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Managing Stigma during a Financial Crisis

Sriya Anbil*[†]

December 2016

Abstract

How should regulators design effective emergency lending facilities to mitigate stigma during a financial crisis? I explore this question using data from an unexpected disclosure of partial lists of banks that secretly borrowed from the lender of last resort during the Great Depression. I find evidence of stigma in that depositors withdrew more deposits from banks included on the lists in comparison with banks left off the lists. However, stigma dissipated for banks that were revealed earlier after subsequent banks were revealed. Overall, the results suggest that an emergency lending facility that never reveals bank identities would mitigate stigma. (*JEL* Codes: G01, G21, G28)

*Board of Governors of the Federal Reserve System, Division of Monetary Affairs, 20th St. and Constitution Ave., NW, Washington, DC 20551, USA. 202-736-5685. sriya.l.anbil@frb.gov

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1 Introduction

During the recent financial crisis, the Federal Reserve (Fed) was frustrated in its efforts to inject liquidity into the financial system through its main emergency lending facility, the discount window (Armantier et al. (2014)).¹ At the time, the Fed Chairman Ben Bernanke explained that “the banks’ concern was that their recourse to the discount window, if it somehow became known, would lead market participants to infer weakness—the so-called stigma problem (Bernanke (2009)).” The consequences of approaching the discount window could be costly and might even lead to bank closures (Madigan (2009)). As a result, banks were reluctant to borrow from the lender of last resort (LOLR) because they feared the public reaction, which prevented the Fed from inserting liquidity into critical parts of the banking sector during the financial crisis.

An important challenge for all central banks is to design emergency lending facilities in a way that alleviates the “stigma problem.”² The stigma problem is not new—arguably, it goes back to the beginning of the 1920s. At the time, the Fed emphasized that lending from the discount window should only be temporary, implying that a bank that borrowed from the discount window must be in trouble (Gorton and Metrick (2013)), which has complicated LOLR policy ever since. Although central bankers are concerned with the stigma problem, direct evidence of its existence is limited.³

¹In the US, the Fed’s traditional means of providing emergency credit to depository institutions is through its discount window. Lending from the discount window is in the form of promissory notes secured by adequate collateral (Furfine (2003)).

²Stigma was a central topic discussed during Federal Open Market Committee (FOMC) meetings. Ben Bernanke said, “We have been looking at . . . how to address the stigma of the discount window. Are there ways to provide liquidity that would help normalize money markets . . .but would avoid the stigma and create a more efficient system?” (FOMC (2007))

³I define stigma as the cost imposed on a bank after its emergency LOLR loan was revealed to the public as in Ennis and Weinberg (2013). However, stigma is also related to banks being ex-ante unwilling to approach their LOLR. Indeed, Vossmeier (2016) finds that after banks that approached the LOLR during the Great Depression were revealed, they subsequently became extremely reluctant to seek assistance from

In this paper, I ask two related questions. First, are banks stigmatized after news is revealed that they received emergency loans from the LOLR? Second, if they are, how can the LOLR effectively control the information environment to manage public reactions during a crisis? There are two requirements that must be met in order to answer these questions. First, the researcher needs to be able to compare the outcomes of banks that borrowed from the LOLR whose identities were revealed with the outcomes of similar banks that also borrowed but whose identities were not revealed. Second, the researcher needs to know specifically *when* the banks' identities were revealed. No setting exists during the recent crisis where both requirements are met.

I address these two requirements by using an unexpected event from the Great Depression. Beginning on August 22, 1932, the Clerk of the House of Representatives took it upon himself to publish several partial lists of banks that had secretly borrowed from the Reconstruction Finance Corporation (RFC), which functioned as a LOLR during the Great Depression. Using a unique hand-collected data set of balance sheet information for 3,074 banks, I implement a difference-in-differences (DID) approach to estimate the effect of the publication of the lists on deposit levels and other bank outcomes. To do so, I compare the performance of those banks included on the lists (“revealed banks”) with that of banks not included on the lists (“never revealed banks”). Banks were included on the lists according to the date of their loan authorization; never revealed banks had a loan authorized by the RFC before revealed banks.

This framework meets both requirements. First, all banks in the analysis borrowed from the RFC, which eases any sampling problems by allowing me to compare revealed and never revealed banks that borrowed from the RFC. Second, the public learned the identities of borrowers beginning on August 22, 1932, via the publication of the lists. Consequently, the

their LOLR.

mechanism and timing of how the public discovered news of emergency loans are clear.

I first ask whether depositors stigmatized banks after their emergency loans were revealed. I find results consistent with depositors interpreting news of a LOLR emergency loan as a sign of financial weakness; deposit levels dropped by 18-25% at revealed banks compared with never revealed banks. Moreover, revealed banks were 78% more likely to fail after the lists were published and, conditional on failing, the time-to-failure for revealed banks was nearly nine weeks sooner than that for never revealed banks. Altogether, the results provide evidence that deposits imposed a liquidity risk on revealed banks and increased the speed and likelihood of their failure.⁴

To the best of my knowledge, this paper is the first to identify and quantify the magnitude of stigma that banks experienced after their emergency loans were revealed to market participants. Specifically, in 1932, banks lost, on average, 81% of their deposits before failing (Wicker (1996)), where 18-25% of the drop can be attributed to stigma. Accordingly, the marginal contribution of stigma toward the likelihood of failure was considerable and was significant enough to warrant the attention.

Having found evidence of the stigma problem, I next ask how this information can be used to inform the design of emergency lending facilities and manage the information environment during a crisis to alleviate the problem. One possibility is to resolve a coordination problem between banks that results from banks' reluctance to borrow from the discount window (Armantier et al. (2014)). Because of stigma, a bank's decision to borrow from the LOLR involves predicting the actions of other banks. If a bank believes that other banks are reluctant to borrow, it may decide not to borrow from the LOLR. To identify the optimal policy design to address this coordination problem, I must determine the conditions under

⁴These results are from an era before FDIC deposit insurance was implemented. Once it was implemented, depositors were less likely to withdraw funds from their banks despite changes in the bank's fundamentals.

which market participants did not withdraw more deposits from revealed banks. To do so, I focus on the timing of the publication of the lists. Would depositors still interpret news of an LOLR emergency loan as a sign of financial weakness if a bank was revealed on an earlier list relative to a later one?

No such setting is available from the recent crisis, but it is possible during the Great Depression. I find that deposit levels dropped by 13.5-16.8 percentage points more at banks that were revealed on a later list relative to banks on an earlier list. Moreover, I find evidence that depositors re-deposited their funds into banks that were revealed on an earlier list. Altogether, the results are consistent with depositors no longer interpreting news of an LOLR emergency loan as a sign of financial weakness after subsequent banks were revealed. Therefore, all else being equal, revealing bank identities later led to larger withdrawals of deposits.

Having shed light on the timing of the stigma problem, I next ask whether news of an LOLR emergency loan should be interpreted as a sign of financial weakness. I answer this question by examining whether depositors differentially withdrew from banks that privately borrowed from their LOLR compared with banks that never borrowed. If depositors were unable to ex-ante determine banks that approached their LOLR, banks that successfully maintained the secrecy of their loans should be similar on observables to banks that never borrowed. I find that banks that privately borrowed LOLR loans experienced a highly statistically significant *increase* in their level of deposits of 47-65% relative to banks that never borrowed. This finding suggests that these banks were able to credibly signal their healthier balance sheet to depositors. Altogether, these results imply that keeping LOLR loans secret will lead to higher deposit funding at banks that borrow from their LOLR.

Designing emergency lending facilities that never reveal information about bank borrow-

ers may be associated with less stigma, which can be achieved by providing a coordination mechanism at the lending facility for banks to request loans simultaneously.⁵ In the case that identities are revealed, it is likely that an individual bank would not be targeted when approaching the LOLR for a loan. This design feature prevents an asymmetric release of information and avoids banks that borrow later from being stigmatized more than banks that borrow earlier.

After considering depositor reactions to the publication of bank lists beginning on August 22, 1932, I examine the behavior of those banks whose identities were revealed. An aim of the central bank is to ease liquidity needs for solvent yet illiquid banks during a financial crisis where, in turn, these banks use the funding to maintain loans to their consumers (Freixas et al. (1999)). After the revelation, how did banks respond to the withdrawals of deposits? I find that revealed banks decreased their level of bonds-and-securities by 14% compared with never revealed banks. Furthermore, revealed banks decreased their level of loans-and-discounts by 5%. These findings may suggest that revealed banks sold assets off their balance sheets while reducing their loan portfolios in response to deposit withdrawals, following the publication of the lists.

Because researchers face major challenges in measuring stigma, the limited size of the literature reflects this difficulty. Furfine (2003) finds preliminary empirical evidence from the federal funds market that banks were reluctant to borrow from the discount window. Armantier et al. (2014) extend this result by showing that banks are willing to pay a premium to avoid borrowing from the discount window. Finally, Kleymenova (2013) finds little evidence of stigma after the disclosure of discount window borrowers on March 31, 2011.⁶ A

⁵New lending programs created by the Fed during the recent crisis provided such a coordination mechanism.

⁶Kleymenova (2013) finds that the disclosure decreased bid-ask spreads and banks' cost of capital. Bloomberg News filed a lawsuit under the Freedom of Information Act to gain access to the names of

possible reason for the lack of stigma in 2011 is that the disclosure occurred well after the acute stage of the financial crisis had ended. Asymmetric information released in non-crisis times may have less effect because financial fragility is low. However, the same asymmetric information released during a crisis can cause a decline in output and consumption (Gorton and Ordonez (2014)), shedding light on why stigma may exist only during times of economic stress.

This paper also contributes to the literature on the effect of information disclosure from the LOLR on bank runs and on the need for banks to keep secrets. Hautcoeur et al. (2013) show that the French LOLR public bailout of the Comptoir d'Escompte, a large bank in France, in 1889 did not turn into a general panic. Because all major banks were involved in designing the bailout, the public interpreted banks receiving emergency loans as acceptable. Furthermore, during the National Banking Era (1863-1913) in the U.S., private clearing houses responded to severe panics by suppressing bank-specific member information in newspapers, and published only aggregate information about the clearinghouse (Gorton and Talman (2014)). My paper offers an explanation for these historical examples by showing that the differential effects of partial and no disclosure of banks that accept emergency credit can have strong adverse effects on the health of the financial system.

Finally, this paper provides insights into the success of several new emergency lending facilities introduced by the Fed during the recent crisis. The new facilities which included the Term Auction Facility (TAF), all mitigated stigma by providing coordination mechanisms for banks to submit bids for LOLR loans simultaneously through auctions. If leaks were to occur, all borrowers' names would be divulged at the same time. My results show that

discount window borrowers during the recent crisis. As a result, the Fed released the names of the borrowers and cautioned that this action may stigmatize these borrowers. It is not possible to accurately measure the cost of stigma in this setting due to methodological limitations.

such coordination and secrecy help explain why these new facilities were associated with less stigma than the discount window (Benmelech (2012)). By quantifying stigma, central bankers can prioritize whether solving the stigma problem is necessary. I find that the design of an emergency lending facility that allows banks to coordinate requests for LOLR loans can mitigate stigma.

