Journal of Financial Economics 176 (2026) 104205

Contents lists available at ScienceDirect

Journal of Financial Economics

journal homepage: www.elsevier.com/locate/finec

Public goods under financial distress☆

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Caltech, United States of America
NBER, United States of America

ARTICLE INFO

JEL classification:
H7
N3
G3

Keywords:
Local public good provision
Financial distress
Great depression
Municipal bonds

ABSTRACT

I examine the effects of public debt on municipal services and real outcomes during financial crises using a unique archival dataset of U.S. cities from 1924 to 1943. Unlike today's countercyclical fiscal policies, the Great Depression provides a rare setting to observe fiscal shocks without substantial intergovernmental or Federal Reserve support. My findings show that financial market frictions – especially the need to refinance debt – led cities to sharply cut expenditures, particularly on capital projects and police services. As urban development halted during the Depression, cities with high pre-crisis debt levels faced significant austerity pressures, a decline in population growth, a rise in crime, and a departure of skilled public servants from municipal governments.

1. Introduction

During recessions, U.S. local governments often face falling revenues alongside rising service demands. In recent decades, federal support has cushioned these shocks—for example, the $350 billion Local Fiscal Recovery Fund during COVID-19 and the $500 billion Federal Reserve Municipal Liquidity Facility in 2020. Such interventions reflect the modern view that sharp local spending cuts can generate negative spillovers. Yet this fiscal arrangement is historically new: before the New Deal, federal involvement in local affairs was minimal, and cities largely navigated downturns alone (Wallis, 2000). This paper examines how municipal debt affected expenditure and local outcomes during the Great Depression. Using new data on city finances, health, crime, and local employment from 1924–1943, I study how debt-driven constraints shaped service provision and economic outcomes.

I find that financially constrained cities quickly adopted austerity, slashing both service and capital investment. The pre-Depression urban infrastructure boom of the 1920s ended as cities prioritized debt repayment. Local governments cut police and fire budgets, neglected transportation networks, and reduced education spending. The public workforce shrank more in high-leverage cities, with more educated, higher-skilled workers leaving at greater rates than in low-leverage areas. Furthermore, population growth slowed and property crime rose. Given the evidence that the quality and quantity of local public goods provide economic benefits to local economies, the costs of municipal financial distress were seemingly large (Glaeser, 2013).

I identify financial constraints through the debt maturity structure. The consequences of financial fragility during crises are well studied for households and firms, where heavily indebted agents typically suffer most (Chodorow-Reich, 2014; Mian et al., 2013). By contrast, less is known about leveraged local governments, despite the municipal bond market's $4.2 trillion size in 2025. My setting is particularly relevant: most cities had borrowed heavily to fund infrastructure for rural-to-urban migrants, so the Depression's collapse of property tax revenues and financial markets sharply increased the likelihood of distress. I examine how these constraints shaped spending and outcomes in growth, health, crime, and labor markets.

☆ Toni Whited was the editor for this article. I would like to thank Toni Whited and an anonymous referee for helpful guidance. I would also like to thank, without implicating, Scott Baker, Anthony DeFusco, Tracy Dennison, Carola Frydman, Dan Garrett, Chris Hair, Menaka Hampole, Phil Hoffman, Huidi Lin, Adam Jørring, Filippo Mezzanotti, Joel Mokyr, Charles Nathanson, Sarah Quincy, Jean-Laurent Rosenthal, Paola Sapienza, Andrew Sinclair, Laura Taylor, and Adriana Troiano, for helpful comments. This research has been supported by the Kellogg Dean's Office Research Grant, the Dissertation Fellowship from the Economic History Association, and the Linde Institute at Caltech. All remaining errors are my own. The replication file can be found at: https://doi.org/10.3886/E238470V3.

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https://doi.org/10.1016/j.jfineco.2025.104205
Received 29 October 2024; Received in revised form 5 November 2025; Accepted 7 November 2025
Available online 21 November 2025

This historical setting offers two advantages. First, local governments were the primary service providers, largely independent of higher-level fiscal support. Second, the Depression created a rare, severe shock to municipal budgets.[1] Because cities varied in their financial leverage, I exploit cross-city variation to study the role of financial constraints in shaping public goods. Importantly, the absence of large transfers avoids concerns about fiscal backstops that complicate modern analysis. Although today's system relies on countercyclical transfers, this arrangement is ultimately a political choice that, as history shows, could change.[2]

I construct a new historical dataset from archival sources on cities and municipal bonds. I digitize annual financial reports for nearly all cities over 1,000 residents in New York, Massachusetts, Ohio, Indiana, and California from 1924–1938 and 1941–1943, plus all U.S. cities over 100,000. These states and cities appear to be the only ones reporting annual data on local services at the time. I also observe debt amounts and assessed property values, which I combine into measures of financial leverage. Finally, I assemble a database of 28,000 municipal bonds from *Moody's Manual of Governments* (1929), the primary bond reference source at the time. With falling tax revenues, debt repayment became increasingly important. Highly leveraged cities were closer to default and thus more likely to be rationed by lenders because of information asymmetries (Bernanke, 1983; Stiglitz and Weiss, 1981). In fact, the leverage ratios I use are those state regulators and rating agencies applied to assess municipal creditworthiness at the time.

To estimate the effect on public goods, I compare spending in high- and low-leverage cities before and after 1929 using a difference-in-differences design. Cities at the 75th percentile of leverage cut service expenditures by 3–7 percentage points and capital investment by 15 percentage points relative to those at the 25th percentile. By the early 1940s, service spending converged, but capital gaps persisted. Highly levered cities also received lower credit ratings after—but not before—the Depression. Borrowing costs rose, which likely hindered infrastructure maintenance and upgrades.

I then distinguish two plausible mechanisms: refinancing tightness (higher leverage made borrowing harder when credit tightened) and investment-cycles (cities with high pre-Depression leverage may already have built heavily and thus had less need to invest). I study refinancing by exploiting the timing of bond maturities. The 1929 crash collapsed bond markets (Hillhouse, 1936), making it difficult to roll over maturing debt. Cities with more bonds due during 1930–1933 faced acute repayment shocks unrelated to contemporaneous investment demand. Using bond-level data, I compare similar cities with different shares of debt maturing in this window, following approaches in Almeida et al. (2009) and Benmelech et al. (2019). Results show cities with more maturing debt cut capital and service spending more. Cities in counties with banking panics also adopted deeper austerity, implying high intermediation costs. I further test demand-side explanations. Excluding low-demand cities, results remain: financing constraints, not investment cycles, drove the sharp and persistent declines in public goods during the 1930s.

I conclude with the effects on real outcomes: population, crime, health, and labor. A one–standard deviation increase in maturing debt predicts a persistent 0.9 (2.7) percentage point drop in population growth to 1940 (1950) and 107 more property crimes per 100,000 in 1933—about 4 percent of the 1930 mean. Micro-level data show a 1.3 percentage point higher likelihood of male workers leaving the public sector in high-debt cities. Overall, departing workers moved to lower-ranked occupations, but those leaving high-debt cities appeared to be positively selected: they were more educated, older, and higher paid by 1940.

This paper contributes to three literatures. First, while existing research has explored their impact of financial constraints on firms (Kaplan and Zingales, 1997; Fazzari et al., 1988) and macro policy (Gertler and Gilchrist, 1994; Bernanke et al., 1996), we know less about municipalities, especially in crises.[3] An exception is Cromwell et al. (2015), who studies Florida cities after the Great Recession. Closest to my work, Adelino et al. (2017) and Yi (2020) examine credit supply shocks and public goods today. I instead study constraints during a crisis, when large federal support was absent, highlighting asymmetric effects. Complementary to Yi (2020), I focus on municipalities and their labor force during a financial collapse, when cities were fiscally independent. Second, I add to the economic history of local government in the Depression, which has focused on New Deal programs such as the Federal Emergency Relief Administration and Works Progress Administration (Fishback and Wallis, 2012). These programs boosted retail consumption (Fishback et al., 2005), in-migration (Fishback et al., 2006), and crime reduction (Fishback et al., 2010). I instead study how *local* governments responded. Building on Siodla (2020), who analyzes fiscal strain in 93 large cities, I use new, comprehensive data to isolate debt-driven constraints from demand-side factors across a broader set of municipalities. Finally, I extend the literature on urban public economics and development in early 20th-century U.S. Cities' infrastructure investments improved health (Ferrie and Troesken, 2008; Cutler and Miller, 2005), spurred private economic activity (Kline and Moretti, 2014), and were closely tied to municipal debt (Gunter and Siodla, 2018). I show how financing arrangements behind these investments contributed to reduced public goods during the Depression.

The paper proceeds as follows. Section 2 describes historical context and institutions. Section 3 details the dataset. Section 4 tests whether leverage drove cuts in public goods. Section 5 explores mechanisms and outcomes. Section 6 concludes.

2. Historical background

This section outlines the institutional setting of public goods provision and debt in the U.S. during the first half of the 20th century, a pivotal period in American economic development. From 1900–1940, local and state governments were the main taxing authorities and largest public spenders. Only with the Depression and the World Wars did the U.S. shift toward a cooperative fiscal system, as the federal government began raising revenue from new sources (notably income and excise taxes) and redistributing funds to states and localities (Wallis, 1984).

Since the mid-19th century, cities, counties, and school districts undertook major infrastructure projects—schools, roads, utilities, police and fire departments, hospitals for the poor, jails, and libraries. This made local governments the largest spenders and debtors in the U.S. Before 1932, expenditure shares were roughly 50 percent local, 25 percent state, and 25 percent federal. By 1940, they shifted to about 10 percent local, 5 percent state, and 85 percent federal. The surge in federal spending reflected New Deal programs such as Social Security, Federal Emergency Relief Administration (FERA) and the Works Progress Administration (WPA), many of which were administered with state and local partners.

Fig. 1, using Commercial and Financial Chronicle data, shows average annual municipal bond sales in the interwar period. In the 1920s, yearly sales averaged $1.1 billion, far above the prior decade's $417

[1] The failure of municipalities to aid the unemployed (a quarter of the labor force) led Roosevelt to create a new fiscal regime centered on federal transfers, with the Works Progress Administration and Social Security Act greatly expanding federal involvement in the care for the unemployed and the elderly.

[2] Examples include President Ford's reluctance to bail out New York in 1975 ("Drop Dead") and President Trump's threats to withhold funds from cities in 2020.

