CRISES AND THE DEVELOPMENT OF ECONOMIC INSTITUTIONS: SOME MICROECONOMIC EVIDENCE

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This paper studies the long run effects of financial crises using new bank and town level data from around the Great Depression. We find evidence that banking markets became much more concentrated in areas that experienced a greater initial collapse in the local banking system. This con There is also evidence that financial regulation after the Great Depression, and in particular limits on bank branching, may have helped to render the effects of the initial collapse persistent. All of this suggests a reason why post-crisis financial regulation, while potentially reducing financial instability, can also have longer run real consequences.

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Introduction

How do economic institutions develop? Much of the recent literature has emphasized the long arc of history, where the nature of early legal systems (e.g., LaPorta, Lopez de Silanes, Shleifer, and Vishny (1998)), constitutions (North and Weingast (1989)), colonial origins (e.g., Aceomglu, Johnson, and Robinson (2001)), or technologies of production (Marx (1848) and Engerman and Sokoloff (2002)) influence the development of economic institutions, and subsequent growth. Of course, these views do not suggest that history is destiny – for after all, these theories are predicated on strong initial social upheavals that establish colonies, constitutions, or legal systems, or initial radical innovations that change the technology of production significantly. So these theories suggest some degree of persistence, but subsequent upheavals could change the underlying factors that determine economic institutions.

In this paper, we provide suggestive evidence that large economic upheavals can affect economic institutions, and influence the subsequent political economy of economic development. Specifically, we study banking structure in different towns in the United States, after the Great Depression led to a massive cull of the banks. We show that across nearly two thousand towns in the United States, those towns that suffered greater national bank failures during the 1930s tended to have significantly fewer banks well up until the 1994 Riegle-Neal Act permitted greater banking competition. Interestingly, there is a notable exception. Towns that suffered a total failure of all national banks tended to have more banks in subsequent years than towns that suffered some failures but not a total collapse. This evidence is consistent with a crisis-driven increase in concentration of the banking sector empowering incumbents who could then affect the subsequent development of the local banking sector.

A number of other factors could explain this persistence, including lower demand for finance in the towns most affected by the Depression. However, we find that for towns located in states that deregulated their banking system in the 1970s and 1980s, the correlation between the Depression-era failures and the subsequent number of banks weakens considerably in the years immediately after the state began permitting intra-state branching. That is, regulations limiting branching and the spread of banking, many strengthened in the wake of the Great Depression, might have been a key source of power and rents for those incumbents that survived the failures of the 1930s. And local incumbents might have successfully blocked entry in these towns for many decades until the state-led and then federal dismantling of these regulations.

There is a growing literature on the effect of economic upheavals on changing the political economy of development. For instance, North and Thomas (1973) describe the economic and institutional consequences of the Black Death in Europe in the 14th century, while Olson (1982) argues that the Second World War promoted growth in Europe by destroying various rent seeking interests. In this vein, Rajan and Zingales (2003) suggest that the disruptions to trade and cross-border capital flows caused by the Great Depression facilitated the emergence of corporatist policies that kept arm's length financial markets suppressed for many decades, till cross-border flows resumed.

While these studies are suggestive, the channel through which upheavals enhance or suppress development is less easily teased out, especially in cross-country studies, since the upheavals also affect social and political institutions in a country, in addition to affecting economic institutions.[1] By focusing on the

[1] For instance, disruptions in intermediation capacity associated with financial crises can lead to sharp economic downturns ((Benmelech, Meisenzahl and Ramcharan (2015), Chowdow-Reich (2015), Ramcharan, Verani, and Van den Heuvel (2014)). There is also some evidence that financial crises might shape the subsequent path of output growth through a number of different economic channels—less innovation, slower rates of human capital accumulation (Ball (2014), Reinhart and Rogoff (2014).

consequences of failures in different towns in the United States on post-crisis development, we are able to correct for state and country level economic and political changes that might otherwise cloud interpretation. In focusing on more micro-level evidence, our paper resembles Acemoglu, Hassan, and Robinson (2011), who show that areas in the Soviet Union where the Holocaust was more severe grew more slowly, perhaps of the loss of Jews, who earlier constituted more of the growth-supportive middle class. We, however, focus more directly on surviving institutions and the associated political economy emerging from the upheaval.

In section 2, we briefly describe the context of our study, we describe the data in section 3, and present some stylized facts in section 4. We conclude in Section 5.

2. The Context

Landed Interests and Access to Finance

In Rajan and Ramcharan (2011), we explore how the structure of banking across counties in the United States in the early part of the 20th century was driven by the distribution of land within the county. We focus on banks because they were, and in many areas still are, the most important source of local finance, and thus are important economic institutions. Likewise, we focus on the distribution of land because it represents the distribution of agricultural wealth and interests at a time when agriculture was still a key sector in the U.S. economy.

