Public Goods Under Financial Distress: Evidence from Cities in the Great Depression*

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Abstract

Local governments in the U.S. issue debt to fund infrastructure projects and provide important public services to residents. When a financial crisis occurs, financially leveraged cities can suffer distress and curtail public spending, which may lead to long-term consequences for urban growth. In this paper, I collect novel archival panel data on cities and municipal bonds during the 1920s and 1930s and estimate the effect of financial leverage on local public good provision during the Great Depression. I find that distressed cities significantly lowered public good provision - roughly 20 percent of the drop in expenditure can be explained through a reallocation of budgets towards debt repayment. Despite large institutional differences between cities and firms, the effects of financial distress on wages are surprisingly similar. In response, I find suggestive evidence that households subsequently relocated away from distressed cities.

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1 Introduction

Financial distress caused by leverage can prolong and exacerbate economic shocks. The consequences of financial fragility during economic crises has been widely studied for both households and firms. Research has shown that the effects of financial crises are not evenly felt as highly indebted households and firms with fractured creditor relationships seem to bear the brunt of recessions (Chodorow-Reich (2014), Mian et al. (2013)). The impact of economic shocks on leveraged local governments has received much less attention, despite the of vast size of the municipal bond market—$4 trillion as of 2021—and the economic importance of the services they provide—education, health, and police to name a few. Understanding to what extent cities are constrained and how financial market frictions propagate to public spending programs is of first-order importance for guiding fiscal and macroprudential policy.

Recent work has emphasized the importance of credit on governments, linking changes in a government's ability to borrow to its provision of public services and investment. Given that the majority of external financing comes from the municipal bond market—which has grown consistently in recent U.S. history—much of this research has paid particular attention to the role of credit *booms* that have made it easier for municipalities to access financing. However, direct causal evidence on the role of debt-driven financial constraints for local public good provision during a financial *crisis* is difficult to establish, and crucial questions remain. When faced with constraints during a crisis, how much do cities curtail spending, which public goods are effected the most, and how do households respond?

This paper sheds new light on these questions by studying how cities responded during and after the largest financial crisis in U.S. history: the Great Depression. This setting is a particularly relevant one, as cities invested in infrastructure to accommodate the large influx of rural-to-urban migrants and financial leverage increased substantially in the first decades of the 20th century (Figure 1). This raised the likelihood of financial distress once the Depression decimated the underlying tax base for local governments—real estate—and financial markets were in turmoil. In a departure from prior work, I study public good provision in an empirical context in which highly indebted cities are faced with important trade-offs between defaulting on financial obligations and providing local public goods.

The historical context allows me to overcome two of the key challenges confronted by empirical research on modern-day cities. First, local governments in the U.S. were the main government service providers before and during the Depression and had to react to the Depression without

modern-day policy instruments such as fiscal stimulus from Federal or State governments.[1] Intergovernmental transfers, however, present an identification challenge to the study of city financial constraints because fiscal support from state and federal governments can smooth out economic and financial shocks. By going back to the 1920s and 1930s, this paper overcomes this challenge. Second, the systematic financial distress of cities experienced during the Depression is a historically rare but potentially devastating event, and studying it is particularly important in order to inform policy responses to future crises. In terms of an empirical laboratory, the Depression is close to ideal: cities neither experienced the economic shocks of Depression uniformly nor did they lever up equally during the Roaring '20s, which creates across-city variation that enables me to study the causal effect of financial constraints on local public good provision. If the link between financial constraints and public good provision is significant, this should be observable in the most prominent financial shock of the 20th century. Importantly, however, the historical context does not limit the applicability of my results, as many countries around the world operate under fiscally decentralized systems without significant federal government transfers (Stegarescu (2005)). Moreover, even though the current system in the U.S. relies on transfers, the fiscal arrangement between governments is ultimately a political choice that could change in the future, as it did in the past.

To investigate these questions, I construct a historical dataset from multiple novel archival sources on U.S. cities. First, I digitize the *Financial Statistics of Cities* produced by the Bureau of the Census, which reports city revenue and expenses only for cities with a population of over 100,000 before and during the Great Depression. I expand the scope of this dataset by digitizing and standardizing annual financial transaction reports for the near-universe of cities that have a population over 1,000 (770) in New York, Massachusetts, Ohio, Indiana, and California for the years 1924 - 1938. To the best of my knowledge, these are the only states to report data on local public good provision during this time. The granularity of the data allows me to study specific spending programs at the city level (e.g., infrastructure) and directly control for observed federal and state government transfers originating, for example, from the New Deal programs. Furthermore, I observe both the amount of - and in some cases, the duration of - debt and total assessed property values, which I combine to construct leverage, my proxy for financial constraints. Finally, I supplement the

[1] In fact, their failure to provide adequate support to the unemployed (a quarter of the labor force) is the reason why the Roosevelt administration introduced a new fiscal regime centered around the Federal government, which included the transfers that are common today.The Federal Emergency Relief Administration provided over $500mil to states and cities in grants and loans, while the Works Progress Administration and the Social Security Act greatly increased the scope of the Federal government's involvement with unemployment and assistance. By 2006, transfers accounted for 38 percent of all local government revenues.

city-level data by building a second database of over 29,000 municipal bonds contained in *Moody's Manual of Governments* in 1929, the primary source of information on government bonds at the time. These individual bonds account for 85 to 95 percent of the total bonded debt as reported by the official government sources in the financial transactions data.

To study the economic consequences of public financial constraints on households, I utilize a sample of linked micro-level U.S. decennial census records between 1930 and 1940 by combining crosswalks provided by the Census Linking Project (Abramitzky et al. (2020)) and publicly available 100% count data from IPUMS (Ruggles et al. (2020)). My sample includes roughly 3.47 million males between the ages of 18–56 (as reported in 1930) living within the boundaries of one of the approximately 950 Census-identified cities in 1930.

To measure variation in financial constraints, I exploit a channel that works through the cost side of a city income statement and the liability side of its balance sheet—the interest and debt channel—that has been largely ignored so far in the public good provision literature. Following the corporate finance literature on financial constraints, I use both a stock (the ratio of debt over property value) and a flow measure (the annual tax revenue over interest expense) of leverage before the Depression (Kaplan and Zingales (1997)). Hypothetically, there are numerous reasons why financial leverage is a good proxy for financial constraints in this setting. First, with falling tax revenues during the Depression, cities had to choose which expenditures to maintain and cut. One significant expenditure they faced was debt repayment and interest, which varied substantially across cities and whose significance rose in proportion with leverage. Second, highly leveraged agents are closer to defaulting on existing obligations and hence may be credit rationed by a recovering financial sector because of information asymmetries (Bernanke (1983), Stiglitz and Weiss (1981)). In fact, in the context of 1930s U.S. cities, the leverage ratios I define below are the same ones used by state regulators and credit ratings institutions to measure municipal creditworthiness and, thus, the price of credit. Lastly, as I describe in more detail in Section 2, municipal default before the establishment of Chapter 9 in the Bankruptcy Code was costly and higher levered cities were closer to default. Thus, city governments had to weigh the long-lasting repercussions of default with the consequences of short-run spending cuts.

To estimate the impact of financial constraints on local public good provision, I compare expenditure in more or less constrained cities before and after the onset of the Great Depression using a difference-in-differences framework. I find that cities in the 75th percentile of the debt to value ratio saw a 5 percentage point decrease across current expenditures and a 15 percentage point

decrease in capital investment relative to cities in the 25th percentile. The results are conditional on regional trends, contemporaneous city revenue, and historical population trends. Likewise, cities with high tax-to-revenue ratios cut spending by less than those with higher interest cost burdens by quantitatively similar magnitudes. As an alternative way to gauge the magnitude of these estimates, I use a back-of-the-envelope exercise to compute an elasticity of spending cuts during the Depression to the (real) rise in leverage between 1924 and 1932. I find that the average leverage increase during the 1920s resulted in a 1.4 percent decrease in annual public service expenditure and a 3.6 percent decrease in annual capital investment in U.S. cities during the Depression. The effect, however, is heterogeneous across cities. I find that cities that grew more during the 1920s (before the Depression) were also the ones in which financial constraints during the Depression effected expenditure the most and that the effect was largest for small (below 10 thousand) and medium (10 to 100 thousand) cities. Even though leveraged cities decreased their service expenditure equally across counties with varying levels of banking sector panic, I find that most of the effect on capital expenditure, on the other hand, is driven by cities in counties where the banking sector was under stress.

Undeniably, pre-existing city-level differences in public goods can correlate with financial leverage ratios in a non-causal way. To explore whether the financial constraints on local public goods are causal—and not driven by pre-existing city-level differences in public goods that happen to correlate with financial leverage ratios—I implement an additional analysis which exploits the quasi-exogenous timing of bonds becoming due. The financial market crash of 1929 and the recession that followed led to a collapse of bond markets in the early 1930s (Hillhouse (1936)). Municipalities could not easily issue new debt to repay the principal owed on bonds that were becoming due during this time and cities which had more of these bonds outstanding were plausibly more constrained in allocating revenue between debt-service and public goods. These bonds, however, were primarily issued well in advance of the onset of the Depression such that the specific timing of these debt-repayment shocks was unlikely to be driven by the demand for new investment during the Depression. Using newly collected bond-level data, I use the variation in the proportion of a city's debt issued *before* the Depression that was contractually obligated to be repaid *during* it as a proxy for financial constraints. The experiment thus compares public good provision in two similar cities that have different levels of potentially exogenous amount of debt maturing during a specific window of time when a bad financial event occurred (Almeida et al. (2009), Benmelech et al. (2019)).

I find that cities that had more debt that matured during the Depression curtailed public good provision on capital investment (construction) and public service expenditure more than similar cities that did not face the same financial shock. Specifically, a one standard deviation in the amount of bonds due is associated with a decline in current spending and capital investment of about 2.5 and 20 percent, respectively. Altogether, these results suggest that roughly 20 percent of the drop in expenditure can be explained through a reallocation of budgets towards debt repayment. These magnitudes are quantitatively similar to the estimates found using similar methods for firm employment during the Depression (4.2 to 5 percent as in Benmelech et al. (2019)) but larger than those found for firm investment during the Great Recession (2.2 to 2.5 percent as in Almeida et al. (2009)).

I then explore the effect on different types of public goods, on tax rates, and on credit ratings. I find a large effect on police and firefighting spending as well as on capital investment, and quantitatively smaller but significant effects on the other categories such as health and sanitation. As a placebo check, I do not find that city welfare spending was impacted by leverage. Since welfare expenditure was dictated mainly by the amount of federal transfers to each city from New Deal programs and outside of local government control, the effect of leverage on welfare should be negligible. Second, I find that cities that were more indebted subsequently raised taxes for debt repayment but not total tax rates. Lastly, I find that highly levered cities, on average, received lower credit ratings than similar cities that went into the Depression with lower leverage. This result provides some evidence that the cost of borrowing and issuing bonds was higher during the Depression for higher levered cities, which may help explain why these cities spent less on infrastructure.

To explore the robustness of my main findings, I show that the main results are not driven by alternate explanations of local demand, city-bank relationships, or the city's government type or electoral cycle. To disentangle debt-driven financial constraints from local demand for public goods, I show that the results are robust when I exclude recent (1927-1929) borrowers from the sample. I also show that the results do not change when I directly account for the severity of the Great Depression in the retail sector. That is, demand for public services and investment—and the correlation between leverage and local demand—does not seem to explain away the observed effect of leverage on expenditure. Second, I collect information on a city's connections to various banks and construct a binary measure of whether a city was connected to a bank in a financial hub (Chicago, New York, or Cleveland). I do not find that the inclusion of this connection proxy in my

specification alters my main results—potentially meaningful city-bank relationships may correlate with financial leverage but they do not determine local public good provision during recessions. Lastly, I find that the main findings are robust to alternative political explanations by directly and flexibly controlling for each city's government type and electoral cycle.

Finally, I investigate the urban growth consequences of the curtailment of local public good provision by exploring whether individuals responded to changes in local fiscal policy through their migration decisions. The theory that differences in public goods can affect location decisions of individuals dates to the seminal contribution of Tiebout (1956) with important and recent empirical work (e.g., Barseghyan and Coate (2016), Jehiel and Lamy (2018), Yi (2020)). Using a linked sample of 3.7 million urban males and Census variables that allow me to geolocate people in 1930, 1935, and 1940, I find that people moved away from cities with greater economic downturn and higher financial constraints. Further, in a sample of public employees, I find evidence that local administrators moved at a higher rate than other public workers due to local shocks, and that the effect is pronounced for short-distance movers, suggesting that local public goods may have played an important role in the spatial mobility of households during the Depression.

This paper contributes to several strands of academic literature. First, the existing literature on financial constraints has extensively explored their impact on firm investment (Kaplan and Zingales (1997), Fazzari et al. (1988) and the implications for macroeconomic policy (Gertler and Gilchrist (1994), Bernanke et al. (1996)).[2] Yet, we know relatively little about the effects on municipalities, especially during times of financial market failures. My paper is related to Adelino et al. (2017) and Yi (2020), who study the effect of credit supply shocks on public good provision in the contemporary context. Complementary to their work, the focus of this paper is to study cities under financial distress due to a macroeconomic shock, which has recently come to the attention of policymakers as cities and states wrestle with financial shocks stemming from the Covid-19 pandemic.[3] Second, I contribute to the economic history literature of the public sector during the Great Depression in the U.S., which has primarily focused on federal programs stemming from the New Deal such as the Federal Emergency Relief Administration and the Works Progress Administration (Fishback and Wallis (2012).This literature has found that Federal programs had a positive impact

[2]Examples of research on financial constraints for firms during the Great Depression include Benmelech et al. (2019), Ziebarth (2013), and Lee et al. (2015). For evidence of financial constraints in the modern context, see, for example, Chodorow-Reich (2014) and Almeida et al. (2009).

[3]The Covid-19 crisis has decreased local revenues and increased demands on health, education, and other services during the pandemic, which led to the creation of the $500 billion Municipal Liquidity Facility (MLF) by the Federal Reserve in April 2020. Policymakers recognized that funding pressures and disruptions in credit markets may adversely impact municipalities and pledged to act as a lender of last resort.

on retail consumption (Fishback et al. (2005)), in-migration (Fishback et al. (2006)), and crime reduction (**?**) among others. This paper is among the first papers to study how *local* governments responded to the Great Depression. Notably, I add to the work of Siodla (2020), who explores fiscal strain in the largest U.S. cities during the Great Depression.[4] While that paper explains fiscal strain on city budgets using tax delinquency and debt, the focus of this paper is leverage and the debt-driven financial constraints that arise from it *conditional* on taxpayer behavior. Lastly, this paper also adds to the broader literature on urban public economics and local economic development in the U.S. during the first half of the 20th century. Specifically, this literature has found large positive effects of local urban infrastructure investments on public health (for example, Ferrie and Troesken (2008), Cutler and Miller (2005)), large spillovers on private economic activity (for example, Kline and Moretti (2014)), and a strong connection between residential construction and the rise of municipal debt (Gunter and Siodla (2018)). I extend this literature by showing how financial shocks originating from the financing arrangements of these infrastructure initiatives at the local level contributed to lower public good provision during the Depression.

The rest of the paper is organized as follows. Section 2 describes the historical context and institutional details of local public good provision in the first half of the 20th century. Section 3 then describes the construction of the dataset. Section 4 provides an overview of revenues, expenditures, and debt for a large sample of cities in 1924–1938 and uncovers relationships between spending, investment, and debt. Section 5 tests whether financial leverage drove public goods spending cuts after the onset of the Great Depression using a difference-in-differences design. Section 6 introduces the empirical strategy of using bond-level data to identify short-run financial constraints and shows the resulting adverse effects on public good provision. Section 7 tests whether urban growth was adversely affected through a household migration channel. Section 8 concludes the paper.

2 Historical Background

This section describes the institutional setting of public good provision and debt in the U.S. during the first half of the 20th century. The period from 1900–1940 represents a crucial inflection point of the economic development of the U.S. economy. Before the Great Depression and the World Wars, local and state governments were the primary taxation authorities and largest public spenders. The transition from a fiscal system dominated by local government to one of

[4]For similar evidence on expenditure cuts during the Great Recession, see, for example, Cromwell et al. (2015) On how declining property values effected government revenues in the 2000s, see, for example, Doerner and Ihlanfeldt (2011) and Lutz et al. (2011).

local-state-federal cooperation was characterized by the federal government raising revenue through new sources (individual income, excise) and distributing taxes back to states and localities (Wallis (1984)).

Local governments

Since the mid-19th century, local governments - e.g., cities, counties, and school districts - have undertaken infrastructure projects in education, roads, and public utilities. Cities also invested in police and firefighting departments, built publicly-funded hospitals to care for the poor, and constructed jails and public libraries. As a result, local government, not the federal government, became the largest public spender (and debtor) in the U.S. Using data from the Historical Statistics, Figure A.1 plots the share of non-military spending by level of government in the United States from 1900 to 1970. Before 1932, the relative shares for each level were roughly 50 percent local, 25 percent state, and 25 percent federal. After 1940, relative shares were approximately 10 percent local, 5 percent state, and 85 percent federal.

A significant driver of increasing federal government expenditures was public relief programs instituted by the New Deal (e.g., Social Security). Most were administered alongside state and local governments, such as the Federal Emergency Relief Administration (FERA) and the Works Progress Administration (WPA). Federal funds financed most of these programs with matching state and local contributions, even though program eligibility was the responsibility of state or local governments.[5]

Municipal debt

Using data from the Commercial and Financial Chronicle, Figure 1 plots the average annual municipal bond sales for the interwar period. The yearly average of municipal bond sales in the 1920s stood at the unprecedented height of $1.1 million. The preceding ten-year average was $417,000. There are three key factors that trace the expansion of public infrastructure.

For starters, the first four decades of the 20th century are referred to as the "high-school movement" due to the substantial rise in enrollment in secondary education from 10 percent in 1900 to 70 percent by 1940 (Goldin and Katz (1997)). The increase in schooling necessitated the

[5]For example, WPA administrators were forced to choose work relief recipients from a list of qualified needy workers supplied by the local relief agency (Wallis (1987)).

construction of schools and investments in equipment to furnish them. To finance these construction projects, cities and school districts issued bonds, which were eagerly bought by wealthy private individuals, banks, and corporations (Brown (1922)). Second, significant rural-to-urban migration led to an increase in urban density and a surge in demand for new investments in electrification and sanitation: power plants, water supply systems, and chlorination. Third, the rise of the automobile and the beginning of the suburban migration in the latter part of the period led to the construction of paved roads and public transportation systems.

Whether or not the local debt boom in the 1920s was consequential for cities during the Depression is the first question of this paper. Qualitatively, the narrative evidence from this period shows that contemporary observers understood the risks cities were taking. For example, on Dec. 4, 1922, the *Wall Street Journal* stated that "the consequence will not come today or tomorrow, but we shall see a number of bankrupt townships and counties before we are many years older, as an incident of the next spell of bad times. The thing is as certain as tomorrow's sunrise. The real estate values on which the present taxes are assessed are for the most part grossly inflated." Unlike firms that can cut losses and exit the market due to macroeconomic shocks, municipalities cannot be liquidated or sold to private investors. However, they can lay off public workers and severely limit services when faced with financial constraints, a warning issued by economists at the time (Upson (1935)).

