Lender of Last Resort and Local Economic Outcomes*

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Abstract

This paper provides new causal evidence that lender-of-last-resort (LLR) policies can reshape the long-run geography of economic activity and labor market trajectories. I exploit a natural experiment from the Great Depression: counties in the Atlanta Federal Reserve district were exposed to aggressive liquidity support following the 1930–31 banking crisis, while neighboring counties just outside the district were not. Counties within the district experienced significantly fewer bank failures during 1930–31 and exhibited substantially stronger manufacturing outcomes in subsequent years, offsetting roughly half of the national decline in output, employment, and establishment counts between 1929 and 1931. Using newly digitized county-level manufacturing data and linked 1930–1940 Census microdata, I find that residents of LLR counties were more likely to remain in or enter manufacturing employment by 1940, and were significantly less likely to migrate out of state. These effects are concentrated among younger and less-educated individuals. The results suggest that early financial stabilization shaped the long-run spatial distribution and composition of manufacturing employment.

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1 Introduction

Financial crises have historically inflicted significant damage on economies. In the United States, the Federal Reserve's lender of last resort (LLR) policies have been a cornerstone of efforts to mitigate systemic bank failures since the Great Depression. The 2008 subprime mortgage crisis offers a recent illustration: the Federal Reserve implemented unprecedented LLR actions, introducing novel mechanisms for injecting liquidity into financial intermediaries to stave off a severe economic downturn. Yet despite banks being flush with liquidity after 2009, lending to households and firms remained constrained for years, potentially reflecting the heightened role of asymmetric information (Mishkin (2011)). However, the limited downstream benefits during the Great Financial Crisis do not conclusively negate the efficacy of LLR policies in supporting local economies; rather, they underscore the difficulty of evaluating their impact without a clear counterfactual.

Assessing the impact of the Federal Reserve's actions in modern contexts is challenging, as aggregate economic changes, whether across countries or within the United States, simultaneously reflects endogenous responses by households, firms, and governments. This paper provides new causal evidence that lender-of-last-resort (LLR) policies can reshape the geography of economic recovery by stabilizing financial institutions, preserving local industrial activity, and altering long-run labor market trajectories. Leveraging a natural experiment created by the Atlanta Federal Reserve's uniquely interventionist response during the Great Depression and newly digitized data, I show that counties just inside the Atlanta district experienced significantly fewer bank failures and stronger manufacturing sector performance, as measured by output, employment, and firm counts, than adjacent counties across the district border. Using newly linked person-level census data, I document persistent effects on individual workers: residents in treated counties were more likely to remain in or enter manufacturing and less likely to migrate out a decade later. These results underscore that financial stabilization policies can have durable, composition-shifting effects on local labor markets, long after the financial crisis has passed.

Between 1929 and 1933, more than half of U.S. commercial banks ceased operations. Some closed temporarily due to liquidity shortages, others became insolvent and shut down permanently, while many merged with other institutions to avoid liquidation. This widespread disruption in the financial sector has spurred over eight decades of research into the causes and consequences of bank failures (e.g., Friedman and Schwartz (1963), Bernanke (1983), Temin (1976), Wicker (2000)). A significant portion of this research critiques the Federal Reserve's inaction during the early stages of the Depression, particularly its failure to curb the decline in the money supply or to act as a comprehensive LLR for banks. Unlike other Federal Reserve banks, Atlanta proactively acted as an LLR during the onset of the Depression, extending credit to solvent but illiquid banks to prevent runs on otherwise healthy institutions. By exploiting the quasi-exogenous division of Federal Reserve district borders, first highlighted by Richardson and Troost (2009) and Ziebarth (2013), I compare local economic trajectories and individual worker decisions across counties just inside and outside Atlanta's jurisdiction.

Ideally, evaluating the real effects of LLR policy would require firm-level data on capital structure, credit access, and matched employer-employee outcomes across treated and control regions. In the absence of such data, my analysis proceeds in three steps. Each step is necessary to build the overall argument, but no single step is sufficient on its own. Together, however, they form a coherent and compelling case: preferential access to liquidity through the Atlanta Fed's lender-of-last-resort policies stabilized the banking sector, preserved manufacturing activity, and durably reshaped labor market outcomes across the region.

In the first step, I establish that external finance dependence strongly correlated with county-level manufacturing outcomes: simply put, financing mattered for a wide swath of industries and counties in the manufacturing sector. 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 U.S. county. 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 across U.S. counties, I compare economic activity before and after the onset of the Great Depression across counties with different levels of external capital dependence and across the Atlanta border.

In the second step, I find that LLR prevented failures: banks inside the Atlanta region in 1929 and 1930 had an 8-13 percent lower failure rate. 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.

In the third and final step, I assess the real effects of LLR exposure on both local manufacturing activity and individual labor market trajectories. Using biennial county-level data from the Census of Manufactures, I estimate that counties within the Atlanta district experienced 26 log points more manufacturing output, 39 log points more employment, and 57 log points more establishments between 1929 and 1931 relative to adjacent control counties. These effects represent a substantial offset to the roughly 50 log point national decline in manufacturing output over the same period. I then leverage newly linked individual-level census records to estimate long-run impacts on workers. Among individuals employed in manufacturing in 1930, LLR exposure is associated with a 2.9 percentage point increase in manufacturing employment by 1940, a 9.7% increase relative to the baseline mean. For those outside of manufacturing in 1930, I find a 0.7 percentage point increase in manufacturing entry, representing a 9.4% increase relative to the base rate. Aggregated across groups, approximately 30% of the net gain is attributable to retention and 70% to new entry. I also find that LLR exposure reduced interstate migration among 1930 manufacturing workers by 1.7 percentage points, or 15% relative to the mean. These effects are most pronounced among younger and less-educated individuals, suggesting that access to early liquidity support shaped the long-run allocation and composition of labor across space and sectors.[1]

This paper provides new causal evidence that lender-of-last-resort (LLR) policies can reshape the long-run geography of economic activity. To my knowledge, it is the first study to exploit the full geographic extent of local economic outcomes in the quasi-experimental setting of the Atlanta Federal Reserve district border. While both Richardson and Troost (2009) and Ziebarth (2013)) focus on Mississippi, this paper tests whether the positive LLR effects they document generalize beyond that predominantly rural context by using newly digitized county-level manufacturing data across the U.S. Second, I introduce newly linked individual-level census data and identify a novel transmission channel: persistent labor market effects that connect locally aggregated manufacturing outcomes to long-run employment and migration outcomes. Third, unlike existing studies on Atlanta, I explicitly examine the role of financial constraints by integrating industry-level location

[1] 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 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.

data with newly digitized survey evidence on external finance dependence. In line with Benmelech et al. (2019), I find strong evidence of credit rationing and localized employment losses tied to financing conditions.

The remainder of the paper is structured as follows: Section 2 outlines the historical context and reviews the relevant literature. Section 3 describes the data, while Section 4 shows the relationship between external finance constraints and manufacturing outcomes in the 1930s. Section 5 then outlines the empirical strategy and the banking findings across the Atlanta border regions. Section 6 examines county manufacturing and person-level labor outcomes, and Section 7 offers concluding remarks.

2 Historical Background

This section provides an overview of the historical and institutional background and outlines the paper's contribution to the literature.

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 and macroeconomic policy. 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 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

[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).

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.

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. His 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. 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.

Recent work also provides complementary evidence on the microeconomic transmission of banking distress to real activity in earlier periods. Using newly assembled national data from the Progressive Era (1900–1929), del Angel et al. (2024) show that bank failures causally increased bankruptcies among firms that depended on commercial banks for short-term credit, while firms that were largely bank-independent (primarily large manufacturers with access to retained earnings and capital markets) were largely insulated. They demonstrate that credit supply disruptions operated through clearly identifiable bank-firm linkages, rather than through broad aggregate demand channels. Their findings underscore that bank failures mattered precisely because of firms' reliance on relationship banking for working capital, reinforcing the importance of financial intermediation in shaping real outcomes even before the Great Depression.

This paper makes three key contributions that extend the historical and empirical literature. First, whereas prior studies such as Richardson and Troost (2009) and Ziebarth (2013) focus narrowly on Mississippi, I leverage the full spatial discontinuity created by the Atlanta Federal Reserve's jurisdiction - spanning multiple state borders - to evaluate the generalizability of LLR policy effects in a broader quasi-experimental setting. Second, I link newly digitized county-level data with individual-level census records, enabling the first analysis of long-run labor market impacts from Depression-era central bank interventions. This allows me to document not only local aggregate manufacturing outcomes but also persistent changes in employment composition, occupational mobility, and migration at the individual level. Third, I directly show that effects of credit market exposure by merging county-level industry composition data with measures of external finance dependence from both a contemporaneous credit survey and structural proxies. This integration of industry-level financial frictions into a spatial identification strategy permits a more precise evaluation of the mechanisms through which LLR policy operated, advancing beyond earlier studies that lacked direct measures of financing constraints at the aggregate local level. Together, these innovations allow for a richer characterization of the real effects of LLR during financial crises.

3 Data

I briefly describe the main sources of county, firm, and individual-level data in this section. For a detailed review of the sources, please see Online Appendix A.

3.1 Banking and Finance

To assess local credit conditions during the Depression, I draw on a 1934 U.S. Commerce Department survey of over 6,000 small and medium-sized manufacturers, which found widespread credit use and credit difficulties: over 80% reported that commercial banks provided the main source of external funding, and 45% of borrowing firms reported trouble accessing loans (U.S. Census Bureau (1935)). I construct an industry-level measure of credit constraint (the proportion of all firms that reported credit difficulty) from this survey and aggregate it to the county level using output-weighted averages.[4] While informative, the survey reflects conditions several years into the Depression and may understate constraints in industries where many firms had already failed. To address this limitation, I complement it with the measure of industry-level external financial dependence from Rajan and Zingales (1998), which captures structural financing needs computed using

[4] Online Figure A.1 plots the main results of the survey by industry.

accounting data from publicly traded firms in the 1980s. I further incorporate county-level data on banking disruptions using bank suspension data from the Federal Deposit Insurance Corporation and newly digitized balance sheet data from the Office of the Comptroller of the Currency (Federal Deposit Insurance Corporation (1992); United States Office of the Comptroller of the Currency (1920-1932)).

3.2 Manufacturing and Industry Composition

To construct measures of local industrial activity and composition, I digitize county-level output, employment, and establishment count data from the Census of Manufactures for 1927–1935, including a 1927 special tabulation of plant counts by industry and county published in the Market Data Handbook (U.S. Census Bureau (1937); Stewart (1929)). I transform the establishment counts by industry to the 15-sector classification used in the 1935 Credit Survey and convert these counts into economic shares using state-industry averages of output, employment, and wages, thereby accounting for variation in establishment size across sectors. Due to Census confidentiality thresholds, coverage is incomplete in low-manufacturing counties; I address this by restricting to a balanced panel across census years. I construct outcomes including log output, log employment, and log wages. Baseline county characteristics are well balanced across the border, though manufacturers just outside the boundary were marginally larger on average.

