Bank Supervision, Regulation, and Financial Instability During the Great Depression

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How do supervisory and regulatory regimes affect financial stability? Drawing on the variation in financial distress across U.S. counties during the Great Depression, this paper presents new evidence on what factors matter. The insulation of state bank supervisors from industry influence, supervisory powers to liquidate and license banks, and state differences in capital requirements and branching laws help account for the wide variation failure rates for state-chartered banks from 1929 to 1933 – even after controlling for local economic conditions.

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I. Introduction

The 1997 East Asian financial crisis, the prolonged stagnation of the Japanese economy in the 1990s, and financial crises in many developing countries since the end of the Bretton Woods System have rekindled interest in understanding financial sector distress. Anemic banking sectors incapable of weathering economy-wide shocks have disrupted credit channels and exacerbated economic contractions. It is tempting to accept recent history as prima facie evidence for changing the current system of financial regulation and supervision. Consequently, policymakers have proposed a variety of measures aimed at stabilizing banking systems: risk-adjusted deposit insurance, increased supervision and prudential regulation, disclosure rules, and institutional reform of the banking sector.

Even if we accept the notion that policy change is part of the prescriptive cure for ailing banking systems, what are the appropriate lessons policymakers should draw on in deciding how to reform the current regulatory system? For example, what are the characteristics of regulatory and supervisory regimes that promote financial stability? Are financial crises less likely if countries harmonize their regulations and supervisory structures and adopt "best-practice" features universally? And what are the advantages or disadvantages of vesting regulatory authority in a single agency – either internationally or within regional boundaries such as the European Union?

Research aimed at providing insight into the relationship between supervision, regulation, and financial stability has faced significant analytical and empirical challenges, especially in its ability to assemble evidence with sufficient identifying variation to test this relationship. Several recent studies have drawn on cross-sectional evidence from developing countries, but the inability to obtain comprehensive data on supervisory and regulatory regimes have limited their saliency.[1] Moreover, even in cross-sectional studies that have succeeded in assembling some data, little if any research has fully integrated both supervision and regulation into the analysis. The empirical strategy of analyzing time-series variation within a single country generally has not been a fruitful alternative because supervisory and regulatory regimes change infrequently within a country and do not provide sufficient identifying variation.

Given the data limitations associated with recent financial crises, the lens of history may provide better analytical focus for understanding which supervisory and regulatory factors matter for financial instability. During the 1930s, there were many examples of severe banking crises. In Europe, at least 12 countries (including France, Germany, Italy, Belgium, and Hungary) experienced financial crises; across the Atlantic, the United States also experienced a massive banking crisis during the Great Depression. Of the approximately 25,000 commercial banks that were in existence in the U.S. in 1929, nearly 10,000 of them suspended operations between 1929 and 1933. Of these, approximately three-quarters of them were state-chartered financial institutions that

[1] For example, World Bank studies such as Demirguc-Kunt and Detragiache (1999) lack data on differences in the quality of supervision across countries, and have had to rely on indirect measures (like the rule of law and the quality of the bureaucracy) to proxy for supervisory quality. More recently, Barth, Caprio, and Levine (2000) have made some significant progress on gathering information on developing-

operated under *different* supervisory and regulatory regimes depending on the state in which they were located. Indeed, the dual banking system that exists in the U.S. allowed state banking departments to co-exist with national regulatory agencies (such as the Federal Reserve and the Office of the Comptroller of the Currency); these state regulators established supervisory practices and enforced prudential regulations that applied to state-chartered commercial banks consistent with their respective state banking codes. Given the significant differences in regulatory and supervisory regimes that had emerged across U.S. states by 1929, the banking crises of the U.S. Depression provides a fertile testing ground for exploring the relationship between supervision, regulation, and financial stability and surmounting estimation problems that have plagued previous research.

This paper employs a previously underutilized data set with information on county-level bank failures from 1929-33 to test whether differences in financial regulation and supervision across states are important in accounting for the significant variation in failure rates across U.S. counties and to provide new insight into factors that affect financial stability. The empirical approach used here has several advantages over previous research. First, since the sample of county-level bank failures is considerably larger than comparable cross-country data sets, the accuracy of the estimates should be improved. Second, rather than using general proxies for institutional quality to test the relationship between supervision and bank failures (as has been the practice in recent cross-country studies), surveys of state banking departments from the 1920s and 1930s are used to construct direct measures of the quality of bank supervision and powers

country banking regulations, but their analysis on financial stability does not incorporate the effects of supervision on financial stability.

granted to supervisors. Third, data on differences in prudential regulations (capital and reserve requirements) and branching laws across states are also collected to provide an integrated framework for analyzing the effects of *both* bank supervision and regulation on financial stability. Fourth, the estimation procedure makes use of the unique bifurcated nature of the American dual banking system to control for other state-specific effects and differences in economic conditions across localities, which if left unaccounted for, would likely lead to simultaneous equations bias. Finally, matching methods (which have been used by labor economists and in medical studies, but which are novel to this literature) are used to provide an alternative estimate of the effects of regulation and supervision. I create a sample of counties that share a common border and which are assumed to share similar geographical and structural characteristics, but which are located in different states and therefore have different bank supervisory and regulatory regimes. This "matched-border" sample of counties is used to control for unobservable differences in failure rates, which if otherwise left unaccounted for, may lead to biased estimates.

The empirical findings in this paper show that differences in state regulatory and supervisory regimes help account for the variation in bank failures across U.S. counties during the Depression. In particular, counties located in states that permitted branching, enacted higher capital requirements, and gave their supervisors the authority to liquidate banks without first going through a court receivership process experienced lower failure rates from 1929-33. On the other hand, those located in states that appointed their supervisors to longer terms and which gave them unlimited authority to charter banks experienced higher failure rates.

The estimates from the "matched-border" sample of counties paint a somewhat different picture. While the length of the supervisors' term and liquidation authority granted to supervisors continue to be associated with bank failures during the Depression, regulation no longer seems to be important. Neither capital requirements nor branching laws are statistically significant in the matching methods estimation. The matching methodology in theory controls for variation in county unobservables, but further examination of the matched-border sample suggests that the Southern and Midwestern counties are underrepresented relative to the sample of all counties. Moreover, tests based on observable census characteristics suggest that the identifying assumption – that border counties are very similar – may be invalid. It is therefore likely that both regulation and supervision impacted bank failures during the Depression.

The following section provides a framework for evaluating the county-level bank failure data from the Great Depression. Section III describes the regulatory and supervisory differences that existed across states on the eve of the Depression, and explains how these differences could have affected bank failure rates. Section IV estimates the relationship between county bank failure rates, regulation and supervision, and performs sensitivity tests of the regressions. The next section considers an alternative estimator based on matching methods. Section VI offers concluding comments and relates the findings to current policy debates.

II. Modeling Bank Failure Rates during the Great Depression

The Great Depression was a period of unparalleled financial distress. Approximately 40% of all banks in existence in the U.S. in 1929 were suspended by 1933 and were then closed during the intervening period of economic hardship (table 1). As figure 1 and table 2 show, the pattern of failures across the U.S. was uneven. Some regions, states and counties were struck particularly hard experiencing very high proportions and numbers of bank failures, while others weathered the severe downturn much better. Some counties experienced a complete loss of all their commercial state banks, while others had no suspensions during the Depression. The average county failure rate for state-chartered banks from 1929-33 was 9.5 percent, and the standard deviation was 9.4 percent.[2] By census region, counties in the Midwest had the highest average failure rate over the five-year period (12.4 percent) while those in the Northeast had the lowest (4.0 percent).

Differences in business and agricultural conditions likely account for much of the variation in bank failure rates across counties. Additionally, differences across states in the supervisory and regulatory environment for state-chartered banks as of 1929 may have also contributed to an explanation of banking distress during the Great Depression.[3] For example, laws permitting statewide branching may have allowed banks to diversify their portfolios. Prudential regulations, such as capital and reserve requirements, may have prevented bank runs, provided banks with a buffer for periods of insolvency, or

[2] The mean and standard deviation of the failure rate using county data are comparable to those based on the state-level figures shown in table 1. Data on bank failures also comprise bank suspensions. For a discussion of the difference between suspensions and failures, see footnote 24.

alternatively encouraged banks to take on additional risk in their portfolios. And supervisory arrangements (such as the authority given to supervisors and their insulation from interest-group influence) may have contributed to stability in some states' banking systems but made others more fragile.

We can examine the sources of variation in failure rates in more detail by decomposing the failure rate for state-chartered banks, FR_{iq}, in county $_q$, state $_i$ into two pieces:

(1) $FR_{iq} = a_i + b_{iq}$,

where a_i is the combined effect of all covariates common to the state and b_{iq} is the orthogonal, county-specific component that combines the effect of all factors purely idiosyncratic to county q. Equation 1 implies that a properly specified estimation procedure will control for both state effects, such as differences in state-chartered bank supervision and regulation, and county-specific effects, such as differences in economic structure. Decomposing the county failure rate into county and state factors in order to isolate the effects of state supervision and regulation requires that other state-level influences, such as differences in economic conditions during the Depression, be taken into account. Including such information, however, potentially introduces simultaneous equations bias as bank failures in a particular locality may have worsened the severity of the Depression in that area. Finally, it is also possible that unobserved differences across counties are present, and could lead to biased estimates of the regulatory and supervisory

[3] Elsewhere (Mitchener 2001a), I have examined how supervisory and regulatory arrangements affected failure rates at the state level. This paper offers a different analytical approach to the problem as well as a

variables. Before tackling these estimation issues, the next section further elucidates the regulatory and supervisory differences that existed across states on the eve of the Depression.

III. The Regulatory and Supervisory Landscape

The unique vantage point that the experience of state banks during the Great Depression is the result of the variation in regulatory regimes across states. But why did a fractured supervisory and regulatory environment exist on the eve of the Depression? In short, the National Banking Acts of the 1860s gave birth to a dual banking system in the U.S. and created the possibility that banks in the same state could face different regulatory regimes depending on charter status. All national banks were under the supervisory and regulatory authority of the Office of the Comptroller of the Currency and later the Federal Reserve Board, while state-chartered financial institutions were under the jurisdiction of state banking departments. State banking departments could have adopted the framework put in place by the Office of the Comptroller of Currency, but politics intervened and states chose to adopt their own regulatory and supervisory systems in response to public concerns over financial fragility and the lobbying efforts of well-organized interest groups (Mitchener, 2001b).[4]

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new, much larger data set.

