Does Substance Control Affect Local Public Finance? Historical Evidence from Alcohol Prohibition

Yiyu Xing*

February 16, 2026

Abstract

This paper examines how substance regulation shaped municipal finance, focusing on alcohol prohibition in the early twentieth-century United States. Using a newly digitized dataset and the staggered rollout of state and federal prohibition laws between 1911 and 1925, I implement a difference-in-differences design to estimate local fiscal effects. Prohibition led to a 67.27% decline in local business tax revenue, driven primarily by the loss of liquor license taxes. Municipal responses depended on initial fiscal conditions: financially constrained cities cut government expenditures, while fiscally healthier cities reduced outstanding debt and substituted toward greater reliance on property taxes. These results show how dependence on sin taxes can create fiscal vulnerability and shape adjustment strategies following major regulatory shocks.

Keywords: substance use disorder, sin tax, alcohol prohibition, local public finance
Classification: I13, I18

*Integrated Substance Use and Addiction Programs (ISAP), Semel Institute, University of California, Los Angeles. E-mail: sarrusxing10@g.ucla.edu. I am indebted to my advisor, Nicolas L. Ziebarth, for always providing kind support, insightful feedback, and discussion. I thank discussants and participants at the 2025 Annual Conference of the American Society of Health Economists. All errors are my own.

1 Introduction

The United States is experiencing a significant shift in substance control policies, most notably through the rapid legalization of cannabis across many states. This regulatory transformation has introduced substantial new revenue streams for state and local governments.[1] For example, since January 2018, total cannabis tax revenue in California alone has exceeded $6 billion.[2] Such revenues are often framed as solutions to fiscal constraints, particularly for jurisdictions facing budget pressures.[3][4] Yet this growing reliance on sin taxes raises fundamental questions about local public finance: To what extent does fiscal resilience depend on specific tax instruments, and how do local governments adjust when these revenue streams are disrupted?

While contemporary policy changes provide valuable opportunities to study the fiscal consequences of substance control (see for example, Cheng, De Franco and Lin, 2023), establishing credible causal interpretations remains empirically challenging. A primary concern is endogeneity: jurisdictions that adopt cannabis legalization may differ systematically from those that do not in ways that also shape their fiscal outcomes. Reverse causality further complicates interpretation: localities facing budget shortfalls may be more likely to legalize cannabis precisely to generate new revenue, rather than legalization itself improving fiscal health. In addition, intergovernmental fiscal transfers (i.e., federal grants or state revenue sharing) can buffer or obscure the direct fiscal effects of cannabis taxation, making it difficult to isolate the impact of local policy choices. Finally, these reforms typically unfold within a complex and evolving political environment, where multiple concurrent policy changes may simultaneously influence fiscal trajectories. Disentangling these confounding forces therefore requires careful empirical design and credible identification strategies.

[1] See for example, https://taxfoundation.org/research/all/state/cannabis-tax-revenue-reform/, last accessed February 16, 2026.

[2] See https://www.phi.org/thought-leadership/california-cannabis-tax-revenues-a-windfall-for-law-enforcement-or-an-opportunity-for-healing-communities/, last accessed February 16, 2026.

[3] See for example, https://taxpolicycenter.org/briefing-book/how-do-state-and-local-cannabis-marijuana-taxes-work, last accessed February 16, 2026.

[4] Revenue impacts could arise as a result of new excise taxes, regulatory fees, and changes in labor supply. Outlay impacts could include reduced expenditures on prohibition enforcement and increased expenditures on prescription cannabis and cannabis product regulation, offset by potential cost savings on other healthcare services (Brill, Miller and Veuger, 2024).

In this paper, I address this empirical challenge by examining the largest substance control experiment in U.S. history: alcohol prohibition. The historical setting offers several unique advantages for identification: First, during the 1900s, liquor license fees constituted 8.44% of cities' total revenues, representing a relatively sizable portion of local budgets. Second, the primary determinant of prohibition was religious sentiment, providing a plausibly exogenous source of variation in policy adoption. Third, local governments of this era operated with strong fiscal autonomy and minimal reliance on intergovernmental transfers. Taken together, when state and federal prohibition suddenly eliminated liquor-related revenue streams, cities had to develop alternative funding mechanisms and potentially distort public goods provision.

To implement the analysis, I construct a novel historical dataset on U.S. cities using the *Financial Statistics of Cities* published by the Bureau of the Census, which reports detailed information on municipal revenues, expenditures, and other fiscal outcomes for cities with populations above 30,000 between 1911 and 1925. I find that, following alcohol prohibition, municipal business tax revenue declined significantly and persistently by approximately 67.27 percent. At the same time, the share of business tax revenue in total municipal tax revenue fell by 4.3 percentage points, or 24.85 percent relative to its 1911 level. These results confirm that alcohol prohibition imposed substantial negative revenue shocks on municipal budgets, highlighting the fiscal vulnerability created by the loss of sin-tax revenue streams.

Second, I find that alcohol prohibition gradually led to a significant increase in local non-business tax revenue, with the most pronounced growth occurring in property tax collections. Within two years of prohibition's adoption, property tax revenue rose by approximately 8.22 percent. This shift suggests that, in response to negative revenue shocks, municipalities tried to stabilize their fiscal base by relying on more predictable and enforceable tax instruments. Unlike business taxes, which depended substantially on alcohol-related commercial activity, property taxes provided a more resilient and stable source of revenue that was less sensitive to sector-specific disruptions. This pattern is consistent with a form of tax substitution, whereby local governments offset losses from prohibition by increasing their reliance on immobile, broad-based tax bases. Notably, this substitution is more pronounced among cities that faced fewer fiscal constraints prior to prohibition. The evidence therefore highlights an important

mechanism of fiscal adaptation in response to policy-induced revenue shocks.

I also document an additional margin of fiscal adjustment among municipal governments. Following alcohol prohibition, local debt declined immediately, significantly, and persistently by approximately 12.45 percent. These reductions in total debt were concentrated in cities with relatively stable financial positions, proxied by a below-average leverage ratio in 1911 (defined as the ratio of total debt to total tax revenue). This pattern suggests that fiscally healthier cities were more willing or better able to absorb revenue shocks by reducing debt issuance as a financing strategy. In contrast, cities facing tighter financial constraints have to rely more heavily on alternative fiscal adjustments, potentially due to limited credit access or higher borrowing costs.

Turning to local public goods provision, I find little evidence that alcohol prohibition affected overall municipal expenditures. Total government spending remained largely unchanged in the years following prohibition, and I detect no significant shifts across disaggregated spending categories such as policing, public health, or welfare programs. However, among cities with higher pre-existing financial leverage, total government expenditures began to decline gradually after prohibition. These results suggest that, despite substantial revenue shocks, municipalities with lower debt burdens were still able to maintain baseline levels of public service provision, likely through adjustments on the revenue side or changes in debt management strategies.

This paper can provide key insights regarding the relationship between substance control and fiscal capacity for contemporary policy levers. My historical evidence highlights the potential vulnerability of reliance on sin taxes: if federal or state-level regulatory changes were to suddenly restrict these sin-tax revenue sources, fiscally dependent local governments could face severe distress and disruptions in public services. Thus, the core of fiscal resilience is not merely about securing new revenue sources but also about ensuring diversified and structurally stable local tax systems. These findings have crucial relevance in the contemporary context, helping policymakers better understand the fiscal impacts of regulatory changes and design more robust revenue systems.

This paper makes several novel literature contributions through a lens of history. First, my

work closely relates to a strand of economic history papers studying the consequences of alcohol prohibition. Prior research extensively focuses on its effects on health outcomes (Evans et al., 2016; Jacks et al., 2024), crime (Owens, 2011; Heaton, 2012), industrial outcomes (Hernández, 2016), innovation (Andrews, 2023), and local economies (Howard and Ornaghi, 2021). Though descriptive evidence shows that prohibition had negative effects on tax revenues (e.g., Blanchette, Ross and Naimi, 2020), causal evidence remains scarce. My paper fills this gap by providing rigorous empirical assessment of prohibition's fiscal impacts.

Second, this paper adds value to the literature on sin taxes by exploring their fiscal externalities. A large body of research focuses on the horizontal fiscal externalities that arise due to consumer behaviors (see Allcott, Lockwood and Taubinsky (2019)). However, there is limited empirical evidence on vertical fiscal externalities—how higher-level tax instruments influence local government budgets, as discussed in Keen (1998). The prohibition era provides a natural setting to test this mechanism, as it introduced a sharp regulatory shift that directly altered local revenue structures.

This paper speaks to a broader literature investigating the determinants of local fiscal outcomes, including federal grants and aid programs (see, for example, Knight, 2002; Gordon, 2004; Baicker, 2005), sales tax shocks (Shoag, Tuttle and Veuger, 2019), public employee pensions (Myers, 2023), migration (Tabellini, 2020), economic downturns (Poterba, 1994; Janas, 2025), municipal fiscal restraints (Brooks, Halberstam and Phillips, 2016), and public health crises (Clemens and Veuger, 2020). Most relevant to this study, several economic history papers have examined fiscal centralization and interjurisdictional fiscal dynamics during the Great Depression (see, for example, Wallis, 1984; Fetter, 2017; Gillitzer, 2017; Coen-Pirani and Wooley, 2018). My paper advances this literature by leveraging an earlier historical setting to highlight an underexplored determinant: the role of higher-level government regulation of sin goods.

The rest of this paper proceeds as follows: section 2 provides the institutional background. Section 3 specified the primary empirical strategies. Section 4 describes the primary datasets I use in the empirical analysis; section 5 presents the main results. Section 6 shows how the effects vary with respect to the financial constraint faced by each municipal. Eventually,

section 7 concludes and highlights some future work, as well as policy implications.

2 Historical Background

2.1 Alcohol Taxation Prior to the 1920 Prohibition

Alcohol tax was levied at different levels before the national prohibition. By the early 1900s, as much as 30 to 40 percent of the government's income came from taxes generated by liquor, wine, and beer. By 1910, the federal government was drawing more than $200 million per year from the alcohol industry; only external trade tariffs generated greater revenue. Prior to the 1920 implementation of the Volstead Act, approximately 14% of federal, state, and local tax revenues were derived from alcohol commerce.

The taxation of alcoholic beverages and related commerce in the United States dates back to the late 18th century. In 1791, the federal government introduced its first internal excise taxes, including a levy on whiskey. However, this "whiskey tax" was repealed in 1802. It was not until the fiscal pressures of the War of 1812 that alcohol taxation resumed. As revenues from import duties declined sharply in the postwar years, a special session of Congress in 1813 enacted license taxes on distilleries and on the retail sale of both wines and spirituous liquors. These taxes, however, were short-lived and were again repealed after a few years. A more permanent system of alcohol taxation was established in 1862, when the financial demands of the Civil War prompted the federal government to reintroduce excise taxes. Since then, the United States has maintained federal-level excise taxes on the production and sale of alcoholic beverages.

In addition to federal excise taxes, state and local governments—including county and municipal authorities—also imposed taxes on the trade of alcoholic beverages. These taxes were typically administered as part of license or occupational taxes and were considered a key component of local business taxation. Notably, even during the economically prosperous 1790s, when states enjoyed substantial revenues from holdings in federal bonds, state bank stocks, sales of public land, and confiscated loyalist properties, the liquor tax remained one of the few taxes that was consistently maintained. This persistence highlights its fiscal and administra-

tive importance in early American public finance.

