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Claremont McKenna College

Spatial Spillovers in Emergency Lending: Evidence from Bayesian Quantile Models of Reconstruction Finance Corporation Lending During the Great Depression

submitted to

Professor Angela Vossmeyer

by

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for

Senior Thesis

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Abstract

This thesis investigates whether emergency lending by the Reconstruction Finance Corporation (RFC) generated spatial spillovers across U.S. counties between 1932 and 1936. Using a monthly panel of 2,979 counties constructed from digitized RFC card index records, we estimate Bayesian quantile regression models for binary longitudinal data (QBLD) that allow spillover effects to vary across the conditional distribution of lending propensity. Neighboring lending exposure is positively associated with own-county lending outcomes across a wide range of quantiles and subsamples, with the strongest effects at the lower tail, where counties are least likely to receive lending. This pattern is consistent with a mechanism in which nearby lending increases the visibility of the program, enhances its credibility, and improves its perceived accessibility, particularly in places with limited prior engagement with federal credit. Decomposing adjacent lending by charter type reveals that the state-bank channel consistently dominates the national-bank channel, suggesting that spatial diffusion operated primarily through state-chartered banking networks. These findings suggest that evaluating emergency lending programs solely on the basis of direct local exposure understates their total impact, with implications extending from the Depression-era RFC to modern interventions such as TARP and the Paycheck Protection Program.

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1 Introduction

Financial crises pose a recurring challenge to economic stability. When banking systems come under severe stress, the resulting disruptions to credit, confidence, and economic activity rarely remain confined to their point of origin. Instead, financial distress tends to propagate across institutions and regions, transmitted through interbank relationships, depositor behavior, trade networks, and shared expectations. Governments have repeatedly responded to such episodes with emergency lending programs designed to stabilize affected institutions and prevent localized failures from escalating into broader systemic collapse. A central question for evaluating these interventions is whether their effects remain limited to the institutions and localities that directly receive support, or whether they extend into surrounding areas through various channels.

The Great Depression offers a powerful historical setting for studying this question. Bank failures, collapsing asset values, shrinking credit, and widespread economic uncertainty created conditions in which local financial distress could easily become regional or national instability. In response, the federal government increasingly relied on emergency intervention to stabilize banking institutions and restore confidence. Among the most important of these efforts was the Reconstruction Finance Corporation (RFC), which became a major source of support for banks and other institutions during the early 1930s. Existing literature has shown that RFC assistance mattered for bank survival and for the functioning of financial markets, making it a natural setting for studying how crisis-era public lending affected local economies.

The relevance of this question extends well beyond the 1930s. During the 2007–2009 Global Financial Crisis (GFC), the Troubled Asset Relief Program (TARP) and Federal Reserve emergency lending facilities channeled hundreds of billions of dollars to banks and financial institutions. More recently, the COVID-19 pandemic prompted the Paycheck Protection Program (PPP) and other emergency credit interventions that were distributed through banking intermediaries to businesses and localities nationwide. In each episode,

the geographic reach and indirect effects of government-supported lending have been central concerns for policymakers and researchers alike. By studying the spatial dynamics of RFC lending during the Depression, this thesis speaks directly to the broader question of whether emergency lending programs generate effects that extend beyond their immediate recipients.

We ask whether RFC-era government lending to banks generated spatial spillovers across counties between 1932 and 1936. More specifically, we investigate whether the effects of RFC lending were limited to the counties whose banks directly received support, or whether those effects extended into neighboring counties as well. This question matters because local banking systems were not sealed off from one another. Banks and depositors observed nearby lending decisions, local balance-sheet conditions, and signs of federal support, so information about government assistance could travel across county lines even when the loans themselves did not. In this sense, lending in neighboring counties may have increased the visibility of the program, enhanced its credibility, and improved its perceived accessibility, particularly in areas with limited prior engagement with federal credit. If RFC lending to banks in one county stabilized nearby banks through indirect channels such as transparency, information spillovers, or improved confidence, then analyses that focus only on directly exposed counties may understate the total impact of federal intervention.

The idea of spatial spillovers is especially relevant in a crisis setting. During a severe banking contraction, conditions in one local banking market can influence nearby banking markets through confidence effects, interbank ties, depositor responses, and information revealed by government support decisions. This makes it plausible that RFC assistance, even if formally directed at specific banks in specific counties, may have improved conditions for nearby banks as well.

Empirically, we study county-level monthly panel data from 1932 to 1936. The core banking data come from digitized card index records of the Reconstruction Finance Corporation, converted into usable data frames following the methodology of Amujala, Vossmeyer, and Das (2023), recording county-level bank counts, total assets, and closures. County adja-

cency is defined through a Queen-contiguity spatial weights matrix derived from U.S. Census TIGER shapefiles.

We mainly estimate two Bayesian quantile binary panel models of RFC lending outcomes, following the QBLD framework of Rahman and Vossmeyer (2019). The first (main) model regresses a binary indicator of whether banks in a county receive any positive RFC lending on lagged own-county RFC lending and a lagged binary indicator of whether any adjacent county's banks received positive RFC lending. The second (decomposed) model replaces the composite adjacent indicator with two separate binary indicators: lagged adjacent state-bank RFC lending and lagged adjacent national-bank RFC lending, allowing us to distinguish spillovers by bank type given that national banks were more directly connected to Federal Reserve liquidity channels such as the discount window. This distinction may matter because differences in outside liquidity access could have affected banks' need for RFC assistance and the extent to which RFC lending to banks in neighboring counties translated into subsequent local lending outcomes. To capture various forms of heterogeneity, both models include county-specific random intercepts and a full set of sample-month (year×month) time fixed effects. Both models are estimated for the full sample, bottom-25% and rest-75% population subsamples at quantiles ranging from 0.1 to 0.9, and for each of the twelve Federal Reserve districts at quantiles 0.1, 0.5, and 0.9.

Three main findings emerge. First, neighboring lending exposure is positively associated with own-county lending outcomes across a wide range of specifications, quantiles, and subsamples. The evidence is strongest at the lower tail of the conditional distribution, where counties are least likely to receive lending—precisely where spillover support from neighbors would matter most. Second, when the composite adjacent indicator is decomposed, the state-bank channel consistently dominates the national-bank channel in magnitude, suggesting that the spatial diffusion of lending during the Depression operated primarily through state-chartered banking networks rather than nationally chartered ones. Third, the relationship is heterogeneous across county size and Federal Reserve districts: the rest-75% subsample

shows stable, significant effects throughout the distribution, while the bottom-25% subsample and individual district runs reveal more variable patterns with effects concentrated at the tails.

Historical work has established that the RFC played a central role in financial stabilization during the Depression. Mason (2003) showed that RFC allocations were not simply driven by standard rent-seeking measures, which makes geographic patterns of lending and their consequences worth studying more closely. Vossmeyer (2016) provided a multivariate selection-and-treatment framework tailored to the RFC's three-stage "apply/approve/perform" process, highlighting how program participation itself had positive effects on bank lending and was endogenous. Calomiris, Mason, Weidenmier, and Bobroff (2013) provided perhaps the most direct mechanism for this thesis: their evidence that RFC assistance improved the survival of Michigan banks suggests that emergency lending could stabilize institutions in ways that might plausibly benefit neighboring counties as well.

Modern studies of the Paycheck Protection Program show that bank intermediation shaped the geographic allocation and effects of emergency credit during a modern crisis. Granja, Makridis, Yannelis, and Zwick (2022) and Autor et al. (2022) document substantial heterogeneity in how PPP funds were distributed and transmitted across places and firms. Anbil, Carlson, and Styczynski (2023) show that the Federal Reserve's PPPLF facility causally increased PPP lending, especially among smaller community banks, and that the facility also operated as a funding backstop by encouraging lending even among banks that did not borrow from it directly. Their results also suggest a broader pattern relevant for this thesis: banks that were already familiar with interacting with government liquidity programs were more likely to engage with a new one when it became available. More recently, Berger, Karolyi, and Roman (2025) study the Bank Term Funding Program during the 2023 Regional Banking Crisis and find that subsidized BTFP borrowing allowed banks to substitute cheaper facility funds for deposits while mitigating what would otherwise have been a sharper contraction in credit provision, broadly consistent with the Bagehot Principle.

These studies reinforce the broader idea that government-supported liquidity can generate uneven and indirect regional effects through financial intermediaries and local conditions. Related evidence from outside banking points in the same direction: Bloom, Schankerman, and Van Reenen (2013) document large inter-firm technology spillovers, and Moretti, Steinwender, and Van Reenen (2025) show that defense-funded R&D raised productivity beyond directly funded locations. Together, this literature supports the general proposition that government-driven shocks can produce meaningful spillover effects beyond directly treated units.

Cameron and Trivedi (2005) provide the general panel-data and fixed-effects framework needed for county-by-time analysis. Anselin (1988) is foundational for thinking about adjacency-based spillovers and about how outcomes in one location may depend on conditions in neighboring locations. Lee and Yu (2010) extended spatial autoregressive models into panel data with fixed effects, which is conceptually close to the central question of this thesis. The key insight from this literature is that geographically proximate units often influence one another, and ignoring that structure can understate or distort policy effects.

