NORTHWESTERN UNIVERSITY

Financial Crises and Economic Growth:
U.S. Cities, Counties, and School Districts During the Great Depression

A DISSERTATION

SUBMITTED TO THE GRADUATE SCHOOL
IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

for the degree

DOCTOR OF PHILOSOPHY

Field of Finance

By

Pawel Janas

EVANSTON, ILLINOIS

June 2022

ABSTRACT

This dissertation is the culmination of a four-year project on local governments and local economic activity during the Great Depression in the United States (1929 - 1937).

Chapter 1 investigates how U.S. municipal governments coped during the Depression and studies whether debt-induced financial constraints affected local public good provision. 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 out-migration by households. I collect novel archival panel data on cities and municipal bonds during the 1920s and 1930s and examine local public good provision during the Depression. I find that distressed cities significantly lowered public good provision - roughly 20 percent of the drop in expenditure can be explained through a re-allocation of budgets towards debt repayment. In addition, I find suggestive evidence that households subsequently relocated away from distressed cities in response.

In Chapter 2, I shift my attention to school districts. We know that investment in formal schooling varies with macroeconomic conditions that alter the resources available and the opportunity cost of education. I study whether recessions can serve a long-run benefit to youth by pushing them out of the labor market and back into school and investigate if education spending cuts attenuate this effect. I collect novel archival data on youth unemployment and school quality during the Depression and study how each affected overall high school graduation rates and average earnings across U.S. cities during the last stage of the High School Movement. My difference-in-differences empirical strategy attempts to explain the within-city variation in high school graduation rates across cohorts using across-city variation in unemployment and public education spending. I find that worsening local labor markets for youth significantly increased their secondary school atten-

dance and graduation rates while education spending cuts decreased them, but to a smaller extent. The effect is most prominent for youth from lower socioeconomic backgrounds. I estimate that 80 thousand urban youth obtained a high school diploma due to the Depression, and 7.5 thousand dropped out due to school district expenditure cuts.

Finally, in Chapter 3, I explore a central question of the Great Depression: what was the local economic effect of banking failures? Economic recovery from financial crises is typically slower than from other crises, possibly due to credit rationing by financial intermediaries. I study whether lender-of-last-resort policies of the Atlanta Federal Reserve Bank during the Great Depression eased firms' financial constraints using a novel database of local economic conditions from 1927 to 1937. My identification strategy relies on the willingness of the Federal Reserve to extend credit in some regions and not in others and plausibly exogenous placement of Federal Reserve boundaries. I find evidence that Fed intervention stymied banking panics, but I do not, surprisingly, find any meaningful effect of Fed policies on local economic outcomes.

ACKNOWLEDGEMENTS

I would like to extend my sincere thanks to my outstanding committee members: Paola Sapienza (Chair), Carola Frydman, Scott Baker, and Joel Mokyr, for their patience, guidance, and encouragement. For suggestions, comments, and guidance on these (and other) projects throughout my PhD studies, I also thank Efraim Benmelech, Louis Cain, Anthony DeFusco, Chris Hair, Menaka Hampole, Walker Hanlon, Sean Higgins, Huidi Lin, Seema Jayachandran, Adam Jørring, Robert Korajczyk, David Matsa, Gregor Matvos, Ralf Meisenzahl, Filippo Mezzanotti, Charles Nathanson, Dimitris Papanikolaou, Alessandro Pavan, and Adriana Troiano, as well as seminar participants at the Northwestern Economic History Seminar, the Kellogg Finance Brown Bag, the Applied Micro graduate student workshop, the virtual meeting of the Urban Economics Association, the Virtual Economic History Seminar, the NBER Development of the American Economy Summer Session, and the Economic History Association Annual meetings. This research has been generously supported by a Kellogg Dean's Office, the Economic History Association (Dissertation Fellowship), and the Center for Economic History at Northwestern. All remaining errors are my own.

Dedication.

I dedicate thirty-three percent of this dissertation to my cohort-mate, room-mate, office-mate, and best friend, Menaka Hampole. Without her support, I would have dropped out of graduate school sometime between the fall of 2018 and spring 2021.

I dedicate another thirty-three percent to my older sisters, Anna and Maja. They blazed the academic path for me from the first grade up through the PhD and have set the bar at a frustratingly high level for 30 years now. I wouldn't have it any other way.

I dedicate the last thirty-three percent to my parents, Teresa and Tadeusz, for instilling in me a love of learning and giving me every opportunity in the world to be successful.

The remaining one percent is for me.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

CHAPTER 1

PUBLIC GOODS UNDER FINANCIAL DISTRESS: EVIDENCE FROM CITIES IN THE GREAT DEPRESSION

1.1 Introduction

Financial distress caused by leverage can prolong and exacerbate economic shocks. The consequences of financial fragility during economic crises have 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 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 to 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. For example, when faced with constraints during a crisis, how much do cities curtail spending, which public goods are most affected, and how do households respond?

This chapter 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 particularly relevant 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 A.1). This influx of debt raised the likelihood of financial distress once the Depression decimated local governments' underlying tax base– 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 essential trade-offs between defaulting on financial obligations and providing local public goods.

The historical context allows me to overcome two critical challenges confronted by empirical research on modern-day cities. First, local governments in the U.S. were the leading government service providers before and during the Depression. As a result, they 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. This chapter overcomes this challenge by going back to the 1920s and 1930s. Second, the systematic financial distress of cities experienced during the Depression is a historically rare but potentially devastating event. Studying it is particularly important to inform policy responses to future crises. In terms of an empirical laboratory, the Depression is close to

[1]Their failure to provide adequate support to the unemployed (a quarter of the labor force) is 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 grants and loans of over $500mil to states and cities. At the same time, the Works Progress Administration and the Social Security Act significantly 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.

ideal: cities neither experienced the economic shocks of the 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.

I construct a historical dataset from multiple novel archival sources on U.S. cities to investigate these questions. 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. Second, I expand the scope of this dataset by digitizing and standardizing annual financial transaction reports for the near-universe of cities with a population of over 1,000 (770) in New York, Massachusetts, Ohio, Indiana, and California for the years 1924 to 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 city-level data by building a second database of over 29,000 municipal bonds in *Moody's Manual of Governments* in 1929, the primary source of information on government bonds. 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 percent 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 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. 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 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 rating institutions to measure municipal creditworthiness and, thus, the price of credit. Lastly, as I describe in more detail in Section 1.2, 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. To gauge the magnitude of these estimates, I use a back-of-the-envelope exercise to compute the elasticity of spending cuts during the Depression to the (real) rise in leverage between 1924 and 1932. 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 affected expenditure the most and that the effect was most prominent 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 correlate with financial leverage ratios—I implement an additional analysis that 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]. As a result, municipalities could not easily issue new debt to repay the principal owed on bonds. Cities with more outstanding bonds were plausibly more constrained in allocating revenue between debt-service and public goods. However, these bonds were primarily issued well in advance of the onset of the Depression. 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 with different levels of potentially exogenous amounts 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 with 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, 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 expenditure drop can be explained by 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, tax rates, and credit ratings. I find a significant impact on police and firefighting spending and capital investment and quantitatively more minor 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 more indebted cities 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. First, to disentangle debt-driven financial constraints from local demand for public goods, I show that the results are robust when excluding 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. Demand for public services and investment- and the correlation between leverage and local demand- does not explain 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 had a link to a bank in a financial hub (Chicago, New York, or Cleveland). I do not find that including 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 the 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 the location decisions of individuals dates to the seminal contribution of Tiebout [Tiebout, 1956] with important and recent empirical work ([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 a more significant 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. 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 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 explore 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

[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.

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 positively impacted retail consumption [Fishback et al., 2005], in-migration([Fishback et al., 2006], and crime reduction [Fishback et al., 2010]. This paper is among the first 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 pressure on city budgets using tax delinquency and debt, this paper focuses on 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 ([Ferrie and Troesken, 2008], [Cutler and Miller, 2005]), large spillovers on private economic activity [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 chapter is organized as follows. Section 1.2 describes the historical context and institutional details of local public good provision in the first half of the 20th century. Section 1.3 then describes the construction of the dataset. Next, section 1.4 provides an overview of revenues, expenditures, and debt for a large sample of cities from 1924–1938 and uncovers relationships between spending, investment, and debt. Then, section 1.5 tests whether financial leverage drove

[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].

public goods spending cuts after the onset of the Great Depression using a difference-in-differences design. Section 1.6 introduces the empirical strategy of using bond-level data to identify short-run financial constraints and shows the adverse effects on public good provision. Section 1.7 tests whether urban growth was adversely affected through a household migration channel. Section 1.8 concludes the chapter.

1.2 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 in 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 local-state-federal cooperation started once the federal government raised revenue through new sources (individual income, excise) and distributed 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 most extensive public spender (and debtor) in the U.S. Using data from the Historical Statistics, Figure 4.1 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 class 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

Figure A.1 plots the average annual municipal bond sales for the interwar period using data from the Commercial and Financial Chronicle. 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. Three key factors trace the expansion of public infrastructure.

First, the first four decades of the 20th century are 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 construction of schools and investments in equipment to furnish them. Cities and school districts issued bonds to finance these construction projects, which wealthy private individuals, banks, and corporations eagerly bought [Brown, 1922]. Second, significant rural-to-urban migration led to increased 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

[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].

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 chapter. 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]. Due to a slump in the housing market and tax delinquency, property tax revenues fell. 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 typically matured at once. This maturity problem was especially acute for cities that expanded in the 1920s by financing infrastructure investment with long-term bonds, which became due during the Depression[7].

[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.

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 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 and then issue a judgment. Next, 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 bondholders sued a city after a 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 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].

1.3 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

[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.”

44.7 million people, or roughly 64.7 percent 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 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 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. Additionally, each state reports totals for revenue and spending categories which allows me to check the accuracy of the digitization process. I manually corrected all OCR errors such that the totals reported in the publications match the sums of individual categories. 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 4.2. Table 3 shows the summary statistics. I leave the description of the time series evolution of the main variables for Section 1.4.

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

[9]For comparison with other research in the field, recent modern studies (notably [Siodla, 2020] and [Gunter and Siodla, 2018]) have used data on the largest 94 cities with populations above 100,000. As I discuss in Section 1.4, 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 1930s. However, the coverage misses critical years during the Great Depression; thus, it is not included in this project.

publishing statistics in reports called *Financial Statistics of Cities.*[11] 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 quantitative security-level data, a qualitative review of significant industries in a city, and a Moody's credit rating. This source provides detailed information on the debt structure, such as repayment and maturity, which is helpful for identification as described in Section 1.6. For example, this data allows me to see that the city of Chicago issued a 4 percent bond in 1920 with an outstanding balance of $50,000 that was left to be repaid annually from 1936–1950. The data contains 29,366 bonds outstanding across 316 cities in 1929. Table 4.1 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 percent count Census in 1930 and 1940 on IPUMS [Ruggles et al., 2020]. Not all cities are identified in the Census enumerations - of the roughly 850 cities with financial transaction data, only 262 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.

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

1.4 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 are 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. I create an index based on the average per-capita outcomes in each population category for time-series comparisons. Finally, 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.

1.4.1 Revenues

Figure 4.2 presents the breakdown of revenues by city size in 1930.[12] Three important facts stand out. First, the bulk of income (70–80 percent) comes from local property taxation or revenue 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 propagated directly to city revenue. 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 4.3 shows that cities experienced parallel growth in revenues from 1924 to 1931. After

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

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 steep drop of 15 percent relative to 1929. Due to data limitations, whether these tax revenues were lower because of taxpayers' inability 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 less than 100,000.[13]

Table 6 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.

1.4.2 Expenditures

Figure 4.4 shows the average 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 went towards capital outlays - constructing permanent fixtures such as buildings, roads, dams, canals, and public hospitals.[14]

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

[14] 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

Figure 4.5 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. Overall, construction spending decreased by 60 percent from 1929 levels across all cities. Across city sizes, 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 6. City officials also curtailed current (non-capital) non-welfare expenditures, but less drastically later in the 1930s.[15] 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 fall similarly, with over 25 percent of medium and large cities reporting an *increase* in interest costs, consistent with the fixed-repayment schemes of long-term liabilities and the severe deflation of this period.

1.4.3 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 costs 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 4.6 shows that large cities were more than twice as indebted as small cities before the Depression, resulting from the rising demand for urban infrastructure in the preceding decades, as described in Section 1.2.

Figure 4.7 shows an index of the average per-capita debt levels across city population size and year. The total inflation-adjusted debt stock increases by about 30 percent from 1929 to 1933,

historical sources from which the data comes do not allow me to separate the two.

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

driven by 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 different types of debt flows. 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 4.8. 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 1.2, 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 primary determinant of pre-Depression municipal debt in my sample of cities. Table 7 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 and 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 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 A.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. Indeed, the data reveals that, 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 economically significant - equivalent to 50 percent of the average budget for a large city health department in 1930 - and abrupt, as the figure reveals.

1.5 Effect of Debt on City Expenditures During the Great Depression

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

1.5.1 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 (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, property tax rates do not adjust perfectly because of fluctuations in property values, the mobility of taxpayers, and limits on tax rate increases in state constitutions. Sticky rates may cause a larger share of revenue to pay creditors (i.e., fixed costs), with less going towards other services. A higher DOV may also increase new debt riskiness and price for the same reasons.[16] 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 strain than $100 million in short-term, high-interest rate debt for another. After all, a city defaults 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 the 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.

[16]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].

I define pre-Depression DOV and TOI using data from 1930[17]:

$$DOV = \log(1 + \frac{\text{Total Debt}_{1930}}{\text{Assessed Value}_{1930}}) \tag{1.1}$$

$$TOI = \log(1 + \frac{\text{Tax Revenue}_{1930}}{\text{Interest Expense}_{1930}}) \tag{1.2}$$

Figure 4.9 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 percent, and the mean tax/interest ratio is 19.22 percent, 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 essential 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 A.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 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.

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

1.5.2 Empirical approach

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

$$y_{it} = year_t + city_i + \theta X_{it} + \beta(Post = 1) \times leverage_{30,i} + \epsilon_{it} \tag{1.3}$$

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:

$$y_{it} = year_t + city_i + \theta X_{it} + \sum_{j \neq 1928} \beta_j year_{j=t} \times leverage_{30,i} + \epsilon_{it} \tag{1.4}$$

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 X_{it} include a set of Census region-by-year fixed effects to account for known regional dynamics of Depression severity [**?**], 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 reelection 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 1.6.

1.5.3 Results

1.5.3 Main results

In Table 8, 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 percent 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 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 A.4 plots the β coefficients for the event study using DOV as $leverage$, while Figure A.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 the 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 stood at about \$2,626 per capita in 1932, significantly lower than the inflated value of \$3,225 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 8, 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.

1.5.3 Heterogeneity

This section explores whether financial constraints impacted public expenditure heterogeneously across cities of different population sizes, across cities with varying population growth paths before the Depression, and across cities in counties with differing 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 affected expenditure the most. Interestingly, I further find that the effect was most prominent 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 10 reports the results of the heterogeneity analysis concerning population and population growth. In columns (1) and (2), I recapitulate the baseline 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 following 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. However, another interpretation of this finding 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 1.5.4, I explore this demand channel in more detail and show that it cannot account for my main findings. Regarding population size, I find that financially constrained small and medium-sized cities decreased service expenditure more (-0.1 to -0.07) 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 more significant 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 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.[18] 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. Second, I use the county-level, panel dataset produced by the Federal Deposit Insurance Corporation (FDIC) on commercial bank suspensions between 1920 and 1936. In columns (5) - (8), I separate the sample into cities in counties with and without a bank suspension between 1929 and 1933. The results show that there was a slight, statistically insignificant, increase in magnitude of the effect of financial constrains on public service expenditure in cities in counties that experienced a bank failure.

[18] 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.

1.5.3 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 significant 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: cities raised property tax rates 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 9 reports the estimated coefficients from Equation 1.3 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 more minor 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 be negligible. The results in column (7) seem to suggest that the limited stimulus from federal programs for welfare (work relief and cash relief) was not allocated based on financial constraints, which is consistent with other evidence that New Deal spending may have been distributed with different motives, such as political, in mind [Fishback et al., 2003]. On the other hand, services 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 *Manual* 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 A.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 was a divergence between the two groups. By 1936, low leverage cities were 0.98 ratings above high leverage ones. Systematically, Table 12 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 percent total tax rate in Los Angeles in 1935 was divided between a 2 percent rate for general purposes - wages for city workers and services - and a 0.2 percent 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 delinquency and tax revolt [Siodla, 2020]. Table 12 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. One standard deviation in DOV coincided with a 0.17 percentage point increase in the bond-repayment property tax rate, compared to an average total tax rate of 2.8 percent and an average bond-only rate of 1.3 percent. Overall, 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.

1.5.4 Robustness

This section shows that my main results are robust to alternative, non-debt-driven explanations, and alternative measurements. 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 including time-varying variables that may reflect political motives, such as the 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.

1.5.4 Demand channel

The difference I find between indebted cities and less indebted ones is undoubtedly 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 financial 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.[19]

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 the *Manual*, I proxy for future demand in two different ways: (1) the share of bonds issued in 1927–1929 to the total amount issued[20] and (2) the weighted average age of each city's bond portfolio, weighted by the face value of each bond.[21] Conceptually, I am assuming that cities with a large value of (1) also have newer infrastructure and in which demand in the 1930s would be low. Likewise, cities with a low value of (2) are cities that have more recently invested and would hypothetically not need new investment in the Depression years.

Table 13 reports the estimation results of Equation 1.3 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. Next, in columns (2) and (5), I exclude the cities in the highest

[19] Even though Figure A.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.

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

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

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.

1.5.4 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 14. First, another concern is that the effect on public expenditure is instead driven by varying local economic conditions that are not adequately captured by 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 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 *Manual* as of 1929. Typically, these agents are banking institutions or brokers that act as underwriters for a city's bond issue or agree with a city to pay interest coupons to local investors. While the institution's identity 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 percent 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 confound 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, critical 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– and the length of each election cycle for all cities over 5 thousand in population from the 1938 edition of *The Municipal Year Book*, a trade publication for city officials. In all, roughly 60 percent of the cities in my sample have election cycles longer than three years, and the remaining have a two 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.

1.6 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 cash flow shocks and present direct evidence that the inability to pay obligations during the Depression is the primary 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 permits a deconstruction of financial leverage into cash flow shocks at different points in time, and it also provides two advantages for identification. First, while debt issuance around 1930 may undoubtedly be endogenous to outcome variables in the 1930s, debt issued 10 or 20 years before the Depression is plausibly less so. For example, a city planning to refinance a 20-year bond issued in 1911 would find it difficult 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 collected the "Schedule of Bonded Debt" information from the *Moody's Manual of Governments* in 1929. For each bond listed, the data includes the year the bond was issued, the year it will mature in the future, 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 4.1.

Notably, 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 the *Manual* 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 the sample ($\rho = 0.9$) and average coverage of 85 to 95 percent. Similarly, I estimate the expected interest payments, aggregate them to the city level and find an equally high correlation coefficient. More information regarding validating bond-level data appears in Appendix 4.3.

1.6.1 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:

$$shock_{30,j} = \frac{\sum_{t=1930}^{1935} \sum_{\forall i \in j} repay_{i,t}}{\text{Total Debt}_{29}} \tag{1.5}$$

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 A.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 with 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 had access to more sophisticated bankers who could endogenously select a constant repayment scheme and re-negotiate repayment during the Depression. To help alleviate this concern, I performed a balance test on 1929 city characteristics based on below and above median repayment shock. The results are displayed in Table 15. 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. However, cities did not significantly differ in total revenue collected, assessed property value, public service expenditure, or total capital investment in 1929.

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

$$\widehat{DOV} = \log(1 + \frac{\text{Total Debt}}{\text{Assessed Value}} \times shock_{30,j}) \tag{1.6}$$

$$\widehat{TOI} = \log(1 + \frac{\text{Tax Revenue}}{\frac{1}{6}\sum_{t=1930}^{1935} \text{Est. Interest Expense}_t}) \tag{1.7}$$

where $shock_{30,j}$ is defined in Equation 1.5 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:

$$y_{it} = year_t + city_i + \theta X_{it} + \beta(Post = 1) \times \widehat{leverage}_{i,30} + \epsilon_{it} \tag{1.8}$$

As before, the coefficient of interest is β, representing 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.

1.6.2 Results

Table 16 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 1.5, 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 approximately 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. These impacts account for 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 decreased their infrastructure, health, and protection spending and subsequently faced higher borrowing costs and a reduced 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. They are not sensitive to different measures 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 to expenditure cuts on other local public goods.

1.7 Urban Growth and Financial Constraints

The debt-induced expenditure cuts shown in the previous section inevitably 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 [**?**]. It stands to reason that smaller police budgets may have encouraged more criminal activity, lower education spending may have hampered human capital formation, or cutting infrastructure made some cities less appealing for firms, whether 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 explore, as models in economic geography[**?**] 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 primary local determinant of migration. In addition, I control for 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 1.5 - 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 three people. Theoretically, I expect the costs of migration to be higher for individuals with children (direct moving costs and education search costs) and 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 the costs of moving but have more considerable opportunity costs and foregone wages. The total cost of migration for these individuals is, thus, ambiguous. Table 4 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:

$$\mathbb{1}(migrate)_{i(j)} = HighLev_j + GDSevere_j + HighLev_j \times GDSevere_j + X_i^1 + X_j^2 + \epsilon_{ij} \quad (1.9)$$

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.[22]

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 homeownership. 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 and 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 4 reports the summary statistics.

Table 17 reports the results of estimating Equation 1.9 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 and person-level, city-level, and regional controls, respectively. The results in column (1) 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,

[22]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.

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) using the quasi-exogeneous leverage measure introduced in Section 1.6. The results are quantitatively very similar.

I explore whether the migration result varies for various groups. Table 18 shows the regressions results for six 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 based on occupational income. However, I 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. In addition, 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.

1.8 Conclusion

How economic crises affect the level or composition of local public goods and, in turn, affect 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 chapter, 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 chapter shows that debt-driven financial constraints can induce significant public expenditure cuts during a crisis. Still, it does not take a stand on the welfare implications of local public debt issuance. To conduct such an analysis in the setting of this chapter, 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 chapter's scope. Instead, 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 chapter 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 expanded significantly in the first half of the 20th century, especially in secondary schools. How financing constraints impacted human capital accumulation during this period certainly warrants a closer examination [Janas, 2021b]. 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 today.

CHAPTER 2

RECESSIONS, CONSTRAINTS, AND PUBLIC EDUCATION: IMPACT OF THE GREAT DEPRESSION ON THE HIGH SCHOOL MOVEMENT

2.1 Introduction

Education is a central determinant of economic growth. Yet, investment in formal schooling - both at the institutional and individual level - varies with macroeconomic conditions that alter the available resources and opportunity cost of education. The historical experience in the United States during the first half of the 20th century, which saw the increase in high school graduation from less than 10 percent in 1900 to 50 percent in 1940, is one such example. Understanding the interaction between macroeconomic shocks and microeconomic mechanisms driving changes in human capital investment is of first-order importance for guiding education and labor policy.

The literature studying household schooling decisions includes an emerging body of work investigating how individual behavior changes due to rising demand for low-skill labor in developed ([Betts and McFarland, 1995]; [Charles et al., 2015]) and developing economies ([Shah and Steinberg, 2017]; [Bau et al., 2020]; [Atkin, 2016]). In the former, much of this work focuses on the post-secondary setting when youth choose between college or low-skill jobs in booming sectors. In the latter, researchers have studied the impact of positive shocks in agriculture or trade, most of which have incentivized youth to drop out before finishing secondary education. We have less evidence, however, on whether the effect of business cycles is symmetric during *busts* when both youth labor market opportunities and public expenditure for public K-12 education dip. Disentangling these mechanisms and providing direct causal evidence on how business cycles drive aggregate changes in education is difficult to establish. When a recession hits, do youth continue their education, which individuals respond the most, and what are the implications for outcomes in adulthood?

This paper sheds new light on these questions by studying the schooling behavior of individuals during the Great Depression. In a departure from prior work, which has focused primarily on how schooling decisions respond to positive trade shocks in recent decades, I study individual behavior across a large cross-section of cities in the United States during the worst economic recession of the 20th century. The historical context provides several empirical advantages. First, a decentralized public funding system created local variation in education quality in the 1930s, which does not exist - to the same extent - under the current state equalization schemes and federal support for K-12 education. Likewise, the effect of the Depression was unevenly felt across the country, creating considerable variation in youth opportunities and economic deprivation across local labor markets. Second, most states in the U.S. today have laws preventing youth from entering the formal labor market, making studying this relationship impractical with modern survey data. In contrast, youth labor was much more common in the first half of the 20th century. Lastly, the availability of microeconomic Census records of the whole population during the high-school movement provides measurable short- and long-run outcomes and permits a holistic analysis of heterogeneous effects.

Suggestive evidence that the Depression changed individual schooling decisions comes from trends in high school graduation rates between 1910 and 1940. This period of U.S. economic history – typically referred to as the "High School Movement" – was characterized by a marked increase in the number of youth completing at least 12 years of education [Goldin and Katz, 1997]. Using data from the U.S. Department of Education, Figure B.1 plots the ratio of high school graduates to 17-year-olds decennially between 1910 and 1930 and annually after. In 1910, this ratio stood at just 8.8 percent. While the graduation rate more than tripled to 29 percent by the eve of the Great Depression in 1929, there was an evident gain relative to the trend beginning in the early 1930s. This uptick did not subside until the U.S. entered the Second World War in the early 1940s.

Figure B.1 also offers a first pass assessment of whether the Depression at the local level is related to an increase in educational attainment. I combine a linked sample of males in the 1930 and 1940 Censuses with the change in county-level retail spending between 1929 and 1933 obtained from the Retail Census collected by [Fishback et al., 2003]. I restrict attention to persons in this sample from the 1906-1922 birth cohorts who reported living in a city in 1930. I then compute the share of the cohort that reported finishing at least 12 years of education in the 1940 Census, separately by whether or not the individual was living in a county in the top or bottom tercile of the change in retail sales. The figure shows no differences in high school attainment by cohort across the two groups of counties from 1926 until the 1930 graduating cohort. However, beginning with those who turned 14 during and after the Depression started and thus at the point of making high school-going decisions, the graduation rate for persons in counties with substantial adverse economic shocks surpassed those for persons from the same birth cohort in counties with milder shocks. By 1936, the average rate rose by 12.0 percent in worse-off counties and only 10.2 percent in better-off counties.

The patterns in Figure B.1 indicate a boost in the high school graduation rate for individuals during the Depression and raise the possibility that the recession pushed youth into schooling. I present a conceptual model that portrays the trade-off between higher local unemployment and school quality to fix ideas. In the model, households invest in an additional year of schooling if the marginal return to education is larger than the opportunity cost of education, conditional on the perceived returns to education based on observable quality measures. While an economic downturn reduces the opportunity cost for everyone and entices some to opt-in to secondary school, this influx of youth is attenuated as schools shift from high to low quality, and the greater cost of attending low-quality institutions outweighs the return.

I construct my historical data set from multiple novel archival sources. First, I combine unemployment-by-occupation-by-age data from the Special Unemployment Census of 1931 with occupation-by-city shares of youth aggregated from the 1930 Census 100 percent count records available on IPUMS. Since the Unemployment Census canvassed only 18 regionally dispersed cities and three boroughs of New York City, I estimate youth unemployment for all other cities by taking a weighted average of regional unemployment by occupation rates. I show that this measure is a good predictor of the actual unemployment rate in the Census sample and predicts youth welfare enrollment in 1934. To the best of my knowledge, mine is the first attempt to quantify locally disaggregated and age-specific unemployment rates during the Great Depression in the U.S. context.[1] To obtain education measures, I digitize biennial records from the Census of Education from the U.S. Office of Education on revenues and expenditures at the city level from 1922 to 1938. I follow the economics of education literature and proxy quality with the change in the total real spending per pupil. I show that expenditure is closely related to the student-teacher ratio, average real teacher wages, and term length.

Figure B.2 shows significant variation in 1931 unemployment of 10-19 year-olds in the cities enumerated by the Census. To obtain the unemployment rate, I divide the total number of people unemployed, able to work, and looking for work (”Class A”) as well as persons having jobs but on lay-off without pay (”Class B”) by the total labor force as reported in the 1930 Census. I compute the unemployment rate for each age group separately. Consistent with the literature showing regional patterns of the Depression across the U.S.[2], shows that the unemployment was

[1] However, numerous efforts have been made to compute accurate unemployment rates at a higher level of aggregation, notably [Sundstrom, 1992], [Darby, 1975], [Margo, 1991] and [Wallis, 1989].

[2] In particular, see [Rosenbloom and Sundstrom, 1999]

above 40 percent in industrialized cities specializing in durable goods manufacturing (Buffalo, Detroit, Cleveland) and relatively low (25 percent) in cities specialized in trade and services (San Francisco, Seattle, Manhattan). The occupational distribution of youth in these cities drives the variation in total rates. For example, consider the difference between Detroit and San Francisco. The largest share (11.5 percent) of the youth labor force in Detroit was employed as laborers in the iron and steel industry and experienced a staggering 53 percent unemployment rate. On the other side of the country, youth in San Francisco primarily worked in low-skill white-collar clerical work, which experienced a much milder Depression of 10.5 percent youth unemployment.

Turning to education quality, Figure B.4 plots the average expenditure per pupil for the cities in my sample, inflated to 1967 dollars. Average expenditure rose throughout the 1920s at 7.5 percent a year, dropped 18 percent between 1932 and 1934, and did not recover to its 1932 level until 1940. Similarly, average secondary school teacher wages dropped 20 percent between 1930 and 1934, and student-teacher ratios increased by 15 percent in the same period. The Depression was a period of declining school quality by all widely used measures.

My empirical strategy attempts to explain the within-city variation in high school graduation rates across cohorts using a difference-in-differences design using across-city variation in unemployment and public education spending cuts. Notably, the variation in either is not systematically related to changes in the graduation rate before 1930, giving some evidence that the underlying parallel-trends assumption is not prohibitively strong. I provide event study evidence that confirms the lack of pre-trends before 1930. The experiment thus compares outcomes of persons on the cusp of making secondary schooling decisions during the Great Depression (1915-1920 birth cohorts) with those who graduated before the Great Depression within the same city, conditional on national trends in educational attainment. Concretely, using the example of Detroit and San Francisco from above, the test is whether the 15 percent difference between the youth unemployment rate between the two cities caused the graduate rate of cohorts to diverge in these two cities during the Depression, accounting for changes in education quality.

Beginning with education attainment outcomes, I find that increases in the youth unemployment rate in 1931 significantly increased secondary school graduation rates: a 10 percent increase in unemployment caused a 2 percentage point increase in the graduation rate for the 1934-38 cohorts and a 9 percentage point increase in weekly earnings in 1940. Also, at the cohort level, I examine heterogeneity in the response on both the blue- and white-collar education attainment rates and find that the average effect is driven by an influx of students from blue-collared families.

To provide additional evidence on the role of education spending on quality, I examine the impact on teacher wages, student-teacher ratio, and term length. Reductions in wages and term length and increases in the student-teacher ratio are all significantly related to expenditure cuts, affirming the assumption that quality - and not just spending - decreased in cities across cohorts during the Depression. Furthermore, the Depression impacted specifically the exit and entry into secondary schools. I present evidence on the cumulative distribution function of completed years of schooling and find a sharp jump in the distribution of 9th and 10th-grade graduates. I conclude the cohort-level analysis by showing that the effects are driven by cities that, in 1940, had higher teacher quality - as proxied by years of education - and in states without youth labor regulation as of 1931. I do not find evidence that the effects differed based on county-level New Deal spending, an unsurprising result given that the largest work-relief program that could impact the opportunity cost of youth - the Civilian Conservation Corps - did not employ men below the age of 17.

I then turn to the regression results using linked person-level data. I alleviate concerns surrounding omitted confounders by investigating *within family* differences in schooling choices using a sample of roughly 187 thousand linked siblings. I find that including household fixed effects increases the magnitude of the estimates by 20-50 percent above baseline results. I rule out a selective-migration alternate explanation and find that the results remain unchanged when migrants are excluded from the sample. In total, extrapolating the estimated impact on the total urban economy reveals that lower labor opportunity costs of the Depression caused over 80 thousand more youth to finish high school and that 7.5 thousand dropped out due to worsening education quality.

This paper contributes to three pieces of literature. First is the literature that studies the elasticity of schooling choices with respect to changes in labor markets. Researchers have shown that local labor market conditions affect education attainment ([Blanchard and Olney, 2017]; [Black et al., 2005]; [Atkin, 2016]; [Cascio and Narayan, 2015]; [Charles et al., 2015]). Most of this body of work uses trade or industry-specific labor-demand shocks and finds that youth discontinue schooling when opportunities *increase* and the skill premium is low. I extend this literature by studying the elasticity during macroeconomic *downturns* from a non-trade perspective and by measuring the effect's attenuation due to pro-cyclical (declining) education quality.

The second piece of literature my work builds on is the research studying the consequences of U.S. educational investments in the first half of the 20th century, specifically the high school movement ([Goldin and Katz, 1997]; [Schmick and Shertzer, 2019]; [Card et al., 2018]). To the best of my knowledge, mine is the first paper that quantifies the effect of the Depression on education attainment and disentangles the quality and opportunity cost channels during this time period[3].

The third is the literature on the public sector during the Great Depression in the U.S., 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 on retail consumption ([Fishback et al., 2005]), migration ([Fishback et al., 2006]), and crime reduction ([Fishback et al., 2010]).

[3]Other papers that study the determinants of educational attainment during this period are [Baker et al., 2020] (boll weevil), [Baran et al., 2020] (Great Migration), and [**?**] (public libraries), and [**?**] (compulsory schooling laws).

At the local level, research has found large reductions in public good provision during this time period ([Janas, 2021a]; [Siodla, 2020]). To the best of my knowledge, this paper is one of the first papers to study how local school districts responded and the resulting consequences on schooling choices.

The rest of the chapter is organized as follows. Section 2.2 describes the conceptual model of human capital investment. Section 2.3 provides an overview of youth unemployment in 1931 and school expenditure shocks during the Depression. Section 2.4 discusses the difference-in-differences empirical design while Section 2.5 presents the results, provides evidence against alternative mechanisms, and explores heterogeneous effects. Finally, Section 2.6 concludes by discussing the economic implications of the results and directions for future research.

2.2 Conceptual Overview

In my framework, I consider 14-17-year-old youth who have a choice of either continuing education through a secondary school, which is either of "High" ($c = H$) or "Low" ($c = L$) quality, or participating in the labor market or household production.[4] The notion that schooling returns vary based on school quality has been extensively debated in the economics of education literature [Hanushek, 1986], both in the modern context [Jackson et al., 2016] and in the historical setting of this paper [Card and Krueger, 1992]. These potential students differ in innate academic ability $\theta_i \in [0, 1]$ which determine the psychic cost of schooling each year given by $\kappa_c(1 - \theta_i)$. Further, I assume that attending a type-L school is more difficult - lower quality schooling systems may employ less qualified teachers who make learning less enjoyable or provide fewer non-academic

[4] If youth do not attend school or work, what do they do? Broadly, I consider the outside option of youth to be home production - either of goods or child-rearing services - or idleness. According to [Elder, 2018], children from economically deprived families were an important factor in the household economy during the Depression, as their labor and monetary contributions were needed.

amenities such as school meals and transportation, such that $\kappa_L > \kappa_H$.

