FINANCIAL STABILITY POLICIES AND BANK LENDING: QUASI-EXPERIMENTAL EVIDENCE FROM FEDERAL RESERVE INTERVENTIONS IN 1920-21*

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Abstract

I estimate the comparative causal effects of monetary policy "leaning against the wind" (LAW) and macroprudential policy on bank-level lending and leverage by drawing on a single natural experiment. 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 macroprudential policy with the same goal. Using sharp geographic regression discontinuities, I exploit the resulting policy borders with the remaining four Federal Reserve districts which did not change policy stance. Macroprudential policy caused both bank-level lending and leverage to fall significantly (by 11%-14%), whereas LAW had only weak and, in some areas, even perverse effects on these bank-level outcomes. I show that the macroprudential 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. The perverse effects of the rate hike in some areas ensued because LAW lifted a pre-existing credit supply friction by incentivizing regulatory arbitrage. My results highlight the importance of context, design and financial infrastructure for the effectiveness of financial stability policies.

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monetary policy, macroprudential policy, leaning against the wind, credit boom, bank lending, leverage, Federal Reserve System, progressive discount rate, recession of 1920/1921

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I. INTRODUCTION

Credit booms can amplify business cycle fluctuations by fueling excessive credit growth for local conditions (Rey, 2013; Borio, 2014). When they "go bust", credit booms tend to end in financial crises which inflict large costs on creditors, tax payers and the real economy (Cerra and Saxena, 2008; Schularick and Taylor, 2012; 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). Reignited by the Great Financial Crisis of 2008-09, the question which precise measure should be deployed to rein in financial excesses, however, remains subject to an ongoing debate (Gambacorta and Signoretti, 2014; IMF, 2015; Svensson, 2016, 2017; Gourio et al., 2018; Schularick et al., 2020). Should central banks "lean against the wind"[1] (LAW) using their conventional interest rate or are more targeted macroprudential tools[2] better suited to tame bank lending?

Policy endogeneity, regulatory arbitrage, and the fact that the two policy options are rarely employed simultaneously explain why empirical work on their relative effectiveness has proven elusive so far. The present paper addresses this gap in the literature. I estimate the comparative causal effects of monetary policy leaning against the wind and macroprudential policy on bank-level lending and leverage by exploiting a single natural experiment. To identify the causal effect 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 late spring 1920, four Federal Reserve Banks (Boston, Chicago, Minneapolis and New York) leant against the wind by hiking their interest rate from 6% to 7% to address financial stability concerns. Four other Reserve Banks (Atlanta, Dallas, Kansas City and St Louis) used a macroprudential tool to safeguard financial stability, while keeping their baseline policy rate constant at 6%. Both financial stability policies were implemented in late May/early June 1920 and they remained in place until late June/early July 1921. The remaining four districts (Cleveland, Philadelphia, Richmond and San Francisco) never changed their policy stance and simply maintained the prevailing 6% rate (Figure I).

(Figure I here)

My 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.[3] Within these bands, bank-level characteristics and local economic conditions exhibit statistically identical 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

[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. More precisely, LAW is defined as "monetary policy that is somewhat tighter (i.e. with a somewhat higher policy interest rate) than what is consistent with flexible inflation targeting without taking any effects on financial stability into account" (Svensson, 2017, p.193).

[2]Macroprudential policies represent targeted tools designed to address the build-up of systemic risks in the financial system or some of its sub-sectors (e.g. loan-to-value ratios, reserve requirements and countercyclical buffers).

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

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 can 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 Office of the Comptroller of the Currency (1920, 1921a,b, 1922) reports, 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 II) which provides borders for all relevant policy combinations (including Placebo borders with identical policies). Controlling for time and bank fixed effects, I find that macroprudential policy caused both lending and leverage to fall significantly relative to districts without a policy change. Treatment led to a reduction in both outcome variables by between 11% and 14%. The conventional interest rate hike had a differential impact depending on which borders are considered. In the West (district 7, Chicago), the policy marginally eased credit pressures by around -1%, but the coefficient is not statistically different from zero. In contrast, in the second district (New York), LAW had a 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.

(Figure II here)

These results are robust to a wide range of falsification checks. Apart from changes in the specifications, the computation of standard errors and the inclusion of control variables, I conduct a series of Placebo tests to verify that treatment effects do not exist before treatment began and do not persist after treatment ended. Furthermore, I show that there are no systematic discontinuities across district borders with identical policies. Building on Richardson and Troost (2009), I also limit my sample to bank-level data from states which were split by Federal Reserve district borders to show that my estimates are not merely driven by other (economic) policy discontinuities across state borders unrelated to LAW or macroprudential policy. Finally, the split border specification enables me to implement a Placebo test drawing on state-chartered non-member banks.[4] Since these banks could not borrow from the Federal Reserve System, they should not have been affected by the policies to the same degree.[5] I find that the policies had no statistically significant treatment

[4]All national banks automatically became member banks of the Federal Reserve System when the System was founded in 1914. State-chartered banks could opt in and become members on a voluntary basis.

[5]Anderson et al. (2018) show that state-chartered banks partly circumvented this restriction by borrowing via their correspondent national banks. Overall, however, state-chartered banks' access to discount window finance was likely significantly curtailed relative to member banks.

effects on non-member banks.

To identify the mechanisms driving my empirical results, I proceed in two steps. First, I show that the specific macroprudential policy used in 1920-21 equipped Federal Reserve Banks with a stronger and more targeted tool to exert pressure on over-leveraged counterparties than LAW. In 1920, monetary policy transmission functioned through the so-called "reserves channel" (Carlson and Duygan-Bump, 2018).[6] The primary motive for borrowing from a Federal Reserve Bank was to make good on reserve requirements: member banks had to hold reserves against their deposit liabilities and all reserves needed to be stored with the Reserve Banks. When a commercial bank granted a new loan to a customer, it usually created a deposit for the borrower. This increase in deposits meant a higher absolute reserve requirement and implied borrowing from the Federal Reserve Bank to abide by the new requirement.[7] Both LAW and the macroprudential tool increased the marginal cost of reserves and thus acted upon banks' incentive to grant new loans. The rate hike translated into a 100 basis point flat increase in the marginal cost, irrespective of the amount a member bank wanted to borrow. In contrast to LAW, the macroprudential tool - officially named the "progressive discount rate" (PDR) - 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. The maximum line was calculated 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 became it was charged for additional loans from its Reserve Bank. The rate increased by 50 basis points for every 25% a member bank borrowed in excess of its basic line. Thus, the macroprudential tool endowed Federal Reserve Banks with the power to exercise price discrimination against banks they regarded as over-leveraged.

The design of the PDR clarifies why macroprudential policy was more successful in taming banks' credit expansion, but it does not explain the perverse treatment effect of LAW in the second district. Hence, in a second step, I investigate why the New York district experienced higher credit growth and leverage in response to the conventional interest rate hike. One plausible explanation relates to the differences in prevailing state usury rates along the Eastern and Western LAW borders (Ryan, 1924). The maximum legal rate was 6% in the East (districts 2 and 3) and 8% in the West (districts 4 and 7). When usury rates are binding, they can introduce a credit friction preventing banks from adequately pricing riskier lending: higher risk projects cannot get funding, although demand for more loans at increased rates exists (Temin and Voth, 2008). Usury rates thus accelerate the advent of quantity rationing in credit markets as described by Stiglitz and Weiss (1981). I collected bank-level interest rate data from individual bank examiner reports which show that the 6% usury rate on local loans was highly binding for banks located in district 2 before the LAW policy was introduced, whereas banks in district 7 charged rates considerably below the maximum ceiling of 8% (but on average 80 basis points above 6%). With binding usury rates, the introduction of LAW in district 2 incentivized banks to seek alternatives to local loans which were not subject to usury rates. I show that banks reacted to treatment by increasing their call loans to the New York City stock exchange and by purchasing outside commercial paper. Channeling funds into these alternative

[6]The Federal Reserve Banks' credit facilities constituted so called "standing facilities" which relied on banks to initiate the interaction with the central bank. Before the mid-1920s, Federal Reserve Banks did not engage in open-market operations to make their policy rates effective.

[7]In 1920, borrowing from the Federal Reserve System could take two different forms. First, it could mean the *rediscount* of bills of exchange (strictly speaking, the sale of bills at a discount). Second, borrowing could take the form of collateralized loans (*advances*, also called *bills payable*).

investments allowed banks to charge higher average interest rates and to increase overall outstanding credit volumes.

The paper is organized as follows. In the next subsection, I discuss my contributions to the different strands of literature related to this study. Section II. describes my primary sources and presents the new data sets compiled for this paper. Section III. discusses experiment validity based on the historical background of this study and explains my identification strategy in detail. Section IV. provides the empirical results and robustness checks. Section V. investigates the channels of policy transmission. Section VI. concludes. A detailed online appendix complements the paper.[8]

I.A. Contributions to the Literature

This study relates to several literatures. First, I contribute to the current debate on the choice of optimal financial stability policies (Gambacorta and Signoretti, 2014; Gourio et al., 2018; Svensson, 2016, 2017; Martinez-Miera and Repullo, 2019; Bergant et al., 2020; Schularick et al., 2020). Existing theoretical studies reach opposing conclusions on the relative merits of LAW and its macroprudential alternatives.[9] Whereas LAW famously "gets into all cracks" of both regulated and shadow financial sectors (Stein, 2013), macroprudential tools are less likely to cause collateral damage[10] but they are more prone to regulatory arbitrage and more difficult to deploy[11] (Smets, 2014). To my knowledge, my paper is the first to stage a true empirical "horse race" between the two types of policies while fixing time and environment. Running a similar test is hardly possible with modern data because most policy-makers consider LAW and macroprudential policies as substitutes rather than complements.[12] Moreover, I exploit conditions of swift macroprudential policy deployment and limited arbitrage under which there is no clear *a priori* case for LAW. This special setting allows me to disentangle other caveats against LAW, which are independent of the greater collateral damage it may cause. I show not only that macroprudential policy can be more effective than conventional monetary policy in taming bank credit, but also that LAW can have severe counterproductive effects. My results highlight the importance of context, design and financial infrastructure for the effectiveness of financial stability policies.

Second, my paper contributes to the existing empirical literature on the effects of financial stability policies in two distinct ways. On the one hand, my results relate to earlier studies on the mechanics of regulatory arbitrage (Aiyar et al., 2014; Reinhardt and Sowerbutts, 2017; Forbes, 2019; Araujo et al., 2020).[13] I show that pre-existing credit frictions

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

[9] Most recent contributions use DSGE models (Gambacorta and Signoretti, 2014; Gourio et al., 2018) or static cost-benefit analysis (Svensson, 2016, 2017) to model the impact of LAW. These two approaches cannot be easily mapped into each other *and* the authors reach different conclusions. While the former suggest LAW can be a first-best policy response in some scenarios, the latter argues in favor of more targeted prudential policies because the costs of "leaning against the wind" almost always outweigh its benefits. On the empirical side, Schularick et al. (2020) draw on long-run historical data to argue that LAW policies during credit and asset price booms are more likely to trigger crises than to prevent them. Bergant et al. (2020) show that macroprudential policies are more effective than capital controls when it comes to dampening global financial shocks.

[10] Monetary policy tightenings have costs in terms of higher inflation volatility, foregone output and employment. Incorporating financial stability into the monetary policy reaction function can therefore lead to trade-offs between price stability and financial stability with direct macroeconomic consequences (IMF, 2015). By weakening the economy, LAW may even become counterproductive. The economy faces future negative shocks in a more fragile state, potentially implying higher costs during future crises than without the preemptive rate increase (Svensson, 2017).

[11] Macroprudential tools are more difficult to adjust and deploy than conventional monetary policy because they often require legal changes and direct political voting/backing.

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

[13] Aiyar et al. (2014) show that time-varying, bank-specific capital requirements were effective in harnessing credit growth in

can lead to dynamics that obviate the dampening effect of LAW on bank credit by incentivizing alternative lending. On the other hand, my research design addresses an often overlooked identification challenge for recent empirical work on the impact of financial stability policies (e.g. Barroso et al. (2017); Camors et al. (2017); Jiménez et al. (2017); Alam et al. (2019)).[14] Treated credit institutions may try to circumvent policy-induced higher refinancing costs by borrowing from control group banks. On the "benign" side, this reaction can bias treatment effects towards zero, turning available estimates into lower bound effects.[15] Regulatory arbitrage, however, triggers an increase in the (interbank) loan portfolio of banks in the control group. Studies which use other financial intermediaries as control groups may therefore suffer from violations of the stable unit treatment value assumption (SUTVA). As a corollary, treatment coefficients for total lending outcomes may be biased upwards if regulatory arbitrage causes lending by control group banks to increase by more than for treated banks. For example, an upward bias could materialize if treated banks only lend out a fraction of the funds they receive by borrowing from untreated peers, while holding the remainder as liquid reserves.

The unique setting of this paper works as a first line of defense against this form of SUTVA violation. As Jaremski and Wheelock (2017) argue, the very design of the Federal Reserve districts aimed at ringfencing interbank networks into separate districts. To prove this point, 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). The network data allows me to check whether the interbank connections of treated banks in my sample could have induced SUTVA violations in my local discontinuity models. Consistent with the pyramid structure of the U.S. interbank network structure at the time (Mitchener and Richardson, 2013; Anderson et al., 2018; Mitchener and Richardson, 2019; Jaremski and Wheelock, 2020b), I find that links to local banks across the nearest Federal Reserve district border were practically non-existent. This constellation makes an upward bias in my local discontinuity regressions highly unlikely.[16]

Third, my paper provides new insights regarding the design of effective financial stability policies. Ultimately, the relative effectiveness of LAW and alternative policies rests on their successful transmission to the financial sector.[17] The transmission mechanism of LAW and macroprudential policy in my setting closely resembles an idea put forward in a seminal paper by Stein (2012). Stein (2012) proposes designing financial stability policies based on the introduction of a system of cap-and-trade permits to regulate banks' money creation. This system can be implemented by making use of

the United Kingdom but also led to regulatory arbitrage via non-regulated banks. Reinhardt and Sowerbutts (2017) document differential regulatory arbitrage behavior in a large cross-country panel, depending on the type of macroprudential tool used. Tighter domestic capital regulation induces domestic non-banks to borrow from foreign banks, whereas stricter lending standards have no such effects. Forbes (2019) and Araujo et al. (2020) conduct meta-analyses revealing evidence of leakages and spill-overs in available estimates.

