JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS

doi:10.1017/S0022109023000406

An Experiment in Tight Monetary Policy: Revisiting the 1920–1921 Depression

Bruce Carlin
Rice University Jones Graduate School of Business
carlin@rice.edu

William Mann
Emory University Goizueta Business School
william.mann@emory.edu (corresponding author)

Abstract

Is there a trade-off between the short-run and long-run real effects of monetary policy "leaning against the wind"? We provide novel evidence on this question from the United States in 1920–1921. Our identification strategy exploits county-level variation in access to the Federal Reserve's discount window, and hand-collected data on banking and agriculture in Illinois. In the short term, tightened conditions at the discount window decreased bank lending and lowered crop prices and farm revenues. In the long term, however, they lowered debt-to-output levels and led to greater farmland utilization, suggesting an avoidance of debt overhang problems.

I. Introduction

In the wake of the global pandemic, policymakers in the United States are confronting a familiar question: When does it make sense to "lean against the wind" in the face of an overheated economy? After years of accommodative policies, the Federal Reserve is sharply raising rates to combat inflation and unsustainably tight labor markets. These moves seem sure to hurt short-run economic growth, but are advocated due to their alleged positive long-run effects.

Yet there is surprisingly little evidence to date on what those long-run effects may be, or how they compare to their short-run costs. Empirical macroeconomic analysis has relatively few experiments that can cleanly identify this trade-off over a window of several years. Instead, most analysis relies on structural models matched to aggregate data that struggle to address such questions.

We thank two anonymous referees, Thierry Foucault (the editor), and conference discussants and seminar participants at the University of Chicago, UC Berkeley, UCLA, Rice University, Arizona State University, the University of Colorado, the University of Rochester, Indiana University, Michigan State University, the University of Queensland, the UBC Summer Finance Conference, the SFS Cavalcade, the Financial Intermediation Research Society Meetings, the Yale Junior Finance Conference, and the Australia National University Summer Research Camp for valuable comments and suggestions. Joseph Needle, Alex Simon, Darren Tan, and Billy Xu provided excellent research assistance. Mann gratefully acknowledges financial support from the Laurence and Lori Fink Center for Finance and Investments.

We provide novel evidence on these questions by revisiting a century-old episode with many parallels to the present. During 1920–1921, following several years of low interest rates and credit growth, the nascent Federal Reserve system faced an economy that was overheated, yet fragile: European demand for U.S. exports was declining sharply with the end of World War I, but the economy was still flush with credit.

In response to these conditions, Federal Reserve districts tightened the availability of credit at the discount window and increased discount rates nationwide. The economy entered a recession, and the policy has been widely criticized ever since (e.g., Friedman and Schwartz (1963)). But, then as now, the motivation for tight policy was its alleged long-run effects, which still have not been cleanly analyzed.

To study the effects of the policy during this time, we hand-collect county-level panel data on bank lending and agricultural output in Illinois from 1916 to 1929. For identification, we exploit cross-sectional variation in the county-level composition of national versus state banks, as a proxy for the differential effects of discount window policy on local lending.[1] Nonmember state banks did have some indirect access to the discount window through interbank networks (e.g., Anderson, Calomiris, Jaremski, and Richardson (2018), Anderson, Erol, and Ordoñez (2022)), but we find that this has no predictive power for local lending when we control for state bank bills payable and rediscounts, as in Anderson et al. (2022).

We find that when discount window policy was tightened in both the Chicago and St. Louis districts, bank lending and agricultural revenues fell sharply: A 1-standard-deviation increase in a county's per capita number of national banks was associated with a 5% lower volume of both of these outcomes at the county level, in 1920 as compared to 1916. Combining the credit and revenue effects in an instrumental variable (IV) specification, we estimate that counties lost approximately $0.60 in agricultural revenues in response to a marginal dollar of bank credit withdrawn due to discount window policy.

We also show that this decline in revenue is completely accounted for by shifts in prices rather than quantities (and is separate from any aggregate deflation, since we remove year effects). A plausible mechanism for these effects, supported by the historical narrative, is the forced liquidation of crops at lower prices due to a contraction in credit supply. Farmers faced a maturity mismatch between the short-term loans they could secure from local banks and their longer-term production cycle. When discount window policy tightened, banks stopped rolling over short-term loans, which in turn forced the liquidation of crops at unattractive prices. This mechanism is consistent with claims made by farm representatives at the time, as we document using records of congressional hearings in 1921, but, to the best of our knowledge, we provide the first evidence that their claims were correct.

Interestingly, however, we find that the effect on agricultural revenues was temporary, and disappeared when policy was loosened again by the end of 1921.

[1] In our empirical analysis, we document that the presence of national banks during this time period was stable, was not already correlated in 1916 with important observables such as credit conditions or agricultural output, and was strongly correlated with actual discount window usage at the two dates when we can directly measure this usage (July 1916 and July 1921). See Section IV.

In contrast, the effect on local debt levels persisted. As a result, the ratio of bank loans outstanding to agricultural output was much lower in counties with higher exposure to discount window policy. This ratio remained persistently lower through 1929, up to the eve of the Great Depression. Prior research has established that lower debt levels were associated with better outcomes for farms during these later years (Alston (1983), Hausman, Rhode, and Wieland (2019)). With this in mind, our findings suggest that regions that experienced lower output during 1920–1921 may have avoided more protracted problems over the next decade.

Finally, we document one mechanism by which counties may have fared better with lower long-term debt ratios, using hand-collected county-level data from the U.S. Census of Agriculture. Counties with little exposure to discount window policy lost both farms and farming acreage in the aftermath of the 1920–1921 depression. In contrast, counties with more exposure experienced consolidation: The number of farms in these counties decreased at a similar rate to other counties, but the total farming acreage did not decrease. As with the effects on debt-to-output ratios, these discrepancies persisted into the 1930s.

These findings are consistent with an avoidance of debt overhang problems in counties that were more exposed to changes in discount window policy. Farms eventually failed at similar rates across counties, regardless of this policy. However, farms in counties with less exposure were able to accumulate larger debt burdens before failure (as suggested by our findings on bank loans). These debt burdens would likely present a challenge to successful reorganization, and increase the probability that the land was neglected or abandoned, consistent with our findings. Tight policy thus appears to have encouraged better adaptation by the agricultural sector to the lower demand of the postwar years. Looser policy would have supported existing farms for a brief time, but ultimately would not have increased their survival rates, and would have decreased farming acreage in the long run.

Our findings do not conclusively establish that tight credit conditions were the best policy in this episode. However, our identification strategy and panel data provide a more complete picture of the real effects of this policy, both short-term and long-term. High discount rates hurt short-term agricultural prices and revenues, as claimed by farmers at the time, but arguably left the agricultural sector better positioned to handle stress in the long run.

II. Related Literature and Historical Background

A. Related Literature

In terms of identification strategy, the closest papers to us are Park and Van Horn (2015) and Kandrac (2021), who also study the real effects of Federal Reserve policy by comparing the responses of banks that were members to those that were not. However, they study different episodes. Kandrac (2021) studies the local provision of subsidized loans in the 1970s, whereas Park and Van Horn (2015) study an increase in reserve requirements during 1936–1937.

In terms of the episode we study, Rieder (2020) also examines discount window policy during 1920–1921. However, his focus is on comparing different

implementations of this policy, by studying discontinuities at Fed district borders within split states. Illinois is not one of the states in his sample, and within our data, we are unable to detect significant discontinuities in outcomes between the St. Louis and Chicago districts (results available from the authors). Thus, we do not attempt to implement the split-state strategy or make comparative statements between the districts. Instead, we pool them together and simply investigate the combined average effect of their policies. A further distinction between our articles is that we focus on comparing short-run and long-run real effects, whereas Rieder focuses more on policy design and implementation, including issues such as bank leverage and regulatory arbitrage.

Richardson and Troost (2009) also exploit a split-state strategy in Mississippi to understand the real effects of Fed policy during the 1930s. Jalil (2014) extends their work to compare the dovish effects of the Atlanta Fed to the more hawkish policies of the St. Louis, Dallas, and Richmond Feds in the 1930s. Broadly, they find that banks in the sixth district (Atlanta) survived at higher rates and had faster recoveries than banks in other districts, due to the activism of the Atlanta Fed. We share the goal of using cross-sectional variation to identify the effect of policy in the early twentieth century, but we focus on an earlier time period, and on county-level credit and output effects rather than on bank survival. Also, the prior literature focuses on the use of monetary policy to avoid bank runs on demand deposits, whereas we focus on the effect of this policy on local lending.

Our article is also closely related to the analysis of Hausman et al. (2019). They find that dollar devaluation in 1933 stimulated recovery from the Great Depression by raising crop prices, and that this effect was most pronounced among regions with the greatest farm debt burdens. We show that tight discount window policy in 1920–1921 helped to constrain these debt burdens from arising in the first place. We also document a price mechanism connecting monetary policy with farm output and indebtedness, but our mechanism appears during a downturn instead of a recovery, and through the specific channel of local bank lending.

Anderson et al. (2018) hand-collect data on state banks in New York over a similar time period. They investigate the drivers of state banks' decisions on whether to join the Federal Reserve. Their focus on New York is appropriate because New York state banks joined the Fed at a high rate: By 1920, they find that 62% of state banks in New York City, and 23% outside the city, were members of the Fed. Since our goal is to understand the Fed's effects, we focus on Illinois, where few state banks joined the Fed during 1916–1926, creating the identifying variation that we exploit. Indeed, by 1929, only 5% of Illinois state banks were members of the Federal Reserve, in line with the national average of 7.5% (see Board of Governors of the Federal Reserve System ((1943), p. 26)).

A broader literature studies the history and real effects of the banking industry in the United States. Anderson, Paddrik, and Wang (2019) show that the National Bank Acts of 1863–1864 led to an interbank deposit network structure that was more diversified but more subject to contagion. Jayaratne and Strahan (1996) demonstrate that bank credit is also constrained by branching restrictions, such as Illinois's complete prohibitions on bank branching in the early twentieth century. Petersen and Rajan (1995) show that small firms borrow from local lenders even in recent history.

Last, our article is related to work on rollover risk (e.g., He and Xiong (2012)), which contributed to the severity of the 2008 financial crisis (e.g., Brunnermeier (2009), Krishnamurthy (2010)). In that episode, banks were exposed to a maturity mismatch, having financed longer-term investments with short-term liabilities like repurchase agreements. When access to short-term financing dried up, this created significant stress on the financial system. In our setting, a similar mismatch took place in a nonfinancial sector: Long-maturity investments in crop production were financed with short-term loans. Once access to financing became scarce, farmers facing liquidity constraints sold crops at fire-sale prices.

B. Brief Historical Background

This section provides a brief historical background for our study and draws on several sources.[2] Rather than providing an exhaustive description of the entire era, we highlight the primary details that form the basis of our analysis and empirical strategy.

The National Banking Acts of 1863–1864 created a system of federally chartered national banks in an attempt to centralize the banking system. By 1870, national banks had almost completely displaced state banks due to a punitive tax enacted by Congress on state bank notes. However, high capital requirements for national banks and the declining importance of note issuance led to a comeback by state banks in the 1870s to 1880s. By 1914, both types of banks were scattered throughout the country, including Illinois.

When the Federal Reserve system began operations in 1914, national banks were required to join. State banks could not be forced to join, and very few did in Illinois, especially outside of Chicago. Even by 1929, only 5% of Illinois state banks had joined (see Board of Governors of the Federal Reserve System ((1943), p. 26)).

One benefit conveyed to member banks was access to the discount window, which they soon used widely and for extended periods (Gorton and Metrick (2013)). By contrast, nonmember state banks had no direct access to the discount window, and even their indirect access was limited at best. The Federal Reserve Act explicitly stated that member banks could not act as intermediaries for nonmembers without explicit approval, and member banks seemed to take this rule seriously, as Federal Reserve Bulletins report several requests by them for clarification in ambiguous cases (e.g., June 1918 (p. 520) and Aug. 1918 (p. 745)).[3]

The outbreak of World War I created a massive demand for U.S. exports. Agriculture especially was encouraged by the government to expand production. This ultimately led to inflation, due first to gold inflows, and later to the government's borrowing to extend credit to its allies, accomplished via Liberty Bonds that

[2]Primarily United States House of Representatives (1921), Link (1946), Friedman and Schwartz (1963), White (1983), Meltzer (2003), Frederico (2005), Gorton and Metrick (2013), White (2014), and Anderson et al. (2018).

[3]The prohibition on intermediation was further underscored by a ruling in the 1915 Bulletin (p. 213). In Aug. 1921 (after the main short-term effects that we observe), the Board temporarily granted wider authority for member banks to apply to act as conduits for nonmembers (p. 963), but still left these decisions to the discretion of the Federal Reserve Banks. This ruling was revoked by Aug. 1923 (p. 891).

were marketed to citizens. The Fed maintained low discount rates at this time to encourage Liberty Bond purchases. Aggregate indebtedness in the farming sector roughly tripled to $140 billion during 1910–1920 (Frederico (2005)). However, government borrowing and European demand dropped sharply during 1919 with the war's end. During 1919–1920, the agricultural and manufacturing sectors exhibited steep declines in output and prices (though notably, there was no banking panic as in 1907 or 1930).

In response to the downturn, Federal Reserve banks raised discount rates sharply at the beginning of 1920. Some sources contend that this action was forced by the need to avoid violating gold reserve requirements, whereas others argue that it was at least partly a deliberate decision to contain credit growth. In either case, the effect was a clear tightening of discount window policy.

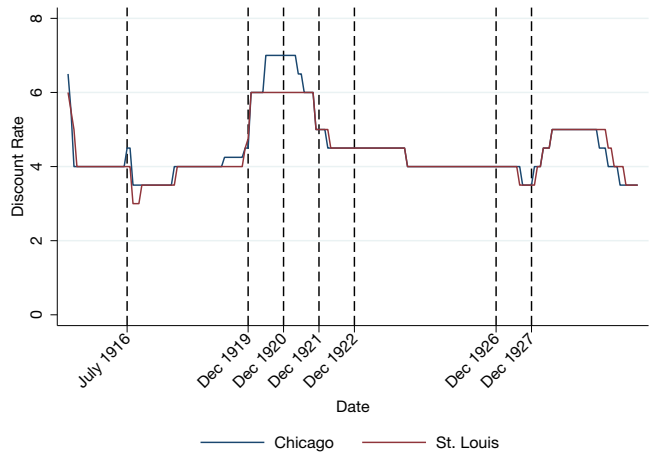
Figure 1 shows a timeline of discount rates for the two Illinois Federal Reserve districts: Chicago and St. Louis. Initially, both districts raised rates from 4% at the beginning of 1919 to 6% at the beginning of 1920. During the next year, Chicago raised its discount rate again to 7%, as depicted in the timeline, whereas St. Louis left its official rate at 6% but pursued a progressive discount rate policy that targeted certain borrowers with greater rates.[4]

Immediately following the increase in discount rates, farm representatives nationwide implored the Federal Reserve system to reverse the discount rate

FIGURE 1

Discount Rates at Federal Reserve Banks of Chicago and St. Louis (1915–1930)

In Figure 1, the dashed lines are the dates for which we have data on both state and national banks.