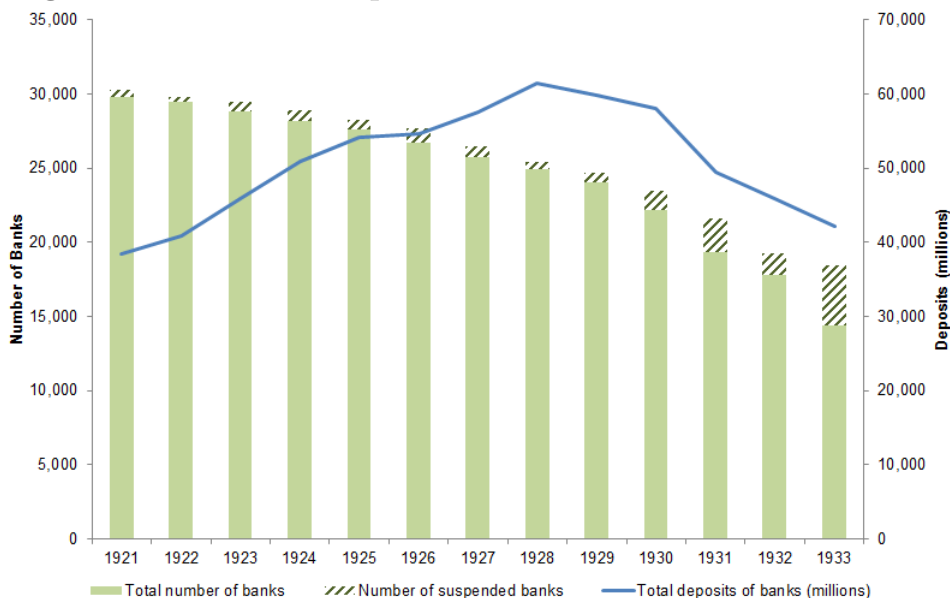
The remainder of the paper is organized as follows. Section 2 discusses the RFC as the LOLR to the U.S. banking system during the Great Depression and also presents the publication of the list on August 22, 1932, the main source of identification used in this paper. Section 3 describes the data. Section 4 presents the results of testing for stigma, and Section 5 describes how revealed banks adjusted their balance sheet following the publication of the lists. Section 6 discusses the relevance of the findings to the recent financial crisis and Section 7 concludes.

2 Identification Strategy

2.1 The Reconstruction Finance Corporation

In response to an acceleration of bank suspensions after Britain left the gold standard in 1931, President Hoover created the RFC (Olson (1977)). Figure 1 illustrates this acceleration and the steady drop in deposit levels after the stock market crash in October 1929.

Figure 1: Trends of Deposits and Banks from 1921 to 1933



This figure describes the banking sector in the U.S. from 1921 to 1933. The line illustrates total deposits (in millions) across all banks in the U.S. over time. The solid bars depict the total number of banks, while the dashed bars reflect the total number of suspended banks. The data are from Wicker (1996).

I assume that the RFC served as the LOLR to the U.S. banking system during the Great Depression despite the presence of the Federal Reserve. At the end of 1931, only 39% of banks were eligible to borrow from the discount window at the Fed (henceforth referred to as “member banks”).⁷ Mitchener and Richardson (2015) show that the withdrawal pressures of non-member banks on member banks magnified liquidity risk during the Great Depression. If all banks had been member banks, systemic withdrawal pressures would have been substantially lower (Calomiris et al. (2016); Board (1932, p. 240)).⁸ As a result, President Hoover argued that another LOLR was needed to provide emergency liquidity

⁷There were 18,734 banks operating in the US as of June 30, 1932. Of these banks, 7,246 were Federal Reserve member banks (Board (1959); Board (1932)).

⁸A bank was more likely to be a Fed member if it was chartered as a national bank rather than a state bank. See Calomiris and Carlson (2015) for more discussion on the decision to become a member bank.

assistance to the remaining non-member banks (Olson (1977)). The RFC Act was submitted to Congress on December 7, 1931, and it was passed into law on January 22, 1932.

The RFC began privately authorizing loans on February 2, 1932 to several types of institutions including commercial banks, insurance companies, and building and loan associations.⁹ Interest rates on loans to banks were typically 5.5% which was about 2 percentage points above the discount rate (Times (1932); Congress (Dec. 18-22, 1931; Jan. 5-7, 1932, p. 62)), and collateral requirements were at the discretion of the RFC. Accepted collateral included gold, Treasury securities, and commercial, industrial, and agricultural paper (Olson (1977)). RFC loans were given with maturities up to three years but banks could roll over their loans for an additional two years (Act (1932b)).¹⁰

Given that 61% of banks were non-member banks and 44% of all banks received loans from the RFC by June 30, 1933, it is likely that the RFC lent to banks for liquidity support. Moreover, it is unlikely that *all* of these banks were facing only solvency concerns (Corporation (1933); Board (1959)). Nonetheless, there were three differences between the RFC and the Fed's discount window. First, the RFC interest rate was 1.5-2 percentage points higher than the Fed's.¹¹ This difference may suggest that banks approaching the RFC were of better quality because they could pay a higher interest rate. Second, discount window loans were offered for a maximum of fifteen days. This inconvenience likely directed member banks to the RFC which was offering longer maturity loans (Board (1932, p. 216)). Third, although the RFC and the Fed were accepting the same set of collateral, the Fed was unable to use it against reserves without expanding its balance sheet by buying gold (the Federal

⁹Of the total amount of bank loans requested from the RFC, 80% were granted. According to Corporation (1932), the amount of bank loan applications received in 1932 was \$1,188,957,193. The amount authorized was \$949,858,000.

¹⁰For more information about the RFC see Olson (1977), Butkiewicz (1995), Mason (2001), Mason and Schiffman (2003), Mason (2009), and Calomiris et al. (2013).

¹¹The discount rate averaged 3.5% across Fed Districts -with the exception of Dallas, at 4% as of January 25, 1932 (Board (1932)).

Reserve Act required that 40% of the Fed’s balance sheet be backed by gold) (Board (1932, p. 19); Act (1932a, Section 10(b))). Therefore, extending a discount window loan could be inconvenient for the Fed because the U.S. was on the gold standard. The Fed authorized only \$33 million of discount window loans to 50 member banks of less than \$5 million in capital in 1932, which likely illustrates this inconvenience (Board (1932, p. 20)). Furthermore, as shown in Figure A.1 in the Online Appendix, aggregate loans outstanding at the Fed declined to \$254 million by December, while the RFC authorized over \$1 billion in loans which funded, on average, 5% of bank balance sheets to more than 5,000 banks in 1932.¹² By the end of 1932, 6,865 eligible institutions (banks and nonfinancial firms) had been authorized over \$1.6 billion in loans by the RFC (Corporation (1932)). This fact highlights the significance of the RFC’s effect on the financial system and its function as an LOLR.¹³

2.2 The publication of the initial list on August 22, 1932

The main source of identification in this paper is the unexpected publication of the identities of banks that had privately borrowed from the RFC beginning on August 22, 1932. I analyze the response of depositors and banks to the publication of the list in a DID setting. In this framework, I avoid the sample selection problem of comparing the performance of banks that chose to borrow from the LOLR with the performance of banks that chose *not* to borrow, because all banks in the estimation borrowed from the RFC.

Initially, the identities of all RFC borrowers (banks and non-banks) were kept secret from the public. Since its establishment, the RFC had used elaborate secret codes to transmit

¹²It is not possible to know which member banks that borrowed from the RFC (which account for about 20% of my sample) also borrowed from the discount window because the data are not yet available.

¹³The RFC’s temporary charter for one year might have introduced uncertainty about its existence and hindered its ability to restore confidence. However, the charter was extended in July 1932 to January 23, 1934 (Act (1932b, Section 201 (h))). But the discount window also had a key temporary feature. The Glass-Steagall Act relaxed the collateral requirements at the discount window to those at the RFC only for one year. This development signals that the easing of standards at the discount window was also temporary perhaps hindering its ability to restore confidence.

messages to its loan agencies and individual banks (Olson (1977)). However, on July 21, 1932, the Emergency Relief and Construction Act of 1932 (ERCA) amended the original RFC Act to expand the RFC's authority into state and local relief, public works construction, slum clearance, and so on. In this act, Section 201 (b) required that monthly reports of borrower names be made known *to Congress only* (Act (1932b)). President Hoover initially planned to veto the bill because of the addition of the last-minute clause but was assured by the Senate Majority Leader that RFC loans would not be revealed to the public without congressional approval (Chronicle (1932b)). It was decided that the monthly reports of borrower names would be confidential and held by the Clerks of the Senate and the House of Representatives until Congress resumed session in December (Act (1932b)). Despite this decision, on August 22, 1932, South Trimble, the Clerk of the House of Representatives, took it upon himself to release a partial list of the identities of banks that accepted loans from the RFC to inform the U.S. public. The list was first published in the *New York Times* and the *Commercial & Financial Chronicle* and coverage of this list was widespread.¹⁴ It was likely that the publication of the list was unexpected given the assurances that no borrower list would be released without congressional approval.¹⁵

The loan authorization date for a bank determined whether the bank identity was revealed. The first monthly report that was submitted by the RFC to Congress revealed banks that had loans authorized between July 21 and July 31, 1932. Since ERCA was passed on July 21, this first monthly report was the only one Mr. Trimble had access to. He regarded the details of any loan authorized after July 20, 1932, to be subject to ERCA, and interpreted the phrase “made known to Congress” in Section 201 (b) of ERCA to indicate the

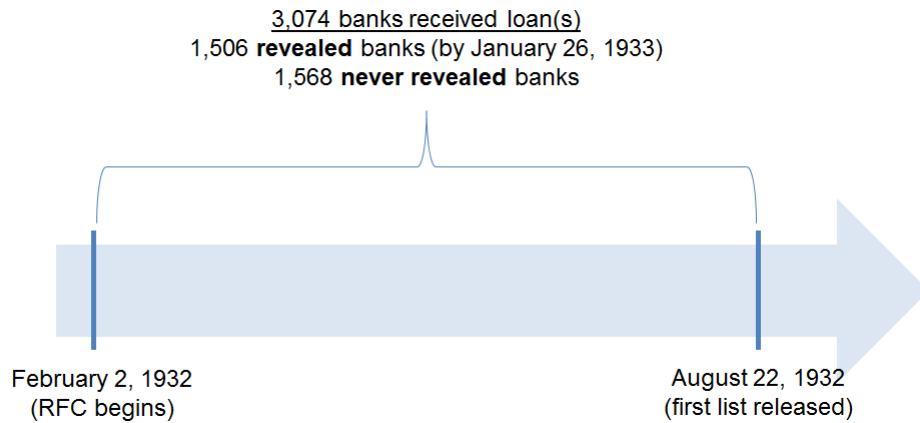
¹⁴I confirm this information by checking several local newspapers such as the *Hartford Courant* and the *Ames Tribune* to determine when the list was published.

¹⁵Newspaper articles do not discuss the revelation of bank borrower identities to the public until Mr. Trimble forewarns of his intended announcement on August 18, 1932.

public (Chronicle (1932a)). Banks not revealed had a loan authorized on or before July 20, 1932, and therefore were not subject to ERCA. Because Congress was not in session, Mr. Trimble published four additional lists of borrower names following the August 22, 1932 list, finishing on January 26, 1933.¹⁶

Figure 2 displays a timeline illustrating which banks were revealed in the *New York Times* and which were never revealed. Between February 2 and August 22, 1932, the RFC authorized loans to 3,074 banks; of that number, 1,506 bank identities were revealed to the public on or after August 22 (henceforth referred to as “revealed banks”), 1,568 bank identities remained secret (henceforth referred to as “never revealed banks”).