2.2 Temperance Movement and Alcohol Prohibition in U.S.

The first wave of alcohol prohibition in the United States emerged in the mid-19th century, driven by a complex interplay of social, religious, and political forces. The American Temperance Society (ATS), established in 1826, played a pivotal role in launching the early temperance movement and laid the foundation for later reform organizations. By 1835, the ATS had amassed approximately 1.5 million members, with women comprising between 35

In the latter half of the 19th century, the movement broadened its focus beyond personal abstinence to include moral and institutional critiques of alcohol consumption. Religious figures such as Reverend Mark A. Matthews framed saloons as sources of moral decay and political corruption. These early prohibition efforts were largely localized, taking root in rural areas and small towns where evangelical Protestant churches held considerable sway. A milestone came in 1851, when Maine—under the leadership of Neal Dow, later known as the "Father of Prohibition"—became the first state to enact a comprehensive prohibition law.

Over the next two decades, about a dozen states adopted similar laws, though many were poorly enforced or eventually repealed. This first wave was marked by moral suasion campaigns, early legislative experiments, and the formation of organizations like the Women's Christian Temperance Union (WCTU) in 1874. However, the movement lost momentum by the 1870s, primarily due to enforcement challenges and growing political opposition.

The second major wave of prohibition started to take place between 1880 and 1890. During this period, more state legislatures than ever before voted on prohibition and referred the issue to voters via constitutional amendments. Although the movement was contested in three-quarters of all states, only six had enacted prohibition laws by 1890.

Some regions, particularly in the South, sought to strike a balance between prohibition and unrestricted licensing. Drawing inspiration from the Gothenberg Plan—which allowed municipal bodies to control the liquor trade—various towns and counties in North Carolina, Georgia, Alabama, and Virginia began operating local dispensaries. These systems aimed to promote

temperance by removing the profit motive from alcohol sales while simultaneously generating public revenues. The most extensive experiment with this model took place in South Carolina, where in 1893, Governor "Pitchfork" Ben Tillman, elected on a prohibition platform, brokered a compromise: a state board of control would oversee alcohol distribution through a regulated system of state-run dispensaries.

In this paper, I focus on the period from 1900 to 1920, commonly referred to as the Third Wave of Prohibition, a critical phase in the evolution of the temperance movement marked by the rapid spread of local and state-level "dry" laws. Unlike earlier, more fragmented efforts concentrated in rural areas, this wave was distinguished by a coordinated, politically strategic national campaign that ultimately culminated in constitutional prohibition. Central to this transformation was the Anti-Saloon League, founded in 1893, which emerged as the most influential political force in the prohibition movement. Unlike earlier temperance organizations that relied primarily on moral persuasion and grassroots activism, the Anti-Saloon League functioned as a disciplined and highly effective single-issue political pressure group. It endorsed candidates based solely on their stance on prohibition and successfully mobilized voters to reward or punish politicians accordingly.

The League's strategy focused on building momentum from local ordinances to statewide prohibition statutes, creating a patchwork of dry jurisdictions that gradually expanded across the country. By 1919, 33 states had enacted some form of prohibition, encompassing nearly two-thirds of the U.S. population. This accumulation of state-level victories created the political conditions necessary to push for national reform. The federal prohibition effort culminated in the ratification of the 18th Amendment to the U.S. Constitution, which banned the manufacture, sale, and transportation of intoxicating liquors. The amendment was proposed in 1917, ratified in 1919, and formally took effect in 1920.

In Figure 1, I plot how the number of cities newly adopted alcohol prohibition laws evolved over time. And in Figure 2, I show how the ratio between a city's business tax revenue and total tax revenue changed over time: note that starting from 1921, we could observe this striking decline in this metric, suggesting that alcohol prohibition indeed imposed substantial revenue shocks.

3 Empirical Strategy

The identification strategy leverage the adoption of local and federal alcohol prohibition across cities over time, from 1911 to 1925. In the baseline analysis, I exploit the following staggered difference-in-difference (DID) model to identify their causal effects:

$$y_{cst} = \alpha + \beta \text{Prohibition}_{cs} \times Post_t + \rho_c + \eta_t (\times \text{population group}) + \epsilon_{cst} \tag{1}$$

where c denotes cities, s represents states, and t indexes years. y_{cst} is the key outcome of interest, financial outcomes in city c located in state s in year t. $Prohibition_{cs}$ is an indicator for whether there is ever an alcohol prohibition in city c, state s. Post is a dummy variable showing whether this is a year post the prohibition. I also include city fixed effects (ρ_c) and year fixed effects, respectively. In the preferred specification, I further incorporate year–population-group fixed effects ($\eta_t \times \text{population group}_c$) to flexibly control for time-varying factors correlated with city size. The population group is a categorical variable based on each city's population in 1911, with five categories: 500,000 and above, 300,000–500,000, 100,000–300,000, 50,000–100,000, and 30,000–50,000. Therefore, β would identify the impact of imposing alcohol prohibition on local fiscal outcomes.

The key assumption of this difference-in-differences model is that controlling for city FEs and year FEs, changes in key outcomes would have been comparable among different cities in the absence of adopting alcohol prohibition. To directly test this assumption, I estimate a stylized event study model as follows in equation 2:

$$y_{cst} = \alpha + \sum_{i=-6}^{8} \beta_{t+i} Treat_{cs} \times year_{t+i} + \rho_c + \eta_t + \epsilon_{cst} \tag{2}$$

where $year_{t+i}$ is a set of indicator variables denoting whether a given observation corresponds to the i^{th} year relative to the enactment of a prohibition law. In this specification, the coefficient for one year prior to treatment is normalized to zero and serves as the reference period. The coefficients β_{t+i} therefore trace out the dynamic treatment effects of prohibition over time.

Another potential threat to identification is reverse causality. For example, states that introduced local alcohol prohibition prior to 1920 may also have had a stronger demand for higher tax revenues. If changes in prohibition were correlated with broader legal, economic, political, or social trends, it would be difficult to use such variation to isolate the causal effects on government finances.

A priori, however, this concern is mitigated by the institutional context. The prohibition movement was primarily motivated by moral and social reform objectives, aiming to address what reformers perceived as an ill society plagued by alcoholism, domestic violence, and saloon-based political corruption. In this sense, the adoption of prohibition was not directly driven by fiscal considerations.

Some historians argue that national prohibition was partly facilitated by the introduction of the federal income tax in 1913, which reduced the federal government's reliance on alcohol tax revenues.[5] To address this concern, I re-estimate the main specifications excluding the pre-1913 period (as shown in Table A2). The results remain quantitatively and qualitatively similar, suggesting that the findings are not driven by this potential fiscal motive.

Finally, a growing strand of work highlights the sensitivity of traditional staggered difference-in-differences estimators in the presence of staggered adoption and heterogeneous treatment effects (Goodman-Bacon, 2021; Sun and Abraham, 2021). To demonstrate the robustness of the main findings, I re-estimate the models using alternative estimators designed for such settings (e.g., Callaway and Sant'Anna, 2021). The results, reported in the Appendix (Table A1), are consistent with the baseline estimates, indicating that this issue is unlikely to drive the main conclusions.

[5]See, for example, https://www.heinzhistorycenter.org/blog/at-the-history-center-beer-taxes-prohibitions-connection-national-income-tax/ , last accessed February 16, 2026.

4 Data

This section summarizes the data assembled from multiple resources and provides motivational evidence. To implement the empirical analysis, I collect data on (1) the timing of adopting prohibition across cities, and (2) financial outcomes of municipal governments. I then describe how to construct each measure, enabling me to conduct the empirical analysis in turn.

4.1 Adoption of Alcohol Prohibition

Following Andrews (2023), I collect data on the timing and location of alcohol prohibition from Sechrist (1986, 2012). These datasets were compiled through extensive archival work, drawing on hundreds of historical sources. These include annual state-level accounts of local prohibition movements (often accompanied by prohibition maps), national yearbooks published by both pro- and anti-saloon organizations, local newspapers, and a wide range of secondary materials such as regional dissertations documenting prohibition activities.

The data report, for each U.S. county from 1801 to 1920, whether the county was "wet" or "dry," the number of historical sources supporting this classification, and whether the entire state had enacted statewide prohibition. This information allows me to identify the timing of local option adoptions at the county level.

Because the number of cities in my sample is limited, it is not feasible to implement the same identification strategy as Andrews (2023), who exploits within-state, cross-county variation in prohibition status. Instead, I focus on the variation introduced by the adoption of statewide prohibition and, later, federal prohibition. As shown in Figure 1, between 1915 and 1920, approximately 28% of cities in my sample transitioned from wet to dry status due to state laws. In the baseline sample, I exclude states in which either (i) all counties were wet when statewide prohibition was enacted (before the national one), or (ii) all counties had already adopted local prohibition by the time the state law came into force. In both cases, the state law prior to the national one does not generate any additional variation in prohibition status.

4.2 Financial Statistics of Cities

The primary data source for the empirical analysis is the *Financial Statistics of Cities*, compiled by the U.S. Census Bureau beginning in 1902.[6] The Census collected financial data on large cities and published the information in annual reports under the same title. Depending on the year, these reports covered cities with populations above either 30,000 or 100,000.[7] Before 1931, the publication generally covered all cities with populations exceeding 30,000, but reporting was curtailed following federal budget cuts during the Great Depression. Several prior studies have used these data (see, for example, Siodla, 2020; Janas, 2025). However, these papers do not fully exploit the detailed, item-specific information on revenues, expenditures, and municipal debt available during my sample period.

I restrict the analysis to the period from 1911 through 1925 for several reasons. First, prior to 1911, liquor license fees and related taxes were reported under a different revenue category, making it difficult to construct a consistent measure of alcohol-related fiscal exposure across cities. Beginning in 1911, these revenues were systematically classified as part of business tax receipts, allowing for cleaner and more comparable measures of municipal reliance on alcohol-related revenue sources.

Second, I end my analysis in 1925 to avoid confounding effects from the Great Depression, which substantially reshaped local public finances and the provision of public goods (Janas, 2025). The onset of the Depression was associated with dramatic declines in tax bases, emergency relief expenditures, and major institutional reforms, all of which would complicate the interpretation of the fiscal effects of prohibition.

This sample window also captures the key institutional changes of interest, including the wave of statewide prohibition laws in the mid-1910s and the onset of federal prohibition in 1920, while maintaining a consistent fiscal reporting framework. The 1911–1925 period therefore provides both measurement consistency and substantial policy variation for identifying the

[6] The dataset is available at https://fraser.stlouisfed.org/title/financial-statistics-cities-164?browse=1910s, last accessed February 16, 2026.

[7] During the reference period of this study, the reports for 1913, 1914, and 1920 were not published for tax and debt measure. The 1922 report does not have information about detailed government expenditures. The number of cities thus varies due to the availability of data for the key outcomes of interest.

fiscal effects of prohibition.

Figures 3-5 present examples of the raw data, showing the distribution of tax receipts, total payments for expenses, and total debt across cities, respectively. The reports provide detailed breakdowns of major revenue sources, including general property taxes and business taxes. The latter category includes a range of commercial levies, among which liquor license fees constituted a prominent component in many cities prior to prohibition. This level of disaggregation allows me to track how cities adjusted their revenue structures in response to the disappearance of alcohol-related tax bases.

