Lender of Last Resort and Local Economic Outcomes[*]

Pawel Janas[†]

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Abstract

Financial crises often lead to slow economic recovery, possibly due to credit rationing by financial intermediaries. This study explores the relationship between lender-of-last-resort (LLR) policies, bank failures, and borrower outcomes during the Great Depression, using new archival data from the U.S. Focusing on a unique historical episode of divergent LLR policies across Federal Reserve banks at the beginning of the Depression, I assess the impact of LLR on manufacturing production and employment. While I find strong confirmatory evidence that these policies bolstered the banking sector, their downstream impact on firm outcomes at the plant and county levels is inconclusive, despite the manufacturing sector's documented reliance on bank credit at the time. These results highlight that LLR policies, by themselves, may not suffice for broader economic recovery after a financial crisis.

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[†]Division of Humanities and Social Sciences, Caltech, email: pawel@caltech.edu, site: paweljanas.com

1 Introduction

Financial crises have historically wreaked havoc on economies. In the U.S., the Federal Reserve's lender of last resort (LLR) policies have been a primary tool to counter systemic bank failures since the Great Depression. Most recently, the 2008 subprime mortgage crisis serves as a poignant example, where the Federal Reserve took unprecedented LLR actions and created novel ways of injecting liquidity to financial intermediaries with the stated purpose of avoiding a sharp economic downturn. However, even with banks flush with liquidity after 2009, lending to households and firms remained restricted long thereafter, possibly due to the rise in asymmetric information (Mishkin (2011)). The lack of a positive downstream spillover into the production economy during the Great Financial Crisis, however, is not evidence that LLR policies have no effect on local economies: in the case of the Great Financial Crisis, we simply do not know what would have happened in a counterfactual world.

Determining the effects of the Federal Reserve's actions in modern contexts is, thus, challenging: changes in aggregate statistics - across countries or within the U.S. - could also be the result of simultaneous and endogenous reactions by households, firms, and governments. Ideally, we would like to observe the actions of banks and firms in two otherwise similar regions: region "A" that was randomly endowed with a central bank willing to extend LLR during a crisis, and region "B" that was not. By leveraging a unique identification strategy from a historical event along with newly digitized disaggregated data on local manufacturing and banking, this paper gets close to this scenario. I show that while LLR policies averted a breakdown of the banking sector in region "A", the local economic outcomes of these regions did not significantly diverge. That is, I do not find clear evidence of an additional benefit of LLR above and beyond the one on the banking sector.

From 1929 to 1933, over half of U.S. commercial banks ceased operations. While some closed temporarily due to liquidity issues, others became insolvent and shut down permanently, and the remainder merged with other institutions to evade liquidation. This considerable disruption in the financial sector initiated over eight decades of research into the underlying causes and outcomes of bank failures (e.g., Friedman and Schwartz (1963), Bernanke (1983), Temin (1976), Wicker (2000)). A significant portion of this research delves into the Federal Reserve's role, particularly its criticized inaction early in the Depression and its inability to halt the decline in the money supply or act as a collective LLR for banks.

This study uses novel archival panel data on U.S. local manufacturing and banking conditions in the 1920s and 1930s to explore the connection between LLR policies, bank failures, and industrial output and employment. I examine the divergent policies enacted by the Atlanta Federal Reserve Bank (henceforth “Atlanta") , first brought to the literature by Richardson and Troost (2009). Atlanta, unlike other Federal Reserve banks (or the system a whole), acted as a lender of last resort within its region at the onset of the Depression, extending credit to solvent but illiquid banks to prevent runs on otherwise healthy institutions. Given that Federal Reserve borders cut across states and consumer markets, I analyze local economic trajectories before and during the Great Depression by considering quasi-exogenous liquidity-driven bank failures along the boundary of Federal Reserve regions.

My analysis proceeds in two complementary steps. In the first step, I show that LLR policies stymied bank closures by comparing bank failures between counties just outside the Atlanta border and those just inside it. I find that LLR prevented failures: banks inside the Atlanta region in 1929 and 1930 had an 8-13 percent lower failure rate. Building off of Jalil (2014), I further find that this relationship was unlikely to have been driven by other factors. It persists even after accounting for pre-existing differences in local banking conditions, excluding outliers and individual border segments, and measuring bank distress in various ways. I conduct placebo randomization tests of the Federal Reserve borders and determine that the baseline results are unlikely to have occurred by chance. Moreover, I do not find substantial evidence that bank distress varied across borders of Federal Reserve districts that adopted similar lender of last resort policies in other regions. Taken together, these results provide new evidence that banking conditions were more favorable in the early years of the Depression within the counties of the Atlanta district.

Despite the greater availability of banking resources, manufacturing outcomes were no better inside the Atlanta region. In the second part of my analysis, I turn to plant- and county-level manufacturing outcomes. By combining an industry-level credit survey with pre-Depression industry-by-county data, I construct proxies for the financial constraints of small and medium-sized manufacturers in each county around the Atlanta region border. The survey indicates that the vast majority of these manufacturers relied on commercial banks to finance both working capital and long-term investment, though the extent of these constraints differed by industry. Exploiting the geographical variation in types of manufacturing along the Atlanta border, I compare economic activity of similarly financially constrained counties with higher or lower incidence of banking failures.

I do not find strong evidence that LLR improved economic conditions during the Depression. Manufacturing outcomes, *ceteris paribus*, were no better inside the Atlanta region, despite the greater availability of banking resources. Plant-level data suggests that while plants remained operational at a higher rate in Atlanta, they produced less output and employed a smaller workforce. Additionally, while I observe that county-level financing constraints predict a 20 to 30 percent worse outcomes post-Depression (but not pre-Depression), the interaction between pre-Depression financial constraints and the banking crises of the Depression did not significantly affect local economic outcomes. Despite the Atlanta Federal Reserve's policies bolstering the banking sector early in the Depression, there was not an unambiguous positive impact on small and medium-sized manufacturing firms, even on constrained borrowers. Collectively, these results provide empirical evidence for a more nuanced policy interpretation: LLR policies, although effective at stopping liquidity-driven banking panics during financial crises, were not uniformly sufficient for economic recovery across capital-intensive sectors during the Depression. Other programs - such as direct government lending to firms and households - may have ultimately stimulated recovery more efficiently.

The underlying assumption of the production-based financial distress theory that I test in this paper is that there are no systematic differences in (1) local producers relying on local banks to finance working capital and (2) local consumers purchasing local manufacturer goods across the boundary of the Atlanta Federal Reserve Region. Otherwise, the lack of a correlation between banking and manufacturing outcomes may be due to reasons unrelated to LLR policies, such as demand shocks from other parts of the country on exporting firms. While I do not possess the firm-bank level data that would allow me to conclusively test this assumption, I am not concerned that it is prohibitively strong: the regions just-inside and just-outside of the Atlanta border are remarkably similar in their local economic structure before the Depression.

This paper contributes to our understanding of LLR policies by expanding the scope of the seminal work of Richardson and Troost (2009). Their study, along with Ziebarth (2013), demonstrated a meaningful impact of LLR on wholesale trade and manufacturing activity within Mississippi during the Depression. However, by bringing in novel data on local industrial structure, local manufacturing activity, and realized credit constrains of a sizable portion of borrowing firms in the economy (manufacturing) across the United States, I show that positive local economic impacts of LLR were not unambiguously present beyond those 82 counties. Yet, I find that the LLR policies did lead to a more robust banking sector along the entire Atlanta border, confirming the evidence first reported by Jalil (2014). Our results are complementary, as we should hardly expect the banking sector to allocate loans uniformly across industries or space. Taken together, however, it becomes evident that there is a need for a more thorough investigation of how LLR policies - then and now - may re-allocate bank credit across different industrial sectors and regions.

The remainder of the paper is structured as follows: Section 2 outlines the historical context and reviews the relevant literature. Section 3 delves into the data, while Section 4 describes the empirical strategy and the banking findings. Section 5 examines county and plant-level manufacturing outcomes, and Section 6 offers concluding remarks.

2 Historical Background, Literature Review, and Contribution

This section provides an overview of the historical and institutional backdrop of banks and firms during the Depression.[1] I then describe my contribution to this literature and introduce new survey evidence highlighting the interactions between commercial banks and small to medium-sized manufacturers.

2.1 Banking crises

The events of the Great Recession (2007–2009)—a financial crisis followed by a deep economic recession and a slow recovery—renewed interest in the study of how financial market distress affects households and firms. The Great Depression (1929–1937) serves as the most pertinent historical comparison to the economic challenges faced in the late 2000s. Initiated by a stock market crash in 1929 and exacerbated by subsequent banking failures, the Depression remains a focal point in U.S. economic history. Notably, Friedman and Schwartz (1963) and Bernanke (1983) identify bank panics as pivotal in shaping the economic downturn and determining the Depression's duration and severity. From a policy standpoint, discerning the causes of financial institution failures and the efficacy of preventive measures is crucial for financial market regulators and central banks.

Two main hypotheses underpin the causes of bank failures during the Depression in the United States. The first hypothesis posits that banks faced insolvency as the assets they held—primarily mortgages, business loans, and bonds—diminished in value (Temin (1976), White (1984), Calomiris and Mason (2003)). In essence, banks had made risky investments before 1929: excessive loans to businesses that would later collapse, to stock speculators who suffered significant losses in the 1929 crash, and mortgages during the post-World War I construction boom. Empirical support for this solvency hypothesis manifests in various ways, such as state-level bank failures correlated with economic and loan characteristics, bank-level data on loan quality, and trends in bond yields. Typically, these studies consider the probability or severity of bank suspensions, concluding that

[1] For a more comprehensive literature review, see Wicker (2000) and Temin (1976).

underlying economic shocks are potent predictors of bank failure. They suggest that liquidity-assistance policy interventions would likely have had limited impact during the Depression.

The second hypothesis, the illiquidity hypothesis, contends that a surge in withdrawals by panic-stricken depositors crippled the banking sector. The stock market crash sowed doubts about future economic prospects (Romer (1990)). Additionally, news about the failures of major, interconnected institutions stirred concerns about the banking sector's stability. Consequently, public trust eroded, leading to a rush on banks, which, unable to liquidate assets swiftly, had no choice but to halt operations (Friedman and Schwartz (1963), Wicker (2000)). Under this theory, the crisis might have been mitigated had the Federal Reserve acted as a lender of last resort, offering cash to banks in exchange for illiquid assets at non-fire sale prices. Some critiques center on the Federal Reserve's perceived inaction during the Depression based on this perspective.

