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Independent Regulators and Financial Stability: Evidence from Gubernatorial Campaigns and a Progressive Era Policy Experiment

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ABSTRACT

Regulatory independence forms a foundation for modern financial systems. To illuminate the value of this ubiquitous institution, we examine a Progressive Era policy experiment in which hitherto independent regulators came under gubernatorial supervision. After this change, failure rates declined during gubernatorial election campaigns for banks under gubernatorial jurisdiction. Declines did not occur during campaigns for other officials or for nationally chartered banks. Declines in bank resolutions during campaigns reduced business bankruptcies. We corroborate these claims with new data and novel IV regressions. Our results indicate that political subservience of financial regulators links electoral and economic cycles.

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1. Introduction

A recent Supreme Court decision, Seila Law v. Consumer Financial Protection Bureau (CFPB), ruled the CFPB's independence from Presidential oversight was unconstitutional.[1] This case is the spearpoint for a series of suits seeking to overturn Progressive Era precedents underlying independence of federal agencies.[2] The suits reflect the theory of the unitary executive, popularized by the Federalist Society, which asserts independent federal agencies are unconstitutional. This campaign may induce the Supreme Court's Federalist majority to compel federal agencies to operate under political supervision, where their executives serve at the will of the President, rather than independently, where their executives can be removed only for good cause. This possibility triggered discussions about optimal structures for government agencies, particularly those regulating banks and other financial institutions, for which independence may be particularly important. Does independence have optimal properties? Would political supervision influence regulatory decisions regarding financial institutions such as closing failed banks, a process known as bank resolution?

Modern data shed little light on these questions. Today, regulatory variation across U.S. states is limited, while banks and businesses frequently operate across many states. The Federal Deposit Insurance Corporation (FDIC) resolves insolvencies at all banks with FDIC insurance. Federal regulators and the Federal Reserve frequently intervene to alleviate bank distress and economic downturns. These features complicate efforts to analyze regulatory policies of individual states and impede inference concerning causes and consequences of bank resolutions.

The Progressive Era of the 1890s through the 1920s can serve as a laboratory for these issues.

[1] Seila Law v. CFPB (2020) determined the constitutionality of "good cause" protections for directors of federal agencies with single directors, such as the CFPB and the Federal Housing Finance Agency (FHFA). For these agencies, good-cause protections were deemed unconstitutional. So, their directors now serve at the will of the President and can be removed from their positions at any time. While the Office of the Comptroller of the Currency (OCC) also has a single director with "good cause" protections, its unique structure and direct legal protections for its directors' decisions insulate it from the immediate impact of the decision.

[2] Key precedents include Myers v United States (1926) and Humphrey's Executor v. United States (1935).

Lessons drawn from that period, particularly those regarding financial and monetary institutions, are useful for policymakers in the present (Friedman and Schwartz 1963, Bernanke 2002 and 2013). Progressive Era intermediaries fulfilled the same economic functions, faced similar economic and legal constraints, and used essentially the same set of contracts as their descendants today (Gorton 2010). Economic and political structures of the time simplify empirical analysis. Most banks and businesses operated in a single state. Federal and state governments seldom attempted counter-cyclical interventions and had limited ability to influence the economy even if they tried. These features facilitate inference and amplify analogies between the past and the present.

This essay examines a Progressive Era policy experiment in which states imposed political supervision on regulators who determined whether and when to resolve troubled banks. Between the Gilded Age and the Roaring 20s, legislatures in most states shifted decisions regarding bank resolution from judges to bureaucrats. Judges operated independent of gubernatorial (and in many cases any) political oversight. Bureaucrats worked for governors who could remove them at will and allocate the positions for political patronage (Komai and Richardson 2014, Mitchener and Jaremski 2015). This shift occurred in different states at different times. It did not affect supervision of banks with national charters; decisions about their resolutions remained with the Comptroller of Currency. It did not affect businesses other than commercial banks; decisions about their bankruptcies remained with federal district bankruptcy courts and federal regulators. We ask how subordinating the bank resolution process to political supervision influenced bank-resolution and business-bankruptcy rates, particularly during gubernatorial election campaigns, when governors had strong incentives to delay resolutions in hopes that better news about the economy would yield votes at the ballot box.

Statistical analysis of this Progressive Era policy experiment is impeded by two constraints. The first is information. Data on bank resolutions, business bankruptcies, and economic activity are limited

(Bodenhorn 2020). The second involves inference. Bank resolutions and business bankruptcies are endogenous and interrelated. Determining whether changing bank resolution procedures influenced business bankruptcy rates requires an instrument which is exogenous and impacts the former but not the latter.

We overcome the first obstacle by creating new data series of business bankruptcy and bank resolution rates by state and quarter from 1902 through 1929 and combining this data with extant information on the political economy of the Progressive Era.[3] We overcome the second obstacle using an instrument whose effect has been observed in postwar data, gubernatorial election campaigns (Liu and Ngo, 2014).

Our initial analysis examines how the shift from politically independent to politically subservient bank resolution procedures influenced state-by-quarter resolution rates. These rates were extremely skewed, with a cluster of observations at zero. In these circumstances, an appropriate estimator can be based on a zero inflated beta (ZIB) distribution (Papke and Wooldridge, 1996; Maarteen Buis, 2010). The procedure yields robust results. After states placed bank resolutions under gubernatorial control, regulators deferred resolution of troubled banks during gubernatorial campaigns. The magnitude of the effect was substantial. According to our estimates, regulators reduced the resolution rate to zero in one-third of all gubernatorial elections when they had the opportunity to do so. This effect only occurs for state-chartered banks and only in states where gubernatorially-subordinate bureaucrats controlled resolution procedures. It did not occur for nationally chartered banks or during campaigns for officials

[3] 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 U.S. states and territories, 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 U.S. states and territories excluding Wisconsin, for which data was incomplete. 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 data on weather, farm failures, and interest rates. We disaggregate this information as much as possible to the state-quarter level.

other than the governor. It did not occur in states where judges, rather than regulators under the governor's jurisdiction, supervised bank resolutions.

Strong results from this initial regression indicate gubernatorial campaigns in states whose governors supervised bank resolutions can serve as an instrument in a two-stage procedure where we first estimate how gubernatorial campaigns influenced bank resolutions and then use that result to estimate how bank resolutions influenced business bankruptcies. It is important to note that the exclusion criterion holds. Governors and their subordinates lacked the ability to directly defer business bankruptcies, since creditors determined when suits were filed, and adjudication occurred in federal courts. No procedure exists for directly employing the results from a zero inflated beta distribution as the first stage in an IV. The first stage is non-linear. So, results from standard IV methods would be biased. Credible inference in this setting is an active area of research. Recommended procedures involve employing less informative first-stage estimators which yield an unbiased second stage. Candidates include estimating the first stage as a linear, logistic, or fractional logit regression and calculating the second stage using standard or control-function approaches. In our application, all recommended methods yield similar results. Reducing bank resolutions by 1% reduced business bankruptcies by 0.2% to 0.3%. During gubernatorial campaigns, in other words, the suppression of bank resolutions reduced bankruptcies of business to about three-quarters of the typical rate.

Our paper contributes to four literatures in the social sciences. The first investigates independence of financial regulators, which is a core principle underlying bank supervision in OECD countries (Angeloni 2019, Alford 2005, Carstens 2019, Lee 2011). Regulatory independence increases financial stability, limits political uncertainty, reduces corruption, solves time-consistency problems, and shapes perceptions of investors and flows of capital (Majone 1997, De Figueiredo and Rui 2002, Gilardi 2005, Quintyn and Taylor 2007, Bertelli and Whitford, 2009, Delatte, Matray, and Pinardon-Touati

2019, Kim and Lee 2006, Beck, Demirguc-Knut, and Levine 2006, Dincer and Eichengreen 2012, Lucas 2017; Barth, Caprio, and Levine 2004). Independence of bank regulators also influences decisions and independence of central banks (Kohn 2013; Quintyn and Taylor, 2002 and 2004; Hüpkes, Quintyn, and Taylor, 2006). Most empirical work on the issue is international. We provide the first evidence that regulatory independence influenced bank stability and performance in the United States. We also show that incentives of state regulators a century ago led them to be more lenient than their federal counterparts, as Agarwal et. al. (2014) found recently. Subordinating financial regulators to politicians aligned their behavior closer to politicians' priorities and farther from the publics, as Besley and Coate (2003) argue is the case for modern industries. Finally, we find an important counterpoint to Glaeser and Shleifer's (2003) rationale for the rise of the regulatory state. Glaeser and Shleifer argue that regulation replaced litigation as the principle means for controlling industry during the Progressive Era because the rich could not control regulators as easily as they controlled judges. For financial regulation, we find the opposite. Judges were less susceptible to political pressures than were bureaucrats who worked at the will of politicians.

