Financial Stability Policies and Bank Lending: Quasi-experimental Evidence from Federal Reserve Interventions in 1920-1921

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Abstract

How can policy-makers successfully tame excessive credit growth? I exploit a single natural experiment to estimate the comparative causal effects of different financial stability policies on bank-level credit. In 1920, four Federal Reserve Banks hiked their interest rate indiscriminately to safeguard financial stability. Another four Reserve Banks employed targeted rate action aimed at over-leveraged banks instead. For identification, I draw on border regression discontinuities with the remaining Federal Reserve districts which did not change their stance. The uniform rate hike had weak and partly counterproductive effects, whereas targeted policy caused credit to contract significantly. (*JEL* E52, E58, N12, N22)

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Credit booms "gone bust" tend to end in financial crises which inflict large costs on creditors, tax payers and the real economy at large (Cerra and Saxena, 2008; Schularick and Taylor, 2012; Jordà et al., 2013; Romer and Romer, 2017). These pecuniary and aggregate demand externalities of unconstrained credit growth provide a clear rationale for financial stability policy (Stein, 2012; Farhi and Werning, 2016; Martinez-Miera and Repullo, 2019; Caballero and Simsek, 2020). Yet, the question which precise measures are most effective in reining in financial excesses remains subject to an ongoing debate (Gambacorta and Signoretti, 2014; International Monetary Fund, 2015; Svensson, 2017; Gourio et al., 2018; Schularick et al., 2021; Stein, 2021). Should central banks "lean against the wind" (LAW)[1] using their conventional interest rate or are more targeted policies[2] better suited to tame bank lending?

Endogeneity concerns, regulatory arbitrage, and the fact that different tools are rarely employed simultaneously have so far thwarted empirical work on the relative effectiveness of financial stability policies. The present paper addresses this gap in the literature. I exploit a single natural experiment to estimate the comparative causal effects of leaning against the wind and targeted monetary policy on bank-level credit. To identify the causal impact of the policies, I draw on geographic policy discontinuities across U.S. Federal Reserve district borders, at a time when each of the twelve Federal Reserve Banks still had the power to conduct independent monetary policies. In 1920, four Federal Reserve Banks (Boston, Chicago, Minneapolis and New York) leant against the wind by hiking their interest rate indiscriminately for all member banks from 6% to 7% to address financial stability concerns. Four other Reserve Banks (Atlanta, Dallas, Kansas City and St Louis) implemented targeted rate action to safeguard financial stability. Expressly passed for this purpose, the U.S. Phelan Act of 1920 enabled Federal Reserve Banks "to require member banks habitually borrowing *in excess of their legitimate requirements* to pay higher discount rates for their excess borrowings" (Logan, 1922, p.121, emphasis added). While leaving the baseline policy rate unchanged at 6%, targeted monetary policy allowed Federal Reserve Banks to exercise price discrimination by adjusting their interest rate only when lending to commercial banks they regarded as over-leveraged.[3] Both financial stability policies were implemented in late spring 1920 and remained in place until the summer of 1921. The remaining four districts (Cleveland, Philadelphia, Richmond and San Francisco) did not change their policy stance and simply maintained the prevailing 6% rate.

My paper's identification strategy builds on a unique institutional setting. First, although the different policy choices were endogenous to aggregate financial developments in the twelve Federal Reserve districts, my discontinuity design compares treated and control group banks in close bandwidths of 25 kilometers around borders of districts with different policies.[4] Within these bands, bank-level characteristics and local economic conditions exhibit statistically identi-

[1] When "leaning against the wind", central banks raise their conventional monetary policy instrument, the nominal interest rate, to steer against financial market developments deemed unsound.

[2] Targeted monetary policy involves tailored adjustments of the conventional interest rate tool to exercise price discrimination, e.g. against counterparties which are considered over-leveraged. Similarly, so called "macroprudential policies" (e.g. loan-to-value ratios, reserve requirements and countercyclical buffers) are specifically designed to address the build-up of systemic risks in some sub-sectors of the financial system.

[3] The available evidence indicates that the average rate paid by treated banks in districts with targeted rate action amounted to 6.76% (Joint Commission of Agricultural Inquiry, 1922a, p.62).

[4] I also provide results for the full sample, and 200km, 100km, 75km and 50km bandwidths.

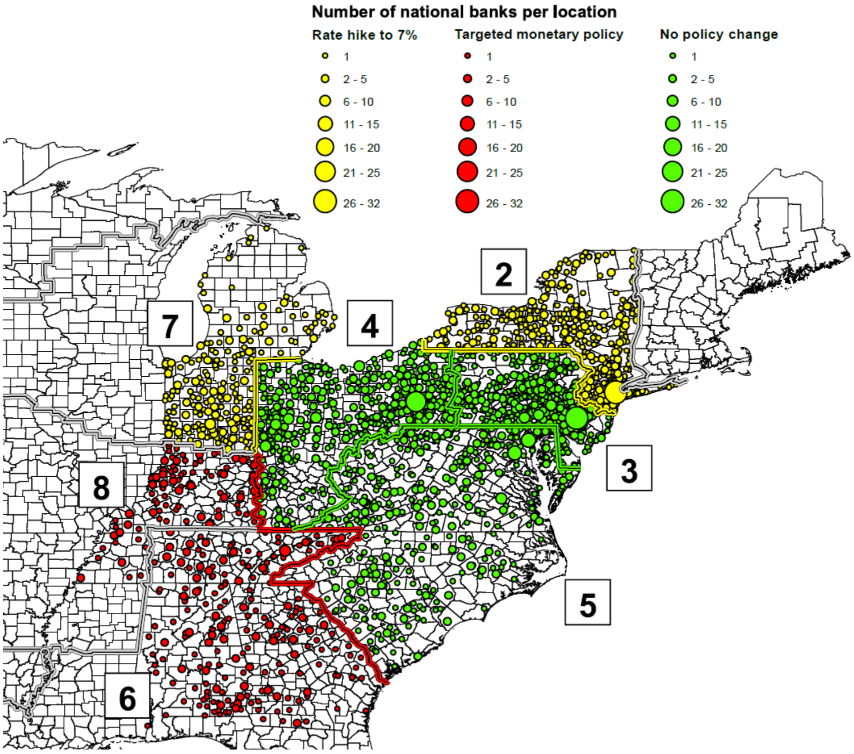
cal pre-treatment levels and pre-trends. The homogeneity in baseline characteristics minimizes the risk of omitted variable bias and allows me to disentangle supply-side from demand-side drivers of bank lending. Second, banking laws established a uniform regulatory framework for national banks across the entire territory of the United States (Mitchener, 2005). Hence, my setting rules out spurious correlation concerns related to legal discontinuities in bank regulation and supervision. Third, the U.S. banking system in the 1920s was characterized by a combination of *de jure* and *de facto* financial segmentation. National banks did generally not have the right to establish branches (Carlson and Mitchener, 2006, 2009). As "unit banks", they operated predominantly within strict geographic confines (Jaremski and Wheelock, 2020a). The Law also forbade national banks to borrow from Federal Reserve Banks (and their branches) outside their district. Moreover, I show that national banks did not sort across borders in anticipation or in reaction to policy differences. Finally, the borders of the twelve Federal Reserve districts were explicitly designed to ringfence large parts of the existing interbank links between bank locations (Jaremski and Wheelock, 2017). The prevailing financial segmentation thus significantly limited the scope for regulatory arbitrage which complicates the identification of causal effects in modern settings.

I exploit almost 13,000 bank-level balance sheets for the period between September 1919 and September 1921, newly hand-collected from the annual reports of the Office of the Comptroller of the Currency (1920, 1921a,b, 1922), Rand McNally bankers directory (1920, 1921a,b) and individual national bank examiner reports located at the U.S. National Archives at College Park, Maryland. My bank-level panel data covers large parts of the East Coast of the United States (Federal Reserve districts 2 to 8, see Figure 1) which was home to borders for all relevant policy combinations (including Placebo borders with identical policies). Controlling for time and bank fixed effects, I find that targeted monetary policy based on the Phelan Act of 1920 caused both lending and leverage to fall relative to districts without a policy change. Treatment led to a statistically significant reduction in both outcome variables by between 11% and 14%. In contrast, the impact of the uniform interest rate hike was less clear-cut. In the West (district 7, Chicago), the policy marginally eased credit pressures by around -1%. In the second district (New York), LAW triggered a statistically significant perverse effect on bank-level outcomes: leaning against the wind increased both lending and leverage by between 8% to 9% relative to control group banks.

These results are robust to a wide range of falsification checks, including changes in the estimation strategy, different specifications and methods of computing standard errors, and the incorporation of control variables. I also conduct a series of Placebo tests. First, I verify that treatment effects do not exist before treatment began and do not persist after treatment ended. Second, I show that there are no systematic discontinuities across district borders with identical policies. Third, building on Richardson and Troost (2009), I limit my sample to bank-level data from states which were split by Federal Reserve district borders to show that my estimates are not driven by other (economic) policy discontinuities across state borders unrelated to LAW or targeted monetary policy. Finally, the split border specification enables me to implement a Placebo test drawing on state-chartered non-member banks.[5] Since these banks could not

[5]All national banks became member banks of the Federal Reserve System when the System was

FIG. 1
Locations of national banks included in sample (color-coded for different policies)



Source: Rand McNally bankers directory (1920); OpenCage Geocoder
This map shows all national bank locations (incl. the number of banks in each location) featuring in my sample.

directly borrow from the Federal Reserve System, they should have not been affected by the policies to the same degree.[6] I find that the policies had no statistically significant treatment effects on non-member banks.

Turning to mechanisms, the relative strengths of the bank-lending and risk-shifting channels of U.S. monetary policy at the time explain the differential effectiveness of the two interventions. In 1920, the primary motive for borrowing from a Federal Reserve Bank was to make good on reserve requirements (Carlson and Duygan-Bump, 2021)[7]: member banks had to hold reserves against their deposit liabilities and all reserves needed to be stored with the Reserve

founded in 1914. State-chartered banks could opt in on a voluntary basis.

[6]Anderson et al. (2018) show that state-chartered banks partly circumvented this restriction by borrowing via their correspondent national banks. Moreover, between summer 1921 and June 1923, the Federal Reserve System temporarily enabled member banks to act as "agents of non-member banks in rediscounting paper with Federal Reserve Banks" (Federal Reserve Board, 1924, p.50). Most of my sampling period (September 1919 to September 1921) precedes this exemption and the LAW/PDR policies in my sample were discontinued before the summer of 1921 (see Appendix A.2). Hence, state-chartered banks' access to discount window finance likely still remained curtailed relative to member banks during the period relevant to this study.

[7]The Federal Reserve Banks operated so called "standing facilities" which relied on banks to initiate the interaction with the central bank. Borrowing from the Federal Reserve System could take two different forms. First, it could mean the *rediscount* of commercial paper. Second, borrowing could take the form of collateralized loans (*advances*, also called *bills payable*). Before the mid-1920s, Federal Reserve Banks did not engage in open-market operations to make their policy rates effective (Bordo and Sinha, 2016).

Banks. When a commercial bank granted a new loan, it usually created a deposit for the customer. This increase in deposits meant a higher absolute reserve requirement and triggered borrowing from the Federal Reserve Bank.[8] Both LAW and targeted monetary policy increased the marginal cost of reserves and thus acted upon banks' willingness and ability to grant new loans. Hence, the reserve mechanism constituted a proto-version of the modern bank-lending channel of monetary policy (Bernanke, 2007). In LAW districts, the rate hike translated into a 100 basis point flat increase in the marginal cost of reserves, irrespective of the amount a member bank needed to borrow. In contrast to LAW, targeted monetary policy – officially named the "progressive discount rate" (PDR) scheme – involved price discrimination. It turned the cost of new borrowing from the Federal Reserve Bank into a function of a bank's current level of outstanding borrowings from the Reserve Bank relative to a maximum credit line. This maximum line was calculated individually for each bank on the basis of its reserves and capital position. The more a given bank was already borrowing, the higher the interest rate it was charged for additional loans from its Reserve Bank became. The rate increased by 50 basis points for every 25% a member bank borrowed in excess of its maximum credit line. Thus, the design of targeted monetary policy amplified the contemporary bank-lending channel of monetary policy by exerting stronger and more fine-tuned pressure on over-leveraged counterparties than LAW.

Why did LAW trigger perverse treatment effects in the second district? I find that the rate hike had counterproductive consequences in the New York district because risk-shifting motives led banks to resist deleveraging. Exhibiting higher *ex ante* reliance on deposit funding than financial intermediaries in the LAW district of Chicago, member banks located in district 2 experienced relatively heavier funding cost shocks following the uniform rate increase. In addition, the prevailing state usury rates on local loans in the second district were substantially lower and more binding than in the West, obviating a direct interest rate pass-through to bank loans (Ryan, 1924). The interest rate hike therefore depressed banks' charter values and net worth. I show that diminishing "skin in the game" and the ability to raise additional funds through tighter interbank connections to New York City pushed treated banks in district 2 to take on more risk and to desist from deleveraging. These risk-shifting motives did not prevail in PDR districts. Since the rate action in PDR districts was specifically targeted at over-extended banks, the contractionary effect of the contemporary bank-lending channel was largest for precisely those firms which had the highest *ex ante* incentive to gamble for survival. Moreover, PDR banks suffered heavy deposit withdrawals during the agricultural crisis of 1920-21 – while targeted rate action was in force –, because they operated in farming-intensive regions (Rajan and Ramcharan, 2015; Jaremski and Wheelock, 2020a). These withdrawals interacted with the PDR scheme to reinforce deleveraging incentives and they mitigated risk-shifting behavior by augmenting shareholders' relative stake in the firm.

This study contributes to several literatures. First, I contribute to the ongoing debate on the choice of optimal financial stability policies (Stein, 2013; Gambacorta and Signoretti, 2014; International Monetary Fund, 2015; Gourio et al., 2018; Svensson, 2017; Martinez-Miera and

[8]The final report of the Joint Commission of Agricultural Inquiry (1922a, p.17–18; p.519) provides a detailed description of this process. Member banks with deficient reserve accounts incurred a penalty charge on the missing amount of 200 basis points above the prevailing interest rate.