In any year t, labor market participants with and without a high school diploma receive labor market incomes of Y_t^c and Y_t^0, respectively, which vary from one year to the next because of macroeconomic conditions. The high school premium in a given year for persons educated at a given type of high school is thus $\Pi_t^c = Y_t^C - Y_t^0 > 0$.

I define the lifetime payoff that a person of ability θ_i gets from attending a type-c of high school in year t as:

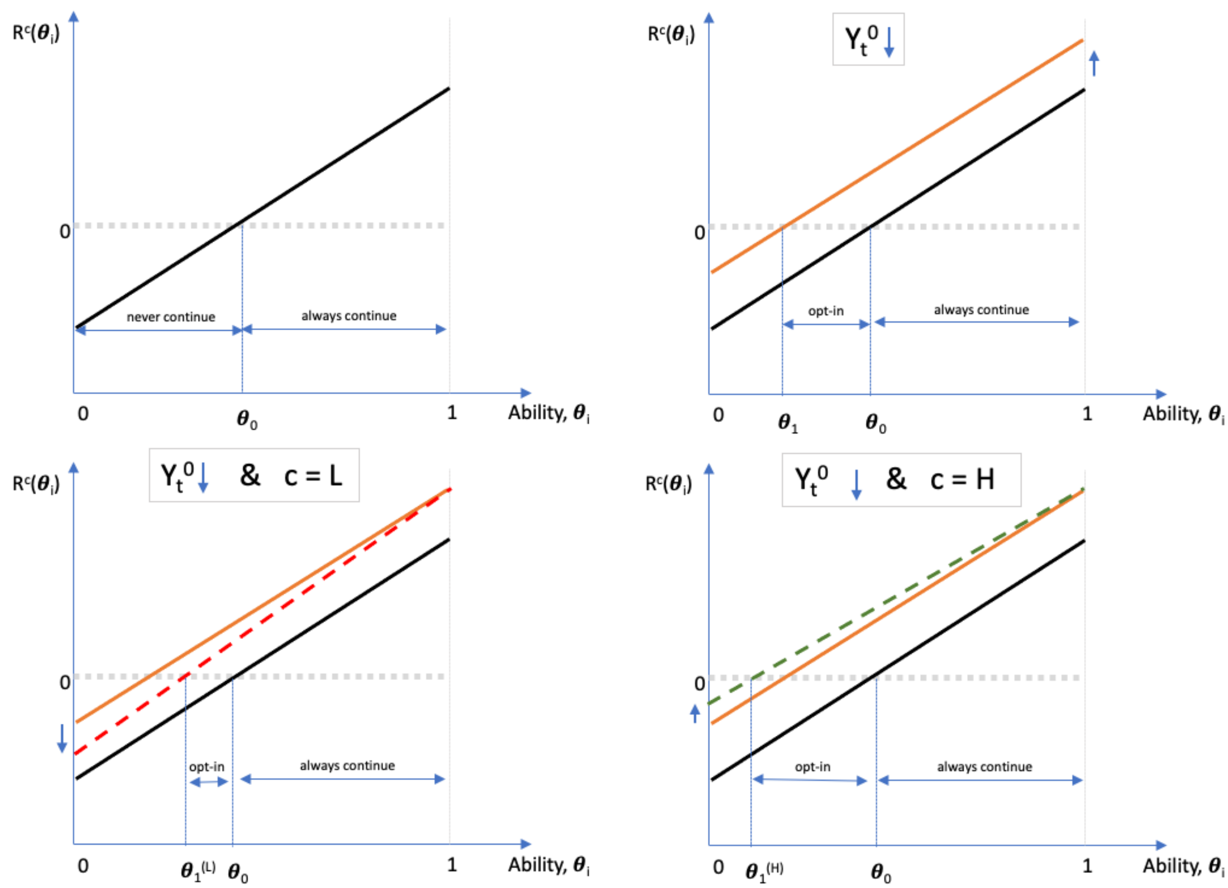
$$R_{it}^c(\theta_i) = \sum_{k=1}^{L-a_{it}} E[\Pi_{t+k}^c] - \kappa_c(1-\theta_i) - Y_t^0 \tag{2.1}$$

where $L - a_{it}$ denotes the total lifetime years remaining for a person of age a_{it}.

I present the basic premise of this illustrative model in Figure 2.1. At t = 0, the economy begins (top-left panel) with youth of θ_i above θ_0 attending secondary school and those below dropping out.

The effect of a shock on average high school going in the population is determined by how the shock shifts the payoff function 2.1. The top-right panel illustrates the equilibrium when youth labor opportunities decrease - an upward shift due to decreasing Y_t^0, as typically occurs during recessions. Now, share 1 - θ_1 decides to go to school, on average, across school districts. When education resources are tied to local property values and pro-cyclical taxation, changes in school quality due to a recession locally will further shift enrollments in either direction. The bottom left panel portrays the situation of a low-quality district ($\kappa_L < \widehat{\kappa_0}$) where the change in quality hinders the influx of new entrants, and $\theta_1^{(L)} - \theta_1 > 0$ do not opt-in. On the other hand, a high-quality district ($\kappa_H > \widehat{\kappa_0}$) sees larger than average, $\theta_1 - \theta_1^{(H)} > 0$, gains in enrollment.

Figure 2.1: Model of Education Attainment



2.3 Data Construction

This section describes the construction of novel city-level youth unemployment rates and school quality measures during the Great Depression. In section 2.3.1, I present my method of measuring opportunity cost, which I proxy by the youth unemployment rate. Locally dis-aggregated unemployment data during the Depression for youth (or adults) is not available systematically. Therefore, I use three sources of information to estimate unemployment rates: city-level occupation reports within the state-level publications of the 1930 decennial Census, the Special Unemployment Census of 1931, and the full count records of the 1930 Census publicly available on IPUMS ([Ruggles et al., 2020]). In Section 2.3.2, I discuss school district expenditure, enrollments, teacher wages, number of teachers, and term lengths, which I obtain from the *Biennial Survey of Education*.

2.3.1 Local youth unemployment rates

Figure 2.2: Summary of Sources used to Construct Youth Unemployment Rates

	Name	Variable	Level	Scope	Source
(1)	Unemployment Rate - Numerator	Class A and Class B Unemployed - Male	Occupation-city	21 cities*	1931 Special Census of Unemployment (U.S. Census Bureau)
(2)	Unemployment Rate - Denominator	Number of Employed - Male	Occupation-city	same as (1)	1930 Population Census (U.S. Census Bureau)
(3)	Regional occupation-unemployment rates	Average{(1) / ((1) + (2))} across cities within region	Occupation-region	4 regions	Author calculation
(4)	Occupational share	Number of employed in occupation/number in all occupations	Occupation-city	981 cities	1930 Census 100% count records (IPUMS)
(5)	Youth unemployment estimate	$\sum(3) \times (4)$	City	981 cities	Author calculation

(1) Unemployment rate (numerator): Special Census of Unemployment 1931

The U.S. Census Bureau organized the Special Census of Unemployment in January 1931 in 21 selected urban areas - 18 cities and three boroughs of New York City. The same schedule form was used to make the returns of this census comparable with the employment census in April 1930. Crucially, the Census also added questions on sex, age, occupation, marital condition, race, and nativity. As far as possible, the same enumerators who canvassed these areas in 1930 were reemployed for the special census in 1931. These enumerators were instructed to visit each family and ask whether or not any member of the household who ordinarily worked at a gainful occupation was unemployed on the preceding day and, if so, ask the specified questions and make detailed entries.

The majority of the unemployed fall under two classes, and I collect data on both. Class A contains persons out of a job, able to work, and looking for a job. Across the 1931 census, 20.4 percent of gainful workers from 1930 were classified as Class A unemployed. Class B includes persons having jobs but on lay-off without pay, excluding those sick or voluntarily idle. This class constituted another 3.9 percent of all gainful workers in 1930.

As stated in the introduction to the Special Census statistics, the data in 1931 was published such that comparisons to 1930 could be made. Therefore, the age and occupation distribution in these tables was made to conform as closely as possible with the age and occupation distribution of the gainful workers as presented in the 1930 Census. Likewise, the occupations in 1931 were classified into the same occupations as in 1930.

To obtain the number of unemployed persons by age group and occupation in each of these cities by 1931, I digitize Table 12 of the Special Unemployment Census of 1931. For example, I observe that 167 deliverymen in the 10-19 age group are Class A unemployed, and 12 are Class B unemployed in Birmingham, AL. In total, I collect data on 21 cities spanning the special enumeration area of the 1931 Census: Boston MA, Buffalo NY, New York - Bronx NY, New York - Brooklyn NY, New York - Manhattan NY, Philadelphia PA, Pittsburgh PA, Cleveland OH, Dayton OH, Chicago IL, Detroit IL, Duluth MN, Minneapolis MN, St. Louis MO, Birmingham AL, New Orleans LA, Houston TX, Denver CO, Seattle WA, Los Angeles CA, and San Francisco CA.

Summary statistics are shown in Table 19. On average, there are 36 occupations reported. In the published tables for cities in 1931, only those occupations in which 100 or more persons were returned as unemployed are published at the age-occupation level. I discuss possible issues presented by this missing data below.

(2) Unemployment rate (denominator): Census 1930

The term "gainful workers" in Census usage includes all persons, 10 years old and over, who usually follow a gainful occupation. It does not include women doing housework in their own homes, without wages, and having no other employment, nor does it include children working at home, merely on general household work, or chores, or at odd times on other work. The detailed occupation classification for gainful workers by States comprises 534 occupations and occupation groups. In the tabulation of the unemployment returns, this list of occupations was reduced by consolidation to 330.

Employment by occupation for different age groups in 1930 comes from Table 12 in the state reports from the 1930 Census. Specifically, this table reports the number of employed persons by occupation in cities of 100,000 or more for 330 different occupations. Continuing with the example from (1), I observed 458 delivery men enumerated by the Census in the 10-19 age group in Birmingham, AL, in 1930. I collect occupation-city data for the same 21 urban areas enumerated by the special census of unemployment in 1931 in (1)[5].

(3) Constructing regional occupation-unemployment rates

For each occupation in cities reported in both the 1930 and 1931 censuses, I define the youth

[5]The age brackets are: 10-17, 18-19, 20-24, 25-34, 35-44, 45-54, 55-64, 65-74, 75 and over. The brackets 10-17 and 18-19 were combined in 1931.

unemployment rate as:

$$unemp_{ij} = \frac{ClassA_{1931,ij} + ClassB_{1931,ij}}{ClassA_{1931,ij} + ClassB_{1931,ij} + Employed_{1930,ij}} \tag{2.2}$$

where i denotes the occupation, and j denotes the city, and all measures are for the age group 10-19. I then compute the average unemployment rate by occupation for each region by calculating the average occupation unemployment for all cities within a given region, weighted by total males in the labor force in 1930.

(4) Occupational shares

To extrapolate unemployment rates to all cities enumerated by the Census in 1931 (approximately 900), I first obtain youth occupational shares for all cities. Specifically, I aggregate person-level records from the 100 percent count 1930 Census returns available on IPUMS. My sample includes all 10-19 year-olds reporting an occupation in 1930. The occupation variable available in the 100 percent count records was standardized to 1950 occupational definitions, which vary slightly from those published in Census reports in 1930. I discuss this potential issue in more detail below.

(5) Youth unemployment estimates

Finally, using occupational shares from (4) and the regional rates described in (3) I compute average city-level youth unemployment rates:

$$unemp_{j(k)} = \sum_{\forall i} \omega_{i,j} \times unemp_{i,k} \tag{2.3}$$

where $\omega_{i,j}$ denotes the occupational share of occupation i in city j and $unemp_{i,k}$ is the unemployment rate of occupation i in region k.

Figure B.5 plots $unemp_{j(k)}$ and shows strong regional clustering with relatively high rates in the Midwest and Northeast and low rates throughout the South and West. To help explain the causes of this clustering, Table 20 presents the most common youth occupations by region. The column "# Cities" reports the number of cities in which the occupation is the most common one and the "Weight" column reports the share of the youth labor force in that occupation. In both the Midwest and Northeast, operatives and laborers in manufacturing constitute much more of the youth labor force - with a higher estimated unemployment rate of between 30 and 40 percent - than in Southern cities. Additionally, the weight placed on these occupations in the computation of the total unemployment rate is considerable, between 20 and 30 percent. On the other hand, the South youth labor force is dominated by servants and retail workers, who saw lower unemployment rates. In only two cities manufacturing laborers make up the largest share in the South, and the weight is below 15 percent. The methods in the empirical section will flexibly control for region-by-year fixed effects to account for regional patterns.

Data limitations and measurement validation

Admittedly, the estimated youth unemployment rate using the extrapolation procedure discussed above is an imperfect representation of local labor market opportunities for youth. In the literature, opportunity costs are typically proxied by youth/low skill (no high school diploma) wages. However, to the best of my knowledge, wages for youth during the Depression across many cities do not exist. Unemployment rates are, conceptually, the closest proxy for opportunity and, empirically, the best available systematic data.

Three sources of potential mismeasurement could impact my empirical work: (1) incomplete occupation data for cities in 1931, (2) regional, not city-level, average occupation unemployment rates, and (3) employment shares derived from aggregating 1930 census records with imperfect occupation categorization.

I take several steps to quantify the possible mismeasurement induced by (1) - (3) and find that the extrapolation procedure produces highly predictive estimates of actual values in a subsample of cities and youth welfare enrollments later in the Depression. Using the sample of 21 urban places for which I have actual unemployment rates in 1931, I estimate the true measurement error at each step (1) - (3). The scatter plot of actual vs. estimated rates for these cities, along with a 45-degree line that denotes a perfect fit, is plotted in Figure 4.11. Panel (A) plots actual (total) youth unemployment rates vs. estimates computed using a weighted average of occupation rates. The difference between the two comes from missing, but relatively unimportant, data on occupations in 1931. The correlation between the two is very high (0.9), meaning the omitted occupations in 1931 do not significantly alter city-level youth rates.

Next, in Panel (B), I plot the scatter plot of actual rates versus weighted averages using $regional$ (versus city-level as in the previous step) unemployment rates. The weights assigned to each regional rate are actual employment shares reported by the 1930 Census publications. Though the fit worsens, there is still a robust correlation between the true and estimated rates (0.8).

Finally, in Panel (C), I plot the actual rates versus weighted averages using regional rates and aggregated employment shares from the 100 percent Census records instead of the reported employment shares from the 1930 publications. The difference between the two is that occupational categories in the 1930 complete count census were standardized to 1950 occupational categories. Even though the fit becomes worse (correlation coefficient of 0.5), it is still remarkably high in this small sample.

As a second exercise, I find that my 1931 youth unemployment rates predict youth welfare enrollment by 1934 in a larger sample of cities. The data from 1934 comes from the *Survey of Urban Workers on Relief in May 1934* produced by the Works Progress Administration ([Wood et al., 1937]). Specifically, I collect data on the number of people receiving welfare payments from the WPA in 59 cities by age group. Next, I construct a measure of the relief rate (16-19-year-old males on relief rolls over the total number of 16–19-year-old males in the 1930 Census) and regress this rate on the estimated youth unemployment rates, controlling for the average relief from the WPA in the city (total relief over total population). Using ordinary least squares, I find that the youth unemployment estimate is a strong predictor (R^2 of 0.60, p-value of 0.06 with robust standard errors) of the 1934 relief rate. The residualized scatter plot of the two is shown in Figure B.8.

2.3.2 Public education quality

This section describes data on K-12 education quality at the city level during the 1920s and 1930s in the United States. Educational statistics come from the *Biennial Survey of Education*, a publication of the U.S. Office of Education. The *Survey* contains information on state-run school systems, city school systems, universities, colleges, professional schools, teachers colleges, and private high schools and academies. The statistics were assembled by contacting the roughly 31 thousand school systems. In this paper, I use the city-school system data for each report between 1922 and 1938, which had a very high (98-99 percent) survey response rate throughout the period.

The scope of the data for cities is vast - over 118 variables regarding enrollments, expenditures, and revenues - and I use only a sample of these variables in this paper.[6] While my primary variable of interest is total education spending per pupil, I also collect data on enrollments and teachers and show, in Section 2.5, that reductions in expenditure had a direct impact on more direct measures of school quality: student-teacher ratio, average teacher wages, and term length.

In all, I collected expenditures in 1930 and 1934 for all cities with a population of above 10,000 as of 1930 (564 cities). For panel analysis, I collect all other data for cities above 30,000 in population (220 cities)[7].

Figure B.4 shows the extent to which public school quality in the United States suffered during the Depression. Panel A shows that the student-teacher ratio increased, on average, from around 33.5 to 37.5 from 1930 to 1934 for secondary schools, with no significant change in elementary schools. This increase is driven both by higher enrollments into secondary schools and higher teacher dismissals. Nominal teacher wages, as indexed to other non-teaching wages measured in 1940, Panel B, show that salaries decreased by around 12 percent from 1930 and 1934, with a complete rebound by 1938. Real total per-pupil expenditure, Panel C, decreased significantly during the Depression after continually growing during the 1920s, and the school term was reduced by approximately four days on average. My primary measure of school quality shock is the difference in total real per-pupil expenditure between the 1934 (Depression trough) and 1930 (Depression peak) school years:

$$\Delta exp_j = \log\left(exp_{j,1934}\right) - \log\left(exp_{j,1930}\right) \tag{2.4}$$

[6] The full dataset is available at paweljanas.com.

[7] I combine data for cities reporting multiple districts: Aurora, IL, Evanston IL, Beaumont TX, Berwyn IL, Dearborn MI, Pueblo CO, Saginaw MI, Troy NY, Waterloo IA, Wheeling WV, Clarksburg WV, Corning NY, Berwyn IL, Manchester CT, Clinton IA

2.3.3 Linked Census records, 1930 - 1940

Outcome variables - education attainment and wages - as well as a host of demographic controls included in the microeconomic analysis of Section 2.5.2.1 come from the 100 percent count Census records in 1930 and 1940. I use the crosswalks provided by the Census Linking Project ([Abramitzky et al., 2020]) and IPUMS publicly available data ([Ruggles et al., 2020]) to link records over time using the ABE procedure. I conduct my analysis on both the person and cohort-city level. Cohorts were aggregated based on reported age in 1940 and city of residence in 1930.

Starting with the entire sample of 7.49 million records of 20-40-year-olds in 1940, I imposed several restrictions to arrive at my primary analysis sample. I first dropped any person that living not in a city in 1930, which eliminated 4.6 million people located in rural areas. Next, I further drop all people without education attainment information. This restriction eliminates 50 thousand people. Finally, as my primary sample, I dropped anyone below ten and above 22 in 1930, eliminating 1.29 million observations. This leaves me with my final sample size of roughly 1.4 million people.

I replace reported income (*incwage*) for the 99th percentile and above as $5,250 (1.5 times earnings at 98th percentile) following the methods in [Acemoglu and Angrist, 2000]. I compute weekly wages based on this winsorized total earnings and divide by the number of weeks worked in 1940 to arrive at log weekly earnings.

I classify a person to be a non-native if the person is either foreign born (*nativity* = 5) or if both parents are foreign born (*nativity* = 4). I classify students into blue- vs. white- collared families regarding socioeconomic status. Blue-collar is an indicator variable taking the value of 1 if the person's father reported working in a blue-collar occupation as a craftsman (e.g., blacksmith), operative (e.g., bus driver), service worker (e.g., bartender), or a non-farm laborer (e.g., teamster)[8].

[8]Census variable $occ1950$ in the 500s, 600s, 700s, and 900-978.

All other respondents with a working father are classified as white-collared.

In Section 2.5.2.1, I explore the robustness of my results based on an analysis of siblings, which is deduced based on data at the household level. For each household in 1930, I check whether an individual reports living with any siblings, and I compute the total number of individuals under the age of 19 in the household. If an individual reports siblings and the number of youth in the household is above 1, I assume that the youth in the household are related.

2.3.4 Summary statistics

Table 19 presents the summary statistics. In Panel A, the unit of observation is the city-cohort (e.g., Chicago 1930 graduating class); in panel B, it is the person; and in Panel C, it is the occupation-city (e.g., carpenters in Chicago). All dollar amounts here and throughout the paper are converted to real 1967 dollars using the Consumer Price Index.

In panel A, I present summary statistics for 564 cities across 12 cohorts from 1927 through 1938. I define a cohort as the year a person turns 18. In total, my balanced panel has 6,768 cohort-city observations. 12th-grade completion denotes the proportion of the cohort that reported at least 12 years of education. Blue-collar 12th grads/total is the proportion of the cohort that graduated and came from a blue-collar family, with a similar definition for white-collar, the share of blue-collar youth, and the share of white-collar youth. Unemployment - Youth denotes the estimated 1931 unemployment rate, discussed at length in Section 2.5. Δ Edu. Spend is the 1934-1930 change in log expenditure per pupil as described in Section 2.3.2. School expenditure is log expenditure per pupil in 1930. Youth labor share is the share of 10-19-year-olds reporting to be in the labor force according to the 1930 Census divided by the total number of 10-19-year-olds at the city level.

In panel B, I present summary statistics of my person-level sample of linked Census records. The variable descriptions can be found in Section 2.3.3. Panel C summarizes the data used to construct youth unemployment rates estimates for 1931. Starting with 981 enumerated cities in the Census of 1930, I drop all cities for which 1931 occupation data covers less than 50 percent of youth workers in 1930, which leaves me with 925 total cities. There are 67 distinct occupation categories of youth on average, and data on 36 of them exist in 1931. The average weight given to an occupation in the total youth unemployment rate computation is around 2.8 percent. The average regional unemployment rate for an occupation is 25.5 percent, while the median is 24.2 percent.

2.4 Empirical Framework

2.4.1 Identification Strategy

I estimate the dual effect of youth unemployment and education spending on schooling choices and wages using a difference-in-differences research design that compares outcomes of cohorts in cities before and after the onset of the Great Depression. The fundamental identifying assumption is that, in the absence of the Depression, schooling choices of households across cities would have evolved in parallel. As described in Section 2.5 below, I present direct evidence to support the validity of the parallel trends assumption in Figure B.6 that shows outcomes for cohorts in low and high unemployment cities moving together during the period preceding the Great Depression and their trends only beginning to diverge afterward.

This fact is both reassuring and plausible. In the short period considered in this paper, there is no particular reason to expect that the trends of school-going should vary significantly across cities unevenly hit by the Depression *before* the 1930s. Indeed, the factors that contribute to different levels of educational attainment in regular times, such as the skill-premium, cultural norms, or the availability and proximity of schools, evolved over the preceding three decades, not years. Conversely, the sharp turn of the economy starting at the end of 1929 was an unexpected and severe shock for households.

Another concern is that collinearity between education spending and youth unemployment will render unstable estimates of the effects of interest. However, I do not find evidence that collinearity is prohibitive in my setting. Using standard diagnostic tests, I find that the variance inflation factors for both are less than 1.03 and that the condition number is 8.4, which are both smaller than the standard thresholds of 5 and 10, respectively. Moreover, the bivariate correlation between these two shocks is not significant. In Figure B.3, I plot the relationship between the two treatment variables for the 564 cities in my sample and find a correlation coefficient of roughly 0.09. This lack of correlation is not surprising: at the time, education funding came primarily from local property taxes while most youths worked in industries (retail, manufacturing operatives) that were not directly affected by the housing market crash. Certainly, collinearity would be an issue if youth worked in construction: worse housing market conditions would decrease the local tax base (and thus education funding) and youth employment opportunities. However, this was not the case.

2.4.2 Estimation

My baseline econometric model is a difference-in-differences regression at the cohort level. Specifically, I estimate regressions of the following form:

$$S_{jk} = \alpha_j + \beta_k + (Unemp_j T_k) \cdot \gamma_1 + (\Delta Exp_j T_k) \cdot \gamma_2 + (C_j T_k) \cdot \delta_1 + \epsilon_{jk} \tag{2.5}$$

where S_{jk} is a cohort outcome in city j and year k, $Unemp_j$ is the youth unemployment estimate, T_k is a binary variable taking the value of 1 for all k after 1933 and 0 before 1930, β contains cohort fixed effects, α includes the city of residence fixed effects, ΔExp_j is the 1934 - 1930 change in log per-pupil expenditure, and C_j is a vector of location-specific variables.

The coefficients of interest are γ_1 and γ_2, which measure the differential change in the outcome for cohorts following the onset of the Great Depression, holding constant characteristics and aggregate differences in outcomes across cities and over time. To account for serial correlation and city-specific random shocks, I cluster the standard errors at the city level in all specifications.

As a more flexible alternative to Equation 2.5, I also estimate specifications that allow the effects to differ by year:

$$S_{jk} = \alpha_j + \beta_k + \sum_{i=1923}^{1938} (Unemp_j T_i) \cdot \gamma_{1,i} + \sum_{i=1923}^{1938} (\Delta Exp_j T_i) \cdot \gamma_{2,i} + (C_j T_i) \cdot \delta_i + \epsilon_{jk} \quad (2.6)$$

where T_i is a year dummy taking the value of 1 if $i = k$ and 0 otherwise. All other variables are as previously defined. The 1928 cohort is always the omitted category, so the coefficients should be interpreted relative to that year. These coefficients are informative about the timing of the effect of youth unemployment and expenditure cuts on education attainment and the validity of the parallel trends assumption. If cities exposed unevenly to the Depression have common pre-trends, then the coefficients should be equal to zero for any cohort before 1930.

I control for several observable and plausibly confounding variables in all specifications above, but I also report estimated coefficients before and after inclusion. First, due to the regionally clustered nature of the youth unemployment measure, I include region by year fixed effects, such that the results rely only on *within* region variation. Second, to address potential bias arising from city-level omitted time-varying variables, I include interaction terms between baseline expenditure and county unemployment with cohort dummies. For example, variables that are potentially correlated with baselines - such as teacher recruitment or attitudes toward public spending and education - that would also differently change the incentives of youth to exit school over the Depression are accounted for. Additionally, these interactions partially account for heterogeneity in the effect size such that the estimated coefficients are averages across different types of cities. I explore heterogeneity in more detail in Section 2.5.2.1.

2.5 Results

In this section, I present the paper's main results. To quantify the causal effects of interest, I present a series of formal difference-in-differences estimates for education attainment and mean log weekly earnings of cohorts. Also, at the cohort level, I examine heterogeneity in the response on both the blue- and white-collar education attainment rates and find that the average effect is driven by an influx of students from blue-collared families. To provide additional evidence on the role of education spending on quality, I examine the impact on teacher wages, student-teacher ratio, and term length. Reductions in wages and term length and increases in the student-teacher ratio are all significantly related to expenditure cuts, affirming the assumption that quality - and not just spending - decreased in cities across cohorts during the Depression. I conclude the cohort-level analysis by showing that the effects are driven by cities that, in 1940, had highly educated teachers and in states without youth labor regulation as of 1931. I do not find evidence that the effects differed based on county-level New Deal spending, an unsurprising result given that the largest work-relief program that could impact the opportunity cost of youth - the Civilian Conservation Corps - did not employ men below the age of 17.

I then turn to the regression results using person-level data. I alleviate concerns surrounding omitted confounders by investigating *within family* differences in schooling choices using a sample of roughly 187 thousand linked siblings. I find that including household fixed effects increases the magnitude of the estimates by 20-50 percent above baseline results. I rule out a selective-migration alternate explanation and find that the results remain unchanged when migrants are excluded from the sample.

2.5.1 Cohort estimates

Table 21 presents estimates from the pooled difference-in-differences specification given by equation 2.5 using cohort graduation rates as outcomes in columns (1) - (3) and mean log weekly wages in columns (4) - (6). The sample across all specifications includes only city cohorts with at least 100 linked records between 1930 and 1940. The first column reports estimates from a baseline specification that includes only the main shock variables interacted with the $Post$ indicator, fixed effects for both the cohort and city of residence in 1930. The second (fourth) and third (fifth) column add region by year fixed effects, baseline expenditure, youth employment, and unemployment interacted with cohort indicators. The pre-period includes the years 1927-1929, and the post-period is 1934-1938 for schooling outcomes and 1934-35 for wages[9]. I standardized the main independent variables to have a mean of zero and a standard deviation of one to ease interpretation.

The coefficient estimate on education spending (top row) implies that, after the onset of the Depression, cohorts that lived in cities with one standard deviation (17 log points) lower spending graduated at a 0.75 percent lower rate compared to their city peers before the Depression. To put this number in perspective, note that the graduation rate of cohorts in my sample increased from approximately 40.5 percent in 1927 to 54.5 percent by 1936, or roughly 1.4 percentage points per year. Thus, cohorts in low spending districts experienced nearly half of the growth in educational attainment during the Depression as compared to their peers in high spending districts. The estimate on youth unemployment (second row) is even starker. After the onset of the Depression, the coefficient implies that cohorts that lived in cities with one standard deviation higher youth unemployment (5 percent) graduated at roughly 1.1 percent higher rate than their city peers before the Depression. In all, a 10 percent cut in education spending resulted in a 0.44 percentage point

[9] I trim the post sample to only include these years to capture wages of individuals with post-secondary education in the $Post$ = 1 group.

decrease. In comparison, a 10 percent jump in unemployment resulted in a 2.8 percentage point increase in the graduation rate of Depression-era cohorts.

Even though both education spending and unemployment appear to contribute to higher education attainment in Depression-era cohorts, I do not find consistent evidence that both also contributed to higher earnings by 1940. The post-period includes only two cohorts (1935-36) to allow Depression-era students to graduate post-secondary education when earnings are reported in the 1940 Census. Turning to the results on mean cohort wages in columns (4) - (6), I find that cohorts in higher spending districts had *lower* wages by 1940. The magnitude of the estimate suggests that one standard deviation in spending results in 1 log point lower mean weekly wages. On the other hand, I find a significantly more significant increase in wages for cohorts in higher youth unemployment cities with one standard deviation in unemployment resulting in a 4 log point increase in weekly wages.

These earnings results should be interpreted with caution: the earnings data, especially for Depression-era cohorts, are for very young men (early 20s) that may not indicate actual lifetime income. In an ideal experiment, the difference-in-differences design would compare earnings at the peak of one's career (e.g., 40-50 years old) of pre- vs. during Depression cohorts at the same age. Furthermore, the earnings for the small proportion of men who continue to graduate education (high wages) are missing in the Depression-era cohorts, which would bias the earnings results if youth unemployment or education spending correlated with a student's desire to pursue graduate education.

2.5.1 Cohort composition of graduates

The factors leading to higher education attainment during the Depression impacted students differently across socio-economic strata. At the cohort level, I compute the proportion of 10th and

12th grade "graduates" from blue vs. white-collar families. In the sample, around 70 percent of the cohort comes from a family with a blue-collar father and 30 percent with a white-collar father (see Table 19). However, the composition of secondary *graduates* does not reflect the composition of the cohort: across all years, only 57 percent (22/22+29) of graduates come from a blue-collar family, suggesting that less advantaged students drop out earlier than their more advantaged peers. Did the Depression widen or narrow this achievement gap?

Table 24 reports the results from the same difference-in-differences specification using graduation rates of different groups in columns (1) - (3) for at least 10 years of schooling and columns (4) - (6) for at least 12 years. In column (1), the blue-collar proportion of the total cohort that completed at least 10 years of education is the outcome variable. In column (2), the outcome is the same as in column (1) for the white-collar proportion, and in column (3), it is the non-native proportion. The estimated coefficients show that most of the effect is driven by blue-collar students entering secondary education (column (1)) and eventually graduating high school (column (3)), with no statistically significant effect on the proportion of white-collar graduates. One standard deviation in the youth unemployment rate increased the proportion of 10th (12th) grade blue-collar grads during the Depression by 1.05 (0.97) percentage points.

To put this number into context, consider the 1929 cohort (43 percent graduation rate). This cohort's high school graduates were equally split between white- and blue-collar grads (21.5), although the total number (both grads and non-grads) of blue-collar youth was roughly twice as large as that of white-collar youth, a 70:30 split in the population. The results indicate that, over eight years that followed, cities with one standard deviation larger youth unemployment share increased the share of blue-collar graduates to roughly 26.05 percent: 25 percent baseline - half of the cohort graduation rate of 50 percent by 1938- plus an additional 1.05 percent due to the Depression. Thus, the unemployment shock increased the ratio from 50:50 blue:white-collar grads in 1929 to 53:47 (26.5:23.5) by 1938 or approximately 15 percent of the gap between high school graduation and population composition of 70:30.

These results are reassuring: if the opportunity cost model accurately describes the incentives faced by youth during the Depression, we should not see the effects found in the previous section to be uniform across different socio-economic classes. Students from more privileged backgrounds typically continue to secondary school, regardless of the local labor market for youth, so we should not see pronounced effects on this group. On the other hand, the results should, in fact, load on the youth who typically did not finish secondary education and who were likely to drop out in the first place - in the context of the model, the high κ_c individuals - which is precisely what the results indicate.

2.5.1 Education quality measures

Using panel data on school districts, I now turn to possible mechanisms. I show that decreases in education spending were concentrated on expenses related to instruction and led to a worse learning environment. I report the results of the difference-in-differences design when education quality measures are outcomes in Table 23. In column (1), the outcome is average log teacher wages defined by total teacher payroll divided by the number of teachers. In column (2), the outcome is the student-teacher ratio in secondary schools. In column (3), it is the number of school days in a term. The same control variables and pre/post period are used as in all specifications before.

The estimates in the first row all show a significant correlation between spending and quality. One standard deviation in spending cuts resulted in a 6.7 log point decrease in teacher wages, a 0.85 increase in the student/teacher ratio, and a 1.1 day decrease in the school year. Considering that average real teacher wages decreased by 12 percent between 1930 and 1934, the 6.7 log point decrease is significant. This is the expected result if school districts cut spending evenly across the various expenditure categories, as over 50 percent of a school district budget was dedicated to teacher wages (Figure 4.12). In addition, the baseline student-teacher ratio for secondary schools in 1929 was approximately 33.5, meaning that this ratio increased only about 2.5 percent (annually) for Depression cohorts in cities with one standard deviation larger spending cuts. Likewise, the average term length in 1929 was 184 days, and the 1.1 day decrease resulting from cuts represents only a 0.6 percent reduction.

The results in Section 2.5.1 above indicated that youth unemployment pushed students into school, deduced by the years of schooling reported at the individual level in the Census. This result is bolstered by the positive impact of youth unemployment on *district* reported enrollments in this analysis. Turning to the second row of Table 23, I report the coefficient estimate of unemployment and find no significant impact on teacher wages or term length (columns (1) and (3)) but do find an impact on the student-teacher ratio (column (2)). To sum up, I find that education quality was directly related to expenditure cuts during the Depression and argue that this is the mechanism through which spending enticed (or dissuaded) students from entering or finishing secondary school.

2.5.1 Cumulative distribution function of education

Next, I investigate whether the Depression drove attainment at all levels of education or whether it was concentrated on entry into and exit from secondary school. Difference-in-differences in the cumulative distribution function of education provide information on which grades experienced the most significant influx of students. I estimate the following specification:

$$S_{jkm} = \alpha_j + \beta_k + (Unemp_j T_i) \cdot \gamma_{1m} + (\Delta Exp_j T_i) \cdot \gamma_{2m} + (C_j T_i) \cdot \delta_{1m} + \epsilon_{jk} \tag{2.7}$$

where S_{jkm} measures the attainment rate for cohort j in city k of at least m years of education. Figure B.7 plots the vector γ_{1m} in the top panel and γ_{2m} in the bottom panel. The shape of the figures indicates that both youth unemployment and expenditure resulted in significantly higher rates of 9th-grade completion (the peak) with some attrition between grades 10 through 12. The effect on entry into secondary school is twice as large as the one reported in Table 21: a 2 pp increase in entry per standard deviation in unemployment. However, there was no impact on elementary education or college-going, as many states by this time had compulsory education laws that mandated at least six years of education, and college education was costly. The Depression thus increased average schooling primarily through entry into secondary schools.