The second is the political economy of American elections. A large literature finds that voters hold incumbent politicians accountable for economic conditions (Atkenson and Partin 1995; Brown 2010; Carsey and Wright 1998; Charles and Stephens 2013; Ebeid and Rodden 2006; King 2001; Leyden and Borrelli 1995; Niemi, Stanley, and Vogel 1995). Governors strive to send signals that they are working hard to improve the economy and create jobs for voters (Slattery 2021). The prototypical example is governors offering incentives for highly visible firms to relocate to their state (Slattery and Zidar 2020). Historians trace governors' efforts to generate good economic news back to the Great Depression and subsequent expansion of government (Turner 2003). We show that governors' efforts to

influence economic news began during the Progressive Era, when governors had few direct ways to influence firms' fates but could use their regulatory authority to suppress bad news about banks.

The third examines the impact of elections and electoral uncertainty on financial and economic activity. In the run up to gubernatorial elections, Jens (2017) shows electoral uncertainty reduces investment by 5%. Çolak, Durnev, and Qian (2017) show that electoral uncertainty reduces the number and prices of initial public offerings, particularly for firms with economic activity concentrated in single states. Liu and Ngo (2014) show that bank resolution rates fall substantially in the year leading up to gubernatorial elections. This literature examines postwar data. We show that elections' impact on banking and finance began in the United States during the Progressive Era, when federal and state governments began to regulate financial and economic activity at the turn of the twentieth century.

The fourth is on the role of banks in the economy and the relationship between bank distress and economic activity. According to some scholars, financial intermediaries like banks transform liquid low-yield funds into longer-term higher-yielding assets, screen and monitor recipients of this credit, and facilitate economic activity that would not occur in the absence of this intermediation. Bank failures disrupt this intermediation, inhibiting production and exchange (Bernanke 1983, Calomiris and Mason 2003, Richardson and Troost 2009). According to other scholars, banks and their failure have little impact on the economy, because bank activity is endogenous and bank credit has many substitutes. The endogeneity of bank failures impedes empirical analysis of this issue. We use an exogenous instrument (gubernatorial elections) and a policy experiment (introduction of political control of the bank resolution process) to conduct causal inference on this issue. We find that bank resolutions contributed to economic instability in the Progressive Era. Our estimate – that a 1% increase in the bank resolution rate increased business bankruptcy rate by ~0.3% or one-quarter of its mean – lies near the median of current estimates (Grossman, 1983; Ramirez and Shively, 2012).

The rest of our essay corroborates our claims. Section 2 describes the nature of the policy experiment, discusses historical accounts describing governors intervening in the process of bank resolution, and presents an initial analysis of the data. Section 3 examines the impact of gubernatorial campaigns on bank resolution rates. Section 4 examines how gubernatorial interventions into bank resolution rates influenced outcomes for business firms. Section 5 discusses the implications and external validity of our analysis.

2. Historical Background

This section discusses facts important for understanding our analysis and provides an initial look at the evidence. The discussion begins with bankruptcies of businesses. Their timing was determined by decisions of debtors, creditors, and judges under national laws adjudicated in federal courts. The discussion continues with resolutions of banks. Their timing and characteristics differed across states. In some states, depositors and courts determined when banks failed. In other states, regulators subordinate to the state's governor determined when banks failed. In the later but not the former, data demonstrates that bank 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 (Warren 1935, Skeel 2001, and Hansen 2001). All bankruptcies of businesses, except railroads and banks, were adjudicated in federal district courts.[4] 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 be removed from office only after investigation

[4] Railroad bankruptcy was handled by a separate process which allowed for the reorganization of liabilities and continued operation of enterprises. Legal procedures for reorganizing debts of non-railroad firms were created by legislation during the Great Depression.

and impeachment for violating the law. Individual judges dealt with small proportions of cases and lacked ability to influence aggregate failure rates.[5] State officials – including governors, legislators, and judges – had no authority over the timing of bankruptcy filings or the adjudication of cases, and therefore, lacked the ability to administratively influence business bankruptcy rates.

Quarterly business bankruptcy 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.[6] 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 bankruptcy 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 skewed.[7]

[5] No evidence exists that judges ever collectively attempted to influence aggregate business bankruptcy 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.

[6] We begin our study in 1902 when our sources begin reporting consistent data on business bankruptcies following the reforms of the Banking Act of 1898. We end in 1929 with the onset of the Depression because this event generally marks the end of the Progressive Era and because of changes in the nature of the data sources.

[7] 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.

2.2 Bank Resolutions and the Dual Banking System

The process for dealing with banks that could not pay their debts 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 (Board of Governors, 1959, Tables A-2 & A-3, pp. 38-54).[8] The Office of the Comptroller of Currency (OCC) supervised national banks. It was responsible for determining when banks under its jurisdiction could not pay their obligations and for resolving those 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 and resolution.

The OCC did not supervise state-chartered banks. Before 1900, "many states had no supervision 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 violated the law or failed to make payment as promised. After ascertaining the existence of unpaid debts, judges would take possession of a bank and appoint a receiver to liquidate the corporation and repay claimants.

About half of all states established bank regulatory agencies in the nineteenth century. The rest

[8] 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 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.

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. The supervisory agencies, according to Mitchener and Jaremski, "rose from the ashes of banking crises (2015, p. 820)."

In states with supervisory agencies, the agency oversaw bank resolutions, particularly after the Bankruptcy Act of 1898 excluded suits about banks from federal bankruptcy courts. Bank superintendents and their staffs routinely examined banks, determined which banks were insolvent or might in the future fail to fulfill their obligations, and decided whether those banks could be saved by remedial action or should be shut down to protect depositors' rights and the public's interests. Superintendents also determined whether banks that 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 2007, Mitchener and Richardson 2019).

Quarterly resolution 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 resolutions per thousand banks. The median resolution rate, however, was zero. In other words, no banks were resolved 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 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 for those quarters. Rates varied with the business cycle, rising during recessions in 1907-8, 1913-4, and 1921 and falling during expansions. Rates increased over time, from an average of 1.4 per thousand in the 1900s

to 4.4 per thousand in the 1920s.

During these decades, all banks operated within the borders of a single state, and almost always, within a single city.[9] Fluctuations in resolution 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 bank's] 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).

Creditors, in other words, could not force a bank into liquidation by suing in court. Only regulators had the authority to initiate resolutions. Regulators had a great deal of discretion. The law required regulators to provide opportunities to correct problems if possible before shuttering banks. When regulators detected problems, they informed offenders and encouraged managers to fix flaws. When necessary, regulators could take strong actions, such as recommending a merger, removing a bank's management, or compelling shareholders to contribute additional capital. Liquidation was a last resort. Regulators were compelled to act aggressively only when depositors faced imminent losses and delay threated to exacerbate harms.

2.3 Political Influence and Bank Supervision

The nature of bank resolution, which provided regulators with discretion over when to close

[9] In 1900 in the United States, 87 banks possessed a total of 119 branches. The number of branches grew gradually, initially for state-chartered, and after the passage of the McFadden Act in 1927, 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 (Board of Governors 1932, *Branch Banking*, Table 2, p. 6).

banks, provided opportunities for regulators and the politicians whom they served to use that discretion to satisfy a variety of objectives. Governors had good reasons for encouraging regulators to defer resolutions during gubernatorial campaigns, which lasted about half to three-quarters of a year in the U.S. at the turn of the twentieth century (Kleppner 1981 and 1987). Incumbents believed that they received more votes during economic booms and fewer votes during busts. Bank resolutions 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 2000). Bank resolutions 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, and Van Horn 2020). Governors and the bank superintendents that worked for them were often blamed for bank resolutions. 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, incompetence, or political favoritism in the years 1906, 1911, 1920, 1923, and 1929.[10] Other governors in New York touted effective supervision of banks and passage of stronger banking laws as accomplishments worthy of reelection (New York Times, 17 April 1914 p. 8). Similar news coverage appeared in many other states, including Alaska, Georgia, Illinois, Kansas, Massachusetts, Michigan, North Carolina, Ohio, Pennsylvania, and Tennessee.[11]

Substantial evidence details governors' interventions to aid well-known banks. Notorious examples come from Oklahoma. In 1910, the state's first governor, Charles Haskell, was accused of

[10] For examples, see New York Tribune 1906; New York Herald Tribune, 23 April 1929 p. 1; New York Times, 4 May 1911 p. 3, 23 April 1929 p. 1; Wall Street Journal, 17 August 1923 p. 4.