Both hypotheses hold conceptual and empirical merit, contingent on the specific timeframe, geographical context, and granularity of the data. No singular cause can encapsulate the magnitude of banking panics during the Great Depression. The most insightful evidence on the solvency and liquidity channels' relative importance comes from Richardson (2007). Utilizing quarterly bank-level data from 1929 to 1933, he observes a temporal evolution in bank failures: initially, small rural banks faced increasing failure rates. The collapse of Caldwell and Company and the Bank of the United States in 1930 triggered bank runs. By 1931, following Britain's exit from the gold standard and declining asset values, the majority of failing banks were indeed insolvent. Nearly three-fourths of these institutions were deemed insolvent, with one-fourth solvent and either resuming business or merging. Among those that suspended operations, half did so due to depositor withdrawals.

2.2 What was the scope of Federal Reserve intervention?

The initial response of the Federal Reserve to the Depression has been a subject of criticism, especially its support (or lack thereof) for the money supply and banking institutions. However, pinpointing its direct impact is intricate due to potential responses from households and local bodies. The ideal approach would involve comparing regions with analogous economic trajectories but divergent policy responses during the Depression. Mississippi serves as a pertinent case study in this regard, with its northern and southern counties subjected to distinct policy regimes up until 1931 (Richardson and Troost (2009), Jalil (2014), Ziebarth (2013)).

The southern counties of Mississippi fell under the purview of the Atlanta Federal Reserve Bank (6th District). Atlanta leaders adhered to "Bagehot's rule", a doctrine advocating that central bankers should provide credit to illiquid but solvent institutions during financial upheavals, thereby preventing losses from runs on otherwise healthy banks. Gamble (1989), in his historical overview of the Atlanta Federal Reserve Bank, recounts instances where bank officials physically transported currency into banks to reassure apprehensive depositors of the bank's solvency. Conversely, in the north, the St. Louis Federal Reserve Bank (8th District) championed the "Real Bills" perspective, asserting that credit supply should diminish during downturns, as reduced economic activity necessitated less credit. This stance persisted until the summer of 1931.[2] Therefore, during the Depression's initial two years, banks in Mississippi experienced two contrasting policy approaches. What were the implications?

Drawing from bank-level and county-level data, Richardson and Troost (2009) unveil compelling findings. Banks in the 6th District counties of Mississippi consistently outperformed their counterparts in the 8th District in terms of survival rates, credit availability, and commercial activity, particularly during the panics of 1930-1931. Their research suggests that broad application of LLR policies might have mitigated the onset of banking panics. Broadening the analysis to encompass the entire border of the 6th District, Jalil (2014) investigates whether bank performance in counties within 50 miles of this boundary depended on the specific Federal Reserve policy regime. Utilizing county-level banking data, he discerns that bank suspension rates in 1929 and 1930 were consistently lower inside the 6th District compared to adjacent counties just beyond the border.[3]

2.3 Banking panics and local economic outcomes

Bank closures carry direct costs, such as the wealth loss for depositors who only recover a portion of their claims after a bank's liquidation. However, this is merely one avenue through which bank failures can adversely affect local economies. An alternative channel is the escalation of financial intermediation costs (Bernanke (1983)). In an atmosphere riddled with uncertainty, banks lean towards risk aversion, investing predominantly in secure assets and showing hesitancy in extending credit—even to creditworthy businesses (Cornett et al. (2011)). Consequently, these businesses curtail hiring and production. When these firms seek funds for investment or debt refinancing, the absence of willing lenders leads to a decline in their output.

[2]The St. Louis Federal Reserve adapted to the seasonal business cycles of its predominant industry, agriculture, by cyclically expanding and contracting credit. Amid panics, the St. Louis Fed mandated double collateral (surrendering 2 dollars of liquid assets for 1 dollar in cash), deterring banks from using the discount window (Wheelock (1997)). This policy shifted in July 1931, easing collateral requirements.

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

The experimental scenario in Mississippi, combined with the Atlanta Federal Reserve border, offers a framework to investigate the influence of bank failures on local economic conditions. Ziebarth (2013) assembles plant-level data from the Census of Manufactures during the Depression and employs a difference-in-differences approach to juxtapose plants in northern and southern Mississippi. The findings reveal a 37 percent reduction in physical output in the north with no discernible impact on total workforce numbers. This effect predominantly arises from the intensive margin. When viewed at the county level, he finds a pronounced negative impact on the number of workers.

However, a limitation of this establishment-level data is its lack of financial details, rendering it insufficient in accurately gauging the requirements for external finance. By examining a selection of prominent industrial firms—where both employment and financing needs are observable—Benmelech et al. (2019) attribute a significant portion of employment decline to financial frictions. They estimate that in the absence of these frictions, employment within these large enterprises would have surged by approximately 9–30 percent. Aligning with Ziebarth's findings, they observe a more pronounced employment drop in firms situated in counties that experienced at least one national bank failure.

On a broader scale, at the state level, Mladjan (2019) demonstrates that manufacturing sectors, which were heavily reliant on finance, faced sharper output declines compared to their counterparts. The most significant disparities emerged in states profoundly impacted by banking suspensions. The evidence suggests that these bank suspensions could account for up to a third of the manufacturing output slump during the Great Depression. To estimate external access to credit, he uses the proportion of capital expenditure not funded by cash flow from operations, segmented by industry.

2.4 Contribution to the literature

My contribution to the empirical banking and financial constraints literature is twofold. First, to the best of my knowledge, this is the first study of divergent LLR policies that uses the entire geographical scope of local economic outcomes in the quasi-experimental setting of the Atlanta Federal Reserve border region. While both Richardson and Troost (2009) and Ziebarth (2013) focus on Mississippi, it is important to understand whether (and why) the positive LLR impacts they find are externally valid outside of this mostly rural state. On the contrary, I fail to find strong evidence that a broad positive downstream implication of LLR is warranted. I further build on the work of Jalil (2014), who was the first to find systematic evidence in favor of a stronger banking sector within Atlanta across the entire border. In the first part of the paper, I investigate the robustness of his results and incorporate a host of local time-varying controls on both the banking and industry side that could account for the observed outcomes, address concerns about sample selection, and conduct placebo tests that all point towards a causal interpretation. Second, unlike the existing studies on Atlanta, I explicitly account for financial constraints in my analysis by incorporating new (Depression) survey evidence with (pre-Depression) industry location data. Since small and medium sized manufacturers relied on bank credit, we would expect to see relatively better outcomes for firms in financially constrained counties in Atlanta. Like Benmelech et al. (2019), I find strong evidence of credit rationing and employment decline due to constraints, but at the county-level across the entire region. Unlike their study, however, I do not find a noticeable heterogeneous effect: financially constrained counties in banking-rich regions (Atlanta) performed no better than those that experienced more bank failures (outside Atlanta). Again, this finding points to a limited effect of LLR on non-bank sectors.

2.5 New evidence on bank lending: the 1935 Survey of Credit Conditions

Were firms credit rationed during the Depression? For small and medium-sized manufacturing businesses, the answer leans towards the affirmative. In reaction to claims from leaders of these manufacturing units—allegations that banks were withholding loans and that accessing credit for both working capital and long-term needs was an uphill task—the U.S. Commerce Department embarked on a credit conditions survey in 1935. These leaders argued that this credit scarcity was stalling industrial recovery. Designed by the U.S. Census Bureau, questionnaires were dispatched to all manufacturing entities employing an average of 30–190 wage earners, as recorded in 1933. Of the 16,500 firms approached, over 46 percent responded, with 6,158 responses deemed suitable for analysis. Within this subset, 71 percent were identified as capital borrowers, and 45 percent of these borrowers reported difficulties in securing credit (U.S. Census Bureau (1935)).

Figure 1 shows the survey's primary findings, segmented by industry. Three observations emerge from the data. First, small manufacturers exhibited a strong dependency on banks for working capital, with around 80 percent acknowledging some degree of reliance on bank loans for operational financing. Second, a significant portion of these manufacturers also turned to banks for long-term investment funds, with only a minority resorting to security markets for this purpose. Finally, a substantial segment of firms, despite their need for long-term financing assistance, found themselves in the bracket of credit-constrained entities due to the unavailability of sources.[4]

(Figure 1 around here)

3 Data

3.1 Banking

I use data on bank suspensions from the Federal Deposit Insurance Corporation (Federal Deposit Insurance Corporation (1992)) and gather new county-level data on the condition of national banks from the annual report of the Office of the Comptroller of Currency (OCC). The FDIC's county-level panel, accessible through the Inter-university Consortium for Political and Social Research (ICPSR), has been a cornerstone in prior research about the Great Depression. This dataset captures the annual total number of suspended banks, deposits within the calendar year, and deposits of operational banks as of the year's end from 1920 to 1936. The data encompasses all continental U.S. counties, excluding Wyoming.

However, the FDIC dataset does not offer insights into local banking conditions at the county level during the 1920s and 1930s, aside from suspensions. To address this gap, I digitized tables from the OCC annual reports, revealing aggregated call report statistics (assets and liabilities) of national banks at the county level (United States Office of the Comptroller of the Currency (1920-1932)).[5] The assets data encapsulates total loans and discounts, bond and securities values, total due from other banks, real estate values, and cash holdings. Liabilities data encompasses total deposits, capital stock, circulation, rediscounts, and surplus and profits. The reporting period spans the last week of March or the first week of April for all years, except 1928 (reported on February 28). I collected the data from 1924 to 1931, which was the last year of OCC county-level reporting.

[4] A potential critique of the survey results might be the notion that only financially vulnerable firms participated. Contrary to this, many small manufacturing units reporting credit challenges appeared financially robust and worthy of credit, as indicated by their current and net-worth-to-debt ratios. For instance, of the 1,964 firms that reported borrowing challenges, 42 percent had ratios of 2.0 or higher—a metric considered indicative of safe credit risks at the time. Furthermore, 23 percent boasted current ratios exceeding 3.0. The survey also highlighted that 33 percent of the total firms reporting credit challenges had net-worth-to-debt ratios of 3 or higher, with half of the respondents having ratios of at least 2.0, as shown in Tables 15 through 26 in the Survey report. The survey collated data on current liabilities, short-term notes, fixed assets, and long-term obligations.

[5] The OCC annual report can be accessed on FRASER.