3.3 Linked Decennial Censuses, 1930 - 1940

Data regarding manufacturing workers comes from the 100 percent count U.S. census records. This paper focuses on working-age adults (ages 18-55) in Mississippi, Tennessee, and Louisiana – the three states bisected by the Atlanta federal reserve border. I used the crosswalks provided by the Census Tree Project (Price et al. (2023)) and IPUMS publicly available census data (Ruggles et al. (2024)) to link records over time.[5] In total, the 1930-1940 sample includes over 1.88 million individuals, with 65 percent living in counties within the Atlanta district. This linked sample covers roughly 51 percent of the manufacturing workforce in 1930 in those states but is not perfectly representative. To address this issue, I use inverse probability weighing in my empirical analysis, creating weights after predicting the characteristics associated with a successful link. I construct three primary outcome variables: a binary variable taking the value of 1 if the individual reported working in the manufacturing sector in 1940, the logarithm of the change in the individual's occupational

[5]For more details about how these links were created, see Price et al. (2021) and Buckles et al. (2023).

income score, and two binary variables indicating inter-county and inter-state migration.

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 1. My approach closely mirrors the method employed by Jalil (2014). For robustness checks, I transcribed 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. Lastly, I use New Deal county-level Reconstruction Finance Corporation (RCFC)and Agricultural Adjustment Administration (AAA) expenditure data collected by Fishback et al. (2003) in various robustness exercises.

(Figure 1 around here)

3.5 Summary Statistics

Table 1 presents the summary statistics. 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 roughly 1,000 observations for manufacturing span between 190-210 border counties reporting manufacturing activities biennially from 1927 to 1935.

(Table 1 around here)

Across counties, there is a pronounced negative correlation between manufacturing activity and bank failures. Figure 2 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 1,623 counties). On average, a ten percentage point increase in bank failure is associated with 2 log point decrease in manufacturing output across counties.

(Figure 2 around here)

4 External Financing and Manufacturing Outcomes during the Depression

The empirical analysis proceeds in three steps. First, I establish that external finance dependence is a key predictor of manufacturing decline during the early Depression, using industry-level credit survey data and pre-Depression manufacturing composition to construct county-level proxies for financial constraints. Second, I exploit the Atlanta Federal Reserve's border discontinuity to show that counties within its jurisdiction experienced significantly fewer bank failures during 1929–1930, consistent with more aggressive LLR intervention. Third, I examine downstream effects on firms and workers, finding that treated counties saw greater manufacturing survival, employment retention, and persistent changes in individual labor market trajectories. Together, these steps provide a unified framework linking financial frictions, central bank policy, and long-run local economic outcomes.

I begin by documenting the strong relationship between manufacturing decline and credit difficulty across all U.S. manufacturing counties. Using a balanced panel of biennial 1927 - 1935 manufacturing data, I estimate the following model:

$$S_{jk} = \alpha_j + \beta_{jk} + \sum_{i=1927}^{1935} (constraint_j T_i) \cdot \omega_i + X_{jk} + \epsilon_{jk} \tag{4.1}$$

where T_i is a year dummy taking the value of 1 if $i = k$ and 0 otherwise and $constraint_j$ is a standardized (mean zero, standard deviation one) measure of credit difficulty (*Borrower-Difficulty*) based on the 1935 credit survey or external dependence as in Rajan and Zingales (1998). I use county (α_j) and state by year (β_{jk}) fixed effects to account for all unobserved but static county variables and state-level trends in manufacturing. To account for differences in business failure rates based on firm size, I include terciles of average manufacturing plant size based on the number of wage earners in 1929 by year fixed effects (X_{jk}) . The coefficients of interest are ω_i, which capture the time-varying difference in outcome S in counties based on industry-weighted credit difficulty. The omitted reference year is 1929, and I cluster the standard errors at the county level.

(Table 2 around here)

Table 2 displays the results when *Borrower-Difficulty* is used as the $constraint_j$ measure.

Columns (1) - (4) use log output (revenue), log county manufacturing wage-earner wages, log number of establishments, and the log number of wage-earners as the outcome variables, respectively. Outside of a small negative pre-trend in the number of establishments, I do not observe significant pre-trends in 1927. By 1931, however, counties with more borrowers facing credit challenges corresponded to significantly worse outcomes. A 5.6 percent (1 standard deviation) increase in borrowing difficulty corresponded to an 8 log point decrease in total output and 9 log point decrease in the number of wage-earners employed. These impacts are persistent to 1935 and largest at the trough of the Depression in 1933. The quantitatively similar results using *RZ-Difficulty* are reported in the Online Appendix (Table A3).

I next decompose the results presented in Table 2, analyzing the impact of credit difficulty versus credit demand. Using the total share of small and medium manufacturing plants that reported *any* borrowing - as opposed to those that reported *difficulty* in borrowing - I find that the estimates in Table 2 are not driven by differential credit demand across counties. Counties with manufacturing firms in high-borrowing industries saw an increase, not a decrease, in economic activity. Figure 3 plots the 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 $constraint_j$. The figure shows that a county's industry-weighted borrowing propensity (green line) corresponds to slightly better, not worse, outcomes. On the other hand, the unwillingness of banks to extend credit (red and blue lines) as a share of all firms predicts significantly worse outcomes after, but not before, 1929.

(Figure 3 around here)

5 LLR Policy Regimes and Banking Outcomes across Federal Reserve Borders

Having established that counties with higher exposure to credit constraints experienced sharper manufacturing declines during the early Depression, I now turn to the identification of a plausibly exogenous policy intervention that alleviated those constraints. Specifically, I exploit the discontinuity at the border of the Atlanta Federal Reserve district to test whether liquidity provision stabilized the banking sector in treated counties.

5.1 Empirical Design

The 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 of these metrics are key, as banks might recommence operations after 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 the logarithm of 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.

I do not find strong underlying *ex ante* differences between counties across the border. Tables3 shows that counties on the border of the Atlanta District were similar.[6] 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.

(Table 3 around here)

I compare county-level outcome variables before and after the onset of the Great Depression across the Atlanta boundary using the following specification:

[6] Jalil (2014) reports additional covariate balance checks using demographic and economic variables in counties around the border in Table 2.

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

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. Like in the previous section, I use county (α_j) and border-region by year fixed effects to account for all unobserved but static county variables and region-level trends in bank failure rates. The control variables in X_{jk} include, 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.

5.2 Baseline Results

Table 4 presents the results. 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) indicate that, relative to 1927 levels, the share of suspended banks was 6.0 and 5.2 percentage points lower in Atlanta counties in 1929 and 1930 (s.e. $=$ 0.025 and 0.023), 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. 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 results are consistent when using the number of active banks and total deposits as outcomes. By the end of 1930, counties within the Atlanta district retained 10.1 percentage points more active banks and held 7.3 percentage points more deposits relative to 1927 levels compared to neighboring counties (s.e. = 0.029 and 0.03), conditional on county fixed effects and region-specific trends. 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.

5.3 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. In this section, I discuss the results with additional controls and different samples, and argue that the stability of the baseline estimates provides strong evidence in favor of a causal interpretation.[7]

The first concern is that the small pre-Depression differences in county characteristics across the Atlanta border as shown in Table 3 (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. 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 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

[7] Online Appendix Tables A1 and A2 contain the regression results of all robustness checks discussed in this section.

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. The results remain significant.

Lastly, I address concerns of sample selection and standard error clustering. 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, and find little change in the estimates. 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. Furthermore, the results are robust to using spatial standard errors via the methods of Colella et al. (2019) and sequentially excluding border regions.

5.4 Placebo Tests and Other Policy Regimes

To further assess the validity of the identification strategy and rule out spurious spatial correlations, I now turn to a series of placebo tests that randomize treatment assignment and compare effects across other Federal Reserve district borders. 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 5.1 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 4. 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.

(Figure 4 around here)

Finally, 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, as the robustness exercises suggest, 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 5. The outcome variable is the total bank suspension rate. The variable Atl_k in 5.1 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 5 around here)

6 Downstream Impact of LLR on the Manufacturing Sector

6.1 County Manufacturing

Having established that credit constraints were strongly associated with manufacturing decline and that LLR policies improved banking stability, I now examine whether these financial interventions translated into better outcomes for manufacturing establishments. Using county-level data from the Census of Manufactures, I analyze economic activity along the Atlanta Federal Reserve border between 1929 and 1935. Because local banks were a critical source of working capital for manufacturers, particularly for small and medium-sized firms, we would expect to observe relatively better outcomes within the Atlanta district, where banks remained operational.

The empirical strategy follows the framework similar to Ziebarth (2013), with county fixed effects and a comparison between 1929 and 1931. I extent the geographic scope beyond Mississippi and incorporate the entire border region. Specifically, I estimate the following specification:

$$y_{it} = \alpha_i + \alpha_1 Atlanta_i \times 1931_t + X_{it} + \epsilon_{it} \tag{6.1}$$

where i denotes county, t denotes the year, and Atl_i is an indicator variable that takes the value of 1 if the county is located within the Atlanta Federal Reserve district. The control variables in X_{it} include state by year fixed effects in the main specification and, additionally, industry share by year fixed effects and 1927-1929 trends by year in robustness exercises.

(Table 5 around here)

Table 5 reports the main results using just Mississippi counties (Panel A) and the entire region (Panel B).[8] Relative to counties just outside the Atlanta district, manufacturing output in counties within the Atlanta district was 21 to 26 log points higher in 1931 compared to 1929 (s.e. = 0.14). Given that the average U.S. county experienced a 50 log point decline in manufacturing output between 1929 and 1931, this differential implies that LLR exposure offset roughly half of the national contraction. These effects are even more pronounced along other margins of firm activity: the number of wage earners was higher by 25-39 log points (s.e. = 0.13), and the number of establishments, in Mississippi, by 57 log points (s.e. = 0.21). These patterns suggest that LLR policies played a crucial role in preserving the extensive margin of production—keeping plants open and workers employed.[9]

When extending the panel through 1935, the magnitude of these differences declines over time, particularly in Mississippi, as policy differences across Federal Reserve districts diminished and the national economy began a slow recovery. Nevertheless, the coefficients remain positive and statistically significant through 1933, highlighting the early and localized nature of Atlanta's intervention. Panel B of Table 6 expands the analysis to the full Atlanta border region and confirms these findings: the estimated impacts on output and employment remain stable, while the establishment effect, although still present, is somewhat smaller than in Mississippi alone.