[14]Jiménez et al. (2017) find that dynamic provisioning proved an effective policy tool to tame over-leveraged banks in the Spanish case. A long series of hitherto unpublished working papers (for example, c.f. Barroso et al. (2017), Camors et al. (2017) and Reinhardt and Sowerbutts (2017)) provide similar evidence using credit register data from a variety of countries. A more exhaustive list of relevant contributions can be found in the conference proceedings of the BIS CCA CGDFS Working Group closing conference on "The impact of macroprudential policies: an empirical analysis using credit registry data" (June 2016). Alam et al. (2019) use an IMF database of macroprudential policies to highlight the nonlinear effects of LTV tightenings.

[15]Interference might also occur among treated units if the intensity of treatment varies across banks, as e.g. under the PDR. In this case, interbank borrowing likely triggers a downward bias in the coefficient, stacking the cards against finding a significant treatment effect.

[16]Most banks entertained correspondents in Eastern financial centers (Chicago, Cleveland, New York, Philadelphia) which were generally located further away from the district borders. Although correspondent links to major financial centers may have helped treated banks to circumvent the policies, this arbitraging behavior likely turns my estimates into lower bound effects: it does not artificially blow up the total lending portfolio of the very local control group banks in my sample but only affects control group banks 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 very "sticky" at the time.

[17]A large variety of potential transmission channels has been explored in the literature and a detailed discussion is beyond the scope of this paper (c.f. IMF (2015) for a survey).

existing reserve requirements for short-term liabilities.[18] In my historical setting, LAW and macroprudential policy were both directly transmitted to bank balance sheets because they increased the marginal cost of reserves. My contribution thus closely corresponds to a tailored empirical test of two different implementations of Stein's (2012) proposal.[19] In addition, the design of the progressive discount rate in 1920-21 caused the marginal cost of reserves to become a function of individual banks' leverage. My findings reveal that this form of customized price discrimination against central bank counterparties was highly effective in reducing bank-level leverage and credit growth, whereas LAW was not. Central bank price discrimination represents one of the elephants in the discussion room where LAW and its alternatives are currently debated. The results in 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.[20] This conclusion particularly applies to emerging market economies, where reserve requirements remain an important lever of monetary policy (Cordella et al., 2014).

Fourth, this paper adds new complementary insights to recent economic history contributions relevant to my quasi-experimental setting. 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. My findings showcase the Federal Reserve System's early use of sophisticated macroprudential tools, in line with the System's pre-occupation with the quality and quantity of bank credit at the time (Rotemberg, 2013). Following an earlier contribution by Wallace (1956), Tallman and White (2020) argue that interdistrict borrowing between Federal Reserve Banks allowed districts to re-allocate credit capacity across regions, thereby preventing a banking panic during the recession of 1920-21. Tallman and White (2020) categorize expansionary and hawkish Federal Reserve Banks according to the total amount of liquidity provided to member banks in each district. Intriguingly, four out of the five most expansionary districts in their aggregate analysis had implemented the progressive discount rate in 1920. Together, our contributions thus suggest that an ample liquidity provision in the aggregate, coupled with the use of the PDR targeting over-leveraged banks, constituted a successful policy mix in 1920-21.

In other work related to my study, Carlin and Mann (2019) draw on county-level data from Illinois to explore the real effects of the Federal Reserve System's interest rate policy during the recession of 1920-21. Their paper suggests that higher interest rates may have had short-term costs causing agricultural hardship, but also long-term benefits in terms of lowering debt-to-output levels until the Great Depression. These insights shed valuable light on the short-run vs. long-term trade-offs of financial stability policies. In contrast to Carlin and Mann (2019), 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 highlights that the various Federal Reserve Banks implemented different policies with quite heterogeneous effects on bank credit.[21] I explain the rationale underlying the different policy choices and I provide

[18]Required reserves represent the permits and the cost of permits is dictated by the central bank policy rate (i.e. the marginal cost of reserves).

[19]Stein (2012) builds his theoretical case for LAW on the existence of a market failure: financial institutions over-issue short-term debt because they do not take into account the negative externalities of asset fire sales in distressed times (c.f. also Gorton and Ordoñez (2014) and Oehmke (2014)).

[20]Rules-based price discrimination was 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 interest rate tierings to enhance the transmission of monetary policy and to limit the negative side effects of negative interest rates.

[21]In fact, Illinois represents a state split between district 7 (Chicago) and district 8 (St Louis). The two districts implemented

detailed evidence on their transmission mechanisms. Moreover, I show that (identically sized) interest rate increases led to very different outcomes depending on the district one examines. My paper thus raises the question whether the interesting findings regarding the real costs of policies provided by Carlin and Mann (2019) also apply to areas where LAW appears to have had perverse effects on bank credit.

Finally, I extend the methodology of seminal papers by Richardson and Troost (2009) and Jalil (2014) who exploit historical 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 and macroprudential 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 depression (Gorton and Metrick, 2013; Anbil, 2018).[22] I also exploit several so far 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, to the best of my knowledge, I am the first to provide actual quantitative evidence backing the crucial non-interference assumption based on interbank network data. This assumption needs to hold to allow for the identification of unbiased effects of Federal Reserve policies on bank credit using district border discontinuities before 1935.

II. DATA

This paper combines 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 3,334 individual banks which are observed at four points in time, yielding a total of 12,996 observations.[23] 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 use the annual reports of the Office of the Comptroller of the Currency (1920, 1921a,b, 1922) and for the January 1920 call date I draw on the Rand McNally bankers directory (1920) bankers directory.[24] The four call dates are partly dictated by data availability. The Comptroller

different policies in 1920-21 (LAW in district 7 and PDR in district 8), which also explains why - despite an overall increase relative to 1919 - substantially different baseline discount rates prevailed in the two districts (7% in district 7 and 6% in district 8).

[22]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).

[23]Some banks fail or are founded after September 1919 which explains why my sample is not fully balanced.

[24]Both sources are freely accessible on-line (FRASER, Office of the Comptroller of the Currency reports and HathiTrust, Rand McNally bankers directory; last accessed 14 July 2020). 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) and more disaggregate data on banks located in central reserve cities, Federal Reserve branch cities and other large financial centers. To

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 in order to satisfy the data needs of my research design. The January 1920 data contain the last available balance sheet information before LAW and macroprudential policy implementation in late May/early spring 1920. Together, 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, reflect policy discontinuities between LAW districts and Federal Reserve Banks which did not change policy stance (see Figure I 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 (non)discontinuity in my robustness checks. I draw on the borders between the Cleveland, Philadelphia and Richmond districts for Placebo tests, because 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, 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 are split between two Federal Reserve districts with different financial stability policies starting in spring 1920. The Western part of Kentucky is located in district 8 (St Louis, a macroprudential policy 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). I apply my 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/regulations. For these two states, I compiled bank-level data for the whole population of commercial banks (state-chartered banks and national banks), including information on whether a given state-chartered bank was a member of the Federal Reserve System. In addition

compare bank-level variables over time, I merge positions from the Comptroller reports to match them exactly to the positions in the bankers directory published by Rand McNally bankers directory (1920). For example, to mirror the aggregate loan and investment portfolio in the bankers directory, I take the sum of the following positions from the Comptroller reports: loans and discounts; government securities; other bonds and investments.

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 gather balance sheets for the call dates in January 1920 and January 1921. These additional data enable me to conduct Placebo tests checking whether treatment effects for member banks persisted after treatment had ended and whether non-member banks were affected by the policies. Figures III and IV plot the split state data. Together 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.

(Figure III here)

(Figure IV here)

Apart from my main panel data sets, I also compile two new complementary bank-level data sets. The first complementary data set contains all interbank connections (so called "correspondent links") for the national banks in my sample, as published by the Rand McNally bankers directory (1920) bankers directory 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 according to whether they were domiciled in a Federal Reserve district subject to LAW, to the PDR, or belonged to one of the districts which did not change policy stance. I draw on these interbank network data to check for the presence of local continuity regarding banking connectedness and to assess whether my econometric results are likely to suffer from SUTVA violations.

Second, based on individual national bank examiner reports available at the U.S. National Archives at College Park, Maryland, I assemble bank-level interest rates and loan portfolio decompositions for all national banks located in Indiana, Kentucky and New Jersey. I concentrate on reports for examinations which took place throughout 1920. Although the pacing and frequency of examinations differs 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 use these micro data sets to trace the transmission channels explaining the size and sign of treatment effects found in this study.

On top of the systematic new data collection effort described above, I employ other descriptive data from various issues of the Federal Reserve Bulletin (Federal Reserve Board, 1920a)[25], the National Bureau of Economic Research (NBER) Macrohistory Database[26] and the U.S. Agricultural Census (1910 and 1920) as provided by Haines et al. (2016). Finally, I draw on a large range of qualitative information from contemporary sources such as annual reports, board meetings minutes and mimeos of the Federal Reserve Board (1920c,d,e,f,g,h,i,j, 1921, 1922)[27] and the final report of the Joint Commission of Agricultural Enquiry (1922)[28]. My discussion of experiment validity in the next section is furthermore

[25]The source is freely accessible on-line in scanned format (FRASER, Federal Reserve Bulletin; last accessed 14 July 2020).
[26]The source is freely accessible on-line (NBER, Macrohistory Database; last accessed 14 July 2020).
[27]The source is freely accessible on-line in scanned format (FRASER, Annual Reports of the Federal Reserve Board; last accessed 14 July 2020).
[28]The source is freely accessible on-line in scanned format (HathiTrust, Final Report of the Joint Commission of Agricultural Enquiry; last accessed 14 July 2020).

informed by 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.

III. EXPERIMENT VALIDITY AND IDENTIFICATION STRATEGY

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

III.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 which characterized the American economy after World War I. The strong economic expansion following armistice took the form of a commodity price boom, a subsequent rise in asset and 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 booms/crises (Rajan and Ramcharan, 2015, 2016) and stress its connection to bank failures during the 1920s (Jaremski and Wheelock, 2020a). Appendix A.1 provides more detail on the nature, extent and evolution of the post-World War I boom phase.

Monetary policy remained passive until January 1920, when discount rates were hiked from 4.75% to 6% uniformly across all Federal Reserve districts.[29] 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 those policy differences which are at the core of this paper (in Appendix A.5, I discuss the historical background of U.S. monetary policy decentralization before 1935 in more detail). 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

[29] 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 rate at the peak of the boom (Federal Reserve Board, 1923, p.16).

conferred by the Phelan Act of 13 April 1920. Congress had explicitly passed the 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 amount of credit a given member bank would be able to obtain *pro rata* if all member banks in a district were to borrow simultaneously, without the Federal Reserve Bank having to violate its own reserve requirements.[30] The basic line of each member bank was computed on the basis of the bank's reserves maintained with and its capital contribution[31] to the Federal Reserve Bank:

$$BL = 2.5[0.65R + 0.03(C + S)]$$ [32]

where BL stands for the basic line,

R represents lawful reserves held with the Federal Reserve System,

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

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 pay 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 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.[33] Since it directly connected the marginal cost of reserves to the individual situation of a given bank, the progressive discount rate followed a rationale closely resonating with 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 (at least) part of the potential systemic risk externalities generated by excessive credit expansion.[34]

Table I summarizes the exact dates on which the second wave of policies was implemented in the various districts

[30] The Federal Reserve Banks had to hold gold reserves to cover note issuance and deposit liabilities. These gold reserve requirements must not be confused with the member banks' reserve requirements for their deposit liabilities. For more details on the Federal Reserve System's own gold reserve requirements, see Appendix A.2.

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

[32] The exact rationale for this formula is explained in the report of the Joint Commission of Agricultural Enquiry (1922, 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.

[33] For more details on the "reserve channel", c.f. Sections I. and V..

[34] "Excessive" credit growth is difficult to define objectively. What counts is that authorities at the time considered the build up to be "excessive", posing a threat to financial stability.

and also shows their respective end dates. The four districts hitherto unmentioned (Philadelphia, Cleveland, Richmond and San Francisco) neither changed the rate schedule adopted in January 1920, nor did they implement the progressive discount rate.

(Table I here)

In order to establish 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 disaggregate, 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, the renewed policy action in late spring was motivated primarily by financial stability concerns. As documented by the Joint Commission of Agricultural Enquiry (1922, 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 strain on their solvency if debtors' ability to repay loans were to dwindle (Joint Commission of Agricultural Enquiry, 1922, p.88). The Joint Commission of Agricultural Enquiry (1922, 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 subsequently opted for a rate hike, 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 Enquiry, 1922, 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 the macroprudential tool of 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 but only 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 macroprudential tool used by some Federal Reserve Banks to dampen excessive credit growth fueled by some subgroups of member banks only.

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

Estimated treatment effects can be an artifact of spurious correlations, if baseline covariates and/or pre-trends were significantly different for treated and control group banks in my sample. Given that the policy variation across districts was motivated by differences in aggregate financial sector developments across Federal Reserve districts, the assumption of covariate balance is most likely violated when the full district data are considered. Thus, 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 II summarizes the continuity tests for variables describing the local banking structure in Panel A. Panel B checks for local continuity in economic characteristics. I obtain the coefficients and standard errors displayed in Table II 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 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 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 tests suggest that "going local" is a crucial element of my identification strategy: virtually all differences in Panel A disappear for both border types once one concentrates on bandwidths of 25 kilometers around the borders. Some minor differences in the number and location of bank-level correspondent links remain. Given that interbank connections were highly "sticky" (at least for short time horizons), the bank fixed effects in my regressions directly control for the number and nature of banks' correspondent links.

(Table II here)

Turning to local economic characteristics, 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 those 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, the estimated treatment effect could be subject to confounding factors stemming from this shock.[35]

"Going local" is one solution to control as much as possible for the differential exposure to confounding price shocks. Local economic characteristics likely determined the relative strength of the 1920-21 recession in different locations across the United States. Concerns about confounding bias might be unfounded if locations close to the district border exhibited similar structural economic features irrespective of treatment status. Panel B in Table II 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 wash out once I focus on the area within 25 kilometers of the district borders. Moreover, to make sure 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. Finally, 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 Table III, 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, Table III confirms that my main outcome variables and other bank-level characteristics exhibit no remaining, locally diverging pre-trends.

(Table III here)

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 states. Furthermore, national banks never joined any of the state-sponsored deposit insurance schemes put in place after the panic of 1907 (Calomiris, 1989).[36] 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 1920 in the 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

[35]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).

[36]After 1907, Oklahoma, Kansas, Texas, Nebraska, North Dakota, South Dakota, Mississippi, and Washington introduced deposit insurance open to 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.

the impact of LAW and macroprudential policy from other policy differences, I focus exclusively on states whose territories were split between Federal Reserve districts with different policy responses to the financial stability concerns in 1920. The availability of split states also harbors a complementary advantage. It allows me to include state-chartered banks[37] into my discontinuity regressions because, at the state-level, the regulatory continuity precondition holds for these banks too. The inclusion of state-chartered banks enables me to check whether and how non-member banks in treated districts reacted to the policy changes and to what extent the impact in 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. Systematic qualitative, let alone quantitative information on the importance of these challenges 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, 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. I discuss moral suasion and its potential implications in more detail in Appendix A.4.