3.2 County manufacturing output and spatial industry composition

Manufacturing revenue, wage-earner employment, and establishment count come from the Census of Manufactures. I digitized biennial observations from 1929 to 1935 from a special 1937 publication of the Census Bureau (U.S. Census Bureau (1937)). Additionally, I digitized the Census's special 1927 county-level tabulation as presented in the Market Data Handbook of the United States (Stewart (1929)).[6]

The Census's geographic coverage is not comprehensive. For confidentiality, the Census refrains from reporting aggregated data that may inadvertently reveal firm-level information. Establishments (individual plants or factories) reporting products valued at less than $5,000 are not included. Consequently, counties with minimal manufacturing operations are omitted from the data. Moreover, the Depression's adverse impact on manufacturing establishments caused some counties to fall below the reporting threshold in 1931, 1933, or 1935. The data coverage is not consistent throughout, though my analysis will strictly use balanced samples.[7]

The 1927 Census's special tabulation in the Market Data Handbook of the United States provides pre-Depression industrial composition. Specifically, it shows the total count of establishments per manufacturing industry in each county. I group these industries into the 15 primary manufacturing sectors outlined in the Survey of Credit Conditions.[8]

A key limitation of this data is that economic significance varies widely across industries. For instance, the average textile industry establishment employed 63 wage-earners nationwide, while the chemical industry's average was about 33 in 1927. I use state-industry averages from the 1927 Census reports to transform count distributions into revenue and employment distributions. Concretely, I multiply the county-level count shares by state-industry averages of per-establishment wages, number of wage-earners, and output. To help illustrate, consider county "A" in Michigan that reported 10 establishments in textile manufacturing and 5 in iron and steel manufacturing in 1927. If Michigan's average output per textile plant is half that of a steel plant, the estimated output shares for county "A" would be 0.5 textile and 0.5 iron and steel. For robustness, I compute analogous shares using total wages paid to wage-earners, total number of wage earners, and the

[6] To my knowledge, both of these sources are new to the literature, though some have previously used plant-level or state-level variables from the Census.

[7] For a comprehensive account of the Census of Manufactures and its coverage across the years, refer to Vickers and Ziebarth (2019)

[8] The industries include 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.

number of establishments.

Lastly, as covered in Section 2.5, I utilize the aggregated results from the 1935 Survey as industry-level metrics of financial constraints, as outlined in Figure 1, Panel B. My analysis draws from three key data points from Tables 2 and 26 of the Survey: (1) the subset of borrowers facing borrowing challenges (my preferred method for proxying for constraints), (2) the number of borrowing firms, and (3) the share of all firms struggling to borrow. Continuing the example from the previous paragraph, county A's constraint measure would equal the weighted average of (1) between textile and iron and steel industries, where the weights are determined by output shares - in this case, 0.5 for each industry. Figure 2 plots the distribution of constraints in the sample of counties, leveraging each of the mentioned data points and the aforementioned weighing schemes.

(Figure 2 around here)

3.3 Plant-level manufacturing data

The plant-level manufacturing data from the Census of Manufactures for 1929, 1931, 1933, and 1935 was first digitized by Vickers and Ziebarth (2023). From the complete 1929 plant dataset, I filter the sample to 582 plants in the Atlanta border regions that report between 30 and 190 annual wage earners and that appear in the database in all years after 1929.[9] This filtering aims to minimize the effects of measurement error and potential sample selection biases.

I then merge this plant-level data with the industry-level financing constraints measures and county-level characteristics described earlier. The vast majority of these plants operate in two industries, both possessing nearly identical constraint measures. This similarity made a nuanced analysis based on these constraints unfeasible at the plant level. However, I still utilize the plant-level data to discern variations across the Atlanta border, setting aside the constraints' differential impacts.

Figure 3 plots the survival rate of the plants in my final sample. A plant is marked as operational in year t if it is present in the database in that year. The raw data reveals that Atlanta plants survived at a higher rate throughout the Depression.

(Figure 3 around here)

[9]These thresholds align with the Credit survey discussed earlier.

3.4 Other Data

Using the Geographic Information Systems (GIS) software, I identify counties within a 50-mile radius of all Federal Reserve Districts. These bordering counties were further segmented based on their proximity to specific Federal Reserve Districts (e.g., Atlanta – St. Louis segment) as shown in Figure 4. My approach closely mirrors the method employed by Jalil (2014). I also transcribe the 1927 consumer markets map from the U.S. Department of Commerce Market Data Handbook. This map categorizes counties into 632 distinct consumer markets, which were crafted from a consumer-centric viewpoint based on an initiative by the International Magazine Company to optimize sales efficiency. The selection of trading centers was based on parameters like population, geography, economic sources, transportation, and trade channels.

(Figure 4 around here)

3.5 Summary Statistics

Table 1 presents the summary statistics for both counties (Panel A) and manufacturing plants (Panel B). Overall, county-level banking variables are available for 365 border counties across nine years and for 364 counties for a single year (1934, due to missing bank data for one county). The 1204-1230 observations for manufacturing span between 190-210 border counties reporting manufacturing activities biennially from 1927 to 1937. The aggregated call reports for the condition of national banks cover only 168 to 184 border counties.

(Table 1 around here)

Across counties, there is a pronounced negative correlation between manufacturing activity and bank failures. Figure 5 illustrates a binned scatter plot of county-level changes in the logarithm of nominal per-capita manufacturing output between 1933 and 1929 (y-axis) against the cumulative number of bank suspensions as a proportion of all 1927 banks (x-axis) in all counties with at least one suspended bank and manufacturing activity in both 1933 and 1929 (totaling 1623 counties). On average, a ten percentage point increase in bank failure is associated with 2 log point decrease in manufacturing output across counties.

(Figure 5 around here)

4 Policy regimes and banking outcomes across Federal Reserve borders

I begin my analysis by testing whether banks within the Atlanta District counties failed at a lower rate compared to their counterparts just outside the district. Corroborating the main findings of Jalil (2014), I find that the answer is a resounding "yes". This pattern is particularly pronounced in 1929 and 1930 when policy disparities between the Atlanta District and other Federal Reserve banks were most evident. I then explore the robustness of these findings.

4.1 Empirical Design

My primary outcome of interest is the rate of bank failure. At the county level, I gauge this using two metrics: suspensions and the proportion of deposits retained by active banks at year-end, with the latter assessed against pre-Depression (1927) data on banks and total deposits. Both these metrics are indispensable, as banks might recommence operations post a brief suspension. However, the suspensions dataset does not differentiate between permanent liquidations and temporary suspensions. Hence, the end-of-year deposit values, reflecting more enduring banking sector shocks, act as a supplementary measure. Additionally, I create a binary variable, which is set to 1 if any bank within the county was suspended within the year and 0 otherwise , as a mechanism to mitigate the influence of outliers using the continuous variables.

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 logarithmic 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 manufacturing revenue by industry shares in order to identify the dominant industry in each county. I interact the dominant industry dummy variable with time dummies to account for national industry trends.

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. Tables 2 and 3 shows that counties on the border of the Atlanta District were similar. Notably, the counties did not differ in their suspension rates as of 1927, had the same (estimated) proportion of manufacturing firms facing financial constraints, and their industrial structure was similar. There are some differences, but they are small. For example, although fewer banks were in the average county inside Atlanta, the total amount of deposits in 1928 was the same. There were slightly fewer manufacturing establishments on average, and the farms were smaller.

(Tables 2 and 3 around here)

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

$$S_{jk} = \alpha_j + \beta_k + \sum_{i=1926}^{1933} (Atl_j T_i) \cdot \gamma_i + X_{jk} + \epsilon_{jk} \tag{4.1}$$

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 (e.g., Atlanta-Dallas region denoted by light blue and orange in Figure 4) by year fixed effects and, in various specifications, proxies for baseline banking and manufacturing interacted with year dummies as described above. The coefficients of interest are γ_i, which capture the time-varying difference in outcome S in counties inside Atlanta compared to average outcomes within border regions. The omitted interaction year is 1927, and I cluster the standard errors at the county level.

4.2 Baseline Results

Table 4 presents the estimates of the specification in 4.1. Panel A gives the estimates of the coefficients of interest when the outcome variables are suspension rates - proportion of suspended to non-suspended banks - and Panel B presents them for active rates - proportion of active to 1927 active banks and deposits.

(Table 4 around here)

In both panels, the estimates show that county bank failure occurred at similar rates around the Atlanta border before the Depression, relative to 1927 levels and controlling for local shocks inside a border region. The results in column (1) show that banks inside Atlanta counties failed at rates 6 and 5 percent lower in 1929 and 1930, respectively, which is consistent with the results in Jalil (2014). At the mean number of banks, this translates to 0.24 and 0.2 fewer suspended banks in each year, or approximately 0.45 more banks remaining on average in a county 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 Atlanta 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 compared to banks in neighboring counties after accounting for time-invariant county level characteristics. These results are qualitatively similar to those using suspensions as the outcome variable in Panel A, and more pronounced for nationally-chartered banks, where the effect is present even at the end of 1931.

4.3 Robustness Results

Despite generally balanced counties on either side of the border and the plausibly exogenous historical placement of Federal Reserve boundaries, concerns about interpreting the results in Table 4 as the causal effects of the Atlanta Federal Reserve policies remain. Tables 5 and 6 present the results with additional controls and different samples. The stability of the baseline estimates in columns (2) - (13) provides strong evidence in favor of a causal interpretation.

(Tables 5 and 6 around here)

The first concern is that the small pre-Depression differences in county characteristics across the Atlanta border as shown in Table 2 (e.g., average farm size) could contribute to the divergence in bank failure rates during 1929 and 1930. For example, if smaller farms tend to default at higher rates on mortgages, the presence of relatively smaller farms just outside Atlanta may explain larger bank failure rates in 1929 and 1930. In column (2), I control for these pre-Depression differences by interacting the non-balanced covariates with time dummies, and I do not find any change in the baseline estimates.

The second concern is that omitted underlying differences in bank conditions – such as bank leverage or historical loan growth before the Depression – could be causing the differences in bank failures. Using the county-level OCC data on nationally-chartered banks, 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. I also compute logarithmic loan growth between 1924 and 1929. Higher loan growth could potentially correlate with decreased loan quality and a higher default rate in the 1930s. I interact both county-level measures with time dummies and include them in the specification. The results are shown in column (3) and, again, I do not find any meaningful differences with the baseline.

The third concern is that firms in counties just outside the border are in more pro-cyclical industries than are those just inside the Atlanta region and thus caused more strain on the banking sector in 1929 and 1930. To control for industry-level time-varying confounders, I use the 1927 manufacturing revenue by industry shares in order to identify the dominant industry in each county. I interact the dominant industry dummy variable with time dummies to capture dynamic effects and include them in the specification in column (4). The results are slightly attenuated but remain significant.

In columns (5) - (13) I address the remaining concerns of sample selection and standard error clustering. In column (5), I limit the analysis to the counties in Mississippi, and I find larger, but noisier, point estimates consistent with Richardson and Troost (2009) and Ziebarth (2013). Next, to account for unobserved differences in state-level policies, I limit the analysis to counties that belong to consumer market areas bisected by the Federal Reserve border.[10] I then show that potential spillovers across the region are not driving the results by estimating the specification with and without counties within 25 miles (columns 7 and 8). Column 9 reports spatial standard errors using the methods of Colella et al. (2019) and 100 kilometer cutoff distance. Finally, columns 10 - 13 exclude outside of the specified border region. I find quantitatively similar results even after these counties are excluded.