According to the most recent estimates, the Depression caused over 4,800 municipal bond defaults during the 1930s (Fons et al. (2011)). Property tax revenues fell due to a slump in the housing market and tax delinquency. Additionally, the early 1930s was a period of organized tax revolts (Beito (1989)) in addition to banking panics that induced default for cities holding funds in a suspended or failed bank.[6] Finally, as predicted by observers in the decade prior, local officials were faced with sizable municipal bond repayments that were typically scheduled to mature all at once. This maturity problem was especially acute for cities that expanded in the 1920s by financing infrastructure investment with long-term bonds that became due during the Depression.[7]

There are several reasons why a local jurisdiction would prefer cutting local public good provision over defaulting on its debt. Before the establishment of Chapter 9 of the Bankruptcy Code in 1937, the process of defaulting was costly and time-consuming. In general, creditors first

[6] A 1933 survey of over 1,200 state, city, and county financial officials found that half had funds in closed banks (Faust (1934))

[7] Some cities set aside revenue in "sinking funds" to meet these large balloon payments. However, these funds typically invested the cash in assets that later declined in value during the Depression such that many governments were unable to roll over maturing issues.

needed to obtain a "writ of mandamus" ruling from a state or federal court. A judge first needed to check the legitimacy of the defaulting bonds, then issue a judgment, after which a creditor could petition public officials to levy and collect a tax sufficient to pay the judgment. If a city refused to pay, bondholders would organize and sue the defaulting city.

Importantly, once a city was sued for default, it was effectively barred from accessing capital in regulated capital markets such as insurance companies and state savings banks. Many state regulators produced lists of securities that named firms or public entities in which fiduciary institutions could invest. According to Hillhouse (1936), a default "may cause a loss of this favored status for fifteen or twenty-five years, thereby materially narrowing the market for future bond issues. Thus, when one large city of the Southwest defaulted in 1898, and again in 1904, it was withdrawn from New York State's legal list and was not reinstated until in the late 1920s." Thus, a city in distress had to decide between losing access to capital markets for the long run or reducing public goods in the short run.[8]

3 Data

I begin by describing the main features of my novel annual data on municipal finances during the 1920s and 1930s. Overall, the dataset contains over ten thousand observations on revenue, expenditure, and debt across 850 cities from 1924–1938. In 1930, these cities included approximately 44.7 million people or roughly 64.7% of the U.S. urban population. The median population of a city in my sample is about 8,000, and the average duration of a city in my panel is 13.3 years.[9] I deflate all dollar figures using the Consumer Price Index (Federal Reserve Bank of Minneapolis (2020)) unless otherwise noted.

I digitize and standardize municipal financial statements from various state agencies for Massachusetts, New York, Indiana, Ohio, and California. To the best of my knowledge, these five states are the only ones that produced annual statistics for municipalities before, during, and after the Great Depression.[10] While all states report statistics on revenue sources, expenditures, and debt

[8] According to Hillhouse (1936), the prospect of higher taxation post-default was yet another factor that discouraged it: "It is natural that private capital should avoid communities in which local governments are in financial troubles, since this usually means that there has been mismanagement of local governmental affairs and that property is subject to heavy tax burdens. It also serves as a warning that if creditors are successful in litigation, property may be subject to levies to pay judgments."

[9] For comparison with other research in the field, recent modern studies (notably Siodla (2020) and Gunter and Siodla (2018)) have used data pertaining to the largest 94 cities with populations above 100,000. As I discuss in Section 4, there are interesting and important differences in how cities of different sizes fared during the Great Depression.

[10] New Jersey, Wisconsin, and Connecticut also published statistics for only a couple of years in the 1920s and

levels, the granularity varies by state. For example, Massachusetts (highest quality) reports taxes collected by source (property, corporate, personal income) while Indiana (lowest quality) aggregates all taxes into one category. Within states, the reporting is constant over time.[11] California and Ohio report detailed expense categories (e.g., administrative wages vs. inspection services vs. police officer wages), and one state, Massachusetts, reports a detailed account of new debt issues and retirements. In all cases, the reported figures are actual payments and receipts (reported after the conclusion of a fiscal year). For more information on these reports, please see Appendix C. Summary statistics are shown in Table 1. I leave the description of time series evolution of key variables for Section 4.

I complement the above dataset by digitizing reports from the U.S. Bureau of the Census. The Census has been collecting data on large cities (with a population over 100,000) since 1905 and publishing statistics in reports called *Financial Statistics of Cities*.[12] Before 1931, this report also covered all cities with a population of over 30,000, but the reporting was curtailed after federal budget cuts during the Great Depression. In all, data on 93 cities is available for all years in my sample period. In cases when a city from this source is duplicated in the State documents (e.g., Boston appears in both the Census and MA State documents), the statistics from the Census are used.

Next, I hand collect bond-level data from *Moody's Manual of Governments* for the year 1929. The *Manual* was sold to retail investors in the U.S. and contained both quantitative security-level data, a qualitative review of major industries in a city, and a Moody's credit rating. The advantage of this source is that it provides detailed information on the debt structure, such as repayment and maturity, which is useful for identification as described in Section 6. For example, this data allows me to see that a 4% bond was issued by the city of Chicago in 1920 with an outstanding balance of $50,000 that was left to be repaid annually in the period of 1936–1950. In total, the data contains 29,366 bonds outstanding across 316 cities in 1929. Table A1 presents the summary statistics.

Finally, I construct demographic and economic characteristics of cities from the Decennial Census. I aggregate person and household level observations to the city level from the publicly available 100% count Census in 1930 and 1940 on IPUMS (Ruggles et al. (2020)). Not all cities are

1930s. However, the coverage misses key years during the Great Depression; thus, it is not included in this project.

[11] Additionally, each state reports totals for revenue and spending categories which allows me to check the accuracy of the digitization process. All OCR errors were manually corrected such that the totals reported in the publications match the sums of individual categories.

[12] These reports are available from the digital library maintained by the Federal Reserve Bank of St. Louis (FRASER).

identified in the Census enumerations - of the roughly 850 cities with financial transaction data, only 262 of them are identified in the Census. Further, I use the crosswalks provided by the Census Linking Project (Abramitzky et al. (2020)) between the 1930 and 1940 Censuses to link individuals across time to study the effect of local public goods on migration.

4 Local Public Goods, 1920–1940

This section summarizes the key variables of city revenues, expenditures, and debt during the 1920s and 1930s. Because trends in local public good provision for non-major cities is novel to the literature, I present broad patterns based on the average per capita values in cities of three population categories: less than 10,000, 10,000 to 100,000, and above 100,000 as reported by the 1930 census. For time-series comparisons, I create an index based on the average per-capita outcomes in each population category, and I normalize this index to have the value of 1 in 1930 such that an index value of 1.05 denotes a 5 log point (roughly 5 percent) increase from the 1930 level.

4.1 Revenues

Figure A.2 presents the breakdown of revenues by city size in 1930.[13] Three important facts stand out. First, the bulk of income (70–80%) comes from local property taxation or income from publicly-owned utilities, and larger cities collect (and spend) more per capita than smaller cities. The reliance on local property assessments highlights the usefulness of this period in U.S. history in studying the effect of local shocks on public goods provision. That is, housing market shocks are reflected in city revenue, and thus confounding fundamentals that could also affect the provision of local goods are accounted for once tax revenue is included as a control. Second, intergovernmental grants, government earnings, fines, and license fees individually contribute less than 10 percent to city budgets on the eve of the Great Depression. Finally, while cities of all sizes rely on property taxes as a primary source of income, there is some variation in the contribution from non-tax sources. For example, there is lower reliance on revenue from publicly-owned utilities in larger cities and higher support from user fees generated by special construction projects (e.g., highways).

Figure A.3 shows that cities experienced parallel growth in revenues from 1924 to 1931. Small cities saw a severe drop in total revenue more than larger cities - driven primarily by smaller tax collections. Large cities maintained tax revenue at 10 to 12 percent above their 1929 values (a 2–4 percent decrease relative to the peak in 1932), whereas small cities saw a much more precipitous

[13] I trim the sample at the 2–98 percentiles by year-population category to reduce the influence of outliers.

drop of 15 percent relative to 1929. Due to data limitations, whether these tax revenues were lower because of taxpayers being unable to pay their taxes (delinquency) or because of a larger house (tax base) price collapse in smaller cities remains an open question. Third, grants from the state and federal governments increased by 150–200 percent across all cities, albeit from a low starting base and more so in municipalities with a population size of less than 100,000.[14]

Table 3 reports the percent change from the peak in 1931 to the trough in 1935 at the 25 and 75 percentiles: of the 281 small cities that reported any grants in 1931, more than 25 percent reported an increase of above 446 percent by 1935. For the largest cities, the top quartile saw a 181 percent increase.

4.2 Expenditures

Figure A.4 shows the average level breakdown of spending by city size computed in 1930. In general, cities of all sizes spent between 1 and 10 percent on protective services (police and fire), the local health department (e.g., sanitation and inspection of waterways), welfare (e.g., local unemployment support and poor houses), government expenses, and interest payments. In addition, education and public utility expenditures comprise another 30 percent of city budgets. Lastly, 15 to 20 percent of spending was directed towards capital outlays - constructing permanent fixtures such as buildings, roads, dams, canals, and public hospitals.[15]

Figure A.5 shows how these expenditures evolved over the sample period. Peak nominal spending occurred in 1929, followed by steep declines by 1934. Notably, the most severe and immediate drop in spending was in infrastructure. On average across all cities, construction spending decreased by 60 percent from 1929 levels. Across city size, I observe that construction spending declined to zero by 1935 in 25 percent of small cities that reported *any* construction spending in 1931, as shown in Table 3. City officials also curtailed current (non-capital) non-welfare expenditures, but less drastically and later in the 1930s.[16] Police and firefighting protective services declined by 20 percent, government expenses by 10 percent, and health department payments by 15 percent. Interestingly, interest costs did not decline similarly, with over 25 percent of medium and large cities

[14]Using detailed tax collection data (not shown) from Massachusetts cities, approximately 95% of all taxes collected are property taxes.

[15]Education spending represents both the expenditures of independent school districts for instruction and operation in some cities (typically in larger cities and more industrialized/urban states), while in others it is only the expense not covered by independent school districts such as the maintenance of city libraries and city colleges (typically for smaller cities and states such as Indiana where education is primarily provided by the county). Unfortunately, the historical sources from which the data comes from do not allow me to separate the two.

[16]The Depression afflicted regions and cities at different times between 1930 and 1933 and there is no one "treatment" time in this context. Thus, in the empirical analysis, I consider all years after 1930 to be treated years.

reporting an *increase* in interest costs, consistent with the fixed-repayment schemes of long-term liabilities and the severe deflation of this period.

4.3 Debt, financing costs, and constraints on expenditures

Lastly, I investigate the stock and flows of debt on city balance sheets and assess the importance of financing cost in relation to other city expenditures.

Across all population categories, long-term city debt used for general purposes (5–50 year bonds) comprised 50 percent of city debt. The remainder was debt issued for publicly-owned utilities (electric and waterworks systems) and short-term (1–3 years) loans. Figure A.6 shows that large cities were more than two times as indebted as small cities before the Depression, which is the result of the rising demand for urban infrastructure in the preceding decades as described in Section 2.

Figure A.7 shows an index of the average per-capita debt levels across city population size and year. The total inflation-adjusted stock of debt increases by about 30 percent from 1929 to 1933, driven by both long and short-term debt. Again, heterogeneity across population categories is evident: larger cities became relatively more indebted during the Depression than smaller cities, primarily due to their stock of long-term bonds. From 1929 to 1933, short-term borrowing across all cities increased by roughly 60 percent.

One limitation of looking at per-capita debt levels using end-of-year balance sheets is that such an analysis does not reveal much about the flows of different types of debt. However, data from Massachusetts allows for further investigation into debt flows with statistics on retired vs. newly-issued debt by city, as shown in Figure A.8. The story revealed by the data is consistent with the macroeconomic view of the Great Depression. As bond markets froze up by 1930, new debt issuance dropped severely (black dashed line) and only slowly recovered to its 1930 level by 1938. On the other hand, cities maintained their payments to existing bondholders, resulting in a net decrease in leverage between 1932 and 1936 (red dashed line). There appeared to be a substantial increase in short-term borrowing (and repayment) between 1930 and 1934, most likely to shore up budgets due to tax revenue shortfalls.

What causes one city to be more indebted than another? In Section 2, I claimed that investment in infrastructure was the primary driver of municipal debt in the 1920s. Here, I estimate within-city regressions of total debt on various potential explanatory variables and find that capital investment was in fact the main determinant of pre-Depression municipal debt in my sample of cities. Table 4 shows the ordinary least squares coefficients of a linear model with the independent

variables standardized to have mean zero and standard deviation one. Column (1) contains city fixed effects and contemporaneous and lagged values of assessed property value and outlays, as well as total current expenditure on non-outlay non-interest. Column (2) adds year fixed effects to account for national trends (in interest rates), and column (3) adds the 1930 population category (three dummies: 1–10k, 10k–100k, 100k+) by year fixed effects to account for population trends. Finally, column (4) adds in Census region by year effects to account for regional dynamics. Standard errors are clustered at the city level. All independent variables were transformed to have a mean of zero and a standard deviation of one. The sample includes annual observations from 1924–1930 only. Consistent with the historical narrative, I find that within-city changes in debt are primarily driven by capital investment and assessed property values, with one standard deviation in both accounting for approximately 37 percent of average debt per capita ((9.86 + 6.47 + 6.42 + 24.3)/126.39).

As a proportion of total expenditure, financing costs (debt service and interest payments) deviated from their trends by the start of the Depression. Figure 2 plots the ratio of short-term loan, bond, and interest payments to non-welfare and non-capital expenditure total expenses. If the financial constraints story is relevant, we should see an increasing burden on cities to meet financial payments. This is exactly what the data reveal: on average, interest costs increased from a steady 12 percent to 14 percent, and bond repayments increased from 10 to 13 percent. This combined 5 percent increase in the proportion of payments dedicated to debt service was both economically significant - equivalent to 50 percent of the average budget for a large city health department in 1930 - and abrupt, as the figure reveals.

5 Effect of Debt on City Expenditures During the Great Depression

This section explores the first question posed in this paper: does financial leverage impact local public good provision during crises?

5.1 Measuring distress using financial leverage

I use two complementary leverage measures to proxy for financial distress at the city level. The first is the ratio of total end-of-year debt to assessed property value, which I call debt-over-value (henceforth DOV). In theory, a large DOV can impact expenditures in two ways. First, a higher DOV (but not necessarily the debt level itself) implies a higher debt principal repayment burden because cities derive their revenue primarily from property taxes. A lower tax base relative to debt suggests that municipalities may need to maintain higher than typical tax rates to repay

debt. Second, because of fluctuations in property values, the mobility of taxpayers, and limits on tax rate increases in state constitutions, property tax rates do not adjust perfectly. Sticky rates may then cause a larger share of revenue to go towards paying creditors (i.e., "fixed costs"), with less going towards other services. For the same reasons, a higher DOV may also increase the riskiness (and price) of new debt.[17] Thus, all else being equal, a city with a higher DOV should be more financially constrained when it needs to refinance existing debt or tries to borrow in the short term to smooth out revenue shocks. In practice, however, the main issue with DOV is that two equally levered cities may face vastly different financing costs at any given point in time. For example, $100 million in long-term, low-interest rate debt for one city presents less of a strain than $100 million in short-term, high-interest rate debt for another. After all, a city is in default not because it is highly levered but because it missed a payment to creditors.

To alleviate this concern, I use a second measure that directly measures a distance to default: the ratio of tax revenue to interest expense, tax-over-interest (TOI). Unlike DOV, which uses stocks of debt and assets, this is a flow measure. TOI is analogous to the interest coverage ratio in corporate finance and represents the ability of a city to repay annual interest payments. A city with a higher TOI can absorb tax losses and theoretically represents a lower risk to creditors. Thus, all else being equal, a city with a higher TOI should be less financially constrained and have more income available to spend on public goods.

I define pre-Depression DOV and TOI using data from 1930[18] as follows:

$$DOV = \log(1 + \frac{\text{Total Debt}_{1930}}{\text{Assessed Value}_{1930}}) \tag{5.1}$$

$$TOI = \log(1 + \frac{\text{Tax Revenue}_{1930}}{\text{Interest Expense}_{1930}}) \tag{5.2}$$

Figure A.9 shows the distribution of total debt/assessed value and tax revenue/interest ratios across cities in my sample in 1930. The mean debt/value ratio is 4.43%, and the mean tax/interest ratio is 19.22%, and both are highly skewed to the right, which motivates the use of the logarithm in the DOV and TOI leverage measures. Thus, if debt-related frictions do not play an important

[17] This exact measure was used in some states (in theory) to prohibit cities from issuing more debt after it reached a certain level. However, exemptions were frequently given and this cap was rarely binding. For more, see Chamberlain (1928).

[18] I use 1930 as opposed to earlier years because city fiscal years in my sample typically ended in June or July. Thus, 1930 is the last year where tax revenue on 1929 property assessments was collected. However, the results are robust to the 1929 DOV and TOI measures.

role in local public spending, we should observe no differential patterns in spending between low and high DOV cities or between low and high TOI cities during the Depression.

Figure 3 offers a first pass assessment of whether leverage is related to decreased public goods provision during the Depression. I compute the DOV in 1930 for each city and separate the sample into those that are in the top ("High Leverage") or bottom tercile ("Low Leverage"). This figure shows no differences in public service expenditure (e.g., wages paid for police, sanitation, health departments) or long-term infrastructure spending (e.g., roads) across the two groups of cities from 1925 to 1932. However, beginning in 1933, cuts to both types of public expenditure in "High Leverage" cities surpassed those in "Low Leverage" cities. By 1936, services in "High Leverage" cities were down 10 percent, but they were only down 0.1 percent in "Low Leverage" cities.

5.2 Empirical approach

To test whether the patterns in Figure 3 are causal at the city-level, I utilize the panel structure of my data and use a difference-in-difference research design:

$$y_{it} = year_t + city_i + \theta X_{it} + \beta(Post = 1) \times leverage_{30,i} + \epsilon_{it} \tag{5.3}$$

$$y_{it} = year_t + city_i + \theta X_{it} + \beta(Post = 1) \times shock_i + \epsilon_{it} \tag{5.4}$$

$$y_{it} = year_t + city_i + \theta X_{it} + \sum_{j \neq 1928} \beta_j year_{j=t} \times shock_i + \epsilon_{it} \tag{5.5}$$

The coefficient of interest is β, representing the average marginal change in spending outcomes in high vs. low leverage cities during the Depression. The variable $(Post = 1)$ takes the value of 1 for all years after 1930 and 0 before. This approach relies on two main assumptions. First, I assume that differences in public good provision would have been the same across cities with different financial leverage in the absence of the Great Depression. Second, the specific channel I propose is that financial constraints were more binding in more levered cities.

In my setting, the parallel trends assumption is that city spending in highly indebted cities would have evolved similarly to spending in lower indebted cities after 1930 had the Great Depression and the resulting credit market freeze not happened. Using an event study methodology, I present evidence that spending did not differ significantly before 1930 between high and low debt cities. I

estimate the following model:

$$y_{it} = year_t + city_i + \theta X_{it} + \sum_{j \neq 1928} \beta_j year_{j=t} \times leverage_{30,i} + \epsilon_{it} \tag{5.6}$$

Here, t denotes the year and i the city. The dependent variables are log city-level spending outcomes in per-capita terms. The fixed effect $city_i$ captures time-invariant city-specific variables that could also affect average spending levels (e.g., geographic location), while $year_t$ captures time-varying macroeconomic shocks that do not vary by city (e.g., monetary policy). The coefficients of interest are β_j, which are the coefficients on the interaction between the year indicators and 1930 leverage measures. These denote the relative change in outcomes y in each year due to leverage in 1930, conditional on the city's average spending behavior and national macroeconomic movements. The regression uses the entire sample period (1924–1938). In my preferred specification, control variables in X_{it} include a set of Census region-by-year fixed effects to account for known regional dynamics of Depression severity (Rosenbloom and Sundstrom (1997)), contemporaneous and lagged revenues to account for both the economic Depression shocks and the inter-temporal budgeting process of municipalities, and log city population in 1930 interacted with year fixed effects as well as the change in log city population between 1920 and 1930 to account for heterogeneous effects correlated with city size and past growth. I cluster all standard errors at the city level.

A second concern is that leverage could lead to changes in other aspects of city management that drive changes in spending. While I control for observable differences in characteristics, there could be unobserved, time-varying differences due to non-random assignment. For example, leverage and expenditure could have been both impacted by the political motives of mayors seeking re-election during the Depression. To deal with non-random assignment, I develop a second strategy using quasi-exogeneous bond-level shocks using a smaller sample of cities, which I describe in more detail in Section 6.