III.C. Financial Segmentation

While "going local" is necessary to address the endogeneity of policy reactions and to disentangle 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 V and VI graphically compare the geographic distribution of national banks with respect to the nearest district border at three

[37] 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 state.

[38] The circular can be read here: FRASER, Circular Federal Reserve Bank of St Louis; last accessed 22 July 2020.

[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.

points in time. Figure V looks at borders separating districts which hiked rates to 7% and districts which did not enact policy changes. Figure VI in turn looks at borders shared by PDR districts and no policy districts. In both figures, Panel A compares the distribution around the border in September 1919 to the distribution in January 1920, Panel B contrasts the situation in January 1920 to the one prevailing in September 1920 and Panel C displays the distributions in January 1920 and September 1921. Both figures testify 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. In Appendix A.6, I formally confirm these insights using statistical distribution and density tests. These results correspond to intuition. 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.

(Figure V here)

(Figure VI here)

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.

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 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 on 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 suggest 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, policy differences could have simply not been meaningfully 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 into 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 macroprudential policies downwards.

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. 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. 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. Second, 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 evidence on the premise that cross-border interbank links do not violate SUTVA is depicted in Figures VII and VIII. The figures 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 state but in different Federal Reserve districts, these banks seem most likely to have interbank ties that cut through district borders. Figures VII and VIII show clearly 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. 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.5.

(Figure VII here)

(Figure VIII here)

[41] For an example relevant to the specific context of this study, c.f. Meltzer (2003, p.107).
[42] Inter-district flow of funds for member banks are only available for major (central) reserve cities, c.f. Cohen-Setton (2016).

IV. RESULTS

IV.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-difference 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 macroprudential 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 macroprudential policy on bank-level outcomes $Y_{i,t}$. To estimate the policy-specific 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 macroprudential policy and districts which did not change policy stance in late spring 1920. In this second case, δ measures the treatment effect of macroprudential 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.

Monetary policy can affect bank balance sheets by triggering changes in quantities as well as in (asset) prices (IMF, 2015). Disaggregated bank-level information on asset composition at market prices is not available for the 1920s. Consequently, I focus on bank-level changes in balance sheet quantities and ratios as my main outcome variables of interest ($Y_{i,t}$). In particular, I estimate the effect of LAW and macroprudential 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 Federal Reserve Banks motivated policy action with reference to what they deemed excessive upward trends in these variables. To facilitate the interpretation of the estimated treatment coefficient, I transform both outcome variables by taking their the natural logarithm.

The regressions using total lending as the main outcome variable 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 IV here)

Table IV 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. For 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, 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 corresponding treatment effects of the progressive discount rate are shown in Panel B. The full sample results in the leftmost column of Table IV 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 less precisely estimated, but tends to become even more pronounced (10% to 11% for the 25km radius). The PDR thus emerges as an effective macroprudential tool in 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 IV. 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 provides econometric evidence that the results reported in Table IV 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. Finally, 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.

(Table V here)

In order to check for 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 Panel A and B of Table V. The results in Table V reveal that aggregate treatment effects for LAW mask substantial geographic heterogeneity. While the interest rate hike did reduce credit growth in the Midwest, the downward pressure exerted by LAW on banks' credit expansion was both economically and statistically weak relative to the PDR's effects. In particular, Panel A in Table V demonstrates that the policy impact vanishes as one approaches the border. In the New York district, however, the LAW policy triggered a strong perverse impact (see Panel B in Table V) which drives the aggregate results for LAW displayed in Table IV. 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 underlying reasons for the considerable geographic heterogeneity in the treatment effects of LAW in Section V. below.

(Table VI here)

In Table VI, I investigate potential differences in treatment effects for PDR districts. While the Southern regions and districts included in the PDR borders 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 IV. As shown in Section III. (c.f. Table I), district 6 adopted the PDR only for the period between 31 May and 1 November 1920, after which date the Federal Reserve Bank of Atlanta switched to the LAW policy. 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 findings in Table VI are consistent with the estimated treatment effects of LAW. The exclusion of the mixed policy district 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 VI). When concentrating on the Atlanta district (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. These results thus confirm that the PDR was more effective than LAW in taming credit growth.

IV.B. Robustness Checks

I pursue five different strategies to test the robustness of the treatment effects induced by LAW and the PDR. 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 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 III. (c.f. Table III), 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 for pre-trends suggesting that pre-existing trends do not 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 Figures I and II, 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 I assume 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 replicate the local difference-in-differences regressions drawing exclusively on bank-level data from two federal states which were split by Federal Reserve district borders with different policies: New Jersey and Kentucky. New Jersey's territory is split between district 2 (LAW) and district 3 (no policy). Kentucky is split between district 8 (PDR) and district 4 (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 reason for this concern is that the estimated treatment coefficients for LAW and the PDR tend to increase in size as one approaches the border (c.f. Tables IV to VI). 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 VI, 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 New Jersey and Kentucky. In the 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. State-chartered banks located in treated territories, which did not become members of the Federal Reserve System (so called "non-member banks"), should have been less strongly affected by the financial stability policies because they did not directly interact with the Federal Reserve Bank in their districts. Non-member banks were not allowed to borrow from the Federal Reserve Banks. I implement the Placebo test using bank-level data from the split states of New Jersey and Kentucky. 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 (the treatment effects are not statistically different from zero).

V. MECHANISM

The econometric results give rise to two questions. First, why was the PDR more effective in dampening credit growth and leverage than LAW in district 7? Second, why did LAW trigger perverse treatment effects in district 2? The present section analyzes the mechanisms underlying these findings. To answer the first question, I proceed in three steps. First, I investigate the relative impact of LAW and PDR policies on banks' incentives to grant new loans. Second, I explore how funding shocks affected banks under the two different policy regimes and I provide narrative evidence on the interaction between these shocks and financial stability policies at the time. Third, I back up the insights from step 1 and 2 by reporting descriptive evidence on the distribution of banks' borrowing relative to the basic line in PDR districts. To address the second question, I concentrate on differences in usury laws along the Western and Eastern LAW borders and I show that these differences confronted banks with varying incentives to engage in regulatory arbitrage.

V.A. *The Reserves Channel: Incentives to Grant new Loans*

In 1920-21, the transmission of monetary policy to bank balance sheets worked primarily through the so called "reserves channel" (Carlson and Duygan-Bump, 2018).[43] Banks' reserve requirements for demand and time deposits initiated regular direct interactions between the Federal Reserve System and its member banks. In order to address a deficient reserve position, member banks had to borrow from their Federal Reserve Bank at the prevailing policy rate. In normal times, a deficient reserve position resulted mainly from deposit creation which, in turn, was a consequence of granting new loans to bank customers. Since changes in the nominal policy rate i directly impacted the marginal cost of reserves, the reserves channel endowed Federal Reserve Banks with the ability to influence banks' 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 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 or even identical to the cost of reserves in districts which did not change policy stance in late spring 1920. 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, the relative impact of LAW and the PDR on bank-

[43]Other transmission channels of monetary policy signals rose to importance only later during the 1920s when the Federal Reserve System started to engage in open market operations (Bordo and Sinha, 2016).

level outcomes is not obvious *ex ante* and constitutes an empirical question: it depends on average basic line usage in PDR districts. In order to corroborate the statistically and economically significant effect of the PDR – without resorting to additional mechanisms at play –, 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 more significant treatment effects than LAW.

Unfortunately, systematic bank-level data on the actual level of banks' borrowing from their Federal Reserve Bank are not available.[44] Hence, I resort to balance sheet simulations akin to a "stress-test" to illustrate the average impact of the two policies on treated banks under different scenarios of basic line usage. Rather than making assumptions about the entire distribution of banks' pre-treatment level of borrowing from the Federal Reserve Bank, I pursue a strategy of reverse engineering. I focus on how the marginal incentives of banks to expand their loan portfolio play out under different policy regimes. This approach allows me to pin down the average level of basic line usage necessary to make the PDR more binding than LAW. In subsection 3 below, I plausibilize these scenarios by exploiting the available descriptive evidence on member banks' aggregate basic line usage published in the final report of the Joint Commission of Agricultural Enquiry (1922).

To compute the mean marginal rate faced by banks in my sample, I proceed as follows. I start by calculating the individual basic lines of banks in my sample as of September 1919. I focus on balance sheet data from September 1919 for two different reasons. First, drawing on data recorded after treatment had begun (late spring 1920) would induce post-treatment bias in my calculations. Second, I use balance sheets from September 1919 rather than January 1920 because only the OCC reports feature dis-aggregated data that allows for the precise reconstruction of banks' individual basic lines. Furthermore, only the OCC balance sheets provide information on the amount of lawful reserves individual national banks maintained with their Federal Reserve Bank as well as data on the banks' cash held in vaults. These variables enable me to plausibilize assumptions I have to make for the simulation exercise (see next paragraph) and they allow for an extension of the simulation exercise to include funding shocks (see subsection 2 below).

I assume banks face the decision to grant a new loan of size x, where x is measured as a percentage of banks' currently outstanding loan portfolio. For the simulation exercise, I consider new loans sized between 5% and 90% of banks' actual loan portfolio in September 1919. The new loan constitutes the source of "stress" in my simulation exercise: each new loan makes it necessary for banks to borrow from the Federal Reserve System at interest rate i to fulfill the higher absolute reserve requirements. To calculate the cost i of additional reserves required following the granting of a new loan of size x, I assume that banks did not maintain excess reserves with their Federal Reserve Bank.[45] The interest rate i depends on the policy regime in place (LAW vs PDR). For LAW, i is always 7%. In the case of the PDR, i is a function of basic line usage and of the new loan's size. If the new loan is large enough, it may trigger an additional reserve requirement that causes

[44]The detailed national bank examiner reports provide data on current borrowing from the Federal Reserve System (including both discounts and advances). The examinations are conducted on different dates for each bank, however, and the snapshots they represent are therefore not easily comparable.

[45]A comparison of required reserves to lawful reserves actually maintained in September 1919 shows that national banks did not generally maintain excess reserves with the Federal Reserve Bank. This finding is intuitive because banks had a strong incentive to deposit excess reserves with their correspondents in larger cities where these deposits were remunerated.

a given bank's exposure vis-à-vis the Federal Reserve Bank to exceed its basic line or push it into the next cost bracket (e.g. from 100-124% basic line usage at a marginal i of 6.5%, to 125-149% basic line usage at an i of 7%). Having thus calculated i for different levels of basic line usage and for different sizes of new loans, I aggregate the bank-level results to compute the mean marginal rate faced by banks in my sample.

(Figure IX here)

Figure IX shows the mean marginal rate for newly borrowed reserves faced by banks in my sample, as a function of loan size and for different scenarios of basic line usage. As Figure IX illustrates, for a given size of new bank loans, higher basic line usage shifts the mean marginal interest rate schedule upwards. This relationship 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%; a basic line utilization of 200% in turn will shift the minimum marginal rate up to 8.5%. In other words, as soon as the mean bank in the macroprudential policy districts utilizes more than 125% of its basic line, the average impact of PDR on the marginal cost of reserves will be at least equal to the impact of LAW (keeping all else equal).[46]

By definition, in Figure IX the size of the new loan to be granted has no effect on the mean marginal interest rate in LAW districts. Even in PDR districts, however, loan size exercises but a small influence on the marginal interest rate. A non-linear increase in mean marginal interest rates starts to appear only when the new loans become larger than 50% of the current portfolio for prior basic line usage of 100% and 200%, and even in these cases the impact on the mean marginal rate is negligible. Nonetheless, above the 50% threshold the loan alone is large enough to shift the bank into progressively higher rate schedules. In the case of 0% basic line usage prior to the new loan, large loan sizes never make the average bank transgress the 100% usage threshold and therefore the marginal interest rates never surpass the flat 6% rate.

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

In his primer on the progressive discount rate, Wallace (1956) discusses an additional twist to the story of 1920-21. Wallace (1956) argues that deposit withdrawals from banks in treated districts may have substantially reinforced the treatment effect of the macroprudential tool. Although Wallace (1956, p.68) does not formally test his idea, his contribution connects the effect of the progressive discount rate 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

[46]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 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 as fixed.

such transaction an equivalent amount of reserves was transferred from the bank in the agricultural area to the bank in the non-agricultural area, [...] 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's (1956) argument proceeds in five steps. First, agricultural price declines during the crisis of 1920-21 forced farmers to withdraw deposits to redeem their debts. Second, in order pay out farmers, banks had to tap into their reserves stored with the Federal Reserve Bank because the cash in banks' vaults did not suffice. Third, banks had to borrow from the Federal Reserve Banks for two reasons: on the one hand, to replenish the reserve balance since the withdrawals forced banks' reserve balances below the required minimum; on the other hand, to obtain the liquidity necessary to honor deposit liabilities. Fourth, given that banks' reserve balances had fallen, their basic lines, which were directly coupled to the reserve balance (c.f. Section III., subsection 1), decreased too. Fifth, for banks located in PDR districts the marginal cost of reserves increased (in part drastically) because basic lines had fallen to levels so low that even a small increase in borrowing from the Federal Reserve Bank led to a transgression of the basic line. One could add a final sixth element to this narrative in order to complete the story: the high costs incurred by some of the banks must have entailed a strong deterring effect to grant new loans given that the marginal cost of reserves was so elevated.[47]

The narrative in Wallace (1956) suggests an interesting additional transmission channel of the PDR policy: the impact of funding shocks. I illustrate the effect of funding withdrawals in a stress-testing exercise akin to the one presented in the previous subsection. I consider the case of a static, one-off funding shock that occurs at one specific moment in time.[48] For each bank, I compute a range of differently sized funding shocks as a percentage of its current demand deposits. The shock may 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.[49] 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.[50]

[47]To be sure, 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 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.

[48]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. 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.

[49]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 because it also contains exchanges and cheques.

[50]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 is generally representative of banks' average cash reserve position and, in particular of the position in late spring 1920. Potential window-dressing on call dates and more extended loan portfolios in late spring 1920 could also stack the cards against finding large impacts of funding shocks.

(Figure X here)

Figure X 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. When comparing Figure X to Figure IX above, it becomes clear that 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 (0% basic line usage), funding shocks can push the mean marginal rate above the 7% flat rate. For scenarios with pre-treatment basic line usage above 50%, already small to medium size funding shocks can make the macroprudential policy more binding than the flat LAW rate hike.