These estimates are robust to concerns about differential industry composition, pre-existing growth trajectories, as well as potential interactions with New Deal programs instituted after 1932. I conduct various robustness checks and include year-by-county industry share fixed effects, control for manufacturing trends between 1927 and 1929, use 1927 as the base year, and exclude counties

[8] With the inclusion of state x year fixed effects, α_1 is identified solely from Mississippi, Tennessee, and Louisiana.

[9] These results are in line with those found in Mississippi by Ziebarth (2013), who finds that manufacturing plants in the Atlanta region experienced a 21 log point increase in output in 1931 compared to those in the St. Louis region.

within 25 miles of the border to account for spillovers. I further directly control for New Deal spending programs, specifically from the AAA and RFC. Results are qualitatively unchanged under these alternative specifications (see Appendix Table A4).

Taken together, these results show that LLR policy exposure was associated with higher manufacturing output, employment, and firm counts during the early Depression period. This stabilization of local industrial activity suggests that emergency liquidity support helped preserve labor demand in affected counties. In the next section, I examine whether these effects were reflected not only in persistently higher retention and entry, but also in changes to the composition of the manufacturing workforce.

6.2 Labor Response

The preceding sections establish that the Atlanta Federal Reserve's lender of last resort (LLR) policies supported the manufacturing firms at the onset of the Great Depression. However, these findings reflect outcomes over a relatively short window (1929–1933) and do not speak directly to the persistent labor market consequences of LLR intervention. To assess whether early financial stabilization translated into durable gains for local workers, I turn to individual-level panel data from the full-count 1930 and 1940 U.S. censuses.

This section uses census records in the three states bisected by the Atlanta federal reserve border–Mississippi, Tennessee, and Louisiana–to examine how 10-year employment trajectories varied depending on whether individuals resided in counties exposed to Atlanta's LLR policies at the start of the Depression.[10] Because the 1940 outcomes are observed well after the banking crisis subsided and after Atlanta's emergency liquidity provision had ceased, this exercise tests for persistent, long-run effects of LLR on local labor markets and mobility.

Specifically, I estimate the following regression:

$$Y_{i(j)} = \beta Atl_j + X_i + \Omega_j + \epsilon_i \tag{6.2}$$

where outcome Y is observed in 1940 for individual i who resides in county j as of 1930. The variable Atl_j takes the value of 1 if county j is within the Atlanta federal reserve district. At the individual level, the controls in X_i include age, sex, the logarithm of occupational income score in 1930, and state of residence in 1930 and 1940 fixed effects. To account for persistent county-level

[10] These three states were chosen so that the analysis could include state fixed effects to account for unobserved state-level labor policies.

manufacturing trends driven by unobservable *local* characteristics, Ω_j includes county-level retention, entry, and migration rates between 1910 and 1920, which I compute using the same linking and sample selection methods as for the main sample. In essence, this specification compares 10-year outcomes of two individuals who were employed in the same state in 1930, in counties with similar historical trends of manufacturing retention and exit, where one lived in a county that received LLR support during the banking crises and the other did not. I cluster the standard errors at the county level.

(Table 6 around here)

Table 6 presents the regression results of β for three outcome variables: (A) an indicator for employment in manufacturing in 1940, (B) the log change in occupational income score between 1930 and 1940, and (C) indicators for inter-county and inter-state migration. In Panels A and B, columns 1 and 3 restrict the sample to individuals employed in manufacturing in 1930, while columns 2 and 4 focus on those not in manufacturing in 1930. Panel C uses only 1930 manufacturing workers to assess migration outcomes. For each panel, I report results separately for Mississippi (columns 1–2) and for the pooled sample across all three states (columns 3–4).

Panel A shows that among 1930 manufacturing workers in Mississippi, those residing in Atlanta District counties were 2.9 percentage points more likely to be employed in manufacturing by 1940 (s.e. = 0.017), a 9.7% increase relative to the group mean of 30%. For non-manufacturing workers in 1930, LLR exposure was associated with a 0.7 percentage point increase in entry into manufacturing by 1940 in Mississippi (column 2), and a 0.4 percentage point increase across the full sample (column 4). Scaled relative to the baseline manufacturing share, these estimates imply increases of 9.4% in Mississippi and 4.1% in the pooled sample.[11] Approximately 30% of the manufacturing gain in Mississippi was driven by retention, and 70% by new entry. Given that the policy treatment occurred nearly a decade prior, these results suggest that LLR support had long-lasting effects on local labor structure.

Panel B reports results for the log change in occupational income score between 1930 and 1940. The *occscore* is a commonly used proxy for labor earnings based on occupation-specific median income as of 1950, retroactively applied to occupations in previous censuses. For retained

[11] The share of workers in manufacturing in Mississippi is 9.7 percent = 29,458/(29,458+273,534), such that the combined impact of retention and entry is 0.029*0.097 + 0.007*(0.903) = 0.009134. As a share of manufacturing in 1930, the scaled effect is 9.41 percent = 0.009134/0.097. Share of linked workers in manufacturing in the three states is 15 percent = 162,446/(162,446+919,399). Thus, the combined effect is 0.0061 = 0.018*0.15 + 0.004*(0.85), and the scaled effect from the base of manufacturing is 0.0061/0.15 = 4.1 percent.

manufacturing workers, I do not find statistically significant evidence that LLR exposure led to greater occupational mobility. However, for those in Mississippi, I find a 0.022 log point increase (s.e. = 0.009) for new entrants into manufacturing, or about 13 percent of the average increase in the sample between 1930 and 1940.

Panel C turns to migration outcomes for workers employed in manufacturing in 1930. LLR exposure did not significantly affect inter-county migration (columns 1 and 2) but did significantly reduce interstate migration (columns 3 and 4), suggesting that LLR policies anchored workers locally. These results suggest that LLR intervention during the banking crisis had persistent impacts on individual employment outcomes. Residents of LLR counties were more likely to remain in or transition into manufacturing jobs a decade after the policy was implemented, and less likely to migrate out of state.

Why did this relatively brief episode in 1930–31 generate persistent differences in labor market outcomes observed in 1940? The patterns in Table 6 suggest several complementary mechanisms. Panel A shows that long-run manufacturing gains arise from both retention of incumbent workers and new entry, consistent with the idea that manufacturing production networks are costly to reestablish: early lender-of-last-resort intervention preserved local employment relationships, capital, and organizational capacity, while firm exit was permanent and restarting activity substantially more costly. Panel B indicates that new entrants into manufacturing experienced some occupational upgrading, suggesting that surviving firms continued to generate relatively stable, higher-quality employment opportunities over time. Panel C further shows that LLR exposure reduced interstate migration among 1930 manufacturing workers, implying that early stabilization anchored workers locally and limited spatial reallocation. Together, these results are consistent with slow-moving labor market adjustment frictions and local general equilibrium effects, whereby short-run differences in financial conditions and firm survival translate into persistent divergence in individual employment trajectories even as aggregate manufacturing outcomes partially converge.

6.3 Compositional Effects of the Labor Response

To understand whether LLR exposure changed the composition of the manufacturing workforce, I test for heterogeneous effects across observable characteristics. I extend Equation 6.2 by interacting the LLR treatment indicator Atl_j with individual-level characteristics (e.g., age, education, gender, income score, urban status). I then compute predicted probabilities of remaining in or entering manufacturing for different subgroups. Figures 6 and 7 plots these probabilities.[12]

(Figure 6 around here)

Figure 6 presents predicted probabilities of staying in manufacturing between 1930 and 1940 by subgroup. I find limited compositional differences across the Atlanta border among stayers. In both types of counties, workers in the 1st and 3rd terciles of the occupational income distribution had higher retention than those in the middle tercile. Retention was also higher among younger, male, and less-educated workers. Notably, urban workers in Atlanta counties saw greater retention, likely due to selective out-migration elsewhere.

(Figure 7 around here)

In contrast, Figure 7 shows clear compositional effects among new entrants into manufacturing. Entry was significantly more likely for individuals who were younger, male, and had lower educational attainment in Atlanta counties. This pattern suggests that LLR support enabled broader access to manufacturing jobs throughout the Depression, especially for low-skilled workers.

7 Summary

This paper shows that the Federal Reserve's lender-of-last-resort (LLR) interventions during the Great Depression had long-run consequences for the geography of economic activity and labor market outcomes. Exploiting a discontinuity at the border of the Atlanta Federal Reserve district, I provide causal evidence that access to emergency liquidity stabilized banks, preserved manufacturing firms, and shaped local economic recovery. Counties on the Atlanta side of the district border experienced significantly fewer bank failures, higher manufacturing survival, and stronger rebounds in output and employment during the early 1930s.

These firm-level effects translated into durable labor market impacts. Using person-level panel data linking individuals from the 1930 and 1940 censuses, I show that manufacturing workers in treated counties were more likely to remain in the sector, while non-manufacturing workers were more likely to enter it. These gains were not transient: they persisted a decade after the crisis and after the LLR policies had ended. Treated individuals were also less likely to move across state lines, particularly among young, less-educated men. Together, the evidence points to a persistent reallocation of labor shaped by firm survival and local industrial activity.

[12] Appendix Table A5 and A6 report these regression results.

These findings show that financial stabilization policies can have lasting real effects—not only by averting collapse but by altering the long-run composition of economic activity and the spatial distribution of opportunity. LLR policy, often viewed narrowly as a short-run tool to manage liquidity and financial contagion, can function more broadly as a regional development instrument. Future research could explore whether similar dynamics occur in modern crises, such as the 2008 financial crisis or pandemic-era interventions, particularly where firm survival and credit access varied across space. In addition, understanding the heterogeneous incidence of these effects across workers, industries, or institutions could shed light on the broader distributive consequences of finance during crises. As central banks continue to evolve as crisis managers, identifying the long-run channels through which liquidity support translates into economic restructuring remains an important frontier.