[4] Mitchener (2001b) shows that the influence of well-organized interest groups (such as unit banks, small manufacturing firms, and insurance companies), public-interest response to the bank failures, failed state deposit insurance systems of the 1920s, and competition with federal regulators help account for the differences in regulation across states that were present in 1929.

A. Prudential Regulation and Branching Laws

Several key differences in bank regulation had thus emerged across states by 1929, and these in turn may have influenced bank failure rates for state-chartered financial institutions during the Great Depression. In particular, as figures 2 and 3 show, branching laws, capital requirements, and reserve requirements differed considerably.

As of 1929, interstate branching was forbidden in the U.S., and national banks were severely restricted from opening branches.[5] On the other hand, state-chartered banks were allowed to operate branches if their state banking codes authorized it. By 1929, 25 states and the District of Columbia permitted some form of branching or did not explicitly prohibit it by law. More narrowly, 8 states and Washington, D.C. allowed statewide branching in 1929.[6] States with laws permitting branch banking may have experienced fewer bank failures because banks in these states could potentially diversify their loans and deposits over a wider geographical area or customer base, and insulate themselves from shocks particular to certain regions or sectors of the economy.[7] Moreover, just as foreign banks have brought increased competition to inefficient domestic markets (International Monetary Fund, 1998), states with branch banking may have brought

[5] The McFadden Act permitted national banks to establish local branches in the city of their home office if a state law allowed branching, but the number of branches was also restricted. They could open no new branches in cities with fewer than 25,000 people, only two branches in cities with populations between 50,000 and 100,000, and at the discretion of the Office of the Comptroller of the Currency for cities of over 100,000 (Tippetts, 1929).

[6] Data on branching laws are from Federal Reserve *Bulletin* (February, 1929 and December 1929).

[7] Studies by Wacht (1968) and Lauch and Murphy (1970) have found reduced variance in deposit flows for branch banks as a result of greater diversification.

increased competition in local markets and therefore improved the stability of the banking system by forcing weaker banks to merge or exit.[8]

Prudential regulations also differed across states. Prior to the enactment of federal deposit insurance in the U.S., capital was the main form of protection for depositors and minimum capital requirements (the amount of capital required to receive a bank charter) were the main tool used by state banking departments to induce banks to hold more capital.[9] Capital requirements can cushion depositors against banks' losses due to normal business operations; the larger the contributions of the owners of the bank, the greater can be the shrinkage of assets without impairing depositors' claims. Since charter capital represents the contribution from owners, high capital requirements can also serve to chasten owners (and their managers) from taking on excessively risky loan portfolios, thus reining in moral hazard.[10] Finally, capital requirements can affect entry into banking. Many states had set very low capital requirements prior to the Depression, and some contemporaries argued that these had encouraged the formation of too many small unit

[8] Wicker (1980) argues that Caldwell and Company, which had over 100 branches in the South, was a primary force behind the bank failures of 1930. Carlson (2001) finds that branched banks in three states had a lower probability of surviving the Great Depression than those without branches. The Study Commission for Indiana Financial Institutions (1932, p.130) examined the causes of banking distress in Indiana while the Depression was taking place and concluded that branching was not important in preventing bank failures. In contrast, Wheelock's (1992) study of the 1920 Kansas banking system suggests that branching might have reduced failures in that state by spreading agricultural distress across a broader area. And using data from 1946-75, Rose and Scott (1978) found that bank failures were concentrated in states limiting or prohibiting branch banking, principally in the Midwest and South.

[9] During this period, banks held significantly higher capital ratios than today. It was common for banks in the 1920s to prominently display signs in their front windows stating "Capital and Surplus $XX million," in order to signal to depositors that they were financially sound. After enactment of federal deposit insurance, these signs were replaced by "Member FDIC or FSLIC." See Benston and Kaufman (1994).

[10] On the other hand, Blum (1999) has argued that if raising equity is prohibitively costly and banks face higher capital requirements tomorrow, then they will rationally respond by increasing risk today.

banks and "excess competition."[11] For these three reasons, states with higher capital requirements may therefore have experienced fewer failures.[12]

National banks also faced capital requirements, which were enforced by the Comptroller of Currency; but as figure 3 shows, many states did not adopt the same standards as the Comptroller's office. In 1929, 21 states had capital requirements lower than the national bank requirement of $25,000 that applied to institutions located in cities of fewer than 3,000 people.[13]

Reserve requirements are a second prudential regulation that may have also affected financial stability. They operate as a tax on banks in the hope of averting liquidity crises resulting from fractional reserve banking. Without reserve requirements, banks may be prone to holding too few reserves since they are driven to compete over depositors to increase the returns on assets. Such behavior can increase systemic risk, suggesting that a socially superior outcome is possible through regulation (Cothren and Waud, 1994). On the other hand, reserve requirements can potentially reduce monitoring incentives and result in lower quality bank portfolios (Besanko and Kantas, 1993; Boot and Greenbaum, 1993). Further, since they act as a tax on bank liabilities, reserve requirements can induce banks to take on more portfolio risk to offset their reduced profits. As of 1929, all states

[11] As George Dowrie, a professor of Finance in the Graduate School of Business at Stanford University explained, "Since one needs only a small amount of capital to start a bank, a group of farmers and townspeople can much more easily organize an enterprise of their own than be dependent on some existing institution. In most states they are entitled to a charter so long as they comply with simple formalities." (George Dowrie (1930) as quoted in Study Commission for Indiana Financial Institutions, 1932, p.91).
[12] According to Federal Reserve Board Reports, the majority of the banks that failed in 1930 and 1931 had little capital: 35% of the failures had a capital stock less than $25,000 and 83% had a capital stock less than $100,000. Calculations based on Study Commission for Indiana Financial Institutions (1932, p.59).
[13] Data on capital requirements are from Polk's Bankers Encyclopedia Co. (1929).

had instituted reserve requirements for state-chartered banks, and many had rates higher than the required rate of 7% for Fed members or national banks (figure 3).[14]

B. Supervision

In its "Core Principles for Effective Banking Supervision," the Basle Committee has drawn attention to reducing systemic risk by overhauling supervisory structures and implementing "best practices" across national borders. Disparate supervisory systems also existed across U.S. states on the eve of the Great Depression. The supervisory authority and political autonomy granted to state banking departments differed considerably, and as a consequence, some states' regulators operated with more discretion than others. On the one hand, this discretion may have enabled supervisors to make informed decisions concerning the integrity and competence of bank managers. On the other, such discretion may have opened the door to policymaking that was not impartial and objective.[15] Moreover, unlike national banks (which had a uniform system of bank examinations and reports and, which under the direction of the Office of the Comptroller of Currency, had improved the handling of the liquidation of failed banks), the quality of supervision across state banking departments also varied. The quality of supervision, the political autonomy, and the powers given to state banking departments may have in turn influenced stability of state banking systems; differences in these features of state supervisory systems are now briefly described.

[14] At 20%, Florida, Louisiana, Wyoming, Texas, and North Dakota had the highest reserve requirements on demand deposits. Iowa, Kentucky, and South Carolina had the lowest rates at 7%. Data on reserve requirements are from the Federal Reserve *Bulletin* (November 1928, September 1930).

In the 1920s and 1930s, many state bank commissioners were particularly concerned that their ability to monitor banks or make chartering decisions was compromised by a lack of insulation from the governors that appointed them (44 of the 48 state banking superintendents were appointed by the state's governor).[16] Lengthening the appointment of the supervisor beyond the election cycle of the governor might effectively shield a superintendent from the political motives of the state's governor (such as reconsidering licensing or receivership decisions), but it could also induce bankers and their shareholders to attempt to increase their influence over the decisions of superintendents, such as the granting of charters or the timing of bank examinations. The term length of state bank supervisors may have affected decisions about chartering, receivership, and the timing of bank examinations, and hence the stability of state banking systems.

States also differed in terms of the quality of supervision with some devoting more resources towards bank examination. If the ratio of examiners to banks in a state were small, it may have been difficult for the state supervisory agency to identify non-performing banks and intervene before banks became insolvent. Even the most competent examination staff would have a limited capacity to monitor bank behavior effectively, so differences in the number of examiners per bank across states may account for some of the variation in failure rates as well.

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[15] As the IMF (1998) has emphasized, reconfiguring the powers, policies, and structure of supervisory agencies is complicated by the tradeoffs between rules (which are transparent) and discretion (which depends on the integrity and credibility of the supervisory agency).

In addition to the quality of supervision and the political autonomy of the state-banking department, supervisory authorities differed in their authority and powers, which in turn could have limited their effectiveness.[17] The authority of the state bank supervisor to expedite the resolution of failed banks is one such power that varied across state banking departments and could have influenced bank failure rates during the Depression. States which granted their supervisors the authority to liquidate banks without having a court appoint a receiver may have limited contagion and credit-channel effects and reduced the incentives for banks to gamble for resurrection.[18] A second power that varied across states was whether the authority to deny or grant charters was placed solely in the hands of the state bank supervisor.[19] On the one hand, many state bank supervisors in the 1920s argued that lack of licensing authority weakened their banking systems because they could not restrict charters, and therefore resulted in higher failure rates.[20] On the other, concentrating chartering decisions in the hands of a single individual may have increased

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[16] In the four other states, they were either appointed by the state bank board or a commission of corporations, or elected. Two of the 44 are first selected by banks (American Bankers Association, 1929) and then appointed by the governor.

[17] The International Monetary Fund (1998, p.43)) has argued that "Ideally, the supervisory authority is endowed by law with a clear mandate and powers to carry out its function. The law defines the scope of authority of the supervisor and confers authority to license and to withdraw licenses of financial institutions, approve new owners, issue prudential regulations, obtain periodic prudential reports, conduct on-site inspections, take corrective actions (including the imposition of restrictions on a bank's business activities), and close and liquidate banks."

[18] The IMF (1998) and Mason, Anari, and Kolari (1999) present evidence consistent with the contagion and credit-channel stories, respectively; Fry (1995) argues that troubled banks in developing countries show evidence of taking on excessive risk in the time it takes for a receiver to be appointed.

[19] In contrast to the situation for state banks, the Comptroller of Currency's office had established a system that permitted a great degree of control over the granting of new charters for national banks. Its office reviewed charter applications and based chartering decisions on economic surveys of the community where the proposed bank was to be set up. These surveys included an examination of the existing banks in the service area, the local population of businesses and depositors, and regional economic conditions. Rules formulated to limit chartering were codified as early as 1909 in the Comptroller's *Instructions Relative to the Organization and Powers of National Banks*.

