Analysis of Stigma and Bank Credit Provision

Angela Vossmeyer*

Claremont McKenna College

March 13, 2018

Abstract

Bank rescue programs are designed to provide assistance to struggling financial intermediaries during financial crises. A complicating factor is that participating banks are often stigmatized by accepting assistance from the government. This paper investigates stigma in two ways: (i) it examines how stigma changes a bank's decision to seek assistance from the rescue program, and (ii) it analyzes how stigma affects a bank's ability to operate as a financial intermediary using a joint model for bank-level application, approval, and lending decisions. The empirical results indicate that stigma hinders the objectives of the rescue program, slows the production of credit, and prolongs the economic recovery.

Keywords: Bayesian inference; Financial crises; Financial intermediation; Lender of last resort policy.
JEL: E58, G21, G01, C11

*Robert Day School of Economics and Finance, Claremont McKenna College, 500 E. Ninth Street, Claremont, CA 91711; email: angela.vossmeyer@cmc.edu. The author thanks Sriya Anbil, Jim Barth, Michael Bordo, Charles Calomiris, Marcelle Chauvet, Sean Dowsing, Judith Goff, Christopher Hoag, Ivan Jeliazkov, Kris Mitchener, Gary Richardson, Michael Sacks, and Marc Weidenmier for their helpful comments and discussions. The author is also grateful for the useful suggestions from the editor, Robert DeYoung, and the anonymous referee. Feedback from the Federal Reserve System Conference on Economic and Financial History, NBER-DAE Summer Institute, and the Federal Reserve Bank of San Francisco is greatly appreciated. Support from the Lowe Institute of Political Economy and Financial Economics Institute is acknowledged. Research assistance from Kendall Greenberg, Amy Ingram, and Shirin Mollah is appreciated.

1 Introduction

During times of economic hardship, the objective of the central bank and its programs is to provide assistance to weak banks and prevent institutions that are illiquid but solvent from falling victim to runs and undue failure. A complication of such programs is "the stigma problem" (Bernanke, 2009), namely, if the identities of the banks being helped are revealed, market participants may infer that those banks are weak and thereby lose confidence in them. The concerns about stigma have existed since the Great Depression and remain a topic of active discourse in academic and policy circles. Yet, because of methodological problems and data limitations, few studies examining the presence and magnitude of stigma exist; methodological problems stem from the several non-random selection mechanisms that qualify banks for emergency assistance, and data limitations arise from the need to have high-frequency observations and variation in the timing or revelation of banks receiving assistance.

The actions taken to minimize stigma during the recent financial crisis render it difficult to quantify; for a review, see Geithner (2014) and Gorton (2015). However, the Great Depression offers a unique program and event to examine, namely, the Reconstruction Finance Corporation (RFC). The RFC was established in early 1932 as a government-sponsored rescue program to reduce the incidence of bank failure. By July 1932, the House of Representatives mandated that the RFC report the names of banks receiving assistance and amounts lent, thus revealing the banks to the public. Before this date, the public did not know which banks were receiving assistance, although they did know about the program itself. This paper exploits these events to investigate (i) the stigmatization of the rescue program, wherein banks become reluctant to seek assistance, rendering the special lending facility ineffective, and (ii) the stigmatization of banks that face loss of confidence from market participants because they are receiving emergency assistance. The goal of the paper is to examine how these two kinds of stigma play a role in financial intermediation, rescue program effectiveness, and economic recovery.

The literature on stigma and reluctance to borrow includes papers on the Great Depression (Wheelock, 1990), 1980-2000 period (Peristiani, 1998; Furfine, 2001; Darrat et al., 2004), the Great Recession (Armantier et al., 2015; Blau et al., 2016) and theoretical work (Ennis and Weinberg, 2013; Ennis, 2017). Many of these papers seek to explain why depository institutions avoided the discount window and *potential* stigma during periods of financial stress. The key element here is

"avoided," indicating that banks chose to pay higher costs to access an alternative, less stigmatizing funding source, a phenomenon that is particularly prevalent in more recent periods. This study focuses on quantifying the consequences associated with *realized* stigma, where alternative funding sources were sparse and where the program and banks faced actual market scrutiny and subsequent repercussions.

To capture *realized* stigma studying the RFC is the only viable option because alternative funding sources were not available for a majority of the banks. Previous studies on the RFC include Butkiewicz (1995, 1999), Mason (2001, 2003), Calomiris et al. (2013), and Vossmeyer (2014, 2016). While stigma is not the main focus of any of these papers, Butkiewicz (1995) and Mason (2001) address it in their analyses. Butkiewicz (1995) employs a time series of RFC lending and finds that the revealing of the RFC loan recipients offset the RFC's initial effectiveness. Mason (2001), on the other hand, uses a micro-level data set of Federal Reserve member banks and finds that the revealing had positive effects on bank survival. Additionally, Anbil (2018) shows that the presence of stigma imposed an 18%-25% loss in deposit levels at the RFC revealed banks. These results align with the findings of Friedman and Schwartz (1963) who state that when a bank was revealed to have received assistance, it was interpreted as a "sign of weakness and hence frequently led to runs on the bank" (pg. 325). While some of the adverse consequences of stigma and the RFC have been documented (e.g., deposit withdrawals), this paper seeks to understand whether stigma played a role in the disruption of credit intermediation that negatively affected macroeconomic activity (Bernanke, 1983).

This study contributes to the literature in several ways. First, it examines the banks' perspective, as opposed to investigating depositors' withdrawal decisions. The banks' perspective encompasses the two dimensions of stigma – a stigmatized rescue program and a stigmatized recipient bank – both of which have yet to be addressed in a single study. The specific questions of interest are: (i) Did banks become reluctant to seek assistance from the RFC program after the previous recipients' names became public? (ii) Once the recipient names became public, did stigma affect those banks' ability and willingness to operate as financial intermediaries and facilitate credit channels? The purpose of most rescue programs is to provide liquidity to a struggling economy, so the connection between a stigmatized program and credit intermediation is clear. There are two possible channels linking a stigmatized recipient bank and bank lending: One is that depositors

and investors may be less willing to fund a bank revealed as a recipient of emergency funds; the other is that banks that borrowed may act more conservatively, change their asset allocation to less risky assets, and thus make fewer loans. In either case, the production of credit falls, and this paper estimates the significance of the effect.

The second contribution of this paper is in the novel micro-level data and methodological approaches (time series, multivariate, and panel) that are used to answer the aforementioned questions. Specifically, to address (i), a daily time series of applications and renewals submitted to the RFC by financial institutions is constructed and modeled using an autoregressive Poisson. This element provides insights as to the magnitude of the change in the application rate due to the revelation of names. The panel approach then investigates the economic consequences that occur when the application rate drops and banks become reluctant to seek assistance. To address (ii), the paper presents a multivariate selection model for banks' application decisions, the RFC's approval decisions, and bank lending following the disbursements. By employing a multiple selection framework, the treatment effects of stigma on bank lending and the probability of bank failure are computed. The third contribution of this paper is in the model comparison studies, which aid in disentangling stigma from time dynamics and other forms of financial restructuring.

The results of the paper demonstrate a major drop in bank participation with the RFC following the release of the RFC's loan authorizations. The drop in participation stunted economic activity, as many banks did not receive support and dampened their lending. For the banks that were publicly named, the conversion of RFC lending to bank lending contracted, which further slowed the production of credit. Overall, the findings in this paper demonstrate that stigma can contribute to the breakdown of financial intermediation, hinder the objectives of the rescue program, and prolong the economy's return to normalcy.

The rest of the paper is organized as follows: Section 2 describes the historical background and relation to the 2007-2008 crisis. Section 3 looks at the stigmatized rescue program question and presents the times series data and methods employed to answer it. Section 4 discusses the multivariate approach to analyzing how stigma affects banks' financial intermediary functions. Section 5 presents the panel analysis on the treatment effect of reluctance, and Section 6 offers concluding remarks.

2 Historical Background and Contemporary Relevance

As stress on the financial system increased and bank health deteriorated in the early 1930s, it was apparent that additional assistance was necessary to resuscitate financial markets. President Hoover did not believe a government credit institution would be successful and turned to voluntary action. Hoover authorized the National Credit Corporation (NCC) in 1931 in which bankers formed a temporary credit pool, wherein major banks were to lend money to smaller banks experiencing difficulty. However, the NCC was not successful because banks were reluctant to lend and the program failed to provide the necessary relief funds (Nash, 1959). Eugene Meyer, then Governor of the Federal Reserve Board, convinced President Hoover that a public agency was needed to lend to troubled banks. On December 7, 1931, a bill was introduced to establish the Reconstruction Finance Corporation. The legislation was approved, and the RFC opened for business on February 2, 1932.

The RFC was a government-sponsored agency of the Executive Branch of the United States government. It was funded by the United States Treasury and was granted an initial capital stock of $500 million (Mason, 2003). The RFC disbursed $51.3 billion over its lifetime (Jones, 1951). During early operations, the RFC made short maturity loans at high rates collateralized by banks' best quality, most liquid assets. Eugene Meyer was concurrently appointed chairman of the RFC, and thus kept terms, rates, and collateral of loans at the RFC similar to loans at the Federal Reserve (Mason, 2001). It is important to note that the majority of banks did not have access to the discount window, making the RFC the only funding source for most of the banking population. After the 1933 Emergency Banking Act, the RFC could purchase preferred stock and recapitalize banks. The RFC's operations were straightforward. Any struggling bank could apply for assistance, the RFC reviewed the submitted applications in a reasonable amount of time (typically 2-5 weeks) and determined whether or not the bank was fit to receive assistance. Vossmeyer (2016) reviews the RFC's selection procedures and finds that the RFC was successful at screening out insolvent, helpless institutions and assisting those that could benefit from support.

From February - July 1932, the public knew about the RFC, but the RFC did not reveal the names of banks receiving assistance. After July, the House of Representatives amended the act and required that lists of RFC bank loan recipients be made available to Congress. Despite being told to keep the lists confidential, the Clerk of the House of Representatives, South Trimble, disobeyed

and made them available to the *New York Times*.[1] The first *New York Times* list was published in late August and revealed loan authorizations that occurred between July 21 – July 31, 1932. Subsequent lists published in the *New York Times* during the fall of 1932 and early 1933 detailed loans over $100,000 from February - July and all loans between August - December. Note that the names of banks that were declined assistance, of which there were many, were never published (details on declined applications are in Section 4.2.1).

During the 2007-2008 financial crisis, events surrounding emergency lending programs unfolded much as they did in the 1930s. Special lending facilities were developed to assist banks in need, and initially those banks' identities were not revealed. The news organization Bloomberg L.P. later filed requests for the identities of borrowing banks under the Freedom of Information Act (FOIA) to the Board of Governors of the Federal Reserve System (Gorton, 2015). Though the Federal Reserve was unsuccessful at withholding the names of the borrowers, it took many actions to reduce the consequences of stigma (see Geithner (2014) for a discussion). The availability of alternative funding sources during the recent crisis makes the analysis and situation surrounding stigma somewhat different from the Great Depression. Specifically, in the recent crisis, banks could pay excess costs and access alternative lending facilities to avoid stigmatized programs. Because this study quantifies the consequences of *realized* stigma at the bank level during the Great Depression when banks did not have alternative funding options, its findings should help explain why banks were willing to pay such high costs to avoid stigma during the recent crisis.

The two types of stigma studied – program stigma and bank stigma – were concerns during the recent crisis and are an active topic of interest to policymakers. Regarding program stigma, Timothy Geithner (United States Secretary of the Treasury, 2009-2013) writes about how he made large institutions' participation in the Troubled Asset Relief Program (TARP) inevitable, saying, "our hope was that smaller institutions would then feel free to apply for TARP funding without stigma" (Geithner, 2014, p. 236). Later, in writing about meetings with bankers, Geithner states, "I warned the bankers that if they all didn't accept the capital, TARP would become stigmatized, the system would remain undercapitalized, and they all would remain at risk" (pg. 238). The repercussions of a stigmatized rescue program are addressed in the next section and further in Section 5.

[1] The lists were published in several major outlets, as well as local newspapers. The *New York Times* is featured in the text because it was the initial data source for the paper.

Bank stigma is also a current issue. Despite the presence of the FDIC, which reduces depositor flight, currently half of the deposits at large commercial banks are uninsured (Egan et al., 2017). Egan et al. (2017) find that the large amount of uninsured deposits in the U.S. banking system can lead to unstable banks, given the elasticity of deposits to financial stress. Furthermore, bank stigma was a key point in the Federal Reserve Board's argument against the FOIA request submitted by Bloomberg. This line of cases and the Federal Reserve's legal briefs demonstrate the importance of bank stigma for policymakers presently. The Federal Reserve fought against Bloomberg's FOIA requests and argued via affidavits from economists that revealing the names of the banks receiving emergency assistance would stigmatize those banks, leading to a loss of public confidence in those banks, withdrawal of market sources of liquidity, and in extreme cases, closures of the banks (*Bloomberg L.P. v. Bd. of Governors of Fed. Reserve Sys.*, 649 F. Supp. 2d 262, 266, S.D.N.Y. 2009). The Board ultimately lost this argument because the court found that the affidavits were insufficient to carry its burden against Bloomberg's FOIA request. Specifically, the court noted that "conjecture, without evidence of imminent harm simply fails to meet the Board's burden." This line of cases highlights the need for empirical work on bank stigma, which is addressed in Section 4.