2.5.2 Heterogeneity: teacher quality, New Deal, youth labor regulation

I conclude my discussion of the effect of youth unemployment and education spending on schooling by presenting a set of results regarding the heterogeneity of the average impact across types of cities: high vs. low teacher quality cities, high vs. low New Deal federal spending cities, and finally regulated vs. non-regulated cities.

Table 24 reports the estimated coefficients of the difference-in-differences specification across six different sub-samples. In columns (1) and (2), I investigate whether the impact varies based on the quality of public teachers within a city, which I proxy by the average reported years of education for teachers within a city in the 1940 Census[10]. The median years of education of teachers in my sample cities are 15.01 years. Column (1) reports the results for cohorts in cities above this median, and column (2) does so for those below. The estimates in column (1) are much larger - though not statistically different from those in (2) at the 90 percent confidence level.

[10]Using the 100 percent count 1940 Census, I compute the average years of schooling of teachers ($occ1950$ = 93) for each city which, unfortunately, includes college professors. The distinction between K-12 and post-secondary teaching did not exist in the 1940 Census.

In columns (3) and (4), I investigate whether work relief programs instituted by New Deal legislation attenuated the effect of Depression shocks on high school enrollment. Using county-level data on total Works Progress Administration grants from [Fishback et al., 2003], I again split the sample based on cohorts in cities in counties that received above and below median per-capita grants ($55.57). I do not find that the impact of education spending or youth unemployment to be different in these two samples, suggesting federal work relief programs did not significantly alter the opportunity costs for youth.

Finally, in columns (5) and (6), I check whether the effect is more pronounced in cities in states which had stricter pre-Depression youth labor regulation statutes. I collect data on the minimum age, grade requirements, and maximum daily and weekly hours for employment in factories and stores as of 1931 for each state from the U.S. Department of Labor Children's Bureau ([United States Department of Labor, 1933]). Then, I split the sample into cities with and without maximum work hours restrictions for 16-18-year-old factory workers. The effect is concentrated in no-regulation states - column (5) - and statistically different (p-value 0.05) from the effect in states with pre-Depression regulation. My interpretation of this result is that the opportunities available to cohorts in regulated states did not change much because of the Depression because they were limited *ex ante*. On the other hand, the change in opportunity cost for cohorts in non-regulated states was plausibly higher because the cost for youth to drop out before the Depression in these states was higher.

2.5.2 Estimates using person-level data

The previous section showed that both changes in education spending and employment opportunities for youth affected schooling choices during the Depression using cohort-level data. This section uses person-level data to show that the results hold even when accounting for unobserved household characteristics and are robust to self-selection bias arising from migration.

Namely, I estimate the following fixed effects specification:

$$S_{ijk} = \alpha_j + \beta_k + \zeta_i + (Unemp_j T_k) \cdot \gamma_1 + (\Delta Exp_j T_k) \cdot \gamma_2 + (C_j T_k) \cdot \delta_1 + \epsilon_{ijk} \tag{2.8}$$

where outcome S is an outcome for person i who reported city of residence j and is of cohort k. All the other variables are as defined before, with the addition of ζ_i which includes household-fixed effects in some regressions. The primary outcomes of interest are whether the individual finished at least 12 years of education, the number of years completed, and log weekly wages reported in 1940. I cluster standard errors at the cohort-city level.

Table 25 reports the results. In columns (1), (4), and (7), I use the full sample of 1.09 million youth in 564 cities. In columns (2), (5), and (8), the sample only includes the 187 thousand individuals who lived with a sibling as of 1930 and whose sibling is also present in the linked sample[11]. Lastly, in columns (3), (6), and (9), I exclude the roughly 380 thousand individuals who reported a different city of residence between 1930 and 1940.

Column (1) recapitulates the main results as found for cohorts: a standard deviation in youth unemployment is correlated with roughly a 1.6 percentage point increase in the probability of finishing high school across individuals as compared to a 1.04 percent increase in the total cohort graduation rate found in the difference-in-differences analysis in section 2.5.1. Interestingly, the coefficient increases to 2.8 percentage points once I account for household-fixed effects.

Turning to the results on the total number of years of school completed, I find that one standard deviation in youth unemployment and education spending led to 0.144 and 0.017 more years of schooling for the 1934-38 cohorts, respectively. The comparison between columns (1) and (3), (4) and (6), and finally (7) and (9) shows that selective migration does not bias the previously found

[11] See Section 2.3.3 for how I deduced relationships within households.

results. In all three cases, the estimates are not significantly different from one another.

2.6 Implications and conclusion

Taking the reduced-form estimates from the previous section at face value, I conclude by estimating the total number of youth pushed into secondary schools across the United States in the 1930s by using a back-of-the-envelope exercise. In a partial-equilibrium world, what would have happened to human capital investment in the 1930s had the U.S. not suffered the largest recession of the century? If transfers from the state or federal governments shored up school district budgets, how many more youth would have stayed in school?

I first compute the actual estimated total number of graduates by cohort in my sample of cities. Then, I apply the graduation rates found in the linked sample for each city-cohort to the total (unlinked) size of the city-cohort in 1940. Across 564 cities, I estimate that the number of graduates rose from around 170 thousand in 1930 to roughly 220 thousand by 1936.

To construct the counterfactuals, I adjust actual graduation rates by the cohort-specific estimates found in the event study as portrayed in Figure B.6. I take the changes between actual youth unemployment and actual education spending cuts at the city level and their steady-state counterparts and multiply them by estimated γ to develop a predicted graduation rate absent the Depression. To do so, I need to make assumptions about the development of unemployment and education spending: across all cities and cohorts, I assume a youth unemployment rate of 10 percent and education spending at 1926-1930 real growth rate at the city level (average = 0.10):

$$\begin{aligned} \widehat{rate_{j,k}}^{1} &= rate_{j,k} + \widehat{\gamma_{1,k}} \times (0.1 - Unemp_j) + \widehat{\gamma_{2,k}} \times (\Delta Exp_{j,26-30} - \Delta Exp_{j,30-34}) \\ \widehat{rate_{j,k}}^{2} &= rate_{j,k} + \widehat{\gamma_{2,k}} \times (\Delta Exp_{j,26-30} - \Delta Exp_{j,30-34}) \end{aligned} \tag{2.9}$$

The top panel of Figure B.9 presents the total number of actual high school graduates (solid line), number of graduates absent the Depression (using $\widehat{rate_{j,k}}^1$, dotted line), and number of graduates assuming no education spending cuts (using $\widehat{rate_{j,k}}^2$, dashed line). The bottom panel presents the difference between these three lines for cohorts 1930 - 1938.

I find that of the total 1.764 million high school graduates in graduating cohorts between 1930 and 1938 in my sample of cities, approximately 64 thousand were pushed into secondary schools due to the decreasing opportunity cost channel due to Depression shocks. Noting that the urban population of the cities in my sample is about 80 percent of the total urban population, I estimate that 80 thousand youth (64/0.80) finished a high school education due to the Depression across urban America in the 1930s. Conversely, the effect of spending cuts - mainly occurring in the 1935-1938 cohorts (see Figure B.9) - resulted in only 6 thousand fewer graduates or 7.5 thousand in the total urban economy.

This paper used new data sources on youth unemployment and school quality during the Depression and found that children from blue-collared families obtained more education due to deteriorating labor market conditions than they otherwise would. By 1940, these same children earned about 9 percent more than children who did not experience a severe recession in their local labor markets. It further showed that expenditure cuts in the publicly financed education sector attenuated this effect - fewer students stayed in lower-quality schools compared to higher-quality schools, suggesting an essential dual role of labor market opportunity costs and education expenditure on human capital investment decisions. While this project uses short-term outcomes, important questions regarding how the Depression changed lifetime earnings and outcomes in adulthood will remain unanswered until future waves of the Census are released.

CHAPTER 3

LENDER OF LAST RESORT: LOCAL FINANCIAL CONSTRAINTS AND FEDERAL RESERVE POLICY IN THE 1930S

3.1 Introduction

Between 1929 and 1933, more than half of all commercial banks in the United States closed their doors. Some closed temporarily only to reopen after depositor panic abated. Others closed permanently after becoming insolvent due to poor investment choices. The rest were merged with other financial institutions to avoid liquidation. This significant negative shock to financial intermediaries propelled eight decades - and counting - of economic research into the causes and consequences of bank failures.

One important strain of the academic literature regarding the Great Depression concerns the role of the Federal Reserve. The Federal Reserve has been criticized for not taking interventionist actions early during the Depression, mainly for failing to stem the decline in the money supply and not acting, collectively, as a lender of last resort for banks. Empirically, however, it is difficult to estimate the causal effects of Federal Reserve policies because changes in aggregate statistics could also be the result of simultaneous and endogenous reactions by households, firms, and subnational governments. The ideal scenario is to observe the differences in two places that are ex-ante on a similar economic trajectory, but that experienced different policy regimes before and after the onset of the Depression.

This paper uses novel, archival, panel data on local manufacturing and banking conditions in the United States to investigate the link between policy, bank failures, and, ultimately, firm production and employment. I use the divergent policies enacted by the Atlanta Federal Reserve Bank as my empirical laboratory. Atlanta, unlike the other Federal Reserve banks at the time, acted as a lender of last resort for banks inside their region, and Federal Reserve borders sometimes bisected states and consumer markets. I can thus compare the local economic trajectories before and after a quasi-exogenous placement of bank failures, which creates an appealing research setting. This observation was first brought to the literature by [Richardson and Troost, 2009], with important follow-up work by [Jalil, 2014] and [Ziebarth, 2013].

In the first part of my analysis, I investigate whether banks indeed suffered more in regions just outside the Atlanta border than in regions just inside it. How robust is this difference after accounting for pre-exisiting differences in local banking conditions? Is the result driven by a particular set of outliers or does it hold generally? Does it hold after considering outcome variables - such as the value of non-suspended deposits, instead of number of suspended banks - that are, in theory, even more important to lending? Does it survive placebo border permutations or is the result an artifact of chance? Was there a negative impact on bank lending? Do we see an *absence* of bank suspension differentials in border regions of Federal Reserve districts that did *not* follow different lender of last resort policies? In these endeavors and more, I find that the result is, in fact, robust. Credit conditions appeared more favorable in the early years of the Depression inside counties of the Atlanta district and I do not observe any meaningful difference in bank suspension rates in border regions without policy differences.

I turn to local manufacturing outcomes in the second part of my analysis. By combining an industry-level credit survey and 1927 industry-by-county data, I construct measures of financial constraints for each county. In all, the vast majority of small to medium sized manufactures relied on commercial banks to finance operations and investment. It stands to reason, then, the failure of these institutions would hinder their ability to produce and hire. Since commercial banks survived at a higher rate inside the Atlanta region, my hypothesis is that manufacturing output and employment did not drop to the same extent and recovered quicker there as compared to counties just outside the Atlanta region.

I do not find any evidence to support this hypothesis, which contrasts with the results of the existing literature. Manufacturing outcomes were worse, not better, in counties inside the Atlanta region, despite having more banking resources. I do find strong evidence that the county-level financing constraints predict worse outcomes after, but not before, the Depression. However, the interaction between pre-Depression measures of financial constraints and banking panics during the Depression is, curiously, not an important determinant of local economic outcomes.

The rest of the chapter is organized as follows. Section 3.2 describes the historical background and surveys the literature. Section 3.3 discusses the data while Section 3.4 presents the empirical strategy and reports the robust banking results. Section 3.5 analyzes manufacturing outcomes and Section 3.6 concludes.

3.2 Historical Background and Literature Review

This section summarizes the historical and institutional background of banks and firms during the Depression.[1] I then describe new survey evidence linking commercial banks and economically significant firms: small to medium-sized manufacturers [Bureau, 1935].

3.2.1 Banking crises

The sequence of events during the Great Recession (2007 – 2009) – a financial crisis, a deep economic recession, a slow recovery - revitalized the study of how distress in securities markets propagates to households and firms. The closest historical analog to what the U.S. economy was

[1] For a more holistic literature review, see [Wicker, 2000] and [Temin, 1976]

experiencing in the late 2000s is the Great Depression (1929 – 1937). Sparked by a stock market crash in the fall of 1929 and fueled by banking failures in the ensuing four years, the Depression has been one of the most studied events in U.S. economic history. Among others, [Friedman and Schwartz, 2008] and [Bernanke, 1983] consider bank panics as a critical driver of the economic contraction and the Depression's depth and length. From a policy perspective, understanding why financial institutions fail and which policies (if any) prevent failure are perhaps the defining questions for financial market regulators and central banks.

There are two main hypotheses surrounding the causes of bank failures during the Depression in the United States. The first hypothesis is that banks became insolvent once the underlying value of their assets - mainly mortgages, business loans, and bonds – declined in value. ([Temin, 1976], [White, 1984], [Calomiris and Mason, 2003]). Simply put, banks invested poorly in the years leading up to 1929. They made too many loans to businesses that would eventually fail, too many loans to stock speculators who would get wiped out in the 1929 crash, and too many mortgages during the post-World War I construction boom of the 1920s. The empirical evidence for the solvency hypothesis comes in various forms: state-level bank failures and economic and loan characteristics, bank-level data and loan quality, and the quantity and changes in bond yields. The outcome of interest in these studies is, typically, the probability or severity of bank suspensions, and they have found that fundamental economic shocks can predict bank failure well. It is doubtful, these studies conclude, that policy interventions, such as lender of last resort policies that provide liquidity assistance, could have done much to rescue failing banks during the Depression.

The second hypothesis is that mass withdrawals brought down the banking sector by fearful and anxious depositors – the illiquidity hypothesis. The stock market crash created uncertainty about future economic prospects. In addition, news reports on failures of large and connected institutions created uncertainty about the financial soundness of the banking sector. As a result, individuals lost trust in the system and rushed to withdraw money from banks, while banks could not liquidate assets to meet demands and had no other option but to cease operations ([Friedman and Schwartz, 2008], [Wicker, 2000]). Under this hypothesis, the Federal Reserve could have alleviated the crisis by acting as a lender of last resort: lending cash to banks in exchange for illiquid assets at non-fire sale prices. Some have heavily criticized the Federal Reserve System for not doing enough to stop the Depression with this reasoning.

Both hypotheses are conceptually plausible and empirically justified, depending on the time, place, and level of aggregation of the data. No single factor can fully explain the extent of banking panics during the Great Depression. The best available evidence on the relative significance of the solvency and liquidity channels comes from [Richardson, 2007]. Using quarterly data at the bank level during 1929-1933, he finds that the bank failures changed over time: early on, small rural banks failed at an increasing rate. The collapse of Caldwell and Company and the Bank of the United States in 1930 propagated bank runs. By 1931, once Britain left the gold standard, asset value declined, and most banks that failed were, in fact, insolvent. Nearly three-fourths of failed institutions were deemed insolvent, one quarter was solvent and reopened for business or merged with other banks, and one-half closed due to depositor withdrawals.

3.2.2 What was the scope of Federal Reserve intervention?

The Federal Reserve has been criticized for not taking interventionist actions early during the Depression, mainly for failing to stem the decline in the money supply and not acting, collectively, as a lender of last resort for banks. Empirically, however, it is difficult to estimate the causal effects of Federal Reserve policies because changes in aggregate statistics could also be the result of simultaneous and endogenous reactions by households, firms, and subnational governments. The ideal scenario is to observe the differences in two places that are ex-ante on a similar economic trajectory, but that experienced different policy regimes before and after the onset of the Depression.

The application of Federal Reserve policies to Mississippi, whose northern and southern counties were under two policy regimes that differed significantly until 1931, is such an example ([Richardson and Troost, 2009], [Jalil, 2014], [Ziebarth, 2013]). The southern counties were under the jurisdiction of the Atlanta Federal Reserve Bank (6th District), whose leaders followed "Bagehot's rule": a doctrine stipulating that central bankers should extend credit to illiquid institutions during financial panics, thus staving off losses due to runs on healthy banks. In his historical account of the Atlanta Federal Reserve Bank, [Gamble, 1989] recounts instances where Bank officials physically carried currency into banks to portray to worried depositors that banks were solvent. In the north, however, the St. Louis Federal Reserve Bank (8th District) was a proponent of the "Real Bills" view that the supply of credit should contract during recessions since a lower level of economic activity required less credit to sustain it. The Bank maintained this stance until the summer of 1931.[2] Thus, for the first two years of the Depression, banks in Mississippi were subject to two fundamentally different policies. What was the result?

Using bank-level and county-level data, [Richardson and Troost, 2009] find striking results. Bank survival rates, credit, and commercial activity were all quantitatively higher in 6th District Mississippi counties when compared to nearby Mississippi counties in the 8th District, especially during the 1930-1931 panics. The evidence presented in their paper indicates that lender of last resort policies, if applied broadly, may have curtailed the initial wave of banking panics. Expanding the geographic scope to the full border, [Jalil, 2014] tests whether bank performance in counties located within 50 miles of the entire 6th District border, not just in Mississippi, depends on the

[2] The St. Louis Federal Reserve accomodated the seasonal business cycles of its main industry (agriculture) by expanding and contracting credit procyclically. During panics, the St. Louis Fed required double collateral (giving up 2 dollars of liquid assets for 1 dollar in cash) which discouraged banks from accessing the discount window [Wheelock, 1997]. In July 1931, it reversed course and eased collateral requirements.

Federal Reserve policy regime. Using county-level bank data, he finds that bank suspension rates in 1929 and 1930 were systematically lower inside the 6th District than in nearby counties across the border.[3]

3.2.3 Banking panics and local economic outcomes

The direct cost of bank closures – loss of wealth to depositors who were paid back a fraction of their claims after receivers liquidated a bank – is only one channel through which bank failures can hurt local economies. Another channel is the increased costs of financial intermediation [Bernanke, 1983]. Banks, unwilling to take on risks in an uncertain environment, invest in safe assets and are reluctant to extend credit even to credit-worthy businesses, halting hiring and production. Thus, when firms need funds to invest or refinance debt obligations, they cannot find lenders and their output declines.

The experimental setting in Mississippi and the Atlanta Federal Reserve border can be used to study whether bank failures drive local economic conditions. [Ziebarth, 2013] collects plant-level data from the Census of Manufactures during the Depression and uses a difference-in-differences design to compare north versus south Mississippi plants. He finds a 37 percent fall in physical output in the north but no differential effect on total workers – the effect is driven by the intensive margin. Aggregating at the county level, he finds a significant adverse effect on the number of workers.

However, this establishment-level data contain no financial information and, therefore, cannot adequately measure the needs for external finance. Studying a set of large industrial firms whose employment and financing needs can be jointly observed, [Benmelech et al., 2019] provides new evidence that financial frictions were responsible for much of the decline in employment. They

[3] The Atlanta Fed (District 6) shared a border with four other Federal Reserve Districts: Richmond (District 5), St. Louis (District 8), Cleveland (District 4), and Dallas (District 11).

estimate that employment in these large firms would have been about 9–30 percent higher without financial frictions. Consistent with Ziebarth's work, they find a larger drop in employment if the firm was located in a county where at least one national bank failed.

At the state level, [Mladjan, 2019] provides evidence that financially dependent manufacturing industries also exhibited steeper declines in output relative to peers. He shows that this differential is largest in states most affected by banking suspensions. His results show that bank suspensions could explain a third of the decline in manufacturing output during the Great Depression. He proxies for external access to credit by the fraction of capital expenditure that is not covered by cash flow from operations by industry.

3.2.4 New evidence on bank lending: the 1933 Survey of Credit Conditions

Responding to the allegations of credit rationing coming from the leaders of small and medium-sized manufacturing plants, the U.S. Commerce Department decided to conduct a survey of credit conditions in 1935. These leaders were alleging that banks were withholding loans and that it was hard to obtain credit for working capital purposes or long-term requirements. They claimed that the lack of adequate credit had delayed industrial recovery. The questionnaires, prepared by the U.S. Census Bureau, were sent to all manufacturers employing, on average, 30 – 190 wage earners as reported in 1933. Out of 16,500 firms surveyed, over 46 percent submitted returns. Of these, 6,158 were judged suitable for tabulation. Of the 6,158, 71 percent were classified as borrowers of capital. Of the 4,387 borrowers, 45 percent reported credit difficulty.

Figure C.1 displays the main results of the survey by industry. There are three triking features of the data. First, small manufacturers relied heavily on banks as a source of working capital: approximately 80 percent reported some reliance on bank lending to finance their operations. Second, many manufacturers also depended upon these banks to finance long-term investment, while relatively few tapped security markets to do so. Lastly, the manufacturers in need of assistance in funding their long-term needs who found no sources available constituted a significant proportion of credit-constrained firms.

Were the respondents bad credit risks? No, they were not. In summary, many small manufacturing establishments which reported credit difficulty appeared to be financially sound and creditworthy based on current and net-worth-to-debt ratios. For example, of the 1,964 firms reporting problems in borrowing, 23 percent had current ratios of 3.0 or over, and 42 percent had 2.0 or more, which were, at the time, regarded as safe credit risks. In addition, the survey found that 33 percent of the total number reporting credit difficulty had net worth to debt ratios of 3 or more, while in the group with net worth to debt ratios as high as 2.0 was 50 percent.[4]

3.3 Data

I discuss the details of the archival data sources used to construct my sample in this section. The unit of observation is the county between 1926 and 1937. Except for those from the 1930 U.S. Decennial Census and the 1927 Market Data Handbook, variables are reported at an annual (for banking) or biennial (for manufacturing) frequency. Sources were explicitly digitized for this project and were merged with publicly available datasets available on the Inter-university Consortium for Political and Social Research (ICPSR) website.

3.3.1 Banking

I use data on bank suspensions from the Federal Deposit Insurance Corporation (FDIC) and collect new data on county-level conditions of national banks from the Office of the Comptroller of Currency (OCC) annual report. The county-level panel compiled by the FDIC in 1937 (cite)

[4]See Tables 15 through 26 in the Survey report. The survey collected information about current liabilities, short-term notes, fixed assets, long-term obligations.

is available on ICPSR and has been extensively used in previous research on the Great Depression. The variables include the total number of banks and deposits suspended within the calendar year and deposits in banks in operation as of the last day of the year for 1920 – 1936, reported separately for national- and state-chartered banks. The data is available for all counties in the continental United States besides Wyoming.

However, the FDIC data does not contain any information about local banking conditions in the 1920s or 30s at the county level besides suspensions. Therefore, I digitize tables from the OCC annual report, which reports aggregated call report statistics (assets and liabilities) of national banks at the county level.[5] Regarding assets, the variables include total loans and discounts, the value of bonds and securities, total due from other banks, the value of real estate owned, and cash holdings. Regarding liabilities, the variables include total deposits, capital stock, circulation, rediscounts, and surplus and profits. These variables are reported during the last week of March or the first week of April for all years except for 1928, when the call date was February 28. I collected the data from 1924 to 1931, the OCC's last year of county-level reporting. Every county with at least one national bank active on the call date is included in the sample.[6]

3.3.2 Manufacturing output and spatial industry composition

Manufacturing revenue, employment of wage-earners, and the number of manufacturing establishments come from the Census of Manufactures. I digitized the 1937 publication containing 1929 – 1935 biennial observations [Bureau, 1937]. To the best of my knowledge, this source is new to the literature, though others have used plant-level or state-level variables from the Census before. I also digitized the special tabulation of the Census done by the Commerce Department in 1927,

[5]The OCC annual report is available on FRASER.

[6]I drop all observations where the banking variables are missing (negative), which removes 23 counties from sample.

as reported in the Market Data Handbook of the United States [Stewart, 1929].

The geographic coverage of the manufacturing sample is not nationwide. The Census does not disclose aggregated data if the reporting of such data would allow for firm-level identification. The Census covers all establishments (single plants or factories) reporting products produced of $5,000 or more. Thus, counties with minimal manufacturing activity are not included in the sample. Furthermore, the loss of manufacturing establishments during the Great Depression brought some counties under the reporting threshold for 1931, 1933, or 1935. Therefore, the coverage is not balanced in the full sample, though I will only use balanced samples in my analysis.[7]

Pre-Depression industry composition comes from a special tabulation of the 1927 Census contained in the Market Data Handbook of the United States, Table 8. This source gives the total number of establishments by manufacturing industry in each county. I aggregate industries to the 15 primary manufacturing industries in the Survey of Credit Conditions.[8] The main limitation of establishment count-level data is that the economic significance of establishments varies widely. For example, nationally, the average establishment in the textile industry employed 63 wage-earners, while an average establishment in chemicals employed about half as many - 33 - in 1927.

To go from county-level establishment count distributions to other, plausibly more informative ones, I use state-industry averages of wage earners/establishment, total wages/establishment, and output/establishment. Then, I transform establishment shares by multiplying the establishment shares, which results in estimated employees or revenue. The state-industry averages come from the same reports of the Census in 1927.[9] As my primary measure of classification, I then take the

[7]For more information about the Census of Manufactures and its coverage across years, see [Vickers and Ziebarth, 2019]

[8]The industries are food and kindred products, textiles, iron and steel, forest products, leather, rubber, paper and allied products, printing and publishing, chemicals, petroleum and coal, stone/clay/glass, nonferrous metals, machinery, transportation equipment, and miscellaneous.

[9]Of course, this method cannot account for within-state variation of industries, which may bias the results.

share of products produced by industry in each county. However, the results do not change if I use other weights.

Finally, as discussed in Section 3.2.4, I take the aggregated survey results from the 1935 Survey as industry-level measures of financial constraints arising from bank failures, as listed in Figure C.1, Panel B. I base my analysis on three pieces of information contained in Tables 2 and 26 of the Survey: (1) how many firms were borrowing, (2) how many firms found it difficult to borrow, and (3) how many borrowers found it difficult to borrow. Then, using estimated product shares, I compute the industry weighted average of credit difficulty at the county level. Figure C.5 displays the distribution of constraints in the sample of counties using each (1) – (3) and the four different weighing schemes described above.

3.3.3 Other Data

I use two pieces of spatial data. First, I use Geographic Information Systems (GIS) software to identify counties within 50 miles of all Federal Reserve Districts.I further classify these bordering counties into border segments – e.g., Atlanta – St. Louis segment – including counties on both sides of the border. I follow [Jalil, 2014] as closely as possible in identifying these segments. Figure C.2 plots the Atlanta Federal Reserve District border regions.

Second, I manually transcribe the consumer markets map from the Market Data Handbook. This map groups counties into mutually exclusive consumer markets as of 1927. According to the makers of the map, the 632 areas were determined from consumer buying, stemming from an effort by the International Magazine Company to simplify consumer selling by “determining the minimum number of points from which maximum results might be expected.” According to the source, the trading centers (cities) were selected after studying population, geographical characteristics, sources of wealth, transportation, and trade outlets. About each of these central points, boundaries were defined by a study of those factors which influence the trend of buying habits.

3.3.4 Summary Statistics

Table 26 reports the summary statistics. The total number of observations for the banking variables includes 365 border counties for nine years and 364 counties for one year (1934, missing bank data for one county). The 1204-1230 observations for manufacturing include between 190 - 210 border counties reporting manufacturing activity for six years (1927 – 1937, biennially). Manufacturing industry data as of 1927 is for 212 border counties. The condition of national banks from aggregated call reports is available for between 168 and 184 border counties.

3.4 Policy regimes and banking outcomes across Federal Reserve borders

I begin my analysis by investigating whether banks failed at a lower rate in counties in the Atlanta District than in similar counties located just outside it. Corroborating the main findings of [Jalil, 2014], I find that the answer is: yes. This result is especially strong when the policies between the District and the other Federal Reserve banks differed in 1929 and 1930. I then explore the robustness of this finding.

3.4.1 Empirical Design

The outcome of interest is bank failure. I measure bank distress at the county level using suspensions and deposits in active banks at the end of the year as a share of pre-Depression (1927) banks and total deposits, respectively. Both measures are essential as banks can reopen after a temporary suspension with a limited impact on lending behavior. The suspensions data, however, does not differentiate between liquidations and suspensions. The total value of deposits at the end of the year thus serves as additional evidence that reflects more permanent changes in bank liabilities. To further control for the effect of outliers, I create a binary variable that takes the value of 1 if any bank was suspended within the year and 0 otherwise. I further define each variable separately for national and state banks.

I introduce various control variables in my analysis. I compute two measures to account for unobserved time-varying confounders due to fundamental banking differences between counties. First, I define the pre-Depression (1927) "capitalization ratio" as the total surplus and capital divided by total assets. Higher capitalization ratios reflect lower leverage of the banking sector and a higher probability of withstanding depositor withdrawals. Second, I compute log loan growth between 1924 and 1929. Higher loan growth could potentially correlate with decreased loan quality and a higher default rate in the 1930s. Finally, to control for non-financing industry-level time-varying confounders, I use the 1927 revenue shares and find the dominant industry in each county. I interact each with time dummies to capture dynamic effects.

Are there underlying differences between counties that could potentially explain differences in bank failure rates? I use several variables from the 1930 Decennial Census to check for significant differences among counties across the border. I define the unemployment rate in 1930 as total unemployed over total population, "crop failure" as the proportion of land crops failed divided by total cropland in the county, and "labor force participation" as gainfully employed workers divided by total county population.

Table 28 shows that counties on the border of the District were similar. There are some differences, but they are small. For example, although fewer banks were in the average county inside the District, the total amount of deposits in 1928 was the same. There were slightly fewer manufacturing establishments on average, and the farms were smaller. Notably, the counties did not differ in their suspension rates as of 1927 and had the same (estimated) proportion of manufacturing firms facing financial constraints.

I compare county-level outcome variables before and after the onset of the Great Depression across the District boundary using a dynamic difference-in-differences design. The specification is:

$$S_{jk} = \alpha_j + \beta_k + \sum_{i=1926}^{1933} (Atl_j T_i) \cdot \gamma_i + X_{jk} + \epsilon_{jk} \tag{3.1}$$

where T_i is a year dummy taking the value of 1 if $i = k$ and 0 otherwise and Atl_j takes the value of 1 if the county belongs to the Atlanta District and 0 otherwise. I use county (α_j) and year (β_k) fixed effects to account for all unobserved but static county variables and national trends in bank failure rates. The control variables in X_{jk} include border-region by year fixed effects and, at various stages, proxies for baseline banking and manufacturing. The omitted interaction is 1927. The coefficients of interest are γ_i, which capture the time-varying difference in outcome S in counties inside the District compared to average outcomes within border regions. I cluster the standard errors at the county level.

3.4.2 Results

Table 27 presents the descriptive results with no other control variables for the four outcome variables. Panel A gives the estimates for suspension rates, and Panel B presents them for active rates.

In both panels, we do not observe pre-trends on observables: the estimates on pre-1929 interaction terms are not statistically different from zero. The estimate in (1) shows that, relative to their 1927 levels, banks in District counties failed at rates 6 and 5 percent lower in 1929 and 1930, respectively. At the mean number of banks, this translates to 0.24 (0.06 x 4) and 0.2 (0.05 * 4) fewer suspended banks in each year, or approximately 0.45 more banks remaining on average after 1930 in the Atlanta District. After 1931, the coefficients are not significantly different than zero. These years are also when more banks closed to due solvency issues and when there was a convergence of policy between Atlanta and its neighboring districts. Columns (2) and (3) show that the effect on the suspension rate is similar for both state and national banks. Finally, columns (4) – (6) show that a county in the District was 14 percent less likely to experience any bank failure in 1929, but the effect does not extend to 1930.

In Panel B, I show that the qualitative evidence is very similar when considering the number and deposits of active banks at the end of each year. On average, the estimates reveal that counties in the Atlanta region contained 10 percent more banks by the end of 1930 and 7 percent more deposits, which are qualitatively similar to the results using suspensions as the outcome variable. Moreover, these effects are pronounced for national banks, where the effect is present even at the end of 1931.

3.4.3 Robustness

Despite generally balanced counties on either side of the border, there are concerns about interpreting these results causally. First, the differences that appear in the covariate balance table could drive differences in later outcomes once the Depression starts. Second, omitted underlying differences in bank conditions – such as bank leverage or historical loan growth – could be causing the differences in bank failures. Third, the industrial structure at the county level could also explain why some places were susceptible to bank failures more so than others.

Moreover, using the Mississippi sample, as used by [Richardson and Troost, 2009] and [Ziebarth, 2013], should reveal significant estimates if this method is valid. More generally, the effect should be present when the District boundary bisects a consumer trade area - as defined by the Market Data Handbook - and not just one state like Mississippi. The effects should not be sensitive to the choice of 50 miles. Finally, the results should be present even after dropping, sequentially, border regions - the District policies varied not with one but all four of its neighbors.

Tables 29 and 30 show that the main findings on bank suspensions and active deposits are robust to all the concerns raised in the previous paragraphs. In column (1), I replicate the baseline result. The remaining columns (2) through (11) address the concerns in the same order as they were stated above. For deposits, the estimates are noisier but are qualitatively similar.

As a further test of the parallel trends assumption underlying the main difference-in-differences estimates, I also conduct a series of placebo tests for the effect of the Federal Reserve regime and banking panics. Each placebo estimate is generated by randomly assigning a false border to each of the border counties within each border region. Using those false borders, I then replicate the generalized difference-in-differences estimates for bank suspensions. Figure C.3 plots the distribution of estimated coefficients after 1000 random assignments of border counties separately for the 1929 x in-ATL and 1930 x in-ATL interaction terms. The true estimate is also shown in the figure as the red vertical line. The true estimate is taken from column (3) of Table 27. As is clear from the figure, the true estimates lie in the tail of the distribution (98th percentile) of the placebo estimates, and the distribution of placebo estimates for both years is centered around zero. This suggest that the results I find are unlikely to have been generated by pure chance.

Finally, instead of permuting counties into placebo borders, I extend the analysis to actual border counties in regions that did not differ in their policy regimes. If the differences in Federal Reserve policies are driving these outcomes – and the robustness exercises have convincingly pointed to a causal interpretation – then it must also be true that the *absence* of these differences should result in *little or no change* in bank failures. In districts that did not differ in their policies from their neighbors, what is the prevalence of significant differences in bank failures in their border counties? I re-run the regression for different regions which did not border the District, dropping SF and MIN because of missing bank data from Wyoming, which leaves Boston, KC, NY, Chicago, and Philadelphia. Figure C.4 shows the distribution of the interaction terms of these six regressions. Of the 30 interaction terms before 1934 (six regions x five years), only six are significantly different from zero at the 90 percent level, and two of them come from the Atlanta regression.

3.4.4 What happened to bank lending?

While the results in the previous subsection provide evidence that the incidence of bank failure differed significantly based on the federal reserve regime, they say nothing with respect to how the remaining banks responded. Banks may respond to local banking panics by refusing to lend and, instead, amassing safe assets like government bonds. Using the OCC data on national banks for years up to 1931, I next investigate the composition of assets and liabilities.

Table 31 presents the estimates of the difference-in-differences specification using the available OCC data between 1926 and 1931. The result in column (1) shows that national banks had, on average, 11 percent more outstanding loans as of 1931 inside the District than outside it. They did not, as column (2) shows, own more bonds, and they did report more surplus and profits as reported in column (4).

