Politics, Regulation, and Economic Stability:

Evidence from the Progressive Era

Abstract

The federal judiciary increasingly subscribes to the theory of the unitary executive, which asserts that federal agencies should be controlled directly by the President, particularly agencies that regulate financial institutions, which Congress typically provides independence from political oversight, to prevent partisan considerations from impacting economic activity. The effects of regulatory independence and political subservience can be observed by examining policy experiments from the Progressive Era. We use gubernatorial elections as an instrument to determine the impact of reforms to the structure of bank regulation and bankruptcy adjudication. We find that enhancing bank regulation smoothed failure rates for banks and businesses. This smoothing promoted economic stability by limiting leverage cycles. Strengthening political control of bank regulatory agencies, however, offset much of this positive effect, because regulators manipulated bank failure rates around elections. These machinations linked electoral and economic cycles. Our results have implications for current debates about the value of regulatory independence and suggest that Supreme Court decisions concerning the constitutionality of independent financial regulation could have consequences for the aggregate economy.

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1. Introduction

During the Progressive Era, from the 1890s through the 1920s, federal and state legislatures established arrays of agencies to regulate economic activity. Bank regulation underwent extensive reform (Komai and Richardson 2014, Mitchener and Jaremski 2015). The optimal structure of these agencies and the value of regulatory independence was widely debated at the time. In the early 20th century, two Supreme Court decisions, *Myers v United States* and *Humphrey's Executor v. United States*, established the constitutionality of independent government agencies. A recent Supreme Court case (June 29, 2020), *Seila Law v. Consumer Financial Protection Bureau* (CFPB), however, calls into question those Progressive precedents by finding the CFPB's independent single-director structure unconstitutional. This case is the spearpoint for a series of lawsuits supported by attorney generals of many states and an increasingly popular legal philosophy, the theory of the unitary executive, which seeks to overturn the independence of federal agencies by declaring their independence unconstitutional. These events have triggered discussion about the optimal structure of government agencies, particularly those regulating banking and financial institutions, for which independence may be particularly important. Should the current structure of agencies be defended from attacks on their constitutionality because independence has optimal properties? How do independent financial regulatory agencies perform relative to plausible alternatives, if courts or legislatures limited independence and strengthened political supervision?

Answering these questions empirically using modern data is difficult, because regulatory agencies' structure converged to a standard model by the mid-twentieth century and now varies little across states and over time. The Progressive Era can serve as a laboratory for learning about these issues. From the 1890s through the 1920s, state governments established an array of regulatory agencies operating under a variety of organizations forms. In many states, for example, bank regulators worked directly for the governor, who could remove them at will or allocate the positions for political patronage.

In other states, the courts played a large role in regulating financial institutions, particularly in determining whether and when banks failed, until legislation shifted the authority to make these decisions to regulatory agencies. The heterogeneity of Progressive Era institutions provides opportunities for statistical analysis that enable scholars to learn about the relationship between politics, industry, banking, and economic stability.

One of the largest Progressive-Era reforms along these lines involved regulations regarding how, when, and whether businesses and banks failed. Failures of businesses are typically termed bankruptcies. The Bankruptcy Act of 1898 created national standards and procedures for business bankruptcies and required all such cases to be adjudicated in federal bankruptcy courts. Failures of banks are typically termed resolutions. The Act prohibited federal courts from handling bank resolutions. Instead, resolutions of nationally chartered commercial banks were supervised by the Comptroller of Currency. Resolutions of state chartered commercial banks were also excluded from federal courts and left to states themselves. Initially in most states, courts supervised bank resolutions, which began when depositors sued to force banks to honor withdrawal requests. Depositors tended to do this during panics or when economic shocks weakened the balance sheets of many banks, leading to long periods without few (or no) resolutions and short bursts with high resolution rates. During the Progressive Era, states shifted regulation of commercial banks to agencies in their executive branches which reported to their governors. The goal of these reforms was to stabilize the banking system and thereby stabilize business operations and bankruptcies. Regulators were supposed to monitor banks continuously and intervene to correct or close those which were weak. This monitoring should raise failure rates at most dates since regulators would monitor banks more steadily than depositors while reducing the frequency and intensity of high failure rates. Did these reforms succeed?

Analysis of this experiment is impeded by two hurdles. The first is limited information. For the

Progressive Era, data on business failure rates does not exist, while data on bank failure rates is limited (Bodenhorn 2020). The second set of obstacles involves inference. Bank and business failures are endogenous and interrelated. Determining whether changing bank-resolution procedures influenced business failure rates requires an instrument which is exogenous and affects the former but not the latter. Using the instrument in our panel of related rates is not straightforward. The first-stage regression is non-linear. So, results from standard instrumental variables methods are biased. Credible causal inference in this setting is an active area of research.

We overcome the first obstacle by creating new data series of bank and firm failure rates by state and quarter from 1900 through 1930. We combine this data with three types of existing information on the political economy of the Progressive Era. The first is information on financial regulation in all 50 states, the federal government, and for the twelve districts of the Federal Reserve System (Federal Reserve 1932, Komai and Richardson 2014, Mitchener and Jaremski 2015, White 1983). The second is information on political structures and outcomes in all 50 states. This information includes the identities, political parties, and dates of election for all federal and state legislatures, executives (e.g. President and governors), and officials. The third is information on shocks to localities and the economy. This information includes information such as weather, crop yields, interest rates, and financial panics.[1]

We overcome the second obstacle using an instrument, gubernatorial elections, whose effect has been observed in postwar data. Governors and their subordinates have incentives to defer resolution of troubled banks during elections, in hopes that better news about the economy yields more votes for their party at the ballot box. Governors and their subordinates lacked the ability to defer business bankruptcies, since creditors determined when suits were filed, and adjudication occurred in federal courts.

We initially use elections to illuminate how states' creation of regulatory agencies influenced

[1] We disaggregate as much of this information as possible to the state-quarter level, although available sources limit some of this information in frequency (to annual) or geography (to region or national).

bank failure rates. Data on bank failure rates by state and quarter is extremely skewed, with a discontinuity and cluster of observations at zero. For this distribution, the appropriate estimator is a zero inflated beta distribution (Papke and Wooldridge 1996). We estimate this using the maximum likelihood procedure of Maarteen Buis (2010) with state and time fixed effects as well as a wide range of controls. The procedure yields clear and robust results. After states established regulatory agencies which controlled bank resolution, these agencies worked as advertised. Regulators shuttered banks which became weak. The frequency of quarters with no bank failures fell substantially. Regulators also reduced the intensity of panics. The frequency of quarters with high failure rates also declined. Regulators, however, deferred resolution of troubled banks during gubernatorial election campaigns. In the two quarters prior to a gubernatorial election, the probability that regulators would close zero banks rose substantially. This effect only occurs for state-chartered banks and in states where a regulatory agency under the governor's jurisdiction controls the resolution process. It does not occur for nationally chartered banks. It does not occur prior to elections for the President, state or federal legislators, state judges, or other state officials (e.g. attorney generals). It also does not occur in states where courts, rather than a regulatory agency under the governor's jurisdiction, controls the bank-resolution process. Regulatory forbearance, in other words, did not occur in states whose regulators were not controlled by the governor or before elections for other state and federal officials.

The strong results from this initial regression indicate that gubernatorial elections can serve as an instrument for bank failure rates in a two-stage procedure where we first estimate how gubernatorial elections influenced the bank failure rate and then use that result to estimate how the bank failure rate influenced the business failure rate. No procedure exists for directly employing the results from a zero inflated beta distribution as the first stage in an IV; such a procedure would be biased. In this situation, the recommended procedure involves employing a less informative first stage which yields an unbiased

second stage. The current literature provides three candidates: estimate the first stage as a linear, logistic, or fractional logit regression and calculate the second stage using the normal, residual, or control-function approach. A linear regression focuses on movements in the mean of the distribution. The logistic regression, in which our dependent variable equals zero if the bank failure rate is zero and one otherwise, emphasizes the left tail of the distribution (i.e. the spike at zero). The fractional logit regression emphasizes the right tail (i.e. changes in non-zero failure rates). We employ all three approaches. All indicate that exogenous reductions in bank failure rates reduced business failure rates. The magnitude of the effect is substantial. The instruments are strong.

The last stage of our analysis examines the impact of these Progressive Era banking reforms on business failure rates at the state and national level. Our initial regression revealed that establishing state banking agencies reduced the volatility of bank failure rates by raising bank failure rates in normal time and reducing the intensity of financial panics, except during elections, when bank failure rates shifted towards zero. Our IV indicates how changing bank failure rates influenced business failure rates. We use the distribution derived from the first regression and the coefficient from the second regression to determine the impact of Progressive Era banking reforms on state and national business failure rates. We find that these reforms reduced economic volatility but increased the correlation between political and business cycles.

2. Historical Background

This section discusses facts important for understanding our analysis and provides an initial look at the evidence. It begins by discussing bankruptcies of businesses. The timing of these bankruptcies was determined by decisions of debtors, creditors, and federal judges. It then discusses bankruptcies of banks, typically termed resolutions. In some states, depositors and courts controlled the timing bank resolutions. In other states, regulators subordinate to the state's governor determined the timing of resolutions. For banks in the later, qualitative and quantitative evidence indicates resolution rates declined during gubernatorial election campaigns.

2.1 Businesses Bankruptcy and the Bankruptcy Act of 1898

The Bankruptcy Act of 1898 standardized procedures for bankruptcies throughout the United States (Campbell 1898, Bush 1899, Talley 1903, G.E.H 1912, Tardy 1920, Warren 1935, and Hansen 2001). All bankruptcies of businesses, except railroads and banks, were adjudicated in federal district courts.[2] The timing was determined by creditors, who could choose to file suit at any time after debtors failed to make required payments. The judges who supervised the process held lifetime tenures. They could not be fired without cause and then only after investigation and impeachment. Individual judges dealt with a small proportion of the cases and lacked the ability to influence aggregate failure rates.[3] State officials – including governors, legislators, and judges – had no authority over the timing of bankruptcy

[2] Railroad bankruptcy was handled by a separate process which allowed for the reorganization of assets and continued operation of the enterprise. Reorganization and voluntary liquidation for firms were allowed only after amendments to the Bankruptcy Act during the Great Depression.

[3] No evidence exists that judges ever collectively attempted to influence aggregate business failure rates in hopes of influencing an election, for example, by deferring cases until after an election or by adjudicating cases near elections with strong partisan bias. Such actions would have been observable and illegal. Aggrieved parties – including litigants who lost money, judges from competing political parties, and lawyers concerned with the violation of judicial norms – would have an incentive to report such electoral shenanigans. The federal Congress could then have impeached the judges who had violated their oaths of office, removed them from office, and deprived them of their lifetime-guaranteed salaries.

filings or the adjudication of cases, and therefore, lacked the ability to directly influence business failure rates.

Quarterly business failure rates for U.S. states exhibited stable patterns from turn of the century through the end of the Roaring 20s, which are displayed in Table 1 and Figure 1. Rates were low. Rates averaged 2.39 failures per thousand firms per quarter of the year. At this time, failures of conglomerates operating in multiple states were rare except for railroads which are excluded from the data. So, the business failure rates represent the ebbs and flows of economic activity in each state. Rates rose during recessions, peaking in 1908, 1915, and 1922. Rates fell during booms, particular the economic expansion of World War I. While rates varied over from year to year, averages over time varied less. About 2.5 firms per thousand failed from 1902 to 1916, the last year before the United States entered World War 1. About 2.7 per thousand failed during the 1920s. Rates varied across states. Interstate differences were stable over time. States with higher than average rates in the 1900s also had higher than average rates in the 1910s and 1920s. The distribution of rates displayed in Figure 1 appears smooth and slightly skewed rightwards.[4]

2.2 Bank Resolution and the Dual Banking System

The process for dealing with banks that could not pay their debts (termed bank resolution) differed from the process for businesses that could not pay their debts. The process also differed between national and state banks. National banks, which received their charters from the federal government, comprised one-quarter to one-third of all banks (details in Table 2).[5] The Office of the Comptroller of

[4] Figure 1 reveals a small clump of failure rates at zero. The zeroes come from states with low populations at peculiar points in time. These include the territories of Arizona and New Mexico before statehood in 1912; the mining state of Nevada during the Tonopah silver boom from 1900 to 1913; and the grain growing states of North Dakota, South Dakota, and Wyoming during the agricultural export boom of World War 1. Three zeros also occur in Delaware (with 2 coming during the World War 1 boom) and one zero in Maine.