The stark differences between the case of new loans and the case of funding shocks come about because the amount borrowed from the Federal Reserve Bank is an order of magnitude higher in the context of the latter exercise. 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.1, I also report the underlying distributions of the maximum marginal rate at which the banks in my sample subject to the PDR were borrowing under three different scenarios of pre-treatment basic line utilization. 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 quickly reach levels twice as high as the 7% LAW rate and, in extreme cases, also exceed 20%.

The present subsection shows that it was not necessary for basic line utilization to be unrealistically skewed in PDR districts, nor was it essential that basic lines were already fully exhausted when treatment was introduced. Funding shocks may have been an additional catalyst of the policy effect. The dynamics of the recession of 1920-21 may have endogenously reinforced the impact of macroprudential policy. Facing deposit withdrawals, banks had to borrow from the Federal Reserve System at interest rates which could have increased rapidly if the funding shock was large. 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 even merely the expected impact of these dynamics – may have drastically reduced the incentives to grant new loans.

V.C. Descriptive Evidence on Basic Line Usage

For a convincing explanation of the transmission channels and the strong treatment effect of macroprudential policy relative to LAW, the remaining challenge consists in plausibilizing scenarios which result in stronger impacts of the PDR scheme. The crucial question is whether the distributions of basic line utilization prior to and during the treatment period could result in average policy impacts significantly larger than those of LAW. Some of the most relevant information in this regard was collected and published by the Joint Commission of Agricultural Enquiry (1922). Qualitative evidence from its final report suggests that basic line utilization was indeed highly skewed just before the macroprudential policy was enacted. The report mentions that within the very same district, basic line utilization could range from 1500% (i.e. 15 times the basic line) to 0% (i.e. banks which did not borrow at all from their Federal Reserve Bank) (Joint Commission of Agricultural Enquiry, 1922, p.53). While the number of banks effectively paying high average rates following the start of the PDR scheme remained rather modest[51], 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.

To further plausibilize the scenarios discussed in the previous subsections and to shed light on the accuracy of the narrative put forward by Wallace (1956), I exploit 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) as reported by the Federal Reserve Bank of Kansas City to the Joint Commission of Agricultural Enquiry (1922). The Federal Reserve district of Kansas City was the only district which published this type of information. 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% – which I identified in the first subsection above – 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 likely more skewed than conveyed by the aggregate numbers in Appendix D.2. Additional descriptive data from the report of the Joint Commission of Agricultural Enquiry (1922) shows that, although on average a third of all member banks was borrowing in excess of their basic line in district 10, 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.[52] Eight months after the introduction of the PDR scheme, these banks' share had been reduced to 49% while the share of banks which did not borrow from the Federal Reserve Bank had decreased markedly from 61.7% to 33.8% (Wallace, 1956, p.63). Hence, the qualitative evidence available suggests that the constraints introduced by the PDR scheme were binding for highly leveraged banks and led to a redistribution of borrowing from the Federal Reserve.

[51] 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 districts paid average interest rates higher than 10%.

[52] The lending power of the Federal Reserve Bank was computed on the basis of reserves and capital deposited by member banks with the Federal Reserve Bank. The idea of a specific amount of lending power was a theoretical concept which normally did not have direct policy relevance because Federal Reserve Banks could borrow from each other via the interdistrict settlement fund (Wallace, 1956; Tallman and White, 2020).

Finally, Appendix D.2 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). The evolution of lending by the Federal Reserve Bank in district 10 and the relative number of banks in each of the three categories (excessive borrowers, borrowers, non-borrowers) should thus reflect the course of the crisis of 1920-21 fairly well – both on aggregate and in the different states. 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.[53] 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 seems to 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)).

V.D. *Usury Rates and the Perverse Effects of LAW*

In order to shed light on the mechanism underlying the perverse treatment effects in district 2, I build on a seminal insight by Stiglitz and Weiss (1981): credit market imperfections which inhibit the adequate pricing of risk can induce quantity rationing. In Stiglitz and Weiss (1981), banks ration credit to customers at rate r^* (i.e. they grant no loans at rates higher than r^*) because expected profits decline at rates higher than r^* due to adverse selection. In the United States of the 1920s, state usury rates on local bank loans represented an additional credit friction, on top of potential imperfect information problems (Ryan, 1924). Akin to the dynamics in Stiglitz and Weiss (1981), binding usury rates may have caused the quantity of credit to remain capped due to the state usury rate u^s – despite the fact that demand for more loans at interest rates higher than u^s did exist.[54] In this situation, if $u^s < r^*$, lifting the usury ceiling can result in an increase in credit supply at a range of rates r, where $u^s < r \leq r^*$. In this subsection I show that the introduction of LAW policy in district 2 eased the credit friction induced by u^s and thus likely caused higher costs for borrowing from the Federal Reserve to go hand in hand with an increased quantity of bank lending.

I proceed in several steps to explain why the interest rate hike to 7% eased the usury rate ceiling and triggered an increase in total bank lending in district 2, but not in district 7. First, I show that u^s differed substantially from state to state.[55] Figure XI depicts the state usury rates u^s prevailing in my sample of LAW borders. Whereas u^s amounted to 7% and 8% in the Western states (Michigan, Indiana, 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.

[53] 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 most likely resulted from the selection of banks into the excessive borrowing category rather than increases in the individual basic lines of banks.

[54] For example, Temin and Voth (2008) show that the introduction of usury rates in eighteenth century England worsened the access to credit for loan applicants with little social capital as they were rationed out of the market.

[55] 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 Table 3 in Appendix A.2.

(Figure XI here)

Second, I can show that, before 1 June 1920, u^s was a binding ceiling for local loans in the East, but not in the West. For this purpose I collect and compare bank-level data from Indiana (district 7) and New Jersey (district 2). I describe my data sources for the bank-level interest rates and bank-level loan decomposition used below in Appendix D.3. Figures XII and XIII display the universe of interest rates on local loans charged by national banks in Indiana and New Jersey in 1920. The x-axis reflects the date of the examiner report corresponding to a given bank's interest rate. The horizontal dashed red lines represent the respective usury rate ceilings (6% for New Jersey national banks and 8% for national banks located in Indiana). Figure XII illustrates that the usury rate ceiling was highly binding for local loans in New Jersey before and after 1 June 1920. On average, national banks 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 XIII show that the usury rate ceiling of 8% was not binding for local interest rates before 1 June 1920. On average, national banks located in Indiana charged average interest rates slightly below 7% (6.78%) before 1 June 1920, and the banks only increased rates marginally to 7.14% after 1 June 1920. Hence, while u^s was not binding for national banks in Indiana, the banks in this state did already charge interest rates substantially above the level prevailing in New Jersey before the LAW policy was enacted. Granted the *ceteris paribus* assumption, this difference in rates could be interpreted as evidence for the presence of unsatisfied loan demand at higher, but illegal rates in New Jersey.

(Figure XII here)

(Figure XIII here)

Third, I provide evidence that national banks in New Jersey engaged in regulatory arbitrage to circumvent binding state usury laws, and particularly so after 1 June 1920. Instead of continuing to lend below the Federal Reserve's policy rate, national banks in New Jersey resorted to granting call loans to New York City Stock Exchange brokers and purchased non-local commercial paper in the open market. Both call loan rates and non-local commercial paper rates were exempt from the state usury rates at the time (Federal Reserve Board, 1920b; Ryan, 1924). In contrast to Figure XII, Figure XIV plots the average interest rates for all loans (rates on local loans, call loans and commercial paper purchases) for New Jersey banks. Figure XIV shows that national banks routinely circumvented the ceiling on interest rates for local loans. Although some national banks seem to have charged higher average rates already before LAW was enacted, average interest rates increased visibly during the months following 1 June 1920. A considerable number of banks began to charge 7% average rates after 1 June 1920. While the average rate on all loans before 1 June 1920 had amounted to 6.01%, it increased to 6.44% after this date. In contrast, Figure 16 in Appendix D.4 suggests that the evolution of average rates on local loans in Indiana did practically not differ from the evolution of average rates on all loans (including call loans and commercial paper purchases) in this state.[56] The same conclusion applies to national banks located in the split-PDR state of Kentucky

[56]The rate on all loans in Indiana averaged at 6.80% and 7.11% before and after 1 June 1920 respectively. Furthermore, the average rate on all loans was not statistically different from the average rate on local loans.

(Figures 17 and 18 in Appendix D.4). In fact, the national examiner reports for Indiana and Kentucky demonstrate that national banks in these states almost never lent to call markets and only rarely purchased outside commercial paper.

(Figure XIV here)

The evidence presented suggests that New Jersey banks reacted to the combination of binding usury rates on local loans and LAW policy by shifting their credit supply to loan categories exempt from state usury laws. This narrative raises several questions. First, if regulatory arbitrage allowed treated New Jersey banks to expand their credit supply by charging a wider range of rates r, why did the banks not lend to the call and commercial paper markets before 1 June 1920, given that u^s was already binding before the introduction of LAW? Two reasons can account for the relative absence of non-local bank lending before 1 June 1920. On the one hand, call market loans did simply not constitute an attractive alternative investment option. According to a lead article in the Federal Reserve Bulletin of April 1920, call market rates remained subdued after World War I and only rose during the peak of the boom in early spring 1920 (Federal Reserve Board, 1920b). During and in the immediate aftermath of World War I, the issuance of other securities had been restricted to boost the success of government bond floatings. This form of financial repression had dampened the demand for call loans conventionally used to finance speculative stock market investments. Furthermore, banks' willingness to furnish funds to the call market initially remained low in the post-war months because of their experience with "frozen" call loans during the war: funds provided during or just before World War I had become unrealizable, illiquid assets due to the closing of the NYC stock exchange. In addition, according to the Federal Reserve Board (1920b), banks' supply of funds to the call market had also been affected by the creation of the Federal Reserve System. The establishment of the System had introduced a bias towards commercial paper lending and away from the call market: whereas the former represented eligible securities for rediscount at the Fed, loans for the purpose of carrying investment securities (other than U.S. treasury bonds) remained excluded from the central bank's rediscount facilities. Moreover, in reaction to the foundation of the Federal Reserve, the Treasury had withdrawn government funds from national banks to deposit them with the Federal Reserve Banks instead. This reduction in deposit liabilities had forced national banks to call in and to reduce their demand loans to the stock exchange.

On the other hand, national banks had always conceived their local customers as their main commercial responsibility. In the April 1920 edition of its bulletin, the Federal Reserve Board (1920b, p.371) noted that national banks also served their self-interest by concentrating on their local customer base:

> It is the universal custom of the banks to satisfy first the commercial needs of their customers. They feel an obligation to customers but none to those who borrow in the open market on securities. Besides, as the resources of the banks mainly come from the commercial customers, their own self-interest compels a preference in favor of their commercial borrowers, since failure to grant them reasonable accommodation would induce them to withdraw their deposits and so reduce the ability of the banks to do business.

This local bias of national banks' credit activities may also help to partly answer a second crucial question: why did national banks in other LAW and PDR states not resort to outside lending to the same extent as financial institutions located in the New York district? One explanation for this differential reaction to treatment could be that as long as state usury rates were not (or less) binding, banks' focus on local lending trumped the temptation to deviate credit to call markets and to purchase outside commercial paper. The greater distance to stock exchanges and the lower availability of direct commercial ties with call market brokers constitute another plausible reason for why national banks in districts other than New York engaged less in non-local lending.

In order to formally test the narrative above, I embed the data on average interest rates and lending to non-customers into my local discontinuity design in Model 1. I augment Model 1 by introducing an interaction term between the treatment dummy and the average interest rate charged by a given bank. In an alternative specification, I also draw on an interaction term between the treatment dummy and the total logarithmized sum of loans to non-customers granted by a given bank. I estimate this augmented version of Model 1 by drawing on my split state sample for New Jersey for which I collected the corresponding bank-level interest rates data and loan portfolio decompositions from individual national bank examiner reports. Table 18 in Appendix D.5 reports the results of this exercise. The crucial take-away from Table 18 is that the perverse treatment effect disappears completely once I include the interaction terms. The econometric evidence suggests that treated banks in New Jersey indeed reacted to the LAW policy by shifting their loan supply to non-local customers which enabled them to charge higher average interest rates. Thus, this shift can explain the perverse treatment effect of LAW in district 2.

VI. CONCLUSION

In this paper, I estimate the comparative causal effects of monetary policy leaning against the wind (LAW) and macroprudential policy on bank-level credit and leverage 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 macroprudential policy 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 policy stance. I show that macroprudential policy caused both bank-level lending and leverage to fall significantly, whereas LAW had only weak and, in some areas, even perverse effects on these bank-level outcomes. The macroprudential 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 and environment, I show that macroprudential policy is more effective than conventional monetary policy in taming bank credit. Second, my findings suggest that LAW can have severe counterproductive effects if pre-existing credit frictions lead the rate hike to incentivize regulatory arbitrage. Third, in contrast to recent empirical work on the impact of modern financial stability policies, my research design allows me to rule out violations of the stable unit treatment value assumption (SUTVA) in my setting. Finally, this paper also complements recent economic history contributions relevant to my quasi-experimental setting. It showcases the Fed's early use of sophisticated macroprudential tools and highlights that the various Federal Reserve Banks implemented different policies with quite heterogeneous effects on bank credit during the boom and bust phase of 1920-21.

The findings presented in this paper underscore the importance of economic history for modern policy-making in several ways. First, I show that history can provide us with 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 macroprudential policy 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 begin to deviate from the dogma of uniform policy rates for all their counterparties.[57] My paper shows that the Federal Reserve effectively used policies involving customized price discrimination to regulate bank credit already a century ago. Looking back can be a powerful tool to enlarge the breadth of current policy debates (Eichengreen, 2012): the design of the progressive discount rate scheme of 1920 comes surprisingly close to modern proposals for how to conceive financial stability policies (e.g. Stein (2012)).

OESTERREICHISCHE NATIONALBANK (EUROSYSTEM)

SUPPLEMENTARY MATERIAL

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

[57] For example, 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"). Currently, 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, however, the ECB's policies aim at boosting bank lending, rather than curtailing it.