[20] The state bank supervisor of Maryland stated in 1933, "A number of failures might have been prevented by greater care in granting new charters." Likewise, the Virginia Superintendent wrote, "The damage which existed began more than five years ago. We were over-banked but had no authority to prevent the organization of new banks until six years ago." (American Bankers Association, 1934).

the incentives for banking industry lobbyists to attempt to "capture" regulators; this may have had counterproductive consequences for financial stability if it resulted in the granting of charters for personal or political gain rather than for economic reasons (Laffont and Tirole, 1991; Mortimont, 1999).

IV. Accounting for Differences in Bank Failure Rates across U.S. Counties

A. Empirical Issues

Perhaps the biggest hurdle standing in the way of identifying the relationship between bank failures, regulation, and supervision has been obtaining reliable data from periods when there was sufficient distress in banking systems. To meet this challenge, this paper draws on a previously underutilized data set of bank failures occurring in U.S. counties during the Great Depression.[21] The county data not only provide a larger sample relative to cross-country or state-based studies for conducting empirical tests, but they also closely approximate the competitive environment in which banks in the U.S. (especially rural ones) operated in the 1920s and 1930s.[22] The data are derived from information

[21] Previous research on bank regulation during the Great Depression has used either aggregate state-level data on all commercial banks or individual bank data from a few states. (Supervision has not been examined). Wheelock (1995) and Gambs (1977) use Federal Reserve data on all commercial bank failures at the state level to analyze the effects of bank regulation; however, they use a series which contains both national and state banks even though national and state banks had *different* regulatory regimes and significantly different failure rates during the Depression (As table 1 shows, state banks had an average failure rate that was approximately 4 percentage points higher than national banks). Carlson (2001) and Ramirez (2000) use limited, individual bank balance sheet data to examine the effects of branching laws, but many state banking departments did not systematically gather this sort of information, and those that did collect it only did so infrequently (every one or two years). Their results are thus based on a limited sample of only a few states using data at low frequencies, making it difficult to pinpoint the factors that contributed to individual bank failures.

[22] There were approximately 25,000 banks operating in the U.S. at the end of the 1920s, 80 percent of which were located in towns of less than 10,000 people and 43 percent of which had a capital stock of

collected by the Federal Deposit Insurance Corporation and compiled in computer format by the Inter-university Consortium for Political Science Research (FDIC, 1960). The computer files consist of county-level observations of bank suspensions, deposits in suspended banks, and figures for total number of banks and total bank deposits for 47 states on a yearly basis.[23] In addition, the data on commercial banks are further disaggregated by charter status, permitting an analysis of the relationship between state-chartered financial institutions and regulations that varied for these institutions across state borders. My analysis focuses on bank suspensions (henceforth called bank failures) as these data contained fewer coding errors than the bank deposit data.[24]

I regress the average (1929-33) county failure rate for state-chartered financial institutions on the state bank regulations and supervisory practices that were described in section III. In contrast to recent cross-country studies that rely on characteristics of a country's institutional environment to proxy for supervision (e.g., Demirguc-Kunt and Detragiache (1999)), survey data from the American Bankers Association (1929) on the

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$25,000 or less (National Industrial Conference Board, 1932, pp.13-14). Given the fact that the automobile had only gained more widespread use in the 1920s, most depositors did not travel beyond the largest town in their area to conduct banking. In rural areas, this range likely encompassed counties as well.

[23] There are no data on Wyoming, Alaska, or Hawaii.

[24] To be sure, there are differences between suspended and closed banks. This paper follows the Federal Reserve's convention: state bank suspensions comprise "all banks closed to the public, either temporarily or permanently by supervisory authorities or by the banks' boards of directors on account of financial difficulties, whether on a so-called moratorium basis or otherwise, unless the closing was under a special bank holiday declared by civil authorities. If a bank closed under a special holiday declared by civil authorities and remained closed only during such holiday or part thereof, it has not been counted as a bank suspension" (Federal Reserve *Bulletin*, September 1937, p.866). Banks which become insolvent and are placed under receivership or which liquidate their assets voluntary might properly be termed failures, and are a subset of suspensions. The county data do not permit one to distinguish between failures, temporary suspensions, and bank mergers. While the data used here potentially overstate the true number of failures for state-chartered banks, unless there is some systematic reason to believe that there are important regional differences between failures and suspensions, the hypotheses tested should not be adversely affected if suspended banks are included. In their study of contagion effects during the Depression, Calomiris and Mason (2000, p.30) point out that this distinction in theory can be important, but they do not find that their results for the 1930-32 sample period are sensitive to the choice of failure data versus suspension data.

authority and quality of state banking departments permit the construction of *direct* measures. Also in contrast to previous studies, data on state bank regulations can be combined with the survey data on state baking departments to permit an integrated analysis of the effects of supervision *and* regulation on financial stability. The identifying assumption is that the regulatory and supervisory aspects of banking are uncorrelated with the random element of bank failures.

The dependent variable is the average failure rate from 1929-33 for state-chartered banks located in county q, state i.[25] The number of bank suspensions in a given year is divided by the number of banks in existence on December 31st of the previous year. Individual years are then averaged over the 1929-33 period.[26] The regulatory and supervisory variables are for 1929 and are treated as time invariant over the sample period in order to identify the impact of regulations that were in place at the start of the Depression.[27] Indicator variables for whether the state bank supervisor had the sole authority to license or charter state banks and whether the supervisor could liquidate failing banks without first having a court appoint a receiver are included. The number of banks per examiner and the length of the supervisor's term (in years) are used as measures for the quality of state supervision and political insulation, respectively. Minimum capital requirements for

[25] Averaged failure rates over the five-year period were used to reduce the censored data problem. Even so, 29 percent or 779 observations (out of 2679 with failure rate data on state banks) have a value of 0 when the data are averaged from 1929-33. Because the failure rate can never be negative, a tobit censored regression model is presented in Appendix table 1. The non-negativity constraint was non-binding for a majority of counties; thus when the regressions are compared to ones with an equivalent specification, as shown in table 5, the results are quite similar.

[26] An alternative way of computing the failure rate is to specify it as the sum of failures from 1929-33 divided by the number of banks in existence on December 31, 1928. Regressions using this specification produced similar results as those presented here.

[27] Changes in regulatory and supervisory regimes were relatively infrequent until the second half of 1933, when the banking crisis had subsided.

state-chartered banks in small municipalities are measured in thousands of dollars, reserve requirements are expressed as a percentage of country-bank demand deposits, and a branch banking dummy variable takes on a value of 1 in states where branching was prohibited in 1929.

As suggested by equation 1, county-specific covariates, b_{iq}, are also included in order to disentangle the effects of regulatory and supervisory variables from local factors that may have influenced failure rates. In particular, the percentage of a county's population that is rural and farm-based and the percentage of population that is employed in each of three different sectors (manufacturing, retail, and wholesale occupations) are computed using U.S. census files on counties (U.S. Department of Commerce, Bureau of the Census, 1920, 1930). The percentage change in the average value of farms between 1920 and 1930 is also calculated to account for the fact that bank failures in the 1930s may have been a continuation of ongoing rural banking distress that started in the 1920s – a question that has been widely debated by economic historians.[28] Since declining farm values are associated with declining land prices, falling farm incomes, and agricultural bank failures, they are a reliable proxy for local agricultural distress (Kliesen and Gilbert, 1996).

B. Estimating Cross-Sectional Differences

Table 3 provides evidence that supervisory and regulatory differences across states affected financial stability during the Great Depression. Column 1 presents OLS

regression estimates with robust standard errors based on 2,211 county-level observations.[29] Counties located in states where supervisors were appointed to longer terms experienced higher failure rates: an additional year in office raised county failure rates from 1929-33 by approximately 1 percentage point (The range of term lengths varied from 2 to 6 years). This result is consistent with the regulatory capture literature (Laffont and Tirole, 1991; Martimont, 1999). That is, while longer terms may have provided better insulation from any political influence that the governors who appointed them may have exerted, they also increased the incentives for lobbyists to expend more resources to influence supervisory decisions, capture the regulator, and exploit the superintendent's discretionary power. Supervisors with longer terms, seeking to maximize their own utility or post-supervisory-employment opportunities, appear to have responded by acting with less independence and more discretion in timing on-site examinations, granting forbearance, and issuing charters or banking licenses – all of which potentially undermined financial stability and raised bank failure rates. Consistent with the statistical evidence, several bank superintendents commented on the importance of integrity in the surveys conducted by the American Bankers Association. On the eve of the crisis, the Massachusetts banking superintendent remarked, "Statutes of this state give commissioner ample authority to supervise banks. Quality of supervision depends on the character of the incumbent of the office of the commissioner."[30]

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[28] See, for example, Temin (1976) and White (1984).

[29] There are 3,051 counties in the full sample of 47 states; 372 do not report data on state bank failures. In three states (Arizona, Nevada, and New Mexico), the percentage of counties with missing values for failure rates is greater than 40 percent. When all the observations from these states are dropped from the sample, the regression results for the remaining counties are very similar to those reported here. The other counties not included are due to missing values in one of the independent variables.

[30] American Bankers Association (1929). A similar concern was expressed in 1934 by the head of bank supervision from Pennsylvania in the wake of the banking crisis: "Honest, experienced department heads,

Counties located in states that granted supervisors the sole authority to issue charters also experienced failure rates that were on average 2.1 percentage points higher, a result that is both economically and statistically significant (*p* value = 0). The positive sign on this coefficient is consistent and complementary with the positive sign on the supervisor's term length and stands in contrast to a public interest view of regulation, which would emphasize that supervisors with full discretionary authority would limit unneeded or economically infeasible banks. Instead, the result instead suggests that supervisors with full discretionary authority over licensing decisions destabilized the state banking system by granting charters to banks whose applications had been rejected by the Office of the Comptroller of the Currency or to cronies in the banking industry who would not have otherwise had their applications approved, but who could focus their lobbying on persuading the single individual with authority over chartering decisions. It is difficult to find evidence of regulators admitting to "being on the take" – issuing charters in exchange for personal benefit – but some former bank superintendents have indeed admitted that their departments were corruptible to this sort of industry influence. As the state bank supervisor Linwood Neal of Oklahoma described, superintendents of banking systems working in the 1920s could be influenced by industry lobbyists. "For a period of about 3 years from 1919 until 1922 graft and corruption played a dominant role in the operation of the State Banking Department. Charters were to be had at a price."[31]

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provided with means to thoroughly examine and supervise, can do more to improve the state banking system throughout the country than more restrictive banking laws." (American Bankers Association, 1934).