Reich, Fuentes, and Dunson (2011) developed a Bayesian spatial quantile framework, and Dai, Yan, Tian, and Tang (2020) developed quantile methods for spatial panel data models with fixed effects. These papers are relevant because the effect of neighboring lending may not be constant across the distribution of county outcomes. Rahman and Vossmeyer (2019) developed the Bayesian quantile regression model for binary longitudinal data (QBLD) that this thesis employs directly. Their framework places an asymmetric Laplace distribution on the error term, enabling quantile-specific inference for binary outcomes while accommodating random effects for panel structure.

This work contributes to the existing literature in three important ways that speak directly to why spatial spillovers in emergency lending matter for both historical interpretation and modern policy design. First, it brings a spatial perspective to the analysis of RFC lending, motivated by the idea that financial stabilization during the Great Depression

was unlikely to remain confined within county boundaries. If lending to one bank increases the probability that nearby banks receive funds, then the RFC's impact extends beyond directly treated institutions, implying that studies focusing only on direct exposure may substantially underestimate the true reach and effectiveness of the program. More broadly, documenting such patterns provides evidence that government interventions can generate information spillovers—through mechanisms such as increased transparency, learning, or signaling—which shape how financial resources are allocated across space. Second, the paper employs a Bayesian quantile regression framework for binary longitudinal data, allowing spillover effects to vary across the conditional distribution rather than imposing a constant mean effect; this is particularly important in crisis settings where responses may be highly heterogeneous across locations. Third, it separates overall adjacent lending exposure into state-bank and national-bank components, making it possible to identify whether different institutional channels transmitted these spillovers differently, and thereby offering more precise insight into how the structure of the banking system influences the diffusion of policy effects.

The rest of the thesis proceeds as follows. Section 2 describes the data sources, variable construction, and summary statistics. Section 3 presents the econometric methodology. Section 4 reports the main estimation results. Section 5 discusses alternative approaches and robustness. Section 6 concludes.

2 Data

2.1 Data Sources

The empirical analysis uses a county-level monthly panel covering 1932 to 1936, coming from digitized card index records of the RFC, converted into structured data frames using the machine learning and natural-language processing pipeline developed by Amujala, Vossmeyer, and Das (2023). Their methodology leverages AWS Textract for optical character recog-

nition and AWS Comprehend for entity classification, enabling the systematic extraction of bank names, locations, loan dates, loan types, decisions, and amounts from over 22,000 archival card images held at the National Archives. The resulting dataset records county-level bank counts, total assets, and closures. The panel universe is defined as all counties with at least one operating bank in the January 1932 snapshot, and these counties are then followed through all subsequent months to 1936. RFC lending records, including separate counts and dollar amounts for state-chartered banks, nationally chartered banks, and non-bank borrowers, are merged from a separate monthly lending panel. County population is incorporated from historical census data. Because population is not observed at monthly frequency, it is treated as a time-invariant county characteristic over the 1932–1936 sample period; each county is assigned its first observed population value, typically corresponding to the 1930 Census, and that value is carried across all months in the panel.

2.2 Data Aggregation and Cleaning

County FIPS codes are assigned by matching county and state names to the U.S. Census TIGER county shapefile, with manual corrections applied for historical name changes, independent cities, and consolidated city-counties. In particular, special adjustments are required for cases such as Virginia's independent cities and New York City boroughs. After geographic identifiers are standardized, the county panel is used to construct a Queen-contiguity spatial weights matrix, in which two counties are treated as neighbors if they share either a border or a vertex. Using this matrix, adjacent lending exposure variables are computed by aggregating lending indicators across all neighboring counties. The resulting adjacency structure is binary and unweighted, so each neighboring county contributes equally to the spillover measure.

2.3 Panel Dataset and Variable Construction

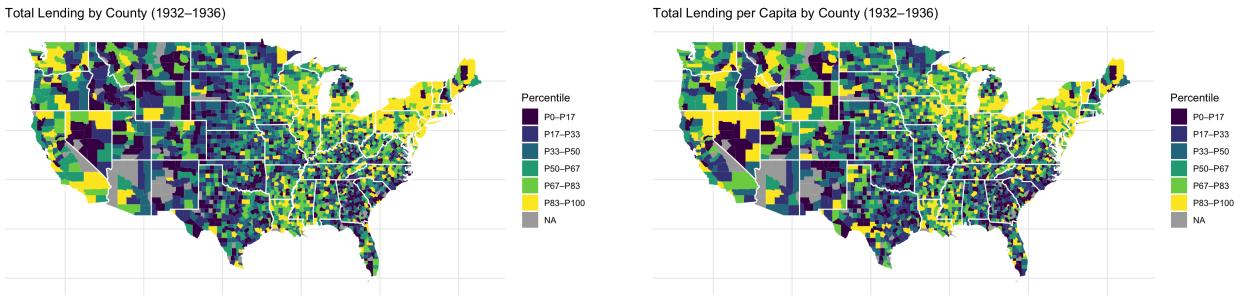
The final panel contains 142,992 county-month observations: 2,979 counties observed over 48 months from January 1932 through December 1935. The panel is balanced—every county appears in all 48 months. Because all regressors enter in one-period lagged form, the first month for each county is consumed by the lag and the QBLD estimation samples contain at most 47 usable periods per county. For district-specific and population-split subsamples in the decomposed specification, an iterative rebalancing procedure ensures that every retained county appears in every retained period, which may reduce the estimation sample further. The core lending variable, $\text{FinalAmount_Bank}_{it}$, records the total dollar amount of RFC lending to banks in county i during month t, with analogous variables recording lending separately to state-chartered banks ($\text{FinalAmount_StateBank}_{it}$) and nationally chartered banks ($\text{FinalAmount_NationalBank}_{it}$). The panel also includes adjacent-county aggregates constructed from the Queen-contiguity matrix: $\text{Adjacent_FinalAmount_Bank}_{it}$, $\text{Adjacent_FinalAmount_StateBank}_{it}$, and $\text{Adjacent_FinalAmount_NationalBank}_{it}$, which sum lending across all counties sharing a border or vertex with county i.

From these continuous measures, we construct the binary and log-transformed variables used in estimation. The main dependent variable is FAB_binary_{it}, equal to one if county i receives any positive RFC lending to banks in month t. Only 12.7% of county-month observations record positive RFC lending to banks, reflecting the heavily zero-inflated nature of the data and motivating the binary outcome approach. The adjacent lending indicator AFAB_binary_{it} equals one if any neighboring county received positive bank lending; 43.9% of observations have at least one actively lending neighbor. Log-transformed continuous versions (log(FAB), log(AFAB), and their state-bank and national-bank decompositions) are also constructed for use in the alternative specifications described in Section 5. All regressors enter the models in one-period lagged form (prefixed L1_) to reduce simultaneity concerns and better capture whether prior lending activity in a county or its neighbors is associated with subsequent lending outcomes.

County-level controls drawn from census and agricultural sources include total population (Total_Pop). Population is available for 2,968 counties and is treated as time-invariant over the sample period. Each county is also assigned to one of twelve Federal Reserve districts using a county-to-district crosswalk, with one manual correction for FIPS 29210 (assigned to District 8). For the population-based sample split, each county's first non-missing population value is used to compute the 25th percentile threshold (approximately 10,644 residents). The 743 counties at or below this threshold form the bottom-25-percent subsample, while the remaining 2,225 counties form the rest-75-percent subsample. Both subsets retain the balanced panel structure. Banking characteristics from the January 1932 baseline snapshot—including initial bank counts (1932_banks) and total assets (1932_TotalAssets)—are available as cross-sectional controls capturing pre-sample financial conditions.

2.4 Descriptive Statistics and Motivating Evidence

The descriptive evidence establishes that RFC-related lending during 1932–1936 was geographically clustered rather than evenly dispersed. Figure 1 shows strong spatial concentration in total lending: upper-percentile counties (P83–P100) cluster visibly in the Northeast, Great Lakes, and parts of the Pacific coast, while much of the rural South, Great Plains, and Mountain West falls in the lowest percentile bins. Figure 1 also shows that the total lending per capita map partially reshuffles this picture—some Western and Southern counties move into higher per-capita bins despite modest absolute totals—but the overall pattern of geographic unevenness persists.



(a) Total lending by county

(b) Total lending per capita by county

Figure 1: Geographic distribution of RFC total lending, 1932–1936. Counties are binned by percentile of cumulative lending.

Figure 2 shows that Districts 2 (New York) and 7 (Chicago) each received over $1 billion in total lending, with District 4 (Cleveland) close behind at approximately $800 million. By contrast, Districts 9 (Minneapolis), 10 (Kansas City), and 11 (Dallas) each received under $150 million. Figure 2 also highlights a notable bank-share gradient across districts: Districts 4 and 5 show relatively low bank shares (around 40–50%), while Districts 9 and 12 show high bank shares (above 80%), suggesting that the institutional composition of lending and the economic stress of the Great Depression varied substantially across regions.