5.3 Results

5.3.1 Main results

In Table 5, panel A shows the results of the difference-in-differences regression using DOV, and panel B shows the results using TOI. The outcome variable is log total per-capita public service expenditure in columns (1)-(4) and log per-capita capital investment in column (5). The specification in column (1) includes no covariates besides year and city fixed effects. Specification

(2) controls for population trends, while specification (3) also controls revenue and lagged revenue. Finally, specification (4) adds controls for Census regions by year fixed effects. I report the coefficient of interest (post = 1 x DOV) in the first row. Columns (4) and (5) show that one standard deviation in the DOV leverage measure (0.69) is associated with a 5 log point decrease in total public service expenditure and a 12 log point decrease in capital investment, or about 20% of the drop in spending in the median city in my sample. The results using TOI provide a similar pattern, with higher TOI cities (those with more cash to cover interest expenses in 1930) reducing expenditures by *less* after 1930. Cities with a one standard deviation higher TOI (1.03) in 1930 spent roughly 4 percent more on total public service expenditures and 9 percent more on capital investment.

Figure 4 plots the β coefficients for the event study using DOV as *leverage*, while Figure 5 presents the results for the event study using TOI. Consider the event study results using the DOV leverage measure. Consistent with the financial constraints hypothesis of the Depression, places with higher initial leverage saw larger *decreases* across public goods only after 1930. However, I do not observe pre-trends in the spending or investment in the years before 1930, which provides some credence to the parallel trends assumption.

To put these numbers into context, I use a back-of-the-envelope exercise to compute an elasticity of spending cuts during the Depression to the (real) rise in leverage between 1924 and 1932. In 1924, the total debt and assessed property value per capita in my sample stood at $99 and $2,242, respectively. By 1932, per-capita debt was $147. Assuming a 2 percent real growth rate in property values, I estimate that assessments actually stood at about $2,626 per capita in 1932, which is significantly lower than the inflated value of $3,225 that was reported by cities. Thus, the average city DOV increased by 0.21 log points from roughly 1.68 to 1.89 between 1924 and 1932. Using the estimates from columns (4) and (5) in panel A of Table 5, the average leverage increase during the 1920s resulted in a 1.4 percent decrease in annual public service expenditure and a 3.7 percent decrease in annual capital investment in U.S. cities.

5.3.2 Heterogeneity

In this section, I explore whether financial constraints impacted public expenditure heterogeneously across cities of different population size, across cities with varying population growth paths before the Depression, as well as across cities in counties with varying levels of banking sector distress. I find that cities that grew more during the 1920s (before the Depression) were also the ones in which financial constraints during the Depression effected expenditure the most. Interestingly,

I further find that the effect was largest for small (below 10 thousand) and medium (10 to 100 thousand) cities. Finally, even though leveraged cities decreased their service expenditure equally across counties with varying levels of banking sector panic, I find that most of the effect on capital expenditure, on the other hand, is driven by cities in counties where the banking sector was under stress.

Table 7 reports the results of the heterogeneity analysis with respect to population and population growth. In columns (1) and (2), I recapitulate the baseline average results while all the subsequent columns report the results of the difference-in-differences specification in the specified subsamples. High 20-30 growth refers to cities with above-median 1920-1930 population growth (average = 12 percent) while Low 20-30 growth refers to those below-median. The next three sets of results in columns (7) - (12) refer to cities with specified population size as of 1930. Comparing the baseline results to those in columns (3) - (6) it appears that the effect of financial leverage on capital investment was largest in cities with high population growth (-0.22) during the 1920s as compared to those with low population growth (-0.08), suggesting that the Depression may have negatively impacted rapidly urbanizing places. Another interpretation of this finding, however, is that high growth cities could also have been the ones with little demand for new infrastructure in the 1930s (even absent the Depression) thus they naturally lowered investment in the 1930s after accumulating debt in the 1920s. In Section 5.4, I explore this demand channel in more detail and show that it cannot account for my main findings. With respect to population size, I find that financially constrained small and medium sized cities decreased service expenditure more (-0.1 to -0.07) so than constrained large cities (-0.03). Small and medium cities, unlike large ones, may suffer from greater information asymmetry problems when seeking financing, which may lead them to greater expenditure cuts if financial constraints cannot be alleviated with additional credit.

Turning to the heterogeneity analysis with respect to county-level banking conditions during the Depression, I find that the effect on capital expenditure is driven by cities in counties where the banking sector was under stress. Table 8 reports the results. In order to classify counties in terms of banking conditions, I use two data sources. First, in columns (1) - (4), I use data from the annual report of the Office of the Comptroller of Currency, which reports the total amount of loans—mortgages, business, municipal loans—of all nationally chartered banks by county in the United States. I compute the log change in the 1931 and 1929 county levels to proxy for the severity of the financial crisis on the banking sector and split counties in above and below median groups.[19]

[19] County level reporting stopped in 1932 thus the only and best predictor of actual county level credit during the Depression (absent bank level data) is the change in the 1931 and 1929 county-level values.

In columns (1) - (4), I find that the effect of financial constraints was higher for both service and investment in cities in counties with lower (more negative) loan growth, suggesting a direct effect of the banking crisis on local public goods.

5.3.3 Effects on services, cost of capital, and tax rates

In this section, I explore the effects on different types of spending and sources of funds. I show that large negative effects on infrastructure coincide with an increase in the cost of capital for highly indebted cities during the Depression. Similarly, I find that cities effectively did not (or could not) raise revenue locally: property tax rates were increased to pay off debt but not to maintain services.

The granularity of my dataset allows me to investigate the effect by type of public expenditure. Table 6 reports the estimated coefficients from Equation 5.4 using log per-capita spending on different public goods, as indicated in the column headers. The largest effect is on capital expenditure (infrastructure) listed in column (1), with one standard deviation in DOV resulting in a 12 log point annual decrease (0.69 x 0.17). The results in columns (2)–(6) portray a range of negative effects on governing expenses (city administration), sanitation (inspection and upkeep of sewer and trash disposal systems), health departments, maintenance of roads, and police and firefighting ("Protection"). Of these, I observe a large effect on police and firefighting spending (11 log point decrease) and relatively smaller effects on the other categories of between 1 and 5 log points.

Reassuringly, I do not find that city welfare spending was impacted by debt-driven constraints. Since welfare expenditure was dictated mainly by the amount of federal transfers to each city from New Deal programs and outside of local government control, the effect of leverage on welfare should, in theory, be negligible. The results in column (7) seem to suggest that the limited stimulus from federal programs for the purpose of welfare (work relief and cash relief) was not allocated on the basis of financial constraints, which is consistent with other evidence that New Deal spending may have been distributed with other motives, such as political, in mind (Fishback et al. (2003)). On the other hand, services that were controlled by local mayors and councils, such as infrastructure, police, health, and sanitation, could be (and were) scaled back due to financial constraints.

Returning to the large effects on infrastructure, I now show that highly indebted cities also saw their cost of capital increase relative to other cities, which may help explain why their investment was disproportionately lower. I examine the effect of leverage on a city's cost of capital by using

credit ratings as a proxy. Credit ratings, now and historically, correlate closely with the cost of issuing municipal bonds. I collect annual ratings from the *Moody's Manual of Governments* from 1929 to 1939. The ratings range from AAA (best) to CA (worst), which I transform into numerical values by assigning 10 to AAA and subtracting 1 for each level below AAA (AA = 9, A = 8, etc.). Figure 6 plots the average Moody's rating for the first (low leverage) and third (high leverage) terciles of DOV. The vast majority of cities in both groups were AAA rated before the Depression, but starting in 1933 there is a divergence between the two groups. By 1936, low leverage cities were 0.98 ratings above high leverage ones. Systematically, Table 9 reports the results of a difference-in-differences specification, with $Post = 0$ denoting 1929 and $Post = 1$ all the years after 1929. As before, I control for revenue, region fixed effects, and population dynamics. I find that highly levered cities were, on average, rated 0.36 levels below similar cities that went into the Depression with lower leverage. This result provides some evidence that the cost of borrowing and issuing bonds was higher during the Depression for higher levered cities, which may help explain why these cities spent less on capital investment.

Another option for cities was to raise funds internally by raising tax rates on residents to smooth out these financing shocks. However, I find that cities could not generate tax revenue internally by raising statutory tax rates. I investigate the response of property tax rates in more vs. less leveraged cities using detailed California tax rate data, which splits property tax rates for general vs. bond-repayment purposes. For example, I observe that a 2.2% total tax rate in Los Angeles in 1935 was split between a 2% rate for general purposes - wages for city workers and services - and a 0.2% rate earmarked for debt repayment. Theoretically, financially constrained cities could raise property tax rates to raise revenue and continue providing public services. Practically, however, the 1930s was a time of not only delinquency but also tax revolt (Siodla (2020)). Table 9 shows the results on total tax rates in columns (2) and (5) and on the debt-repayment tax rate in columns (3) and (6) using DOV and TOI as leverage, respectively. The coefficients for total tax rates are not significant and close to zero, while those for bond repayment are large. A standard deviation in DOV coincided with a 0.17 percentage point increase in bond-repayment property tax rate, compared to an average total tax rate of 2.8 percent and average bond-only rate of 1.3 percent. All in all, the results show that it was more expensive for constrained cities to access public credit markets, and that they did not, or could not, raise revenue through higher tax rates on local property.

5.4 Robustness

In this section, I show that my main results are robust to alternative, non-debt driven explanations and alternative measurement. I consider, and reject, the hypothesis that the demand for public spending (especially infrastructure) between high and low leverage cities - and not the supply - was the main driver of the observed curtailment of public goods. I also show that potentially time-varying confounding variables related to city-bank connections - such as access to sophisticated bankers or wealthy individuals in banking hubs - cannot account for my main results. I further show that the main results are robust to the inclusion of time-varying variables that may reflect political motives, such as form of government (e.g., mayor vs council) or election cycle length. Finally, to address concerns regarding the validity of the measurement of Depression severity using tax revenue, I find that the inclusion of an alternative measurement using changes in retail sales produces quantitatively similar results.

5.4.1 Demand channel

The difference I find between indebted cities and less indebted ones is surely driven by a combination of supply side debt-financing constraints and demand side ones. This is especially true in the case of infrastructure investment: because they had already completed these investments in prior years, it was easier for highly indebted cities to cut infrastructure spending when their fiscal situation worsened. That is, high leverage cities were also the ones who did not plan on investing more in the 1930s *regardless* of the Depression and the ensuing cuts to public spending.[20]

To investigate the quantitative significance of the demand channel, I classify cities into "high" and "low" infrastructure demand cities based on pre-1930 bond issuance behavior and show that the main results on the supply side remain significant once low demand cities are excluded from the analysis. Using the bond-level data obtained from $Moody's$, I proxy for future demand in two different ways: (1) the share of bonds issued in 1927–1929 to the total amount issued[21] and (2) the weighted average age of each city's bond portfolio, weighed by the face value of each bond.[22] Conceptually, I am assuming that cities with a large value of (1) are also cities that have newer infrastructure and in which demand in the 1930s would be low. Likewise, cities with a low value of

[20] Even though Figure 4 shows no significant pre-trends between high and low leverage cities immediately before the Depression, it is still plausible that the same trends in infrastructure spending fulfill different demands in these cities, which would also affect future investment decisions.

[21] This share is computed based on the face–not outstanding–value of all bonds listed for each city in 1929.

[22] Bond age is defined as 1929 minus the year the bond was issued.

(2) are cities that have more recently invested and would hypothetically not need new investment in the Depression years.

Table 10 reports the estimation results of Equation 5.4 when various groups of cities are excluded from the analysis. In columns (1) and (6), I present the main result for the sample of cities for which data in $Moody's$ exists. In columns (2) and (5), I exclude the cities in the highest quartile based on their share of outstanding bonds issued between 1927–1929 (28 percent). Finally, in columns (3) and (6), I exclude the lowest quartile of bond portfolio age (6 years). The coefficient remains significant and decreases only slightly in magnitude from -0.31 to -0.28 - and -0.20 using DOV as leverage proxy and from 0.33 to 0.26 and 0.22 using TOI.

5.4.2 Alternative measures of Depression severity, city-bank connections, and political motives

I next perform several other robustness checks and I present the results in Table 11. First, another concern is that the effect on public expenditure is instead driven by varying local economic conditions that are not adequately captured city revenue controls in my preferred specification. Using the log change in county retail sales per capita between 1929 and 1933 interacted by year fixed effects as a proxy for Depression severity (Fishback et al. (2003)) as an additional control, I find that the main estimated coefficients remain stable (column (2)). The coefficient estimate for service expenditure (Panel A) is unchanged and the one for capital expenditure is only slightly larger in magnitude than the main results, and not statistically different.

The second alternative explanation is that the effect of leverage is confounded by the omitted impact of access to financial institutions or wealthy individuals that are also correlated with leverage. To address this concern, I compare cities with and without connections to banks in the important financial centers at the time: New York City, Chicago, and Cleveland. To do so, I collect data on the location of the "disbursing agent" for interest listed under each city in the $Moody's\ Manual$ as of 1929. Typically, these agents are banking institutions or brokers that act as underwriters for a city's bond issue or have an agreement with a city to pay interest coupons to local investors. While the identity of the institution is not always listed, the city in which they are located is always listed. I create a binary variable that takes the value of 1 if a city lists an agent in the three money centers listed above and 0 otherwise. In total, the data on 477 cities is available, and 33% of these are connected to a bank in a financial hub. Column (4) presents the results when the specifications include bank connections in a money center by year fixed effects while Column (3) is the baseline

effect in this subsample of cities. Again, the results do not significantly change.

Lastly, I investigate whether the differences in the political landscape across cities confounds the main effect of debt-driven financial constraints. For example, the political motives of officials in cities with powerful mayors, as opposed to city council managers, could be an equally if not more so important determinant of public good provision once a financial crisis hits. I collect data on city government type—mayor-council, council-manger, commission, and town meeting—as well as the length of each election cycle for all cities over 5 thousand in population from the 1938 edition of the *The Municipal Year Book*, a trade publications for city officials. In all, roughly 60 percent of the cities in my sample have election cycles longer than 3 years and the remaining have a 2 or 3 year cycle. In Columns (5) and (6), I control flexibly for the dynamic impact of government type and election cycle using year by type fixed effects and find that the effect of debt remains significant.

6 Identifying Financial Constraints Using Shocks from Bond Repayment

So far, I have shown that local public good provision in U.S. cities was lower during the Great Depression in financially leveraged cities. In this section, I unpack financial leverage into short-run vs. long-run cashflow shocks and present direct evidence that the inability to pay obligations during the Depression is the main mechanism through which adjustments to local public expenditure were made.

To isolate the impact of debt-driven financial constraints, I take advantage of the quasi-exogeneous maturity structure of local debt: cities issue long term bonds that expire at different points in time (e.g., 5, 10, 30, and 50 years). This phenomenon not only permits a deconstruction of financial leverage into cashflow shocks at different points in time, it also provides two advantages for identification. First, while debt issuance around 1930 may certainly be endogenous to outcome variables in the 1930s, debt issued 10 or 20 years prior to the Depression is plausibly less so. For example, a city planning to refinance a 20 year bond issued in 1911 would find it difficult to do so in 1931 with the financial markets in turmoil. Second, the choice of bond duration is related to market norms and the quantity borrowed and is typically determined at the state or national level, which alleviates local endogeneity concerns (Chamberlain (1928)).

I utilize the difference in the maturity structure of each city's bond portfolio to identify plausible exogenous shocks by merging my city-level panel of local public good provision with novel bond-level data. Specifically, I collect the "Schedule of Bonded Debt" information from *Moody's Manual of Governments* in 1929. For each bond listed, the data includes the year the bond was

issued, the year it matures, the amount outstanding in 1929, the interest rate, and the bond's purpose (e.g., road construction). In total, the *Manual* contains information on 29,366 bonds across 316 cities in my sample. Summary statistics are in Table A1.

Importantly, the bond level data is consistent with city-level debt reported on balance sheets from official government sources, both in levels and cross-sectional correlation. I aggregate the amount outstanding of bonds listed in $Moody's$ to the city level and compare the totals to the balance sheet data in 1929. I find a remarkably high correlation between the two sources across cities in sample ($\rho = 0.9$) as well as average coverage of 85 to 95 percent. Similarly, I estimate the expected interest payments and aggregate to the city level and find a similarly high correlation coefficient. More information regarding validating bond-level data appears in Appendix D.

6.1 Constructing shocks and empirical strategy

The advantage of bond-level data is that I can produce forward-looking estimates of how much debt needs to be repaid during a "bad state" in the future, which will serve as my proxy for debt-driven, short-run financial constraints. Concretely, I define a "shock" measure as the fraction of total bonded debt that matures in 1930–1935:

$$shock_{30,j} = \frac{\sum_{t=1930}^{1935} \sum_{\forall i \in j} repay_{i,t}}{\text{Total Debt}_{29}} \tag{6.1}$$

where $repay_{i,t}$ is the estimated repayment for bond i for city j in year t.

To illustrate the identifying variation of this strategy, Figure 7 plots the average repayment over time by quartile of $shock_{30,j}$. Cities in the largest quartile were obligated to repay between 5 and 15 percent of their debt per year in the early 1930s and less in the 1940s (solid red line), while those least affected maintained a steady 3–4 percent per year throughout 1930–1950. In essence, the empirical strategy compares outcomes in cities that had maturing schemes that resembled the red and orange lines (concentrated during the Depression) with those that resembled the green lines (evenly distributed).

One remaining concern is that this shock measure is correlated with omitted city variables that drive local public good provision and are unrelated to financial constraints. For example, larger or richer cities potentially have access to more sophisticated bankers who can endogenously select a constant repayment scheme and re-negotiate repayment during the Depression. To help alleviate this concern, I perform a balance test on 1929 city characteristics based on below and above median

repayment shock. The results are displayed in Table 12. Cities in the above-median group were scheduled to repay 51 percent, on average, of their outstanding debt during 1930–35, as compared to only 28 percent in the below-median group. Importantly, however, cities did not significantly differ on total revenue collected, total assessed property value, total public service expenditure, or total capital investment in 1929.

To isolate the plausibly exogenous portion of leverage, I compute the following:

$$\widehat{DOV} = \log(1 + \frac{\text{Total Debt}}{\text{Assessed Value}} \times shock_{30,j}) \tag{6.2}$$

$$\widehat{TOI} = \log(1 + \frac{\text{Tax Revenue}}{\frac{1}{6}\sum_{t=1930}^{1935} \text{Est. Interest Expense}_t}) \tag{6.3}$$

where $shock_{30,j}$ is defined in Equation 6.1 and the estimated interest expense (Est. Interest Expense$_t$) is computed analogously by aggregating interest payment by bond/year for each city. This leads to the following modification in my main specification:

$$y_{it} = year_t + city_i + \theta X_{it} + \beta(Post = 1) \times \widehat{leverage}_{i,30} + \epsilon_{it} \tag{6.4}$$

As before, the coefficient of interest is β, which represents the marginal change in spending outcomes in high vs. low "shocked" cities as proxied by either the value of bonds maturing or the total forecasted interest payment during the Depression.

6.2 Results

Table 13 presents the main results of the causal effect of short-run debt repayment on total public spending at the city level during the Depression. The results show that financial constraints resulted in large and significant expenditure cuts. The outcome variables are total capital outlays (construction) and all non-welfare, non-interest expenditures. The specification in column (1) includes no covariates besides year and city fixed effects. Specification (2) controls for contemporaneous population size and (3) additionally controls for contemporaneous and lagged revenue. Specification (4) adds controls for Census regions by year fixed effects, and specification (5) reports the results using the non-shocked leverage measures. Columns (1) through (5) report the effect on total capital investment and columns (6) – (7) report the effect on total non-interest, non-welfare expenditure.