Repullo, 2019). Existing theoretical studies reach opposing conclusions on the relative merits of different financial stability policies. Whereas LAW famously "gets into all cracks" of both regulated and shadow financial sectors, targeted tools are less likely to cause collateral damage.[9] In contrast, targeted policies are more prone to regulatory arbitrage and more difficult to deploy than LAW.[10] This theoretical ambiguity calls for empirical testing. While the recent empirical literature provides insights regarding the isolated impact of specific financial stability policies (Jiménez et al., 2012; Aiyar et al., 2014; Barroso et al., 2017; Camors et al., 2017; Jiménez et al., 2017; Reinhardt and Sowerbutts, 2017; Alam et al., 2019; Forbes, 2019; Araujo et al., 2020; Bergant et al., 2020; Schularick et al., 2021), evidence on the direct comparative effects of different tools remains scarce. Fixing time and environment, my paper stages a true empirical "horse race" between different types of financial stability policies with a comparable level of average treatment intensity. Running a similar test is difficult with modern data because most policy-makers consider LAW and its alternatives as substitutes rather than complements.[11]

Second, my findings complement recent contributions on risk-shifting and the so called "leverage ratchet effect". My paper suggests that financial stability policies can have severe counterproductive consequences if endogenous risk-shifting motives induce banks to resist deleveraging. According to standard theories of the bank-lending channel, contractionary monetary policy reduces financial intermediaries' loan supply by increasing funding costs via higher external finance premia (Bernanke, 2007; Martinez-Miera and Repullo, 2019).[12] At the same time, tighter monetary policy is also known to induce risk-shifting behavior in settings characterized by limited shareholder liability and asymmetric information between bank owners and creditors (Dell'Ariccia et al., 2014, 2017). Since higher funding costs depress net worth, contractionary monetary policy reduces bank owners' "skin in the game". Lower charter values in turn provide an incentive to take on more risk, and, in case the contraction severely affects banks' profitability, to gamble for survival by resisting deleveraging pressure (Turner, 2014; International Monetary Fund, 2015). Negative profitability shocks emanating from a monetary contraction can thus amplify a more general "leverage ratchet effect" (Admati et al., 2018). This effect typically besets firms with many small creditors who experience free-riding problems in organizing and enforcing covenant limits on the issuance of new debt. Banks are a case in point. All else equal, the net impact of contractionary LAW policy should therefore depend on the relative strength of the bank-lending and risk-shifting channels.[13]

Third, my paper provides new insights regarding the design of effective financial stability

[9] Monetary policy tightenings have costs in terms of higher inflation volatility, foregone output and employment (Korinek and Simsek, 2016). LAW may also cause the economy to face future negative shocks in a more fragile state (Svensson, 2017).

[10] Targeted tools are more difficult to adjust and deploy than conventional monetary policy because they often require legal changes and direct political voting/backing (Stein, 2013; Smets, 2014).

[11] Recent theoretical advances show, however, that it is possible to design optimal policy mixes (Farhi and Werning, 2016; Collard et al., 2017).

[12] The standard bank lending channel may be amplified by a separate deposits channel (Drechsler et al., 2017) and the risk premium channel of monetary policy (Drechsler et al., 2018).

[13] The mechanisms underlying the unexpected effects of contractionary monetary policy in my setting differ from those reported in other recent contributions. To explain the shadow-credit driven U.S. housing boom in the 2000s, Drechsler et al. (2021) document a contractionary deposits channel of monetary policy based on Drechsler et al. (2017), which was subsequently offset by non-bank originators of mortgage credit.

policies. As a variant of targeted monetary policy, the PDR scheme of 1920-21 occupied middle ground between LAW and modern macroprudential tools. Targeted monetary policy combines an existing conventional instrument (the interest rate) with the possibility to gear tightenings towards selected parts of the financial sector. This feature can make targeted monetary policy easier to deploy than standard macroprudential tools. At the same time, targeted monetary policy is also less likely to cause collateral damage than LAW. My findings reveal that PDR-induced price discrimination was highly effective in reducing bank-level leverage and credit growth, whereas LAW was not. Thus, the results of this paper suggest that policy-makers may gain from initiating a conversation on the benefits and costs of rules-based price discrimination in the context of their financial stability mandates.[14] This conclusion may particularly apply to emerging market economies, where reserve requirements remain an important lever of monetary policy (Cordella et al., 2014). More generally, my findings resonate with financial stability policies designed to regulate banks' incentive to engage in money creation (Stein, 2012).

Fourth, this paper adds several new insights to a still understudied episode in U.S. economic history. In terms of deflationary pressures and the ensuing contraction in national output, the crisis of 1920-21 was one of the sharpest recessions in the history of the United States (Romer, 1988; Meltzer, 2003). An extensive literature investigates the medium to longer-run effects of this downturn (Alston, 1983; Wheelock, 1992; Alston et al., 1994; White, 2014; Rajan and Ramcharan, 2015; Jaremski and Wheelock, 2020a). Yet, an important unsettled debate is whether contractionary Federal Reserve policy amplified the recession starting in early 1920 (Benner, 1925; Link, 1946; Friedman and Schwartz, 1963; Wicker, 1966; Kuehn, 2012; Shaw, 2016). My paper provides plausibly causal estimates that are consistent with a short-run amplification of the recession in 1920-21 due to the PDR policy. Whereas Tallman and White (2020) take a macroeconomic perspective focusing on aggregate credit developments within Federal Reserve districts in 1920-21, I provide a micro-data based econometric analysis of the causal effects of financial stability policies on bank credit. Recent complementary work by Carlin and Mann (2021) draws on county-level data from Illinois to explore the short-run and medium-run real effects of the Federal Reserve System's interest rate policy. In contrast to Carlin and Mann (2021), I exploit bank-level data from districts 2 to 8 to dis-aggregate the Federal Reserve System's policy stance at the time. Building on Goldenweiser (1925) and Wallace (1956), my paper emphasizes that the various Federal Reserve Banks implemented different policies with quite heterogeneous effects on bank credit. I explain the rationale underlying the different policy choices, I provide detailed evidence on their transmission mechanisms and I show that (identically sized) interest rate increases led to very different outcomes depending on the district one examines. Thus, my findings shed light on the impact of the Federal Reserve System's early experiments with sophisticated financial stability policies, in line with the System's pre-occupation with the quality and quantity of bank credit at the time (Rotemberg, 2013).

Moreover, my paper extends the methodology of seminal papers by Richardson and Troost

[14] Rules-based price discrimination was already part of the day-to-day business in nineteenth century central banking practice (Wood, 1939; Anson et al., 2017). New Zealand, Japan and the Eurozone have recently implemented targeted lending programmes and interest rate tierings to enhance the transmission of monetary policy and to limit the negative side effects of negative interest rates.

(2009) and Jalil (2014) who exploit Federal Reserve border discontinuities to show that liquidity provision by the Federal Reserve System mitigated banking panics during the Great Depression of the 1930s. My study differs from theirs along several dimensions. I study the effects of explicit monetary policy decisions rather than implicit differences in the willingness of Federal Reserve Banks to provide emergency liquidity. Furthermore, I analyze an earlier episode at the beginning of the 1920s when the Federal Reserve System was still in its infancy, the stigma on discount window borrowing was limited at best, and the economic environment was initially characterized by a strong boom rather than a severe recession (Gorton and Metrick, 2013; Anbil, 2018).[15] I also exploit several unexplored border discontinuities on the East coast of the United States which hosted both large financial centers and a much higher number of banks than the southern districts studied in previous contributions. Finally, I provide quantitative evidence backing a non-interference assumption which is crucial for the identification of unbiased effects of Federal Reserve policies using historical border discontinuities. For unbiased causal estimates, cross-district interbank connections of treated banks must not violate the stable unit treatment value assumption (SUTVA) of local discontinuity models. I hand-collected the universe of interbank correspondent links for the banks in my sample (>35,000 links) from the Rand McNally bankers directory (1920). Consistent with the literature on the pyramid structure of the U.S. interbank network at the time (Mitchener and Richardson, 2013b; Anderson et al., 2018; Mitchener and Richardson, 2019; Jaremski and Wheelock, 2017, 2020b; Anderson et al., 2019), I confirm that links to local banks across the nearest Federal Reserve district border were practically non-existent in my sample.[16]

The paper is organized as follows. Section I. describes my primary sources and presents the new data sets compiled for this paper. Section II. discusses experiment validity based on the historical background of this study and explains my identification strategy in detail. Section III. provides the empirical results and robustness checks. Section IV. investigates the channels of policy transmission. Section V. concludes. A detailed online appendix complements the paper.[17]

I. Data

This paper is based on several hand-collected and newly digitized historical data sets. First, I compiled a bank-level panel data set containing balance sheet information for all national banks located in the following 17 states: Alabama, Delaware, District of Columbia, Georgia, Indiana, Kentucky, Maryland, Michigan, New Jersey, New York, North Carolina, Ohio, Pennsylvania, South Carolina, Tennessee, Virginia and West Virginia. The bank-level panel data set contains

[15] The literature on multiplier effects suggest that differences in the underlying setting influence the size of treatment effects. For a recent example, c.f. Hausman (2016).

[16] Correspondent links to major financial centers may have helped treated banks to circumvent the policies. Yet, this arbitraging behavior could not have artificially blown up the total lending portfolio of local control group banks in my sample. It only affected control group banks in financial centers further away from the border line. Moreover, my bank fixed effects specifications directly control for the number and nature of banks' correspondent links, because interbank connections were sticky at the time.

[17] The online appendix can be downloaded here. The online appendix is also attached to this submission as a separate document.

3,334 individual banks which are observed at four points in time, yielding a total of 12,996 observations.[18] I track national banks on four call dates: 12 September 1919, 31 January 1920, 8 September 1920 and 6 September 1921. I rely on two sources to collect the balance sheet data. For the September call dates, I used the annual reports of the Office of the Comptroller of the Currency (1920, 1921a,b, 1922). For the January 1920 call date, I drew on Rand McNally bankers directory (1920).[19] The four call dates are partly dictated by data availability. The Comptroller reports were published only once a year with individual bank-level data recorded in September, while the bankers directory was published bi-annually (in January and July). I also sampled call dates specifically to satisfy the data requirements of my research design. The January 1920 data contain the last available balance sheet information before LAW and targeted monetary policy were first implemented in late spring 1920. The September 1919 and January 1920 call dates enable me to analyze pre-trends.

I concentrate on banks located in the 17 states on the U.S. East Coast for several reasons. First, this region is home to all policy border discontinuities relevant for this study. The Federal Reserve district borders between the districts of New York and Philadelphia as well as Cleveland, but also the border line between the Cleveland district and the Chicago district, capture policy discontinuities between LAW districts and Federal Reserve Banks which did not change policy stance (see Figure 1 above). In contrast, the district borders in the South separate Federal Reserve districts which implemented the PDR (Atlanta and St Louis) and Federal Reserve districts which kept their policy stance unchanged (Richmond and Cleveland). Furthermore, I exploit a third set of borders in my robustness checks. I draw on the borders between the Cleveland, Philadelphia and Richmond districts for Placebo tests. None of these three districts implemented policy changes in late spring 1920.

The second reason for concentrating on the 17 states mentioned above is that only very few national banks were located close to the district borders in the Western part of the United States (Jaremski and Wheelock, 2017, c.f. their Figure 1 on p.24). The border line between the San Francisco district on the one hand and the Dallas, Kansas City and Minneapolis districts on the other hand is mostly located in the Rocky Mountains. The inclusion of banks in locations far away from the border line would likely violate crucial identification assumptions of my local discontinuity design (see next section). The third reason for limiting my sample to the 17 states listed above – as opposed to including banks located in additional states on the East coast as,

[18] My sample is not fully balanced because some banks fail or are founded after September 1919.

[19] Both sources are freely accessible on-line (FRASER, Office of the Comptroller of the Currency reports and HathiTrust, Rand McNally bankers directory; last accessed 22 August 2021). The annual reports list six asset side positions (loans and discounts; government securities; other bonds and investments; lawful reserve; cash and exchanges; other assets) and six liabilities side positions (paid-up equity; surplus and undivided profits; circulation; demand deposits; time deposits; due to banks and other liabilities) for each national bank. The reports also indicate the sum of total assets. Rand McNally bankers directory (1920) provides information on at least five positions for each bank (paid-up equity; surplus and undivided profits; deposits including due from banks; loans, discounts, bonds and securities; cash, exchanges and due from banks). More disaggregated data are available for banks located in central reserve cities, Federal Reserve branch cities and other large financial centers. To compare bank-level variables over time, I merge positions from the Comptroller reports to match them to the positions listed in Rand McNally bankers directory (1920). For example, to mirror the aggregated loan and investment portfolio in the bankers directory, I sum up the following positions from the Comptroller reports: loans and discounts; government securities; other bonds and investments.

for example, Massachusetts or Florida – is that I focus on states which have at least one bank domiciled at a distance smaller than 200 kilometers from the relevant Federal Reserve district border. Using geographic information system (GIS) software, I geo-located all national banks in my sample to obtain their airline distance (in kilometers) to relevant Federal Reserve district borders whose geographic location I also geocoded.

Kentucky and New Jersey represent two states of particular interest in my sample because their territories were split between two Federal Reserve districts with different policies. The Western part of Kentucky is located in district 8 (St Louis, a PDR district), whereas the state's Eastern half forms part of district 4 (Cleveland, a no policy district). New Jersey in turn is divided into a Northern part located in the New York district which lent against the wind in spring 1920, and a Southern part belonging to district 3 (Philadelphia, again a non-policy district). In my robustness checks, I apply the local discontinuity framework to split-state banks to show that my estimated treatment effects are not spuriously driven by differences in other state-level economic policies and regulations. I compiled bank-level data for the whole population of commercial banks (i.e. state-chartered banks and national banks) in Kentucky and New Jersey, including information on whether a given state-chartered bank was a member of the Federal Reserve System. In addition to the four call dates listed above, I collected balance sheet data on split-state national banks for 31 January 1921 and 31 July 1921 (both from the Rand McNally bankers directory (1921a,b)). For state-chartered banks, I gathered balance sheets for the call dates in January 1920 and January 1921. These additional data enable me to conduct Placebo tests investigating whether treatment effects for member banks persisted after treatment had ended and to what extent non-member banks were affected by the policies.[20] The split-state samples contain data for about 700 individual state-chartered banks, which I collected on top of the data for the 3,334 national banks mentioned above.

Apart from my balance sheet panel data, I also compiled two new complementary bank-level data sets. The first of these data sets contains all interbank connections (so called "correspondent links") for the national banks in my sample, as published by the Rand McNally bankers directory (1920) in January 1920. I collected the names of more than 35,000 banks which served as correspondents for the national banks in my sample. I also geo-coded the correspondents' geographic location in the United States. Hence, for each national bank in my sample, I am able to differentiate between correspondents that were domiciled in a Federal Reserve district subject to LAW or to the PDR and those which belonged to one of the districts without policy change. I draw on these interbank network data to check for local continuity in banking connectedness and to assess whether my econometric results are likely to suffer from SUTVA violations due to arbitrage via interbank networks.

Second, I draw on individual national bank examiner reports available at the U.S. National Archives at College Park, Maryland, to assemble bank-level interest rates for all national banks located in Indiana, Kentucky and New Jersey. I concentrate on reports of examinations which took place in 1920. Although the pacing and the frequency of examinations differ from bank to bank, many national banks were examined at least twice in 1920 – once before and once after the introduction of financial stability policies. I exploit these micro data sets to trace the

[20] Appendix A.3 contains maps plotting the split-state data.

transmission channels explaining the size and sign of treatment effects found in this study.