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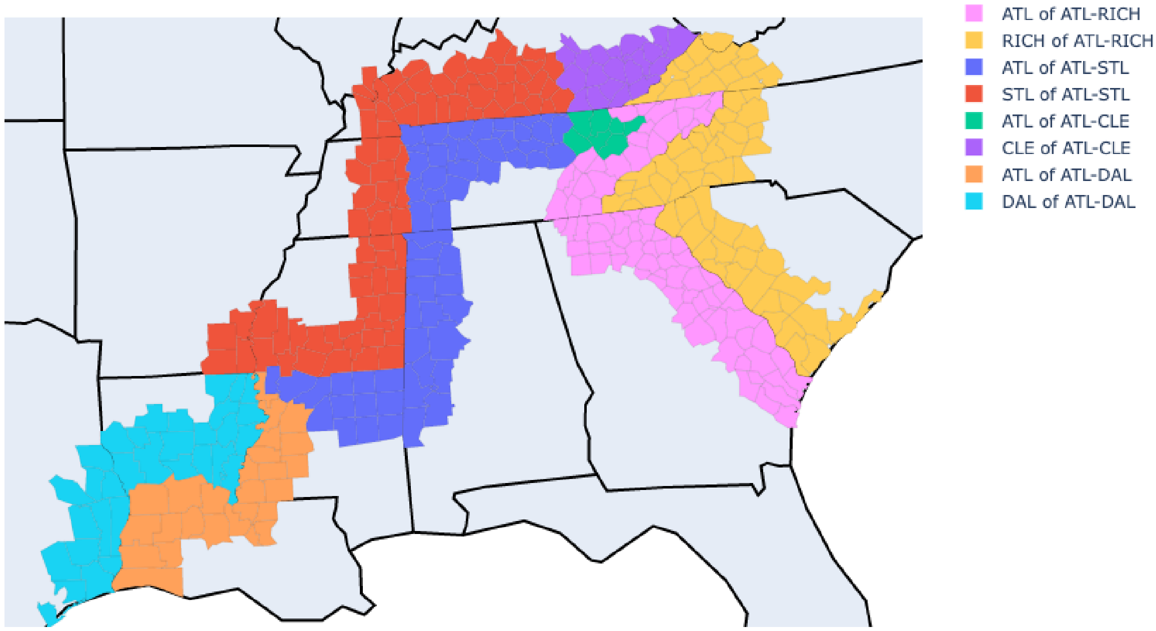
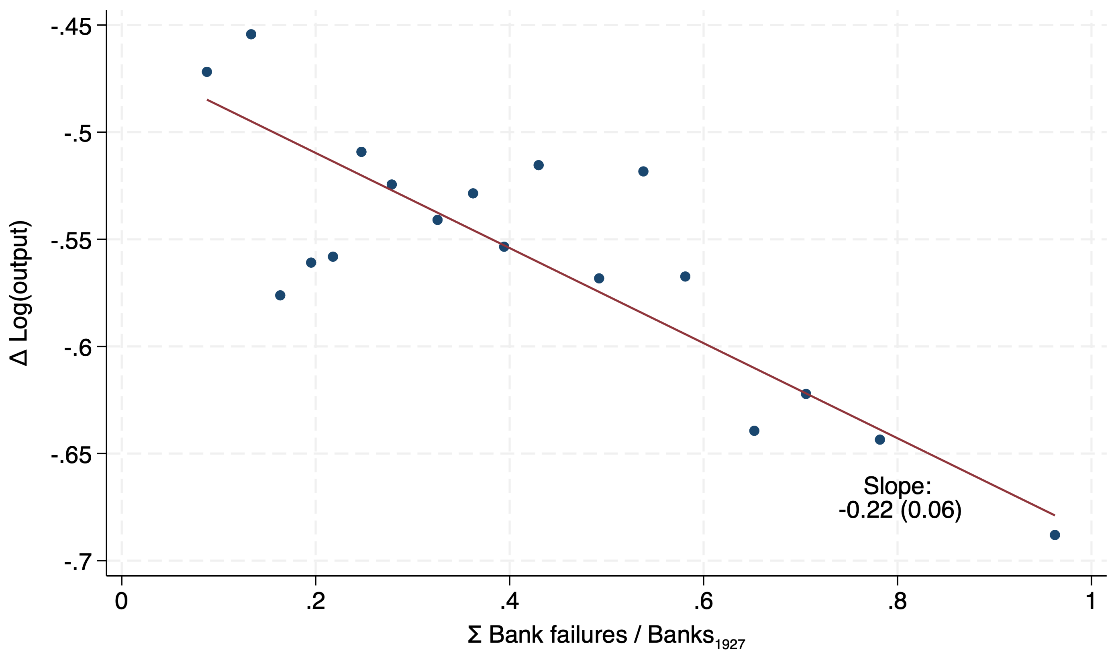
Figure 1: Counties arrund the Atlanta Federal Reserve District Border



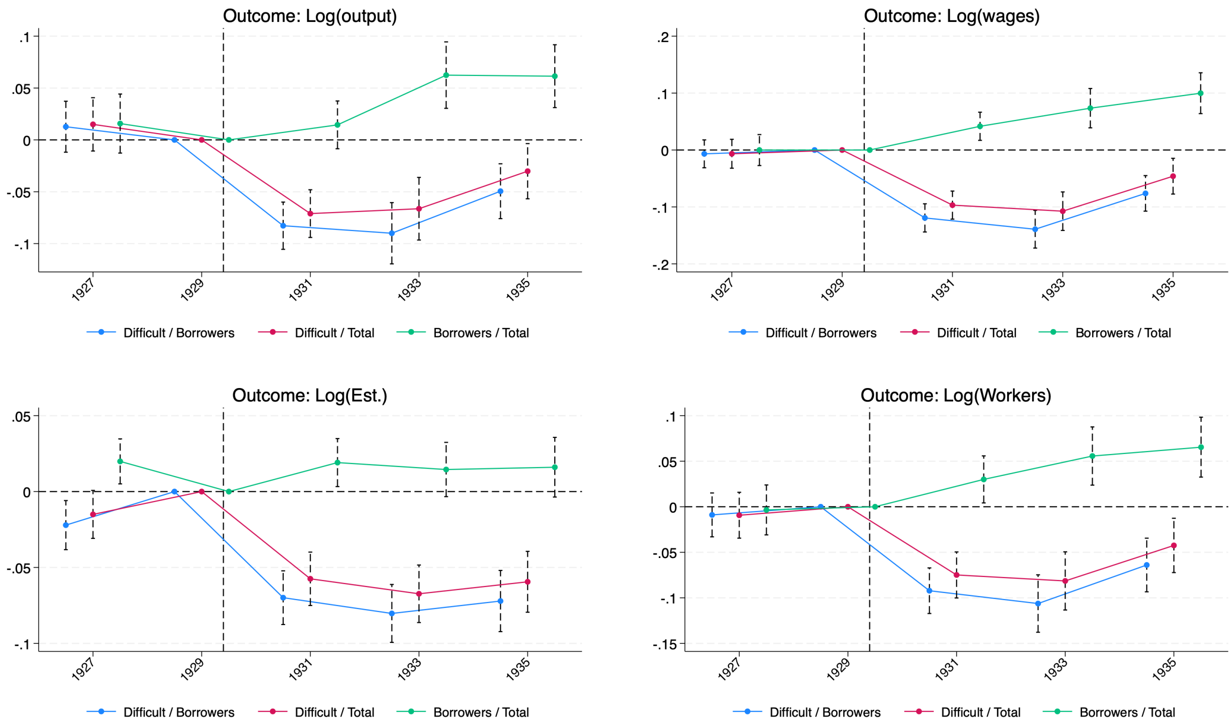
Note: This map shows the border regions of the Atlanta Federal Reserve district. The four border regions are: Atlanta - St. Louis, Atlanta - Dallas, Atlanta - Cleveland, and Atlanta - Richmond. The 50 mile buffer was generated using Geographic Information System (GIS) software.

Figure 2: Manufacturing Decline during the Great Depression and Banking Failures



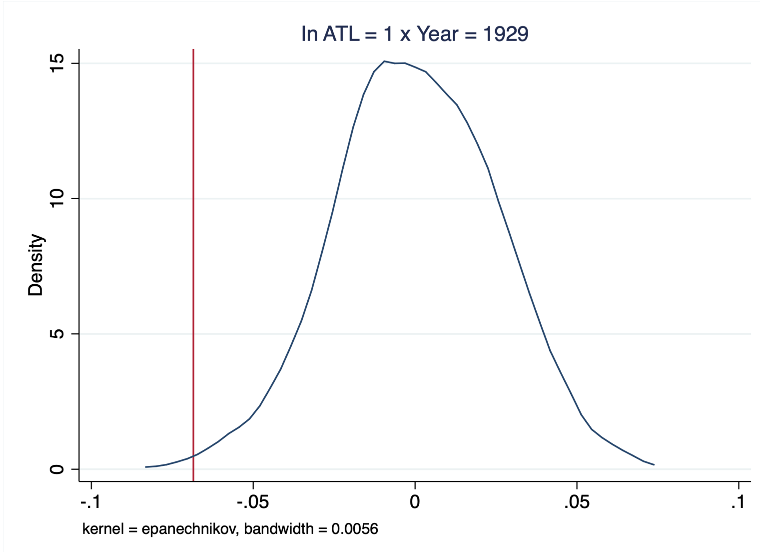
Note: This figure plots a binscatter plot of county-level change in the log of manufacturing output in real dollars between 1933 and 1929 (y-axis) and the cumulative number of bank suspensions (1929-1933) as a proportion of all banks in 1927 (x-axis). Only the counties with at least one suspended bank during 1929-1933 and manufacturing activity in both 1933 and 1929 are included (1,623 counties). Manufacturing data comes from Census of Manufactures and the banking data comes from the FDIC. Standard error in parenthesis.

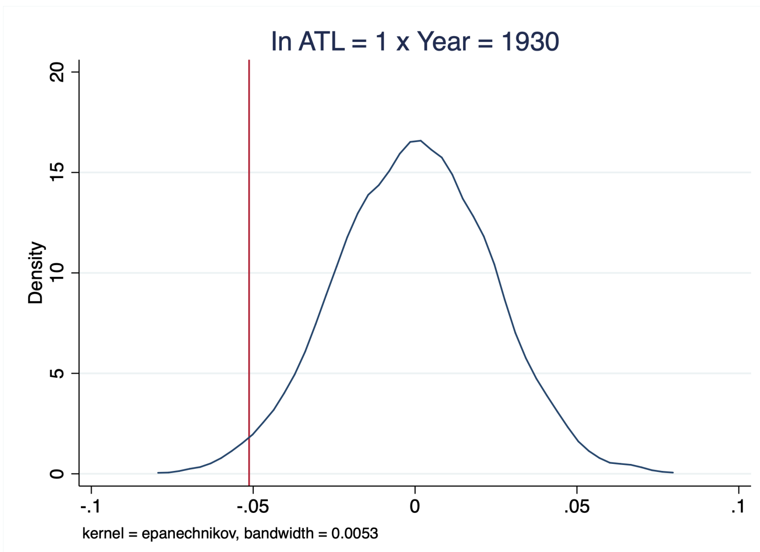
Figure 3: Manufacturing and Industry-Weighted Credit Difficulty Measures at the County Level



Notes: This figure plots the estimated coefficients of the *constraint* x year variables in Equation 4.1 using three separate standardized (mean zero, standard deviation one) measures of county-level *constraint* in 1934. The blue line shows the estimates when *constraint* is defined as the industry-weighted share of manufacturing borrowers experiencing difficulty obtaining credit within the county. The green lines shows them when *constraint* is defined as industry-weighted share of borrowers. Finally, the red line shows them when *constraint* is defined as industry-weighted share of all firms reporting borrowing difficulty. Controls include state by year fixed effects and average manufacturing plant size tercile by year fixed effects. Size is measured by number of wage earners in 1929. The outcome variables come from Census of Manufactures. The time period is 1927 - 1935 (biennially) for all specifications and the standard errors are clustered at the county level. 90 percent confidence intervals shown.