Table 1 shows the summary statistics of the *Financial Statistics of Cities* data. The sample includes 1,199 to 1,272 city–year observations between 1911 and 1925, depending on data availability for each variable. On average, cities collected about $413,606 (in 1925 dollars) in business tax revenue and $8.25 million (in 1925 dollars) in non-business tax revenue. Property taxes constituted the largest source of tax revenue, with a mean of approximately $7.27 million, reflecting the central role of property taxation in municipal finance during this period. Average total municipal debt amounted to roughly $36.7 million, while average total government expenditures were about $7.18 million.

The distribution of fiscal outcomes is highly skewed, as indicated by the large gaps between the 25th and 75th percentiles, reflecting substantial heterogeneity in city size and fiscal capacity. For example, the median business tax revenue is $80,397, compared to a 75th percentile of $218,108. Overall, these summary statistics highlight the considerable cross-city variation in fiscal scale during the early twentieth century.

5 Main Results

In this section, I present my main results for tax revenues, government debt, and government expenditures based on equation 1 and discuss how to interpret the magnitudes and the implications.

5.1 Effects on Business Tax Revenue

In Table 2, I first present the results on how prohibition directly affected municipal business tax revenue. Beginning in 1911, the *Financial Statistics of Cities* consistently reported each city's business tax revenue. Prior to 1920, the reports also provided the corresponding liquor license fees and taxes, which constituted a primary component of business tax revenue. In columns (1)–(4), the dependent variable is the logarithm of total business tax revenue for city c in year t. In columns (5)–(8), the main outcome is the ratio of total business tax revenue to total tax revenue. Because my preferred specification includes the year-by-population-group fixed effects, the estimated coefficient captures how alcohol prohibition affected business tax revenue on a per capita basis, netting out differential trends across cities of different sizes.

In columns (1) (2) (4) and (5), the sample includes all 106 cities. In columns (3) and (7), I restrict the sample to cities that had full autonomy in levying liquor license fees and taxes—that is, cities that did not share liquor tax revenues with state governments. Focusing on these cities allows for a cleaner test of the fiscal consequences of prohibition, as any changes in business tax revenue more directly reflect the loss of locally retained liquor taxes. In columns (4) and (8), I further limit the sample to cities with populations below 100,000. This restriction helps ensure comparability across municipalities and reduces the influence of very large cities, whose fiscal structures and administrative capacities may have differed substantially from the rest of the sample.

During the sample period, a number of states also adopted Home Rule provisions, typically granted through state constitutions or statutes, which allocated greater fiscal and administrative autonomy to local governments subject to certain conditions.[8] Under Home Rule,

[8]These states include Arizona (1912), Colorado (1912), Michigan (1909), Nebraska (1912), Ohio (1912), Oklahoma (1907), Oregon (1906), and Texas (1912).

different levels of government were assigned distinct spheres of authority, limiting the extent to which state governments could interfere in certain local fiscal matters. To account for this potential confounding factor, I include an indicator for Home Rule status as a control variable throughout the analysis.

According to the point estimates reported in Table 2, alcohol prohibition led to a substantial and statistically significant decline in business tax revenue. As shown in column (1), the estimated coefficient implies a 67.27% reduction in business tax revenue (calculated as $\exp(-1.117)-1$), or approximately \$419,461.54 in 1925 dollars relative to the pre-treatment average. Similarly, the share of business tax revenue in total tax revenue fell by 4.3 percentage points, representing a 24.85% decline from its 1911 level of 0.173. This indicates that prohibition not only reduced business tax receipts in absolute terms, but also significantly weakened the role of business taxes within the overall municipal revenue structure.

Notably, these negative effects are particularly pronounced in cities with full autonomy over liquor license taxation and in smaller cities with relatively low population levels (see columns (3) (4) (7) and (8)). This pattern is consistent with the idea that municipalities that retained a larger share of liquor-related revenues, or relied on narrower and less diversified tax bases, were more exposed to the fiscal consequences of prohibition. Taken together, these findings suggest that alcohol prohibition is indeed a substantial negative revenue shock, especially for cities that were more fiscally dependent on alcohol-related business activity.

To assess the validity of the parallel trends assumption and to explore the dynamic effects of alcohol prohibition, I implement an event-study specification based on Equation 2. The results are presented in Figure 6 and Figure 7. Figure 6 reveals a sharp and statistically significant decline in local business tax revenue beginning just one year after the adoption of prohibition. This negative impact persisted for at least seven years. Similarly, Figure 7 shows that the ratio of local business tax revenue to total business tax revenue declined substantially and remained depressed for approximately four years. The observed "recovery" after four years suggests that municipal governments gradually adjusted by adopting alternative tax or financing instruments in response to the prohibition-induced revenue shock.

5.2 Effects on Non-Business Revenue

I next examine how alcohol prohibition affected other, non-business sources of municipal tax revenue. Table 3 reports the estimated treatment effects on total non-business tax revenue and, more specifically, on property tax revenue.

In the first four columns, the dependent variable is the logarithm of total non-business tax revenue for city c in year t. As shown in column (1), the estimated effect is positive but not statistically significant. The relatively large standard error indicates that the estimate is imprecise, and we cannot reject the null hypothesis of no effect. A similar pattern emerges when the sample is restricted to cities with full autonomy in collecting liquor taxes and to cities with smaller populations: none are statistically significant (columns (2)–(4)).

The results for property tax revenue follow the same pattern. Although there is a modest increase in local property tax revenue after prohibition, the estimated effects are not statistically significant on average. These findings suggest that municipal governments, on average, did not fully offset the loss of liquor-related business tax revenue through increases in other major tax sources.

As noted in Helm and Stuhler (2024), spending and investment decisions may adjust gradually in response to revenue shocks in municipal budgets. Consequently, the insignificant average effects discussed above may mask important dynamic responses by local governments. To better understand these temporal adjustments, I re-estimate the event-study specification and present the results in Figures 8 and 9.

Figure 8 shows an interesting pattern for total non-business tax revenue: beginning in the fourth year following alcohol prohibition, the point estimates become statistically significant, indicating an increase of 12.49% that persists for approximately three years. Similarly, Figure 9 reveals a comparable trend—starting in the second year after prohibition, local property tax revenue rises by 8.22% significantly.

5.3 Effects on Total Debt

Another important margin of fiscal adjustment is municipal debt. This channel matters because a local government's outstanding debt and its borrowing capacity are key determinants of its ability to respond to fiscal shocks (Adelino, Cunha and Ferreira, 2017). To finance long-term investments and public works (e.g., infrastructure, utilities, and other capital-intensive projects), municipal governments typically rely on a combination of tax revenues and debt issuance. Both sources, however, depend heavily on the stability and credibility of the underlying tax base (Adelino, Cunha and Ferreira, 2017; Jerch, Kahn and Lin, 2023).

A sudden decline in a major revenue source, such as liquor-related business taxes, may therefore affect not only current fiscal balances but also local borrowing behavior. Cities facing revenue shortfalls may increase debt to smooth expenditures, while fiscally healthier municipalities may instead reduce borrowing or pay down existing obligations. Examining changes in municipal debt thus provides an additional margin through which to assess how local governments adjusted to the fiscal shock induced by alcohol prohibition.

The primary outcome of interest is the logarithm of total government debt outstanding in city c in year t, compiled from the *Financial Statistics of Cities*. The main results are summarized in Table 4. Column (1) reports the estimates for all 106 cities in the sample. Columns (2) and (3) restrict the sample to cities with full autonomy over liquor license taxation and to those with populations below 100,000, respectively. All specifications control for the adoption of Home Rule status at the city level. Across all columns, I find a statistically significant decline in municipal debt following alcohol prohibition. The estimated effect corresponds to a reduction of approximately 13.3 log points, or 12.45%, relative to pre-treatment levels. This decline is more pronounced in cities with greater autonomy over alcohol-related taxation (column (2)).

These findings suggest that municipal governments responded to the fiscal disruptions associated with prohibition by adopting more conservative financing strategies, relying less on debt to fund long-term capital projects. Rather than increasing borrowing to offset lost revenues, some cities appear to have reduced their debt burdens. One possible mechanism is an increase in the cost of borrowing or a decline in investor confidence following the prohibition shock,

which may have constrained local governments' access to credit.

5.4 Effects on Government Expenditures

I next examine how the loss of tax revenue due to alcohol prohibition affected different categories of municipal government expenditures. Since the mid-nineteenth century, local governments (cities, counties, and school districts) had been the primary provision of public goods and service in the United States, well before the expansion of federal fiscal activity during the New Deal. These local governments financed a wide range of public investments, including schools, roads, water and sewer systems, and other public utilities. They also funded core public services such as police and fire protection, public hospitals and charity organizations for the poor, jails, and libraries. As a result, a persistent negative shock to local tax revenue had the potential to constrain the provision of essential public goods and alter the trajectory of local public investment.

The main results are presented in Tables 4, which reports the estimated effects on total government expenditures and spending on police service and health and sanitation service. Across all specifications, I do not find substantial or statistically significant reductions in spending following alcohol prohibition on average. Despite the large and persistent decline in business tax revenue documented earlier, municipal governments appear to have avoided immediate and broad-based cuts to major spending categories.

One possible explanation is that local governments adjusted along other fiscal margins, such as taxation and borrowing, before altering their expenditure levels. To explore the timing of these adjustments, I examine the dynamic responses of government spending in Figures 11, 12, and 13. The event-study estimates reveal that total government expenditures began to rise significantly in the fourth year after prohibition, with an estimated increase of 11.67%.

This delayed increase in spending is consistent with the earlier evidence on municipal revenue adjustments. As shown in Figures 8 and 9, local governments gradually compensated for the loss of business tax revenue by increasing their reliance on property taxes and other non-business tax instruments. The gradual expansion of these alternative revenue sources likely helped stabilize municipal budgets and, over time, supported higher levels of public

spending. Taken together, these results suggest that although alcohol prohibition generated a large negative shock to a key revenue source, municipal governments were able to smooth its impact on public expenditures by adjusting other fiscal instruments.

6 Heterogeneity by Financial Constraint

As noted by Janas (2025), financial constraints play a central role in shaping both the provision of local public goods and the financing strategies of cities.[9] Building on this idea, I examine heterogeneity in the treatment effects based on cities' pre-existing financial conditions.

To proxy for financial constraints, I construct a measure of *financial leverage*, defined as the ratio of total debt to total tax revenue within each municipal government. In the municipal context, this measure reflects the overall fiscal burden of debt relative to a city's current revenue capacity. I use *total tax revenue* as the baseline denominator (instead of using property tax revenue as Janas (2025)) because, as prohibition diffused across cities, I do not observe a significant change in aggregate local tax revenue, which helps alleviate concerns about endogenous movements in the denominator.[10] In addition, as argued by Dray, Landais and Stantcheva (2023), the structure of the property tax system changed substantially over the twentieth century, which may introduce additional measurement concerns when using the property tax base as the reference base.

For this heterogeneity analysis, I classify a city as financially constrained if its 1911 financial leverage was above the sample median (the "high-leverage" group). Otherwise, I categorize it as a low-leverage city with a relatively healthier fiscal position prior to the negative revenue shock. Figures 14–21 show the dynamic fiscal responses of municipal governments with differing levels of financial leverage in 1911.

Figures 14–15 show that the magnitude of the negative revenue shock from alcohol prohibition was similar across both high- and low-leverage cities. This pattern suggests that the loss of

[9] Janas (2025) show that when urban growth stalled during the Great Depression, cities with higher pre-crisis debt burdens faced intensified austerity pressures. Skilled workers were more likely to exit public service, and these financially constrained cities implemented rapid cutbacks in both service expenditures and capital investment.