[3] On firms in the Depression see Benmelech et al. (2019), Ziebarth (2013), Lee et al. (2015). On modern evidence see Chodorow-Reich (2014), Almeida et al. (2009).

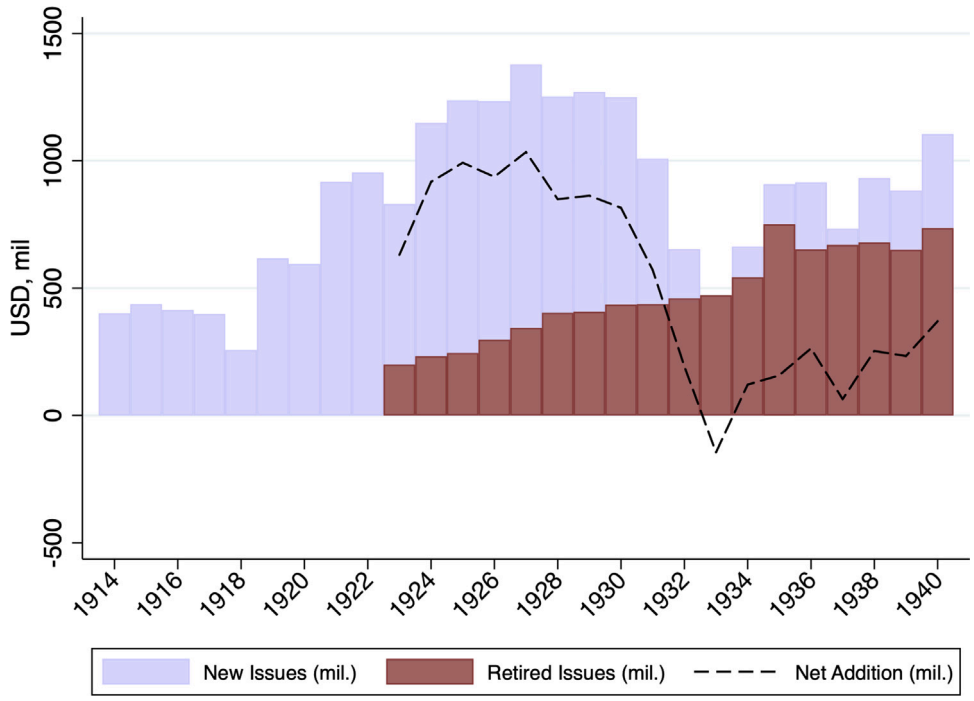

Fig. 1. Municipal debt sales and retirements.
Note: This figure plots the volume of municipal bond sales and retirement as reported by Hillhouse (1936) in Tables 1 and 5. The original source of the data is the State and Municipal Compendium (June issue of the *Commercial and Financial Chronicle*). The 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.

million. Three forces drove this infrastructure boom. First was the "high school movement": secondary school enrollment rose from 10 percent in 1900 to 70 percent by 1940 (Goldin and Katz, 1997), requiring new schools and equipment financed by bonds. Wealthy investors, insurance companies, and state savings banks eagerly bought these tax-exempt, legally favored bonds (Brown, 1922). Second, rural-to-urban migration raised density and demand for electrification, sanitation, and water systems. Third, automobiles and early suburbanization spurred heavy investment in paved roads and public transit.

Observers at the time also recognized the risks of debt-financed growth. On December 4, 1922, the Wall Street Journal warned: "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... The real estate values on which the present taxes are assessed are for the most part grossly inflated". Unlike firms that can exit during downturns, municipalities cannot be liquidated, but they can lay off workers and cut services—an outcome economists of the day also cautioned against (Upson, 1935).[4]

3. Data

3.1. City financials

I begin with the annual data on municipal finances, which contain over 12,000 observations on revenue, expenditure, and debt across 730 municipalities from 1924–1943. In 1930 these cities housed 44.7 million people, about 65% of the U.S. urban population. The median city had 8,000 residents, and the average panel length is 16.9 years. All dollar figures are deflated using the Consumer Price Index (Federal Reserve Bank of Minneapolis, 2020).

I digitize and standardize municipal financial statements from state agencies in Massachusetts, New York, Indiana, Ohio, and California—the only states that produced annual municipal statistics across this period. All report revenue, expenditures, and debt, though detail varies: Massachusetts provides the most granularity (taxes by source, bond issues and retirements), while Indiana aggregates taxes into a single line. California and Ohio report fine expense categories (e.g., administrative vs. inspection vs. police wages). Reporting is consistent within states, and all figures are actual payments and receipts. The Online Appendix describes sources in more detail, and the summary statistics appear in Table 1, Panel A.

I add Census data on large cities (population over 100,000) published in the *Financial Statistics of Cities*, available from FRASER. Coverage began in 1905 and, until 1931, included all cities above 30,000 before budget cuts curtailed reporting. Ninety-three cities are observed for all years in my period. When a city appears in both Census and state reports (e.g., Boston), I use the Census figures.

[4] There are several reasons why a local jurisdiction would hesitate to default 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 needed to obtain a "writ of mandamus" ruling from a state or federal court. A judge would first assess the legitimacy of the defaulted 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 could organize and sue. Once sued for default, a city was effectively barred from accessing capital from regulated fiduciaries such as insurance companies and state savings banks. Many state regulators produced legal lists identifying firms and public entities in which these institutions could invest. According to Hillhouse (1936) (pg. 419), 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 the late 1920s".

Table 1
Summary statistics.

Panel A: City Level Revenue and Expenditure, 1924–1943.

	N	Mean	SD	Median	25 pct	75 pct
Population (k)	12,602	40.83	154.02	7.95	4.99	19.56
Total revenue, excluding debt issuance	12,602	84.65	81.68	64.88	39.15	112.99
Tax revenue	12,602	47.45	38.66	32.64	21.02	66.82
All non-tax revenue (earnings)	12,602	26.46	51.04	18.91	8.67	34.94
Debt receipts	11,717	26.01	70.13	3.93	0.00	32.23
All other non-tax, non-debt receipts	12,602	10.74	21.31	2.81	0.00	13.79
Payments: Total service	12,602	48.75	39.42	33.09	21.30	68.58
Payments: government administration	12,602	4.72	3.59	4.04	2.71	5.79
Payments: health and sanitation	12,602	3.84	4.75	2.81	1.01	5.36
Payments: roads and highways	12,602	7.86	6.24	7.14	4.52	10.35
Payments: protection of persons and property	12,602	9.94	9.77	8.79	5.24	13.01
Payments: charities, welfare, and corrections	12,602	5.45	10.19	0.49	0.00	5.70
Payments: recreation	12,602	1.47	2.53	0.81	0.13	2.05
Payments: school and libraries	12,602	11.99	17.61	1.00	0.00	27.67
Other service payments	12,602	3.48	8.54	1.10	0.17	4.01
Public utilites	10,340	12.76	60.23	6.89	3.31	13.28
Interest	12,602	5.28	15.01	3.13	1.16	6.57
Capital outlays	12,602	12.50	24.36	4.69	0.78	15.50
All other non-maintenance, non-outlay payments	11,717	11.09	20.31	4.37	0.11	13.59
Total debt	12,599	123.87	312.22	73.23	32.62	148.89
Total bonded debt	12,602	111.14	305.27	67.47	28.25	132.04
Assessed value of property	9,833	2685.44	3209.81	2283.07	1679.40	3013.03
Defaulted 1930–1937 (any district)	12,602	0.13	0.34	0.00	0.00	0.00
Defaulted 1930–1937 (city)	12,602	0.12	0.33	0.00	0.00	0.00
Bond debt/assessed value	9,833	0.04	0.04	0.03	0.02	0.06
Interest payment/tax revenue	12,602	0.11	0.13	0.08	0.04	0.15
Debt/total revenue	12,599	1.41	1.56	1.11	0.55	1.91

Panel B: Bonds (1929)

	N	Mean	SD	Median	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	55 000

Panel C: Other City and County Data

	N	Mean	SD	Median	25 pct	75 pct
Sus. Bank Deposits (1930–33)	727	0.21	0.28	0.14	0.04	0.28
Δ Log(Loans 1931–29)	666	−0.10	0.19	−0.10	−0.17	−0.03
Debt age	382	8.13	2.87	7.79	6.38	9.64
Debt share, 1925–1929	382	0.34	0.23	0.32	0.16	0.51
Δ Log(Pop 1920–30)	746	0.24	0.30	0.17	0.04	0.36
City age in 1930	439	92.66	74.66	72.00	32.00	139.00
Total outlay 1924–29/capita	655	61.26	83.28	39.30	15.79	84.00
WPA grants/capita	733	50.05	30.26	45.66	29.56	63.51
RFC grants/capita	733	41.89	90.65	22.24	11.31	39.00
Murder per 100k	56	9.22	9.04	7.50	3.19	12.70
Rape per 100k	57	4.55	4.12	3.35	1.16	6.28
Robbery per 100k	58	107.67	81.25	105.01	33.29	157.17
Assault per 100k	57	58.69	59.25	37.46	17.23	76.55
Burglary per 100k	57	416.70	242.60	386.46	195.45	575.24
Auto theft per 100k	56	531.68	260.00	509.50	341.45	741.73
Communicable Disease Deaths per 100k	101	208.69	85.04	192.73	155.32	243.48
Δ Log(Pop 1930–40)	746	0.08	0.13	0.05	0.00	0.13

Note: Summary data for all observations across cities in the period 1924–1938, 1941–1943. Panel A: Population is in thousands. All dollar values (revenues, payments, debt) are in per-capita dollars deflated to 1961 using the CPI. The sample consists of all cities with at least 8 years of data in the sample time period. Panel B: Outstanding bonds for 332 cities as of 1929 from *Moody's Manuals of Governments*. Type "term" indicates balloon payment bonds. Panel C: Debt age and debt issued in 1925–1929 come from *Moody's* in 1929. Suspended bank deposit data comes from the Federal Deposit Insurance Corporation. Log loan growth of national banks at the county level comes from the Office of the Comptroller of the Currency. WPA and RFC data come from (Fishback et al., 2003). See text for sources for all other variables.