The literature suggests three main reasons why landed interests might have opposed more local bank competition. First, limits on bank competition and control over exit could provide landowners insurance during periods of agricultural distress. While large national banks or state banks with branches could foreclose more easily on loans and exit the locality, transferring capital to

urban or less distressed rural areas, small local banks would have fewer options, and would have to continue lending to large local farmers during periods of distress (Calomiris and Ramirez (2004)).

Second, control over bank entry could accord landed interests greater influence over the local financial system, enabling them to prevent or delay the emergence of alternative centers of economic power and status (Chapman (1934)). For example, a rapidly growing manufacturing sector could increase the returns to schooling and attract labor away from agriculture, and there is evidence that landed interests may have used their political influence to restrict not just finance, but education and other public goods (Galor et. al (2009), Ramcharan (2010)).

Third, large landowners generated surpluses which they could lend. They also often had a stake in, or influence over, the local bank. The entry of more formal credit institutions could be competition for their lending business. They also had indirect reasons to keep out competition in lending. Landowners often owned the local store. Small farmers, sharecroppers, and agricultural workers needed credit to buy supplies from the local store. By limiting credit from alternative sources – for instance by keeping banks out -- the local merchant cum landlord could lock the farmer in and charge exorbitant prices, perpetuating the lucrative debt peonage system (Haney (1914, p55-56)).

We find that in counties in which agricultural land holdings were more concentrated, there were significantly fewer banks per capita, even correcting for state level effects. Of course, land concentration can be endogenous. Noting that patterns of rainfall affect the optimal crops and the optimal economic size of land holdings, with more rainfall favoring more plantation-style crops and therefore concentrated land holding, we instrument land concentration with rainfall. The results continue to hold. Moreover, credit appears to have been costlier, and access to it more limited, in concentrated counties. We also find that proxies for loan losses were lower in counties that had more concentrated land holdings,

suggesting that the greater riskiness of the underlying pool of borrowers cannot explain our results.

The correlation between banks per capita and land concentration is stronger in counties with more share-cropping (where there is a disproportionately more vulnerable population for the elite to squeeze) and weaker in counties with more manufacturing (where agricultural elites have competition from manufacturing elites). Broadly, therefore, the evidence is consistent with elites using their political influence to limit access to finance, and doing so even in countries like the United States with well-developed democratic institutions.

The Political Influence of Landed Interests

Of course, this evidence does not point directly to a political channel of influence. In Rajan and Ramcharan (forthcoming), we examine congressional voting on the McFadden Act of 1927 for this. The United States has long had a dual banking system, where state banks are chartered and regulated at the state level, while national banks operate under federal oversight. Before the McFadden Act, some states allowed state banks to open multiple branches, while others prohibited all branching. However, nationally chartered banks were, in all cases, not allowed to open branches (Cartinhour and Westerfield (1980), Southworth (1928)). As a result, an increasing number of national banks gave up their charter (which typically meant their leaving the Federal Reserve System also).

The McFadden Act attempted to level the playing field by forcing states to accord largely the same branching rights to national banks as to state banks but only in those states that already allowed state banks the right to open branches (Preston (1927)). It was widely expected that if the McFadden Act allowed national banks liberal branching powers, then subsequent to the passage of the act, national banks would unite with large state banks to push for branching in all states. Thus landed elites in non-branching states could be expected to be even more opposed to the McFadden Act than were landed elites in branching states,

especially because the latter's rents would already have been diminished by branching by state banks.

Examining the initial congressional roll call data, controlling for state fixed effects (which allow us to absorb state differences in regulations, among other factors), we find that congressmen from districts with more concentrated land holdings were far more likely to oppose the McFadden Act. The results are stronger still when we instrument land concentration with rainfall in the area.

The association of land concentration with congressional opposition was especially strong in those districts where agriculture was relatively more important than manufacturing, suggesting that landed elites were politically more effective when they also dominated economically. Similarly, the association of landed interests with congressional opposition was particularly strong in non-branching states, perhaps because of the fears of the local elite about the incipient spread of branching.

Also, using hand collected data from the 1930 Census on measures of credit cost and access, such as the interest rate and loan to value ratios of farms, we find that congressional support for the McFadden Act, was significantly lower in those districts with high credit costs and limited credit availability, suggesting that the desire to protect incumbents' rents may have indeed inspired political opposition to the Act.

Eventually, technological changes in banking, which allowed banking at a distance, made it hard to maintain bank branching restrictions in the US (Kroszner and Strahan (1999)). The Riegle-Neal Act repealed the remaining restrictions on bank branching in 1994. A summary evaluation then is that by restricting the scope of the McFadden Act and protecting small banks from competition, landed interests strengthened small community banks, which gained political influence and maintained the protections long after their initial protectors lost their economic and political heft.