In addition to the systematic collection of new data, I employ other descriptive information from various issues of the Federal Reserve Bulletin (Federal Reserve Board, 1920a)[21], the National Bureau of Economic Research (NBER) Macrohistory Database[22] and the U.S. Agricultural Census (1910 and 1920) as provided by Haines et al. (2016). I also draw on a large range of qualitative sources such as annual reports, board meeting minutes and mimeos drafted by staff of the Federal Reserve Board (1920b,c,d,e,f,g,h,i, 1921, 1922).[23] The transcripts of hearings and the final report of the Joint Commission of Agricultural Inquiry (1922a) proved equally valuable.[24] My discussion of experiment validity in the next section and in Appendix A.2 was informed by the Governors' conference proceedings in 1920 later published by the Federal Reserve Board (1923). Several other archival sources, such as speeches and testimonials before U.S. Congress, are duly referenced throughout the paper.

II. Experiment Validity and Identification Strategy

The specific historical context of the early 1920s in the United States constitutes a quasi-experimental setting which allows me to estimate the comparative causal effects of LAW and targeted monetary policy. My research strategy exploits four unique features of this setting: regional variation in the policy response of Federal Reserve Banks to the post-World War I boom, local continuity of baseline covariates around district borders including the absence of pre-trends in key (in)dependent variables, the uniform regulatory framework of one constituent part of the U.S. banking sector and regional financial segmentation.

II.A. Variation in Policy Responses to the post-World War I Boom

The policy measures at the core of this paper were taken in response to a pronounced boom phase after World War I. The strong economic expansion following armistice took the form of a commodity price boom, a subsequent rise in asset as well as real estate prices and rapid credit growth. In their classic study, Friedman and Schwartz (1963, p.222) describe the immediate post-war context as an "intense boom, marked by rapid accumulation of inventories and commodity speculation" and a "speculative climate, characterized by a strong demand for bank loans – which itself, of course, partly reflected the effect of prior monetary expansion". The nature and consequences of the extraordinary economic upswing attracted considerable attention in the economics and economic history literature. Recent contributions exploit the immediate post-war phase as an archetypal example to shed light on the anatomy of credit crises (Rajan and Ramcharan, 2015, 2016) and stress its connection to bank failures during the 1920s (Jaremski

[21]The source is freely accessible on-line in scanned format (FRASER, Federal Reserve Bulletin; last accessed 22 August 2021).

[22]The source is freely accessible on-line (NBER, Macrohistory Database; last accessed 22 August 2021).

[23]The source is freely accessible on-line in scanned format (FRASER, Annual Reports of the Federal Reserve Board; last accessed 22 August 2021).

[24]The source is freely accessible on-line in scanned format (HathiTrust, Final Report of the Joint Commission of Agricultural Enquiry; last accessed 22 August 2021).

and Wheelock, 2020a). Appendix A.1 provides more detail on the nature, extent and evolution of the post-World War I boom.

Monetary policy remained passive until January 1920, when discount rates were uniformly hiked from 4.75% to 6% in all Federal Reserve districts.[25] A second wave of policy decisions followed in late spring 1920. In contrast to January 1920, the decisions taken in late spring were not uniform across districts and resulted in the policy differences which are at the core of this paper.[26] Discount rates remained unchanged until 1 June 1920, but on or very shortly after this date four Federal Reserve Banks (Boston, Chicago, New York, and Minneapolis) hiked their policy rate to 7%. In the meantime, another four Federal Reserve Banks (Atlanta, Dallas, Kansas City and St Louis) had started a policy experiment by implementing the so called "progressive discount rate" (PDR), a new tool based on recently gained powers conferred by the Phelan Act of 13 April 1920. Congress had explicitly passed the Phelan Act to enable Federal Reserve Banks to establish graduated discount rates, and it had done so upon a recommendation of the Federal Reserve Board published in the System's annual report for 1919 (Wallace, 1956, p.61). The PDR scheme left the baseline discount rate unchanged at 6% but entailed progressive rate increases for member banks that were borrowing from Federal Reserve Banks at a level above their so called "basic line". The basic line represented the maximum amount of credit a member bank was entitled to receive from its Federal Reserve Bank. It reflected the hypothetical amount of credit a given member bank would have been able to obtain *pro rata* if all member banks in a district had been borrowing simultaneously, given the constraint of the Federal Reserve Bank's own reserve requirements.[27] The basic line of each member bank was computed on the basis of the bank's reserves maintained with and its capital contribution[28] to the Federal Reserve Bank:

$$BL = 2.5[0.65R + 0.03(C + S)]$$ [29]

where BL stands for the basic line,

R represents lawful reserves held with the Federal Reserve System[30],

C is the bank's paid-up capital and S its surplus.

[25]This paper focuses on the Federal Reserve Banks' commercial paper rate which was the main interest rate for central bank discounts of all bills maturing within 90 days, secured by collateral other than government securities. In 1920, this class of bills constituted approximately between 30% and 50% of the System's discount holdings at the end of each month and between 15% and 50% of the total amount discounted each month (Federal Reserve Board, 1921). The share of commercial paper in the System's discount portfolio was continuously on the rise after mid-1919. Hence, Federal Reserve Bank directors considered the commercial paper rate as the most relevant interest rate at the peak of the boom (Federal Reserve Board, 1923, p.16).

[26]In Appendix A.6, I discuss the historical background of U.S. monetary policy decentralization before 1935 in more detail.

[27]The Federal Reserve Banks had to hold gold reserves to cover note issuance and deposit liabilities. For more details on the Federal Reserve System's own gold reserve requirements, see Appendix A.2.

[28]When the Federal Reserve System was established in 1913, commercial banks desiring to become members of the System had to contribute a share of their own capital to the equity of the Federal Reserve Bank in their district.

[29]The exact rationale for this formula is explained in the report of the Joint Commission of Agricultural Inquiry (1922a, p.24-25): 65% of R equals the member bank's reserve deposit minus the reserve which the Federal Reserve Bank is required to hold against this deposit. 3% of $C + S$ is the amount each member bank had to contribute to the Federal Reserve Bank's capital. Finally, the factor of 2.5 derives from the Federal Reserve Bank's 40% gold reserve requirement.

[30]More precisely, R represented the average monthly reserve balance (Wallace, 1956, p.61).

The PDR penalized borrowing from the System in excess of the basic line: for every 25% by which a bank's borrowing exceeded the basic line, the bank had to pay a surcharge of 50 basis points. Hence, a bank with a basic line of \$100 intending to borrow \$200 from its Federal Reserve Bank would have paid 6% for the first \$100 borrowed, and then 6.5%, 7%, 7.5% and 8% for each \$25 increment respectively, up to the full sum of \$200 (an average rate of 6.625%). Thus, the impact of the PDR on banks' borrowing costs depended on the individual leverage of each bank. The link between bank leverage and borrowing costs ran through the costs of required reserves for deposit liabilities. Due to deposit creation, a bank's deposit liabilities increased one to one with the loan portfolio. The more loans a bank had granted, the more leveraged it became (i.e. the higher the ratio of total assets to capital) and the more of its basic line it had to use to fulfill reserve requirements. Since it directly connected the marginal cost of reserves to the individual situation of a given bank, the progressive discount rate followed a rationale resonating with the core idea of modern macroprudential policy tools. Similar to countercyclical buffers or reserve requirements, the scheme became particularly binding during the build-up phase of systemic risk: when financial institutions leveraged up in a boom phase, the PDR acted as a correcting force by dampening the incentives of financial institutions to grant additional loans and by forcing banks to internalize a part of the potential negative externalities generated by excessive credit expansion.[31] Table 1 in Appendix A.2 provides the exact dates on which the second wave of policies was implemented in the various districts and it also shows their respective end dates. The four districts hitherto unmentioned (Philadelphia, Cleveland, Richmond and San Francisco) did not change the rate schedule adopted in January 1920, nor did they implement the progressive discount rate.

To establish the validity of this historical setting as a convincing case study for the effects of financial stability policies, I provide a detailed discussion of experiment validity in Appendix A.2. Two questions stand out in this regard. First, was the Federal Reserve Banks' policy reaction in late spring 1920 effectively motivated by financial stability concerns? Second, what exactly were the financial developments the Federal Reserve Banks wished to counteract? In Appendix A.2, I show that the policy decisions taken in late spring 1920 were by no means simple, quasi-automatic consequences of the standard monetary policy rules at the time. Neither gold reserve requirements, nor any variant of the so called "real bills doctrine" can fully account for the introduction of LAW and PDR. The key to understanding the motivations driving Federal Reserve policy is to dis-aggregate policies, both geographically and over time. While the uniform rate hike in January 1920 is most convincingly explained by the gold reserve position of the System as a whole, the renewed policy action in late spring was motivated primarily by financial stability concerns. As documented by the Joint Commission of Agricultural Inquiry (1922a, p.51-52), Federal Reserve Banks which adopted financial stability policies aimed at "the preservation of the integrity of the banking system and the prevention of a financial panic". The authorities' thinking was that too accommodative a policy in their districts would induce banks to continue to expand loans at a time when commodity prices had started to fall, putting

[31] In this paper, I do not attempt to provide an objective definition of "excessive" credit growth. What counts for my natural experiment is that authorities at the time considered the build up to be "excessive", i.e. posing a threat to financial stability.

strain on their solvency if debtors' ability to repay loans were to dwindle (Joint Commission of Agricultural Inquiry, 1922a, p.88). The Joint Commission of Agricultural Inquiry (1922a, p.87) explicitly mentioned the gradual erosion of safety buffers for depositors as major concern for the Federal Reserve Banks which implemented financial stability policies.

The PDR enacted by the Federal Reserve Bank of Atlanta, St Louis, Kansas City and Dallas also targeted financial stability concerns but responded to the particular conditions prevailing in these districts. In contrast to Reserve Banks which opted for a rate hike, authorities in PDR districts observed large differences in the situation of individual member banks: "Some banks were greatly extended and borrowing heavily at the Federal Reserve Bank, in some instances as high as 10 or 15 times the basic line. Some banks were only slightly extended, borrowing moderately from the Federal Reserve Bank. Other banks were not extended at all, and were not borrowing from the Federal Reserve Bank in any amount" (Joint Commission of Agricultural Inquiry, 1922a, p.53). Appendix A.2 reveals that districts which later adopted the PDR had indeed experienced the most skewed distribution of bank-level leverage and deposits-to-capital ratios prior to June 1920. Hence, the rationale for adopting progressive rates was to distribute Federal Reserve Bank credit more evenly among the member banks in the PDR districts (Goldenweiser, 1925, p.42). The PDR did not penalize borrowing in general. It only dis-incentivized borrowing in excess of the basic line. Given the direct link between bank loans and reserve requirements, borrowing in excess of the basic line represented the very definition of what Federal Reserve Banks considered to be an "excessive credit expansion". The PDR constituted a targeted monetary policy tool used by some Federal Reserve Banks to dampen excessive credit growth fueled by some subgroups of member banks only.

II.B. Local Continuity, Pre-trends and uniform Regulatory Framework

This paper "goes local" to tackle the endogeneity of policy reactions and to disentangle the supply-side response to financial stability policies from demand-side factors. I focus on small geographic bandwidths of 25 kilometers around Federal Reserve district borders. Within this distance of the district borders, banking structure, local economic characteristics and pre-trends were largely statistically identical for treated and control group banks.

Table 1 summarizes the continuity tests for variables describing the local banking structure in Panel A.[32] I obtain the coefficients and standard errors displayed in Table 1 by running a simple cross-sectional regression of the variable of interest on the treatment dummy. I run this regression separately for each border type, comparing bank- and county-level covariates of treated regions to their control group peers. Full sample tests based on my bank-level data clearly reject the continuity assumption for both border types in the case of several banking sector characteristics. Banks subsequently treated by LAW were on average larger and exhibited significantly higher average leverage as well as deposit to capital ratios prior to June 1920 than banks located in districts which did not change policy stance. In contrast, the average bank in PDR districts was smaller, less leveraged and had a lower deposits-to-capital ratio than its

[32] I report conventional standard errors throughout Table 1 to stack the cards against detecting continuity.

control group peer prior to June 1920. The full sample continuity tests therefore confirm the endogeneity of policy decisions, as described in Appendix A.2. The test results emphasize that "going local" is a crucial element of my identification strategy: virtually all differences in Panel A disappear for both border types once I concentrate on bandwidths of 25 kilometers around the borders. Some differences in the number and location of bank-level correspondent links remain. Given that interbank connections were highly sticky (at least for the short time horizons considered in my paper), the bank fixed effects in my regressions directly control for the number and nature of banks' correspondent links. Moreover, I will delve into the special role of New York City correspondents when discussing the mechanism underlying my estimation results for LAW borders in Section IV. below.

Turning to local economic characteristics (Panel B of Table 1), the most pressing concern relates to the impact of the sharp recession of 1920-21. The post-World War I boom ended abruptly in the third quarter of 1920. According to the National Bureau of Economic Research (NBER), the business cycle peaked in January 1920. In fall 1920, the U.S. economy slid into a severe recession reaching a trough in July 1921 (Friedman and Schwartz, 1963). Commodity price collapses constituted one of the most important triggers for the sharp deterioration of economic conditions in late 1920. European agriculture had recovered much more quickly than expected from the devastation caused by World War I and started to displace American exports on world markets. Product prices imploded during the summer of 1920, putting farmers under severe pressure who had indebted themselves to heavily expand production capacities during the boom phase (Rajan and Ramcharan, 2015; Jaremski and Wheelock, 2020a). If treated and control groups were affected differentially during the fall of 1920 due to their different exposure to the dramatic agricultural price declines, my estimated treatment effect could be subject to confounding factors stemming from this shock.[33]

"Going local" is one solution to control as much as possible for the differential exposure to confounding price shocks. Local economic characteristics determined the relative strength of the 1920-21 recession in different locations across the United States. Concerns about spurious correlations bias might be unfounded if locations close to the district border exhibited similar structural economic features irrespective of treatment status. Panel B in Table 1 shows that a range of local economic characteristics related to agriculture and the commodity/land price boom (as reported by the U.S. Agricultural Census of 1920) are not statistically different in treated and control group areas. Stark differences in average farm values and mortgage debt exposure are observable in the full sample, but these decrease substantially once I focus on the area within 25 kilometers of the district borders.[34]

Finally, the absence of level differences prior to the policy decision in late spring 1920 does not rule out the possibility of diverging pre-trends in local banking characteristics. In Panel C of Table 1, I display the coefficients and standard errors obtained from a panel OLS regression of bank-level variables on a standard difference-in-differences treatment-time interaction. Controlling for time and bank fixed effects, Panel C confirms that my main outcome variables and

[33]Depending on the characteristics of treated and control regions, the bias in the treatment effect could be both upwards (amplifying the estimated coefficient) or downwards (muting the effect).