3 Time Series Analysis

3.1 Data and Methodology

To address the concerns of a stigmatized rescue program in the context of the RFC, a daily time series of RFC application and renewal requests is constructed from the *RFC Card Index to Loans Made to Banks and Railroads, 1932-1957*. These cards were collected from the National Archives in College Park, Maryland; they record the name and address of the borrower, date, request and amount of the loan, whether the loan was approved or declined, and loan renewals. Further information is obtained from the *Paid Loan Files* and *Declined Loan Files*, also gathered from the National Archives, which include the exact information regulators had about the banks from the applications and the original examiners' reports on the decisions. Because the data need to be hand-coded from the cards and applications, the current analysis focuses on only five states: Alabama, Arkansas, Michigan, Mississippi, and Tennessee. These states were selected mainly for reasons pertaining to the multivariate analysis, and they are outlined in Section 4.2.1.

Figure 1 presents a bar graph that details the number of inquiries (applications and renewals) that banks submitted to the RFC each day from early 1932 - early 1934. The first dashed line marks July 21, 1932, the date that the House of Representatives amended the act to require that lists of RFC loan recipients be made available to the Congress. The second dashed line marks August 22, 1932, the date that the *New York Times* first published a list of RFC loan authorizations. It is easy to see from Figure 1 that there is a small dip in the applications and renewals following the *New York Times* publication date of August 22, 1932.

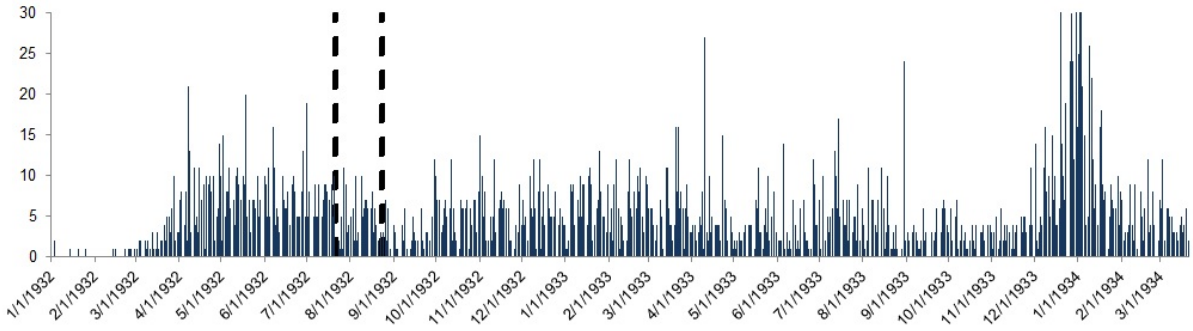

Figure 1: Number of bank inquiries (applications and renewals) submitted to the RFC each day.

In examining Figure 1, it is important to note that banks could submit multiple applications to the RFC. Thus, Figure 1 shows all inquiries, including repeat applications from banks already receiving assistance. In terms of understanding stigma, it is worth examining an image that only displays inquiries submitted from new applicant banks, not repeats. If the program itself is stigmatized, evidence of reluctance to participate will show up in lower numbers of new applications rather than in lower numbers of repeat applications from banks already receiving assistance. This is because repeat applicants may have already been revealed, so any perceived damage from the revelation would have already occurred.

Figure 2 uses the same data but reports only new applications, and here we see quite a different picture. Following the *New York Times* publication date, there is a major drop in new applications, and the drop lasts for over a year. Therefore, the applications reported in Figure 1 after August 22, 1932, are mostly from repeat applicants; many of these banks had been revealed to the public and were likely requesting more RFC assistance to combat the deposit withdrawals noted in Anbil (2018). The drop in new applications indicates that banks that had not already participated in the RFC program were reluctant to get involved – evidence of the program form of the "stigma effect".

It is worth noting that the rejection rates were consistent through these periods, thus the notion that banks stopped applying because of the costs associated with being declined is not supported. Rejected applications are reviewed in Section 4.2.1.

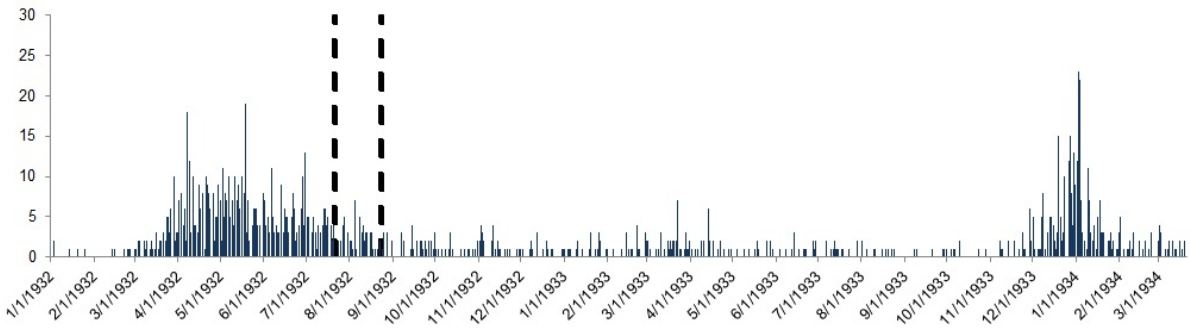

Figure 2: Number of inquiries submitted to the RFC from new applicant banks each day.

Common to both figures is the increase in applications in early 1934. This increase is due to the introduction of the Federal Deposit Insurance Corporation (FDIC) and expansion of the RFC's preferred stock purchases. The FDIC and RFC worked together in this period to help banks in need of assistance (Mitchener and Mason, 2010). Both agencies shared their examination reports of each bank and influenced decisions for support, which is evident from the *Paid Loan Files* and *Declined Loan Files* collected for the study. With deposit insurance protection, some banks were willing to apply to the RFC and receive the liquidity or capital they needed. While the introduction of the FDIC alleviated some of the reluctance to borrow issues and combated the deposit withdrawal consequences, it certainly does not cure stigma as it remains an issue today. In the 2008 crisis, financial institutions worried about certain forms of short-term funding (e.g., repo loans) fleeing and nearly half of deposits were uninsured, which led to bank instability (Egan et al., 2017).

Table 1 offers simple summary statistics, detailing daily averages, standard deviations, and totals for the three periods of interest. Before the revealing, the average number of applications submitted to the RFC from new applicant banks was 3.45 applications a day and afterward it was 0.60 applications. These findings align with Wheelock (1990) who finds evidence of a downward shift in borrowed reserve demand during the Depression.

To control for lags and changes in the series, the daily time series data is modeled using an autoregressive Poisson. Let y_t be the number of assistance requests submitted to the RFC on day

	Daily Mean	St. Dev.	Total
Before revealing: All Inquiries	4.71	4.3	953
Before revealing: New Applicants	3.45	4.2	696
After revealing, before FDIC: All Inquiries	4.03	3.7	1858
After revealing, before FDIC: New Applicants	0.60	1.0	278
After FDIC: All Inquiries	4.65	5.8	1860
After FDIC: New Applicants	1.23	3.4	491

Table 1: Summary statistics for inquiries submitted to the RFC from financial institutions.

t from new applicant banks. The model is as follows,

$$y_t \sim Po(\lambda_t), \quad \lambda_t = \exp(\boldsymbol{x}_t'\boldsymbol{\beta} + \rho \log(y_{t-1} + 1)), \tag{1}$$

where $\boldsymbol{x}_t$ includes indicators for the amended act and newspaper publication dates. Because the new applicant series is being used, the series should be decreasing as more banks apply and there are fewer new applicants. To control for this natural decline, $\boldsymbol{x}_t$ also includes a variable that represents the fraction of banks remaining in the population that have not applied for assistance. The model is estimated using Markov chain Monte Carlo (MCMC) simulation techniques, specifically the Accept-Reject Metropolis-Hastings (ARMH) algorithm (Tierney, 1994). For a review of this algorithm, see Chib and Greenberg (1995) and Chib and Jeliazkov (2005). The Bayesian methods implemented in this paper are attractive for several reasons, in particular for marginal likelihood and model comparison purposes. With Bayesian methods, interest lies in the posterior density as the target density

$$\pi(\boldsymbol{\theta}|\boldsymbol{y}) \propto f(\boldsymbol{y}|\boldsymbol{\theta})\pi(\boldsymbol{\theta}),$$

where $f(\boldsymbol{y}|\boldsymbol{\theta})$ is the likelihood obtained from the Markov transition matrix and $\boldsymbol{\theta}$ is all model parameters. Here, a description of the general ARMH algorithm is offered. Let $h(\boldsymbol{y}|\boldsymbol{\theta})$ denote a source density and $\mathcal{D} = \{\theta : f(\boldsymbol{y}|\boldsymbol{\theta})\pi(\boldsymbol{\theta}) \leq ch(\boldsymbol{\theta}|\boldsymbol{y})\}$, where c is a constant and $\mathcal{D}^c$ is the complement of $\mathcal{D}$. Then the ARMH algorithm is defined as follows.

Algorithm 1 *ARMH*

1. A-R Step: Generate a draw $\boldsymbol{\theta}' \sim h(\boldsymbol{\theta}|\boldsymbol{y})$. Accept the draw with probability

$$\alpha_{AR}(\boldsymbol{\theta}'|\boldsymbol{y}) = min\left\{1, \frac{f(\boldsymbol{y}|\boldsymbol{\theta}')\pi(\boldsymbol{\theta}')}{ch(\boldsymbol{\theta}'|\boldsymbol{y})}\right\}$$

   and repeat the process until the draw is accepted.

2. M-H step: Given the current value $\boldsymbol{\theta}$ and the proposed value $\boldsymbol{\theta'}$

   (a) If $\boldsymbol{\theta} \in \mathcal{D}$, set $\alpha_{MH}(\boldsymbol{\theta}, \boldsymbol{\theta'}|\boldsymbol{y}) = 1$

   (b) If $\boldsymbol{\theta} \in \mathcal{D}^c$ and $\boldsymbol{\theta'} \in \mathcal{D}$, set $\alpha_{MH}(\boldsymbol{\theta}, \boldsymbol{\theta'}|\boldsymbol{y}) = \frac{ch(\boldsymbol{\theta}|\boldsymbol{y})}{f(\boldsymbol{y}|\boldsymbol{\theta})\pi(\boldsymbol{\theta})}$

   (c) If $\boldsymbol{\theta} \in \mathcal{D}^c$ and $\boldsymbol{\theta'} \in \mathcal{D}^c$, set $\alpha_{MH}(\boldsymbol{\theta}, \boldsymbol{\theta'}|\boldsymbol{y}) = min\left\{1, \frac{ch(\boldsymbol{\theta}|\boldsymbol{y})}{f(\boldsymbol{y}|\boldsymbol{\theta})\pi(\boldsymbol{\theta})}\right\}$

   Return $\boldsymbol{\theta'}$ with probability $\alpha_{MH}(\boldsymbol{\theta}, \boldsymbol{\theta'}|y)$, otherwise return $\boldsymbol{\theta}$.

3.2 Time Series Results

The results for the autoregressive Poisson model are displayed in Table 2, and are based on 11,000 MCMC draws (burn-in of 1,000) with the priors centered at 0 and a variance of 25. The posterior means and standard deviations were very close to the MLE and standard errors which were obtained as a robustness check (available in Table 9 of the Appendix, along with OLS results). Table 2 also displays the marginal likelihood associated with each model specification. Marginal likelihood calculations follow from Chib (1995) and Chib and Jeliazkov (2001). Evidenced in the table is the support from the data for the third specification, which contains the highest marginal likelihood (on the log scale). The third specification supports indicators for the July amendment and August *New York Times* publication. Model (4) includes additional indicators for later *New York Times* publications in which they released more RFC loan authorizations, however, this specification is less supported by the data. Thus, one can conclude that the model with the first two date indicators best represents the data, temporal changes in the series, and the dates in which the series shifts.