[11] See, for example, Arizona Republican, 26 October 1922, p. 1; Atlanta Constitution, 12 September 1923 p. 14, 3 February 1924 p. 5, 4 September 1926 p. 1, 4 September 1926 p. 1, and 17 October 1926 p. 1; Baltimore Sun, 8 September 1906 p. 14, 12 September 1923 p. 1, 14 August 1925 p. 1, 17 August 1925 p. 1, and 18 August 1925 p. 1; Chicago Daily Tribune, 27 July 1906, p. 5; Cincinnati Enquirer, 24 May 1908, p. 9; Indianapolis Star, 2 July 1905 p. A28; New York Times, 17 October 1926, p. 24; San Francisco Chronicle, 21 October 1907 p. 3 ; Wall Street Journal, 2 June 1922 p. 15.

directing state officials to delay actions against the Columbia Bank and Trust Company, which ultimately failed with large losses to depositors. The legislature investigated the issue and considered impeachment, although they eventually found no legal cause for action. Newspapers nationwide covered the accusations during his reelection campaign, which was ultimately unsuccessful.[12]

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 during his election campaign. The Republican-dominated legislature investigated.[13] Robertson was indicted along with Fred Dennis, the State Banking Commissioner, of accepting a bribe of $25,000 in return for regulatory discretion.[14] The Governor retorted that the accusations were political smears advanced by Republicans who opposed his reelection. Robertson was cleared of the charges, when the court found that he and bank commissioner did not accept remuneration for official acts (New York Times, 12 February 1922, p. 3).

These examples were widely reported because the press reveled in their 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 statistics presented in Figure 2, which plots the difference between state-bank resolution rates in quarters with and without gubernatorial campaigns (excluding quarters with elections, which include periods both before and after elections) for the four quarters of the year. We separate the data by quarters due to the seasonality of bank distress and election campaigns. The figure clearly illustrates that resolution rates shifted to zero in quarters with campaigns. During the second quarter of the year (April, May, and June), for example, the share of observations with no bank resolutions (i.e. resolution rate of zero) was 8.6 percentage points higher for quarters with campaigns compared to

[12] See, for example, 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.
[13] New York Times, 4 March 1922, p. 7; Chicago Tribune, 3 March 1922, p. 3
[14] Chicago Daily Tribune, 23 March 1922 p. 5; New York Tribune (from the Associated Press), 23 March 1922 p.1; New York Times, 23 March 1922 p. 1; San Francisco Chronicle, 23 March 1922 p. 1; Washington Post, 6 July 1922 p. 3.

quarters without campaigns, while the share of observations with resolution rate between 0% and 0.5% was 6 percentage points lower. A similar shift occurred in all seasons of the year. Zero resolution rates became more common. Positive resolution rates became less common, with the decline concentrated at resolution rates less than 1%. The share of observations with resolution 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 resolution rates during gubernatorial elections. From 1902 to 1929, 449 gubernatorial campaigns spanned 898 of the 5,376 quarters in the data. The data underlying Figure 2 indicate that zero failures occurred in just over 10% more of these quarters than comparable quarters without campaigns or elections. Gubernatorial campaigns, in other words, resulted in 90 more than expected quarters with zero bank resolutions. Regulatory discretion during these campaigns delayed the failure of about 400 banks.[15] 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 resolutions in only one of the two quarters of a campaign.[16] Combining these facts yields our back-of-the-envelope result.[17] In one of three Progressive-Era gubernatorial campaigns where banks

[15] The number 400 comes from the following calculation. Without campaigns, resolution 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 yields a number that rounds to 400.

[16] This assumption makes sense because bank resolutions occurred in under than half of all quarters. So, governors and their subordinates would have the need or opportunity to defer banks resolutions in a little less than half of all quarters.

[17] 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 resolution rate was independent across quarters, then without gubernatorial intervention, approximately 41% of campaigns would have experienced no bank resolutions, 46% of campaigns would have experienced bank resolutions in only one quarter, and 13% of campaigns would have experienced bank resolutions during both quarters. The number of

faced financial difficulties, regulators working for governors delayed the resolution of 4 or 5 banks by about 6 months to prevent bad news of bank failures from weakening their chances at the polls.

These patterns are consistent with stories 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 campaigns, regulators provided more time than usual. This delay limited bad economic news in the run up to the election, improving the incumbent's chances.

This story is consistent with several facts. First, the shift of bank resolution 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 resolutions was determined by courts and depositors rather than by subordinates of the governor, gubernatorial elections were not correlated with shifts in bank resolution rates. Second, shifts of resolution rates towards zero before gubernatorial elections preceded shifts of resolution rates from zero after elections. Third, the shift toward zero during campaigns only occurred for state banks during gubernatorial election campaigns. It did not occur for national banks. 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.

The last point was noted by researchers past and present. The Federal Reserve's 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

campaigns when regulatory discretion could have lowered bank resolution 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.

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). This difference remains true today (Sharma, 2018).

3. Gubernatorial Elections and Bank Resolutions

To understand the relationship between gubernatorial campaigns and bank resolutions, we turn to statistical analysis. The data shape our method. Bank resolution rates are bounded in [0,1]. The mode is 0. The data must be analyzed with a piecewise distribution that accounts for the probability mass at zero and the probability density on the unit interval. At least one factor, gubernatorial campaigns, influences whether the rate equals zero, but appears to have had little influence on higher rates. The appropriate distribution for this data is a zero-inflated beta distribution (ZIB). We estimate it using the discrete-continuous mixture model of Ospina and Ferrari (2011) using the maximum-likelihood procedures of Buis (2012). The probability mass at zero is estimated using a probit. The continuous proportion is estimated using a beta distribution, whose density has a wide range of shapes depending on the values of the two parameters that determine its shape.

Results appear in Table 2. Column 1 estimates the ZIB model on the data underlying Figure 2. The first column, in other words, excludes observations for quarters when a state lacks a bank regulatory agency or 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 (labelled *Campaign*) is an indicator for election campaigns which are the two quarters before an election occurs. 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 quarters

without. 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.[18] 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 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, *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

[18] 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.

bank regulatory agency. The marginal effect of *Agency* reveals the impact of creating a bank regulatory agency. After states create regulatory agencies, failure rates 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 goal 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 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*Agency*. 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 resolutions (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 resolution rates where high, like during financial panics, because regulatory forbearance could not cover up an event as large and public as a panic.

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 extreme 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 marginal effect 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 with 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 caused the trend over time.

The next three columns include additional observations and explanatory variables. The key conclusion drawn from the addition of this information is the stability of our regression results. 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.[19] Column 6 adds to the regression a vector of variables that includes lags of the business bankruptcy rate, the farm failure rate, and the state average interest rate. While bank resolutions clearly impacted current and future business and farm failure rates, lags of these variables are plausibly exogenous. The coefficients and marginal effects for *Campaign*Agency* change little when we include this additional information.

The remainder of the table presents placebo tests. The specifications in Columns 7 and 8 are identical to Columns 4 and 6 but the dependent variable is the national bank resolution rate. The result indicates gubernatorial campaigns did not influence national bank resolution rates. This result has two implications. First, it supports our supposition that regulators' actions altered state bank resolution rates during gubernatorial campaigns. State regulators had discretion over the timing of state bank resolutions, which diminish during gubernatorial campaigns, but state regulators lacked authority over national bank resolutions, which do not decline during gubernatorial campaigns. Second, it rules out the counterproposition that unobserved economic shocks produced our results. Economic conditions that helped or harmed national banks should also have helped or harmed state banks, since they provided similar services to similar clients using similar technology. If unobserved changes in economic conditions generated our result for state banks, we should observe the same result for national banks, but we do not.

The specifications in Columns 9 and 10 are identical to Columns 4 and 6 but the campaign indicator is changed from gubernatorial to presidential, excluding of course, national presidential

[19] 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 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-air conditioning)

campaigns that coincided with gubernatorial contests. Estimated marginal effects are near zero, statistically insignificant, and with a sign opposite to that 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 (or his political 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 over short periods of time. Relative to today, federal spending was lower and the appropriations process was slower. Overall, we find that bank resolution rates fell substantially during gubernatorial campaigns in states whose governors had authority over the bank resolution process. Most of the decline came from quarters when regulators reduced the resolution rate to zero, or in other words, when regulators decided to close no troubled banks.

While Table 2 demonstrates that regulators deferred resolutions during gubernatorial campaigns, 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 may 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 whether shifts in bank resolutions impacted business activity. There is an obvious approach to this issue. Use gubernatorial campaigns as an instrument for bank resolution

rates in a regression that would serve as the first stage in a two-stage procedure where the second stage estimates the relationship between bank resolutions and business bankruptcies. Due to its non-linearity, however, a zero-inflated beta regression cannot serve as the first stage in a two-stage IV estimator since the results of the second stage would be biased. This bias is akin to the infamous forbidden regression. Linear regressions that are closely equivalent to our ZIB, however, can serve as an unbiased first stage in an IV. In addition, a range of models that are nearly equivalent to ZIB can serve as the first stage of a control function. Regressions of these types appear in Table 3.

Initial columns of Table 3 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 2, Column 4. The specification in the even-numbered columns is identical to Table 2, Column 6. The regressions are variants of the equation for zeros in our ZIB. 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*Agency* is statistically significant and ranges from 0.12 to 0.16, which overlaps with the range 0.12 to 0.14 for the equation on zero in Table 2. The two types of regressions, in other words, yield equivalent results.