[31] Neal (1942).

In contrast to chartering authority, granting bank supervisors the sole authority to liquidate banks without first having a court appoint a receiver *reduced* county failure rates by approximately 5 percentage points. There are several explanations for this negative association. Providing supervisors with the authority to liquidate banks resulted in a speedier liquidation process than using the court system and potentially limited contagion effects from failing institutions to the rest of the system (IMF, 1998). As the Colorado state superintendent wrote in 1929, "The administration of banks in liquidation may be handled at less expense and more diligently wherein it is not necessary to constantly refer matters to the courts, which in many instances are extremely slow in the determination of important matters."[32] It also allowed depositors to realize partial claims more quickly and limits credit channel effects that operate through the assets tied up in failed banking institutions.[33] As the banking supervisor from the state of Mississippi commented in 1929, "The bank commissioner having authority to appoint liquidating agents in failed banks and being responsible for the liquidation of these banks, has materially improved the efficiency of the department in this respect, thereby enabling the liquidation of banks to function in a businesslike way; enabling the commissioner to realize more from the assets of failed banks than would ordinarily be possible."[34] Finally, speeding up bank liquidation potentially reduced the incentives for managers to engage in

[32] American Bankers Association (1929).
[33] As Mason, Anari, and Kolari (1999) show, when banks failed and were not resolved during the Great Depression, liquid deposits were involuntarily transformed into illiquid securities; this slowed down the ability to rehabilitate insolvent debtors or liquidate their collateral, and affected bank depositors' consumption decisions and the banking sector's ability to make loans.
[34] American Bankers Association (1929). Similarly, the head of bank supervision in Montana commented that "less interference should be had from the courts, however, where liquidation is carried on by the banking departments." (American Bankers Association, 1929).

perverse behavior, such as gambling for resurrection before a receiver is appointed.[35] Moreover, supervisors given the power to liquidate banks quickly did not have a clear incentive to delay the proceedings on behalf of the failing institution since many of the claimants were other banks that it supervised.

Differences in state bank regulations also contribute to the variation in failure rates across counties during the Great Depression. As table 3 shows, failure rates were around 2 percentage points higher in counties located in states that prohibited branching. Prohibitions on branching limited the ability for banks to diversify their loan portfolios and depositor base. As many firms located in the same area became distressed during the Depression, the credit quality of banks located in counties where branching was illegal likely suffered more than in counties where branching enabled banks to disperse credit across economic markets. Moreover, liquidity risk was higher in geographically concentrated banks because these banks relied on deposits from less economically diverse entities. On the other hand, in counties where branching was permitted, banks could attract deposits from a broader base of individual and business customers, and therefore realized reductions in the variance in deposits and withdrawals – those factors which can lead to bank runs.[36] By limiting competition from banks with home operations located in other counties, restrictions on branching also may have allowed more inefficient banks to persist until 1929; so that when the Depression hit, failure rates were higher in these areas.

[35] In Chile, for example, managers of troubled banks began engaging in Ponzi-type lending schemes (Fry, 1995, p.458).

The results in table 3 also show a negative and statistically significant relationship between capital requirements and bank failure rates. A $10,000 increase in the minimum charter capital required to receive a bank license reduced county failure rates for state-chartered financial institutions by 0.8 percentage points from 1929-33.[37] The coefficient reported in table 3 is consistent with the theory that capital requirements stabilize banking systems by reducing moral hazard (by putting more of the owners' capital at stake), serving as a buffer to cushion banks in the short term, and reducing competition in local markets (allowing banks to earn monopoly profits).

County characteristics were included to control for observable differences across counties (b_{iq} in equation 1) so that more precise estimates of the regulatory and supervisory variables could be obtained. Several of these covariates are also statistically significant in the regressions. As might be predicted, those counties with a more diversified economic base had lower failure rates; banks in these counties would have typically been able to hold more diversified portfolios of assets and had a depositor base that was less homogeneous. The percentage of a county's population engaged in manufacturing is negatively associated with county bank failure rates (with a point estimate of –0.12) and is statistically significant with a *p* value of .001 (not reported in table 2). Banks in counties that had a mix of both industry and agriculture were thus more likely capable of coping with idiosyncratic shocks.[38]

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[36] See Liang and Rhoades (1988) for a discussion of this point.
[37] A one standard deviation change in charter capital requirements is $16,300.
[38] One covariate that was not statistically significant was the rural, farm population of a county – a result that is perhaps surprising since literature on bank failures during the Depression has emphasized that they were overwhelmingly rural and agricultural (Friedman and Schwartz, 1963; Temin, 1976; Stauffer, 1981; Thies and Gerlowski, 1983). This was true even when an alternative measure (the number of farms per 1,000 people in a county) is used (Column 2, Table 3).

The negative and statistically significant coefficient on the percentage change in farm land values suggests that those counties that had larger percentage declines in farm land values between 1920 and 1930 experienced higher bank failure rates during the Great Depression. This is contrary to Temin (1976), but consistent with Stauffer (1981) and White (1984), who emphasize that banks in the 1920s, especially small ones, had slow adjustment processes and were left vulnerable to a further decline in asset values or shocks to the money supply.

C. Specification Tests

While the regressions in the first two columns of table 3 control for variation in local economic structure at the county level, these are predetermined variables which do not take into account differences in economic conditions during the Great Depression. Omitted variables bias is potentially a problem if contemporaneous economic conditions are not also included in the analysis.[39] This suggests including a broad-based measure such as changes in state personal income as an additional regressor (as shown in column 3 of table 3).[40] But including contemporaneous measures such as personal income likely introduces simultaneous equations bias (economic conditions can influence bank failures

[39] Finding data to address the regional differences in economic conditions has been a significant impediment to research on the feedback effects during the Depression. Broad-based measures of economic decline during the 1930s are difficult to assemble. They are nonexistent at the county level and problematic at the state level. Estimates of state GDP are constructed starting in 1929, and since these figures were constructed ex post, they may suffer from significant measurement error. Alternative series such as state unemployment statistics are not ideal. They typically lag the business cycle, are generally estimated based on only a few observations per state, or they omit coverage of rural areas (which during the 1920s and 1930s were areas that suffered the largest number of bank failures and severe economic distress).

and vice versa) – an econometric issue that has proved vexing in previous studies.[41] It is possible to find instruments for bank failures in the income equation (e.g., the regulatory and supervisory variables), but it is difficult to find measures using historical data that are correlated with changes in state income but uncorrelated with the error term in the bank failure equation. Without these, the system of equations would not be identified.

I exploit the unique bifurcated nature of regulation and supervision in the U.S. dual banking system as an alternative way of dealing with simultaneity, and include the failure rate for national banks at the *state* level as a regressor in the table 4. While this does not entirely solve the simultaneity problem, it does yield downwardly- biased estimates of the impact of regulation and supervision on bank failures, since national banks were solely regulated and supervised by the Office of the Comptroller of the Currency rather than state banking departments. Moreover, it conditions the state-chartered bank failure rate on a wider set of potential influences than a single measure such as personal income.[42] In the framework of equation 1, including the national bank failure rate at the state level controls for differences in a_i which are not attributable to supervisory and regulatory policy. Since state and national banks have different regulatory agencies overseeing

[40] The coefficient is negative and statistically significant at the 1-percent level, and the signs and significance of the regulatory and supervisory variables are similar to those reported in the previous columns. Data are from U.S. Department of Commerce, Bureau of Economic Analysis (1995).

[41] For example, Wheelock (1995, p.30) suggests that his results on state bank regulation (he does not consider supervision) do not account for the feedback between the severity of the Great Depression in a state and bank failures in that state, and therefore likely suffer from simultaneous equations bias. In his analysis, he controls for differences in the severity of the Depression by including either state-level changes in farm land prices and industrial production figures or the decline in a state's personal income, but he does not allow bank failures to in turn affect these macroeconomic indicators.

[42] I condition on state economic conditions rather than those at the county level because county-level economic information such as county unemployment statistics, employment growth, and changes in personal income have been shown to be poor predictors of banking conditions such as profitability, whereas *state* business cycle variables have been shown to be reliable predictors of bank profitability (Meyer and Yeager, 2001 and Neely and Wheelock, 1997).

them, and a county's failure rate contributes to only a portion of a state's average failure rate, simultaneity bias is likely to be quite limited.[43]

As might be expected, the coefficient on the national bank failure rate is strongly statistically significant and large in magnitude: a one percentage point increase in the national bank failure rate at the state level raises the county failure rate by 82 basis points. State-chartered bank failure rates were thus significantly influenced by economic conditions in the state or those factors that also affected national banks (The R-squared also improves from around 0.12 to 0.21.) What is impressive about the results is that most of the regulatory and supervisory variables are still statistically significant despite the fact that the national bank failure rate absorbs some of the identifying variation across counties and potentially imparts a small downward bias on these coefficients. The estimated coefficients for liquidation authority, supervisory term length, capital requirements, and branch banking laws are statistically significant (although some are smaller in size). However, the coefficient on the dummy variable for supervisors having the sole authority to charter banks is no longer statistically significant.

On the other hand, the coefficient on reserve requirements is statistically significant at the 2 percent level. The positive sign on reserve requirements is consistent with the hypothesis that, *ceteris paribus*, higher reserve requirements can induce banks to increase the share of risky assets (relative to riskless ones) in order to increase return, which could potentially increase the probability of failure. It may be the case that this perverse effect

[43] The coefficients on regulation and supervision will be biased to the extent that the county failure rate for state-chartered financial institutions affects the overall failure rate for *national* banks at the *state* level.

of reserve requirements is driven by nonlinearities: that only in states where the reserve requirements were set very high do banks begin to switch their portfolios to hold riskier assts. To test for this, column 2 includes both a linear and quadratic term on reserve requirements. Once nonlinearities are taken into account, neither of the reserve requirement coefficients is statistically significant.

Table 4 also reports on results for alternative sample periods to further test the robustness of these initial findings. Some scholars define the start of Depression-era banking distress as beginning in 1930 and view 1929 as a continuation of the banking distress of the 1920s. The statistically significant coefficient on the percentage change in farm land values between 1920 and 1930 provides some evidence that the starting date may not be driving the results. Nevertheless, to test whether this is the case, the dependent variable in column 3 of table 4 is redefined to be the average failure rate for 1930-33. The coefficient estimates on the regulatory and supervisory variables do not appear to be sensitive to excluding 1929 and closely resemble those presented in the other columns of the table.

