	0.004	Table 2, Col. 3
Non-South only	0.627	0.241	0.009	Excludes Southern states
Panel B: Alternative treatment measures				
Bartender share	0.902	0.439	0.040	OCC1950 = 730
Bev. manufacturing share	0.023	0.006	<0.001	IND1950 = 216
Binary high-exposure	0.29	0.23	0.197	Above-median indicator
Panel C: Placebo and validation				
Zero-exposure placebo	0.05	0.21	0.800	Counties with zero alc. share
Panel D: Inference				
Wild cluster bootstrap	0.803	0.333	0.002	Rademacher weights, 499 reps
Randomization inference	—		0.098	500 permutations
Panel E: Leave-one-out range				
LOO minimum	0.396	—	—	Excluding NY
LOO maximum	1.010	—	—	40 iterations, all positive

Notes: Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator. All specifications use the preferred Column (3) setup from Table 2 (region FE, individual controls, 1910 OCCSCORE) unless otherwise noted. Dependent variable is ΔOCCSCORE. Standard errors clustered at the state level except where noted. The binary specification uses an above-median alcohol share indicator interacted with treatment status. Wild cluster bootstrap uses Rademacher weights with 499 replications. Randomization inference permutes state prohibition assignments 500 times. LOO estimates exclude one state at a time from the 25 states that adopted during 1910–1919 and the 15 control states (40 iterations; the five pre-1910 adopters are held fixed). The minimum is obtained excluding New York.

Figure 5 presents the leave-one-out analysis, showing that the main treatment coefficient remains positive and significant when any single state is excluded. The range of coefficients (0.40 to 1.01) brackets the full-sample estimate of 0.80, indicating that no individual state is responsible for the result.

Figure 6 shows the randomization inference distribution. The true treatment coefficient (vertical line) falls in the right tail of the permutation distribution, yielding a two-sided p-value of 0.098.

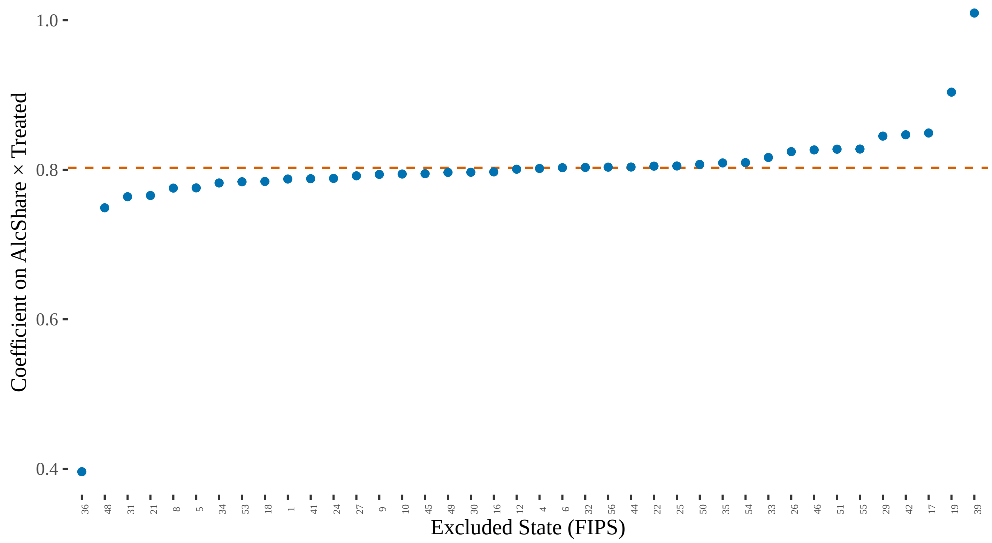

Figure 5: Leave-One-Out Sensitivity: Treatment Coefficient Excluding Each State

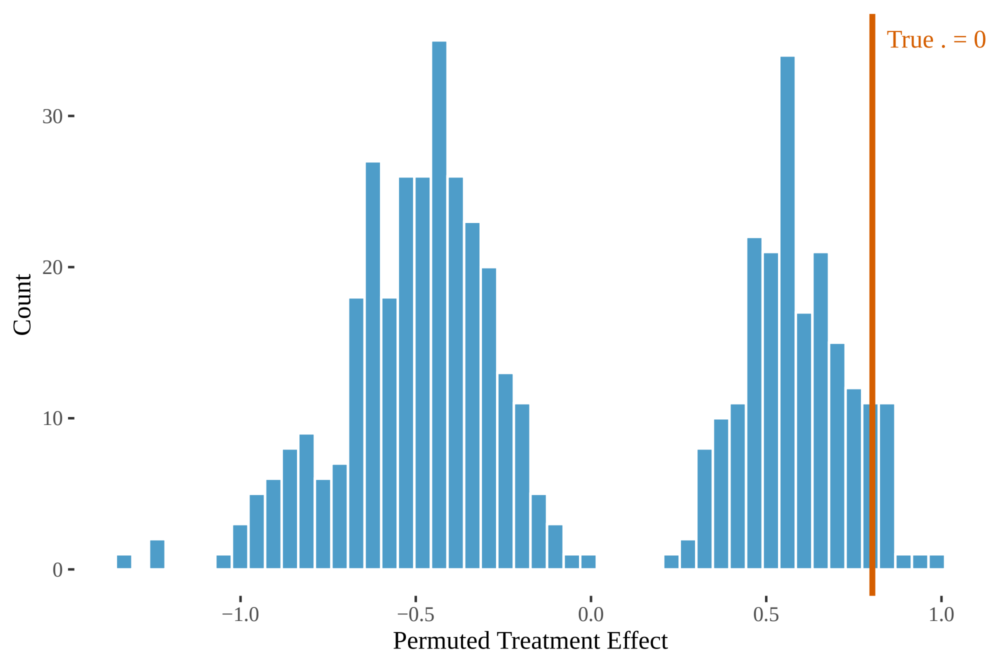

Figure 6: Randomization Inference: Distribution of Permuted Treatment Effects

D. Heterogeneity Appendix

Full heterogeneity results are reported in Table 4 in the main text. All six subgroup specifications use the preferred Column (3) specification with region fixed effects, individual controls, and initial OCCSCORE.