3.5 Did banking suspensions lead to worse local economic outcomes?

I have shown so far that the commercial banking sector inside the District fared relatively better during the first two years of the Depression than it did just outside it. If the hypothesis that bank suspensions lead to more costs of credit intermediation and if bank lending is an essential input to production, then, *ceteris paribus*, we should see less economic activity outside the district than inside it.

The empirical strategy is unchanged from the one described in the previous section: using a dynamic difference-in-differences design, I am comparing manufacturing outcomes between counties

50 miles within and outside the District border, before and after the onset of the Great Depression. I add, however, an additional explanatory variable: the average estimated credit difficulty based on 1927 industry count data, which I code as a binary variable taking the value of 1 if a county is above the median and zero otherwise. In all specifications, the reference year is 1927, and standard errors are clustered at the county level.

Table 32 shows the estimated coefficients of the difference-in-differences specification using manufacturing outcomes. Columns (1) - (5) use log output (revenue) as the outcome variable. In column (1), I do not control for any other covariates while the remaining columns of the table add a series of control variables that increasingly restrict the nature of the comparison that is being used to identify the effect of the Federal Reserve policy on manufacturing output. In column (2), I discard outlier counties in the bottom two or top 2 percentile in the change in manufacturing revenue between 1929 and 1931. In columns (3), I control for pre-period banking by interacting the capitalization ratio and log loan growth between 1924 and 1929 with year fixed effects. In column (4), I add the estimated credit difficulty difference-in-differences effect to the specification. Finally, in column (5), I conduct a pooled difference-in-difference-in-differences analysis, comparing geographically across the District border, below and above median estimated credit difficulty, and across years where the variable post takes the value of 1 for all years after 1929 and 0 otherwise. The remaining columns take specifications (4) and (5) and apply it to the other manufacturing outcome variables: wage-earner payroll, number of establishments, and number of wage-earners.

Unlike the banking suspension and lending results shown so far, I do not find evidence that local manufacturing fared better inside the District. On the contrary, the results show that local economic outcomes were worse across all the outcome variables. Consider the estimates in column (1): The results in column (4), my preferred specification, show a 3 to 10 percent decrease in annual revenue for the manufacturing sector in the Atlanta counties, though noisily estimated.

On the other hand, I do find that credit difficulty estimates correlated negatively and significantly with manufacturing output. Columns (4), (6), (8), and (10) report the results for revenue, wages, number of establishments, and number of wage earners. The estimated effects are all highly significant and relatively stable across specifications, implying that counties with estimated above median credit difficulties had outcomes 20 to 30 percent lower than those without difficulty.

However, I do not find that the effect of financial constraints was magnified in counties that also experienced a banking panic. That is, the coefficient estimate on triple interaction term in the last row implies that manufacturing activity was not different across the border in counties that, ex-ante, were more likely to suffer from financial rationing, as was the hypothesis.

Figure C.6 plots the difference-in-differences estimates for the three possible definitions of credit constraints. The figure shows that having an industry composition that typically borrows (green) cannot explain the difference in outcomes – what matters is that different industries were denied credit at different rates, irrespective of how pervasive borrowing was within industry.

3.6 Summary - Chapter 3

This paper used novel, archival, panel data on local manufacturing and banking conditions in the United States to investigate the link between policy, bank failures, and, ultimately, firm production and employment. Like researchers before me, I used the divergent policies enacted by the Atlanta Federal Reserve Bank as my empirical laboratory. I found that credit conditions appeared more favorable in the early years of the Depression inside counties of the Atlanta district. The robustness of this result, as well as a host of placebo checks, points to a causal interpretation of how lender of last resort policies from the Federal Reserve stymied banking panics.

I then combined industry-level credit survey and 1927 industry-by-county data to construct measures of financial constraints for each county. Using manufacturing panel data, I do not find any evidence to support the hypothesis that banking panics translated to more local economic distress to counties just outside Atlanta, which contrasts with the results of the existing literature. Manufacturing outcomes were worse, not better, in counties inside the Atlanta region, despite having more banking resources. I do find strong evidence that the county-level financing constraints predict worse outcomes after, but not before, the Depression. However, the interaction between pre-Depression measures of financial constraints and banking panics during the Depression is, surprisingly, not an important determinant of local economic outcomes.

REFERENCES

[Abramitzky et al., 2020] Abramitzky, R., Boustan, L., and Rashid, M. (2020). Census linking project: Version 1.0 [dataset].

[Acemoglu and Angrist, 2000] Acemoglu, D. and Angrist, J. (2000). How large are human-capital externalities? evidence from compulsory schooling laws. *NBER Macroeconomics Annual*, 15:9–59.

[Adelino et al., 2017] Adelino, M., Cunha, I., and Ferreira, M. A. (2017). The economic effects of public financing: Evidence from municipal bond ratings recalibration. *The Review of Financial Studies*, 30(9):3223–3268.

[Almeida et al., 2009] Almeida, H., Campello, M., Laranjeira, B., and Weisbenner, S. (2009). Corporate debt maturity and the real effects of the 2007 credit crisis. Technical report, National Bureau of Economic Research.

[Atkin, 2016] Atkin, D. (2016). Endogenous skill acquisition and export manufacturing in mexico. *American Economic Review*, 106(8):2046–85.

[Baker et al., 2020] Baker, R. B., Blanchette, J., and Eriksson, K. (2020). Long-run impacts of agricultural shocks on educational attainment: Evidence from the boll weevil. *The Journal of Economic History*, 80(1):136–174.

[Baran et al., 2020] Baran, C., Chyn, E., and Stuart, B. A. (2020). The great migration and educational opportunity. Technical report, Working Paper.

[Barseghyan and Coate, 2016] Barseghyan, L. and Coate, S. (2016). Property taxation, zoning, and efficiency in a dynamic tiebout model. *American Economic Journal: Economic Policy*, 8(3):1–38.

[Bau et al., 2020] Bau, N., Rotemberg, M., Shah, M., and Steinberg, B. (2020). Human capital investment in the presence of child labor. Technical report, National Bureau of Economic Research.

[Beito, 1989] Beito, D. T. (1989). Taxpayers in revolt: Tax resistance during the depression.

[Benmelech et al., 2019] Benmelech, E., Frydman, C., and Papanikolaou, D. (2019). Financial frictions and employment during the great depression. *Journal of Financial Economics*, 133(3):541–563.

[Bernanke et al., 1996] Bernanke, B., Gertler, M., and Gilchrist, S. (1996). The financial accelerator and the flight to quality. *The Review of Economics and Statistics*, pages 1–15.

[Bernanke, 1983] Bernanke, B. S. (1983). Non-monetary effects of the financial crisis in the propagation of the great depression. Technical report, National Bureau of Economic Research.

[Betts and McFarland, 1995] Betts, J. R. and McFarland, L. L. (1995). Safe port in a storm: The impact of labor market conditions on community college enrollments. *Journal of Human Resources*, pages 741–765.

[Black et al., 2005] Black, D., McKinnish, T., and Sanders, S. (2005). The economic impact of the coal boom and bust. *The Economic Journal*, 115(503):449–476.

[Blanchard and Olney, 2017] Blanchard, E. J. and Olney, W. W. (2017). Globalization and human capital investment: Export composition drives educational attainment. *Journal of International Economics*, 106:165–183.

[Brown, 1922] Brown, F. (1922). *Municipal Bonds: A Statement of the Principles of Law and Custom Governing the Issue of American Municipal Bonds*. Prentice-Hall, Incorporated.

[Bureau, 1935] Bureau, U. C. (1935). Survey of reports of credit and capital difficulties submitted by small manufacturers.

[Bureau, 1937] Bureau, U. C. (1937). Biennial census of manufactures. alabama to wyoming. *Washington: U.S. Government Print Office*.

[Calomiris and Mason, 2003] Calomiris, C. W. and Mason, J. R. (2003). Fundamentals, panics, and bank distress during the depression. *American Economic Review*, 93(5):1615–1647.

[Card et al., 2018] Card, D., Domnisoru, C., and Taylor, L. (2018). The intergenerational transmission of human capital: Evidence from the golden age of upward mobility. Technical report, National Bureau of Economic Research.

[Card and Krueger, 1992] Card, D. and Krueger, A. B. (1992). Does school quality matter? returns to education and the characteristics of public schools in the united states. *Journal of Political Economy*, 100(1):1–40.

[Cascio and Narayan, 2015] Cascio, E. U. and Narayan, A. (2015). Who needs a fracking education? the educational response to low-skill-biased technological change. *ILR Review*.

[Chamberlain, 1928] Chamberlain, L. (1928). *The Principles of Bond Investment*.

[Charles et al., 2015] Charles, K. K., Hurst, E., and Notowidigdo, M. J. (2015). Housing booms and busts, labor market opportunities, and college attendance. Technical report, National Bureau of Economic Research.

[Chodorow-Reich, 2014] Chodorow-Reich, G. (2014). The employment effects of credit market disruptions: Firm-level evidence from the 2008–9 financial crisis. *The Quarterly Journal of Economics*, 129(1):1–59.

[Cromwell et al., 2015] Cromwell, E., Ihlanfeldt, K., et al. (2015). Local government responses to exogenous shocks in revenue sources: Evidence from florida. *National Tax Journal*, 68(2):339–376.

[Cutler and Miller, 2005] Cutler, D. and Miller, G. (2005). Water, water, everywhere: municipal finance and water supply in american cities. Technical report, National Bureau of Economic Research.

[Darby, 1975] Darby, M. R. (1975). Three-and-a-half million us employees have been mislaid: Or, an explanation of unemployment, 1934-1941. Technical report, National Bureau of Economic Research.

[Doerner and Ihlanfeldt, 2011] Doerner, W. M. and Ihlanfeldt, K. R. (2011). House prices and city revenues. *Regional Science and Urban Economics*, 41(4):332–342.

[Elder, 2018] Elder, G. H. (2018). *Children of the Great Depression: Social change in life experience*. Routledge.

[Faust, 1934] Faust, M. L. (1934). Public funds in closed banks. *Municipal Finance*, 6(3):17–19.

[Fazzari et al., 1988] Fazzari, S., Hubbard, R. G., and Petersen, B. (1988). Investment, financing decisions, and tax policy. *American Economic Review*, 78(2):200–205.

[Federal Reserve Bank of Minneapolis, 2020] Federal Reserve Bank of Minneapolis (2020). Consumer price index, 1913 -. `https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator/consumer-price-index-1913-`. Accessed: 2020-06-01.

[Ferrie and Troesken, 2008] Ferrie, J. P. and Troesken, W. (2008). Water and chicago's mortality transition, 1850–1925. *Explorations in Economic History*, 45(1):1–16.

[Fishback et al., 2005] Fishback, P. V., Horrace, W. C., and Kantor, S. (2005). Did new deal grant programs stimulate local economies? a study of federal grants and retail sales during the great depression. *The Journal of Economic History*, 65(1):36–71.

[Fishback et al., 2006] Fishback, P. V., Horrace, W. C., and Kantor, S. (2006). The impact of new deal expenditures on mobility during the great depression. *Explorations in Economic History*, 43(2):179–222.

[Fishback et al., 2010] Fishback, P. V., Johnson, R. S., and Kantor, S. (2010). Striking at the roots of crime: The impact of welfare spending on crime during the great depression. *The Journal of Law and Economics*, 53(4):715–740.

[Fishback et al., 2003] Fishback, P. V., Kantor, S., and Wallis, J. J. (2003). Can the new deals three rs be rehabilitated? a program-by-program, county-by-county analysis. *Explorations in Economic History*, 40(3):278–307.

[Fishback and Wallis, 2012] Fishback, P. V. and Wallis, J. J. (2012). What was new about the new deal? Technical report, National Bureau of Economic Research.

[Fons et al., 2011] Fons, J., Randazzo, T., and Joffe, M. (2011). An analysis of historical municipal bond defaults. *Kroll BondRatings*.

[Friedman and Schwartz, 2008] Friedman, M. and Schwartz, A. J. (2008). *A monetary history of the United States, 1867-1960*, volume 14. Princeton University Press.

[Gamble, 1989] Gamble, Q. R. H. (1989). A history of the federal reserve bank of atlanta.

[Gertler and Gilchrist, 1994] Gertler, M. and Gilchrist, S. (1994). Monetary policy, business cycles, and the behavior of small manufacturing firms. *The Quarterly Journal of Economics*, 109(2):309–340.

[Goldin and Katz, 1997] Goldin, C. and Katz, L. F. (1997). Why the united states led in education: Lessons from secondary school expansion, 1910 to 1940. Technical report, National Bureau of Economic Research.

[Gunter and Siodla, 2018] Gunter, S. and Siodla, J. (2018). Local origins and implications of the 1930s urban debt crisis.

[Hanushek, 1986] Hanushek, E. A. (1986). The economics of schooling: Production and efficiency in public schools. *Journal of Economic Literature*, 24(3):1141–1177.

[Hillhouse, 1936] Hillhouse, A. M. (1936). *Municipal bonds: A century of experience*. Prentice-Hall, Incorporated.

[Jackson et al., 2016] Jackson, C. K., Johnson, R. C., and Persico, C. (2016). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1):157–218.

[Jalil, 2014] Jalil, A. J. (2014). Monetary intervention really did mitigate banking panics during the great depression: Evidence along the atlanta federal reserve district border. *The Journal of Economic History*, 74(1):259–273.

[Janas, 2021a] Janas, P. (2021a). Public goods under financial distress: evidence from cities in the great depression.

[Janas, 2021b] Janas, P. (2021b). Recessions, constraints, and public education: impact of the great depression on the high school movement.

[Jehiel and Lamy, 2018] Jehiel, P. and Lamy, L. (2018). A mechanism design approach to the tiebout hypothesis. *Journal of Political Economy*, 126(2):735–760.

[Kaplan and Zingales, 1997] Kaplan, S. N. and Zingales, L. (1997). Do investment-cash flow sensitivities provide useful measures of financing constraints? *The Quarterly Journal of Economics*, 112(1):169–215.

[Kline and Moretti, 2014] Kline, P. and Moretti, E. (2014). Local economic development, agglomeration economies, and the big push: 100 years of evidence from the tennessee valley authority. *The Quarterly Journal of Economics*, 129(1):275–331.

[Lee et al., 2015] Lee, J., Mezzanotti, F., et al. (2015). Bank distress and manufacturing: Evidence from the great depression. Technical report, Working paper, Harvard University.

[Lutz et al., 2011] Lutz, B., Molloy, R., and Shan, H. (2011). The housing crisis and state and local government tax revenue: Five channels. *Regional Science and Urban Economics*, 41(4):306–319.

[Margo, 1991] Margo, R. A. (1991). The microeconomics of depression unemployment. *The Journal of Economic History*, 51(2):333–341.

[Mian et al., 2013] Mian, A., Rao, K., and Sufi, A. (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics*, 128(4):1687–1726.

[Mladjan, 2019] Mladjan, M. (2019). Accelerating into the abyss: Financial dependence and the great depression. *Available at SSRN 2366291*.

[Richardson, 2007] Richardson, G. (2007). Categories and causes of bank distress during the great depression, 1929–1933: The illiquidity versus insolvency debate revisited. *Explorations in Economic History*, 44(4):588–607.

[Richardson and Troost, 2009] Richardson, G. and Troost, W. (2009). Monetary intervention mitigated banking panics during the great depression: quasi-experimental evidence from a federal reserve district border, 1929–1933. *Journal of Political Economy*, 117(6):1031–1073.

[Rosenbloom and Sundstrom, 1999] Rosenbloom, J. L. and Sundstrom, W. A. (1999). The sources of regional variation in the severity of the great depression: evidence from us manufacturing, 1919–1937. *The Journal of Economic History*, 59(3):714–747.

[Ruggles et al., 2020] Ruggles, S., Sarah, F., Goeken, R., Grover, J., Meyer, E., Pacas, J., and Sobek, M. (2020). Ipums usa: Version 10.0 [dataset], minneapolis, mn. `https://doi.org/10.18128/D010.V10.0`. Accessed: 2020-06-01.

[Schmick and Shertzer, 2019] Schmick, E. J. and Shertzer, A. (2019). The impact of early investments in urban school systems in the united states. Technical report, National Bureau of Economic Research.

[Shah and Steinberg, 2017] Shah, M. and Steinberg, B. M. (2017). Drought of opportunities: Contemporaneous and long-term impacts of rainfall shocks on human capital. *Journal of Political Economy*, 125(2):527–561.

[Siodla, 2020] Siodla, J. (2020). Debt and taxes: Fiscal strain and us city budgets during the great depression. *Explorations in Economic History*, page 101328.

[Stegarescu, 2005] Stegarescu, D. (2005). Public sector decentralisation: Measurement concepts and recent international trends. *Fiscal Studies*, 26(3):301–333.

[Stewart, 1929] Stewart, P. W. (1929). *Market Data Handbook of the United States*. Washington: U. S. Govt. Print. Offs.

[Stiglitz and Weiss, 1981] Stiglitz, J. E. and Weiss, A. (1981). Credit rationing in markets with imperfect information. *American Economic Review*, 71(3):393–410.

[Sundstrom, 1992] Sundstrom, W. A. (1992). Last hired, first fired? unemployment and urban black workers during the great depression. *The Journal of Economic History*, 52(2):415–429.

[Temin, 1976] Temin, P. (1976). Lessons for the present from the great depression. *The American Economic Review*, 66(2):40–45.

[Tiebout, 1956] Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of political economy*, 64(5):416–424.

[United States Department of Labor, 1933] United States Department of Labor, C. B. (1933). Child labor: facts and figures. Publication No. 197.

[Upson, 1935] Upson, L. D. (1935). Local government finance in the depression. *National Municipal Review*, 24:503.

[Vickers and Ziebarth, 2019] Vickers, C. and Ziebarth, N. (2019). The census of manufactures: An overview. In Diebolt, C. and Haupert, M., editors, *Handbook of Cliometrics*.

[Wallis, 1984] Wallis, J. J. (1984). The birth of the old federalism: financing the new deal, 1932–1940. *The Journal of Economic History*, 44(1):139–159.

[Wallis, 1987] Wallis, J. J. (1987). Employment, politics, and economic recovery during the great depression. *The Review of Economics and Statistics*, pages 516–520.

[Wallis, 1989] Wallis, J. J. (1989). Employment in the great depression: New data and hypotheses. *Explorations in Economic History*, 26(1):45–72.

[Wheelock, 1997] Wheelock, D. C. (1997). The banking panics of the great depression. *The Journal of Economic History*, 57(4):977–978.

[White, 1984] White, E. N. (1984). A reinterpretation of the banking crisis of 1930. *The Journal of Economic History*, 44(1):119–138.

[Wicker, 2000] Wicker, E. (2000). *The banking panics of the Great Depression*. Cambridge University Press.

[Wood et al., 1937] Wood, K., Klueter, D., and Palmer, G. L. (1937). Urban workers on relief. *Washington: U.S. Government Print Office*.

[Yi, 2020] Yi, H. L. (2020). Finance, public goods, and migration. Technical report, Working paper, Rice University.

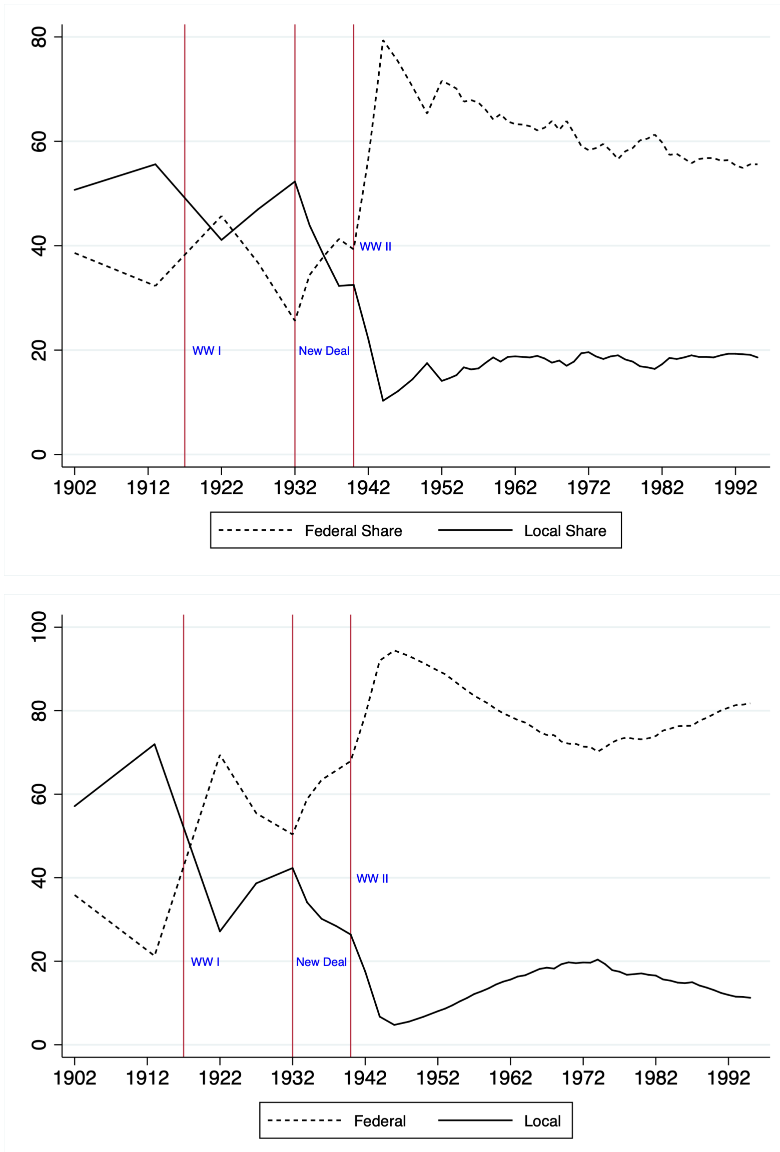
[Ziebarth, 2013] Ziebarth, N. L. (2013). Identifying the effects of bank failures from a natural experiment in mississippi during the great depression. *American Economic Journal: Macroeconomics*, 5(1):81–101.

CHAPTER 4

APPENDIX

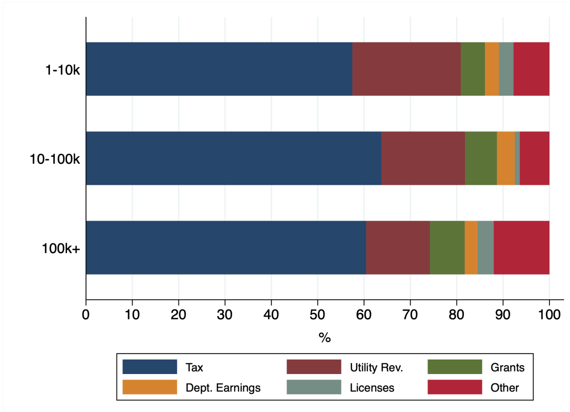
4.1 Appendix Chapter 1 - Figures

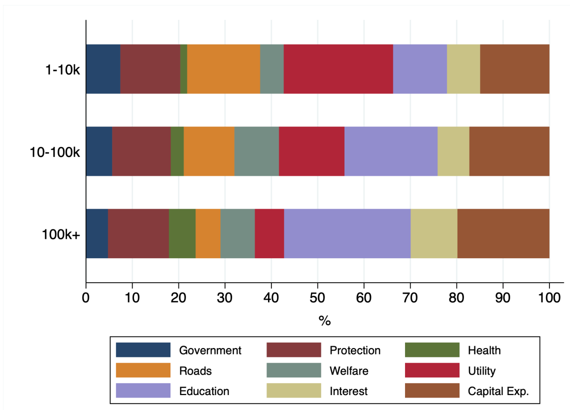
Figure 4.1: Revenue [Top] and Debt [Bottom], % of Total



Notes: 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. Three vertical lines denote the start of World War I, the New Deal, and the start of World War II.

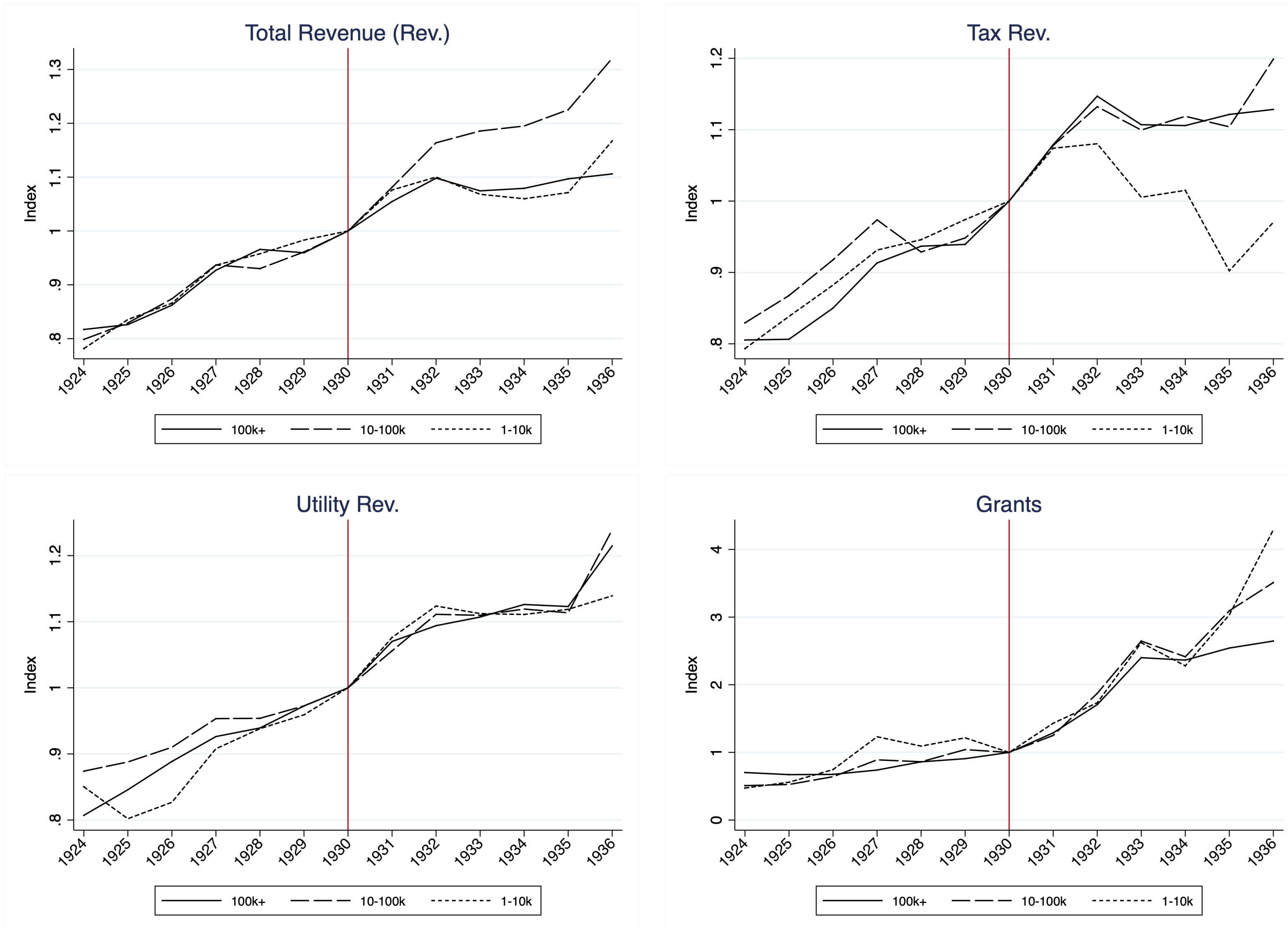
Figure 4.2: Composition of City Revenue and Expenditure, 1930





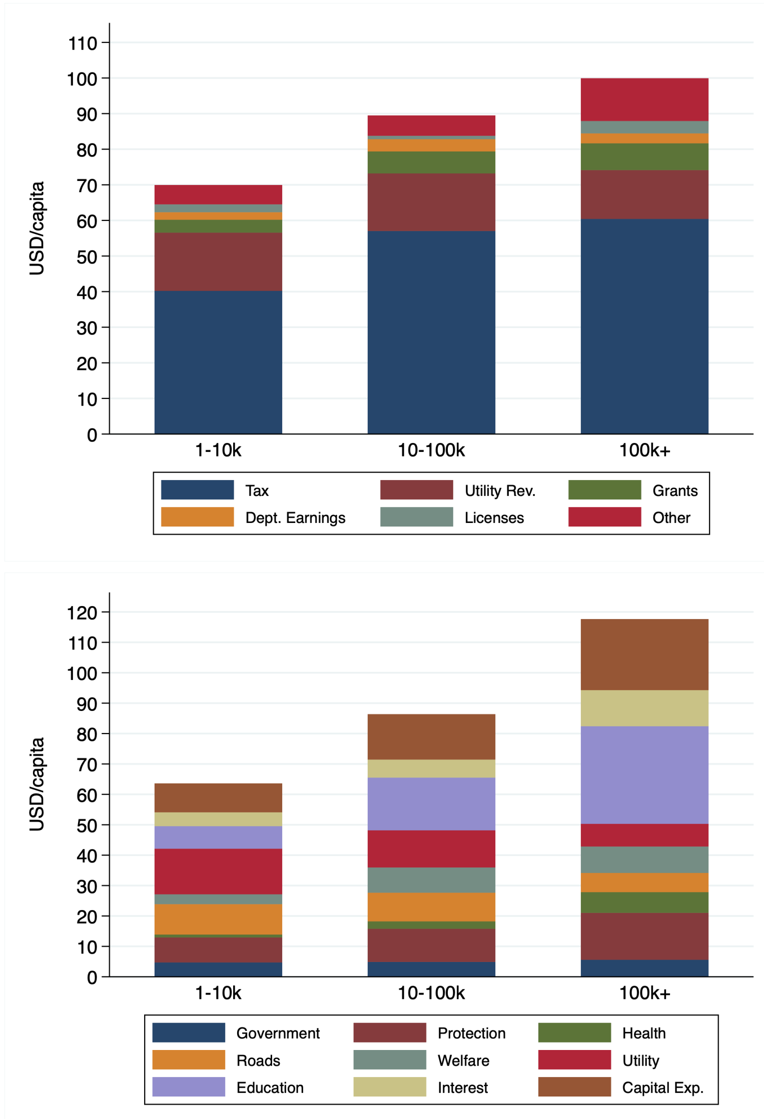
Notes: The figures show the average composition of city revenue and expenditure in 1930 by population category. Tax includes property tax, local personal income tax, local corporate income tax, and excise taxes. Utility revenue is income from publicly-owned water, gas, and electric utilities. Department 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 4.3: Average Revenue (1930 = 1.0), by City Size



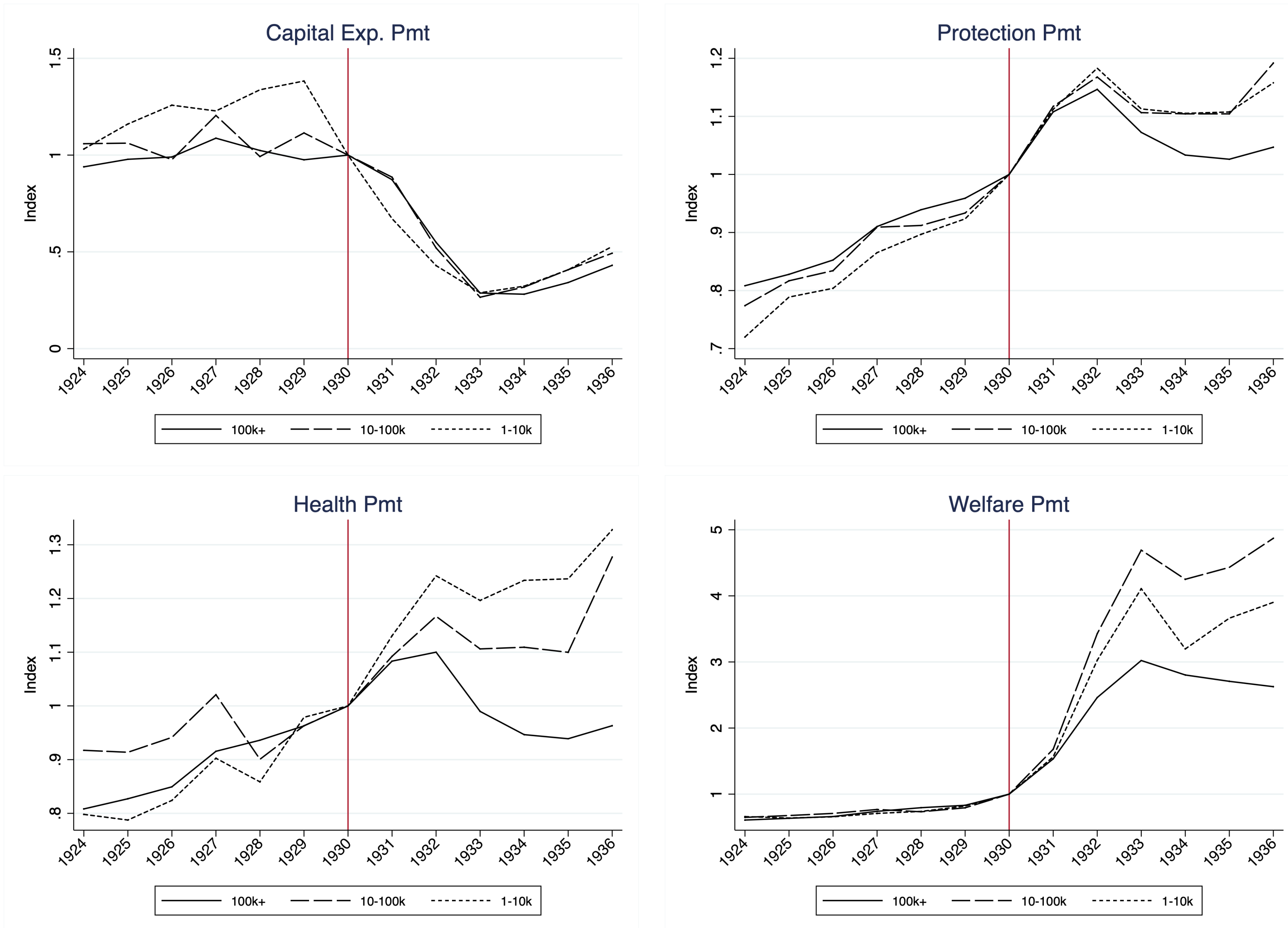
Notes: This figure plots the average per-capita revenues across cities within population categories (1930 = 1). The population category is static for each city and was assigned using the population obtained from the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities 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 4.4: Composition of City Revenue and Expenditure, 1930