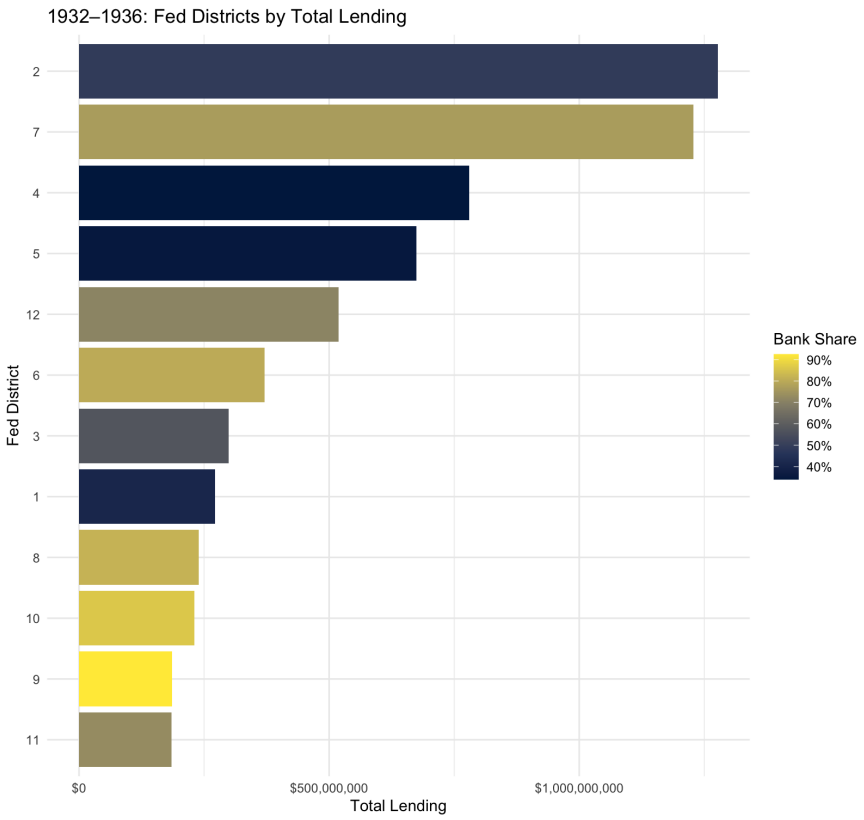
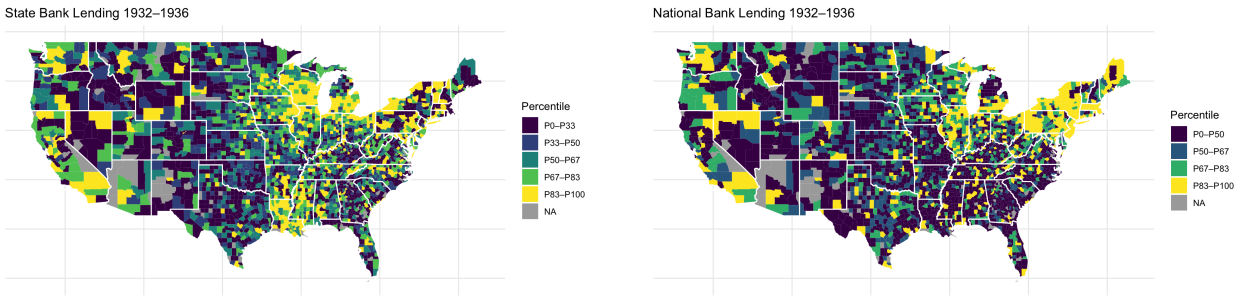


Figure 2: Total RFC lending by Federal Reserve district, 1932–1936. Bar shading indicates the bank share of total district lending.

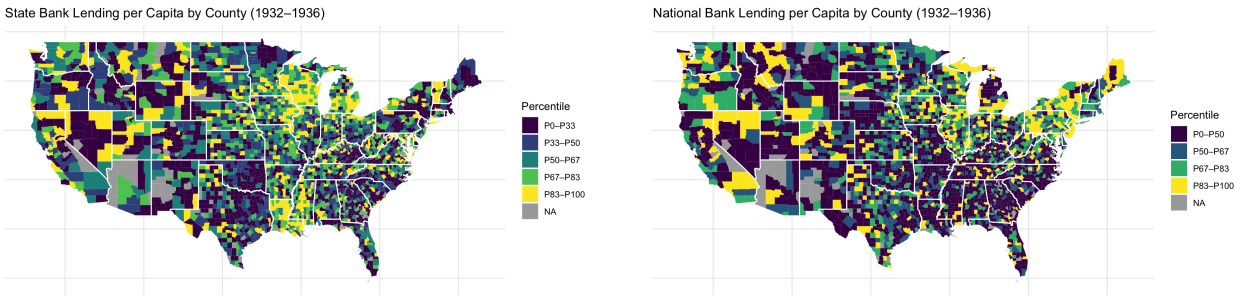
Figure 3 reveals an important asymmetry between state-bank and national-bank lending. The state-bank map uses five percentile bins starting at P0–P33, indicating that roughly a third of counties fall in the lowest category but with meaningful variation above. The national-bank map, by contrast, collapses its bottom bin into P0–P50—half of all counties received so little national-bank lending that the percentile breaks could not differentiate them—with upper-percentile national-bank counties concentrated in the Northeast and scattered urban centers. This descriptive pattern foreshadows the model result that the state-bank channel is the more geographically diffuse and statistically dominant source of neighboring lending exposure.



(a) State-bank lending (b) National-bank lending

Figure 3: Geographic distribution of RFC lending by bank charter type, 1932–1936.

Figure 4 reinforces this point using per-capita measures. State-bank lending per capita is more evenly spread across the country, with visible upper-percentile counties throughout the Midwest, parts of the South, and the Pacific coast. National-bank lending per capita remains heavily concentrated in the Northeast, with large swaths of the South and interior West in the P0–P50 bin.

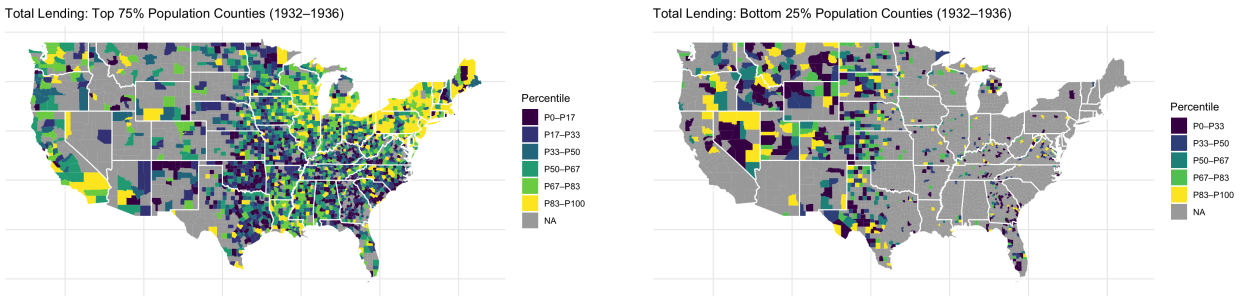


(a) State-bank lending per capita (b) National-bank lending per capita

Figure 4: Per capita RFC lending by bank charter type, 1932–1936.

Figure 5 further highlights structural differences across the two population subsamples. The top-75% population map shows dense, well-differentiated lending activity across the

eastern half of the country, with large gray (NA) areas in the West where small counties are excluded. The bottom-25% population map is its mirror image: small counties scattered across the West and rural South, many in the lowest percentile bins, with vast gray areas in the East where larger counties dominate. This visual contrast motivates the population-based sample splits in the model estimation.



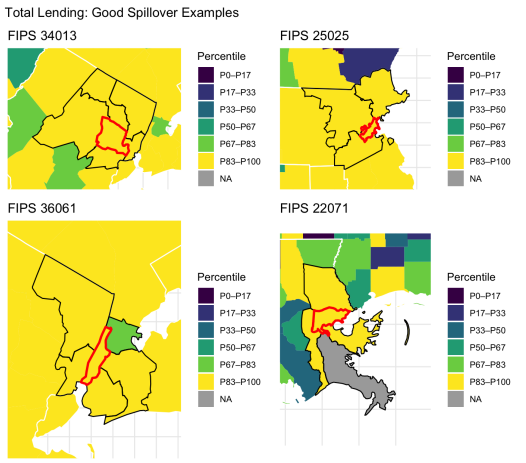
(a) Top 75% population counties

(b) Bottom 25% population counties

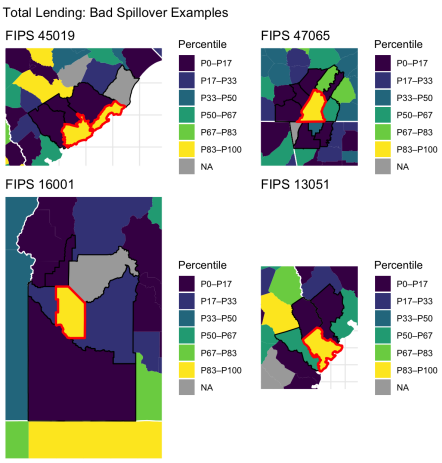
Figure 5: Total lending by county population subsample, 1932–1936. Gray (NA) areas indicate counties excluded from the respective subsample.