[10] Instead, as I show later in Table 6, cities can adjust one's non-business tax (primarily property tax) significantly due to the prohibition.

alcohol-related tax revenue constituted a sizable and broadly uniform shock across municipalities, regardless of their initial fiscal conditions. More interesting patterns emerge across other adjustment margins. Figures 16–17 examine changes in non-business tax revenue. In fiscally healthier cities, I find evidence of greater reliance on alternative tax instruments: non-business tax revenue increased significantly, which is primarily driven by property tax revenues.

In Figures 18-19, I find the level of total debt also declined significantly within cities having a less constrained budget, immediately following the alcohol prohibition. Interestingly, financially constrained cities turned to cut one's total government expenditures (20), suggesting a decline in public goods provision. Taken together, these heterogeneous effects indicate that while the revenue shock from prohibition was broadly similar across cities, the fiscal adjustments that followed were shaped by initial financial conditions.

This heterogeneity in responses might reflect differences in borrowing capacity and intertemporal budget constraints. Municipal governments with higher pre-existing debt burdens faced tighter borrowing limits and greater default risk, restricting their ability to use debt to smooth the shock. As a result, high-leverage cities have to directly cut back spending on public goods. Cities with relatively low financial leverage, by contrast, had greater fiscal space and less immediate pressure to replace lost revenues. As a result, these municipal governments can expand their tax base and further adjust by cutting or postponing capital projects, issuing fewer new bonds, and reducing outstanding debt—consistent with a more precautionary and conservative response to revenue uncertainty.

7 Concluding Remarks

Governments have long relied on sin taxes as an important source of public revenue. However, these taxes are inherently unstable because they depend on regulated activities whose tax bases can shrink rapidly following policy changes. This raises a central question in public finance: how resilient are local governments when sin-tax revenues disappear, and how do they adjust their fiscal strategies in response to such shocks?

In this paper, I examine this question by studying the fiscal consequences of alcohol prohibition

in the early twentieth-century United States. Using a newly digitized panel of municipal finances and the staggered adoption of state and federal prohibition laws between 1911 and 1925, I show that prohibition caused a large and persistent decline in local business tax revenue, driven primarily by the loss of liquor-license taxes. Despite this sizable shock, I find little evidence of broad-based reductions in municipal expenditures.

Instead, cities adjusted along different fiscal margins, and such responses depended crucially on their initial financial conditions. Municipalities with higher pre-existing debt burdens were more likely to cut government expenditures. In contrast, cities with stronger financial capacity substitute toward alternative tax instruments to replace lost liquor-related revenues and reduce their outstanding debt, likely by cutting or postponing capital projects and slowing new bond issuance. These patterns indicate that the fiscal consequences of prohibition were shaped not only by the magnitude of the revenue loss, but also by differences in borrowing capacity and fiscal health across cities.

The results speak to contemporary debates over the use of sin taxes to finance public spending. Modern governments increasingly rely on revenues from tobacco, alcohol, gambling, and cannabis, often to support specific programs or to close budget gaps. While these taxes can provide valuable fiscal resources, they may also expose governments to substantial revenue volatility when policies or consumption patterns change. The historical experience of alcohol prohibition suggests that such shocks can lead to significant fiscal adjustments, particularly in jurisdictions with limited financial capacity.

By documenting how municipal governments responded to one of the largest regulatory shocks to sin-tax revenue in U.S. history, this paper contributes to a broader understanding of fiscal resilience, tax substitution, and the interaction between public health regulation and local public finance. Future research may further investigate two important margins. First, it would be valuable to examine in greater detail how prohibition-induced fiscal shocks shaped the design, composition, and long-run evolution of local tax systems across jurisdictions. Such an analysis could clarify whether temporary revenue disruptions generated persistent institutional changes in municipal tax structures. Second, the availability of more detailed data on municipal borrowing (e.g., bond yields, interest rates, or credit ratings) would help

better characterize the financial constraints faced by cities during this period. These data would also allow for a more direct examination of the mechanisms through which prohibition affected local debt issuance and balance-sheet adjustments.

References

Adelino, Manuel, Igor Cunha, and Miguel A Ferreira. 2017. "The economic effects of public financing: Evidence from municipal bond ratings recalibration." *The Review of Financial Studies*, 30(9): 3223–3268.

Allcott, Hunt, Benjamin B Lockwood, and Dmitry Taubinsky. 2019. "Regressive sin taxes, with an application to the optimal soda tax." *The Quarterly Journal of Economics*, 134(3): 1557–1626.

Andrews, Michael. 2023. "Bar Talk: Informal Social Networks, Alcohol Prohibition, and Invention." *Alcohol Prohibition, and Invention (June 25, 2023)*.

Baicker, Katherine. 2005. "Extensive or intensive generosity? The price and income effects of federal grants." *Review of Economics and Statistics*, 87(2): 371–384.

Blanchette, Jason G, Craig S Ross, and Timothy S Naimi. 2020. "The rise and fall of alcohol excise taxes in US states, 1933–2018." *Journal of studies on alcohol and drugs*, 81(3): 331–338.

Brill, Alex, Brian J Miller, and Stan Veuger. 2024. "Implications of cannabis legalization for the US federal budget." *Public Budgeting & Finance*, 44(3): 22–43.

Brooks, Leah, Yosh Halberstam, and Justin Phillips. 2016. "Spending within limits: Evidence from municipal fiscal restraints." *National Tax Journal*, 69(2): 315–351.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230. Themed Issue: Treatment Effect 1.

Cheng, Stephanie F., Gus De Franco, and Pengkai Lin. 2023. "Marijuana liberalization and public finance: A capital market perspective on the passage of medical use laws." *Journal of Accounting and Economics*, 75(1): 101516.

Clemens, Jeffrey, and Stan Veuger. 2020. "Implications of the COVID-19 pandemic for state government tax revenues." *National Tax Journal*, 73(3): 619–644.

Coen-Pirani, Daniele, and Michael Wooley. 2018. "Fiscal centralization: Theory and evidence from the great depression." *American Economic Journal: Economic Policy*, 10(2): 39–61.

Dray, Sacha, Camille Landais, and Stefanie Stantcheva. 2023. "Wealth and property taxation in the United States." National Bureau of Economic Research.

Evans, Mary F, Eric Helland, Jonathan Klick, and Ashwin Patel. 2016. "The developmental effect of state alcohol prohibitions at the turn of the twentieth century." *Economic Inquiry*, 54(2): 762–777.

Fetter, Daniel. 2017. "Local government and old-age support in the New Deal." *Explorations in Economic History*, 66: 1–20.

Gillitzer, Christian. 2017. "Do output contractions cause investment in fiscal capacity?" *American Economic Journal: Economic Policy*, 9(2): 189–227.

Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*, 225(2): 254–277. Themed Issue: Treatment Effect 1.

Gordon, Nora. 2004. "Do federal grants boost school spending? Evidence from Title I." *Journal of Public Economics*, 88(9-10): 1771–1792.

Heaton, Paul. 2012. "Sunday liquor laws and crime." *Journal of public economics*, 96(1-2): 42–52.

Helm, Ines, and Jan Stuhler. 2024. "The Dynamic Response of Municipal Budgets to Revenue Shocks." *American Economic Journal: Applied Economics*, 16(4): 484–527.

Hernández, Carlos Eduardo. 2016. "Adaptation and survival in the brewing industry during prohibition." *Unpublished Manuscript*.

Howard, Greg, and Arianna Ornaghi. 2021. "Closing time: The local equilibrium effects of prohibition." *The Journal of Economic History*, 81(3): 792–830.

Jacks, David S, Krishna Pendakur, Hitoshi Shigeoka, and Anthony Wray. 2024. "Later-life mortality and the repeal of federal prohibition." *Journal of Public Economics*, 238: 105192.

Janas, Pawel. 2025. "Public Goods Under Financial Distress." Working paper.

Jerch, Rhiannon, Matthew E. Kahn, and Gary C. Lin. 2023. "Local public finance dynamics and hurricane shocks." *Journal of Urban Economics*, 134: 103516.

Keen, Michael. 1998. "Vertical tax externalities in the theory of fiscal federalism." *Staff Papers*, 45(3): 454–485.

Knight, Brian. 2002. "Endogenous federal grants and crowd-out of state government spending: Theory and evidence from the federal highway aid program." *American Economic Review*, 92(1): 71–92.

Myers, Sean. 2023. *Public employee pensions and municipal insolvency*. SSRN.

Owens, Emily Greene. 2011. "Are underground markets really more violent? Evidence from early 20th century America." *American Law and Economics Review*, 13(1): 1–44.

Poterba, James M. 1994. "State responses to fiscal crises: The effects of budgetary institutions and politics." *Journal of political Economy*, 102(4): 799–821.

Sechrist, Robert P. 2012. "Prohibition movement in the United States, 1801-1920."

Sechrist, Robert Paul. 1986. *A comparative analysis of multidimensional diffusion models: The diffusion of the prohibition movement in the United States of America, 1876-1919.* Louisiana State University and Agricultural & Mechanical College.

Shoag, Daniel, Cody Tuttle, and Stan Veuger. 2019. "Rules versus home rule local government responses to negative revenue shocks." *National Tax Journal*, 72(3): 543–574.

Siodla, James. 2020. "Debt and taxes: Fiscal strain and US city budgets during the Great Depression." *Explorations in Economic History*, 76: 101328.

Sun, Liyang, and Sarah Abraham. 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, 225(2): 175–199. Themed Issue: Treatment Effect 1.

Tabellini, Marco. 2020. "Racial heterogeneity and local government finances: Evidence from the great migration."

Wallis, John Joseph. 1984. "The birth of the old federalism: financing the New Deal, 1932–1940." *The Journal of Economic History*, 44(1): 139–159.

Figures and Tables

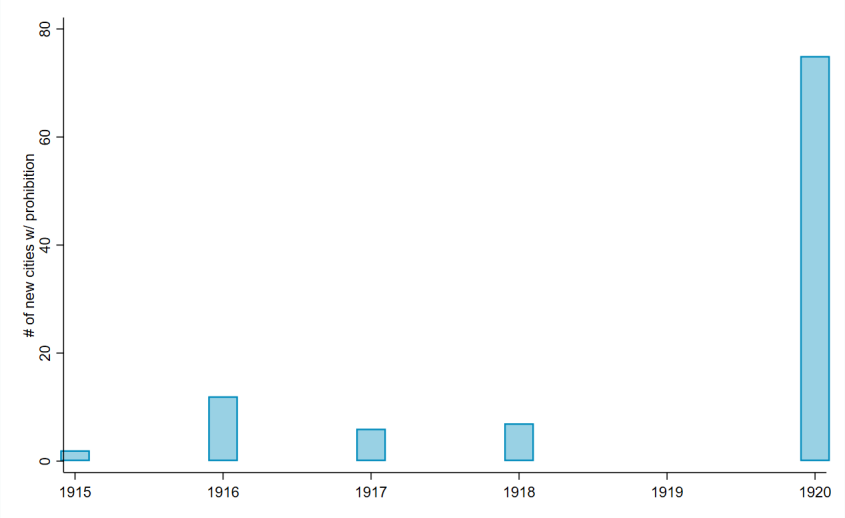


Figure 1: Number of New Prohibition Adoptions Across Cities

Notes: This figure shows the number of new prohibition adopted across cities from 1915 to 1920. Data are collected from Sechrist (1986, 2012). These numbers are calculated by the author.