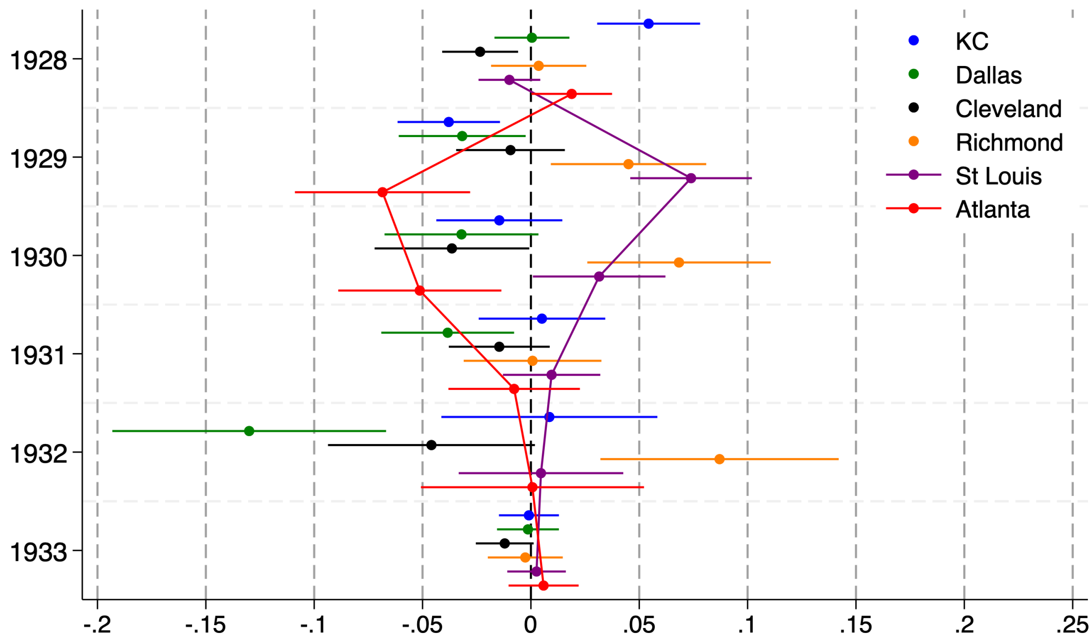
Figure 4: Distribution of Estimated Coefficients of 1,000 Placebo Border Permutations





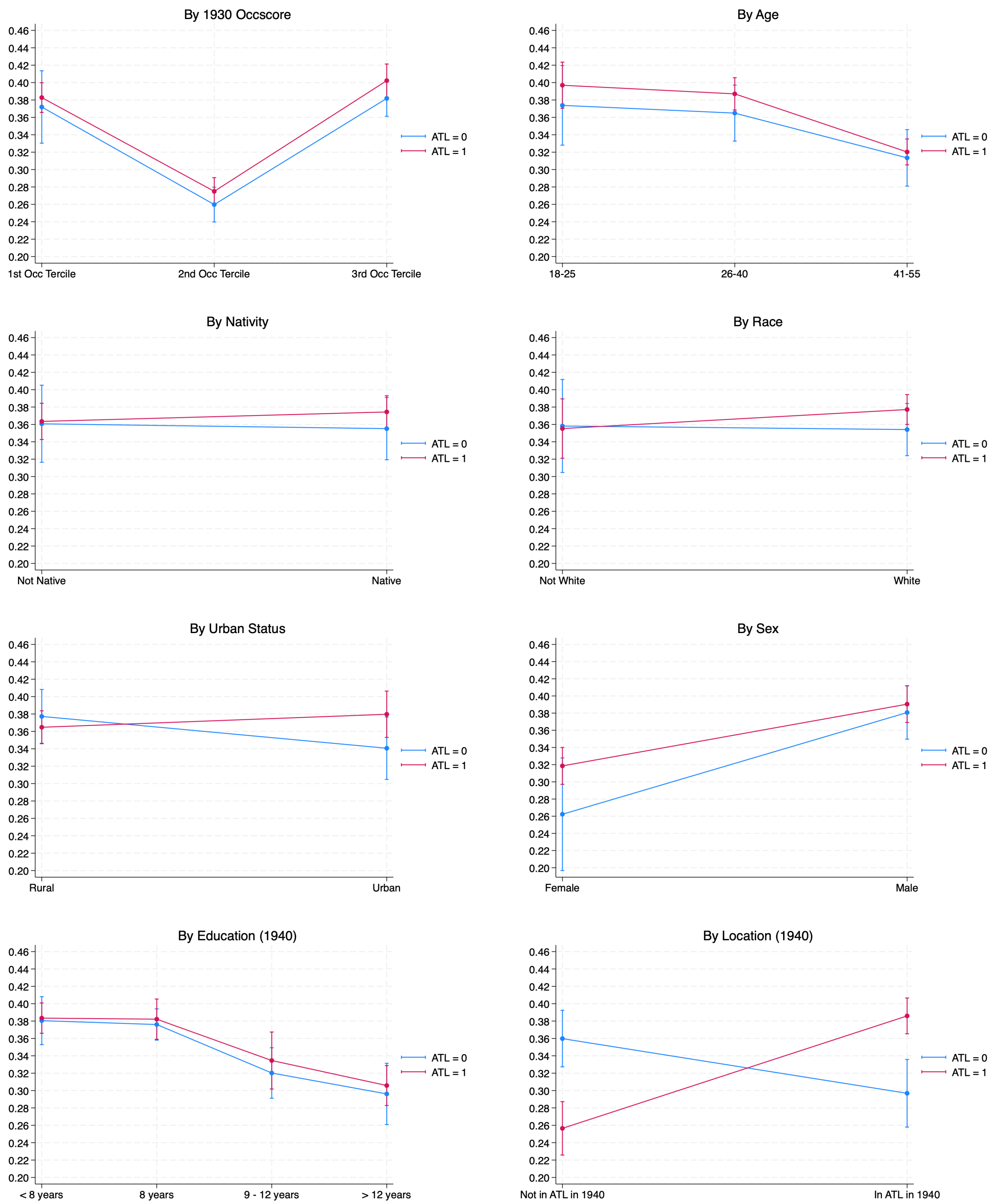
Notes: This figure plots the distributions of placebo effects computed using a randomization test as follows: using the tools developed in Heß (2017), I resample each county's Atlanta status ("in" vs. "out") within each border-region, keeping the share of "in" and "out" counties constant. I conduct this 1,000 times and re-estimate Equation 5.1 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 5: Estimates across Federal Reserve Boundaries



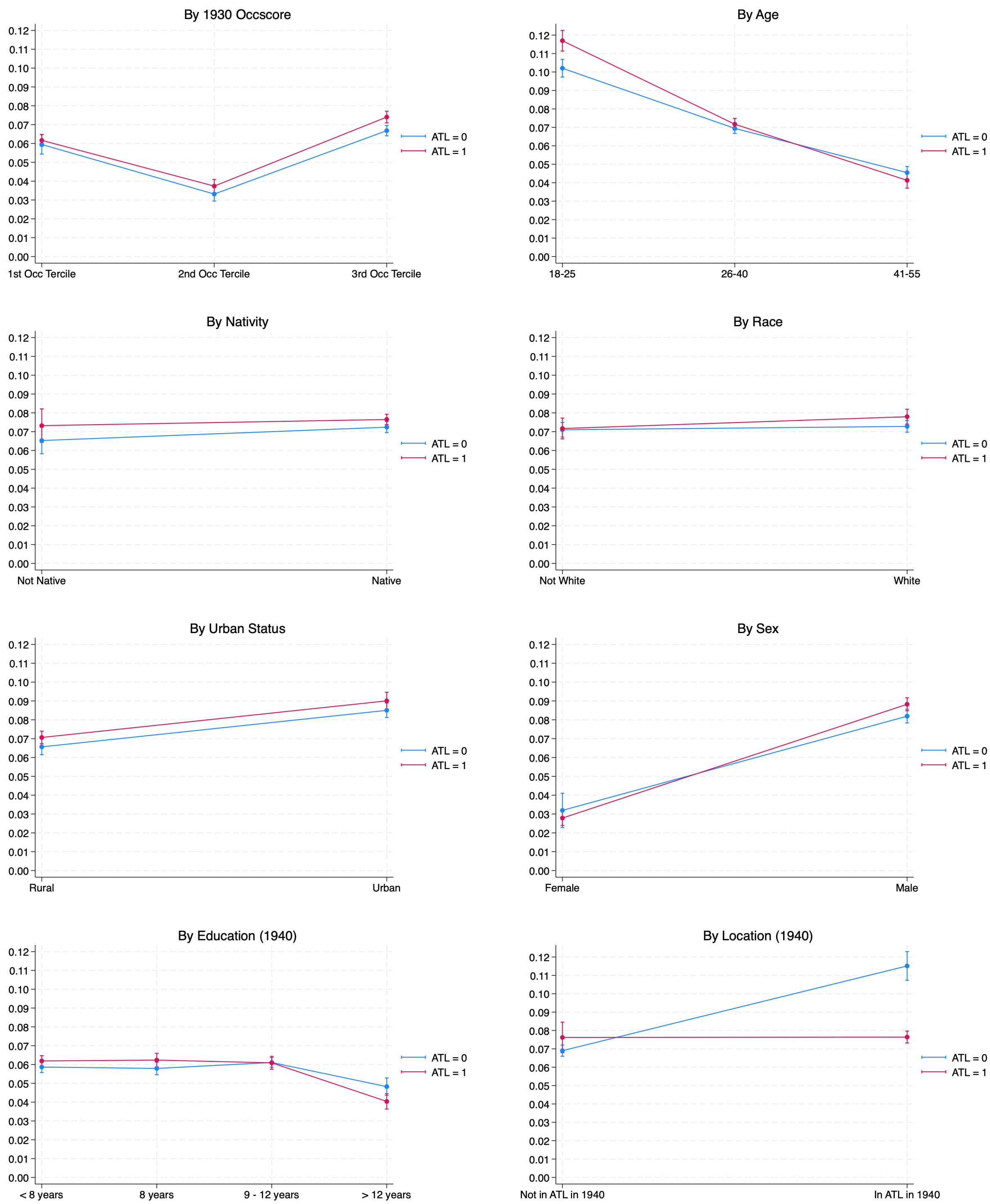
Notes: This figure plots the estimated coefficients γ_i from Equation 5.1 using the Atlanta, St. Louis, Richmond, Cleveland, Dallas, and Kansas City Federal Reserve border regions in separate regressions. The outcome variable is the annual suspension rate of all banks within a county. Control variables 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 6: Predicted Probabilities of Manufacturing Retention by Subgroups



Notes: This figure plots the predicted probabilities of retention for various sub-groups of manufacturing workers inside (ATL = 1) and outside (ATL = 0) the Atlanta district border based on the regression in Equation 6.2. The sample includes all working-age individuals (men and women ages 18–55 in 1930) residing in the three states bisected by the Atlanta federal reserve district: Mississippi, Tennessee, and Louisiana. Individual level controls include age, sex, state of residence in 1930 and 1940, the logarithm of occupational income score in 1930. County-level controls include historical manufacturing retention computed using linked census data between 1910 and 1920. Standard errors are clustered at the county level.