[4]As mentioned earlier, we do not attempt to estimate the differential effects of these two policies. Our focus is not on this type of comparative analysis, and we are not able to detect significant differences in policy outcomes between the two districts (results available from the authors). Therefore, we simply track the differential effects of banks that were and were not exposed to Fed policy throughout the time. Rieder (2020) does pursue a comparative strategy for this episode using a sample of several split states, though not Illinois.

increases, ultimately leading Congress to convene a Commission of Inquiry in July 1921 to investigate their concerns. Farmers were already suffering from falling demand and prices, and steep discount rates were perceived to have made matters worse, by forcing them to liquidate crops at low prices to pay off their maturing debt when it was not rolled over.

The underlying problem was a maturity mismatch, as rediscounted agricultural loans were limited to a 6-month maturity, whereas most crops faced a 9- to 12-month growing cycle. One of the commission's major recommendations was to address this maturity-mismatch problem:

> It is not enough that the producer of the crop might possibly be able to get a renewal of his original production credit, he must have the assurance when he first seeks the credit to produce his crop that if he so desires that credit will be of sufficient length to carry him through the period of orderly distribution as well as production.(U.S. House of Representatives ((1921), p. 149))

Federal Reserve Governor William Harding countered that the discount rate policy was simply a scapegoat, playing on innate suspicions of the Federal Reserve system. He dismissed the idea that credit would allow farmers to realize higher prices, arguing that credit for this purpose would simply be speculation and not sound lending. The controversy is vividly illustrated by a form telegram sent by farmers across the Midwest, and by the response they received, both of which were recorded in the commission's proceedings, and are reproduced in this article's Appendix.

In his response, Harding also argued that "of course, the Federal reserve system has no control whatever over the credits of nonmember banks." The same idea is illustrated in a key excerpt from the commission's final report:

> Obviously, [the Federal Reserve Board and reserve banks could in no way directly control the loan policies of about 20,000 State banks and trust companies, representing 35 or 40 per cent of the banking resources of the country, that were not members of the Federal reserve system. Whatever restraining influence was exercised could be exercised only against member banks, and could be exercised only by restriction of credit, either through refusing loans to member banks in individual cases, or by pressure of discount rates applied to those member banks whose necessities required them to borrow from the Federal reserve banks. (U.S. House of Representatives ((1921), p. 53))

These arguments illustrate our identification strategy: Member banks, and their communities, were more affected than nonmember banks by the increase in discount rates.

The commission ultimately sided with the farmers' arguments, but the bank-lending channel behind this argument has been debated ever since. For example, Link (1946) forcefully argues that increased discount rates did not restrict agricultural credit by banks. Friedman and Schwartz (1963) argue that policy amplified the damage of the recession by restricting the supply of money, not credit. More recently, discussion about this episode has resurfaced in the debate over austerity policies as a response to economic downturns.

However, most prior analyses have been based on aggregated time-series evidence. Our study uses granular bank- and county-level data to revisit these questions, establish causal mechanisms, and re-evaluate the prior conclusions about this episode.

III. Data

A. Sources

We exploit several hand-collected data sets on banking and agricultural output in Illinois in the early twentieth century. This section describes each data set in detail.

1. Federal Reserve Member Bank Call Reports

Data on Federal Reserve member banks come from two sources. The first is call reports that were filed with the Federal Reserve. Selected dates of these reports were microfilmed in 1946 and later scanned into PDF format and posted on the website of the FRASER library at the Federal Reserve Bank of St. Louis (http://fraser.stlouisfed.org/). The remaining dates were lost or destroyed after 1946.[5] For our window of interest, the call report dates that survived are June 30, 1916, June 30, 1921, Dec. 31, 1926, and Dec. 31, 1929. Graph B of Figure 2 shows an example call report from June 30, 1916 for the Farmers National Bank in the city of Cambridge, Henry County. From each call report for each Illinois member bank on each of these dates, we hand-collect the name, city, county, charter number, and total loan assets (item 1A in the report). For the June 1916 and June 1921 reports, we also collect liabilities for rediscounts with Federal Reserve banks (items 1d and 47 for the two dates, respectively).

2. Comptroller's Reports of Condition for National Banks

This is the second source of data for Federal Reserve member banks. We collect the reports dated December of the years 1913–1929. For each national bank in Illinois, we collect the name, city, county, and total loans outstanding. For the years 1915, 1916, 1919, and 1920, we also collect total assets, demand deposits, and time deposits, which are useful for our later robustness checks. Graph C of Figure 2 shows an example of the entry for national banks located in Cambridge, Illinois in 1916, including the same bank identified in Graph B. The first column reports total loans. For the 1926 and 1929 reports, we verify that there is negligible difference from the loan volumes reported in the call reports for those same dates (see above).

3. State Bank Reports of Condition

Data on lending by state banks come from Statements of Condition published by the State of Illinois from the years 1915–1929. Each report contains a list of all state banks, their cities and counties, and financial information including total loans outstanding and total assets. Graph A of Figure 2 shows an example report on the Cambridge State Bank. However, the timing of these reports was more erratic:

[5]These details are discussed in Mason (1998) and Calomiris and Mason (2003).

https://doi.org/10.1017/S0022050723000406 Published online by Cambridge University Press

FIGURE 2

Examples of Bank Data Sources

Graph A of Figure 2 shows a statement of condition for a nonmember state bank as of June 30, 1916. Graph B shows a call report for a national bank as of July 1, 1916. Graph C shows an entry for the same national bank in the Comptroller's Report of Condition published December 1916. Both banks are located in Cambridge, Illinois.

Graph A. Statement of Condition

CAMBRIDGE STATE BANK—CAMBRIDGE.

(Organized September 26, 1903.)

James Pollock, President. **F. L. Brodd, Cashier.**

Resources.	Amount.	Liabilities.	Amount.
Loans on real estate	$ 8,750 00	Capital stock paid in	$ 25,000 00
Loans on collateral security	2,325 51	Surplus fund	15,000 00
Other loans and discounts	235,420 72	Undivided profits, net	1,447 03
State, county and municipal bonds	1,000 00	Deposits—	
Banking house	25,000 00	Time certificates	154,013 60
Real estate other than bkg. house	2,400 00	Savings subject to notice	50,771 37
Due from banks—		Demand subject to check	51,311 29
State	5,777 77	Demand certificates	3,528 75
National	14,461 27	Dividends unpaid	1,000 00
Cash on hand—			
Currency	2,958 00		
Gold coin	2,560 00		
Silver coin	588 10		
Minor coin	61 53		
Checks and other cash items	769 14		
Total resources	$302,072 04	Total liabilities	$302,072 04

Graph B. Call Report

PLEASE FOLD THIS SIDE UP.

PLEASE FOLD THIS SIDE OUT.

REPORT of condition of "The FARMERS NATIONAL BANK,

Reserve District No. 7

Enter Charter Number of Bank Here.

No. 2572

DR.

At CAMBRIDGE, in the State of ILLINOIS

at the close of business on JUNE 30th., 1916.

RESOURCES.			Dollars.	Cts.	
1. a Loans and discounts (except those shown on b)		$ 403,728.13			
b Acceptances of other banks discounted					
Total loans	(See schedule No. 8)		403,728.13		1
2. Overdrafts, secured, $; unsecured, $ 40.98	(See schedule No. 28)		40.98		2
3. U. S. Bonds:					
a U. S. bonds deposited to secure circulation (par value)	NOTE.—With U. S. bonds are NOT to be included District of Columbia nor any Territory or insular possession bonds.	$ 50,000.00			
b U. S. bonds pledged to secure U. S. deposits (par value)					
c U. S. bonds pledged to secure postal savings deposits (par value)					
d U. S. bonds pledged as collateral for State or other deposits					
e U. S. bonds loaned					
f U. S. bonds owned and unpledged					
g Premium on U. S. bonds					
Total U. S. bonds			50,000.00		3

Graph C. Comptroller's Report of Condition

33	Calodonia, Caledonia	J. A. Brown	J. A. Greenlee	54,196	12,750	9,188
34	Cambridge, First	Henry White	B. Hadley	449,900	50,000	16,400
35	Cambridge, Farmers	Clyde B. Taylor	A. L. Arthens	436,910	50,000	36,808

In some years, multiple reports were filed throughout the year, whereas in other years, there were none. As we describe below, we only analyzed data from these reports that were closely matched to the data described above about the national banks.

Timing Details Across Banking Data Sets. We focus our analysis on the years 1916, 1919–1922, 1926, and 1927, when we are able to closely match the timing of state and national bank data. In 1916, we match state bank data from July with member bank call reports from June 30. In all the remaining years, we match the data as closely as possible to December: The Comptroller reports on national banks were always published in December, based on underlying data collected from banks sometime between September and December depending on the year. The state bank

reports were always published in December except for 1919, when the closest available report comes from Feb. 1920.

Figure 1 plots the time series of discount rates for the Chicago and St. Louis Federal Reserve districts (the two reserve districts covering Illinois), and adds vertical bars for July 1916, and December of all the remaining years in our analysis.

4. County-Level Crop Data

Data on county-level agricultural output come from the Statistical Reports of the Illinois State Board of Agriculture for the years 1916–1929. For a given calendar year, these reports provide the yields of major crops by county in bushels or tons, as well as their dollar values when sold at the prices prevailing in each county. We collect the yields and dollar values for corn, oats, hay, wheat, and barley. Figure 3, reproduced from the crop report for 1927, shows that these crops collectively accounted for over 90% of the gross value of Illinois crops. We sum the dollar amounts to construct a county-year measure of agricultural output. We use the reports for the years 1916, 1919–1922, 1926, and 1927.[6]

5. County-Level Farm and Acreage Data

Data on the number of farms and acreage put to use in each Illinois county come from reports from the U.S. Census of Agriculture. We obtained reports for the years 1920, 1925, 1930, and 1935. For each of these years, we collected information on the number of farms in existence and the acreage used for farming.

6. Population Data

We collect the county-level population from census numbers for 1910, 1920, and 1930 from the U.S. Census website (https://www.census.gov). We interpolate these figures to the dates we analyze by fitting a log-linear model of population growth for each county.

The call reports were downloaded from the FRASER website. The remaining data sources listed above were located through university libraries and through Google Books.

In all the analysis that follows, we exclude Cook County, which is home to the city of Chicago and contained over 47% of the population of Illinois as of 1920, making it an extreme outlier in every dimension of our data.

B. Stylized Facts and Motivation

We start by demonstrating several basic facts of interest to motivate our analysis.

Table 1 summarizes the county-level banking and agricultural data as of 1916. The agricultural data are as of the end of the year, as are the data on national banks' deposits, total capital, and total resources. All other banking data are as of July.

The median Illinois county had 4 national banks and 6 state banks, $2.4 million in bank loans outstanding as of 1916, and 24,000 residents (interpolated between the 1910 and 1920 censuses). The mean (median) dollar value of total agricultural output

[6]Many other crops and agricultural products were also recorded, but we do not analyze these because they collectively constitute only a negligible share of aggregate agricultural output for Illinois.

FIGURE 3

Value and Acreage of Illinois Crops

The graphs in Figure 3 are reproduced from page 15 of the 1927 Illinois crop report.

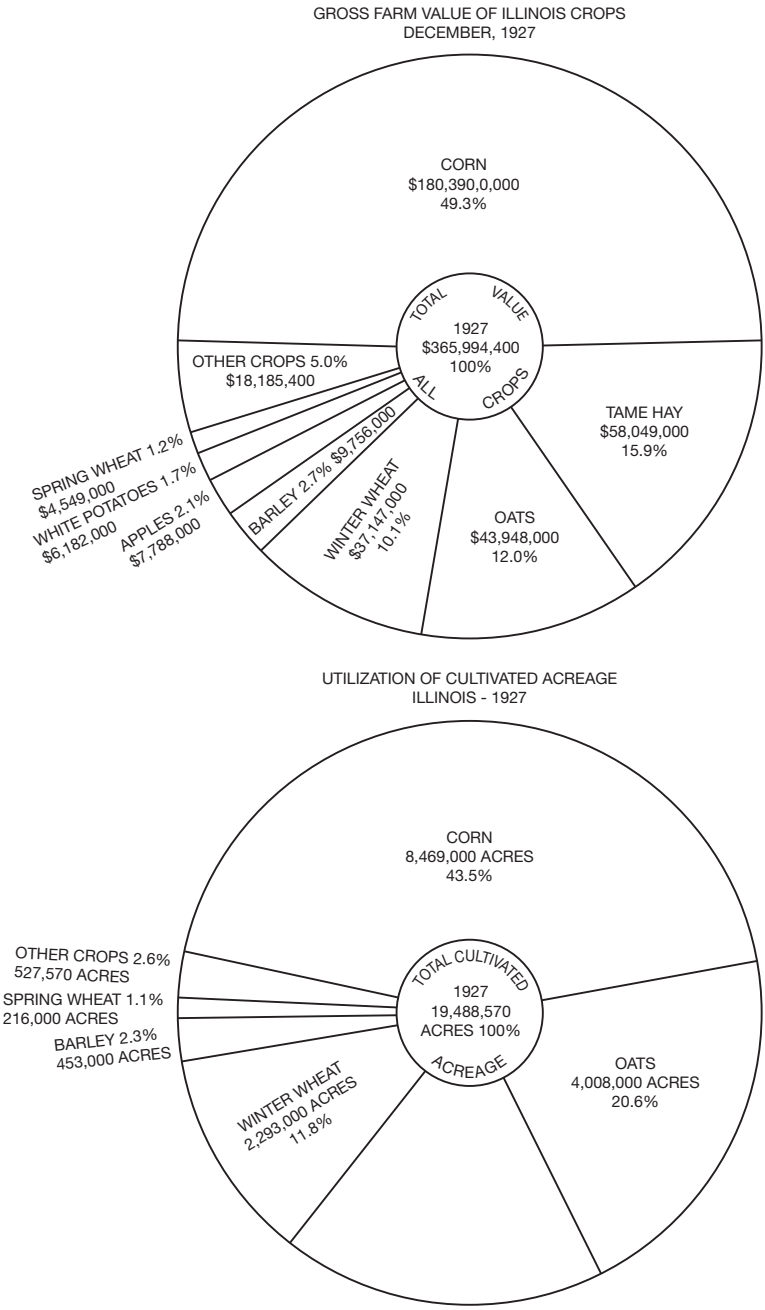


TABLE 1

County-Level Summary Statistics as of 1916 for Banking and Agricultural Data in Illinois

In Table 1, the sample is all counties in Illinois excluding Cook County.