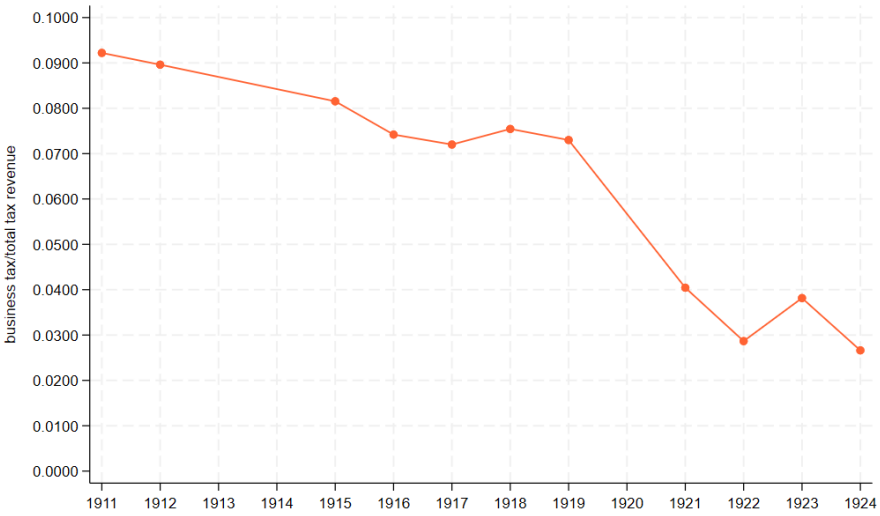


Figure 2: Ratio between Business Tax Revenue and Total Tax Revenue

Notes: This figure shows the average ratio between total business tax revenue and total tax revenue among all 106 cities in my sample from 1911 to 1924. Data are collected from the Financial Statistics of Cities. These numbers are calculated by the author.

TABLE 7.—REVENUE RECEIPTS FROM TAXES, SPECIAL

[For a list of the cities arranged alphabetically by states, with the number

City number.	CITY.	RECEIPTS FROM TAXES.									
		Total.	The general property tax.			Special property taxes.	Poll taxes.	Business taxes.			
			Total.	Original levies.	Penalties and interest.			Total.	On liquor traffic.	Other than on liquor traffic.	
										Collected without issue of license.	Collected with issue of license.
	Grand total	$742,320,878	$666,402,637	$660,314,185	$6,088,452	$14,739,512	$1,998,033	$53,454,528	$36,974,797	$6,534,834	$9,944,897
	Group I	392,351,260	353,211,330	349,232,943	3,978,387	8,396,044	215,706	26,910,893	20,337,029	3,285,091	3,288,773
	Group II	102,769,427	91,217,109	90,702,623	514,486	1,159,418	116,453	9,493,171	5,888,567	2,296,440	1,308,164
	Group III	123,703,933	111,352,227	110,588,019	764,208	3,124,892	786,755	7,823,300	5,331,332	302,584	2,189,384
	Group IV	67,095,028	59,993,593	59,525,626	467,967	952,189	446,566	5,298,214	3,224,765	203,930	1,869,519
	Group V	56,401,230	50,628,378	50,264,974	363,404	1,106,969	432,553	3,928,950	2,193,104	446,789	1,289,057
	GROUP I.—CITIES HAVING A POPULATION OF 500,000 AND OVER IN 1917.										
1	New York, N. Y.	$176,638,703	$164,248,462	$161,470,704	$2,777,758	$4,586,190	.....	$6,543,977	$5,196,588	$359,179	$988,210
2	Chicago, Ill.	58,983,661	49,162,919	48,996,720	166,199	.....	.....	8,480,895	7,067,172	222,580	1,191,043
3	Philadelphia, Pa.	30,209,375	27,856,283	27,693,371	162,912	.....	$75,038	2,152,182	1,916,645	.....	235,537
4	St. Louis, Mo.	18,688,080	13,989,340	13,827,243	162,097	723,456	.....	3,757,874	1,088,616	2,470,042	199,216
5	Boston, Mass.	30,048,628	25,985,727	25,757,836	227,891	2,666,540	140,668	1,135,293	1,043,339	.....	91,954
6	Cleveland, Ohio	14,867,804	13,796,546	13,796,546	.....	24,248	.....	980,051	943,265	.....	45,786
7	Detroit, Mich.	15,845,110	15,004,429	14,933,840	70,589	96,188	.....	720,179	654,836	.....	65,343
8	Baltimore, Md.	12,309,212	10,787,777	10,634,941	152,836	299,422	.....	1,108,729	1,019,151	.....	89,578
9	Pittsburgh, Pa.	18,223,396	17,373,532	17,153,389	220,143	.....	.....	798,173	713,242	.....	84,931
10	Los Angeles, Cal.	16,537,291	15,006,315	14,968,353	37,962	.....	.....	1,224,540	694,175	233,190	297,175

Figure 3: Example of the *Financial Statistics of Cities* showing government revenue receipts from taxes (1917)

TABLE 12.—GOVERNMENTAL COST PAYMENTS FOR EXPENSES OF GENERAL DEPARTMENTS, BY

[For a list of the cities arranged alphabetically by states, with the number

City number.	CITY.	Total.	I.—GENERAL GOVERNMENT.									
			Total.	Legislative branch.			Executive branch.					
							Chief executive.		Financial.			
				Council and board of aldermen.	Clerk of council.	Legislative investigations.	Mayor.	Executive boards and commissions.	Auditor or comptroller.	Special accounting and auditing.	Treasurer or chamberlain.	Assessment and levy of revenue.
	Grand total	$508,543,048	$59,717,194	$2,097,828	$876,971	$142,267	$1,142,573	$1,013,451	$2,910,893	$249,895	$2,115,653	$4,515,385
	Group I	267,873,581	35,942,623	932,531	250,887	59,434	500,385	134,391	1,588,625	95,124	791,232	2,424,724
	Group II	68,822,094	8,781,930	328,962	176,723	60,630	103,231	172,021	354,271	44,927	279,349	759,835
	Group III	86,064,056	7,312,118	398,157	176,396	19,319	212,030	222,041	490,337	51,001	545,707	639,676
	Group IV	49,062,918	4,290,706	233,086	150,109	1,953	182,573	229,444	280,688	34,527	261,284	398,552
	Group V	36,920,399	3,389,817	205,092	122,856	931	144,354	255,554	196,972	24,316	238,081	292,598
	GROUP I.—CITIES HAVING A POPULATION OF 500,000 AND OVER IN 1912.											
1	New York, N. Y.	$124,492,413	$16,920,019	$224,754	$63,566	$18,256	$263,349	.....	$925,321	.....	$174,230	$889,130
2	Chicago, Ill.	42,319,339	6,455,558	333,002	67,031	23,613	27,968	$82,736	261,835	$42,079	238,344	647,197
3	Philadelphia, Pa.	28,857,858	3,990,716	75,332	22,755	.....	64,343	.....	91,789	252	65,412	340,495
4	St. Louis, Mo.	12,744,453	1,455,098	45,787	14,367	6,123	14,831	.....	66,663	3,270	17,665	76,891
5	Boston, Mass.	19,952,503	2,147,585	30,701	1,800	5,827	45,485	.....	45,990	.....	55,335	229,827
6	Cleveland, Ohio	9,610,153	1,331,236	42,143	25,159	.....	18,661	16,809	56,410	14,017	35,564	133,607
7	Baltimore, Md.	8,621,861	1,000,751	66,415	.....	.....	16,791	.....	18,153	8,500	15,508	78,526
8	Pittsburgh, Pa.	12,401,997	1,726,443	58,499	39,036	1,218	38,534	30,976	73,934	22,085	105,705	171,338
9	Detroit, Mich.	8,873,004	915,217	55,898	17,173	4,397	10,423	3,870	48,530	4,921	83,469	57,713

Figure 4: Example of the *Financial Statistics of Cities* showing government cost payments for expenses (1912)

TABLE 28.—TOTAL AND PER CAPITA OF ALL DEBTS AND OF THE PRINCIPAL CLASSES THEREOF AT CLOSE SINKING FUND

[For a list of the cities arranged alphabetically by states, with the number

City number.	CITY.	GROSS DEBT AT CLOSE OF YEAR.									
		Total.	Classified by the governmental unit by which incurred.			Classified by character of outstanding debt obligations.					
			City corporation.	School district.	Other governmental units of city.	Funded or fixed.	Floating.	Current.			
								Special assessment bonds and certificates.	Revenue bonds and notes.	Warrants.	Obligations on trust account.
	Grand total	$3,305,260,020	$3,101,301,990	$102,620,038	$101,337,992	$2,899,688,626	$84,252,504	$164,643,588	$110,522,259	$26,569,560	$19,583,483
	Group I	2,006,923,977	1,919,563,742	21,404,760	65,955,475	1,789,574,707	74,479,716	58,534,364	63,999,970	12,061,265	8,273,955
	Group II	405,651,945	361,632,444	15,039,115	28,980,386	362,011,397	4,503,325	17,679,768	10,374,553	5,873,949	5,208,953
	Group III	479,191,144	450,148,417	28,082,321	960,406	395,788,533	3,259,007	56,924,293	14,172,923	5,101,752	3,944,336
	Group IV	239,743,836	213,218,959	21,083,152	5,441,725	204,351,035	1,904,517	17,935,182	12,170,855	2,017,897	1,334,350
	Group V	173,749,118	156,738,428	17,010,690		147,962,654	105,939	13,569,981	9,803,958	1,484,697	821,889
	GROUP I.—CITIES HAVING A POPULATION OF 500,000 AND OVER IN 1915.										
1	New York, N. Y.	$1,378,625,297	$1,378,625,297			$1,216,332,860	$70,534,402	$32,076,384	$48,536,947	$4,890,238	$6,254,466
2	Chicago, Ill.	101,701,887	62,523,763	$1,261,336	$37,916,788	65,087,645	3,945,314	15,436,465	12,636,057	4,068,606	527,710
3	Philadelphia, Pa.	126,110,912	120,383,116	5,713,996	13,800	123,426,475			31,966	2,570,499	81,972
4	St. Louis, Mo	25,481,490	25,481,490			24,867,000				387,928	226,562
5	Boston, Mass.	125,183,404	125,183,404			124,813,514					369,890
6	Cleveland, Ohio	66,005,638	49,540,064	5,716,000	10,749,574	61,136,968		3,460,333	765,000		643,337
7	Baltimore, Md.	89,509,102	89,509,102			87,467,680			2,000,000		41,422
8	Pittsburgh, Pa.	68,103,646	44,155,891	8,713,428	15,234,327	63,622,156		4,337,308		143,904	278
9	Detroit, Mich.	26,202,601	24,161,615		2,040,986	22,820,409		3,223,874	30,000		128,318

Figure 5: Example of the *Financial Statistics of Cities* showing government debt (1915)