Focusing on Model (3), the results show a large negative effect stemming from the *New York Times* initial announcement (August 22, 1932), which accords well with Friedman and Schwartz (1963). In order to gauge the magnitude, estimated covariate effects for the parameters are considered. Let $\boldsymbol{x}_t^\dagger$ represent the case when no loan authorizations are revealed and $\boldsymbol{x}_t$ is the original case with the amendment and *New York Times* publication. Thus, interest lies in the average difference in the implied probabilities $\{\Pr(y_t = j|\boldsymbol{x}_t) - \Pr(y_t = j|\boldsymbol{x}_t^\dagger)\}$, where j represents a particular number of applications submitted that day. As discussed in Jeliazkov et al. (2008), a practical procedure is to marginalize out the remaining covariates using their empirical distribution, while the parameters are integrated out with respect to their posterior distribution. The goal is to obtain a sample of

Variables	Specifications for the number of new applications (1)	(2)	(3)	(4)
Intercept	-0.83 (0.06)	0.127 (0.17)	0.54 (0.17)	0.64 (0.22)
$\rho,\ y_{t-1}$	0.78 (0.03)	0.72 (0.03)	0.69 (0.03)	0.69 (0.04)
$Frac_t$, Fraction remaining	0.97 (0.10)	0.06 (0.18)	-0.36 (0.18)	-0.46 (0.22)
$1\{t \geq July\ 21,\ 1932\}$		-0.60 (0.10)	-0.14 (0.10)	-0.16 (0.10)
$1\{t \geq August\ 22,\ 1932\}$			-0.74 (0.11)	-0.37 (0.16)
$1\{t \geq October\ 7,\ 1932\}$				-0.65 (0.46)
$1\{t \geq October\ 22,\ 1932\}$				0.20 (0.48)
$1\{t \geq November\ 28,\ 1932\}$				-0.31 (0.31)
$1\{t \geq December\ 22,\ 1932\}$				0.13 (0.26)
$1\{t \geq January\ 26,\ 1933\}$				0.21 (0.20)
Log-Marginal Lik.	-572.6	-560.2	-549.2	-556.8

Table 2: Posterior means and standard deviations are based on 11,000 MCMC draws with a burn-in of 1,000.

draws and find the mean of the following predictive distribution:

$$\{\Pr(y_t = j|\boldsymbol{x}_t) - \Pr(y_t = j|\boldsymbol{x}_t^{\dagger})\} = \int \{\Pr(y_t = j|\boldsymbol{x}_t, \mathbf{z}_t, \boldsymbol{\theta}) - \Pr(y_t = j|\boldsymbol{x}_t^{\dagger}, \mathbf{z}_t, \boldsymbol{\theta})\}\pi(\mathbf{z}_t)\pi(\boldsymbol{\theta}|y)d\mathbf{z}_t d\boldsymbol{\theta},$$

where $\mathbf{z}_t = \{y_{t-1}, Frac_t\}'$. In order to examine the magnitude of the stigma effect in terms of banks' reluctance to seek assistance from the RFC, the probabilities from the Poisson distribution are calculated with the number of daily applications submitted to the RFC, j, set to values surrounding the pre-revealing and post-revealing averages.

Table 3 presents the results for the estimated covariate effects and a histogram of the distribution of the effect appears in Appendix Figure 7. The results demonstrate that revealing the loan authorizations reduced the probability of the RFC receiving 3 applications a day (near the pre-revealing average) by 10.4 percentage points. Revealing the loan authorizations actually increased the probability of the RFC receiving 0 applications a day by 27.9 percentage points, relative to no revealing. Thus, in agreement with the raw data, the model finds a significant negative stigma effect from the revealing, where banks became reluctant to seek assistance from the RFC. As a result, the RFC likely failed to restore confidence in the financial system and, as in the Geithner quote, the whole system likely remained undercapitalized and at risk.

Other Considerations. Using the original RFC applications and *RFC Card Index* is useful for two additional reasons. They allow for the examination of cancelled loans as well as loans to

	Revealing – No Revealing
$\triangle \Pr(y_t = 0)$	0.279
$\triangle \Pr(y_t = 2)$	−0.086
$\triangle \Pr(y_t = 3)$	−0.104
$\triangle \Pr(y_t = 4)$	−0.072

Table 3: Estimated covariate effects. $\triangle \Pr(\cdot)$ expresses the difference in the probability of a particular count if the bank names were not revealed.

non-depository institutions. Cancelled loans are of interest because they reflect an interruption in the interactions between banks and the RFC. Before the July amendment and during the first six months of the RFC's operations, eight RFC loans were cancelled in the five state sample. Between the July amendment and the January 1933 *New York Times* publication of names (the next 6 months), 42 loans were cancelled, with particular bunching around the *New York Times* publication dates. The data suggest that banks were changing their decisions and behavior with regard to the emergency program due to stigma from the *New York Times* publication of names. Cancelled loans are further examined in Section 5.

Examining the behavior of applications from non-depository institutions is particularly useful to ensure that the stigma incident was isolated to the banking sector. The powers of the RFC were widespread, as the program was allowed to lend to many types of businesses, not just banks (Mitchener and Mason, 2010). Since the theory on stigma is bank-specific (Ennis and Weinberg, 2013), the RFC should be stigmatized only from banks' standpoint and not from the standpoint of other non-depository institutions that were applying to the RFC. To ensure that the effects of the revealing episode are unique to depository institutions, a daily time series of new applications submitted to the RFC is constructed for building and loan associations, railroad companies, and insurance companies. The series is displayed in Figure 3. Again, the first dashed line marks the date the House of Representatives amended the act and the second dashed line marks the date of the initial *New York Times* publication. Clearly, there are no major changes after the dashed lines as there were for banks. To control for lags in the series and the natural decline in applications as time goes on, the non-depository series is modeled using the autoregressive Poisson in equation (1). The results, which are available in Table 10 of the Appendix, show that the estimates for the amended act and newspaper publication indicators are not statistically different from zero. Thus,

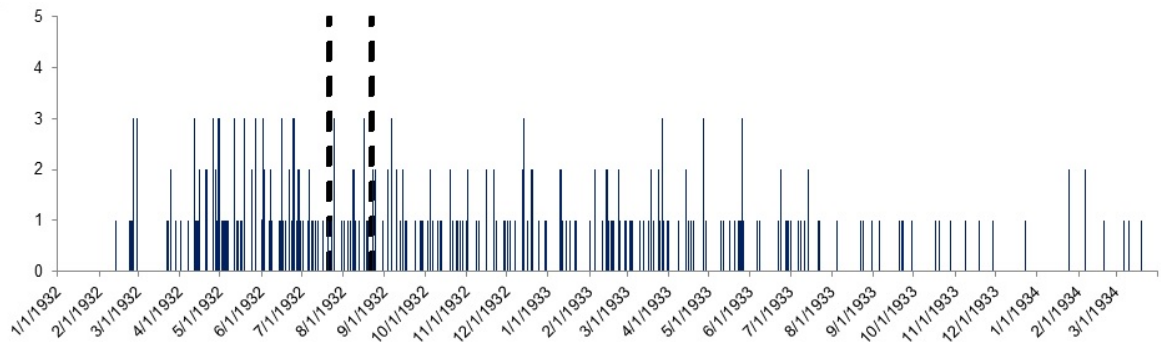

Figure 3: Number of inquiries submitted to the RFC from new non-depository applicants each day.

the stigmatized rescue program effect is isolated to depository institutions.

Another complication is the condition of the banking system in general. While the analysis of the behavior of non-depository applications isolates the incidence of stigma to the banking system, perhaps something else was going on within that industry to make the application rate decline. Daily data for macro-factors that might influence borrowing supply and demand are difficult to come by. However, Fama and French's (1997) 48 industry portfolios contain daily returns for banking. They construct this series from NYSE, AMEX, and NASDAQ stocks classified in the banking industry and compute daily returns. If a macroeconomic event hit the banking industry, it should be captured within these returns. The data are included as an additional covariate in $\boldsymbol{x}_t$ in equation (1). The results for the new specification are displayed in Table 4 and demonstrate that the findings are robust in the new specification. The negative estimate coming from the August publication remains large. Furthermore, the marginal likelihood is lower here (relative to specification (3) in Table 2), and thus the data support the original specification.

Variable	Specification for the number of new applications
Intercept	0.54 (0.17)
$\rho,\ y_{t-1}$	0.70 (0.03)
Bank Returns	0.01 (0.01)
Fraction remaining	-0.37 (0.18)
$1\{t \geq July\ 21,\ 1932\}$	-0.17 (0.10)
$1\{t \geq August\ 22,\ 1932\}$	-0.71 (0.11)
Log-Marginal Lik.	-554.7

Table 4: Posterior means and standard deviations are based on 11,000 MCMC draws with a burn-in of 1,000.

The time series analysis offered in this section answers the first question of interest: Did announcing the RFC's loan authorizations deter bank participation in the rescue program? Yes, there is a major drop in participation with the probability of the RFC receiving no applications a day increasing by 27.9 percentage points. The two natural follow-up questions are: (a) Once the names were released, what happened to those banks and to their ability to facilitate credit channels? (b) How did this drop in participation affect economic activity? These two questions are addressed in Section 4 and Section 5, respectively.

4 Multivariate Analysis

4.1 Model and Estimation

The purpose of the multivariate analysis is to examine how the publication of the RFC's loan authorizations affected revealed banks' ability and willingness to operate as financial intermediaries. Specifically, what is the effect of stigma on bank lending and the probability of bank failure? The channel linking stigma and bank lending could be that, once the bank is revealed, investors may be less confident in the institution and less willing to fund it, or that the bank acts more conservatively and makes fewer loans, possibly in response to deposit withdrawals (Calomiris and Wilson, 2004). Understanding the consequences of bank stigma is important with regard to the recent crisis because it was a key argument in the Federal Reserve Board's fight against Bloomberg's FOIA request (*Bloomberg L.P. v. Bd. of Governors of Fed. Reserve Sys.*, 649 F. Supp. 2d 262, 266, S.D.N.Y. 2009). The Board lost this argument due to insufficient evidence. Empirical research in this area has been sparse because of methodological and data limitations. This section works to overcome these limitations and provides an empirical evaluation of the Board's claims.

This section is methodologically intensive in order to properly control for the several selection mechanisms that qualify banks for rescue assistance. Hence, a multivariate treatment effect model in the presence of sample selection is employed, which was developed in Vossmeyer (2016). The methodology deals with several important issues prevalent in policy and program evaluation, including application and approval stages, non-random treatment assignment, endogeneity, and discrete outcomes. It is applicable in the case of the RFC because banks had to apply for assistance from the RFC. Following the application stage, the RFC reviewed the submitted material and determined whether or not the bank was fit to receive assistance. After these 2 selection stages, the resulting

set of treatment response or potential outcomes are for banks that did not apply for assistance, banks that applied and were declined assistance, and banks that applied and were approved assistance (with a subsets that were revealed and not revealed), thereby capturing the entire banking population.

The model differs dramatically from conventional treatment models which only consider the treated and untreated groups. The conventional structure ignores the initial selection mechanism in which banks chose to apply for assistance. Overlooking the application stage erroneously groups banks that did not apply for assistance with those that were declined assistance. Thus, the untreated group comprises the most and least healthy banks, leading to a fundamental misspecification. Motivated by these difficulties, this article does not use the conventional methods and instead utilizes the multivariate model in the presence of sample selection, which is graphically presented in Figure 4.

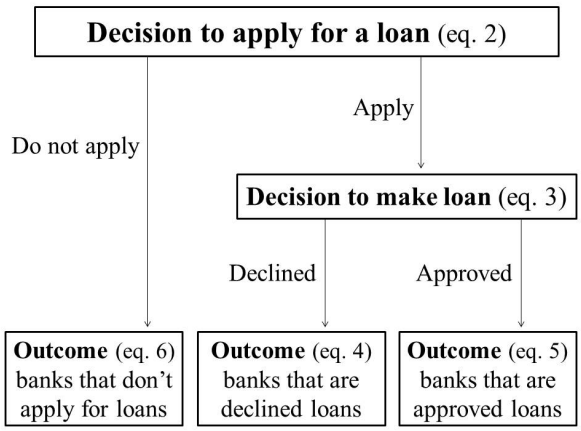

Figure 4: Multivariate treatment effect model in the presence of sample selection.

The model is a system of 5 equations with 2 selection mechanisms and 3 treatment response

outcomes given by:

$$\textit{Application Selection Mechanism} \quad : \quad y_{i1}^* = \mathbf{x}_{i1}'\boldsymbol{\beta}_1 + \varepsilon_{i1} \quad (\textit{always observed}) \tag{2}$$

$$\textit{Approval Selected Treatment} \quad : \quad y_{i2}^* = \mathbf{x}_{i2}'\boldsymbol{\beta}_2 + \varepsilon_{i2} \quad (\textit{observed for applicants}) \tag{3}$$

$$\textit{Potential Outcomes} \quad - \quad \text{Treatment Responses } (\textit{only one is observed})$$

$$\text{Applied-declined sample} \quad : \quad y_{i3}^* = (\mathbf{x}_{i3}' \; y_{i1})\boldsymbol{\beta}_3 + \varepsilon_{i3} \tag{4}$$

$$\text{Applied-approved sample} \quad : \quad y_{i4}^* = (\mathbf{x}_{i4}' \; y_{i1} \; y_{i2} \; (y_{i2} \times Stig_i))\boldsymbol{\beta}_4 + \varepsilon_{i4} \tag{5}$$

$$\text{Non-applicant sample} \quad : \quad y_{i5}^* = \mathbf{x}_{i5}'\boldsymbol{\beta}_5 + \varepsilon_{i5} \tag{6}$$

It is further characterized by 5 dependent variables of interest where $\mathbf{y}_i^* \equiv (y_{i1}^*, y_{i2}^*, y_{i3}^*, y_{i4}^*, y_{i5}^*)'$ are the continuous latent data and $\mathbf{y}_i \equiv (y_{i1}, y_{i2}, y_{i3}, y_{i4}, y_{i5})'$ are the corresponding observed censored data. The latent variables relate to the observed censored outcomes by $y_{ij} = y_{ij}^* \cdot 1\{y_{ij}^* > 0\}$ for equations $j = 1, \ldots, 5$, (Tobin, 1958). A discussion about the censoring appears in Section 4.2. The outcome for equation (2), y_{i1}, is the total amount of RFC assistance requested by bank i. The outcome for equation (3), y_{i2}, is the total amount of RFC assistance approved for bank i. This equation is only observed for the selected sample of applicant banks. The outcome for equations (4)-(6) represent bank loan volume for the respective subsamples of banks that did not apply, banks that applied and were declined, and banks that applied and were approved. Only one of these equations is ever observed, the other two are the counterfactuals. Note that y_{i1} and y_{i2} enter potential outcome equations (4) and (5) as endogenous covariates for the applicant sample. This can be understood as the requested and approved treatments entering the performance equations. Additionally, an interaction term enters equation (5). This term interacts the endogenous approved RFC funds with an indicator variable ($Stig_i$) that takes the value "1" if a bank's name was revealed as an RFC recipient. This is the key covariate of interest. Detailed descriptions of this variable appear in Section 4.2 and treatment effect calculations are presented in Section 4.3.