[34]To ensure that aggregate time trends (e.g. the sharp downturn starting in 1920) do not spuriously drive my estimation results, I also include time fixed effects in all my specifications.

TABLE 1
Local continuity tests and pre-trends

Panel A. Local banking structure

	LAW borders		**PDR borders**	
	Full sample	<25km	Full Sample	<25km
Total assets (ln, Sep 1919)	0.153	-0.179	-0.203	0.227
	(0.048)	(0.122)	(0.063)	(0.172)
Leverage ratio† (Jan 1920)	1.125	0.041	-0.511	0.567
	(0.128)	(0.321)	(0.157)	(0.549)
Deposits to equity ratio† (Jan 1920)	1.207	0.088	-0.370	1.152
	(0.132)	(0.337)	(0.191)	(0.798)
Cash reserves & exchange to deposits ratio†† (Jan 1920)	-0.005	0.015	0.051	-0.004
	(0.006)	(0.022)	(0.012)	(0.030)
Total number of correspondents (Jan 1920)	-0.095	-0.134	0.020	0.129
	(0.060)	(0.152)	(0.078)	(0.250)
Total number of correspondents per 100K loans (Jan 1920)	-0.056	0.140	0.185	-0.240
	(0.035)	(0.115)	(0.046)	(0.140)
Correspondent in New York City (dummy, Jan 1920)	0.049	0.058	-0.050	-0.154
	(0.013)	(0.034)	(0.020)	(0.118)
Correspondents in New York City (number, Jan 1920)	0.170	0.223	-0.009	-0.068
	(0.028)	(0.078)	(0.040)	(0.148)
Observations (number of banks)	2,621	261	1,287	65

Coefficients obtained by simple cross-sectional regression on treatment dummy. Standard errors in parentheses.

Panel B. Local economic characteristics (all variables measured at year-end 1919)

	LAW borders		**PDR borders**	
	Full sample	<25km	Full Sample	<25km
Total population (ln)	0.047	-0.250	-0.289	-0.078
	(0.111)	(0.207)	(0.076)	(0.150)
Number of farms per inhabitant	0.003	0.005	0.032	0.003
	(0.004)	(0.011)	(0.004)	(0.013)
Number of farms per acre	-0.001	-0.001	0.001	0.001
	(0.009)	(0.001)	(0.000)	(0.001)
Improved farm land per acre	-0.008	-0.001	-0.038	0.014
	(0.021)	(0.051)	(0.020)	(0.060)
Average farm value	4,960.559	797.311	-3,936.005	-1,160.913
	(900.106)	(980.817)	(482.860)	(1,084.003)
Average share of farms mortgaged	0.095	0.040	-0.011	0.003
	(0.008)	(0.018)	(0.007)	(0.016)
Average debt to value ratio of farms	0.555	0.515	2.400	1.213
	(0.654)	(1.163)	(0.563)	(1.559)
Average mortgage interest rate	-0.182	-0.040	0.736	0.146
	(0.073)	(0.065)	(0.077)	(0.164)
Exposure to traded crops†††	-0.027	-0.058	0.029	0.001
	(0.014)	(0.042)	(0.012)	(0.028)
Observations (number of counties)	515	60	542	43

Coefficients obtained by simple cross-sectional regression on treatment dummy. Standard errors in parentheses.

Panel C. Pre-trends in local banking characteristics (Sep 1919 – Jan1920)*

	LAW borders		**PDR borders**	
	Full sample	<25km	Full Sample	<25km
Total lending (ln)	0.014	0.027	-0.044	0.001
	(0.011)	(0.028)	(0.018)	(0.051)
Leverage ratio (ln)	0.012	0.023	-0.041	-0.013
	(0.010)	(0.023)	(0.017)	(0.051)
Deposits to equity ratio (ln)	-0.023	0.017	0.041	0.033
	(0.012)	(0.029)	(0.022)	(0.076)
Cash reserves & exchange to deposits ratio	0.000	-0.003	0.080	-0.021
	(0.019)	(0.009)	(0.040)	(0.024)
Total deposits (ln)	-0.021	0.022	0.038	0.047
	(0.012)	(0.034)	(0.023)	(0.076)
Bank equity (ln)	0.003	0.007	-0.001	0.014
	(0.009)	(0.014)	(0.014)	(0.013)
Observations (number of banks)	5,217	517	2,567	129

Standard errors in parentheses. County-level data weighted by number of banks in county.

†The leverage ratio is defined as the ratio of total lending to equity. Since the Rand McNally bankers directory (1920, 1921a,b) does not report total balance sheet size, I use total lending as the denominator for all call dates instead. Equity is defined as the sum of total paid-up capital, surplus and undivided profits.

††Cash reserves include cash in vaults, reserves deposited with other banks and lawful reserves. Deposits constitute the total amount of deposits received, i.e. time and demand deposits.

†††*Exposure to traded crops*: this variable measures the share of barley, corn, cotton, oats, rye, tobacco and wheat acreage as a percentage of total county area. During the recession, all of these crops experienced heavy price declines of between 50% and 75% relative to their January 1920 values, c.f. NBER Macrohistory Database (Feenberg and Miron, 1995) and Figure 2 in Appendix A.1.

*I estimate the following model to check for pre-trends: $Y_{i,t} = \alpha + \beta Jan1920_t \times T_i + \phi_b + Jan1920_t + u_{i,t}$, where T_i indicates treated banks (treated either by LAW or by the PDR), ϕ_b captures bank fixed effects, and $Jan1920$ is a dummy flagging observations from January 1920. $Y_{i,t}$ are the variables tested for the presence of pre-trends and β represents the coefficient of interest displayed in Panel C.

other bank-level characteristics exhibit no remaining, locally diverging pre-trends.

While observable variables show statistically identical pre-treatment levels and pre-trends within bands of 25 kilometers around the policy borders, less evident or not easily measurable discontinuities in financial/economic policies could represent an additional source of concern for identification. In this paper, I thus focus on so called "national banks" to preempt potential discontinuities in banking regulation and supervision. By 1920, national banks constituted a homogeneous class of Federal Reserve member banks with consistently enforced reserve requirements. National banks were subject to the same supervisory architecture and operated according to a uniform regulatory framework across all U.S. states. Furthermore, national banks never joined any of the state-sponsored deposit insurance schemes put in place after the panic of 1907 (Calomiris, 1989).[35] Due to this uniform regulatory framework, national banks represent an ideal study and control group. I provide more historical details regarding the U.S banking and regulatory landscape in Appendix A.3.

Despite the absence of policy discontinuities in national banking regulation at the federal level, differences in other economic, legal or political interventions might thwart identification whenever state borders coincide with Federal Reserve district borders. To ensure that estimated treatment effects are not driven by other discontinuities across these "double" borders, I exploit an additional quasi-experimental feature of my setting in the robustness checks of this paper. To isolate the impact of LAW and targeted monetary policy from other policy differences, I focus on states whose territories were split between Federal Reserve districts with different policy responses to rising financial stability concerns in 1920. The availability of split states also entails another advantage. It allows me to include state-chartered banks[36] into my discontinuity regressions. At the state-level, state-chartered banks were also subject to uniform regulation and supervision. The inclusion of state-chartered banks enables me to check whether and how nonmember banks in treated districts reacted to the policy changes and to what extent the impact on their balance sheets differed from the one experienced by Federal Reserve member banks.

One final continuity assumption of this paper is that – apart from the variation in policy responses in late spring 1920 – the Federal Reserve Banks implemented homogeneous lending policies across all the districts. In this regard, the presence of differential moral suasion strategies to "talk down credit" in 1920-21 could constitute a challenge for my identification strategy. Moral suasion, also known as "direct action", describes attempts by Federal Reserve Banks to prevent further loan expansion by formally or informally communicating their opinion on acceptable levels of credit growth to banks in their district.[37] Systematic qualitative, let alone quantitative information on the importance of these attempts is scarce. For the period of interest, I could only identify one relevant bank credit-related circular by the Federal Reserve Bank of St Louis (dated 22 July 1920). Rather than focusing on the quantity of credit, however,

[35] After 1907, Oklahoma, Kansas, Texas, Nebraska, North Dakota, South Dakota, Mississippi, and Washington introduced deposit insurance for state-chartered banks. Deposit insurance introduces further differences between state-chartered banks which cause cross-state (and intra-state) comparisons of these banks to become even less feasible.

[36] In Appendix A.3, I contrast national banks with state-chartered banks. State-chartered banks were regulated according to different laws from state to state and cannot be easily compared to each other across states.

[37] I discuss moral suasion and its potential implications in more detail in Appendix A.4.

the circular merely admonished banks for passing on higher policy rate to their customers.[38] A second concern is the potentially different application of collateral eligibility rules, loan to value ratios and/or haircuts across Federal Reserve Banks.[39] The little available anecdotal evidence shows that individual Federal Reserve Banks sometimes adjusted these lending conditions on the spot, to account for particular borrower characteristics.[40] Their tailored on-the-spot approach suggests that Federal Reserve Banks did not consistently or systematically differ in their application of these risk management techniques. Overall, the available information corroborates the premise that neither of these two concerns fundamentally undermines my empirical strategy.

II.C. Financial Segmentation

While "going local" is necessary to address the endogeneity of policy reactions and to disentangle banks' the credit supply response, this strategy may also come at a price. In modern day settings, banks situated close to policy borders would seem to be particularly prone to engage in regulatory arbitrage via relocation, branching or cross-border borrowing. The unique historical setting of my paper, however, largely rules out these possibilities to circumvent treatment and alleviates concerns that cross-border inter-bank borrowing results in SUTVA violations.

First, I show that banks in my sample did not relocate in anticipation, nor in reaction to policy differences. Figures 10 and 11 in Appendix A.5 graphically compare the geographic distribution of national banks with respect to the nearest district border at three points in time. The evidence testifies to the fact that changes in the geographic distribution of banks with respect to district borders are practically nonexistent during the time periods considered in this paper. Appendix A.5 also formally confirms these insights using statistical distribution and density tests. These results are not surprising. Given the costs and time involved in relocation, it is unlikely that national banks could or even wished to switch districts simply in order to avoid treatment. Moreover, the relatively short time window during which the LAW and the PDR scheme were in place probably preempted any relocation attempts which may have resulted from longer lasting policy differences.

Second, national banks were not authorized to engage in inter-state branch banking (Mitchener, 2005; Richardson and Troost, 2009). Before 1922, even intra-state branching was prohibited for national banks. Since the National Bank Act had not provided any explicit directives on the regulation of interstate banking, the Comptroller of the Currency issued the decisive direction in this regard (Johnson and Rice, 2007). After 1865, the OCC explicitly forbade national banks to open an office in more than one location. Consequently, the national banking sector was characterized by a true unit banking structure.

[38] The circular can be read here: FRASER, Circular of the Federal Reserve Bank of St Louis; last accessed 22 August 2021.

[39] I would like to thank David Wheelock for making me aware of this caveat. See also Tallman and White (2020) for this point.

[40] I am grateful to Mark Carlson for sharing this information with me, which is based on archival material from his ongoing project on Federal Reserve thinking on emergency liquidity provision in the years prior to the Great Depression. For more details, see also Carlson and Duygan-Bump (2021).

Third, member banks located in a given Federal Reserve district could only borrow from the Federal Reserve Bank heading their district. Direct borrowing from a Federal Reserve Bank in another district was ruled out from the beginning by the very organization of the Federal Reserve System (Hackley, 1973). For example, a national bank located in the Federal Reserve district of Boston was not allowed to apply for loans from the Federal Reserve Bank of New York. This form of financial segmentation thus regulated access to central bank lending facilities in a way which made direct regulatory arbitrage impossible. Banks subject to different monetary policies could not directly avoid treatment by cross-district borrowing from another Reserve Bank.

Fourth, whether member banks circumvented monetary policy decisions by borrowing from their correspondent banks in other districts remains an open question. The available empirical evidence from the 1920s shows that differentials in Federal Reserve Bank discount rates did not trigger corresponding flows of funds between districts (Cohen-Setton, 2016). This finding would seem to indicate that interbank markets were not used to engage in policy arbitrage. The fact that Federal Reserve Banks maintained different policy rates throughout the 1920s suggests that the districts were at least partly financially segmented. Otherwise, meaningful policy differences could have not been maintained inside the U.S. monetary union. At the same time, historical anecdotes on the use of correspondent networks to bypass "unpleasant" monetary policy decisions point in another direction.[41] Since limitations in the data for inter-district flows of funds between member banks do not allow for an encompassing study[42], the available empirical evidence should be interpreted with caution. In the context of my study, arbitrage via correspondent banks stacks the deck against finding significant treatment effects because it biases treatment coefficients for LAW and PDR policies downwards (i.e. towards zero).

Finally, even if banks exploited their interbank network to circumvent financial stability policies, violations of the no interference component of SUTVA are unlikely to result from this form of regulatory arbitrage in my setting. Due to the pyramid structure of the U.S. banking system, most of my sample banks' out-of-district correspondents were located in central reserve cities or reserve cities.[43] Therefore, the nature of the interbank network mostly ruled out direct correspondent lending from banks just across the district border. Given that my treatment and control groups are located in close bands around the district borders, arbitrage via correspondent banking is unlikely to breach the no interference assumption in my estimation samples. In addition, the very design of Federal Reserve district borders captured major regional correspondent networks within a single district (Jaremski and Wheelock, 2017). Hence, by construction, correspondent links between less important banking locations had a high probability of being "fenced" into one common Federal Reserve district. First-hand graphical evidence on the premise that cross-border interbank links did not violate SUTVA is depicted in Appendix A.6. The figures in Appendix A.6 focus on the case of split states because banks in these states appear least immune to SUTVA violations due to interbank borrowing. Located within the same

[41] For an example relevant to the specific context of this study, c.f. Meltzer (2003, p.107).

[42] Inter-district flows of funds for member banks are only available for major (central) reserve cities, c.f. Cohen-Setton (2016).

[43] I investigate the role of links to New York City in Section IV. below when discussing the mechanisms underlying the econometric results of my paper.

state but in different Federal Reserve districts, these banks seem most likely to have interbank ties that cut through district borders. Appendix A.6 shows that treated (non)member-banks in Kentucky and New Jersey maintain virtually no interbank links with their peers in the the untreated half of the state. Hence, even my arguably most demanding specification is unlikely to fall prey to SUTVA violations that could otherwise bias treatment coefficients upwards.[44]

III. Results

III.A. *Policy Effects on Bank Lending and Leverage*

Drawing on the identification strategy explained above, I estimate the causal effects of financial stability policies using a local difference-in-differences design:

$$Y_{i,t} = \delta(T_i \times Post_t) + \mathbf{\Psi}'\mathbf{X}_{i,t} + \phi_b + \gamma_t + u_{i,t} \tag{1}$$

> where $Y_{i,t}$ is the bank-level outcome variable; T represents an indicator taking the value of one if a given bank i is located in a district which implemented LAW or targeted monetary policy (and zero otherwise); $Post_t$ is a dummy flagging observations from the treatment period (i.e. call dates after late May/early June 1920); $\mathbf{X}_i$ stands for bank-level controls; ϕ_b are bank-level fixed effects absorbing all time-invariant bank-specific differences in the outcome variables; γ_t represents time fixed effects capturing call date-specific aggregate time trends and u_i is the bank-specific error term.