Figure 2: Timeline of Loans Authorized prior to August 22, 1932



This figure displays the timeline of publications of bank borrower lists after the RFC began giving loans on February 2, 1932. 3,074 banks received loans between February 2 and August 22, 1932. 1,506 banks were revealed on or after August 22, 1932 (“revealed banks”), while 1,568 banks were never revealed (“never revealed banks”).

2.3 The publications of the subsequent lists

In this paper, I focus on banks with loans authorized before the publication of the first list on August 22, 1932. After this date, a bank choosing to borrow may have anticipated that

¹⁶See Figure A.2 in the Online Appendix for a timeline of all list publications.

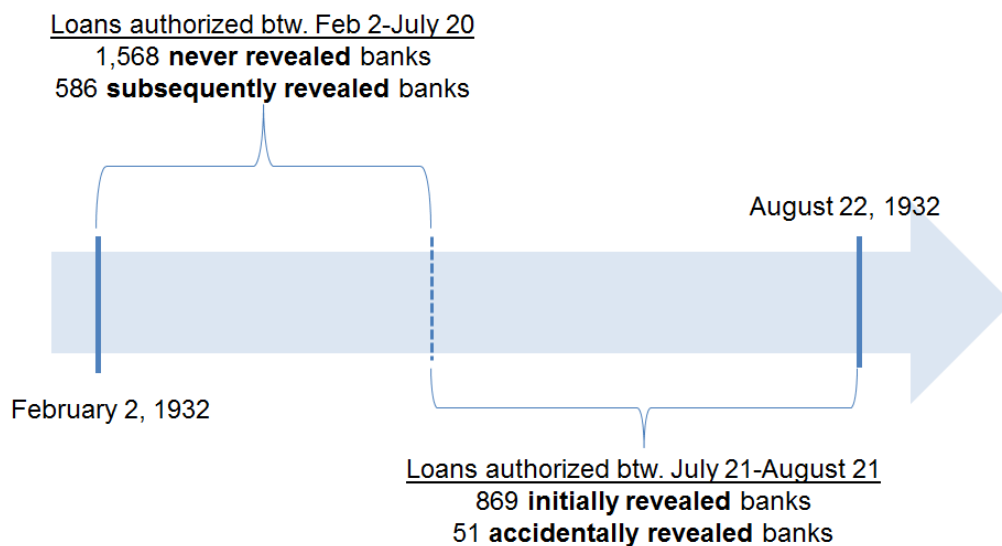
its identity would be revealed following its loan authorization. Moreover, I am unconcerned about the later history of the RFC under President Roosevelt because I focus on loans authorized only in 1932.

Figure 3 displays a timeline illustrating which banks were initially revealed and which were subsequently revealed. Of the 1,506 revealed banks, 869 bank identities with loans authorized between July 21 and August 21, 1932, were initially revealed to the public in 1932. These banks were included on the first and second monthly reports submitted by the RFC to Congress. Mr. Trimble then published each report in the *New York Times* on August 22 and October 7, 1932, respectively. In the following discussion, I refer to these 869 bank identities as “initially revealed banks.”

On January 26, 1933, Mr. Trimble published a final unexpected retroactive list that included the names of RFC borrowers with loans of less than \$100,000 that were authorized on or before July 20, 1932.¹⁷ In the following discussion, I refer to these 586 bank identities as “subsequently revealed banks.” Consequently, comparisons of initially revealed banks with subsequently revealed banks focus on outcomes between August 22, 1932, and January 26, 1933, before the subsequently revealed group was revealed. Finally, 51 banks were revealed in the *New York Times* that had not actually applied for any RFC loans. In the following discussion, I refer to these bank identities as “accidentally revealed banks.”

¹⁷Of the 2,154 banks that had loans authorized prior to July 20, 1932, 586 had loans larger than \$100,000, and 1,568 bank identities remained private (never revealed banks) because their loans authorized were less than \$100,000 (Corporation (1933)).

Figure 3: Timeline by List Publication



This figure displays the timeline illustrating which bank identities were initially revealed and which were subsequently revealed. Of 2,154 bank identities with loans authorized on or before July 20, 1932, 1,568 bank identities were never revealed to the public (“never revealed banks”). The remaining 586 bank identities were revealed on January 26, 1933 (“subsequently revealed banks”). Banks with loans authorized between July 21 and August 21, 1932 were initially revealed on either August 22 or October 7, 1932 (“initially revealed banks”). 51 bank identities were revealed that had not actually applied for any RFC loans (“accidentally revealed banks”).

It is important for my identification that the list of revealed banks be chosen by Mr. Trimble in a way that is uncorrelated with the outcome variables used in the estimation. There should be nothing systematically important about the dates of loan authorizations that he chose to publish implying that the decision of *when* a bank chose to borrow from the RFC also needs to be uncorrelated with all outcome variables. Otherwise, the revealed and never revealed groups may differ along a number of observable dimensions, thereby biasing the results of the estimation.

I perform four robustness checks to test the validity of my identification strategy. First, I restrict the never revealed group to include only banks with loans authorized between June

20 and July 20, 1932, and compare the performance of these banks with that of initially revealed banks. In this test, I compare short symmetric windows of authorized loans to alleviate any concerns of loan authorization timing.

Second, I compare the performance of banks that were revealed that had not actually applied for any RFC loans (accidentally revealed banks) with that of the never revealed banks. If depositors were unable to ex-ante determine which banks approached their LOLR, we should observe drops in deposit levels at the accidentally revealed banks relative to the never revealed banks, as depositors would respond to the list publications rather than alternative bank characteristics.

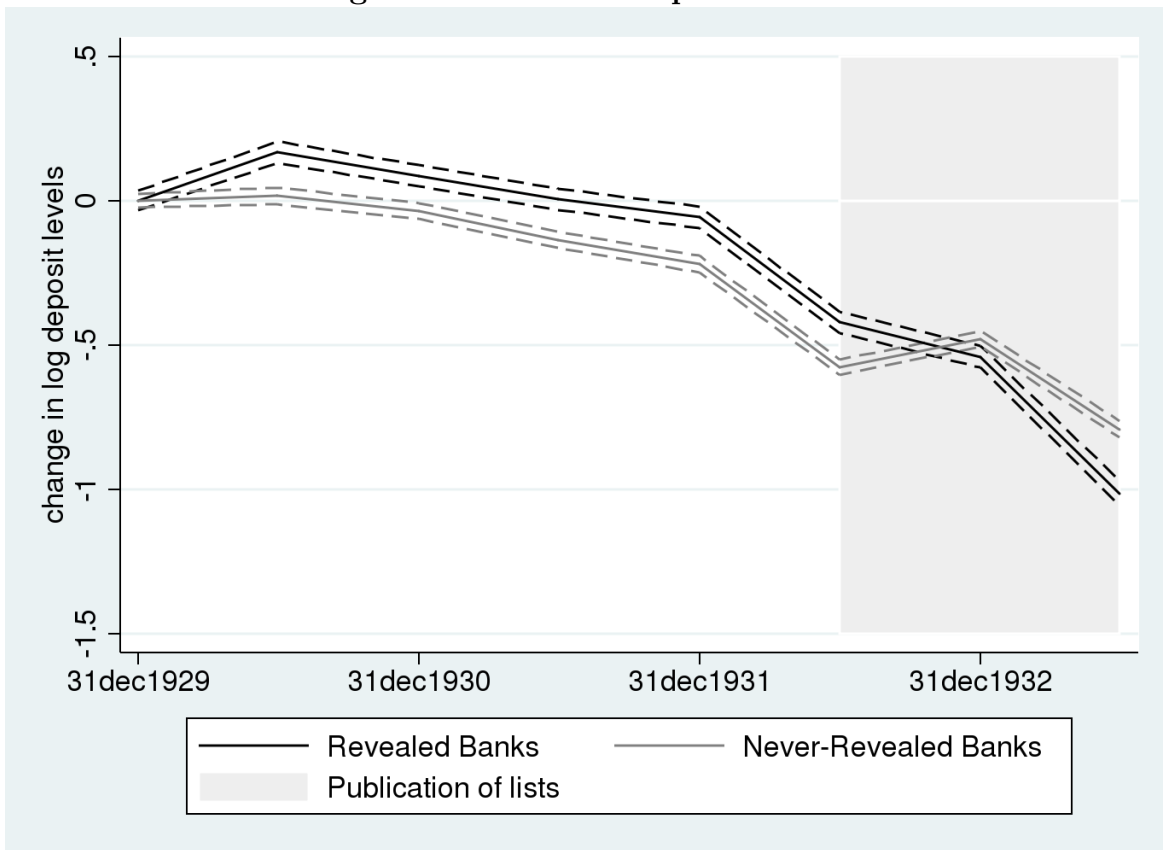
Third, I exploit differences in the timing of loan authorization and loan application dates. Mr. Trimble initially revealed banks with *loans authorized* between July 21 and August 21, 1932, which does not necessarily correlate with the actual date the bank applied for the loan. Historical evidence suggests that the time period between loan application and authorization varied across banks. For example, Olson (1977) describes how RFC board examiners would visit a bank to discuss a loan application and assess the bank's financial condition. He explains that a loan authorization might be delayed if there was not a prior relationship between the bank and the RFC.¹⁸ As long as the period between loan application and authorization varied across banks, the revealed and never revealed groups were assigned quasi-randomly.

Fourth, a crucial assumption for the DID estimation to be valid is that revealed banks and never revealed banks should have parallel trends were it not for the publication of the list. In the absence of the event, the observed DID estimates should be zero. In Figure 4, I plot the average change in log deposit levels at revealed and never revealed banks from December 31,

¹⁸Olson (1977) discusses how the process of authorizing an RFC loan was often delayed. The RFC Act, Section 8, mandates that every applicant must consent to an examination by RFC loan examiners which resulted in delays in loan authorization because of the examiners' physical travel to the bank's location. These delays would also result in the governor intervening on behalf of a bank in her state to request that the RFC to send funds expeditiously. For example, the governor of Tennessee intervened on behalf of the Bank of America and Trust Company.

1929 to June 30, 1933, along with 5% confidence intervals. The graph shows that both groups follow parallel trends prior to the publication of the lists, providing more evidence that the revealed and never revealed groups did not differ with respect to log deposit levels. Deposits then dropped sharply for revealed banks compared with never revealed banks following the publication of the lists.

Figure 4: Trends in deposit levels



This figure displays the average change in log deposit levels for 1,506 revealed banks compared with 1,568 never revealed banks, normalized by deposit levels at December 31, 1929. The dashed lines denote 5% confidence intervals. The darker shaded area between June 30, 1932 and June 30, 1933 denotes the period banks were revealed (on or after August 22, 1932).

3 Data

3.1 Reconstruction Finance Corporation loan data and bank balance sheet information

RFC loan information and borrower names are from the *RFC Card Index to Loans Made to Banks and Railroads 1932-1957*, acquired from the National Archives.¹⁹ The cards report the name and address of the borrower, date, request and amount of the loan, whether the loan was approved or declined, and loan renewals. The names of banks revealed to the public are from the *New York Times* and verified in the *Commercial & Financial Chronicle*. These announcements included the loan amounts and interest rates. All data are hand-collected.