[10] With the exception of Mississippi and Tennessee, the border runs along state lines.

4.3.1 Placebo tests

Next, I conduct a randomization test in which I resample placebo borders within each border-region of all counties in the sample using tools developed in Heß (2017). Specifically, at each permutation, the share of counties inside the Atlanta region in each border-segment remains fixed, but each individual county is randomly chosen to be either "in Atlanta" or "out of Atlanta." I do this 1,000 times and estimate equation 4.1 using the main outcome variable, bank suspension rates, and collect the estimated coefficients in γ for 1929 and 1930. I plot the two distributions in Figure 6. The vertical lines indicate the baseline effects estimated using the true Atlanta Federal Reserve borders. 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. These results provide supporting evidence that the estimated baseline impacts of the differing policies of the Atlanta Fed on banking failures is unlikely to have occurred by chance.

Instead of permuting counties into placebo borders, I extend the analysis to actual border counties in other Federal Reserve regions. If the differences in LLR are driving these outcomes – and the robustness exercises have 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? Using the border-regions of the Kansas City, Dallas, Cleveland, Richmond, and St. Louis Federal Reserve Bank regions, I re-estimate the main specification and plot the results in Figure 7. The outcome variable is the total bank suspension rate. The variable Atl_k in 4.1 is now defined as follows: it takes the value of 1 if the county is located inside the Federal Reserve district specified and 0 if it is located in the border region of that same district but outside the border. For example, the Cleveland regression uses all the Cleveland border regions, and all counties within the Cleveland region around the border are assigned $Atl_k = 1$ and those just outside Cleveland are assigned $Atl_k = 0$. In the same figure, I plot the Atlanta and St. Louis estimates in red and purple, respectively, for reference. While some coefficients are statistically large - namely, Dallas and Cleveland counties in 1932 and Richmond counties that appeared worse off throughout - the baseline results for the Atlanta district are still relatively larger in 1929 and 1930 than in the other regions. Taken together with the permutation results above and the host of robustness exercises in the preceding section, it is likely that banking conditions in Atlanta were more favorable because of LLR policies.

(Figure 7 around here)

4.3.2 What happened to bank lending?

While the results in the previous subsection provide evidence that the incidence of bank failure differed significantly based on Federal Reserve policies, they say little about how the non-failed banks responded. Banks may respond to local banking panics by refusing to lend and, instead, amassing liquidity and safe assets like government bonds (Cornett et al. (2011)). Ideally, a full panel of national and state-level bank detailed balance sheets would be available through 1937. However, the extant data from the OCC is for national banks, contains only aggregated information on all lending activity, and stops in 1931. Even with this limited sample, I can investigate broad differences in the county-level composition of assets and liabilities for national banks across the Atlanta border.

Table 7 presents the estimates of specification 4.1 using the available OCC data between 1926 and 1931. Columns (1) - (5) use log total loans, book value of bonds, assets, surplus, and number of banks as outcome variables while columns (6) - (9) use the same variables on a per-bank basis. The result in column (1) shows that national banks had in total, on average, 11 percent more outstanding loans as of 1931 inside the Atlanta border region, as compared to those banks in counties outside the border. These loans include all loans - mortgages, government loans, as well as business credit. They did not, as columns (2) and (3) show, own more bonds or have more assets, but they did report more surplus. Turning to the results in column (6) - (9), I find that the average national bank in Atlanta invested in fewer bonds as of 1930, but in general the differences on the intensive margin are imprecisely estimated.

(Table 7 around here)

5 Did LLR lead to better local economic outcomes?

I have shown so far that the commercial banking sector inside Atlanta fared relatively better during the first two years of the Depression than it did just outside it due to LLR policies. I now turn to local economic outcomes and explore downstream effects on firms.

5.1 County Level Results

The empirical strategy is unchanged from the one described in the previous section: using a difference-in-differences design, I compare manufacturing outcomes between counties 50 miles within and outside the Atlanta border, before and after the onset of the Great Depression, accounting for time-varying border-region confounders. I add, however, an additional explanatory variable to the specification: the average estimated credit difficulty at the county level interacted with year fixed effects. This variable measures the (county-average) difficulty in obtaining credit by small and medium-sized manufacturers. Econometrically, the inclusion of this variable provides two crucial benefits to the analysis. First, it captures the direct association of pre-Depression industrial composition and local economic outcomes during the Depression, limiting omitted variable bias in the estimation of the impact of LLR policies. Second, the interaction of financial constraints and LLR policies is itself a testable hypothesis: counties with firms in more constrained industries should have better outcomes just inside Atlanta as compared to their similarly constrained counterparts outside of Atlanta.

To ease interpretation, I code the variable $constraint_j$ as a binary variable taking the value of 1 if county j is above the median and zero otherwise. The omitted interaction year is 1927 in all specifications, and standard errors are clustered at the county level. I estimate the following augmented model:

$$S_{jk} = \alpha_j + \beta_k + \sum_{i=1926}^{1933} (Atl_j T_i) \cdot \gamma_i + \sum_{i=1926}^{1933} (constraint_j T_i) \cdot \omega_i + X_{jk} + \epsilon_{jk} \tag{5.1}$$

where the variable definitions are the same as in Equation 4.1.

I also test whether the higher incidence of banking failures in counties just outside Atlanta amplified the impact on financially constrained manufacturing firms. I conduct a difference-in-difference-in-differences analysis by comparing geographically across the Atlanta border, below and above median estimated credit difficulty, and across years in the following specification, where the variable $post_k$ takes the value of 1 for all years k after 1929:

$$S_{jk} = \alpha_j + \beta_k + (constraint_j \times Atl_j \times post_k) \cdot \nu + X_{jk} + \epsilon_{jk} \tag{5.2}$$

Table 8 shows the estimated coefficients of Equation 5.1 using manufacturing outcomes. Columns (1) - (4) use log output (revenue), log county manufacturing wage-earner wages, log number of establishments, and log number of wage-earners as the outcome variable, respectively. To ensure that the results are not driven by a handful of outliers, I discard counties in the bottom two or top two percentiles in the change in manufacturing revenue between 1929 and 1931. I control for differences in pre-period banking by interacting the county-average capitalization ratio and log loan growth between 1924 and 1929 of national banks with year fixed effects. Finally, to account for differences in business failure rates based on firm size, I include terciles of average manufacturing plant size (defined as number of workers divided by number of plants in 1929) by year fixed effects. In columns (5) - (8), I add the estimated credit difficulty by year fixed effects to the specification.

(Table 8 around here)

Unlike the banking suspension and lending results shown so far, I do not find evidence that local manufacturing fared better inside Atlanta. 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 show a 3 to 10 percent decrease in annual revenue for the manufacturing sector in the Atlanta counties, though noisily estimated. In columns (5) - (8), I do find that credit difficulty correlated negatively and significantly with manufacturing output after, but not before, the Depression started. The estimated effects are all highly significant and stable across the outcome variables, implying that counties with estimated above median credit difficulties had outcomes 20 to 30 percent lower than those without difficulty. In essence, these results substantiate the claims of business leaders who urged the Commerce Department to conduct a study of bank lending to businesses during the Depression.

Is this an effect of credit rationing or an association with more capital-intensive and borrowing industries in these counties? Using the total share of small and manufacturing plants that reported any borrowing - as opposed to those that reported difficulty in borrowing - I find that the estimates in Table 8 are not driven by differential credit demand across counties. Figure 8 plots the difference-in-differences estimates ω_i in three separate regressions using total borrowing firms, total firms with credit difficulty, and total borrowers with credit difficulty as a proportion of all firms as $constraint_j$. Each variable is defined as an indicator taking the value of 1 if the county is above the sample median, and 0 otherwise. Across all outcomes, the figure shows that borrowing propensity (green line) cannot explain the differences in outcomes - what matters is the willingness of banks to extend credit (red and blue lines). In terms of output and the amount of wage-earners employed, the point estimates when are close to zero when $constraint_j$ is defined as above/below median in the proportion of total borrowers. For the number of establishments and total wages, the estimates are positive in some years, but not statistically significant at the 90 percent confidence level.

(Figure 8 around here)

To conclude the county-level results, I directly test whether financially constrained firms performed better inside Atlanta by estimating the triple difference specification in Equation 5.2. Table 9 reports the estimation results. The small and statistically insignificant coefficient estimates of ν across the four aforementioned outcomes in the last row imply that manufacturing activity of constrained manufacturing firms was not better in Atlanta. Even though LLR policies improved banking conditions early on in the Depression in the region, I do not find positive spillovers to bank borrowers. This finding, along with those in Table 8, imply that the impact on local manufacturing of LLR policies enacted by the Atlanta Federal Reserve was negligible.

(Table 9 around here)

5.2 Plant Level Results

One concern with the county level analysis is that it is unable to distinguish, or control for, important heterogeneity with respect to firm size or internal capital markets. To shed light on these factors, I use disaggregated plant-level Census of Manufacturing data, which contains information on both size and subsidiary status. My sample contains all small- and medium-sized plants in the Atlanta border regions that appear in 1929. To limit measurement error, I discard all plants that re-appear in the dataset in any year after 1929 after being absent at least once in any intervening years. That is, the sample only includes firms that appear in all years of the Census, or that appear in 1929 and in one or two consecutive years thereafter (1929 and 1931, or 1929 and 1931 and 1933).

As with counties, I compare plant-level outcome variables before and after the onset of the Great Depression across the Atlanta boundary. Using an expanded balanced panel, I define a binary outcome variable *survive* that takes the value of 1 if the firm appears in the dataset and 0 otherwise. The other outcome variables remain the same as in the county-level analysis. I then estimate the following specification:

$$S_{i(j)k} = \alpha_i + \beta_k + \sum_{o=1929}^{1933} (Atl_j T_o) \cdot \gamma_i + X_{i(j)k} + \epsilon_{i(j)k} \tag{5.3}$$

where T_o is a year dummy taking the value of 1 if $o = k \in \{1931, 1933, 1935\}$ and 0 in 1929 and Atl_j takes the value of 1 if the firm operated in county j located in the Atlanta District and 0 otherwise. I use firm ($\alpha_{i(j)}$) and year (β_k) fixed effects to account for all unobserved but static firm variables and national trends. The control variables in X_{jk} include border-region by year fixed effects, firm size (in 1929) quartile by year fixed effects, and industry by year fixed effects. The coefficients of interest are, again, γ_i, which capture the time-varying difference in outcome S in firms inside Atlanta compared to average outcomes within border regions. The omitted interaction year is 1929, and I cluster the standard errors at the county level.