Remaining columns of Table 3 address the second issue. Subgroups of columns begin with specifications similar to the ZIB model and end with a specification that serves as the first stage of an unbiased two-stage estimator. Columns 5 and 6 report pooled probits similar to the ZIB's equation for zeros. Their marginal effects span the same range as our ZIB 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 bankruptcy 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 on the likelihood of a resolution rate of zero which is 0.07, or about half that in most other specifications.

Fractional logit specifications similar to the ZIB's equation for rate appear in columns 8 and 9. We estimate these using the method of Papke and Wooldridge (1996). 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 ZIB 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 ZIB, as well as the impact of lowering resolution rates in nonzero observations, which was captured in ZIB'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 this 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 bankruptcy rate. These modifications

allow the regression to serve as the initial stage of a control function. While the coefficient on *Campaign* in these regressions may be biased towards zero, it remains substantial and statistically significant.

Overall, Tables 2 and 3 reveal a robust relationship between gubernatorial campaigns and bank resolution rates in states with regulatory agencies subordinate to their governor. Comparing marginal effects across specifications is challenging, because they are stated in different metrics – either (i) the fraction of additional observations with resolution rate equal to zero or (ii) 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 Campaigns" in Tables 2 and 3 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:

(1) $$I_{zero} = \beta_{zero} * C * F$$

β_{zero} is the marginal effect in the ZIB 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:

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

β_{rate} is the marginal effect in the ZIB equation for rate or a regression model estimating the resolution rate. A is the average number of state-chartered banks in operation in a state, which equaled 454 over the years of our study. P is the proportion of campaigns to which the estimate applies. For ZIB estimates, P is the proportion of campaigns with resolution rate above zero. For

non-ZIB regressions, P equals 1 since we estimate the same regression for all campaigns. Note that for each ZIB estimate in Table 2, we calculate both equation 1 and 2, while for the estimates in Table 3, we calculate either equation 1 or 2, whichever is relevant. Since C, F, A, and P are constants, similar formulas yield standard errors for the implied resolution rates.

Calculations of the implied decline in resolutions due to gubernatorial campaigns sends a clear message. The array of models that we examine yield results which are remarkably consistent. The implied decline for our bottom-line ZIB specification (Table 2, Column 6) is 471. The null hypothesis that this estimate equals estimates in the other columns in Tables 2 and 3 cannot be rejected for all but two specifications (Table 3, Columns 7 and 12). Those estimates resemble the initial stage of control functions. In both cases, the implied impact of gubernatorial campaigns is lower than our ZIB estimate but remains statistically and practically significant.

4. Bank Resolutions and Business Bankruptcies

How did declines in bank resolutions during gubernatorial campaigns influence broader economic activity? We address this issue by examining the only economic indicator that can be measured by state and quarter throughout the Progressive Era, the business bankruptcy rate. 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 these three requirements. First, elections' timing was exogenous. The electoral schedule was established long in the past and was not changed by economic and financial conditions or by states' creation of bank regulatory agencies. Second, governors and their

subordinates could not dictate the timing of business bankruptcies. Unpaid creditors and federal courts did that. Governors also lacked fiscal and legal tools which would allow them to manipulate business bankruptcy rates over a short span of time, such as the three to six months of a progressive era political campaign. Statistical evidence substantiates this observation; business bankruptcies did not decline systematically during gubernatorial campaigns when governors lacked authority over bank resolution. So, gubernatorial campaigns satisfy the exclusion restriction. Third, governors and their subordinates reduced bank resolution rates 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, business bankruptcy rates – and the instrument. The regression that establishes this relationship, which is presented in Table 4, is called the reduced form. Column 1 reports the baseline OLS estimate. The business bankruptcy rate for each state *i* in quarter *t* is regressed on a constant and the instrument, which is an indicator equal to one in each quarter that a state had a bank regulatory agency and a gubernatorial campaign. The instrument's coefficient indicates that during campaigns when the governor had the power to influence the bank resolution rate, the business bankruptcy rate was 0.0003 (or 3 per ten thousand) lower than the average of 0.0024 (or 24 per ten thousand), a decline of 12.5% relative to the sample mean. Column 2 adds fixed effects and additional controls. The coefficient on *Campaign*Agency* remains significant and substantial. It falls to 0.00013 (or 1 per ten thousand), a decline of 5% relative to the sample mean. This decline is robust across a wide range of specifications using additional specifications and controls.

To check the exclusion criteria and present reduced-forms similar to those underlying control functions, Columns 3 and 4 present results for specifications similar to column 2 split into subsamples.

The sample in column 3 includes only observations with a bank regulatory agency controlled by the governor. The statistical result resembles that for the previous columns. This subsample result should be kept in mind because Wooldridge (2015) recommends against using interaction terms with control functions. In our case, this recommendation leads us to compare results between subsamples of states with and without regulatory agencies. 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 had bank regulatory agencies, in other words, business bankruptcies on average declined during gubernatorial campaigns. When states lacked bank regulatory agencies, gubernatorial campaigns were uncorrelated with bankruptcies of firms. This result helps to confirm the exclusion criteria.

The OLS regressions could be biased, because the business bankruptcy rate is bounded between 0 and 1. To account for this fact, columns (5) to (8) re-estimate the specifications in the initial four columns using the fractional logit model of Papke and Wooldridge (1996). 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 shows that the reduced form regression is close to linear. Estimating it using OLS does not bias the results.

If our first and second stages were linear, our IV estimate would be the instrument's coefficient in the reduced form divided by the instrument's coefficient in the first stage. Complications arise because both stages of our analysis – particularly the first – are non-linear. So, results from standard instrumental variables methods could be biased. Credible causal inference in this setting is an active area of research. Recommended procedures include either control functions or assuming linearity in the first stage.

The control function approach of Wooldridge (2008, 2015) provides consistent estimates of causal effects in applications like ours. A control function is a variable that, when added to a

regression, renders the variable of interest appropriately exogenous (Wooldridge 2008, Heckman and Robb 1985). The control-function procedure consists of two stages. The second stage examines the relationship between the ultimate variable of interest, in our case the business bankruptcy rate, and an endogenous explanatory variable (EEV), in our case the bank resolution rate. The first stage uses an instrumental variable to illuminate exogenous variation in the EEV. The residuals from this regression serve as the control function and are added to the second stage, rending the EEV appropriately exogenous in the second stage estimating equation.

Control functions have several properties that make them particularly appealing in our application. While control functions yield identical results to standard IVs in typical settings, such as linear models with constant coefficients, control functions can be employed in situations where standard IVs cannot. Control functions parsimoniously handle models that are nonlinear in endogenous explanatory variables (EEVs). Their properties have been explored in the estimation of a probit panel with a continuous EEV (Rivers and Vuong 1988). Control functions also work with discrete EEVs (Terza, Basu, and Rathouz 2008). In our application, the bank resolution rate, R_{it}, is a continuous EEV, where $R_{it} \in [0,1]$. Whether the resolution rate equals zero, B_{it}, is a discrete EEV, where B_{it}=1 if R_{it}=0 and is 0 otherwise. Our ZIB estimates indicate that zero or not is an effective way to model governor's influence on bank resolutions during gubernatorial campaigns.

Our first stage regresses the EEV on the instrument, C_{it}, *Campaign;* a vector, θ_{it}, that includes year and quarter fixed effects; a vector of exogenous variables that includes the Palmer drought severity index and the farm failure rate, X_{it}; and state-level averages of exogenous

explanatory variables, $\bar{X}_i$.[20] Mundlak (1978) and Wooldridge (2015) recommend using state averages, rather than state indicators, to control for time-constant state-level heterogeneity because Mundlak devices improve the accuracy and efficiency of control function estimates. Our first stage equation is:

(3) $$Y_{it} = \phi(\beta_1 C_{it} + \varphi_1 \bar{X}_i + \psi_1 X_{it} + \theta_{1t} + \varepsilon_{it})$$

The EEV, Y_{it}, is either continuous, R_{it}, or discrete, B_{it}. $\phi(\cdot)$ is a linear function for the former and the standard normal cumulative distribution function for the latter. The residuals from this regression are $\widetilde{Y_{it}}$. For discrete EEVs, we use as a control function the generalized residuals:

(4) $$\widetilde{B_{it}} = \text{Bit} \times \lambda\left(\hat{\beta}_1 C_{it} + \hat{\varphi}_1 \bar{X}_i + \hat{\psi}_1 X_{it} + \widehat{\theta_{1t}}\right) - (1 - \text{Bit}) \times \lambda\left(-\hat{\beta}_1 C_{it} - \hat{\varphi}_1 \bar{X}_i - \hat{\psi}_1 X_{it} - \widehat{\theta_{1t}}\right)$$, where $\lambda(\cdot)$ is the Inverse Mills Ratio.