[5] While numbers of banks rose and fell, the distribution of the two types of banks remained stable. In 1900, 30% of banks possessed national charters. In 1930, 31% of banks possessed national charters. At both points of time and

Currency (OCC) supervised national banks.[6] It was responsible for determining when banks under its jurisdiction could not pay their debts and for liquidating those insolvent institutions (High 1910, p. 442; Komai and Richardson, 2014). The OCC was an independent agency in the U.S. Department of Treasury whose director, the Comptroller, was appointed by the President and confirmed by the Senate. Comptrollers were appointed for five-year terms, received high salaries, and could only be removed from office with the concurrence of the President and Senate. The length of appointment and limits on removal were unique among federal appointees. Congress structured the office in this manner to limit partisan influence on bank supervision.

The OCC did not supervise state-chartered banks.[7] Before 1900, "many states had no supervision

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every year in between, both types of banks operated in all states as well as in almost every city and county, except for rural counties with very few banks. The fraction of banks possessing national charters was highest in New England, and lower in states in the South and West. These patterns changed little between 1900 and 1930 (Board of Governors, 1959). The territorial zeros appear to reflect missing data.

[6] The OCC examined national banks multiple times each year, collected and published their financial information, and assisted prosecutions for violations of banking laws. National bank regulation was stricter than regulation in almost all states. State and national banks' business models had many common features. Lending tended to be local. Most borrowers were businesses. The businesses typically operated in the same county, and almost always in the same state, as their lender. Banks' bought bonds to diversify investments geographically, although their bond portfolios exhibited home bias. The share of bonds issued by firms and governments from within their state exceeded the share that would exist in a completely diversified portfolio. Both state and national banks were, in other words, subject to similar local lending risks and similar shocks to the asset side of their balance sheet (cite Cohen, Hachem, Richardson). State and national banks differed in some respects. National banks could issue notes (i.e. currency); state banks could not, due to the 10% annual tax imposed upon state-bank notes. State banks typically faced lower minimum capital requirements, which meant that numerous small state banks existed (Dual Banking System, p. 7-9). State banks also typically faced lower reserve requirements, which meant they were required to hold a smaller share of their deposits as cash in their vaults, where they earned no interest, or deposits with correspondent banks, where they earned little interest. In most states, state-chartered banks could invest a higher proportion of their assets with a single individual or firm and invest a higher proportion of their assets in unsecured loans. In one-third of states, state-chartered banks could loan money on mortgages. State banks also faced less strict supervision. Overall, these differences provided state-chartered banks competitive advantages in the decades before the Federal Reserve Act, which explain the growth of state-chartered banking from the 1880s, when it was near nonexistent, to 1915, when assets in state-chartered banks surpassed assets in nationally-chartered banks for the first time since the creation of the national banking system during the Civil War (Dual Banking System pp. 5-11).

[7] Federal and state regulations converged on many other dimensions with the passage of the Federal Reserve Act in 1913 and amendments to the act over the next few years. These dimensions included reserve requirements, the types and quantities of loans that they could make, and fiduciary powers. Differences remained, of course. All national banks, for example, were required to become members of the Federal Reserve System, while state-chartered banks were given the choice, and most chose not to join. Member banks could directly access the Federal Reserve's discount window. Non-member banks could only access this liquidity indirectly, by discounting their loans with a correspondent bank which would in turn rediscount the loan with the Fed. The Fed's discount window was designed to create an "elastic currency." That turn-of-the-century phrase today means the discount window was designed to

of banks and trust companies (Federal Reserve Board of Governors 1932, p. 11)." Prior to the creation of state-supervisory agencies, regulation of state-chartered banks was left to depositors and courts. Depositors shifted funds from banks they deemed too risky and sued banks that failed. If bank failed to pay its obligations, depositors could force it into receivership. Depositors did this by filing suits in state courts. After ascertaining the existence of unpaid debts, the judge would take possession of the bank and all its assets and appoint a receiver to supervise the liquidation of the corporation and repay its claimants.

About half of all states established bank regulatory agencies in the nineteenth century. The rest established them between 1900 and the mid-1920s (Mitchener and Jaremski 2015, pp. 853-4). Seventeen did so in the wake of the panic of 1907. These supervisory agencies, according to Mitchener and Jaremski, "rose from the ashes of banking crises (2015, p. 820)."

In states with supervisory agencies, the agency typically received responsibility for bank resolution, particularly after the Bankruptcy Act of 1898 excluded suits about the issue from federal courts. Bank superintendents and their staffs routinely examined banks, determined which banks were or might become insolvent, decided whether those banks could be saved by remedial action or whether they should be shut down to protect depositors' and the publics' interests. Superintendents also determined whether banks suspended payments to depositors should be liquidated or allowed time to fix their finances. Data from the late 1920s and early 1930s shows that regulators initiated most bank closures, except during banking panics, when the majority of suspensions began with a bank's directors voting to suspend payments. No bank resolutions began with suits by depositors (Richardson 2008, Mitchener and Richardson 2019).

loan funds to financial institutions beset by liquidity shocks. The goal was to limit seasonal and cyclical stringencies of credit and cash. It was observed at the time and has been reported since that the Fed's creation reduced seasonal fluctuations in interest rates (cite Fed annual report, Friedman and Schwartz, Mishkin). It has also been observed that the incidence of local liquidity shocks (i.e. banking panics) changed little if at all from the 1910s to the 1930s. The transition of these local shocks to economic activity has not been studied.

Quarterly failure rates for state-chartered banks in the 48 contiguous states exhibited striking patterns, which are displayed in Table 1 and Figure 1. From 1902 to 1929, rates averaged 2.37 failures per thousand banks. The median failure rate, however, was zero. No banks failed in the majority of quarters in the majority of states. The distribution exhibited extreme kurtosis, with a predominant peak at zero, and was extremely skewed. The failure rate, skewness, and kurtosis varied seasonally. Rates were lower in the spring and summer and higher in the fall and winter. Very high rates, which were indicators of banking panics, were most common in the fourth quarter, but also relatively frequent in the third quarter, which accounts for the extreme skewness in the distribution at that time. Rates varied with the business cycle, rising during recessions in 1908-8, 1913-4, and 1921 and falling during expansions. Rates increased over time. In the 1900s, the quarterly state bank failure rate averaged 1.4 per thousand (with interquartile range 0 to 1.16). In the 1910s, the quarterly state bank failure rate averaged 1.1 per thousand (interquartile range 0 to 0.6). In the 1920s, the quarterly state bank failure rate averaged 4.4 per thousand with iqr = (0, 3.9).

During these decades, all banks operated within the borders of a single state, and almost always, within a single city.[8] Fluctuations in bank failure rates reflected ebbs and flows of economic activity as well as regulatory decisions within states. After a state created a bank regulatory agency, laws typically forbid depositors from "litigation which would likely result in [a banks] premature dissolution (Tardy 1920, p. 1231)." Instead,

> laws regarding banking institutions generally provide a system of legal or administrative procedure under which an institution which is insolvent or is being improperly conducted may be given an opportunity to rehabilitate itself or remove the cause for criticism in its management before being subjected to liquidation (Tardy 1920, p. 1231).

[8] In 1900 in the United States, 87 banks possessed a total of 119 branches. The number branches grew gradually, initially for state-chartered, and after the passage of the McFadden Act, also for nationally chartered banks. In 1930, 750 banks possessed a total of 3,518 branches. Two-thirds of these were in the same city as the head office. All were in the same state as the head office, since interstate branching remained prohibited (Branch Banking, Table 2, p. 6).

Creditors, in other words, could not force a bank into bankruptcy by suing in court. Only regulators had the authority to initiate bank resolution. Regulators had a great deal of discretion. Regulators were supposed to give banks opportunities to correct problems if possible before shutting them down. When regulators detected problems, they warned banks of their findings and encouraged them to fix the issues. When necessary, regulators could take stronger actions, such as recommending a merger, removing a bank's management, or compelling shareholders to contribute additional capital. Liquidating a bank was a last resort. Regulators were compelled to act aggressively only depositors faced imminent losses and when delay threated to increase the problem.

2.3 Political Influence and Bank Supervision

The nature of bank resolution, which provided regulators with broad discretion over when to close banks, provided opportunities for state regulators and the politicians that supervised them to use that discretion to satisfy a variety of objectives. Governors had good reasons for encouraging regulators to use their discretion to lower bank failure rates during election campaigns. Incumbents received more votes during economic booms and fewer votes during busts. Bank failures served as signals of economic distress and were widely noted. Almost all bank failures – no matter how small – were reported in national newspapers of record, like the New York Times and Wall Street Journal, as well as local newspapers (Jalil 2015; Wicker ***). Bank failures often triggered legal investigations leading to civil suits against stockholders and indictments and prosecutions of senior executives. These suits generated continuing streams of news accounts (Koch, Richardson, Van Horn 2020). Governors and the bank superintendents that worked for them were often blamed for bank failures. Partisan political accusations about failure to properly supervise banks were prominently covered in the popular press. In New York, this issue recurred regularly, with the governor and his bank superintendent being accused of laxity or

incompetence in the years 1906, 1911, 1920, 1929, and 1931 (New York Tribune 1906; New York Herald 1929; New York Times 1911 and 1920; and 1930, Richardson and Van Horn ****). Other governors in New York touted effective supervision of banks and passage of stronger banking laws as accomplishments worthy of reelection (New York Times 1914). Similar news coverage appeared in many other states, including Pennsylvania, Massachusetts, Michigan, Illinois, Georgia, North Carolina, Tennessee, Ohio, Kansas, and Alaska (Arizona Republican 1922; Baltimore Sun 1906, 1923, and 1925; San Francisco Chronicle 1907; Chicago Tribune 1906; Atlanta Constitution 1923; Cincinnati Enquirer 1908).

Substantial evidence details governors' interventions to aid well-known banks. A notorious example is Governor Henry Horton's assistance of the Bank of Tennessee, the largest commercial bank in the state and the institution at the center of the Caldwell financial empire (McFerrin 1969). As its financial difficulties grew during his gubernatorial reelection campaign in 1930, Horton directed state officials to deposit millions of dollars of state funds in the bank and encouraged regulators to allow it to continue operations, ostensibly while seeking a merger partner. Four days after Horton won reelection, regulators seized the bank and declared it insolvent. Horton's political opponents assailed him for his actions. A motion to impeach him failed. He served out his term, and then left public service.

Another series of examples comes from Oklahoma. In 1910, the state's first governor, Charles Haskell, was accused of directing state officials to delay actions against the Columbia Bank and Trust Company, which ultimately failed with large losses to the public. The legislature investigated the issue and considered impeachment, although they eventually found no cause for legal action. Newspapers nationwide covered the accusations during his reelection campaign, which was ultimately unsuccessful (Atlanta Constitution, 1 February 1910, p. 3; Minneapolis Morning Tribune, 3 May 1910, p. 2; Nashville American, 1 February 1910, p.1; New York Tribune, 1 February 1910, p. 3; New York Times, 1 February

1910, p. 2; San Francisco Examiner, 1 February 1910, p. 5; Philadelphia Inquirer, 1 May 1910, p.1; Daily Phoenix, 27 September 1910).

During his reelection campaign in 1922, Oklahoma's fourth governor, James Robertson, a Democrat, was accused of allowing the Guaranty State Bank of Okmulgee to operate in an insolvent condition. The Republican-dominated legislature investigated. Robertson was indicted along with Fred Dennis, the State Banking Commissioner, of accepting a bribe of $25,000 in return for regulatory discretion (New York Tribune (from the Associated Press), 23 March 1922, p.1; Washington Post, 6 July 1922, p. 3). James Hepburn, the Attorney for Okmulgee County, who brought the case, accused the Governor of bribing a district judge to grant a change of venue and of paroling a convicted murder so the convict could kill the prosecutor and derail the case (New York Times, 20 November 1922, p. 17; Austin Statesman, 20 November 1922, p. 1 and 3 December 1922, p. 2). The Governor retorted that the accusations were political smears advanced by his Republican opponents and the Ku Klux Klan, who opposed his reelection. Robertson was cleared of the charges. Courts found the he and bank commissioner did not accept remuneration for their official acts, but Robertson lost the election.