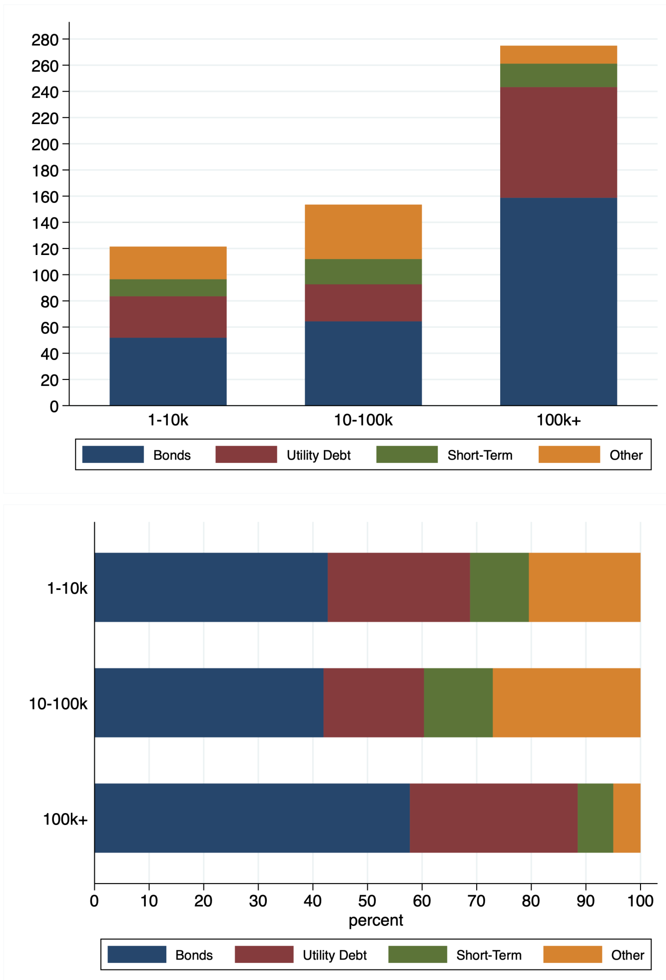
Notes: The figures show the average per-capita level (in 1930 dollars) of city revenue and expenditure in 1930 by population category. Tax includes property tax, local personal income tax, local corporate income tax, and excise taxes. Utility revenue is income from publicly-owned water, gas, and electric 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 4.5: Average Expenditure (1930 = 1.0), by City Size



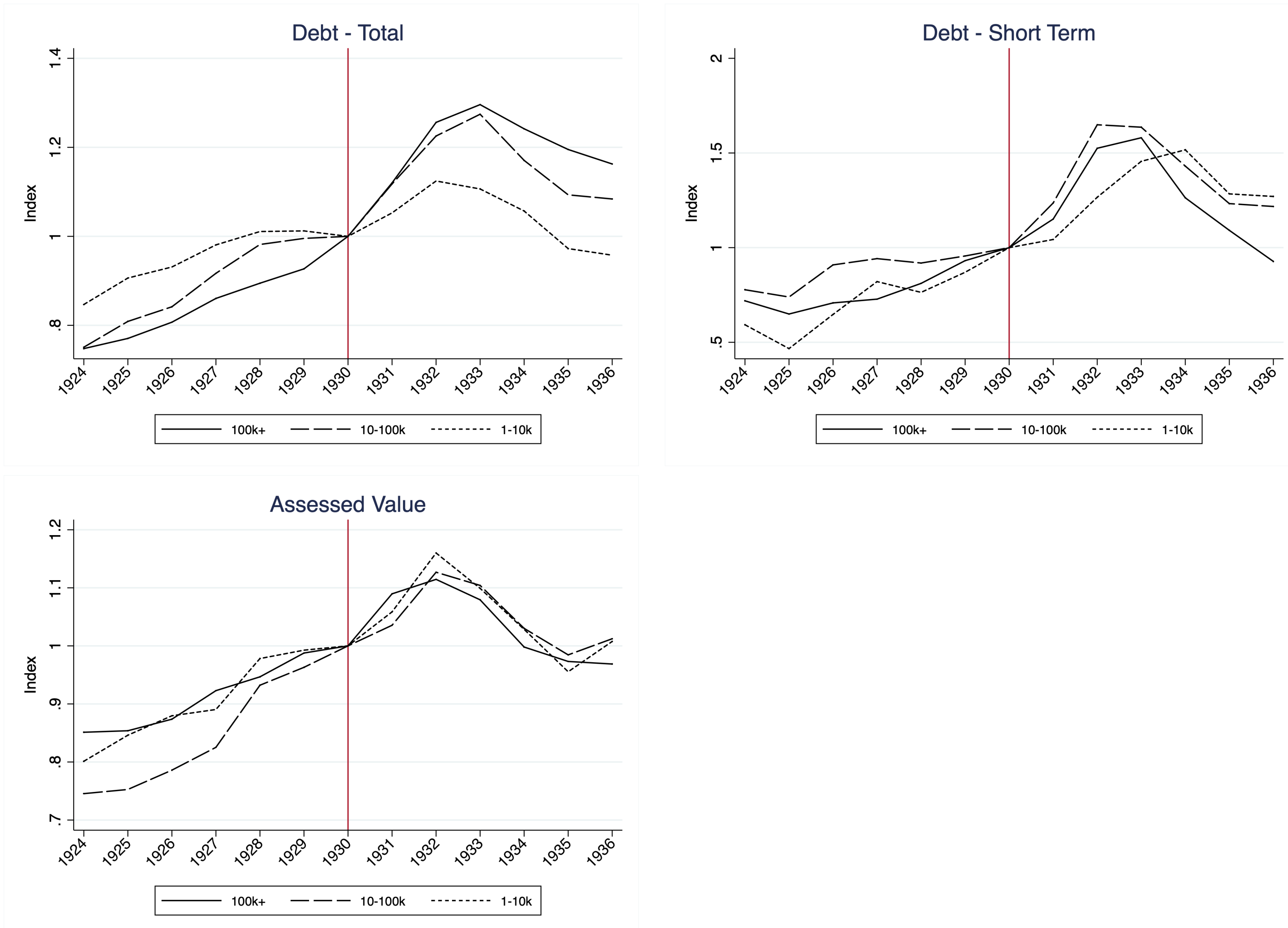
Notes: This figure plots the average per-capita expenditures across all cities within population categories (1930 = 1). Population category is static for each city and was assigned using the population in the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities in from 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 4.6: Breakdown of Balance Sheet Debt



Notes: 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 4.7: Evolution of Per Capita City Debt (real), by Category and Population

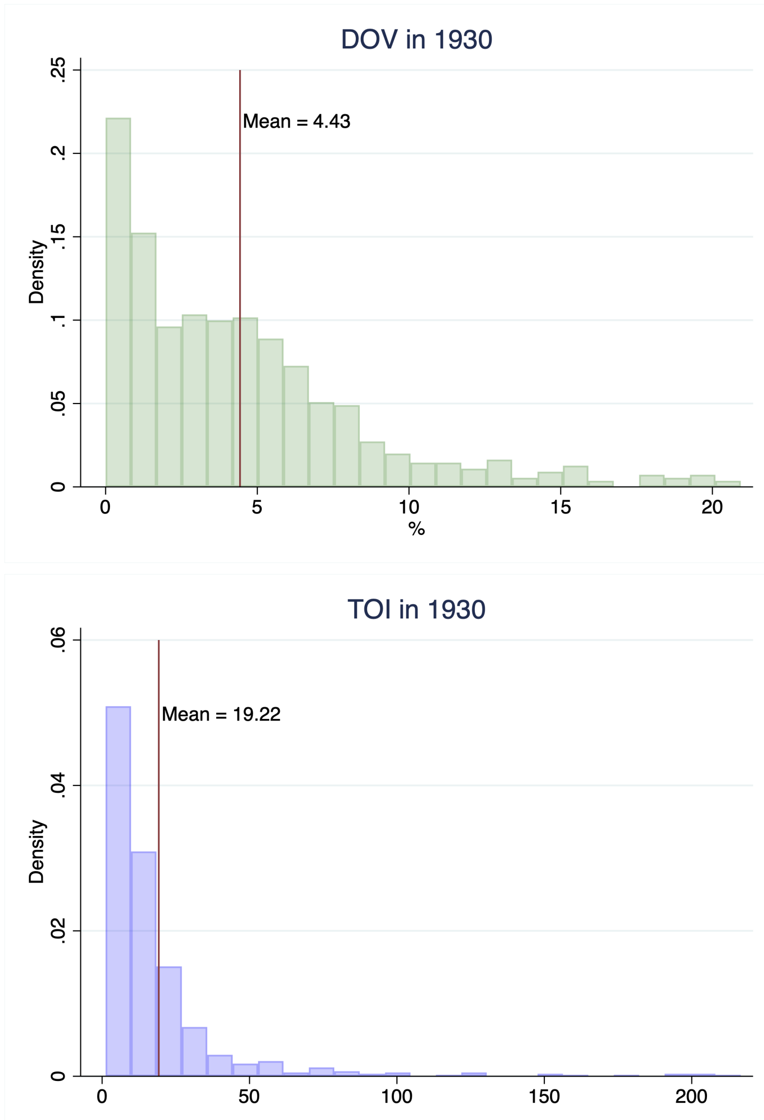


Notes: This figure plots the mean per-capita debt across all cities within population categories (1929 = 1). Population category is static for each city and was assigned using the population in the 1930 Census. The sample is slightly unbalanced and consists of approximately 345 cities from 1,000 to 10,000, 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 4.8: Incurred and Canceled Debt in MA Cities

Notes: 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 4.9: Distribution of Tax-over-Interest and Debt-over-Value (pre log)



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

4.2 Appendix Chapter 1 - 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.

4.3 Appendix Chapter 1 - 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:

$$weight_{i,t} = \begin{cases} \frac{Y_i(N) - year_t}{Y_i(N) - Y_i(0)} & \text{if type = serial} \\ 1 & \text{if type = term} \end{cases} \tag{4.1}$$

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.

$$Moody_{j,t} = \sum_{\forall i \in j} weight_{i,t} \times face_i \tag{4.2}$$

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.

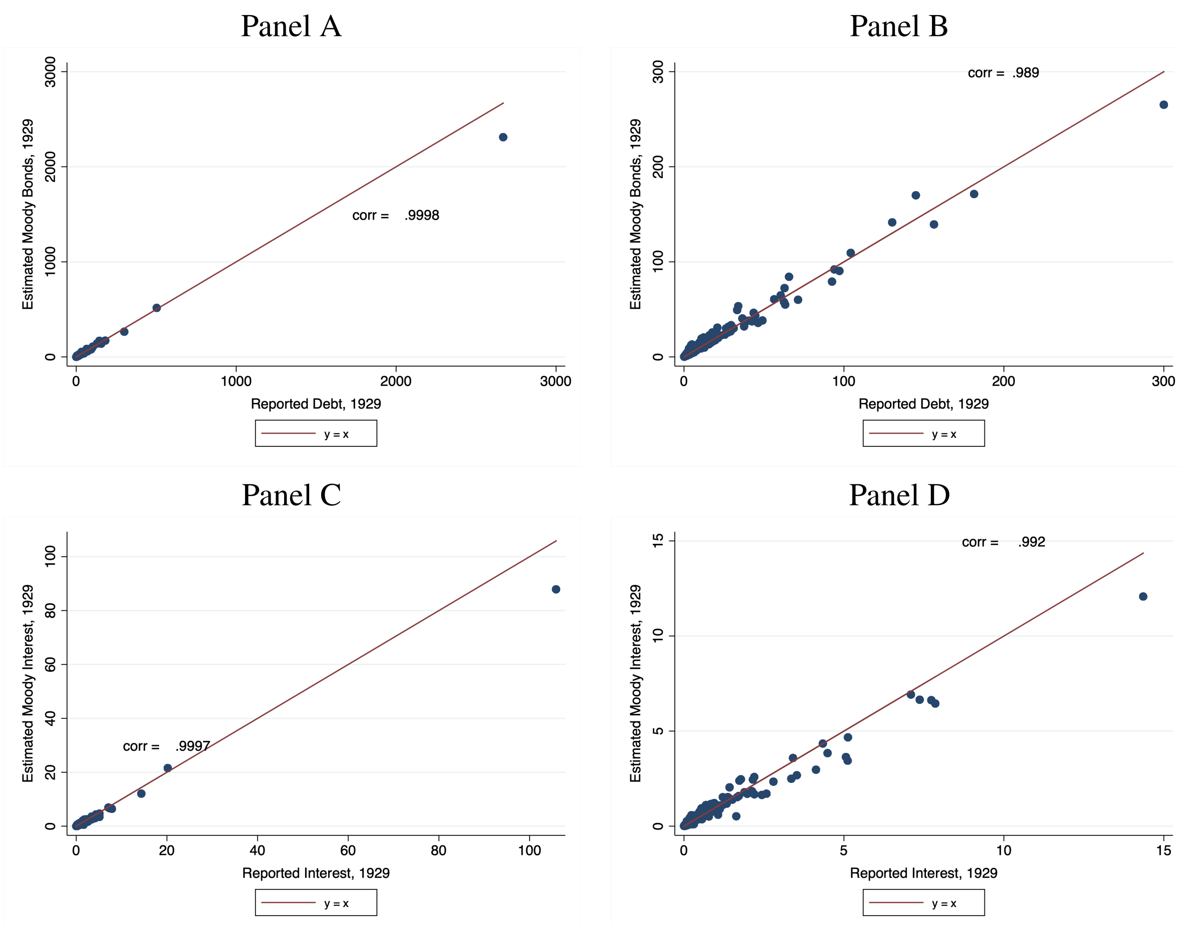
To validate this exercise, I investigate the correlation between imputed Moody aggregates and the totals reported in the financial transactions data. Figure 4.10 reports this relationship for total outstanding debt and total interest payments for 1929. With no measurement error, all cities would lie 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 4.1: 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					

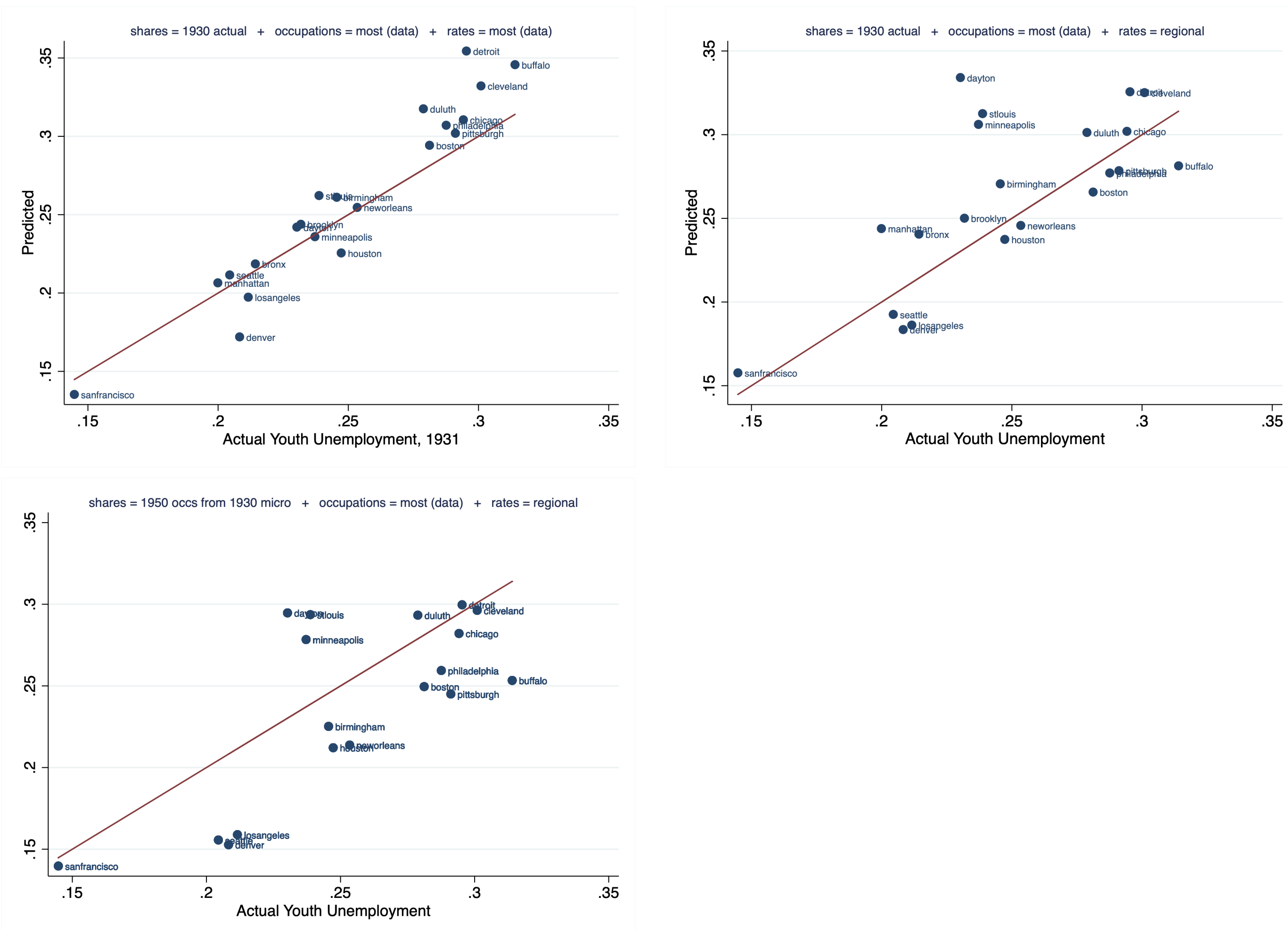
Figure 4.10: Moody Bonds vs. Reported



Notes: 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.

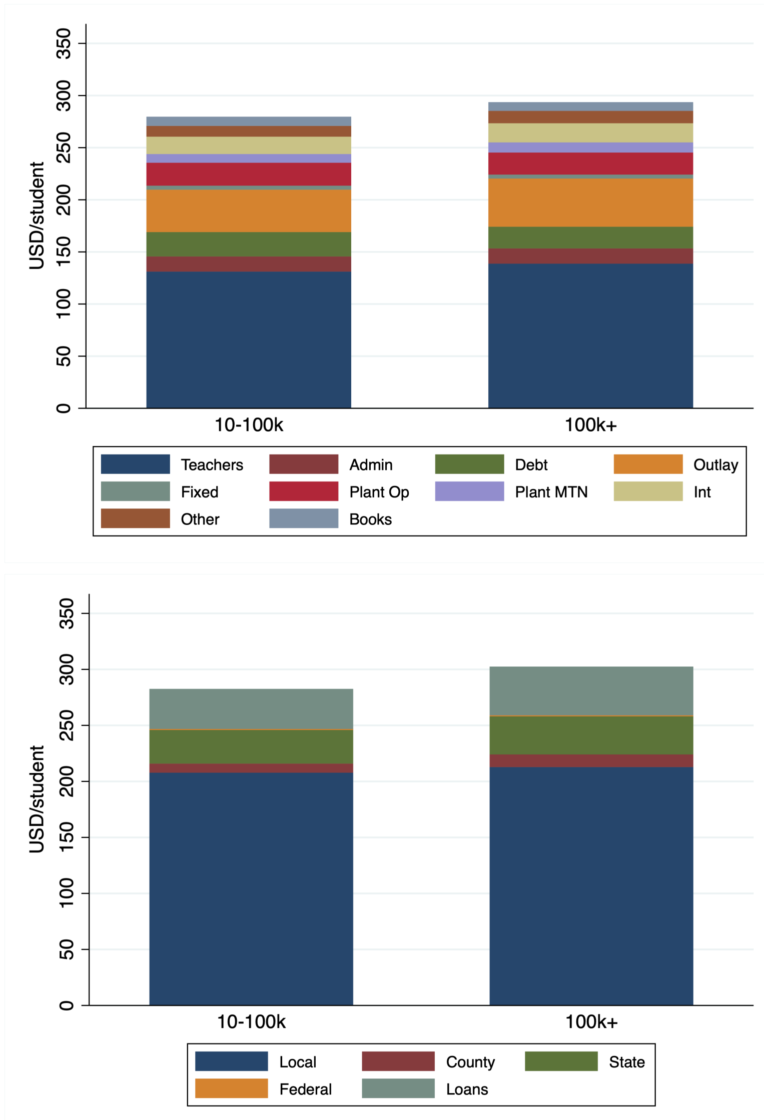
4.4 Appendix Chapter 2 - Figures

Figure 4.11: Unemployment Validation



Notes: This figure plots the actual and estimated youth unemployment rates for 21 cities appearing in the 1931 Census. Title denotes how each estimated rate was constructed. See Section 2.3.1 for details regarding the differences between the panels.

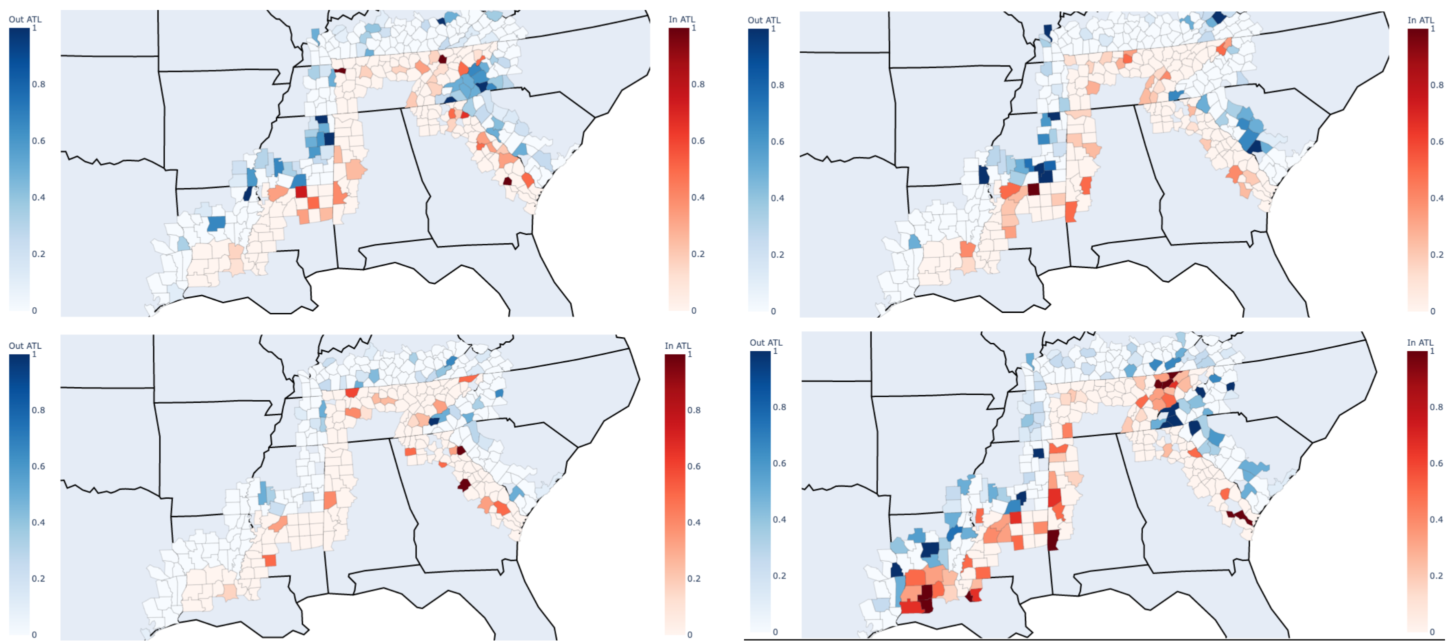
Figure 4.12: School district expenditure and revenue, 1930



Notes: The figures show the average composition of school district expenditure and revenue in 1930 by population category for the districts in my sample. The source of the data is the *Biennial Survey of Education*.

4.5 Appendix Chapter 3 - Figures

Figure 4.13: Bank Suspension Rate



Notes: This figures plots the bank suspension rate in each year from 1929 to 1932 for all counties within 50 miles of the Atlanta Federal Reserve District border. Blue shades denote outside counties and red shades denote inside counties.

4.6 Appendix Chapter 3 - Tables

Table 4.2: Manufacturing Results - Borrowing vs. Financial Constraints

	Constrained: Difficult / Borrowers				Borrowers / All				Difficult / All			
	Wages	Output	Est.	Workers	Wages	Output	Est.	Workers	Wages	Output	Est.	Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
In ATL=1 × Year=1929	-0.060	-0.068	0.063	-0.068	-0.054	-0.060	0.067	-0.062	-0.059	-0.067	0.067	-0.069
	(0.053)	(0.057)	(0.051)	(0.057)	(0.052)	(0.056)	(0.050)	(0.055)	(0.054)	(0.057)	(0.051)	(0.057)
In ATL=1 × Year=1931	-0.103	-0.010	-0.075	-0.043	-0.094	-0.005	-0.067	-0.041	-0.111	-0.016	-0.084	-0.054
	(0.103)	(0.095)	(0.058)	(0.098)	(0.101)	(0.094)	(0.058)	(0.096)	(0.105)	(0.097)	(0.059)	(0.099)
In ATL=1 × Year=1933	-0.103	-0.051	-0.108	-0.041	-0.098	-0.042	-0.107	-0.047	-0.112	-0.057	-0.121*	-0.055
	(0.134)	(0.115)	(0.069)	(0.133)	(0.133)	(0.113)	(0.073)	(0.133)	(0.137)	(0.117)	(0.072)	(0.136)
In ATL=1 × Year=1935	-0.080	-0.080	-0.127*	-0.096	-0.061	-0.072	-0.129*	-0.092	-0.084	-0.083	-0.137*	-0.102
	(0.132)	(0.100)	(0.071)	(0.108)	(0.126)	(0.098)	(0.071)	(0.105)	(0.133)	(0.101)	(0.072)	(0.109)
In ATL=1 × Year=1937	-0.154	-0.035	-0.123*	-0.131	-0.128	-0.034	-0.119*	-0.119	-0.161	-0.039	-0.130*	-0.140
	(0.147)	(0.094)	(0.068)	(0.122)	(0.138)	(0.092)	(0.067)	(0.116)	(0.150)	(0.094)	(0.069)	(0.123)
Constrained=1 × Year=1929	0.013	0.002	0.072	-0.002	0.065	0.087	0.017	0.066	0.014	0.035	0.070	-0.015
	(0.053)	(0.056)	(0.050)	(0.057)	(0.067)	(0.066)	(0.051)	(0.069)	(0.055)	(0.059)	(0.049)	(0.062)
Constrained=1 × Year=1931	-0.274**	-0.201**	-0.152***	-0.238**	0.152	0.089	0.120*	0.088	-0.172	-0.148	-0.115**	-0.195*
	(0.106)	(0.097)	(0.055)	(0.099)	(0.132)	(0.123)	(0.067)	(0.123)	(0.108)	(0.099)	(0.058)	(0.103)
Constrained=1 × Year=1933	-0.376***	-0.289**	-0.245***	-0.367***	0.122	0.146	0.086	0.036	-0.153	-0.083	-0.155**	-0.214
	(0.135)	(0.115)	(0.074)	(0.131)	(0.168)	(0.141)	(0.084)	(0.163)	(0.139)	(0.118)	(0.077)	(0.134)
Constrained=1 × Year=1935	-0.244*	-0.157	-0.193***	-0.229**	0.250	0.123	0.042	0.100	0.007	-0.010	-0.103	-0.003
	(0.130)	(0.098)	(0.070)	(0.112)	(0.152)	(0.113)	(0.081)	(0.130)	(0.133)	(0.099)	(0.075)	(0.114)
Constrained=1 × Year=1937	-0.282*	-0.147	-0.149**	-0.269**	0.333*	0.043	0.082	0.201	-0.120	-0.074	-0.085	-0.118
	(0.162)	(0.095)	(0.062)	(0.134)	(0.198)	(0.113)	(0.073)	(0.155)	(0.161)	(0.098)	(0.067)	(0.135)
R-sq	0.43	0.60	0.59	0.31	0.43	0.59	0.57	0.30	0.42	0.59	0.58	0.30
N	660	660	708	702	660	660	708	702	660	660	708	702
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period banking x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

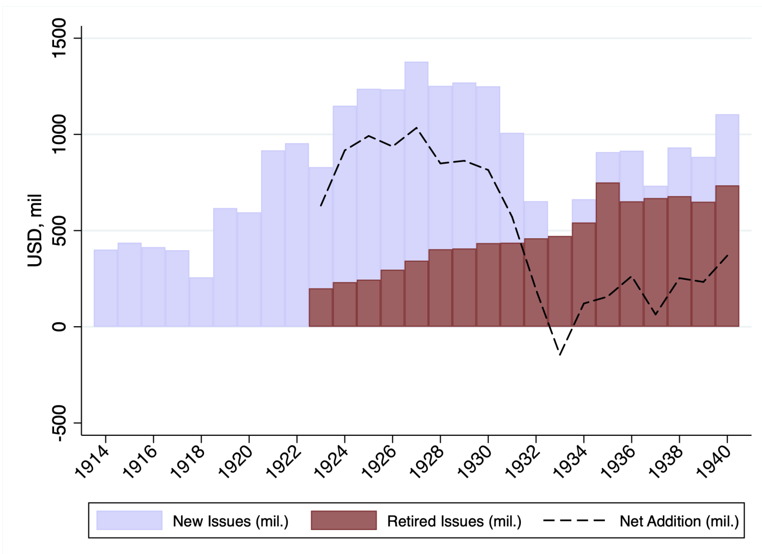
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects and the Constrained x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. Columns (1) - (4) define Constrained as the estimated share of manufacturing borrowers experiencing difficulty obtaining credit. Columns (5) - (8) define Constrained as the estimated share of all firms borrowing. Columns (9) - (12) defined Constrained as number of firms reporting borrowing difficulty over total firms. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. The outcome variables come from Census of Manufactures. The time period is 1927 - 1937 (biennially) for all specifications and the standard errors are clustered at the county level.

FIGURES AND TABLES

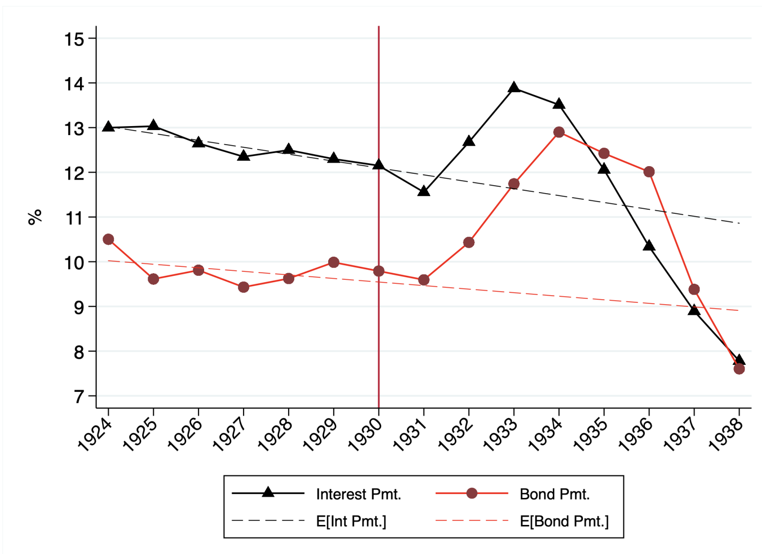
A Figures and Tables from Chapter 1

Figure A.1: Municipal Debt Sales and Retirements



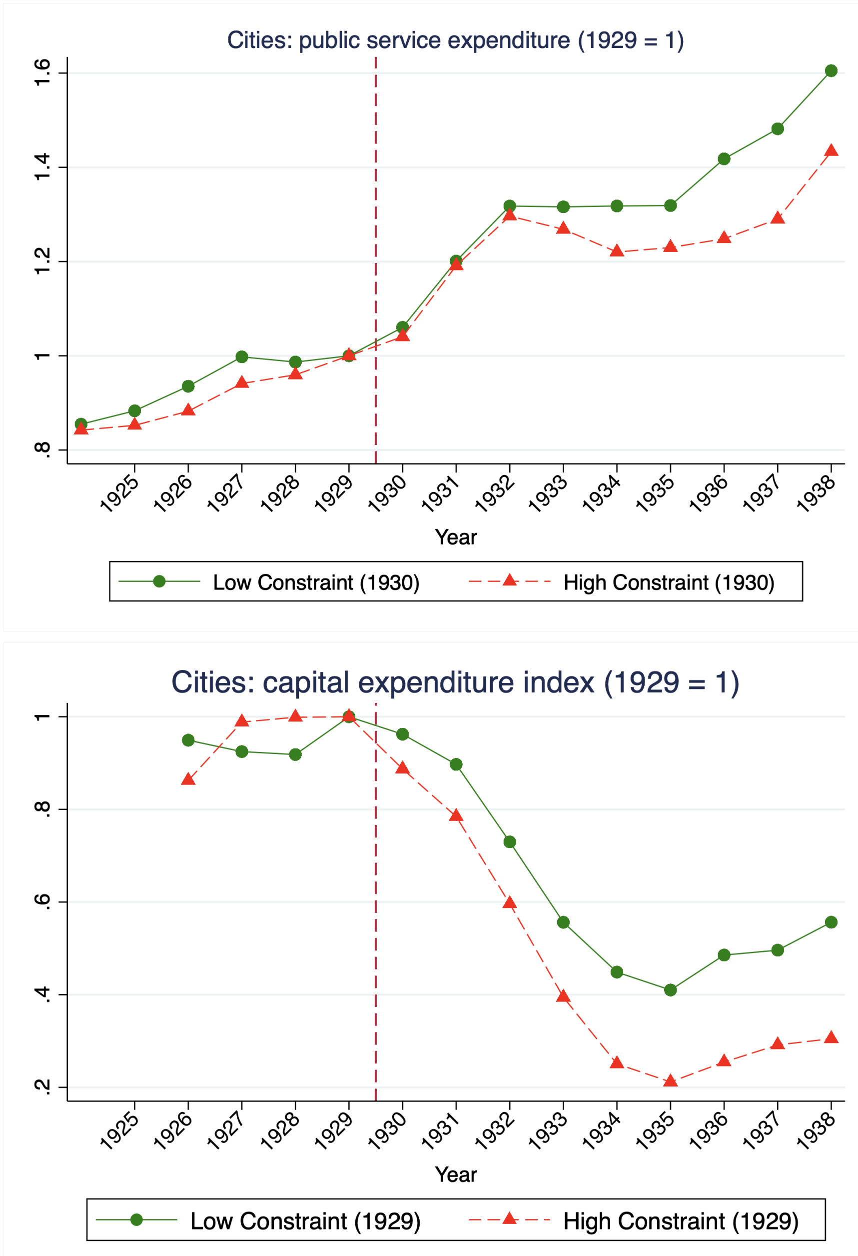
Note: This figure plots the volume of municipal bond sales and retirement as reported by [Hillhouse, 1936] in Tables 1 and 5. The original source of the data is the State and Municipal Compendium (June issue of the *Commercial and Financial Chronicle*). The figures for retired issues were not compiled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