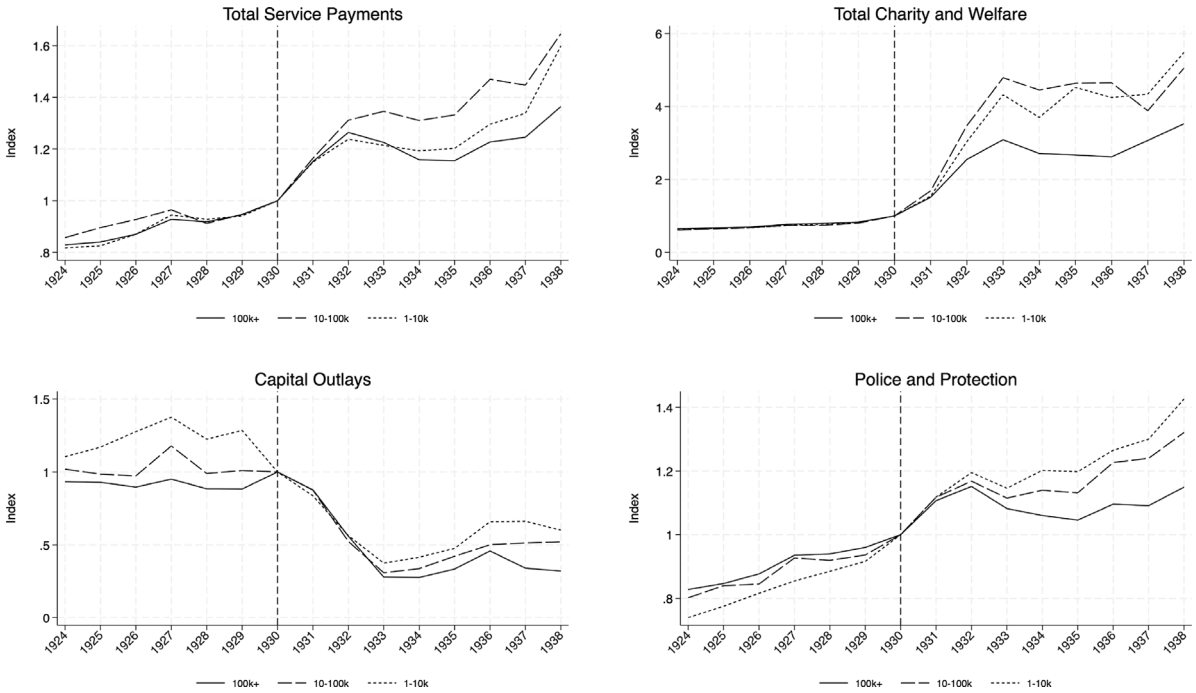

Fig. 2. Municipal services and outlays (1924–1938).
Note: This figure shows average per-capita real expenditures for small, medium, and large cities in Massachusetts, New York, Ohio, Indiana, and California, as well as all cities with a population of above 100 thousand between 1924 and 1938. Averages normalized to have a value of 1 in 1930. Short dash denotes the average for all cities with a population under 10 thousand (in 1930), long dash is the average for cities in 10–100 thousand range, and the solid line is for all cities with more than 100 thousand.

Finally, I hand-collect bond-level data from *Moody's Manual of Governments* for 1929. The *Manual* was sold to retail investors in the U.S. and contained quantitative security-level data, a qualitative review of major industries in a city, and Moody's credit rating. This source details debt structure, past borrowing, and future repayment schedules. For example, it records Chicago's 4% bond issued in 1920 with $50,000 still outstanding for repayment between 1936–1950. Overall, the data cover over 28,000 bonds across 316 cities. Summary statistics are in Table 1, Panel B.

3.2. Other city data

I merge city financials with city default data collected by Joffe (2012). From his database of approximately 5,000 defaults, I retain those on general city bonds, sewer projects, toll bridges, waterworks, and school districts between 1930–1937. I construct two binary variables: *default-city* equals 1 if a city defaulted on general obligation bonds; *default* equals 1 if either the city or one of its districts (e.g., Chicago Park District) defaulted. I add city and county characteristics from several sources: city population from the decennial Census, county bank suspensions (1930–1933) from the FDIC (FDIC, 1992), and county New Deal spending from Fishback et al. (2003). I use FBI Uniform Crime Reports for city-level annual crime (murder, rape, robbery, assault, burglary, auto theft), digitized for the 1930s by Fishback et al. (2010). I also draw annual cause-of-death data from Census Mortality Statistics (Janas, 2024) and construct communicable-disease deaths per 100,000.[5] Summary statistics appear in Table 1, Panel C.

Fig. 2 traces deflation-adjusted expenditures—total services, welfare, capital outlay, and police/fire. Values are normalized to 1930 = 1. Before turning to the expenditure trends, it is important to note that the early 1930s were marked by extraordinary deflation: the U.S. consumer price index fell by roughly 25 percent between 1929 and 1933. This collapse in the price level complicates interpretation of nominal versus real spending. For example, while nominal expenditures peaked in 1929 and then fell sharply, real per-capita spending (adjusted for prices) remained elevated until 1932–1933. Capital outlays saw the steepest and earliest cuts: on average 60% below 1929 levels, and by 1935 one-quarter of small cities had eliminated construction spending entirely. Other current (non-capital) expenditures were reduced more gradually. Police and fire services fell 20%, general government 10%, and health departments 15%.

3.3. Linked census records

My primary outcome variables on public workers come from 100% U.S. Census records. I focus on working-age adults (22–60) employed in local public administration or educational services (1950 industry codes 936 and 888)—the groups most directly affected by Depression-era cuts. I link records across Censuses using the Census Tree Project (Price et al., 2023a,b) and IPUMS (Ruggles et al., 2024).[6] The 1930–1940 linked sample covers 292,000 local government workers in cities with debt data, 51% male and about 37% of the 1930 urban public workforce. The sample overrepresents whites, older workers, and Pacific residents, so I apply inverse probability weights in my analysis. I construct three main outcomes over the decade: (i) an indicator for leaving the local public sector, (ii) the log change in occupational income score, and (iii) the change in occupational income percentile. I also use years of schooling, weekly wages, and geographic mobility from the 1940 Census in supplementary analyses.

[5] Including typhoid, malaria, smallpox, measles, scarlet fever, whooping cough, diphtheria, influenza, meningitis, tuberculosis, pneumonia, diarrhea, and polio.

[6] See Price et al. (2021), Buckles et al. (2023) for details.

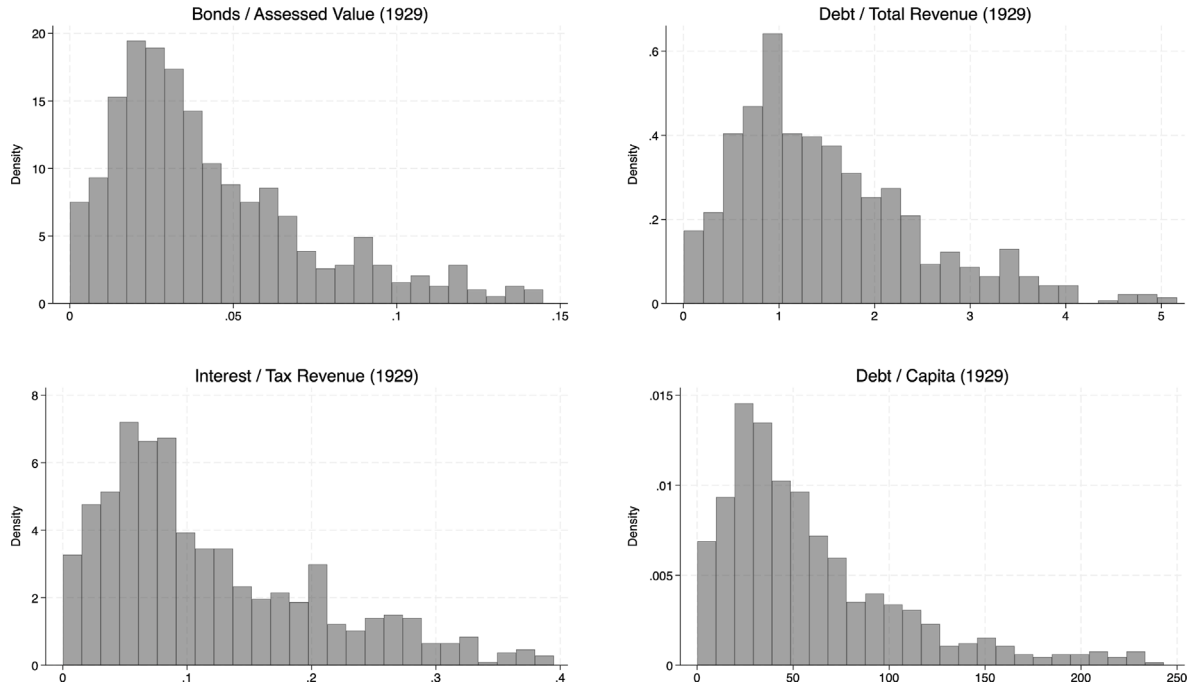

Fig. 3. Pre-Depression municipal financial leverage ratios.
Note: This figure plots the distributions of the four financial leverage ratios (trimmed at the 99 percentile) for all cities in the sample at the end of the 1929 fiscal year. Cities with no debt (roughly 8 percent of all cities) are not shown.

4. Leverage and expenditure during the great depression

With newly digitized city-level panel data on expenditures, debt, and bond maturities, I now turn to the core empirical analysis. This section asks whether variation in municipal leverage — measured before the Depression — predicts cross-city differences in fiscal adjustment during the 1930s. I test whether more indebted cities cut public goods more deeply and weigh the roles of refinancing credit constraints versus investment cycles.

4.1. Municipal financial leverage

Leverage, the ratio of debt to equity, amplifies returns but also risk. For municipalities, debt was mainly long-term bonds (15–50 years) and short-term loans repaid within the fiscal year. I use the property tax base as my main denominator, since property taxes were the bulk of city revenue. For robustness, I also use total revenue and population, and all definitions strongly predict municipal bond default during the Depression (see the Online Appendix). Fig. 3 shows leverage in 1929, excluding the 8% of cities with no debt. Each measure is highly skewed: the average city owed bonds equal to 4% of assessed property, devoted 12% of revenue to interest, carried debt equal to 1.53 times annual revenue, and had $52 debt per capita.

The variation in municipal leverage reflected historical debt issuance and repayment, asset assessments, and taxation levels. Issuance was limited by state constitutional rules and capital needs, while repayment depended on maturity structures and interest rates. Table 2 decomposes the variation in 1929 leverage using available, standardized covariates.