Table 10 reports the results in two separate panels: panel A includes all plants, while panel B excludes subsidiaries. I conduct the analysis in this way because subsidiaries could plausibly borrow from their own internal capital markets. For these plants, local banking conditions may not constrain production as much as for stand-alone plants. Furthermore, I report the estimates separately for the fully balanced sample of plants in odd columns (those that survived the Depression and met the reporting threshold of the Census every year) as well as for the full sample of firms in the even columns. The differences between the odd and even columns thus reflect the impact of survivorship bias on the estimates.

(Table 10 around here)

In column (1), I report the results for the survival dummy. In columns (2) and (3), the outcome variable is the logarithm of total output; in (4) and (5), it the logarithm of total payroll of wage-earners; and in (6) and (7), it the logarithm of total number of wage-earners. Consistent with Figure 3, I find that plants in the Atlanta region did, in fact, survive at 7-14 percent higher rates during the Depression than those plants just outside Atlanta even after controlling for observable confounders. Interestingly, however, the results for the other outcome variables after accounting for survivorship bias across all samples show worse outcomes: the point estimates are statistically indistinguishable from zero or negative in even columns. As expected, the point estimates in the balanced sample are more positive for the most part, but still not indistinguishable from zero. Even after controlling for size and other sources of capital, these plant-level results are consistent with the county-level results of *negative* or null results on wages, value of output, and the number of wage earners. The higher survival rates in the Atlanta border regions did not manifest in more local economic activity.

6 Summary

This paper used novel archival panel data on local manufacturing and banking conditions in the United States to investigate the link between LLR policies, bank failures, and, ultimately, firm production and employment. Using the divergent policies enacted by the Atlanta Federal Reserve Bank as the 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 LLR policies from the Atlanta Federal Reserve stymied banking panics.

However, I do not find positive spillovers of LLR on bank borrowers - local manufacturers. I combined industry-level credit survey and 1927 industry-by-county data to construct measures of financial constraints for each county. Using county and plant-level manufacturing data, I fail to find evidence to support positive downstream impact of LLR on employment and output. These results contrast with the results of the existing literature on the benefits of LLR, which only focus on the state of Mississippi or use other industries in their analysis. Manufacturing outcomes were worse, not better, in counties inside the Atlanta region, despite having more banking resources. Furthermore, I find strong evidence that the county-level financing constraints predict worse outcomes after, but not before, the Depression. The interaction between pre-Depression measures of financial constraints and banking panics during the Depression was not an important determinant of local economic outcomes, however. Said plainly, even though LLR policies bolstered the banking sector, they did not significantly and unequivocally help alleviate borrowing constraints for firms seeking to finance their production.

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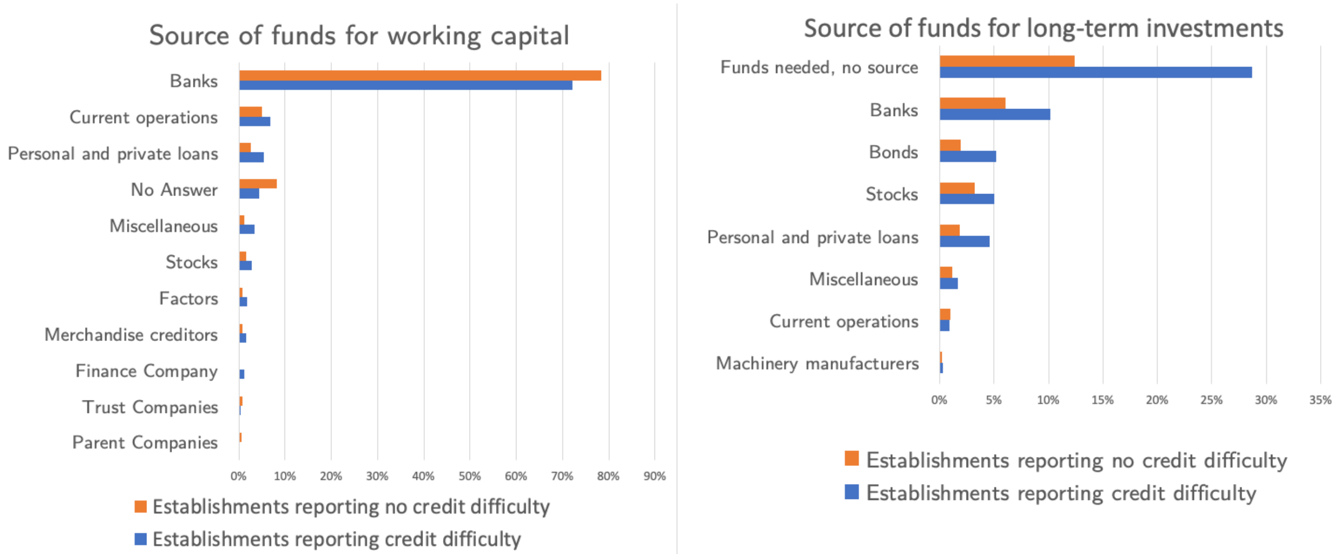
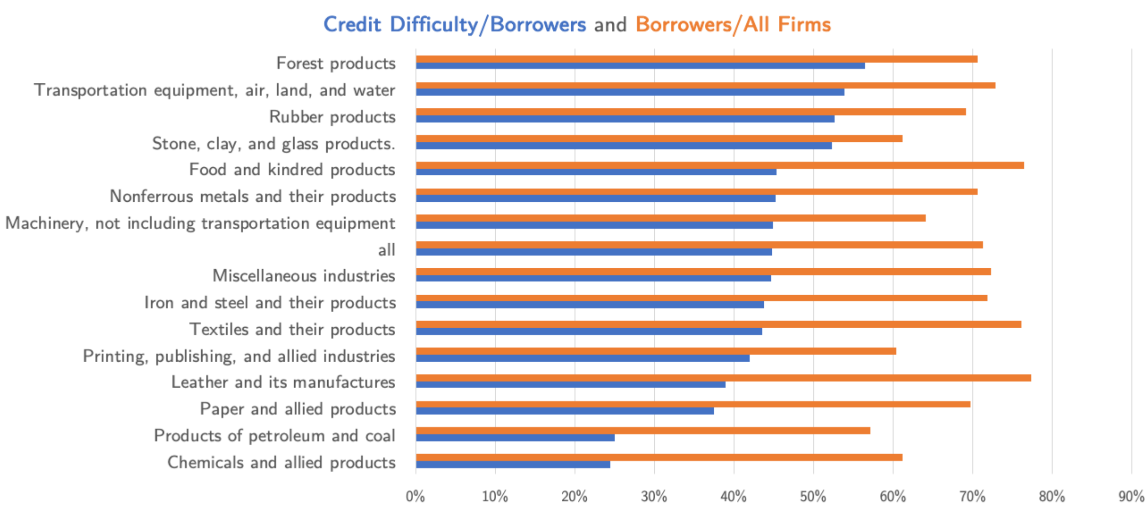
Figure 1: Financing of Small and Medium U.S. Manufacturers, 1935

Panel A: Sources of funds

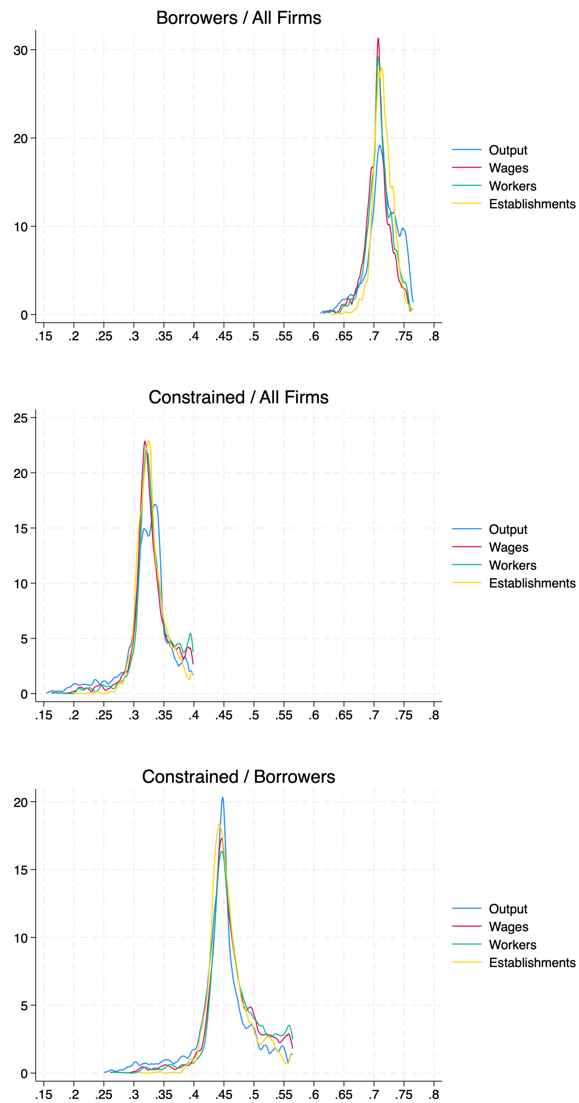


Panel B: Credit difficulty and demand by industry



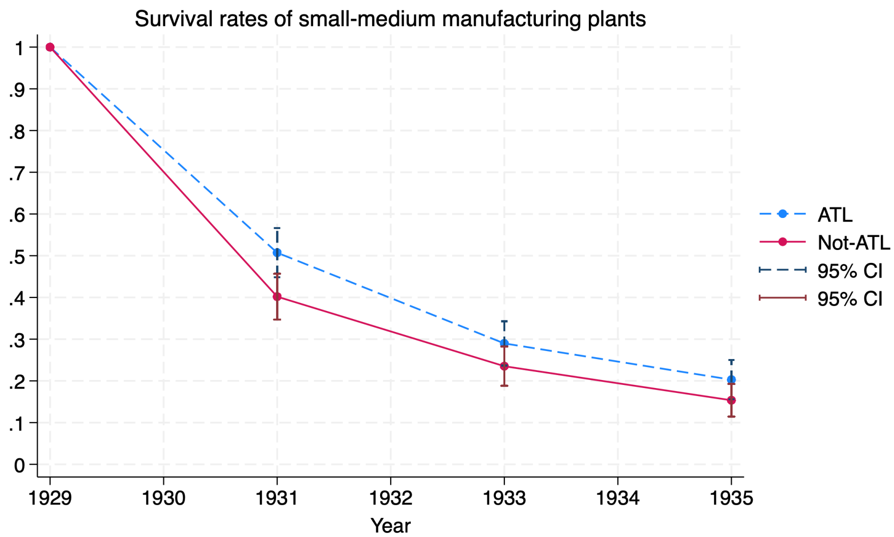
Notes: This figure presents the results of a survey of 6,158 manufacturing firms from the *Survey of reports of credit and capital difficulties* (1935) conducted by the Business Advisory Council for the Department of Commerce. See Section 2.5 for a complete explanation of the representativeness of the 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 2: Distribution of County-Level Financing Constraints of Small - Medium Manufacturers