The main parameter of interest in Model 1 is δ, the effect of LAW or PDR policy on bank-level outcomes $Y_{i,t}$. To estimate the policy-specific coefficient δ, I run two separate series of regressions. The first series exploits the policy variation across the borders between the Federal Reserve districts which implemented LAW and the Federal Reserve Banks which did not change policy stance in late spring 1920. In this case, δ represents the treatment effect of conventional monetary policy leaning against the wind. The second series of regressions exploits policy differences across borders separating districts subject to the PDR policy and districts which did not change policy stance in late spring 1920. In this second case, δ measures the treatment effect of PDR policy. I estimate both series of regressions using the full sample and gradually smaller bandwidths (of 200, 100, 75, 50 and 25 kilometers) around the district borders. For example, the bandwidth of 25 kilometers means that all national banks located within 25 kilometers on either side of the border are included in the estimation sample.

I focus on bank-level changes in balance sheet quantities and ratios as my main outcome variables of interest ($Y_{i,t}$).[45] In particular, I estimate the effect of LAW and PDR policy on banks' total lending and the bank-level leverage ratio. As discussed in Appendix A.2, these two variables constituted the focal point of Federal Reserve officials' discussions in spring 1920. The

[44] I provide more details on the U.S. interbank market structure (including figures depicting the links of banks located in the non-treated half of split states) and on the design of Federal Reserve districts in Appendix A.6.

[45] Monetary policy can affect bank balance sheets by triggering changes in quantities as well as in (asset) prices (International Monetary Fund, 2015). Dis-aggregated bank-level information on asset composition at market prices is not available for the 1920s.

Federal Reserve Banks motivated policy action with reference to what they deemed excessive upward trends in these variables. To facilitate comparison and to allow for a classic log-change-based interpretation of the estimated treatment coefficients, I employ log transformations of my outcome variables.

The regressions using total lending as the main outcome include a time-varying control variable for bank-level liquidity (cash reserves & exchange to deposits ratio). When drawing on the second outcome variable which represents a ratio (leverage, i.e. the ratio of total lending to equity), I also control for changes in bank-level equity over time, in addition to liquidity. These control variables are represented by $\mathbf{X}_i$. I do not control for covariates capturing changes in deposits because these variables vary one for one with banks' lending activity in contexts where loans involve deposit creation. Appendix B.1 shows summary statistics for all the variables included in Model 1.

TABLE 2
Treatment effects of LAW and PDR policy: baseline specification (all border regions)

Panel A. Leaning against the wind

Outcome variable: total lending (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.001	0.020	0.033	0.039	0.063	0.049
	(0.008)	(0.008)	(0.009)	(0.011)	(0.013)	(0.018)
	[0.009]	[0.010]	[0.012]	[0.013]	[0.015]	[0.021]
R-squared	0.22	0.23	0.33	0.33	0.39	0.45
Observations	10,589	8,018	4,560	3,534	2,169	1,047

Outcome variable: leverage ratio (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.001	0.011	0.019	0.026	0.069	0.064
	(0.007)	(0.008)	(0.009)	(0.010)	(0.012)	(0.017)
	[0.008]	[0.010]	[0.011]	[0.013]	[0.015]	[0.023]
R-squared	0.23	0.25	0.34	0.34	0.43	0.44
Observations	10,589	8,018	4,560	3,534	2,169	1,047

Panel B. Progressive discount rate

Outcome variable: total lending (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.061	-0.044	-0.061	-0.045	-0.054	-0.100
	(0.013)	(0.017)	(0.021)	(0.024)	(0.027)	(0.047)
	[0.013]	[0.019]	[0.024]	[0.027]	[0.033]	[0.062]
R-squared	0.18	0.23	0.35	0.37	0.35	0.39
Observations	5,191	2,535	1,272	923	662	262

Outcome variable: leverage ratio (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.056	-0.042	-0.059	-0.042	-0.057	-0.106
	(0.011)	(0.015)	(0.020)	(0.023)	(0.026)	(0.046)
	[0.012]	[0.017]	[0.022]	[0.026]	[0.032]	[0.059]
R-squared	0.27	0.34	0.43	0.46	0.38	0.46
Observations	5,191	2,535	1,272	923	662	262

Standard errors in parentheses. Clustered standard errors (at bank-level) in squared brackets.
All regressions with bank FE, time FE and bank-level controls.

Table 2 summarizes the baseline results for both policy types and outcome variables. The coefficients are estimated on the basis of all LAW and PDR border regions. In the case of LAW, all banks located at the border separating district 4 and district 7, as well as banks located at the border separating district 2 from district 3 or 4 are included in the estimation sample. To estimate the policy effect of the PDR scheme, I draw on all banks in my sample located at the border between district 8 and district 4, as well as all banks at the border between district 6 and district 4 or 5. Panel A displays the treatment effects of LAW on bank-level lending and leverage. The treatment effects of the PDR scheme are shown in Panel B. The full sample results in the leftmost column of Table 2 suggest that the LAW policy did not have an economically, nor a statistically significant impact on bank-level outcomes. The PDR, however, reduced total lending and leverage by around 6%. For the full sample, the PDR treatment effects are statistically different from zero at the 99% confidence level. As one approaches the border, the dampening impact of the PDR on bank credit is slightly less precisely estimated, but tends to become even more pronounced (10% to 11% for the 25km radius). The PDR thus proved to be an effective tool for reining in banks' credit growth at the time. In contrast, the local discontinuity regressions for LAW show that the interest rate hike exerted a perverse influence on bank credit. Focusing on the sample of banks located within 25km of the district borders, LAW appears to have caused total lending and leverage to increase by between 5% to 6% (statistically significant at the 99% confidence level).

I provide several additional results related to Table 2. Appendix B.2 reports coefficients and standard errors for the control variables (bank-level liquidity and equity) alongside the policy treatment effects. Appendix B.3 shows that the results reported in Table 2 continue to hold – and are even strengthened in the case of the PDR – when I compute Conley (1999) standard errors to correct for spatial auto-correlation, instead of conventional and clustered standard errors. Appendix B.3 also reports standard errors computed using cluster bootstrap procedures. In Appendix B.4, I explore an alternative cross-sectional geographic regression discontinuity (RDD) specification (local linear regression). While the size and sign of coefficients I obtain are similar to the results of the local difference-in-differences estimator, the treatment effects are less stable and less precisely estimated with the geographic RDD approach. Since the cross-sectional RDD specification does not allow me to control for bank-level fixed effects, it may not sufficiently capture unobserved heterogeneity at the bank-level. As a corollary, the risk of residual omitted variable bias is higher in the cross-sectional RDD set-up than in the local difference-in-differences model. Thus, the latter constitutes my preferred specification.

To investigate potential heterogeneity in the treatment effects, I first split the LAW sample into a Western border (district 4 vs. district 7) and an Eastern border (district 2 vs. districts 3 and 4) estimation sample. The results for the Western and Eastern border are displayed in Appendix B.5 (Table 14). The results reveal that aggregate treatment effects for LAW mask substantial geographic heterogeneity. On the one hand, the interest rate hike seems to have reduced credit growth in the West. Yet, the downward pressure exerted by LAW on banks' credit expansion in district 7 was both economically and statistically weak relative to the PDR's effects. In particular, Panel A in Table 14 demonstrates that the LAW policy's impact vanishes as one approaches the border. On the other hand, LAW triggered a strong perverse impact in the New York district (see Panel B in Table 14). This perverse effect in district 2 drives the aggregate results for LAW displayed in Table 2. The treatment effect identified off the closest bandwidth around the border (25km) amounts to an 8% to 9% increase in bank lending and leverage in response to the interest rate increase. I analyze the reasons for the considerable geographic heterogeneity in the treatment effects of LAW in Section IV. below.

In Table 15 in Appendix B.5, I investigate potential regional differences in treatment effects for PDR districts. While the Southern regions included in the PDR border sample are arguably more homogeneous than the Western and Eastern LAW border samples, one important caveat may apply to the aggregate results displayed in Table 2. The Federal Reserve Bank of Atlanta adopted the PDR only for the period between 31 May and 1 November 1920, after which date the district switched to the LAW policy.[46] Since parts of my PDR border sample draw on treated banks in district 6, I re-estimate the PDR treatment effects excluding the Atlanta district. For completeness, I also re-estimate the impact of PDR without the banks located in district 8. The exclusion of the mixed policy district of Atlanta leads to even larger and more precisely estimated PDR effects, which now entail a reduction in total lending and leverage of up to 14% relative to control group banks (Panel A of Table 15). In contrast, when I drop district 8 from the estimation sample (see Panel B), the PDR treatment coefficients converge to the effects of LAW on the Western border and also vanish when one approaches the border line.

Finally, in Appendix B.6, I re-estimate my baseline regressions after aggregating bank credit supply at the town-level (column 1 of Table 16 in Appendix B.6). This aggregation procedure reduces the number of observations, but it allows me to gauge whether less affected banks substituted for financial intermediaries that had to cut back the most in lending. I find that the town-level treatment effects of financial stability policies are marginally smaller than my bank-level estimates. Yet, both the contractionary effect of the PDR scheme and the perverse effect of LAW remain intact at the town-level. I also show that bank-level estimates hardly change when the regressions are weighted by bank size (column 2 of Table 16 in Appendix B.6).[47] In addition, I report estimation results for trimmed regressions, dropping the smallest 5% and the largest 5% of observations for the two outcome variables in my sample (column 3 of Table 16 in Appendix B.6). These estimates serve to check the sensitivity of my results to outlier bank observations for the smallest radius of 25km. Sensitivity checks are more insightful for the LAW sample because the very rationale of the PDR consisted in hitting over-leveraged banks more heavily than less extended ones. Eliminating highly leveraged banks from the local PDR estimation sample will almost mechanically result in lower and less precisely estimated treatment effects. While the elimination of outlier observations via trimmming reduces the size of the perverse treatment effect of LAW, the latter remains positive and statistically significant. All the results summarized in Appendix B.6 confirm that the PDR policy was more effective in taming credit growth than LAW.

III.B. Robustness Checks

I pursue five different strategies to test the robustness of the estimated treatment effects for LAW and the PDR scheme. First, I conduct a pre-treatment Placebo test. The financial stability policies were introduced in late spring 1920. Hence, total lending and leverage of treated banks in LAW and PDR districts should not have evolved differently from control group banks due to

[46] See Appendix A.2 (Table 1) for a summary of the exact dates when LAW/PDR was adopted in the various districts.

[47] I draw on bank size as measured by banks' total assets in September 1919, because my call report sources for January 1920 (i.e. the last reports before treatment started) do not record total assets.

treatment before these dates. I test this hypothesis by checking for pre-treatment effects between September 1919 and January 1920. Having already checked for pre-trends in Section II. (c.f. Table 1), I replicate this test for the different radius cut-offs (full sample, 200, 100, 75, 50 and 25km) and include the standard control variables from Model 1. I report the results for the pre-treatment Placebo test in Appendix C.1. I find no evidence suggesting that pre-existing trends could spuriously drive my estimation results.

In my second Placebo test, I replicate the local difference-in-differences regressions above drawing on fictitious policy discontinuities between districts which did not change policy stance in late spring 1920. As shown in Figure 1, Districts 3, 4 and 5 did not change policy stance and simply kept the prevailing policy rate at 6%. Hence, I test for the presence of treatment effects where there should be none by exploiting three combinations of fictitious policy discontinuities between these districts. For each of the three combinations, I "pretend" that banks in one of the districts were treated by a financial stability policy, while asssuming that financial institutions in the other two districts were not. I report the results for this Placebo test in Appendix C.2. I find no evidence for a local treatment effect for any of the fictitious policy discontinuities.

Third, I re-estimate the local difference-in-differences regressions drawing on bank-level data from two federal states which were split by Federal Reserve district borders with different policies: Kentucky and New Jersey. Kentucky is split between district 8 (PDR) and district 4 (no policy). New Jersey's territory is split between district 2 (LAW) and district 3 (no policy). The split-state regressions address the worry that differential (economic) policies at the state-level could bias my estimated treatment effects because such differences may induce a spurious discontinuity in outcome variables across state borders. One motivation for this specific test is that the estimated treatment coefficients for LAW and the PDR tend to increase in size as one approaches the border (c.f. Tables 2 above, and Tables 14 and 15 in Appendix B.5). Hence, to make sure that my results are not driven by discontinuities across state borders unrelated to LAW and the PDR, I apply Model 1 to split-state data only. I report the results for this robustness check in Appendix C.3. I find no evidence for an upward bias in the treatment effects resulting from the LAW policy. In fact, the split-state specification for New Jersey results in even larger (perverse) treatment effects. For the PDR policy, my split-state results suggest a small upward bias (i.e. a more negative coefficient) relative to the results obtained when excluding the Atlanta district (c.f. Table 15 in Appendix B.5, where the reported impact amounts to between -11% and -14%). Overall, however, the local treatment effects for PDR remain stable, pointing to a reduction in total lending and leverage by around 10%.

Fourth, I implement a Placebo test to check for post-treatment effects. Total lending and leverage of treated banks in LAW and PDR districts should not have evolved differently from control group banks due to treatment after the two policies were discontinued. I can test this hypothesis based on split-state data because I collected national bank balance sheets for the July 1921 call date for the federal states of Kentucky and New Jersey. In these two split states, the financial stability policies were discontinued on 16 June 1921 (district 2) and on 23 June 1921 (district 8) respectively. Thus, I replicate the local difference-in-differences regressions by drawing on data from the July and September 1921 call dates only. The results are summarized in Appendix C.4. I find no evidence for the presence of treatment effects after the financial stability policies were discontinued.

Finally, I estimate Placebo regressions exploiting balance sheet data from state-chartered banks. I focus on state-chartered banks which had not opted in to become members of the Federal Reserve System (so called "non-member banks"). State-chartered non-member banks in treated territories should have been less strongly affected by the financial stability policies than national banks, because they did not directly interact with the Federal Reserve System. In particular, non-member were not allowed to borrow from the Federal Reserve Banks in their districts or elsewhere.[48] I implement this Placebo test using bank-level data from the split states of Kentucky and New Jersey. The split-state specification is the cleanest way to test for policy effects on non-member banks because different states had different regulations for state-chartered financial institutions. The Placebo test results are reported in Appendix C.5. The coefficients suggest that the two policies had no measurable effect on non-member banks.