Finally, Figures 6, 7, and 8 provide direct visual motivation for the formal models. Figure 6 shows that the "good spillover" panels for total lending include counties like FIPS 34013 (Essex, NJ), 36061 (Manhattan, NY), and 22071 (Orleans Parish, LA) highlighted within neighborhoods where nearly every adjacent county falls in the P83–P100 bin—high-lending counties embedded in high-lending clusters. Figure 6 also shows the opposite configuration in the "bad spillover" panels: counties like FIPS 45019 (Charleston, SC) and 16001 (Ada, ID) are highlighted at upper percentiles but surrounded by neighbors predominantly in the P0–P17 or P0–P33 range—relatively active lending counties isolated from a low-lending neighborhood. Figures 7 and 8 show analogous patterns within the state-bank and national-bank channels. These maps do not prove causal spillovers, but they illustrate precisely

the kind of spatial configuration—local lending embedded in or isolated from a neighboring lending environment—that the QBLD models are designed to detect.

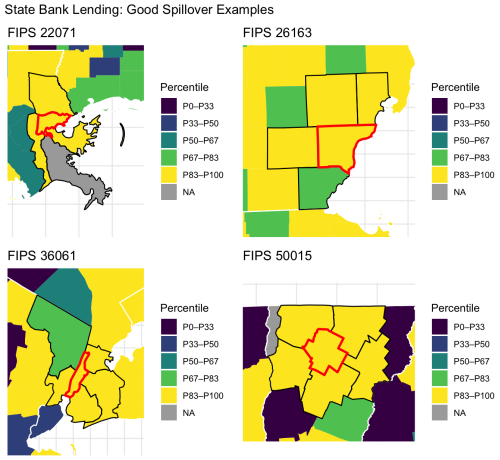


(a) Good spillover examples

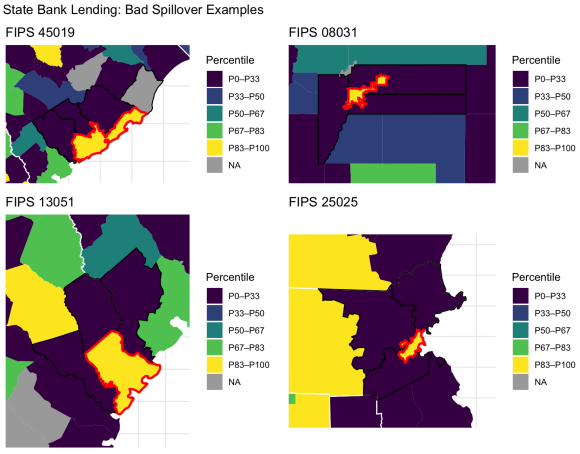


(b) Bad spillover examples

Figure 6: Total lending spillover examples. Good spillovers: high-lending counties embedded in high-lending neighborhoods. Bad spillovers: high-lending counties isolated in low-lending neighborhoods.

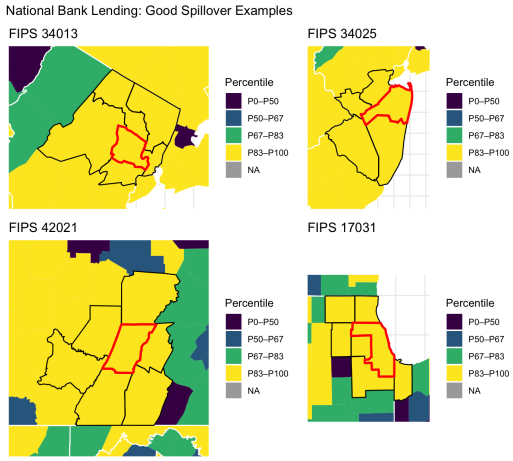


(a) Good spillover examples

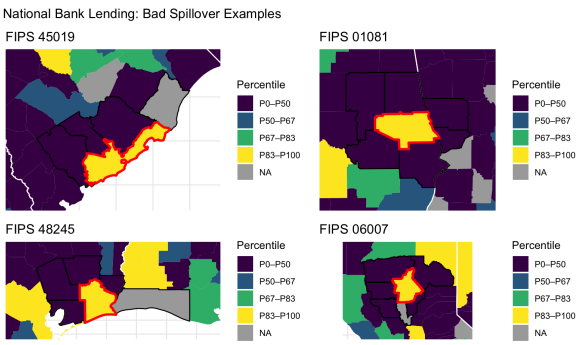


(b) Bad spillover examples

Figure 7: State-bank lending spillover examples.



(a) Good spillover examples



(b) Bad spillover examples

Figure 8: National-bank lending spillover examples.

3 Methodology

3.1 Overview of Methodological Framework

The empirical strategy requires a framework that can handle three features of the data simultaneously: a binary dependent variable (whether a county receives any positive bank lending), a panel structure with repeated county-month observations, and a desire to assess heterogeneity across the conditional distribution of lending propensity rather than focusing only on the conditional mean. Standard linear or logit panel models address the first two features but restrict attention to mean effects. Quantile regression methods allow distributional heterogeneity but are not directly suited to binary outcomes with panel structure. The Bayesian quantile regression model for binary longitudinal data (QBLD), developed by Rahman and Vossmeyer (2019) and implemented through the `qbld` package in R, addresses all three requirements.

3.2 Model Specification

The QBLD model is based on an asymmetric Laplace likelihood, which enables quantile-specific inference for a binary outcome. Because the dependent variable is binary, the estimated coefficients are interpreted as effects on the latent propensity underlying the observed lending indicator at different points of the conditional distribution. Each county enters the model with a random intercept to absorb unobserved time-invariant heterogeneity, while a full set of time fixed effects—one dummy for each sample-month (year×month) period—captures common macroeconomic shocks, seasonal forces, and policy changes that affect all counties simultaneously.

Main Specification: Let $y_{it} \in \{0, 1\}$ denote whether county i receives any positive bank lending in month t. Define the latent variable:

$$y_{it}^* = \beta_1(\tau)\,\text{L1_AFAB_binary}_{it} + \beta_2(\tau)\,\text{L1_FAB_binary}_{it} + \boldsymbol{\gamma}(\tau)'\mathbf{d}_t + \alpha_i + \varepsilon_{it}(\tau), \qquad (1)$$

where $\text{L1_AFAB_binary}_{it}$ is a one-period lag indicating whether any adjacent county received positive bank lending, $\text{L1_FAB_binary}_{it}$ is the lagged own-county lending indicator, $\mathbf{d}_t$ is a vector of time fixed effects (one indicator per sample-month period), α_i is a county-specific random intercept, and $\varepsilon_{it}(\tau)$ follows an asymmetric Laplace distribution indexed by quantile τ. The observed outcome is:

$$y_{it} = \mathbf{1}\{y_{it}^* > 0\}. \qquad (2)$$

Decomposed Specification: The decomposed model replaces the composite adjacent indicator with separate state-bank and national-bank channels:

$$\begin{aligned} y_{it}^* = {} & \beta_1(\tau)\,\text{L1_AFASB_binary}_{it} + \beta_2(\tau)\,\text{L1_AFANB_binary}_{it} \\ & + \beta_3(\tau)\,\text{L1_FAB_binary}_{it} + \boldsymbol{\gamma}(\tau)'\mathbf{d}_t + \alpha_i + \varepsilon_{it}(\tau), \end{aligned} \qquad (3)$$

where L1_AFASB_binary$_{it}$ and L1_AFANB_binary$_{it}$ are lagged binary indicators for adjacent state-bank and national-bank lending, respectively.

3.3 Estimation: MCMC

Posterior inference is based on the unblocked Gibbs sampler implemented in the `qbld` package. The MCMC algorithm draws from the full conditional distributions of the fixed effects, latent variables, random effects, and the random-effect variance parameter. Following the `qbld` implementation of Rahman and Vossmeyer (2019), we place a multivariate normal prior on the fixed-effects coefficients, $\beta \sim \mathcal{N}(b_0, B_0)$, with $b_0 = \mathbf{0}$ and $B_0 = I$. We also use the package-default hyperparameters for the prior on the random-effect variance, φ^2, with $c_1 = 9$ and $d_1 = 10$. We use 10,000 MCMC draws for most specifications and 20,000 draws for the lower quantiles (0.1 and 0.2), where convergence is slower. A burn-in of 20 percent is applied in all cases. Trace plots for the key coefficients and φ^2 across all quantiles show stable mixing around stationary values with no visible drift or trend, supporting adequate convergence of the sampler. Statistical significance is assessed using 95 percent posterior credible intervals, with coefficients treated as significant when the interval excludes zero.

4 Results

4.1 Main Model: Aggregate Spillover Results

The main model estimates the association between a county's probability of receiving RFC lending to banks and the composite binary indicator of whether any adjacent county's banks received positive RFC lending in the previous month, controlling for the county's own lagged RFC lending status. Interpreted substantively, a positive adjacent-county coefficient is consistent with spillovers across nearby banking markets: once banks in neighboring counties receive government support, nearby banks may become more likely to receive support as

well because information about federal intervention, bank condition, or program accessibility diffuses across space.