The two strongest predictors are state fixed effects and population (column 1), which together explain 18 percent of the variation in leverage, with historical leverage differences (columns 3–6) accounting for, roughly, an additional 30 percent. It is unsurprising that state fixed effects explain a significant share of leverage variation: states differed in their regulation of municipal debt issuance, in the legal investment lists for fiduciaries such as savings banks and insurance companies, and in urbanization patterns over the 19th century. Leverage persistence reflects that the average bond was issued in 1918. Given that cities issued debt to fund capital outlays, it follows that cumulative outlays from 1924 to 1929 (column 2) explain an additional 10 percent of the variation in leverage by 1929.

Fig. 4 shows the link between leverage and spending. I divide cities into top and bottom terciles by debt-to-property values, compute three-year rolling averages of real and nominal expenditures, and standardize to 1928. As previously discussed, the early 1930s saw extraordinary deflation: nominal spending peaked in 1929 then plunged, while real per-capita spending stayed high until 1932–1933. Importantly, however, the figure reveals no significant differences in public service expenditures between high- and low-leverage cities from 1925 to 1929. Beginning in the early 1930s, both types of public expenditure fell more sharply in "High Leverage" cities relative to "Low Leverage" ones, with the gap widening to over 20 percentage points for both service and capital expenditures by 1936 and persisting into the early 1940s.[7] Nominally, service spending in high-leverage cities turned from 3% annual growth before 1930 to modest decline; capital spending dropped even more sharply. The lower panel shows police staffing, measured in headcounts and unaffected by price changes. Staffing trends diverged: by 1936, high-leverage cities cut police by 6%, while low-leverage cities increased by 1%.

4.2. Empirical approach

To test the patterns in Fig. 4, I use the panel structure of the data and estimate a difference-in-differences model with five periods:

$$y_{it} = year_t + city_i + \theta X_{it} + \sum_{j \neq 1927,1928} \beta_j \times period_{j \in t} \times leverage_{29,i} + \epsilon_{it} \quad (4.1)$$

The coefficients of interest are β_j, which capture the average marginal change in spending outcomes between high- and low-leverage cities during five periods: j = 1924–1926 (pre-period),1927–1928

[7] The relative drop in real expenditure in the 1941–1943 period reflects inflation and federal restrictions on cities' purchases of heavy equipment and building supplies due to World War II.

Table 2
Determinants of pre-Depression financial leverage (1929).

	Bonds/Assess			Int/Rev	Debt/Rev	Debt/Capita
	(1)	(2)	(3)	(4)	(5)	(6)
Population (1929)	0.23***	0.23***	0.07*	0.05*	0.06**	0.11***
	(0.05)	(0.07)	(0.04)	(0.02)	(0.03)	(0.03)
Σ Outlay, 1924–29		0.36***	0.18*	0.21***	0.17*	0.69***
		(0.09)	(0.09)	(0.06)	(0.09)	(0.11)
Bonds/Assess (1924)			0.48***			
			(0.04)			
Int/Rev (1924)				0.50***		
				(0.05)		
Debt/Rev (1924)					0.67***	
					(0.04)	
Debt/Capita (1924)						0.56***
						(0.04)
State FE	✓	✓	✓	✓	✓	✓
R-sq	0.18	0.28	0.53	0.55	0.63	0.80
N	697	608	410	608	608	613

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the results of an OLS regression of financial leverage ratios in 1929 on covariates. All covariates were standardized to have a mean of zerp and a standard deviation of one to ease interpretation. Columns (1) – (3) use the bonded debt to assessed property value ratio while columns (4) – (6) use interest to tax revenue, debt to revenue, and debt per capita, respectively. Robust standard errors reported in the parentheses.

(reference),1929–1933 (early Depression),1934–1938 (late Depression), and 1941–1943 (post-Depression). Here, t denotes the year, and i denotes the city. The variable $leverage_{29,i}$ is the per-capita bonded debt to assessed property values. The dependent variables are log per-capita real city-level expenditures.

I estimate the regressions using OLS for all service expenditure outcomes. Because a non-trivial share of cities report zero capital outlays in some years, I estimate the capital outlay regressions using Poisson pseudo-maximum likelihood (PPML). The fixed effect $city_i$ controls for time-invariant city-specific characteristics that may also affect spending levels (e.g., geography), while $year_t$ accounts for macroeconomic shocks that are common across all cities (e.g., monetary policy).

In my preferred specification, the control vector X_{it} includes region-by-year fixed effects to absorb region-specific business cycle fluctuations (Rosenbloom and Sundstrom, 1997); contemporaneous and lagged non-debt revenues to account for Depression-era income shocks and the intertemporal budgeting process; log city population in 1930 interacted with year fixed effects; and the change in log population between 1920 and 1930 to control for heterogeneity in city size and pre-Depression growth. Standard errors are clustered at the city level.

A causal interpretation of β_j requires the assumption that, in the absence of the Great Depression, differences in public good provision would have evolved similarly across cities with differing levels of financial leverage, and that leverage itself was quasi-randomly assigned prior to the onset of the crisis. One testable implication of the parallel trends assumption is that pre-Depression provision should evolve similarly across cities, which can be assessed via the estimated coefficients on the 1924–1926 period interactions in Eq. (4.1). As for the plausibility of exogeneity, this section reports estimates that condition on region-level time trends and city-level population growth, both of which explain a large and predictable share of leverage variation. The remaining unexplained variation plausibly reflects long-run debt issuance decisions uncorrelated with near-term outcomes. In Section 5, I further strengthen identification by analyzing a quasi-random assignment of exogenous cash flow shocks based on bond-level repayment schedules.

4.3. Baseline results

4.3.1. Leverage and expenditure

Table 3 reports results with service expenditure and debt revenue (Panel A) and capital outlays (Panel B) as dependent variables. Column (1) includes year and city fixed effects. Columns (2)–(3) add population size groups in 1930 and 1920–1930 growth. Column (4) adds current and lagged non-debt revenue, and Column (5) includes Census region-by-year fixed effects.

Across all models, there are no significant pre-trends. Once the Depression began, spending diverged sharply: in Column (5), Panel A, a one standard deviation increase in leverage predicts a 2-log-point drop in service expenditure during the early Depression and 7 log points in the later period. Column (6) shows a persistent 26–35 log-point decline in debt revenues after 1929.

The effects on capital outlays in Panel B are even more pronounced. Again, across all specifications, there is no evidence that highly leveraged cities increased capital investment prior to the Depression, conditional on the full set of covariates. Column (5) shows that a one standard deviation increase in leverage is associated with an 10-log-point decrease in annual capital outlays, or roughly 30 percent of the average decrease in the early Depression period. By the early 1940s, the effect grows to 21 log points.

4.3.2. Composition of public goods and cost of capital

I next examine spending composition, showing that falling capital outlays coincided with rising borrowing costs in high-leverage cities.

Fig. 5 shows annual event-study estimates. The steepest leverage-related declines occur in capital spending, especially 1934, but extend to police, fire, and road maintenance. Pre-Depression trends are flat across leverage levels. After 1929, spending fell about 6 log points for roads and 10 for police per standard deviation of leverage. By 1933, administrative budgets were also cut. By 1943, leverage effects on non-capital spending had largely faded.

The persistent capital cuts in high-leverage cities coincided with rising borrowing costs. I proxy costs with municipal credit ratings from Moody's Manuals of Governments (1929–1938), converting letter grades to numeric scores. Fig. 6 shows that while both terciles held AAA ratings before the Depression, ratings diverged by 1933. By 1936, high-leverage cities were rated on average 0.8 notches below low-leverage ones.

I conduct several robustness checks, with results reported in the Online Appendix. First, I replicate the analysis using three alternative leverage definitions, confirming that the results hold across specifications. Second, I address the concern that court-mandated austerity in defaulting cities, rather than general fiscal stress in highly indebted