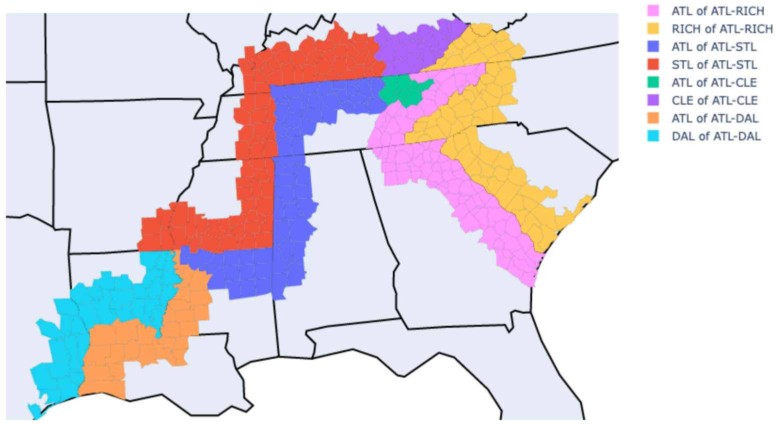
Notes: This figure plots the distribution of county-level financing constraints computed using industry-level measures as reported in the *Survey of reports of credit and capital difficulties* (1935) and industry by county count data as of 1927. Blue line weighs the count data by state-level average 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 borrowers who also report being unable to find financing in 1935.

Figure 3: Survival rate of small-medium manufacturing plants in the border regions



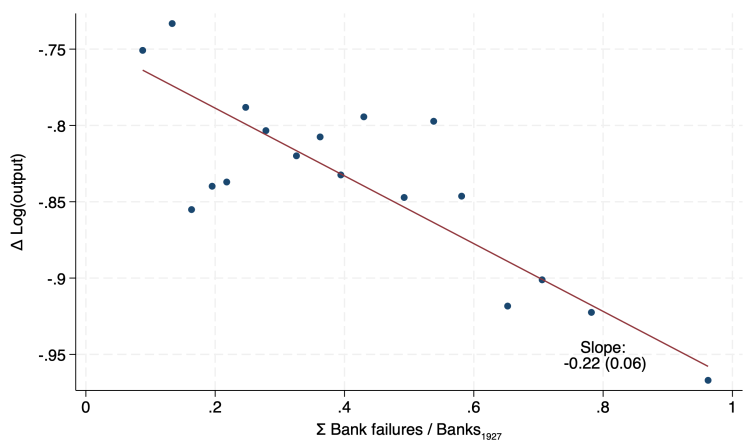
Notes: This figure plots the survival rate of small-medium sized manufacturing plants in the Atlanta Fed border region in the early 1930s. Dashed lines denotes the proportion of firms inside the Atlanta Fed region, and the solid line denotes it for firms just outside the region. 95 percent confidence intervals denoted by the bars. See text for description of the data source.

Figure 4: Counties around the Atlanta Federal Reserve District Border



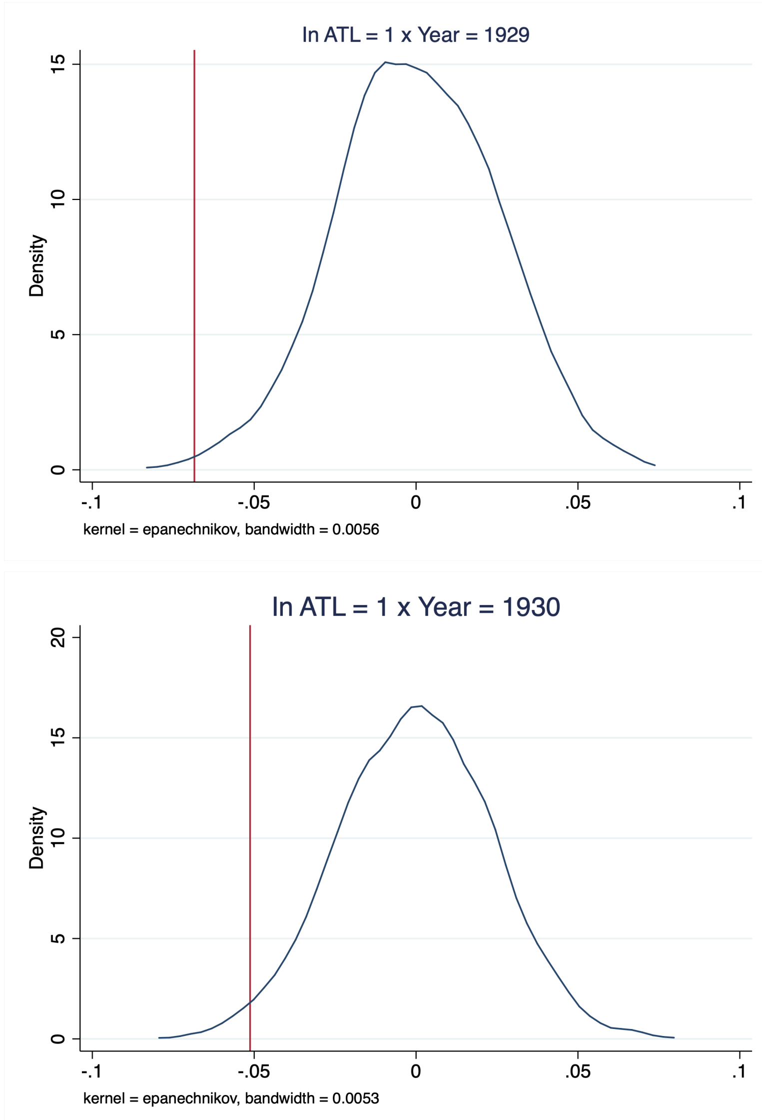
Notes: This maps shows the border regions of the Atlanta Federal Reserve district. The four border regions are: Atlanta - St. Louis, Atlanta - Dallas, Atlanta - Cleveland, and Atlanta - Richmond. The 50 mile buffer was generated using Geographic Information System (GIS) software.

Figure 5: Manufacturing decline during the Great Depression and banking failures



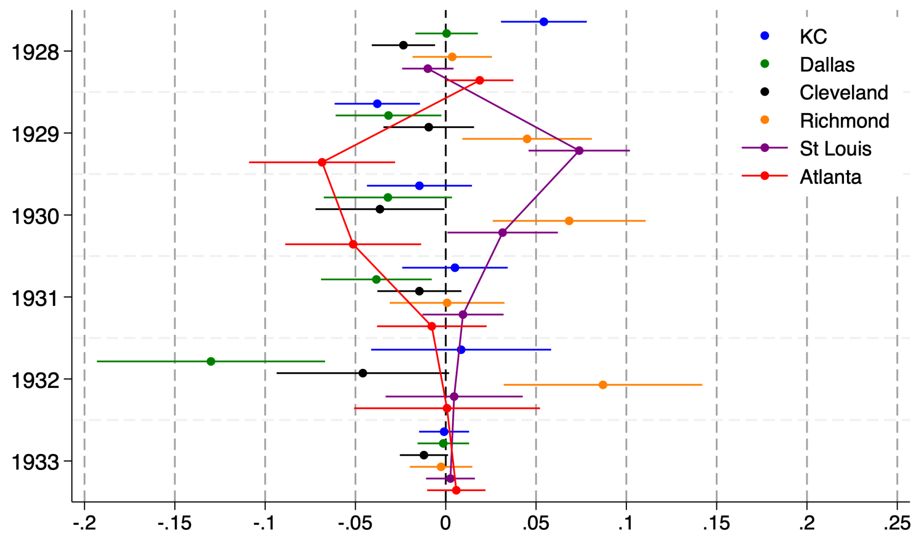
Notes: This figure plots a binscatter plot of county-level change in the log of manufacturing output between 1933 and 1929 (y-axis) and the cumulative number of bank suspensions (1929-1933) as a proportion of all banks in 1927 (x-axis). Only the counties with at least one suspended bank and manufacturing activity in 1933 and 1929 are included (1623 counties). Manufacturing data comes from Census of Manufactures and the banking data comes from the FDIC.

Figure 6: Distribution of Estimated Coefficients of 1,000 Placebo Border Permutations



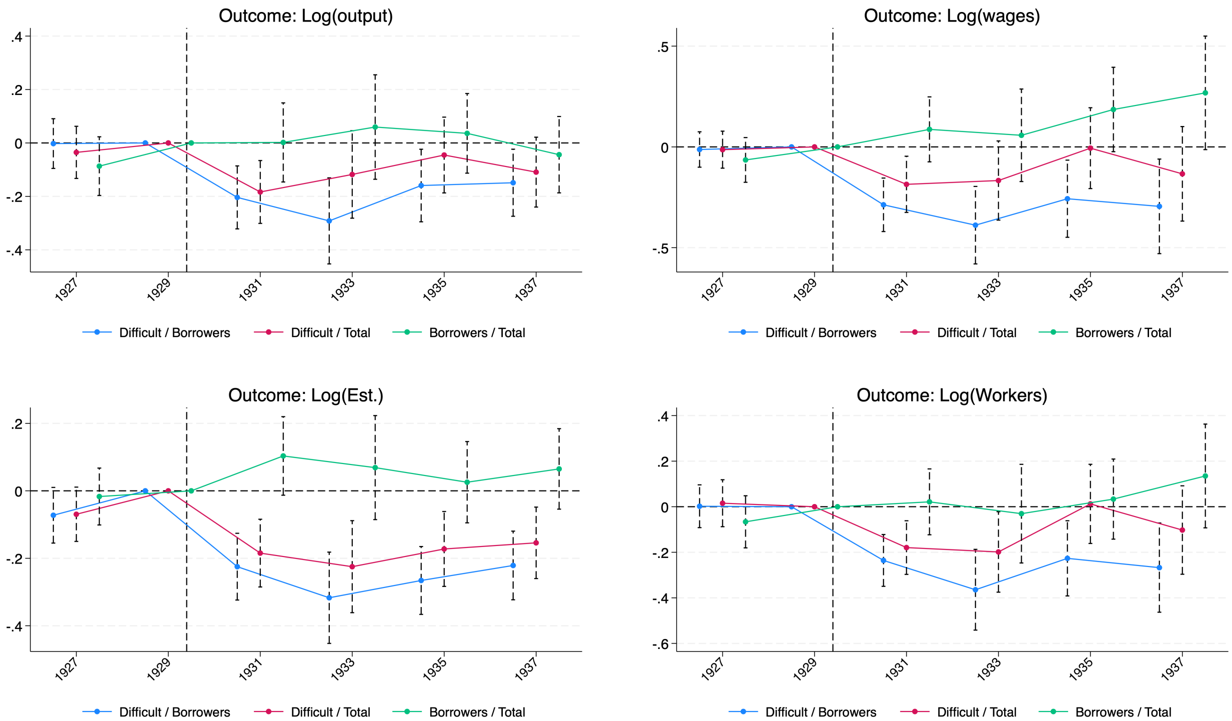
Notes: This figure plots the distributions of placebo effects computed using a randomization test as follows: using the tools developed in Heß (2017), I resample each county's Atlanta status ("in" vs. "out") within each border-region, keeping the share of "in" and "out" counties constant. I conduct this 1,000 times anåd re-estimate Equation 4.1 and store the 1929 and 1930 interaction terms of interest. The outcome variable is the annual suspension rate of all banks within a county. Control variables in X_{jk} include only border-region by year fixed effects. Vertical line shows the point estimates using actual borders.