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LIST OF TABLES

TABLE I
Federal Reserve Bank policies pursued in late spring 1920

District	Policy rate set to 7%	PDR implemented	Policy discontinued*
District 1 Boston	4 June 1920	-	15 April 1921
District 2 New York	1 June 1920	-	16 June 1921
District 3 Philadelphia	-	-	-
District 4 Cleveland	-	-	-
District 5 Richmond	-	-	-
District 6 Atlanta	1 November 1920†	31 May 1920	6 May 1921
District 7 Chicago	1 June 1920	-	30 July 1921
District 8 St Louis	-	26 May 1920	23 June 1921
District 9 Minneapolis	1 June 1920	-	5 October 1921
District 10 Kansas City	-	19 April 1920	1 August 1921
District 11 Dallas	15 February 1921†	21 May 1920	25 June 1921
District 12 San Francisco	-	-	-

* Date when policy rate was reduced to 6% or PDR was abolished.
Source: Federal Reserve Board (1921, 1922); Wallace (1956)

† Districts 6 and 11 replaced the PDR with a rate hike to 7% (i.e. the LAW policy) in fall 1920 and spring 1921 respectively. I discuss the potential implications of this policy change when presenting my econometric results (c.f. Section IV.).

TABLE II
Local continuity tests for banking and local economic structure

Panel A. Local banking structure

	LAW borders		PDR borders	
	Full sample	<25km	Full Sample	<25km
Total assets (ln, Sep 1919)	0.15	-0.18	-0.20	0.23
	(0.05)***	(0.12)	(0.06)***	(0.17)
Leverage ratio† (Jan 1920)	1.12	0.04	-0.51	0.57
	(0.15)***	(0.32)	(0.15)***	(0.55)
Deposits to equity ratio† (Jan 1920)	1.21	0.09	-0.37	1.15
	(0.15)***	(0.34)	(0.19)**	(0.79)
Cash reserves & exchange to deposits ratio†† (Jan 1920)	-0.01	0.01	0.05	-0.00
	(0.01)	(0.02)	(0.01)***	(0.03)
Total number of correspondents (Jan 1920)	-0.10	-0.13	0.02	0.13
	(0.06)	(0.15)	(0.08)	(0.25)
Total number of correspondents per 100K loans (Jan 1920)	-0.06	0.14	0.19	-0.24
	(0.03)*	(0.12)	(0.05)***	(0.14)*
Correspondent in New York City (dummy, Jan 1920)	0.05	0.06	-0.05	-0.15
	(0.01)***	(0.03)*	(0.02)**	(0.11)
Observations (number of banks)	2,621	261	1,287	65

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

	LAW borders		PDR borders	
	Full sample	<25km	Full Sample	<25km
Total population (ln)	0.05	-0.25	-0.29	-0.08
	(0.20)	(0.23)	(0.10)***	(0.16)
Number of farms per inhabitant	0.00	0.01	0.03	0.00
	(0.01)	(0.01)	(0.01)***	(0.01)
Number of farms per acre	-0.01	-0.00	0.00	0.00
	(0.00)***	(0.00)	(0.00)**	(0.00)
Improved farm land per acre	-0.01	-0.00	-0.04	0.01
	(0.03)	(0.05)	(0.02)*	(0.06)
Average farm value	4,969.56	797.31	-3,936.00	-1,160.91
	(1,812.29)***	(1,099.54)	(475.81)***	(1,256.09)
Average share of farms mortgaged	0.10	0.04	-0.01	0.00
	(0.01)***	(0.02)*	(0.01)	(0.02)
Average debt to value ratio	0.56	0.52	2.40	1.21
	(1.30)	(1.23)	(0.61)***	(1.56)
Average mortgage interest rate	-0.18	-0.04	0.74	0.15
	(0.19)	(0.08)	(0.08)***	(0.15)
Exposure to traded crops†††	-0.03	-0.08	0.02	-0.03
	(0.02)	(0.06)	(0.02)	(0.04)
Observations (number of counties)	515	60	542	43

Coefficients obtained by simple regression on treatment dummy. Robust standard errors in parentheses.
County-level data weighted by number of banks in county.
*** p<0.01, ** p<0.05, * p<0.1

†In this paper, 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 throughout 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 Appendix A.1.

TABLE III
Pre-trends in local banking characteristics (Sep 1919 - Jan 1920)*

	LAW borders		**PDR borders**	
	Full sample	<25km	Full Sample	<25km
Total lending (ln)	0.01	0.03	-0.04	0.00
	(0.01)	(0.03)	(0.02)**	(0.05)
Leverage ratio (ln)	0.01	0.02	-0.04	-0.01
	(0.01)	(0.02)	(0.02)**	(0.05)
Deposits to equity ratio (ln)	-0.02	0.02	0.04	0.03
	(0.01)**	(0.03)	(0.02)*	(0.08)
Cash reserves & exchange to deposits ratio	0.00	-0.00	0.08	-0.02
	(0.02)	(0.01)	(0.04)**	(0.02)
Total deposits (ln)	-0.02	0.02	0.04	0.05
	(0.01)*	(0.03)	(0.02)*	(0.08)
Bank equity (ln)	0.00	0.01	-0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Observations (number of banks)	5,217	517	2,567	129

Robust standard errors in parentheses.
County-level data weighted by number of banks in county.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

*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 Table III. For the exact definitions of the variables, c.f. Table II.

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

Panel A. Leaning against the wind

Outcome variable: total lending (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.00	0.02	0.03	0.04	0.06	0.05
	(0.01)	(0.01)**	(0.01)***	(0.01)***	(0.01)***	(0.02)***
	[0.01]	[0.01]**	[0.01]***	[0.01]***	[0.02]***	[0.02]**
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	-0.00	0.01	0.02	0.03	0.07	0.06
	(0.01)	(0.01)	(0.01)**	(0.01)***	(0.01)***	(0.02)***
	[0.01]	[0.01]	[0.01]*	[0.01]**	[0.02]***	[0.02]***
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	-0.06	-0.04	-0.06	-0.04	-0.05	-0.10
	(0.01)***	(0.02)***	(0.02)***	(0.02)*	(0.03)**	(0.05)**
	[0.01]***	[0.02]**	[0.02]**	[0.03]	[0.03]	[0.06]
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	-0.06	-0.04	-0.06	-0.04	-0.06	-0.11
	(0.01)***	(0.02)***	(0.02)***	(0.02)*	(0.03)**	(0.05)**
	[0.01]***	[0.02]**	[0.02]***	[0.03]	[0.03]*	[0.06]*
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.
*** p<0.01, ** p<0.05, * p<0.1

TABLE V
Treatment effects of LAW: Western vs. Eastern borders

Panel A. Western LAW border (district 4 vs. district 7)

Outcome variable: total lending (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.07	-0.03	-0.03	-0.03	-0.02	-0.01
	(0.01)***	(0.01)**	(0.01)**	(0.02)*	(0.02)	(0.03)
	[0.01]***	[0.01]**	[0.02]*	[0.02]	[0.02]	[0.04]
R-squared	0.19	0.18	0.44	0.48	0.40	0.39
Observations	5,569	3,336	1,375	1,005	648	312

Outcome variable: leverage ratio (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.04	-0.02	-0.03	-0.02	-0.01	-0.00
	(0.01)***	(0.01)*	(0.01)**	(0.02)	(0.02)	(0.03)
	[0.01]***	[0.01]*	[0.02]*	[0.02]	[0.02]	[0.04]
R-squared	0.22	0.24	0.52	0.56	0.57	0.54
Observations	5,569	3,336	1,375	1,005	648	312

Panel B. Eastern LAW border (district 2 vs. districts 3 and 4)

Outcome variable: total lending (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	0.02	0.04	0.06	0.06	0.09	0.08
	(0.01)***	(0.01)***	(0.01)***	(0.01)***	(0.01)***	(0.02)***
	[0.01]**	[0.01]***	[0.01]***	[0.02]***	[0.02]***	[0.02]***
R-squared	0.23	0.24	0.32	0.31	0.40	0.45
Observations	9,512	7,104	4,125	3,209	1,964	935

Outcome variable: leverage ratio (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	0.02	0.03	0.04	0.04	0.09	0.09
	(0.01)**	(0.01)***	(0.01)***	(0.01)***	(0.01)***	(0.02)***
	[0.01]**	[0.01]***	[0.01]***	[0.01]***	[0.02]***	[0.03]***
R-squared	0.23	0.25	0.32	0.31	0.43	0.45
Observations	9,512	7,104	4,125	3,209	1,964	935

Standard errors in parentheses. Clustered standard errors (at bank-level) in squared brackets.
All regressions with bank FE, time FE and bank-level controls.
*** p<0.01, ** p<0.05, * p<0.1

TABLE VI
Treatment effects of PDR: Northern vs. Southern borders

Panel A. PDR borders (without district 6)

Outcome variable: total lending (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.08	-0.05	-0.12	-0.11	-0.12	-0.14
	(0.02)***	(0.03)**	(0.03)***	(0.04)***	(0.04)***	(0.07)**
	[0.02]***	[0.02]**	[0.03]***	[0.03]***	[0.04]***	[0.06]**
R-squared	0.19	0.23	0.34	0.36	0.35	0.42
Observations	4,085	1,972	968	679	469	175

Outcome variable: leverage ratio (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.06	-0.03	-0.09	-0.08	-0.09	-0.11
	(0.02)***	(0.02)	(0.03)***	(0.04)**	(0.04)**	(0.07)*
	[0.02]***	[0.02]	[0.03]***	[0.03]**	[0.04]**	[0.07]*
R-squared	0.25	0.32	0.40	0.44	0.32	0.38
Observations	4,085	1,972	968	679	469	175

Panel B. PDR border (without district 8)

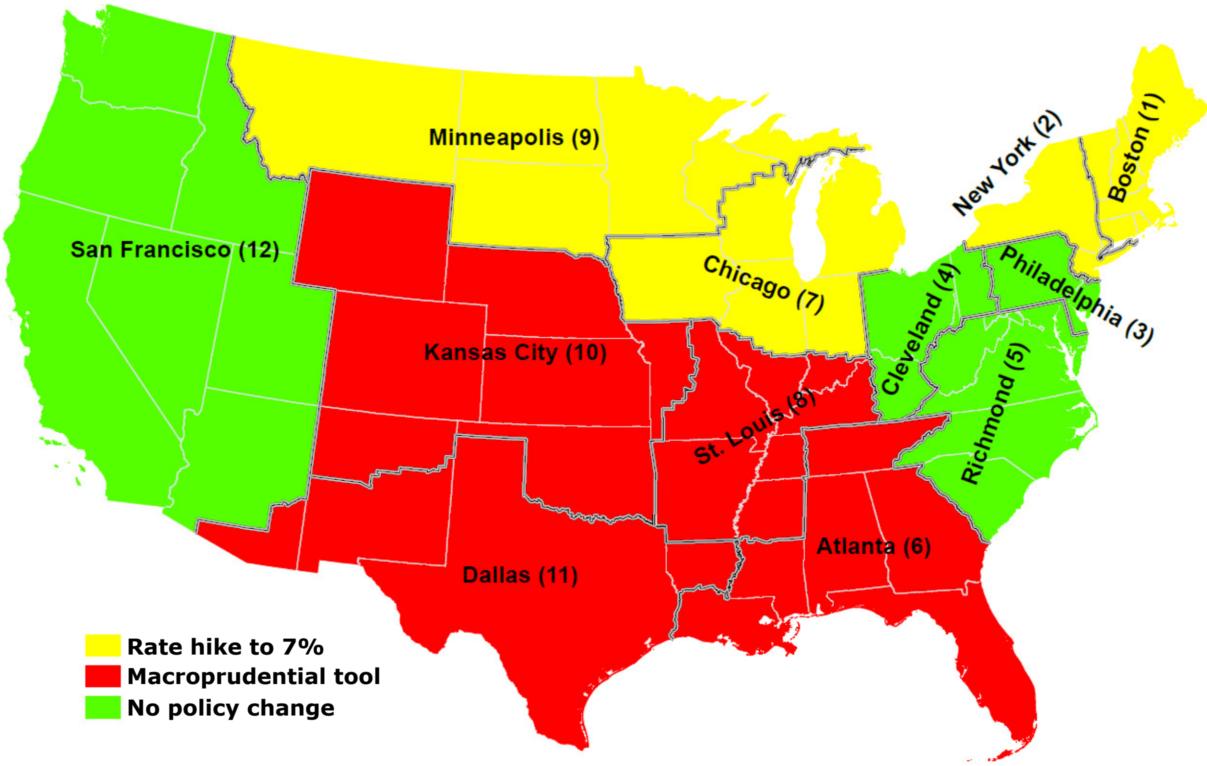
Outcome variable: total lending (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.05	-0.04	-0.03	-0.02	-0.02	-0.08
	(0.01)***	(0.02)**	(0.03)	(0.03)	(0.03)	(0.06)
	[0.02]***	[0.02]*	[0.03]	[0.03]	[0.04]	[0.07]
R-squared	0.19	0.23	0.36	0.37	0.35	0.40
Observations	4,641	2,260	1,107	830	570	210

Outcome variable: leverage ratio (ln)						
	Full sample	<200km	<100km	<75km	<50km	<25km
Treatment effect	-0.05	-0.05	-0.05	-0.03	-0.04	-0.10
	(0.01)***	(0.02)***	(0.02)*	(0.03)	(0.03)	(0.06)*
	[0.02]***	[0.02]**	[0.03]*	[0.03]	[0.04]	[0.07]
R-squared	0.27	0.34	0.44	0.46	0.39	0.47
Observations	4,641	2,260	1,107	830	570	210

Standard errors in parentheses. Clustered standard errors (at bank-level) in squared brackets.
All regressions with bank FE, time FE and bank-level controls.
*** p<0.01, ** p<0.05, * p<0.1