In the full sample, both AFAB and FAB are positive and statistically significant across all nine quantiles from 0.1 to 0.9. The effects are strongest at the lowest quantile: at $\tau = 0.1$, AFAB is approximately 2.0 and FAB approximately 1.6, both with 95% credible intervals well above zero. From $\tau = 0.2$ onward, both coefficients compress to a stable band—AFAB ranges from roughly 0.45 to 0.60, FAB from roughly 0.55 to 0.75—before FAB rises again at $\tau = 0.9$ to approximately 1.45. Importantly, the spillover effect is not trivial relative to a county's own serial dependence in lending. Across much of the distribution, the AFAB coefficient is of roughly the same order of magnitude as the FAB coefficient: it is larger at the lowest quantile, somewhat smaller across most middle quantiles, and still clearly positive throughout. In other words, neighboring-county lending exposure is not merely a weak secondary correlate once own-county persistence is taken into account; at several quantiles it is comparable to, and at $\tau = 0.1$ even stronger than, the effect of the county's own lagged lending status. This makes the substantive result stronger: the probability of receiving RFC lending appears to reflect not only persistence within a county, but also meaningful transmission across nearby counties.

Thus, adjacent-county lending appears to exert the stronger effect at the lower tail, where counties are least likely to receive lending, while at the remaining quantiles own-county lending persistence generally becomes somewhat more important than neighboring exposure. One interpretation is that neighboring lending mattered most in places with the lowest baseline propensity to receive RFC support because those counties may have been less familiar with the program or less connected to the channels through which information about federal credit circulated. In such settings, observing nearby banks obtain RFC assistance may have reduced uncertainty about how the program worked, whether banks like theirs could access it, or whether applying was worthwhile at all. This is consistent with an information-spillover mechanism: neighboring exposure is most valuable where familiarity

is weakest. By contrast, at the middle and upper quantiles, counties may already have had stronger banking networks, better access to information, or greater prior integration into RFC lending channels, so own-county persistence becomes the more stable predictor of continued lending. Even there, however, the AFAB estimates remain economically meaningful because their magnitudes stay fairly close to the FAB estimates through much of the conditional distribution rather than collapsing toward zero. Figure 9 shows this pattern clearly and suggests that neighboring lending exposure matters most in the lower tail of the conditional distribution, whereas own-county persistence is the more stable force through the middle and upper parts of the distribution.

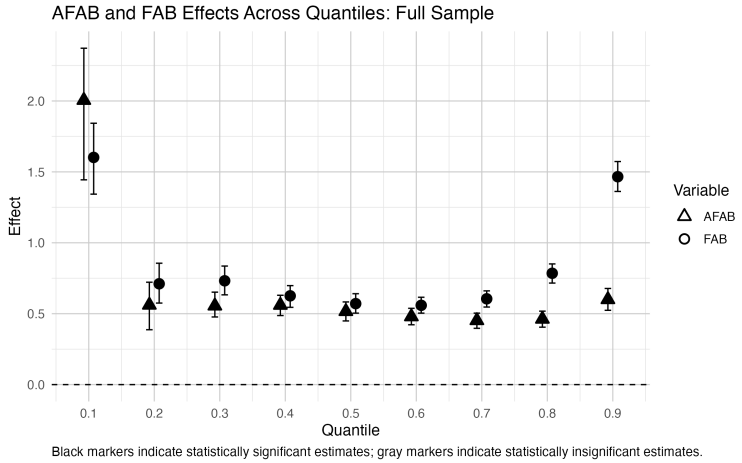


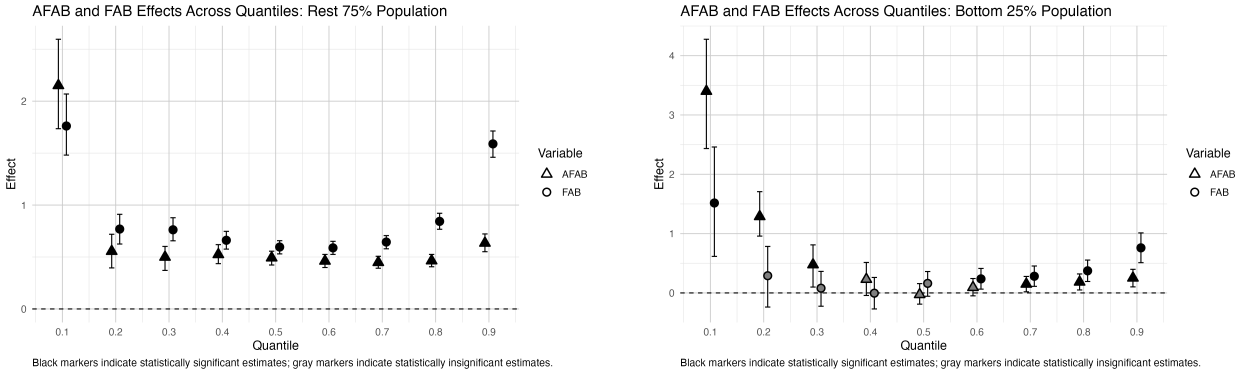
Figure 9: Baseline specification: AFAB and FAB coefficient estimates across quantiles, full sample. Triangles denote AFAB (adjacent lending); circles denote FAB (own-county lending). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

4.2 Population Subsample Results

Figure 10 shows that the rest-75% population subsample closely mirrors the full sample: both coefficients are positive and significant at every quantile, with the same large lower-tail spike and stable middle-range values. As in the full sample, AFAB exceeds FAB at $\tau = 0.1$, but beyond the lowest quantile the FAB coefficient is generally as large as or larger than AFAB. Still, the two are broadly similar in magnitude across much of the distribution, which

indicates that adjacent-county lending is not negligible relative to own-county persistence. Rather, neighboring exposure appears to be an important predictor in its own right, even when it does not dominate the county's own lagged lending history. This again points to a mechanism in which neighboring counties matter most when baseline familiarity or access is relatively limited. In counties that were not already strongly positioned to receive RFC lending, nearby exposure may have served as a signal that the program was accessible, legitimate, and practically usable, helping diffuse knowledge across local banking markets. Once counties moved away from that lower-tail setting, however, the role of neighboring exposure appears to diminish somewhat relative to the county's own lending history, which likely captures persistence in local banking conditions, ongoing relationships, or established access to RFC channels.

The bottom-25% subsample tells a different story. AFAB is very large at $\tau = 0.1$ (approximately 3.5, significant), but FAB is weaker. Through the middle quantiles (0.4–0.6), both coefficients shrink toward zero and lose statistical significance. Significance re-emerges at the upper tail (0.7–0.9), but magnitudes are smaller than at the lower tail. This suggests that in the smallest counties, the spillover relationship was less stable and more concentrated at the extremes of the distribution.

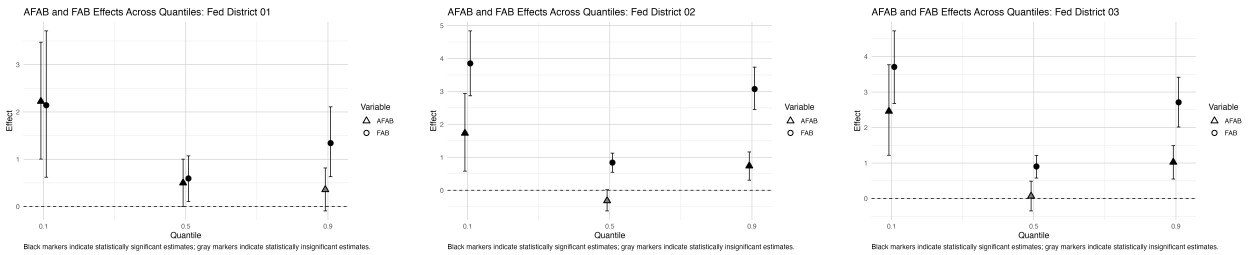


(a) Rest 75% Population (b) Bottom 25% Population

Figure 10: Baseline specification: AFAB and FAB coefficient estimates across quantiles by population subsample. Triangles denote AFAB (adjacent lending); circles denote FAB (own-county lending). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

4.3 Federal Reserve District Results

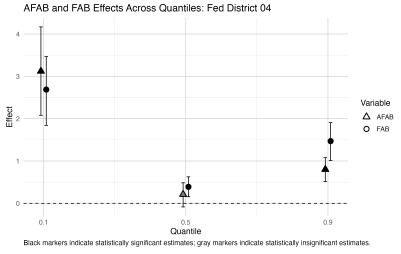
Figure 11 shows that the twelve Federal Reserve district results reinforce the finding of strong lower-tail spillovers but with meaningful regional heterogeneity. In all twelve districts, both AFAB and FAB are large and significant at $\tau = 0.1$, typically ranging from 2 to 5 for AFAB and 1 to 5 for FAB. At $\tau = 0.5$, the pattern is more mixed: AFAB becomes statistically insignificant in several districts (notably 02, 05, and 10), while FAB also loses significance in District 06. At $\tau = 0.9$, most districts recover positive significant effects for both coefficients, though magnitudes are smaller than the lower tail. The strongest combined effects appear in Districts 03, 04, 09, and 12, while District 05 shows the weakest spillovers at the median and upper tail.