Figure A.2: Financing Costs in Relation to Public Good Expenditure



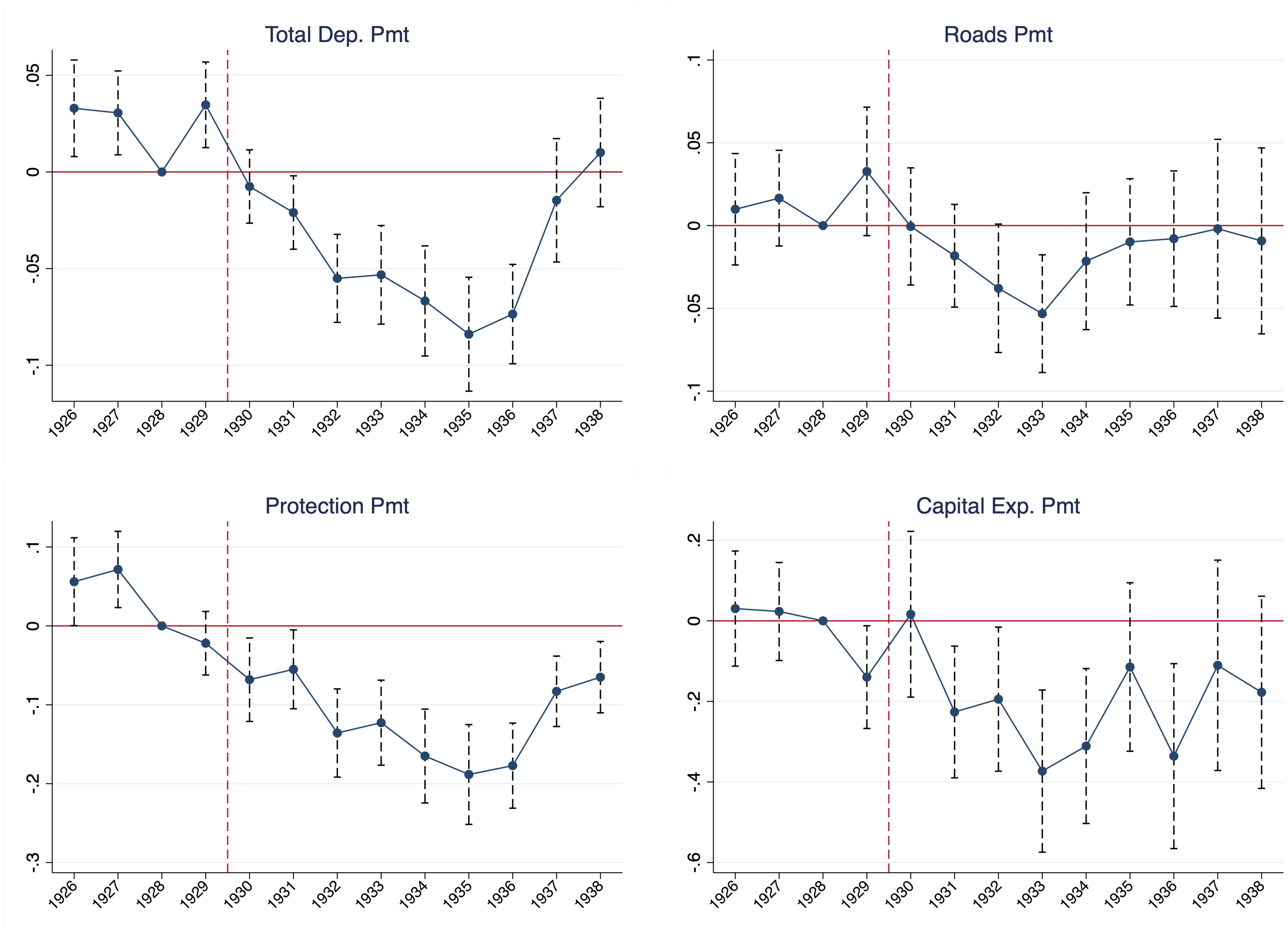
Note: This figure plots the average ratio of interest payments (black line) and long-term bond payments (red line) to total non-welfare and non-debt payments across a balanced panel of cities. Dashed lines denote the best linear fit from 1924–1930 extrapolated to 1931–1938. Both measures were trimmed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

Figure A.3: Leverage and Local Public Goods



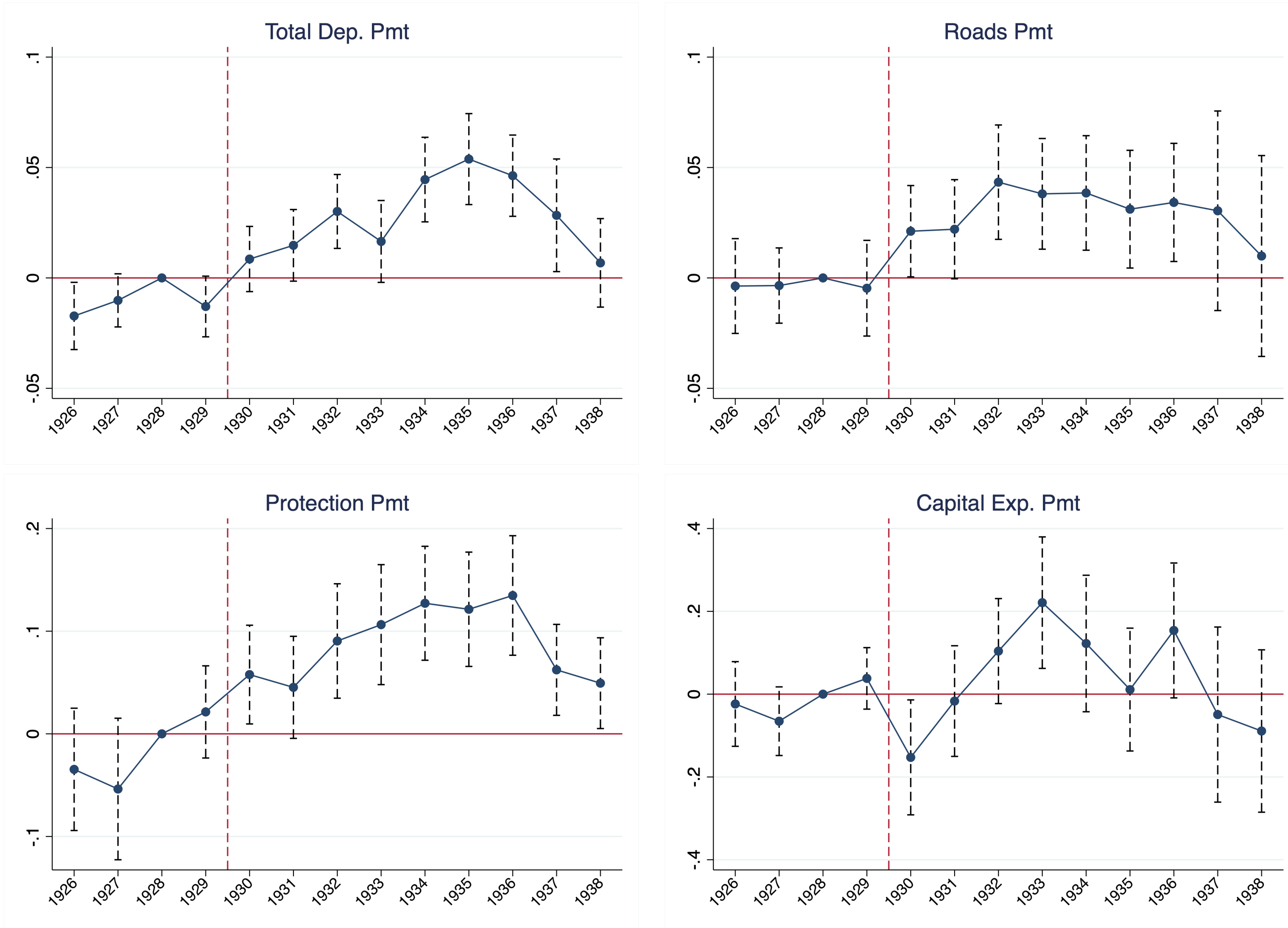
Note: This figure plots the average total public service expenditure (left) and capital expenditure (right) in cities by leverage."Low Constraint" is defined as the first tercile of debt/property value in 1930 and "High Constraint" denotes the third tercile. All values are deflated using the CPI and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

Figure A.4: Event Study using DOV



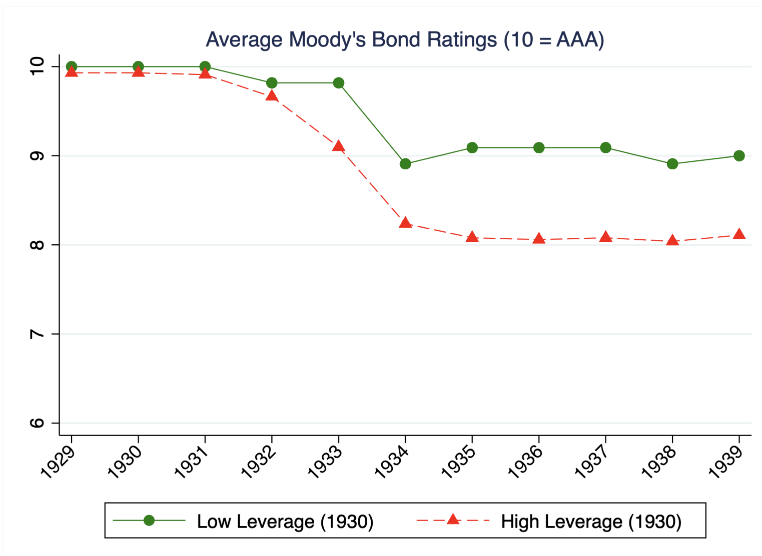
Note: This figure shows the estimated coefficient on $year_{j=t} \times leverage_{30,i}$ in Equation 2.6 using DOV as the leverage measure. Total Dep. Pmt refers to total public service expenditure (i.e., total payments not including capital expenditure or financing costs). Roads Pmt. refers to all expenditure 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 A.5: Event Study using TOI



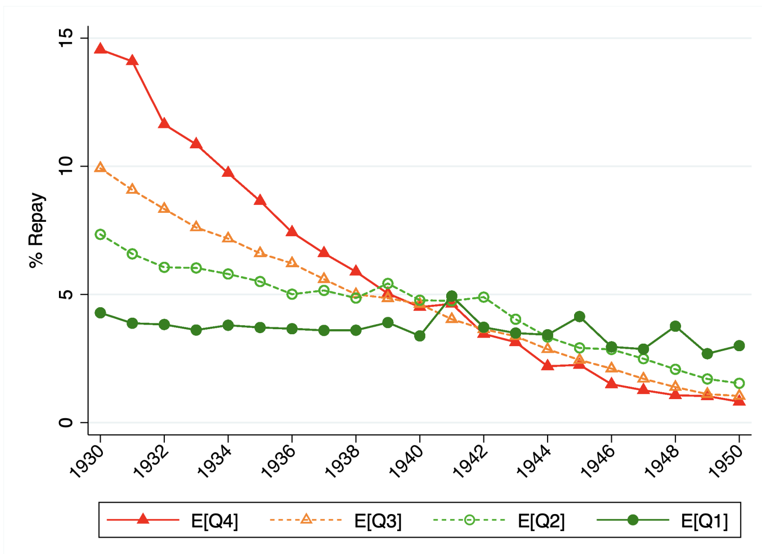
Note: This figure shows the estimated coefficient on $year_{j=t} \times leverage_{30,i}$ in Equation 2.6 using TOI as the leverage measure. Total Dep. Pmt refers to total public service expenditure (i.e., total payments not including capital expenditure or financing costs). Roads Pmt. refers to all expenditure 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 A.6: Moody's Ratings



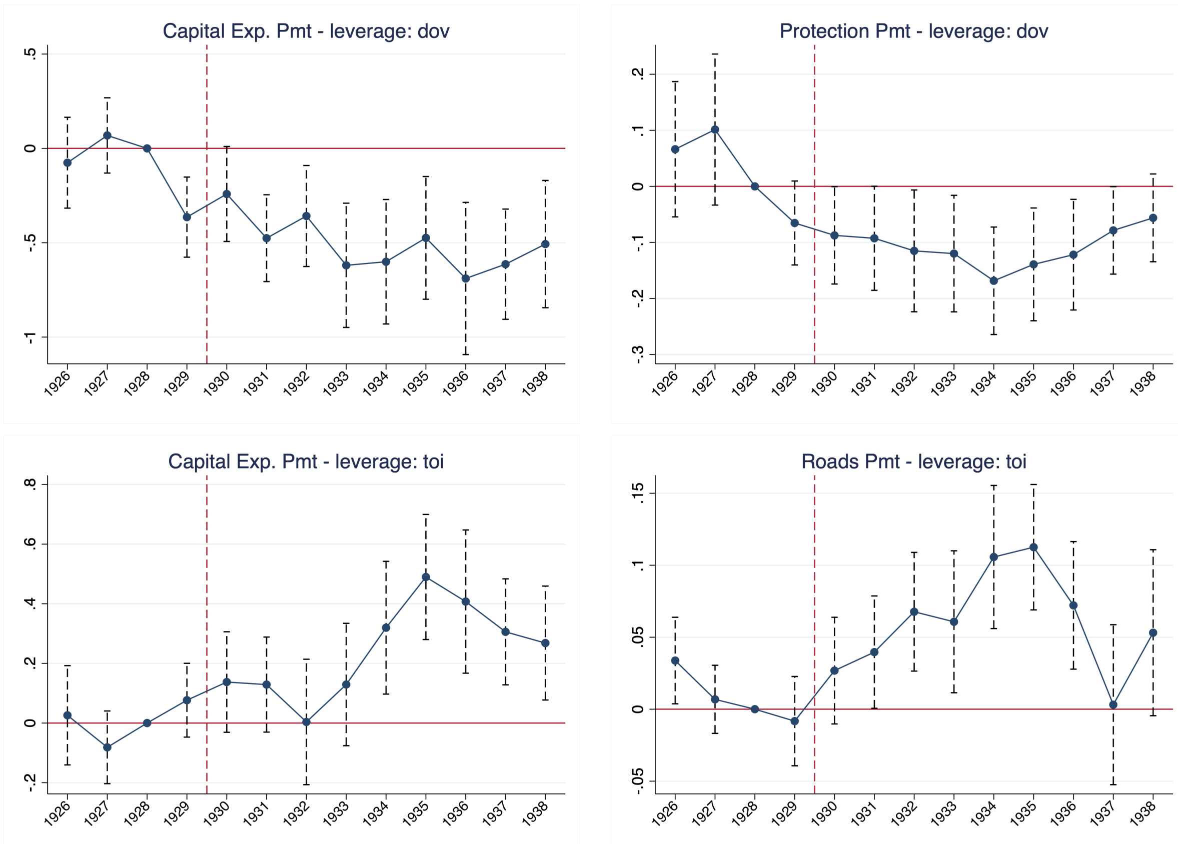
Note: This figure plots the average Moody's Bond rating of cities by leverage. Low leverage is defined as the first tercile of DOV in 1930 and high leverage is denoted by the third tercile. The sample includes 189 cities with complete data from 1929 to 1940. A rating of AAA is assigned the value of 10,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

Figure A.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 A.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 2.6 using the shocked meaasures of leverage as in Equation 1.6. 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 3: Summary Statistics - Chapter 1

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

Notes: 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 4: 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

Notes: 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 5: Average real per capita city revenue and spending data by population and year (1929 = 1.0)

	1924	1930	1931	1932	1933	1934	1935	1936
Population: 1-10k								
Total Revenue (Rev.)	0.81	1.03	1.10	1.15	1.11	1.10	1.11	1.15
Tax Rev.	0.79	1.05	1.12	1.14	1.07	1.06	0.94	0.98
Department Earnings Rev.	0.91	1.08	1.10	1.46	1.58	1.48	1.40	1.65
Total Dep. Pmt	0.87	1.05	1.18	1.26	1.22	1.16	1.20	1.23
Health Pmt	0.79	1.04	1.17	1.29	1.23	1.24	1.25	1.28
Roads Pmt	0.85	1.03	1.16	1.17	1.06	0.99	1.03	1.09
Protection Pmt	0.74	1.11	1.24	1.31	1.49	1.29	1.28	1.31
Welfare Pmt	0.78	1.19	1.86	3.68	4.84	4.70	5.84	5.15
Education Pmt	0.99	1.04	1.17	1.24	1.19	1.15	1.16	1.13
Utility Pmt	1.06	0.98	1.04	1.09	0.80	0.89	0.98	1.04
Interest Pmt	0.94	1.00	1.05	1.18	1.18	1.06	0.91	0.88
Capital Exp. Pmt	0.70	0.71	0.47	0.35	0.22	0.22	0.25	0.32
Population: 10-100k								
Total Revenue (Rev.)	0.79	1.04	1.13	1.23	1.25	1.26	1.29	1.27
Tax Rev.	0.79	1.06	1.14	1.22	1.18	1.20	1.18	1.16
Department Earnings Rev.	0.91	1.02	1.20	1.47	1.63	1.67	1.62	1.55
Total Dep. Pmt	0.84	1.05	1.23	1.36	1.36	1.33	1.36	1.34
Health Pmt	0.83	1.03	1.13	1.21	1.16	1.15	1.14	1.18
Roads Pmt	0.83	1.01	1.14	1.10	1.02	1.02	1.03	1.02
Protection Pmt	0.77	1.08	1.21	1.27	1.29	1.24	1.23	1.23
Welfare Pmt	0.70	1.27	2.14	4.41	5.73	5.66	6.03	5.37
Education Pmt	0.84	1.06	1.19	1.28	1.25	1.21	1.24	1.24
Utility Pmt	0.99	1.01	1.16	1.14	0.95	0.99	1.06	1.13
Interest Pmt	0.71	1.02	1.10	1.37	1.42	1.29	1.13	1.07
Capital Exp. Pmt	0.90	0.90	0.82	0.47	0.28	0.33	0.40	0.44
Population: 100k+								
Total Revenue (Rev.)	0.85	1.04	1.10	1.14	1.12	1.12	1.14	1.16
Tax Rev.	0.86	1.06	1.14	1.21	1.17	1.17	1.19	1.19
Department Earnings Rev.	0.89	1.08	1.15	1.27	1.18	1.19	1.23	0.99
Total Dep. Pmt	0.84	1.05	1.20	1.29	1.25	1.20	1.19	1.21
Health Pmt	0.84	1.04	1.12	1.14	1.02	0.97	0.97	1.00
Roads Pmt	0.89	1.02	1.14	1.09	0.97	0.99	0.96	0.98
Protection Pmt	0.85	1.04	1.15	1.20	1.12	1.08	1.07	1.09
Welfare Pmt	0.74	1.20	1.84	2.98	3.68	3.36	3.22	3.14
Education Pmt	0.85	1.05	1.18	1.25	1.15	1.09	1.09	1.13
Utility Pmt	0.91	1.03	1.12	1.11	1.03	1.04	1.06	1.19
Interest Pmt	0.76	1.07	1.21	1.40	1.47	1.41	1.30	0.89
Capital Exp. Pmt	1.03	1.11	0.97	0.62	0.33	0.32	0.39	0.51

Notes: Reported values are the mean revenues and expenditures of cities in each population category by year (1929 = 1). Population category is static for each city and was assigned using the population in the 1930 Census. All cities with fewer than 12 years of data were dropped from the sample. The sample is slightly unbalanced and consists of approximately 374 cities in 1-10 thousand, 223 between 10-100 thousand, and 91 cities of 100 thousand and above. To calculate per capita values, census population data were linearly interpolated between census years. All values deflated using the CPI.

Table 6: 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

Notes: 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 7: 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

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

Notes: 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 8: 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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5. 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 9: Difference-in-Differences: Types of Expenditure

Panel A: Using DOV as Leverage Measure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Cap. Ex.	Gov.	Sanit.	Health	Road	Protection	Welfare	Education	Rec.	Interest	Δ Total Debt	Δ Short Debt	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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Cap. Ex.	Gov.	Sanit.	Health	Road	Protection	Welfare	Education	Rec.	Interest	Δ Total Debt	Δ Short Debt	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

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

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 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 10: 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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 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 11: 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 $\times$ 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 $\times$ 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

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

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 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 12: 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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 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 13: 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

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

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 for capital expenditure. Columns (1) and (4) recapitulate the main results in Table 8. 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 14: Robustness: Alternative Measurement, City-Bank Connections, Political Motives

Panel A. Outcome: Public Service Expenditure

	Baseline (1)	Retail (2)	Baseline (4) (3)	Bank Connections (4)	Gov Form (5)	Election Cycle (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 (1)	Retail (2)	Baseline (4) (3)	Bank Connections (4)	Gov Form (5)	Election Cycle (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

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

Notes: This table presents the results of the difference-in-differences regression of Equation 2.5 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 15: 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 16: 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
$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Notes: This table presents the results of a within-city regression of Equation 1.8 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 17: 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

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

Notes: This table presents the estimation results of Equation 1.9. 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 **??** 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 **??** 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 **??**. Standard errors are shown in parentheses and are clustered at the county level.

Table 18: Migration Behavior: Heterogeneity

Outcome: I(moved)

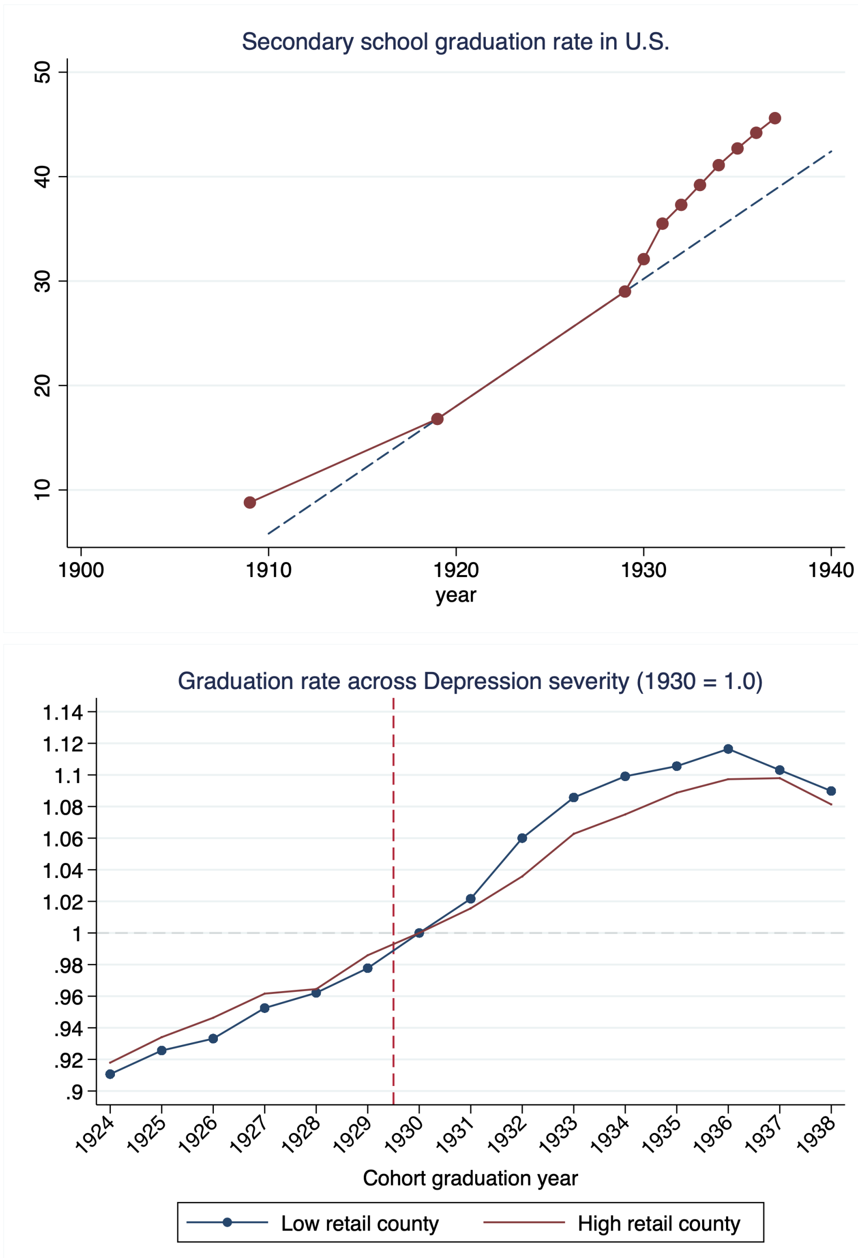
	(1) Poor	(2) Rich	(3) Short moves	(4) Long moves	(5) Local admins	(6) Non-local admins
$\widehat{DOV}$=1 × GD Severe=1	0.066**	0.064**	0.062*	0.039**	0.055	0.037
	(0.030)	(0.031)	(0.037)	(0.017)	(0.035)	(0.037)
GD Severe=1	0.013	0.015	0.019	0.017	0.024	0.015
	(0.019)	(0.020)	(0.021)	(0.011)	(0.022)	(0.029)
$\widehat{DOV}$=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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents the estimation results of Equation 1.9 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 **??** 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 **??** 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 **??**. 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.

B Figures and Tables from Chapter 2

Figure B.1: High School Movement and the Great Depression



Note: **Top.** This figure plots the number of high school graduates divided by 100 17-year-olds in the United States for the years 1910, 1920, and 1930-1938. The blue dashed line denotes the average 1920-1930 growth rate extrapolated to earlier and later decades. The source of the data is U.S. Department of Commerce, Bureau of the Census, *Historical Statistics of the United States, Colonial Times to 1970* as reproduced in Table 19 of *120 Years of American Education: A statistical portrait* published by the National Center for Education Statistics.
Bottom. This figure plots the evolution of the graduation rate of cohorts based on a linked sample of Census records based on geographical location. Low retail county denotes all counties in the lowest tercile of retail sales growth between 1929 and 1933 and high retail county denotes all cohorts in the top tercile. Measures were divided by 1930 rate such that 1.05 denotes a 5 percent increase from 1930.

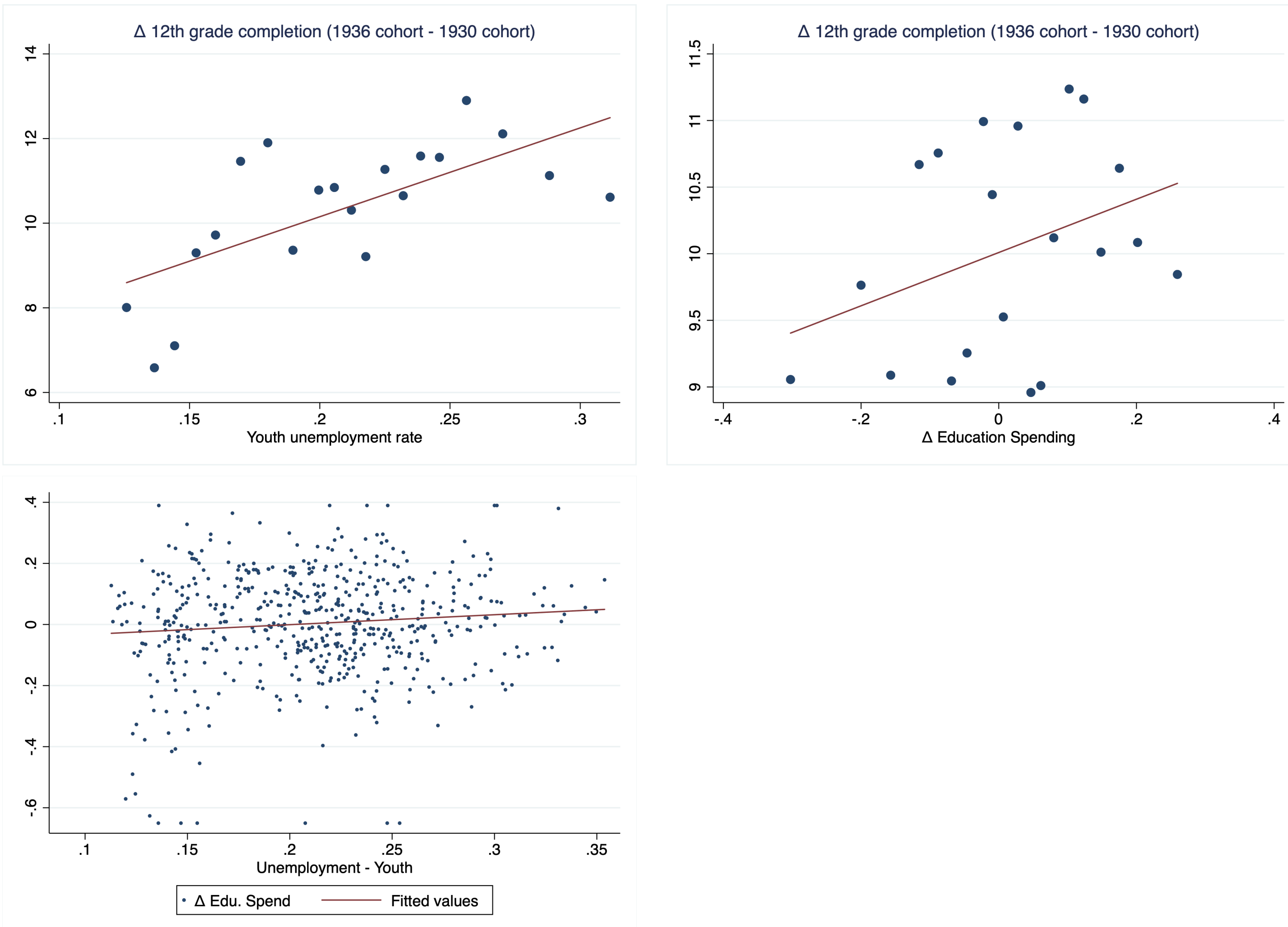
Figure B.2: Unemployment Rates for Youth in 1931 in Select Occupations and Cities

City	Rate	Weight	Occupation
Duluth	68.1%	6.2%	Laborers : Iron and steel industries
Seattle	59.4%	1.1%	Operatives : Iron and steel industries
Chicago	55.9%	2.0%	Operatives : Paper, printing, and allied industries
Chicago	54.6%	1.6%	Operatives : Food and allied industries
Denver	54.1%	1.0%	Operatives : Iron and steel industries
Detroit	53.9%	11.5%	Laborers : Iron and steel industries
Philadelphia	53.7%	1.7%	Operatives : Electrical machinery and supply factories
Buffalo	53.2%	1.1%	Laborers : Food and allied industries
San Francisco	53.0%	1.1%	Laborers : Building construction, laborers, and helpers
Detroit	52.8%	2.0%	Mechanics
Philadelphia	52.3%	1.1%	Operatives : Leather industries
Boston	51.5%	5.0%	Operatives : Leather industries
Duluth	51.4%	3.5%	Laborers : Building construction, laborers, and helpers
Detroit	50.8%	9.0%	Operatives : Iron and steel industries
Boston	50.7%	1.1%	Operatives : Clothing industries
Buffalo	50.4%	1.1%	Operatives : Leather industries
New Orleans	50.0%	1.4%	Porters (except in stores)
Chicago	50.0%	1.0%	Laborers : Building construction, laborers, and helpers
Buffalo	49.5%	3.3%	Laborers : Building construction, laborers, and helpers
Seattle	49.1%	3.3%	Laborers : Building construction, laborers, and helpers
Boston	48.9%	2.4%	Laborers : Building construction, laborers, and helpers
Chicago	48.9%	1.8%	Mechanics
Buffalo	48.8%	1.5%	Operatives : Textile industries

Birmingham	2.8%	1.3%	Engineers (stationary), cranemen, hoistmen, etc
San Francisco	4.3%	7.9%	Servants (except cooks)
Denver	4.9%	3.5%	Bookkeepers, cashiers, and accountants
San Francisco	5.2%	2.3%	Waiters
San Francisco	5.4%	8.8%	Sailors, deck hands, boatmen, etc.
Denver	5.4%	2.1%	Retail dealers
San Francisco	7.1%	3.3%	Bookkeepers, cashiers, and accountants
Minneapolis	7.6%	3.9%	Bookkeepers, cashiers, and accountants
St. Louis	8.0%	2.8%	Bookkeepers, cashiers, and accountants
Seattle	8.1%	2.3%	Bookkeepers, cashiers, and accountants
San Francisco	8.5%	2.2%	Retail dealers
Denver	9.0%	27.6%	Clerks (except "clerks" in stores)
Seattle	9.4%	8.5%	Servants (except cooks)
Los Angeles	9.8%	3.1%	Bookkeepers, cashiers, and accountants
San Francisco	10.5%	31.0%	Clerks (except "clerks" in stores)
Seattle	10.6%	17.5%	Clerks (except "clerks" in stores)
Manhattan	11.1%	1.5%	Retail dealers
San Francisco	11.1%	2.0%	Machinists, millwrights, and toolmakers
Los Angeles	11.2%	19.9%	Clerks (except "clerks" in stores)
Manhattan	11.8%	6.5%	Salesmen and saleswomen
New Orleans	11.8%	2.7%	Bookkeepers, cashiers, and accountants
Denver	11.9%	3.2%	Laborers : Iron and steel industries
Seattle	11.9%	6.8%	Sailors, deck hands, boatmen, etc.

Note: The largest and smallest youth unemployment rates as defined in Section 2.3.1 across cities in the 1931 Special Census of Unemployment. Column "Weight" refers to share of the youth labor force at the occupation-city level, as of 1930.

Figure B.3: Youth Unemployment, Education Spending, and High School Graduation during the Depression

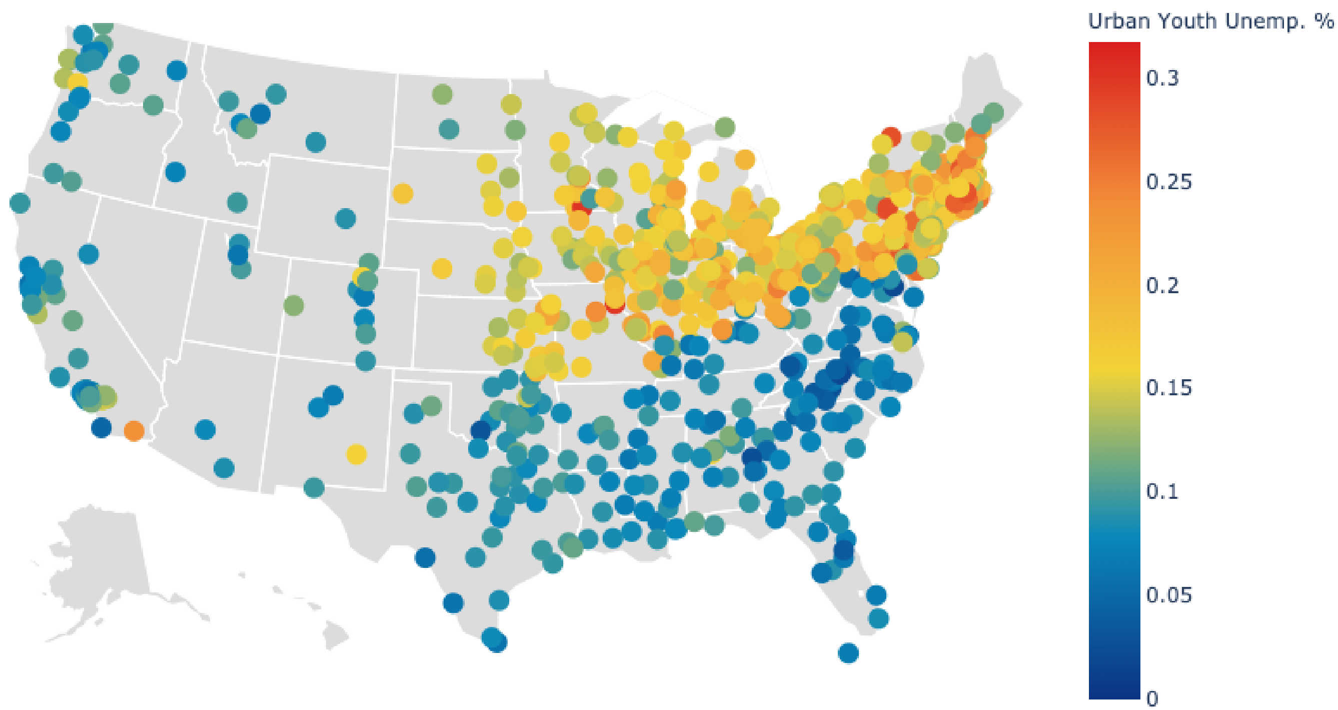


Note: This figure plots a binned scatterplot of the change in the high school graduation rate between pre- and post-Depression cohorts and the estimated youth unemployment rate in 1931 (top left) and the change in log per-pupil spending between 1934 and 1930 (top right) at the city level. The red line denote a linear fit, weighed by the log number of 7-13 year olds at the county level. Each marker on the plot accounts for roughly 45 cities and the total number of cities is 917 (top left) and 555 (top right). The panel in bottom left shows a scatterplot between unemployment and change in education spending for 555 cities (correlation = 0.09). The cohort graduation rate is defined as the number of respondents reporting at least 12 years of schooling in the 1940 Census divided by total cohort size in a 1930-1940 linked sample. The youth unemployment rate is estimated using city-level occupation shares and the Special Unemployment Census of 1931. Section 2.3.1 describes the construction in more detail. Grade completion in 1936 is the average between 1935-37 and in 1930 it is the average between the 1929-1931 cohorts to minimize year selection bias. All measures were trimmed at 2-98 levels to avoid influence of outliers.