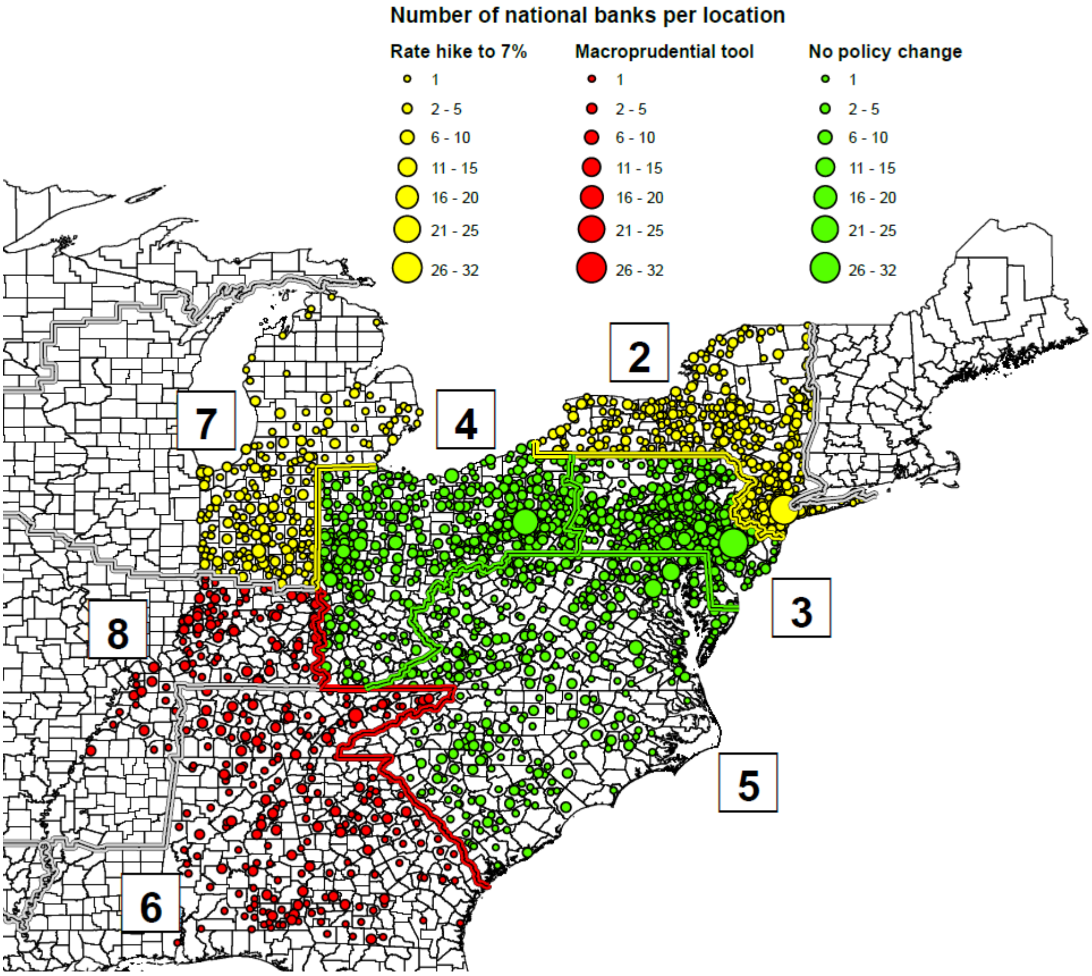
LIST OF FIGURES

FIGURE I
Federal Reserve Bank policies adopted in late spring 1920



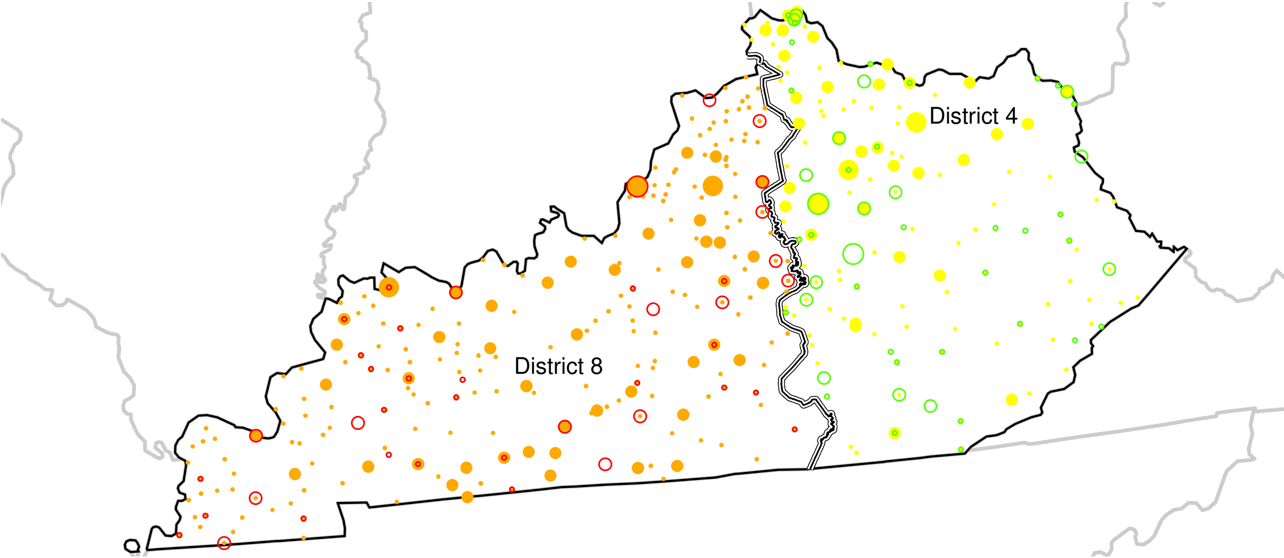
Source: Federal Reserve Board (1921)
This map shows the different policies adopted by Federal Reserve districts in late spring 1920.

FIGURE II
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) contained in the sample of this study.

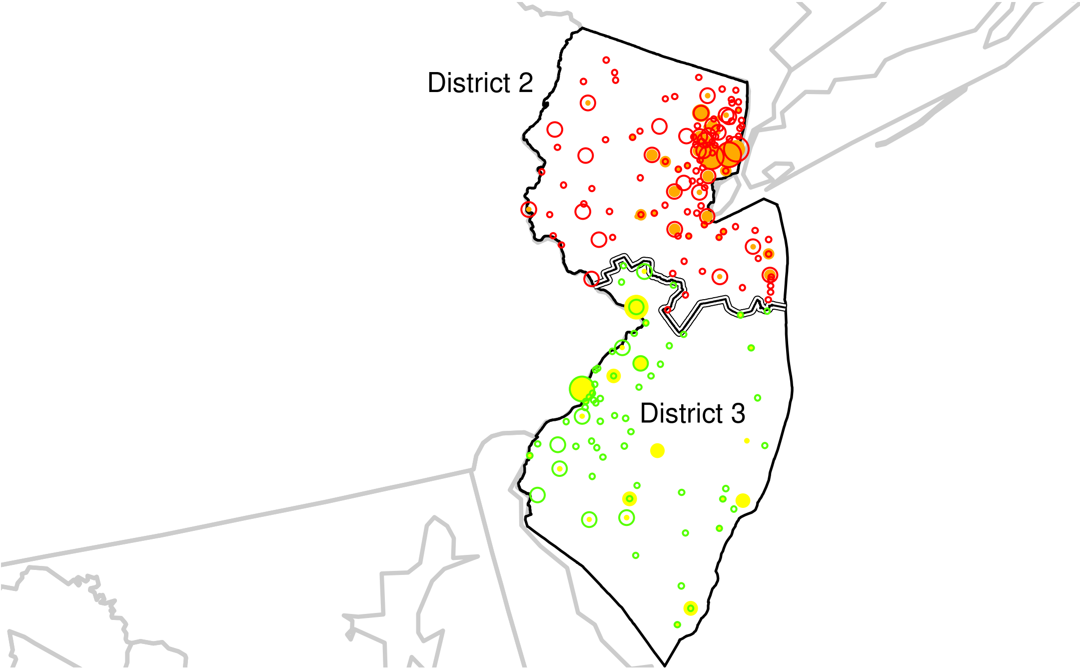
FIGURE III
Banks located in the split state of Kentucky



Source: Rand McNally bankers directory (1920); OpenCage Geocoder

This graph plots the location of all commercial banks in the split state of Kentucky. The four marker symbols represent treated Federal Reserve member banks (red hollow circles), Federal Reserve member banks in the control district (hollow green circles), non-member banks in the treated district (full orange circles), and non-member banks in the control district (full yellow circles). The different sizes of hollow/full circles represent the number of banks of a particular category in a given city (the smallest circles represent a single bank, the medium sized circles indicate locations with 2-4 banks and the largest circles stand for cities with 5-9 banks.

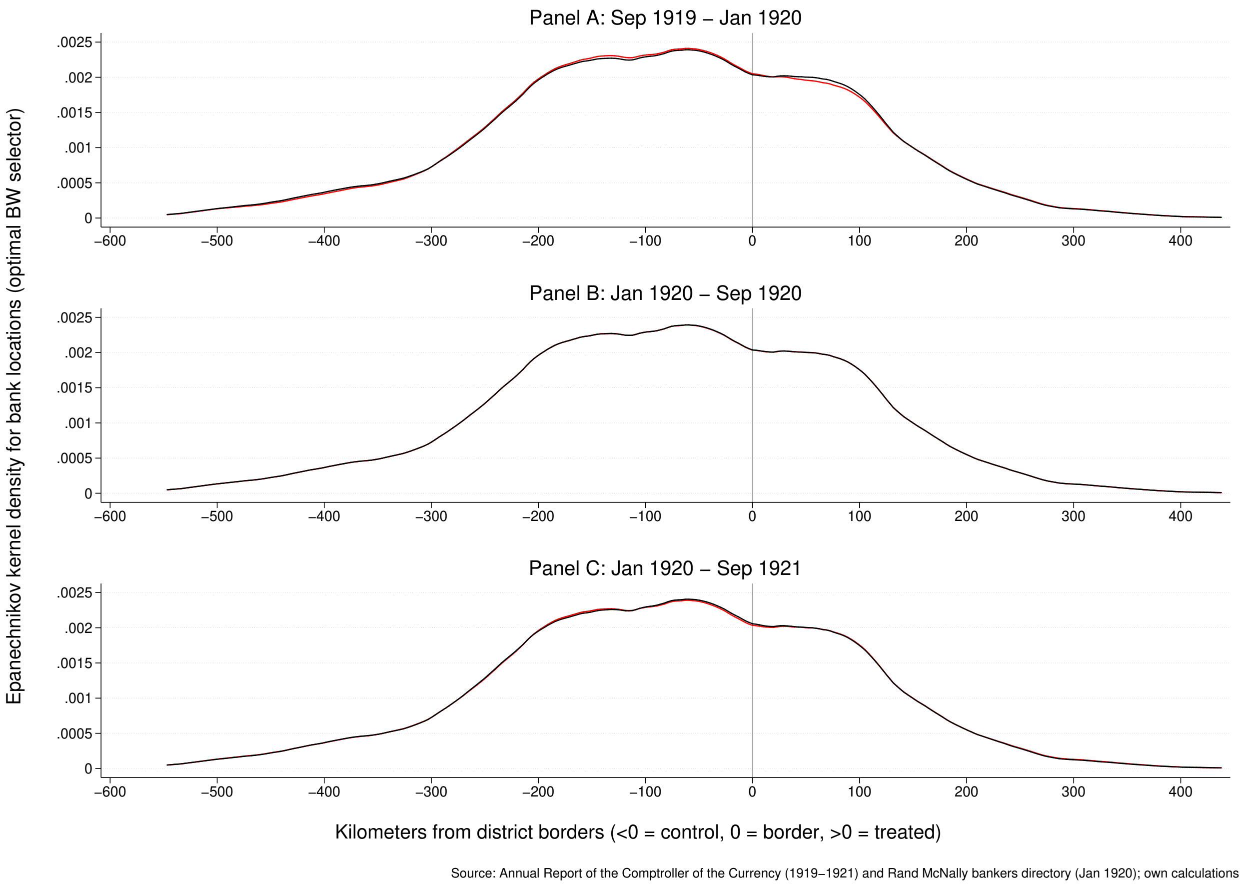
FIGURE IV
Banks located in the split state of New Jersey



Source: Rand McNally bankers directory (1920); OpenCage Geocoder

This graph plots the location of all commercial banks in the split state of New Jersey. The four marker symbols represent treated Federal Reserve member banks (red hollow circles), Federal Reserve member banks in the control district (hollow green circles), non-member banks in the treated district (full orange circles), and non-member banks in the control district (full yellow circles). The different sizes of hollow/full circles represent the number of banks of a particular category in a given city (the smallest circles represent a single bank, the medium sized circles indicate locations with 2-4 banks and the largest circles stand for cities with 5-9 banks. In the case of non-member banks located in the treated region, there is a fourth category (10-15 banks) containing the two largest cities in terms of banks (Jersey City and Newark).

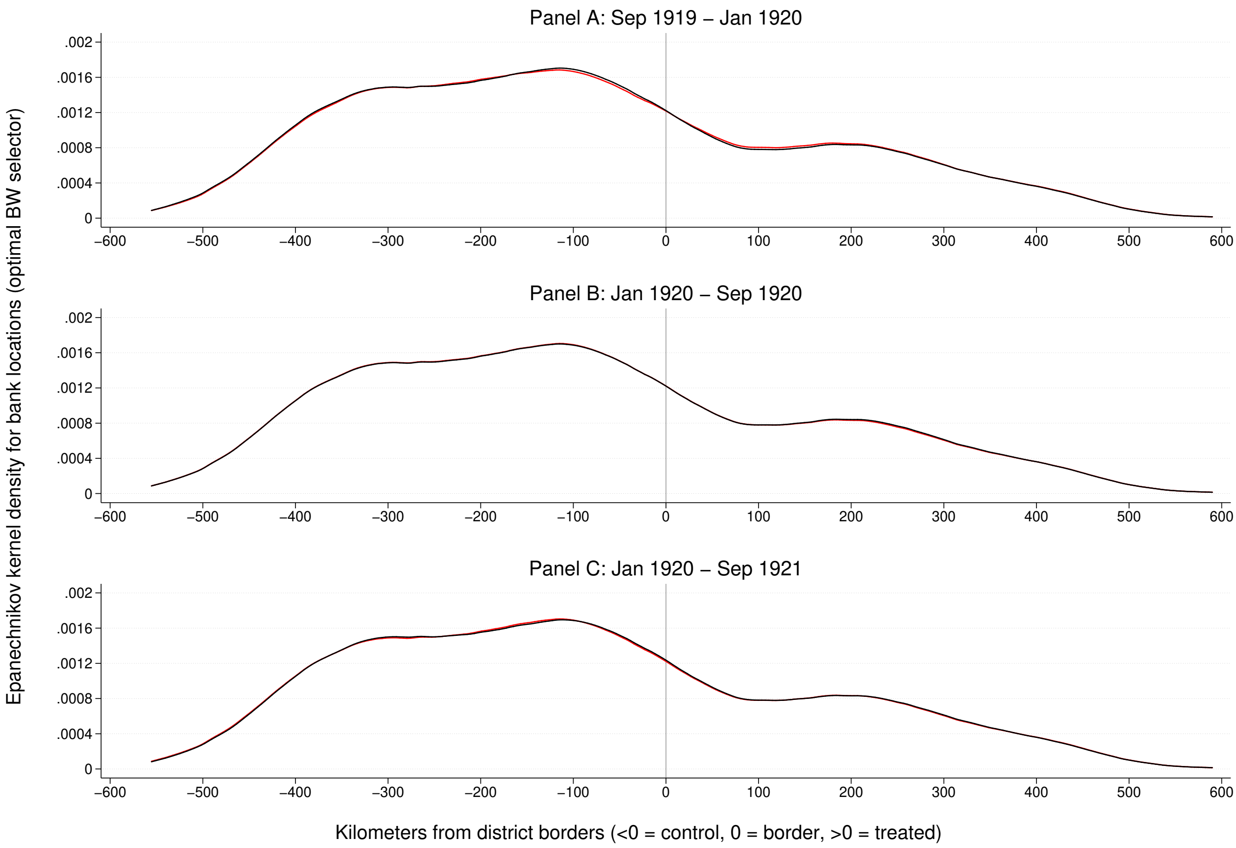
FIGURE V
Kernel densities for national bank locations around LAW* borders (red line represents earlier date in each panel)



*LAW borders constitute Federal Reserve district borders separating districts which hiked the policy rate to 7% and districts which did not change policy stance in late spring 1920. In my sample, these district borders are the borders separating 1) district 4 (Cleveland) and district 7 (Chicago); 2) district 2 (New York) and district 3 (Philadelphia); 3) district 2 (New York) and district 4 (Cleveland).