The coefficient of interest (post = 1 x $\widehat{DOV}$) is reported in the first row. For comparison, I perform the same regression but with the non-shocked leverage measure as in Section 5, and the estimate for this regression is reported in the second row. Consider the estimates in columns (4) and (5): a one standard deviation increase in $\widehat{DOV}$ results in a 20 log point lower capital investment and only a 2 log point lower total public service expenditure during the Depression, which represents roughly 30 and 10 percent of the average decline, respectively. This is slightly lower than the estimate using DOV: one standard deviation of the non-shocked measures results in only about a 24 log point decrease in capital outlays.

I find quantitatively similar results using the shocked flow measure, $\widehat{TOV}$. Again, consider the estimates in columns (4) and (5) in panel B: a one standard deviation increase in $\widehat{TOI}$ results in 18.3 log points higher in capital outlays and 2.6 log points higher in total non-welfare expenditures during the Depression, which represents roughly 27 and 16 percent of the average decline, respectively.

In summary, the findings in this and the previous section suggest that debt-driven financial constraints played a significant role in local public good provision during the Depression. I find cities that entered the 1930s with more leverage not only decreased their infrastructure, health, and protection spending, but they also subsequently faced larger borrowing costs and a decreased ability to raise taxes locally for non-debt purposes. These results are not driven by demand or varying access to credit due to banking panics or city-bank connections, and they are not sensitive to different measurements of Depression severity. Using bond-level data, I decomposed the financial leverage channel into short-run repayment shocks and showed that a city's inability to meet financial obligations ultimately spilled over in the form of expenditure cuts on other local public goods.

7 Urban Growth and Financial Constraints

Inevitably, the debt-induced expenditure cuts shown in the previous section imposed costs on local communities. For example, research has shown that local infrastructure spending during later decades helped stimulate regional economies (Kline and Moretti (2014)) and that sanitation spending reduced waterborne disease rates between 1902 and 1929 (Cain and Rotella (2001)). It stands to reason that smaller police budgets may have encouraged more criminal activity, that lower education spending may have hampered human capital formation, or that cutting infrastructure made some cities less appealing for firms, whether it be via higher transportation costs or less access to reliable electricity. In this section, I empirically investigate whether a short-run shock to local public goods can have adverse long-run consequences for urban growth by studying the

migration response of households between 1930 and 1940. Migration is an important outcome to study, as models in economic geography (Roback (1982)) have emphasized that the spatial allocation of factors of production, prices, and growth depend on migration elasticity assumptions.

I merge city and county-level data with the person-level Census records based on each individual's 1930 location. To measure the severity of the Depression at the local labor market level, I use log change in per-capita county retail sales between 1933 and 1929 as the main local determinant of migration, in addition to the person-level demographics (described below) that have been shown to affect migration decisions. To test whether local shocks to public expenditure further push people out of certain cities above and beyond the standard explanatory variables, I consider city leverage - DOV, introduced in Section 5 - in my empirical analysis. Recall that DOV is the leverage measure of city financial constraints, which I argued was an important determinant of local public good provision during the Depression.

To proxy for the costs of internal migration emphasized by the existing literature, I use the following demographic variables in my analysis: presence of children in the household, marriage status, immigration status, age, ownership of dwelling, and occupational income score. I define a household with children as a married household with at least 3 people. Theoretically, I expect the costs of migration to be higher for individuals with children (direct moving costs and education search costs) and for older individuals or for those who own a house (transaction costs). On the other hand, people in occupations with a higher income score may have more resources to cover costs of moving but also have larger opportunity costs and foregone wages such that the total cost of migration is ambiguous. Table 2 presents the summary statistics. Overall, 39 percent of my sample moved locations between 1930 and 1940: 19 percent moved from city to city, while 20 percent moved from a city to a rural area. The median age is 33 and about 49 percent have children. The median distance traveled, conditional on moving, is 11 miles, and the average is 187 miles.

Empirically, I estimate linear probability models with demographic, economic, and financial explanatory variables to test whether location specific shocks correlate with individual-level migration decisions during the Great Depression:

$$\mathbb{1}(migrate)_{i(j)} = HighLev_j + GDSevere_j + HighLev_j \times GDSevere_j + X_i^1 + X_j^2 + \epsilon_{ij} \tag{7.1}$$

where $\mathbb{1}(migrate)_{i(j)}$ is a binary variable for individual i who lives in city j indicating migration (i.e., moving to a different city of residence) between 1930–1940, $HighLev_j$ is a binary variable

taking the value of 1 if leverage in city j was above the median value as proxied by DOV, and $GDSevere_j$ is a binary variable taking the value of 1 if the county retail sales growth in city j's county was below the median value. I cluster standard errors at the county level.[23]

Control variables are at the person (X_i^1) and city (X_j^2) level as of 1930. At the person level, I control for income (occupational income score) and binary variables indicating whether the individual lived in a household with kids, immigrant status (3 levels), marital status, and home ownership. I further control for age with four age bins. At the city level, I control for a city's total population in 1930, which is a known pull factor in standard gravity equation models of migration, and the share of the labor force employed in manufacturing industries, which has been shown to be a push factor during economic recessions when demand for durable consumption drops. Regionally, I include Census region fixed effects to account for the regional disparities in Depression severity as well as each city's proximity to a better off county to account for the availability of outside options for households. I compute the latter by finding the best available county in terms of both retail sales growth and city leverage within 50 miles of each individual's city in 1930. Table 2 reports the summary statistics.

Table 14 reports the results of estimating Equation 7.1 by ordinary least squares. The outcome variable is an indicator of whether a person moved between 1930 and 1940. In column (1), I include no controls or interaction terms. In columns (2) through (6), I add an interaction term between the two binary shocks, as well as person-level, city-level, and regional controls, respectively. The results in column (1) seem to suggest that neither Depression severity nor public good provision influenced migration decisions independently, in contrast with predictions of Tiebout sorting. However, the interaction between expenditure cuts and Depression severity is positively related to out-migration. Considering the estimates in columns (2) through (6), I find that the effect of public good provision on migration is significantly more pronounced in counties that experienced a severe Depression, even after controlling for person, city, and regional characteristics. The estimates indicate that individuals were approximately 7.8 percentage points more likely to migrate away from a city with a severe Depression if local public spending was also curtailed. In column (7), I re-estimate my preferred specification in column (6) but using the quasi-exogeneous leverage measure introduced in Section 6. The results are quantitatively very similar.

Next, I explore whether the migration result varies for various groups. Table 15 shows the

[23]Overall, the correlation between $HighLev_j$ and $GDSevere_j$ is significant but not prohibitively so. The sample contains roughly 825,000 individuals in low-leverage and low-severity counties, 700,000 to 900,000 individuals in mixed counties, and 512,000 individuals in high-leverage and high-severe counties.

regressions results for 6 subsamples: poor (occupational income score in lowest tercile) in column (1), rich (occupational income score in highest tercile) in column (2), short movers (non-movers and movers of less than 50 miles) in column (3), long movers (non-movers and movers of greater than 200 miles) in column (4), local public employees in column (5), and state and federal employees in column (6). In general, I do not find heterogeneous effects on the basis of occupational income. I do, however, find that the effect is more pronounced for those who decided to move a short distance away (0.062 vs 0.039). This result is consistent with an information mechanism where individuals are more knowledgeable of local, and not distant, opportunities. The estimates in columns (5) and (6) indicate that local public employees - the group most directly affected by expenditure cuts - had a higher response (though not a statistically significant one) than other public employees.

8 Conclusion

How economic crises affect the level or composition of local public goods, and in turn affect real outcomes such as migration, is an urgent and important question for policymakers. Recent empirical research has shown that financial frictions, especially from debt overhang, can result in expenditure cuts. In this paper, I extend this literature by studying the effect of financial leverage of U.S. cities on local public good provision and estimating the impact of financial shocks on migration patterns during the Great Depression.

Using a novel dataset of local public good provision and bond-issuance from a large sample of cities, I find that financial constraints played an important role in hindering local public expenditure during the Depression. I identify causal effects first by using a difference-in-difference analysis and second by isolating quasi-exogenous financial shocks from bonds becoming due. I then extend my analysis by studying whether individuals migrated based on local fiscal policy and find pronounced out-migration from financially levered cities in economically distressed counties.

This paper shows that debt-driven financial constraints can induce significant public expenditure cuts during a crisis, but it does not take a stand on the welfare implications of local public debt issuance. In order to conduct such an analysis in the setting of this paper, one would need to measure the benefits of the infrastructure boom of the 1920s (e.g., life-expectancy improvements due to sanitation systems, human capital returns due to increased schooling) and compare it to the costs of foregone urban growth in the Depression, which is outside of this paper's scope. Rather, the goal here was to establish the consequences of financial constraints at a time of systemic municipal financial distress where identification challenges arising from fiscal transfers from

higher levels of government did not exist to the same extent as they do in the modern-day U.S. Importantly, however, the historical context does not limit the applicability of my results, as many countries around the world operate under fiscally decentralized systems without significant federal government transfers today.

There are at least three interesting extensions of this paper that I leave for future research. The first follow-up research question is whether financial constraints impacted other local public spending, such as independent school districts. School enrollment, especially in secondary schools, expanded significantly in the first half of the 20th century. How financing constraints impacted human capital accumulation during this time period certainly warrants a closer examination (e.g., see Janas (2021)). Second, due to data limitations, this paper does not address the political economy aspect of local public good provision or how local politics interact with financial constraints. Should local political data become available, one interesting exercise would be to measure how the leverage effect is impacted by (or impacts) re-election campaigns or the strength of public employee unions. Lastly, I find evidence that internal migration and local public good provision may be linked. Studying whether and how internal migration drove disparities in long-run regional growth throughout the 20th century may yield important policy implications for today.

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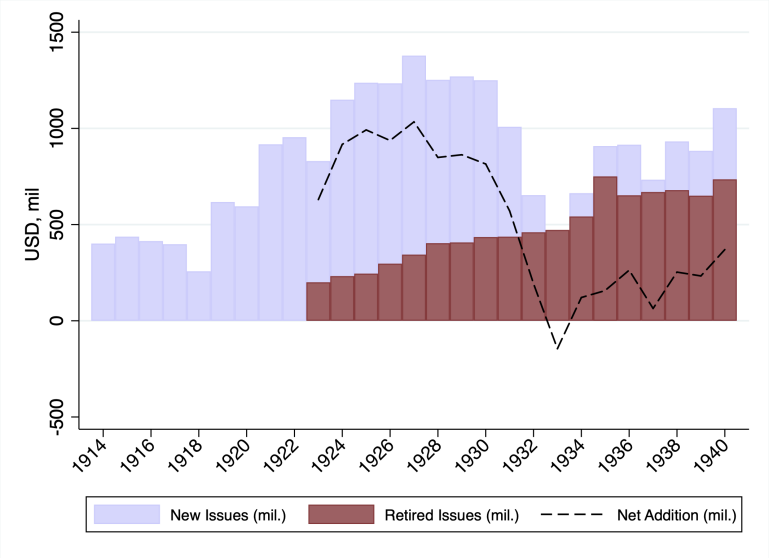
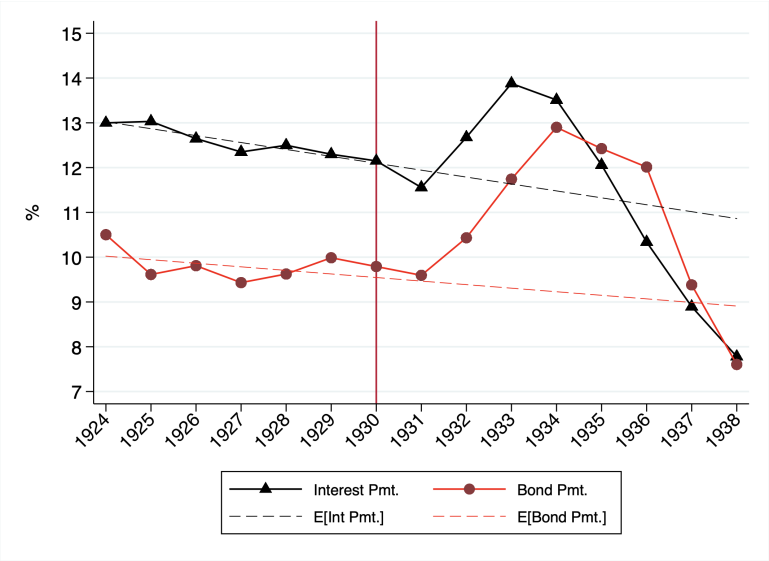
Figure 1: Municipal debt sales and retirements



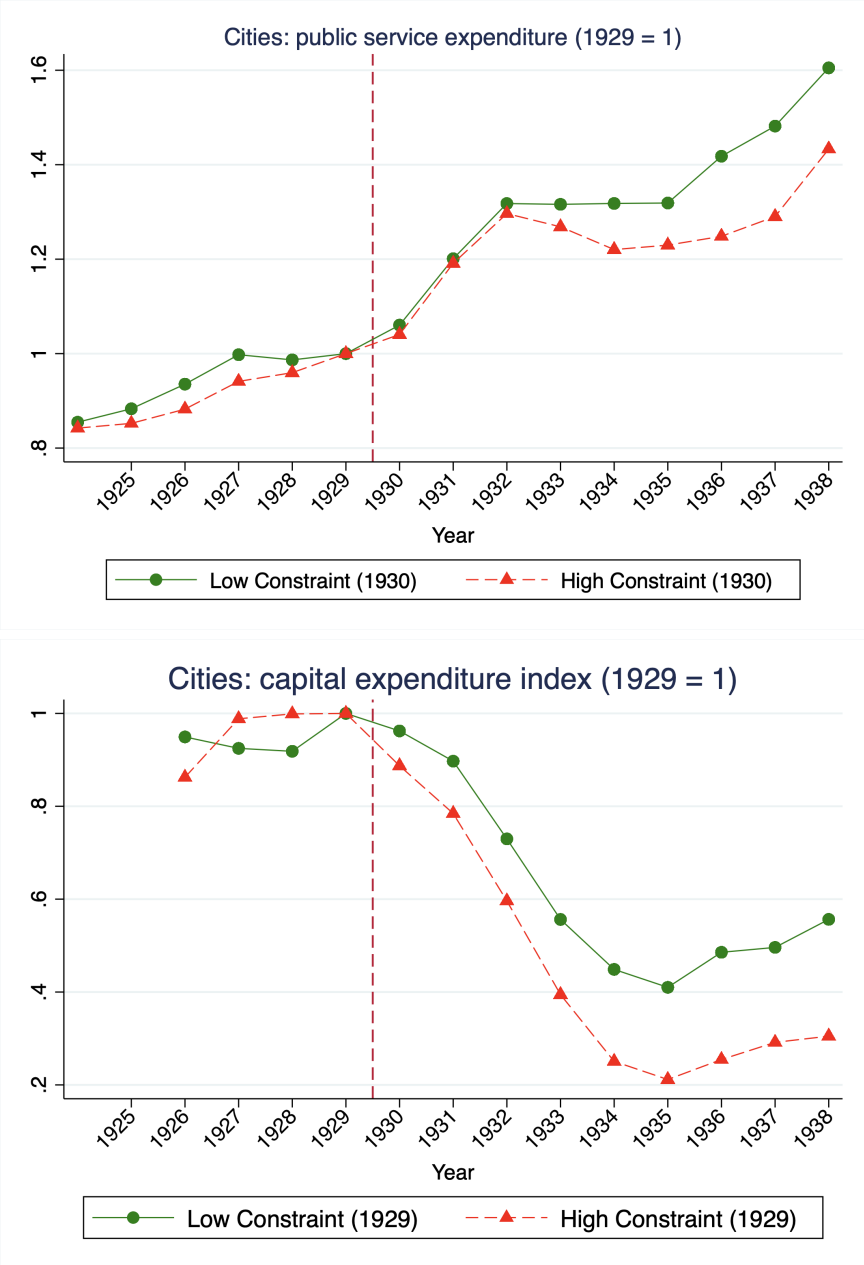
Note: This figure plots the volume of municipal bond sales and retirement as reported by Hillhouse (1936) in Tables 1 and 5. The original source of the data is the State and Municipal Compendium (June issue of the *Commercial and Financial Chronicle*). The figures for retired issues were not compiled before 1923. Net addition (black dashed line) is defined as new issues minus retired issues. Values are nominal.

Figure 2: Financing costs in relation to public good expenditure



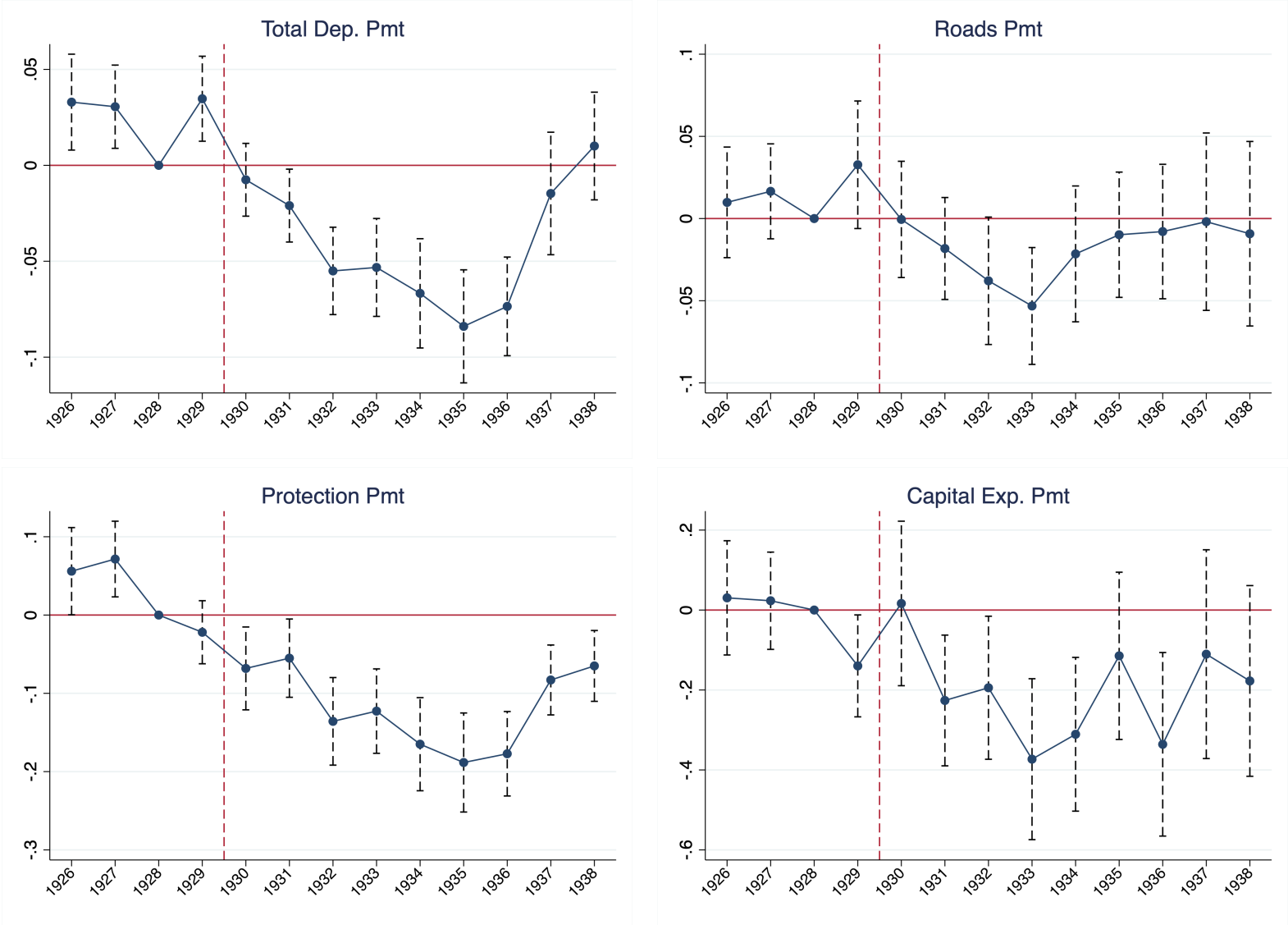
Note: This figure plots the average ratio of interest payments (black line) and long-term bond payments (red line) to total non-welfare and non-debt payments across a balanced panel of cities. Dashed lines denote the best linear fit from 1924–1930 extrapolated to 1931–1938. Both measures were trimmed at the 2-98 percentiles to reduce the influence of outliers.