Bank balance sheet data are from *Rand McNally Bankers' Directory*, which was published every six months. I collect the amounts of paid-up capital, surplus and profits, deposits, other liabilities, loans and discounts, bonds and securities, miscellaneous, cash due from other banks, and the name of the president for each bank. The coverage of the *Directory* seems to be comprehensive across the U.S. as only 59 banks that had loans authorized from the RFC were not matched in the *Directory*. The data are hand-collected from eight books beginning December 31, 1929 and continuing to June 30, 1933, resulting in eight observations per bank. I exclude repeated observations which account for 12% of these data but, in a robustness check, I ensure that this exclusion does not affect any of the results.

The three bank-specific outcomes of interest in this paper are log deposits, log loans-and-discounts, and log bonds-and-securities. I do not observe the detailed composition of deposits, loans and discounts, or bonds and securities. The total book value of deposits provided in *Rand McNally Bankers Directory* equals the sum of demand deposits and time

¹⁹I thank Angela Vossmeier for providing pictures of the cards from the National Archives.

deposits. The total book value of loans-and-discounts equals the sum of the book value of loans to banks and trust companies, loans on securities (to brokers and dealers), real estate loans and mortgages, and all other loans. Finally, the total value of bonds-and-securities in the *Directory* equals the sum of the book value of U.S. government securities; state, county, and municipal bonds; railroad bonds; all other bonds; stock of the Federal Reserve Bank; stock of other corporations; foreign government bonds; other foreign securities; the real estate value of the banking house; other real estate owned; and the value of furniture and fixtures within the banking house.

In addition, I collect the date a bank failed, was suspended (in conservatorship), was merged, or was bought from the *Rand McNally Bankers' Directory*. Bank failures during this period must be recorded carefully because suspended banks may have reemerged under new leadership and should not be considered failed banks (Calomiris and Mason (2003)). I supplement the information from the *Directory* by using *Moody's* volumes and the *Individual Statements of Condition of National Banks* issued by the Office of the Comptroller of the Currency. In these data, receiverships with a confirmed failure date and voluntary liquidations are treated as bank failures, identical to the methodology used by Calomiris and Mason (2003). Because *Rand McNally Bankers' Directory* reports only the date a bank failed (was suspended) and not its balance sheet information directly before its failure (suspension), I record these balance sheet values as zero. However, as a robustness check, I proxy for the level of deposits, the main variable of interest used in this paper, at failed banks with the average amount of suspended deposits in the state of the bank's location.

The RFC authorized 10,641 loans to 5,528 unique banks in 1932. I match 5,469 banks to balance sheet information in *Rand McNally Bankers' Directory*. I exclude banks that failed before August 22, 1932 (the RFC was allowed to authorize loans to banks under

conservatorship) because of unavailable balance sheet data. The final sample includes 4,648 unique banks, of which 3,074 received RFC loans prior to August 22, 1932.

3.2 Additional covariates

To account for differing macroeconomic trends across each state, I include several additional control variables in the estimation. These variables also capture the broader health of the banking system. I use the dollar amount of total deposits and the total number of banks in each state to account for the size, organization, and resources of the banking system. Next, I use the dollar amount of suspended deposits and the total number of suspended banks in each state to account for the health of the banking system. Suspended banks include both banks that closed their doors to depositors for at least one business day and later resumed operations, and banks that ceased operations, surrendered their charters, and repaid creditors under a court-appointed receiver (Heitfield et al. (2013)). The data are from the *FDIC Bank Deposit Data*, 1920-1936 (Inter-university Consortium for Political and Social Research), and are available yearly. I also use yearly state level per capita income data from the U.S. Bureau of Economic Analysis, *Survey of Current Business*, May 2002. Per capita income captures the potential cross-sectional variation in depositor income across states.

4 Testing for stigma

4.1 Empirical specification

To compare the performance of revealed and never revealed banks after the publication of lists beginning on August 22, 1932, I run the following bank-level ordinary least squares

(OLS) regression from June 30, 1930 to June 30, 1933:

$$Y_{i,t} = \alpha + \beta_1 \text{Revealed}_i \times \mathbb{I}\{t \geq \text{List}\} + \gamma X_{i,t-1} + \eta_t + \delta_i + \epsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the outcome of interest measured every six months t for bank i . *Revealed* is a dummy equal to 1 if the bank was ever revealed on or after August 22. $\mathbb{I}\{t \geq \text{List}\}$ is a dummy equal to 1 following the start of list publications on August 22, 1932. The coefficient of interest is β_1 , which measures the change in Y_i following the publication of the lists for revealed banks compared with never revealed banks relative to their controls.²⁰ The main outcome variable used is the log level of deposits. Other outcome variables are the log level of loans-and-discounts and log level of bonds-and-securities. For failed banks, I proxy for the log level of deposits with the average bank amount of suspended deposits in the state of the bank's location so that I can include failed banks in the estimation. The results are also robust to recording zero for the deposit balance at failed banks.

To compare the performance of initially revealed and subsequently revealed banks after the publication of lists beginning on August 22, 1932, I run the following bank-level OLS regression from June 30, 1930 to June 30, 1933:

$$Y_{i,t} = \alpha + \beta_1 \text{Initially} - \text{Revealed}_i \times \mathbb{I}\{t = \text{List}\} + \beta_2 \text{Initially} - \text{Revealed}_i \times \mathbb{I}\{t = \text{List} + 1\} + \gamma X_{i,t-1} + \eta_t + \delta_i + \epsilon_{i,t} \quad (2)$$

where $Y_{i,t}$ is the outcome of interest measured every six months t for bank i . $\mathbb{I}\{t = \text{List}\}$ is a dummy equal to 1 following the publication of the lists on August 22, 1932 and October 7, 1932. December 31, 1932 is the first balance sheet date I observe for banks after the

²⁰Note that I do not include a $\mathbb{I}\{t \geq \text{List}\}$ dummy or a *Revealed* dummy since they are not identified once I include half-year and bank fixed effects.

publication of both lists. $\mathbb{I}\{t = List + 1\}$ is a dummy equal to 1 following the publication of the retroactive list on January 26, 1933 revealing the subsequently revealed banks. *Initially – Revealed* is a dummy equal to 1 if the bank was publicly revealed on either of the first two lists. The coefficient of interest is β_1 , which measures the change in Y_i following the publication of the lists for initially revealed banks compared with subsequently revealed banks relative to their controls.²¹ The coefficient β_2 measures the change in Y_i following the publication of the retroactive list on January 26, 1933 for the initially revealed banks compared with the subsequently revealed banks.

A key issue that prevents both specifications from identifying the effect of the revelation on $Y_{i,t}$ is that $Y_{i,t}$ may be correlated with unexplained macroeconomic conditions and/or bank borrower characteristics in the error term $\epsilon_{i,t}$. Therefore, I include controls, $X_{i,t-1}$, to mitigate this bias and lag the covariates to ensure that $X_{i,t-1}$ does not confound $Y_{i,t}$. $X_{i,t-1}$ is a vector of controls measured six months prior to $Y_{i,t}$ and includes the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state level deposits, the log of state level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. These covariates are intended to capture observable proxies for macroeconomic conditions and bank characteristics that might explain $Y_{i,t}$. However, the specification may still be biased if some bank characteristics are unobservable. Therefore, I rely on bank fixed effects, δ_i , to exclude biases that could result from time-invariant bank characteristics and to capture the extent to which each bank affects $Y_{i,t}$. For example, δ_i will help control for unobservable differences between member and non-member banks. Additionally, I include half-year fixed effects, η_t , to account for time trends in $Y_{i,t}$, eliminating the concern that

²¹Note that I do not include a $\mathbb{I}\{t = List\}$ dummy, a $\mathbb{I}\{t = List + 1\}$ dummy, or an *Initially – Revealed* dummy since they are not identified once I include half-year and bank fixed effects.

aggregate changes in $Y_{i,t}$ and the publication of the lists occurred together.

Finally, standard errors are clustered at the bank level according to Bertrand et al. (2004). $X_{i,t-1}$ also includes the lagged log level of deposits to account for autocorrelation in the dependent variable (Petersen (2009)). Furthermore, all continuous variables are winsorized at the 1% level to avoid outliers driving the estimation results.

4.2 Summary statistics

Table 1 displays descriptive statistics that compare banks having loans authorized between July 21 and August 21, 1932 (initially revealed banks) with banks having loans authorized on or before July 20, 1932 (subsequently revealed and never revealed banks). Panel A presents the summary statistics on bank-level balance sheet information for June 30, 1932, the last balance sheet date I observe prior to the first list being published on August 22, 1932. The summary statistics show few differences between these groups of banks. This result suggests that banks with loans authorized prior to July 20 are similar to banks with loans authorized after July 20, providing evidence that Mr. Trimble did not choose the revealed banks in a way that is correlated with the outcome variables used in the estimation. However, it does appear that banks with loans authorized prior to July 20 funded the asset sides of their balance sheets with less deposits than banks with loans authorized after July 20. Panel B presents summary statistics describing the loans authorized to these groups of banks during 1932. Banks with loans authorized after July 20 and banks with loans authorized on or before July 20 received similar amounts of loans.

Table 1: Summary Statistics as of June 30, 1932

This table presents summary statistics for initially revealed, never revealed, and subsequently revealed banks prior to the publications of lists of bank borrowers beginning on August 22, 1932. There are 869 banks with loans authorized between July 21 and August 21, 1932 (initially revealed banks), and 2,154 banks with loans authorized on or before July 20, 1932 (never revealed and subsequently revealed banks). Panel A presents bank-level statistics as of June 30, 1932, while Panel B presents loan-level statistics for all of 1932. The loans-and-discounts value is the book value of the bank's loans outstanding. The bonds-and-securities value is the book value of the bank's bond portfolio. Total assets and loan amount are in thousands.

June 30, 1932 Panel A

	Initially Revealed Banks (869)			Never/Subsequently Revealed Banks (2154)			Diff. in Means
	Obs.	Mean	Std. Dev.	Obs	Mean	Std. Dev.	
total assets	869	2311.2	10854.4	2,154	2088.9	7955.75	
ln(total assets)	869	6.57	1.33	2,154	6.51	1.33	
$\frac{\text{Deposits}}{\text{Assets}}$	869	0.71	0.12	2,154	0.69	0.11	***
ln(deposits)	869	6.21	1.36	2,154	6.11	1.38	
ln(loans)	869	6.17	1.28	2,154	6.1	1.28	
ln(bonds)	869	5.2	1.66	2,154	5.16	1.66	

1932 Panel B

	Initially Revealed Banks (869)			Never/Subsequently Revealed Banks (2154)			Diff. in Means
	Obs.	Mean	Std. Dev	Obs	Mean	Std. Dev	
Loan Amount (in thous.)	869	30.9	85.5	2,154	29.2	78.5	

Table 2 displays summary statistics describing the number of bank failures among the revealed and never revealed groups. Table 2 illustrates that 4.2% of revealed banks failed within four months of the first list being published while essentially zero of the never revealed banks failed. This outcome provides preliminary evidence that revealed banks were more likely to fail after the start of list publications. Overall, most bank failures occurred after January 26, 1933 because of the national banking holiday between March 6 and March 13, 1933 when President Roosevelt closed all banks across the U.S.

Table 2: Summary Statistics on Failed Banks

This table presents summary statistics for the number of bank failures among the 3,074 banks in the revealed and never revealed groups. Failed banks are banks that did not emerge from conservatorship or were liquidated. Percentages of bank failures are provided in parentheses.