FIGURE VI

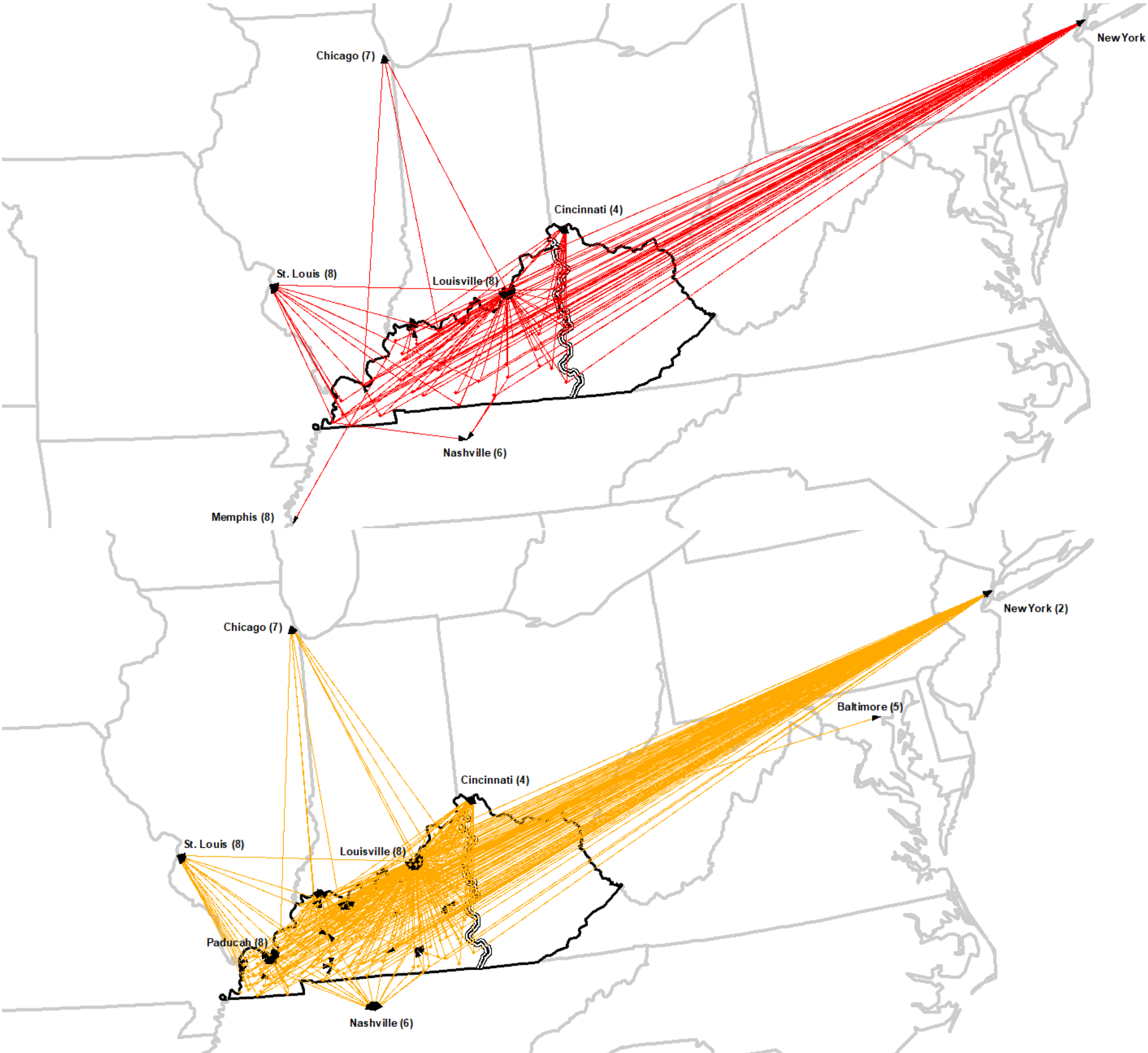
Kernel densities for national banks locations around PDR borders* (red line represents earlier date in each panel)



Source: Annual Report of the Comptroller of the Currency (1919–1921) and Rand McNally bankers directory (Jan 1920); own calculations

*PDR borders constitute Federal Reserve district borders separating districts which introduced the PDR and districts which did not change policy stance in late spring 1920. In my sample, these district borders are the borders separating 1) district 4 (Cleveland) and district 8 (St Louis); 2) district 4 (New Cleveland) and district 6 (Atlanta); 3) district 5 (Richmond) and district 6 (Atlanta).

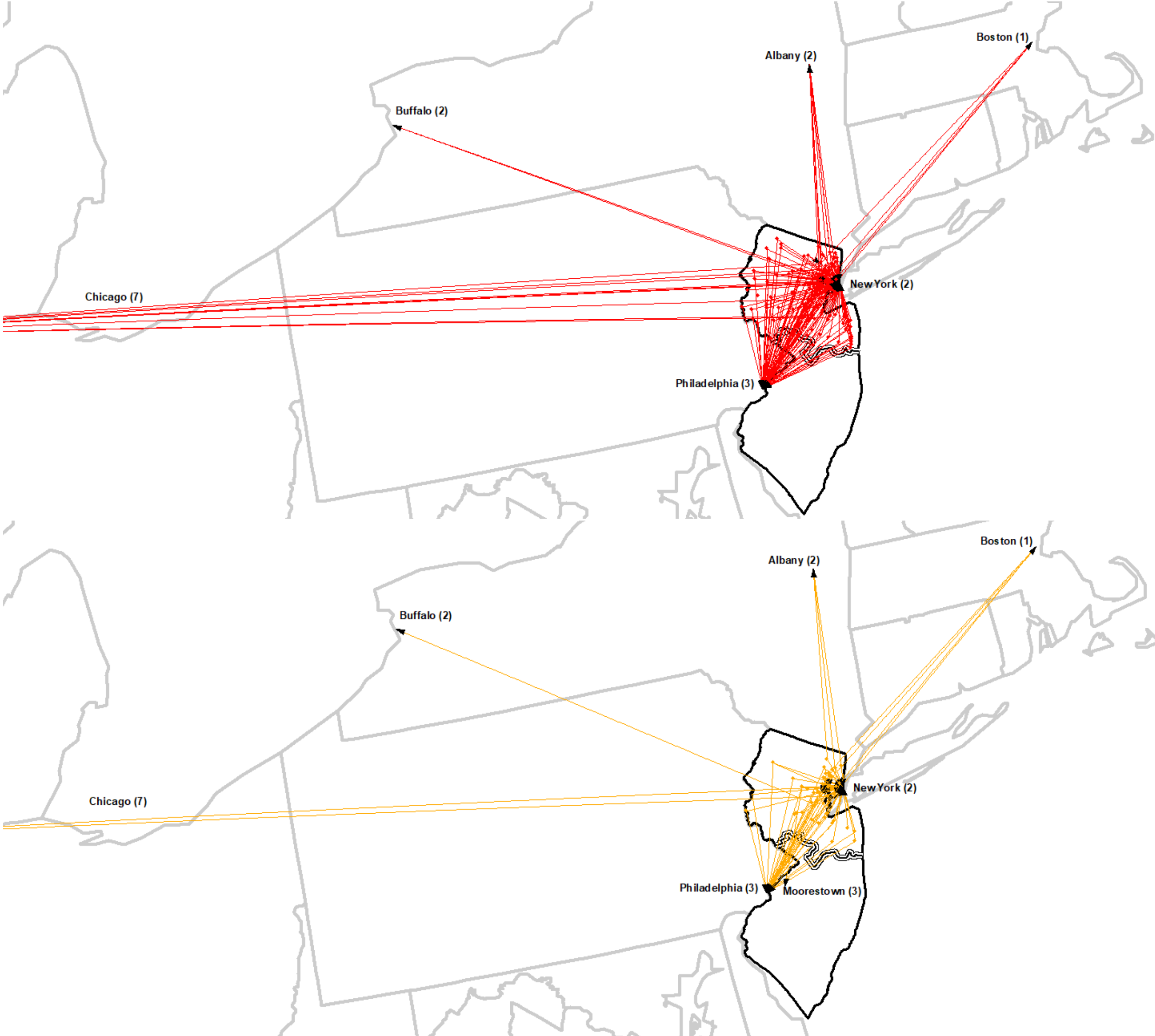
FIGURE VII
Interbank links of banks in the split state of Kentucky (district 8)



Source: Rand McNally bankers directory (1920); OpenCage Geocoder

* This graph plots the interbank links of all commercial banks located in the treated half in the split state of Kentucky. The upper panel shows the outgoing correspondent links of Federal Reserve member banks (red lines). The lower panel shows the outgoing correspondent links of non-member banks (orange lines). The names of the most important correspondent cities are indicated on the map (including the number of the district in which the city is located).

FIGURE VIII
Interbank links of banks in the split state of New Jersey (district 2)



Source: Rand McNally bankers directory (1920); OpenCage Geocoder

* This graph plots the interbank links of all commercial banks located in the treated half in the split state of New Jersey. The upper panel shows the outgoing correspondent links of Federal Reserve member banks (red lines). The lower panel shows the outgoing correspondent links of non-member banks (orange lines). The names of the most important correspondent cities are indicated on the map (including the number of the district in which the city is located).

FIGURE IX
Mean marginal interest rate under LAW and PDR: the case of new loans

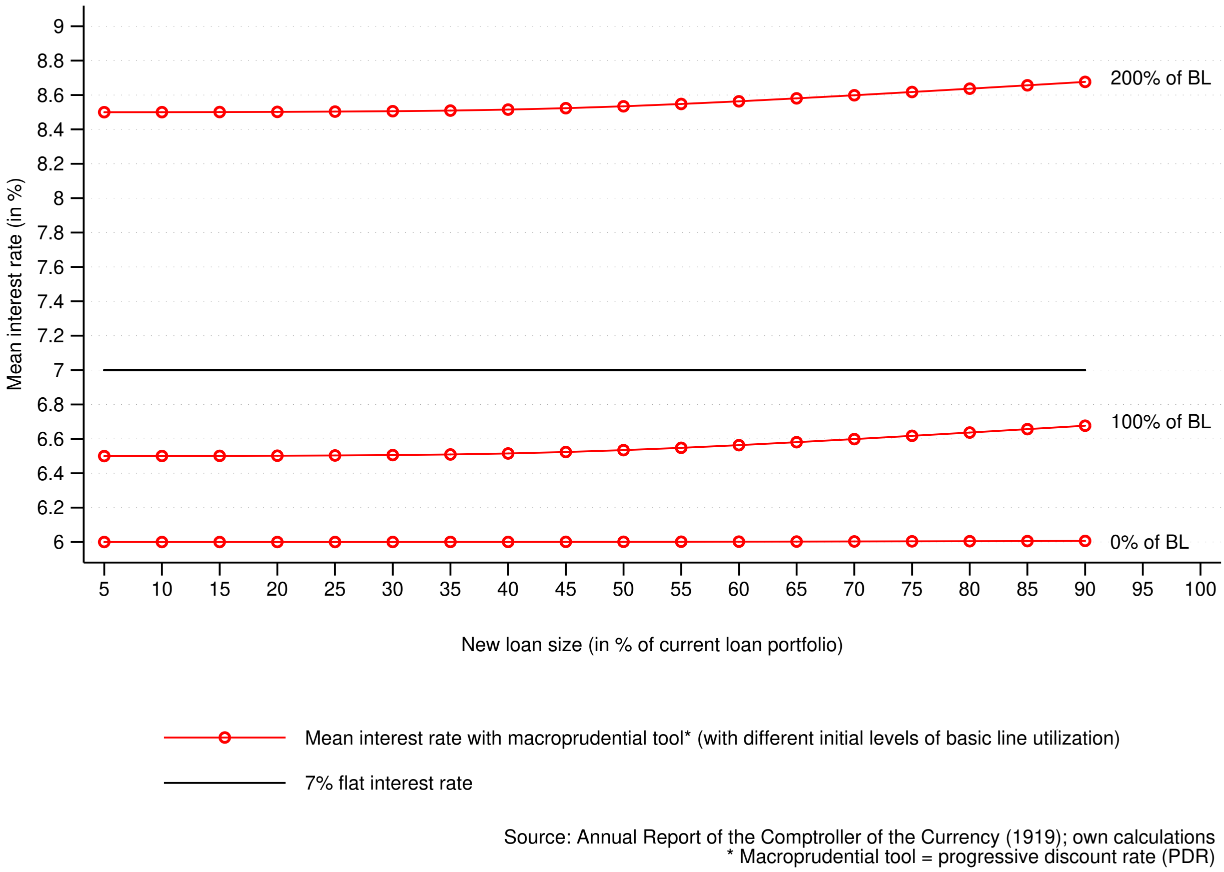
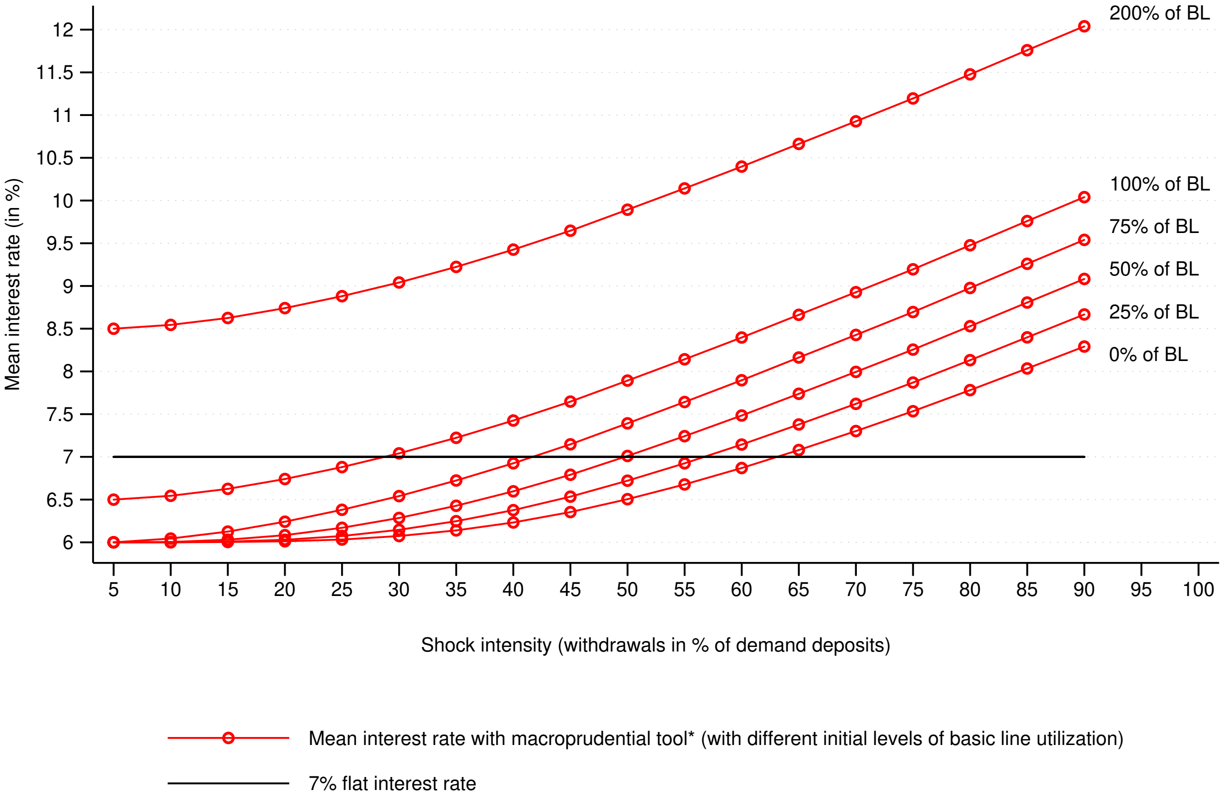