Figure 3: Leverage and local public goods



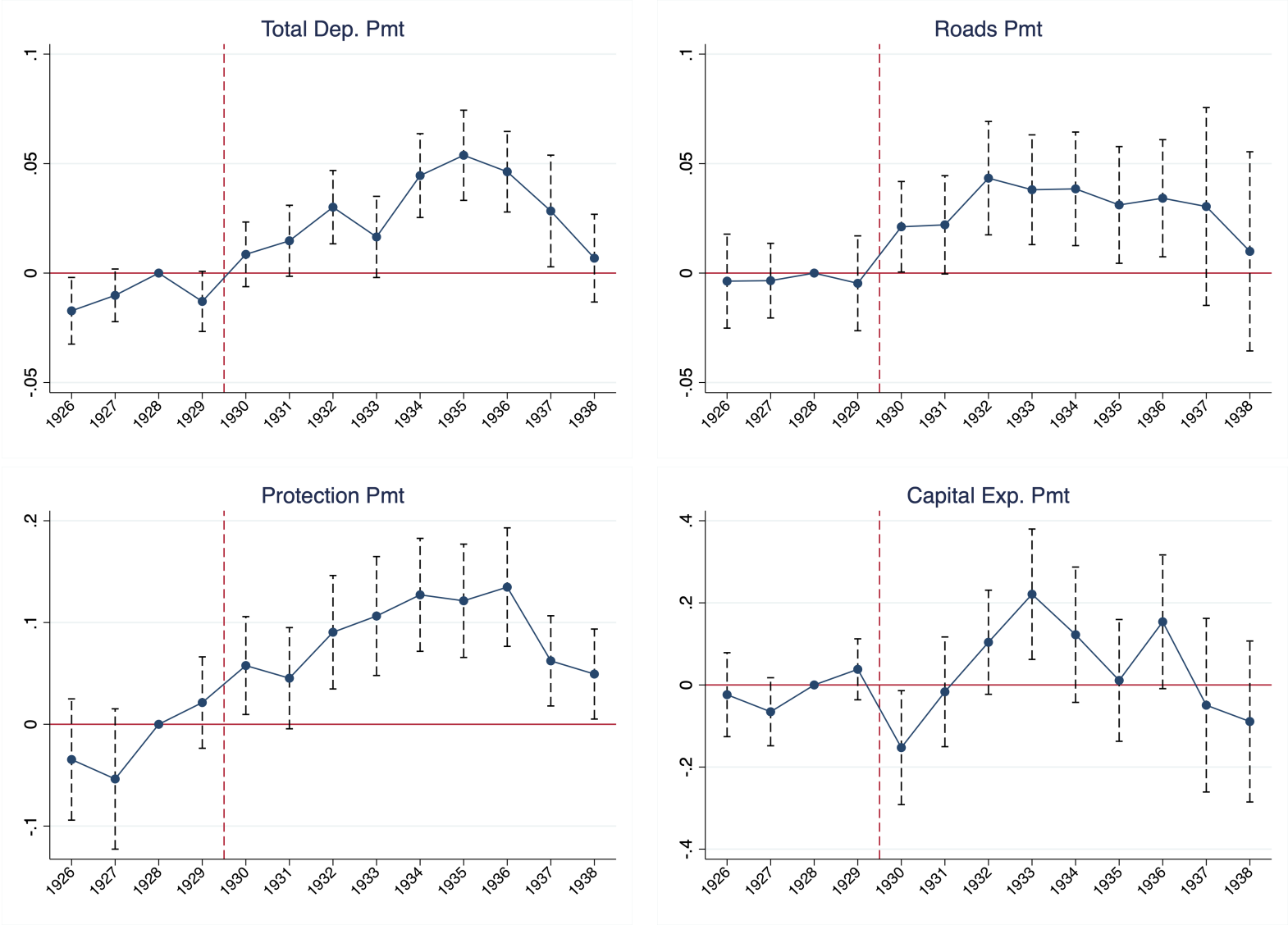
Note: This figure plots the average total public service expenditure (left) and capital expenditure (right) in cities by leverage."Low Constraint" is defined as the first tercile of debt/property value in 1930 and "High Constraint" denotes the third tercile. All values are deflated using the CPI and normalized to 1 in 1929.

Figure 4: Event study using DOV



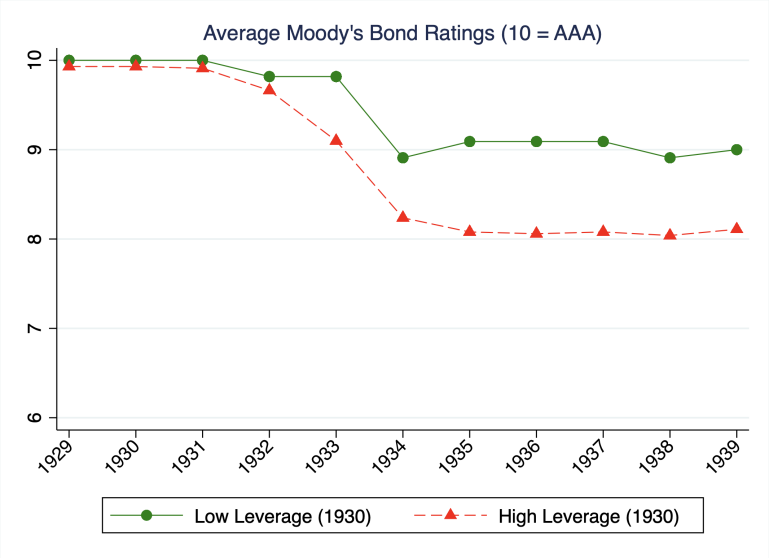
Note: This figure shows the estimated coefficient on $year_{j=t} \times leverage_{30,i}$ in Equation 5.6 using DOV as the leverage measure. Total Dep. Pmt refers to total public service expenditure (i.e., total payments not including capital expenditure or financing costs). Roads Pmt. refers to all expenditure for the maintenance of public roads and highways. Protection is police, jails, and firefighting costs. Capital Exp. Pmt is capital expenditure costs for construction projects. All standard errors are clustered at the city level. Ninety percent confidence intervals are denoted by dashed lines. The omitted year and year-post interaction is 1928. The dashed red line denotes the official start of the Great Depression in the U.S.

Figure 5: Event study using TOI



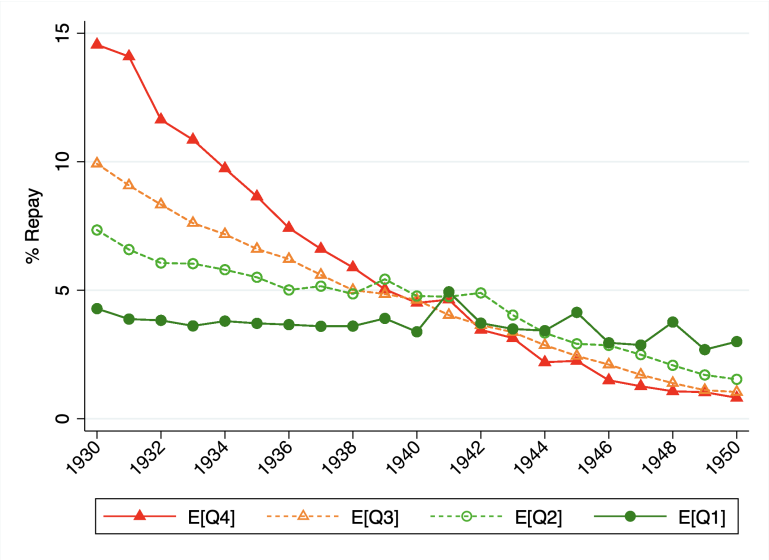
Note: This figure shows the estimated coefficient on $year_{j=t} \times leverage_{30,i}$ in Equation 5.6 using TOI as the leverage measure. Total Dep. Pmt refers to total public service expenditure (i.e., total payments not including capital expenditure or financing costs). Roads Pmt. refers to all expenditure for the maintenance of public roads and highways. Protection is police, jails, and firefighting costs. Capital Exp. Pmt is capital expenditure costs for construction projects. All standard errors are clustered at the city level. Ninety percent confidence intervals denoted by dashed lines. The omitted year and year-post interaction is 1928. The dashed red line denotes the official start of the Great Depression in the U.S.

Figure 6: Moody's ratings



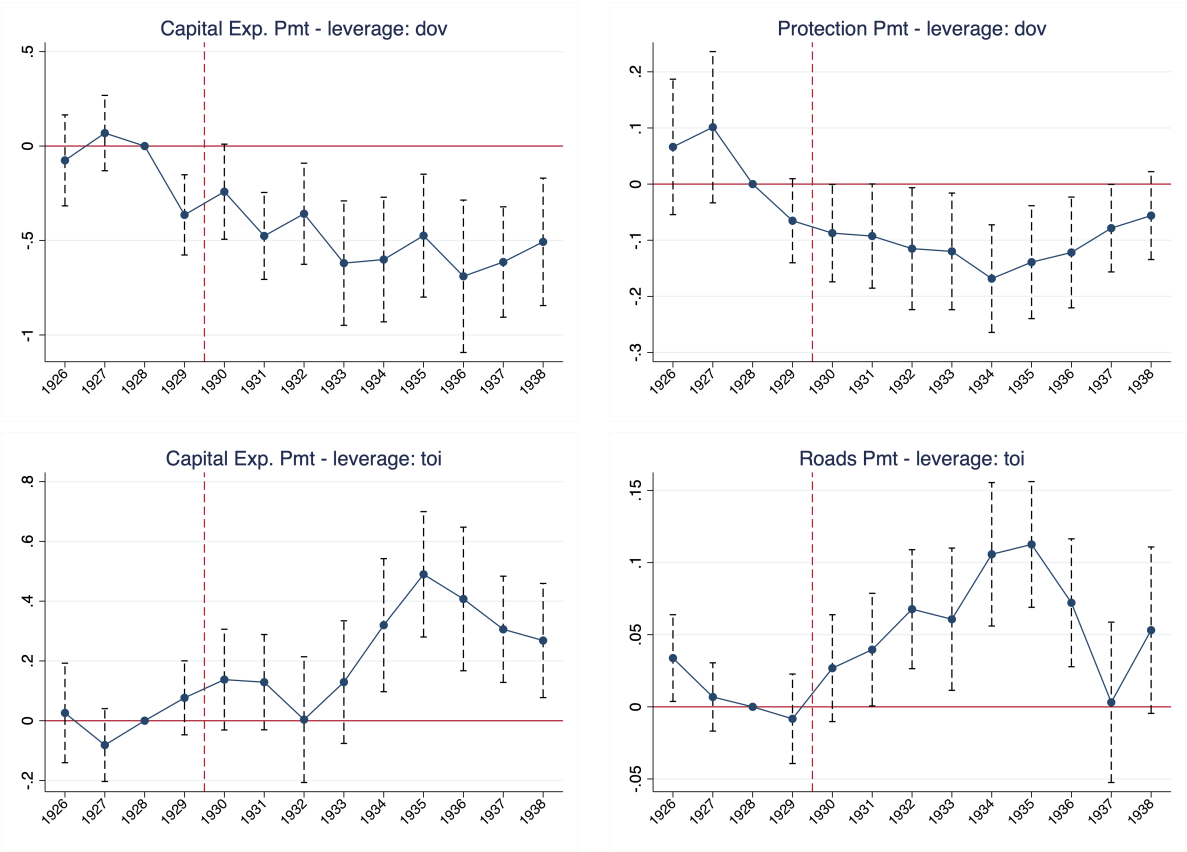
Note: This figure plots the average Moody's Bond rating of cities by leverage. Low leverage is defined as the first tercile of DOV in 1930 and high leverage is denoted by the third tercile. The sample includes 189 cities with complete data from 1929 to 1940. A rating of AAA is assigned the value of 10, AA is 9, and so on.

Figure 7: Annual repayment based on repayment shock quartile



Note: This figure shows the average annual repayment of bonds across 1930–1935 city repayment quartiles. Repayment quartiles are static by city. For example, the solid red line with triangle markers shows the average percentage of bonds that were contractually obligated to be repaid in each year for those cities in the largest repayment quartile, while the solid green line with circle markers shows it for those in the lowest repayment quartile.

Figure 8: Event study using bond repayment leverage measures



Note: This figure shows the estimated coefficient on $year_{j=t} \times leverage_{30,i}$ in Equation 5.6 using the shocked meaasures of leverage as in Equation 6.2. Capital Exp. Pmt is capital expenditure costs for construction projects. Roads Pmt. refers to all expenditure for the maintenance of public roads and highways. Protection is police, jails, and firefighting costs. All standard errors are clustered at the city level. Ninety percent confidence intervals denoted by dashed lines. The omitted year and year-post interaction is 1928. The dashed red line denotes the official start of the Great Depression in the U.S.

Table 1: Summary statistics

Panel A: City Level Revenue and Expenditure, 1924–1938

	N	Mean	SD	Median	25 pct	75 pct
Population	10,507	61.32	318	9	5	23
Total Revenue (Rev.)	10,507	64.24	51	48	31	86
Tax Rev.	10,507	48.69	40	35	22	67
License Rev.	10,507	1.94	3	1	0	3
Grants	10,226	5.04	9	1	0	6
Other Rev.	10,507	6.38	10	4	1	8
Utility Rev.	9,405	15.97	42	10	2	18
Department Earnings Rev.	9,078	2.69	4	1	0	4
Government Pmt	10,507	4.89	4	4	3	6
Health Pmt	10,507	2.23	3	1	0	3
Roads Pmt	9,078	9.24	6	8	6	11
Protection Pmt	10,507	10.11	9	9	5	13
Welfare Pmt	8,454	5.95	11	1	0	7
Recreation Pmt	9,589	1.62	2	1	0	2
Education Pmt	9,078	14.67	18	2	0	31
Other Pmt	10,507	3.09	5	1	0	5
Utility Pmt	9,405	13.13	62	7	3	14
Interest Pmt	10,507	5.98	14	4	2	7
Capital Exp. Pmt	10,507	13.19	26	5	1	17
Debt Pmt	9,127	27.06	41	9	3	35
Debt - Total	10,451	127.62	298	78	34	155
Debt - Bond	10,451	81.80	274	51	20	99
Assessed Value	10,200	2763.91	3277	2293	1670	3079
Tax Rate - Total	5,294	20.00	27	16	13	21
Tax Rate - Bond Rpmt	3,325	3.81	3	3	1	6
Pop under 10k	10,507	0.52	0	1	0	1
Pop 10-100k	10,507	0.35	0	0	0	1
Pop 100k+	10,507	0.13	0	0	0	0

Panel B: Other City and County Data (Static)

	N	Mean	SD	Median	25 pct	75 pct
Δ Retail Sales, 1929-33	819	-0.41	0.13	-0.41	-0.48	-0.31
Connected to NYC bank	477	0.30	0.46	0.00	0.00	1.00
Connected to CLE bank	477	0.03	0.17	0.00	0.00	0.00
Connected to CHI bank	477	0.03	0.17	0.00	0.00	0.00
Connected to NYC, CLE, or CHI	477	0.33	0.47	0.00	0.00	1.00
Sus. Bank Deposits (>0)	908	0.78	0.41	1.00	1.00	1.00
I(Short-term Loans)	389	0.74	0.44	1.00	0.00	1.00
Short-term Loans/Total Debt (1930)	389	0.14	0.20	0.07	0.00	0.19
Moody's Bond rating (1930) AAA = 10	477	9.74	1.14	10.00	10.00	10.00

Note: Panel A: Summary data are given for all observations across cities in the period 1924–1938. Population is in thousands. Revenues (Rev.), expenditures (Pmt.), and assessed property values are in per-capita dollars deflated by the CPI to 1967. Tax Rate is the property tax rate in dollars per one thousand of assessed property value (20 = 2 percent). The sample consists of all cities with at least 8 years of data in the sample time period. Variables across data sources were standardized such that each variable in the final dataset consists of spending and revenue on similar, if not exact, categories. Some variables were only available for the majority of, but not all, cities. Panel B: Change in county retail sales from Fishback et al. (2003). Connections to banks are indicator variables collected from *Moody's Manuals of Governments* in 1930. Suspended bank deposit data comes from the FDIC.

Table 2: Summary Statistics - Linked Census Data

	N	Mean	SD	Median	25 pct	75 pct
I(Moved) 1930-40	3,470,758	0.39	0.49	0.00	0.00	1.00
I(Moved City to City) 1930-40	3,470,758	0.19	0.39	0.00	0.00	0.00
I(Moved City to Rural) 1930-40	3,470,758	0.20	0.40	0.00	0.00	0.00
Move distance (mi)	1,351,885	187.96	471.98	11.18	4.23	79.90
Occupational income score	3,384,615	20.12	13.77	23.00	0.00	29.00
Kids	3,470,758	0.49	0.50	0.00	0.00	1.00
Immigrant	3,470,758	0.41	0.77	0.00	0.00	0.00
Married	3,470,758	0.62	0.48	1.00	0.00	1.00
Owner	3,470,758	0.40	0.49	0.00	0.00	1.00
Age	3,470,735	33.61	9.93	33.00	25.00	41.00
County Population (1930)	3,470,735	13.43	1.13	13.49	12.65	14.45
Manufacturing labor share	3,417,221	0.10	0.05	0.11	0.06	0.13
Best nearby change in GD severity (50mi)	3,045,238	0.17	0.15	0.18	0.09	0.25
Best nearby change in DOV (50mi)	3,470,216	-1.28	1.04	-1.25	-1.99	0.00
Δ Retail Sales, 1929-33	3,087,107	-0.42	0.11	-0.42	-0.48	-0.34

Note: Summary statistics of U.S. Decennial Census variables of a linked sample of urban males between 1930 and 1940. Records were linked using the ABE procedure with NYSIIS standardization. Crosswalks were obtained from Abramitzky et al. (2020). Occupational income score is trimmed at the 0-98 percentiles. I(Moved) is a binary taking the value of 1 if the reported city of residence in 1940 does not match the city in 1930. Move distance is geodetic distance in miles using city (for city to city moves) or county (city to rural) latitude and longitude. Kids is a binary taking the value of 1 if the person reported living in a family of size 3 or more and was married. Immigrant is discrete taking the value of 1 if immigrated after 1920, 2 if before 1920, and 0 if not an immigrant. Owner is binary and refers to home ownership. Manufacturing labor share computed as county level manufacturing labor divided by total county population. See the text for the definition of best nearby changes. Change in county retail sales comes from Fishback et al. (2003). Sample includes 18–56 year old males living in a Census enumerated city in 1930.

Table 3: Summary Statistics for Change in Relevant Variables, 1931–1935

	1-10k			10-100			100k+		
	count	25 pct	75 pct	count	25 pct	75 pct	count	25 pct	75 pct
Tax Rev.	396	-40	1	278	-20	16	92	-11	11
Utility Rev.	274	-12	11	217	-8	16	91	-2	15
Grants	281	-24	446	257	-33	252	89	-9	181
Government Pmt	396	-16	18	278	-13	16	92	-13	5
Protection Pmt	396	-13	20	278	-8	11	92	-14	-1
Welfare Pmt	147	1	275	212	5	438	90	-24	115
Health Pmt	364	-32	22	273	-18	15	92	-22	-6
Roads Pmt	332	-32	7	249	-27	6	92	-29	2
Education Pmt	222	-14	11	198	-15	11	92	-16	-1
Recreation Pmt	302	-53	14	234	-41	2	91	-33	-5
Utility Pmt	278	-27	15	216	-17	12	91	-22	8
Interest Pmt	378	-38	-4	272	-28	11	92	-6	16
Capital Exp. Pmt	282	-100	30	256	-93	-1	92	-82	-46
Assessed Value	396	-23	2	278	-14	6	92	-19	-1

Note: Summary data are given for percentage changes in the 1931–1935 by population category. Values are deflated using the CPI. Cities with fewer than 12 years of observations were dropped from the original sample in order to ensure time-series comparability.