IV. Mechanism

IV.A. The Reserves Mechanism: Incentives to Grant new Loans

Both LAW and the progressive discount rate scheme increased the marginal cost of reserves. LAW translated into a flat increase in the marginal percentage cost of reserves irrespective of the amount a given bank was already borrowing from its Federal Reserve Bank. In contrast, the PDR turned the cost of borrowing from the Federal Reserve System into a function of a bank's current level of borrowing from the Reserve Bank relative to its basic line. For modestly leveraged banks in PDR districts, the marginal cost of reserves could be well below the one faced by banks in LAW districts. Banks which had already been borrowing substantially above their basic line when the PDR was first introduced, however, faced much higher marginal costs than credit institutions located in LAW districts. As a corollary, *ex ante* the relative impact of LAW and the PDR on bank-level outcomes is ambiguous: it depends on the extent of basic line usage in PDR districts. To corroborate the statistically and economically significant effect of the PDR scheme – without resorting to additional mechanisms at play (see next subsection) –, some banks in the PDR districts must have been borrowing far more than their basic line when the progressive discount rate scheme was introduced. Only in this case could initially over-leveraged banks have dragged down the mean value of bank-level outcome variables sufficiently to generate larger negative and statistically more significant treatment effects than LAW.

Systematic bank-level data on the actual level of banks' borrowing from their Federal Reserve Bank are not available.[49] Hence, I resort to simulations of balance sheet dynamics akin to stress-tests to illustrate the average impact of the two policies on treated banks under different scenarios of basic line usage and new loan sizes. I focus on how the marginal incentives of banks to expand their loan portfolio play out in the two different policy regimes. This approach

[48]For more institutional details and temporary exemptions to this rule, please refer to footnote 6 in the introduction.

[49]National bank examiner reports do provide data on current borrowing from the Federal Reserve System (including both discounts and advances). Yet, these examinations were conducted on different call dates for each bank and the snapshots they represent are therefore not directly comparable.

allows me to pin down the average level of basic line usage necessary to make the PDR more binding than LAW.

Appendix D.1 provides the details and results for my simulation exercise. The main takeaways are straight-forward. For a given size of new bank loans, higher basic line usage shifts the mean marginal interest rate schedule upwards. This relationship is trivial and simply reflects the basic dynamics of the PDR. A basic line utilization of 100% prior to the new loan means that the bank borrows the additional required reserves at a minimum rate of 6.5%. Banks with a level of prior basic line usage of 200% face a minimum marginal rate of 8.5%. All else equal, as soon as the mean bank in the PDR policy districts utilizes more than 125% of its basic line, the average impact of the PDR on the marginal cost of reserves will be at least equal to the impact of LAW.[50]

The crucial question is whether the observed distributions of basic line utilization prior to and during the treatment period could plausibly result in average policy impacts significantly larger than those of LAW. Unfortunately, the Federal Reserve district of Kansas City was the only district which published relevant information on this question. In their testimony to the Joint Commission of Agricultural Inquiry (1922a), agents of the Federal Reserve Bank of Kansas City reported aggregate data on basic line utilization in the seven constituent states of the Tenth Federal Reserve district (Colorado, Kansas, Missouri Nebraska, New Mexico, Oklahoma and Wyoming). The data cover each of the 16 months between April 1920 and July 1921. I discuss the origin of these data in Appendix D.2, where I also display the numbers in separate tables for each state. The tables in Appendix D.2 provide direct descriptive evidence compatible with the claim that the PDR scheme generated an incentive structure prone to trigger stronger credit restraint than LAW. Average basic line usage in three of the states (Missouri, Nebraska and New Mexico) exceeded the threshold of 125% several times during the period under observation. According to the stress-testing exercise, the marginal mean costs of granting new loans during these months was therefore higher than it would have been had the tenth district implemented a rate hike to 7%.

The true underlying distribution of basic line utilization, however, was more skewed than can be conveyed by the aggregate numbers in Appendix D.2. Although on average a third of all member banks was borrowing in excess of their basic line in district 10, additional descriptive data from the report of the Joint Commission of Agricultural Inquiry (1922a) shows that 23 banks located in Omaha and Kansas City virtually monopolized borrowing from the Federal Reserve Bank by absorbing 73% of the Bank's lending power.[51] Eight months after the introduction of the PDR scheme, these banks' share had been reduced to 49% while the share

[50]The incentive for banks to grant new loans depends on the costs of required reserves relative to the expected future income generated by the new loan. The expected income in turn depends on the default probabilities of borrowers, other administrative costs and, of course, the interest rate charged by the bank. Since no systematic bank-level data is available for any of the variables relevant for computing loan income, I approach the problem from the cost side while assuming the income side is fixed. I discuss rate pass-through to bank loans in subsection C below.

[51]The Joint Commission computed the lending power of a given Federal Reserve Bank on the basis of the reserves and capital the Reserve Bank had received from member banks in its district. The idea of a limited amount of lending power was a theoretical concept which did not have direct policy relevance: Federal Reserve Banks could borrow from each other via the interdistrict settlement fund (Wallace, 1956; Tallman and White, 2020).

of banks which did not borrow from the Federal Reserve Bank had decreased markedly from 61.7% to 33.8% (Wallace, 1956, p.63). Other descriptive evidence from Joint Commission of Agricultural Inquiry (1922a) also confirms that basic line utilization had been highly skewed just before the PDR policy was enacted. The report mentions that within the very same districts, basic line utilization could range from 1500% (i.e. 15 times the basic line) to 0% (Joint Commission of Agricultural Inquiry, 1922a, p.53). While the number of banks effectively paying high average rates following the start of the PDR scheme remained rather modest[52], Wallace (1956, p.61) emphasizes that the available data do not reflect "the extent to which banks avoided payment of progressive rates by reducing their own loan portfolios". The number of banks deliberately deleveraging in response to the policy or in anticipation of its effects may have been (much) higher than the number of banks effectively borrowing at elevated rates.

IV.B. Basic Line Dynamics and the Marginal Cost of Reserves: the Case of Funding Shocks

The previous subsection suggests that basic line usage in PDR districts must have been highly skewed before the scheme was introduced to plausibilize the average treatment effects found by this study. These comparative statics, however, neglect important dynamics in 1920-21. In his primer on the progressive discount rate experiment, Wallace (1956) discusses an additional twist to the story. Wallace (1956) argues that deposit withdrawals from banks in treated districts may have substantially reinforced the treatment effect of the PDR tool. Although Wallace (1956, p.68) does not formally test his idea, his contribution connects the effect of targeted rate action directly to the roots of the recession in 1920-21:

> Farmers in agricultural districts being unable to sell their products for enough to liquidate bank loans, or in many cases to sell them at all, drew down their deposits to pay debts to merchants and factors and others who in turn paid wholesalers or manufacturers in the cities who in turn liquidated their bank loans. [I]n every such transaction an equivalent amount of reserves was transferred from the bank in the agricultural area to the bank in the non-agricultural area, [which constitutes] the full explanation of why basic lines fell so low in agricultural areas, thereby forcing the banks to borrow heavily at their Federal Reserve Bank. The difficulty of the banks lay not so much in a tremendous increase in deposits relative reserves as in a tremendous decrease in reserves relative to deposits. At the time an Alabama bank was forced to pay a [maximum marginal] rediscount rate of 87.5 per cent, its reserve balance had fallen to $86!

Wallace (1956) sketches out an interesting additional transmission channel of the PDR policy: the impact of funding shocks.[53] I illustrate the effect of funding withdrawals in a stress-

[52] Based on the congressional record, Wallace (1956, p.61) reports that 44 banks in the Atlanta district, 49 banks in the St Louis district, 114 bank in the Kansas City district and 20 banks in the Dallas district paid average interest rates higher than 10%.

[53] The dynamics discussed by Wallace (1956) do not challenge my identification strategy. My estimation framework is based on small bandwidths around the district borders where agricultural intensity, and therefore deposit withdrawals, were highly similar before treatment occurred in late spring 1920. My research design also preempts worries that withdrawals of reserves from agricultural regions and their subsequent transfer to non-agricultural regions could violate the no

testing exercise akin to the one presented in the previous subsection. I consider the case of a one-off funding shock.[54] For each bank, I compute a range of differently sized funding shocks as a percentage of its current demand deposits. The shock can take any size between 5% and 90% of current demand deposits. Furthermore, to obtain conservative simulation results, I assume that each bank's cash position as shown on the balance sheet is perfectly liquid.[55] Cash in vaults represents the first line of defense against funding shocks. Since I assume banks deplete their cash reserves before tapping into Federal Reserve System credit, I deduct the liquid reserves held from the amount to be borrowed following a funding shock. Thus, in my stress-test scenarios, borrowing from the Federal Reserve Bank only occurs once the bank has completely run out of cash reserves.[56]

Figure 2 shows the mean marginal interest rate faced by banks in PDR and LAW districts under different scenarios of funding withdrawal intensity and basic line utilization. Funding shocks trigger much larger increases in the mean marginal interest rates in PDR districts than the different loan size scenarios discussed in the previous subsection. Even for the case of no pre-treatment borrowing from the Federal Reserve Bank (i.e. 0% pre-treatment basic line usage), funding shocks could quickly push the mean marginal rate above the 7% flat rate. For scenarios with pre-treatment basic line usage above 50%, small to medium sized funding shocks were sufficient to make the targeted monetary policy more binding than the flat LAW rate hike.

This amplification effect comes about because the amount borrowed from the Federal Reserve Bank is an order of magnitude higher in the presence of funding shocks. When granting a new loan, the bank in question only needs to borrow a fraction of the loan amount to fulfill higher reserve requirements. In contrast, when funding shocks hit a given bank and cash reserves are not sufficient to honor all withdrawal demands, the bank has to borrow the entire remainder (withdrawals minus cash in vaults minus excess reserves) from its Federal Reserve Bank. If the remainder is large or if the affected bank was already borrowing heavily from the Federal Reserve Bank prior to the shock, the PDR quickly pushed the member bank into higher marginal rate schedules. In Appendix D.3, I also report the underlying distributions of the maximum marginal rate at which the banks in my sample subject to the PDR would have been borrowing from their Federal Reserve Bank under three different scenarios of pre-treatment basic line uti-

interference component of SUTVA. My local estimation strategy makes sure that control regions exhibited a degree of agricultural intensity highly similar to treated areas. Thus, even if inflows of reserves impacted banking in non-agricultural regions further away from the border, the locally randomized natural experiment I exploit in this paper is not affected by these shifts.

[54]Simulating the impact of a one-off shock of size x rather than the impact of consecutive small shocks that together amount to x provides for conservative lower-bound estimates of the effect of funding withdrawals. Consecutive small shocks would gradually reduce the basic line as both deposits and the required reserve balance fall simultaneously. Consecutive small funding shocks thus trigger additional increases in the mean marginal interest rates paid for the liquidity needed by the bank to honor its deposit liabilities.

[55]This assumption stacks the cards against finding a strong impact of deposit withdrawals. It may be overly optimistic to consider banks' cash position as perfectly liquid at the time because it also contained exchanges and cheques.

[56]Since this second stress-testing exercise again uses data from balance sheets recorded in September 1919, one further assumption is implicit in my approach. I assume that the cash reserve position on the call date in September 1919 was generally representative of banks' average cash reserve position and, in particular of the position in late spring 1920. Window-dressing on call dates and more extended loan portfolios in late spring 1920 could thus also stack the cards against finding large impacts of funding shocks.

FIG. 2
Mean marginal interest rate on borrowing from the Federal Reserve System: the case of funding shocks

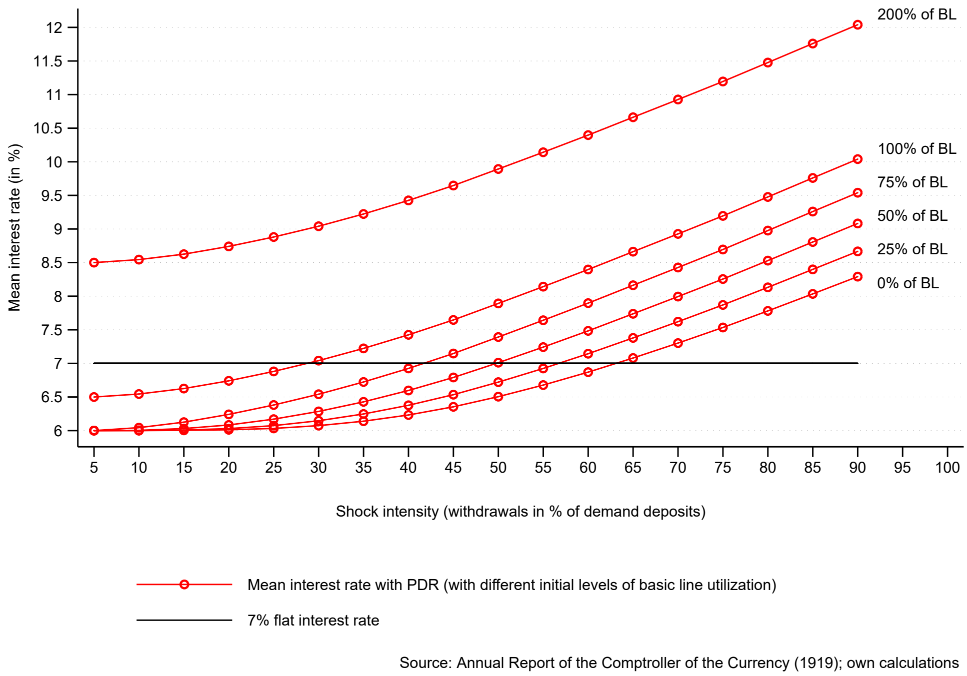


Figure 2 shows the mean marginal interest rate paid by banks in the sample on borrowing from the Federal Reserve Bank after being subject to a one-off funding shock. The graph shows the interest rate as a function of shock intensity (x-axis) and banks' usage of the basic line (BL). The indicated interest rate is faced by the average bank in the sample when it is subject to a funding shock of size x. In the case of LAW, the usage of the basic line does not affect marginal costs as the policy translates into a flat rate increase. The marginal cost of reserves in no-policy districts would correspond to a flat line at 6%. The mean marginal interest rate faced by banks in the PDR districts surpasses the interest rate costs of LAW at different thresholds of shock intensity, depending on the pre-treatment utilization of the basic line.

lization. In contrast to the mean marginal rate, the maximum marginal rate is the rate paid by a given bank on the last bit of borrowing. Depending on prior basic line usage and the size of the funding shock, the maximum marginal rate could rapidly reach levels twice as high as the 7% LAW rate and, in extreme cases, also exceeded 20%.