	Mean	Std. Dev.	p25	p50	p75	Max
NATIONAL_BANKS	4.356	3.002	2.000	4.000	6.000	15.000
STATE_BANKS	6.307	4.507	3.000	6.000	9.000	24.000
FRACTION_NATIONAL_BANKS	0.427	0.212	0.300	0.400	0.542	1.000
NATIONAL_BANKS_PER_10K_RESIDENTS	1.511	0.853	0.775	1.423	2.010	4.408
BANK_LOANS, $1,000	3,730.5	3,960.3	1,192.3	2,358.9	4,480.6	19,213.8
1916_Population	33,016.0	24,985.6	16,492.0	24,020.4	38,034.5	128,898.7
BANK_LOANS/POPULATION	100.40	51.23	56.21	93.21	136.40	230.10
DEPOSITS/LIABILITIES, nat'l banks	0.6906	0.0822	0.6341	0.6932	0.7444	1.0264
DEPOSITS/LIABILITIES, state banks	0.7537	0.0692	0.7251	0.7653	0.8012	0.8704
(DEPOSITS + CAPITAL)/LIABILITIES, nat'l	0.8005	0.0795	0.7568	0.8122	0.8399	1.2166
(DEPOSITS + CAPITAL)/LIABILITIES, state	0.9021	0.0606	0.8869	0.9145	0.9406	0.9775
CORN_OUTPUT (bushels, 1 k)	2,383.7	1,714.6	935.9	2,050.3	3,417.8	8,131.6
HAY_OUTPUT (tons, 1 k)	62.43	42.36	29.41	50.68	83.28	189.60
OATS_OUTPUT (bushels, 1 k)	1,773.3	1,704.6	615.8	1,303.4	2,365.0	9,124.9
WHEAT_OUTPUT (bushels, 1 k)	109.38	112.24	31.58	72.96	161.33	742.50
BARLEY_OUTPUT (bushels, 1 k)	13.228	40.839	0.000	0.000	5.220	307.9920
CORN_PRICE_PER_BUSHEL	0.8444	0.0452	0.8100	0.8400	0.8600	1.0000
HAY_PRICE_PER_TON	9.911	1.981	8.250	10.000	11.000	15.000
OATS_PRICE_PER_BUSHEL	0.3699	0.0347	0.3500	0.3600	0.3800	0.5000
WHEAT_PRICE_PER_BUSHEL	1.1155	0.0621	1.0800	1.1200	1.1500	1.3000
BARLEY_PRICE_PER_BUSHEL	0.6456	0.0770	0.6000	0.6500	0.6500	0.9000
CORN_VALUE (dollars)	2,008.7	1,445.7	792.7	1,742.7	2,909.1	6,505.3
HAY_VALUE (dollars)	618.2	469.0	303.0	506.8	771.9	2,843.4
OATS_VALUE (dollars)	648.6	612.1	227.3	515.2	893.0	3,193.7
WHEAT_VALUE (dollars)	123.82	131.11	35.68	77.70	185.53	891.00
BARLEY_VALUE (dollars)	8.361	25.229	0.000	0.000	3.393	184.752
CORN_VALUE/AG_OUTPUT	0.563	0.129	0.483	0.569	0.654	0.814
HAY_VALUE/AG_OUTPUT	0.210	0.111	0.126	0.197	0.277	0.527
OATS_VALUE/AG_OUTPUT	0.1751	0.0814	0.1129	0.1628	0.2184	0.3821
WHEAT_VALUE/AG_OUTPUT	0.04975	0.05234	0.00932	0.03355	0.06728	0.24608
BARLEY_VALUE/AG_OUTPUT	0.00231	0.00694	0.00000	0.00000	0.00110	0.04178
AG_OUTPUT, $1,000	3,407.7	2,197.5	1,669.0	3,148.9	4,644.9	10,846.3
BANK_LOANS/AG_OUTPUT	1.083	1.028	0.484	0.769	1.169	4.989
No. of obs.	101					

was $3.4 million ($3.1 million). This output was dominated by corn, which accounted for 57% of total value in the median county, and for 48% even at the lower quartile. The agricultural reports also include the average sale price of each crop at the county level, and the table documents substantial heterogeneity in these prices across counties.

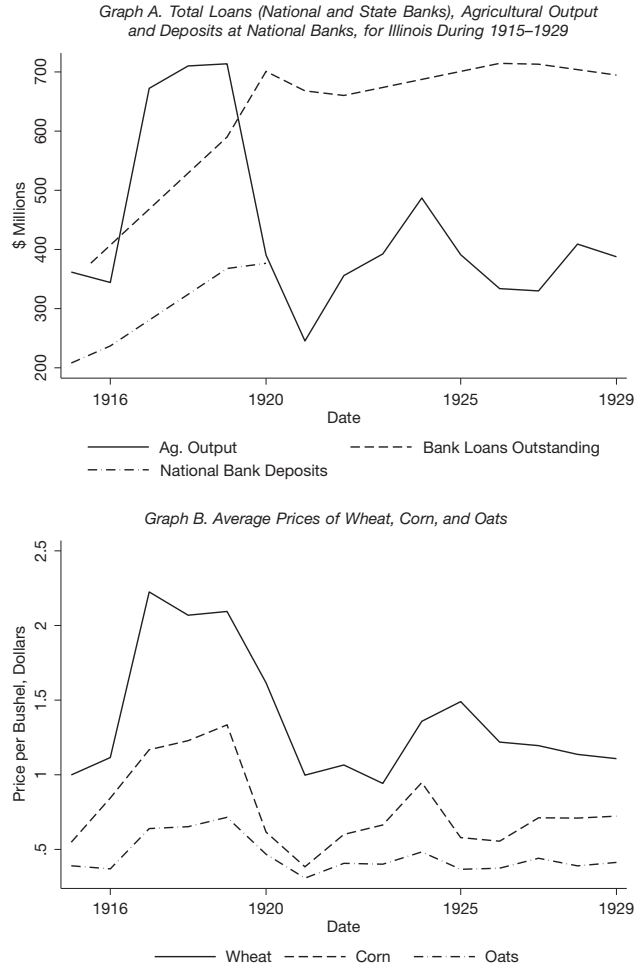
For state banks in a given county, deposits were about 75% of total liabilities on average, whereas for national banks, the comparable figure was only 69%. For state banks, the residual is mostly explained by paid-in capital, as deposits plus paid-in capital averaged over 90% of total liabilities. In contrast, for national banks, this fraction was only 80%. The differences in these fractions across state and national banks are significant at any conventional level.

Figure 4 examines trends in the county-level statistics over time. The years 1916–1920 were a time of rising credit growth nationwide, and this fact is reflected in our sample. Bank loans outstanding in Illinois roughly doubled during this time. Subsequently, the level of credit reached a plateau and very little deleveraging took place in the aggregate during the 1920s. Deposits at national banks similarly grew in the years leading up to 1919 and remained stable through the end of 1920.

FIGURE 4

Trends in Illinois Banking and Agricultural Statistics (1916–1930)

In Figure 4, banking data are from combining state banking reports for state banks with call reports and reports by the Comptroller of the currency for national banks (Graph A). Agricultural data are from crop reports by the Illinois Department of Agriculture (Graph B).



During 1916–1920, there was also explosive growth in agricultural output because of the demand for food products in Europe during World War I. However, as the war ended, European production rebounded, which led to less demand for American products. In response, there was a sharp decline in the value of agricultural output. Consistent with this, all crop prices rose during the war and subsequently fell, reflecting the broad deflation experienced throughout the economy around 1920–1921.

Comparing the aggregate patterns in Figures 1 and 4, higher discount rates are negatively associated with agricultural output, but not bank loans. This might seem to suggest that policy affected the economy through channels other than bank

lending, or not at all. However, we show in the remainder of the article that there was indeed a bank lending channel for the impact of discount window policy on the agricultural sector.

IV. Analysis and Results

A. Empirical Strategy

To study how exposure to discount window policy affected the agricultural sector, we use two specifications in our main analysis and then explore other variations as robustness checks.

Our first specification studies how the number of national banks per capita in a given county affected loan volumes and agricultural output around the interest rate increases in 1920 and in the aftermath of the 1920–1921 depression. We regress county-year outcomes Y_{ct} against county fixed effects, year fixed effects, and interactions between year dummies and the number of national banks per 10,000 people in 1916:

$$(1) \qquad Y_{ct} = \alpha_c + \gamma_t + \sum_y \beta_y \times \frac{\text{NATIONAL\_BANKS}_{c,1916}}{\text{POPULATION}_{c,1916}} \times 1\{y = t\} + \varepsilon_{ct},$$

where Y_{ct} represents the county-level agricultural output, or the total county-level loans across the two systems of banks. Intuitively, this regression captures simultaneously a difference-in-difference effect, relative to 1916, for each later year in the sample.[7] We will initially present results from this specification to illustrate our main findings qualitatively.

While this first specification is straightforward, it may seem more natural to think about the economic mechanisms in terms of the *fraction* of county banks that were national banks at any point in time. We also present results from a second specification based on this idea:

$$(2) \qquad Y_{ct} = \alpha_c + \gamma_t + \sum_y \beta_y \times \frac{\text{NATIONAL\_BANKS}_{c,y}}{\text{TOTAL\_BANKS}_{c,y}} \times 1\{y = t\} + \varepsilon_{ct},$$

where Y_{ct} is again the economic variable of interest. In this regression, the magnitudes of the coefficients reflect a counterfactual shift from 0% to 100% membership by county banks in the Fed for the average county.

However, this latter specification raises a concern about reverse causality. During 1916–1920, many new state banks opened, whereas there was little change in the presence of national banks, causing the fraction of national banks to fall sharply in some counties.[8] This makes the time-varying Fed membership

[7] Time t runs across the years 1916, 1919–1922, 1926, and 1927, whereas y can take all of these values except 1916. Thus, β_y represents the time-varying effect of the 1916 county-level Fed membership rate on economic outcomes. The 1916 effect is subsumed by county fixed effect α_c.

[8] For example, in the region of southern Illinois known as Little Egypt, 6 counties experienced declines of 40 percentage points or more in this fraction from 1916 to 1920 due to entry by state banks.

rate a confounded treatment measure, as entry by state banks could partially be a response to discount rate policy. On the other hand, it also means that a membership rate fixed as of an earlier year, such as 1916, would only be a very weak proxy for the membership rate in later years. This issue does not arise with results based on specification (1), as the number of national banks per capita was extremely stable throughout our sample (the correlation is above 90% between any 2 years).

To combine the stability of the first specification with the interpretability of the second, when we employ specification (2), we will instrument the time-varying membership rate with the 1916 per capita number of national banks. This instrument is not subject to reverse causality, since it is measured before the changes to discount rates occurred in 1920, and it remains extremely stable throughout the time period we study. As we will show, it also strongly predicts the fraction of banks that were national banks in each subsequent year.

More precisely, the endogenous regressors in (2) are the interaction terms $\frac{\text{NATIONAL\_BANKS}_{c,y}}{\text{TOTAL\_BANKS}_{c,y}} \times \mathbb{1}\{y = t\}$. Therefore, the first-stage regressions that implement our IV strategy are as follows:

$$(3)\quad \frac{\text{NATIONAL\_BANKS}_{c,y}}{\text{TOTAL\_BANKS}_{c,y}} \times \mathbb{1}\{y = t\} = \eta_c + \delta_t + \sum_y \psi_y \times \frac{\text{NATIONAL\_BANKS}_{c,1916}}{\text{POPULATION}_{c,1916}} \times \mathbb{1}\{y = t\} + \nu_{ct}.$$

These are estimated simultaneously with (2) to obtain our IV estimates.

This IV specification provides a useful way to think about magnitudes. However, to be clear, all our qualitative results are already illustrated by the reduced-form regression (1). The extra machinery of the IV framework is only important for thinking about magnitudes, and is not necessary to see the general narrative of our results.

B. Identifying Assumptions and Instrument Properties

Before exploring our results, we note the key identifying assumption in our analysis: Systematic correlation between the presence of national banks in 1916, and changes in county lending and agricultural output thereafter, can be attributed to shifts in discount window policy.[9] While this assumption is not testable, we can investigate its plausibility by examining whether counties with many national banks look systematically different from other counties in the cross section as of 1916. If not, it becomes more plausible that they would have followed similar trends, if not for the discount window policy changes.

Figures 5 and 6 map out the geographic distribution of our two measures of policy exposure (the number of national banks per capita and the fraction of national

[9]In the framework of Callaway, Goodman-Bacon, and Sant'Anna (2021), this assumption is referred to as *strong parallel trends.* Granted this assumption, the coefficients β_y from specifications (1) and (2) can be interpreted as estimating a weighted average effect of a marginal increase in the number or fraction of national banks. The exact weighting scheme is described in their paper, but we note here that all the weights are positive and sum to 1. Intuitively, more weight is placed on counties where the number of national banks is closer to the sample average.