Another concern is that the failures of 1933 may be considered anomalous because of the federal banking holiday declared by President Roosevelt in March of that year. The cross-sectional regression results where the dependent variable is defined as the average county failure rate from 1929-32 are shown in the last column of table 4. The estimated coefficients on regulation and supervision are again quite similar to the full-sample results, although the coefficient on capital requirements is only weakly significant (both economically and statistically).

D. Branch Banking Laws

Branch banking has attracted considerable attention as a way to reduce risk in banking systems, and the estimated coefficients shown in tables 3 and 4 are consistent with this notion. Table 5 presents some additional evidence on the robustness of the branch banking result. The first column tests whether those states without laws explicitly permitting or prohibiting branching are driving the results on this variable. The branching indicator variable is redefined so that it has a value of one only in states permitting either statewide or more limited city or county branching; it is zero if a state has no law or forbids branch banking.[44] As column 1 shows, the branch-banking coefficient is negative and statistically significant even when the variable is specified in this way. Counties in states with laws allowing banks to branch (at the city level, county level, or statewide) had failure rates that were on average 2.3 percentage points lower. Column 2 further restricts the definition of branch banking to only those states that permitted *intrastate* branching. The size on the statewide branching coefficient is approximately the same as the coefficient for all types of branching shown in column 1, which suggests that at the county level, there were no additional benefits to *statewide* branching. This finding is not entirely surprising if the benefits from statewide branching result from the consolidation and pruning of weak banks across the entire state. If this were the case, then the benefits of statewide branching laws might be easier to detect using state data since the benefits likely operate across county borders. The results are nevertheless still consistent with the

[44] Six states had no law explicitly forbidding branching: New Hampshire, Vermont, North Dakota, South Dakota, Wyoming, and Oklahoma.

interpretation that states permitting branching had fewer failures during the Great Depression. Finally, column 3 shows that states with more branch-banking activity appear to have fared better. If the extent of branching relative to total bank activity within a state is considered, counties located in states with a higher fraction of branched banks relative to all banks had lower failure rates. A 10-percentage-point increase in state-chartered banks with branches resulted in a failure rate that was almost 1 percentage point lower for a county in that state.

V. Analyzing Bank Failures in Border Counties

A. Matching Methodology

There are strong reasons to believe that location accounts for a large part of the variation in failure rates across counties: bank failures may be linked to the types of enterprises banks finance. For example, Temin (1976) has emphasized that the decline in cotton prices may have strongly influenced the health of rural banks throughout the South, while Wicker (1996) stresses that the regional clustering of bank failures in the 1930s might have been due to local conditions. Moreover, as the large coefficient on national bank failure rates suggests, factors common to both national banks and state banks explain some of the differences in county failure rates.

Equation 1 decomposed a county's state-chartered bank failure rate, FR_{iq}, into two components: state effects, a_i, and county effects, b_{iq}. In the last section, covariates based

on census data were used to account for observable differences across counties. It is possible, however, that unobserved differences across counties remain and are leading to biased estimates of the regulatory and supervisory variables. This section pursues an alternative empirical strategy to account for unobserved heterogeneity. The sample is redefined to consist entirely of border counties. That is, failure rates are only compared for adjacent counties that lie in two different states, but share a common border. So, for example, counties on the perimeter of Oregon are paired with adjacent counties in Washington, Idaho, and California.

The basic strategy in matching counties is to mimic a controlled randomized experiment. Matching techniques (using the propensity score methodology) have been used extensively in the medical and biometric literature (Rosenbaum and Rubin; 1983, 1984), and have been applied more recently by labor economists to study the impact of training programs (Heckman, Ishimura, and Todd, 1997). More closely related to the investigation of spatial differences in policy, Card and Krueger (1994) examine the impact of minimum wage laws on employment by comparing fast food restaurants in Pennsylvania (where minimum wage laws remained constant) and New Jersey (where minimum wage was increased in 1991-2). In a similar vein, Holcombe and Lacombe (2000) examine the effects of state income taxation on economic growth by comparing income growth in border counties of the contiguous 48 U.S. states. Also using county-level data, Holmes (1998) examines the relationship between state policies and the location of industry.[45]

[45] He finds that there is a large, discrete increase in manufacturing activity when one crosses over a border from a state that does not have right-to-work laws (which he calls "pro-business") to one that does.

The matching strategy employed here implicitly assumes a counterfactual failure rate. In principle, the *q*th county of state *i* has a failure rate that corresponds to the (regulatory and supervisory) treatment it actually received in state *i* and another corresponding to a hypothetically different treatment were it located in state *j* (i.e., given b_{iq}, a_i is the failure rate it has in state *i* and a_j is the failure rate it would have if it were in state *j*). Since we can only ever observe one response for a county (i.e., it only receives one treatment), in essence, there is a missing data problem. We surmount this missing data problem by assuming that comparing border counties is equivalent to comparing the observed failure rate for county *q* in state *i* with its hypothetical failure rate if it were in border state *j*.

This identifying assumption implies that for border counties q and r,

(2) $\sigma_{iq} = \sigma_{jr}$,

where the border strategy controls for σ, the unobservable component of county-level variation. So after controlling for σ by matching methods and conditioning on county observables, counties will differ due to treatment (where the treatment in this case is the state banking regulations and supervisory practices that were in place in 1929) or other state effects (i.e., $a_{iq} \neq a_{jr}$). The border-matching procedure thus controls for differences in failure rates due to local factors such as climate or weather, the structure of the economy (how diversified its economic base is), the severity of the depression (foreclosures on real estate, falling agricultural prices, business insolvencies), the competitive nature of the banking industry, and differences in depositor base. If the

identifying assumption is valid, any remaining variation is likely attributable to systematic differences across states which were not accounted for by inclusion of the national bank failure rate computed at the state level: policies such as branching laws, reserve requirements, capital requirements, and supervisory arrangements.

B. Results

Using a map of U.S. counties from 1929, a set of county "matched pairs" was constructed. A matched pair is defined as two counties sharing a common border across a state line. If a county shares its border with multiple counties in another state(s), then each county it shares a border with is coded with a unique identifier. It is therefore possible for a county to appear multiple times in the sample, but each time it appears, it will be with a different corresponding border county.

Table 6 presents results from fixed effects (within estimator) regressions on 1747 observations, where the fixed effect is the unique pair identifier. The results for the supervisory variables are similar to what was reported for the full, unmatched sample in earlier tables. The negative relationship between bank failure rates and liquidation authority, and the positive relationship between bank failure rates and the supervisors' length of term are statistically significant. The coefficient on chartering authority is statistically significant – even when the failure rate for national banks calculated at the state level is included (column 3) – and its effect is quite large.

What is different from the results presented earlier is that none of the regulatory variables is statistically significant across all the specifications in table 6. The coefficient on the reserve requirement is statistically significant, but it enters with the opposite sign from the unmatched, full-sample result shown in table 4. Regardless of specification, neither capital requirements nor the dummy variable for states prohibiting branch banking is statistically significant.[46]

C. Sample Bias?

The lack of statistical significance on the bank regulation coefficients suggests that regulation is unimportant once controls for county unobservables using matching techniques are included (as consistent with the theory presented above), that the underlying sample of matched counties is different from the sample of all counties, or both. To test for the second possibility, I split the original sample of all counties into perimeter and interior counties and estimate simple cross-sectional regressions for each part of the full sample.[47] In this case, the regression for perimeter counties does not control for county fixed effects; this in turn permits a direct comparison with the full-sample and matched-county regressions. As the first column of table 7 shows, the results from the regression on perimeter counties, although not identical, are roughly similar to those for the fixed-effects (i.e. matched pair) regressions: the supervisory variables are statistically significant (although chartering authority is only weakly significant) while the regulatory variables are either insignificant or weakly significant in the case of

[46] Changing the specification of the branching variable did not alter this finding.

branching.[48] In contrast, the coefficients on the regulatory and supervisory variables for interior counties shown in column 2 exhibit signs and levels of significance that are similar to those for the full sample of counties without fixed effects (table 3).[49] Thus what may partially explain the decline in statistical significance of the regulatory variables in the border county analysis is not that the matching technique controls for unobservable county characteristics, but rather that the border counties themselves differ from other counties.[50]

One way that border counties could differ is in terms of their geographical distribution. Table 8 examines the distribution of the sample in regressions where the national bank failure rate is computed at the state level. Relative to the full (non-fixed effects) sample, the perimeter county and matched county samples underrepresent the high failure regions (the Midwest and South), and oversample the lower failure rate region of the Northeast, while the interior county sample does the opposite. If differences in regulation were particularly important in explaining the variation in failure rates in Southern and Midwestern counties, and these are undersampled in the matched and perimeter county samples, then the compositional differences in terms of geography may help account for the reason why the estimated effects on the regulatory variables are statistically insignificant.[51]

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[47] Recall that the matched sample includes multiple entries for some counties as many border counties share a border with more than one other county. In these regressions, repeat entries are eliminated.
[48] The smaller sample sizes of 813 and 576 for perimeter counties may also partly explain why regulatory variables are insignificant.
[49] Rhode Island and Delaware have no interior counties.
[50] It is also possible that there is reduced identifying variation in this more limited sample relative to the full sample of counties.
[51] Another hypothesis (beyond the scope of this paper) is that banks in perimeter counties are affected by the way banks operate on the other side of the state border, and that this effect is more powerful there than

D. Testing the Identifying Assumption

As equation 2 shows, the matching methodology is based on the identifying assumption that the unobservable component of county-level variation for two border counties is identical. How justifiable is this assumption? Since it is impossible to test its validity directly, one way to verify whether it is reasonable is to examine how similar border counties are in terms of *observable* characteristics. The difficult part of designing tests based on observables is determining what the relevant comparison group for counties should be. There is no obvious single candidate. Table 9 therefore reports the difference in county census characteristics between border counties relative to four comparison groups. For each census characteristic shown in the rows of the table, the difference for each border county pair is computed.[52] The absolute value of the difference is then compared to the absolute value of the comparison group, where the comparison groups are listed in the column titles. An entry in the table shows the percentage of matched pairs whose differenced value of a census characteristic is less than that of the comparison group. The four comparison groups are: the difference in the first county of

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in interior counties. While they cannot adjust the way supervisors behave toward them, banks can and possibly do respond to the formal regulatory restraints that govern banks on the other side of a state border. It may be that banks in border county *q*, state *i* not only respond to the required regulations in state *i*, but also take into account regulations in state *j* in order to compete with banks that are physically located near their offices. If this were true, behavior by banks located along state borders would not be symmetric. A bank in state *i* could not lower its reserve or capital requirements to those that applied to state *j*, but it could raise them to these levels if it thought that this would attract customers (because customers would perceive the bank as acting more prudently). If cross-border regulatory pollination took place, then this would violate the so-called stable unit-treatment value assumption (Rubin, 1980) used in much of the matching literature: that the failure rate of county *q* in state *i* does not depend on the (regulatory and supervisory) treatment given to county *r* in state *j*. In contrast, within a state, all banks, regardless of the county they are located in, are operating by the same rules of the game. As one moves farther from state borders, the impetus to compete with banks in other states might therefore decline.