Data missingness restricts the model to systems of 2 or 3 equations depending on the subsample to which the bank belongs. If $y_{i1} = 0$, the bank did not apply for assistance – y_{i1} and y_{i5} are observed, and y_{i2}, y_{i3}, and y_{i4} are not observed. If $y_{i1} > 0$ and $y_{i2} = 0$, the bank applied for assistance and was declined – y_{i1}, y_{i2} and y_{i3} are observed, and y_{i4} and y_{i5} are not observed. If $y_{i1} > 0$ and $y_{i2} > 0$, the bank applied for assistance and was approved – y_{i1}, y_{i2} and y_{i4} are observed, and y_{i3} and y_{i5} are not observed. The exogenous covariates $\mathbf{x}_i = (\mathbf{x}_{i1}, \mathbf{x}_{i2}, \mathbf{x}_{i3}, \mathbf{x}_{i4}, \mathbf{x}_{i5})$ are

needed only when their corresponding equations are observed. For identification reasons, assume that the covariates in $\mathbf{x}_{i2}$ contain at least one more variable than those included in the other equations. Although identification in models with incidental truncation does not require exclusions, they are typically employed so the resulting inference does not solely depend on distributional assumptions (Chib, 2007; Greenberg, 2008). Finally, the model assumes that the errors $\boldsymbol{\varepsilon}_i = (\varepsilon_{i1}, \varepsilon_{i2}, \varepsilon_{i3}, \varepsilon_{i4}, \varepsilon_{i5})'$ have a multivariate normal distribution $\mathcal{N}_5(0, \boldsymbol{\Omega})$, where $\boldsymbol{\Omega}$ is an unrestricted symmetric positive definite matrix. It is possible to explore other distributional forms for this joint model, but the normality assumption provides the groundwork for more flexible distributions, including finite mixtures, dirichlet processes, and scale mixtures.

For the i-th bank, define the following vectors and matrices,

$$\mathbf{y}^*_{iC} = (y^*_{i1}, y^*_{i5})', \quad \mathbf{y}^*_{iD} = (y^*_{i1}, y^*_{i2}, y^*_{i3})', \quad \mathbf{y}^*_{iA} = (y^*_{i1}, y^*_{i2}, y^*_{i4})',$$

$$\mathbf{X}_{iC} = \begin{pmatrix} \mathbf{x}'_{i1} & 0 \\ 0 & \mathbf{x}'_{i5} \end{pmatrix}, \quad \mathbf{X}_{iD} = \begin{pmatrix} \mathbf{x}'_{i1} & 0 & 0 \\ 0 & \mathbf{x}'_{i2} & 0 \\ 0 & 0 & (\mathbf{x}'_{i3}\ y_{i1}) \end{pmatrix},$$

$$\mathbf{X}_{iA} = \begin{pmatrix} \mathbf{x}'_{i1} & 0 & 0 \\ 0 & \mathbf{x}'_{i2} & 0 \\ 0 & 0 & (\mathbf{x}'_{i4}\ y_{i1}\ y_{i2}\ (y_{i2} \times Stig_i)) \end{pmatrix}.$$

Let $N_1 = \{i : y_{i1} = 0\}$ be the n_1 banks that did not apply for assistance and $N_2 = \{i : y_{i1} > 0 \ and \ y_{i2} = 0\}$ be the n_2 banks that applied and were declined assistance. Set $N_3 = \{i : y_{i1} > 0 \ and \ y_{i2} > 0\}$ to be the n_3 banks that applied and were approved assistance. Upon defining $\boldsymbol{\beta} = (\boldsymbol{\beta}'_1, \boldsymbol{\beta}'_2, \boldsymbol{\beta}'_3, \boldsymbol{\beta}'_4, \boldsymbol{\beta}'_5)'$ and $\boldsymbol{\Omega}$, note that in $\boldsymbol{\Omega}$ the elements Ω_{25}, Ω_{35}, Ω_{45}, and Ω_{34} are not identified because their corresponding equations cannot be observed at the same time. Thus, there are 11 unique estimable elements in $\boldsymbol{\Omega}$, whereas the remaining ones are non-identified parameters due to the missing outcomes. The variance-covariance matrix of interest is,

$$\boldsymbol{\Omega} = \begin{pmatrix} \Omega_{11} & \Omega_{12} & \Omega_{13} & \Omega_{14} & \Omega_{15} \\ \Omega_{21} & \Omega_{22} & \Omega_{23} & \Omega_{24} & \cdot \\ \Omega_{31} & \Omega_{32} & \Omega_{33} & \cdot & \cdot \\ \Omega_{41} & \Omega_{42} & \cdot & \Omega_{44} & \cdot \\ \Omega_{51} & \cdot & \cdot & \cdot & \Omega_{55} \end{pmatrix}.$$

The variance-covariance matrices for the three subsamples are as follows,

$$\boldsymbol{\Omega}_C = \begin{pmatrix} \Omega_{11} & \Omega_{15} \\ \Omega_{51} & \Omega_{55} \end{pmatrix}, \quad \boldsymbol{\Omega}_D = \begin{pmatrix} \Omega_{11} & \Omega_{12} & \Omega_{13} \\ \Omega_{21} & \Omega_{22} & \Omega_{23} \\ \Omega_{31} & \Omega_{32} & \Omega_{33} \end{pmatrix}, \quad \boldsymbol{\Omega}_A = \begin{pmatrix} \Omega_{11} & \Omega_{12} & \Omega_{14} \\ \Omega_{21} & \Omega_{22} & \Omega_{24} \\ \Omega_{41} & \Omega_{42} & \Omega_{44} \end{pmatrix}. \tag{7}$$

Let $\boldsymbol{\mu}_j$ define the mean in equations $j = 1, \ldots, 5$. The likelihood is $f(\mathbf{y}|\boldsymbol{\theta}) = \int f(\mathbf{y}, \mathbf{y}^*|\boldsymbol{\theta}) d\mathbf{y}^*$, where $\boldsymbol{\theta}$ is all model parameters and $f(\mathbf{y}, \mathbf{y}^*|\boldsymbol{\theta})$ is the complete-data likelihood given by

$$f(\mathbf{y}, \mathbf{y}^*|\boldsymbol{\theta}) = \prod_{i \in N_1} f_N(\mathbf{y}^*_{iC}|\boldsymbol{\mu}_C, \boldsymbol{\Omega}_C) \times \prod_{i \in N_2} f_N(\mathbf{y}^*_{iD}|\boldsymbol{\mu}_D, \boldsymbol{\Omega}_D) \times \prod_{i \in N_3} f_N(\mathbf{y}^*_{iA}|\boldsymbol{\mu}_A, \boldsymbol{\Omega}_A).$$

The likelihood is defined in terms of the 3 subsets of the sample, without components for the non-identified parameters, which follows from Chib (2007) and Chib et al. (2009). The censoring of multiple outcome variables renders this likelihood analytically intractable and hence estimation relies on simulation-based techniques. A Bayesian framework is implemented, thus standard semi-conjugate priors are applied where $\boldsymbol{\beta}$ has a joint normal distribution and (independently) $\boldsymbol{\Omega}$ has an inverted Wishart distribution. The prior on $\boldsymbol{\Omega}$ implies a distribution on functions of the elements in $\boldsymbol{\Omega}$ that correspond to the subsamples of interest. Combining the likelihood and priors leads to a posterior distribution, which is simulated by MCMC methods. For computational efficiency, a collapsed Gibbs sampler with data augmentation is employed which follows from Chib et al. (2009) and Li (2011). The particular algorithm that is utilized was developed in Vossmeyer (2016). The algorithm is attractive because of its excellent mixing properties, low storage costs, and computational speed. The sampler does not simulate the outcomes that are missing due to the application selection mechanism and does not require the joint distribution for the potential outcomes, which result in an efficient sampler that maintains tractability in the sampling densities. A summary of algorithm is offered below, however, the full derivation and details of the updating formulas are provided in Vossmeyer (2016).[2]

Algorithm 2 *Gibbs Sampler*

1. Sample $\boldsymbol{\beta}$ from the distribution $\boldsymbol{\beta}\ |\mathbf{y}, \mathbf{y}^*, \boldsymbol{\theta} \setminus \boldsymbol{\beta}$.
2. Sample $\boldsymbol{\Omega}$ from the distribution $\boldsymbol{\Omega}|\mathbf{y}, \mathbf{y}^*, \boldsymbol{\theta} \setminus \boldsymbol{\Omega}$ in a 1-block, multi-step procedure.
3. For $i \in N_1$, sample y^*_{i1} from the distribution $y^*_{i1}|\mathbf{y}, \boldsymbol{\theta}, \mathbf{y}^* \setminus \mathbf{y}^*_1$.
4. For $i \in N_2$, sample y^*_{i2} from the distribution $y^*_{i2}|\mathbf{y}, \boldsymbol{\theta}, \mathbf{y}^* \setminus \mathbf{y}^*_2$.
5. For $i : y_{i3} = 0$, sample y^*_{i3} from the distribution $y^*_{i3}|\mathbf{y}, \boldsymbol{\theta}, \mathbf{y}^* \setminus \mathbf{y}^*_3$.
6. For $i : y_{i4} = 0$, sample y^*_{i4} from the distribution $y^*_{i4}|\mathbf{y}, \boldsymbol{\theta}, \mathbf{y}^* \setminus \mathbf{y}^*_4$.
7. For $i : y_{i5} = 0$, sample y^*_{i5} from the distribution $y^*_{i5}|\mathbf{y}, \boldsymbol{\theta}, \mathbf{y}^* \setminus \mathbf{y}^*_5$.

[2]The notation "\" represents "except", e.g., $\mathbf{y}^* \setminus \mathbf{y}^*_1$ says all elements in $\mathbf{y}^*$ except $\mathbf{y}^*_1$:

Bayesian estimation techniques are necessary here for several reasons. First, the censoring of the outcome variables, in conjunction with endogeneity, render most two-stage estimators inapplicable. Second, the missing elements in $\mathbf{\Omega}$ make it unclear how to guarantee positive-definiteness and the Bayesian approach reparameterizes the model to avoid the issue. Finally, maximum simulated likelihood is applicable, however, it is very slow. The availability of a full-set of conditionals makes Gibbs sampling the most attractive option.

4.2 Data

Examining the RFC presents limitations because data are not readily available and need to be hand-coded from record books. As a result, many previous papers either look at a time series of RFC lending (Butkiewicz, 1995) or bank-level data restricted to Federal Reserve member banks (Mason, 2001; Calomiris et al., 2013).[3] In addition, dealing with sample selection is difficult because neither the *New York Times* nor the quarterly and monthly *Reports of Activities of the Reconstruction Finance Corporation* report applied or declined assistance. This paper overcomes these limitations and contributes to this literature by employing a comprehensive, bank-level data set built from the original applications submitted to the RFC. With these more detailed data, the multivariate treatment effect model can be employed to control for the selection mechanisms qualifying banks for assistance, thereby implementing a complete framework to examine the publication of the RFC's authorizations and stigma.

The *RFC cards*, *Paid Loan Files*, and *Declined Loan Files* employed in Section 3.1 are used to define the RFC application and approved amounts (y_{i1} and y_{i2}). These data are merged with a separate data set constructed from the *Rand McNally Bankers' Directory.* The directory describes balance sheets, charters, correspondent relationships, and other characteristics for all banks (Federal Reserve members and nonmembers) in a given state for a given year. Additional data are gathered from the 1930 U.S. census of agriculture, manufacturing and population, describing the characteristics of a county and a bank's business environment. Census covariates include the number of wholesale retailers, number of manufacturing facilities, acres of cropland, and percent of votes that were Democratic.

The data are applied to the 5 equation model as follows: the outcome variable for equation (2),

[3]Looking at Federal Reserve member banks has the advantages associated with a richer set of financial ratios, which are not available for nonmember institutions.

y_{i1}, is the total amount of RFC assistance requested by each bank by December 1933. This outcome is censored with point mass at zero for banks that did not apply for assistance and a continuous distribution for the different loan amounts requested. The outcome variable for equation (3), y_{i2}, is the total amount of RFC assistance approved. This outcome is also censored with point mass at zero for banks that were declined assistance and a continuous distribution for the different loan amounts approved. The RFC's decision to lend was based on the solvency of the banks. However, after going through the examiners' reports, it is apparent that the RFC also considered banks' importance to their local market and features of that market (e.g., agriculture and town size).