These examples were widely reported because the press reveled in their salacious details. Wide reporting makes them easy to document. Most cases of regulatory discretion left less evidence in extant sources, but their symptoms can be observed in the statistics presented in Table 3. For states with a bank regulatory agency whose chief executive was subordinate to the governor, the table indicates the number of quarters that the state bank failure rate fell in the specified ranges during quarters with elections, election campaigns, or neither elections nor campaigns from 1902 to 1929. The top row, for example, indicates that the bank failure rate was zero in 312 of the 442 quarters with campaigns (70.6%) and 430 of the 694 quarters with neither elections nor campaigns (62.0%). No banks failed, in other words, in 8.6% more quarters with campaigns than without excluding quarters with elections from the comparison,

because those quarters mix periods before and after the election. This result is for the second quarter, April through June. We separate the data by quarters due to the seasonality of bank failures, elections, and campaigns. The separation helps to distinguish the impact of campaigns from seasonal factors influencing bank failure rates.

Figure 2 illustrates the issue. It depicts the difference between state-bank failure rates in quarters with and without election campaigns (excluding quarters with elections) for the four quarters of the year. During the second quarter of the year (April, May, and June), for example, the share of observations with no failures (i.e. failure rate of zero) was 8.6 percentage points higher for quarters with campaigns compared to quarters without campaigns, while the share of observations with failure rate between 0% and 0.5% was 6 percentage points lower. A similar shift occurred in all seasons of the year. Zero failure rates became more common. Positive failure rates became less common, with the decline concentrated at failure rates less than 1%. The share of observations with failure rates above 5% – a rate consistent with a financial panic – changed little.

A back of the envelope calculation helps to illustrate the implications of the shift in failure rates during gubernatorial elections. From 1902 to 1929, gubernatorial election campaigns spanned 898 of the 5,376 quarters in the data. The data in Table 3 and Figure 2 indicate that zero failures occurred in just over 10% more of these quarters than comparable quarters without campaigns or elections. Gubernatorial elections, in other words, resulted in 90 more than expected quarters with zero bank failures. Without the campaigns, failure rates would have been between 0 and 5 per thousand in 39 additional quarters, between 5 and 10 per thousand in 24 additional quarters, between 10 and 20 per thousand in 13 additional quarters, between 20 and 50 per thousand in 13 additional quarters, and over 50 per thousand in 1 additional quarter. Multiplying the midpoints of those ranges with the average number of banks per state in 1919 and summing the results indicates that regulatory discretion during gubernatorial campaigns

delayed the failure of about 400 banks. Completing the calculation requires us to recall three additional facts and make one assumption. The facts are that the typical gubernatorial campaign spanned about two quarters. No banks failed in 6 of 10 typical quarters without campaigns or elections. Roughly 15% of gubernatorial campaigns in the Progressive Era occurred in states without an agency that supervised state-chartered banks. The assumption is regulators deferred bank failures in only one of the two quarters of a campaign.[9] Combining these facts yields our back-of-the-envelope result.[10] In one third of all gubernatorial campaigns in the Progressive Era in which financial problems pushed banks towards insolvency, regulators working for governors delayed the failure of 3 or 4 banks by about 6 months to prevent bad financial news from weakening their chances at the polls.

These patterns in the raw data are consistent with a story reiterated in historical sources. Governors appointed leaders of agencies that regulated banks. These appointees had discretion about how to handle troubled banks. The law encouraged regulators to extend time to banks trying to correct deficiencies. During gubernatorial elections, regulators provided more time than usual. This delay limited bad economic news released in the run up to the election, improving the incumbent's chances.

This story is consistent with several facts. First, the shift of bank failure rates to zero that occurred during campaigns in states with bank regulatory agencies did not occur in states without them. In those states, where the timing of bank failures was determined by courts and depositors rather than by subordinates of the governor, gubernatorial elections were not correlated with shifts in bank failure rates. Second, shifts of failure rates towards zero before gubernatorial elections preceded shifts of failure rates

[9] This assumption makes sense because bank failures occurred in under than half of all quarters. So, governors and their subordinates would have the need or opportunity to defer banks failures in a little less than half of all quarters.
[10] In the Progressive Era, 449 gubernatorial campaigns occurred in states with a bank regulatory agency. We assume that each campaign spanned two quarters. Zero failures occurred in 64% of quarters. Assuming the bank failure rate was independent across quarters, then without gubernatorial intervention, approximately 41% of campaigns would have experienced no bank failures, 46% of campaigns would have experienced bank failures in only one quarter, and 13% of campaigns would have experienced bank failures during both quarters. The number of campaigns when regulatory discretion could have lowered bank failure rates equals 265 (i.e. 449*[0.13+0.46]). The fraction of these campaigns in which governors intervened is 90 divided by 265, which is just over 1 in 3.

from zero after elections. This rebound is particularly noticeable in 1930, where high-frequency data on bank suspensions reveals large increases in the number and rate of banks failures following gubernatorial elections in the fall of 1930 (Wicker 1996, Richardson 2008). Third, the shift toward zero during campaigns only occurred for state banks during gubernatorial election campaigns. It did not occur during campaigns for state offices other than governor (e.g. state senate) or for federal offices (President and House of Representatives) that did not coincide with gubernatorial elections, and it did not occur for national banks.

The last point was noted by researchers past and present. The research Committee on Branch, Group, and Chain banking wrote "there was also a greater possibility of direct political influence under State than under national charters (Board of Governors 1932, p. 11)." Tardy (1920) noted that states modelled their bank resolution procedures on the methods pioneered by the Comptroller of Currency, but states' outcomes differed from those of the OCC because their regulations were looser, and their enforcement was laxer. State governments understood that easy regulations were one of the main reasons that banks chose state charters and paid state banking taxes. The Comptroller of the Currency had more political independence, tougher regulations to enforce, and a tradition of strict enforcement (Board of Governors 1932, Mitchener ****). This difference remains true today (Sharma 2018).

3. Gubernatorial Elections and Bank Failures

Bank resolution rates and gubernatorial elections are clearly correlated in states with bank regulatory agencies subordinate to the governor. To control for factors which could influence this correlation, we turn to statistical analysis. The data shape our choice of statistical method. Bank resolution rates range are naturally bounded in [0,1]. The most common rate is 0. The maximum is 0.21. The distribution's skewness and kurtosis shift across over time and across seasons. At

least one factor, elections, strongly influences whether the rate is zero, but may have little influence on higher rates. It may be, for example, when governors pressured regulators to delay bad news, their subordinates reduced resolution rates to zero, but when governors did not exert this pressure, regulators did not alter their actions, and resolution rates were determined by the factors that shape them in normal times.

The appropriate distribution for analyzing this data is a zero-inflated beta distribution (ZOIB). ZOIB is a piecewise distribution that accounts for the probability mass at 0 in addition to the probability density on the open unit interval (0, 1). It is estimated as a discrete-continuous mixture model. The probability mass is estimated using a limited-dependent variable model, in our case a probit. The continuous proportion is estimated using a beta distribution, since its density has a wide range of different shapes depending on the values of the two parameters that index the shape of the distribution. We estimate the zero-inflated beta model Ospina and Ferrari (2011) using the maximum-likelihood procedures of Buis (2012). We cluster the standard errors at year-quarter level to control for within-time correlations.

Results appear in Table 4. Column 1 estimates the ZOIB model on the data underlying Figure 2. The first column, in other words, excludes observations for quarter when a state lacks a bank regulatory agency and is having an election. The dependent variable is the bank resolution rate for state-chartered banks in state *i* in quarter *t*. The sole explanatory variable is an indicator for election campaigns. The columns' top half indicates results for the probit predicting whether the failure rate is zero. In this case, the dependent variable equals 1 if the failure rate equals 0. A positive coefficient indicates that resolution rates of zero occur more often during quarters with campaigns than other quarters. To understand how much more common, we calculate the marginal effect. It is 0.10 or 10%. This result is almost identical to our ball-park calculation from

the raw data.[11] The decline in the number of bank resolutions implied by this estimate can be calculated by multiplying the number of quarters shifted to zero (in this case 90) with the average number of banks failing in states with regulatory agencies during typical quarters (~3.2), for a total of 286. The column's bottom half indicates the results for the beta regression on the bank resolution rate. The marginal effect indicates the average change during campaigns in the state-bank resolution rates for observations which were not zero. The decline in the number of bank resolutions during campaigns implied by this estimate can be calculated by multiplying the marginal effects' absolute value (0.0007), the average number of banks in each state (454 in July 1919), and the number of campaigns (898). The result is 285. This number can be added to number of deferred resolutions implied by the equation for zero, 286, to arrive at total deferred resolutions due to campaigns of 571.

Column 2 estimates the zero-inflated beta model on a broader set of observations including those for states with and without a bank regulatory agency. The only excluded observations are for quarters containing elections. The explanatory variables now include three indicators. The first (labelled *Agency*) equals 1 during quarters when a state has a bank regulatory agency and zero otherwise. The second (labelled *Campaign*) equals 1 during quarters of election campaigns. The third (labeled *Campaign*Agency*) is the interaction of the first and second. It equals 1 when election campaigns occurred in states with a bank regulatory agency and zero otherwise. Now, the marginal effect for *Campaign*Agency* reveals the impact of a campaign in states with a bank regulatory agency. The marginal effect for *Campaign* reveals the impact of a campaign in a state without a bank regulatory agency. The marginal effect of *Agency* reveals the impact of creating a bank regulatory agency. After states create agencies, failure rates

[11] The predicted impact is the marginal effect, 0.1, multiplied by the number of observations with campaigns, 898, or 0.1 * 898 ≈ 90. Our ballpark estimate based on the raw data was 90.

of zero become less common. This is expected, because regulatory agencies periodically examined banks and were supposed to compel institutions in which they detected problems to reform or liquidate. The hope was to smooth failure rates over time, which would reduce the number of quarters with zero failures but also reduce quarters with high failure rates and/or banking panics. The marginal effect for the rate regression indicates that regulatory agencies helped to tame high failure rates, with the average non-zero resolution rate declining substantially.

State, year, and quarter fixed effects are added in Column 3. State fixed-effects control for average differences across states. Year fixed-effects control for trend and cyclical differences over time. Quarterly fixed effects control for seasonal factors. Our key coefficient is on the interaction term *Campaign*Authority*. It indicates how gubernatorial elections influenced resolution rates in states with regulatory agencies subordinate to the governor. The marginal effect in the equation for zero, 0.11, resembles that in our baseline, 0.10, as well as our ballpark estimate from the raw data. The marginal effect in the equation for rate is near zero and statistically insignificant. During campaigns, in other words, regulators subordinate to governors deferred resolutions of banks in a way which increased the number of quarters with no bank failures (i.e. rate equals zero) but did not change the mean resolution rate when failures occurred. Since the distribution of rates is skewed, its mean is strongly influenced by the high rates in its tail. These tail rates, like 2%, 5%, or 10% per quarter, typically occurred during crises.

A reasonable interpretation of this result is that during campaigns regulators subordinate to governors deferred resolutions of small numbers of banks on the margin of failure whose deferral would appear reasonable when challenged, would not raise the hackles of the press, and would not provide grounds for investigations by political opponents. These deferrals would

reduce bad news – and bank failures were always treated as bad news – in the popular press. Regulators did not defer resolutions when failure rates where high, like during financial panic, because these events would be noted regardless of regulators' actions.

The result for *Agency* is also worth noting. In the equation for rates, the marginal effect remains stable and significant. Regulatory agencies, in other words, reduced resolution rates at points in time when banks experienced distress. This reduction stems largely from a decline in quarters with very high resolution rates, or in other words, a reduction in the frequency of crises or panics. In the equation for zero, the marginal effect approaches 0. The null hypothesis that it equals zero cannot be rejected. Regulatory agencies, in other words, did not appear to influence the fraction of quarters when no banks failed. The change in the coefficient from a statistically significant -0.14 in Column 2 to an insignificant 0.02 in Column 3 is due entirely to the inclusion of time fixed effects. The fraction of resolution rates that equaled zero each year was falling over time. The fraction of states that bank regulatory agencies was rising over time. Time fixed effects control for these trends, but do not explain them. It remains possible, therefore, that regulatory agencies influenced the frequency at which resolution rates equaled zero, if regulatory agencies generated the trend over time. Our results indicate, however, that the adoption of regulatory agencies did not coincide with a substantial change in the rate of zeros in the year of adoption.