[52] The census characteristics considered were those used in the regression analysis.

the matched pair and its state average for interior counties (column 1); the difference in the first county of the matched pair and the second county's state average for interior counties (column 2); the difference in the second county of the matched pair and its state average for interior counties (column 3); and the difference in the second county of the matched pair and the first county's state average for interior counties (column 4).

If border counties are similar in terms of observable characteristics, then for a large percentage of matched pairs, the difference in their census attributes ought to be smaller than the corresponding difference between each county and the other county's state average of interior counties. But as columns 2 and 4 show, this is only true for 42 percent to 57 percent of the matched pairs, depending on which of the six census characteristics is considered. Alternatively, if border counties are very similar, then differences in their census characteristics should be smaller than the difference between each border county and the average value for interior counties of that same state. However, columns 1 and 3 show that this is true for only 45 percent to 55 percent of the county pairs. These figures therefore cast doubt on the validity of the identifying assumption used in the matching methodology. Since the observable census characteristics of the border counties are not very similar relative to the four benchmarks used in table 9, it may not be justifiable to assume that the unobservable characteristics of border counties are identical.[53]

[53] It is also possible that county selection is nonrandom. This problem would exist, for example, if treatment variables are correlated with other regressors (See Persson, 2001 for a discussion). To test whether the assignment of a county's regulatory and supervisory system is nonrandom, I eliminate from the sample any border counties with populations that make up more than 20 percent of the state's population – those that might be regarded as having a very strong influence on legislative outcomes and therefore influencing regulatory and supervisory outcomes. When these 37 "influential" counties are removed, the coefficients on the regulatory and supervisory variables are not significantly different from those reported in table 6. This suggests that nonrandom assignment is not the likely the reason for the observed differences in coefficient estimates for the matched border county sample and the full sample of counties.

VI. Concluding Remarks

The intransigence of the Federal Reserve severely strained the American banking system during the 1930s, and in turn worsened the economic severity of the Depression. But the commercial banking system was further destabilized by policy blunders at the state level. States, influenced by interest groups seeking to shape the competitive landscape of banking, adopted different regulatory and supervisory regimes, which in some cases conflicted with those imposed by the Comptroller of the Currency on national banks. As this paper has emphasized, these differing regulatory and supervisory regimes in turn affected the stability of state banking systems during the Great Depression by altering the incentives and behavior of banks and regulators.

A simple model provided an empirical strategy for identifying the sources of variation in failure rates arising from both county and state effects, and the estimation took advantage of previously underutilized data on county bank failures as well as previously untapped information on direct measures of bank supervisory practices. Even after controlling for differences in the local severity of the downturn and county economic characteristics, supervisory and regulatory practices help explain the large differences in failure rates for state-chartered banks at the county level. More stringent capital requirements resulted in lower county failure rates for state-chartered commercial banks, while laws prohibiting branching raised failure rates. Supervisors, given the sole authority to liquidate banks, could do so quickly, minimizing contagion effects across banks and credit-channel

dislocations. However, those states that granted their supervisors sole power to license new banks or gave them long tenure strengthened the incentives for bank lobbyists to influence supervisory decisions and consequently experienced higher failure rates.

The results in this paper provide an interesting historical lesson for policymakers engaged in reforming the framework for cross-border banking today. The implications for Europe are particularly salient given its striking institutional similarity to U.S. states during the Great Depression. With the creation of a common currency and the European Central Bank, ECU members now have a single monetary authority, but supervision and regulation remains decentralized. Each national government has its own prudential regulations and supervisory institutions for monitoring financial institutions, just as U.S. states had during the Depression.

Arming nations with "best-practice" supervisory and regulatory policies, such as those put forth in the Basle Committee's "Core Principles for Effective Banking Supervision," can potentially improve the stability of the international banking system; but the experience of U.S. states during the Depression also shows that implementing such practices may be difficult. Even if policymakers are able to win the support for reform from vested interests, some institutional design decisions involve trade-offs that complicate policymakers' abilities to choose optimal regulations. In the 1920s and 1930s, policymakers in U.S. states had to choose between granting supervisors greater discretionary power and accepting the lack of oversight and outside influence that came

with it, or restricting supervisory powers and acknowledging that it would be more difficult for supervisors to act decisively, quickly, or flexibly.

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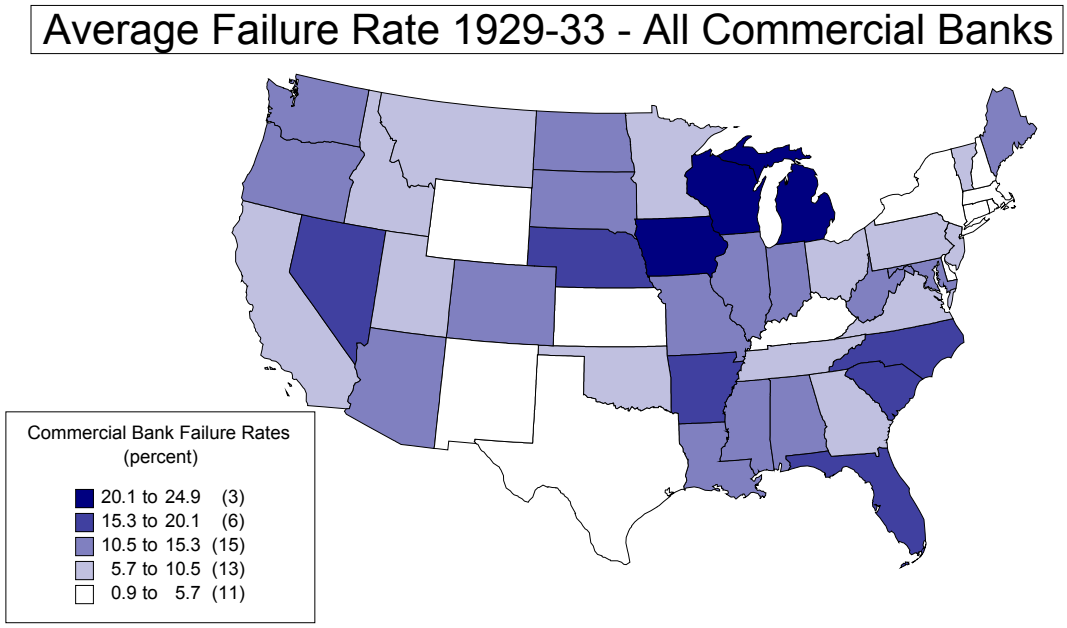
Figure 1. Bank Failure Rates during the Great Depression



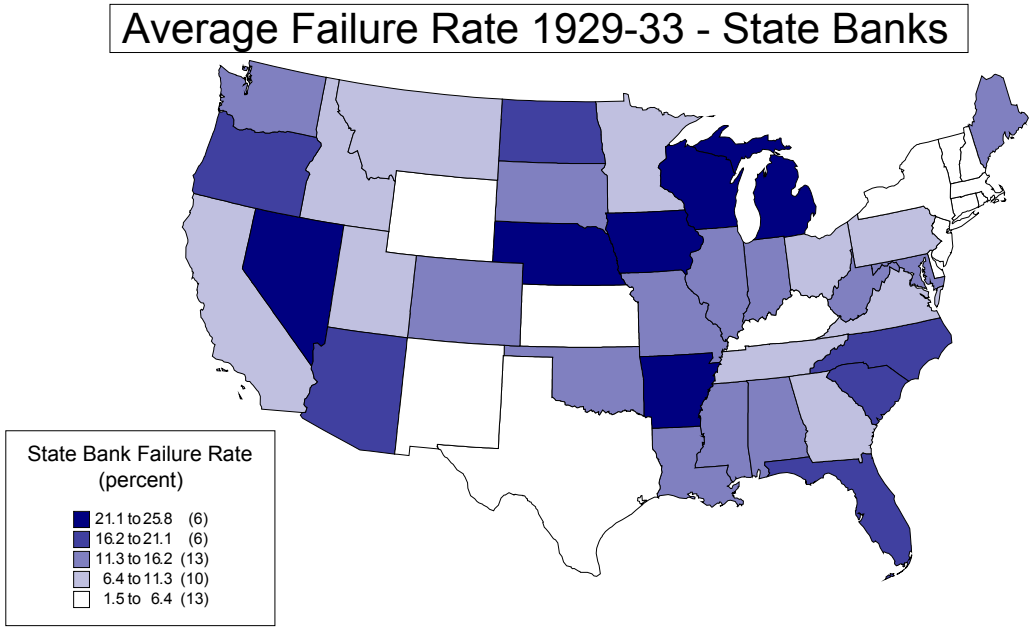
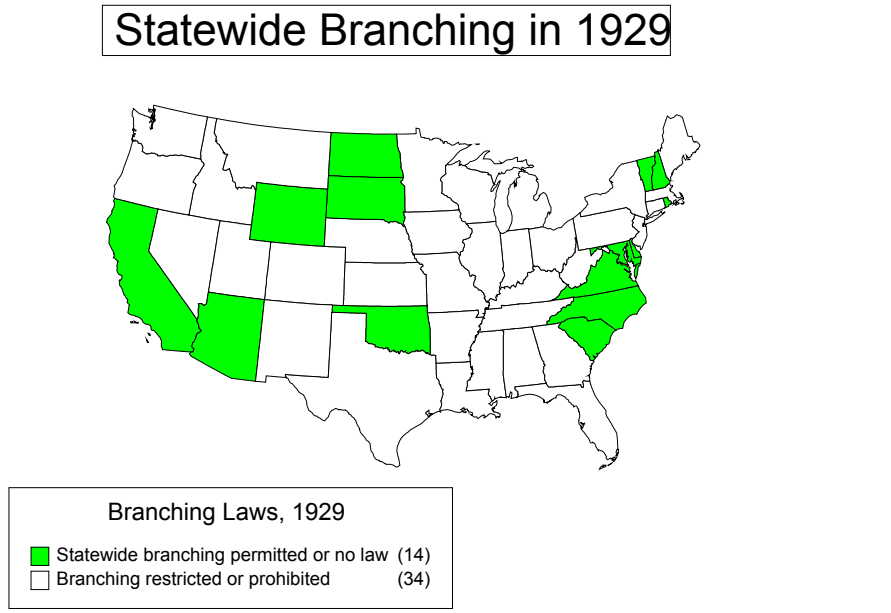