The outcome variable for equations (4)–(6) is the total amount of "loans and discounts" (hereafter, referred to as LD) for each bank taken from its January 1935 balance sheet. The outcome for the treatment responses is again censored with point mass at zero for banks that failed since the time of the loan application period and a continuous distribution with LD representing a bank's health and the state of the local economy. LD is chosen to measure a bank's performance following the literature on the credit crunch and its relation to economic activity (Bernanke, 1983; Calomiris and Mason, 2003). This particular element of the paper is unique because it captures a bank's financial intermediary function. Changes to lending, which can occur either by bank supply or consumer demand of loans, is an important quantity with respect to recovery. Butkiewicz (2002) states that during the recovery period bank lending declined because banks purchased more securities, which involved less risk, and because potential borrowers had weakened financial positions. The decline in bank lending during the Depression was a major concern for RFC officials and the Roosevelt administration (Butkiewicz, 2002). LD in 1935 also represents a long-run outcome. While short-run responses are interesting in their own respect, these outcomes are not available because the balance sheet data are only available every 6 months. Thus, by looking at stigma's effect on bank lending in 1935, this paper is able to investigate stigma's longer-run effect on the resuscitation of the financial system and on the goals of the RFC.

Finally, the *Stig* variable in equation (5) is constructed from historical issues of the *New York Times*. The select lists revealed through the newspapers during the second half of 1932 and early 1933 are the events of interest because newspapers are the most accessible form of the information. Additionally, upon the initial release of the information, the public did not know how extensive the RFC program was and that nearly half of the banks were receiving assistance. Therefore, it is

reasonable to believe that those named in the initial lists were the most stigmatized because people had yet to realize how far the program reached. The *Stig* variable takes the value "1" if a bank's name was revealed in the *New York Times* and "0" otherwise.[4] Further details on the group of named banks are provided in the next section.

4.2.1 Descriptive Statistics

The data set includes all banks operating in 1932 in Alabama, Arkansas, Michigan, Mississippi, and Tennessee. Unlike previous studies on the RFC, this includes non-Federal Reserve member banks, as well as member institutions. Looking solely at member banks may misrepresent the banking population because these institutions were often healthier and had additional outlets for relief funds through the discount window. Furthermore, banks that received assistance only through the discount window were safe from being publicly named. The sample consists of 1,794 banks, of which 908 banks applied for RFC assistance and 800 of those were approved while 108 were declined. Roughly half of the banks in each state applied for assistance from the RFC. From the applicant pool, about 88% of the submitted applications were approved. In this sample, of the 800 banks approved RFC assistance, 192 bank names were published in the initial *New York Times* reports.

These five states are studied because many relief efforts were focused in these areas, and they provide variation across bank and county characteristics, sizes, and Federal Reserve districts. Federal Reserve district variation is necessary because RFC lending was concurrent with lending through the discount window. Federal Reserve policies differed across districts and hence impacted the rate at which banks failed (Richardson and Troost, 2009). The states selected for this study include districts 6, 7, 8, and 9 to capture the differing Federal Reserve policies. The lending capabilities of the RFC were larger than that of the Federal Reserve's discount lending because the RFC could assist nonmember banks.[5] Additionally, some of these states are split between districts, so Federal Reserve district effects can be controlled for separately from state effects. While the study is limited because it only covers five states, it is comprehensive in the sense that the entire banking

[4]Other specifications of the *Stig* variable were considered, where it was defined by particular publication dates and lists. However, the main results were similar across the specifications and the presented case achieved a higher posterior model probability.

[5]The RFC and Discount Window operated similarly in this period. However, the RFC loans had interest rates around 5-5.5 with maturities of up to six months. Whereas, the discount window loans had an average interest rate of 3.5 across the districts with maturities of one month to one year (FRB, 1932).

			Approved	
Variable	**Non-Applicant**	**Declined**	**Non-revealed**	**Revealed**
No. Banks	886	108	608	192
Average Age	25	25	29	35
Financial Characteristics (averages)				
Cash / Assets	0.17	0.11	0.14	0.13
Deposits / Liabilities	0.71	0.70	0.72	0.70
Cash / Deposits	0.29	0.17	0.19	0.19
(Capital+Surp.&Und.Prof.) / Assets	0.21	0.18	0.20	0.19
Total Assets (millions)	1.02	2.50	1.25	2.92
Charters and Memberships (counts)				
State Bank	609	73	510	150
National Bank	198	23	81	35
ABA Member	487	63	364	154
Correspondents (averages)				
Total Correspondents	2.5	2.7	2.5	3.3
Out of State Corres.	1.4	1.6	1.4	2.0
Market Shares (averages)				
Liab. / County Liab.	0.21	0.20	0.22	0.25
Liab. / Town Liab.	0.71	0.66	0.76	0.68
County Characteristics (averages)				
No. Wholesale Retailers	27	33	28	28
% Vote Democratic	67	65	69	65
No. Manufact. Est.	34	44	36	39
Cropland ($\times$1000 acres)	100	116	100	107

Table 5: Characteristics of the banks in each subgroup in 1932 and county characteristics. Data are gathered from the *Rand McNally Bankers' Directory* and the 1930 census.

population in the five states is captured.

Table 5 presents descriptive statistics, separated into four subgroups: non-applicants, declined banks, approved non-revealed banks, and approved revealed banks. The table displays some differences among these groups of banks. Banks that applied for assistance held less cash, with the declined sample holding the lowest amount relative to banks that did not apply for assistance. Approved banks tended to be more important to their local market, coinciding with information in the examiners' files. Additionally, declined banks appear to have operated in areas with more manufacturing and agriculture relative to the other subsamples. Before the fall of 1930, the decrease in agricultural prices concentrated bank failures in farming areas, explaining the harsh economic condition of these regions (Richardson, 2007). These fundamental differences across the banks' balance sheets and locations motivate the joint model employed in this paper.

With respect to banks that were revealed and those that were not, the banks look relatively

similar in terms of financial ratios, charters, market shares, and county characteristics. The main differences lie in bank age, total assets, and correspondent network. Correspondent banks were designated in reserve cities of the Federal Reserve system and often provided smaller, local banks with liquidity (Richardson and Troost, 2009). Correspondent relationships represented a bank's importance to the national network of banking (Calomiris et al., 2013). Table 5 shows that revealed banks were larger in terms of total assets and more connected than non-revealed banks. This is consistent with the fact that part of the published names included loans over $100,000 from February - July. Table 5 also shows that revealed banks had been operating longer. This complication – a relationship between bank size, network size, age, and the *New York Times* list – emphasizes the importance of including these characteristics as control variables in the model. In addition, a model selection exercise is conducted to test whether the stigma variable is simply picking up an effect of the bank's age/network size/asset size or, instead, is actually adding information to the specification. This is discussed in Section 4.3.

Before moving on to the results, it is useful to provide some context about the changes in bank lending over time, as lending is the main outcome of interest. To do so, *Rand McNally* balance sheet data are collected for banks in the sample for 1926 and 1929 in addition to 1932 and 1935, and growth in "loans and discounts" is calculated. Figure 5 shows the growth in lending for the four subgroups. As expected, bank lending fell for the whole sample after 1929. The fall in lending is important because reducing credit intermediation has large effects on the macroeconomy (Bernanke, 1983). The declined bank sample experienced the largest changes and the non-applicants experienced the smallest. Focusing on the approved bank subsample, it is clear that the revealed banks contracted their lending more drastically than the non-revealed banks after 1932, whereas previously the two groups looked somewhat similar. While it seems plausible that the greater contraction in lending was due to being revealed and stigma, recall that the revealing was non-random, and Table 5 demonstrates there are some differences between the revealed and non-revealed banks. These differences, along with the selection mechanisms, can be controlled for in the multivariate model to compute how much of this breakdown in financial intermediation was due to stigma.

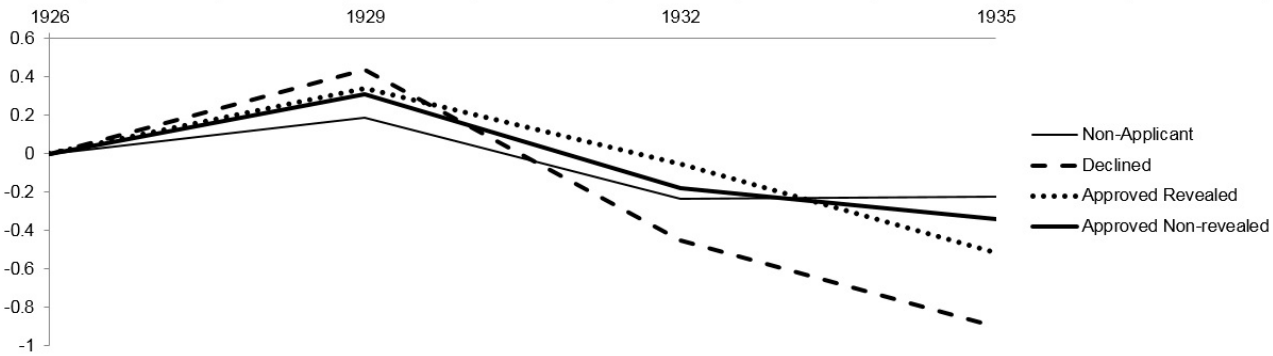

Figure 5: Growth in bank lending for each subgroup.

4.3 Multivariate Results

Table 6 displays the results for the multivariate treatment effect model with sample selection. The results are based on 11,000 MCMC draws with a burn in of 1,000. The priors on $\boldsymbol{\beta}$ are centered at 0 with a variance of 5, and the priors on $\boldsymbol{\Omega}$ imply that $E(\boldsymbol{\Omega}) = .4 \times I$ and $SD(\text{diag}(\boldsymbol{\Omega})) = 0.57 \times I$. A prior sensitivity analysis is done (available in Appendix Table 11) and the results show nearly no sensitivity around these benchmark results.

The basic results on the determinants of applications, approvals, and lending align with intuition and existing research on the RFC. Banks in the 6th district are not statistically different from the 9th district. This finding may align with Richardson and Troost (2009), who show that the 6th district had a more liberal discount lending policy than the 8th district, which is negative here. Richardson and Troost (2009) focus on lending before 1932, but perhaps some of these effects carried over. The results further show that market share has a positive impact on receiving RFC assistance. This finding aligns with the comments from the RFC examiners and agrees with Mason (2001) who also finds that banks' importance to their local market has a positive effect on receiving RFC loans. Lagged bank size has a positive effect on bank lending for all subgroups other than the declined sample (where it is not statistically different from 0). While there are many results presented, the discussion will be focused on stigma and its effect on bank lending.[6]

The key variables of interest are y_2 and $y_2 \times Stig$ in Table 6. After controlling for a bank's health, business environment, and contagion channels, the estimated coefficients on these variables represent the effect of RFC lending on bank lending, and how the revealing impacted the conversion

[6]For a thorough review of the determinants of RFC application and approval decisions, see Vossmeyer (2016).

Variable	2) Application	3) RFC Decision	4) Declined	5) Approved	6) Non-applicant
Intercept	-0.787 (0.170)	-0.740 (0.207)	-0.951 (0.667)	0.381 (0.218)	-0.207 (0.084)
Bank Age	0.002 (0.001)		-0.002 (0.007)	-0.001 (0.001)	-0.001 (0.001)
Financial Characteristics					
Paid-Up Capital	1.377 (0.162)	1.317 (0.187)	8.515 (2.136)	3.470 (0.184)	1.694 (0.169)
Loans & Discounts	0.281 (0.021)	0.346 (0.024)	-0.765 (0.184)	-0.102 (0.130)	0.060 (0.020)
Bonds & Securities	-0.508 (0.043)	-0.563 (0.051)			
Cash / Assets	-1.715 (0.290)	-1.577 (0.345)	-0.207 (1.048)	0.944 (0.296)	-0.119 (0.111)
Deposit / Liab.	0.263 (0.070)	0.185 (0.074)			
Total Assets			0.234 (0.150)	0.183 (0.007)	0.261 (0.011)
Correspondents					
No. Corres.			-0.105 (0.107)	0.062 (0.021)	0.023 (0.016)
Corres. Out State			0.151 (0.116)	0.014 (0.021)	0.009 (0.016)
Charters, Memberships, and Depts.					
Bond Dept.	0.116 (0.034)				
Savings Dept.	-0.046 (0.026)				
Trust Dept.	0.042 (0.029)				
ABA Member		-0.028 (0.026)			
National Bank		-0.050 (0.038)	0.701 (0.361)	-0.078 (0.075)	-0.046 (0.049)
State Bank			0.433 (0.325)	-0.035 (0.070)	-0.009 (0.043)
County Characteristics					
Wholesale Retail			0.005 (0.003)	0.001 (0.000)	0.000 (0.000)
% Vote Demo.		0.000 (0.000)			
Manufact. Est.		0.000 (0.000)	-0.004 (0.002)	-0.001 (0.000)	0.000 (0.000)
Acres Cropland		-0.307 (0.196)	-0.288 (1.130)	0.554 (0.280)	0.283 (0.190)
Town Pop. 1932	-0.964 (0.179)	-1.511 (0.201)			
Town Pop. 1935			-0.463 (0.145)	-4.029 (0.702)	-5.420 (0.625)
Market Shares					
Liab./County Liab.		0.107 (0.056)			
Liab./Town Liab.	0.172 (0.071)		0.452 (0.274)	-0.007 (0.056)	-0.018 (0.038)
Dummies					
Fed Dist. 6	0.137 (0.168)	0.070 (0.207)	0.224 (0.456)	-0.059 (0.196)	0.155 (0.060)
Fed Dist. 7	0.344 (0.167)	0.249 (0.206)	0.258 (0.438)	-0.290 (0.198)	0.003 (0.061)
Fed Dist. 8	0.336 (0.170)	0.332 (0.211)	0.103 (0.471)	-0.297 (0.201)	0.157 (0.061)
RFC Request (y_1)			1.166 (0.369)	-0.906 (0.215)	
RFC Approve (y_2)				1.446 (0.189)	
$y_2 \times Stig$				-0.080 (0.029)	

Table 6: Posterior means and standard deviations are based on 11,000 MCMC draws with a burn-in of 1,000. Column 1 displays the variable names. Columns 2-6 display the results for equations 2-6, respectively.

of RFC lending to bank lending. The results for both variables are statistically different from 0 (95% credibility interval calculated using quantiles that does not include zero). They indicate that the RFC has positive effect on bank lending, however, the revealing has a negative effect.