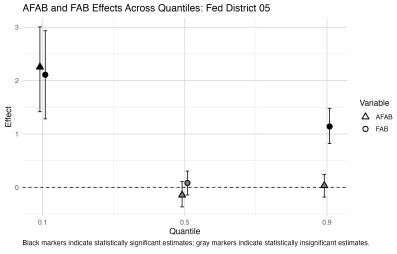
(a) District 1 (Boston)

(b) District 2 (New York)

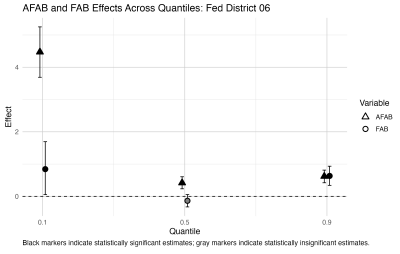
(c) District 3 (Philadelphia)



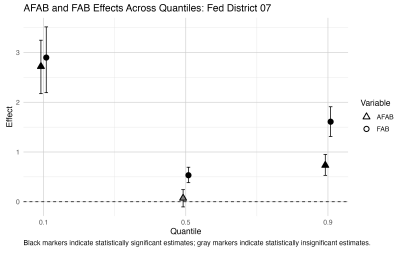
(d) District 4 (Cleveland)



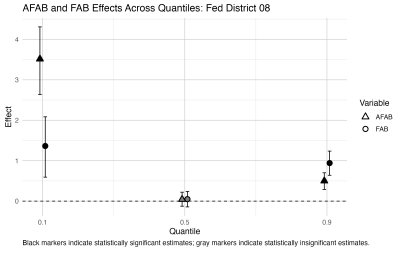
(e) District 5 (Richmond)



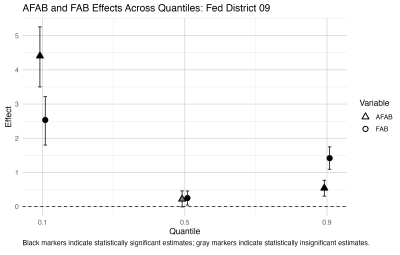
(f) District 6 (Atlanta)



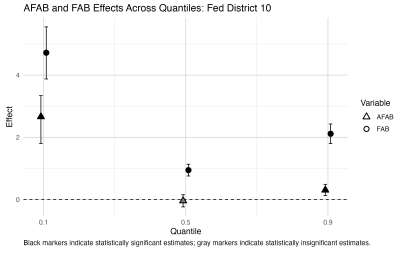
(g) District 7 (Chicago)



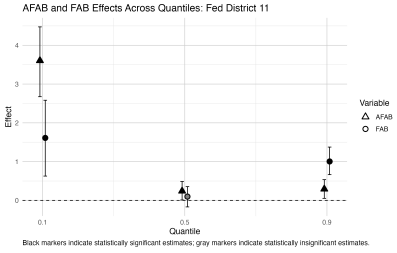
(h) District 8 (St. Louis)



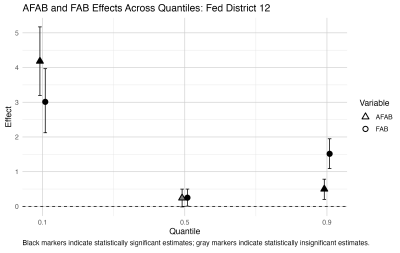
(i) District 9 (Minneapolis)



(j) District 10 (Kansas City)



(k) District 11 (Dallas)



(l) District 12 (San Francisco)

Figure 11: Baseline specification: AFAB and FAB coefficient estimates by Federal Reserve district at $\tau \in \{0.1, 0.5, 0.9\}$. Triangles denote AFAB (adjacent lending); circles denote FAB (own-county lending). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

4.4 Decomposed Model: State-Bank versus National-Bank Spillovers

The decomposed model separates the composite AFAB indicator into adjacent state-bank lending (AFASB) and adjacent national-bank lending (AFANB). This decomposition reveals

an important asymmetry: the state-bank channel is consistently the greater driver of the aggregate spillover effect. This distinction is also substantively meaningful because national banks had direct access to Federal Reserve discount window liquidity, whereas state banks generally did not, so the two bank types may have responded differently to RFC assistance and local financial stress.

In the full sample, AFASB is approximately 1.95 at $\tau = 0.1$ and then settles into a range of roughly 0.45–0.65 through the remaining quantiles, remaining statistically significant throughout. AFANB is smaller at every quantile: approximately 0.8 at $\tau = 0.1$, then roughly 0.25–0.40 from $\tau = 0.2$ onward. Both channels are significant across all quantiles in the full sample, but AFASB is consistently about twice the magnitude of AFANB. Figure 12 shows this pattern clearly. The stronger and more diffuse state-bank effect is consistent with the idea that banks without comparable Federal Reserve liquidity access may have been more dependent on RFC support, making neighboring state-bank lending a more important source of spillover variation.

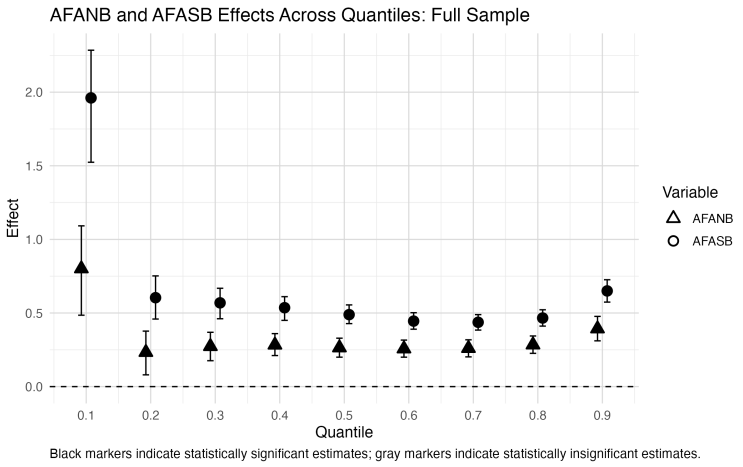
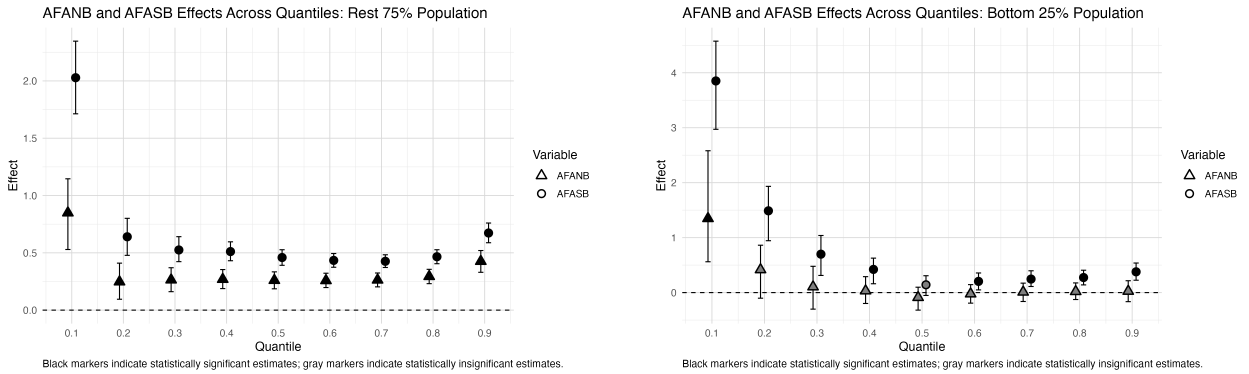


Figure 12: Decomposed specification: AFASB and AFANB coefficient estimates across quantiles, full sample. Circles denote AFASB (state-bank channel); triangles denote AFANB (national-bank channel). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

Figure 13 shows that the rest-75% subsample replicates this pattern closely: AFASB around 2.0 at the lower tail and 0.45–0.65 elsewhere, AFANB around 0.85 at the lower tail

and 0.25–0.45 elsewhere, both significant throughout. In the bottom-25% subsample, the asymmetry becomes even more pronounced. AFASB is very large at $\tau = 0.1$ (approximately 3.85, significant) and remains significant through the 0.2–0.4 range before weakening in the middle quantiles. AFANB is significant at $\tau = 0.1$ (approximately 1.4) but loses significance through most of the middle quantiles (0.4–0.7), only weakly re-emerging at the upper tail. For small counties, the state-bank channel carried the spillover while the national-bank channel was largely irrelevant outside the lower tail. This pattern is again consistent with the institutional difference between the two bank types: national banks' access to the discount window may have reduced their dependence on RFC-linked neighboring dynamics relative to state banks.