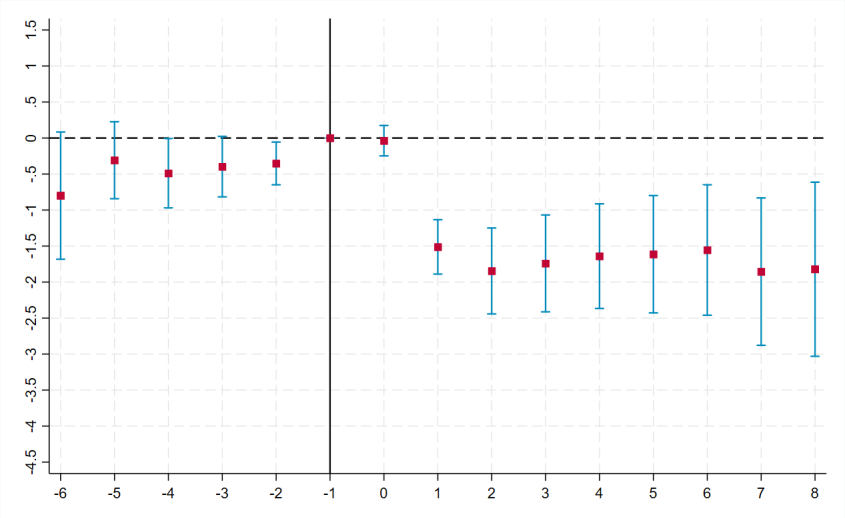


Figure 6: event-study estimates for total business tax revenue

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variable is the logarithm of total business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

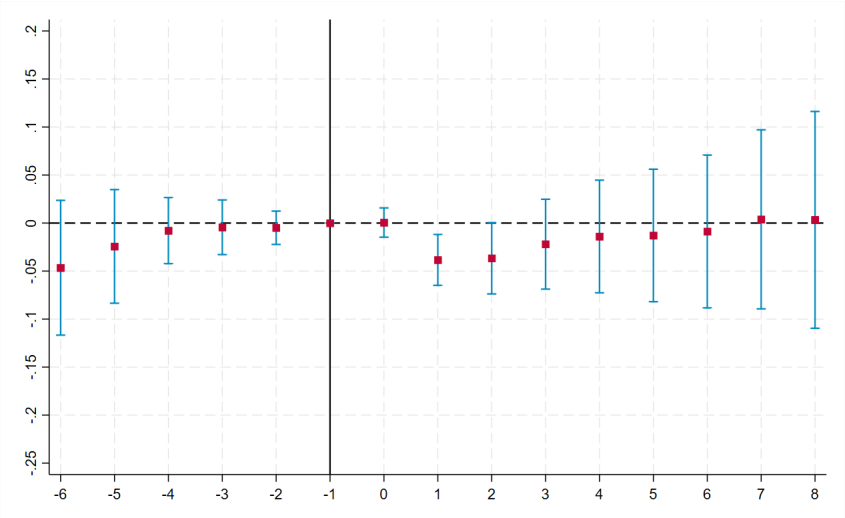


Figure 7: event-study estimates for the ratio between business tax revenue and total tax revenue

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variable is the ratio between local business tax revenue and total tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

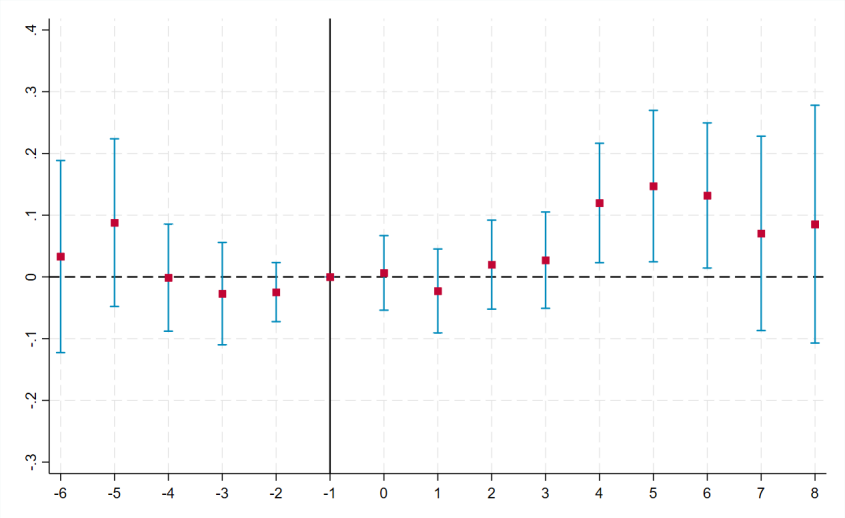


Figure 8: event-study estimates for total non-business tax revenue

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variable is the logarithm of total non-business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

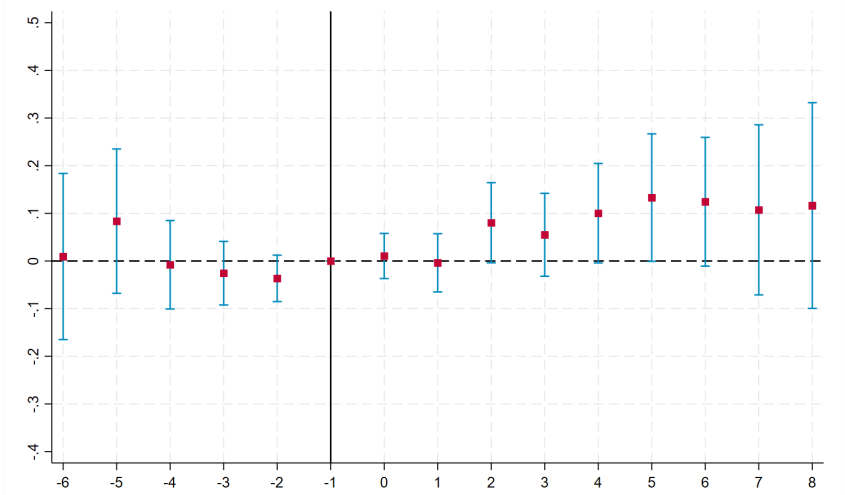


Figure 9: event-study estimates for property tax revenue

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variable is the logarithm of total property tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

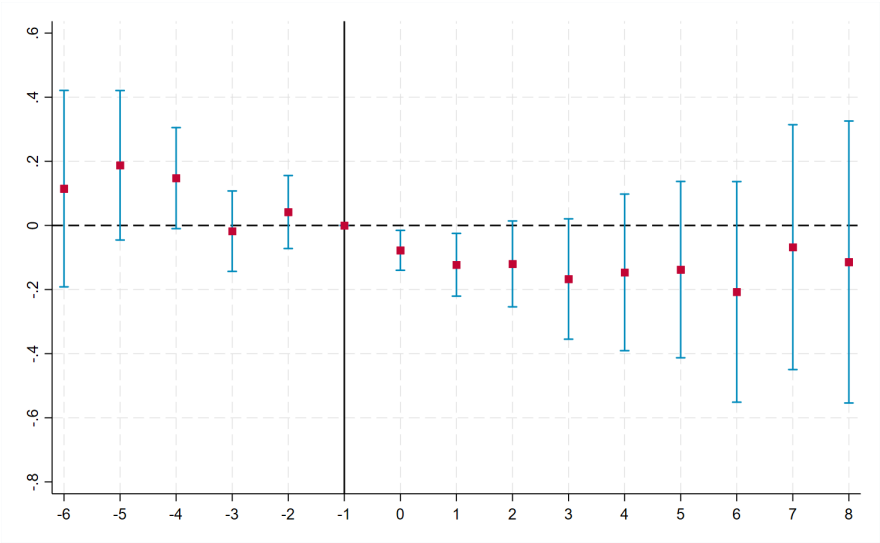


Figure 10: event-study estimates for total debt

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 105 cities in total, with a population over 30,000). The dependent variable is the logarithm of total debt for each city-year pair. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

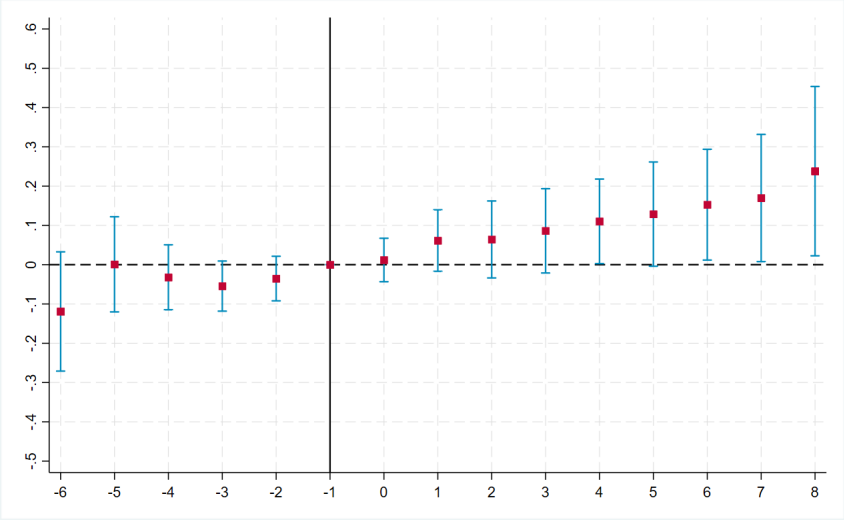


Figure 11: event-study estimates for total government expenditures

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 103 cities in total, with a population over 30,000). The dependent variable is the logarithm of total government expenditure. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

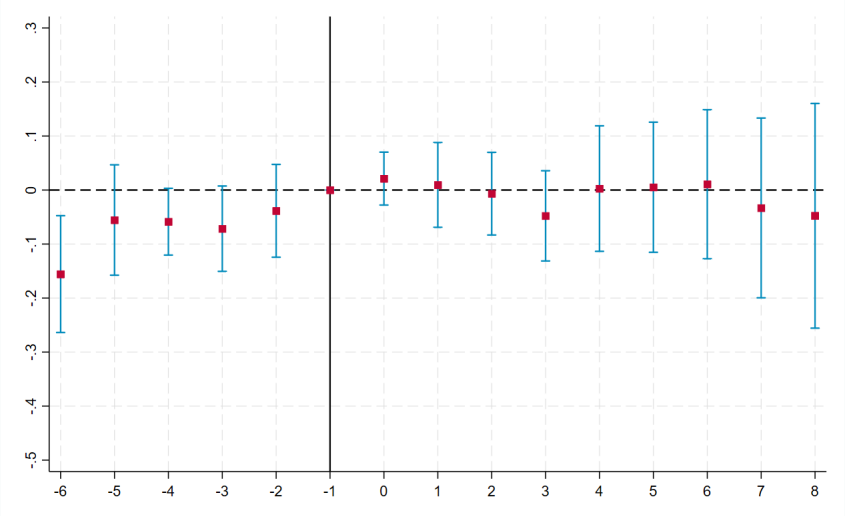


Figure 12: event-study estimates for government expenditures on police

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 112 cities in total, with a population over 30,000). The dependent variable is the logarithm of government expenditures on police. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