The second stage regresses the business bankruptcy rate, F_{it}, on the EEV, controls, year fixed effects, and residuals from the first stage. The regression is a fractional pooled probit. The equation is:

(5) $$F_{it} = \psi\left(\vartheta_Y Y_{it} + \vartheta_{\tilde{Y}} \widetilde{Y_{it}} + \varphi \bar{X}_i + \psi X_{it} + \theta_t + \epsilon_{it}\right).$$

The exogeneity of the bank resolution rate, Y_{it}, can be determined by testing the null hypothesis that $\vartheta_{\tilde{Y}} = 0$. If the null cannot be rejected, Y_{it} is exogenous. If the null can be rejected, then Y_{it} is endogenous. Its endogeneity should be accounted for by including the first-stage residuals into the second stage regression.

The average partial effects of the continuous EEV, A_R, are calculated with the following formula:

(6) $$A_R = \left[{}^{1}/_{NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \Phi\left(\widehat{\vartheta_R} R_{it} + \widehat{\vartheta_{\tilde{R}}} \widetilde{R_{it}} + \hat{\varphi} \bar{X}_i + \hat{\psi} X_{it} + \hat{\theta}_t\right) \times \widehat{\vartheta_R}\right]$$

[20] Note that we can replace the farm failure rate with the Palmer index and its lags, as in the reduced form, and get essentially the same results. Since we use a Mundlak device to control for state fixed effects and time-constant heterogeneity, we cannot use lagged values.

In this equation,$\Phi(\cdot)$ is the standard normal density. An overhat accent, such as $\widehat{\vartheta_R}$, indicates the coefficients estimated via equation (5), with subscript R indicating estimates for the continuous EEV and B indicating estimates for the discrete case. The average partial effect of the discrete EEV, A_B, can be calculated as:

$$(7) \qquad A_B = \left[{}^{1}/_{NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \Big(\Phi\big(\widehat{\vartheta_B} B_{it} + \widehat{\vartheta_{\tilde{B}}} \widetilde{B_{it}} + \hat{\varphi} \bar{X}_i + \hat{\psi} X_{it} + \hat{\theta}_t\big) - \Phi\big(\widehat{\vartheta_{\tilde{B}}} \widetilde{B_{it}} + \hat{\varphi} \bar{X}_i + \hat{\psi} X_{it} + \hat{\theta}_t\big)\Big)\right]$$

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 5. 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 campaigns. Columns 3 and 4 show our preferred specification that includes indicators for years and quarters as well as the level and state-average Palmer drought index and farm failure rate. These regressions exclude the interest rate, which could be endogenous, and quarters with elections, to simplify estimation. Robustness checks including those variables and observations yield the same result. Odd numbered columns present results for observations with bank regulatory agencies. Even numbered columns present results for observations without regulatory agencies. The results differ clearly across columns. In columns (1) and (3), the coefficients and partial effects for the discrete EEV are negative and statistically significant. The average partial effect ranges from -0.0029 to -0.0035. Reducing the resolution rate to zero, in other words, reduced the business bankruptcy rate by 0.0003 (3 per ten thousand) or 12.5% of the mean business bankruptcy rate of 0.00239 (24 per ten thousand). The control function estimates for the first stage residuals are significant. So, we reject the null hypothesis that bank resolutions are exogenous. In columns 2 and 4, the estimated marginal effects for the discrete

EEV are statistically insignificant. These columns serve as a placebo test. They show that in the Progressive Era, a link between gubernatorial campaigns, bank resolutions, and business bankruptcies existed only when governors and their subordinates had regulatory powers over bank resolutions. When governors lacked that authority, business bankruptcies neither rose nor fell on average during gubernatorial campaigns. This pattern corroborates our conjecture that Progressive-Era reforms which placed the bank-resolution process under political control linked political and economic cycles in the early 20th century United States.

Columns 5 through 8, which report results for the bank resolution rate, have the same structure as the first four columns. The results are also consistent with those from the first four columns, although the coefficients and partial effects are easier to interpret. Our preferred specification in column 7 indicates that a 1% decline in the bank resolution rate lowered the business bankruptcy rate by 0.27%. At the mean value of the bank resolution rate 0.00237, this estimate implies a reduction in the business bankruptcy rate by 26.7%. Overall, the estimates in Table 5 reveal a significant and sizable effect of bank resolution rate on the business bankruptcy rate.

To check the robustness of our results, we also estimate our two-stage system as 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 the forbidden regression (Angrist and Pischke, 2008, pp. 190-192). The first stage is:

$$(8) \qquad Y_{it} = \alpha_1 + \gamma C_{it} + \omega_1 X_{it} + \tau_1 \bar{X}_i + \delta_{1t} + \varepsilon_{it}$$

where X_{it} is a vector of exogenous control variables that includes the Palmer drought severity index and the farm failure rate, $\bar{X}_i$ is a vector of state-level averages of the exogenous explanatory variables, and δ_{1t} are time fixed effects. The second stage is:

$$(9) \qquad F_{it} = \alpha + \varsigma Y_{it} + \omega X_{it} + \tau \bar{X}_i + \delta_t + \epsilon_{it}$$

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

Results from this exercise appear in Table 6. The estimated IV coefficients are very similar to the control function's average partial effects. For instance, the average partial effect in Column 5, 0.273, is nearly identical to the corresponding control function estimate, 0.274, in Column 7 of Table 5. We employ the Montiel-Pflueger weak instrument test because it is robust to heteroskedasticity, autocorrelation, and clustering. For states with bank regulatory agencies, we reject the null of weak instruments. For states without agencies, however, we fail to reject the null.[21] This is expected, since these columns serve as placeboes The results of these placebo tests are consistent with our hypothesis, since in states with independent bank resolution procedures, gubernatorial elections should influence neither the bank resolution nor business bankruptcy rates. Gubernatorial campaigns, in other words, should not be an effective instrument in these observations.

Overall, the results from the IVs are consistent with the results from control functions. The IVs' marginal effects estimated are close to the control functions' average partial effects. This consistence suggests that any imprecision or bias in the IV estimates due to the bounded nature of business bankruptcy and bank resolution rates is small.

A series of placebo tests (reported in online replication files) provide additional evidence of the validity of empirical strategy. The placebos estimate the IV regressions on samples restricted to New England or Southern states.[22] In New England, bank regulation was strict. Strictness

[21] The F-tests are above the critical values for the weak instrument threshold τ=20% at the 5% level for the full sample and for states with bank regulatory agencies for the discrete EEV. The F-test is above the τ=30% for the continuous EEV. For states with regulatory agencies in other words, we reject the null that gubernatorial campaigns are a weak instrument. For observations without a regulatory agency, however, the F-tests are below the critical value τ=30%. So, we fail to reject the null of weak instruments, as expected.

[22] The South is defined as Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, Missouri, North Carolina, South Carolina, Texas, Virginia, and West Virginia. New England is defined as Connecticut, Maine, Massachusetts, New Hampshire, Rhode Island, and Vermont.

stemmed both from Puritan government philosophies and the private regional check-clearing cooperative. Stringency kept resolution rates at zero in most (over 90%) quarters and near zero otherwise. Since banks seldom failed, governors could not lower bank resolution rates during campaigns. Given these circumstances, it is not surprising that restricting the sample to New England yields coefficients on our instrument close to zero. In Southern states, the Democratic party had dominated politics since Reconstruction. Democratic dominance reduced the incentive to delay resolutions preceding elections against Republican opponents. Given these circumstances, it is not surprising that restricting the sample to Southern states yields a coefficient on our instrument significantly lower than in states in the Mid-Atlantic, Midwest, Great Plains, Mountains, and Pacific West, where political competition could be intense.

We check the external validity of our estimates by comparing them to coefficients derived from alternative instruments based on panics identified by Jalil (2015). Jalil uses newspaper reports to construct a series of national and regional banking panics. He uses narrative evidence to identify exogenous panics and determine their impact on aggregate industrial production. From Jalil's data, we construct two binary instruments indicating the timing and location of exogenous banking panics. One is for states afflicted by the Panic of 1907. The other is for states impacted by all major or minor banking panics that occurred during the period of our study. We substitute this instrument for our own and rerun our IV regressions. Results of this exercise appear in Table 7. They validate and illuminate our findings. Average partial effects from control functions resemble the coefficients from IV regressions. The null hypothesis that they are identical cannot be rejected. Coefficients on the EEV (i.e. bank resolutions measured as a rate or whether equal to zero) indicate that increasing bank resolutions lead to increases in business bankruptcies. The magnitudes of the

effects measured from Jalil's panic series (0.22 to 0.23) resemble those from our gubernatorial campaigns (0.27 to 0.30), although the latter are consistently larger.

5. Discussion

Regulatory independence is a pillar of financial regulation. The institution's ubiquity and longevity complicate efforts to understand its impact. This essay examines a Progressive Era policy experiment in which U.S. states shifted from politically independent to politically subservient processes for resolving troubled banks. After the switch, state regulators who worked for governors used their new-found authority to defer bank resolutions during gubernatorial election campaigns. This forbearance, in turn, substantially reduced bankruptcy rates for firms that borrowed from banks during these campaigns. The exogenous timing of campaigns allows them to serve as an instrument for examining the relationship between bank resolution and business bankruptcies.