(a) Rest 75% Population

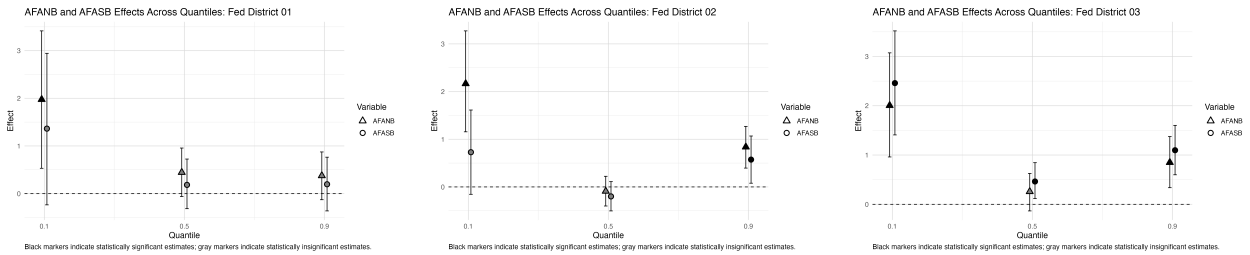
(b) Bottom 25% Population

Figure 13: Decomposed specification: AFASB and AFANB coefficient estimates across quantiles by population subsample. Circles denote AFASB (state-bank channel); triangles denote AFANB (national-bank channel). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

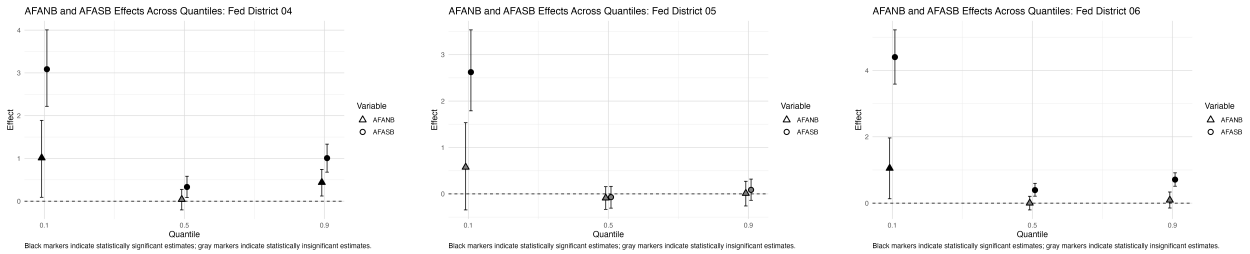
4.5 Decomposed Model: Federal Reserve District Results

Figure 14 sharpens this picture further at the district level. At $\tau = 0.1$, AFASB is large and significant in nearly every district, often exceeding 3.0 (for example, approximately 4.3 in District 06, 4.7 in District 09, 5.0 in District 12), while AFANB at the same quantile is often smaller and in several districts insignificant (Districts 01, 02, 05, 07, and 10 show AFANB with credible intervals crossing zero at $\tau = 0.1$). At $\tau = 0.5$, both channels typically

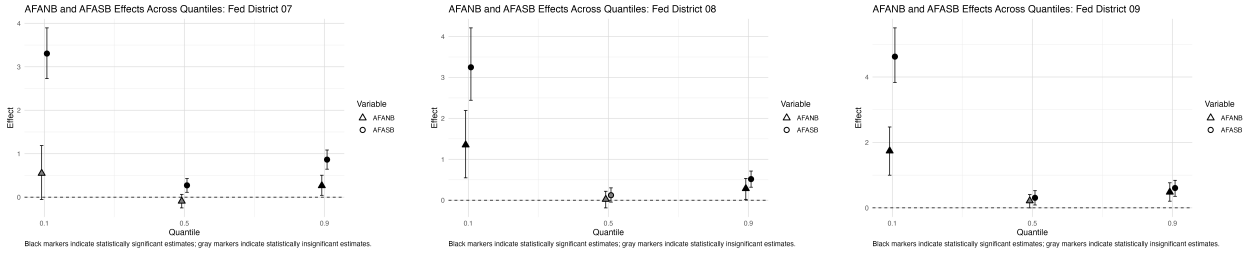
compress toward zero with wider uncertainty; AFANB is insignificant in most districts and AFASB retains significance in only a few (Districts 03, 04, 07, and 11). At $\tau = 0.9$, moderate positive effects re-emerge for both channels in some districts (notably 03, 04, 07, 08, 09, and 11), but the state-bank channel again tends to be the larger and more reliably significant of the two.



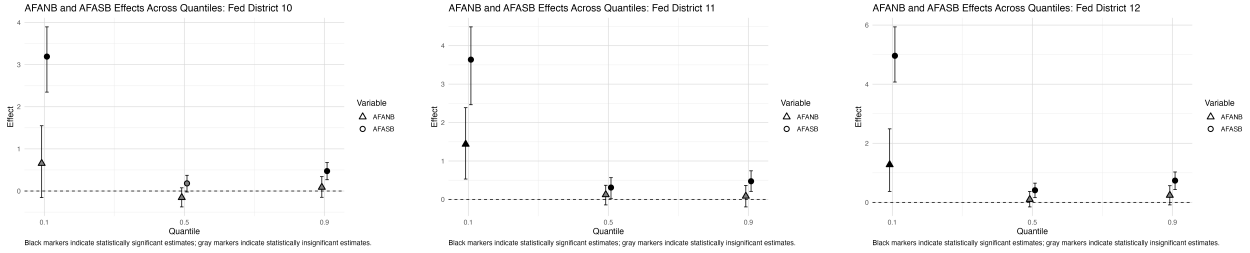
(a) District 1 (Boston) (b) District 2 (New York) (c) District 3 (Philadelphia)



(d) District 4 (Cleveland) (e) District 5 (Richmond) (f) District 6 (Atlanta)



(g) District 7 (Chicago) (h) District 8 (St. Louis) (i) District 9 (Minneapolis)



(j) District 10 (Kansas City) (k) District 11 (Dallas) (l) District 12 (San Francisco)

Figure 14: Decomposed specification: AFASB and AFANB coefficient estimates by Federal Reserve district at $\tau \in \{0.1, 0.5, 0.9\}$. Circles denote AFASB (state-bank channel); triangles denote AFANB (national-bank channel). Black markers indicate statistically significant estimates; gray markers indicate statistically insignificant estimates.

4.6 Synthesis

Three main findings emerge from the results. First, neighboring lending exposure is positively associated with own-county lending outcomes across a wide range of specifications,

quantiles, and subsamples. The evidence is strongest at the lower tail of the conditional distribution, where counties are least likely to receive lending—precisely where spillover support from neighbors would matter most. This pattern is consistent with a mechanism in which nearby lending increases the visibility of the program, enhances its credibility, and improves its perceived accessibility, particularly in counties with limited prior exposure to federal credit. Second, when the composite adjacent indicator is decomposed, the state-bank channel consistently dominates the national-bank channel in both magnitude and statistical significance. This suggests that the spatial diffusion of lending during the Depression operated primarily through state-chartered banking networks rather than nationally chartered ones. Third, the relationship is heterogeneous across county size and Federal Reserve districts: the rest-75% subsample shows stable, significant effects throughout the distribution, while the bottom-25% subsample loses significance across the middle quantiles and significance re-emerges only at the upper tail, suggesting that spillovers in the smallest counties were less stable and more concentrated at the extremes. Individual Federal Reserve district runs reinforce this heterogeneity, with effect sizes and significance varying considerably across regions.

5 Alternative Approaches, Robustness, and Limitations

5.1 Alternative Models

The main QBLD results are estimated on a binary dependent variable using a Bayesian quantile framework. To assess whether the core finding—a positive association between neighboring lending exposure and own-county lending outcomes—is robust to alternative modeling choices, we estimate a series of frequentist specifications using the continuous (log) measure of lending, $\log(\text{FAB})_{it}$, as the dependent variable and lagged log adjacent lending, L1_log_AFAB_{it}, as the main regressor. These models vary in how they handle censoring, fixed effects, and distributional heterogeneity, providing complementary perspectives on the

spillover relationship.

5.1.1 Fixed-Effects Panel Regression

We begin with a standard linear panel regression with two-way fixed effects (county and month), estimated via `reghdfe`:

$$\log(\text{FAB})_{it} = \beta\, \text{L1_log_AFAB}_{it} + \alpha_i + \delta_t + \varepsilon_{it}. \tag{4}$$

The estimated coefficient on lagged adjacent lending is $\hat{\beta} = 0.034$ ($t = 17.90$, $p < 0.001$), with a within R^2 of 0.002. The effect is statistically significant but small in magnitude, suggesting that after absorbing county and time heterogeneity, a one-unit increase in log adjacent lending is associated with a modest increase in own-county log lending. The low within R^2 is expected given the high proportion of zero-lending observations in the panel; the linear model does not account for the censored nature of the dependent variable.

5.1.2 Panel Quantile Regression

To allow the spillover relationship to vary across the conditional distribution of log lending, we estimate a panel quantile regression with county fixed effects and time fixed effects at $\tau \in \{0.10, 0.50, 0.90\}$ using `qregpd`. The results are:

	$\tau = 0.10$	$\tau = 0.50$	$\tau = 0.90$
L1_log_AFAB	0.044**	0.831***	0.256**
	(0.014)	(0.005)	(0.078)

The coefficient is positive and significant at all three quantiles, but its magnitude varies substantially. At the median ($\tau = 0.50$), the effect is largest at 0.831, indicating a strong positive association in the middle of the conditional lending distribution. The effect is considerably smaller at the lower tail ($\tau = 0.10$, coefficient 0.044) and upper tail ($\tau = 0.90$, coefficient 0.256). This contrasts with the QBLD results on the binary outcome, where

spillover effects were strongest at the lower tail. The difference is consistent with the fact that the continuous specification focuses on the intensive amount of lending, whereas the binary QBLD framework focuses on whether counties receive any lending at all.