Our evidence thus far is consistent with the Marxian or Engermann and Sokoloff view that technology (of farm production) determines constituencies (landed elite) who then influence the setting up of economic institutions (banks). Let us now turn to whether upheavals, such as the Great Depression, can affect the nature of constituencies, and thus their influence over economic institutions.

3. Data

We use the 1936 Annual Report of the Office of the Comptroller of Currency to identify the 1,420 national banks that failed between 1930 and 1934. We then collected a variety of balance sheet data about these banks in 1929 from the Banker's Almanac of 1929—the last year before the Depression. We also collected similar balance sheet data in 1929 for all the other banks, both state and national, headquartered in the same town as those national banks that subsequently failed. There were 1,043 such towns. Towns with national banks that subsequently failed might be different from the general population of towns. To address this issue of sample selection bias, for a stratified random sample of 858 towns not in our original data collection exercise, we collected similar bank balance sheet data for all the banks in the town. The details of the sampling exercise are available from the authors.

Panel A of Figure 1 shows the spatial distribution of 1,043 towns with failed national banks in 1930-1934. Panel B of Figure 1 shows the sample of 858 towns without any failed national bank between 1930-1934 from which we collected data. Panel C overlays these two samples. The darker colors indicate areas with more failed national banks, while the lighter shading highlights those towns without any failed national banks during the Depression. Consistent with our sampling methodology, for the most part towns without failed banks are generally geographically proximate to those towns that did experience national bank

failures; these towns are for example more likely to be located in the same county. A notable exception is the upper Northwest, where we have a number of towns relatively distant from those areas with subsequent failed national banks.

Table 1 provides some summary statistics across the two sets of towns. In 1929, towns with failed national banks generally had larger banking systems, measured in terms of total assets; the average bank also had more in assets in these towns. This probably reflects the fact that national banks, on account of their generally larger capital requirements compared to state chartered banks, tended to operate in bigger markets and were bigger banks. Towns without any national bank failures in 1930-1934 may have been too small to attract national banks in the first place. Interestingly, banks in towns that did not experience any failures also appear to have funded themselves to a greater extent with deposits. These towns also had fewer banks in 1929.

Figure 2 plots the distribution of failures across towns with failures. In about 50 percent of the cases, nearly half the number of national banks failed inside the town, and in around 15 percent of the towns, all the national banks failed. Finally, we combine these Depression era data with annual data from the Summary of Deposits on the number of banks inside our sample of towns from 1966 through 2005

4. Some Stylized Facts

In this subsection we study the relationship between the log number of banks, both state and national, headquartered in a town and the log number of national bank failures between 1930-1936. We observe the log number of banks in a town for various years, beginning in 1966, the first year for which the summary of deposits data are available. For each year that we observe the log number of banks in a town, we regress this variable on the log number of national bank failures

between 1930-1936. Because the number of failures inside a town during the Depression might be related to the nature of the local banking market before the Depression, we control for the log number of banks headquartered in the town in 1929, along with the log of total deposits and the log of total assets; apart from the failures variable, we do not distinguish between state and national banks. All regressions also include state fixed effects.

Figure 3 reports the regression coefficient and the ninety five percent confidence band—dashed lines—for the log number of national bank failures estimated for each of the cross-section regressions between 1966 and 2005. In the 1966 cross-section, the correlation between the number of banks and national bank failures in the 1930s is economically and statistically significant. It suggests that a 10 percent increase in the number of national bank failures during the Depression is associated with 1.2 percent fewer banks some 30 years later. Alternatively, a town that had a one standard deviation increase in failures from the mean (1.85 compared to 1) had about 10 percent fewer banks in 1966. However, this correlation declines steadily thereafter, becoming insignificant by the late 1990s and is about two thirds smaller compared with the coefficient obtained using the 1966 cross-section. A qualitatively similar pattern emerges if we use the log assets of those national banks that eventually failed, observed in 1929, as a measure of banking sector distress during the Depression (available from authors).

4.1. Possible Explanations

Why would bank failures have a persistent impact on the subsequent market structure? Weak economic demand is the obvious explanation. Bank failures and the loss of intermediation capacity can diminish real economic activity in the short run, possibly leading to less new businesses and emigration. Reduced

economic activity and significant population losses in an affected town could in turn lower the future demand for finance and the equilibrium number of banks, especially if access to external finance is more easily available in nearby towns. The subsequent equilibrium number of banks might also be much lower post-Depression, as many areas became "overbanked" during the long boom that preceded the Depression.

Bank failures and the loss of knowledgeable financing capacity can also reduce the liquidation value of potential entrants, deterring entry in areas that suffered greater bank failures. That is, because bank lending was primarily local during this period, valuing bank loans often required specialized knowledge about local markets. Failures and the resulting loss of this knowledgeable liquidity could then increase loss given default rates and render loans illiquid in a given area, making subsequent entry unprofitable at feasible interest rates.