Our results indicate that subjecting regulators to political supervision could link electoral and economic cycles. While the aggregate impact of this linkage in the Progressive Era was substantial, the impact today could be larger. Over the last century, financial services' share of GDP has risen to 9% from 2%. Use of credit by households and leverage in firms has also risen substantially. The same is true for the international influence of the American financial sector, the international prominence of the Federal Reserve, and the size of international capital flows. Political shocks to the United States financial network may, therefore, propagate more today than they did during the Gilded Age or Roaring 20s.

Understanding the full impact of Progressive Era subservice of financial regulators requires additional research. Two issues remain to be explored. The first is the post-election rebound in bank resolution rates. The quarterly data examined in this essay obscures the rebound because elections occurred amidst quarters. So, quarters with elections contained pre-election spans of suppressed resolutions and post-election periods of unsuppressed and potentially catch-up resolutions. Examining

the rebound requires higher-frequency data and different methods. Our preliminary examination of the daily resolution data that we have completed for the 1920s and 1930s indicates resolution rates rebounded in the months (and sometimes weeks) after gubernatorial elections, as regulators rushed to resolve troubled banks whose resolution had been deferred during the gubernatorial campaign.

A second issue requiring further investigation is the impact outside of gubernatorial campaigns of shifting to a system of professional regulators employed by the government from a laisse faire system of regulating banks via the market and resolving disputes about banks (including resolving troubled banks) via courts. Our initial estimates suggest that the professional regulatory apparatus worked as intended. On an ongoing basis, regulators examined banks, detected problems, and closed institutions whose problems could not be corrected. So, the percentage of quarters with zero bank resolutions declined. This continuous monitoring reduced the clustering of bank failures and possibly the frequency of panics. So, the percentage of quarters with high bank resolution rates also declined. Professional regulation, in other words, appears to have smoothed failure rates over time. This essay's focus on gubernatorial campaigns obscures this broader issue, which we plan to examine in future research.

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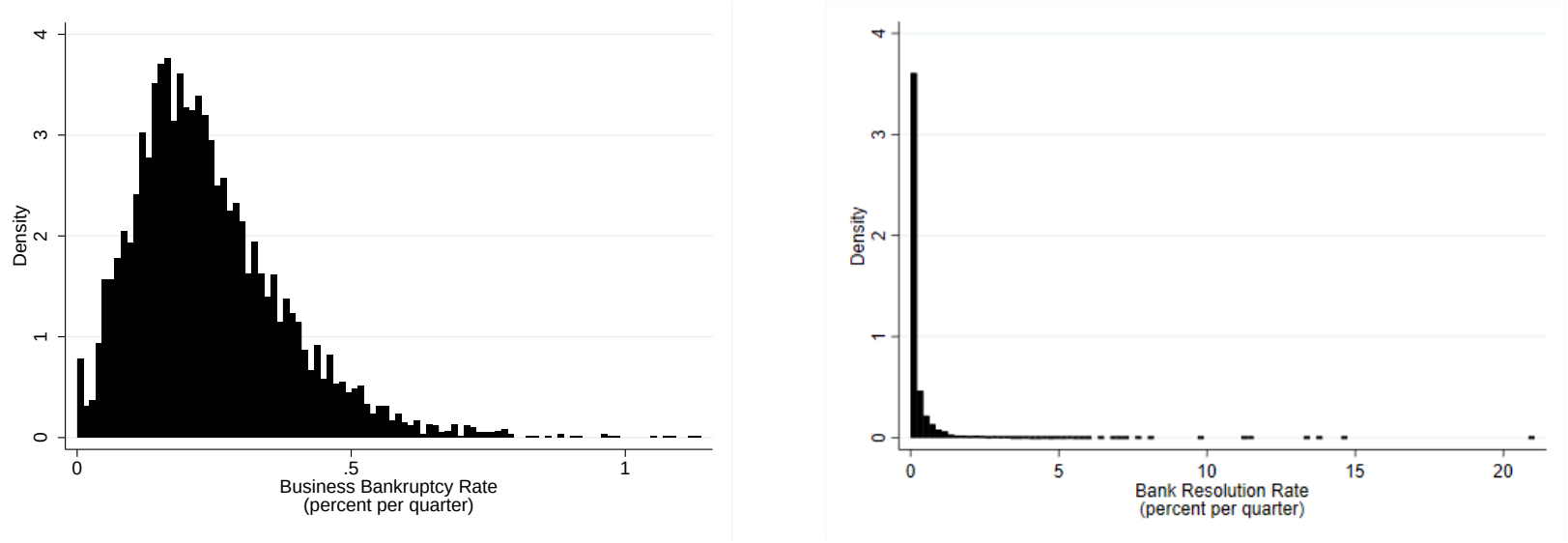
Figure 1: Quarterly Failure Rates for Business and Banks, 1902 to 1929



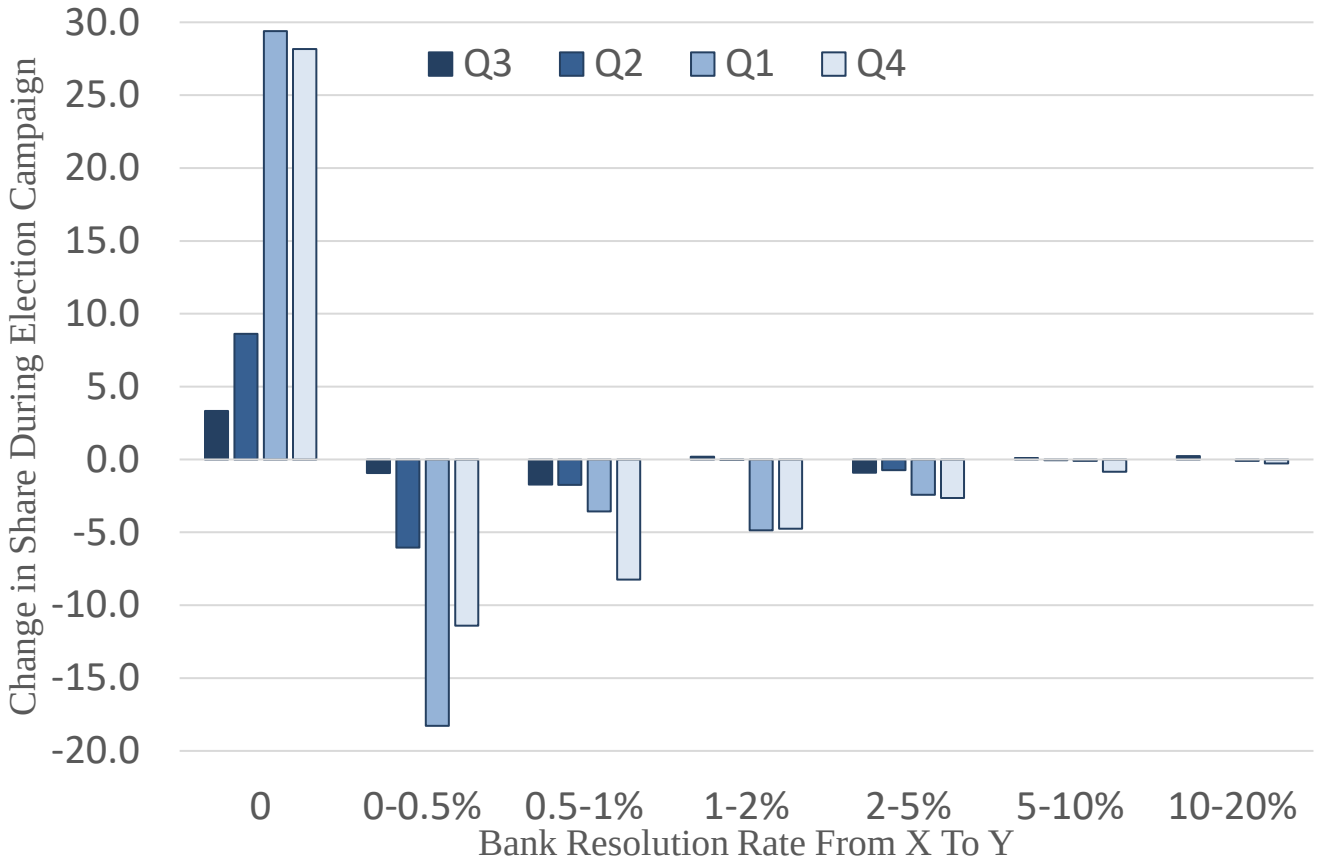
Source: Authors' calculations. See Section 2 and Appendix for sources and methods. Resolution rate is for state-chartered banks.

Table 1. Bank Resolution and Business Bankruptcy Rates, 1902 to 1929

	Resolutions Per Thousand Banks				Bankruptcies 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 sources.