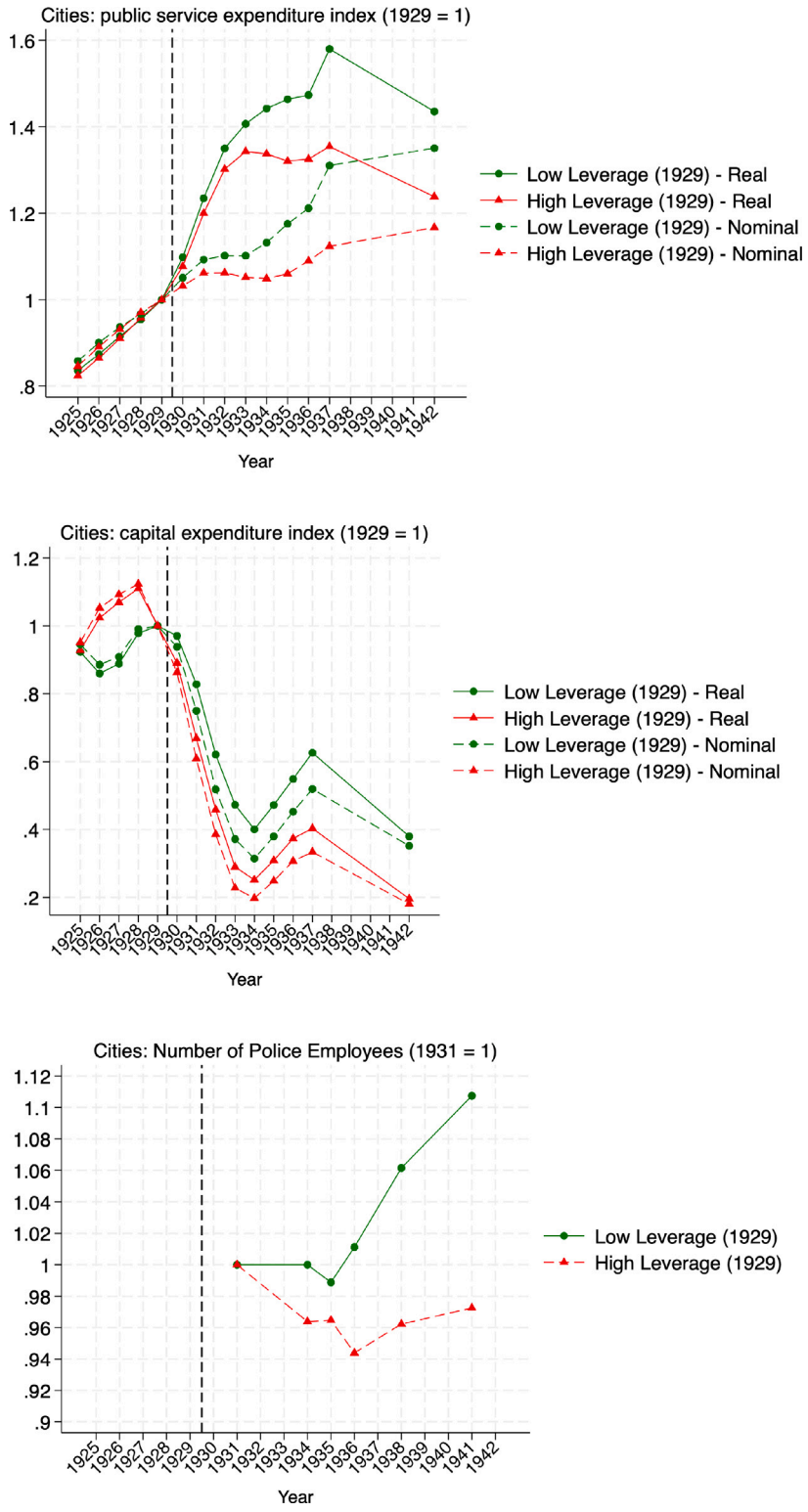

Fig. 4. Leverage and local public goods during the great depression.
Note: This figure plots a 3-year rolling average [t-1, t, t+1] of total public service expenditure (top) and capital outlay expenditure (middle) in cities by leverage. The bottom figure plots the average employment in police department by year. "Low Leverage" captures cities in the first tercile of bonded debt/assessed property value in 1929 1929 and "High Leverage" denotes those in the third tercile. All averages are normalized to 1 in 1929 (expenditure) or 1931 (police counts).

cities, might drive the observed patterns. Excluding all cities that entered formal default between 1930 and 1937 yields nearly identical results. Third, I explore potential non-linearities by replacing the continuous leverage variable with a binary indicator for above- and below-median leverage; the main results remain unchanged. Finally, I confirm that the findings are not driven by differences in data source coverage: excluding cities from each data source one at a time does not materially affect the results.

In summary, the evidence indicates that financially constrained cities made deeper cuts to public goods provision during the Great

Table 3
Leverage and local public expenditure.

Panel A: Financial leverage (1929) and service expenditure						
	Outcome: Log(Service Payments/Capita)					Log(Debt Receipts/Capita)
	(1)	(2)	(3)	(4)	(5)	(6)
leverage × 1924–1926	0.00	0.00	−0.00	−0.01	−0.01	−0.06
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.07)
leverage × 1929–1933	−0.02***	−0.02***	−0.03***	−0.02***	−0.02***	−0.26***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.05)
leverage × 1934–1938	−0.09***	−0.09***	−0.09***	−0.08***	−0.07***	−0.36***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.06)
leverage × 1941–1943	−0.05***	−0.04***	−0.04***	−0.02	−0.01	−0.35***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.08)
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
1930 Pop × Year		✓		✓	✓	✓
Δ1920-30 Pop × Year		✓	✓	✓	✓	✓
Pop Group × Year			✓			
Revenue				✓	✓	✓
Region × Year					✓	✓
R-sq (within)	0.42	0.45	0.46	0.58	0.62	0.15
N	12,305	12,305	12,305	10,903	10,903	6,911
Mean(y)	3.60					
SD(y)	0.74					

Panel B: Financial leverage (1929) and capital outlay					
	Outcome: Outlay/Capita				
	(1)	(2)	(3)	(4)	(5)
leverage × 1924–1926	−0.02	−0.01	−0.02	0.04	0.05
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)
leverage × 1929–1933	−0.11**	−0.09**	−0.12***	−0.08*	−0.10**
	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
leverage × 1934–1938	−0.24***	−0.17***	−0.18***	−0.14***	−0.17***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
leverage × 1941–1943	−0.38***	−0.32***	−0.31***	−0.29***	−0.21***
	(0.09)	(0.07)	(0.08)	(0.08)	(0.08)
City FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
1930 Pop × Year		✓		✓	✓
Δ1920–30 Pop × Year		✓	✓	✓	✓
Pop Group × Year			✓		
Revenue				✓	✓
Region × Year					✓
R-sq (pseudo)	0.54	0.55	0.55	0.58	0.60
N	12,305	12,305	12,305	10,903	10,903
Mean(y)	12.09				
SD(y)	23.33				

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the estimation results of Eq. (4.1) where the outcome variable in Panel A is log per-capita total service expenditure in columns (1) – (5) or log per-capita revenue generated from debt issuance ("debt receipts") in column (6), both estimated using OLS. In panel B, the outcome variable is per-capita capital outlays, and the model is estimated using Poisson pseudo-maximum likelihood. Standard errors are clustered at the city level. Column (1) contains only city and year fixed effects, while columns (2)–(5) add 1930 population-year fixed effects, 1920s population growth-year fixed effects, contemporaneous and lagged log total non-debt per capita revenue, and region-year fixed effects. Leverage is standardized (mean zero and standard deviation of one) bonded debt over assessed property values in 1929.

Depression. These effects are robust across definitions of leverage, only emerge after the onset of the Depression, and persist across multiple outcomes. I next examine the mechanisms behind these patterns.

5. Mechanisms

I examine two channels linking leverage to public good provision during the Depression: a refinancing (supply-side) channel and an investment-cycle (demand-side) channel. I first show that plausibly exogenous refinancing shocks primarily drove the 1930s expenditure cuts, then test demand-side explanations by excluding low-demand cities and find limited support.

5.1. Leverage and refinancing debt

To separate short- from long-run cash-flow constraints, I decompose leverage and show that the inability to refinance debt was central to fiscal retrenchment. I proceed in two steps: (i) forecast required bond repayments and show effects concentrate where refinancing needs were large; and (ii) demonstrate larger spending declines where banking panics were more severe (1930–1933).

I exploit the quasi-exogenous maturity structure of long-term municipal bonds. Staggered maturities (5, 10, 30, 50 years) create variation in repayment timing. Two identification advantages follow. First, bonds issued well before 1930 (e.g., a 1911 issue maturing in 1931) required

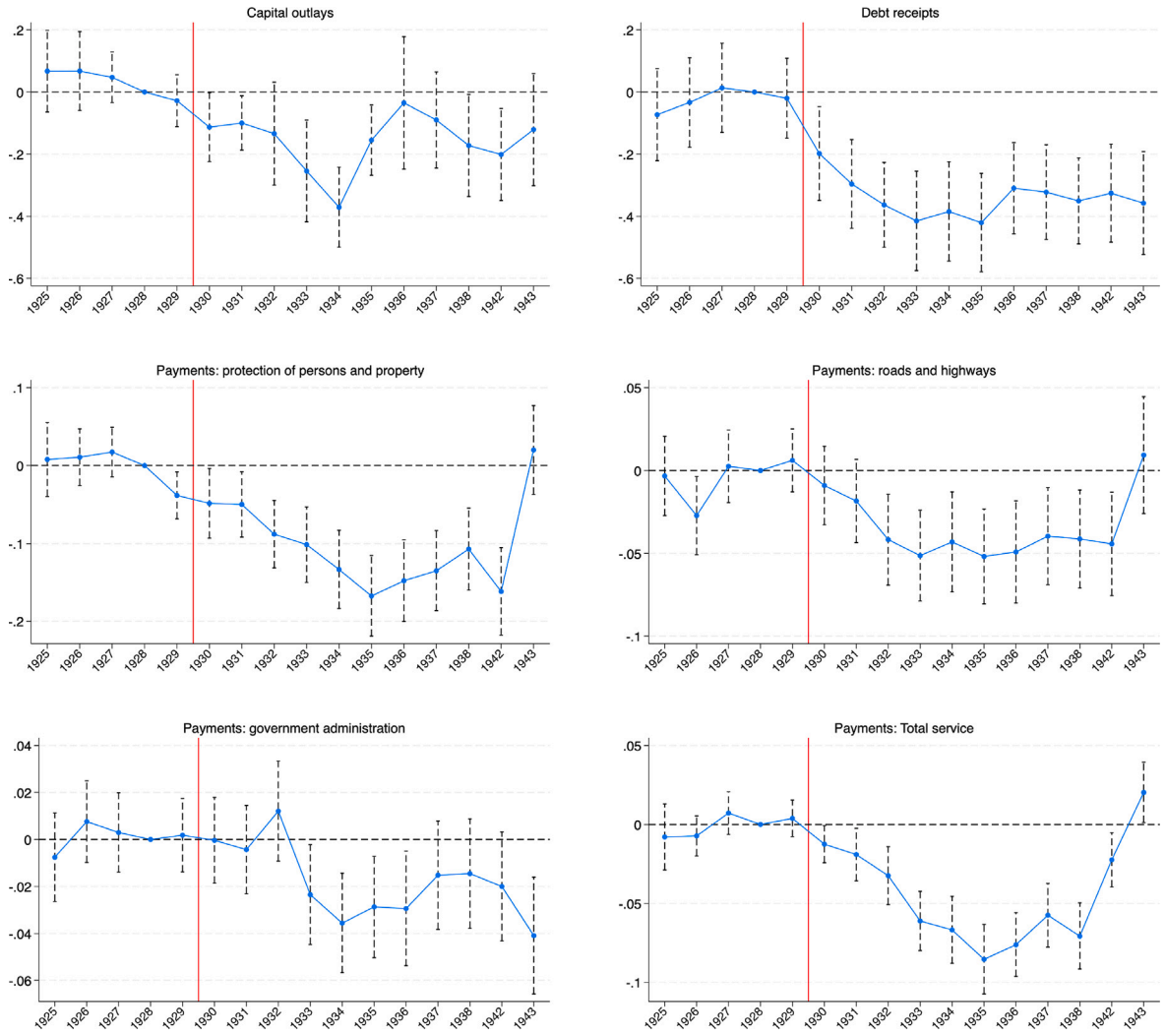

Fig. 5. Leverage and various categories of expenditure and debt revenue.
Note: This figure shows the estimated coefficients on $period_{j=t} \times leverage_{29,i}$ in Eq. (4.1) where period denotes each individual year using bonded debt to assessed property values as the leverage measure. Payment panels are estimated using OLS and Capital outlays panel is estimated using Poisson pseudo-maximum. Total service expenditure excludes capital expenditure or financing costs. Roads and highways refers to all expenditure for the maintenance of public roads and highways. Protection of persons and property denotes police, jails, and firefighting costs. Capital outlay is 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 red vertical line denotes the official start of the Great Depression in the U.S.

refinancing during the crisis, largely independent of contemporaneous policy. Second, durations reflected market conventions and state rules rather than city expectations (Chamberlain, 1928), limiting local endogeneity. Consistent with this, I find that historical bond rates do not differ across cities with more versus less early-1930s maturities, an issue I revisit in the next section

I implement this strategy using the 1929 "Schedule of Bonded Debt" in Moody's Manual of Governments, which lists each bond's issue year, maturity, 1929 outstanding amount, interest rate, and purpose. The data cover 28,000+ bonds across 316 cities. To reduce measurement error, I aggregate Moody's bonds and compare totals to 1929 city balance sheets, retaining cities where listed bonds are at least 90% of reported bonded debt and Moody's totals are within 20% of city reports. These filters align forecasted repayments with actual constraints. The Online Appendix details the validation process.