Figure 7: Predicted Probabilities of Manufacturing Entry by Subgroups



Notes: This figure plots the predicted probabilities of entry by 1940 into manufacturing for various sub-groups inside (ATL = 1) and outside (ATL = 0) the Atlanta district border based on the regression in Equation 6.2. The sample includes all working-age individuals (men and women ages 18–55 in 1930) residing in the three states bisected by the Atlanta federal reserve district: Mississippi, Tennessee, and Louisiana. Individual level controls include age, sex, state of residence in 1930 and 1940, the logarithm of occupational income score in 1930. County-level controls include historical manufacturing retention computed using linked census data between 1910 and 1920. Standard errors are clustered at the county level.

Table 1: Summary Statistics

Panel A: County-level data

	count	mean	sd	p5	p25	p50	p75	p95
Banks (active - all)	3,650	3.51	2.57	1.00	2.00	3.00	5.00	9.00
Banks (suspended - all)	3,650	0.18	0.55	0.00	0.00	0.00	0.00	1.00
Banks (suspended - national)	3,650	0.03	0.19	0.00	0.00	0.00	0.00	0.00
Banks (suspended - state)	3,650	0.14	0.47	0.00	0.00	0.00	0.00	1.00
Deposits (active - all, mil)	3,650	2.60	7.87	0.06	0.40	0.91	1.96	7.57
Deposits (suspended - all, mil)	3,650	0.10	0.82	0.00	0.00	0.00	0.00	0.36
Deposits (suspended - national, mil)	3,650	0.04	0.50	0.00	0.00	0.00	0.00	0.00
Deposits (suspended - state, mil)	3,650	0.06	0.61	0.00	0.00	0.00	0.00	0.22
Capitalization ratio	1,840	0.15	0.05	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
Output (mil)	1,013	6.06	20.28	0.12	0.55	1.33	3.99	18.64
Wages (mil)	1,013	0.98	2.33	0.02	0.12	0.28	0.79	3.51
Establishments	1,023	24.57	31.99	5.00	10.00	16.00	26.00	77.00
Wage Earners (k)	1,021	1.32	2.62	0.04	0.22	0.50	1.19	4.76
Borrower-Difficulty	356	0.48	0.06	0.39	0.44	0.48	0.53	0.56
RZ-Difficulty	356	0.20	0.04	0.11	0.18	0.21	0.24	0.26
Observations	3650							

Panel B: Linked census data

	count	mean	sd	p5	p25	p50	p75	p95
I(Manufacturing 1930)	1,883,289	0.09	0.284	0.00	0.00	0.00	0.00	1.00
I(Manufacturing 1940)	1,883,289	0.09	0.279	0.00	0.00	0.00	0.00	1.00
I(Atlanta)	1,883,289	0.65	0.478	0.00	0.00	1.00	1.00	1.00
I(Native)	1,883,289	0.96	0.201	1.00	1.00	1.00	1.00	1.00
I(Urban)	1,883,289	0.35	0.477	0.00	0.00	0.00	1.00	1.00
I(Male)	1,883,289	0.51	0.500	0.00	0.00	1.00	1.00	1.00
Years of Schooling	1,856,465	7.59	4.013	0.00	5.00	8.00	10.00	16.00
Age 1930	1,883,289	32.86	9.843	19.00	24.00	32.00	40.00	50.00
I(White)	1,883,289	0.76	0.430	0.00	1.00	1.00	1.00	1.00
I(Moved Counties 1930-1940)	1,883,289	0.25	0.433	0.00	0.00	0.00	1.00	1.00
I(Moved States 1930-1940)	1,883,289	0.10	0.294	0.00	0.00	0.00	0.00	1.00
County Manu Retention 1910-1920	1,883,289	0.33	0.085	0.18	0.27	0.34	0.40	0.44
County Manu Entry 1910-1920	1,883,289	0.05	0.023	0.02	0.03	0.05	0.08	0.08
County Manu Migration (County) 1910-1920	1,883,289	0.45	0.151	0.15	0.35	0.44	0.54	0.68
County Manu Migration (State) 1910-1920	1,883,289	0.23	0.094	0.10	0.16	0.23	0.30	0.37
Observations	1883289							

Notes: Panel A presents the summary statistics for all counties within 50 miles of the Atlanta Federal Reserve District border (see Figure 1). 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. Output (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. Borrower-Difficulty is an industry-weighted share of small and medium manufacturers facing credit difficulties. RZ-Difficulty is an industry-weighted external capital dependency measure from Rajan and Zingales (1998). Panel B presents the summary statistics of linked census data in Mississippi, Tennessee, and Louisiana between 1930 and 1940. See text for details.

Table 2: Credit Difficulty and Manufacturing Outcomes across U.S. Counties, 1927 - 1937

	Outcome:			
	Rev.	Wages	#Est	Wage Earners
Year=1927 × Borrower-Difficulty	0.01	-0.01	-0.02**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Year=1931 × Borrower-Difficulty	-0.08***	-0.12***	-0.07***	-0.09***
	(0.01)	(0.02)	(0.01)	(0.02)
Year=1933 × Borrower-Difficulty	-0.09***	-0.14***	-0.08***	-0.11***
	(0.02)	(0.02)	(0.01)	(0.02)
Year=1935 × Borrower-Difficulty	-0.05***	-0.08***	-0.07***	-0.06***
	(0.02)	(0.02)	(0.01)	(0.02)
R-sq	0.41	0.35	0.33	0.16
N	8,615	8,615	9,345	9,290
State x Year FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
E[Size] Quartile x Year	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the estimated coefficients of Equation 4.1 across all U.S. counties where *constraint* (“Borrower-Difficulty") is an industry-weighted measure of credit access difficulty that uses the 1927 manufacturing industry by county establishment data and the 1935 credit survey of manufacturing industries. Controls include state by year fixed effects and average manufacturing plant size tercile by year fixed effects. Size is measured by number of wage earners in 1929. The omitted baseline interaction is 1929 across all specifications. The outcome variables come from Census of Manufactures. Samples are balanced. The time period is 1927 - 1935 (biennially) for all specifications and the standard errors are clustered at the county level.

Table 3: Covariate Balance in 1927

County

	All	In	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)
Log(Output)	16.33	16.39	16.27	-0.12
	(1.44)	(1.43)	(1.45)	(0.56)
Log(Wages)	14.73	14.82	14.65	-0.17
	(1.46)	(1.40)	(1.50)	(0.42)
Log(Wage Earners)	8.16	8.32	8.02	-0.30
	(1.37)	(1.22)	(1.48)	(0.13)
Log(# Establishments)	4.72	4.81	4.64	-0.17
	(0.72)	(0.76)	(0.68)	(0.10)
Bank Suspension Rate (All)	0.01	0.01	0.01	0.00
	(0.06)	(0.05)	(0.06)	(0.48)
Borrower-Difficulty	0.48	0.48	0.48	0.00
	(0.06)	(0.06)	(0.06)	(0.80)
RZ-Difficulty	0.20	0.21	0.20	-0.00
	(0.04)	(0.04)	(0.05)	(0.33)
Observations	365	170	195	365

Industry

	All	In	Out	Difference
Paper and allied	0.436	0.434	0.438	0.004
	(0.340)	(0.337)	(0.343)	(0.910)
Food and kindred products	0.231	0.219	0.242	0.023
	(0.237)	(0.233)	(0.240)	(0.360)
Textiles	0.139	0.146	0.133	-0.013
	(0.243)	(0.230)	(0.253)	(0.613)
Rubber	0.073	0.077	0.069	-0.009
	(0.147)	(0.146)	(0.149)	(0.577)
Petroleum and coal	0.049	0.048	0.049	0.001
	(0.086)	(0.102)	(0.069)	(0.928)
Forest products	0.008	0.016	0.001	-0.014**
	(0.048)	(0.068)	(0.013)	(0.004)
Printing and publishing	0.013	0.007	0.019	0.012*
	(0.057)	(0.027)	(0.073)	(0.053)
Leather	0.026	0.030	0.022	-0.008
	(0.081)	(0.104)	(0.055)	(0.353)
Stone, clay, glass	0.001	0.003	0.000	-0.003
	(0.017)	(0.025)	(0.002)	(0.143)
Nonferrous metals	0.007	0.006	0.007	0.001
	(0.023)	(0.018)	(0.027)	(0.717)
Transportation equipment	0.006	0.004	0.007	0.002
	(0.036)	(0.024)	(0.044)	(0.550)
Miscellaneous	0.010	0.008	0.012	0.004
	(0.049)	(0.042)	(0.054)	(0.424)
Observations	356	164	192	356

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

Notes: This table reports variable averages among counties within 50 miles of the Atlanta Federal Reserve District border. Column “All" reports the averages for all counties along the border and columns “In" and “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. “Borrower-Difficulty" is an industry-weighted measure of credit access difficulty that uses the 1927 manufacturing industry by county establishment data and the 1935 credit survey of manufacturing industries. “RZ-Difficulty" is an industry-weighted measure of external finance dependence that uses the 1927 manufacturing industry by county establishment data and industry measures derived in Rajan and Zingales (1998). Chemicals industry - zero establishments - is omitted.

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

Panel A: Suspension rates

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

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

Panel B: Active rates

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

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in Equation 5.1. 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. The sample period is 1926 - 1933. The standard errors are clustered at the county level.