The next three columns include addition observations and explanatory variables. Column 4 adds to the data observations with elections and to the regression an indicator for election and its interaction with the indicator for regulatory agencies. Column 5 adds to our regression a clearly exogenous variable, the quarterly average of the Palmer Drought Index for each state, which controls for climatic changes that impacted crop yields and industrial activity.[12] Column 6

[12] It's obvious that precipitation impacted crop yields in this highly agricultural economy where farmers comprised half of the labor force. Climate also impacted industry, in part because much industry processed agricultural

adds to the regression a vector of variables that includes lags of the business failure rate, the farm failure rate, and the average interest rate. While bank resolutions clearly impacted current and future business and farm failure rates, lags of these variables are plausibly exogenous. The key conclusion drawn from the addition of this information is the stability of our regression results. The coefficients and marginal effects for *Campaign*Agency* change little when we include this additional information. In the equation for zero, the marginal effect of *Campaign*Agency is stable around 0.12*. In the equation for rates, the marginal effect falls slightly, but is not statistically significant. Statistical tests, however, do not reject the null hypothesis that the marginal effects are the same in Columns 3 through 6.

Placebo tests fill the rest of the table. Columns 7 and 8 run the specifications in Columns 4 and 6 but change the dependent variable from the state bank to the national bank resolution rate. Estimated marginal effects are small, statistically insignificant, and in the equation for zero, the opposite sign of those in our original regressions. These results indicate that national bank resolution rates did not change during gubernatorial elections. This fact indicates that broad changes in economic conditions during campaigns did not generate correlations between campaigns and state bank resolution rates, because economic conditions that helped or harmed national banks also helped or harmed state banks. If changes in conditions generated the result for state banks, therefore, they should generate a similar result for national banks. This fact also supports our contention that regulators' actions altered state bank resolution rated during gubernatorial campaigns. State regulators had discretion over the timing of state bank resolutions, but no authority over national bank resolutions.

commodities, like grain and dairy products, and in part by influencing transportation costs and power generation, with for example, deep snows in the winter, low water levels in canals, sweltering heats in the summer (pre-airconditioning)

The last two columns buttress this result. Columns 9 and 10 run the specifications in Columns 4 and 6 but change the campaign indicator from gubernatorial to presidential, excluding of course, national presidential campaigns that coincided with gubernatorial contests. Estimated marginal effects are near zero, statistically insignificant, and the opposite sign of our earlier regressions. Resolution rates for state-chartered banks were clearly not influenced by national presidential elections. This observation is consistent with our conjecture that state bank resolution rates shifted towards zero during gubernatorial campaigns because the governor wanted to suppress news that might reduce his party's performance at the polls and inconsistent with possible alternative explanations related to national politics or economic conditions. This makes sense. The President had no authority over state bank regulators and limited authority over the Comptroller of Currency, who in the nineteenth century was the only employee of a federal agency that President could not fire without consent of the Senate. The federal government at the time had limited abilities to influence economic activity in individual states for short periods of time. Relative to today, federal government spending was lower and the appropriations process was slower.

While Table 4 provides strong evidence that regulators deferred resolutions during gubernatorial elections, alternative specifications should be considered for two reasons. One is that the zero-inflated beta model does not take into account the panel structure of our data. Alternative estimators that exploit this feature of the data might return more precise results. Two is that the zero-inflated beta distribution is a mixture model that combines two non-linear estimators, probit and beta. We would like to determine how gubernatorial-election-inspired shifts in bank resolution rates impacted business activity, particularly business failure rates. There is an obvious approach to this issue. Use gubernatorial elections as an instrument for bank

resolution rates in a regression that would serve as the first stage in a two-stage instrumental variables regression where the second stage estimated the relationship between the bank resolution and business failure rates. Due to its non-linearity, however, a zero-inflated beta regression cannot serve as the first stage since the results of the second stage would be biased. This bias is akin to the infamous forbidden IV regression. Linear regressions that are closely equivalent to our ZOIB, however, can serve as an unbiased first stage in an IV. In addition, non-linear models that are closely equivalent to ZOIB can serve as the first stage of a control function examination of this issue. Regressions of these types appear in Table 5.

The initial four columns of Table 5 address the first issue. They incorporate the panel structure of the data into our estimates by transforming the dependent variable from the interval [0,1] to the pair {0,1}. The dependent variable in all four regressions equals 1 for quarters when no banks fail (i.e. failure rate equals zero) and zero otherwise. Columns 1 and 2 present a random effect panel probit. Columns 3 and 4 present a fixed effect panel logit. The specification in the odd-numbered columns is identical to Table 4, Column 4. The specification in the even-numbered columns is identical to Table 4, Column 6. The regressions are variants of the equation for zeros in our ZOIB. The advantage of the new regressions is that they incorporate into the analysis information about the panel structure of the data. The cost is that they disregard information about how far resolution rates are above zero. In these regressions, the marginal effect of *Campaign*Authority* ranges from 0.12 to 0.16, which overlaps with the range 0.12 to 0.14 for the equation on zero in Table 4. The two types of regressions, in other words, yield equivalent results.

The remaining columns of Table 5 address the second issue. Subgroups of columns begin with specifications similar to the ZOIB model and end with a specification that serves as the first

stage of an unbiased two-stage estimation procedure. Columns 5 and 6 report probits similar to the ZOIB's equation for zeros. Their marginal effects span the same range as our ZOIB specifications. Column 7 modifies those regression by excluding observations for states without a regulatory agency, replacing state fixed effects with each state's average Palmer drought index and farm failure rate, and excluding lagged or potentially endogenous variables including the business failure rate. These modifications allow the regression to serve as the first stage of a two-stage control function estimate. In this specification, the marginal effect for *Campaign* indicates the impact of gubernatorial campaigns' impact on the likelihood of a resolution rate of zero is indicated by which is 0.07, or about half that in most other specifications.

Fractional logit specifications similar to the ZOIB's equation for rate appear in columns 8 and 9. We estimate these using the method of Papke and Wooldridge (1998). The marginal effect for *Campaign*Agency* indicates the change in the average resolution rate during campaigns in states with regulatory agencies. The absolute value of the marginal effects (-0.0013 & -0.0015) are larger than those in the corresponding ZOIB equation for rates (0.0002 & 0.0002), because they incorporate the impact of shifting rates to zero, which was captured by the equation for zero in ZOIB, as well as the impact of lowering resolution rates in nonzero observations, which was captured in ZOIB's equation for rates.

Ordinary least squares estimates appear in the remaining columns. Columns 10 to 11 report specifications like those in columns 8 and 9. The magnitude of the marginal effects declines slightly, which is expected, since the skewness of the data and bunching at zero biases the OLS result towards zero, but the marginal effect remains large and statistically significant. Since these estimates are linear, they can serve as the first stage of an IV. Column 12 modifies these regression by excluding observations for states without a regulatory agency, replacing state

fixed effects with each state's average Palmer drought index and farm failure rate, and excluding lagged or potentially endogenous variables including the business failure rate. These modifications allow the regression to serve as the initial stage of a control function estimate which examines how exogenous changes in bank failure rates influenced business failure rates. While the coefficient on Campaign in this regression may be biased towards zero, it remains substantial and statistically significant.

Overall, the estimates in Table 4 and 5 reveal a robust relationship between gubernatorial campaigns and resolution rates in states with regulatory agencies subordinate to their governor. The marginal effects, however, may be difficult to compare across specifications, because they are stated in different metrics – either as the fraction of additional observations with resolution rate equal to zero or as the decline in the average resolution rate – and because they apply to different sets of observations. The rows labelled "Implied Decline in Resolutions During Gubernatorial Elections" in Table 5 report the estimates on a standard metric, which is the decline in the number of bank resolutions during gubernatorial campaigns implied by the estimates. The implied number for the shift to zero, I_{zero}, is:

$$I_{zero} = \beta_{zero} * C * F$$

β_{zero} is the marginal effect in the ZOIB equation for zero or a limited dependent variable model estimating whether the resolution rate equals zero. C is the number of quarters spanned by gubernatorial campaigns in states with bank regulatory agencies, which equals 898. F is the average number of failures in non-campaign quarters in states with bank regulatory agencies, which is just over 3.18. The implied number for the decline in the rate, I_{rate}, is:

$$I_{rate} = \beta_{rate} * C * A * P$$

β_{rate} is the marginal effect in the ZOIB equation for rate or a regression model estimating the resolution rate. A is the average number of state banks in operation in a state, which equals 454, or the number of state-chartered banks per state in July 1919. P is the proportion of campaigns to which the estimate applies. P equals 1 - β_{zero} for ZOIB estimates and 1 otherwise. Since C, F, A, and P are constants, the same formulas yield the standard errors of the implied number of failures.

The implied decline in resolutions due to gubernatorial elections sends a clear signal. The implied decline for our bottom-line ZOIB specification (Table 4, Column 6) is 471. We cannot reject the null hypothesis that this estimate equals the estimates for the same specification estimated via alternative methods. We can only reject this null for two specifications (Table 5, Columns 7 and 12) designed to serve as the initial stage of a control function estimate of the impact of bank failures on business failures. In both cases, the implied impact of gubernatorial elections is lower than our ZOIB estimate.

4. Bank Failures and Business Failures

How did declines in bank resolutions during gubernatorial campaigns influence economic activity, particularly business bankruptcy rates, which are the only economic indicators that can be continuously measured by state and quarter throughout the Progressive Era? Resolutions and bankruptcies are endogenous and interrelated. Determining how the former influenced the latter requires an instrument which possesses three properties. One, it must be exogenous or equivalent to randomly assigned. Two, it must not have directly influenced business bankruptcy rates, or in other words, it must satisfy the exclusion restriction. Three, it must have directly influenced resolution rates, or in other words, it must have a substantive first stage.

Gubernatorial campaigns satisfy all three requirements for an effective IV. First, elections' timing was exogenous. The electoral schedule was fixed and established long in the past. Second, governors and their subordinates could not dictate timing of business bankruptcies. Unpaid creditors and federal courts did that. Governors lacked fiscal and legal tools which would allow them to manipulate business bankruptcy rates over a short spans of time, such as the three to six months of a progressive era political campaign, and during specific circumstances, such as only in states where governors' subordinates controlled bank regulation and only when bank resolution rates exceeded zero and were below a threshold. So, gubernatorial campaigns satisfy the exclusion restriction. Third, bank resolution rates fell to zero in a substantial fraction of gubernatorial campaigns. Quantitative and qualitative evidence presented in previous sections corroborates that conclusion concerning the first stage.

The next requirement for an effective IV is a relationship between the ultimate outcome of interest – in this case, bankruptcy rates – and the instrument. The regression that establishes this relationship, which is presented in Table 6, is called the reduced form. The initial columns present ordinary least squares (OLS) estimates. Column 1 contains the baseline. The business failure rate for each state in each quarter is regressed on a constant and the instrument, which is gubernatorial campaigns in states with bank regulatory agencies, or in our statistical set up, the interaction term *Campaign*Agency*. The coefficient indicates that during campaigns, the business failure rate was 0.0003 (or 3 per ten thousand) lower than the average of 0.0024 (or 24 per ten thousand). Column 2 adds state fixed effects, time fixed effects (which control for trends and seasonality), the Palmer drought index and its square, and four lags of the Palmer drought index and its square. These drought index controls for exogenous shocks to weather and crop yields that influenced economic activity in the early twentieth century. The coefficient on gubernatorial campaigns remains significant and substantial, although it falls to 0.0001 (or 1 per ten thousand). Columns 3 and 4 present results for the same specification as Column

2. Column 3 restricts the sample to observations with a bank regulatory agency. The result is similar to that in Column 2. Column 4 restricts the sample to observations that do not have regulatory agencies. The coefficient is close to zero, and the hypothesis that it equals zero cannot be rejected. When states lacked banking regulatory agencies, in other words, gubernatorial elections were uncorrelated with bankruptcies of firms. When states had banking regulatory agencies, business bankruptcies fell during gubernatorial elections.