	3,074 banks Total	1,506 banks Revealed Banks	1,568 banks Never Revealed Banks
Failed	1,028 (33.4%)	521 (34.6%)	507 (32.3%)
Failed in 1932	65 (2.1%)	64 (4.2%)	1 (0.01%)
Failed by June 30, 1933	963 (31.3%)	457 (30.3%)	506 (32.3%)

4.3 The effect of the list publications on deposits

I start with the DID estimation of the effect of list publications in the *New York Times* of RFC borrowers beginning on August 22, 1932 on log deposits at the revealed banks compared with the never revealed banks. Table 3 presents the main results. Revealed banks experienced a highly statistically significant drop in their log level of deposits by 18-25%. This finding translates to the share of deposits over total assets decreasing by 11-16%. Column (2) adds bank- and state-level controls, which slightly lowers the coefficient, though it remains highly economically and statistically significant. Columns (1) and (2) also include one additional interaction term with *Revealed*. I find no differential effect with respect to the log level of deposits between revealed and never revealed banks prior to the publication of the list. This result provides evidence that the parallel trend assumption, which is crucial for the validity of the DID estimate, holds in the data. These results are consistent with depositors interpreting the news that a bank received an LOLR loan as a sign of financial weakness.

Table 3: Effect of the Published Lists on Revealed Banks

This table presents the DID estimates of the effect of list publications of RFC borrowers beginning on August 22, 1932 on log deposits. *Revealed* is a dummy that equals 1 if the bank was revealed on or after August 22, 1932 (revealed banks). There are 1,506 revealed banks and 1,568 never revealed banks. $Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for revealed banks after the August 22 list was published, and 0 otherwise. $Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
$Revealed_i \times \mathbb{I}\{t = List - 1\}$	0.0187 (0.0111)	-0.0042 (0.012)
$Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.2516*** (0.0256)	-0.1815*** (0.0237)
$log\ deposits_{t-1}$	0.4519*** (0.0344)	0.3451*** (0.0616)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	15,795	15,795
R^2	0.9151	0.9232

The main concern of the identification strategy is that the revealed and never revealed groups may differ along a number of observable dimensions that might be correlated with the outcome variables and bias the estimation. I address this concern through a compelling robustness check (mentioned in the “Identification Strategy” section). I restrict the loan authorization window for the never revealed group to a short window of time by including only banks with loans authorized between June 20 and July 20, 1932, and compare these banks with initially revealed banks. Table 6 presents the results of this test, which uses a

narrow time window of authorized loans. Initially revealed banks experienced a 7-11% drop in their log level of deposits compared with never revealed banks. The drop in deposits at initially revealed banks continues to be economically and statistically significant. In addition, Table 4 provides evidence that the parallel trend assumption still holds for banks that had loans authorized in June and July.

Table 4: Effect of the Initial Lists on RFC Borrowers in June and July

This table presents the DID estimates of the effect of the August 22 and October 7, 1932 lists of RFC borrowers on log deposits at the initially revealed banks compared with never revealed banks with loans authorized between June 20 and July 20, 1932. *Initially – Revealed* is a dummy that equals 1 if the bank was revealed on August 22 or October 7, 1932 (and had a loan authorized between July 21 and August 21, 1932). There are 869 initially revealed banks and 454 never revealed banks with loans authorized between June 20 and July 20, 1932. *Initially – Revealed_i × I{t ≥ List}* is a dummy equal to 1 for initially revealed banks after the August 22 list was published, and 0 otherwise. *Initially Revealed_i × I{t = List – 1}* is a dummy equal to 1 for initially revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
<i>Initially Revealed_i × I{t = List – 1}</i>	-0.0015 (0.0174)	-0.0063 (0.0184)
<i>Initially Revealed_i × I{t ≥ List}</i>	-0.1106*** (0.0374)	-0.0695** (0.0353)
<i>log deposits_{t-1}</i>	0.4617*** (0.064)	0.4616*** (0.1249)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	6,687	6,687
R^2	0.9138	0.925

Next, another robustness check I perform (also mentioned in the “Identification Strategy” section) is to exploit the revelation of 51 banks that had not actually applied for any RFC loans (the accidentally revealed banks). If depositors were able to ex-ante determine which banks approached their LOLR, we should not observe any drop in deposit levels at these banks because depositors would not have responded to the list publications in the *New York Times*. Table 5 presents the results for the accidentally revealed banks. These banks experienced a highly statistically significant drop in their log level of deposits by 15-22%. This outcome suggests that depositors did not withdraw deposits from accidentally revealed banks any differently than revealed banks after the publication of the lists; in fact, the magnitudes of deposit drops in Tables 5 and 3 are quite similar. Columns (1) and (2) also include one additional interaction term with *Accidentally Revealed*. I find no differential effect with respect to the log level of deposits between accidentally revealed and never revealed banks prior to the publication of the list. This finding provides evidence that the parallel trend assumption holds for banks that did not apply for an RFC loan compared with never revealed banks that did apply for a loan. Consequently, the results imply that depositors were unable to ex-ante determine which banks approached their LOLR, and that they responded to the list publications in the *New York Times* rather than alternative bank characteristics.

Table 5: Effect of the Published Lists on Accidentally Revealed Banks

This table presents the DID estimates of the effect of the list publications of RFC borrowers beginning on August 22, 1932 on log deposits at accidentally revealed banks. *Accidentally Revealed* is a dummy that equals 1 if the bank was revealed on or after August 22, 1932 but did not apply for a loan. There are 51 accidentally revealed banks and 1,568 never revealed banks. $Accidentally\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for revealed banks after the August 22 list was published but had not applied for a loan, and 0 otherwise. $Accidentally\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for revealed banks before the August 22 list was published but had not applied for a loan, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits</i>	
	(1)	(2)
$Accidentally\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	0.0152 (0.0235)	-0.0004 (0.0238)
$Accidentally\ Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.2155*** (0.0617)	-0.1534*** (0.0563)
$log\ deposits_{t-1}$	0.3261*** (0.0419)	0.2268*** (0.072)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	8,401	8,401
R^2	0.9187	0.9254

Finally, Table A.3 in the Online Appendix provides evidence that the drop in log deposits due to the publication of lists beginning on August 22 was not driven by the amount borrowed from the RFC. Depositors did not withdraw more from banks that borrowed larger amounts.

4.4 The effect of the timing of list publications on deposits

Next, I focus on the timing of list publications and its effect on bank deposits. Would depositors still interpret the news that a bank received an LOLR loan as a sign of financial weakness if the bank was revealed prior to other banks?

First, I focus on the effect of the list publications on deposits at banks revealed earlier (initially revealed banks) compared with banks revealed later (subsequently revealed banks). If stigma were to exist regardless of the date a loan was authorized, we should expect initially revealed and subsequently revealed banks to both experience a drop in log deposits following the publication of the lists relative to never revealed banks. Table 6 presents the results for the initially revealed banks. These banks experienced a highly statistically significant drop in their log level of deposits of 14-19% compared with the never revealed banks. This result suggests that the drop in deposit levels due to the publication of lists occurred regardless of the bank's loan authorization date. Table 7 presents the results for the subsequently revealed banks. These banks also experienced a highly statistically significant drop in their log level of deposits of 31-32% compared with the never revealed banks, a magnitude far larger than that for the initially revealed banks. This outcome suggests that, all else being equal, revealing banks later led to larger withdrawals of deposits.

Table 6: Effect of the Initial Lists on Initially Revealed Banks

This table presents the DID estimates of the effect of the August 22 and October 7, 1932 lists of RFC borrowers on log deposits at the initially revealed banks. *Initially Revealed* is a dummy that equals 1 if the bank was revealed on August 22 or October 7, 1932. There are 869 initially revealed banks and 1,568 never revealed banks. $Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for initially revealed banks after the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for initially revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
$Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	-0.0036 (0.0137)	-0.0055 (0.0138)
$Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.189*** (0.0291)	-0.1414*** (0.0246)
$log\ deposits_{t-1}$	0.4106*** (0.05)	0.3442*** (0.106)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	12,078	12,078
R^2	0.9162	0.9242

Table 7: Effect of the Retroactive List on Subsequently Revealed Banks

This table presents the DID estimates of the effect of the retroactive January 26, 1933 list of RFC borrowers on log deposits at subsequently revealed banks. *Subsequently Revealed* is a dummy that equals 1 if the bank was revealed on January 26, 1933. There are 586 subsequently revealed banks and 1,568 never revealed banks. $Subsequently\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for subsequently revealed banks after the August 22 list was published, and 0 otherwise. $Subsequently\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for subsequently revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
$Subsequently\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	0.0514*** (0.015)	0.0021 (0.0167)
$Subsequently\ Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.3244*** (0.0395)	-0.3091*** (0.0246)
<i>log deposits_{t-1}</i>	0.4489*** (0.0352)	0.2866*** (0.0675)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	10,954	10,954
R^2	0.9152	0.9242

Next, I focus on how deposits flowed between the initially and subsequently revealed banks following the publication of the lists. Table 8 Column (1) presents the results of the DID estimation comparing initially revealed banks with subsequently revealed banks (banks that were revealed on January 26, 1933). Column (1) shows that initially revealed banks experienced a statistically significant drop in their log deposit levels of 8.86%. The drop in the log level of deposits starts immediately after the list was published on August 22, 1932. However, after the subsequently revealed group was revealed, the coefficient on

$Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$ indicates an increase of 23.8% in log deposit levels at initially revealed banks. This result suggests that depositors re-deposited their funds into the initially revealed banks after the subsequently revealed banks were revealed. The results are consistent with depositors no longer interpreting news of LOLR loans as a sign of financial weakness for the initially revealed banks. These banks were able to experience higher deposit funding than subsequently revealed banks following the publication of the lists.

Column (2) presents the results of the DID estimation comparing initially revealed banks with subsequently revealed banks that had loans authorized between June 20 and July 20, 1932. Using this narrow time window of authorized loans, Column (2) shows a drop in the log level of deposits of 13.25%, which starts immediately after the list was published on August 22, 1932. However, although the coefficient on $Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$ indicates an increase in log deposit levels at the initially revealed banks, the coefficient is not statistically significant. Despite this finding, the results imply that log deposit levels at the subsequently revealed banks with loans authorized between June 20 and July 20, 1932 caught up to the initially revealed banks, suggesting that depositors withdrew from all RFC borrowers equally after all banks were revealed. All else being equal, banks revealed earlier experienced higher deposit funding than banks revealed later.

Table 8: Effect of the Subsequent List on RFC Borrowers

Column (1) presents the DID estimates of the effect of the August 22 and October 7, 1932 lists of RFC borrowers on log deposits at the initially revealed banks compared with the delayed revealed banks. There are 869 initially revealed banks and 586 subsequently revealed banks. Column (2) presents the DID estimates of the effect of the August 22 and October 7, 1932 lists on log deposits at the initially revealed banks compared with the 227 subsequently revealed banks with loans authorized between June 20 and July 20, 1932. *Initially Revealed* is a dummy that equals 1 if the bank was revealed on August 22 or October 7, 1932. $Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for initially revealed banks after the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for initially revealed banks before the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$ is a dummy equal to 1 for banks revealed after the retroactive January 26, 1933 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
$Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	0.032 (0.0215)	-0.0176 (0.0309)
$Initially\ Revealed_i \times \mathbb{I}\{t = List\}$	-0.0886** (0.0455)	-0.1325** (0.0548)
$Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$	0.238*** (0.088)	0.2139 (0.1458)
$log\ deposits_{t-1}$	0.4223*** (0.1317)	0.4558*** (0.1538)
$X_{i,t-1}$	Yes	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	7,226	4,114
R^2	0.9097	0.913

4.5 Empirical specification to analyze bank failure rates

The performance of a bank can also be measured by whether the bank failed. I examine the effect of the revelation on bank failure rates between August 22, 1932 and June 30, 1933 using a duration model. The benefit of using a duration model is that it incorporates the history of bank failures over time, thereby allowing me to exploit knowing the exact date a bank was suspended. I define a failed bank as a bank that did not emerge from conservatorship or was liquidated, identical to the methodology used by Calomiris and Mason (2003). Furthermore, I also test whether the failure of revealed banks was accelerated due to the revelation.