FIGURE 5

Number of National Banks per Capita for Each Illinois County

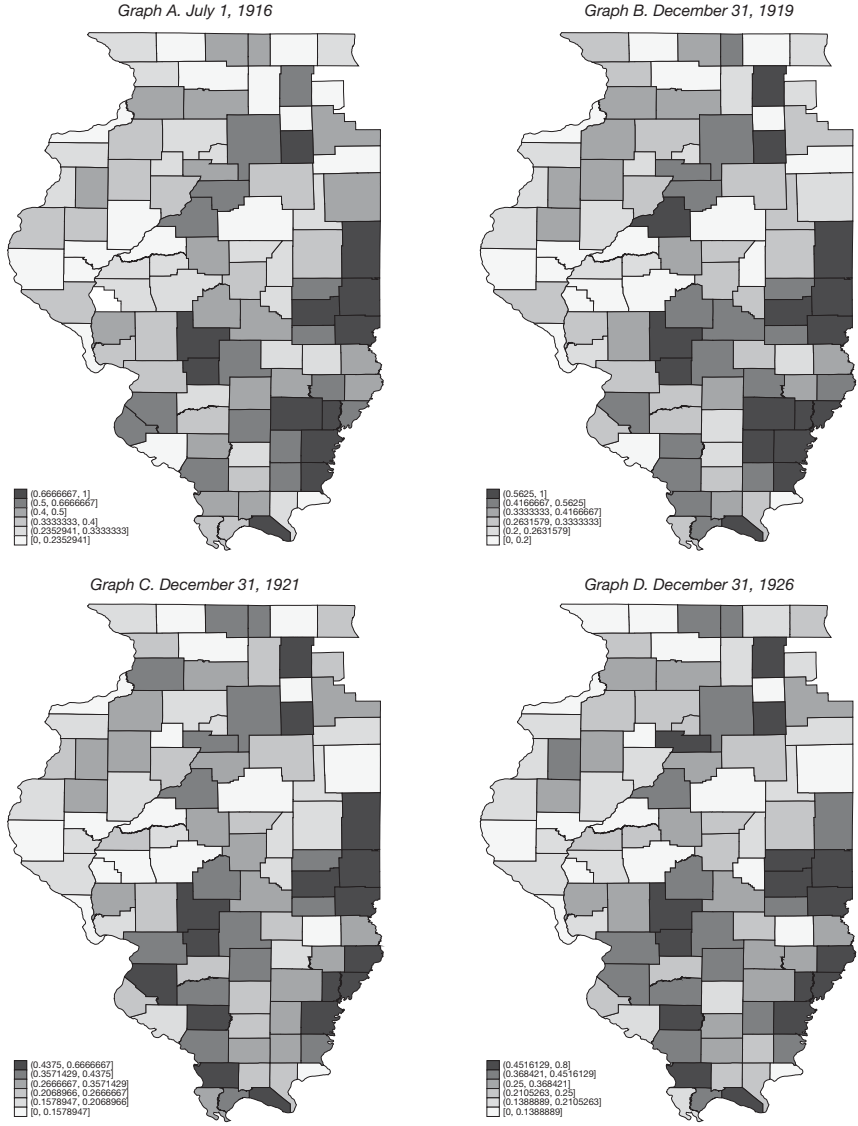
Figure 5 shows the number of national banks per capita for each Illinois county as of 4 dates during our sample period: July 1, 1916 (Graph A); Dec. 31, 1919 (Graph B); Dec. 31, 1921 (Graph C); and Dec. 31, 1926 (Graph D). National banks are compiled from call reports filed with the Federal Reserve for 1916, and from annual reports of the Comptroller of the Currency for other years. Population statistics are interpolated by county between decennial Census numbers. In each year, counties are divided into 6 bins by the number of national banks per capita, with darker shading corresponding to a higher fraction.

banks) for the years 1916, 1919, 1921, and 1926. Encouragingly, neither shows any particular clustering or pattern, with high and low values scattered all across the state. In Figures 7 and 8, we further demonstrate that these two measures do not predict significant county-level differences in the total number of banks, loan

FIGURE 6

Fraction of Banks That Were National Banks for Each Illinois County

Figure 6 shows the fraction of banks that were national banks for each Illinois county as of 4 dates during our sample period: July 1, 1916 (Graph A); Dec. 31, 1919 (Graph B); Dec. 31, 1921 (Graph C); and Dec. 31, 1926 (Graph D). National banks are compiled from call reports filed with the Federal Reserve for 1916, and from annual reports of the Comptroller of the Currency for other years. State banks are compiled from the statements of condition published by the Illinois state government. In each year, counties are divided into 6 bins by the number of national banks per capita, with darker shading corresponding to a higher fraction.

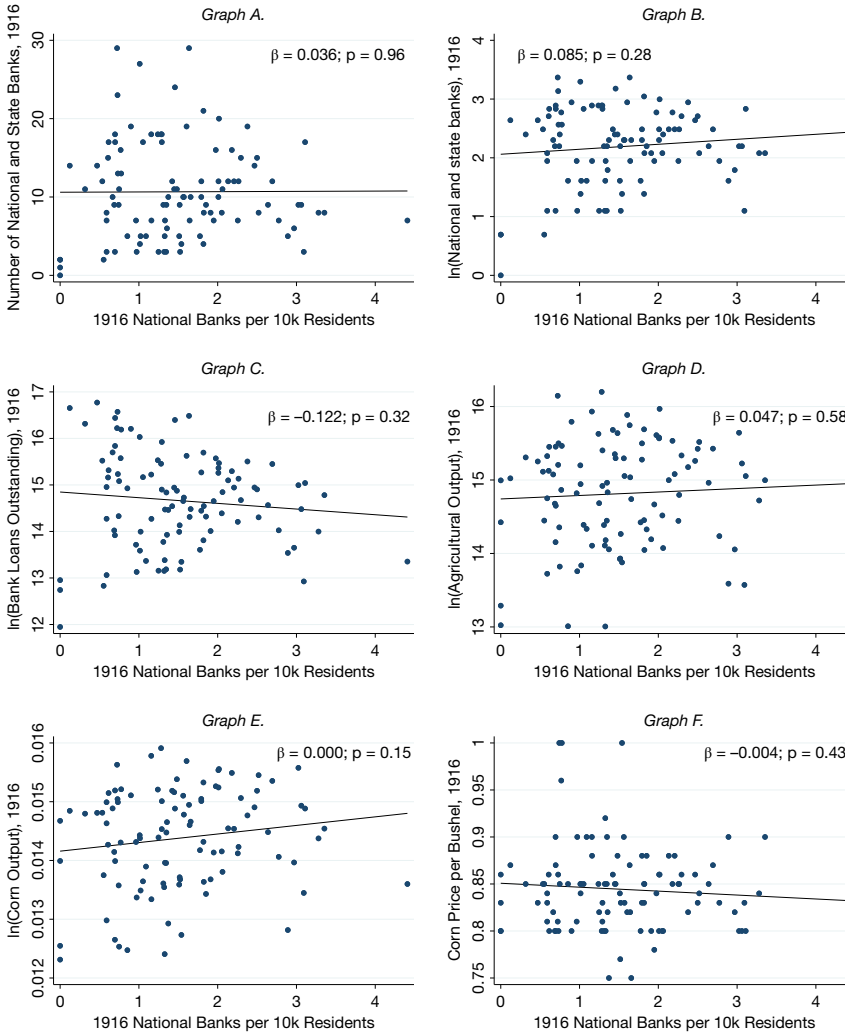


volumes, or agricultural output. While one cannot test the exclusion restriction, these findings are consistent with the view that the relative composition of state versus national banks in a given county was mostly driven by historical trends

FIGURE 7

Balance Checks

In Figure 7, each dot corresponds to a county in Illinois (excluding Cook County), and the horizontal axis plots the number of national banks per 10,000 residents as of 1916. The graphs plot the number of banks in the county as of July 1916 (both national and state banks) (Graph A); the logarithm of this number (Graph B); the logarithm of the total bank loans outstanding as of July 1916 (Graph C); the logarithm of the total value of corn, hay, oats, wheat, and barley produced during 1916 (Graph D); the logarithm of the total bushels of corn produced during 1916 (Graph E); and the average sale price of corn during 1916 (Graph F). Each graph also reports a best-fit line and its slope, with a *p*-value calculated from White (1980) standard errors.



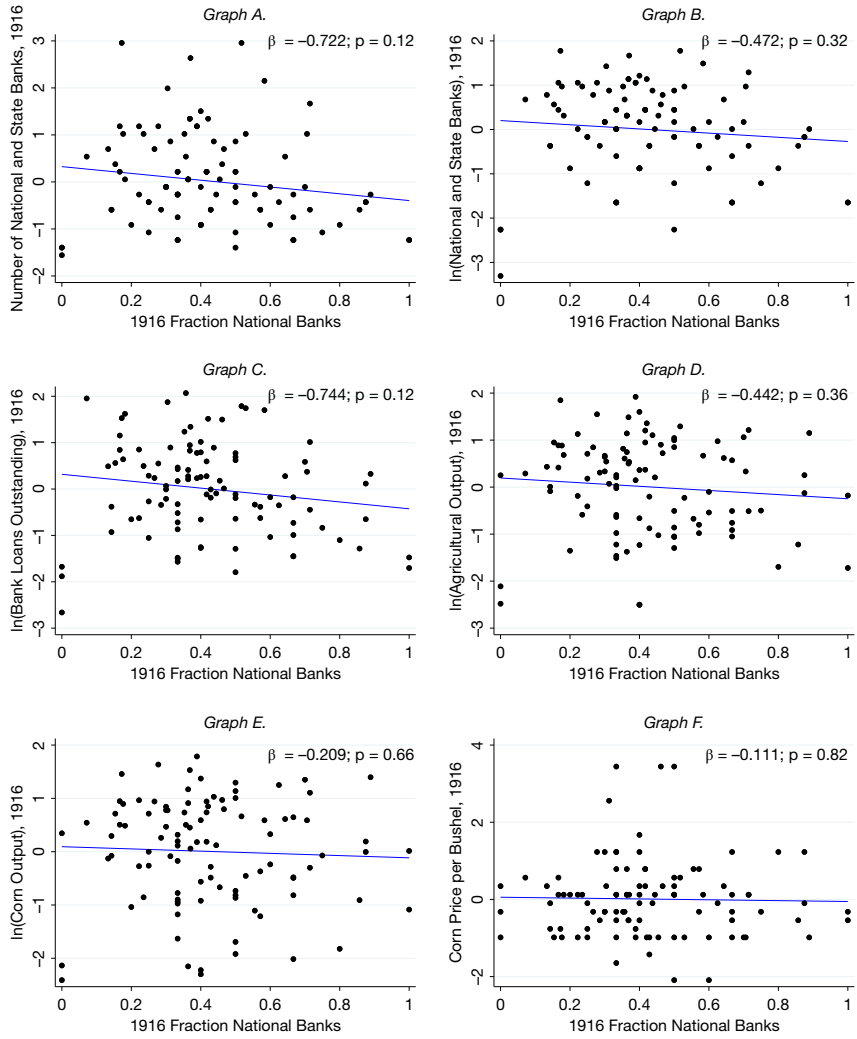
in the late 1800s, which were no longer relevant to relative county-level trends in 1916–1920.[10]

[10]These patterns might seem surprising at first glance since it is well known that national banks at the time were more concentrated in large, populous cities with greater financial and industrial development. The figures suggest that, at least in Illinois, this correlation may be driven by comparing the rest of the

FIGURE 8

Balance Checks

In Figure 8, each dot corresponds to a county in Illinois (excluding Cook County), and the horizontal axis plots the fraction of banks that were national banks as of 1916. The graphs plot the number of banks in the county as of July 1916 (both national and state banks) (Graph A); the logarithm of this number (Graph B); the logarithm of the total bank loans outstanding as of July 1916 (Graph C); the logarithm of the total value of corn, hay, oats, wheat, and barley produced during 1916 (Graph D); the logarithm of the total bushels of corn only produced during 1916 (Graph E); and the average sale price of corn during 1916 (Graph F). Each of these outcomes is standardized by subtracting the sample mean and dividing by the sample standard deviation. Each graph also reports a best-fit line and its slope, with a *p*-value calculated from White (1980) standard errors.



When we employ our IV specification, we need the per capita number of national banks in 1916 to be a strong predictor for the fraction of banks that were national banks in later years. We analyze this relevance condition in Table 2 and

state to Cook County, which is home to Chicago and is excluded from our analysis. The rest of Illinois outside Chicago does not appear to follow the same pattern.

TABLE 2

Relevance of County-Level National Banks per Capita as an Instrument for the County-Level Fraction of National Banks

In Table 2, in each regression, the explanatory variable is the number of national banks per 10,000 residents in each county as of July 1916. The outcome variable is the fraction of county banks that were national banks as of the year listed in each column. Standard errors are reported in parentheses (White (1980)). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	County-Level Fraction of Banks That Are National Banks							
	1	2	3	4	5	6	7	8
1916 num. national banks per 10K residents	0.177*** (0.0171)	0.162*** (0.0179)	0.122*** (0.0150)	0.119*** (0.0148)	0.119*** (0.0148)	0.131*** (0.0159)	0.131*** (0.0159)	0.130*** (0.0188)
Year	1916	1919	1920	1921	1922	1926	1927	1929
No. of obs.	100	101	101	101	101	101	101	101
R^2	0.497	0.502	0.476	0.471	0.466	0.468	0.449	0.414

Figure 9. Table 2 reports that the 1916 number of national banks strongly predicts the fraction of national banks in every subsequent year in our sample, and explains between 40% and 50% of its cross-sectional variation. Figure 9 illustrates these patterns visually with scatter plots.

Because we use the presence of national banks as a measure of local exposure to discount rate policy, it would also be helpful to know that this presence is correlated with the actual usage of the discount window. Discount window usage can only be directly measured with the granular data available in call reports, which have not survived for most of the years in our sample (see Section III). However, there are 2 dates near the events we study, July 1916 and July 1921, for which call reports do survive. To help validate our empirical strategy, we collect the data on member banks' discount window liabilities from both of these reports.[11]

Our analysis with these data faces some limitations compared to the rest of the article. With just 2 dates, separated by 5 years, we cannot exploit meaningful within-county variation. Also, the 2 snapshots understate the general volume of discount window borrowing, because both are from July when this borrowing was typically at a seasonal low.[12] However, we can use the data to help assess the article's main proxy for exposure to discount window policy. In Table 3, we examine how a county's 1916 number of national banks per 10,000 residents predicts discount window liabilities in July 1916 and 1921. Columns 1 and 2 report that when there is one extra national bank per 10,000 residents in a county, the probability of there being any discount window liabilities at county banks increases by 12.9% in 1916, and 12.2% in 1921, respectively. Similarly, columns 3 and 4 report that the amount of discount window liabilities across all county banks (per 10,000 residents) increases by $1,183 in 1916 and $8,594 in 1921, respectively. These findings support our use of national bank presence as a proxy for exposure to discount rate policy.