5.1.3 Tobit with Mundlak Device

Because $\log(\text{FAB})_{it}$ is left-censored at zero for counties receiving no bank lending, a Tobit specification is more appropriate than OLS for the continuous outcome. To approximate fixed effects within the Tobit framework, we apply the Mundlak (1978) device, decomposing the regressor into within-county and between-county components:

$$\log(\text{FAB})_{it} = \beta_W \, \text{L1_}\widetilde{\text{log_AFAB}}_{it} + \beta_B \, \overline{\text{L1_log_AFAB}}_i + \boldsymbol{\gamma}'\mathbf{d}_t + \varepsilon_{it}, \tag{5}$$

where $\text{L1_}\widetilde{\text{log_AFAB}}_{it}$ is the within-county deviation and $\overline{\text{L1_log_AFAB}}_i$ is the county mean. The within-county coefficient is $\hat{\beta}_W = 0.220$ ($t = 14.17$, $p < 0.001$) and the between-county coefficient is $\hat{\beta}_B = 1.928$ ($t = 29.74$, $p < 0.001$), both with standard errors clustered at the county level. The large between-county effect indicates that counties embedded in persistently higher-lending neighborhoods tend to have substantially higher own lending, while the within-county effect confirms that time-varying changes in adjacent lending are also positively associated with own-county outcomes after accounting for censoring.

5.1.4 Panel Tobit

We estimate panel Tobit models in both random-effects and mixed-effects (approximating fixed effects) form. The random-effects specification (`xttobit`) yields a coefficient of $\hat{\beta} = 0.745$ ($z = 54.82$, $p < 0.001$), with an estimated intraclass correlation of $\rho = 0.223$, confirming meaningful county-level heterogeneity. The mixed-effects Tobit (`metobit`) produces a larger coefficient of $\hat{\beta} = 0.946$ ($z = 68.58$, $p < 0.001$). Both specifications confirm a strong positive association between adjacent and own-county lending in a censored-data framework,

and the larger fixed-effects estimate suggests that the random-effects model may attenuate the spillover coefficient by conflating within- and between-county variation.

5.1.5 Summary of Alternative Specifications

Across all four alternative approaches, the core finding is consistent: neighboring lending exposure is positively associated with own-county lending. The linear FE panel, panel quantile, Mundlak Tobit, and panel Tobit models all return positive and significant coefficients on lagged adjacent lending. The magnitudes and patterns differ across specifications in ways that reflect their respective assumptions about censoring, distributional heterogeneity, and the relevant margin of adjustment, but the directional finding is robust.

Importantly, the alternative specifications complement and reinforce the main QBLD results in several ways. At the same time, they are not estimating exactly the same relationship as the main models, since they do not include lagged own-county lending (L1_FAB) as a regressor. As a result, the alternative specifications should be interpreted as showing whether neighboring lending exposure is positively associated with own-county outcomes in a broader reduced-form sense, rather than as direct counterparts to the QBLD coefficients that are estimated conditional on own-county persistence. The QBLD models found that spillover effects were strongest at the lower tail of the conditional distribution—precisely where counties are on the margin of receiving any lending at all. The panel quantile regression's large median coefficient (0.831) and smaller tail coefficients on the continuous outcome highlight that the QBLD's binary framework captures a qualitatively different and policy-relevant margin—the probability of receiving *any* lending—that continuous specifications may partially obscure by focusing on lending intensity. Meanwhile, the Mundlak Tobit's large between-county component ($\hat{\beta}_B = 1.928$) echoes the QBLD finding that counties persistently embedded in high-lending neighborhoods show stronger lending propensities, consistent with the spatial clustering documented in the descriptive evidence of Section 2.4. Taken together, these alternative models confirm that the QBLD's focus on the binary outcome and quantile-specific

inference is not merely a methodological choice but captures an economically meaningful dimension of how neighboring lending exposure appears to operate.

5.2 Robustness and Extensions

One strength of this analysis is that it does not rely on a single specification. The main QBLD results are supported by four alternative frequentist approaches—linear FE panel, panel quantile, Mundlak Tobit, and a panel Tobit—all of which return a positive association between neighboring lending and own-county outcomes. Beyond these alternative estimators, the QBLD framework itself is tested across nine quantiles, twelve district-specific runs, and two population-based splits, providing a multi-dimensional view of the spillover relationship. The consistency of a positive neighboring pattern across these many settings and modeling choices strengthens the argument that the results are not purely driven by a single sample, specification, or estimator.

Future extensions could include explicitly spatial panel specifications following Anselin (1988) or Lee and Yu (2010), alternative spatial weight structures, or direct connections between county-level lending outcomes and downstream economic variables such as bank survival, employment, or local recovery.

5.3 Limitations

The first limitation is interpretive. The models identify associations consistent with spatial spillovers, but they do not establish a definitive causal mechanism. Neighboring lending exposure may proxy for broader regional financial conditions rather than a direct spillover from one county to another. Vossmeyer (2016)'s work on RFC selection and treatment highlights that reduced-form panel approaches cannot fully resolve endogeneity concerns.

Second, adjacency is only one way to define interdependence. Economic spillovers may also operate through trade linkages, banking networks, urban hierarchies, or state-level institutional structures that are not fully captured by a simple geographic neighbor definition.

As a result, the adjacency-based measure may omit important channels of transmission.

Third, the empirical framework relies on a binary measure of lending, which abstracts from variation in lending intensity. This choice is partly driven by the data structure and methodological constraints: the lending outcome is heavily zero-inflated and censored at zero, and there is no straightforward implementation of a censored panel quantile regression with fixed effects that accommodates this setting. As a result, the QBLD approach focuses on whether a county receives any lending rather than the magnitude of lending. While this allows us to model the most policy-relevant margin in a sparse environment, it limits direct interpretation of effect sizes in terms of lending amounts.

Fourth, the district-level results are based on a limited set of quantiles and often exhibit wider credible intervals. These estimates provide suggestive evidence of regional heterogeneity, but they should not be interpreted with the same precision as the full-sample and population-split results, which form the core empirical evidence.

Finally, the alternative specifications serve as useful robustness checks but come with important limitations. Because they rely on the continuous log lending outcome, they do not accommodate the large point mass at zero as naturally as the main binary framework. They also do not estimate the same object as the main QBLD models: the alternative regressions omit lagged own-county lending (L1_FAB), so they do not isolate neighboring exposure conditional on own-county persistence. In addition, the panel quantile regressions are based on the continuous outcome and therefore describe heterogeneity in lending intensity rather than in the probability of receiving any lending, making them not directly comparable to the binary quantile estimates. The linear fixed-effects model is a very basic benchmark and does not account for censoring at zero, while the Tobit-based models impose stronger functional-form assumptions and only approximate fixed effects through the Mundlak device rather than fully absorbing county-specific heterogeneity. For these reasons, the alternative models should be interpreted as simplified complementary checks rather than substitutes for the main analysis. We therefore place the greatest weight on the QBLD results, since

that framework is the one that best matches the central features of the data: a binary outcome, substantial zero inflation, panel structure, county heterogeneity, and interest in quantile-specific effects.

6 Conclusion

Financial crises routinely prompt large-scale government interventions in credit markets. A recurring question—from the Depression-era RFC through TARP and the Paycheck Protection Program—is whether such interventions affect only directly targeted institutions and localities, or whether their effects diffuse into surrounding areas. This thesis has examined that question in the context of RFC lending during 1932–1936, focusing on whether county-level lending outcomes were associated not only with a county's own prior lending, but also with lending activity in adjacent counties.

Using county-level monthly panel data, descriptive geographic evidence, and Bayesian quantile regression models for binary longitudinal data following Rahman and Vossmeyer (2019), the main result is that neighboring lending exposure is positively associated with own-county lending outcomes. The main (AFAB) model shows that adjacent lending matters across much of the conditional distribution, with particularly strong effects at the lower quantiles and positive effects persisting into the upper tail. The decomposed (AFANB/AFASB) model further indicates that both adjacent state-bank and national-bank channels contribute to these patterns, though the state-bank channel is consistently stronger and more stable—often approximately twice the magnitude of the national-bank channel. The results also reveal meaningful heterogeneity across county population groups and Federal Reserve districts, indicating that spatial spillovers were not uniform.

These findings matter for both historical interpretation and broader policy reasoning. Historically, they suggest that the effects of Depression-era emergency lending were not confined to directly exposed counties. The dominance of the state-bank channel highlights

that the institutional structure of local banking systems shaped how financial stabilization diffused across space. More broadly, the results imply that evaluating emergency lending programs solely on the basis of direct exposure likely understates their total impact, as interventions can generate indirect regional effects that extend beyond treated units.

The results are also consistent with an information or familiarity channel. One interpretation is that lending to banks in one county increased the visibility, credibility, or perceived accessibility of the RFC program for banks in nearby counties, lowering informational or administrative barriers to participation. This interpretation aligns with modern evidence from Anbil, Carlson, and Styczynski (2023), who show that pre-existing familiarity with government lending frameworks increased both participation and readiness to participate in the PPPLF, with effects arising even when the facility was not actively used. While this thesis does not directly observe these mechanisms, the positive association between nearby lending and own-county outcomes is consistent with the idea that government programs can diffuse through information and expectations as well as through direct financial channels.

The contribution is therefore twofold. First, it provides evidence that RFC lending exhibited a geographically diffuse component, operating in part through neighboring banking networks and plausibly reinforced by informational spillovers. Second, it demonstrates the value of combining historical policy analysis with Bayesian quantile methods for binary outcomes to uncover heterogeneous effects across the distribution. Taken together, these findings underscore that the effectiveness of emergency lending depends not only on where funds are allocated, but also on how both capital and information propagate across space—an insight that is central to understanding crisis interventions in both historical and modern contexts.

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