To explain large treatment effects in PDR districts, it was neither necessary for basic line utilization to be unrealistically skewed, nor essential that basic lines were already fully exhausted when treatment was introduced. Funding shocks represent a plausible additional catalyst of the policy's effect. The dynamics of the recession of 1920-21 may have endogenously reinforced the impact of PDR policy. Moreover, although I have only considered simple static shocks in this subsection, the fall in reserve balances stored with the Federal Reserve Bank subsequent to the decrease in bank deposits meant that basic lines were gradually diminished at a time when demand for Federal Reserve Bank loans increased. For some banks, these dynamics – or the expected impact of these dynamics – may have drastically reduced the incentives to grant new loans. The descriptive evidence in Appendix D.2 discussed above also speaks to the narrative in Wallace (1956). The Tenth Federal Reserve district includes some of most agriculturally intensive regions of the United States (Haines et al., 2016). In all seven states average basic line utilization indeed reached the highest levels during the peak of the crisis in the fourth quarter of 1920 and the first quarter of 1921. The fourth quarter of 1920 and the first quarter of 1921 cover most of the immediate post-harvest season.[57] Therefore, it seems plausible that losses of reserves constituted an important factor pushing banks up the ranks from borrowing below the basic line (or not borrowing at all) into the group of excessive borrowers. Aggregate data on percentage changes in the deposit liabilities of member banks confirm this link as deposits in agricultural counties fell by more than twice as much as in non-agricultural counties at the time (11.1% relative to 4.4.%, c.f. Wallace (1956, p.67)).

Combining my bank-level data with county-level information from the Agricultural Census of 1920, I implement a direct econometric test of the narrative in Wallace (1956). For this purpose, I augment Model 1 to allow PDR treatment effects to vary by the size of the local *ex ante* exposure to agricultural price shocks and by the amount of *ex ante* farm indebtedness. I draw on pre-treatment realizations of the interaction variables to avoid inducing post-treatment bias in my estimation equation:

$$\begin{aligned} Y_{i,t} = {} & \delta(T_i \times Post_t) + \beta(T_i \times Post_t) \times Shock_i + \epsilon(T_i \times Post_t) \times Debt_i \\ & + \kappa(Post_t \times Shock_i) + \lambda(Post_t \times Debt_i) + \mathbf{\Psi}'\mathbf{X}_{i,t} + \phi_b + \gamma_t + u_{i,t} \end{aligned} \tag{2}$$

where $Shock_i$ stands for agricultural price shock exposure. More precisely, $Shock_i$ is an indicator variable flagging banks located in counties where the area share dedicated to major agricultural crops normalized by total county area (as defined in Table 1, measured at year-end 1919) was in the highest quartile of the distribution across PDR counties. $Debt_i$ stands for farm indebtedness. $Debt_i$ is a dummy variable for banks located in counties where farms' debt to value ratio was in the highest quartile of the distribution across PDR counties (again, measured at year-end 1919). All other variables are defined as in Model 1 above.

Table 3 summarizes the results of this test. It provides direct evidence for the narrative in Wallace (1956). The effect of the PDR policy was particularly amplified for banks located in counties that were home to highly indebted farms. The local bandwidths show that banks in highly indebted farming regions reduced lending and leverage by additional 20–25% relative to treated banks located in the lower quartiles of the agricultural indebtedness distribution. *Ex ante* exposure to price shocks also appears to have played a separate – albeit less stable and more imprecisely estimated – role. Altogether, Table 3 suggests that price shock exposure and farm indebtedness constituted two partly complementary channels through which the PDR policy could become more binding.[58] Appendix D.4 provides additional regression results in which I augment Model 1 with only one pair of interactions at a time. Appendix D.4 underscores the

[57] Perhaps counter-intuitively, the data for individual states show basic lines for excessive borrowing reached their peak at the height of the crisis. This peak resulted from the selection of banks into the excessive borrowing category rather than from increases in the individual basic lines of banks.

[58] Table 3 shows that the coefficients on the two interactions become less significant as the regression bandwidths increase. This finding is intuitive as other mechanisms (e.g. differential basic line usage) not fully captured in Model 2 are likely to play an increasingly important role in the full sample.

importance of regional farm indebtedness relative to the *ex ante* exposure to price shocks. It thus confirms the joint estimation results displayed in Table 3.

TABLE 3
PDR mechanism: *ex ante* exposure to agricultural crop price shocks and farm indebtedness

Panel A. Total lending (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.076	-0.091	-0.085	-0.049	-0.034	-0.073
	(0.016)	(0.020)	(0.025)	(0.028)	(0.030)	(0.052)
	[0.017]	[0.023]	[0.028]	[0.031]	[0.036]	[0.073]
Treatment effect ×	0.021	0.092	0.079	0.004	-0.068	-0.203
price shock exposure†	(0.027)	(0.037)	(0.049)	(0.056)	(0.070)	(0.129)
	[0.028]	[0.040]	[0.059]	[0.068]	[0.078]	[0.111]
Treatment effect ×	-0.000	-0.055	-0.264	-0.280	-0.281	-0.241
high farm indebtedness††	(0.048)	(0.073)	(0.097)	(0.194)	(0.195)	(0.218)
	[0.069]	[0.095]	[0.120]	[0.097]	[0.125]	[0.141]
R-squared	0.19	0.25	0.39	0.39	0.38	0.43
Observations	5,179	2,535	1,272	923	662	262

Panel B. Leverage ratio (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.068	-0.072	-0.075	-0.042	-0.036	-0.081
	(0.014)	(0.018)	(0.023)	(0.027)	(0.029)	(0.050)
	[0.016]	[0.021]	[0.026]	[0.029]	[0.034]	[0.069]
Treatment effect ×	0.006	0.032	0.028	-0.021	-0.084	-0.211
price shock exposure†	(0.025)	(0.034)	(0.046)	(0.054)	(0.067)	(0.125)
	[0.026]	[0.038]	[0.055]	[0.065]	[0.077]	[0.119]
Treatment effect ×	0.037	0.001	-0.164	-0.250	-0.253	-0.211
high farm indebtedness††	(0.043)	(0.066)	(0.090)	(0.186)	(0.188)	(0.212)
	[0.059]	[0.080]	[0.105]	[0.092]	[0.121]	[0.135]
R-squared	0.28	0.36	0.46	0.48	0.41	0.50
Observations	5,179	2,535	1,272	923	662	262

Standard errors in parentheses. Clustered standard errors (at bank-level) in squared brackets.
Interaction of mechanism variables with post-treatment time dummy included (not displayed).
All regressions with bank FE, time FE and bank-level controls.

†*Price shock exposure* is an indicator variable flagging banks located in counties where the area share dedicated to major agricultural crops normalized by total county area (as defined in Table 1) was in the highest quartile of the distribution in the PDR estimation sample (measured at year-end 1919).
††*High farm indebtedness* is an indicator variable flagging banks located in counties where farms' debt to value ratio was in the highest quartile of the distribution in the PDR estimation sample (measured at year-end 1919).

IV.C. Risk-shifting and the Perverse Effects of LAW

I draw on recent theoretical insights into risk-shifting incentives and the so called "leverage ratchet effect" (Dell'Ariccia et al., 2014; Turner, 2014; Dell'Ariccia et al., 2017; Admati et al., 2018) to shed light on why LAW induced perverse effects in the second Federal Reserve district, while causing a credit contraction in the Chicago district (and the PDR districts). I argue that the mechanism leading to perverse effects in the East and conventional contraction in the West rested on several building blocks.

First, national banks located in the second district exhibited a significantly higher average *ex ante* reliance on demand deposit funding than their peers in the seventh district.[59] Demand

[59] Standard t-tests based on bank-level data from these two districts before the introduction of

deposits implied reserve requirements ranging from more than twice to more than four times the requirements for time deposits. The implementation of LAW increased the cost of rolling over borrowed reserves by 100 basis points.[60] As a corollary, compared to financial intermediaries in the Western LAW states, banks in the New York district experienced a relatively more pronounced funding cost shock. Given that demand deposits accounted for roughly half of banks' total balance sheet size in my sample (46% of total assets/liabilities as of September 1919), the average funding cost shock was also quantitatively important in absolute terms.

Second, compared to their homologues in district 7, national banks from the second district were less able to pass on higher funding costs to their local borrowers. State usury rates on local bank loans obviated a direct interest rate pass-through to bank assets (Ryan, 1924). Usury rates differed substantially from state to state.[61] Whereas the ceiling amounted to 7% and 8% in the Western states (Indiana, Michigan, Ohio), the maximum rate national banks were allowed to charge on local loans along the Eastern LAW border (New Jersey, New York, Pennsylvania) was only 6%. In other words, only in the East did the rate hike to 7% cause the cost of borrowing from the Federal Reserve System to exceed the maximum interest rate national banks could charge on local loans – and it did so by a hefty margin of 100 basis points. Although it was possible for banks to circumvent usury rates by lending on commercial paper and to the call market attached to the country's Stock Exchanges, national banks had a strong bias towards their local customer base (see Appendix D.5 for more details). The discussions during the hearings of the Joint Commission of Agricultural Inquiry (1922b) provide first-hand narrative evidence on the implications of limited interest rate pass-through. The spread between the New York state usury rate and the Federal Reserve Bank's lending rate pre-occupied members of Congress at the time who feared that banks' profit margins had been depressed (Joint Commission of Agricultural Inquiry, 1922b, p.507; p.625). Federal Reserve officials confirmed that "it was a mistake to say, except for a very short period, possibly, when the rate was advanced, that it had the effect of generally raising interest rates in the district" (Joint Commission of Agricultural Inquiry, 1922b, p.507).

Third, I show that before 1 June 1920 state usury rates represented a binding ceiling for rates on local loans in the East, but not in the West. For this purpose, I collected bank-level interest rate data from Indiana (district 7) and New Jersey (district 2). I describe my data sources for the bank-level interest rates in Appendix D.5. Figures 3 and 4 below display the universe of interest rates on local loans charged by national banks in Indiana and New Jersey in 1920. The horizontal axis reflects the date of the examiner report corresponding to a given bank's interest rate. The dashed red lines represent the respective usury rate ceilings (6% for New Jersey national banks

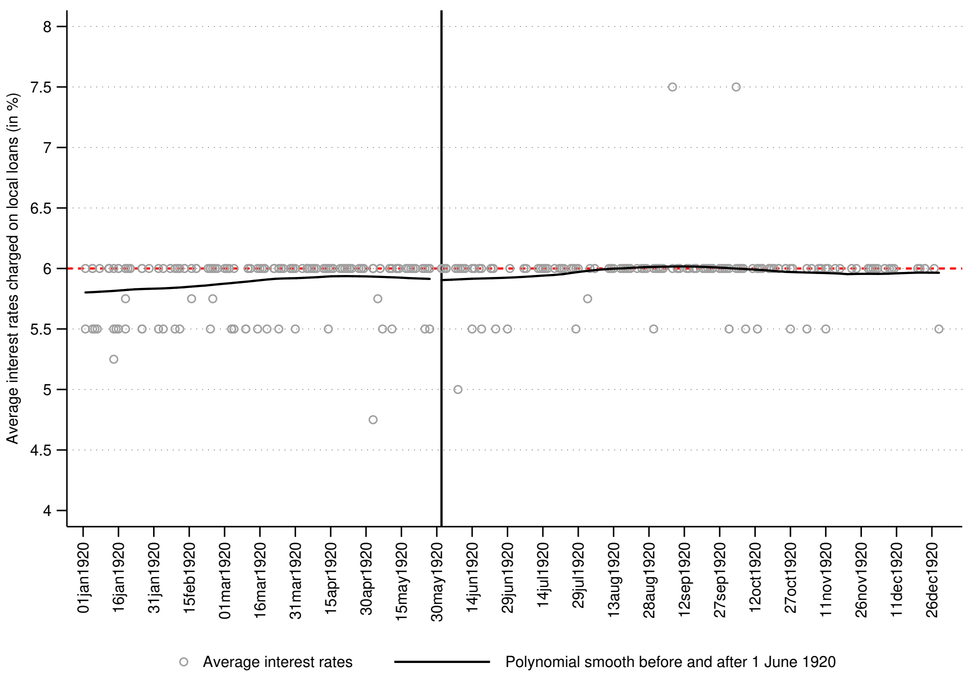
LAW reject the null of equality of demand deposit to capital ratios with more than 95% confidence and the one-sided null with 99% confidence (t-statistic of 2.20). The respective mean values are 4.41 (standard error of 0.10) for district 2 and 4.03 (standard error of 0.12) for district 7. To conduct these tests, I use bank-level data from September 1919. September 1919 represents the last call date before the introduction of LAW policy for which a break-down of deposits into demand and time deposits is available.

[60] Reserve requirements for demand deposits were 7%, 10% and 13% in the countryside, reserve cities and central reserve cities respectively. Reserve requirements for time deposits amounted to 3%.

[61] The differences in state usury rates do not invalidate my research design which relies on a comparison of national banks with identical usury rates in small bandwidths around the district borders. See also my discussion and Table 4 in Appendix A.2.

and 8% for national banks located in Indiana). Figure 3 illustrates that the usury rate ceiling was binding for local loans in New Jersey before and after 1 June 1920. The average national bank in New Jersey charged 5.88% before 1 June 1920 and 5.97% in the months following 1 June 1920. In contrast to the distribution of rates prevailing in New Jersey, the data for Indiana banks in Figure 4 show that the usury rate ceiling of 8% was not binding for local interest rates before, nor after 1 June 1920. On average, national banks located in Indiana charged average interest rates slightly below 7% (6.78%) before 1 June 1920. After this date, average rates on local loans increased to 7.14%.