5.1.1. Repayment forecasts

I construct forward-looking estimates of long-term bond repayments due during a "bad state" (the early 1930s) as a proxy for debt-driven financial constraints. Specifically, I define a repayment "shock" as the fraction of total bonded debt maturing between 1930 and 1935:

$$shock_{30,j} = \frac{\sum_{t=1930}^{1935} \sum_{\forall i \in j} repay_{i,t}}{\text{Total Debt}_{29}} \tag{5.1}$$

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

To address concerns about systematic differences, I show that cities above and below the median of $shock_{30,j}$ are comparable across key observable characteristics. The Online Appendix reports averages for population, tax revenue, property values, local public good expenditures, and historical bond interest rates during the 1920s for the two groups. While above-median cities are somewhat smaller, they are otherwise statistically similar to below-median cities. Crucially, there are no differences in the interest rates of 1920s-issued bonds, indicating that $shock_{30,j}$ does not proxy for differences in historical credit risk or unobserved fiscal conditions.

To illustrate the identifying variation, Fig. 7 plots average repayment profiles by quartile of $shock_{30,j}$. Top-quartile cities repaid

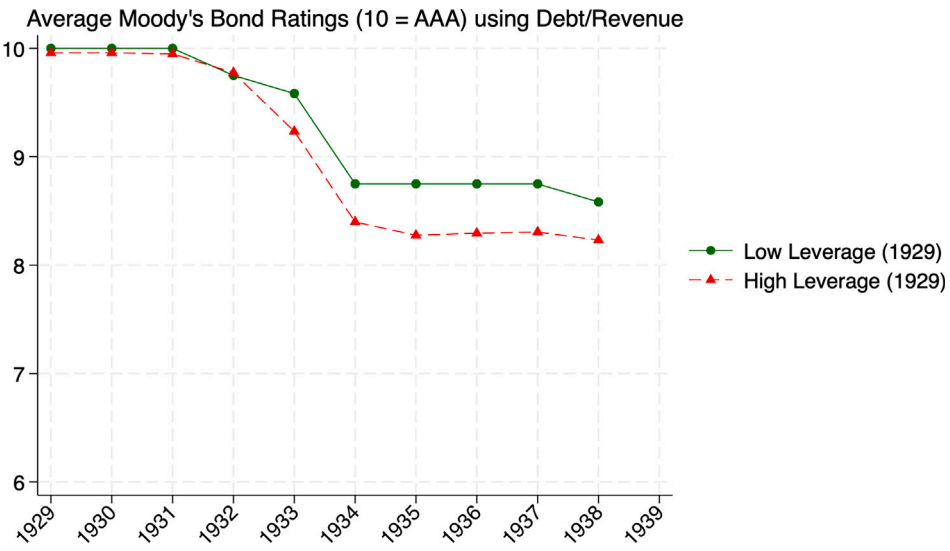

Fig. 6. Moody's credit ratings.
Note: This figure plots the average $Moody's$ Bond rating of cities by leverage. Low leverage is defined as the first tercile of bonded debt/assessed property value1929 and high leverage is denoted by the third tercile. The sample includes 189 cities with complete ratings data from 1929 to 1938. I assign numbers to ratings by subtracting one from each subsequent level, where AAA takes the value of 10, AA takes the value of 9, etc.

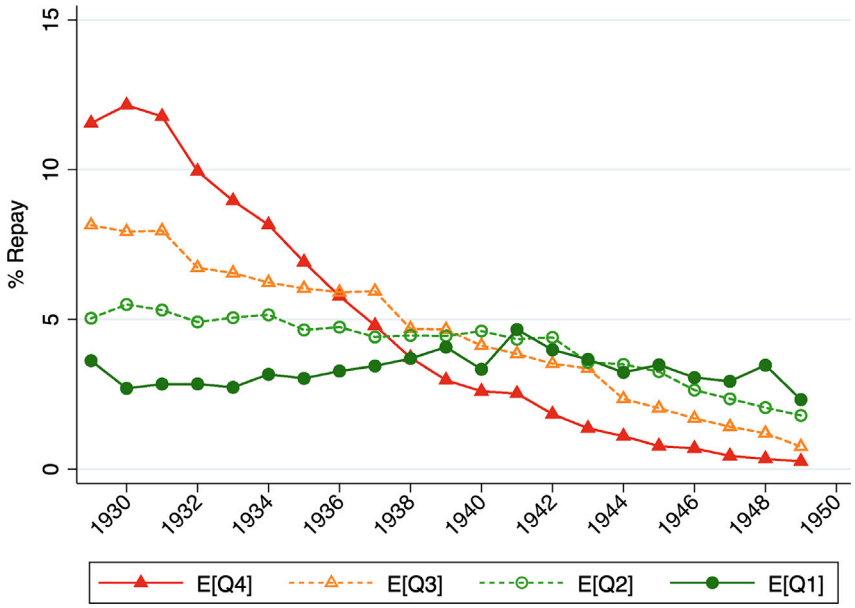

Fig. 7. Annual repayment based on repayment shock quartile.
Note: This figure shows the average annual repayment of bonds across 1930–1935 city repayment quartiles. Repayment quartiles are computed in 1929 and remain static by city. For example, the solid red line with triangle markers shows the average percentage of bonds that were contractually obligated to be repaid in each year for those cities in the largest projected repayment quartile as of 1929, while the solid green line with circle markers shows it for those in the lowest repayment quartile.

5%–12% of total debt annually in the early 1930s, then less thereafter; bottom-quartile cities maintained 3%–4% annually through mid-century. The comparison is thus between front-loaded versus evenly distributed maturities.

To isolate the plausibly exogenous component of leverage, I interact each leverage measure with $shock_{30,j}$, defining the resulting variable as $moodyleverage$. This leads to the following modification in my main specification:

$$y_{it} = year_t + city_i + \theta X_{it} + + \sum_{j \neq 1928} \omega_j \times period_{j \in t} \times moodyleverage_{29,i} + \epsilon_{it} \quad (5.2)$$

The coefficients of interest, ω_j, capture the marginal effect of financial leverage conditional on having a high refinancing burden between 1930 and 1935.

Table 4 reports results using the repayment shock, with log real per-capita service expenditure (Panel A) and capital outlays (Panel B). Columns (1)–(4) use four leverage definitions; all models control for population size and growth, non-debt revenue (current and lagged), and region-by-year effects.

Across all specifications, standardized $moodyleverage$ coefficients account for a large share of baseline leverage effects in 1929–1938. The attenuation after 1940 likely reflects the dissipation of the 1930–1935

Table 4
Bond repayment and expenditure.

Panel A: Moody leverage and service expenditure				
	Bonds/Assessed value	Int/Rev	Debt/Rev	Debt/Capita
	(1)	(2)	(3)	(4)
moodyleverage × 1924–1926	0.02	0.01	0.01	−0.00
	(0.03)	(0.02)	(0.03)	(0.01)
moodyleverage × 1929–1933	−0.03***	−0.01	−0.02**	−0.00
	(0.01)	(0.01)	(0.01)	(0.01)
moodyleverage × 1934–1938	−0.04**	−0.02	−0.04**	−0.03*
	(0.02)	(0.02)	(0.02)	(0.02)
moodyleverage × 1941–1943	−0.03	−0.04	−0.06**	−0.01
	(0.02)	(0.02)	(0.02)	(0.02)
City FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
1930 Pop × Year	✓	✓	✓	✓
Δ1920–30 Pop × Year	✓	✓	✓	✓
Revenue	✓	✓	✓	✓
Region × Year	✓	✓	✓	✓
R-sq (within)	0.67	0.67	0.68	0.71
N	3,810	3,827	3,829	3,794
Mean(y)	4.06	4.07	4.07	4.05
SD(y)	0.62	0.61	0.61	0.61

Panel B: Moody leverage and capital outlay				
	Bonds/Assessed Value	Int/Rev	Debt/Rev	Debt/Capita
	(1)	(2)	(3)	(4)
moodyleverage × 1924–1926	−0.01	−0.09	−0.11**	0.05
	(0.05)	(0.06)	(0.05)	(0.06)
moodyleverage × 1929–1933	−0.27***	−0.16**	−0.19***	−0.02
	(0.05)	(0.07)	(0.07)	(0.06)
moodyleverage × 1934–1938	−0.29***	−0.17**	−0.36***	−0.19***
	(0.07)	(0.08)	(0.08)	(0.07)
moodyleverage × 1941–1943	−0.16	−0.12	−0.09	−0.12
	(0.13)	(0.15)	(0.12)	(0.09)
City FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
1930 Pop × Year	✓	✓	✓	✓
Δ1920–30 Pop × Year	✓	✓	✓	✓
Revenue	✓	✓	✓	✓
Region × Year	✓	✓	✓	✓
R-sq (pseudo)	0.59	0.58	0.59	0.53
N	3,810	3,827	3,829	3,794
Mean(y)	18.01	18.25	18.23	17.03
SD(y)	30.98	31.18	31.15	21.80

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the estimation results of Eq. (5.2) where the outcome variable is log per-capita total service expenditure (Panel A, estimated using OLS) or real per-capita capital outlay (Panel B, estimated using Poisson pseudo-maximum). Control variables as the same as in Table 3. Pre-Depression Moody financial leverage variable is specified in the header. Standard errors are shown in parentheses and are clustered at the city level.

refinancing burden rather than the end of constraints; longer-run frictions (debt overhang, deferred maintenance, fiscal conservatism) may have persisted, and future work using more comprehensive bond-level datasets could help disentangle these mechanisms in the later 1940s. Overall, the results indicate that refinancing constraints during the Depression played a significant role in driving declines in local public expenditure.