I estimate the duration model using maximum likelihood estimation. The likelihood can be written as a function of the hazard and survival functions. Let i index all banks that were operating as of June 30, 1932, i.e. all banks in both the treatment and control groups. If a bank did not suspend operations by June 30, 1933, it is treated as a right-censored observation in the analysis. Let t index the duration time *in weeks* since the August 22, 1932 list was published. Let $\theta(t; X)$ be the hazard function of the probability that bank i failed at time t , conditional on surviving until t , with time-invariant covariates X . For a discrete time duration model, the likelihood function is as follows:

$$\mathcal{L} = \prod_{p-1}^p \left[\theta(t_p; X) \prod_{s=0}^{t_p-1} (1 - \theta(s; X)) \right] \quad (2)$$

To estimate the hazard function, $\theta(t; X)$, I use a parametric hazard function following a Weibull distribution. Under the Weibull assumption, the hazard function takes the following form:

$$\theta(t; X) = \rho\lambda(X)(t\lambda(X))^{\rho-1} \quad (3)$$

$$\lambda(X) = \exp\{X\beta\} \quad (4)$$

The parameter ρ is the scale parameter of the hazard function. For example, if $\hat{\rho}$ is estimated to be between 0.5 and 1, then the hazard function is increasing at a decreasing rate. The shape of the baseline hazard denotes the average bank failure rate over time.

The specification used in the analysis is estimated on a weekly basis from August 22, 1932 to June 30, 1933. However, the ideal hazard specification would also incorporate bank-level time-varying covariates to account for time-varying differences in the baseline hazard each week. Because I observe bank balance sheet information only every six months, I estimate the baseline hazard using time-invariant covariates fixed as of June 30, 1932, the first balance sheet date I observe prior to the publication of the list on August 22, 1932. Therefore, the following specification estimates the probability a revealed bank failed by June 30, 1933 (the date of the last balance sheet data I observe) compared with a never revealed bank. I estimate ρ and β_1 under the following parameterization:

$$\ln(\lambda) = \alpha + \beta_1 \text{Revealed}_i + \gamma X_{i, \text{June } 30, 1932} + \sigma u \quad (5)$$

where u follows a Type-1 extreme value distribution and σ is a shape parameter that is equivalent to $\frac{1}{\rho}$. Hence, β_1 , the coefficient of interest, measures the logged probability that a revealed bank failed by June 30, 1933 compared with never revealed banks. *Revealed* is a dummy equal to 1 if the bank was publicly revealed. $X_{i, \text{June } 30, 1932}$ is a vector of controls measured at June 30, 1932 and includes the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level.

Table 9: Effect of the Published Lists on RFC Bank Failures

This table presents the parametric hazard function following a Weibull distribution estimate of the effect of the publication of lists beginning on August 22, 1932 on bank failure rates. *Revealed* is a dummy that equals 1 if the bank was revealed on or after August 22, 1932 (revealed banks), conditional on surviving until August 21, 1932. Column (1) provides the exponentiated hazard coefficients presenting the probability of failure by June 30, 1933 while Column (2) provides the exponentiated accelerated time-to-failure coefficients. $X_{i,June\ 30,1932}$ are fixed bank-level controls measured prior to August 22, 1932 including the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)
	Hazard	Accelerated Time-to-Failure
<i>Revealed_i</i>	1.779*** (0.221)	0.8033*** (0.039)
$X_{i,June\ 30,1932}$	Yes	Yes
σ	0.38	0.38
Obs.	3,074	3,074
Wald $\chi^2(9)$	150.05	150.05

4.6 Analyzing bank failure rates

Table 9 shows that revealed banks were more likely to fail after the lists were published compared with never revealed banks. The exponentiated regression coefficients for the hazard model and the accelerated time-to-failure model are displayed. Revealed banks were 77.9% more likely to fail by June 30, 1933 compared with never revealed banks. Moreover, conditional on failure, revealed banks failed 19.7% faster than never revealed banks after the publication of the list (1 - 0.8033). This outcome translates to an acceleration of nearly nine weeks.

Although it is difficult to estimate the marginal contribution of the revelation to deposit

withdrawals and likelihood of failure given the poor macroeconomic conditions during the Great Depression, the hazard analysis provides some perspective on the economic importance of the main results. Taken alone, the importance of an 18.15-25.16% drop in the log level of deposits at revealed banks compared with never revealed banks is difficult to assess. However, in 1932, banks lost 81% of their deposits, on average, before failing (Wicker (1996)). Taking this fact into consideration, along with the 77.9% increase in probability that a revealed bank would fail, suggests that the marginal contribution of the revelation was considerable. Given the many problems banks faced during this period, the stigma introduced by the revelation was a problem that could have been fixed.

4.7 Is stigma justified?

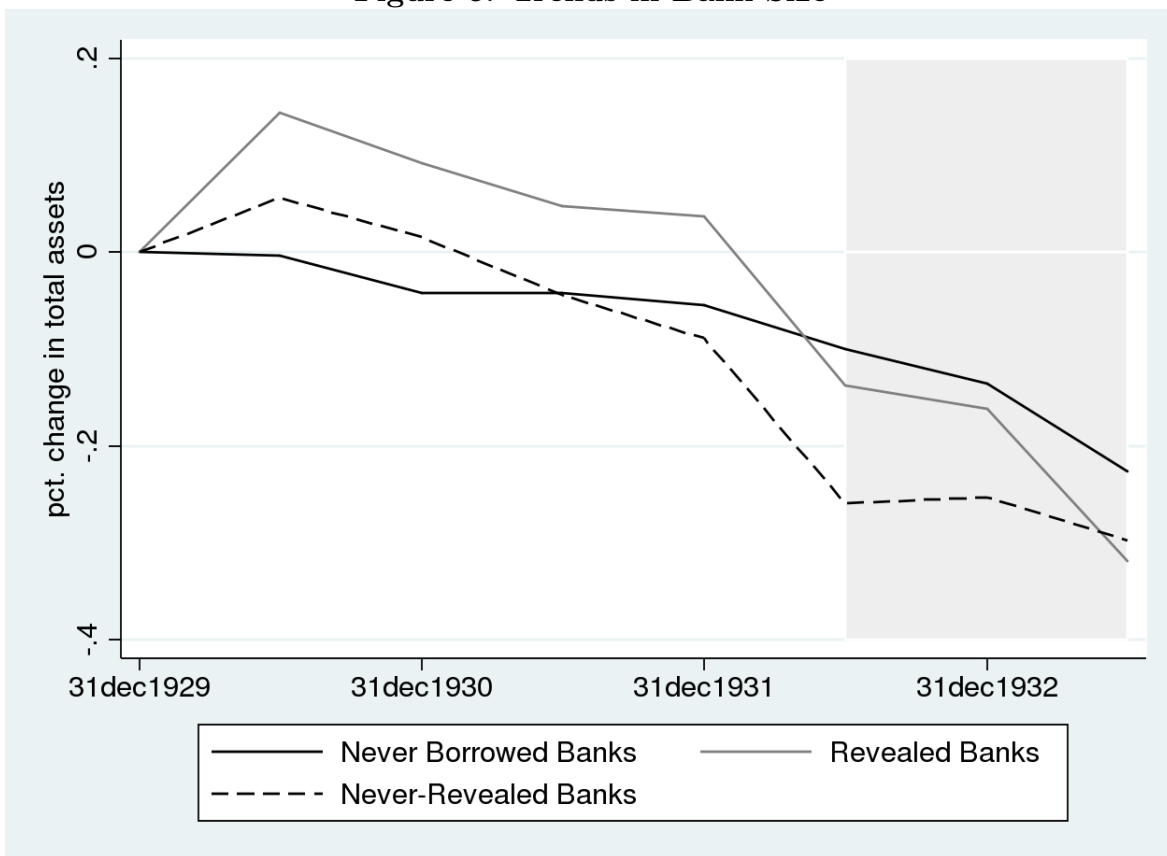
The results suggest that depositors interpreted news of LOLR loans as a sign of financial weakness, especially if the bank was revealed later. However, is this interpretation justified? I try to answer this question by examining whether depositors differentially withdrew from banks that borrowed from the RFC compared with banks that never borrowed. However, this comparison introduces a sample selection bias into the analysis. For example, suppose that banks with loans from the RFC experienced larger deposit withdrawals than banks without such loans. Is this result driven by depositors withdrawing more from the banks with loans because they had borrowed from their LOLR, or because those banks were somehow unobservably worse?

Nonetheless, I collected balance sheet data for 130 randomly selected banks that never approached the RFC for a loan (henceforth referred to as “never borrowed banks”).²² First,

²²These balance sheet data are also from *Rand McNally Bankers' Directory*.

Figure 5 displays the trends in bank size (total assets) for never borrowed banks, revealed banks, and never revealed banks. Initially, banks that would receive an RFC loan were similar in size to banks that would receive no loans. However, after 1932, the size of the borrowing banks dropped faster than that of the non-borrowing banks, despite receiving loans from the RFC.