Examining the magnitudes of these two effects is essential but complicated due to the censoring of the outcome variables. Further interpretation is afforded using covariate and treatment effect calculations, which are important for understanding the model and for determining the impact of a change in one or more of the covariates. Specifically, how does a unit change in RFC lending transfer to bank lending and how is that amount changed by the revelation? In general terms, the covariate effect calculation for the jth covariate is as follows:

$$\begin{aligned} \delta_j &= \int \frac{\partial E(\mathbf{y}_i|\mathbf{x},\boldsymbol{\theta})}{\partial x_j} f(\mathbf{x})\pi(\boldsymbol{\theta}|\mathbf{y})d\mathbf{x}d\boldsymbol{\theta} \\ &\approx \frac{1}{nG}\sum_{i=n}^{n}\sum_{g=1}^{G}\frac{\partial E(\mathbf{y}_i|\mathbf{x}_i,\boldsymbol{\theta}^{(g)})}{\partial x_j} \end{aligned} \tag{8}$$

for $g = 1, \ldots, G$ draws from the posterior distribution. Put simply, the covariate effect is calculated as a standard marginal effect from the Tobit setting and is averaged over the sample to deal with data variability and then averaged over the MCMC draws from the posterior distribution to deal with parameter uncertainty. The advantages of this approach for calculating covariate effects are discussed in Jeliazkov and Vossmeyer (2016).

Implementing the techniques in equation (8) and using the estimated posterior distribution of the coefficient of the endogenous y_2 in Table 6, the covariate effect of the RFC on bank lending is $\delta_{RFC} = 0.574$, and the distribution of the effect is in Appendix Figure 8. This can be interpreted as \$10,000 of RFC assistance translating to \$5,740 of LD in 1935, which is a strong, positive result. RFC assistance was effectively pushed beyond banks, trickling into local economies through lending, thus promoting and restoring confidence in the financial system.

Again, implementing the techniques in equation (8) and using the posterior estimate of the coefficient of the endogenous $y_2 \times Stig$ in Table 6, the covariate effect of the RFC-stigma interaction term is $\delta_{RFC\times Stig} = -0.0319$ (distribution of the effect is in Appendix Figure 9). Thus, publishing a bank's name in the *New York Times* reduced the conversion of RFC lending to bank lending by \$319 for every \$10,000. Once revealed as a bailout recipient, a bank's lending contracted, which aligns with the steeper decline shown for the dotted line in Figure 5. The channels driving this result could be the following: (i) depositors and investors may have been more reluctant to fund a

bank revealed to borrow from the RFC, thus raising the cost of deposits and capital for that bank, and resulting in less lending; (ii) banks could have been hoarding the relief funds in fear of a run (Friedman and Schwartz, 1963) and changing their asset allocation to less risky assets (Butkiewicz, 2002).

A few considerations point to the economic significance of this result. First, the conversion of RFC lending to bank lending goes from .57 to .54, about a 6% change. The standard deviation of the covariate effect δ_{RFC} is 0.094 (see the distribution in Appendix Figure 8). Thus, the reduction in lending is roughly equal to one third of the standard deviation of the RFC result. The 6% change meaningfully offset the RFC's goals of encouraging lending and resuscitating the economy. Though seemingly small in magnitude, the RFC disbursed billions of dollars, which when coupled with a revelation rate of 25 percent during this episode, likely led to a nontrivial contraction in aggregate bank lending considering the macroeconomy (Hanson et al., 2017). Specific to this approved-revealed subsample (192 banks), taking into account the total amount of RFC money disbursed, the contraction would equate to roughly $4.5 million in forgone bank lending, where total bank lending for this sample was about $110 million in 1935. The effect could be further amplified through correspondent networks (Mitchener and Richardson, 2016) and translate to real economic effects in retail sales (Cohen et al., 2017).[7] Stigma was not the key culprit for the overall contraction of lending by banks in the Depression; however, the results show that stigma did, indeed, contribute to the breakdown in financial intermediation. More broadly, stigma thwarted the RFC's objective of encouraging lending (although it did not reverse the RFC's effectiveness) and contributed to the sluggish recovery by weakening credit channels (Bernanke, 1983).

Another aspect to consider, in addition to a bank's lending capabilities, is how stigma affects a bank's probability of failure. Interest lies in the average difference in the implied probabilities between the cases when a bank is revealed as a recipient of RFC assistance and when a bank is not revealed. Let $\mathbf{z}_i$ reflect all covariates other than the interaction term of interest, let $w_i^\dagger$ be the case when a bank's name is not published, $w_i^\ddagger$ be the case when a bank's name is published in the *New York Times*, and set $y_{i4} = 0$ to represent bank failure. As in Section 3.2, a predictive distribution

[7] Mitchener and Richardson (2016) show that interbank networks amplified the contraction and reduced aggregate lending by 15 percent. Cohen et al. (2017) show that through lending channels, suspending 10% of national banks leads to a 3.77% decline in retail sales.

can be constructed by evaluating

$$\{\Pr(y_{i4}=0|w_i^{\dagger})-\Pr(y_{i4}=0|w_i^{\ddagger})\} = \int \{\Pr(y_{i4}=0|w_i^{\dagger},\mathbf{z}_i,\boldsymbol{\theta})-\Pr(y_{i4}=0|w_i^{\ddagger},\mathbf{z}_i,\boldsymbol{\theta})\}\pi(\mathbf{z}_i)\pi(\boldsymbol{\theta}|\boldsymbol{y})d\mathbf{z}_i d\boldsymbol{\theta}. \tag{9}$$

Note that this distribution is marginalized over $\{\mathbf{z}_i\}$ and $\boldsymbol{\theta}$, so there is no residual uncertainty coming from the sample or estimation procedure. The mean of the predictive distribution gives the expected difference in computed pointwise probabilities as being revealed changes to not revealed (Jeliazkov et al., 2008). Computing the probabilities is not straightforward and requires additional simulation techniques. The Chib-Ritter-Tanner (CRT) method is employed to evaluate the likelihood function, which was developed in Jeliazkov and Lee (2010).

Specifically, the question of interest is: In the sample of approved-revealed banks, what is the difference in the probability of bank failure if the bank names had never been published? Implementing the methods in equation (9) – calculating the probabilities inside the integral using the CRT method, then averaging these over the sample and MCMC draws from the posterior – results in the mean of the predictive distribution $\{\Pr(y_{i4}=0|w_i^{\dagger})-\Pr(y_{i4}=0|w_i^{\ddagger})\}$ being -0.0048. In other words, if the *New York Times* did not publish the list of banks receiving assistance, the probability of failure for those banks decreases by 0.48 of a percentage point. In the raw data for this subsample, the unconditional probability of bank failure is 17.71 percent (34/192). Thus, the 0.48 percentage point change is a rather small effect (approximately equal to 1 bank failure).[8] This effect most closely aligns with Mason's (2001) results, who also does not find strong negative effects from the publication on the probability of bank failure. Further, the result sheds some light on the broader picture of stigma. While stigma had moderate negative effects on bank lending, it was not severe enough to cause bank failure in the case of the RFC. These findings offer policy-makers some perspective about how the stigma problem manifests itself in bank lending, as well as the magnitude of the issue.

Model Comparison. In the multivariate analysis, model comparison is necessary to examine the support from the data for the stigma model specification and the potential relationship between bank size/network/age and being revealed in the newspapers, which is discussed in detail

[8]The distribution of the covariate effect is in Appendix Figure 10. The opposite computation was done for the subsample of non-revealed banks (i.e., what happens if they were revealed) and the results again show a minimal effect.

in Section 4.2.1. If the stigma variable is actually just picking up elements of the bank's age, correspondent network, and size, then the marginal likelihood should fall in the stigma specification. Variables for the correspondent network, size, and age are already included in the bank lending equation, so adding stigma would result in overfitting of the model.

Model comparison is done by computing marginal likelihood estimates via Chib (1995). The results of the model comparison are presented in Table 7. The table displays the log-marginal likelihood estimate, numerical standard error, and posterior model probability. The marginal likelihood is 26 points higher on the log scale in favor of the stigma specification, giving it a posterior model probability of nearly 1. The specification without the stigma measure is not supported by the data. The information brought forth by a single covariate, the interaction between name publication and RFC assistance, is immense. This result has two important implications. First, in addition to the credible point estimate for the interaction, it is clear that the data heavily support this variable entering the model. Second, the complication with the stigma indicator and bank network/size/age is not an issue, as there is no evidence of overfitting. The model comparison results strengthen the previous finding of negative stigma effects.

	Stigma	**No Stigma**
Log-Marginal Lik.	-7952.0	-7978.6
Numerical S.E.	(0.423)	(0.445)
$\Pr(\mathcal{M}_k \mid y)$	0.999	2.8×10^{-12}

Table 7: Log-marginal likelihood estimates, numerical standard errors, and posterior model probabilities.

5 Treatment Effect of Reluctance

The time series analysis in Section 3 addressed the question of whether and how much the revealing deterred bank participation in the rescue program. Interest remains in how this drop in participation affected economic activity. Because the program became stigmatized, many banks that may have needed additional support during the Depression did not seek assistance. What was the effect of this reluctance on credit channels in the economy?

This question is challenging to answer because one must be able to determine why a bank did not participate in the RFC program. Perhaps, it did not participate because of business opportunities,

stable bank health, insolvency, or fear of having its name revealed as a recipient of emergency help – stigma. The data and methodology in the Multivariate Analysis provide a unique platform to answer this question. With the entire population of banks, one can focus on the "non-applicants" and match them with similar banks in the approved bank sample. This exercise is done and a counterfactual scenario in which these matched banks approached the RFC is considered. The counterfactual effect on bank lending and failure is presented in Appendix 7.3.

While the matching exercise is interesting, there is uncertainty stemming from the matched group as well as the simulated RFC support. To take a more straightforward and less uncertain approach, this section focuses on cancelled loans. As mentioned in Section 3.2, after the revealing, many banks cancelled their RFC loan applications, and bunches of these cancellations occurred around the revealing dates. Given the timing of these cancellations, it is plausible that they were due to fear of stigma. By using the cancelled loan applications, this paper teases out the groups of banks that did not participate in the RFC program because of stigma. Had these banks not cancelled their applications, they would have received RFC support. Thus, understanding the changes to their lending portfolio after they cancelled their loan provides insights into the treatment effect of reluctance.

In the five states studied in this paper, the sample of cancellations is not large enough for a full econometric analysis. Therefore, this section of the paper investigates loan cancellations in all 50 states. The cancellations of interest occurred between July 21 and the end of August, 1932. In particular, banks in this "cancelled" sample submitted their RFC applications just prior to July 21, had their applications approved, but then cancelled them shortly after the revealing (i.e., no funds were disbursed from the RFC); hence, these banks' names were not revealed. "Cancelled" banks submitted their applications to the RFC without any knowledge of the upcoming revealing or stigma; however, once they became aware of stigma, they were reluctant to receive the approved support.

The "cancelled" banks are compared to banks that submitted their RFC applications around the same time but received their RFC loan disbursements between mid-July and July 20, 1932. These "never revealed" banks submitted their applications before July 21, had their applications approved just before July 21, and therefore received their RFC disbursements without ever being revealed. Given that the loan approval process typically took 2-5 weeks, many of the banks in

the two subgroups submitted their applications around the same time. However, due to varying application processing times, some banks had their funds disbursed more quickly and thus were not eligible to be revealed. Others, that received approval just days later, were eligible to be revealed but canceled the application to avoid being stigmatized.[9]

To be clear, all of the banks in the two samples had their RFC applications approved and were not revealed. "Cancelled" banks were not revealed because they cancelled their loan just after July 21, and "never revealed" banks received their disbursement just before July 21. The difference is that "never revealed" banks received their RFC funds and "cancelled" banks did not. Cancelled banks were reluctant to use the RFC's support. The window of time around the July date is small to make sure that all of these banks were submitting applications just weeks apart, which allows for a more straightforward econometric analysis and comparison.