5.1.2. Leverage and local banking conditions

Refinancing risk also arose from local bank distress. If local credit supply tightened, cities would struggle to finance capital projects. I test whether leverage effects were larger in counties with severe banking panics and find stronger impacts after 1933: high-leverage cities in panic counties cut capital outlays by about 30% more than those in non-panic counties.

I measure county-level banking stress using two proxies: (i) the log change in total loans for nationally chartered banks from OCC reports, 1929 to 1931 (the last county-level year), and (ii) the FDIC share of deposits suspended,1930–1933. To estimate the interaction between financial leverage and banking distress, I implement a triple difference-in-differences (DDD) framework. Specifically, I interact the two banking distress proxies with leverage, time period, and leverage-by-period terms from Eq. (4.1). Table 5 reports PPML estimates for real per-capita capital outlays.

In column (1), at the mean suspended-deposit share (18%), leverage reduces 1934–1938 capital outlays by an additional $0.06 per capita (0.36 × 0.18) relative to counties without suspensions — about 38% of the average effect in Table 3, Panel B — and the effect persists into the early 1940s. Column (2) shows similar magnitudes using the loan-decline proxy.

Could city defaults cause bank failures? Reverse causality appears limited. First, contemporaneous surveys underscore how bank closures constrained cities: in 1933, over half of 1,000+ governments reported

Table 5
Leverage, banking conditions, and capital outlays.

	Outcome: Outlay/Capita	
	(1)	(2)
leverage × 1924–1926 × suspended bank deposits	0.02	
	(0.12)	
leverage × 1929–1933 × suspended bank deposits	−0.04	
	(0.11)	
leverage × 1934–1938 × suspended bank deposits	−0.32*	
	(0.17)	
leverage × 1941–1943 × suspended bank deposits	−0.43**	
	(0.20)	
leverage × 1924–1926 × Δ bank loan growth		−0.19
		(0.15)
leverage × 1929–1933 × Δ bank loan growth		0.14
		(0.16)
leverage × 1934–1938 × Δ bank loan growth		0.33**
		(0.16)
leverage × 1941–1943 × Δ bank loan growth		0.24
		(0.17)
City FE	✓	✓
Year FE	✓	✓
1930 Pop × Year	✓	✓
Δ1920–30 Pop × Year	✓	✓
Revenue	✓	✓
Region × Year	✓	✓
Leverage × Period	✓	✓
Suspended Bank Deposits × Period	✓	
Δ loan growth × Period		✓
R-sq (within)	0.60	0.60
N	10,633	10,359
Mean(y)	11.88	12.09
SD(y)	23.42	23.66

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the Poisson pseudo-maximum estimation results of Eq. (4.1) augmented with a triple interaction term of leverage × period × county-level share of national bank deposits suspended during 1930–1933 (column 1) and county log national bank loan growth between 1929 and 1931 (column 2). The outcome variable is real per-capita capital outlay. Control variables include population, population growth, contemporaneous and lagged log total non-debt per capita revenue, region by year fixed effects, and the interaction terms denoted in the table. Leverage is standardized (mean zero and standard deviation of one) bonded debt over assessed property values in 1929. Standard errors are shown in parentheses and are clustered at the city level.

public funds trapped in closed banks (Faust, 1934), totaling $98 million; estimated losses across governments reached $400–$500 million—about 2% of outstanding municipal principal. Second, Richardson (2007) show municipal bond depreciation explains only 24% of bank suspensions, and municipal/state bonds comprised merely 10%–15% of all bank securities in 1929. These facts imply city defaults were unlikely the main cause of banking collapses.

In sum, constraints in long-term bond markets and curtailed bank lending significantly amplified how municipal leverage translated into spending cuts during the Depression.

5.2. Leverage and demand

A second mechanism comes from the demand-side. For capital spending, cities that invested heavily earlier may have needed less new infrastructure once their finances worsened. Highly indebted cities could therefore have lower marginal demand for additional capital outlays in the 1930s, independent of the Depression or financing constraints.

A canonical example of this mechanism is the Florida land boom and bust of the 1920s. Drawn by warm weather and cheap land, migrants flooded into Florida's coastal cities. Miami, for instance, grew from under 30,000 residents in 1920 to over 110,000 by 1930. In response, local governments heavily invested in infrastructure such as schools, roads, and drainage systems, anticipating continued growth. This expansion was largely debt-financed: local government debt rose from $23 per capita in 1912 to $337 by 1931 (Joffe, 2012). However, during the Depression, tourism and in-migration collapsed, and with most infrastructure already in place, local governments sharply curtailed further capital spending.

I test whether similar boom–bust dynamics elsewhere drive the results by building proxies for *ex ante* infrastructure demand and re-estimating the baseline model on targeted subsamples. If demand explained the patterns, leverage effects should attenuate once low-demand cities are excluded. However, I find no consistent evidence that such mechanisms substantially weaken the leverage effect. Fig. 8 reports the heterogeneity analysis.

First, I proxy forward-looking demand with recent issuance from Moody's Manual (1929): (1) the share of outstanding debt issued in 1925–1929; and (2) the average (principal-weighted) bond age in 1929. High (1) implies much new infrastructure already in place; low (2) signals recently issued bonds—both consistent with lower marginal demand.

Panel A shows estimates from Eq. (4.1) by subsample with 90% confidence intervals. Relative to the full "Base" sample, both "Above-median 1925–1929 issuance" and "Below-median bond age" groups yield effects that are statistically and quantitatively similar to baseline. Recent investment therefore did not materially mute responsiveness to leverage. Panel B uses city age (1930 minus incorporation year; median = 57) as a long-run demand proxy. Effects in younger cities are somewhat smaller and less precise, but the leverage effect persists — and often strengthens — in older, denser, more established places,

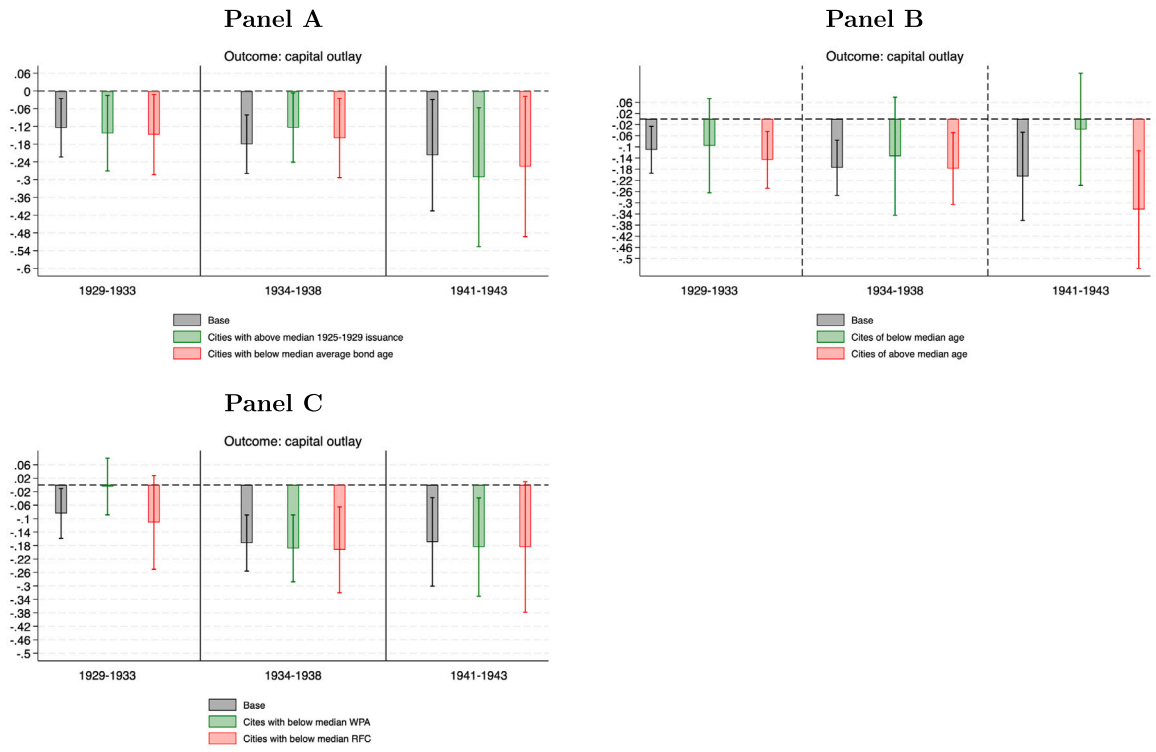

Fig. 8. Excluding low-demand cities.
Note: This figure shows the estimated coefficients on $period_{j=t} \times leverage_{29,i}$ in Eq. (4.1) for the full sample of cities (gray) vs. subsamples of cities based on various measures of forward-looking infrastructure demand. The outcome variable is real per capita capital outlay and the regressions are estimated using Poisson pseudo-maximum. In Panel A, the green bars denote the estimates when cities with above-median share of bonds issued during 1925–1929 form the sample, while the red bars denote them when cities with below-median age of bond portfolio age are used. In Panel B, the green and red bars denote the estimates when cities with below and above median age are used, respectively. Finally, in Panel C, I exclude cities in counties with above-median New Deal and Reconstruction Finance Corporation expenditures. The standard errors are clustered at the city level and the ninety percent confidence intervals are denoted by the lines.

Table 6
Leverage and population growth, death rates, and crime.