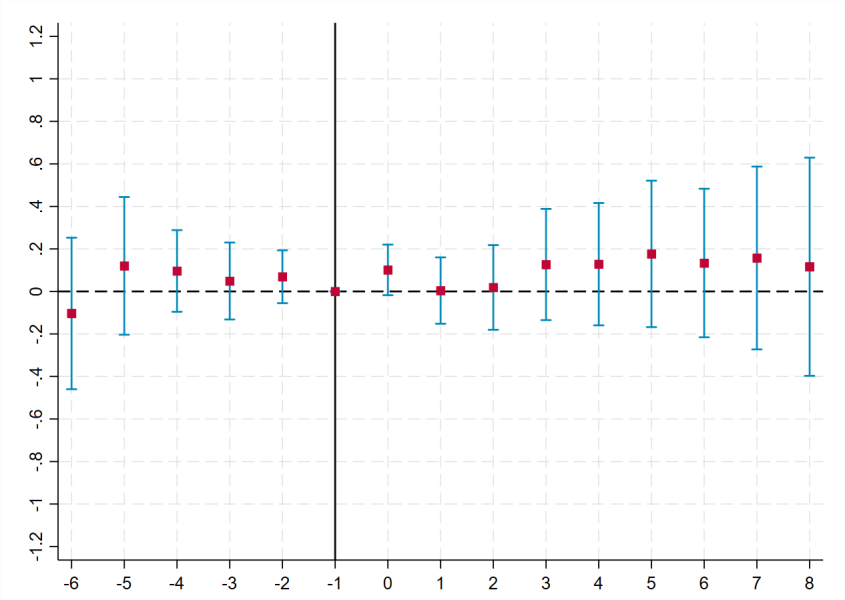


Figure 13: event-study estimates for government expenditures on public health

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 109 cities in total, with a population over 30,000). The dependent variable is the logarithm of government expenditures on public health. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

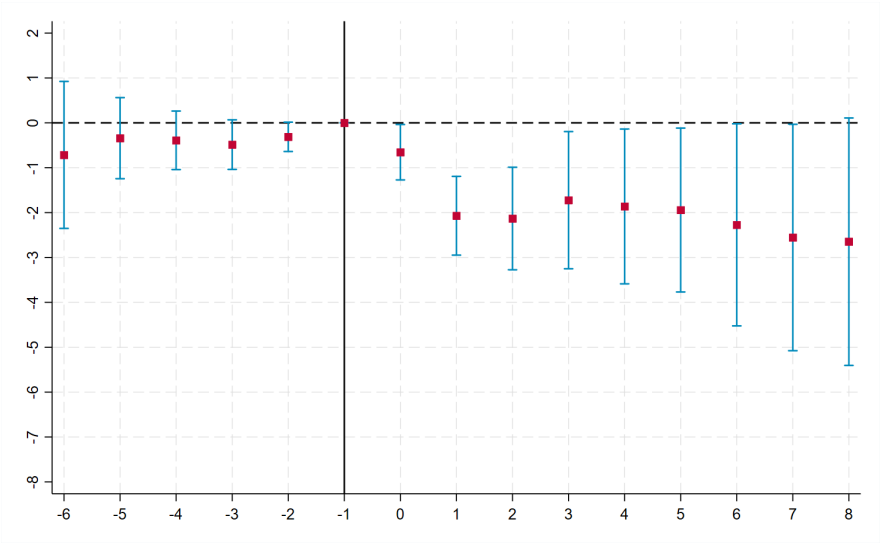


Figure 14: event-study estimates for business tax revenue (cities with high leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 47 cities in total, with a population over 30,000). The dependent variable is the logarithm of total business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

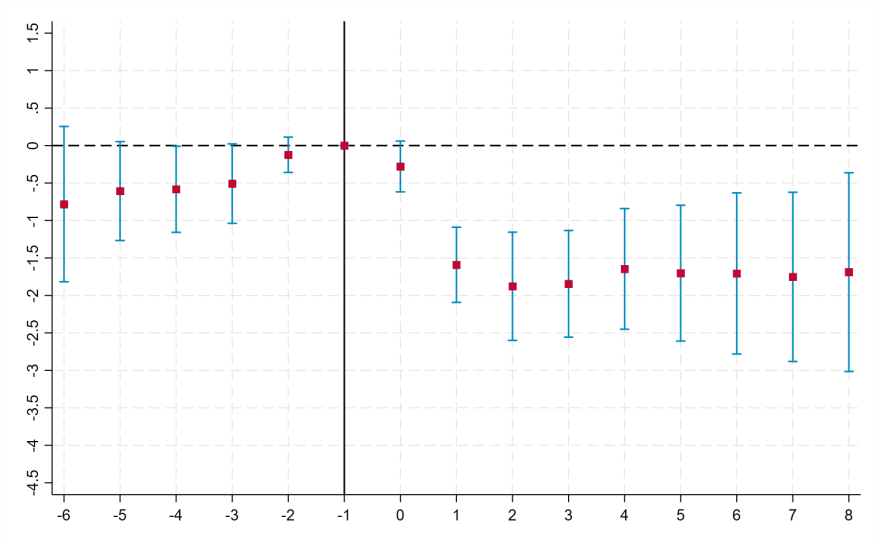


Figure 15: event-study estimates for business tax revenue (cities with low leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 58 cities in total, with a population over 30,000). The dependent variable is the logarithm of total business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

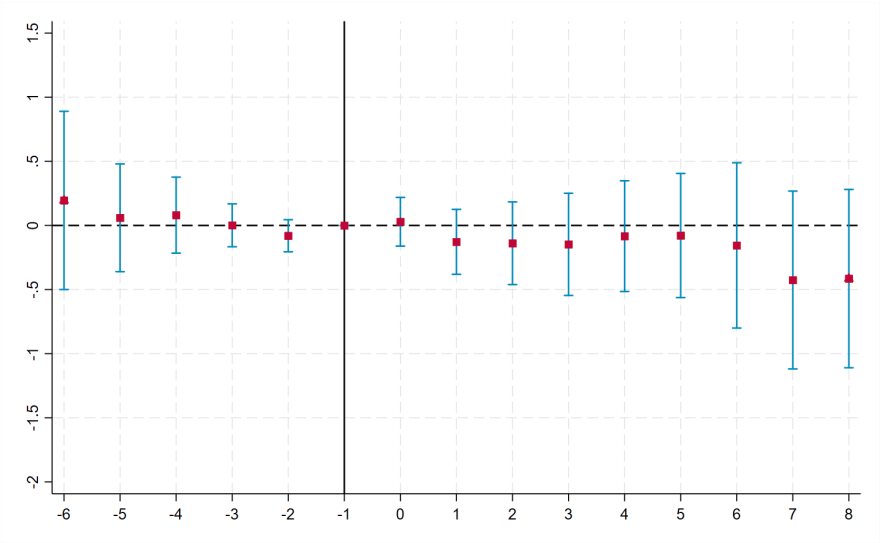


Figure 16: event-study estimates for non-business tax revenue (cities with high leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 47 cities in total, with a population over 30,000). The dependent variable is the logarithm of total non-business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

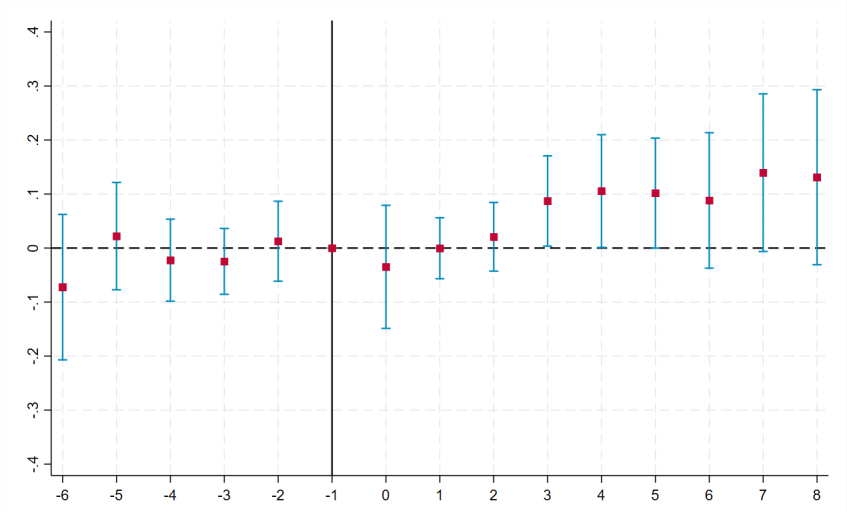


Figure 17: event-study estimates for non-business tax revenue (cities with low leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 58 cities in total, with a population over 30,000). The dependent variable is the logarithm of total non-business tax revenue. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

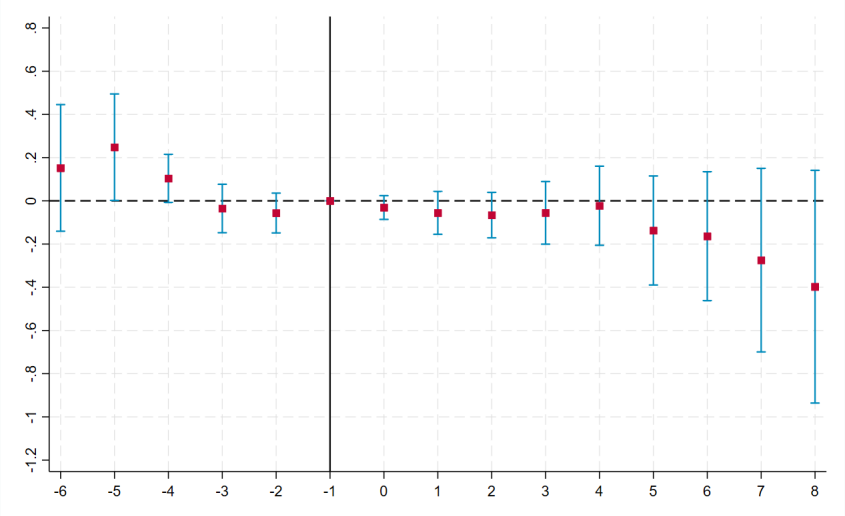


Figure 18: event-study estimates for total debt (cities with high leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 47 cities in total, with a population over 30,000). The dependent variable is the logarithm of total debt for each city-year pair. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

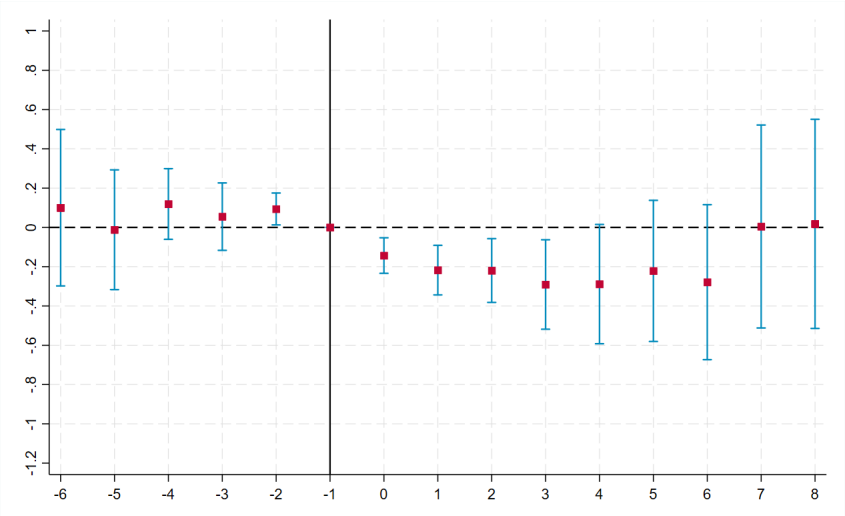


Figure 19: event-study estimates for total debt (cities with low leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 58 cities in total, with a population over 30,000). The dependent variable is the logarithm of total debt for each city-year pair. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