[11] Specifically, the items we collect are a liability labeled "Rediscounts with Federal Reserve bank" in 1916, and a contra-asset labeled "Notes and bills rediscounted with Federal Reserve Bank" in 1921.

[12] See, for example, Figure 5 in Carlson and Wheelock (2016).

FIGURE 9

Visual Illustration of the Regressions in Table 2

In Figure 9, the horizontal axis in each graph is the number of national banks per 10,000 residents as of July 1916. The vertical axis plots the fraction of banks that were national banks as of July 1916 (Graph A), Dec. 1919 (Graph B), Dec. 1922 (Graph C), Dec. 1926 (Graph D), Dec. 1927 (Graph E), and Dec. 1929 (Graph F).

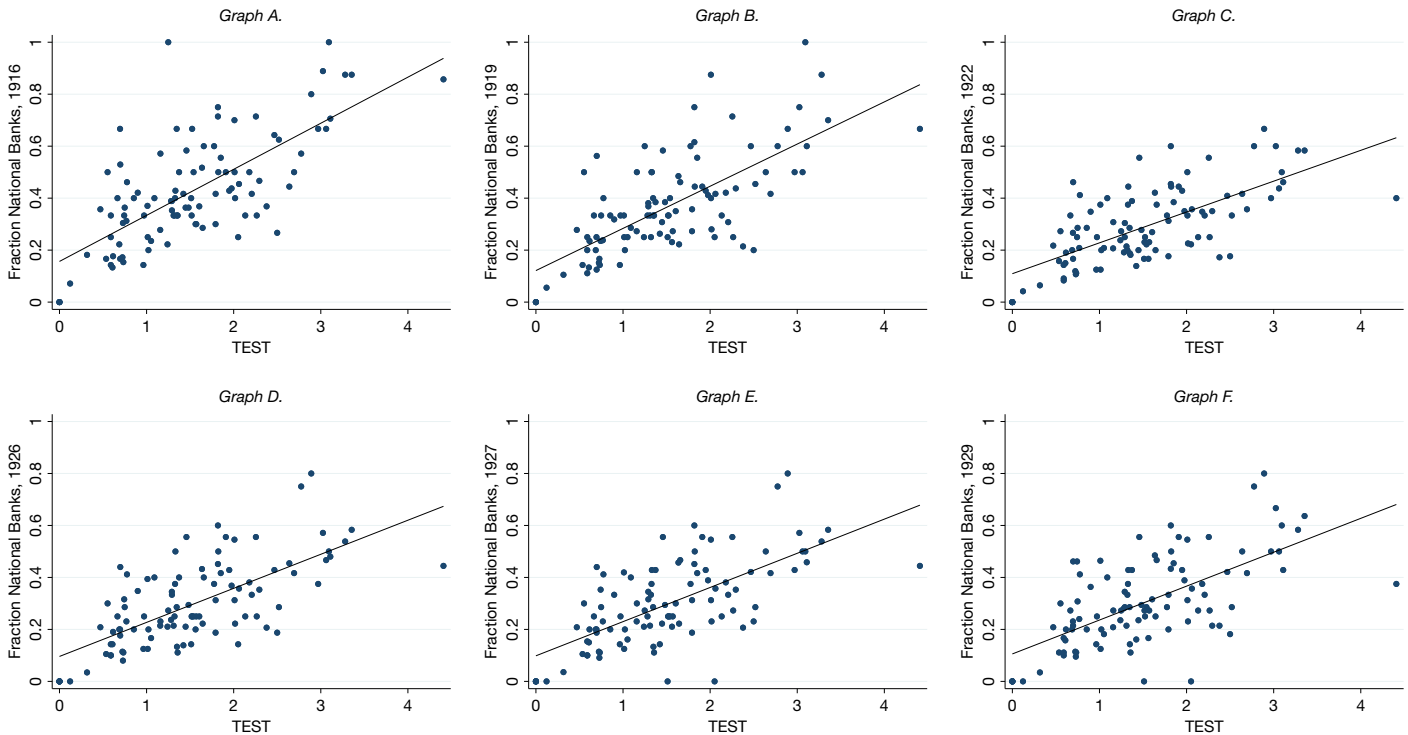


TABLE 3

Validating That National Bank Presence Is a Good Proxy for Exposure to Discount Window Policy

In Table 3, we collect data on discount window liabilities at national banks using call reports for July 1916 and July 1921. Then we regress county-level measures of discount window usage at these 2 dates, on the 1916 number of national banks per 10,000 residents in the county, which is the key instrument that we use throughout the article to measure exposure to discount window policy. In columns 1 and 2, the outcome is an indicator of the county having any discount window liabilities at all. In columns 3 and 4, the outcome is the volume of discount window liabilities per 10,000 residents. Standard errors are robust (White, 1980) and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Discounts > 0		Discounts per 10K Residents	
	1	2	3	4
NATIONAL_BANKS	0.129**	0.122**	1,183.7**	8,594.4**
	(0.0557)	(0.0493)	(536.6)	(4,037.3)
INTERCEPT	0.103	0.549***	−280.5	12,008.3**
	(0.0863)	(0.0939)	(672.0)	(6,015.8)
Date	July 1916	July 1921	July 1916	July 1921
R^2	0.0570	0.0544	0.0881	0.0428
No. of obs.	101	101	101	101

C. Regulatory Differences Between National and State Banks

We note that national and state banks in Illinois faced different regulatory and supervisory regimes. It is important to check that this does not invalidate our preferred interpretation of our findings. Specifically, we need to check that key regulations did not 1) change at the same times and in the same directions as discount window policy, nor 2) constrain member banks so much more, compared to nonmember banks, that a tightening of discount window policy could not have further affected their relative lending activity.

Drawing on White ((2014), Chapter 3, pp. 126–188), we identify and analyze the following key regulatory differences between the two types of banks in Illinois at this time:

- *Reserve requirements*: White's Table 3.3 reveals that reserve requirements for both member banks, and Illinois nonmember state banks, hardly changed from the time the Federal Reserve system began operations, which addresses concern #1. The table also reports that Illinois state banks consistently faced *higher* reserve requirements than member banks, across all regions and deposit categories, addressing concern #2.
- *Supervision*: Nonmember state banks were only answerable to their state regulators, whereas member banks were auditable by the Federal Reserve Board, their district bank, and (until 1917) the Comptroller of the Currency. However, these differences were constant throughout our time period, addressing concern #1. As for concern #2, we have seen no source suggesting that the extra burden of supervision on member banks would have rendered other factors irrelevant at the margin to their lending activity.
- *Par clearance*: Country banks often cleared checks below par, as an implicit exchange fee. The Fed forced its member banks to end this practice. White (2014) mentions that this was a contentious issue, but his analysis in Table 3.7 suggests that it had a little real impact on membership decisions. Hence, it seems unlikely

to have been a binding constraint on lending, nor to have fluctuated along with discount window policy.

- *Branching limitations on member banks, and deposit insurance available to state banks*: These were important issues in other states, but not in Illinois, which prohibited state bank branching and did not offer deposit insurance during our sample period.
- *Minimum capital requirements*: White's Table 1.4 (p. 20) reports that capital requirements in Illinois were identical between national and state banks for towns with fewer than 3,000 people or more than 25,000 residents. For towns with 3,000–25,000 residents, capital requirements were slightly higher for national banks,[13] but there is no evidence that this gap fluctuated during our sample period (concern #1), nor that it constrained member banks' lending from responding to other factors (concern #2).
- *Portfolio restrictions*: White ((2014), pp. 23–25) explains that national banks faced restrictions in lending against real estate, whereas most state-chartered banks faced fewer such restrictions (he does not specifically describe the policy in Illinois). Mortgages have a long duration, so it seems unlikely that variation in real estate lending drives our main results, which appear rapidly alongside changes in discount window policy.

In sum, although there were differences between the two systems, they seem unlikely to invalidate our interpretation that discount window policy affected local lending and agriculture.

D. Effects of Discount Window Policy Tightening

Table 4 summarizes the results of our estimation of specification (1). The outcome variable is listed at the top of each column. The main effects of the time dummies are listed in the lower rows of the table. These reflect the average change in the outcome variable since 1916 for a hypothetical county with no national banks. The key terms of interest are the interaction terms listed at the top of the table. These reflect the additional growth in each outcome variable since 1916 exhibited by a hypothetical county with one more national bank per 10,000 residents.

The nearly zero coefficients in the top row show that, regardless of national bank presence, Illinois counties grew at the same average rate in the expansion from 1916 to 1919, during which time the discount rate was approximately 4%. There does not appear to be a difference for counties that had more member banks. This is a useful placebo test that shows that Federal Reserve membership does not correlate with differential trends across counties during the time period *before* discount window policy changed.

However, after discount window conditions tightened at the end of 1919, the interaction terms demonstrate that the two types of counties exhibited very different responses. A hypothetical county with no national banks would still have realized agricultural revenues that were roughly 23% greater than in 1916, based on the main

[13]Minimum capital was $25,000 for state banks versus $50,000 for national banks in towns with 3,000–6,000 residents, and $50,000 for state banks versus $100,000 for national banks for towns with 6,000–25,000 residents.

TABLE 4

County-Level Regressions of Total Loans Outstanding, and Agricultural Output, as a Function of the 1916 Number of National Banks in a County per 10,000 Residents

In Table 4, the excluded year in each column is 1916, so all effects are relative to that date. County fixed effects are included in all regressions. Standard errors are clustered by county and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ln(OUTPUT)	ln(LOANS)	LOANS/OUTPUT
	1	2	3
12/31/19 × 1916 NATIONAL_BANKS	−0.0245	0.0148	0.0914
	(0.0302)	(0.0238)	(0.0571)
12/31/20 × 1916 NATIONAL_BANKS	−0.0700**	−0.0625*	−0.1942
	(0.0325)	(0.0336)	(0.1354)
12/31/21 × 1916 NATIONAL_BANKS	−0.0461	−0.0702*	−0.6028**
	(0.0351)	(0.0362)	(0.2641)
12/31/22 × 1916 NATIONAL_BANKS	−0.0511	−0.0721**	−0.2658*
	(0.0339)	(0.0356)	(0.1440)
12/31/26 × 1916 NATIONAL_BANKS	0.000924	−0.113368***	−0.558093***
	(0.0313)	(0.0408)	(0.1922)
12/31/27 × 1916 NATIONAL_BANKS	−0.0108	−0.1163***	−0.5298***
	(0.0331)	(0.0428)	(0.1828)
12/31/29 × 1916 NATIONAL_BANKS	−0.0000123	−0.1594363***	−0.4491417***
	(0.0318)	(0.0396)	(0.1311)
12/31/19	0.769***	0.455***	−0.384***
	(0.0569)	(0.0441)	(0.1323)
12/31/20	0.228***	0.795***	1.058***
	(0.0592)	(0.0659)	(0.2885)
12/31/21	−0.275***	0.759***	2.707***
	(0.0704)	(0.0706)	(0.5536)
12/31/22	0.119*	0.751***	1.245***
	(0.0641)	(0.0695)	(0.3026)
12/31/26	−0.0108	0.8645***	1.9955***
	(0.0629)	(0.0735)	(0.4067)
12/31/27	−0.0289	0.8643***	2.0123***
	(0.0673)	(0.0752)	(0.3987)
12/31/29	0.125**	0.871***	1.457***
	(0.0592)	(0.0717)	(0.2799)
Fixed effect	County	County	County
No. of obs.	808	807	808

effect of 0.23 for 1920. In contrast, the same county with 4 national banks per 10,000 residents (the maximum in the data) would have seen its output fall to approximately 1916 levels: The interaction coefficient of −0.07, multiplied by 4, completely offsets the 1920 main effect of 0.23.

In column 2 of Table 4, we examine the patterns for the county-level volume of loans outstanding. As with the output results in column 1, there is no differential effect of national bank presence in 1919, but then a strong effect in 1920. This suggests that the mechanism for the relatively lower agricultural output in this year was a restriction of bank credit, an interpretation that we continue to develop below.

We next return to column 1 of Table 4, and examine the years after 1920. When discount rates were subsequently lowered, the discrepancy in agricultural output across counties quickly vanished: The interaction coefficients are not statistically significant in any year after 1920. However, column 2 reports that the differential effect on loans outstanding persists: Indeed, there is a statistically significant difference across counties with more national banks in every subsequent year in

TABLE 5

Robustness to Different Measures of National Bank Presence

In column 1 of Table 5, the measure is national bank deposits per capita; in column 2, it is national bank assets per capita; and in column 3, it is national bank loans outstanding per capita. All are measured at year-end 1916, in thousands of dollars. The specifications are as in column 2 of Table 4. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ln(LOANS)	ln(LOANS)	ln(LOANS)
	1	2	3
12/31/19 × proxy	−0.552	−0.438	−0.859
	(0.528)	(0.360)	(0.596)
12/31/20 × proxy	−2.703***	−1.886***	−3.524***
	(0.751)	(0.503)	(0.806)
12/31/21 × proxy	−3.405***	−2.337***	−4.280***
	(0.815)	(0.544)	(0.871)
12/31/22 × proxy	−3.279***	−2.257***	−4.154***
	(0.779)	(0.521)	(0.841)
12/31/26 × proxy	−3.551***	−2.526***	−4.701***
	(0.826)	(0.537)	(0.873)
12/31/27 × proxy	−3.850***	−2.720***	−5.163***
	(0.928)	(0.614)	(0.959)
12/31/29 × proxy	−3.248***	−2.322***	−4.640***
	(0.918)	(0.615)	(0.998)
12/31/19	0.511***	0.516***	0.521***
	(0.0425)	(0.0428)	(0.0418)
12/31/20	0.862***	0.865***	0.878***
	(0.0624)	(0.0612)	(0.0586)
12/31/21	0.856***	0.856***	0.868***
	(0.0692)	(0.0676)	(0.0647)
12/31/22	0.837***	0.838***	0.850***
	(0.0657)	(0.0645)	(0.0620)
12/31/26	0.905***	0.913***	0.929***
	(0.0702)	(0.0675)	(0.0636)
12/31/27	0.918***	0.925***	0.948***
	(0.0746)	(0.0720)	(0.0671)
12/31/29	0.824***	0.832***	0.863***
	(0.0755)	(0.0735)	(0.0685)
Fixed effect	County	County	County
Proxy	1916 nat'l deposits	1916 nat'l assets	1916 nat'l loans
No. of obs.	807	807	807

the sample. This discrepancy plays a key role in our interpretation of the long-term effects of the policy changes, and we will return to it later.