Table 4: Determinants of Debt, 1924–1930

Outcome: Debt per capita

	(1) Debt - Total	(2) Debt - Total	(3) Debt - Total	(4) Debt - Total
Total Dep. Pmt.	29.81***	24.29***	17.68***	10.71
	(6.79)	(6.67)	(6.80)	(7.30)
Capital Exp.	6.43***	6.56***	6.50***	6.42***
	(2.05)	(2.10)	(2.10)	(2.05)
L.Capital Exp.	9.63***	9.65***	9.79***	9.86***
	(1.62)	(1.64)	(1.59)	(1.57)
L2.Capital Exp.	6.00**	6.17**	6.22**	6.47**
	(2.67)	(2.67)	(2.63)	(2.62)
Assessed Value	25.64**	24.26**	25.57**	24.30**
	(11.64)	(11.33)	(11.16)	(10.58)
L.Assessed Value	15.57**	13.45**	11.48*	8.82
	(6.08)	(6.41)	(6.07)	(5.93)
L2.Assessed Value	-3.71	-4.99	-5.10	-6.14
	(6.25)	(6.05)	(6.06)	(5.90)
City FE	✓	✓	✓	✓
Year FE		✓	✓	✓
Pop. Cat x Year			✓	✓
Region x Year				✓
R-sq	0.69	0.70	0.72	0.72
N	2,916	2,916	2,916	2,916
Mean(Y)	126.39	126.39	126.39	126.39

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of a within-city OLS regression of total debt per capita on covariates during 1924–1930. All covariates were standardized to have a mean of 0 and a standard deviation of 1 to ease interpretation. L and L2 denote one- and two-year lagged variables. Standard errors are clustered at the city level.

Table 5: Difference-in-differences: Total Expenditure

Panel A: Using DOV as Leverage Measure
Outcome: Total Public Service Expenditure (1) - (4) and Capital Expenditure (5)

	(1) No Controls	(2) + Pop.	(3) + Rev.	(4) + Region x Year FE	(5)
post=1 × dov	-0.038***	-0.052***	-0.046***	-0.074***	-0.172**
	(0.014)	(0.015)	(0.012)	(0.013)	(0.078)
City FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
1930 Pop x Year		✓	✓	✓	✓
Δ1920-30 Pop x Year		✓	✓	✓	✓
Revenue			✓	✓	✓
Region x Year				✓	✓
R-sq (within)	0.41	0.44	0.56	0.59	0.23
N	10,451	10,399	9,632	9,632	7,981
Mean(dov)	1.42				
SD(dov)	0.69				

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Using TOI as Leverage Measure
Outcome: Total Public Service Expenditure (1) - (4) and Capital Expenditure (5)

	(1) No Controls	(2) + Pop.	(3) + Rev.	(4) + Region x Year FE	(5)
post=1 × toi	0.053***	0.056***	0.042***	0.036***	0.084*
	(0.009)	(0.010)	(0.008)	(0.008)	(0.051)
City FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
1930 Pop x Year		✓	✓	✓	✓
Δ1920-30 Pop x Year		✓	✓	✓	✓
Revenue			✓	✓	✓
Region x Year				✓	✓
R-sq (within)	0.42	0.44	0.58	0.59	0.23
N	10,623	10,598	9,813	9,813	8,156
Mean(toi)	2.62				
SD(toi)	1.03				

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4. post = 1 denotes all years after 1930, and the sample period is 1924–1938. Standard errors are clustered at the city level. Column (1) contains no control variables, while columns (2)–(4) add population, revenue, and region by year controls. The outcome in columns 1- 4 is total public service expenditure. Column 5 reports the result for capital expenditure (i.e., construction) using the specification in Column (4). Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 6: Difference-in-differences: Types of Expenditure

Panel A: Using DOV as Leverage Measure

	(1) Cap. Ex.	(2) Gov.	(3) Sanit.	(4) Health	(5) Road	(6) Protection	(7) Welfare	(8) Education	(9) Rec.	(10) Interest	(11) Δ Total Debt	(12) Δ Short Debt	(13) Δ Long Debt
post=1 × dov	-0.172**	-0.036***	-0.066	-0.033	-0.039**	-0.155***	-0.005	-0.044	0.004	0.131***	-0.098***	-0.243***	-0.051***
	(0.078)	(0.013)	(0.060)	(0.043)	(0.017)	(0.027)	(0.078)	(0.038)	(0.048)	(0.048)	(0.015)	(0.052)	(0.018)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.23	0.38	0.07	0.10	0.17	0.34	0.53	0.11	0.12	0.21	0.03	0.10	0.04
N	7,981	9,632	5,869	9,118	8,296	9,630	5,652	6,768	8,060	9,336	9,316	3,330	8,733
Mean(dov)	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42	1.42
SD(dov)	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69	0.69

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Using TOI as Leverage Measure

	(1) Cap. Ex.	(2) Gov.	(3) Sanit.	(4) Health	(5) Road	(6) Protection	(7) Welfare	(8) Education	(9) Rec.	(10) Interest	(11) Δ Total Debt	(12) Δ Short Debt	(13) Δ Long Debt
post=1 × toi	0.084*	0.020**	0.071	0.029	0.033***	0.114***	-0.003	0.011	0.027	-0.078*	0.069***	0.130***	0.035**
	(0.051)	(0.009)	(0.046)	(0.032)	(0.011)	(0.025)	(0.043)	(0.032)	(0.039)	(0.040)	(0.012)	(0.046)	(0.017)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.23	0.38	0.08	0.09	0.17	0.28	0.54	0.10	0.13	0.21	0.03	0.09	0.04
N	8,156	9,813	5,801	9,276	8,533	9,805	5,952	6,987	8,013	9,653	9,570	3,518	8,783
Mean(toi)	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62	2.62
SD(toi)	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03	1.03

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for different types of public service expenditure. post = 1 denotes all years after 1930 and the sample period is 1924–1938. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 7: Heterogeneity by 1920-30 Population Growth and Size

Panel A: Using DOV as Leverage Measure

	All		High 20-30 Growth		Low 20-30 Growth		Pop < 10k		10k < Pop < 100k		Pop > 100k	
	Service	Investment	Service	Investment	Service	Investment	Service	Investment	Service	Investment	Service	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
post=1 × dov	-0.074***	-0.172**	-0.084***	-0.223**	-0.063***	-0.089	-0.072***	-0.162	-0.098***	-0.249**	-0.032*	-0.229*
	(0.013)	(0.078)	(0.020)	(0.093)	(0.016)	(0.133)	(0.015)	(0.129)	(0.028)	(0.123)	(0.019)	(0.137)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.59	0.23	0.56	0.31	0.63	0.17	0.50	0.18	0.70	0.28	0.83	0.60
N	9,632	7,981	5,005	4,079	4,627	3,902	5,032	3,709	3,342	3,014	1,258	1,258
Mean(dov)												
SD(dov)												

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Using TOI as Leverage Measure

	All		High 20-30 Growth		Low 20-30 Growth		Pop < 10k		10k < Pop < 100k		Pop > 100k	
	Service	Investment	Service	Investment	Service	Investment	Service	Investment	Service	Investment	Service	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
post=1 × toi	0.036***	0.084*	0.037***	0.141**	0.033***	-0.005	0.032***	0.030	0.050***	0.123	0.034*	0.246**
	(0.008)	(0.051)	(0.012)	(0.067)	(0.012)	(0.079)	(0.011)	(0.065)	(0.014)	(0.105)	(0.018)	(0.101)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.59	0.23	0.56	0.31	0.64	0.18	0.53	0.19	0.70	0.28	0.83	0.60
N	9,813	8,156	5,059	4,129	4,754	4,027	5,261	3,924	3,266	2,946	1,286	1,286
Mean(toi)												
SD(toi)												

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for different subsamples of cities. Columns (1) and (2) present the main results across all cities. The sample in columns (3) and (4) includes only cities that experienced above-median population growth between 1920 and 1930 while the sample in columns (5) and (6) includes only cities below the median. The cities in columns (7) through (12) include only those in specified population categories as of 1930. post = 1 denotes all years after 1930 and the sample period is 1924–1938. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 8: Heterogeneity by Banking Conditions

Panel A: Using DOV as Leverage Measure

	Low Loan Growth		High Loan Growth		No Bank Suspended		Bank Suspended	
	Service	Investment	Service	Investment	Service	Investment	Service	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post=1 × dov	-0.089***	-0.312**	-0.066***	-0.081	-0.066***	-0.279	-0.075***	-0.159*
	(0.015)	(0.124)	(0.020)	(0.102)	(0.025)	(0.173)	(0.015)	(0.084)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.61	0.23	0.58	0.24	0.57	0.27	0.60	0.23
N	4,231	3,629	5,401	4,352	1,426	1,007	8,206	6,974
Mean(dov)								
SD(dov)								

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Using TOI as Leverage Measure

	Low Loan Growth		High Loan Growth		No Bank Suspended		Bank Suspended	
	Service	Investment	Service	Investment	Service	Investment	Service	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post=1 × toi	0.044***	0.163*	0.034***	0.031	0.032	0.138	0.037***	0.085
	(0.010)	(0.091)	(0.012)	(0.063)	(0.021)	(0.114)	(0.009)	(0.054)
City FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓
R-sq (within)	0.61	0.23	0.59	0.24	0.57	0.27	0.60	0.24
N	4,358	3,742	5,455	4,414	1,384	992	8,429	7,164
Mean(toi)								
SD(toi)								

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for different subsamples of cities. Low loan growth denotes cities in counties where nationally-chartered banks experienced below median loan growth between 1931 and 1929 as reported by the Office of the Comptroller of the Currency. High Loan growth denotes cities in counties with above median growth in total loans. No bank suspended denotes cities in counties with no state or national banking suspensions during 1930-1933 as reported by the Federal Deposit Insurance Corporation, while Bank suspended refers to cities in counties with at least one state or national bank suspended during the same time period. post = 1 denotes all years after 1930 and the sample period is 1924–1938. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 9: Effect on Bond Ratings and Property Tax Rates

Outcome: Rating (10 point scale)

	(1) Rating	(2) Tax Rate - Total	(3) Tax Rate - Bond Rpmt	(4) Rating	(5) Tax Rate - Total	(6) Tax Rate - Bond Rpmt
post=1 × dov	-0.360***	-0.003	0.242***			
	(0.087)	(0.014)	(0.063)			
post=1 × toi				0.388***	0.002	-0.144***
				(0.089)	(0.012)	(0.042)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
R-sq (within)	0.77	0.17	0.07	0.77	0.17	0.07
N	1,615	4,803	2,398	1,627	4,749	2,390

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for city Moody's Bond ratings (proxy for cost of credit) as well as property tax rates using data from California cities, which report total and bond-repayment tax rates separately. In columns (1) and (2), ratings are measured on a discrete scale with 10 denoting AAA rated bonds, 9 denoting AA rated bonds, and so on. In columns (3) - (6), total refers to the total tax rate, while Bond Rpmt is the tax rate solely for debt-repayment purposes. post = 1 denotes all years after 1930, and the sample period is 1930-1938 for Moody's ratings and 1924-1938 for tax rates. The sample in (1) and (2) includes only the 189 cities that have complete data throughout the sample period. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 10: Robustness: Demand for Infrastructure

	(1) All	(2) No 1927-1929	(3) E[bond age] > 6 years	(4) All	(5) No 1927-1929	(6) E[bond age] > 6 years
post=1 × dov	-0.31***	-0.28**	-0.20*			
	(0.10)	(0.11)	(0.10)			
post=1 × toi				0.33***	0.26***	0.22**
				(0.09)	(0.09)	(0.09)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
R-sq (within)	0.32	0.32	0.31	0.33	0.33	0.31
N	4,439	3,436	3,514	4,497	3,484	3,562

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for capital expenditure. Columns (1) and (4) recapitulate the main results in Table 5. Columns (2) and (5) exclude cities that issued more than 28 percent (top quartile) of all their bonds outstanding between and including 1927–1929, according to data from $Moody's$. Columns (3) and (6) exclude cities with an average bond age of 6 or less (bottom quartile). Both measures are meant to capture cities that invested in infrastructure right before the Depression and that may have lower investment demand in the 1930s. Post = 1 denotes all years after 1930, and the sample period is 1924–1938. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 11: Robustness: alternative measurement, city-bank connections, political motives

Panel A. Outcome: Public Service Expenditure

	Baseline	Retail	Baseline (4)	Bank Connections	Gov Form	Election Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
post=1 × dov	-0.074***	-0.074***	-0.042**	-0.045**	-0.038***	-0.075***
	(0.013)	(0.013)	(0.020)	(0.020)	(0.014)	(0.014)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
Δ Retail 1929-33 x Year		✓				
I(Connected) x Year				✓		
Gov. Form x Year					✓	
Cycle Length x Year						✓
R-sq (within)	0.59	0.59	0.58	0.58	0.67	0.59
N	9,632	9,632	3,504	3,504	6,293	9,632
Mean(dov)						
SD(dov)						

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B. Outcome: Capital Expenditure

	Baseline	Retail	Baseline (4)	Bank Connections	Gov Form	Election Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
post=1 × dov	-0.172**	-0.178**	-0.131	-0.085	-0.194**	-0.215***
	(0.078)	(0.076)	(0.103)	(0.105)	(0.087)	(0.079)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
1930 Pop x Year	✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year	✓	✓	✓	✓	✓	✓
Revenue	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
Δ Retail 1929-33 x Year		✓				
I(Connected) x Year				✓		
Gov. Form x Year					✓	
Cycle Length x Year						✓
R-sq (within)	0.23	0.23	0.31	0.32	0.28	0.23
N	7,981	7,981	3,180	3,180	5,661	7,981
Mean(dov)						
SD(dov)						

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of the difference-in-differences regression of Equation 5.4 for total public service expenditure (Panel A) and capital expenditure (Panel B). post = 1 denotes all years after 1930, and the sample period is 1924 - 1938. Column (1) reports the baseline results. Column (2) additional controls for county-level change in log per capita retail between 1933 and 1929 by year fixed effects. Column (3) reports the baseline results for the subsample of cities in Column (4), which controls for city-bank connections with $I(Connected)$ taking the value of 1 if the city reported paying interest on bonds in a bank located in New York City, Cleveland, or Chicago and 0 otherwise. This data comes from $Moody's$ manuals and is available only for a subsample of cities. Column (4) controls for city government type by year fixed effects where type is one of mayor-council, manager-council, commission, town meeting, see text for details. Across all specification, additional controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 12: Balance Test on Bond Shocks - 1929 Variables

	All	Shock <Median	Shock >Median	Difference
Shock (All)	44.7	30.3	59.2	-28.9***
	(19)	(8.7)	(16)	(1.7e-54)
Total Revenue (Rev.)	4.21	4.2	4.23	-.0352
	(.54)	(.54)	(.55)	(.56)
Assessed Value	7.97	7.97	7.97	.0019
	(.48)	(.54)	(.41)	(.97)
Total Dep. Pmt	3.9	3.9	3.9	-6.9e-04
	(.6)	(.57)	(.64)	(.99)
Capital Exp. Pmt	2.79	2.88	2.7	.176
	(1.1)	(1.2)	(1.1)	(.16)
Population	3.47	3.66	3.29	.372**
	(1.4)	(1.6)	(1.1)	(1.8e-02)
Observations	316	159	157	316

Notes: This table presents summary statistics and a t-test between the treated (above median *shock*) and control (below median *shock*) groups. The variable *shock* is defined as the proportion of 1929 city debt that was contractually obligated to be repaid between 1930 and 1935, inclusive. The median *shock* is 44.2 percent.

Table 13: Difference-in-Differences Using Bond-level Shocks

Panel A: Using $\widehat{DOV}$ as Leverage Measure
Outcome: Capital Expenditure (1)-(5) and Public Service Expenditure (6)-(7)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post=1 × $\widehat{DOV}$	-0.396***	-0.363***	-0.453***	-0.498***		-0.053**	
	(0.101)	(0.089)	(0.086)	(0.096)		(0.025)	
post=1 × dov					-0.415***		-0.071***
					(0.091)		(0.020)
City FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year		✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year		✓	✓	✓	✓	✓	✓
Revenue			✓	✓	✓	✓	✓
Region x Year				✓	✓	✓	✓
R-sq (within)	0.29	0.31	0.33	0.34	0.34	0.69	0.69
N	4,041	4,034	3,726	3,726	3,726	4,004	4,004
Mean(dov)	1.14				1.44		1.44
SD(dov)	0.53				0.73		0.73

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel B: Using $\widehat{TOI}$ as Leverage Measure
Outcome: Capital Expenditure (1)-(5) and Public Service Expenditure (6)-(7)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
post=1 × $\widehat{TOI}$	0.331***	0.252***	0.224***	0.218***		0.021*	
	(0.062)	(0.061)	(0.063)	(0.065)		(0.012)	
post=1 × toi					0.489***		0.042*
					(0.100)		(0.022)
City FE	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓
1930 Pop x Year		✓	✓	✓	✓	✓	✓
Δ1920-30 Pop x Year		✓	✓	✓	✓	✓	✓
Revenue			✓	✓	✓	✓	✓
Region x Year				✓	✓	✓	✓
R-sq (within)	0.29	0.31	0.32	0.33	0.34	0.69	0.69
N	4,041	4,034	3,726	3,726	3,726	4,004	4,004
Mean(toi)	10.06				2.62		2.62
SD(toi)	0.84				1.03		1.03

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the results of a within-city regression of Equation 6.4 using (in Panel A) the shocked debt-over-value ($\widehat{DOV}$) as a proxy for Depression time financial constraints. The measure only uses the total amount of debt maturing from 1930 -1935 in the numerator and total debt in 1930 in the denominator. Panel B uses the shocked tax-over-interest ($\widehat{TOI}$), which is analogously constructed using the average expected interest payments. Columns (1)–(3) add the specified controls. Column (5) uses the non-shocked leverage measure for reference. Columns (6) and (7) use total public service expenditure as the outcome variable. The sample includes 339 cities for which bond-level and city-level data are available. Controls include log population in 1930 by year fixed effects, change in log population between 1920 and 1930 by year fixed effects, contemporaneous and lagged log per-capita revenue, and region by year fixed effects. Standard errors are shown in parentheses and are clustered at the city level.

Table 14: City Leverage, Local Economic Shocks, and Migration

Outcome: I(moved) 1930-1940

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
High Lev.=1 × GD Severe=1		0.083**	0.096**	0.093***	0.105***	0.078***	
		(0.038)	(0.042)	(0.030)	(0.027)	(0.026)	
$\widehat{DOV}$=1 × GD Severe=1							0.067**
							(0.032)
GD Severe=1	-0.007	-0.041	-0.040	-0.025	-0.017	0.004	0.019
	(0.021)	(0.030)	(0.033)	(0.024)	(0.020)	(0.018)	(0.020)
High Lev.=1	-0.023	-0.061***	-0.069***	-0.066***	-0.060***	-0.062***	
	(0.019)	(0.023)	(0.026)	(0.024)	(0.021)	(0.019)	
Person Controls			✓	✓	✓	✓	✓
City Controls				✓	✓	✓	✓
Nearby Controls					✓	✓	✓
Region FE						✓	✓
R-sq	0.00	0.00	0.03	0.03	0.03	0.04	0.03
N	3,087,107	3,087,107	3,010,074	2,957,325	2,916,452	2,916,452	2,399,724

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the estimation results of Equation 7.1. High Lev. takes the value of 1 if the person lived in an above-median leverage city in 1930 as defined by DOV (see Section 5 for definition). GD Severe takes the value of 1 if the person lived in a county with below-median retail sales growth between 1929 and 1933. $\widehat{DOV}$ denotes the shocked leveraged measure, which uses only the proportion of debt maturing between 1930 and 1935 (see Section 6 for definition). Controls include age (4 bins), immigrant status (3 levels), indicator for children, indicator for house owner, occupational income score, and region fixed effects (4 levels). Nearby chars are described in detail in the text of Section 7. Standard errors are shown in parentheses and are clustered at the county level.