Figure 7: Estimates across Federal Reserve Boundaries



Notes: This figure plots the estimated coefficients γ_i from Equation 4.1 using the Atlanta, St. Louis, Richmond, Cleveland, Dallas, and Kansas City Federal Reserve border regions in separate regressions. The outcome variable is the annual suspension rate of all banks within a county. Control variables in X_{jk} include only border-region by year fixed effects. Standard errors are clustered at the county level. 90 percent confidence intervals shown.

Figure 8: Explaining the Drop in Manufacturing using Credit Demand vs. Credit Difficulty



Notes: This figure plots the estimated coefficients of the *constraint* x year variables in Equation 5.1. The blue line shows the estimates when *constraint* is defined as the estimated share of manufacturing borrowers experiencing difficulty obtaining credit. The green lines shows them when *constraint* is defined as the estimated share of all firms borrowing. Finally, the red line shows them when *constraint* 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 1: Summary Statistics

Panel A: County

	count	mean	sd	p5	p25	p50	p75	p95
Banks (active - all)	4,015	3.44	2.53	1.00	2.00	3.00	5.00	8.00
Banks (suspended - all)	4,015	0.16	0.53	0.00	0.00	0.00	0.00	1.00
Banks (suspended - national)	4,015	0.03	0.18	0.00	0.00	0.00	0.00	0.00
Banks (suspended - state)	4,015	0.13	0.45	0.00	0.00	0.00	0.00	1.00
Deposits (active - all, mil)	4,015	2.64	8.09	0.05	0.40	0.92	1.97	7.66
Deposits (suspended - all, mil)	4,015	0.09	0.78	0.00	0.00	0.00	0.00	0.31
Deposits (suspended - national, mil)	4,015	0.03	0.48	0.00	0.00	0.00	0.00	0.00
Deposits (suspended - state, mil)	4,015	0.05	0.58	0.00	0.00	0.00	0.00	0.19
Capitalization ratio	2,162	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
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
Output (mil)	1,204	6.47	21.89	0.14	0.58	1.47	4.15	20.93
Wages (mil)	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
Wage Earners (k)	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	4225							

Panel B: Plants

	count	mean	sd	p5	p25	p50	p75	p95
Total days in operation	1,081	254.95	86.80	85.00	200.00	300.00	310.00	365.00
Total wages (k)	1,100	5.70	4.44	1.28	2.86	4.49	7.01	15.02
Wage earners by month, total	1,100	77.42	53.69	24.00	40.00	60.00	96.50	179.00
Value of product (k)	1,099	46.57	50.96	8.48	18.27	32.00	54.51	141.31
I(survive)	2,328	0.47	0.50	0.00	0.00	0.00	1.00	1.00
In ATL	2,328	0.47	0.50	0.00	0.00	0.00	1.00	1.00
I(subsidiary)	2,128	0.18	0.39	0.00	0.00	0.00	0.00	1.00
Observations	2328							

Notes: This table presents the summary statistics at the county-level (panel A) and plant-level (panel B) 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. Plant level data is for 1929, 1931, 1933, and 1935 and comes from Vickers and Ziebarth (2023).

Table 2: Covariate Balance (Atlanta vs. Rest in Border Regions)

County

	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)
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

Plants

	All	ATL in	ATL out	Difference
Log(Output)	10.52	10.54	10.50	-0.04
	(0.79)	(0.79)	(0.79)	(0.55)
Log(Wages)	8.53	8.52	8.54	0.02
	(0.64)	(0.66)	(0.62)	(0.74)
Log(Wage Earners)	4.24	4.29	4.21	-0.08
	(0.52)	(0.52)	(0.52)	(0.07)
I(subsidiary)	0.18	0.15	0.22	0.07
	(0.39)	(0.35)	(0.41)	(0.03)
Observations	582	276	306	582

Notes: This table reports variable averages among counties (Panel A) and small-medium manufacturing plants in 1929 (Panel B) within 50 miles of the Atlanta Federal Reserve District ("District") border. Column "All" reports the averages for all counties along the border (365) and columns "ATL in" and "ATL out" report them only for those in the district and for those outside the district, respectively. Column "Difference" 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 by combining the 1927 manufacturing industry by county establishment data and the 1935 credit survey of manufacturing industries. All other variables in County panel come from the 1930 U.S. Census. Plant panel variables are from the Census of Manufactures.For detailed variable descriptions and sources, please see the text.

Table 3: Industry Structure (Atlanta vs. Rest in Border Regions)

	All	ATL in	ATL out	Difference
Food and kindred products	0.28	0.26	0.29	0.03
	(0.19)	(0.19)	(0.19)	(0.29)
Textiles	0.06	0.06	0.06	-0.00
	(0.11)	(0.09)	(0.12)	(0.99)
Forest products	0.00	0.01	0.00	-0.00
	(0.02)	(0.02)	(0.01)	(0.02)
Paper and allied	0.42	0.45	0.40	-0.05
	(0.28)	(0.29)	(0.26)	(0.16)
Printing and publishing	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.72)
Chemicals	0.00	0.00	0.00	0.00
	(0.01)	(0.00)	(0.02)	(0.50)
Petroleum and coal	0.10	0.09	0.11	0.02
	(0.07)	(0.07)	(0.07)	(0.07)
Rubber	0.04	0.04	0.04	0.00
	(0.08)	(0.09)	(0.08)	(0.95)
Leather	0.04	0.03	0.04	0.01
	(0.07)	(0.08)	(0.07)	(0.48)
Stone, clay, glass	0.00	0.00	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.39)
Iron and steel	0.01	0.01	0.01	0.00
	(0.02)	(0.03)	(0.02)	(0.78)
Nonferrous metals	0.01	0.02	0.01	-0.00
	(0.03)	(0.03)	(0.03)	(0.63)
Transportation equipment	0.01	0.00	0.01	0.00
	(0.02)	(0.02)	(0.02)	(0.82)
Miscellaneous	0.01	0.01	0.01	0.00
	(0.03)	(0.02)	(0.03)	(0.24)
Observations	212	101	111	212

Notes: This table reports the share of establishments per industry among counties within 50 miles of the Atlanta Federal Reserve District ("District") border. Column "All" reports the averages for all counties along the border nd columns "ATL in" and "ATL out" report them only for those in the district and for those outside the district, respectively. Column "Difference" computes the difference and reports the T-test on the equality of means. All data is from the Market Data Handbook (Stewart (1929).

Table 4: Bank Suspension and Active Rates around the Atlanta Border

Panel A: Suspension rates

	Bank Suspension Rate			I(Bank Suspended)		
	All (1)	National (2)	State (3)	All (4)	National (5)	State (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	✓	✓	✓	✓	✓	✓
Border-region x Year FE	✓	✓	✓	✓	✓	✓

Panel B: Active rates

	Bank Active Rate			Deposit Active Rate		
	All (1)	National (2)	State (3)	All (4)	National (5)	State (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	✓	✓	✓	✓	✓	✓
Border-region x Year FE	✓	✓	✓	✓	✓	✓

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 4.1. 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 5: Robustness: Banks Suspended (All Types)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
In ATL=1 × Year=1926	0.007	0.002	0.002	-0.006	-0.024	-0.048	-0.000	0.021	0.007	0.004	0.014	0.006	0.007
	(0.013)	(0.017)	(0.008)	(0.018)	(0.027)	(0.043)	(0.014)	(0.026)	(0.019)	(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	0.029	0.019	0.009	0.024	0.018	0.023
	(0.011)	(0.012)	(0.015)	(0.014)	(0.055)	(0.024)	(0.013)	(0.024)	(0.018)	(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	-0.078	-0.068	-0.052	-0.061	-0.083	-0.070
	(0.025)	(0.028)	(0.029)	(0.029)	(0.091)	(0.052)	(0.032)	(0.037)	(0.030)	(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	-0.075	-0.051	-0.031	-0.047	-0.053	-0.068
	(0.023)	(0.027)	(0.031)	(0.033)	(0.118)	(0.063)	(0.028)	(0.040)	(0.029)	(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	0.012	-0.008	-0.018	0.005	-0.004	-0.013
	(0.018)	(0.020)	(0.028)	(0.026)	(0.048)	(0.048)	(0.022)	(0.036)	(0.020)	(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	0.028	0.001	0.058	0.002	-0.016	-0.023
	(0.031)	(0.033)	(0.036)	(0.037)	(0.091)	(0.061)	(0.039)	(0.054)	(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	0.021	0.006	0.000	0.012	0.004	0.007
	(0.010)	(0.009)	(0.007)	(0.008)	(0.030)	(0.009)	(0.010)	(0.021)	(0.017)	(0.011)	(0.013)	(0.010)	(0.011)
R-sq	0.14	0.16	0.20	0.20	0.27	0.33	0.15	0.14	0.14	0.18	0.14	0.14	0.11
N	2,820	2,504	1,331	1,672	302	907	1,805	1,015	2,820	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: 0-25mi							✓						
Distance: 25mi-100mi								✓					
Spatial SE									✓				
Removing border:										RICH	STL	CLE	DAL