Figure 5: Trends in Bank Size

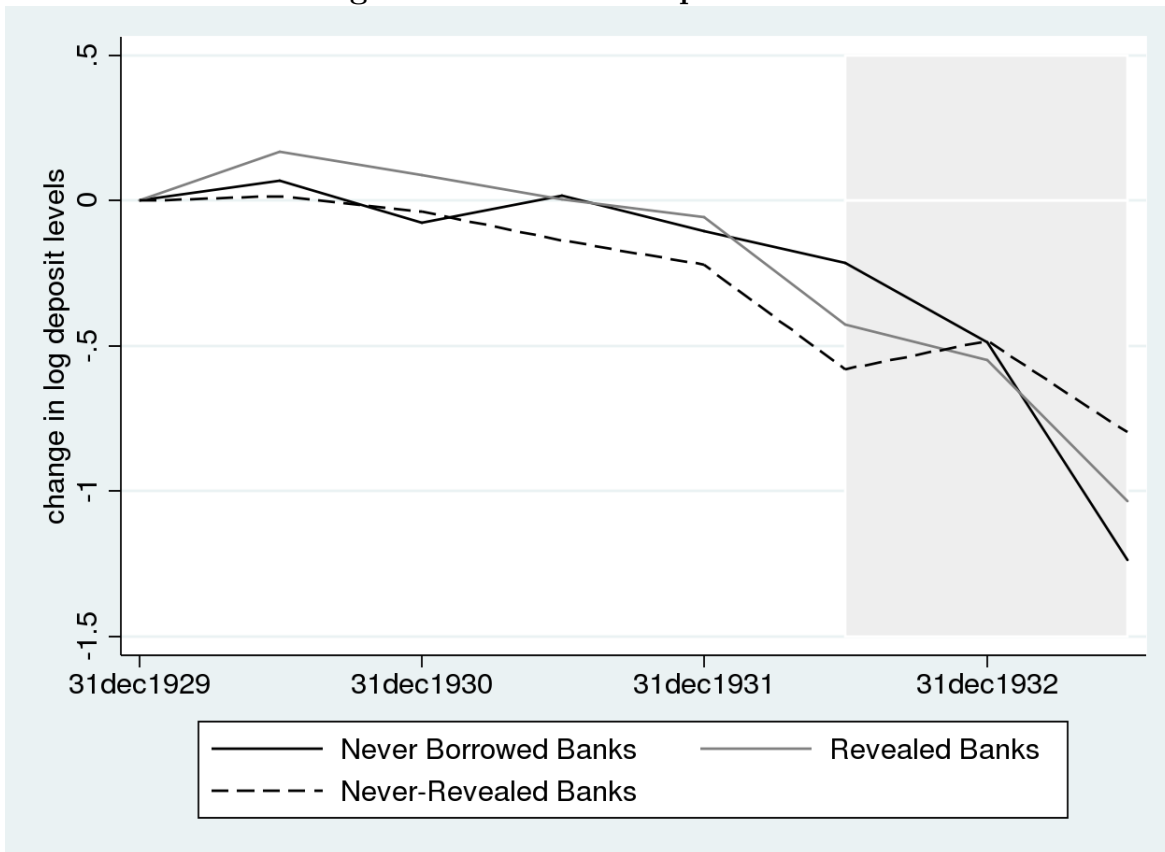


This figure illustrates the average percent change in total bank assets for banks that borrowed from the RFC (revealed and never revealed banks) and banks that did not borrow from the RFC (never borrowed banks), normalized by December 31, 1929 total asset levels. Total assets for each bank is the sum of its loans and discounts, bonds and securities, cash due from other banks, and real estate owned. The darker shaded area between June 30, 1932 and June 30, 1933 denotes the period the *Revealed* group was revealed (on or after August 22, 1932).

Second, Figure 6 illustrates the trend in deposit levels for never borrowed banks, revealed banks and never revealed banks. Initially, banks that did and did not receive loans had similar funding from deposits. Then, as the Depression wore on, banks that received no loans were unable to maintain higher deposit funding compared with banks that received loans after December 1932.

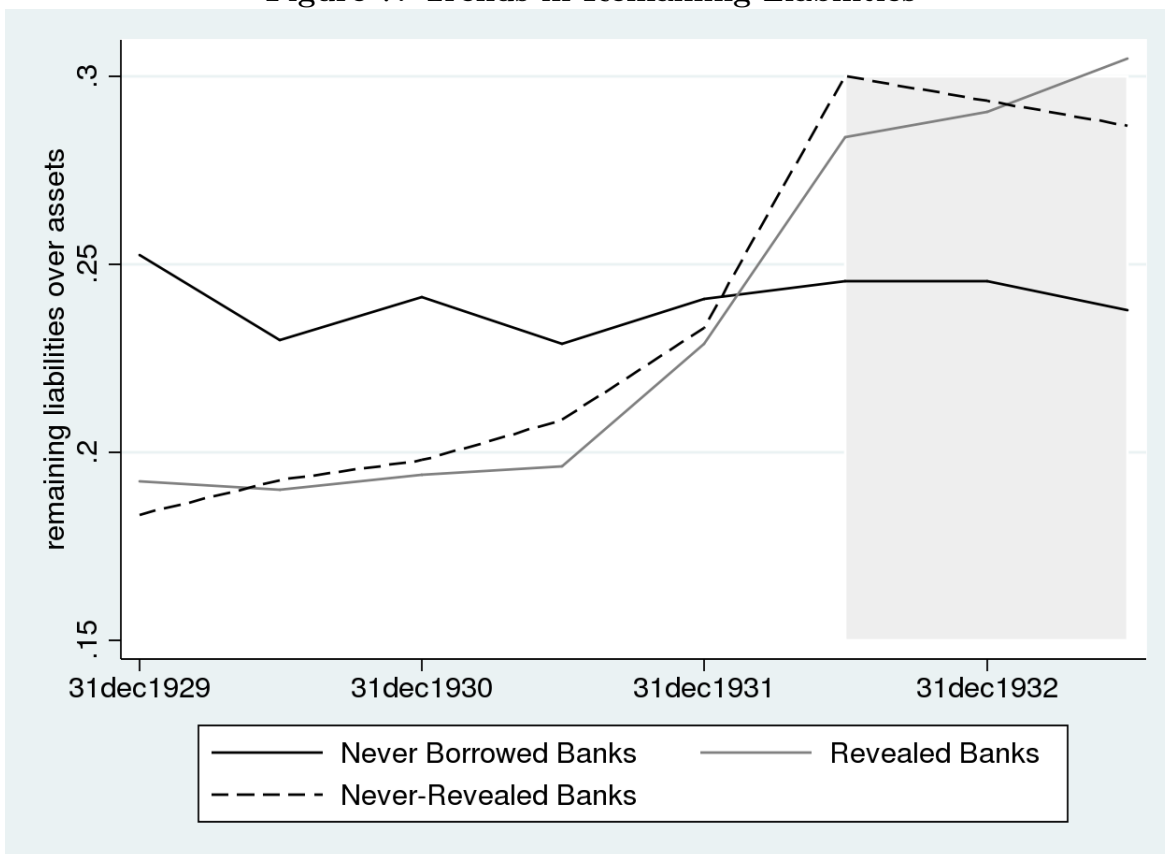
Third, Figure 7 displays the trends in total liabilities minus deposits over total assets (the remaining sources of funding for banks). We can see that banks that did not receive RFC loans initially had a larger share of alternative funding sources. These alternative funding sources included paid-up capital, bank surplus and profits, and loans from other banks. Paid-up capital is a function of the bank president's own capital. Figure 7 possibly provides evidence that banks that never borrowed from the RFC may have depended more on the interbank system prior to 1932. These banks indirectly had access to the discount window through interbank loans with member banks (Calomiris et al. (2016)). After 1932, the share of alternative funding sources for borrowing banks increased because they received loans from the RFC, while the share for non-borrowing banks did not. Mitchener and Richardson (2015) show that the interbank transfer system collapsed in 1932, suggesting that non-borrowing banks may have lost their indirect discount window access.

Figure 6: Trends in Deposit Levels



This figure illustrates the average change in the log level of deposits for banks that borrowed from the RFC (revealed and never revealed banks) and banks that did not borrow from the RFC (never borrowed banks), normalized by deposit levels at December 31, 1929. The darker shaded area between June 30, 1932 and June 30, 1933 denotes the period the *Revealed* group was revealed (on or after August 22, 1932).

Figure 7: Trends in Remaining Liabilities



This figure illustrates the average ratio of total liabilities minus deposits over assets for banks that borrowed from the RFC (revealed and never revealed banks) and banks that did not borrow from the RFC (never borrowed banks). The darker shaded area between June 30, 1932 and June 30, 1933 denotes the period the *Revealed* group was revealed (on or after August 22, 1932).

Finally, Table 10 presents the DID estimation of the effect of the list publications beginning on August 22, 1932 on log deposits at never revealed banks compared with never borrowed banks. Never revealed banks experienced a highly statistically significant *increase* in their log level of deposits of 47.3-65.44% following the publication of the lists. This result suggests that never revealed banks were able to credibly signal their healthier balance sheet to depositors without revealing their loans from their LOLR relative to never borrowed banks. Therefore, depositors possibly re-deposited their funds into these banks because they

could credibly signal their healthier balance sheets. Moreover, these results imply that if banks can keep LOLR loans private, they will enjoy higher deposit funding than banks that never borrowed. Columns (1) and (2) also include one additional interaction term with *Never Revealed*. I find no differential effect with respect to the log level of deposits between never revealed and never borrowed banks prior to the publication of the list once controls are added to the estimation, providing evidence that the parallel trend assumption holds.

In summary, stigma may not be justified. Banks that borrowed from their LOLR that were able to keep their loans private experienced higher deposit inflows relative to banks that never borrowed, following the publication of the lists. However, one caveat of the analysis is that my sample of 130 never borrowed banks is much smaller than my sample of never revealed banks. Therefore, the magnitude of the deposit inflows should be interpreted with caution.

Table 10: Effect of the Published Lists on Deposits at Never Revealed Banks

This table presents the DID estimates of the effect of the publication of lists of RFC borrowers beginning on August 22, 1932 on log deposits at the never revealed banks compared with never borrowed banks. *Never Revealed* is a dummy that equals 1 if the bank was never revealed. There are 1,568 never revealed banks and 130 never borrowed banks. $Never\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for banks never revealed after the August 22 list was published, and 0 otherwise. $Never\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to one for banks never revealed before the August 22 list was published, and zero otherwise. $X_{i,t-1}$ are controls measured six months prior to log deposits and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	<i>log deposits_t</i>	
	(1)	(2)
$Never\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	-0.1034*** (0.0111)	-0.0455 (0.0334)
$Never\ Revealed_i \times \mathbb{I}\{t \geq List\}$	0.473*** (0.1208)	0.6544*** (0.1275)
$log\ deposits_{t-1}$	0.4203*** (0.0344)	0.0909 (0.1147)
$X_{i,t-1}$	No	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	8,409	8,409
R^2	0.9064	0.912

4.8 Further analysis

In the Online Appendix, I provide supplementary tables that presenting alternative specifications that test for stigma. First, Table A.4 in the Online Appendix shows that adding bank-level loan variables controlling for the length of time since the bank had a loan authorized, the number of times the bank approached the RFC for a loan, and the amount the

bank borrowed does not alter the effect of the disclosure on the log level of deposits. Second, Table A.5 in the Online Appendix illustrates that subtracting the bank's total borrowings from the RFC from its total assets to control for the change in the balance sheet after the bank borrowed has little effect on the results. The effect of the revelation for revealed banks after the list was published continues to be economically large and statistically significant. Third, the results hold when the outcome variable is changed to the deposits-to-assets ratio. Finally, because the log level of deposits at subsequently revealed banks meets the levels of deposits at the initially revealed banks after all RFC borrowers were revealed, it is difficult to posit a theory that creates a drop in the log level of deposits at the revealed banks, and then sees the levels merge after the subsequently revealed banks were revealed, that is uncorrelated with the publication of lists beginning on August 22, 1932.²³

5 Ex-post bank behavior

Thus far, I have considered only the reactions of depositors to the start of list publications on August 22, 1932. However, an aim of the central bank is to ease liquidity needs for solvent yet illiquid banks during a crisis where, in turn, these banks may use the funding to maintain loans to their consumers (Freixas et al. (1999)). Did banks behave differently after their identities were revealed? Answering this question with these data is difficult. The lack of detail regarding a bank's loan and bond portfolio makes it difficult to say precisely how revealed banks adjusted their balance sheets in response to the withdrawal of deposits after

²³In all my analysis, I do not account for banks that borrowed from the National Credit Corporation (NCC) in late 1931, a private precursor to the RFC. Chandler (1971) states that the NCC only gave 10 million in loans and proved to be an inadequate organization.

the list was published, because of data limitations. For example, I do not observe whether revealed banks stopped issuing new loans after the publication of the list or whether they wrote down the value of existing loans.²⁴ Nonetheless, I examine the effect on loans and bonds at revealed banks compared with never revealed banks but do not observe the detailed compositions of loans/discounts or bonds/securities. Table 11 presents the results of the DID estimation of the effect of the publication of lists beginning on August 22, 1932 on log loans-and-discounts and log bonds-and-securities at revealed banks. Column (1) shows a highly statistically significant drop in log loan-and-discounts by 14.32% at revealed banks compared with the never revealed banks. However, the coefficient on $Revealed_i \times \mathbb{I}\{t = List - 1\}$ indicates that a parallel trend does not exist between revealed and never revealed banks with respect to the log level of loans-and-discounts, so the decrease in the loan portfolio should be interpreted with caution. Column (2) shows a highly statistically significant drop in log bonds-and-securities of 4.97% at revealed banks. This finding provides suggestive evidence that revealed banks reduced their loan portfolios and sold assets off their balance sheets to compensate for their losses in deposits following the publication of the lists.