Figure B.4: Average Quality of Public Schools over Time

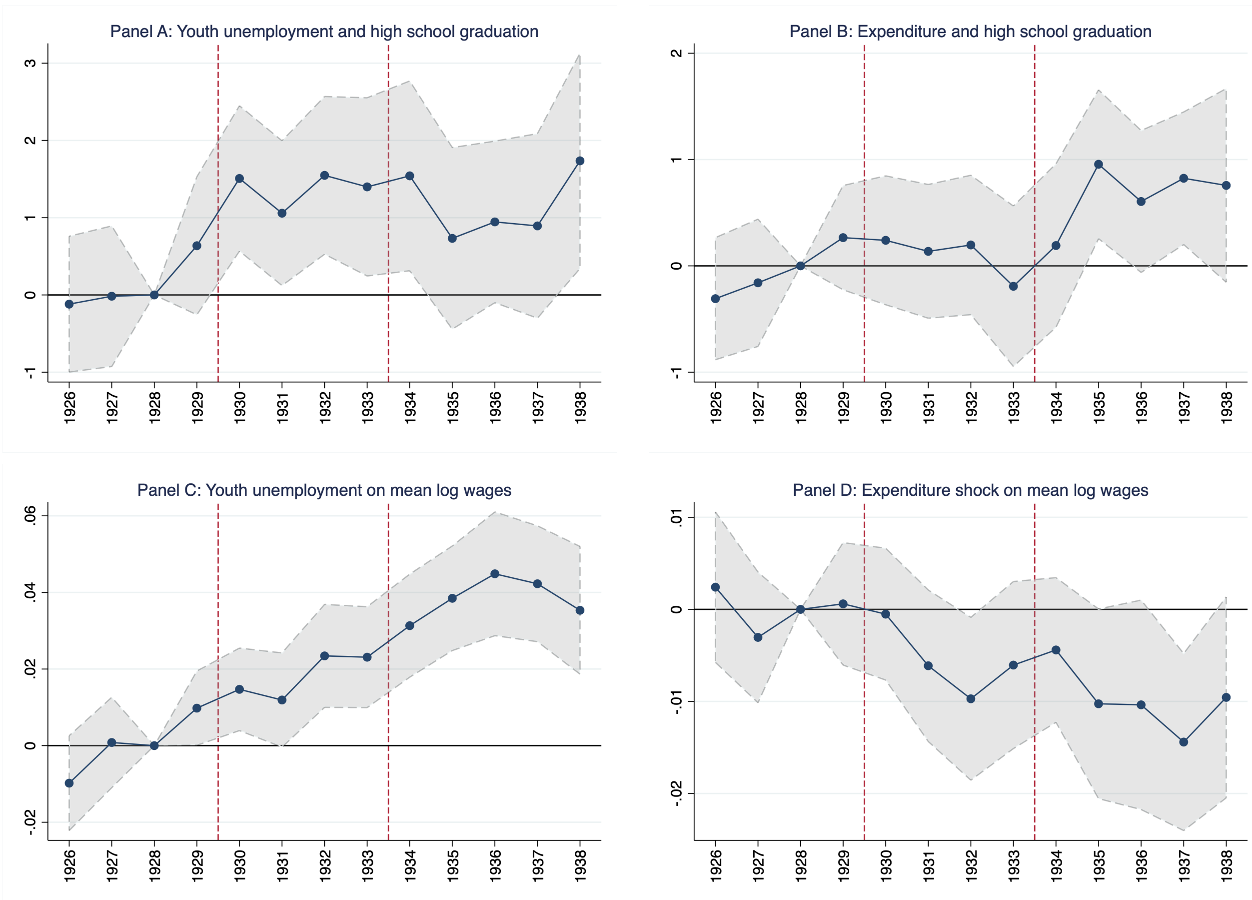
Note: This figure plots measures of average school quality for 220 large U.S. cities between 1922 and 1938. The data comes from the *Biennial Census of Education* of the U.S. Office of Education. Panel A: Student teacher ratio is total enrolled students over total number of teachers for elementary (typically grades 1-8) and secondary (typically grades 9 - 12) schools. Panel B: Teacher wage index is average city-level teacher wages deflated by the CPI over average city-level wages in 1940. To compute average city-level wages in 1940 I average reported earnings of all white, 22-65 year old, non-teachers (Census variable *occ* does not equal 18) in the 100 percent count Census records reporting at least 13 years of education. Panel C: Total school expenditure per pupil, deflated by the CPI to 1967 dollars. Panel D: Number of school days in term.

Figure B.5: Map of Urban Youth Unemployment Estimates



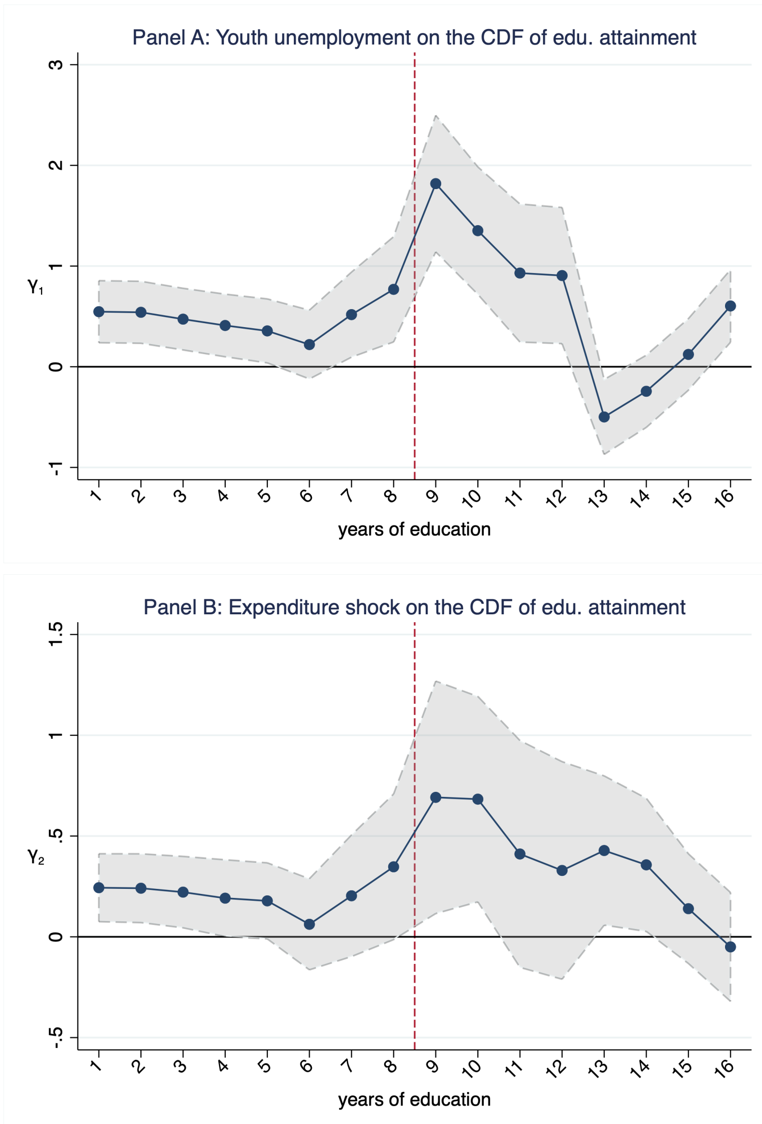
Note: This figure plots the geographical distribution of the youth unemployment estimate in 1931. Youth unemployment is estimated using city-level occupation shares and regional occupational unemployment shares as computed from the Special Unemployment Census of 1931. Section 2.3.1 describes the construction in more detail.

Figure B.6: Event Study: High School Graduation and Wages in 1940



Note: This figure shows the estimated coefficients in Equation 2.6, the event study coefficient denoting the marginal difference between places with varying levels of estimated youth unemployment and public school expenditure during the Depression across U.S. cities. Dashed red veritical lines represent the peak and trough of the Depression. Panels A and B show the annual coefficients when the outcome is city-cohort graduation rates and Panels C and D show them when the outcomes are mean cohort log weekly wages. City-cohort measures were derived using a linked sample of white males using 1930 and 1940 Census records and crosswalks obtained from [Abramitzky et al., 2020]. 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. Panel A: The p-value for the omnibus hypothesis test of zero average pre-event effects (1926, 1927, 1929) is 0.29. The p-value for zero average post-event effect (1934 - 1938) is 0.026. Panel B: The p-value for the omnibus hypothesis test of zero average pre-event effects (1926, 1927, 1929) is 0.94. The p-value for zero average post-event effect (1934 - 1938) is 0.10.

Figure B.7: Effect on the Cumulative Distribution Function of Education Attainment

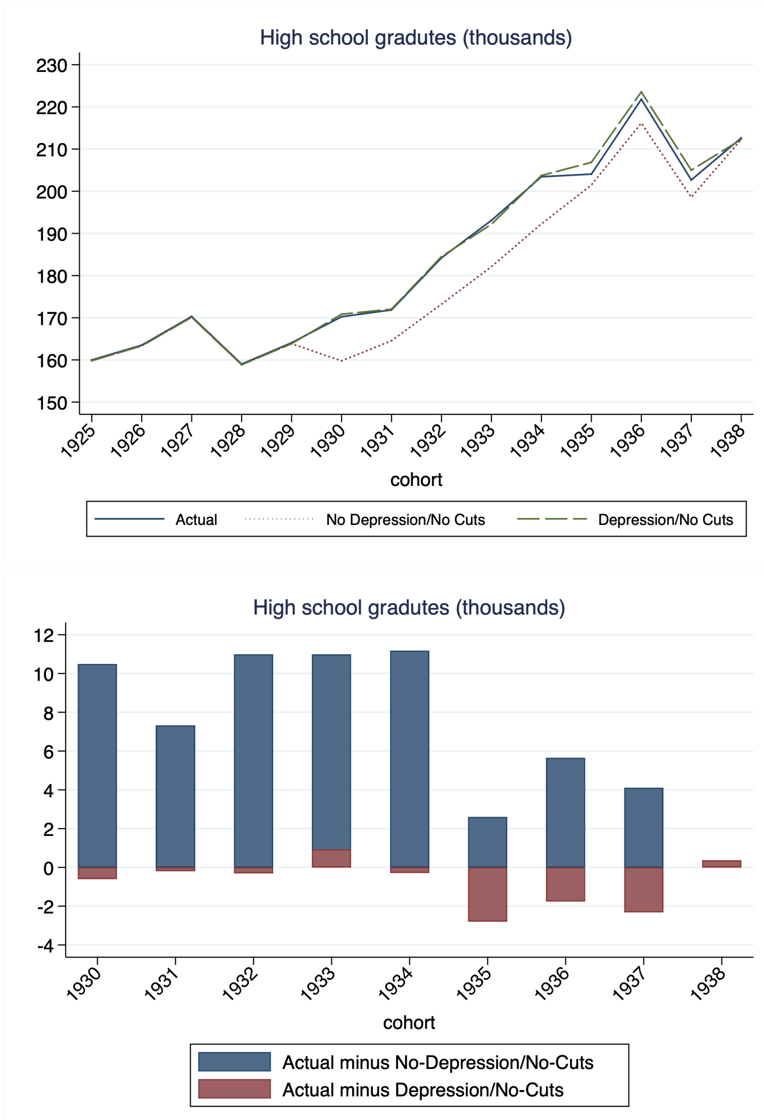


Note: This figure shows the estimated coefficients γ_1 (Panel A) and γ_2 (Panel B) in Equation 2.5, the difference-in-differences coefficients on the impact of estimated youth unemployment and public school expenditure during the Depression on the cumulative years of education completed 1-16. City-cohort measures were derived using a linked sample of white males using 1930 and 1940 Census records and crosswalks obtained from [Abramitzky et al., 2020]. Outcome variable is proportion of linked cohort that completed at least x-axis number of years, as reported in the 1940 Census. All standard errors are clustered at the city level. Ninety percent confidence intervals denoted by dashed lines.

Figure B.8: Youth Unemployment and Youth Workers on Relief during the Depression

Note: This figure shows a binned scatterplot of estimated youth unemployment in 1931 and the ratio of 16-19 year olds on relief rolls in 1934 at the city level. Both measures are residualized to account for the total relief rate in city. The data for 59 cities in 1934 come from a Works Progress Administration study titled *Urban Workers on Relief* ([Wood et al., 1937]). The study reports the number of people in each age group who were, in 1934, receiving aid from the WPA. To compute relief rates, I divide the relief in each age group by total in age group as reported by city in the 1930 Census. The youth unemployment rate is estimated using city-level occupation shares and the Special Unemployment Census of 1931. Section 2.3.1 describes the construction in more detail.

Figure B.9: High School Graduates: Counterfactuals



Note: [top] This figure shows the actual estimated number of high school graduates (solid), the counterfactual estimated number of graduates under a no-Depression/no-spending cuts and Depression/no-spending cuts scenarios (dotted, dashed) as described in Section 2.6. Bottom figure plots the difference between actual and counterfactual, starting in 1930.

Table 19: Summary Statistics - Chapter 2

Panel A: City-Cohorts 1927-1938

	N	Mean	SD	Median	25 pct	75 pct
Education years completed: 8+	6,768	89.957	8.83	92.59	86.98	95.83
Blue collar 12th grads/cohort size	6,768	29.490	10.71	29.17	22.24	36.30
White collar 12th grads/cohort size	6,768	22.170	10.73	20.83	14.55	28.57
Share of blue collar youth	6,768	69.217	12.20	70.59	61.36	77.78
Share of white collar youth	6,768	30.783	12.20	29.41	22.22	38.64
Log wages	6,768	2.992	0.24	3.02	2.84	3.17
Unemployment - Youth	6,768	0.212	0.05	0.21	0.17	0.25
Δ Total Spend	6,768	-0.081	0.26	-0.06	-0.22	0.08
Δ Edu. Spend	6,768	0.003	0.17	0.01	-0.08	0.12
School expenditure (1930)	6,768	5.331	0.38	5.34	5.11	5.57
Youth labor share (1930)	6,768	0.163	0.06	0.15	0.12	0.20
County Unemployment (1930)	6,768	0.138	0.06	0.13	0.09	0.18
Cohort size	6,768	229.870	833.68	71.00	46.00	161.00

Panel B: Person-level 1940 Census

	N	Mean	SD	Median	25 pct	75 pct
12th grade+ completion	1,671,140	0.47	0.50	0.00	0.00	1.00
Weekly wages (log)	1,317,808	3.09	0.50	3.14	2.81	3.40
I(Blue collar hh)	1,671,140	0.40	0.49	0.00	0.00	1.00
Age	1,671,140	16.04	3.74	16.00	13.00	19.00
I(Native)	1,671,140	0.65	0.48	1.00	0.00	1.00
Unemployment - Youth	1,671,140	0.20	0.04	0.20	0.18	0.23
Δ Edu. Spend	1,671,140	0.04	0.13	0.06	-0.04	0.15
Δ Total Spend	1,671,140	-0.07	0.20	-0.06	-0.17	0.02
School expenditure (1930)	1,671,140	5.53	0.31	5.61	5.35	5.71
Youth labor share (1930)	1,671,140	0.18	0.06	0.18	0.14	0.22
County Unemployment (1930)	1,671,140	0.20	0.08	0.20	0.14	0.22
Siblings (linked)	1,671,140	1.37	0.65	1.00	1.00	2.00
I(moved)	1,671,140	0.36	0.48	0.00	0.00	1.00

Panel C: City-Occupations in 1930 and 1931

	N	Mean	SD	Median	25 pct	75 pct
Total under 20 workers [city, 1930]	925	1537.0	8271.5	459.0	288.0	948.0
Youth occupation categories [city, 1930]	925	67.1	25.1	59.0	49.0	79.0
Youth occupation categories w/rates [city, 1931]	925	35.8	13.3	33.0	26.0	43.0
%Youth covered by occupation categories w/rates [city, 1931]	925	73.4	10.5	74.0	65.6	82.3
%Weight per occupation [city x occ, 1930]	33,071	2.8	5.4	0.9	0.4	2.5
%Regional unemployment rate [city x occ, 1931]	33,071	25.5	11.7	24.2	16.6	33.9

Note: **Panel A:** Summary data are given for cohorts across 564 cities for the years 1927-1938 aggregated from Census person-level records. Cohort is approximated by year a respondent turned 18. Education years completed: 8+ denotes the share of cohort that has completed at least 8 years of schooling. Blue collar and white collar pertain to the occupation of the respondent's father, see Section 2.3. Log wages denote the log of weekly wages, after replacing the 98th percentile and above by 1.5 times the 98th percentile. School expenditure is log per pupil expenditure obtained from the *Biennial Survey of Education*. Unemployment (1930) and youth labor share (1930) denote the unemployment rate and proportion of 10-17 year olds in the labor force at the county level in 1930. **Panel B:** Summary statistics of U.S. Decennial Census variables of a linked sample of urban males between 1930 and 1940. Records linked using the ABE procedure using NYSIIS standardization. Crosswalks obtained from [Abramitzky et al., 2020]. I(Blue collar hh) is a binary taking the value of 1 if the reported occupation of father in 1930 is in a blue collar sector. I(Native) is a binary taking the value of 1 if father is native and respondent was born in the U.S. Sample only includes linked records of males for city-cohort as in Panel A. **Panel C:** Summary statistics of the 1931 Special Census of Unemployment and youth labor share in 1930. Total under 20 workers reports, at the city level, the number of workers who report working in the labor force and being under 20 years old in 1930. Weight per occupation denotes occupational share (in percent). Regional unemployment rate denotes the 1931 unemployment estimates in percent. Youth occupation categories denotes the number of occupation categories reported in a city in the 1930 Census. Youth occupation categories denotes occupation unemployment by city in the 1931 Special Census.

Table 20: Most Common Youth Occupations and Unemployment Rates by Region

Rank	Modal Occupation	Unemployment Rate	# Cities	Weight
	Midwest			
1	Retail workers	7%	176	16%
2	Servants (except cooks)	16%	50	24%
3	Operatives: Leather industries	43%	15	24%
4	Clerks (except "clerks" in stores)	15%	15	21%
5	Laborers: Iron and steel industries	50%	6	17%
6	Operatives: Clothing industries	44%	4	19%
7	Laborers: Food and allied industries	45%	3	30%
8	Farm laborers (wageworkers)	33%	3	17%
9	Operatives: Metal industries (except iron and steel)	33%	3	17%
10	Operatives: Iron and steel industries	46%	2	17%
11	Laborers: Metal industries	34%	1	19%
12	Operatives: Clay, glass, and stone industries	31%	1	15%
	Northeast			
1	Retail workers	5%	91	15%
2	Operatives: Textile industries	42%	72	31%
3	Clerks (except "clerks" in stores)	13%	61	20%
4	Servants (except cooks)	12%	40	23%
5	Operatives: Leather industries	47%	29	29%
6	Laborers: Iron and steel industries	49%	20	24%
7	Operatives: Clothing industries	44%	16	20%
8	Stenographers and typists	18%	8	14%
9	Operatives: Cigar and tobacco factories	27%	5	15%
10	Laborers: Clay, glass, and stone industries	38%	4	21%
11	Operatives : Metal industries (except iron and steel)	29%	3	14%
12	Public service - non-laborers	11%	3	38%
13	Operatives: Rubber factories	33%	2	29%
14	Farm laborers (wageworkers)	29%	1	13%
15	Operatives: Electrical machinery and supply factories	29%	1	13%
	South			
1	Retail workers	5%	78	15%
2	Servants (except cooks)	8%	69	18%
3	Clerks (except "clerks" in stores)	11%	2	13%
4	Laborers : Food and allied industries	26%	1	26%
5	Stenographers and typists	15%	1	15%
6	Laborers : Iron and steel industries	42%	1	13%
7	Waiters	15%	1	15%
8	Laborers : Clay, glass, and stone industries	43%	1	15%
	West			
1	Retail workers	4%	56	16%
2	Servants (except cooks)	6%	10	24%
3	Farm laborers (wageworkers)	39%	10	23%
4	Laborers : Lumber and furniture industries	29%	5	24%
5	Clerks (except "clerks" in stores)	9%	5	16%
6	Oil and gas well operatives	29%	1	20%
7	Operatives : Food and allied industries	17%	1	14%
8	Fishermen and oystermen	40%	1	12%
		Total	878	

Note: This table shows the most common occupations reported by urban youth and their estimated unemployment rates in 1931 from city-level data obtained from the Special Census of Unemployment. The column "# Cities" reports the number of cities in which the occupation listed is the most common occupation within the city. The "Weight" column reports the share of youth that hold the occupation as a proportion of all city youth workers. Midwest includes the states: IA, IL, IN, KS, MI, MN, MO, ND, NE, OH, SD, WI. Northeast includes the states: CT, MA, ME, NH, NJ, NY, PA, RI, VT. South includes the states: AL, AR, DC, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, WV. West includes the states: AZ, CA, CO, ID, MT, NM, NV, OR, UT, WA, WY.

Table 21: Difference-in-Differences Estimates: Main Results

	Outcome: % Finishing Secondary			Mean(log wage)		
	(1)	(2)	(3)	(4)	(5)	(6)
Post=1 $\times$ Δ Edu. Spend	0.28	0.75**	0.75**	-0.01***	-0.01*	-0.01*
	(0.323)	(0.302)	(0.303)	(0.003)	(0.004)	(0.004)
Post=1 $\times$ Unemployment - Youth	0.66*	1.13**	1.04**	0.01***	0.04***	0.04***
	(0.387)	(0.522)	(0.514)	(0.004)	(0.006)	(0.006)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Region x Year		✓	✓		✓	✓
School Expenditure (1930) x Year		✓	✓		✓	✓
Youth Labor Share (1930) x Year		✓	✓		✓	✓
County Unemployment (1930) x Year			✓			✓
Pre period	1927-29	1927-29	1927-29	1927-29	1927-29	1927-29
Post period	1934-38	1934-38	1934-38	1934-35	1934-35	1934-35
E[Y], 1928	41.55	41.55	41.55	3.23	3.23	3.23
R-sq	0.23	0.16	0.16	0.63	0.65	0.65
N	1,944	1,944	1,944	1,280	1,280	1,280

Standard errors in parentheses

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

Note: This table presents the estimation of the difference-in-difference specification of Equation 2.5. Cohorts of size 100 or more included. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. Column (1) controls for city and cohort fixed effects while while columns (2) - (3) add region by year controls, baseline school expenditure, county youth employment share, and unemployment rate by year fixed effects. The outcome in columns (1) - (3) is proportion of cohort finishing at least 12 years of education, aggregated from person-level records from the 1940 Census. The outcome variable in Columns (4) - (6) is mean log weekly wages in 1940, obtained from the same sample as education outcomes. Standard errors shown in parentheses and are clustered at the city level. Regressions are weighed by log number of of school aged children in county as of 1930.

Table 22: Difference-in-Differences Estimates: Graduate Composition

	10th grade+ grads/cohort			12th grade grads/cohort		
	Blue %	White %	Non-native %	Blue %	White %	Non-native %
	(1)	(2)	(3)	(4)	(5)	(6)
Post=1 × Δ Edu. Spend	0.75**	0.20	0.15	0.77**	0.21	0.16
	(0.32)	(0.24)	(0.31)	(0.33)	(0.23)	(0.23)
Post=1 × Unemployment - Youth	1.05**	-0.40	0.78	0.97*	-0.30	0.16
	(0.53)	(0.30)	(0.53)	(0.50)	(0.32)	(0.40)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓
Pre period	1927-29	1927-29	1927-29	1927-29	1927-29	1927-29
Post period	1934-38	1934-38	1934-38	1934-38	1934-38	1934-38
R-sq	0.27	0.18	0.00	0.03	0.15	0.23
N	1,944	1,944	1,944	1,944	1,944	1,944

Standard errors in parentheses

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

Note: This table presents the estimation of the difference-in-difference specification of Equation 2.5. Cohorts of size 100 or more included. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. Column (1) controls for city and cohort fixed effects while while columns (2) - (3) add region by year controls, baseline school expenditure, county youth employment share, and unemployment rate by year fixed effects. The outcome in columns (1) - (3) is ratio of individuals with at least 10 years of education attainment with blue-collar fathers, white-collar fathers, and non-natives with respect to total cohort size, respectively. The outcome variable in Columns (4) - (6) is ratio of high school (or more) graduates. Standard errors shown in parentheses and are clustered at the city level. Regressions are weighed by log number of of school aged children in county as of 1930.

Table 23: Difference-in-Differences Estimates: School Quality Measures

	Teacher Wages	S/T Ratio	Term Days
	(1)	(2)	(3)
Post=1 $\times$ Δ Edu. Spend	0.067***	-0.852**	1.122***
	(0.017)	(0.405)	(0.357)
Post=1 $\times$ Unemployment - Youth	-0.025	0.852*	-0.178
	(0.024)	(0.516)	(0.659)
City FE	✓	✓	✓
Cohort FE	✓	✓	✓
Region x Year	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓
Pre period	1927-29	1927-29	1927-29
Post period	1934-38	1934-38	1934-38
R-sq	0.45	0.57	0.22
N	1,778	1,778	1,838

Standard errors in parentheses

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

Note: This table presents the estimation of the difference-in-difference specification of Equation 2.5. Cohorts of size 100 or more included. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. The outcomes in columns (1) - (3) are: mean teacher wages, the student-teacher ratio, and the length of the school year in days, respectively. The outcome variable in Columns (4) - (6) is ratio of high school (or more) graduates. Standard errors shown in parentheses and are clustered at the city level. Regressions are weighed by log number of of school aged children in county as of 1930.

Table 24: Difference-in-Differences Estimates: Heterogeneity

	High teacher edu	Low teacher edu	High WPA	Low WPA	No regulation	Regulation
	(1)	(2)	(3)	(4)	(5)	(6)
Post=1 × Δ Edu. Spend	1.125***	0.389	0.750	0.452	0.722*	0.557
	(0.364)	(0.528)	(0.495)	(0.348)	(0.388)	(0.562)
Post=1 × Unemployment - Youth	3.335***	0.134	1.092	1.127	2.121***	0.337
	(0.998)	(0.571)	(0.685)	(0.803)	(0.798)	(0.683)
City FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓
R-sq	0.805	0.772	0.795	0.776	0.766	0.809
N	990	954	1,003	941	1,133	811

Standard errors in parentheses

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

Note: This table presents the estimation of the difference-in-difference specification of Equation 2.5. Cohorts of size 100 or more included. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. Columns (1) and (2) split the sample based on average 1940 education of teachers as aggregated from the 1940 micro census (High = above 15.01 years). Columns (2) and (3) split the sample based on per capita Works Progress Administration grants at the county level (High = 55.57 dollars per capita). Columns (5) and (6) split the sample based on state-level youth labor regulation, specifically states with and without maximum daily hours imposed on 16-18 year-olds. Standard errors shown in parentheses and are clustered at the city level. Regressions are weighed by log number of of school aged children in county as of 1930.

Table 25: Estimates using Person-Level Linked Data

	P(finishing 12)			Years of education			Log wages		
	All	Within family	Excluding migrants	All	Within family	Excluding migrants	All	Within family	Excluding migrants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post=1 × Δ Edu. Spend	0.003**	0.007**	0.003	0.017**	0.062***	0.012	-0.003**	-0.002	-0.002
	(0.001)	(0.004)	(0.002)	(0.008)	(0.019)	(0.009)	(0.001)	(0.004)	(0.002)
Post=1 × Unemployment - Youth	0.016***	0.028***	0.015***	0.122***	0.157***	0.106***	0.031***	0.021***	0.034***
	(0.002)	(0.005)	(0.003)	(0.011)	(0.028)	(0.014)	(0.003)	(0.006)	(0.003)
City FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Region x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓
School Expenditure (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓
Youth Labor Share (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓
County Unemployment (1930) x Year	✓	✓	✓	✓	✓	✓	✓	✓	✓
Family FE		✓			✓			✓	
R-sq	0.051	0.706	0.068	0.054	0.735	0.069	0.291	0.686	0.294
N	1,092,693	187,764	707,569	1,092,693	187,764	707,569	843,029	119,149	548,990

Standard errors in parentheses

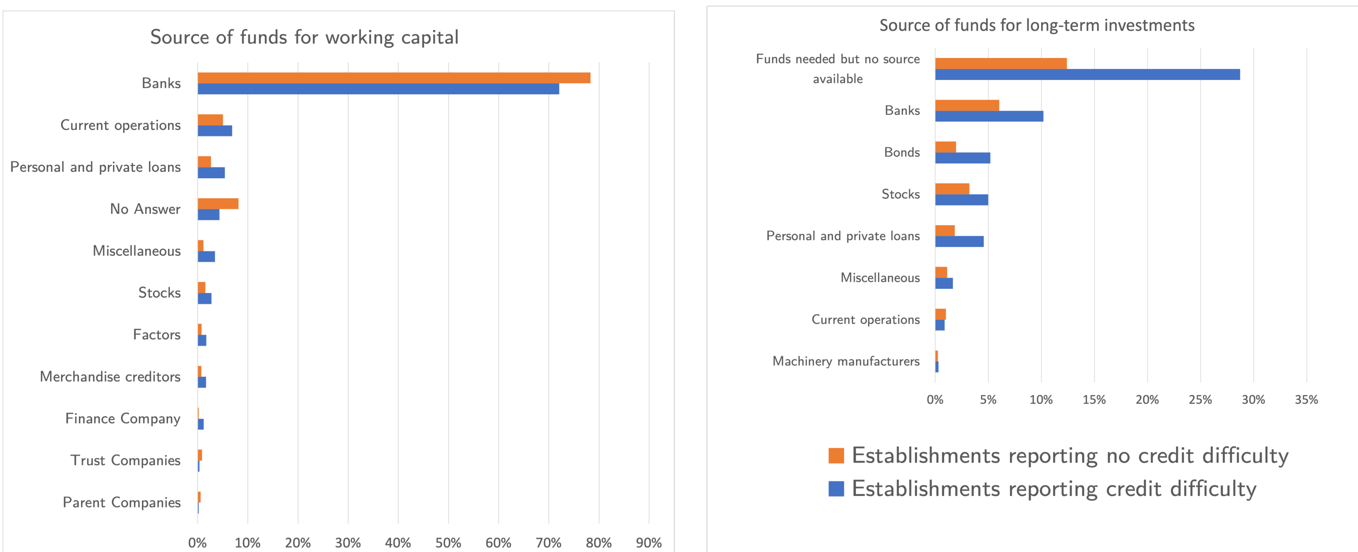
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: This table presents the estimation of a cross-sectional regression using a linked sample of 1930-1940 Census records. $Post = 1$ denotes cohorts 1933-1937 and $Post = 0$ denotes cohorts 1927-1929. Cohorts between 1930-1933 are excluded from the estimation. Columns (1), (4), and (7) use the full sample of all linked males in 564 large cities. Columns (2), (5), and (8) use only males with at least 1 other male (linked) sibling in order to control for unobserved family differences. Columns (3), (6), and (9) exclude all those who report a different city of residence in 1930 and 1940. Columns (7), (8), and (9) additionally control for labor market experience and experience squared, see text for more details. Standard errors shown in parentheses and are clustered at the cohort-city level.

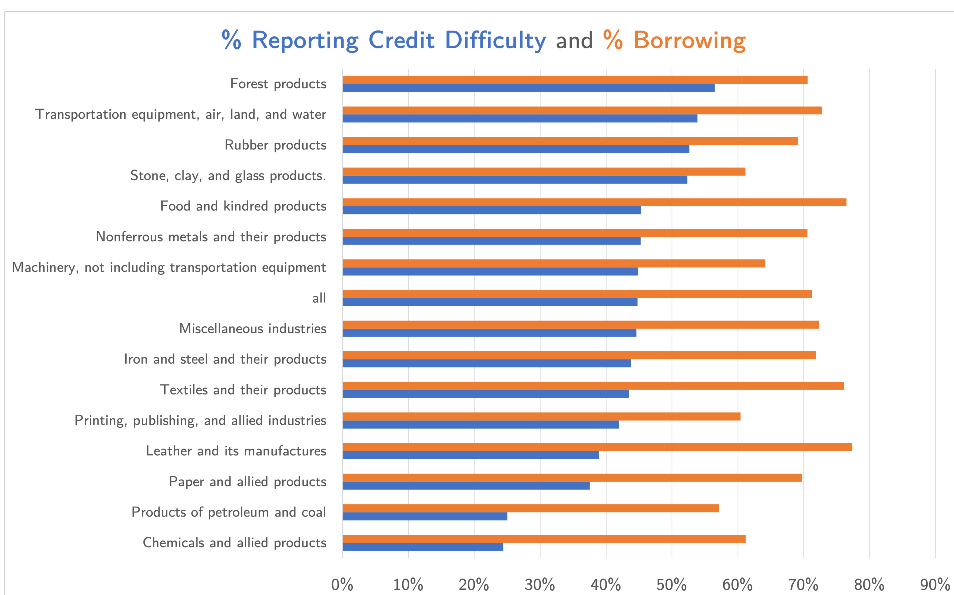
C Figures and Tables from Chapter 3

Figure C.1: Financing of Manufacturing Firms in the U.S. during the 1930s

Panel A: Sources of funds

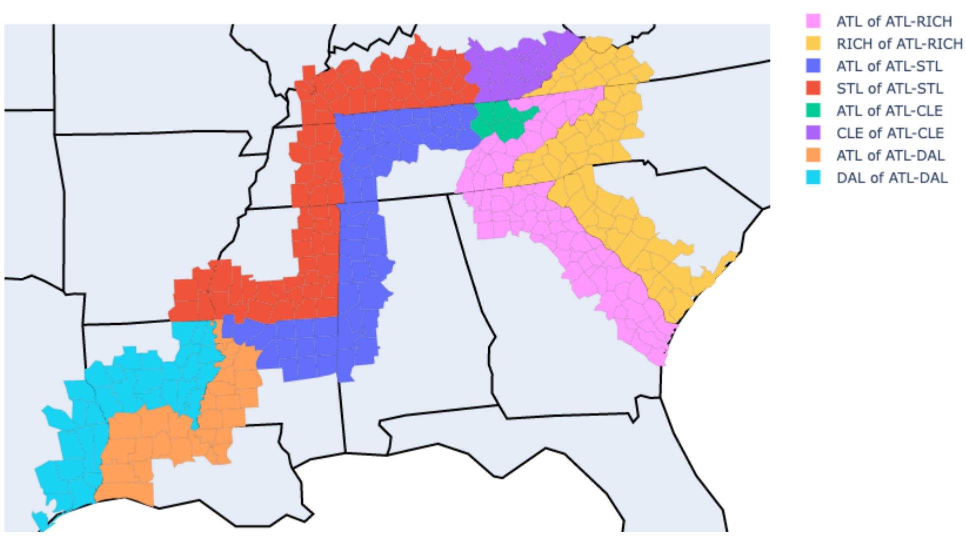


Panel B: Credit difficulty by industry in 1935



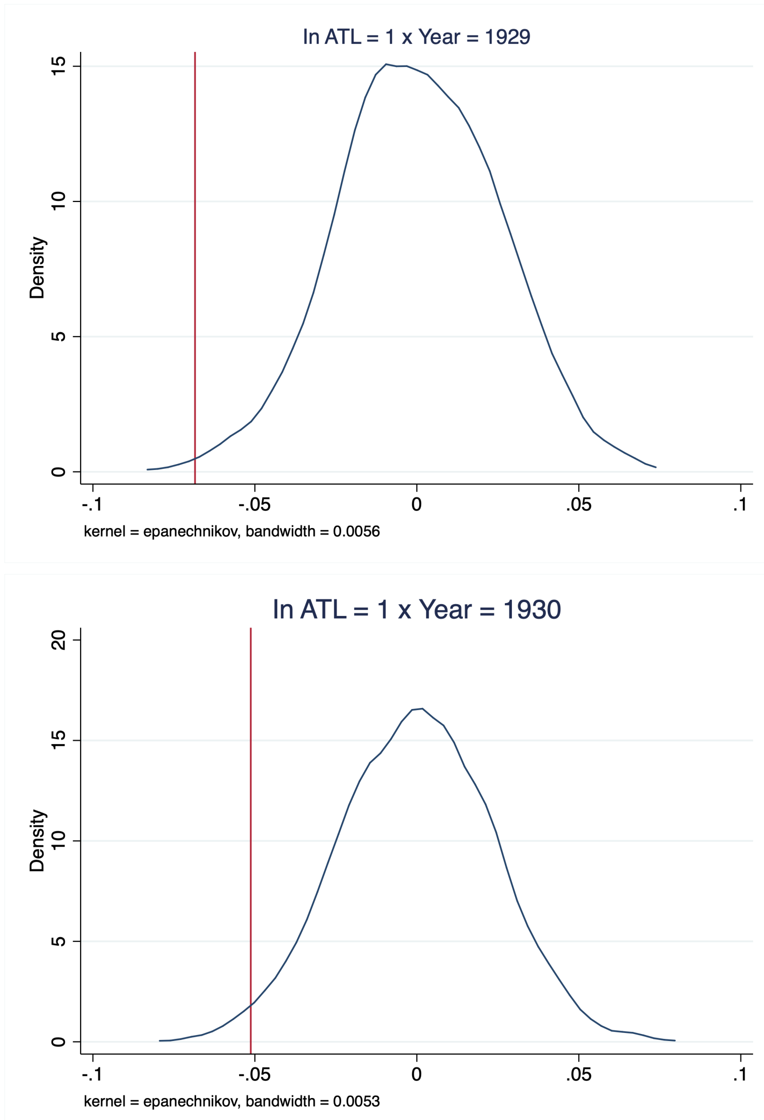
Notes: This figure presents the results of a survey of 6,158 manufacturing firms from the *Survey of reports of credit and capital difficulties* (1935) conducted by the Business Advisory Council for the Department of Commerce. See Section X for a complete explanation of the representativeness of the sample and survey collection methods. Panel A: author calculation from Table 26 of the *Survey*. Panel B: author calculation of Table 6 of the *Survey*.