	1930–1940	1930–1950	1928–1934	1930–1933		1930–1937	
	Δ Pop. growth	Δ Pop. growth	Δ Death rate	Δ Property crime	Δ Violent crime	Δ Property crime	Δ Violent crime
Moody Leverage	−0.07**	−0.09**	0.10	0.39**	0.17	−0.06	0.01
	(0.04)	(0.04)	(0.10)	(0.17)	(0.17)	(0.14)	(0.13)
Retail Sales	✓	✓	✓	✓	✓	✓	✓
Population	✓	✓	✓	✓	✓	✓	✓
Revenue (1930)	✓	✓	✓	✓	✓	✓	✓
R-sq	0.32	0.33	0.08	0.14	0.09	0.19	0.04
N	369	364	97	57	57	53	53
Std[Y]	0.13	0.30	41.72	274.97	50.99	350.15	64.85

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the estimation results of an OLS regression where the outcome variable and the time period are specified in the column header. See text for details. All outcome variables are standardized to have mean zero and standard deviation one to ease interpretation. Control variables include log city population in 1930, population growth between 1920 and 1930, city expenditure in 1930, and log per capita change in county-level retail sales between 1929 and 1933 obtained from Fishback et al. (2003). Robust standard errors are shown in parentheses.

counter to a demand-only explanation. Panel C addresses potential crowd-out from New Deal infrastructure spending (WPA, RFC). Re-estimating on cities in below-average federal-spending counties yields estimates that track baseline after 1933, implying municipal cuts are not simply substitution away from federal support.

In sum, I find no compelling evidence that demand-side factors account for the observed declines in municipal capital spending during the Depression. Across proxies — recent issuance, bond maturity, city age, and New Deal exposure — pre-Depression leverage remains a robust predictor of post-1929 reductions in public goods.

5.3. Impact on growth, health, and crime

The spending cuts tied to high pre-Depression debt likely carried real costs. Prior work links infrastructure to regional activity (Kline and Moretti, 2014) and sanitation to lower waterborne mortality (Cain and Rotella, 2001). Reduced police funding may raise crime; deferred capital may deter migrants and firms via higher transport costs, degraded infrastructure, or unreliable utilities. I test whether leverage-induced cuts translated into adverse outcomes for population growth, communicable-disease mortality, and crime in the 1930s.

Table 7
Impact on the local public workforce.

Panel A: 1930–1940 Changes							
	Outcome: I(Left Public)			Δ Log(Occscore)		Δ Rank(Occscore)	
	All (1)	Males (2)	Females (3)	Males (4)	Males (5)	Males (6)	Males (7)
Moody Leverage	0.004	0.013***	−0.005		−0.002*		−0.002
	(0.003)	(0.004)	(0.003)		(0.001)		(0.094)
I(Left Public)				−0.042***	−0.047***	−2.088***	−2.371***
				(0.004)	(0.004)	(0.284)	(0.318)
Moody Leverage × I(Left Public)					0.008***		0.489***
					(0.002)		(0.149)
Retail Sales	✓	✓	✓	✓	✓	✓	✓
Population	✓	✓	✓	✓	✓	✓	✓
Revenue (1930)	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓
Household	✓	✓	✓	✓	✓	✓	✓
R-sq	0.08	0.06	0.10	0.01	0.01	0.01	0.01
N	292,238	175,335	116,903	161,213	155,093	161,213	155,093
Mean(y)	0.46	0.54	0.41	−0.02	−0.02	−0.46	−0.44
SD(y)	0.50	0.50	0.49	0.29	0.29	23.78	23.80

Panel B: 1940 Outcomes								
	Age (1940)		Years of schooling (1940)		Log(Weekly wages) (1940)		I(Moved out of city) (1940)	
	Males (1)	Males (2)	Males (3)	Males (4)	Males (5)	Males (6)	Males (7)	Males (8)
I(Left Public)	3.005***	2.869***	−1.617***	−1.660***	−0.358***	−0.365***	0.140***	0.143***
	(0.171)	(0.175)	(0.054)	(0.061)	(0.008)	(0.008)	(0.007)	(0.007)
Moody Leverage		−0.091		0.005		0.014**		0.008
		(0.080)		(0.044)		(0.006)		(0.005)
Moody Leverage × I(Left Public)		0.206***		0.063**		0.009***		−0.005
		(0.067)		(0.025)		(0.003)		(0.004)
Retail Sales	✓	✓	✓	✓	✓	✓	✓	✓
Population	✓	✓	✓	✓	✓	✓	✓	✓
Revenue (1930)	✓	✓	✓	✓	✓	✓	✓	✓
Region FE			✓	✓	✓	✓	✓	✓
Age	✓	✓	✓	✓	✓	✓	✓	✓
Household	✓	✓	✓	✓	✓	✓	✓	✓
R-sq	0.14	0.14	0.13	0.13	0.23	0.24	0.08	0.08
N	175,335	175,335	174,902	174,902	140,464	140,464	175,335	175,335
Mean(y)	39.59	39.59	9.86	9.86	3.58	3.58	0.18	0.18
SD(y)	10.55	10.55	4.12	4.12	0.50	0.50	0.39	0.39

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: This table presents the estimation results of an OLS regression using a sample of 1930–1940 linked U.S. decennial census records of 22–60 year old urban local government workers. Moody Leverage is total municipal debt in 1929 multiplied by *shock* as defined in Eq. (5.1). The outcome variables are denoted in the headers. Control variables include standardized log city population in 1930, Census region fixed effects, a 4th order polynomial in age, city expenditure in 1930, log per capita change in county-level retail sales between 1929 and 1933 obtained from Fishback et al. (2003), and categorical variables denoting the number of children, marriage status, and race in 1930. Regressions are weighted using inverse probability weighting. Standard errors are shown in parentheses and are clustered at the city level.

Table 6 tests impacts using standardized *moodyleverage* —the share of 1929 bonded debt maturing 1930–1935—as the main regressor. Outcomes (standardized) are: (1) population growth to 1940 (and to 1950), (2) communicable-disease mortality, and (3) property and violent crime per 100,000. All regressions include controls for baseline population levels, population growth from 1920 to 1930, per capita public expenditures in 1929, and county-level retail sales growth from 1929 to 1933, the latter capturing the broader local economic environment.

The estimates suggest that a one standard deviation increase in *moodyleverage* is associated with a 0.91 percentage point reduction in population growth over the 1930–1940 period (0.07 × 0.13), rising to 2.7 percentage points by 1950 (0.09 × 0.30). This finding implies that early fiscal constraints induced persistent deviations in urban growth trajectories. In terms of public safety, I find that cities with a one standard deviation higher debt service burdens experienced an increase of 107 reported property crimes per 100,000 (0.39 × 275) by 1933, equivalent to approximately 4 percent of the baseline mean in 1930. These crime effects appear transitory: they do not persist beyond 1933, and I find no comparable increase in violent crime. Regarding public health, I detect no statistically significant relationship between leverage and mortality from communicable diseases. This null result may stem from limited statistical power or mitigating public health interventions introduced during the period.

Overall, these results suggest that fiscal distress constrained municipalities' ability to deliver core public services, with downstream effects on population retention and short-term crime outcomes. The persistent decline in population growth is particularly noteworthy: it supports the view that financial frictions, not solely declines in demand, impeded the capacity of local governments to maintain public goods and support urban growth during the Depression.

5.4. Impact on the local public workforce

City aggregates can mask heterogeneity in how institutions absorbed shocks. I therefore use linked individual census data to study

whether pre-Depression debt shaped public employment outcomes and workforce composition—who exited, and with what consequences for administrative capacity.

Using the linked 1930–1940 census sample of local government workers described in Section 3.3, I estimate regressions where the dependent variable is either (1) a binary indicator for whether an individual exited public sector employment by 1940, or (2) the change in occupational standing over the decade. Control variables include changes in per capita county-level retail sales from 1929 to 1933 to account for non-financial Depression severity, 1930 city population and total city revenues to proxy for the size and depth of the local public sector labor market, region fixed effects, a fourth-order polynomial in age, and indicators for sex, race, marital status, and number of children.

Table 7 presents the results. In aggregate, 46 percent of 1930 local government workers were no longer in the public sector by 1940. A one standard deviation increase in Moody Leverage is associated with a statistically insignificant 0.4 percentage point increase in public sector exit (column 1). However, this average masks substantial heterogeneity by gender. Column (2) shows that male public workers in high-debt cities were 1.3 percentage points more likely to exit the public sector than their counterparts in low-debt cities – a statistically significant effect consistent with the fiscal distress documented earlier.

Among leavers, men experienced downward occupational mobility. Columns (4)–(7) of Panel A show that, on average, public sector leavers moved into occupations with 4.2 percent lower income scores and a 2.1 percentile decline in the occupational income distribution, based on 1950 median earnings. However, this effect was attenuated in high-leverage cities: columns (5) and (7) reveal that the negative occupational consequences of public sector exit were 15–20 percent smaller in these municipalities,suggesting sorting or selection among those who exited.

Panel B compares leavers' characteristics by debt burden: those from high-leverage cities were slightly older, had 0.063 more years of schooling, and earned 0.9% higher log weekly wages by 1940, with similar migration rates. Displaced workers in high-debt cities were thus somewhat more experienced and higher-skilled and transitioned into relatively better-paid jobs.

Overall, public employment contracted more in high-debt cities, disproportionately affecting relatively skilled workers. The sector shrank and its composition shifted: more educated, higher-earning workers were less likely to remain in municipal jobs by 1940, plausibly weakening administrative capacity post-Depression.

6. Concluding remarks

There is growing evidence that municipalities in the U.S. are facing financial instability and that another crisis in local public finance will likey recur (Giesecke et al., 2022). Understanding how these crises affect the level or composition of local public goods and public sector employment is crucial for policymakers and urban economists. In recent history, countercyclical federal and state fiscal stimulus in the U.S. has limited researchers' ability to study these consequences. I instead examine a decentralized, fiscally-stressed era—the Great Depression.

Using a new city-level dataset on public goods and bonds, I show that financial constraints significantly reduced local spending during the Depression. Leveraging quasi-exogenous shocks from maturing bonds identifies causal effects. Demand-side investment cycles explain little of the leverage impact, and persistence into the 1940s is not accounted for by multiple demand proxies. I find a negative effect on population growth and a positive effect on short-run property crime in high-constraint cities, and sharper contractions of the public workforce. Exiting workers generally moved down the occupational ladder, and leverage shaped who left—more educated, higher-skilled workers were less likely to remain, plausibly weakening post-Depression administration. Overall, debt-driven constraints led to sizable spending cuts with adverse consequences. Ultimately, however, spending decisions are political choices — this paper does not address the political economy of local public goods provision or the interaction between local politics and financial constraints during crises, which I leave for future research.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jfineco.2025.104205.

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