These OLS regressions could be biased, because the business failure rate is bounded between 0 and 1. To account for this fact, the right-hand columns estimate the initial columns using the fractional logit model of Papke and Wooldridge (1998). The marginal effects from these non-linear estimates have the same sign, magnitude, and significance level as OLS estimates in the initial columns. Their similarity indicates that the reduced form regression (and the second stage of our IV) is approximately linear. Estimating it using OLS does not bias the results.

If our first and second stages were linear, our IV estimate would be the coefficient for the interaction *Campaign*Agency* in the reduced form divided by the coefficient for campaigns in the first stage. Complications arise because both stages of our analysis are non-linear. So, results from standard instrumental variables regressions could be biased. Credible causal inference in this setting is an active area of research. The current literature suggests several possibilities. Recommended procedures include (a) two-stage control function estimation and (b) standard IV estimating employing a linear and potentially less informative first stage which yields a consistent two-stage procedure. We employ both and recover similar results.

We begin by describing the control function approach of Wooldridge (2008, 2015), which provides unbiased inference in situations like ours. Control functions work with both continuous and discrete endogenous explanatory variables (EEVs). The bank resolution rate, R_{it},

is a continuous EEV, where $R_{it} \in [0,1]$, *i* indicates state, and *t* indicates time. Whether the resolution rate equals zero is a discrete EEV, where B_{it}=1 if R_{it}=0 and is 0 otherwise. The procedure consists of two steps. In the first, we regress the EEV on the instrument, $Campaign_{it}$; a vector, γ_{it}, that includes year and season fixed effects as well as an indicator for quarters with elections; and state-level averages of exogenous explanatory variables, $\bar{X}_i$. The latter includes the average Palmer drought index and average farm failure rate for each state. The inclusion of time averages of exogenous variables follows from the Mundlak device (1978) to control for time constant heterogeneity. The first stage equation is:

$$(1) \qquad Y_{it} = \phi(\beta Campaign_{it} + \varphi \bar{X}_i + \gamma_{it})$$

Where the EEV Y_{it} is either B_{it} or R_{it}. $\phi(\cdot)$ is a linear function for the continuous EEV and the standard normal cumulative distribution function for the discrete EEV. From this regression, we obtain the residuals, $\widehat{Y_{it}}$. In the second step, we estimate a fractional pooled probit of the bankruptcy rate, F_{it}, on Y_{it}, $\widehat{Y_{it}}$, $\bar{X}_i$, and the time fixed effects. The second stage equation is:

$$F_{it} = \psi\left(\vartheta_0 Y_{it} + \vartheta_1 \widehat{Y_{it}} + \tau \bar{X}_i + \gamma_t + \theta_t\right) \quad .$$

A test for endogeneity of Yit is obtained by testing the null hypothesis that $\vartheta_1 = 0$. Under the null hypothesis Y_{it} is exogenous, so there is no need to take into account the first stage estimation of the residuals. We compute average partial effects of R_{it}, AR, with the following formula:

$$\text{AR} = \left[{}^{1}/_{NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \Phi\left(\widehat{\vartheta_0} R_{it} + \widehat{\vartheta_1} \widehat{R_{it}} + \hat{\tau} \bar{X}_i + \hat{\theta}_t\right) \times \widehat{\vartheta_0} \right]$$

To calculate average partial effects of B_{it} ,AB, we employ the following formula:

$$\text{AB} = \left[{}^{1}/_{NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \left(\Phi\left(\widehat{\vartheta_0} B_{it} + \widehat{\vartheta_1} \widehat{B_{it}} + \hat{\tau} \bar{X}_i + \hat{\theta}_t\right) - \Phi\left(\widehat{\vartheta_1} \widehat{B_{it}} + \hat{\tau} \bar{X}_i + \hat{\theta}_t\right)\right)\right]$$

Where $\Phi(\cdot)$ is the standard normal density. Standard errors of the coefficients and average partial effects are calculated by bootstrapping over states using 500 replications.

The results from this exercise appear in Table 7. Columns 1 to 4 present results for the

discrete EEV, which equals 1 when no banks fail and 0 otherwise. In Columns 1 and 2, the only explanatory variable in the first stage is an indicator for gubernatorial campaign. Columns 3 and 4 show our preferred specification that includes indicators for quarters of the year, and years as well as each state's average Palmer drought index and average farm failure rate. Odd numbered columns present results for observations with bank regulatory agencies. Even numbered columns present results for observations without regulatory agencies. The results clearly differ across columns. In states with regulatory agencies, the coefficients and partial effects for the discrete EEV are negative and statistically significant at 1% level. The average partial effect ranges from -0.0032 to -0.0035. During quarters with no bank failures (i.e. rate equals zero) the business failure rate was around 0.003 (or 3 per thousand) lower than the average of 0.0024 (or 24 per ten thousand). The control function estimates for the first stage residuals are significant, so we reject the null hypothesis that the discrete bank resolution EEV is exogenous. Columns 2 and 4 restrict the sample to observations that do not have regulatory agencies. The estimated marginal effects for the discrete EEV are statistically insignificant. When states lacked bank regulatory agencies bankruptcies of firms were not influenced by bank failures. The magnitude and significance of the partial effects do not change appreciably when time and state average controls are excluded.

Columns 5 through 8 report results for the continuous EEV, the bank resolution rate. In Columns 5 and 6, the only explanatory variable in the first stage is an indicator for gubernatorial campaign. Columns 7 and 8 add indicators for quarters of the year, and years as well as each state's average Palmer drought index and average farm failure rate. Odd numbered columns present results for observations with bank regulatory agencies. Even numbered columns present results for observations without regulatory agencies. Consistent with the results obtained when the discrete EEV is examined, the coefficients and partial effects for the bank resolution rate are only significant in states with regulatory agencies.

According to the estimates of our preferred specification in column 7, a 1% decline in the bank resolution rate in turn lowered the business bankruptcy rate by 0.26%. Lastly, the coefficients for the first stage residuals are only significant in columns 5 and 7, so we reject the null hypothesis that the bank resolution rate is exogenous. Overall, the estimates in Table 7 reveal a robust and causal relationship between resolution rates and the business bankruptcy rate.

To check the robustness of our results, we also estimate an IV via 2SLS using OLS in the first stage. This estimate is consistent but could be biased in finite samples. This method has been recommended as a reasonable approach when confronting forbidden regressions (Angrist and Pischke 2008). The first stage is:

(1) $B_{it} = \alpha_1 + \gamma Campaign_{it} + \omega_1 X_{it} + \delta_{1t} + \delta_{1i} + \varepsilon_{it}$

where X_{it} is a vector of controls including the Palmer drought severity index and the farm failure rate, δ_{1t} are time fixed effects and δ_{1i} state fixed effects. The second stage is:

(2) $F_{it} = \alpha + \varsigma B_{it} + \omega X_{it} + \delta_t + \delta_i + \epsilon_{it}$

It regresses the busines failure rate, F_{it}, on $\widehat{B_{it}}$ from the first stage and controls. Standard errors are clustered by time.

Columns 1 and 2 report results for the discrete EEV for observations with and without bank regulatory agencies, respectively. In a similar way, columns 3 and 4 report results for the continuous EEV and for states with and without a regulatory bank agency. Columns 5 and 6 report results using all the sample using as instrument the interaction *Campaign*Agency*. In all regressions we include time and state average control variables. The results of the first four columns of Table 8 show that the IV partial effects are very similar to the control function average partial effects. For instance, the average partial effect of column 3, 0.264, is of same magnitude as the control function estimate of column 7 in Table 7. As an additional test, we estimate the IV regression using all the sample and we find average

partial effects of similar magnitude when we split the sample between states with and without bank regulatory agencies. The Montiel-Pflueger effective F tests indicate that the first stages of some specifications are weak, especially for the IV regressions that exclude observations for states without a regulatory agency. Although the IV estimates are not precise, these results are consistent with the control function average partial effects.

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Private Credit Under Political Influence: Evidence from France
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Figures

Figure 1: Quarterly Failure Rates for Business and Banks, 1902 to 1929

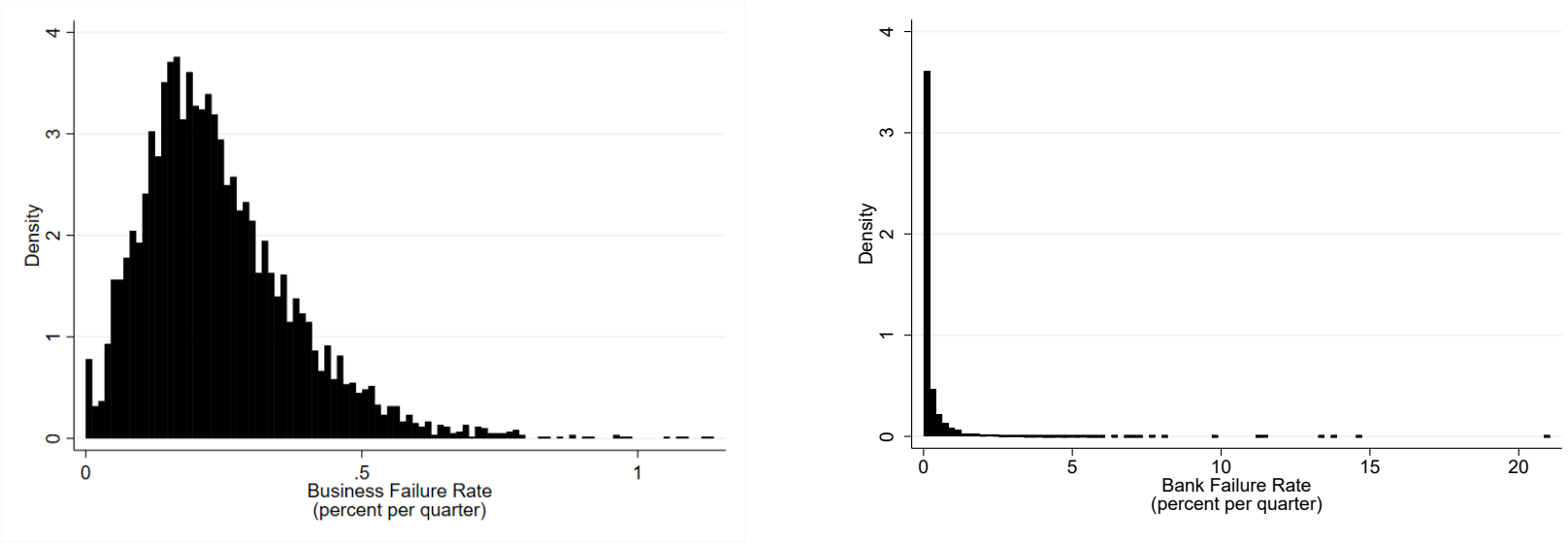
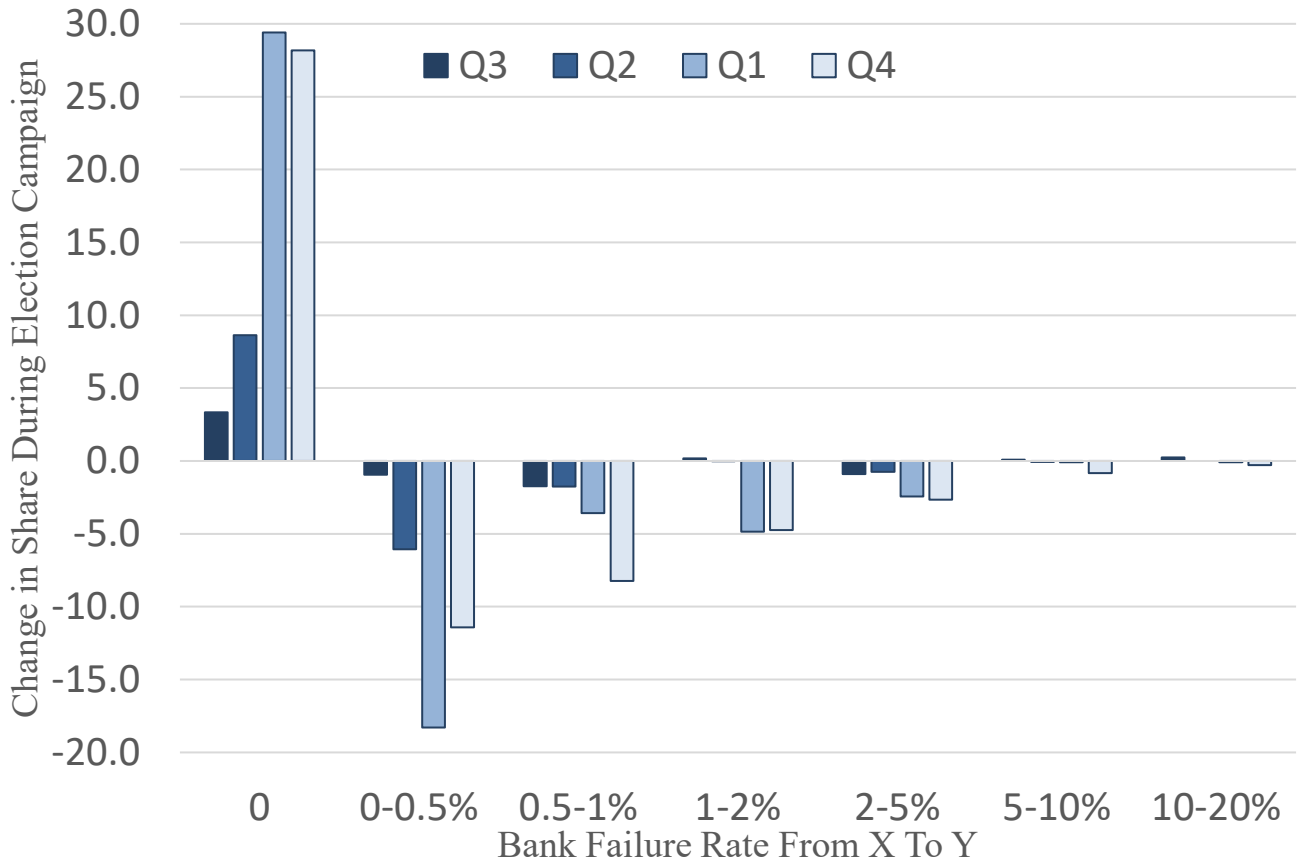