Figure 2. State Branching Regulations in 1929



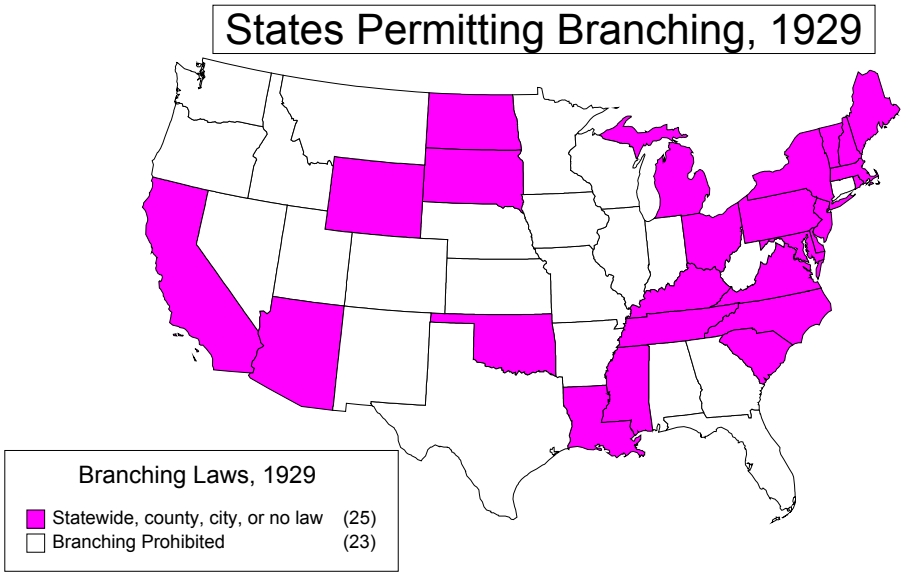
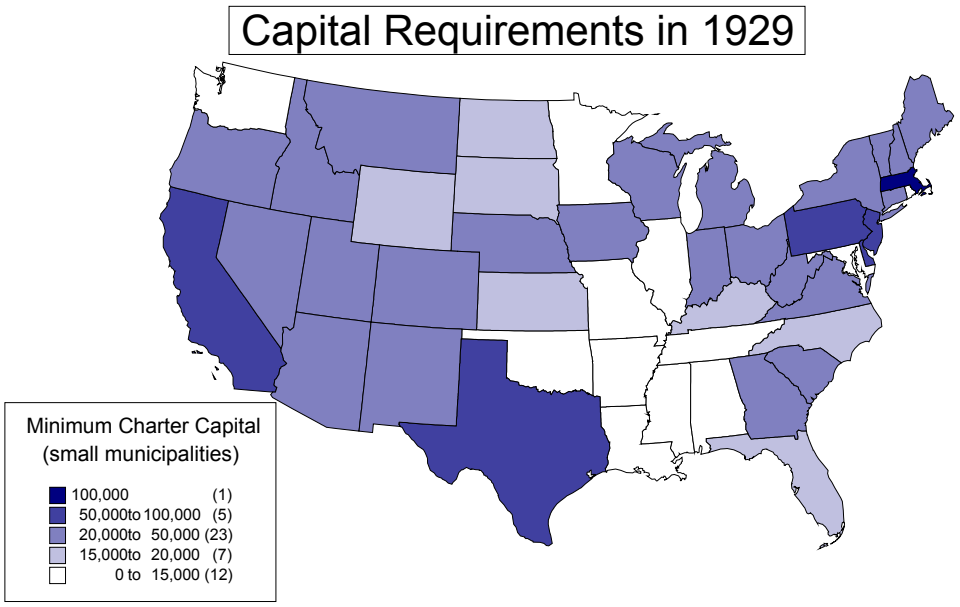


Figure 3. Capital and Reserve Requirements for State Banks



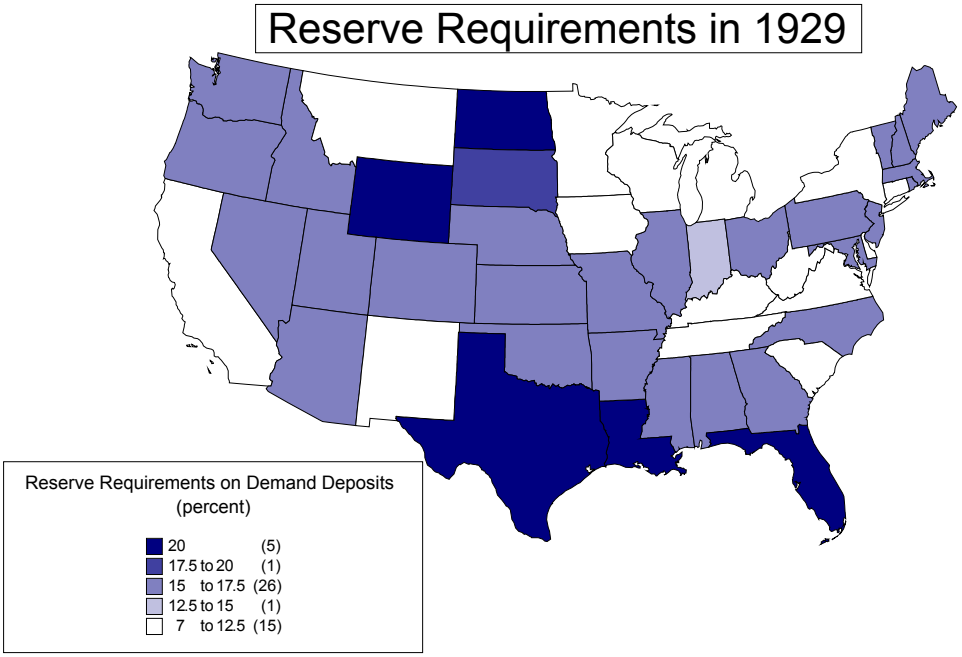


Table 1. Commercial Bank Suspensions by Charter Status, 1929-33

	All Commercial Banks	State Banks	National Banks
Total Suspensions, 1929-33	9440	7429	2011
Share of Total Bank Suspensions (%)		79%	21%
Average Failure Rate (1929-33)	9.2%	10.2%	6.8%
Standard Deviation of Failure Rate	10.1	11.2	8.9
Total Deposits ($billions), 1929	49.4	27.8	21.6
Share of Total Deposits, 1929		56%	44%
Total Banks in 1929	24970	17440	7530
Share of Total Banks		70%	30%
Deposits per bank ($millions), 1929	1.98	1.59	2.87

Sources: Calculations based on bank suspension data from Federal Reserve *Bulletin*, September 1937, and deposit, asset, and charter data from Board of Governors, Federal Reserve System (1959).
Failure rates in this table (only) are computed using state-level data from the sources listed above.

Table 2. County Bank Failures for State-Chartered Commercial Banks

Average Failure Rate	All Counties	West	Northeast	Midwest	South
1929-33	9.47	7.47	3.96	12.41	8.32
	(9.31)	(8.17)	(4.90)	(8.55)	(9.94)
1929-32	6.86	5.83	2.44	8.23	6.68
	(8.38)	(8.91)	(4.02)	(7.86)	(8.89)
1930-33	11.10	9.15	4.90	14.60	9.59
	(11.15)	(10.18)	(6.05)	(10.30)	(11.81)

Notes: Standard deviation shown in parentheses. Calculations based on county-level data from data files from FDIC (1960).The failure rate for a county is defined as the number of state-chartered banks suspended during one year divided by the number of banks in existence at the end of the previous year. Regions are defined according to census definitions.

Table 3. Explaining the Variation in State Bank Failure Rates Across U.S. Counties

(Dependent Variable: Average County Failure Rate from 1929-33 for State-Chartered Banks)

Independent Variable	Column 1	Column 2	Column 3
Sole Authority to Charter Banks	2.066 ***	2.055 ***	2.786 ***
(standard error)	0.453	0.453	0.456
[p value]	0.000	0.000	0.000
Sole Authority to Liquidate Banks	-5.034 ***	-5.047 ***	-4.855 ***
	0.497	0.497	0.490
	0.000	0.000	0.000
Supervisors' Term	1.009 ***	1.001 ***	1.311 ***
	0.202	0.203	0.196
	0.000	0.000	0.000
Banks per Examiner	-0.009	-0.010	-0.006
	0.013	0.013	0.013
	0.520	0.440	0.648
Capital Requirement	-0.084 ***	-0.084 ***	-0.050 ***
(in $1000s)	0.013	0.013	0.013
	0.000	0.000	0.000
Reserve Requirement	0.005	0.002	0.029
	0.068	0.068	0.067
	0.941	0.982	0.661
States Prohibiting Branching	1.996***	2.039 ***	1.512***
	0.471	0.472	0.474
	0.000	0.000	0.001
Farm Land Value	-0.041***	-0.040***	-0.030***
(Percentage Change: 1920-30)	0.007	0.007	0.007
	0.000	0.000	0.000
% Rural Farm Population	-0.016		
	0.016		
	0.310		
Farms per 1,000 people		-0.004	
		0.007	
		0.529	
% Change in State Personal Income			-0.240***
			0.034
			0.000
County Economic Structure	Yes	Yes	yes
R-Squared	0.123	0.123	0.145
Number of Observations	2211	2211	2211

Notes: A constant term (not reported) and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included. Standard errors are Eicker-White heteroskedasticity consistent.