Standard errors in parentheses

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 4.1. 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 6: Robustness: Deposits Active (All Types)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
In ATL=1 × Year=1926	-0.004	-0.026	-0.016	-0.029	-0.025	0.001	-0.004	0.004	-0.004	-0.003	0.003	-0.006	-0.007
	(0.020)	(0.023)	(0.018)	(0.026)	(0.055)	(0.049)	(0.024)	(0.035)	(0.043)	(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	0.020	0.006	-0.024	0.015	0.006	0.021
	(0.015)	(0.016)	(0.017)	(0.019)	(0.037)	(0.028)	(0.016)	(0.031)	(0.036)	(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	0.007	-0.007	-0.038	0.014	-0.006	0.000
	(0.020)	(0.023)	(0.023)	(0.027)	(0.069)	(0.057)	(0.023)	(0.039)	(0.036)	(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	0.075	0.073	0.019	0.091	0.080	0.091
	(0.030)	(0.034)	(0.035)	(0.033)	(0.082)	(0.074)	(0.039)	(0.050)	(0.040)	(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	0.040	0.030	-0.018	0.060	0.032	0.039
	(0.029)	(0.032)	(0.041)	(0.035)	(0.076)	(0.057)	(0.036)	(0.054)	(0.040)	(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	0.001	0.010	-0.013	0.033	0.013	0.008
	(0.025)	(0.028)	(0.035)	(0.032)	(0.045)	(0.059)	(0.030)	(0.047)	(0.037)	(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	0.030	0.048	-0.002	0.060	0.047	0.077
	(0.028)	(0.032)	(0.038)	(0.036)	(0.060)	(0.064)	(0.035)	(0.050)	(0.039)	(0.032)	(0.038)	(0.029)	(0.031)
R-sq	0.62	0.64	0.70	0.69	0.79	0.77	0.63	0.63	0.62	0.65	0.60	0.63	0.62
N	2,861	2,534	1,344	1,687	312	919	1,838	1,023	2,861	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: 0-25mi							✓						
Distance: 25mi-100mi								✓					
Spatial SE									✓				
Removing border:										RICH	STL	CLE	DAL

Standard errors in parentheses

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 4.1. 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 7: Banking outcomes around the Atlanta border

	County totals					Per bank			
	Loans	Bonds	Total Assets	Surplus	Banks	Loans	Bonds	Total Assets	Surplus
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
In ATL=1 × Year=1926	0.015	0.029	-0.005	0.004	0.041	-0.026	-0.012	-0.045	-0.036
	(0.041)	(0.055)	(0.039)	(0.056)	(0.023)	(0.034)	(0.050)	(0.032)	(0.046)
In ATL=1 × Year=1928	0.025	-0.045	-0.008	0.010	0.029	-0.004	-0.074	-0.038	-0.019
	(0.038)	(0.062)	(0.036)	(0.053)	(0.019)	(0.035)	(0.059)	(0.032)	(0.049)
In ATL=1 × Year=1929	0.031	-0.010	0.035	0.085	0.023	0.008	-0.033	0.012	0.062
	(0.039)	(0.078)	(0.036)	(0.061)	(0.024)	(0.034)	(0.074)	(0.030)	(0.055)
In ATL=1 × Year=1930	0.025	-0.098	0.014	0.015	0.035	-0.011	-0.133*	-0.021	-0.021
	(0.057)	(0.083)	(0.052)	(0.078)	(0.034)	(0.052)	(0.079)	(0.046)	(0.074)
In ATL=1 × Year=1931	0.116	0.008	0.090	0.162*	0.059	0.057	-0.051	0.031	0.103
	(0.065)	(0.105)	(0.057)	(0.091)	(0.052)	(0.056)	(0.099)	(0.047)	(0.083)
R-sq	0.15	0.16	0.12	0.07	0.08	0.12	0.20	0.11	0.06
N	1,061	1,061	1,061	1,060	1,061	1,061	1,061	1,061	1,060
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period cap	✓	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 4.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects and the 1928 capitalization ratio of national banks (surplus and capital over total assets) by year fixed effects ("Pre-period cap"). 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 8: County manufacturing outcomes during the Depression

	log(rev.)	log(wages)	log(est.)	log(workers)	log(rev)	log(wages)	log(est.)	log(workers)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In ATL=1 × Year=1929	-0.08	-0.07	0.04	-0.09	-0.08	-0.07	0.04	-0.09
	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.06)
In ATL=1 × Year=1931	-0.02	-0.08	-0.06	-0.04	-0.02	-0.08	-0.06	-0.04
	(0.07)	(0.09)	(0.06)	(0.09)	(0.07)	(0.09)	(0.05)	(0.09)
In ATL=1 × Year=1933	-0.02	-0.08	-0.09	-0.06	-0.03	-0.09	-0.09	-0.05
	(0.10)	(0.12)	(0.07)	(0.14)	(0.10)	(0.12)	(0.07)	(0.13)
In ATL=1 × Year=1935	-0.09	-0.08	-0.13	-0.13	-0.09	-0.09	-0.13	-0.13
	(0.10)	(0.13)	(0.07)	(0.11)	(0.10)	(0.13)	(0.07)	(0.11)
In ATL=1 × Year=1937	-0.01	-0.08	-0.12	-0.08	-0.01	-0.09	-0.11	-0.08
	(0.09)	(0.14)	(0.07)	(0.12)	(0.09)	(0.14)	(0.07)	(0.12)
Above Median: Difficult/Borrow=1 × Year=1929					0.01	0.04	0.03	0.03
					(0.05)	(0.05)	(0.05)	(0.05)
Above Median: Difficult/Borrow=1 × Year=1931					-0.17	-0.22	-0.14	-0.16
					(0.08)	(0.09)	(0.06)	(0.08)
Above Median: Difficult/Borrow=1 × Year=1933					-0.24	-0.28	-0.21	-0.26
					(0.11)	(0.13)	(0.08)	(0.12)
Above Median: Difficult/Borrow=1 × Year=1935					-0.19	-0.25	-0.23	-0.24
					(0.10)	(0.12)	(0.08)	(0.11)
Above Median: Difficult/Borrow=1 × Year=1937					-0.19	-0.30	-0.18	-0.30
					(0.10)	(0.17)	(0.07)	(0.14)
R-sq	0.65	0.45	0.61	0.34	0.66	0.46	0.63	0.35
N	636	636	678	672	636	636	678	672
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓
No outliers	✓	✓	✓	✓	✓	✓	✓	✓
Pre-period banking x Year	✓	✓	✓	✓	✓	✓	✓	✓
Border-region x Year	✓	✓	✓	✓	✓	✓	✓	✓
Avg. size tercile (1929) x Year	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

Notes: This table reports the estimated coefficients of Equation 5.1. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects, 1928 capitalization ratio of national banks (surplus and capital over total assets) and 1924-1929 log growth rate of national bank loans by year fixed effects, and terciles of the average size of manufacturing plant in 1929 by year fixed effects. 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.

Table 9: Interaction between financial constraints and bank failure around the Atlanta border

	log(rev.)	log(wages)	log(est.)	log(workers)
	(1)	(2)	(3)	(4)
Above Median: Difficult/Borrow=1 × post=1	-0.24	-0.29	-0.22	-0.34
	(0.10)	(0.12)	(0.07)	(0.12)
post=1 × In ATL=1	-0.03	-0.06	-0.13	-0.12
	(0.10)	(0.13)	(0.06)	(0.11)
Above Median: Difficult/Borrow=1 × post=1 × In ATL=1	0.06	0.01	0.03	0.17
	(0.15)	(0.19)	(0.10)	(0.17)
R-sq	0.65	0.46	0.62	0.35
N	636	636	678	672
Year FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
No outliers	✓	✓	✓	✓
Pre-period banking x Year	✓	✓	✓	✓
Border-region x Year FE	✓	✓	✓	✓

Standard errors in parentheses

Notes: This table reports the estimated coefficients of Equation 5.2. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects, the 1928 capital ratio and 1924-1929 log growth rate of national bank loans by year fixed effects, and terciles of the average size of manufacturing plant in 1929 by year fixed effects. The variable *post* takes the value of 1 for all years after 1929 and 0 otherwise. 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.

Table 10: Manufacturing plant outcomes during the Depression in the Atlanta border regions

Panel A: Including subsidiary plants

	Survival	Output		Wages		Workers	
		all	balanced	all	balanced	all	balanced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
year=1931 × In ATL=1	0.14	-0.02	-0.07	-0.15	0.07	-0.06	0.02
	(0.04)	(0.06)	(0.07)	(0.08)	(0.12)	(0.06)	(0.13)
year=1933 × In ATL=1	0.08	-0.08	-0.04	0.02	0.17	0.09	0.19
	(0.04)	(0.09)	(0.10)	(0.11)	(0.11)	(0.08)	(0.13)
year=1935 × In ATL=1	0.07	0.03	0.03	-0.17	-0.05	-0.04	0.02
	(0.03)	(0.11)	(0.12)	(0.14)	(0.15)	(0.10)	(0.12)
R-sq	0.65	0.46	0.40	0.29	0.23	0.20	0.14
N	2,328	1,057	396	1,057	396	1,055	391
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
Border-region x Year	✓	✓	✓	✓	✓	✓	✓
Size Quartile x Year	✓	✓	✓	✓	✓	✓	✓
Industry x Year	✓	✓	✓	✓	✓	✓	✓

Panel B: Excluding subsidiary plants

	Survival	Output		Wages		Workers	
		all	balanced	all	balanced	all	balanced
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
year=1931 × In ATL=1	0.14	-0.05	-0.05	-0.18	0.10	-0.09	0.06
	(0.04)	(0.06)	(0.08)	(0.08)	(0.13)	(0.07)	(0.14)
year=1933 × In ATL=1	0.08	-0.10	-0.03	-0.00	0.17	0.07	0.21
	(0.04)	(0.10)	(0.11)	(0.12)	(0.12)	(0.09)	(0.14)
year=1935 × In ATL=1	0.07	0.03	0.06	-0.27	-0.13	-0.07	0.02
	(0.04)	(0.12)	(0.13)	(0.13)	(0.14)	(0.10)	(0.13)
R-sq	0.61	0.47	0.41	0.30	0.24	0.22	0.17
N	1,736	863	357	865	356	858	351
Year FE	✓	✓	✓	✓	✓	✓	✓
Firm FE	✓	✓	✓	✓	✓	✓	✓
Border-region x Year	✓	✓	✓	✓	✓	✓	✓
Size Quartile x Year	✓	✓	✓	✓	✓	✓	✓
Industry x Year	✓	✓	✓	✓	✓	✓	✓

Notes: This table reports the estimated coefficients of Equation 5.3. Controls include boundary-region (e.g., Atlanta-St. Louis border) by year fixed effects, size-quartile by year fixed effects, and industry by year fixed effects. The omitted baseline interaction is 1929 across all specifications. The outcome variables come from Census of Manufactures, trimmed at the 2-98 percentiles to minimize the influence of outliers. Panel A includes all plants, while panel B excludes plants that reported being a subsidiary. The sample in columns (3), (5), (7), (9) includes only a balanced set of plants. The time period is 1929 - 1935 (biennially) for all specifications and the standard errors are clustered at the county level.