Figure 2: Resolution Rates Shift to Zero During Campaigns When Governors Controlled Bank Regulation



Notes: Figure plots difference in the share of state bank resolution rates in each range between quarters with and without gubernatorial campaigns (excluding quarters with elections). During campaigns in the second quarter (Q2), for example, the percentage of observations with the resolution rate of zero rose by 8.6 percentage points while the percent of observations with resolution rate between 0% and 0.5% fell by 6 percentage points.

Table 2. Gubernatorial Campaigns and Bank Resolution Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Resolution 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 Resolutions During Gubernatorial Campaigns										
Shift to Zero	285	601	315	343	343	400				
Decline in Rate	286	16	181	72	72	70				
Total	571	617	496	415	415	470				
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	4,119	4,756	4,756	5,264	5,170	5,170	5,264	5,264	5,170	5,170

Notes: Estimates via zero inflated beta distribution. Top equation estimates probability rate equals zero. Bottom equation estimates rate if not zero. Dependent variable 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. ~Agency indicates observations with state-bank regulatory agency excluded .~E indicates observations with elections excluded. "Exog" indicates exogenous contemporaneous variables, which include Palmer drought index. "Lag" indicates lagged variables (e.g. farm failure rate, business bankruptcy rate, and interest rate). Samples with elections include indicators for election and interaction with agency. Clustered standard errors at year-quarter level in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Gubernatorial Campaigns and Bank Resolutions, 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		
Depend. Variable Resolution Rate	= 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 Campaigns												
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					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: Probit and logit reports pseudo R2. Fraction logit estimated via method of Papke and Wooldridge (1996). Additional controls include indicator for election, interaction of election and campaign, Palmer drought index, and four quarterly lags of business bankruptcy rate, farm failure rate, and interest rate. State average controls include mean of Palmer drought index and farm failure rate. . ~Agency indicates observations with state-bank regulatory agency excluded. Clustered standard errors at year-quarter level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Business Bankruptcies 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				

Notes: Additional controls include indicator for election, campaign, existence of regulatory agency, interaction of election and agency, and four quarterly lags of the Palmer drought index and its square. Sample "Agency" indicates sample restricted to observations with a state-bank regulatory agency. ~Agency indicates observations with state-bank regulatory agency excluded. Clustered standard errors at year-quarter level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. 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	-0.476***	-0.171	-0.400***	-0.162	35.42***	96.01	36.67***	94.44
	(0.104)	(0.288)	(0.117)	(0.303)	(8.989)	(58.56)	(9.026)	(58.21)
EEV Residuals	0.282***	0.0656	0.231***	0.0662	-34.11***	-91.48	-35.17***	-90.57
	(0.0606)	(0.165)	(0.0694)	(0.178)	(8.897)	(59.18)	(9.021)	(58.93)
Average Partial Effect	-0.00355***	-0.00129	-0.00299***	-0.00122	0.264***	0.722	0.274***	0.708
	(0.000789)	(0.00226)	(0.000883)	(0.00239)	(0.0689)	(0.443)	(0.0695)	(0.442)
Sample	Agency	~Agency	Agency	~Agency	Agency	~Agency	Agency	~Agency
Time Controls	N	N	Y	Y	N	N	Y	Y
State Average Controls	N	N	Y	Y	N	N	Y	Y
Additional Controls	N	N	Y	Y	N	N	Y	Y
Observations	4,568	680	4,568	680	4,568	696	4,568	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. "~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. Additional controls include Palmer drought index and farm failure rate. *** p<0.01, ** p<0.05, * p<0.1

Table 6. IV Estimates of Gubernatorial Campaigns' Impact on Business Bankruptcies

	Discrete EEV Resolution Rate = 0			Continuous EEV Resolution Rate		
	(1)	(2)	(3)	(4)	(5)	(6)
EEV	-0.0047***	-0.0043***	-0.0016	0.3039***	0.2728***	0.7135**
	(0.0008)	(0.0007)	(0.0019)	(0.0586)	(0.0566)	(0.3357)
Montiel-Pflueger robust weak instrument test	11.86	11.65	3.57	10.33	8.37	1.57
Sample	All	Agency	~Agency	All	Agency	~Agency
Time Controls	Y	Y	Y	Y	Y	Y
State Average Controls	Y	Y	Y	Y	Y	Y
Additional Controls	Y	Y	Y	Y	Y	Y
Observations	5264	4568	696	5264	4568	696

Note: Sample agency includes observations from all quarters in which a state has a bank regulatory agency. Sample ~Agency includes observations from all quarters in which a state does not have a bank regulatory agency. For sample agency and ~Agency, the instrument is an indicator for gubernatorial campaign. State average controls include state mean of Palmer drought index and farm failure rate. Additional controls include Palmer drought index and farm failure rate. When the sample includes all observations, the instrument is an indicator for gubernatorial campaigns in states with bank regulatory agencies, or in other words, the interaction term campaign*agency.

Table 7. IV Estimates of Banking Panics' Impact on Business Bankruptcies Using Conventional Instruments

	Discrete EEV Resolution Rate = 0				Continuous EEV Resolution Rate			
	Control Function		IV		Control Function		IV	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Instrument:	1907 Panic	All Panics	1907 Panic	All Panics	1907 Panic	All Panics	1907 Panic	All Panics
EEV	-0.3579***	-0.5776***	-0.0029***	-0.005***	30.547***	29.343***	0.230***	0.219***
	(0.069)	(0.123)	(0.001)	(0.001)	(6.56)	(8.375)	(0.0568)	(.047)
EEV Residuals	0.2006***	0.333***			-28.746***	-27.577***		
	(.040)	(0.074)			(6.61)	(8.347)		
Average Partial Effect	-.0026***	-0.004***			0.227***	0.218***		
	(0.0005)	(0.0009)			(0.051)	(0.061)		
Montiel-Pflueger Robust Weak Instrument Test			20.8	3.9			10.1	9.23
Time Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Average Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,376	5,376	5,376	5,376	5,376	5,376	5,376	5,376

Note: Bootstrapped standard errors in parentheses for control function regressions. Clustered standard errors at state level for IV regressions are reported in parentheses. Time controls include year and quarter fixed effects. State average controls include state mean of Palmer drought index and farm failure rate. Additional controls include Palmer drought index and farm failure rate. *** p<0.01, ** p<0.05, * p<0.1

Data Appendix
For Online Distribution Only

This appendix discusses details of the data and issues that influence our analysis. Key issues include definitions of bankruptcies (by firms) and resolutions (by banks), sources of information for our control variables, and the nature of the data generating process for firm bankruptcies and bank resolutions.

Bankruptcies of Businesses

Data on bankruptcies of firms comes from publications of R.G. Dun and Company (a predecessor of today's Dun and Bradstreet Corporation). 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 the monthly reviews and annual reports of the Federal Reserve banks and board. Dun's data formed the basis of articles published in newspapers such as the New York Times, Wall Street Journal, and Commercial and Financial Chronicle. Dun's Review noted the popularity of its data 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 fact that both businessmen and bureaucrats used Dun's data indicates that they found it valuable. Dun's data on business bankruptcies was certainly watched by everyone interested in economic trends from the 1890s through the 1930s.We process these sources to create accurate, consistent, and high-frequency data on bankruptcies of firms from 1901 through 1929 by state. Dun's collected this information by establishing a reporting network which collected information on court filings in every county in the United States during each month of the year. Dun's published data on bankruptcies disaggregated by manufacturing, trading and other economic sectors, defined according to classifications devised by the census bureau in the early 1890s. Dun's defined a business bankruptcy 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 destruction of the organization). Personnel bankruptcies of profession individuals such as doctors, dentists, and lawyers were excluded. This data includes all bankruptcy proceedings filed under the Bankruptcy Act of 1898. Dun's published this series using consistent definitions through the mid 1930s. Then, the census bureau switched from classifying firms according to branches of business to classifying firms according to division of industry. Congress passed the Bankruptcy Act of 1934 (and subsequent legislation) which altered the nature of bankruptcy throughout the United States. Congress also passed Banking Acts in 1933 and 1935.

We followed a similar procedure than the one used by Richardson and Gou (2011). In a first step, we collected resolution data from existing publications. In a second step, we checked for typos, transpositions and missing information generated by the digitization process and corrected those errors. To check for typos and data entry errors, we summed disaggregated data and checked if our sum equaled the totals reported in the original sources. We corrected all errors whose cause seemed clear, but we left other minor discrepancies in the data. In a fourth step, we calculated quarterly data when the information was not reported on a quarterly basis.

For years 1900 to 1920, Dun's published data on commercial bankruptcies for half of the year and for the first nine months of the year. For all years in our sample Dun's reported annual

aggregates instead of data for the fourth quarter. To obtain data for the second, third and fourth quarters we extracted the missing information using the following formulas:

(A.1) $Q_{y,\,2} = Q_{y,\,\text{half year}} - Q_{y,\,1}$

(A.2) $Q_{y,\,3} = Q_{y,\,\text{nine months}} - Q_{y,\,\text{half year}}$

(A.3) $Q_{y,\,4} = Q_{y,\,\text{total}} - Q_{y,\,\text{nine months}}$

Where $Q_{y,\,t}$ indicates business bankruptcies in quarter t of year y , with t={1, 2, 3, 4} and y= {1901, …, 1929}.