Table 15: Migration Behavior: Heterogeneity

Outcome: I(moved)

	(1) Poor	(2) Rich	(3) Short moves	(4) Long moves	(5) Local admins	(6) Non-local admins
$\widehat{DOV}$=1 × GD Severe=1	0.066**	0.064**	0.062*	0.039**	0.055	0.037
	(0.030)	(0.031)	(0.037)	(0.017)	(0.035)	(0.037)
GD Severe=1	0.013	0.015	0.019	0.017	0.024	0.015
	(0.019)	(0.020)	(0.021)	(0.011)	(0.022)	(0.029)
$\widehat{DOV}$=1	-0.060***	-0.059**	-0.059**	-0.029**	-0.055**	-0.081***
	(0.022)	(0.023)	(0.026)	(0.012)	(0.025)	(0.028)
Person Controls	✓	✓	✓	✓	✓	✓
City Controls	✓	✓	✓	✓	✓	✓
Nearby Controls	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓
N	682,091	939,600	2,083,257	1,669,916	36,733	24,766

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the estimation results of Equation 7.1 for various subsamples. High Lev. takes the value of 1 if the person lived in an above-median leverage city in 1930 as defined by DOV (see Section 5 for definition). GD Severe takes the value of 1 if the person lived in a county with below-median retail sales growth between 1929 and 1933. $\widehat{DOV}$ denotes the shocked leveraged measure which uses only the proportion of debt maturing between 1930 and 1935 (see Section 6 for definition). Controls include age (4 bins), immigrant status (3 levels), indicator for children, indicator for house owner, occupational income score, region fixed effects (4 levels). Nearby chars are described in detail in the text of Section 7. Standard errors are shown in parentheses and are clustered at the county level. Column (1) includes only those in the lowest tercile of occupational income score. Column (3) includes only those who did not move or moved within 20 miles. Column (4) includes only those who did not move or moved further than 50 miles. Column (5) includes only persons reporting working in local administration in the 1930 Census. Column (6) includes only state and federal employees. Controls include age (4 bins), immigrant status (3 levels), indicator for children, indicator for house owner, occupational income score, region fixed effects (4 levels). Nearby chars are described in detail in the text. Standard errors are shown in parentheses and are clustered at the county level.

Appendix

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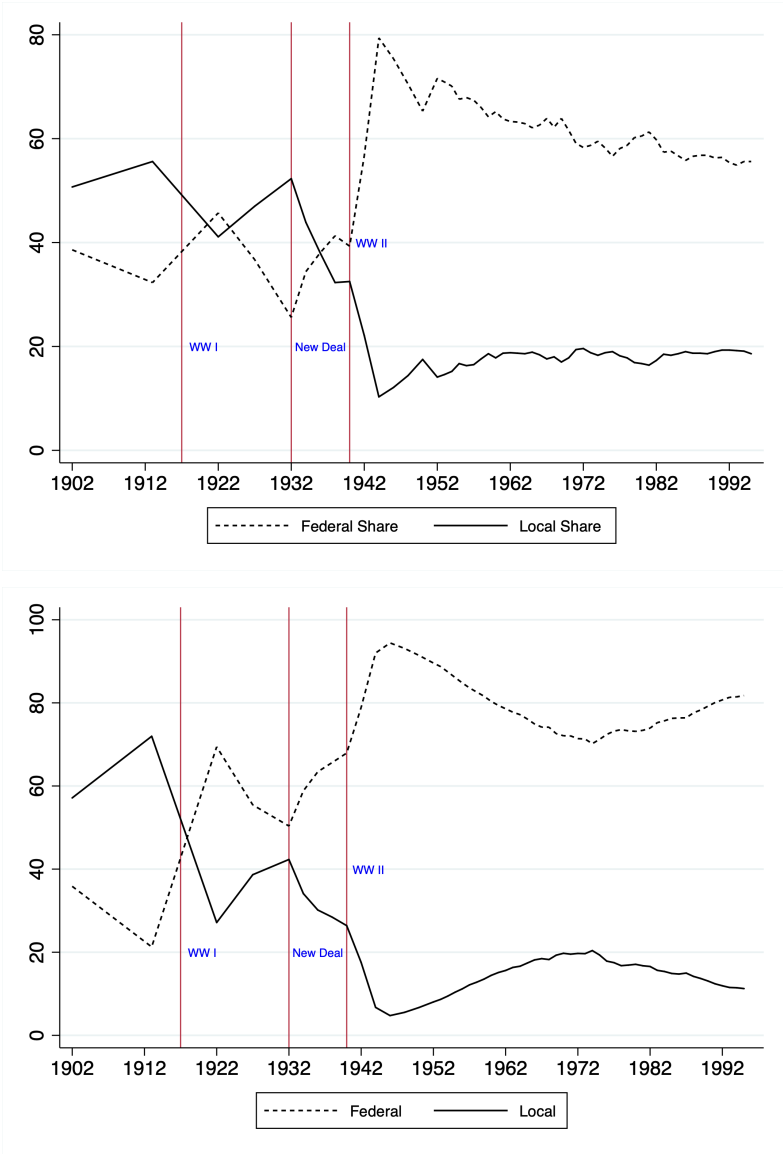
A Historical Statistics

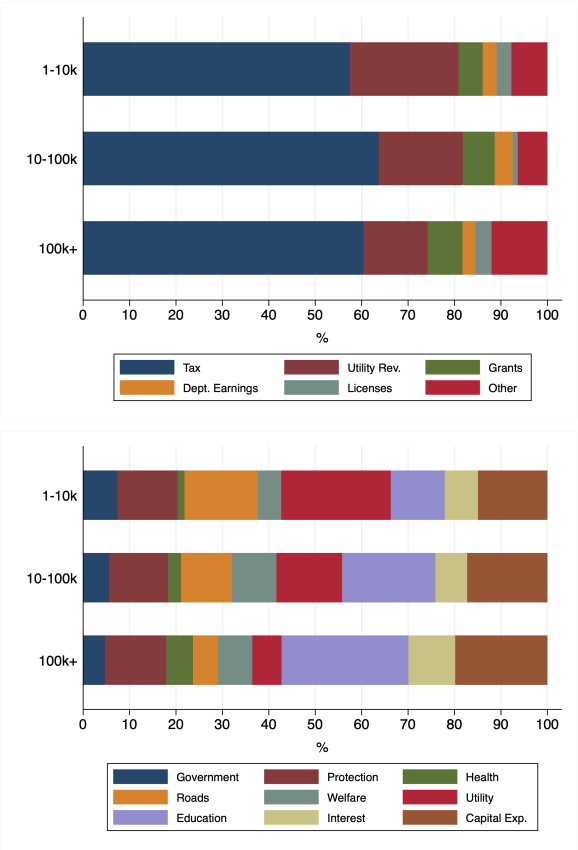
Figure A.1: Revenue [Top] and Debt [Bottom], % of Total



Note: This figure plots total local and federal government revenue and debt as percent of total in the U.S. in the 20th century. Author calculations of data in Historical Statistics of the United States, Series Ea125–131. State government shares are not shown.

B City Revenue and Payments

Figure A.2: Composition of City Revenue and Expenditure, 1930

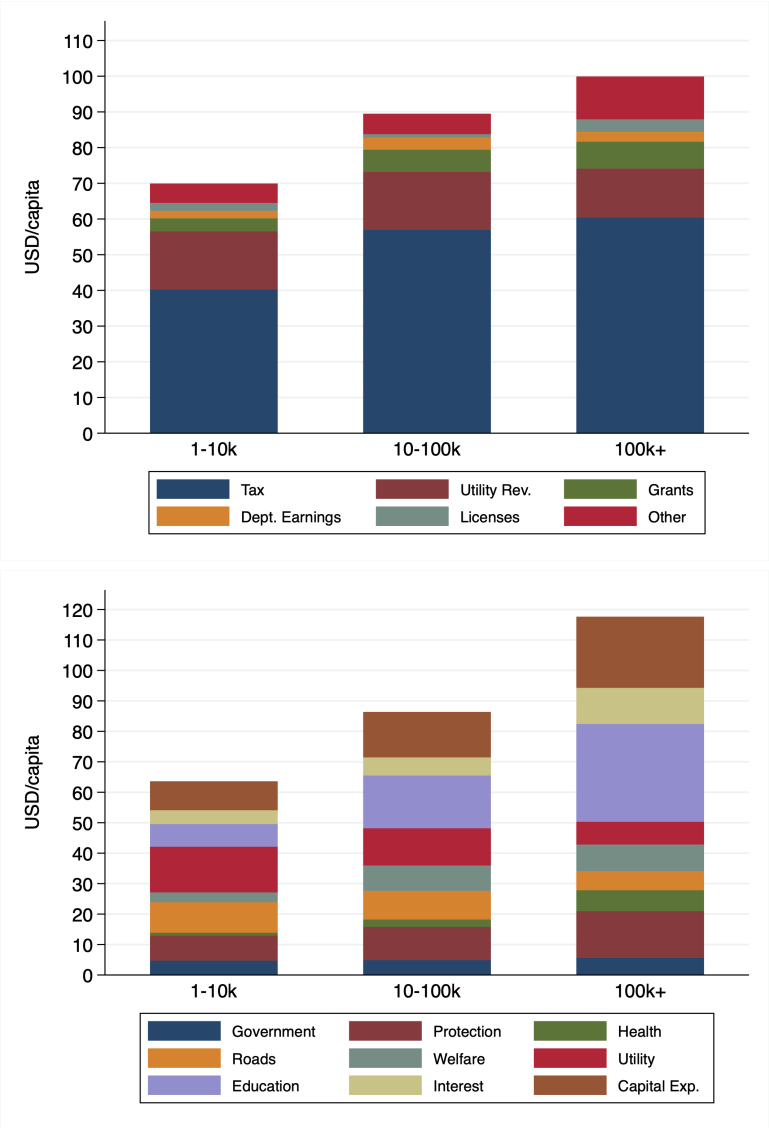


Note: The figures show the average composition of city revenue and expenditure in 1930 by population category. Tax includes property tax, local personal income tax, local corporate income tax, and excise taxes. Utility revenue is income from publicly-owned water, gas, and electric utilities. Department earnings is income from government operations. Revenue from special projects is user fees (e.g., tolls). Grants include intergovernmental transfers from the State and Federal governments. Roads expenditure is for maintenance and improvement of roads. Capital expenditure includes construction and land purchases. Protection includes police and fire departments. Health includes all expenses related to the health department and sanitation services. Welfare includes all unemployment benefits, almshouses, and charity hospitals.

Figure A.3: Average Revenue (1930 = 1.0), by City Size

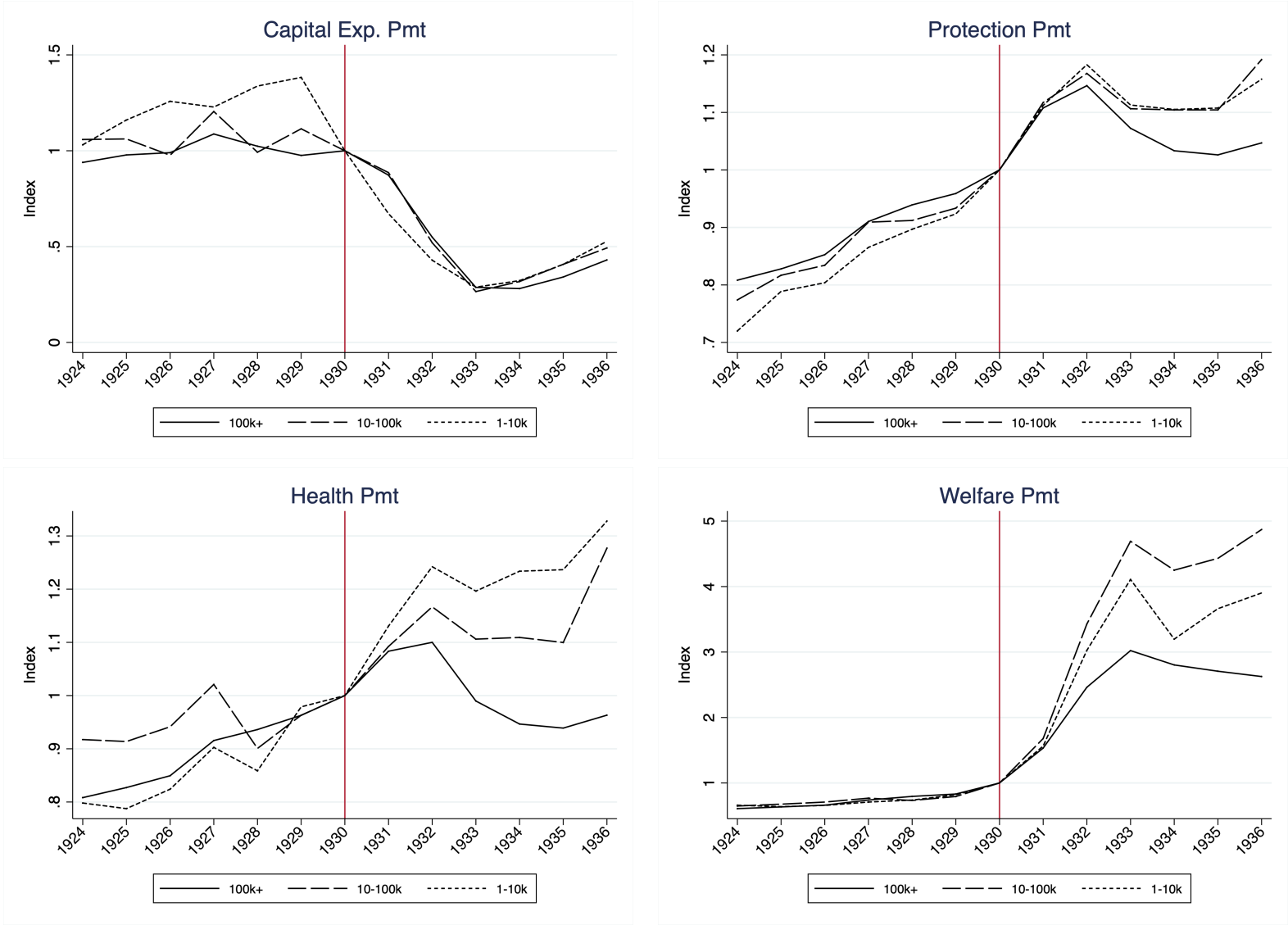
Note: This figure plots the average per-capita revenues across cities within population categories (1930 = 1). The population category is static for each city and was assigned using the population obtained from the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities of between 1,000 and 10,000, 216 between 10,000 and 100,000, and 93 cities of 100,000 and above. To calculate per capita values, census population data were linearly interpolated between census years (1920, 1930, 1940). All values were deflated using the CPI.

Figure A.4: Composition of City Revenue and Expenditure, 1930



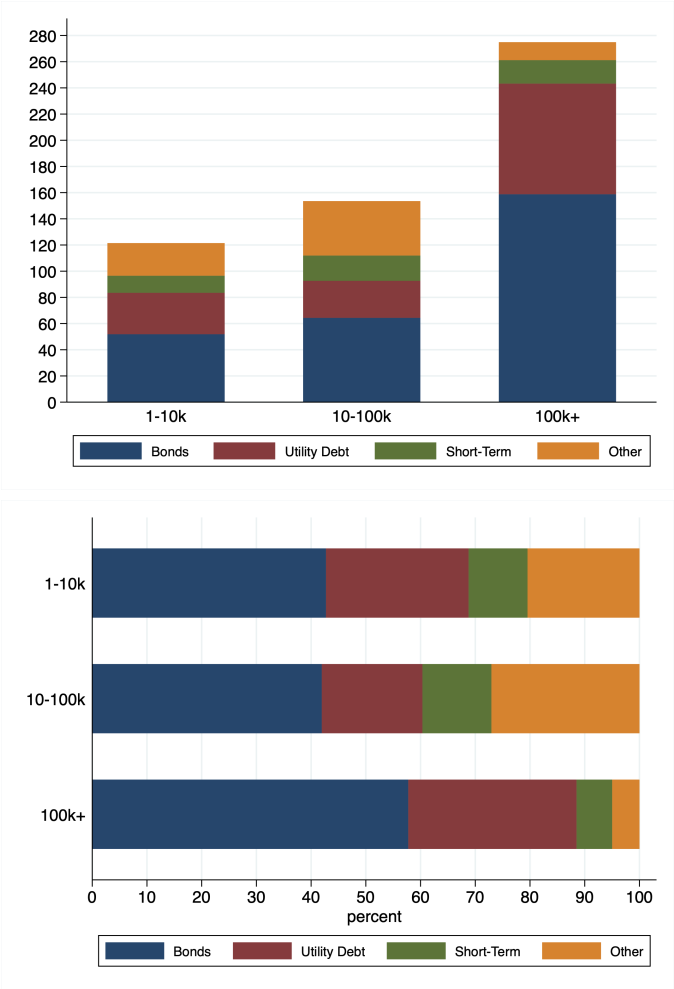
Note: The figures show the average per-capita level (in 1930 dollars) of city revenue and expenditure in 1930 by population category. Tax includes property tax, local personal income tax, local corporate income tax, and excise taxes. Utility revenue is income from publicly-owned water, gas, and electric utilities. Department earnings is income from government operations. Revenue from special projects is user fees (e.g., tolls). Grants include intergovernmental transfers from the State and Federal governments. Roads expenditure is for the maintenance and improvement of roads. Capital expenditure includes construction and land purchases. Protection includes police and fire departments. Health includes all expenses related to the health department and sanitation services. Welfare includes all unemployment benefits, almshouses, and charity hospitals.