Figure IX shows the mean marginal interest rate paid by banks in the sample for additional reserves required after granting a new loan. The graph shows the interest rate as a function of the size of the loan (x-axis) and banks' usage of the basic line (BL). The indicated interest rate is faced by the average bank in the sample when it grants a new loan 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 (i.e. 6% flat) districts corresponds to PDR costs under the scenario of 0% basic line usage.

FIGURE X
Mean marginal interest rate paid on borrowing from the Federal Reserve System: the case of funding shocks



Source: Annual Report of the Comptroller of the Currency (1919); own calculations
* Macroprudential tool = progressive discount rate (PDR)

Figure X 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.

FIGURE XI
Usury rates for LAW borders in estimation sample

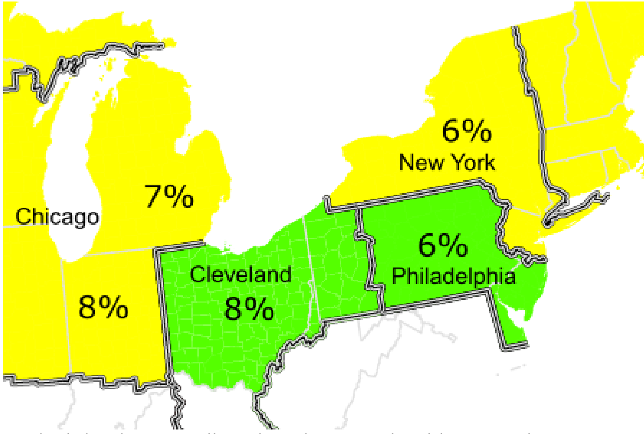
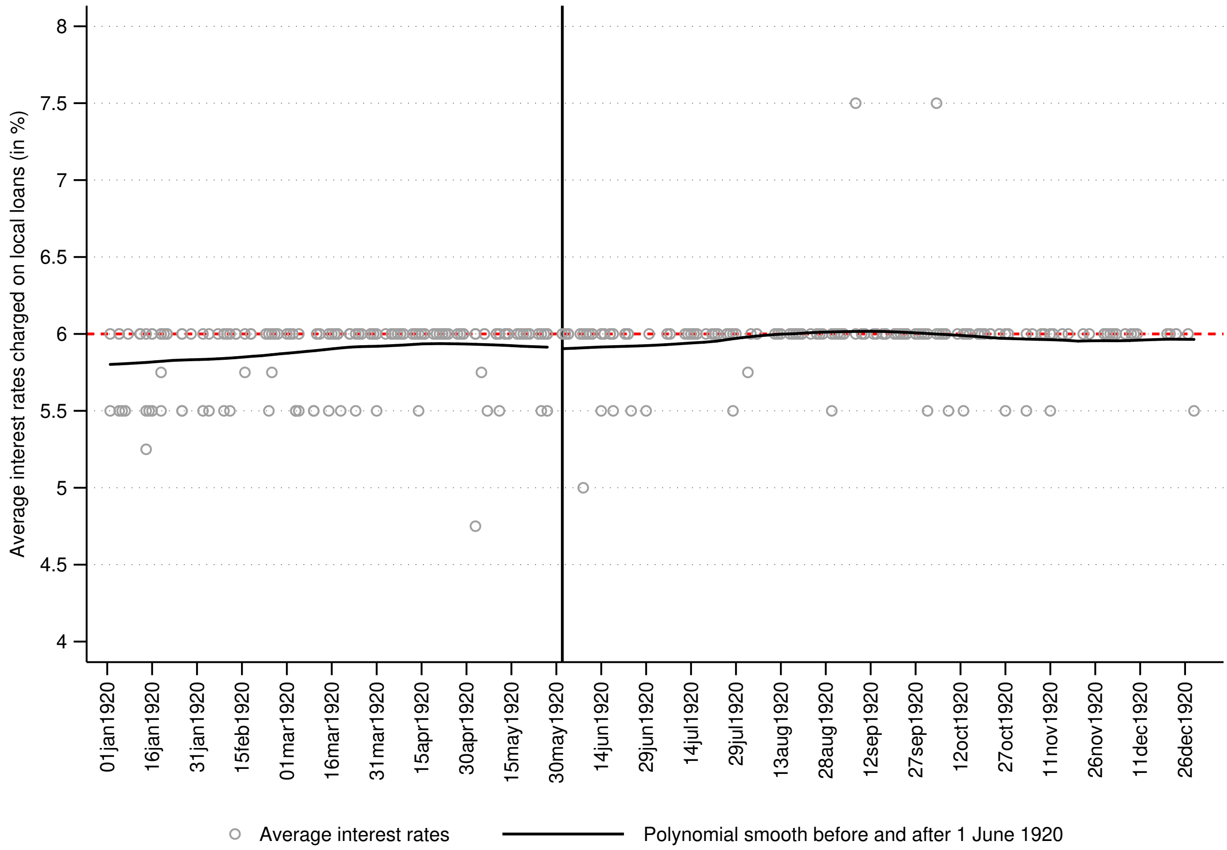


Figure XI shows the maximum rates which banks were allowed to charge on local loans in the states included in my sample to estimate the treatment effect of LAW.

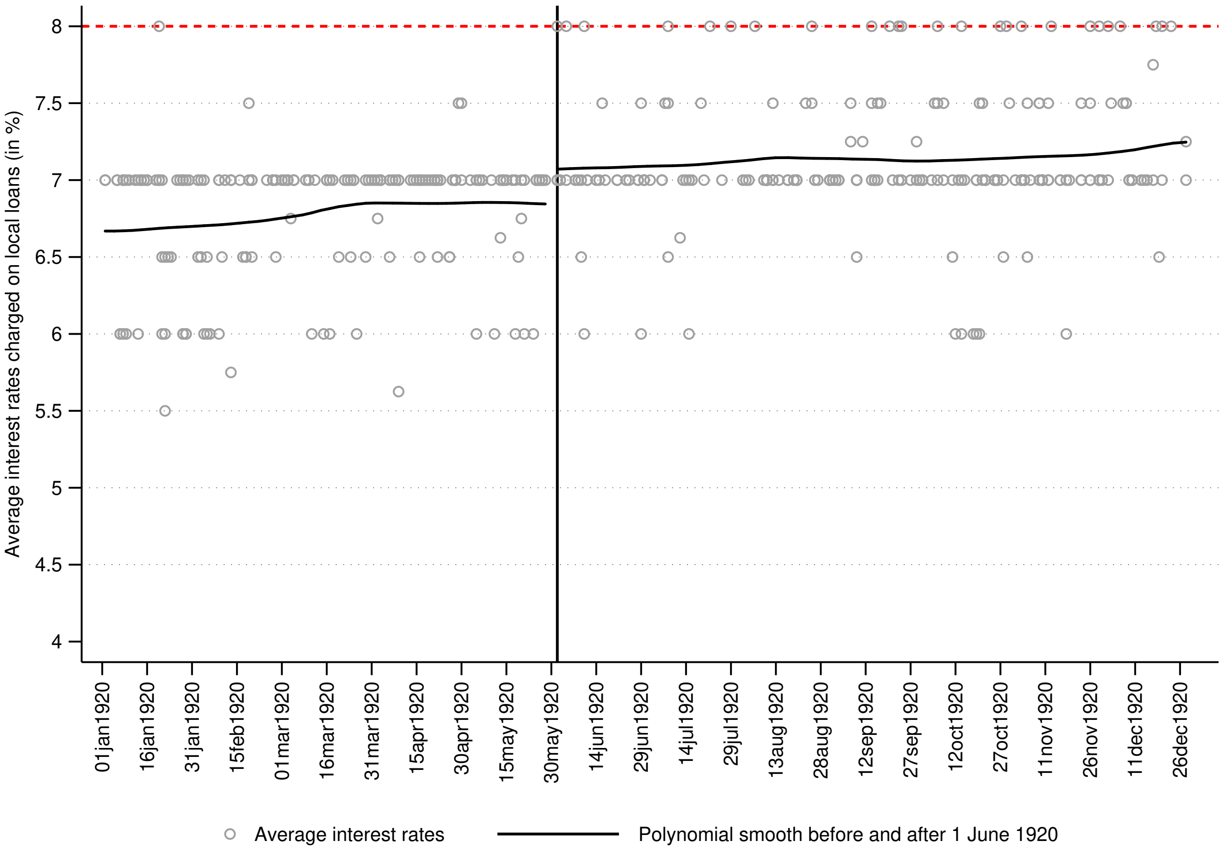
FIGURE XII
Interest rate on local loans charged by national banks in New Jersey in 1920



Source: National Bank Examiner Reports for 1920

Figure XII 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 of degree zero with an Epanechnikov kernel function.

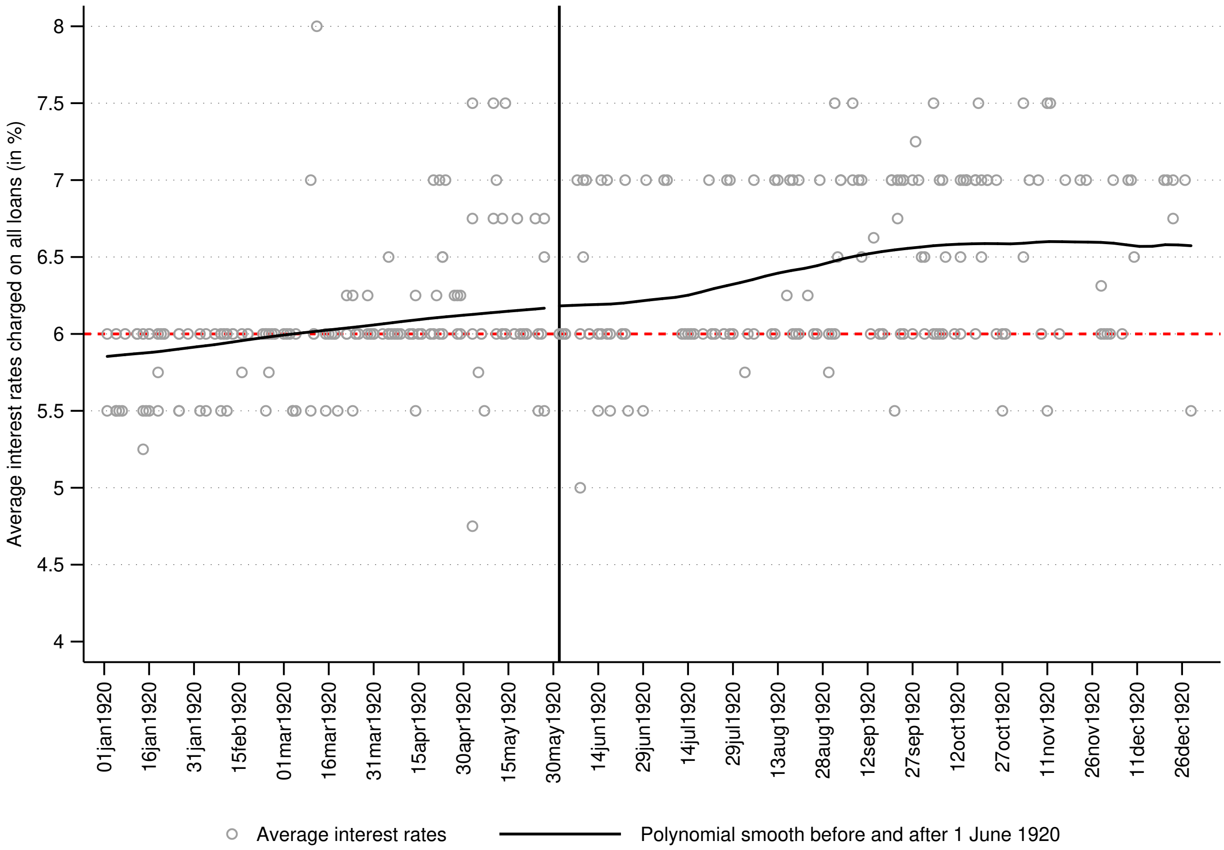
FIGURE XIII
Interest rate on local loans charged by national banks in Indiana in 1920



Source: National Bank Examiner Reports for 1920

Figure XIII 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 of degree zero with an Epanechnikov kernel function.

FIGURE XIV
Average interest rate on all loans charged by national banks in New Jersey in 1920



Source: National Bank Examiner Reports for 1920

Figure XIV shows bank-level average interest rates on all loans (i.e. local loans, call loans and commercial paper purchases) 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 of degree zero with an Epanechnikov kernel function.