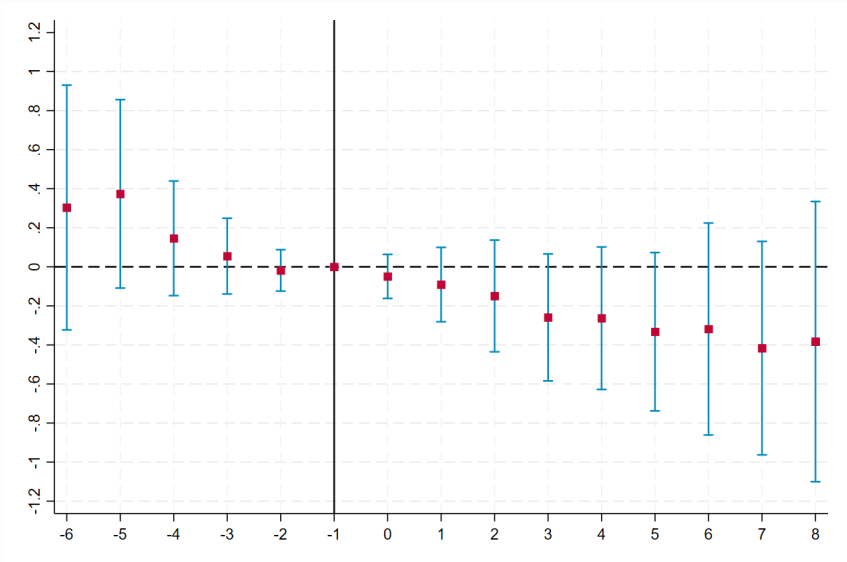


Figure 20: event-study estimates for total expenditures (cities with high leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 47 cities in total, with a population over 30,000). The dependent variable is the logarithm of total expenditures for each city-year pair. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

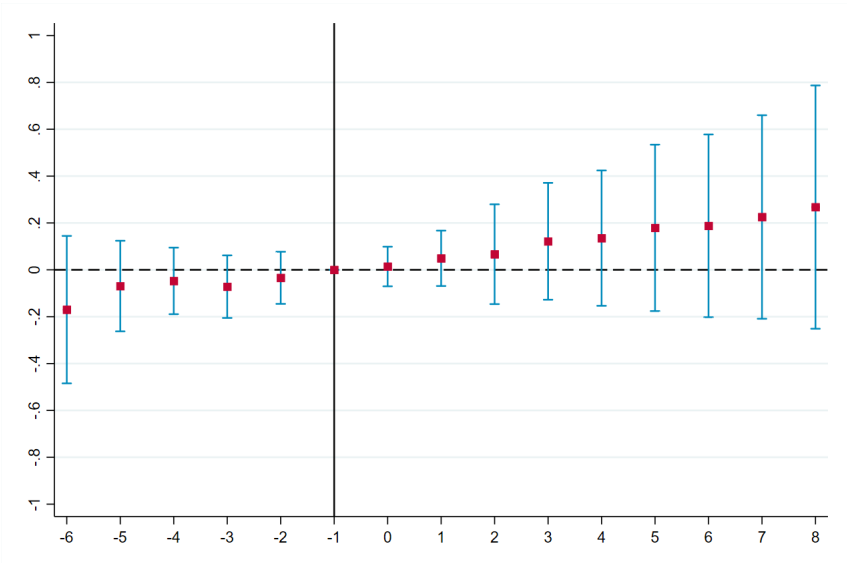


Figure 21: event-study estimates for total expenditures (cities with low leverage ratio)

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 58 cities in total, with a population over 30,000). The dependent variable is the logarithm of total expenditures for each city-year pair. City control includes the adoption of Home Rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level.

Table 1. Summary Statistics (1911-1925)

	N	Mean	Median	25 pct	75 pct
Business tax revenue	1,272	413,606	80,397	24,112	218,108
Non-business tax revenue	1,272	8,249,515	1,980,925	1,064,274	4,489,925
Property tax revenue	1,272	7,270,457	1,696,179	992,608	3,770,107
Total debt	1,260	36,745,010	5,853,315	2,838,466	14,444,013
Total government expenditure	1,260	7,177,108	1,690,017	985,184	3,873,009
Payments: police	1,232	743,384	149,429	75,945	364,026
Payments: health and sanitation	1,199	155,284	34,093	13,812	96,981

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the *Financial Statistics of Cities* for the period 1911–1925, covering between 103 and 109 cities with populations above 30,000. All monetary variables are deflated to 1925 dollars using the Consumer Price Index (CPI). The number of cities with available observations varies slightly by outcome: 106 cities for tax revenue measures, 105 for municipal debt, and 105 for total government expenditures. For detailed spending categories, the sample includes 112 cities for police expenditures and 109 cities for public health expenditures. Data on police and public health spending are unavailable for the year 1922.

Table 2. Effects of Alcohol Prohibition on Total Business Revenue

	log(business tax revenue)				ratio of business tax revenue			
$prohibition_{ct}$	-1.117***	-1.157***	-1.286***	-1.262***	-0.043***	-0.044***	-0.049***	-0.047***
	(0.151)	(0.156)	(0.180)	(0.159)	(0.009)	(0.009)	(0.011)	(0.010)
# of obs.	1272	1272	1008	1068	1272	1272	1008	1068
# of clusters	106	106	84	89	106	106	84	89
sample	all cities	all cities	full autonomy	small cities	all cities	all cities	full autonomy	small cities
city FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	No	No	No	Yes	No	No	No
year-pop group FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
mean of DV (1911)	626,062	626,062	632,608	157,617	0.173	0.173	0.169	0.170

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variables are the logarithm of total business tax revenue in columns (1) - (4), and the ratio between local business tax revenue and total tax revenue in column (5) - (8). City controls include the home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All monetary variables are deflated to 1925 dollars using the Consumer Price Index (CPI).

Table 3. Effects of Alcohol Prohibition on Total Non-Business Tax Revenue

	log(total non-business tax revenue)				property tax revenue			
$prohibition_{ct}$	0.003	0.005	-0.000	0.013	0.032	0.035	0.024	0.031
	(0.026)	(0.027)	(0.033)	(0.023)	(0.024)	(0.024)	(0.024)	(0.023)
# of obs.	1272	1272	1008	1068	1272	1272	1008	1068
# of clusters	106	106	84	89	106	106	84	89
sample	all cities	all cities	full autonomy	small cities	all cities	all cities	full autonomy	small cities
city FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	No	No	No	Yes	No	No	No
year-pop group FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
mean of DV (1911)	6,681,407	6,681,407	7,046,481	1,613,463	6,376,511	6,376,511	6,685,577	1,530,310

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 106 cities in total, with a population over 30,000). The dependent variables are the logarithm of total non-business tax revenue in columns (1) - (4), and the logarithm of total property tax revenue in column (5) - (8). City controls include the home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All dollar values are deflated to 1925 using the CPI.

Table 4. Effects of Alcohol Prohibition on Debt

	log(total debt)			
$prohibition_{ct}$	-0.127**	-0.134**	-0.185***	-0.121**
	(0.049)	(0.051)	(0.053)	(0.054)
# of obs.	1272	1272	1008	1068
# of clusters	105	105	84	88
sample	all cities	all cities	full autonomy	small cities
city FE	Yes	Yes	Yes	Yes
year FE	Yes	No	No	No
year-pop group FE	No	Yes	Yes	Yes
mean of DV (1911)	36,681,125	36,681,125	38,327,944	6,448,620

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 105 cities in total, with a population over 30,000). The dependent variables are the logarithm of total debt outstanding per city each year. City control includes home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All dollar values are deflated to 1925 using the CPI.

Table 5. Effects of Alcohol Prohibition on Government Expenditures

	log(total expenditures)			log(police expenditures)			log(health expenditures)		
$prohibition_{ct}$	0.012	0.005	-0.002	0.003	0.022	0.013	-0.066	-0.074	-0.060
	(0.023)	(0.026)	(0.023)	(0.024)	(0.026)	(0.025)	(0.068)	(0.076)	(0.071)
# of obs.	1260	1008	1056	1232	979	1045	1199	957	1012
# of clusters	105	84	88	112	89	95	109	87	92
sample	all cities	full autonomy	small cities	all cities	full autonomy	small cities	all cities	full autonomy	small cities
city FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	No	No	Yes	No	No	Yes	No	No
year-pop group FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
mean of DV (1911)	6,303,309	6,219,085	1,580,370	733,991	736,337	155,647	115,466	118,917	29,191

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925, with cities having a population over 30,000). The dependent variables are the logarithm of total government expenditures per city for each year in columns (1)-(3). The dependent variables are the logarithm of total government expenditures on police per city for each year in columns (4)-(6). The dependent variables are the logarithm of total government expenditures on health and sanitation per city for each year in columns (7)-(9). City control includes home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All dollar values are deflated to 1925 using the CPI.

Appendix

A.1. Robustness Check: Alternative TWFE Model

In this section, I employ the alternative two-way fixed effects (TWFE) estimator proposed by Callaway and Sant'Anna (2021) to assess the robustness of my findings. The results are presented in Table A1. Comparing these estimates to the baseline specifications, I find no substantial differences, suggesting that my main results are robust to alternative TWFE estimation.

Table A1. Effects of Alcohol Prohibition on Local Public Finance (Alternative TWFE)

	business tax	**non-business tax**	**total debt**	**total expenditure**	**police expenditure**	**health expenditure**
$prohibition_{ct}$	-1.443***	-0.031	-0.077**	-0.013	-0.033	-0.008
	(0.173)	(0.033)	(0.037)	(0.023)	(0.038)	(0.078)
# of obs.	1272	1272	1260	1236	1232	1911
# of clusters	106	106	105	103	112	109
city FE	Yes	Yes	Yes	Yes	Yes	Yes
year FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1911-1925; 103-106 cities in total, with a population over 30,000). City controls include the home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All monetary variables are deflated to 1925 dollars using the Consumer Price Index (CPI).

A.2. Robustness Check: Addressing the 1913 National Income Tax

In 1909, progressive members of Congress once again attached a provision for a federal income tax to a tariff bill. Conservative lawmakers, hoping to defeat the idea permanently, instead proposed a constitutional amendment authorizing such a tax, believing it would never be ratified by three-fourths of the states. To their surprise, state legislatures approved the amendment one after another, and on February 25, 1913, Secretary of State Philander C. Knox certified its ratification, bringing the Sixteenth Amendment into effect. Yet in 1913, because of generous exemptions and deductions, fewer than 1 percent of Americans paid federal income taxes, and those who did faced a rate of only 1 percent on net income.

Because this reform introduced a new federal tax regime affecting all cities simultaneously, it could potentially confound the estimated effects of prohibition. To address this concern, I restrict the sample to the post-1913 period and re-estimate the baseline specifications. The results, presented in Table A2, are consistent with the main estimates.

Table A2. Effects of Alcohol Prohibition on Local Public Finance (Post the National Income Tax)

	business tax	**non-business tax**	**total debt**	**total expenditure**	**police expenditure**	**health expenditure**
$prohibition_{ct}$	-1.103***	0.003	-0.130*	0.009	0.023	-0.094
	(0.160)	(0.033)	(0.037)	(0.033)	(0.030)	(0.073)
# of obs.	1040	1040	1030	870	990	963
# of clusters	104	104	103	87	110	107
city FE	Yes	Yes	Yes	Yes	Yes	Yes
year-pop group FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Data are drawn from the Financial Statistics of Cities (1915-1925; 87-110 cities in total, with a population over 30,000). City controls include the home rule law, which is an indicator for whether the home rule had been introduced in city c in year t. All standard errors are clustered at the city level. These results are robust when including region-year fixed effects and clustering standard errors at the state level. All monetary variables are deflated to 1925 dollars using the Consumer Price Index (CPI).