The sample comprises 444 banks, of which 311 are in the "never revealed" group and 133 are in the "cancelled" group. Information on the application decisions and timing come from the *RFC Card Index* and *Cancelled Loan Files*. A panel data set is constructed for these banks with their complete balance sheet taken from the 1926, 1929, 1932, and 1935 *Rand McNally Bankers' Directory*. Figure 6 shows the ratio of loans over total assets for banks in each subgroup from 1926-1935. Evident from the figure is the parallel trend between the two subgroups from 1926-1932. However, after 1932 the loan/asset ratios appear to diverge slightly, with the dashed line of cancelled banks declining more sharply. The figure supports a simple econometric specification, given that the revealing was unexpected. The loan to asset ratio is considered here to provide insights into a bank's asset allocation and to support a more intuitive, linear econometric approach.

A simple linear panel data model is considered. A differences-in-differences approach is implemented so as to allow for straightforward comparisons between the two groups of banks. The specification is supported by the parallel trends pictured in Figure 6. The model is as follows:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \alpha_i + \gamma_t + \varepsilon_{it},$$

where y_{it} is the ratio of loans/assets for bank i at time t, for all $i = 1, ..., 444$ and $t = 1926, 1929, 1932, 1935$. The vector $\mathbf{x}_{it}$ includes: an interaction between a "cancelled" indicator and an after July 21

[9]The October 7th *New York Times* revealing actually had a section on the identities of banks that cancelled their application. The cancelled sample in this paper does not include these banks to ensure no one was revealed. If a bank cancelled its loan rapidly after the approval, it avoided being on the cancelled list.

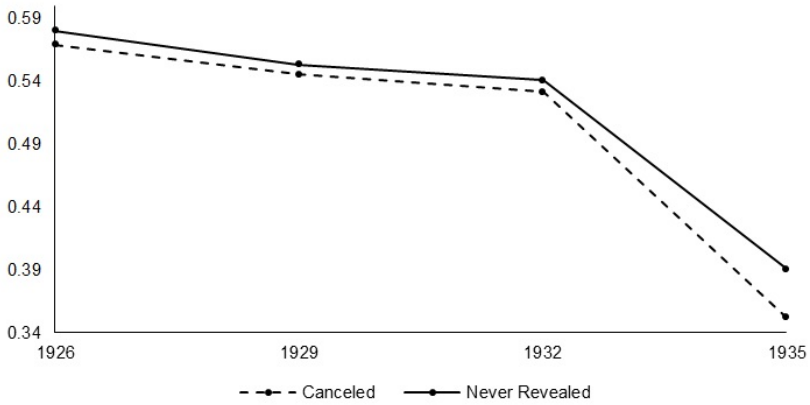

Figure 6: The ratio of loans over total assets for banks in each subgroup is plotted for 1926, 1929, 1932, and 1935.

indicator ($Cancel_i \times 1\{t \geq July\ 21,\ 1932\}$), an interaction between a "cancelled" indicator and a one period before the revealing indicator ($Cancel_i \times 1\{t = (July\ 21,\ 1932) - 1\}$), and other controls for asset growth for bank i and state effects interacted with an after time period indicator so the effects do not get differenced away (Controls).[10] Because $Cancel_i$ is an indicator that takes the value 1 if the bank is in the cancelled group and 0 otherwise, the results are relative to the "never revealed" group. Bank fixed effects are captured in α_i and time fixed effects are captured in γ_t. The simple specification is estimated by OLS and the results are presented in Table 8.

Variable	$y_{it} = Loans/Assets_{it}$
$Cancel_i \times 1\{t = (July\ 21,\ 1932) - 1\}$	0.014 (0.018)
$Cancel_i \times 1\{t \geq July\ 21,\ 1932\}$	-0.045 (0.022)
Time Effects	Yes
Individual Bank Effects	Yes
Controls	Yes
R-squared	0.72

Table 8: Results are estimated by OLS and the standard errors are clustered at the bank-level.

The results from the linear panel data model show that, relative to banks that received RFC loans and were never revealed (because their approval came days before the cutoff), cancelled banks

[10]The results in this section are robust to considering cancellations closer to specific dates of interest and a lagged dependent variable. The specification here gives the largest sample of cancellations and time periods.

decreased their loan to asset ratio by 4.5 percentage points after they cancelled their approved RFC loan. However, before the cancellation, these banks were not statistically different from one another in terms of their loan portfolios. These findings have several implications: (i) given that these banks are not statistically different before the cancellation, the parallel trends in Figure 6 are confirmed in the econometric analysis; (ii) cancelled banks reduced their loan portfolios which suggests that because they were reluctant to receive RFC support, they decreased lending on their balance sheet as a ratio of total assets.

A cancelled bank may have reduced its position on lending because, without the extra liquidity from the RFC, it may have changed its asset allocation to more liquid, safer assets (White, 1984). Throughout the Depression, lending as a ratio of total loans and investments fell about 25 percentage points (FRS, 1943). Thus, a 4.5 difference between subgroups of banks that were statistically similar up until 1932 is meaningful. Government safety nets should eliminate the incentive for banks to reduce asset risk in times of high capital costs and pressure to reduce deposit risk (Calomiris and Wilson, 2004). However, when the program is stigmatized, this incentive is not eliminated.

The story here seems to be that, because the RFC program became stigmatized and saw a massive drop in bank participation (as pictured in Figure 2), many banks became reluctant to seek support, and high numbers actually cancelled their approved support. Had stigma not been an issue, these banks would have had higher loan portfolios, and the increase in bank lending would have converted to more economic activity. These results provide insights into the economic consequences and implications of the reluctance phenomenon, which was otherwise minimally explored.

6 Concluding Remarks

This paper considers two dimensions of the stigma effect that arise when banks receive assistance from an emergency lending program during a financial crisis. The effect is examined by looking at banks' reluctance to borrow from the rescue program and banks' ability to operate as financial intermediaries. The particular program of interest is the Reconstruction Finance Corporation and the event of interest is the sudden publication of the names of banks receiving assistance in the *New York Times*.

The results of the stigmatized rescue program demonstrate that revealing the loan authorizations drastically reduced bank applications for support, and the probability that no applications

would be submitted on a given day increased by 27.9 percentage points. This drop in participation affected credit channels, and as a result, lending did not reach its full capacity. The results of the stigmatized recipient bank show a contraction in the conversion of RFC lending to bank lending.

The induced slowdown in credit production from these two channels indicates that stigma contributed to the breakdown of financial intermediation and impeded the rescue program's objective of restoring confidence in the financial system. The stigma effect, however, was not drastic enough to cause many bank failures; hence, its shock to the overall banking system was limited. The findings in this paper provide useful insights for policy-makers looking to combat the many obstacles involved in crises and corresponding market interventions.

References

Anbil, S. (2018), "Managing Stigma During a Financial Crisis," *Journal of Financial Economics*, Forthcoming.

Armantier, O., Ghysels, E., Sarkar, A., and Shrader, J. (2015), "Discount Window Stigma During the 2007-2008 Finanial Crisis," *Journal of Financial Economics*, 118, 317–335.

Bernanke, B. (2009), "The Federal Reserve's Balance Sheet: An Update," in *Speech at the Federal Reserve Board Conference on Key Developments in Monetary Policy.*

Bernanke, B. S. (1983), "Nonmonetary Effects of the Financial Crisis in the Propagation of the Great Depression," *American Economic Review*, 73, 257–276.

Blau, B., Hein, S., and Whitby, R. (2016), "The Financial Impact of Lender-of-Last-Resort Borrowing from the Federal Reserve during the Financial Crisis," *The Journal of Financial Research*, 39, 179–206.

Butkiewicz, J. (1995), "The Impact of Lender of Last Resort during the Great Depression: The Case of the Reconstruction Finance Corporation," *Explorations in Economic History*, 32, 197–216.

Butkiewicz, J. (1999), "The Reconstruction Finance Corporation, the Gold Standard, and the Banking Panic of 1933," *Southern Economic Journal*, 66, 271–293.

Butkiewicz, J. (2002), *Reconstruction Finance Corporation*, EH.net Encyclopedia.

Calomiris, C. and Mason, J. (2003), "Consequences of Bank Distress During the Great Depression," *American Economic Review*, 93, 937–947.

Calomiris, C. W. and Wilson, B. (2004), "Bank Capital and Portfolio Management: The 1930s "Capital Crunch" and the Scramble to Shed Risk," *Journal of Business*, 77, 421–456.

Calomiris, C. W., Mason, J. R., Weidenmier, M., and Bobroff, K. (2013), "The Effects of the Reconstruction Finance Corporation Assistance on Michigan's Banks' Survival in the 1930s," *Explorations in Economic History*, 50, 525–547.

Chib, S. (1995), "Marginal Likelihood from the Gibbs Output," *Journal of the American Statistical Association*, 90, 1313–1321.

Chib, S. (2007), "Analysis of Treatment Response Data without the Joint Distribution of Potential Outcomes," *Journal of Econometrics*, 140, 401–412.

Chib, S. and Greenberg, E. (1995), "Understanding the Metropolis-Hastings Algorithm," *The American Statistician*, 49, 327–335.

Chib, S. and Jeliazkov, I. (2001), "Marginal Likelihood from the Metropolis-Hastings Output," *Journal of the American Statistical Association*, 96, 270–281.

Chib, S. and Jeliazkov, I. (2005), "Accept-Reject Metropolis-Hastings Sampling and Marginal Likelihood Estimation," *Statistica Neerlandica*, 59, 30–44.

Chib, S., Greenberg, E., and Jeliazkov, I. (2009), "Estimation of Semiparametric Models in the Presence of Endogeneity and Sample Selection," *Journal of Computational and Graphical Statistics*, 18, 321–348.

Cohen, J., Hachem, K., and Richardson, G. (2017), "Relationship Lending and the Great Depression," *Working Paper*.

Darrat, A., Elkhal, K., Banerjee, G., and Zhong, M. (2004), "Why Do U.S. banks Borrow From the Fed," *Applied Financial Economics*, 14, 477–484.

Egan, M., Hortacsu, A., and Matvos, G. (2017), "Deposit Competition and Financial Fragility: Evidence from the US Banking Sector," *American Economic Review*, 107, 169–216.

Ennis, H. (2017), "Interventions in Markets with Adverse Selection: Implications for Discount Window Stigma," *Working Paper*.

Ennis, H. and Weinberg, J. (2013), "Over-the-counter Loans, Adverse Selection, and Stigma in the Interbank Market," *Review of Economic Dynamics*, 16, 601–616.

Fama, E. F. and French, K. R. (1997), "Industry Costs of Equity," *Journal of Financial Economics*, 43, 153–193.

FRB (1932), *Nineteenth Annual Report of the Federal Reserve Board*.

Friedman, M. and Schwartz, A. (1963), *A Monetary History of the United States, 1867-1960*, Princeton University Press.

FRS (1943), *Banking and Monetary Statistics*, Board of Governors of the Federal Reserve System.

Furfine, C. (2001), "The Reluctance of Borrow from the Fed," *Economic Letters*, 72, 209–213.

Geithner, T. (2014), *Stress Test: Reflections on Financial Crises*, Random House.

Gorton, G. (2015), "Stress for Success: A Review of Timothy Geithner's Financial Crisis Memoir," *Journal of Economic Literature*, 53, 975–995.

Greenberg, E. (2008), *Introduction to Bayesian Econometrics*, Cambridge University Press, New York.

Hanson, S., Kashyap, A., and Stein, J. (2017), "A Macroprudential Approach to Financial Regulation," *Journal of Economic Perspectives*, 25, 3–28.

Jeliazkov, I. and Lee, E. H. (2010), "MCMC Perspectives on Simulated Likelihood Estimation," *Advances in Econometrics*, 26, 3–39.

Jeliazkov, I. and Vossmeyer, A. (2016), "The Impact of Estimation Uncertainty on Covariate Effects in Nonlinear Models," *Statistical Papers*, forthcoming.

Jeliazkov, I., Graves, J., and Kutzbach, M. (2008), "Fitting and Comparison of Models for Multivariate Ordinal Outcomes," *Advances in Econometrics*, 23, 115–156.

Jones, J. H. (1951), *Fifty Billion Dollars: My Thirteen Years with the RFC, 1932-1945*, New York: Macmillan Co.

Li, P. (2011), "Estimation of Sample Selection Models with Two Selection Mechanisms," *Computational Statistics and Data Analysis*, 55, 1099–1108.

Mason, J. R. (2001), "Do Lender of Last Resort Policies Matter? The Effects of Reconstruction Finance Corporation Assistance to Banks During the Great Depression," *Journal of Financial Services Research*, 20, 77–95.

Mason, J. R. (2003), "The Political Economy of Reconstruction Finance Corporation Assistance During the Great Depression," *Explorations in Economic History*, 40, 101–121.