Figure 2: Failure Rates Shifted Toward Zero During Campaigns When Governors Controlled Bank Regulation



Notes: Figure plots difference in the share of state bank failure rates in each range between quarters with election campaigns and quarters with neither elections nor election campaigns. During election campaigns in the second quarter (Q2), for example, the percentage of observations with the failure rate of zero rose by 8.6 percentage points while the percent of observations with failure rate between 0% and 0.5% fell by 6 percentage points.

Tables

Table 1. Quarterly Failure Rates for Business and Banks, 1902 to 1929

	Failures Thousand Banks				Failures Per Thousand Businesses			
	Mean	Median	Skewness	Kurtosis	Mean	Median	Skewness	Kurtosis
All Quarters	2.37	0	10.58	179.24	2.39	2.16	1.22	5.82
Quarter 1	2.62	0	12.95	263.22	2.82	2.58	1.11	5.29
Quarter 2	1.91	0	6.74	66.64	2.24	2.05	1.11	5.43
Quarter 3	1.76	0	12.51	197.97	2.09	1.90	1.12	5.41
Quarter 4	3.21	0	7.22	78.06	2.41	2.22	1.23	5.90

Source: Authors' calculations. See Appendix for details.

Table 2. Numbers and Assets of Banks in the United States

	National		State	
	#	Assets ($ mil)	#	Assets ($ mil)
1900	3,731	4,944	8,696	4,115
1910	7,138	9,892	17,376	9,432
1920	8,024	23,267	22,267	24,242
1930	7,247	28,828	16,432	35,297

Source: Board of Governors (1959), Tables A-2 & A-3, pp. 38-45.

Table 3. Bank Failure Rates in States Whose Governors Control Bank Regulation, 1902 to 1929

	Failure Rate		Quarters With		
	>	≤	Campaign	Neither	Election
Apr to Jun	0	0	312	430	2
Quarter 2	0	0.005	93	188	4
	0.005	0.01	19	42	
	0.01	0.02	12	19	
	0.02	0.05	5	13	
	0.05	0.1	1	2	
	0.1	0.2			
	# Observations		442	694	6
Jul to Sep	0	0	300	476	20
Quarter 3	0	0.005	91	158	1
	0.005	0.01	18	42	1
	0.01	0.02	8	12	
	0.02	0.05	1	8	1
	0.05	0.1	1	1	
	0.1	0.2	1	2	
	# Observations		420	699	23
Oct to Dec	0	0	6	412	262
Quarter 4	0	0.005	1	184	99
	0.005	0.01		59	28
	0.01	0.02		34	15
	0.02	0.05		19	14
	0.05	0.1		6	1
	0.1	0.2		2	
	# Observations		7	716	419
Jan to Mar	0	0	26	670	
Quarter 1	0	0.005	2	280	1
	0.005	0.01	1	78	
	0.01	0.02		54	
	0.02	0.05		27	
	0.05	0.1		1	
	0.1	0.2		1	
	# Observations		29	1112	1

Notes: Entries indicate the number of quarters that state bank failure rates fell in the indicated range during quarters with elections, election campaigns, or neither elections nor campaigns from 1902 to 1929.
Source: Dun's Review (various years) and authors' calculations. See Appendix for details.

Table 4. Gubernatorial Campaigns and Bank Failure Rates, Estimates Using Zero-Inflated Beta Distributions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Failure Rate of …. Banks	State	State	State	State	State	State	National	National	State	State
Election of ….	Gov.	Gov.	Gov.	Gov.	Gov.	Gov.	Gov.	Gov.	Pres.	Pres.
Equation for Zero										
Campaign*Agency		0.94***	0.68**	0.71**	0.85***	0.86***	-0.57	-0.51	-0.05	-0.13
		(0.23)	(0.27)	(0.31)	(0.27)	(0.31)	(0.89)	(0.85)	(0.37)	(0.38)
Agency		-0.63***	-0.13	-0.09	-0.18	-0.24	-0.45	-0.53*	0.08	-0.09
		(0.11)	(0.16)	(0.15)	(0.16)	(0.15)	(0.28)	(0.31)	(0.15)	(0.16)
Campaign	0.45***	-0.49*	-0.37	-0.38	-0.38	-0.51	0.80	0.75	0.09	0.06
	(0.082)	(0.22)	(0.25)	(0.30)	(0.31)	(0.31)	(0.89)	(0.89)	(0.37)	(0.39)
Marginal Effects										
Campaign*Agency		0.21***	0.11***	0.12**	0.12**	0.14***	-0.04	-0.04	-0.01	-0.00
Agency		-0.14***	-0.02	-0.01	-0.14	-0.04	-0.03	-0.04*	0.01	-0.01
Campaign	0.10***	-0.11***	-0.06	-0.07	-0.06	-0.08	0.06	0.05	0.02	0.01
Equation for Rate										
Campaign*Agency		0.01	0.06	0.02	0.02	0.03	0.20	0.31	-0.18	-0.19*
		(0.08)	(0.11)	(0.11)	(0.10)	(0.11)	(0.29)	(0.37)	(0.11)	(0.10)
Agency		-0.13**	-0.11*	-0.11***	-0.09	-0.09*	0.15	0.13	-0.10**	-0.06
		(0.05)	(0.06)	(0.04)	(0.06)	(0.05)	(0.10)	(0.11)	(0.04)	(0.05)
Campaign	-0.11***	-0.12*	-0.14	-0.09	-0.10	-0.09	-0.30	-0.42	0.05	0.07
	(0.039)	(0.07)	(0.09)	(0.09)	(0.09)	(0.09)	(0.31)	(0.40)	(0.09)	(0.10)
Marginal Effects										
Campaign*Agency		0.0000	0.0005	0.0002	0.0002	0.0002	0.0035	0.0053	-0.0015	-0.0015
Agency		-0.0008**	-0.0009*	-0.0010**	-0.0009**	-0.0008*	0.0026	0.0022	-0.0008	-0.0004
Campaign	-0.0007***	-0.0008*	-0.0011	-0.0007	-0.0007	-0.0007	-0.0053	-0.0071	0.0004	0.0006
Implied Decline in State Bank Resolutions During Gubernatorial Elections										
Shift to Zero	286	601	315	343	343	400				
Decline in Rate	257	16	181	72	72	70				
Total	543	617	496	415	415	471				
Sample	Agency, ~E	~E	~E	All	All	All	All	All	All	All
State, Year, Quarter FE			Y	Y	Y	Y	Y	Y	Y	Y
Additional Controls					Exog	Exog+Lag		Exog+Lag		Exog+Lag
Observations		4,756	4,756	5,264	5,170	5,170	5,264	5,264	5,170	5,170

Notes: Columns estimate zero inflated beta distributions. Top equation estimates whether rate equals zero. Bottom equation estimates rates if not zero.

Dependent variable (failure rate of state or national banks) indicated in first row. Type of election (gubernatorial or presidential) indicated in second row. Sample "Agency" indicates sample restricted to observations with a state-bank regulatory agency. ~E indicates observations with elections excluded. "Exog" indicates exogenous variables, which is the Palmer drought index. "Lag" indicates lagged variables (e.g. farm failure rate, business failure rate, or interest rate). Clustered standard errors at year-quarter level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 5. Gubernatorial Campaigns and Bank Failures, Alternative Models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Model	Panel Probit Random Effect		Panel Logit Fixed Effect		Probit			Fractional Logit		OLS		
Dependent Variable: State Bank Failure	= 0	= 0	= 0	= 0	=0	= 0	=0	Rate	Rate	Rate	Rate	Rate
Coefficients												
Campaign * Agency	0.40***	0.48***	0.70***	0.85***	0.40***	0.48**		-0.55*	-0.55**	-0.0010*	-0.0010**	
	(0.14)	(0.15)	(0.26)	(0.28)	(0.19)	(0.28)		(0.30)	(0.30)	(0.0006)	(0.0005)	
Agency	-0.07	-0.14	-0.09	-0.23	-0.07	-0.14		0.07	0.18	0.0005	0.0007*	
	(0.15)	(0.14)	(0.15)	(0.16)	(0.09)	(0.09)		(0.19)	(0.19)	(0.0005)	(0.0004)	
Campaign	-0.22*	-0.28**	-0.38	-0.50*	-0.22	-0.28	0.22***	0.23	0.26	0.0005	0.0005	-0.0006**
	(0.13)	(0.14)	(0.25)	(0.26)	(0.18)	(0.18)	(0.07)	(0.25)	(0.25)	(0.0005)	(0.0005)	(0.0003)
Marginal Effect												
Campaign * Agency	0.12***	0.14***	0.14***	0.16***	0.12**	0.14**		-0.0013*	-0.0015**	-0.0010*	-0.0010*	
Agency	-0.02	-0.04	-0.02	-0.4	-0.02	-0.04		0.0002	0.0005	0.0005	0.0007*	
Campaign	-0.06	-0.08**	-0.08	-0.9*	-0.06	-0.08	0.07***	0.0005	0.0007	0.0005	0.0005	-0.0006**
Implied Decline in Resolutions During Gubernatorial Elections												
Shift to Zero	343	400	400	458	343	400	200					
Decline in Rate								530	612	408	408	245
Total	343	400	400	458	343	400	200	530	612	408	408	245
Sample	All	All	All	All	All	All	Agency	All	All	All	All	Agency
Year & Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE	Y	Y			Y	Y		Y	Y	Y	Y	
Additional Controls		Y		Y		Y			Y		Y	Y
State Averages							Y					
Observations	5,264	5,170	5,264	5,170	5,264	5,170	4,568	5,264	5,170	5,264	5,170	4,568
R-squared					0.21	0.23	0.09			0.11	0.15	0.08

Notes: Clustered standard errors at year-quarter level in parentheses . *** p<0.01, ** p<0.05, * p<0.1. Logit reports pseudo R2. Fraction logit estimated via method of Papke and Wooldridge (1998). Additional controls include indicator for election, interaction of election and campaign, Palmer drought index, and four quarterly lags of business failure rate, farm failure rate, and interest rate. State average controls include mean of Palmer drought index and farm failure rate. Sample "Agency" indicates sample restricted to observations with a state-bank regulatory agency.