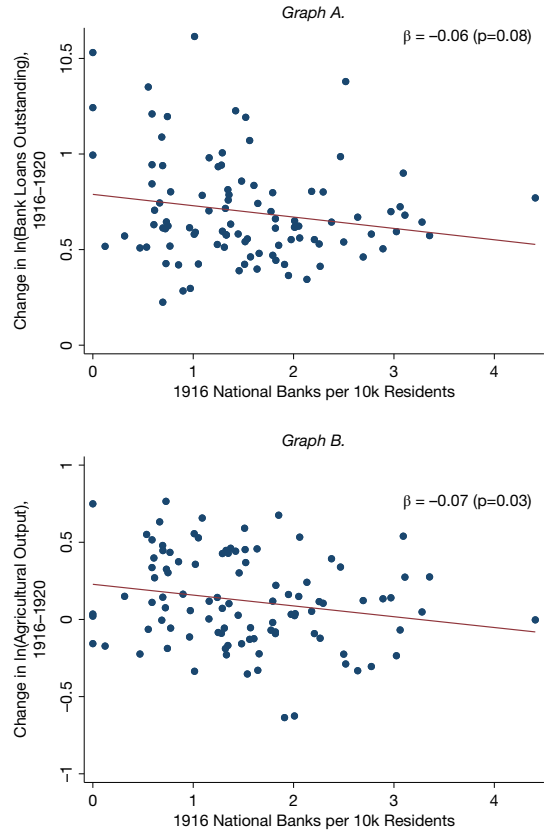
While we use the 1916 number of national banks per capita as the proxy for national bank presence in specification (1), our results are robust to using alternative proxies. In particular, Table 5 reports the discrepancy in county-level loans beginning in 1920 is qualitatively unchanged if we use the end-of-1916 dollar value of county-level, per capita national bank deposits, assets, or loans.

Our main specification was set up with the outcome variable in levels. However, we can obtain numerically identical results by dropping the county fixed effects, and replacing the outcome variable Y_{ct} with its change since 1916, that is, $\Delta Y_{c,1916\rightarrow t}$. Following this intuition, Figure 10 depicts the underlying variation behind our credit and output results for the years 1916 and 1920, by plotting $\Delta Y_{1916\rightarrow 1920}$ for each county against that county's 1916 number of national banks per 10,000 residents, where Y is the log bank loans outstanding in Graph A, and log agricultural revenues in Graph B.

FIGURE 10

Visually Illustrating the Regression Results

In Figure 10, each dot in the graphs corresponds to a county in Illinois (excluding Cook County). The graphs plot the change in the logarithm of the county-level loans outstanding (Graph A) and the agricultural output (Graph B) from 1916 to 1920. The explanatory variable on the horizontal axis is the county's 1916 number of national banks per capita. The slope and *p*-values are reported for a best-fit line with White (1980) standard errors.



The slopes of the fitted lines in Figure 10 correspond to the coefficients in the second row of Table 4. The figure demonstrates that the variation driving those regressions is spread out across the state, and importantly is not driven by any small subset of observations or by any outlier counties. This helps address identification concerns: Any confounding variable would have to vary quite smoothly both with membership rates in 1916 and with credit and output growth during 1916–1920.

We can interpret the patterns in Figure 10 as capturing a credit-output multiplier for the year 1920: The ratio of the two coefficients is an IV estimate of the impact of a marginal dollar of credit on county-level output. To make this calculation explicit, we estimate the following 2-stage system:

$$\Delta L_{c,1916\to1920} = \alpha + \beta \times \frac{\text{NATIONAL\_BANKS}_{c,1916}}{\text{POPULATION}_{c,1916}} + \varepsilon_c, \tag{4}$$

TABLE 6

Instrumental Variables Regressions for the Marginal Effect of Bank Credit on Agricultural Output During 1920

In Table 6, the sample is each county in Illinois excluding Cook County. Each regression models the within-county change in agricultural output from 1916 to 1920 as a function of the within-county change in bank loans outstanding from 1916 to 1920. In columns 1 and 2, both output and loans are measured in logs, whereas in columns 3 and 4, they are measured in levels. In each regression, the change in the bank lending volume is instrumented with the county's number of national banks per 10,000 residents as of 1916, following specification (5). The other explanatory variables are measured as of 1916 and left as exogenous. Columns 1 and 3 are unweighted regressions. In columns 2 and 4, observations are weighted by the inverse of the county's 1916 population. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ΔIn(OUTPUT)	ΔIn(OUTPUT)	ΔOUTPUT	ΔOUTPUT
	1	2	3	4
ΔIn(LOANS), 1916–1920	0.869**	1.026*		
	(0.403)	(0.529)		
ΔLOANS, 1916–1920			0.575**	0.641**
			(0.289)	(0.266)
1916 In(LOANS)	0.555***	0.717***	687,035.0*	562,961.5*
	(0.164)	(0.230)	(375,122.0)	(338,759.8)
1916 In(AG_OUTPUT)	−0.591***	−0.765***	−1,759,741.5**	−1,617,086.2***
	(0.161)	(0.226)	(776,360.1)	(574,619.2)
LOANS/AG_OUTPUT	−0.182**	−0.277***	−1,115,939.5*	−936,004.9*
	(0.0728)	(0.1043)	(665,627.6)	(481,800.0)
Weighting	None	(1920 pop.) − 1	None	(1920 pop.) − 1
No. of obs.	100	100	100	100

$$\Delta Y_{c,1916\to1920} = \eta + \theta \times \Delta L_{c,1916\to1920} + v_c. \tag{5}$$

In the first-stage regression (4), we predict county-level loans as a function of the number of national banks per 10,000 residents in 1916, and rely on the identifying assumption that changes in county loans from 1916 to 1920 should not correlate with the 1916 number of national banks except due to changes in the Fed's policy. Using this first stage to instrument the structural equation (5), we obtain a consistent estimate of θ.

Table 6 implements this approach. column 1 uses a log specification, and reports an estimated multiplier of 0.869, implying that a 1% increase in credit in 1920 would have led to a 0.869% increase in output. The median county-level loan-to-output ratio in 1920 was approximately 1.4, so dividing the 0.869 estimate by 1.4 suggests a dollar multiplier of $0.62. In the levels specification (column 3), the estimated effect of a marginal dollar of credit is $0.58 in additional output, closely aligned with the log specification. Table 6 also includes analysis weighted by the inverse of population (columns 2 and 4), to focus on the rural counties where credit frictions are likely to be important, and reports magnitudes that are similar or slightly larger.

Next, we revisit the magnitudes of our findings so far with our alternative specification (2), which explains outcomes in terms of the *fraction* of banks that were national banks, and instruments this fraction with the 1916 number of national banks per capita. The results are presented in Table 7. Note that the interaction coefficients of interest here are roughly equal to the interaction coefficients in Table 4, divided by their respective year's coefficient in Table 2. The trends in

TABLE 7

County-Level Regressions of Total Loans Outstanding, and Agricultural Output, as a Function of the Fraction of National Banks in a County

In Table 7, the excluded year in each column is 1916, so all effects are relative to that date. County fixed effects are included in all regressions. The interaction terms are instrumented with interactions between time dummies and the 1916 number of national banks per capita, following specification (3). Standard errors are clustered by county and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ln(OUTPUT)	ln(LOANS)	LOANS/OUTPUT
	1	2	3
12/31/19 × FRAC_NATL	−0.1659	0.0820	0.4488
	(0.195)	(0.145)	(0.417)
12/31/20 × FRAC_NATL	−0.591**	−0.523**	−1.737
	(0.284)	(0.262)	(1.186)
12/31/21 × FRAC_NATL	−0.406	−0.600**	−5.202**
	(0.319)	(0.288)	(2.283)
12/31/22 × FRAC_NATL	−0.451	−0.620**	−2.396*
	(0.293)	(0.300)	(1.315)
12/31/26 × FRAC_NATL	−0.0114	−0.8765***	−4.4011***
	(0.251)	(0.312)	(1.586)
12/31/27 × FRAC_NATL	−0.100	−0.896***	−4.171***
	(0.263)	(0.329)	(1.521)
12/31/29 × FRAC_NATL	−0.0186	−1.2336***	−3.5857***
	(0.254)	(0.345)	(1.185)
12/31/19	0.794***	0.448***	−0.401**
	(0.0822)	(0.0597)	(0.2010)
12/31/20	0.293***	0.851***	1.272***
	(0.0911)	(0.0892)	(0.4285)
12/31/21	−0.226**	0.826***	3.300***
	(0.1081)	(0.0971)	(0.8181)
12/31/22	0.173*	0.821***	1.544***
	(0.0975)	(0.1001)	(0.4689)
12/31/26	−0.00490	0.95163***	2.45499***
	(0.0889)	(0.1022)	(0.5861)
12/31/27	−0.0142	0.9555***	2.4598***
	(0.0951)	(0.1057)	(0.5760)
12/31/29	0.131	1.004***	1.872***
	(0.0875)	(0.1110)	(0.4393)
Fixed effect	County	County	County
Instrument	1916 nat'l p.c.	1916 nat'l p.c.	1916 nat'l p.c.
No. of obs.	807	807	807

these coefficients are similar to Table 4, but the specification allows for a clearer interpretation of the economic magnitudes.

The coefficient of −0.591 implies that agricultural output would have been about 45% lower ($e^{-0.591} - 1 = -0.446$) in 1920, for a hypothetical county with 100% membership rate in the Fed compared with one with 0% membership, and bank lending would have been about 41% lower ($e^{-0.523} - 1 = -0.407$). A more salient way to assess these magnitudes is to compare the upper and lower quartiles of national bank fractions in Table 1 (0.5 and 0.3, respectively). The discount window tightening would then be expected to cause output to decline by 9% more for a county at the upper as compared with the lower quartile ($0.45 \times [0.5 - 0.3] = 0.09$), and lending by a similar 8.2%.

Finally, we investigate potential indirect usage of the discount window by nonmember banks, via correspondent relationships with member banks. This activity is the focus of a growing literature focusing on the same era as we study

(e.g., Anderson et al. (2018)). Our identification strategy assumes that these indirect connections did not undo the greater sensitivity of member banks' local lending to discount window policy. To investigate this assumption, we ask whether nonmember banks' discount window usage predicts local lending and drives out our main effects of interest.

Usage of the discount window by a nonmember bank creates a liability on that bank's balance sheet. Following Anderson et al. (2022), we focus on "rediscounts" and "bills payable" as the liabilities that would most likely reflect such usage. We record these liabilities for each of the years 1916–1922 in our sample, for all state banks in Illinois (continuing to exclude Cook County). The aggregate amount of these liabilities in our sample grew from about $5 million in 1916 and 1919 to $25 million by 1921, then declined to $18 million in 1922.

In Table 8, we repeat our main analysis from Table 4, but control for the per-capita amount of bills payable and rediscounts at state banks within a given county and year. Across our three specifications, we find no predictive power of this new control variable for loans, output, or the loans/output ratio, neither economically nor statistically. The main interaction terms on which we focus are essentially unchanged from Table 4.

We conclude that, while nonmember banks did use the discount window indirectly to some extent, this does not account for our main findings. A plausible explanation is that nonmembers mainly used the discount window to meet sudden depositor withdrawals, which jeopardize the bank's survival, but this borrowing faced enough extra costs compared to member banks to deter them from using it opportunistically to fund local lending.

TABLE 8

Controlling for State Bank Bills Payable and Rediscounts

Table 8 revisits the analysis of Table 4, controlling for the county-level per capita amount of bills payable and rediscounts outstanding on the liability side of state banks' balance sheets. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	ln(OUTPUT)	ln(LOANS)	LOANS/OUTPUT
	1	2	3
STATE_BANK_BILLS/REDISCOUNTS, p.c.	−0.00217	0.00439	−0.00685
	(0.00186)	(0.00311)	(0.00883)
12/31/19 × 1916 NATIONAL_BANKS	−0.0328	0.0168	0.0847
	(0.0319)	(0.0243)	(0.0636)
12/31/20 × 1916 NATIONAL_BANKS	−0.0809**	−0.0752**	−0.2025
	(0.0321)	(0.0352)	(0.1450)
12/31/21 × 1916 NATIONAL_BANKS	−0.0578	−0.0811**	−0.6139**
	(0.0367)	(0.0364)	(0.2732)
12/31/22 × 1916 NATIONAL_BANKS	−0.0630*	−0.0826**	−0.2774*
	(0.0358)	(0.0346)	(0.1533)
12/31/19	0.784***	0.445***	−0.361**
	(0.0601)	(0.0456)	(0.1448)
12/31/20	0.257***	0.779***	1.116***
	(0.0625)	(0.0707)	(0.3170)
12/31/21	−0.241***	0.733***	2.779***
	(0.0778)	(0.0766)	(0.5729)
12/31/22	0.148**	0.735***	1.302***
	(0.0701)	(0.0730)	(0.3266)
Fixed effect	County	County	County
No. of obs.	501	501	501

The findings so far suggest a specific interpretation: The tightening of discount window policy constrained lending growth among member banks during a time of rapid debt accumulation. In the remaining sections, we characterize the mechanisms behind this finding.

E. Decomposing Price and Quantity Effects

Here, we assess the mechanism behind the temporary drop in agricultural output. As described in Section II.B, farm interests argued before Congress that higher discount rates had disrupted their access to credit and forced them to liquidate crops at lower prices. Although this argument was dismissed at the time in the comments made by Governor Harding (see the Appendix), we show that it is supported in our data. On the other hand, quantity adjustments do not appear to be important to the drop in output.

To establish these findings, in Table 9, we revisit our result on agricultural revenues from column 1 of Table 7. In column 1 of Table 9, we capture the price margin, by recalculating the output measure every year using the initial quantities measured for each county in 1916. In other words, the only source of variation in this table is the fluctuations in crop prices over time. One can think of this as an index of average crop prices, weighted by initial output quantities. The table reports that this margin of adjustment behind our earlier results is quite large, with prices showing no differential pattern in 1919 (as in the earlier placebo results), but then dropping sharply in 1920, and recovering over several years thereafter.

In column 2 of Table 9, we perform the reverse analysis, recalculating output each year using that year's quantities but 1916 price levels. Here, we see no significant impact of exposure to discount window policies, even though the main effects for several years are quite significant.