Figure A.5: Average Expenditure (1930 = 1.0), by City Size



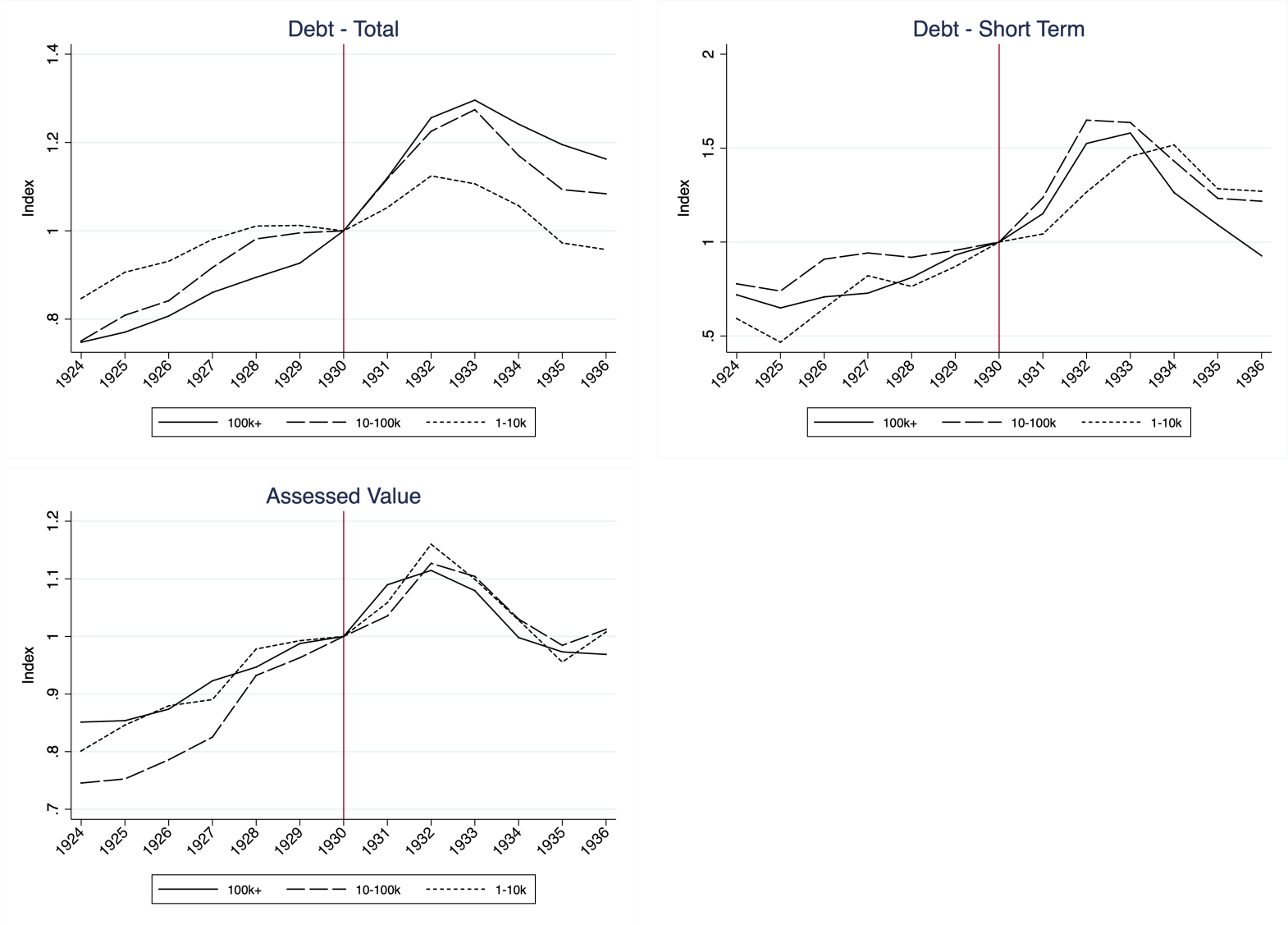
Note: This figure plots the average per-capita expenditures across all cities within population categories (1930 = 1). Population category is static for each city and was assigned using the population in the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities in from 1,000 to 10,000, 216 between 10,000 and 100,000, and 93 cities of 100,000 and above. To calculate per capita values, census population data were linearly interpolated between census years (1920, 1930, 1940). All values were deflated using the CPI.

Figure A.6: Breakdown of Balance Sheet Debt



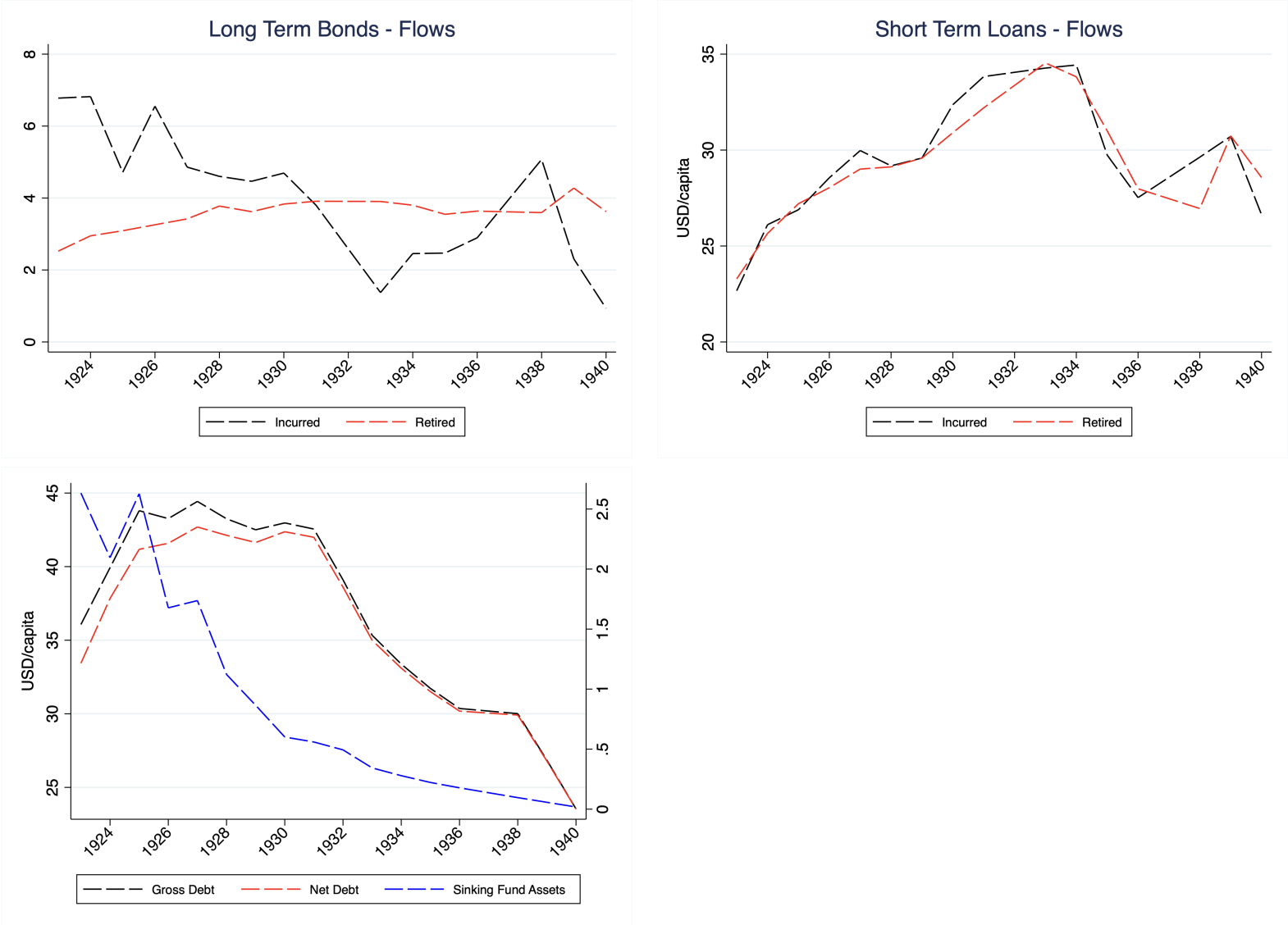
Note: The figures show the average composition of city debt in 1930 by population category. Bonds are those long-term (typically over 5 years) issued for general funding purposes or for specific infrastructure projects. Short-term loans are those with a duration below 5 years, primarily collateralized by anticipated tax revenue. Utility debt is all debt incurred by public utilities. Other debt includes any debt incurred by special taxing districts within the city, e.g., water reclamation or sewage districts.

Figure A.7: Evolution of Per Capita City Debt (real), by Category and Population



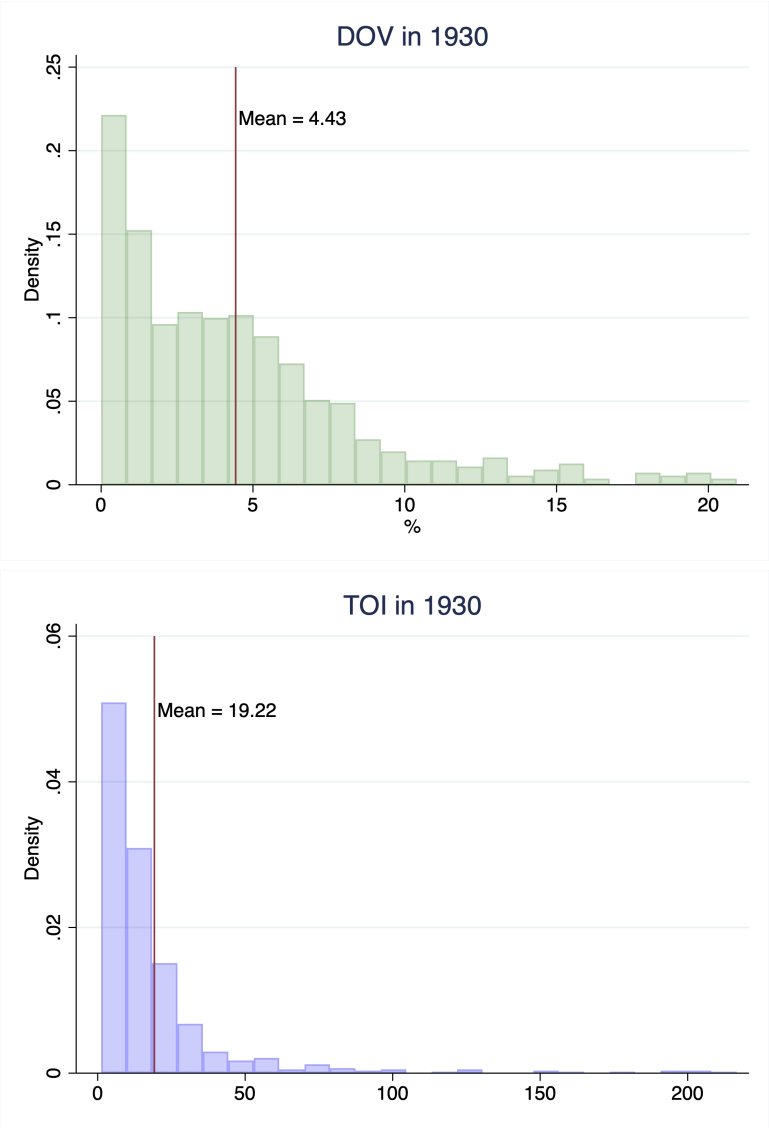
This figure plots the mean per-capita debt across all cities within population categories (1929 = 1). Population category is static for each city and was assigned using the population in the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities from 1,000 to 10,000, 216 between 10,000 and 100,000, and 93 cities of 100,000 and above. To calculate per capita values, census population data were linearly interpolated between census years. All values were deflated using the CPI.

Figure A.8: Incurred and Canceled Debt in MA Cities



Note: These figures present the average per-capita debt flows across 108 cities in Massachusetts. Incurred denotes all new bonds issued, and retired denotes all outstanding bonds which were paid off fully during the year. Sinking fund assets consist of cash savings and government securities. Net debt is gross debt minus the sinking fund assets.

Figure A.9: Distribution of Tax-over-Interest and Debt-over-Value (pre log)



Note: This figure plots the histogram for debt-over-value and tax-over-interest ratios in 1930 for all cities in my sample. DOV and TOI are defined as the log of 1 plus these measures.

C Local Government Sources

City-level data on tax revenues, expenditures, and debt come from various publications produced at the state and federal levels. I describe them in this section.

Massachusetts. Data for Massachusetts cities appear in the report *Statistics of Municipal Finances* produced by the Department of Corporations and Taxation of the Commonwealth of Massachusetts. This annual report, first published in 1905, has three parts: list of financial transactions, cash balances, and debt for all cities (Part 1, around 40 cities), for all towns with a population of over 5,000 (Part 2, around 79 towns), and for all towns with a population under 5,000 (Part 1, around 237 towns). Due to budget constraints, this paper only uses data from Parts 1 and 2.

New York. Data for New York cities, towns, and villages appear in the report *Special Report on Municipal Accounts by the State Comptroller* produced by the New York Department of Audit and Control. This annual report is mandated by law (Article 3 of the General Municipal Law). It contains roughly 25 revenue and 25 expenditure variables across 57 cities, 527 villages, and 932 towns. Due to budget constraints, this project uses only the information for all cities and the largest 50 villages and towns.

Indiana. Data for Indiana cities are obtained from the *Statistical Report for the State of Indiana* compiled by the Division of Accounting and Statistics of the state of Indiana. This annual report aggregates, audits, and revises schedules filed by local officers. Of all the sources used in this project, this one is most limited in scope, with only 15 revenue and 24 expenditure variables. Until 1934, this report also contained judicial statistics of municipal and county courts. This publication contains data on roughly 95 cities.

Ohio. Data for Ohio cities come from the report *Comparative Statistics, cities of Ohio* produced by the Bureau of Inspection and Supervision of Public Offices of the State of Ohio. City auditors are required by law (section 291 of the General Code of Ohio) to report financial statements with the Bureau. The report contains four parts: (1) Receipts, (2) Expenditures, (3) Debt, and (4) Memorandum (supplementary data) and contains data for roughly 100 cities.

California. Data for California cities come from the report *Annual Report of Financial Transactions of Municipalities and Counties of California* produced by the Office of State Controller compiled by the authority of Chapter 550 of the State Code. This report contains detailed reports on payments and revenue sources for roughly 280 California cities.

Examples of services funded by expenditure category

This information accompanies the data provided by the Census Bureau in *Financial Statistics*.

- **Roads.** Maintenance of roads, snow removal, street lighting, and waterways.
- **Education.** All costs related to schools and libraries, supplementary to independent school districts.
- **Welfare.** Charities and poor relief, mental institutions.
- **Health.** Health department, prevention/treatment of communicable diseases, collection of vital statistics, food regulation and inspection.
- **Sanitation.** Sewage disposal, street cleaning, garbage collection, public restrooms.
- **Fire.** Wages of fireman and water costs.
- **Police.** Wages of police officers, building inspectors, employment agencies, examiners.
- **Miscellaneous.** Pension expenses, burial of soldiers, administration of trust funds, judgments against the city.
- **Utility** Utilities such as water supply systems, electricity, gas supply, docks, cemeteries, railways.
- **Recreation.** Maintenance of parks and general recreational areas.
- **Government** Wages of all government workers (council members, mayors, treasurer, judges, etc), cost of elections, and rent on government buildings.

D Moody's

Bond-level data was collected from the publication *Moody's Manual of Governments.* The main limitation of this data source is that bonds are not updated annually by Moody's. For example, I observe (in the 1929 Manual) Chicago bonds that *had* $50,000 remaining during the years *1924*–1940, but the amount that is still left to be unpaid by 1929 must be estimated by assuming a plausible repayment scheme from 1924 to 1929.

First, I assume that bonds that are not paid off serially (i.e., have one maturity date, "term" bonds) remain on the city's books at full value. Second, I assume a linear repayment structure for bonds that are listed as serial, and I assign the following weight to each bond:

$$weight_{i,t} = \begin{cases} \frac{Y_i(N) - year_t}{Y_i(N) - Y_i(0)} & \text{if type} = \text{serial} \\ 1 & \text{if type} = \text{term} \end{cases} \tag{D.1}$$

where $Y_i(0)$ is the first year of bond i's repayment schedule and $Y_i(N)$ is the last. For example, a $10,000 bond that matures between 1930–1940 is assigned a weight of 0.9 in 1931, as 90% of the bond is assumed to be outstanding in 1931. For each city, I sum all weight-adjusted bonds to arrive at an aggregate debt figure in each year.

$$Moody_{j,t} = \sum_{\forall i \in j} weight_{i,t} \times face_i \tag{D.2}$$

where the sum is over all reported bonds for city j that have not year matured fully by year t. Furthermore, I compute the total implied interest payment by multiplying the interest rate by the face value and summing across all bonds.

To validate this exercise, I investigate the correlation between imputed Moody aggregates and the totals reported in the financial transactions data. Figure A.10 reports this relationship for total outstanding debt and total interest payments for 1929. With no measurement error, all cities would lie on the 45 degree line. Though imperfect, this imputation strategy produces totals that are close to the truth; the correlation coefficients are 0.98 for debt and interest payments, respectively.

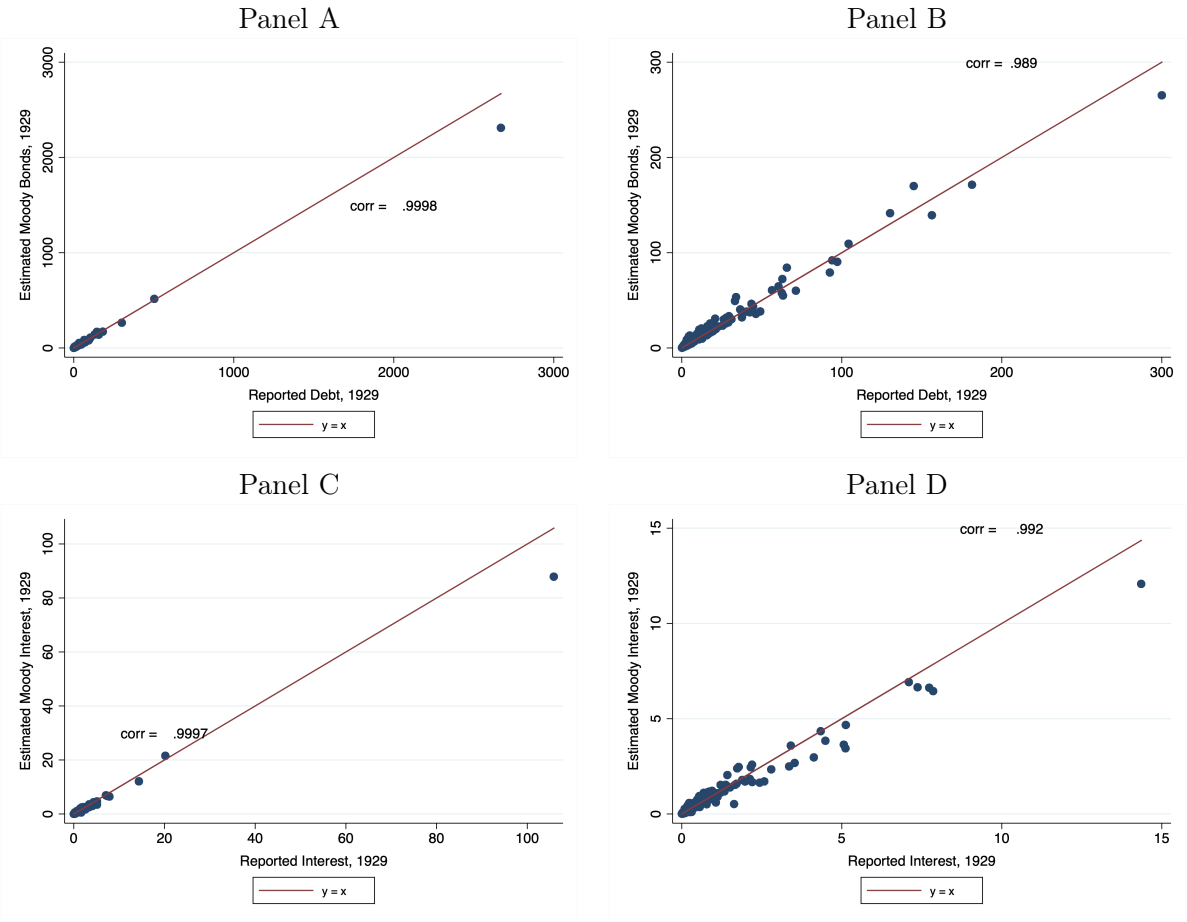
The mean interest rate paid is 4.53. The average bonds in 1929 were issued in 1918. 36% of the bonds were "term" bonds–repaid in full at the end of the maturity period–and the remaining 64% were "serial" bonds–repaid proportionally over time, typically through annual contributions to city-established trust funds called "sinking funds." The median nominal face value of these outstanding

bonds in 1929 was $261,000.

Table A1: Summary Statistics - Moody's Bonds

	Year = 1929					
	count	mean	sd	p50	min	max
Rate	28,970	4.59	1	4	2	8
Year Issued	28,893	1918.44	8	1921	1871	1930
Repayment Starts	28,810	1932.65	9	1929	1904	1991
Repayment Ends	28,810	1940.72	10	1938	1929	2002
I(type = term)	29,366	0.36	0	0	0	1
Face Value (k)	29,310	261.58	1467	50	0	55000
Observations	29366					

Figure A.10: Moody Bonds vs. Reported



Note: This figure shows the scatterplots of actual reported bonded debt and interest as reported in the financial transactions data vs. estimated bonded debt and interest using data from the *Moody's* Manuals. The red line is the 45 degree line. The graphs on the left (Panels A and C) include outliers (New York and Philadelphia), while the graphs on the right (Panels B and D) exclude them. The sample includes 341 cities. Both axes are in millions of nominal U.S. dollars.