Table 6. Business Failures and Gubernatorial Campaigns: Reduced Form

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				Fractional Logit			
Campaign*Agency	-0.00030***	-0.00013***			-0.129***	-0.0555***		
	(5.10e-05)	(4.31e-05)			(0.0207)	(0.0186)		
Campaign			-0.00011***	-5.40e-05			-0.0499***	-0.0575
			(4.45e-05)	(0.000127)			(0.0189)	(0.0495)
Marginal Effect	-0.00030***	-0.00013***	-0.00011***	-5.40e-05	-0.00031***	-0.00013***	-0.00012***	-0.00014
	(5.10e-05)	(4.31e-05)	(4.45e-05)	(0.000127)	(0.00005)	(0.00004)	(0.00005)	(0.00012)
Sample	All	All	Agency	~Agency	All	All	Agency	~Agency
State FE		Yes	Yes	Yes		Yes	Yes	Yes
Time FE		Yes	Yes	Yes		Yes	Yes	Yes
Additional Controls		Yes	Yes	Yes		Yes	Yes	Yes
Observations	5,264	5,076	4,464	612	5,264	5,076	4,464	612
R-squared	0.006	0.558	0.590	0.570				
F	33.71	47.32	47.44	13.49				

Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 7. Control Function Estimates of Gubernatorial Campaigns' Impact on Business Bankruptcies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Discrete EEV (Resolution Rate = 0)				Continuous EEV (Resolution Rate)			
EEV	-0.44***	0.2	-0.474***	-0.166	37.73***	119.52	35.49***	95.33
	(0.164)	(1.87)	(0.115)	(0.291)	(12.947)	(4276.44)	(9.133)	(58.054)
EEV Residuals	0.244**	-0.15	0.276***	0.071	-35.78***	-114.751	-33.90***	-91.21
	(0.099)	(1.09)	(0.070)	(0.17)	(12.89)	(4276.37)	(9.137)	(58.791)
Average Marginal Effect	-0.0032***	0.001	-0.0035***	-0.001	0.282***	0.899	0.264***	0.715
	(0.001)	(0.014)	(0.0008)	(0.002)	(0.095)	(32.64)	(0.070)	(0.438)
Sample	Agency	~Agency	Agency	~Agency	Agency	~Agency	Agency	~Agency
Time Controls			Y	Y			Y	Y
State Average Controls			Y	Y			Y	Y
	4568	696	4568	696	4568	696	4568	696

Note: Bootstrapped standard errors in parentheses. Control function two-step procedure estimated via method of Wooldridge (2008, 2015). Sample "Agency" indicates sample restricted to observations with a state-bank regulatory agency. Sample "~Agency" indicates sample restricted to observations without a state-bank regulatory agency. Time controls include year and quarter fixed effects. State average controls include state mean of Palmer drought index and farm failure rate.

Table 8. IV Estimates of Gubernatorial Campaigns' Impact on Business Bankruptcies

	(1)	(2)	(3)	(4)	(5)	(6)
	Discrete EEV (Resolution Rate = 0)		Continuous EEV (Resolution Rate)		Discrete EEV (Resolution Rate = 0)	Continuous EEV (Resolution Rate)
EEV	-0.0043***	-0.0012	0.2645***	0.7168*	-.0055***	0.2978***
	(0.0007)	(0.0010)	(0.0637)	(0.4200)	(.001)	(0.0678)
Montiel-Pflueger robust weak instrument test	7.945	1.68	5.35	0.99	5.623	5.5
Sample	Agency	~Agency	Agency	~Agency	All	All
Time Controls	Y	Y	Y	Y	Y	Y
State Average Controls	Y	Y	Y	Y	Y	Y
Observations	4568	696	4568	696	5264	5264

Note: Clustered standard errors at year-quarter level in parentheses. Sample "Agency" indicates sample restricted to observations with a state-bank regulatory agency. Sample "~Agency" indicates sample restricted to observations without a state-bank regulatory agency. Time controls include year and quarter fixed effects. State average controls include mean of Palmer drought index and farm failure rate.

Our first strategy involves identifying factors that exogenously altered bank failure rates. Our principal instrument, gubernatorial elections, works with data tabulated by state and quarter. The data needs to be tabulated by state, because governors influenced the timing of failures for state-chartered banks under their jurisdiction. The data needs to be tabulated at a high frequency because gubernatorial elections shifted failures through time, from the months preceding an election to the months following an election. These shifts occurred because governors supervised state banking departments, which regulated and examined state-chartered financial institutions and decided which banks should be closed to protect depositors' and the publics' interests and which banks deserved forbearance for a few months to see if their prospects improved. Political considerations have been shown to induce forbearance or increase scrutiny in related contexts (cite modern paper, Richardson and Van Horn). We show that bank failure rates systematically fell in quarters before and systematically rose in quarters following gubernatorial elections, particularly close elections when the sitting governor's electoral prospects seemed in doubt, when compared to states that did not have gubernatorial elections at that time. Temporal shifts of this sort did not coincide with other types of elections (e.g. federal presidential and congressional), did not occur for financial institutions that were not under supervision of bureaucracies reporting to governors (e.g. nationally-chartered banks), and did not occur in states which did not have regulators who reported to the governor. These placebo tests and an array of robustness checks demonstrate that gubernatorial elections shifted bank failures through time. These shifts, in turn, shifted the timing of failures for businesses.

A series of additional instruments, including climatic shocks, financial panics, and institutional features of statewide deposit insurance systems, which have been suggested by other scholars, yield similar results.[13] This abundance of instruments demonstrates that failures of banks induced failures of firms. We calculate the aggregate size of this effect.

Closest papers … JFE, Jalil, Ramirez, Ford and Schwartzman

The remainder of this essay lays out our argument. Section 2 describes the structure of the commercial credit system, the types of firms that relied on banks for credit, the American political system and governors' role supervising regulators of state-chartered financial institutions. Section 3 discusses the nature of business bankruptcies and bank failures, the stability of the data-generating process, and the ways in which we identify banking panics. Section 4 describes our statistical methods and results. Section 5 discusses the implications of our estimates.[14]

[13] The last method builds upon the initial examinations of the data, operationalizing the idea of exogenous shocks. We do this by identifying financial crises whose origins appear to lie within the dynamics of the financial system. We base identification of these crises on recent research by Andrew Jalil (2011) and Gary Richardson (2006, 2007). The ultimate source of this information comes from observations of contemporary financial professionals as reported in the business press and routine reports of bank regulators, particularly the Division of Bank Operations of the Federal Reserve Board.

[14] Failure of banks and failures of firms were clearly correlated during the first three decades of the twentieth century. All of the correlation above that which could be attributed to random chance appears to be due to the failure of bank-dependent firms in the six months following banking panics. Our research design – including the

What about

federal elections … no for state chartered banks, maybe for nationally chartered banks

crises … yes, do not need to exclude from sample to see result … => stronger than post WW2 when FDIC is receiver. Then, you need to exclude panics to see impact of gubernatorial elections

Federal cycle … cite RFC and New Deal literature, find some newspaper articles,

comparison of exogenous and endogenous shocks and the comparison of effects on treatment and control groups – enables us to identify causal relationships. These patterns indicate that failures of banks triggered failures of firms that depended on banks for credit. The pattern stems almost entirely from pronounced increase in failure of bank-dependent firms following events identified as financial panics. These statistical findings appear consistent with widespread claims by contemporary observers that banking panics influenced economic activity through what we now describe as a bank-lending channel.

Appendix: Data Sources and Issues

This appendix discusses details of the data and issues that influence our analysis. Key issues include definitions of bankruptcies (by firms) and failures (by banks), ways in which we identify banking panics, sources of information for our other instrumental variables, and the nature of the data generating process for firm bankruptcies and bank failures, which explains why our analysis spans the years from 1900 to 1930 and discusses the difficulties of expanding the analysis to later years. Lists and tables provide citations to sources of data.

This section synthesizes information from those essays, focusing on issues underlying the identification strategies that we will implement in later sections of this paper. This section also describes data that we draw from other scholars' research into the statistical structure of the United States economy in the late nineteenth and early twentieth centuries.

A. Firm Bankruptcy Data

Data on bankruptcies of firms comes from publications of R.G. Dun and Company, a predecessor of today's Dun and Bradstreet Corporation (see Richardson and Gou, 2011, for details and data tables). The quality of Dun's data on bankruptcies was widely recognized. Dun's data appeared in the *Survey of Current Business*, *The Statistical Abstract of the United States*, and dozens of publications by the Federal Reserve System. Dun's data formed the basis of articles and tables of data routinely published in newspapers including the *New York Times*, *Wall Street Journal*, and *Commercial and Financial Chronicle*. *Dun's Review* noted the popularity of this information when the editors wrote that "not only trade and manufacturing organizations recognize the importance of the records regarding their especial lines, but annual books of reference, almanacs, and even the monthly report of the Bureau of Statistics publishes the figures under the direction of the Treasury Department at Washington (*Dun's Review*, 13 July 1901, p. 6.)." The consistency and accuracy of Dun's data on business bankruptcies made it one of the most widely watched indicators of economic activity from the 1890s through the 1930s.

Dun's defined a business failure as the involvement of a firm in a court proceeding or voluntary action which was likely to end in loss to creditors and in most cases involved the liquidation of the organization. Firms were frequently corporations, but also included proprietorships and partnerships. Dun's reporting network collected information from court filings in every county in the United States. Dun's strove for comprehensive coverage in which they counted every bankruptcy filed in every courthouse in the United States.

Dun's categorized the data by branches of business according to classifications devised by the Census Bureau for the census of 1890, which appeared a few years before Dun's began publishing its bankruptcy data series. Dun's defined branches of business consistently through the early 1930s. The Census Bureau, however, revised their industrial classification scheme extensively between 1890 and 1920. The Internal Revenue Service employed a different industrial classification scheme in their publications. Differences in cross-sectional industrial classification schemes complicate efforts to examine and interpret differences in patterns at the level of individual industries or branches of business, except at the sectoral level of all manufacturing firms and all trading firms, which all sources appear to distinguish in a consistent manner.

Dun's classified firms by size using measures of revenue in the year prior to bankruptcy. Large firms had revenues above $5,000 per year. Small firms had revenues below $5,000. The Census Bureau and IRS defined the size of firms in several ways, including number of employees, total assets, net profits, and total revenues. All of these measures appear highly correlated in our data sources, all of which provide tables classifying firms by size in multiple ways. All of these measures also appear inversely correlated with bank borrowing. Larger firms, no matter the measure of size, borrowed less from banks. Smaller firms borrowed more from banks. The size measure in all of our sources, in other words, is a

useful proxy for reliance on bank credit. The primary credit source for all firms classified as small by Dun's would have been their local commercial banks. Some firms classified as large by Dun's would have relied on the same source, but the larger firms in this category would have been creditors to banks.

Dun's tabulated data about bankruptcies in several ways. Dun's classified firms into economic sectors of manufacturing and trading. Within those sectors, Dun's classified firms by branch of business and as large (revenues above $5,000 per year) or small (revenues below $5,000) starting in 1900. Dun's published this information each month aggregated at the national level. Dun's also tabulated data by state and quarter. These state-quarter tabulations disaggregated data by sectors of manufacturing, trading, and other, but did not disaggregate by size or branch of business.

Dun's data included all bankruptcy proceedings filed under the Bankruptcy Act of 1898. According to the Act:

> "Involuntary proceedings in bankruptcy may be taken against 'any corporation engaged principally in manufacturing, trading, printing, publishing, or mercantile pursuits, owing debts to the amount of one thousand dollars or over. Private bankers, but not national banks or banks incorporated under state or territorial laws, may be adjudged involuntary bankrupts (Black 1898, 106)."

It is important to note that the Bankruptcy Act of 1898 left resolution of insolvent national banks to the Office of the Comptroller of the Currency, which determined which banks should be closed, appointed receives to oversee their liquidation, and adjudicated most disputes that arose in the process. The Act left the resolution of insolvent state-chartered banks to state courts or state banking departments. Personal bankruptcies of professional individuals, such as doctors, dentists, and lawyers, were adjudicated in federal court under the Bankruptcy Act, but Dun's excluded these bankruptcies from its calculations.

Dun's tabulates both the number and liabilities of failed firms. We convert this information to failure rates by combining it with data on the number of firms in operation each year and an assumption about the rate of net new firm formation.