Table 4. Differences in State Economic Conditions & Alternative Sample Periods

(Dependent Variable: Average County Failure Rate for State-Chartered Banks)

Independent Variable	1929-33	1929-33	1930-33	1929-32
Sole Authority to Charter Banks	0.518	0.521	0.501	0.419
(standard error)	0.445	0.446	0.523	0.447
[p value]	0.245	0.243	0.339	0.348
Sole Authority to Liquidate Banks	-3.656 ***	-3.686 ***	-4.235 ***	-2.561 ***
	0.494	0.505	0.583	0.452
	0.000	0.000	0.000	0.000
Supervisors' Term	0.970 ***	0.988 ***	0.961 ***	0.834 ***
	0.189	0.198	0.234	0.175
	0.000	0.000	0.000	0.000
Banks per Examiner	0.005	0.005	-0.007	0.007
	0.013	0.013	0.016	0.012
	0.678	0.702	0.646	0.570
Capital Requirement	-0.039 ***	-0.041 ***	-0.056 ***	-0.018
(in $1000s)	0.012	0.014	0.016	0.013
	0.002	0.002	0.001	0.161
Reserve Requirement	0.151 **	-0.040	-0.287	0.170
	0.064	0.347	0.405	0.309
	0.018	0.907	0.480	0.581
Reserve Requirement Squared		0.007	0.017	-0.001
		0.013	0.015	0.012
		0.586	0.261	0.904
States Prohibiting Branching	1.734 ***	1.745 ***	2.348 ***	0.822 **
	0.448	0.450	0.537	0.410
	0.000	0.000	0.000	0.045
Farmland Value	-0.020 ***	-0.021 ***	-0.021 **	-0.031 **
(Percentage Change: 1920-30)	0.007	0.007	0.009	0.006
	0.004	0.003	0.014	0.000
Failure Rate for National Banks	0.816 ***	0.812 ***	0.867 ***	0.676 ***
Using State-Level Data	0.055	0.056	0.055	0.067
	0.000	0.000	0.000	0.000
County Economic Structure	yes	yes	yes	yes
R-Squared	0.209	0.209	0.204	0.152
Number of Observations	2211	2211	2213	2253

Notes: A constant term (not reported), the percentage change in farm land values from 1920-30, and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included. Standard errors are Eicker-White heteroskedasticity consistent.

Table 5. Specification Tests for Branch Banking

(Dependent Variable: Average County Failure Rate from 1929-33 for State-Chartered Banks)

Independent Variable	Column 1	Column 2	Column 3
Sole Authority to Charter Banks	0.295	0.694	0.728
(standard error)	0.442	0.450	0.458
[p value]	0.504	0.123	0.112
Sole Authority to Liquidate Banks	-3.675 ***	-3.512 ***	-3.509 ***
	0.489	0.484	0.497
	0.000	0.000	0.000
Supervisors' Term	0.895 ***	1.183 ***	1.048 ***
	0.192	0.196	0.194
	0.000	0.000	0.000
Banks per Examiner	0.005	0.021 *	0.016
	0.012	0.012	0.013
	0.713	0.064	0.203
Capital Requirement	-0.032 ***	-0.024 *	-0.027 **
(in $1000s)	0.012	0.012	0.013
	0.010	0.053	0.037
Reserve Requirement	0.097	0.185***	0.206***
	0.067	0.062	0.063
	0.149	0.003	0.001
Farmland Value	-0.017**	-0.020***	-0.022***
(Percentage Change: 1920-30)	0.007	0.007	0.007
	0.018	0.004	0.002
Failure Rate for National Banks	0.846***	0.860***	0.820***
Using State-Level Data	0.054	0.053	0.055
	0.000	0.000	0.000
States Permitting Any Branching	-2.318***		
	0.479		
	0.000		
Statewide Branching		-2.252***	
		0.628	
		0.000	
Branches as % of State Banks			-0.057**
			0.024
			0.019
County Economic Structure	yes	yes	yes
R-Squared	0.212	0.207	0.204
Number of Observations	2211	2211	2211

Notes: A constant term (not reported), the percentage change in farm land values from 1920-30, and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included. Standard errors are Eicker-White heteroskedasticity consistent.

Table 6. Explaining Failure Rates Using a Sample of Border Counties

(Dependent Variable: Average County Failure Rate for State-Chartered Banks)

Independent Variable	Column 1	Column 2	Column 3
Sole Authority to Charter Banks	4.930 ***	5.036 ***	3.704 ***
(standard error)	0.684	0.694	0.718
[p value]	0.000	0.000	0.000
Sole Authority to Liquidate Banks	-5.130 ***	-5.219 ***	-4.871 ***
	0.696	0.702	0.690
	0.000	0.000	0.000
Supervisors' Term	0.960 ***	0.943 **	0.850 ***
	0.374	0.375	0.367
	0.010	0.012	0.021
Banks per Examiner	-0.003	-0.009	0.008
	0.022	0.023	0.023
	0.898	0.684	0.726
Capital Requirement	0.021	0.025	0.031
(in $1000s)	0.023	0.023	0.022
	0.351	0.274	0.166
Reserve Requirement	0.024	0.583	1.159*
	0.101	0.605	0.600
	0.810	0.335	0.054
Reserve Requirement Squared		-0.022	-0.046*
		0.024	0.024
		0.349	0.054
States Prohibiting Branching	0.114	0.039	-0.397
	0.823	0.827	0.812
	0.890	0.962	0.625
% Rural Farm Population	-0.034	-0.037	-0.037
	0.034	0.034	0.034
	0.321	0.287	0.271
Failure Rate for National Banks			0.549***
Using State-Level Data			0.098
			0.000
County Economic Structure	yes	yes	yes
"R-Squared"	0.123	0.124	0.164
Number of Observations	1747	1747	1747

Notes: The fixed effects (within) estimator is reported for the sample of counties that are located in different states, but share a common border. A constant term (not reported), the percentage change in farm land values from 1920-30, and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included.

Table 7. Comparing Cross-Sectional Results of Perimeter and Interior Counties

(Dependent Variable: Average County Failure Rate for State-Chartered Banks)

Independent Variable	Perimeter Counties	Interior Counties
Sole Authority to Charter Banks	1.252	0.016
(standard error)	0.791	0.548
[p value]	0.114	0.977
Sole Authority to Liquidate Banks	-2.661***	-4.181***
	0.786	0.660
	0.001	0.000
Supervisors' Term	0.793***	1.117***
	0.339	0.252
	0.020	0.000
Banks per Examiner	0.035	-0.014
	0.021	0.017
	0.096	0.401
Capital Requirement	-0.008	-0.063***
(in $1000s)	0.020	0.019
	0.693	0.001
Reserve Requirement	0.323	-0.222
	0.609	0.422
	0.596	0.599
Reserve Requirement Squared	-0.009	0.015
	0.023	0.016
	0.707	0.348
States Prohibiting Branching	1.072	2.160***
	0.696	0.598
	0.124	0.000
% Rural Farm Population	-0.012	-0.008
	0.025	0.020
	0.629	0.691
Failure Rate for National Banks	0.731***	0.837***
Using State-Level Data	0.097	0.070
	0.000	0.000
County Economic Structure	yes	yes
R-Squared	0.183	0.235
Number of Observations	813	1398

Notes: Perimeter counties are those counties that share a border with another state. Interior counties are those not on a state border. A constant term (not reported), the percentage change in farm land values from 1920-30, and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included. Standard errors are Eicker-White heteroskedasticity consistent.

Table 8. Distribution of Samples

(Regression Sample Using National Bank Failure Rate Calculated at State Level)

Sample	Northeast	Midwest	South	West	Total
All Counties	194	858	958	201	2211
a. (% of total)	8.8	38.8	43.3	9.1	
Interior Counties	98	565	610	125	1398
b. (% of interior)	7.0	40.4	43.6	8.9	
c. (% of all counties)	50.5	65.9	63.7	62.2	63.2
d. (difference:b-a)	-1.8	1.6	0.3	-0.1	
Perimeter Counties	96	293	348	76	813
e. (% of perimeter)	11.8	36.0	42.8	9.3	
f. (% of all counties)	49.5	34.1	36.3	37.8	36.8
g. (difference:e-a)	3.0	-2.8	-0.5	0.3	
Matched Border	214	623	753	157	1747
h. (% of matched border)	12.2	35.7	43.1	9.0	
I. (% of all counties)	110.3	72.6	78.6	78.1	79.0
j. (difference:h-a)	3.5	-3.1	-0.2	-0.1	

Table 9. Examining Observable Census Characteristics of Border Counties
(Percentage of Matched Pairs)

County Characteristic	|County 1 - County 2| < |County 1 - State1| ?	|County 1 - County 2| < |County 1 - State2| ?	|County 1 - County 2| < |County 2- State2| ?	|County 1 - County 2| < |County 2- State1| ?
Rural Farm Population (%)	45.1	48.5	47.8	49.0
Farms per 1,000 people	46.0	50.5	47.9	48.5
Pct. Change in Farm Land Value	53.2	56.4	52.8	55.1
Manufacturing Employment (%)	54.8	52.9	55.4	56.8
Wholesale Employment (%)	52.0	50.1	51.3	52.1
Retail Employment (%)	46.4	42.8	42.9	46.2

Notes:

The cells in the table calculate the absolute value of the difference between the observable county characteristic for each matched pair. This value is then compared to one of four other differences shown in the column headings. The percentage of matched pairs with a differenced value that is smaller than that of the comparison group is then recorded in the cell. The percentage change in farm land value is measured from 1920 to 1930. Employment percentages are calculated as the total number of individuals engaged in the occupation divided by the county's population in 1929.
County 1 refers to the value of the county characteristic for the first county of the matched pair.
County 2 refers to the value of the county characteristic for the second county of the matched pair.
State 1 refers to the average of the variable for interior counties that are located in the same state as county 1.
State 2 refers to the average of the variable for interior counties that are located in the same state as county 2.

Appendix Table 1. Tobit Analysis of Bank Failures

(Dependent Variable: Average County Failure Rate for State-Chartered Banks)

Independent Variable	1929-33	
Sole Authority to Charter Banks	-0.116	
(standard error)	0.543	
[p value]	0.831	
Sole Authority to Liquidate Banks	-4.082	***
	0.556	
	0.000	
Supervisors' Term	1.277	***
	0.260	
	0.000	
Banks per Examiner	0.009	
	0.017	
	0.583	
Capital Requirement	-0.075	***
(in $1000s)	0.020	
	0.000	
Reserve Requirement	0.545	
	0.419	
	0.193	
Reserve Requirement Squared	-0.013	
	0.015	
	0.408	
States Prohibiting Branching	2.371	***
	0.544	
	0.000	
Farm Land Value	-0.034	***
(Percentage Change: 1920-30)	0.010	
	0.000	
Failure Rate for National Banks	1.052	***
Using State-Level Data	0.070	
	0.000	
County Economic Structure	yes	
Probability > Chi-Squared	0.000	
Pseudo R-Squared	0.038	
Number of Observations	2211	
Number of Censored Observations	772	

Notes: A constant term (not reported), the percentage change in farm land values from 1920-30, and the share of a county's population that worked in retail, wholesale, and manufacturing, respectively (county economic structure) were also included. Standard errors are Eicker-White heteroskedasticity consistent.