In a fifth step, we calculate the number of new firms per quarter using data on the annual number of business concerns from the Statistical Abstract of the United States. We take this Data is from the *Statistical Abstract of the United States* years 1901, 1904, 1907, 1910, 1913, 1916, 1919, 1921, 1923, 1925, 1928, 1930, and 1931. The titles and pages of the tables appears in our Table A.1 below. The *Abstract* reports the total number of firms at the end of a calendar year (or synonymously, the beginning of the next calendar year). We have not been able to find a source that reports the total number of firms at a higher frequency or at a different date. We have also not been able to locate a source that reports the number of new business firms. This data allows us to calculate the annual rate of business bankruptcies and also to calculate the quarterly rate of business bankruptcies assuming that the rate of new firm formation is the same in all four quarters of a year. With this assumption, we calculate the number of new firms per quarter using the following formula:

(A.4) $\bar{N} = [(N_{t+4} - N_t) + \sum_{i=0}^{3} F_{t+i}]/4$

Where N_t is annual number of firms in quarter *t*, and F_t is number of commercial failures in quarter *t*. For instance, to calculate the quarterly number $\bar{N}$ of new firms from Q3.1901 to Q3.1902, we use N_t as the total number of firms in 1901 reported in Q3.1901, N_{t+4} is the total number of firms in 1902 reported in Q3.1902, and F_t is the number of business bankruptcies in Q3.1901. If the number $\bar{N}$ is not an integer, we distributed the remainder among the quarters using the following rule:

A. If the remainder of equation (4) is 0.25 the only remaining firm is added to quarter 4 of year 1
B. If the remainder of equation (4) is 0.5 each of the two remaining firms are added to quarter 4 of year 1 and quarter 1 of year 2
C. If the remainder of equation (4) is 0.75 each of the three remaining firms are added to quarter 4 of year 1 and quarters 1 and 2 of year 2.

For example, if $\bar{N}$ is 1.75, $\bar{N}_{Q4Y1}$ =2, $\bar{N}_{Q1Y2}$ =2, $\bar{N}_{Q2Y2}$ =2 and $\bar{N}_{Q3Y2}$ =1, for a total of 7 new firms from Q3Y1 to Q3Y2. The number of banks per quarter is calculated using the following formula :

(A.5) $B_t = \bar{N}_t - F_{t-1}$

In a last step, we calculate the quarterly rate of business bankruptcies in quarter *t* by dividing the number of commercial failures F_t, by the number of banks in quarter *t* B_t.

Bank Resolutions

Quarterly data on bank resolutions for state-chartered banks by state from 1901 to 1929 come from the same source as bankruptcies of firms. We calculate resolution rates for nationally chartered banks from the Annual Reports of the Comptroller of Currency.

Election Data

Gubernatorial and presidential election dates are from Dubin (2003).

Farm Failure Rate

We calculate farm failure rates using data on annual number of farm failures from Wickens (1936) and number of farms for years 1900, 1910, 1920, and 1930 from the *Statistical Abstract of the United States*. We construct quarterly farm failures and number of farms by interpolating annual series using Stata's *ipolate* command. Farm failure rate is defined as the number of farm failures divided by the number of farms.

Palmer Drought Severity Index

Monthly Palmer Drought Severity Index is from the National Oceanographic and Atmospheric Administration Historical Palmer Drought Indices database. Quarterly Palmer Drought Severity Index is the average of monthly data.

A1. Tables from the Statistical Abstract of the United States

Year	Title and Page
1901	"Failures of Commercial and Business Concerns; Per cent of Failures and Aggregate Liabilities, by States and Territories, For the Calendar Years 1899, 1900 and 1901," p. 407
1904	"Failures of Commercial and Business Concerns; Per cent of Failures and Aggregate Liabilities, by States and Territories, For the Calendar Years 1902, 1903 and 1904," p. 413
1907	"Commercial Failures, Calendar Years 1905, 1906 and 1907: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and Territories, Arranged Geographically," p. 544
1910	"Commercial Failures, Calendar Years 1908, 1909 and 1910: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and Territories and by Geographical Divisions," p. 502
1913	"Commercial Failures, Calendar Years 1911, 1912 and 1913: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and by Geographic Divisions," p. 605
1916	"Commercial Failures, Calendar Years 1914, 1915 and 1916: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and by Geographic Divisions": Page 207
1919	"Commercial Failures, Calendar Years 1917, 1918 and 1919: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and by Geographic Divisions": Page 224
1921	"Commercial Failures, Calendar Years 1919, 1920 and 1921: Percentage to Total Numbers of Concerns in Business, and Aggregate Liabilities, By States and by Geographic Divisions": Page 292
1923	"Commercial Failures: By States and by Geographic Divisions"; Page 681
1925	"Commercial Failures: By States"; Page 307
1928	"Commercial Failures: By States"; Page 313
1930	"Commercial Failures: By States"; Page 318
1931	"Commercial Failures: By States"; Page 330

A2: Tables from Dun's Review

Year	Title, Issue, and Page
1900	"Commercial Failures"; Apr 7, p2; Jul 7, p2; Oct 6, p2; Jan 5 1901, p7.
1904	"Commercial Failures"; Apr 2, p5; Jul 2, p5; Oct 8, p5; Jan 7, 1905, p5.
1905	"Commercial Failures"; Apr 8, p5; Jul 8, p5; Oct 7, p5; Jan 6, 1906, p11.
1906	"Commercial Failures"; Apr 7, p9; Jul 7, p5; Oct 6, p5; Jan 5, 1907, p12.
1907	"Commercial Failures"; Apr 6, p9; Jul 6, p5; Oct 5, p5; Jan 11, 1908, p14.
1908	"Commercial Failures"; Apr 4, p9; Jul 4, p5; Oct 8, p5; Jan 9, 1909, p15.
1909	"Commercial Failures"; Apr 3, p9; Jul 3, p9; Oct 2, p9; Jan 8, 1910, p12.
1910	"Commercial Failures"; Apr 9, p7; Jul 9, p7; Oct 8, p5; Jan 7, 1911, p12.
1911	"Commercial Failures"; Apr 8, p8; Jul 3 ; Oct 7, p9; Jan 6, 1912, p12.
1912	"Commercial Failures"; Apr 6, p9; Jul 6 p9; Oct 5, p9; Jan 4, 1913, p27.
1913	"Commercial Failures"; Apr 5, p11; Jul 5 p8; Oct 4, p9; Jan 10, 1914, p24.
1914	"Commercial Failures"; Apr 11, p14; Jul 4 p4; Oct 3, p9; Jan 9, 1915, p18.
1915	"Commercial Failures"; Apr 3, p9; Jul 3 p9; Oct 2, p9; Jan 8, 1916, p18.
1916	"Commercial Failures"; Apr 8; Oct 7; Jan 6, 1917, p15.
1917	"Commercial Failures"; Apr 7, p5; p5; Oct , p5; Jan 5, 1918, p15.
1918	"Commercial Failures"; Apr 6, p5; Jul 6 p9; Oct 5, p9; Jan 11, 1919, p7.
1919	"Commercial Failures"; Apr 5, p9; Jul 5 p9; Oct 4, p5; Jan 10, 1920, p13.
1920	"Commercial Failures"; Apr 3, p9; Jul 3 p9; Oct 9, p9; Jan 8, 1921, p20.
1921	"Commercial Failures"; Apr 9, p9; Jul 9 p9; Oct 8, p9; Jan 7, 1922, p18.
1922	"Commercial Failures"; Apr 8, p9; Jul 8 p9; Oct 14, p9; Jan 13, 1923, p19.
1923	"Commercial Failures"; Apr 7 p9; Jul 7 p9; Oct 6, p9; Jan 12, 1924, p9.
1924	"Commercial Failures"; Apr 5 p9; Jul 5 p9; Oct 11, p9; Jan 10, 1925, p7.
1925	"Commercial Failures"; Apr 11 p9; Jul 11 p9; Oct 10, p9; Jan 9, 1926, p24.
1926	"Commercial Failures"; Apr 10 p9; Jul 10 p9; Oct 9, p9; Jan 8, 1927, p24.
1927	"Commercial Failures"; Apr 9 p9; Jul 9 p9; Oct 8, p9; Jan 14, 1928, p7.
1928	"Commercial Failures"; Apr 7 p9; Jul 14 p9; Oct 13, p9; Jan 12, 1929, p9.
1929	"Commercial Failures"; Apr 13 p9; Jul 13 p9; Oct 12, p9; Jan 11, 1930, p9.
1930	"Commercial Failures"; Apr 12 p9; Jul 19 p9; Oct 11, p9;