FIG. 3
Interest rate on local loans charged by national banks in New Jersey in 1920



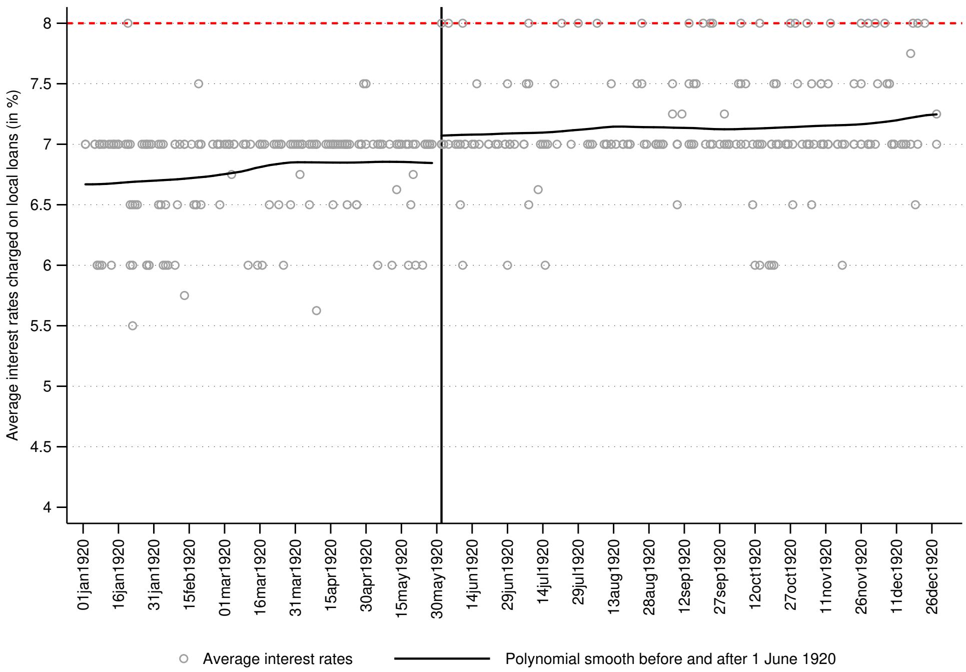
Source: National Bank Examiner Reports for 1920

Figure 3 shows bank-level interest rates on local loans (i.e. loans to local customers) charged by national banks located in New Jersey. Each grey circle stands for one bank. The horizontal red dashed line represents the usury rate ceiling. The black line constitutes a polynomial smooth over time (degree zero and Epanechnikov kernel function). The two outlier observations in fall 1920 (rates of 7.5%) were flagged as illegal in the corresponding national bank examiner reports.

Together, higher *ex ante* reliance on deposit funding, heterogeneous, unequally binding usury rates and, hence, different degrees of interest rate pass-through should have caused the rate hike to impact the financial situation of New York district banks more negatively than that of banks located in district 7. Albeit facing the same uniform interest rate hike, national banks in district 2 likely saw their "skin in the game" decreased by more. Consequently, New York district banks faced stronger incentives to engage in risk-shifting behavior and to resist deleveraging than their Chicago district peers.[62]

[62] Liability rules were uniform for national banks across the entire U.S. territory (i.e. double liability). For more details on shareholder liability and the evolution of legal rules over time, see

FIG. 4
Interest rate on local loans charged by national banks in Indiana in 1920



Source: National Bank Examiner Reports for 1920

Figure 4 shows bank-level interest rates on local loans (i.e. loans to local customers) charged by national banks located in Indiana. Each grey circle stands for one bank. The horizontal red dashed line represents the usury rate ceiling. The black line constitutes a polynomial smooth over time (degree zero and Epanechnikov kernel function).

Finally, the existing literature on U.S. interbank market structure also suggests that national banks in the second district benefited from their relative proximity to New York City, the country's financial center. Due to the hierarchical pyramid structure of the U.S. interbank market (Mitchener and Richardson, 2013b, 2019; Anderson et al., 2019), New York City banks harbored large parts of financial intermediaries' excess reserves and loanable funds. Pre-treatment data show that national banks in district 2 entertained more numerous correspondent links to New York City banks than their counterparts in district 7.[63] These links should have been instrumental in allowing treated banks to maintain their leverage, partly by replacing loans from the Federal Reserve System with loans from New York City correspondents. Contemporary sources confirm the crucial role of correspondent banking with New York City.[64] The pub-

Mitchener and Richardson (2013a), Bodenhorn (2015) and Anderson et al. (2020).

[63]Standard t-tests based on bank-level data from these two districts before the introduction of LAW clearly reject the null of equality (t-statistic of 7.02). The respective mean values are 1.36 (standard error of 0.03) for district 2 and 0.97 (standard error of 0.04) for district 7. To conduct these tests, I use bank-level data on correspondent links from January 1920.

[64]The fact that New York City banks expanded interbank loans to banks elsewhere cannot alone account for the positive treatment effects of LAW. On the one hand, New York City banks are only included in my full sample estimation results starting from the third bracket (<75km) as the LAW border closest to New York City is located at a distance of 66 kilometers. Yet, the results displayed in Panel A of Table 2 show that the perverse treatment effects persist for smaller bandwidths (<50km and <25km). On the other hand, my split-state specification for New Jersey (see Appendix C.3)

lished transcripts of the hearings before the Joint Commission of Agricultural Inquiry (1922b) contain a painstakingly detailed, 400-pages long testimony by Benjamin Strong, then Governor of the Federal Reserve Bank of New York. Strong almost spent an entire week in front of the Joint Commission of Congress (from 8 to 11 August 1921), elaborating *inter alia* on banking developments following the LAW rate hike in 1920 (Joint Commission of Agricultural Inquiry, 1922b, p.643–648):

> [T]here has been a marked tendency for loans to shift from other parts of the country to New York, the financial center. [...] Much of this demand has been upon banks of large resources in New York City doing a nation-wide business. Not only have they given accommodation in large amounts to banks and corporations in other parts of the country, but the balances which banks ordinarily keep with them have been drawn down. [...] Whether by withdrawal of balances or in the reduction of loans placed for the account of out-of-town banks, in either case, movement of funds away from New York has resulted. [...] [B]anks are paying off their loans with [their Federal Reserve Banks] and are transferring those loans to their New York correspondents.

To test the risk-shifting narrative econometrically, I augment Model 1 to allow LAW treatment effects to vary by the size of banks' *ex ante* reliance on deposit funding and their *ex ante* number of correspondents in New York City:

$$\begin{aligned} Y_{i,t} = \delta(T_i \times Post_t) + \beta(T_i \times Post_t) \times High\ deposit_i + \epsilon(T_i \times Post_t) \times NYC_i \\ + \kappa(Post_t \times High\ deposit_i) + \lambda(Post_t \times NYC_i) + \mathbf{\Psi}'\mathbf{X}_{i,t} + \phi_b + \gamma_t + u_{i,t} \end{aligned} \tag{3}$$

where $High\ deposit_i$ is a dummy for high deposit banks. It represents an indicator variable flagging banks whose demand deposit to capital ratio was in the highest quartile of the distribution in the LAW estimation sample (measured in September 1919). NYC_i stands for New York City correspondents and captures the number of a given bank's direct correspondent links to the country's financial center as of January 1920. All other variables are defined as in Model 1 above.

Table 4 summarizes the estimation results. It provides strong evidence for the risk-shifting hypothesis described above. The coefficients on both interaction terms are statistically and economically significant and positive. In reaction to LAW, banks showing high *ex ante* reliance on deposit funding and banks which entertained more pre-treatment correspondent links with New York City expanded their loan supply and leverage relative to control group banks. Controlling for these interactions also causes the pure treatment effect δ to switch sign and become negative throughout. Starting from the third bracket (<75km), δ is not only negative but highly statistically significant. Thus, Table 4 shows that the perverse treatment effect of LAW can be convincingly accounted for by risk-shifting motives. The ability to raise additional funds in New York City allowed treated banks in district 2 to resist deleveraging and gamble for survival. Appendix D.6 reports three sets of additional results bearing on the mechanism driving the positive treatment effect of LAW. Table 24 in Appendix D.6 conveys the results obtained when

also yields highly significant positive treatment effects even though the estimation sample does not include any New York (City) bank.

Model 1 is augmented with only one pair of interactions at a time, confirming the individual significance of the two interaction terms. Table 25 and Table 26 in Appendix D.6 provide separate estimation results for the Eastern and Western LAW borders respectively. Following the inclusion of interaction terms, the perverse treatment effects in the East entirely disappear in the split sample estimates (c.f. Table 25), whereas the economic and statistical significance of the negative treatment effects in the West increase (c.f. Table 26). Consistent with the narrative in this subsection, the coefficients on the interaction terms exhibit lower economic and statistical significance in the Western border sample than in the Eastern border sample.

TABLE 4
LAW mechanism: *ex ante* reliance on deposit funding and correspondent links to New York City

Panel A. Total lending (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.053	-0.040	-0.046	-0.049	-0.042	-0.041
	(0.014)	(0.015)	(0.017)	(0.020)	(0.024)	(0.038)
	[0.017]	[0.019]	[0.022]	[0.026]	[0.030]	[0.048]
Treatment effect ×	0.022	0.031	0.058	0.064	0.073	0.093
high deposit bank†	(0.016)	(0.016)	(0.019)	(0.021)	(0.025)	(0.041)
	[0.020]	[0.021]	[0.022]	[0.024]	[0.029]	[0.040]
Treatment effect ×	0.043	0.048	0.062	0.068	0.068	0.055
NYC correspondents††	(0.010)	(0.010)	(0.012)	(0.013)	(0.016)	(0.028)
	[0.012]	[0.014]	[0.014]	[0.016]	[0.018]	[0.030]
R-squared	0.25	0.27	0.31	0.31	0.36	0.31
Observations	10,224	7,746	4,346	3,362	2,086	1,019

Panel B. Leverage ratio (ln)

	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.046	-0.049	-0.051	-0.052	-0.033	-0.023
	(0.012)	(0.013)	(0.016)	(0.018)	(0.023)	(0.035)
	[0.016]	[0.017]	[0.020]	[0.024]	[0.030]	[0.050]
Treatment effect ×	-0.001	0.009	0.035	0.040	0.063	0.096
high deposit bank†	(0.014)	(0.014)	(0.017)	(0.020)	(0.024)	(0.038)
	[0.018]	[0.020]	[0.022]	[0.024]	[0.028]	[0.043]
Treatment effect ×	0.051	0.061	0.067	0.073	0.069	0.057
NYC correspondents††	(0.009)	(0.009)	(0.011)	(0.012)	(0.015)	(0.026)
	[0.011]	[0.012]	[0.013]	[0.015]	[0.017]	[0.031]
R-squared	0.30	0.33	0.35	0.34	0.42	0.30
Observations	10,224	7,746	4,346	3,362	2,086	1,019

Standard errors in parentheses. Clustered standard errors (at bank-level) in squared brackets.
Interaction of mechanism variables with post-treatment time dummy included (not displayed).
All regressions with bank FE, time FE and bank-level controls.

†*High deposit bank* is an indicator variable flagging banks whose demand deposit to capital ratio was in the highest quartile of the distribution in the LAW estimation sample (measured in September 1919).
††*NYC correspondents* is a count variable capturing the number of a given bank's New York City correspondents (measured in January 1920).

A final question is why national banks in PDR districts did not exhibit a similar resistance to deleveraging. After all, over-extended member banks should have faced large funding cost shocks following the introduction of targeted rate action. Three reasons can explain why the contractionary bank-lending channel outweighed risk-shifting motives in the case of PDR banks. First, the evidence in the previous subsection suggests that the contractionary effect of the bank-lending channel was (much) stronger for over-leveraged banks in PDR districts than for their peers subject to LAW policy. Since price discrimination was specifically targeted at over-extended banks, the contractionary effect of the bank-lending channel was largest for precisely those firms which had the highest *ex ante* incentive to gamble for survival. Second, as discussed above, in contrast to LAW regions, PDR districts were more agriculturally oriented and suffered heavy deposit withdrawals while targeted rate action was in force.[65] Deposit withdrawals forced banks to deleverage further and, by depressing deposit to capital ratios, withdrawals also increased shareholders' relative "skin in the game". Third, compared to banks in LAW districts, financial intermediaries located in the St Louis and Atlanta districts entertained significantly fewer correspondent links to New York City.[66]

V. Conclusion

In this paper, I estimate the comparative causal effects of monetary policy leaning against the wind (LAW) and targeted monetary policy on bank-level credit by drawing on a single natural experiment from economic history. In 1920, when U.S. monetary policy was still decentralized, four Federal Reserve Banks implemented a conventional rate hike to address financial stability concerns. Another four Reserve Banks resorted to targeted rate action with the same goal. Using sharp geographic regression discontinuities, I identify the treatment effects off the resulting policy borders with the remaining four Federal Reserve districts which did not change their policy stance. I show that targeted monetary policy caused both bank-level lending and leverage to fall significantly, whereas LAW only had weak and, in some areas, even perverse effects on these bank-level outcomes. The targeted tool reined in over-extended banks more effectively than LAW because it allowed Federal Reserve Banks to use price discrimination when lending to highly leveraged counterparties.

This paper contributes to the ongoing debate on the choice of optimal financial stability policies and adds new insights to the existing empirical literature on the effects of financial stability policies. First, fixing time, environment and average treatment intensity, I show that targeted monetary policy can be more effective than conventional monetary policy in taming bank credit. Second, my findings suggest that financial stability policies can have severe counterproductive effects if risk-shifting motives encourage banks to resist deleveraging. Third, this paper also complements recent economic history contributions relevant to my quasi-experimental setting. It showcases the Federal Reserve System's early use of sophisticated financial stability tools and highlights that the various Federal Reserve Banks implemented different policies with quite heterogeneous effects on bank credit during the pronounced boom and bust phase in the aftermath of World War I. My findings are consistent with the claim that Federal Reserve policies amplified the short-run real effects of the sharp recession in 1920-21.

[65]Standard t-tests based on county-level data from the Agricultural Census of 1920 provide strong evidence against the null of equality for the number of farms per inhabitant (t-statistic of 42.76, mean values of 0.06 and 0.11, standard errors of 0.00 for both policy regions) and for the share of farm area in total county area (t-statistic of 10.97, mean values of 0.68 and 0.76, again standard errors of 0.00 for both policy regions).

[66]Standard t-tests based on bank-level data recorded before treatment clearly reject the null of equality (t-statistic of 7.84). The respective mean values are 1.05 (standard error of 0.02) for district 6 and 8, and 1.21 (standard error of 0.01) for districts 2 and 7. To conduct these tests, I use bank-level data on correspondent links from January 1920.

The results presented in this paper underscore the importance of economic history for modern policy-making in several ways. First, I show that history can provide a unique laboratory to run true "horse races" between different macroeconomic policy options. History helps us to gauge the comparative causal effects of policies in ways which have proven elusive in modern day settings. Second, my results highlight the importance of context, design and financial infrastructure for the effectiveness of financial stability policies. This paper serves as a reminder that when LAW and its alternatives are activated, they never enter an economic, financial and political vacuum. The impact of the very same policies can vary substantially across time and space. Third, this paper has been written at a time when central banks around the world began to deviate from the dogma of uniform policy rates for all their counterparties.[67] Looking back can be a powerful tool to enlarge the breadth of current policy debates (Eichengreen, 2012), not least because the design of the progressive discount rate scheme of 1920 comes surprisingly close to modern proposals for how to conceive financial stability policies (Stein, 2012). My paper shows that the Federal Reserve System effectively used policies involving customized price discrimination to regulate bank credit already more than a century ago.

[67] Since October 2019, the European Central Bank (ECB) charges average deposit facility rates that vary depending on the size of a counterparty's current account holdings with the central bank ("two-tier system for remunerating excess liquidity holdings"). The ECB also charges different lending rates for its targeted longer-term refinancing operations ("TLTRO"), where the level of rate charged depends on whether the borrowing bank fulfills specific lending targets. In contrast to the PDR, the ECB's policies aim at boosting bank lending, rather than curtailing it.

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