Figure C.2: Sample of Counties around the Atlanta Federal Reserve District Border



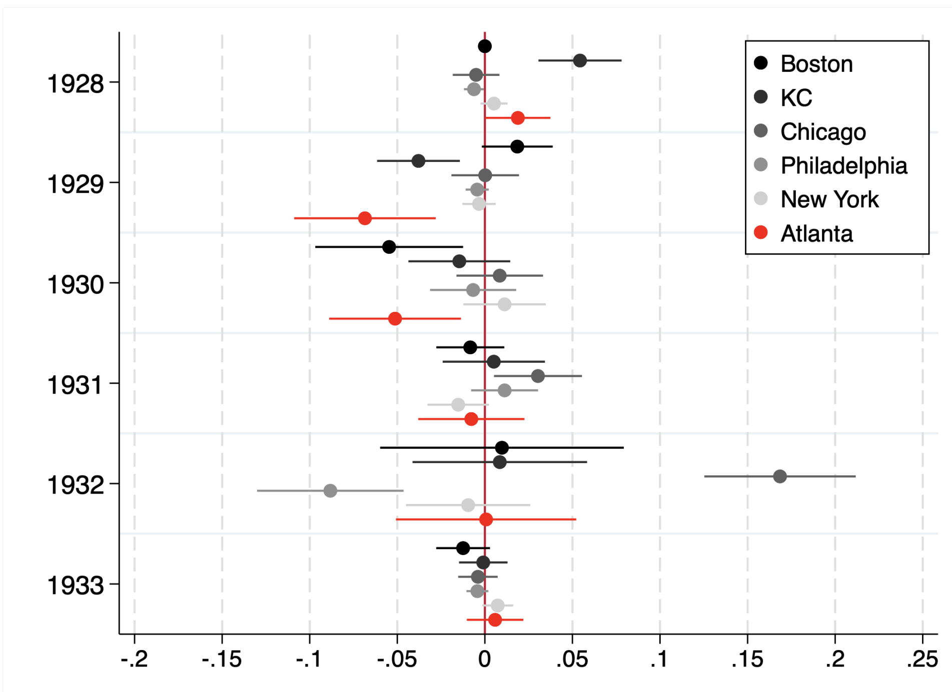
Notes: This maps shows the border regions of the Atlanta Federal Reserve district. The four border regions are: Atlanta - St. Louis, Atlanta - Dallas, Atlanta - Cleveland, and Atlanta - Richmond. The 50 mile buffer was generated using Geographic Information System (GIS) software the the 1996 Census publication of the Census bureau. Panel A not shown is ”No Answer.”

Figure C.3: Distribution of Estimated Coefficients of 1000 Placebo Border Permutations



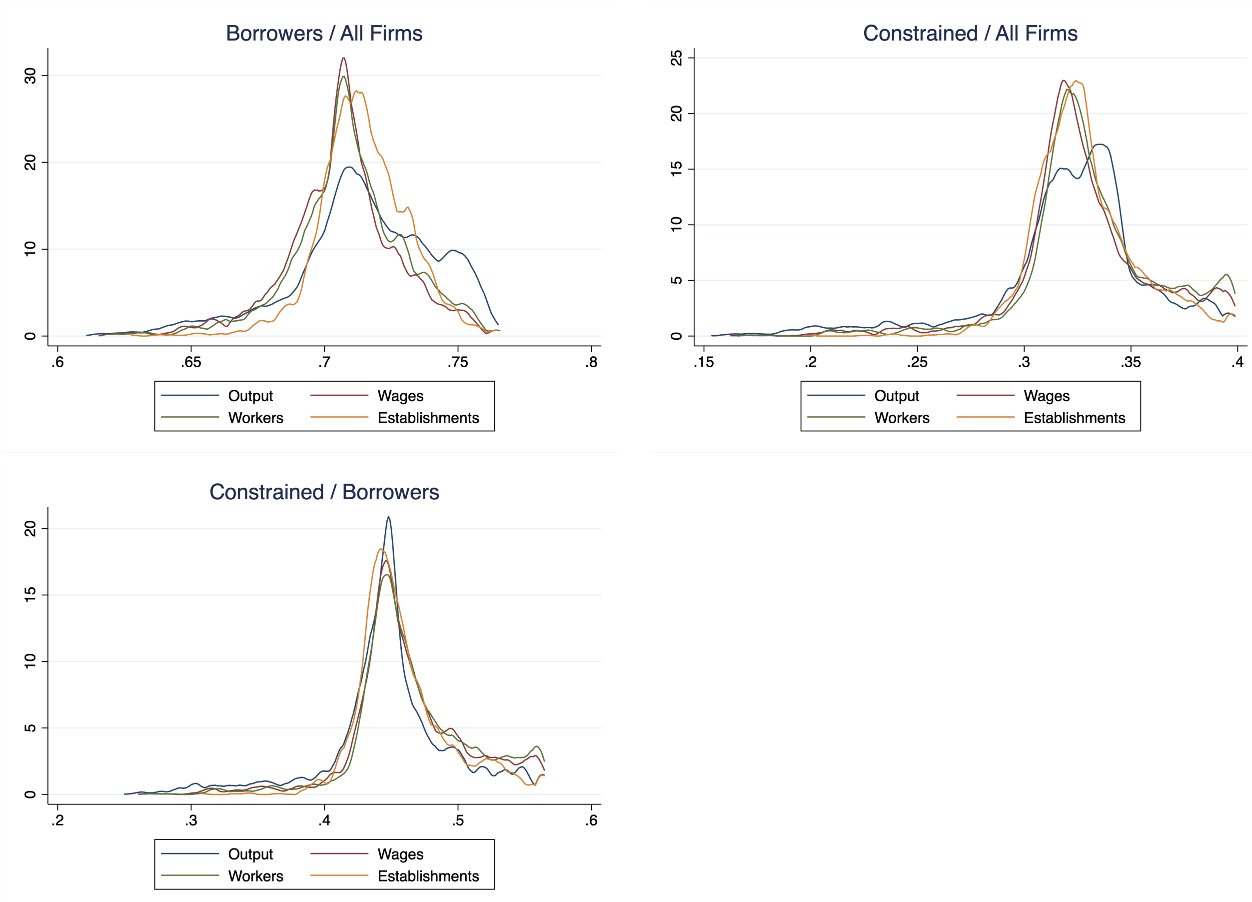
Notes: This figure plots the distribution of estimated coefficients from a stratified permutation test with 1000 replications. Border segments were permuted within border-region. Vertical line shows the point estimates using actual borders.

Figure C.4: Estimated Coefficients across Federal Reserve Bank Boundaries



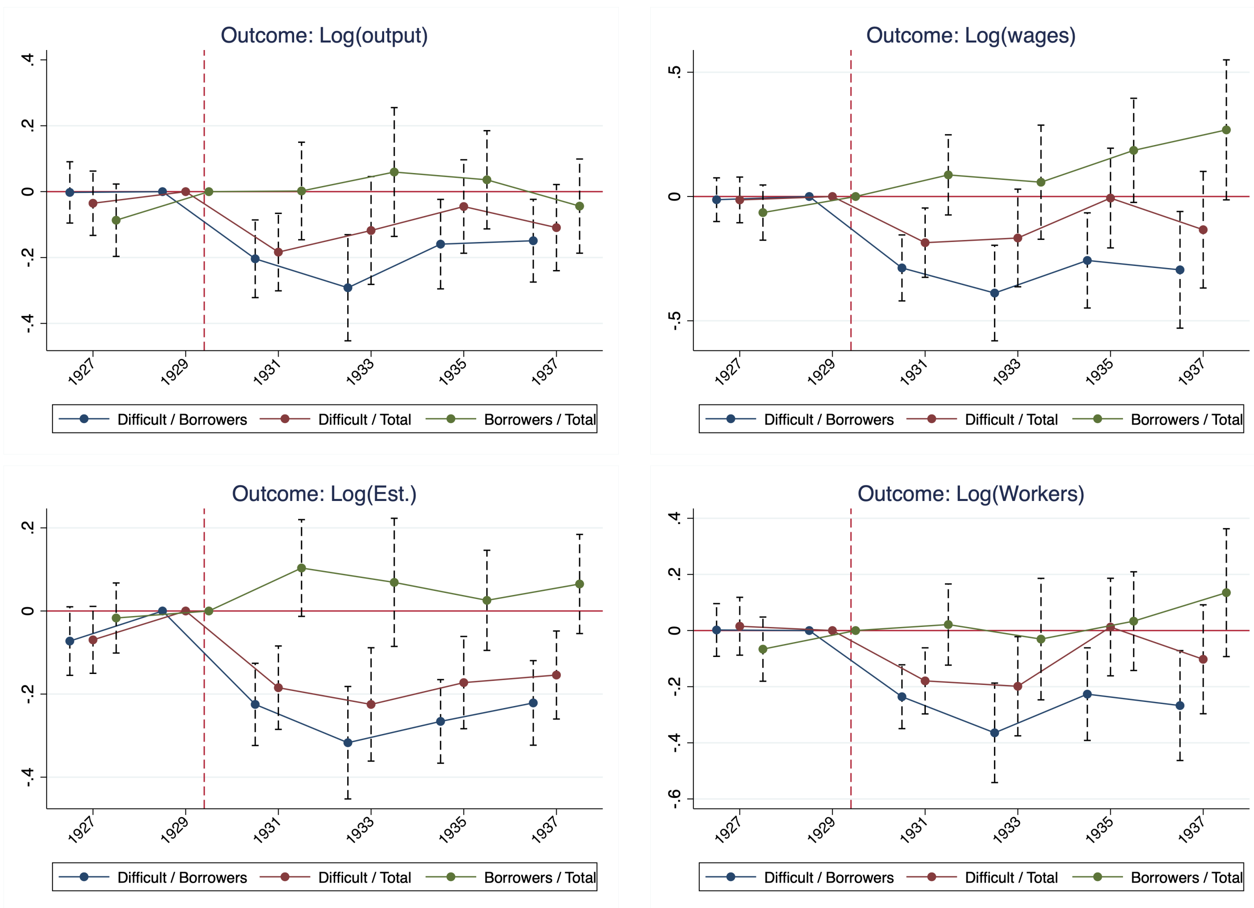
Notes: This figure plots the estimated coefficients from Equation 3.1 using different Federal Reserve border regions as samples. 90 percent confidence interval shown.

Figure C.5: Distribution of Estimated County-Level Financing Constraints of Manufacturing Firms



Notes: This figure plots the distribution of county-level financing constraints computed using industry-level measures as reported in the *Survey of reports of credit and capital difficulties* (1935) and industry by county count data as of 1927. Blue line weighs the count data by state-level average output per industry. Red line weighs the counts by state-level industry wages, while the green and yellow lines weigh it by the state-level number of workers and the state-level number of establishments by industry, respectively. ”Borrower / All Firms” is the estimated number of borrowers divided by total number of manufacturing firms within a county. ”Constrained / All Firms” is the estimated proportion of all manufacturing firms who reported being unable to find financing in 1935. ”Constrained / Borrowers” is the estimated proportion of all borrower who also report being unable to find financing in 1935.

Figure C.6: Estimated Coefficients of a Generalized Difference-in-Differences: Constrained vs. Borrowing



Notes: This figure plots the estimated coefficients of the Constrained x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. The blue line shows the estimates when Constrained is defined as the estimated share of manufacturing borrowers experiencing difficulty obtaining credit. The green lines shows them when Constrained is defined as the estimated share of all firms borrowing. Finally, the red line shows them when Constrained is defined as number of firms reporting borrowing difficulty over total firms. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. The outcome variables come from Census of Manufactures. The time period is 1927 - 1937 (biennially) for all specifications and the standard errors are clustered at the county level. 90 percent confidence intervals shown.

Table 26: Summary Statistics - Chapter 3

	count	mean	sd	p5	p25	p50	p75	p95
Banks (active - all)	4,014	3.44	2.53	1.00	2.00	3.00	5.00	8.00
Banks (suspended - all)	4,014	0.16	0.53	0.00	0.00	0.00	0.00	1.00
Banks (suspended - national)	4,014	0.03	0.18	0.00	0.00	0.00	0.00	0.00
Banks (suspended - state)	4,014	0.13	0.45	0.00	0.00	0.00	0.00	1.00
Deposits (active - all)	4,014	2.64	8.09	0.05	0.40	0.92	1.97	7.66
Deposits (suspended - all)	4,014	0.09	0.78	0.00	0.00	0.00	0.00	0.31
Deposits (suspended - national)	4,014	0.03	0.48	0.00	0.00	0.00	0.00	0.00
Deposits (suspended - state)	4,014	0.05	0.58	0.00	0.00	0.00	0.00	0.19
Capitalization ratio	184	0.15	0.06	0.08	0.11	0.13	0.17	0.25
Loan growth (1924-29)	168	0.11	0.44	-0.37	-0.04	0.10	0.26	0.62
Log(pop)	365	9.87	0.66	8.72	9.44	9.86	10.20	11.07
Unemp. rate	365	0.00	0.01	0.00	0.00	0.00	0.01	0.01
log(farm size)	365	6.59	0.33	6.06	6.43	6.60	6.76	7.11
crop fail	365	0.02	0.03	0.01	0.01	0.01	0.02	0.07
labor force	365	0.36	0.05	0.29	0.32	0.35	0.39	0.46
Products	1,204	6.47	21.89	0.14	0.58	1.47	4.15	20.93
Wages	1,204	1.03	2.44	0.02	0.11	0.29	0.83	3.95
Establishments	1,233	24.41	31.93	5.00	10.00	15.00	26.00	77.00
Workers	1,230	1.36	2.70	0.04	0.22	0.51	1.23	5.22
Difficulty/Borrowers	212	0.47	0.05	0.39	0.44	0.46	0.51	0.56
Difficulty/Total	212	0.34	0.04	0.27	0.32	0.34	0.36	0.40
Borrowers/Total	212	0.72	0.02	0.67	0.70	0.72	0.73	0.75
Observations	4224							

Notes: This table presents the summary statistics at the county-level for all counties within 50 miles of the Atlanta Federal Reserve District border (see Figure 2). Banks (count) and Deposits (millions of nominal dollars) are reported annually between 1926 and 1936. Capitalization ratio is defined as the total surplus and profits divided by assets for nationally chartered banks in 1928. Loan growth is change between the log of all loans between 1924 and 1929 for nationally chartered banks. Loans, surplus, total assets come from the Office of the Controller of Currency. The unemployment rate is defined as the total number of unemployed divided by total population in 1930. Crop fail is the proportion of all crops failed in 1930. Farm size is in acres. Labor force is the number of gainfully employed workers divided by total population in 1930. Products (millions of nominal dollars), Wages (millions of nominal dollars), Establishments (count), and Workers (thousands) come from the Census of Manufacturing, reported biennially between 1927 and 1937.

Table 27: Bank Suspension and Active Rates around the ATL Border

Panel A: Suspension rates

	Bank Suspension Rate			I(Bank Suspended)		
	All	National	State	All	National	State
	(1)	(2)	(3)	(4)	(5)	(6)
In ATL=1 × Year=1926	0.007	0.001	0.009	0.001	-0.001	-0.008
	(0.013)	(0.005)	(0.015)	(0.040)	(0.019)	(0.040)
In ATL=1 × Year=1928	0.019*	-0.032	0.025*	0.071*	-0.030	0.065*
	(0.011)	(0.029)	(0.013)	(0.040)	(0.034)	(0.037)
In ATL=1 × Year=1929	-0.068***	-0.064*	-0.070***	-0.143***	-0.077*	-0.139***
	(0.025)	(0.035)	(0.027)	(0.054)	(0.044)	(0.053)
In ATL=1 × Year=1930	-0.051**	-0.056	-0.044*	-0.004	-0.040	-0.002
	(0.023)	(0.041)	(0.025)	(0.050)	(0.052)	(0.049)
In ATL=1 × Year=1931	-0.008	0.003	-0.003	-0.032	-0.027	-0.035
	(0.018)	(0.029)	(0.020)	(0.048)	(0.044)	(0.046)
In ATL=1 × Year=1932	0.001	0.043	-0.014	-0.006	0.057	-0.034
	(0.031)	(0.059)	(0.033)	(0.056)	(0.075)	(0.054)
In ATL=1 × Year=1933	0.006	-0.013	0.007	0.033	-0.008	0.023
	(0.010)	(0.015)	(0.010)	(0.028)	(0.017)	(0.026)
R-sq	0.14	0.17	0.12	0.13	0.18	0.12
N	2,820	1,340	2,721	2,820	1,340	2,721
Year FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Panel B: Active rates

	Bank Active Rate			Deposit Active Rate		
	All	National	State	All	National	State
	(1)	(2)	(3)	(4)	(5)	(6)
In ATL=1 × Year=1926	-0.001	-0.023	-0.009	-0.004	-0.015	-0.031
	(0.020)	(0.015)	(0.026)	(0.020)	(0.025)	(0.028)
In ATL=1 × Year=1928	0.012	-0.002	0.015	0.006	0.004	0.009
	(0.015)	(0.009)	(0.019)	(0.015)	(0.028)	(0.020)
In ATL=1 × Year=1929	0.011	0.028	0.024	-0.007	0.051	0.003
	(0.020)	(0.037)	(0.025)	(0.020)	(0.049)	(0.031)
In ATL=1 × Year=1930	0.101***	0.142***	0.102***	0.073**	0.096*	0.084**
	(0.029)	(0.054)	(0.034)	(0.030)	(0.055)	(0.040)
In ATL=1 × Year=1931	0.026	0.112*	0.025	0.030	0.038	0.052
	(0.030)	(0.060)	(0.036)	(0.029)	(0.058)	(0.039)
In ATL=1 × Year=1932	0.019	0.086	0.013	0.010	0.022	0.023
	(0.029)	(0.060)	(0.035)	(0.025)	(0.050)	(0.032)
In ATL=1 × Year=1933	0.020	0.054	0.027	0.048*	0.043	0.082**
	(0.033)	(0.068)	(0.037)	(0.028)	(0.062)	(0.038)
R-sq	0.39	0.26	0.32	0.62	0.45	0.52
N	2,863	1,472	2,808	2,861	1,469	2,804
Year FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927. Outcome variables are the county x year level and indicated by the column header. "Bank suspension rate" is defined as the number of banks suspended divided by end of year total number of banks in operation. "I(Bank Suspended)" is a binary variable taking the value of 1 if at least one bank suspended operations during the year. "Bank Active Rate" is the number of banks in operation at the end of the year divided by number of banks in operation in the same county in 1927. "Deposit Active Rate" is defined analogously. "National" and "State" refer to nationally chartered vs. state chartered banks. Not all have both a national and state banks. The sample period is 1926 - 1933. The standard errors are clustered at the county level.

Table 28: Covariate Balance (Atlanta vs. Rest)

	All	ATL in	ATL out	Difference
Banks (active - all)	4.31	3.84	4.72	0.88**
	(2.95)	(2.81)	(3.02)	(0.00)
Deposits (active - all)	3188.22	3546.18	2876.15	-670.02
	(8561.38)	(11171.39)	(5347.80)	(0.46)
Log(pop)	9.87	9.78	9.94	0.17*
	(0.66)	(0.71)	(0.60)	(0.02)
Urban share	0.11	0.12	0.10	-0.02
	(0.17)	(0.19)	(0.15)	(0.35)
Unemp. rate	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.65)
log(est)	2.94	3.05	2.84	-0.21*
	(0.78)	(0.80)	(0.76)	(0.02)
log(farm size)	6.59	6.65	6.55	-0.10**
	(0.33)	(0.29)	(0.36)	(0.01)
crop fail pc	2.35	2.37	2.33	-0.04
	(3.14)	(3.63)	(2.66)	(0.89)
labor force	0.36	0.36	0.35	-0.01*
	(0.05)	(0.05)	(0.06)	(0.10)
Bank Suspension Rate (All) pc	1.25	1.02	1.44	0.41
	(5.52)	(4.87)	(6.03)	(0.48)
Difficulty/Borrowers pc	47.12	46.98	47.25	0.28
	(5.26)	(4.98)	(5.52)	(0.70)
Borrowers/Total pc	71.59	71.63	71.55	-0.07
	(2.41)	(2.32)	(2.51)	(0.83)
Difficulty/Total pc	33.88	33.80	33.95	0.15
	(3.82)	(3.61)	(4.02)	(0.77)
Observations	365	170	195	365

Notes: This table reports variable averages among counties within 50 miles of the Atlanta Federal Reserve District ("District") border. Column (1) reports the averages for all counties along the border (365) and columns (2) and (3) report them only for those in the District and for those outside the District, respectively. Column (4) computes the difference and reports the T-test on the equality of means. The variables "Banks (active - all)", "Deposits (active - all)", and "Bank Suspension Rate (All)" reported here are as of 1927 and come from the FDIC. "Difficulty/Borrowers", "Borrower/Total," and "Difficulty/Total" are estimated measures of credit access estimated using 1927 manufacturing industry by county establishment data and the 1935 credit survey of manufacturing industries. All other variables come from the 1930 U.S. Census. For detailed variable descriptions and sources, please see the text.

Table 29: Robustness: Banks Suspended (All Types)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
In ATL=1 × Year=1926	0.007	0.002	0.002	-0.006	-0.024	-0.048	-0.000	0.004	0.014	0.006	0.007
	(0.013)	(0.017)	(0.008)	(0.018)	(0.027)	(0.043)	(0.014)	(0.013)	(0.018)	(0.014)	(0.015)
In ATL=1 × Year=1928	0.019*	0.026**	0.014	0.015	0.017	0.026	0.012	0.009	0.024	0.018	0.023*
	(0.011)	(0.012)	(0.015)	(0.014)	(0.055)	(0.024)	(0.013)	(0.011)	(0.015)	(0.012)	(0.013)
In ATL=1 × Year=1929	-0.068***	-0.076***	-0.075**	-0.053*	-0.133	-0.066	-0.066**	-0.052*	-0.061*	-0.083***	-0.070**
	(0.025)	(0.028)	(0.029)	(0.029)	(0.091)	(0.052)	(0.032)	(0.030)	(0.033)	(0.024)	(0.028)
In ATL=1 × Year=1930	-0.051**	-0.047*	-0.054*	-0.038	-0.189	-0.106*	-0.039	-0.031	-0.047*	-0.053**	-0.068**
	(0.023)	(0.027)	(0.031)	(0.033)	(0.118)	(0.063)	(0.028)	(0.029)	(0.025)	(0.024)	(0.027)
In ATL=1 × Year=1931	-0.008	-0.006	0.020	0.018	-0.056	-0.020	-0.022	-0.018	0.005	-0.004	-0.013
	(0.018)	(0.020)	(0.028)	(0.026)	(0.048)	(0.048)	(0.022)	(0.018)	(0.026)	(0.019)	(0.021)
In ATL=1 × Year=1932	0.001	-0.006	0.056	0.022	0.029	0.002	-0.019	0.058	0.002	-0.016	-0.023
	(0.031)	(0.033)	(0.036)	(0.037)	(0.091)	(0.061)	(0.039)	(0.039)	(0.045)	(0.031)	(0.033)
In ATL=1 × Year=1933	0.006	-0.001	0.009	0.010	-0.031	-0.010	-0.004	0.000	0.012	0.004	0.007
	(0.010)	(0.009)	(0.007)	(0.008)	(0.030)	(0.009)	(0.010)	(0.011)	(0.013)	(0.010)	(0.011)
R-sq	0.14	0.16	0.20	0.20	0.27	0.33	0.15	0.18	0.14	0.14	0.11
N	2,820	2,504	1,331	1,672	302	907	1,805	1,765	1,715	2,605	2,375
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period balance x Year		✓									
Pre-period banking x Year			✓								
1927 industry x Year				✓							
Mississippi sample					✓						
Split consumer areas sample						✓					
Distance: 25mi							✓				
Removing border:								RICH	STL	CLE	DAL

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. Outcome variable is the bank suspension rate for all banks, defined as the number of banks suspended within the year divided by end of year total number of banks in operation. Column (1) includes no additional controls. Column (1) includes 1927 active banks, log population, log number of manufacturing establishments, log of average farm size, labor force x year fixed effects. Column (3) includes 1928 capital ratio, 1924-1929 log growth rate of national bank loans by year fixed effects. Column (4) includes a set of manufacturing industry dummy variables based on the dominant industry within the county as of 1927. Column (5) uses only Mississippi counties and column (6) uses only consumer markets that are bisected by the District border. Column (7) uses only counties within 25 miles. Columns (8) - (11) remove one border segment. The time period is 1926 - 1933 for all specifications and the standard errors are clustered at the county level.

Table 30: Robustness: Deposits Active (All Types)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
In ATL=1 × Year=1926	-0.004	-0.026	-0.016	-0.029	-0.025	0.001	-0.004	-0.003	0.003	-0.006	-0.007
	(0.020)	(0.023)	(0.018)	(0.026)	(0.055)	(0.049)	(0.024)	(0.020)	(0.028)	(0.021)	(0.023)
In ATL=1 × Year=1928	0.006	0.012	0.000	0.009	0.052	-0.048*	-0.004	-0.024	0.015	0.006	0.021
	(0.015)	(0.016)	(0.017)	(0.019)	(0.037)	(0.028)	(0.016)	(0.017)	(0.020)	(0.015)	(0.015)
In ATL=1 × Year=1929	-0.007	-0.002	-0.006	0.002	0.043	-0.007	-0.014	-0.038	0.014	-0.006	0.000
	(0.020)	(0.023)	(0.023)	(0.027)	(0.069)	(0.057)	(0.023)	(0.024)	(0.027)	(0.021)	(0.022)
In ATL=1 × Year=1930	0.073**	0.088***	0.044	0.023	0.112	0.029	0.069*	0.019	0.091**	0.080**	0.091***
	(0.030)	(0.034)	(0.035)	(0.033)	(0.082)	(0.074)	(0.039)	(0.035)	(0.040)	(0.032)	(0.035)
In ATL=1 × Year=1931	0.030	0.032	-0.005	0.009	0.069	0.028	0.020	-0.018	0.060	0.032	0.039
	(0.029)	(0.032)	(0.041)	(0.035)	(0.076)	(0.057)	(0.036)	(0.033)	(0.039)	(0.031)	(0.033)
In ATL=1 × Year=1932	0.010	0.017	-0.017	-0.005	0.064	-0.041	0.013	-0.013	0.033	0.013	0.008
	(0.025)	(0.028)	(0.035)	(0.032)	(0.045)	(0.059)	(0.030)	(0.029)	(0.034)	(0.026)	(0.028)
In ATL=1 × Year=1933	0.048*	0.049	-0.003	0.000	0.082	0.028	0.055	-0.002	0.060	0.047	0.077**
	(0.028)	(0.032)	(0.038)	(0.036)	(0.060)	(0.064)	(0.035)	(0.032)	(0.038)	(0.029)	(0.031)
R-sq	0.62	0.64	0.70	0.69	0.79	0.77	0.63	0.65	0.60	0.63	0.62
N	2,861	2,534	1,344	1,687	312	919	1,838	1,783	1,742	2,645	2,413
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period balance x Year		✓									
Pre-period banking x Year			✓								
1927 industry x Year				✓							
Mississippi sample					✓						
Split consumer areas sample						✓					
Distance: 25mi							✓				
Removing border:								RICH	STL	CLE	DAL

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. The outcome variable is bank deposits in active banks at the end of the year divided by bank deposits of active banks in the same county in 1927. Column (1) includes no additional controls. Column (1) includes 1927 active banks, log population, log number of manufacturing establishments, log of average farm size, labor force x year fixed effects. Column (3) includes 1928 capital ratio, 1924-1929 log growth rate of national bank loans by year fixed effects. Column (4) includes a set of manufacturing industry dummy variables based on the dominant industry within the county as of 1927. Column (5) uses only Mississippi counties and column (6) uses only consumer markets that are bisected by the District border. Column (7) uses only counties within 25 miles. Columns (8) - (11) remove one border segment. The time period is 1926 - 1933 for all specifications and the standard errors are clustered at the county level.

Table 31: Banking Results

	(1) Log(Loans)	(2) Log(Bonds)	(3) Log(Assets)	(4) Log(profits/surplus)
In ATL=1 × Year=1926	0.015	0.029	-0.005	0.004
	(0.041)	(0.055)	(0.039)	(0.056)
In ATL=1 × Year=1928	0.025	-0.045	-0.008	0.010
	(0.038)	(0.062)	(0.036)	(0.053)
In ATL=1 × Year=1929	0.031	-0.010	0.035	0.085
	(0.039)	(0.078)	(0.036)	(0.061)
In ATL=1 × Year=1930	0.025	-0.098	0.014	0.015
	(0.057)	(0.083)	(0.052)	(0.078)
In ATL=1 × Year=1931	0.116^{*}	0.008	0.090	0.162^{*}
	(0.065)	(0.105)	(0.057)	(0.091)
R-sq	0.15	0.16	0.12	0.07
N	1,061	1,061	1,061	1,060
Year FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Pre-period cap	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 3.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. The outcome variables come from the OCC and represents county totals for national banks only. The time period is 1926 - 1931 for all specifications and the standard errors are clustered at the county level.

Table 32: Manufacturing Results

	log(revenue)					log(wages)		log(est)		log(workers)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
In ATL=1 × Year=1929	-0.034	-0.057	-0.084	-0.084		-0.071		0.051		-0.091	
	(0.051)	(0.048)	(0.053)	(0.053)		(0.050)		(0.052)		(0.055)	
In ATL=1 × Year=1931	0.063	0.033	-0.039	-0.043		-0.112		-0.079		-0.062	
	(0.093)	(0.080)	(0.081)	(0.079)		(0.095)		(0.058)		(0.091)	
In ATL=1 × Year=1933	-0.024	-0.022	-0.066	-0.071		-0.117		-0.105		-0.063	
	(0.106)	(0.098)	(0.109)	(0.107)		(0.125)		(0.069)		(0.128)	
In ATL=1 × Year=1935	0.040	0.045	-0.100	-0.103		-0.103		-0.133*		-0.123	
	(0.097)	(0.093)	(0.095)	(0.096)		(0.127)		(0.071)		(0.108)	
In ATL=1 × Year=1937	0.107	0.141	-0.026	-0.028		-0.144		-0.131*		-0.127	
	(0.094)	(0.092)	(0.094)	(0.094)		(0.151)		(0.068)		(0.125)	
Above Median: Difficult/Borrow=1 × Year=1929				-0.004		0.012		0.059		0.007	
				(0.055)		(0.051)		(0.052)		(0.055)	
Above Median: Difficult/Borrow=1 × Year=1931				-0.212**		-0.262***		-0.170***		-0.206**	
				(0.082)		(0.097)		(0.055)		(0.091)	
Above Median: Difficult/Borrow=1 × Year=1933				-0.286***		-0.365***		-0.273***		-0.343***	
				(0.108)		(0.126)		(0.073)		(0.125)	
Above Median: Difficult/Borrow=1 × Year=1935				-0.162*		-0.250**		-0.221***		-0.221*	
				(0.095)		(0.124)		(0.069)		(0.112)	
Above Median: Difficult/Borrow=1 × Year=1937				-0.131		-0.256		-0.162**		-0.230*	
				(0.095)		(0.166)		(0.063)		(0.137)	
Above Median: Difficult/Borrow=1 × post=1					-0.219**		-0.284**		-0.239***		-0.333***
					(0.093)		(0.122)		(0.061)		(0.116)
post=1 × In ATL=1					-0.044		-0.078		-0.141**		-0.134
					(0.099)		(0.129)		(0.063)		(0.109)
Above Median: Difficult/Borrow=1 × post=1 × In ATL=1					0.046		-0.011		0.006		0.153
					(0.145)		(0.193)		(0.102)		(0.169)
R-sq	0.54	0.57	0.62	0.64	0.63	0.44	0.44	0.60	0.59	0.32	0.32
N	1,026	984	636	636	636	636	636	678	678	672	672
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
No outliers		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period banking x Year			✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 3.1 as well as a triple difference-in-difference specification where post takes the value of 1 for all years after 1929 (columns 5, 7, 9, 11). Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the omitted baseline interaction is 1927 across all specifications. The outcome variables come from Census of Manufactures. The time period is 1927 - 1937 (biennially) for all specifications and the standard errors are clustered at the county level.