E. Women's Employment Results

Table 8: Women's Employment: Effect of Prohibition Exposure

	delta_occscore Female ΔOCC (1)	occ_switch Female Switch (2)
AlcShare × Treated	0.486*	-0.009
	(0.269)	(0.008)
Clustering	State (1910)	
Observations	1,318,730	1,318,730
R^2	0.24350	0.06875
region fixed effects	✓	✓

Standard errors clustered at state level in parentheses.
Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator.
Sample: female workers in linked 1910–1920 panel.
All specifications include region FE and individual controls.

F. Additional Figures and Tables

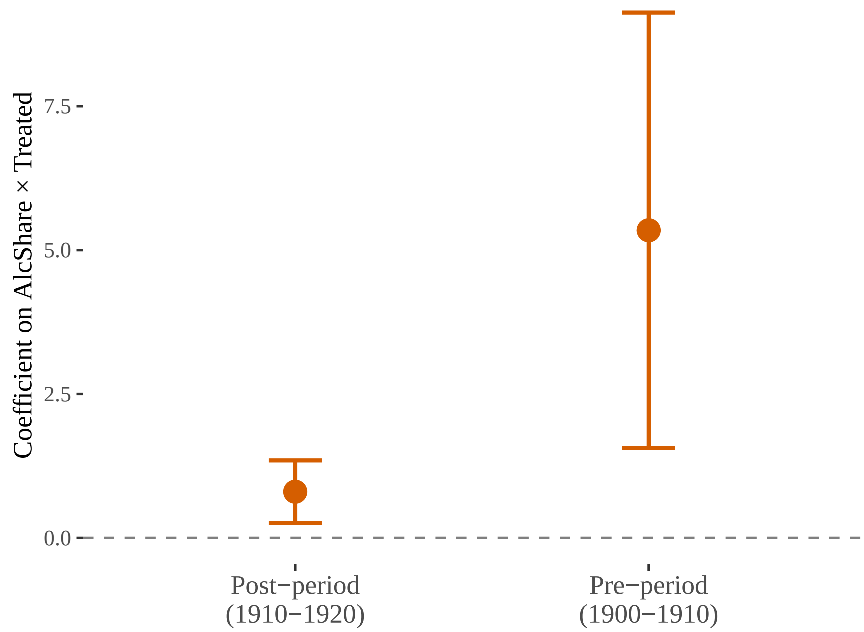

Figure 7: Pre-Trend vs. Post-Treatment Coefficient Comparison

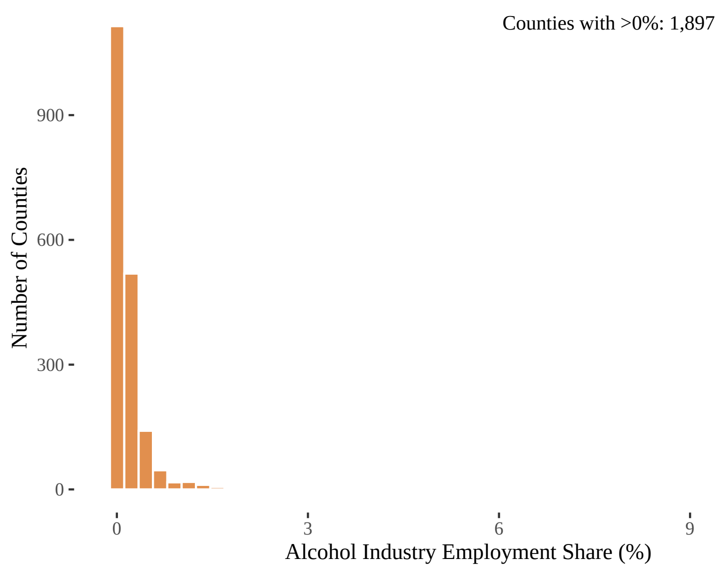

Figure 8: Distribution of County-Level Alcohol Industry Employment Shares

G. Standardized Effect Sizes

Table 9: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
ΔOCCSCORE (1910-1920)	Table 2, Col. 3	0.803	0.253	11.41	0.0178	0.0062	Small positive

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison. Treatment is defined as county alcohol industry share (in percentage points) × state prohibition indicator. SDE $= \hat{\beta} \times \mathrm{SD}(X)/\mathrm{SD}(Y)$, giving the effect of a one-standard-deviation change in treatment, measured in standard deviations of the outcome. Data: linked IPUMS full-count census panels (1910–1920), male workers aged 18–55, excluding alcohol industry workers (N = 8,732,156). Estimation: OLS with region fixed effects and individual controls. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\mathrm{SDE}| < 0.005$), not a failure to reject a null hypothesis.