Mitchener, K. and Mason, J. (2010), "'Blood and Treasure': Exiting the Great Depression and Lessons for Today," *Oxford Review of Economic Policy*, 26, 510–539.

Mitchener, K. J. and Richardson, G. (2016), "Network Contagion and Interbank Amplification during the Great Depression," *NBER Working Paper*.

Peristiani, S. (1998), "The Growing Reluctance to Borrow at the Discount Window: An Empirical Investigation," *Review of Economics and Statistics*, 80, 611–620.

Richardson, G. (2007), "Categories and Causes of Bank Distress During the Great Depression, 1929-1933: The illiquidity Versus Insolvency Debate Revisited," *Explorations in Economic History*, 44, 588–607.

Richardson, G. and Troost, W. (2009), "Monetary Intervention Mitigated Banking Panics During the Great Depression: Quasi-Experimental Evidence from a Federal Reserve District Border 1929-1933," *Journal of Political Economy*, 117, 1031–1073.

Tierney, L. (1994), "Markov Chains for Exploring Posterior Distributions," *Annals of Statistics*, 22, 1701–1761.

Tobin, J. (1958), "Estimation of Relationships for Limited Dependent Variables," *Econometrica*, 26, 24–36.

Vossmeyer, A. (2014), "Treatment Effects and Informative Missingness with an Application to Bank Recapitalization Programs," *American Economic Review*, 104, 212–217.

Vossmeyer, A. (2016), "Sample Selection and Treatment Effect Estimation of Lender of Last Resort Policies," *Journal of Business and Economic Statistics*, 34, 197–212.

Wheelock, D. (1990), "Member Bank Borrowing and the Fed's Contractionary Monetary Policy during the Great Depression," *Journal of Money, Credit, and Banking*, 22, 409–426.

White, E. (1984), "A Reinterpretation of the Banking Crisis of 1930," *Journal of Economic History*, 44, 119–138.

7 Appendix

7.1 Time Series Appendix

The maximum likelihood estimates and standard errors presented in Table 9 are very close to the posterior means and standard deviations presented in Table 2. Ordinary least squares estimates are also presented.

	MLE	OLS
Intercept	0.54 (0.10)	2.41 (0.52)
$\rho,\ y_{t-1}$	0.69 (0.01)	1.36 (0.11)
Fraction remaining	-0.36 (0.12)	-0.77 (0.53)
$1\{t \geq July\ 21,\ 1932\}$	-0.17 (0.04)	-0.50 (0.48)
$1\{t \geq August\ 22,\ 1932\}$	-0.72 (0.04)	-1.28 (0.46)

Table 9: Maximum likelihood and ordinary least squares estimates and standard errors for specification (3).

Table 3 reports that revealing the loan authorization increases the probability that the RFC receives 0 applications a day by 27.9 percentage points. Figure 7 displays a histogram which demonstrates the distribution of the average effect (over the sample units) as a function of parameter uncertainty.

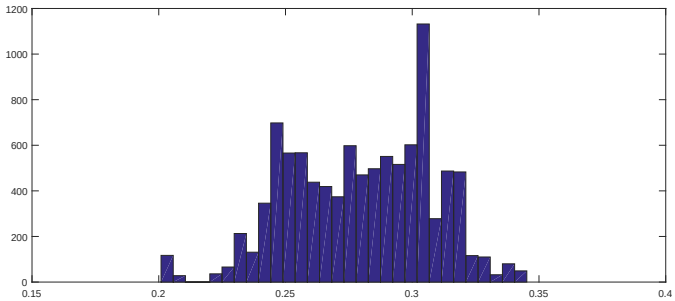

Figure 7: Distribution of the covariate effect of the revealing on the probability that the RFC receives 0 applications.

Figure 3 presents a daily time series for non-depository institutions. This series is analyzed to examine whether the drop in participation is unique to banks. The estimation results of the autoregressive Poisson model for the non-depository series are reported in Table 10, which demonstrate no support for the revealing dates in terms of changing participation rates among non-depository

institutions.

Variable	Specification for the number of applications
Intercept	-2.96 (0.47)
$\rho,\ y_{t-1}$	0.54 (0.11)
Fraction remaining	2.26 (0.49)
$1\{t \geq July\ 21,\ 1932\}$	0.63 (0.64)
$1\{t \geq August\ 22,\ 1932\}$	-0.21 (0.27)

Table 10: Results for the non-depository series. Posterior means and standard deviations are based on 11,000 MCMC draws with a burn-in of 1,000.

7.2 Multivariate Appendix

7.2.1 Sensitivity Analysis

The priors for the multivariate model appear at the beginning of Section 4.3. Prior selection generally involves some degree of uncertainty and this section evaluates how sensitive the results are to the assumptions about the prior distribution.

The key coefficient of interest, $\beta_{RFC \times Stig}$, is the estimate on the endogenous interaction variable $y_{i2} \times Stig$ in equation (5). The coefficient reported in Table 6 shows $\beta_{RFC \times Stig} = -0.080$, which implies that stigma has a negative impact on bank lending. To check the sensitivity of this result to the prior specification, Table 11 reports the coefficient $\beta_{RFC \times Stig}$ for different hyperparameters.

	SD($\beta_{RFC \times Stig}$)		
Mean($\beta_{RFC \times Stig}$)	1.5	4.4	14.14
-1	-0.079	-0.086	-0.087
0	-0.076	-0.085	-0.087
1	-0.074	-0.085	-0.087

Table 11: $\beta_{RFC \times Stig}$ as a function of the hyperparameters. The priors for $\boldsymbol{\beta}$ in the benchmark model are centered at zero with a variance of 5.

The results indicate nearly no sensitivity around the benchmark result of -0.08. This finding holds true for all of the parameter estimates. Skeptics of stigma who would place strong negative priors on its existence would be overridden by the data. The data speak loudly for the multivariate results of stigma and the overall findings. Furthermore, the priors on the time series model (presented in Table 2) are not sensitive to varying hyperparameters.

7.2.2 Results

Section 4.3 reports that covariate effect of RFC lending on bank lending is 0.574, and the covariate effect of the $RFC \times Stig$ interaction is -0.319. The histograms in Figures 8 and 9 display the distribution of the average effect (over the sample units) as a function of parameter uncertainty for each effect.

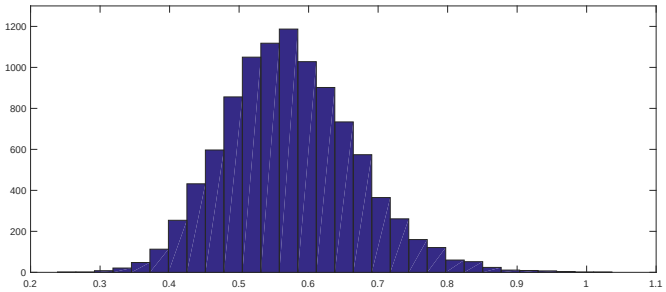

Figure 8: Distribution of the covariate effect δ_{RFC}.

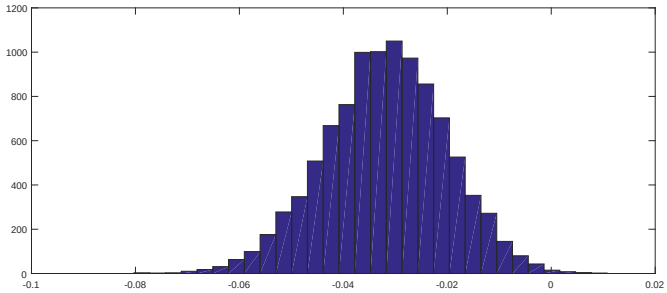

Figure 9: Distribution of the covariate effect $\delta_{RFC \times Stig}$.

Section 4.3 also reports that if the newspapers did not publish the list of banks receiving assistance, the probability of failure for those banks decreases by 0.48 of a percentage point. The histogram in Figure 10 displays the distribution of the average effect on the probability of bank failure as a function of parameter uncertainty.

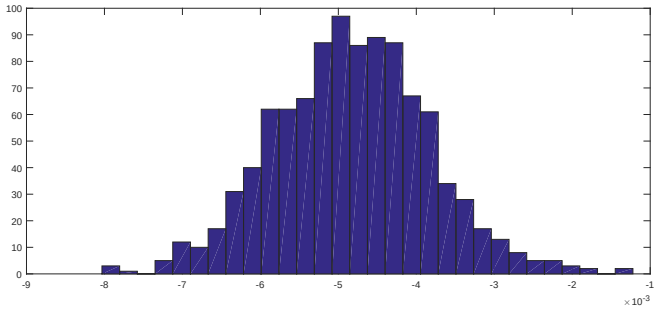

Figure 10: Distribution of the covariate effect of $RFC \times Stig$ on the probability of bank failure.

7.3 Reluctance Appendix

The data and methodological framework developed in the Multivariate Analysis offer a unique platform to investigate the treatment effect of reluctance. With the entire population of banks in the 5 states, one can focus on the non-applicant sample (886 banks). These are banks that did not seek assistance from the RFC. Their particular reasons for not seeking assistance are unclear, however likely fall into 3 classes: stable bank health, insolvency, or fear of having their name revealed as a recipient of emergency help – stigma. In order to tease out the latter group, the banks in the non-applicant sample are carefully matched based on balance sheet characteristics with banks in the approved bank sample. Generally speaking, they were selected on the basis that they were not so unhealthy that they would not have qualified for a loan (i.e., they don't look like the declined bank subsample) and not too healthy in which they did not need assistance. Subsequent characteristics, such as network and county characteristics were considered for more borderline cases. After carefully examining each bank in the 886 non-applicant sample, 218 banks appear very similar to the approved bank subsample, and thus are the potential "stigma non-applicants".

Interest centers upon a scenario in which these banks actually applied for assistance and the difference in economic outcomes between this scenario and the original case in which they did not apply. These quantities are available using the simulation methods described by (9). The predictive distribution is the difference in the probability of bank failure if the 218 stigma non-applicants applied for RFC assistance. The granted RFC amount for each of these banks is matched based on similar banks in the approved pool as a ratio of total assets. Evaluating the likelihood function in each case and computing the probability is done using the Chib-Ritter-Tanner simulation method.

The mean of the predictive distribution is -0.016. In other words, if the stigma non-applicants actually applied for assistance, the probability of failure for those banks decreases by 1.6 percentage points. This is a small effect, indicating that in the sample of stigma non-applicants, being granted RFC assistance would have possibly spared a few banks from failure, but not many. In the raw data, 37 of the 218 banks in this sample failed.

The next aspect to consider is lending. While not applying does not have major implications for bank survival in the sample of stigma non-applicants, perhaps the stigma effect manifests itself in lending as it did for the approved-revealed banks. To answer this question, the methods described in (8) are employed for the 218 banks. In this sample, the covariate effect of RFC lending is $\delta_{RFC} = 0.664$. Thus, \$10,000 dollars of RFC assistance would translate to \$6,640 of LD. This result is positive and actually represents a conversion 16% higher than that of the approved bank subsample. Perhaps the result is higher because these banks tend to be a bit healthier than the average approved bank, thus they had a bit more flexibility on their balance sheet. With these banks not applying for assistance because the RFC was stigmatized, lending could have reached a higher capacity, thereby increasing the production of credit. Taking another look at Figure 5, the negative growth in lending for non-applicants (while it is the least severe), perhaps could have flattened out sooner or by a greater degree if some of these banks applied for assistance.

Notably, the analysis in this section rests on the selection of banks and simulated RFC support. While these procedures add uncertainty to the results, the findings corroborate with that of the actual approved-revealed sample. Figures 11 and 12 display histograms of the distribution of the average effect on bank lending and the probability of bank failure, respectively, as a function of parameter uncertainty.

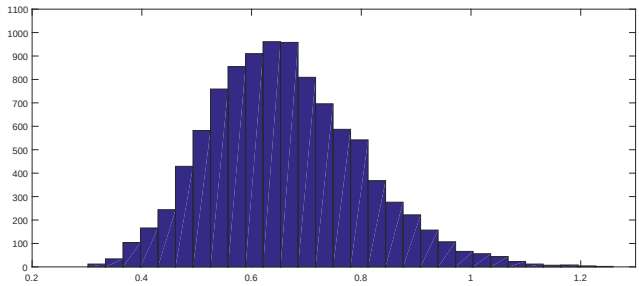

Figure 11: Distribution of the covariate effect of the RFC on bank lending for the stigma non-applicants.

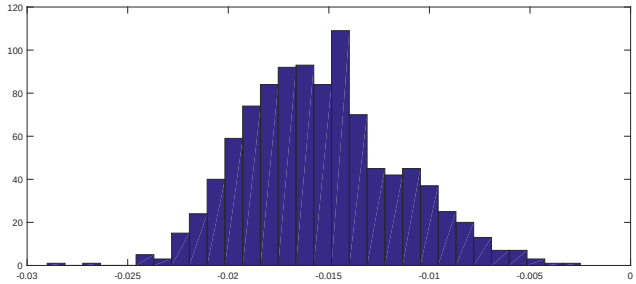

Figure 12: Distribution of the covariate effect of the $RFC \times Stig$ on the probability of bank failure for the stigma non-applicants.