Columns 3 and 4 of Table 9 reiterate the price results in a more tangible way by focusing only on corn, the most important crop. In both levels and logs, the price of corn fell precipitously in 1920 for counties with greater policy exposure, but rebounded immediately thereafter.

To reiterate, the price effects in this table are heterogeneous patterns across counties. They do not simply measure the large secular price movements of the time, which are captured by the year effects in this table, but rather capture the effect of exposure to discount window policies, above and beyond those secular movements. Our findings suggest, as farm interests argued at the time, that this policy induced lower crop liquidation values in the short run, by disrupting farmers' ability to roll over short-term agricultural loans.

F. Long-Term Effects on Debt Burdens and Market Structure

The discount rate increases in 1920 were short-lived. The Chicago Fed lowered rates back to pre-1919 levels by the end of 1921, and the St. Louis Fed did the same in early 1922. Tables 4 and 7 reported that the negative effect of exposure to discount window policy on agricultural production in 1920 was similarly short-lived: In column 1, the regression coefficients for the interaction terms in years after 1920 are economically small and statistically insignificant. In contrast, the effect on bank loans outstanding in column 2 was more persistent. Intuitively, this pattern is consistent with the evidence from Figure 4, which showed that the rapid debt

TABLE 9

Decomposing the Output Effect into Price and Quantity Margins

In the first column of Table 9, the outcome variable is the average crop price, weighted by 1916 quantities. In the second column, it is the total output measured using 1916 prices. In the third and fourth columns, it is the price of corn alone, in logs and levels. Specifications are as in Table 7. Standard errors are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	FIX_YIELD	FIX_PRICE	CORN_PRICE	ln(CORN_PRICE)
	1	2	3	4
12/31/19 × FRAC_NATL	−0.0308	−0.1522	0.0457	0.0420
	(0.0620)	(0.1785)	(0.0720)	(0.0596)
12/31/20 × FRAC_NATL	−0.512***	−0.162	−0.227**	−0.403**
	(0.177)	(0.235)	(0.108)	(0.179)
12/31/21 × FRAC_NATL	−0.44133***	−0.10913	−0.00643	−0.04861
	(0.1444)	(0.3490)	(0.0383)	(0.0862)
12/31/22 × FRAC_NATL	−0.2655***	−0.2531	−0.0476	−0.1019**
	(0.0889)	(0.2988)	(0.0395)	(0.0504)
12/31/26 × FRAC_NATL	−0.2641**	0.1400	−0.0192	−0.0615
	(0.1143)	(0.2256)	(0.0405)	(0.0572)
12/31/27 × FRAC_NATL	−0.1018*	0.0152	0.0327	0.0331
	(0.0616)	(0.2569)	(0.0402)	(0.0486)
12/31/29 × FRAC_NATL	−0.0772	0.0399	0.0257	0.0234
	(0.0644)	(0.2523)	(0.0364)	(0.0424)
12/31/19	0.581***	0.235***	0.473***	0.441***
	(0.0262)	(0.0755)	(0.0290)	(0.0242)
12/31/20	0.250***	0.124*	−0.163***	−0.212***
	(0.0571)	(0.0753)	(0.0337)	(0.0542)
12/31/21	−0.2073***	0.0836	−0.4588***	−0.7791***
	(0.0486)	(0.1175)	(0.0134)	(0.0307)
12/31/22	−0.0298	0.2626***	−0.2301***	−0.317***
	(0.0296)	(0.0992)	(0.0139)	(0.0175)
12/31/26	0.0125	0.0983	−0.2837***	−0.4016***
	(0.0400)	(0.0783)	(0.0148)	(0.0206)
12/31/27	0.00831	−0.02154	−0.14250***	−0.17987***
	(0.0221)	(0.0915)	(0.0139)	(0.0165)
12/31/29	−0.0103	0.1623*	−0.1293***	−0.1611***
	(0.0228)	(0.0844)	(0.0133)	(0.0154)
Fixed effect	County	County	County	County
Instrument	1916 nat'l p.c.	1916 nat'l p.c.	1916 nat'l p.c.	1916 nat'l p.c.
No. of obs.	807	807	807	807

buildup during 1916–1920 was followed by a much slower process of deleveraging in the years leading up to the Great Depression.

Taking these findings together, we form a ratio of (bank) credit to (agricultural) output for Illinois counties during our sample period. This ratio serves as a useful economic indicator of the debt burden facing rural regions in Illinois in the years leading to the Great Depression. We then repeat the specifications from the first 2 columns of Tables 4 and 7 with this ratio as the dependent variable. We report the results in column 3 of each of those earlier tables.

There is no departure across counties in the loans-to-output ratio in 1919, in line with our earlier placebo results showing that exposure to discount rate changes did not predict cross-sectional differences before those discount rate changes actually happened. Even by the end of 1920, there is no statistically significant effect. At this point in time, counties with more exposure to discount window policy had experienced relative decreases in *both* output and lending. However, in 1921, the weakening effect on agricultural output, and the persistent effect on loans

FIGURE 11

Policy Exposure and Change in the Number of Farms (1920–1925)

In Figure 11, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the number of farms in that county from 1920 to 1925, as reported in the U.S. Census of Agriculture.

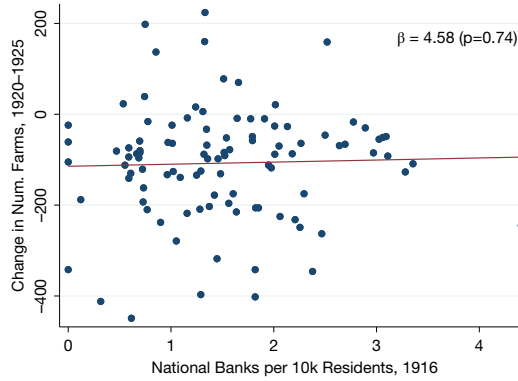
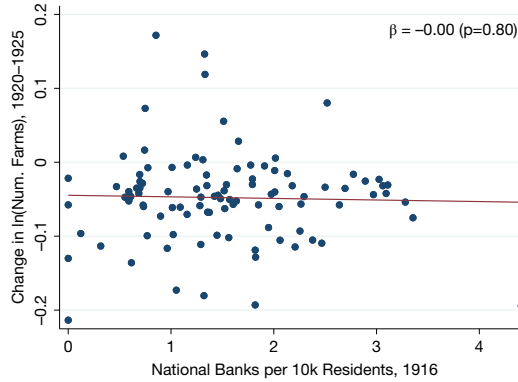


FIGURE 12

Policy Exposure and Change in the Log Number of Farms (1920–1952)

In Figure 12, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the log number of farms in that county from 1920 to 1925, as reported in the U.S. Census of Agriculture.



outstanding, combine to generate a lower loans-to-output ratio, which persisted through the beginning of the Great Depression.

Finally, we investigate other long-term effects on the agricultural sector. We use county-level data from the U.S. Census of Agriculture on the number of farms in operation and the total acreage devoted to farmland (see our description in Section III). Following our earlier approach in Figure 10, we plot the long-run change in various utilization measures against the 1916 number of national banks per capita.

Figures 11 and 12 show that the number of farms decreased significantly across Illinois from 1920 to 1925, but that there was no differential effect depending

FIGURE 13

Policy Exposure and Change in Agricultural Acreage (1920–1925)

In Figure 13, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in agricultural acreage in that county from 1920 to 1925, as reported in the U.S. Census of Agriculture.

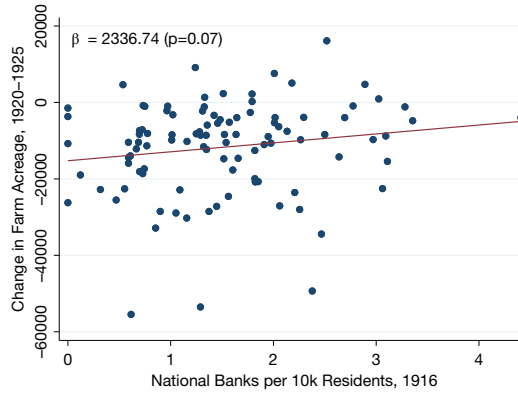
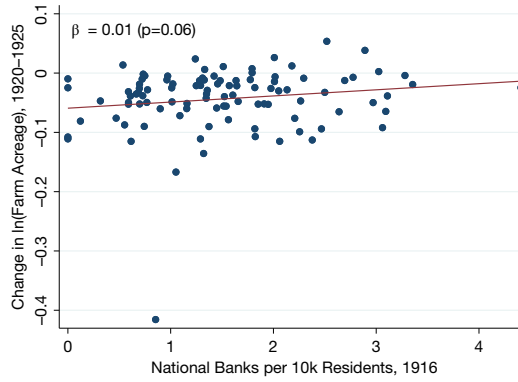


FIGURE 14

Policy Exposure and Change in Log Agricultural Acreage (1920–1925)

In Figure 14, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in log agricultural acreage in that county from 1920 to 1925, as reported in the U.S. Census of Agriculture.



on exposure to discount window policy. (That is, the fitted regression line has a negative intercept with no discernible slope.) However, Figures 13 and 14 show that counties with low exposure experienced a large drop in total land devoted to farming, whereas the counties with the highest exposure (the most national banks per capita) did not experience this drop.

The contrast between these results suggests more efficient consolidation within counties that were more exposed to policy, such that farm failures in these counties did not lead to land sitting idle afterward as often as in counties with lower

FIGURE 15

Policy Exposure and Change in Agricultural Acreage (1920–1925)

In Figure 15, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in farm acreage in that county from 1920 to 1930, as reported in the U.S. Census of Agriculture.

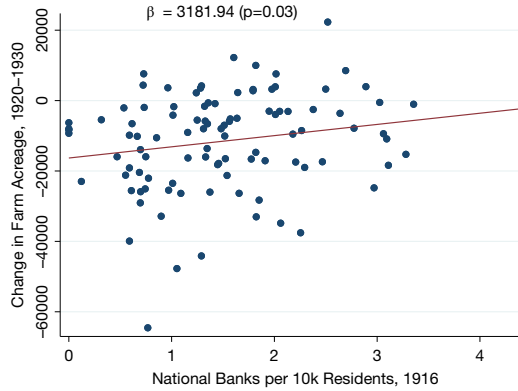
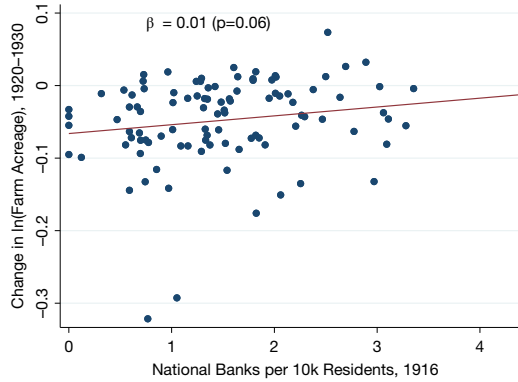


FIGURE 16

Policy Exposure and Change in Log Agricultural Acreage (1920–1925)

In Figure 16, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in log agricultural acreage in that county from 1920 to 1930, as reported in the U.S. Census of Agriculture.



exposure. The trend appears to be long-lasting: Figures 15–18 show that the association between policy exposure and farming acreage persisted, and if anything slightly increased, through 1929 and 1935. By contrast, Figures 19–22 show that this never translated into a different number of farms.

A plausible underlying mechanism for these long-run findings is the avoidance of debt overhang problems. When farms were starved of credit during the 1920–1921 episode, less-efficient owners in areas with more national banks were prevented from taking on debt. Their incentives would have remained aligned with maintaining the land at the highest possible long-run value, making it an attractive

FIGURE 17

Policy Exposure and Change in Agricultural Acreage (1920–1925)

In Figure 17, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in agricultural acreage in that county from 1920 to 1935, as reported in the U.S. Census of Agriculture.

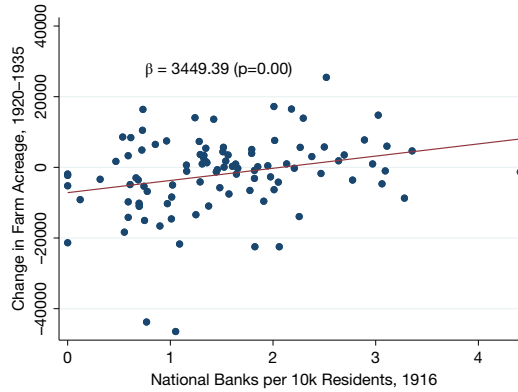
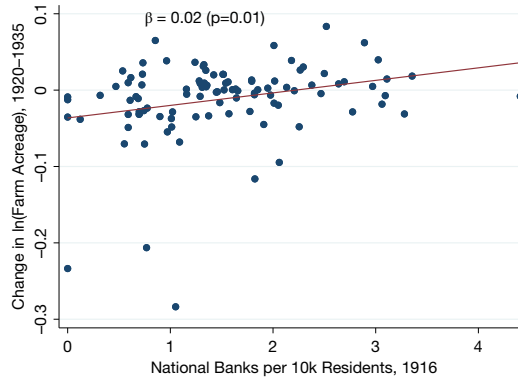


FIGURE 18

Policy Exposure and Change in Log Agricultural Acreage (1920–1925)

In Figure 18, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in log agricultural acreage in that county from 1920 to 1935, as reported in the U.S. Census of Agriculture.



target for reorganization after the business finally failed. In contrast, in counties with less exposure to the policy, underperforming farms would have been able to accumulate greater debt burdens during the times of loose credit. This debt would have weakened owners' incentives to maintain the value of the land, which in turn would make it less attractive for reorganization after the failure of the farm.

In sum, these findings represent novel evidence of a long-run mechanism by which discount window policy, through credit markets, affected the real economy in the 1920s.

FIGURE 19

Policy Exposure and Change in the Number of Farms (1920–1930)

In Figure 19, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the number of farms from 1920 to 1930, as reported in the U.S. Census of Agriculture.