ADD DETAILS OF FORMULA HERE

B. Bank Failures

The definition of a bank failure differed from the definition of a firm bankruptcy. A bank failed we

suspension occurred when a bank ceased payments to depositors on a business day and fails to reopen by 9am on the next business day. Some suspended banks reopened for operations. Others entered liquidation during which a court (or in some states, the superintendent of banking) or the Comptroller of Currency appointed a receiver (typically a lawyer) to wind up the bank's affairs, paying off the depositors and collecting assessments from stockholders.

Bank Suspensions. Data on bank suspensions comes from FRB 37, Dun's Review, NBER Macrohistory.

Bank Suspensions: The data that we examine on bank failures originated with two sources: bank regulators and the financial press. Regulators of state and national banks reported changes in bank status – such as suspensions, mergers, and terminations – to their supervisors, who published these materials periodically. The financial press reported many of these events within days of their occurrence in daily publications like the Wall Street Journal and New York Times. The financial press also compiled information about these events which was published periodically in volumes such as Rand McNally Bankers Directory or R.G. Dun's Bank and Quotation Record. During the 1920s and 30s, the Federal Reserve Board's Division of Bank Operations compiled information from all of these sources to create

data on the number of bank suspensions and amount of deposits of suspended banks. The Federal Reserve published aggregate tabulations from this endeavor in its monthly bulletin in September 1937. Richardson (2006, 2007, 2008) and Chung and Richardson (2007) recovered this microdata and used it to construct the data series analyzed in this essay. These series consists of counts of failures of banks from regulators original reports that have been checked repeatedly by experts over the intervening decades. Data on monthly bank failures from 1921 to 1932 is used to estimate our monthly vector autoregressions.

Quarterly bank failure data is gathered from the National Bureau of Economic Research Macrohistory database which originates from R.G. Dun and Co.. Aggregate data on quarterly bank failures from 1894 to 1924 is gathered from this source. These data are merged with the monthly data to construct a consistent series of quarterly bank failures from 1895 to 1937. Data on quarterly bank failures from 1900 to 1932 is used to estimate quarterly vector autoregressions.

The data that we examine on bank failures originated with two sources: bank regulators and the financial press. Regulators of state and national banks reported changes in bank status – such as suspensions, mergers, and terminations – to their supervisors, who published these materials periodically. The financial press reported many of these events within days of their occurrence in daily publications like the Wall Street Journal and New York Times. The financial press also compiled information about these events which was published periodically in volumes such as Rand McNally Bankers Directory or R.G. Dun's Bank and Quotation Record. During the 1920s and 30s, the Federal Reserve Board's Division of Bank Operations compiled information from all of these sources to create data on the number of bank suspensions. The Federal Reserve published aggregate tabulations from this endeavor in its monthly bulletin in February 1937. The original information gathered by the Division of Bank Operations resides in the National Archives of the United States. Richardson (2006, 2007, 2008) and Chung and Richardson (2007) recovered this microdata and used it to construct the data series analyzed in this essay. These series consists of counts of failures of banks from regulators original reports that have been checked repeatedly by experts over the intervening decades. (We have also analyzed the impact of temporary and permanent suspensions using temporary and permanent suspension data from Richardson and also Ramirez. Should we discuss how we gathered information these data from these sources?)

We identify banking crises whose origins appear to lie within the dynamics of the financial system in two ways. Richardson (2007a) identifies local and national banking panics from examiners' reports of the causes of bank suspensions. Panics are defined as events where the bulk of the banks that suspended operations possessed portfolios that examiners deemed sufficient prior to the crisis but were forced out of business by what examiners deemed to be sudden, sizeable, and unexpected demands by depositors or financial counterparties such as correspondent banks. Andrew Jalil (2010) identifies panics as clusters of bank suspensions reported in the financial press. For years when Jalil and Richardson's series overlap, the different methods yield identical results (I included a table showing the quarters when banking panics occured.

C. Banking Panics

We identify banking crises whose origins appear to lie within the dynamics of the financial system by building on the work of other scholars. Andrew Jalil (2010) identifies panics as clusters of bank suspensions reported in the financial press. Lee Davison and Carlos Ramirez (2015) identify panics as chronological and geographical clusters of banks failures from FDIC reports of all banks failing in the United States during the 1920s and 1930s. Richardson (2007a) identifies local and national banking panics from examiners' reports of runs on banks. Richardson and Mitchener (2016) identify panics in microdata using examiners' reports as well as geographic and temporal clustering and in aggregate data by detecting sudden spikes in suspension rates associated with large numbers of bank runs and large numbers of banks being closed by their own directors, rather than official regulators. These four sets of scholars come up with similar results for the years that they examine. We display this information in

Table 5. We base our estimates on the panics described by Jalil and Richardson and conduct a series of robustness checks to ensure that our results is robust to the panic detection method.

The data that we examine on bank failures originated with two sources: bank regulators and the financial press. Regulators of state and national banks reported changes in bank status – such as suspensions, mergers, and terminations – to their supervisors, who published these materials periodically.[15] The financial press reported many of these events within days of their occurrence in daily publications like the Wall Street Journal and New York Times. The financial press also compiled information about these events which was published periodically in volumes such as Rand McNally Bankers Directory or R.G. Dun's Bank and Quotation Record. During the 1920s and 30s, the Federal Reserve Board's Division of Bank Operations compiled information from all of these sources to create data on the number of bank suspensions. The Federal Reserve published aggregate tabulations from this endeavor in its monthly bulletin in February 1937. The original information gathered by the Division of Bank Operations resides in the National Archives of the United States. Richardson (2006, 2007, 2008) and Chung and Richardson (2007) recovered this microdata and used it to construct the data series analyzed in this essay. These series consists of counts of failures of banks from regulators original reports that have been checked repeatedly by experts over the intervening decades. (We have also analyzed the impact of temporary and permanent suspensions using temporary and permanent suspension data from Richardson and also Ramirez. Should we discuss how we gathered information these data from these sources?)

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D. Election and Other Political Data

E. Information About Governors' Interventions into Bank Failures

[15] Frequency varied across jurisdictions (e.g. national or a particular state), departments (e.g. Federal Reserve or Comptroller of Currency), and time. The materials most commonly found in modern libraries are regulators' annual reports.

New York Times. (1922, Feb 12). OKLAHOMA JUDGE WARNED BY BAR: TOLD NOT TO RECONVENE GRAND JURY HE DISMISSED, PREVENTING INDICTMENTS.ADVISED TO REMAIN AWAY NAME OF GOVERNOR ROBERTSON ANDOTHER STATE OFFICIALS MENTIONEDIN BANK FAILURE SCANDAL. Retrieved from https://search.proquest.com/docview/99648616?accountid=14509

New York Times. (1922, Mar 23).OKLAHOMA GOVERNOR PUT UNDER ARREST: J.B. ROBERTSON IS ACCUSED OF ACCEPTING BRIBES IN CONNECTION WITH BANK FAILURES.LEADING MEN ARE INDICTED FORMER BANKING COMMISSIONER, FACING TWO ACCUSATIONS, HASNOT YET BEEN LOCATED. Retrieved from https://search.proquest.com/docview/99565216?accountid=14509

New York Times. (1922, Mar 4)GOVERNOR IN FIGHT WITH OKLAHOMAN: BATTLE STARTS IN COURT HOUSE WHEN LATTER REFUSES TO SHAKE HANDS. BANK FAILURE CAUSED FEUD COUNTY ATTORNEY REFUSES TO LET EXECUTIVE APPEAR BEFORE GRAND JURY INQUIRY. Retrieved from https://search.proquest.com/docview/99571934?accountid=14509

WARDER STEPS OUT AS STATE BANK HEAD: REFUSES TO BE QUESTIONED, BUT SAYS CHARGES AGAINST HIM IN CITY TRUST CASE ARE FALSE. SUCCESSOR TO INVESTIGATE BRODERICK QUICKLY SWORN IN-- STOCKHOLDERS OF DEFUNCT BANK ASK MORELAND INQUIRY. WEN'T ANSWER QUESTIONS. WARDER STEPS OUT AS STATE BANK HEAD SEES JUSTIFICATION OF COURSE. PROMISES PERSONAL INQUIRY. BANTON AWAITS COMPLAINT. (1929, Apr 23). New York Times (1923-Current File) Retrieved from https://search.proquest.com/docview/104742437?accountid=14509

DEPOSITORS SEEK DIX'S AID.: WANT OFFICIAL INQUIRY INTO FAILURE OF THE UNION BANK OF BROOKLYN. (1911, May 04). New York Times (1857-1922) Retrieved from https://search.proquest.com/docview/97142097?accountid=14509

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New York Times. (1926, Oct 17). GEORGIA BANK CHIEF QUITS.: SUPERINTENDENT BENNETT HAD BEEN CRITICIZED OVER FAILURES. Retrieved from https://search.proquest.com/docview/103703272?accountid=14509

Chicago Daily Tribune. (1922, Mar 04). OKLAHOMAN AND GOVERNOR SAY IT WITH THEIR FISTS: EXECUTIVE IS DEFIED IN BANK FAILURE ROW. Retrieved from https://search.proquest.com/docview/174967326?accountid=14509

Chicago Daily Tribune. (1922, Mar 23).SEIZE GOVERNOR OF OKLAHOMA AS BRIBE TAKER: BIG OFFICIALS INDICTED IN BANK FAILURE CASE. Retrieved from https://search.proquest.com/docview/174960958?accountid=14509

San Francisco Chronicle. (1922, Mar 23). Oklahoma governor arrested: EXECUTIVE AND ALLY BRIBED BY BANK, IS CHARGE J. B. A. Robertson, Fred G. Dennis are indicted in Okmulgee failure. MANY OTHERS ACCUSED officials charged with letting institution run while insolvent. Retrieved from https://search.proquest.com/docview/576884147?accountid=14509

Wall Street Journal. (1920, Jun 02). KANSAS BANK FAILURES. Retrieved from https://search.proquest.com/docview/129834948?accountid=14509

Evidence of regulatory action … Van Horn and Richardson, modern paper, Caldwell story

In summer of 1920, during gubernatorial campaign in Kansas (check), the governor announced

the creation of a committee of professional bankers to investigate the performance of the state banking department (Wall Street Journal, June 2, 1920 p. 15).

In 1911, (1911, May 04). New York Times. Public pressure requesting legislature in Albany

investigate causes of bank failures, the superintendents' role in them, and prosecute wrongdoing.

In New York, performance of bank superintendent a political issue for decades, with accusations that political favoritism and concerns influenced policies of appointees to lead the department. NYT

F. Other Data Issues

Our study focuses on the period from 1900 through 1932, because during that period, the procedures for firm bankruptcies and bank liquidations remained stable. The Banking Act of 1898 standardized bankruptcy procedures for firms throughout the United States. This bankruptcy regime continued in operation until 1933. In that year and the year that followed, a series of amendments to the bankruptcy act altered the nature of bankruptcy throughout the United States. These amendments altered the threshold for forcing firms into bankruptcy, allowed firms to enter bankruptcy voluntarily, and enabled firms to use bankruptcy to reorganize the debts and continue in operation. In those same years, a series of laws altered the nature of bank liquidation. The federal government shut down all banks in the United States, determined which banks would reopen for business, recapitalized thousands of banks both large and small, and forced thousands of other banks to merge or cease operations. These laws created the Federal Deposit Insurance Corporation, which in addition to insuring deposits at most banks in our nation, became the liquidator of all banks that participated in the insurance scheme

LIVE WIRES: AWAIT REPUBLICANS OF HURON COUNTY, WHERE LANING'S IN ...
SPECIAL DISPATCH TO THE ENQUIRER
Cincinnati Enquirer (1872-1922); May 24, 1908;
ProQuest Historical Newspapers: The Cincinnati Enquirer
pg. 9

LIVE WIRES

Await Republicans

Of Huron County, Where Laning's in Danger,

As Indorsement May Be Denied Congressman.

Bank Failure Blamed For Statesman's Plight.

Campbell Buries Bank Failures As Campaign Issue: It <s
Arizona Republican (1890 - 1922); Oct 26, 1922;
ProQuest Historical Newspapers: The Arizona Republican
pg. 1

Campbell Buries Bank Failures As Campaign Issue

It Governor Replies To Charges of Fraud Responsibility.

Replying to charges that he should be held accountable for the loss of state moneys through the failure of the Central Bank of Phoenix, Governor Campbell took direct issue with his critics last night at a rousing meeting held in the grammar school auditorium at Peoria.