Table 5: Manufacturing Results at the County Level

Panel A: Mississippi

	Log(Output)		Log(Workers)		Log(Establishments)	
	1929-1931	1929-1935	1929-1931	1929-1935	1929-1931	1929-1935
Year=1931 × In ATL=1	0.26*	0.26*	0.39***	0.39***	0.57***	0.57***
	(0.14)	(0.14)	(0.14)	(0.14)	(0.17)	(0.17)
Year=1933 × In ATL=1		0.16		0.25		0.64***
		(0.17)		(0.17)		(0.17)
Year=1935 × In ATL=1		0.01		0.17		0.48***
		(0.19)		(0.19)		(0.18)
R-sq	0.71	0.59	0.61	0.33	0.65	0.56
N	94	187	94	188	94	188
Year FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Panel B: All Atlanta Fed States

	Log(Output)		Log(Workers)		Log(Establishments)	
	1929-1931	1929-1935	1929-1931	1929-1935	1929-1931	1929-1935
Year=1931 × In ATL=1	0.21***	0.21***	0.25***	0.25***	0.15	0.15
	(0.06)	(0.06)	(0.08)	(0.08)	(0.09)	(0.10)
Year=1933 × In ATL=1		0.18**		0.23***		0.14
		(0.07)		(0.07)		(0.11)
Year=1935 × In ATL=1		0.08		0.18**		0.12
		(0.07)		(0.07)		(0.09)
R-sq	0.66	0.01	0.43	0.01	0.46	0.00
N	717	1,423	717	1,432	717	1,435
State x Year FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the results of the regression in Equation 6.1. The balanced sample includes U.S. counties with at least $5,000 nominal dollars of manufacturing output in 1927 - 1935, biennially, within Mississippi (Panel A) and the entire Atlanta Federal Reserve border region (Panel B). Standard errors are clustered at the county level.

Table 6: Labor Response, 1930 - 1940

Panel A: I(Manufacturing in 1940)

	MS		MS + TN + LA	
	Manu 1930	Non-Manu 1930	Manu 1930	Non-Manu 1930
	(1)	(2)	(3)	(4)
In ATL=1	0.029*	0.007**	0.018	0.004**
	(0.017)	(0.003)	(0.020)	(0.002)
E[Y]	0.30	0.06	0.37	0.07
R-sq	0.02	0.04	0.03	0.04
N	29,458	273,534	162,446	919,399
Individual Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Panel B: Log Occscore Change

	MS		MS + TN + LA	
	Manu 1930	Non-Manu 1930	Manu 1930	Non-Manu 1930
	(1)	(2)	(3)	(4)
In ATL=1	-0.023	0.022**	-0.009	0.016
	(0.014)	(0.009)	(0.009)	(0.011)
E[Y]	-0.07	0.17	-0.04	0.14
R-sq	0.14	0.44	0.15	0.40
N	27,335	234,149	146,770	801,170
Individual Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Pane C: Migration

	P(Move County)		P(Move State)	
	Manu 1930	Manu 1930	Manu 1930	Manu 1930
	(1)	(2)	(3)	(4)
In ATL=1	-0.013	-0.004	-0.035***	-0.029**
	(0.020)	(0.019)	(0.011)	(0.011)
E[Y]	0.38	0.27	0.17	0.13
R-sq	0.35	0.35	0.01	0.03
N	29,458	162,446	29,460	162,447
Individual Controls	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
State FE	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the results of the regression in Equation 6.2. The sample includes all 1930-1940 linked working-age individuals (men and women ages 18–55 in 1930) residing in the three states bisected by the Atlanta federal reserve district: Mississippi, Tennessee, and Louisiana. Individual level controls include age, sex, state of residence in 1930 and 1940, the logarithm of occupational income score in 1930. County-level controls include historical manufacturing retention (columns 1/3 in panels A and B), entry (columns 2/4 in panels A and B), and migration rates (panel C) computed using linked census data between 1910 and 1920. Columns "Manu 1930" use only manufacturing workers in 1930 in the regression, while "Non-Manu 1930" use those not in manufacturing. Standard errors are clustered at the county level.

Online Appendix

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A Data Appendix

A.1 Industry-level External Dependence Measures

A.1.1 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 A.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.[13]

(Figure A.1 around here)

I utilize the aggregated results from this survey to compute industry-level metrics of credit difficulty, as outlined in Figure A.1, Panel B. My analysis draws from three key data points from Tables 2 and 26 of the Survey: (1) the share of borrowers facing borrowing challenges (my preferred method for proxying for credit difficulty, henceforth *Borrower-Difficulty*), (2) the number of borrowing firms, and (3) the share of all firms struggling to borrow. I compute a county credit difficulty as the weighted average of (1) across industries within the county, where the weights are determined by output shares. Across all U.S. counties, this measure has an average of 0.45 with a standard deviation of 0.056.

A.1.2 External Dependence based on Compustat

The timing of the 1935 firm survey, conducted several years into the Great Depression, presents a potential limitation as it reflects financial constraints *ex post* rather than at the onset of the economic downturn, when the differential impacts of Reserve Bank policies were most pronounced. This timing could introduce selection bias, as firms that failed due to financial constraints prior to 1935 would not appear in the survey, potentially leading to a misrepresentation of industries. Specifically, industries that lost many firms early in the recession might appear less constrained in the survey, as only the more resilient "survivors" are represented.

To address this issue, I use the framework and industry data established by Rajan and Zingales (1998), which measures external financial dependence as an inherent property of industries determined by their technological characteristics using data on publicly traded firms from Standard and Poor's *Compustat* database. Their approach captures long-term, structural financing needs

[13] 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.

rather than transitory conditions, reducing the risk of distortion from firm survival or failure during specific periods. Moreover, they use industry medians to summarize external dependence, ensuring that results reflect broader industry-level characteristics rather than being dominated by large, surviving firms. By aggregating data over time in the 1980s and smoothing temporal fluctuations, the Rajan and Zingales (1998) industry proxies (henceforth *RZ-Difficulty*) provide a more robust measure of financial constraints, mitigating the limitations associated with the survey's timing.

A.2 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)).[14] 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.

A.3 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

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

States (Stewart (1929)).[15]

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.[16]

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.[17]

One 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.[18]

A.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 1. My approach closely mirrors the method employed by Jalil (2014). I also transcribe the

[15] 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.

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

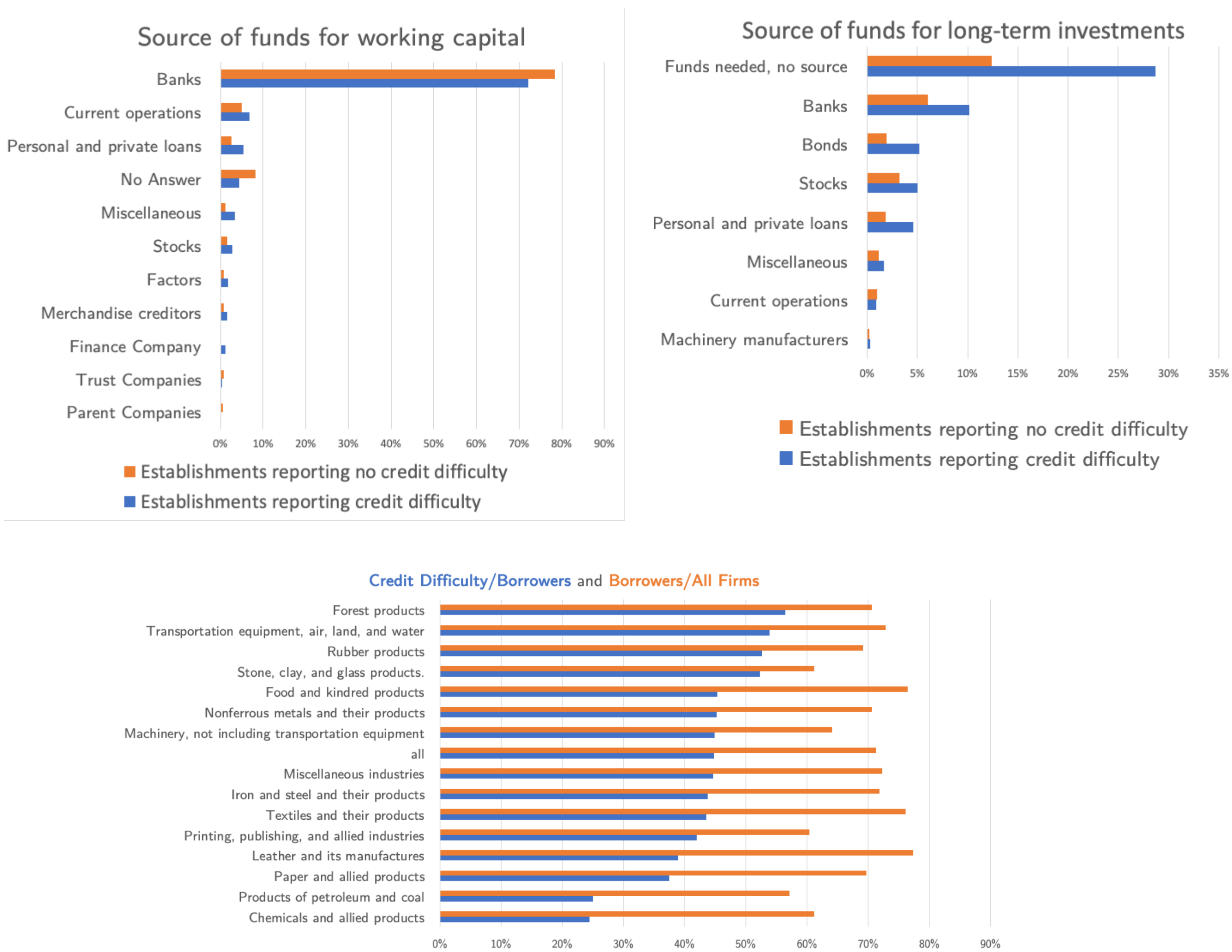
[17] The industries include food and kindred products, textiles, iron and steel, forest products, leather, rubber, paper and allied products, printing and publishing, chemicals, petroleum and coal, stone/clay/glass, nonferrous metals, machinery, transportation equipment, and miscellaneous.

[18] For robustness, I compute analogous shares using total wages paid to wage-earners, total number of wage earners, and the number of establishments, and none of the results change.

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.

B Appendix Figures

Figure A.1: Financing of Small and Medium U.S. Manufacturers, 1934



Note: 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 A.1 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*.