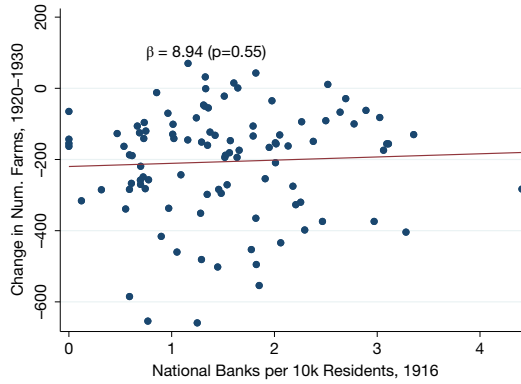
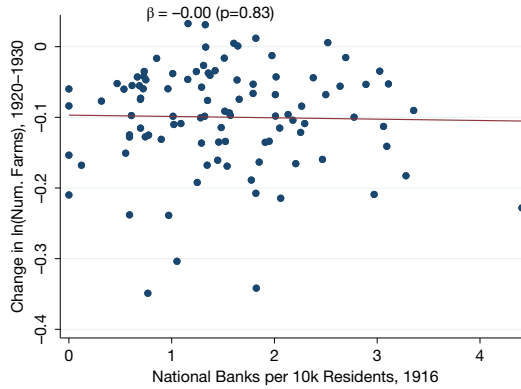


FIGURE 20

Policy Exposure and Change in Log Number of Farms (1920–1930)

In Figure 20, each dot represents a county in Illinois. The horizontal axis plots the number of national banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the log number of farms in that county from 1920 to 1930, as reported in the U.S. Census of Agriculture.



V. Discussion and Conclusions

In this article, we investigate the channels by which discount window policy during 1920–1921 affected credit, prices, and output. We focus on agriculture because of its importance to the U.S. economy at the time, and on the state of Illinois due to its central role in farming, and due to the availability of novel county-level data on bank lending and agricultural output. We use the divide between state and national banks, which pre-dated the Fed, to compare counties based on their exposure to discount rate policy in later years.

FIGURE 21

Policy Exposure and Change in the Number of Farms (1920–1930)

In Figure 21, each dot represents a county in Illinois. The horizontal axis plots the number of banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the number of farms in that county from 1920 to 1935, as reported in the U.S. Census of Agriculture.

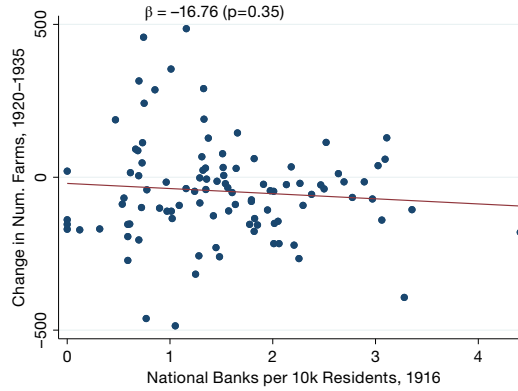
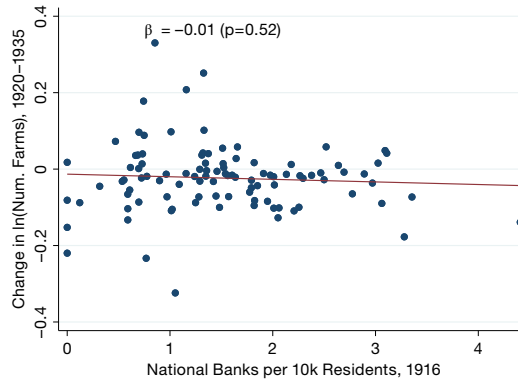


FIGURE 22

Policy Exposure and Change in the Log Number of Farms (1920–1930)

In Figure 22, each dot represents a county in Illinois. The horizontal axis plots the number of banks per 10,000 residents in that county as of 1916, and the vertical axis plots the change in the log number of farms in that county from 1920 to 1935, as reported in the U.S. Census of Agriculture.



We find that the tightening of conditions at the discount window in 1920 caused counties with greater exposure to suffer a relative drop in credit and output. The output effect is mainly explained by liquidating crops at lower prices. We further show that the subsequent lowering of discount rates restored counties' relative output levels, but led to lower debt levels among counties with high membership.

Finally, we document that counties with greater exposure to these policies exhibited a greater long-run ability to reorganize the land of failed farms. A plausible interpretation is the avoidance of debt overhang frictions that can arise

when credit is easily available: In counties with less exposure to the discount window policy, farms could accumulate more debt before finally failing, making their farms a less attractive target for later reorganization.

In sum, our results question the simple conclusion that tight policy at the discount window during 1920 was necessarily harmful. Our findings are consistent with this policy having mitigated at least some of the damaging long-run effects of a temporary credit boom.

Appendix. Telegram and Response from Governor Harding

Reproduced from Sixty-Seventh Congress ((1921), Vol. 2, pp. 372–374):

A. Form Telegram Sent by Midwest Farmers

Hon. W. P. G. Harding, Governor, Federal Reserve Board, Washington, DC:

Our country is full of corn, oats, immature pigs, and partly fatted cattle. Your board is insisting that farmers sell these products to reduce existing indebtedness. The only salable produce is corn at 40 cents and oats at 33 cents per bushel. A sale at this price will not pay the cost of raising the crop and will not liquidate any indebtedness but will simply force the crops into the hands of speculators and a very large part of the farmers into bankruptcy, and at a time when the consumer is able to pay a fair price.

This means that general bankruptcy will follow. We earnestly urge your board to declare that an emergency now exists and to arrange for credit extension to farmers so that some of the existing indebtedness can be paid out of the proceeds of the present crop. Your board outlined a policy last spring to bring down prices and in so far as prices of farm products are concerned you have certainly more than accomplished your purpose. If your board will bring pressure to make a decent market our farmers would gladly sell their products.

B. Response

Nov. 24, 1920.

Dear Sir: A telegram dated Nov. 22, signed by yourself and in behalf of 185 others, has been received and duly considered by the Federal Reserve Board.

Our information is that the price conditions of which you complain are due in part to the difficulty of maintaining a large volume of exports because of the credit situation abroad, in part to the unusually large crops which have been produced this year, and in part to the fact that the farmers, for one reason or another, carried over a larger amount than usual of last year's crops. The fall of prices has also been accelerated because of the greater economy on the part of consumers and their reluctance to pay the prices which prevailed some months ago.

The Federal Reserve Board regrets the embarrassments which are attendant upon the commerce, business, and agriculture of the country, but desires to state most positively that these conditions have not been brought about by any acts or policies of the Federal Reserve Board or the Federal Reserve system. On the other hand, the board confidently asserts that but for the precautionary measures taken several months ago conditions would have been far worse today than they are and the prospects of stabilization and recovery much more remote. The Federal Reserve Board has not insisted that farmers sacrifice their products or that they adopt any particular policy.

The Federal reserve banks do not deal directly with the public but are permitted by law to rediscount for member banks paper defined by the Federal reserve act as eligible.

There has been no reduction in the volume of credit extended by the Federal reserve banks during the past year, for the volume of paper rediscounted by Federal reserve banks and the amount of Federal reserve notes outstanding are each higher by several hundred millions of dollars than a year ago. Member banks have rediscounted with Federal reserve banks a much larger proportion of their total loans and discounts than they have ever done before, an amount about 10 times greater than normal. The Federal reserve banks in the agricultural districts have discounted for their member banks amounts so large that in many cases their own reserves would have been reduced to less than one-half of the legal minimum but for the fact that they were permitted by the Federal Reserve Board to rediscount with other Federal reserve banks. The total volume of these interbank rediscounts which have gone for the larger part to member banks in farming sections has at times been in excess of $250,000,000. This represents a sum more than 8 times as great as the maximum deposits ever made by the U.S. Treasury with national banks to aid in crop-moving operations.

The Federal Reserve Board is advised by the Federal reserve bank in your district that credit facilities are not being denied to your member banks, although many of them have under discount amounts far in excess of what would be regarded as a normal and reasonable line. The Federal reserve bank has merely insisted that as notes which have been rediscounted with it are paid the borrowing member bank apply the proceeds to the reduction of its indebtedness to the Federal reserve bank.

Neither the Federal Reserve Board nor the Federal reserve banks undertake to direct the credit policy of member banks; this rests with the directors of each such institution, and, of course, the Federal reserve system has no control whatever over the credits of nonmember banks. No doubt many banks feel that they are unduly extended and are endeavoring to secure liquidation or reduction of loans at maturity, but any direct pressure brought to bear upon the farmer to sell his products is exerted by the member and nonmember banks and not by the Federal reserve bank.

Numerous telegrams have been received during the past 2 days, couched in substantially the same words and terms as yours, from individuals and trade bodies in your section, indicating organized effort to bring pressure to bear on the Federal Reserve board. In each of these telegrams, the board is urged "to declare that an emergency now exists and to arrange for credit extension to farmers." Such a declaration by the board would be liable to cause a real emergency and would accentuate the distress instead of relieving it. An extension of loans to farmers can be granted only by the banks with which the farmers deal and to which they are indebted. The Federal Reserve Board has been a consistent advocate of orderly marketing of crops, but as has been pointed out by members of the board in public addresses, orderly marketing does not mean the tying up of bank credits and the withholding of entire crops from the market by means of additional credits from banks, but means gradual sales. The application of the proceeds of these sales to existing indebtedness would place the individual farmer, the merchant, and the local bank in stronger position and would make it possible for the local bank to do more for the farmer than it can do if crops are withheld entirely. I am sending you a copy of an address which I made several weeks ago before the chamber of commerce, at Cleveland, Ohio, and would invite your attention particularly to those paragraphs which have been marked.

In conclusion, I would say that the Federal Reserve Board recognizes the fact that agriculture is a basic and vital industry. The board has always assumed a sympathetic attitude toward all matters relating to agriculture, and it has done, is doing, and will continue to do all it can in a legitimate and proper way to assist the farmer in his credit problems. It will not, however, be forced into the adoption of policies which in its judgment would be unwise and ruinous, and which would eventually involve the farmer, as well as others, in disaster.

Very truly, yours,

W. P. G. Harding. Governor.

References

Alston, L. "Farm Foreclosures in the United States During the Interwar Period." *Journal of Economic History*, 43 (1983), 885–903.

Anderson, H.; C. Calomiris; M. Jaremski; and G. Richardson. "Liquidity Risk, Bank Networks, and the Value of Joining the Federal Reserve System." *Journal of Money, Credit, and Banking*, 50 (2018), 173–201.

Anderson, H.; S. Erol; and G. Ordoñez. "Interbank Networks in the Shadows of the Federal Reserve Act." NBER Working Paper No. 27721 (2022).

Anderson, H.; M. Paddrik; and J. J. Wang. "Bank Networks and Systemic Risk: Evidence from the National Banking Acts." *American Economic Review*, 109 (2019), 3125–3161.

Board of Governors of the Federal Reserve System. "Banking and Monetary Statistics, 1914–1941" (1943).

Brunnermeier, M. "Deciphering the Liquidity and Credit Crunch 2007–2008." *Journal of Economic Perspectives*, 23 (2009), 77–100.

Callaway, B.; A. Goodman-Bacon; and P. H. Sant'Anna. "Difference-in-Differences with a Continuous Treatment." University of Georgia Kandrac: Federal Reserve Board of Governors (2021).

Calomiris, C., and J. R. Mason. "Fundamentals, Panics, and Bank Distress During the Depression." *American Economic Review*, 93 (2003), 1615–1647.

Carlson, M., and D. C. Wheelock. "Did the Founding of the Federal Reserve Affect the Vulnerability of the Interbank System to Contagion Risk?" BIS Working Paper No. 598 (2016).

Frederico, G. "Not Guilty? Agriculture in the 1920s and the Great Depression." *Journal of Economic History*, 65 (2005), 949–976.

Friedman, M., and A. Schwartz. *A Monetary History of the United States*. Princeton, NJ: Princeton University Press (1963).

Gorton, G., and A. Metrick. "The Federal Reserve and Panic Prevention: The Roles of Financial Regulation and the Lender of Last Resort." *Journal of Economic Perspectives*, 27 (2013), 45–64.

Hausman, J. K.; P. W. Rhode; and J. F. Wieland. "Recovery from the Great Depression: The Farm Channel in Spring 1933." *American Economic Review*, 109 (2019), 427–472.

He, Z., and W. Xiong. "Rollover Risk and Credit Risk." *Journal of Finance*, 67 (2012), 391–429.

Jalil, A. J. "Monetary Intervention Really Did Mitigate Banking Panics During the Great Depression: Evidence Along the Atlanta Federal Reserve District Border." *Journal of Economic History*, 74 (2014), 259–273.

Jayaratne, J., and P. Strahan. "The Finance-Growth Nexus: Evidence from Bank Branch Deregulation." *Quarterly Journal of Economics*, 111 (1996), 639–670.

Kandrac, J. "Can the Federal Reserve Effectively Target Main Street? Evidence from the 1970s Recession." Working Paper (2021).

Krishnamurthy, A. "How Debt Markets Have Malfunctioned in the Crisis." *Journal of Economic Perspectives*, 24 (2010), 3–28.

Link, A. S. "The Federal Reserve Policy and the Agricultural Depression of 1920–1921." *Agricultural History*, 20 (1946), 166–175.

Mason, J. R. "American Banks During the Great Depression: A New Research Agenda." *Federal Reserve Bank of St. Louis Review*, 80 (1998), 151–152.

Meltzer, A. H. *A History of the Federal Reserve*, Vol. 1. Chicago, IL: University of Chicago Press (2003).

Park, H., and P. Van Horn. "Did the Reserve Requirement Increases of 1936–37 Reduce Bank Lending? Evidence from a Quasi-Experiment." *Journal of Money, Credit, and Banking*, 47 (2015), 791–818.

Petersen, M., and R. Rajan. "The Effect of Credit Market Competition on Lending Relationships." *Quarterly Journal of Economics*, 110 (1995), 407–443.

Richardson, G., and W. Troost. "Monetary Intervention Mitigated Banking Panics During the Great Depression: Quasi-Experimental Evidence from a Federal Reserve District Border, 1929–1933." *Journal of Political Economy*, 117 (2009), 1031–1073.
Rieder, K. "Financial Stability Policies and Bank Lending: Quasi-Experimental Evidence from Federal Reserve Interventions in 1920–1921." CEPR Discussion Paper No. DP16490 (2020).
Sixty-Seventh Congress. "Hearings Before the Joint Commission of Agricultural Inquiry" (1921).
United States House of Representatives. "Report of the Joint Commission of Agricultural Inquiry, Part II: Credit" (1921).
White, E. N. *The Regulation and Reform of American Banking, 1900–1929*. Princeton, NJ: Princeton University Press (1983).
White, E. N. *The Regulation and Reform of American Banking, 1900–1929*. Princeton, NJ: Princeton University Press (2014).
White, H. "A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity." *Econometrica*, 48 (1980), 817–838.