Table 12 presents the results of the DID estimation of the effect of publication of lists beginning on August 22, 1932 on log loans-and-discounts and log bonds-and-securities at initially revealed banks. Column (1) shows a highly statistically significant drop in log loans-and-discounts by 11.1% at the initially revealed banks compared with the never revealed banks. Furthermore, Column (2) shows a statistically significant drop in log bonds-and-securities of 3.4% for the initially revealed banks compared with the never revealed banks.

²⁴I can somewhat address this concern by exploring Call Reports from Federal Reserve member banks that also borrowed from the RFC. Anecdotally, revealed banks reduced their real estate mortgages and called in loans from other banks in response to depositors withdrawing their funds. Moreover, revealed banks increased their bond portfolios by purchasing more U.S. government securities and state or municipal bonds. A few also bought railroad bonds. However, it is difficult to be precise about how revealed banks modified their balance sheets after the publication of the list, due to data limitations.

Table 11: Effect of the Published Lists on LOLR Revealed Banks' Loans and Bonds

This table presents the DID estimates of the effect of list publications of RFC borrowers beginning on August 22, 1932 on log bonds-and-securities and log loans-and-discounts. *Revealed* is a dummy that equals 1 if the bank was revealed on or after August 22, 1932 (revealed banks). There are 1,506 revealed banks and 1,568 never revealed banks. $Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for revealed banks after the August 22 list was published, and 0 otherwise. $Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to the dependent variable and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)
	<i>log bonds_t</i>	<i>log loans_t</i>
<i>Revealed_i</i> × $\mathbb{I}\{t = List - 1\}$	-0.0389** (0.0207)	0.011 (0.01)
<i>Revealed_i</i> × $\mathbb{I}\{t \geq List\}$	-0.1432*** (0.031)	-0.0497*** (0.0152)
AR(1)	0.268*** (0.0294)	0.3598*** (0.05)
$X_{i,t-1}$	Yes	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	15,105	15,105
R^2	0.9306	0.9638

This outcome provides suggestive evidence that the initially revealed banks also reduced their loan portfolios and sold assets off their balance sheets to compensate for their loss in deposits. Similarly, Table 13 presents the results for the subsequently revealed banks. Like the initially revealed banks, subsequently revealed banks also experienced a highly statistically significant drop in both their log bonds-and-securities and their log loans-and-discounts after the start of list publications on August 22, 1932. Moreover, the magnitudes of the drops are much larger than those for the initially revealed banks. But, as in Table 11, the coefficient on $Subsequently\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ indicates that a parallel trend does not exist between subsequently revealed and never revealed banks with respect to the log level of loans-and-discounts.

Finally, Table 14 presents the results of the DID estimation comparing the initially revealed banks with the subsequently revealed banks. Because the initially revealed banks experienced a stop (or increase) in their deposit withdrawals after the subsequently revealed banks were revealed on January 26, 1933, Columns (1) and (2) show that they qualitatively decreased their loan and bond portfolios but were able to recover after January 26, 1933. Initially revealed banks experienced a 6.35% drop in their loan portfolios compared with the subsequently revealed banks, but the coefficient is statistically significant only at the 10% level. Qualitatively, initially revealed banks also experienced a drop in their bond portfolios but the coefficient is not statistically significant. However, after the subsequently revealed banks were revealed, initially revealed banks experienced an increase in their log loans-and-discounts of 11.15% suggesting that the initially revealed banks were able to extend more loans after their deposit levels recovered.

Table 12: Effect of the Published Lists on Initially Revealed Banks' Loans and Bonds

This table presents the DID estimates of the effect of the August 22 and October 7, 1932 lists of RFC borrowers on log bonds-and-securities and log loans-and-discounts at the initially revealed banks. *Initially Revealed* is a dummy that equals 1 if the bank was revealed on August 22 or October 7, 1932. There are 869 initially revealed banks and 1,568 never revealed banks. $Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for initially revealed banks after the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for initially revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to the dependent variable and include the log of bank assets, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)
	<i>log bonds_t</i>	<i>log loans_t</i>
$Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	-0.0398*	-0.0104
	(0.0207)	(0.0127)
$Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.111***	-0.034**
	(0.0318)	(0.0156)
<i>AR</i> (1)	0.2832***	0.3067***
	(0.0321)	(0.0553)
$X_{i,t-1}$	Yes	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	11,530	11,530
R^2	0.9393	0.9696

Table 13: Effect of the Published Lists on Subsequently Revealed Banks' Loans and Bonds

This table presents the DID estimates of the effect of the retroactive January 26, 1933 list publication of RFC borrowers on log bonds-and-securities and log loans-and-discounts at subsequently revealed banks. *Subsequently Revealed* is a dummy that equals 1 if the bank was revealed on January 26, 1933. There are 586 subsequently revealed banks and 1,568 never revealed banks. $Subsequently\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for subsequently revealed banks after the August 22 list was published, and 0 otherwise. $Subsequently\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for subsequently revealed banks before the August 22 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to the dependent variable and include the log of bank assets, the log loan amount borrowed from the RFC, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)
	$log\ bonds_t$	$log\ loans_t$
$Subsequently\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	-0.0257 (0.0235)	0.038*** (0.013)
$Subsequently\ Revealed_i \times \mathbb{I}\{t \geq List\}$	-0.2377*** (0.0676)	-0.1113*** (0.0363)
AR(1)	0.232*** (0.033)	0.3624*** (0.0599)
$X_{i,t-1}$	Yes	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	10,471	10,471
R^2	0.9323	0.9649

Table 14: Effect of the Subsequent List on RFC Banks' Loans and Bonds

Column (1) presents the DID estimates of the effect of the August 22 and October 7, 1932 lists of RFC borrowers on log bonds-and-securities at the initially revealed banks compared with the subsequently revealed banks. There are 869 initially revealed banks and 586 subsequently revealed banks. Column (2) presents the DID estimates of the effect of the August 22 and October 7, 1932 lists on log loans-and-discounts at the initially revealed banks compared with those subsequently revealed banks. *Initially Revealed* is a dummy that equals 1 if the bank was revealed on August 22 or October 7, 1932. $Initially\ Revealed_i \times \mathbb{I}\{t \geq List\}$ is a dummy equal to 1 for initially revealed banks after the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$ is a dummy equal to 1 for initially revealed banks before the August 22 list was published, and 0 otherwise. $Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$ is a dummy equal to 1 for banks revealed after the retroactive January 26, 1933 list was published, and 0 otherwise. $X_{i,t-1}$ are controls measured six months prior to the dependent variable and include the log of bank assets, the log of state per capita income, the log of state-level deposits, the log of state-level deposits at suspended banks, the log of the number of banks at the state level, and the log of the number of suspended banks at the state level. Standard errors are clustered at the bank level and presented in parentheses. All continuous variables are winsorized at the 1% level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels respectively.

	(1)	(2)
	$log\ bonds_t$	$log\ loans_t$
$Initially\ Revealed_i \times \mathbb{I}\{t = List - 1\}$	-0.013 (0.0286)	-0.0539*** (0.0194)
$Initially\ Revealed_i \times \mathbb{I}\{t = List\}$	-0.0404 (0.0648)	-0.0635* (0.0395)
$Initially\ Revealed_i \times \mathbb{I}\{t = List + 1\}$	0.1088 (0.109)	0.1115* (0.065)
$AR(1)$	0.3287*** (0.0475)	0.4219*** (0.0639)
$X_{i,t-1}$	Yes	Yes
Bank Fixed Effects δ_i	Yes	Yes
Half-Year Fixed Effects η_t	Yes	Yes
Obs.	6,940	6,940
R^2	0.9098	0.9499

6 Relevance to the recent financial crisis

During the recent financial crisis, central bankers believed the primary reason banks refused to borrow from the discount window was to avoid stigma (Geithner (2014)).²⁵ The Fed's priority of solving the stigma problem was also illustrated by its extreme reluctance to release a list of discount window borrowers after losing the Freedom of Information Act lawsuit filed by Bloomberg News. The Fed had argued that releasing the identities of discount window borrowers would stigmatize banks and impede the Fed's ability to respond to future crises (Berry (2012)). Indeed, exam reports were kept confidential during the crisis to alleviate stigma concerns, and to provide incentives for banks to report the truth.

The Fed introduced new lending programs designed to eliminate the perception of stigma (Armantier et al. (2014)). The TAF was the first new lending program created on December 12, 2007. It seemed that banks associated less stigma with the TAF than the discount window because total borrowings at the TAF were much higher (Benmelech (2012)). However, the precise reason that the TAF seemed to carry less stigma (or be preferable to banks) was unknown. The TAF and the discount window were identical lending facilities with respect to collateral requirements and borrower eligibility, and both also protected borrower identities. However, the TAF provided a coordination mechanism for banks to submit bids for loans simultaneously decreasing the probability that an individual bank's identity would be leaked. The discount window, on the other hand, required that banks approached the LOLR for a loan individually providing no coordination mechanism across banks that might mask individual identities.

My results shed light on why the TAF seemed to carry less stigma. Because I find that stigma dissipated when subsequent banks were revealed, an emergency facility that

²⁵Geithner (2014) explains how banks' belief in stigma hindered the effectiveness of the Fed in 2007.

provides a coordination mechanism to allow banks to jointly request loans simultaneously may mitigate the presence of stigma. In that facility, it is likely that identities will remain secret because banks do not individually approach the LOLR for a loan. Ideally, central banks should prevent the release of bank borrower identities during a financial crisis, but doing so, this may not be possible given the idiosyncratic leakage of discount window loans during the recent crisis.²⁶

7 Conclusion

In this paper, I provide evidence of the stigma problem. Using data from an unexpected disclosure of several partial lists of banks that secretly borrowed from the LOLR during the Great Depression, I find evidence of the stigma problem in that depositors withdrew more deposits from banks included on the lists in comparison with banks left off the lists. This result sheds light on why banks wish to avoid approaching the discount window. However, for banks that were revealed earlier, stigma dissipated once subsequent banks were revealed. In addition, banks that were able to keep their LOLR loans private experienced higher deposit funding relative to both revealed banks and banks that never borrowed from their LOLR. These conclusions provide evidence that it is in the policy maker's interest to prevent runs on banks associated with stigma to support the financial system during a crisis. Revealing no bank identities dominates revealing few bank identities.

This paper helps explain how the LOLR's management of the information environment can have strong adverse effects on the health of the financial system. Designing emergency lending facilities that allow banks to coordinate requests for LOLR loans will mitigate stigma, solve the coordination problem between banks, and implicitly encourage banks to borrow

²⁶Armantier et al. (2014) identify some media articles that leaked the discount window borrowing of large banks and other idiosyncratic channels.

from the LOLR. Solving the stigma problem is crucial to effectively fight future financial crises.

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