C Appendix Tables

Table A1: 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	0.008	-0.024	-0.048	-0.000	0.021	0.007	0.004	0.014	0.006	0.007
	(0.013)	(0.017)	(0.008)	(0.014)	(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	0.018	0.017	0.026	0.012	0.029	0.019	0.009	0.024	0.018	0.023*
	(0.011)	(0.012)	(0.015)	(0.012)	(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**	-0.063**	-0.133	-0.066	-0.066**	-0.078**	-0.068**	-0.052*	-0.061*	-0.083***	-0.070**
	(0.025)	(0.028)	(0.029)	(0.024)	(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*	-0.051**	-0.189	-0.106*	-0.039	-0.075*	-0.051*	-0.031	-0.047*	-0.053**	-0.068**
	(0.023)	(0.027)	(0.031)	(0.024)	(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	0.001	-0.056	-0.020	-0.022	0.012	-0.008	-0.018	0.005	-0.004	-0.013
	(0.018)	(0.020)	(0.028)	(0.020)	(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	-0.002	0.029	0.002	-0.019	0.028	0.001	0.058	0.002	-0.016	-0.023
	(0.031)	(0.033)	(0.036)	(0.032)	(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	0.002	-0.031	-0.010	-0.004	0.021	0.006	0.000	0.012	0.004	0.007
	(0.010)	(0.009)	(0.007)	(0.010)	(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	0.17	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	2,757	302	907	1,805	1,015	2,820	1,765	1,715	2,605	2,375
Border-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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in Equation 5.1. 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 (2) 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 while column (8) includes only counties in the 25-100 mile range. Column (9) uses spatial standard errors using the methods of Colella et al. (2019). Columns (10) - (13) remove one border segment at a time. The time period is 1926 - 1933 for all specifications and the standard errors are clustered at the county level.

Table A2: 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	-0.002	-0.025	0.001	-0.004	0.004	-0.004	-0.003	0.003	-0.006	-0.007
	(0.020)	(0.023)	(0.018)	(0.022)	(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	0.003	0.052	-0.048*	-0.004	0.020	0.006	-0.024	0.015	0.006	0.021
	(0.015)	(0.016)	(0.017)	(0.015)	(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	-0.014	0.043	-0.007	-0.014	0.007	-0.007	-0.038	0.014	-0.006	0.000
	(0.020)	(0.023)	(0.023)	(0.021)	(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	0.052*	0.112	0.029	0.069*	0.075	0.073*	0.019	0.091**	0.080**	0.091***
	(0.030)	(0.034)	(0.035)	(0.030)	(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	0.008	0.069	0.028	0.020	0.040	0.030	-0.018	0.060	0.032	0.039
	(0.029)	(0.032)	(0.041)	(0.029)	(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)	(0.026)	(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	0.029	0.082	0.028	0.055	0.030	0.048	-0.002	0.060	0.047	0.077**
	(0.028)	(0.032)	(0.038)	(0.029)	(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	0.64	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	2,797	312	919	1,838	1,023	2,861	1,783	1,742	2,645	2,413
Border-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

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

Notes: This table reports the estimated coefficients of the in-ATL x year fixed effects in the generalized difference-in-differences specification of Equation 5.1. 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 (2) 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 while column (8) includes only counties in the 25-100 mile range. Column (9) uses spatial standard errors using the methods of Colella et al. (2019). Columns (10) - (13) remove one border segment at a time. The time period is 1926 - 1933 for all specifications and the standard errors are clustered at the county level.

Table A3: Credit Difficulty and Manufacturing Outcomes across U.S. Counties, 1927 - 1937

Using Rajan Zingales 1998 Industry External Finance Dependence Measures

	Outcome:			
	Rev.	Wages	#Est	Wage Earners
Year=1927 × RZ-Difficulty	-0.01	-0.02	-0.03***	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Year=1931 × RZ-Difficulty	-0.06***	-0.08***	-0.05***	-0.06***
	(0.01)	(0.01)	(0.01)	(0.01)
Year=1933 × RZ-Difficulty	-0.08***	-0.10***	-0.05***	-0.07***
	(0.02)	(0.02)	(0.01)	(0.02)
Year=1935 × RZ-Difficulty	-0.04**	-0.05**	-0.05***	-0.05***
	(0.02)	(0.02)	(0.01)	(0.02)
R-sq	0.40	0.34	0.32	0.15
N	8,615	8,615	9,345	9,290
Year FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
State x Year	✓	✓	✓	✓
E[Size] Tercile x Year	✓	✓	✓	✓

Standard errors in parentheses

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

Note: This table reports the estimated coefficients of Equation 4.1 across all U.S. counties where *constraint* ("RZ-Difficulty") is an industry-weighted measure of credit access difficulty that uses the 1927 manufacturing industry by county establishment data and Rajan and Zingales (1998) industry classification of external finance dependence. Controls include state by year fixed effects and average manufacturing plant size tercile by year fixed effects. Size is measured by number of wage earners in 1929. The omitted baseline interaction is 1929 across all specifications. The outcome variables come from Census of Manufactures. Samples are balanced. The time period is 1927 - 1937 (biennially) for all specifications and the standard errors are clustered at the county level.

Table A4: Manufacturing Results Robustness

All Atlanta Fed States

	Log(Output)						Log(Workers)						Log(Establishments)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Year=1931 × In ATL=1	0.18***	0.10	0.24***	0.22***	0.22***	0.21***	0.19***	0.16**	0.28***	0.24***	0.21**	0.23***	0.10	0.16**	0.15	0.14	0.08	0.14
	(0.06)	(0.07)	(0.06)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.10)	(0.08)	(0.07)	(0.07)	(0.09)	(0.10)	(0.10)	(0.09)
Year=1933 × In ATL=1	0.10	0.07	0.20***	0.19**	0.21**	0.18**	0.14*	0.14*	0.25***	0.22***	0.23**	0.22**	0.12	0.15*	0.14	0.12	0.03	0.12
	(0.08)	(0.08)	(0.08)	(0.08)	(0.10)	(0.09)	(0.07)	(0.09)	(0.07)	(0.08)	(0.10)	(0.09)	(0.08)	(0.08)	(0.11)	(0.11)	(0.12)	(0.11)
Year=1935 × In ATL=1	0.00	-0.04	0.09	0.07	0.16	0.06	0.09	0.09	0.20***	0.14*	0.17*	0.13	0.09	0.13**	0.12	0.09	0.06	0.10
	(0.08)	(0.08)	(0.07)	(0.08)	(0.10)	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)	(0.09)	(0.09)	(0.08)	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)
R-sq	0.13	0.00	0.01	0.01	0.00	0.01	0.10	0.00	0.01	0.01	0.00	0.01	0.20	0.01	0.00	0.00	-0.00	0.00
N	1,423	1,400	1,332	1,071	1,015	987	1,432	1,409	1,340	1,078	1,022	994	1,435	1,412	1,343	1,079	1,023	995
State-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry Comp x Year	✓						✓						✓					
Base = 1927		✓						✓						✓				
Δ 1927-29 x Year			✓						✓						✓			
Distance: 25mi-100mi				✓						✓						✓		
Log(AAA grants) x Year					✓						✓						✓	
Log(RFC grants) x Year						✓						✓						✓

Standard errors in parentheses

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

Notes: This table reports the results of the regression in Equation 6.1 for the various robustness checks described in the text. The balanced sample includes U.S. counties with at least $5,000 nominal dollars of manufacturing output in 1927 - 1935, biennially, within Mississippi (Panel A) and the entire Atlanta Federal Reserve border region (Panel B). Standard errors are clustered at the county level.

Table A5: Heterogeneity of Retention

	MS + TN + LA							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In ATL=1 × 1st Occ Tercile	-0.004							
	(0.017)							
In ATL=1 × 3rd Occ Tercile	0.005							
	(0.008)							
In ATL=1 × 18-25		0.001						
		(0.012)						
In ATL=1 × 41-55		-0.015						
		(0.011)						
In ATL=1 × Native			0.017					
			(0.020)					
In ATL=1 × White				0.026				
				(0.019)				
In ATL=1 × Urban					0.052**			
					(0.024)			
In ATL=1 × Male						-0.047		
						(0.030)		
In ATL=1 × $<$ 8 years							-0.003	
							(0.014)	
In ATL=1 × 9 - 12 years							0.008	
							(0.018)	
In ATL=1 × $>$ 12 years							0.004	
							(0.018)	
In ATL=1 × In ATL in 1940								0.192***
								(0.017)
E[Y]	0.37	0.37	0.37	0.37	0.37	0.37	0.36	0.37
R-sq	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
N	166,852	162,446	162,446	162,446	162,446	162,446	116,457	162,261
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the results of eight regressions of Equation 6.2 augmented with an interaction term between β and the variable listed in each row. The sample includes all 1930-1940 linked working-age individuals (men and women ages 18–55 in 1930) residing in the three states bisected by the Atlanta federal reserve district: Mississippi, Tennessee, and Louisiana. Individual level controls include age, sex, state of residence in 1930 and 1940, the logarithm of occupational income score in 1930. County-level controls include historical manufacturing retention computed using linked census data between 1910 and 1920. Standard errors are clustered at the county level.

Table A6: Heterogeneity of Entry

	MS + TN + LA							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
In ATL=1 × 1st Occ Tercile	-0.002							
	(0.004)							
In ATL=1 × 3rd Occ Tercile	0.003							
	(0.002)							
In ATL=1 × 18-25		0.013***						
		(0.003)						
In ATL=1 × 41-55		-0.006***						
		(0.002)						
In ATL=1 × Native			-0.004					
			(0.006)					
In ATL=1 × White				0.004				
				(0.004)				
In ATL=1 × Urban					0.000			
					(0.003)			
In ATL=1 × Male						0.010*		
						(0.006)		
In ATL=1 × $<$ 8 years							-0.001	
							(0.002)	
In ATL=1 × 9 - 12 years							-0.005**	
							(0.002)	
In ATL=1 × $>$ 12 years							-0.012***	
							(0.004)	
In ATL=1 × In ATL in 1940								-0.046***
								(0.005)
E[Y]	0.06	0.07	0.07	0.07	0.07	0.07	0.06	0.07
R-sq	0.05	0.04	0.04	0.04	0.04	0.04	0.03	0.04
N	1,716,436	919,399	919,399	919,399	919,399	919,399	697,323	918,770
Individual Controls	✓	✓	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓
State FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: This table reports the results of eight regressions of Equation 6.2 augmented with an interaction term between β and the variable listed in each row. The sample includes all 1930-1940 linked working-age individuals (men and women ages 18–55 in 1930) residing in the three states bisected by the Atlanta federal reserve district: Mississippi, Tennessee, and Louisiana. Individual level controls include age, sex, state of residence in 1930 and 1940, the logarithm of occupational income score in 1930. County-level controls include historical manufacturing entry computed using linked census data between 1910 and 1920. Standard errors are clustered at the county level.