Political economy factors might also account for persistence. Models derived from Becker (1983) and Stigler (1971) generally view the regulatory process as one in which well-organized interest groups use the power of the state to gain rents at the expense of more dispersed or less well-financed groups. There is also compelling evidence that the timing of US banking deregulation was shaped by the relative power of interest groups (Krozsner and Strahan (1999)). These arguments imply that because failures can leave surviving banks with sizeable economic rents, incumbents would have strong incentives to protect these rents and block entrants. Also, surviving banks are likely to be particularly effective in manipulating the regulatory and political process to limit entry when their number is few, so that they can more easily solve the collective action problem.

4.2. Evidence from Nature of Survivors

We now provide some suggestive evidence that political economy forces might account for some of this persistence. To this end, we study the impact of the distribution of the number of remaining banks left in the aftermath of the Depression on the subsequent number of banks. If bank failures signal reduced economic opportunity in the town, what matters is the quantum of failures rather than the number of survivors. We construct an indicator variable that equals one if all the banks failed in a town by 1934, and zero if at least one bank survived after 1936. We also create three additional dummy variables. "One" equals one if one bank was left; "two" equals one if two banks were left and 0 otherwise; and "three" equals one if three banks were left after 1936, and 0 otherwise. To put these variables in context, in about 5 percent of the sample, there were four or more banks left after the Depression. As before, we regress the log number of banks in various years on these indicator variables, and the same controls as before for the subsample of towns with failures.

Panel A of Figure 4 plots the coefficient for the variable indicating the failure of all banks, along with the 95 percent confidence band (in dashed lines). The "all bank" coefficient is positive, significant and large. It suggests that in 1966 the number of banks is about 25 percent higher in towns where all national banks failed in 1930-1936 relative to those towns where at least one bank survived. This coefficient estimate declines steadily for later years, and is insignificant by the mid-1970s.

If failure is an indicator of economic opportunity, we would expect towns with exactly one bank left in 1936 (and therefore less failure than towns where all banks failed) to have more banks in 1966. Yet the coefficient on the "one bank" indicator variable—Panel B of Figure 4—is negative. It suggests that in a town with one bank left in 1936, the number of banks in 1966 is 20 percent lower

relative to the baseline category of four or more banks left. These magnitudes are similar in the case of the "Two" (Panel C) and "Three" (Panel B) indicator variables. If failures proxied for a lack of economic opportunity, we would expect the number of banks in 1966 to be lower, not higher, in towns where all banks failed. But consistent with the political economy interpretation, there is a sharp discontinuity when all banks fail—no incumbents are left to block entry—compared to the case when a small number of incumbents are left: While no banks left is associated with greater entry, a small number of incumbents is associated with less entry in the subsequent years.

The timing of bank branching deregulation across states offers another way to gauge the importance of the political economy hypothesis in explaining persistence. Before the Depression, only 12 states allowed some form of intra-state bank branching. In the remaining states that prohibited bank branching, banks could not easily enter local markets. Some states for example forbade multibank holding companies, making it difficult for banks to operate in different locations, even if these separate offices were not integrated, and operated independently with respect to deposits and regulatory requirements. Others allowed branching, but only through mergers or acquisitions that converted the acquired bank into a subsidiary. Beginning in the 1970s however, the remaining 38 states began to deregulate, permitting intra-state branch banking.

We would expect that if incumbents used their influence to limit bank entry after the Depression, then their influence on the number of banks inside a town should wane after a state deregulates. That is, the power of the number of national bank failures during the Depression to explain the subsequent number of banks should decline sharply after banking deregulation. In contrast, if the relationship between bank failures and the subsequent number of banks is driven mostly by local economic conditions, then it should not be affected by the timing of deregulation.

To test this hypothesis, we combine our town-level data into a single panel (1966-2006) and interact the bank failures variable with an indicator that equals 1 for the years in which the state banking system is deregulated and 0 for the years before deregulation. The dependent variable in Table 1 is the log number of banks headquarted in the town, and the regressors in column 1 are the log number of failed national banks (1930-1936) along with the log number of banks in the town, assets and deposits, all observed in 1929; all regressions include state and year fixed effects.

The results are striking. The log number of failed banks is negative and significant, and suggests that a 10 percent increase in the number of failed banks is associated with a 1.6 percent decline in the number of banks in those years before deregulation. However, the interaction term between the failed banks variable and the deregulation indicator is positive, large and statistically significant. This interaction term implies that in the years after deregulation, bank entry might have been higher in those areas most affected by the Depression. That is, state-wide branching deregulation may have attenuated the power of local incumbents to preserve the status quo, permitting increased entry especially in those towns with greater Depression era failures.

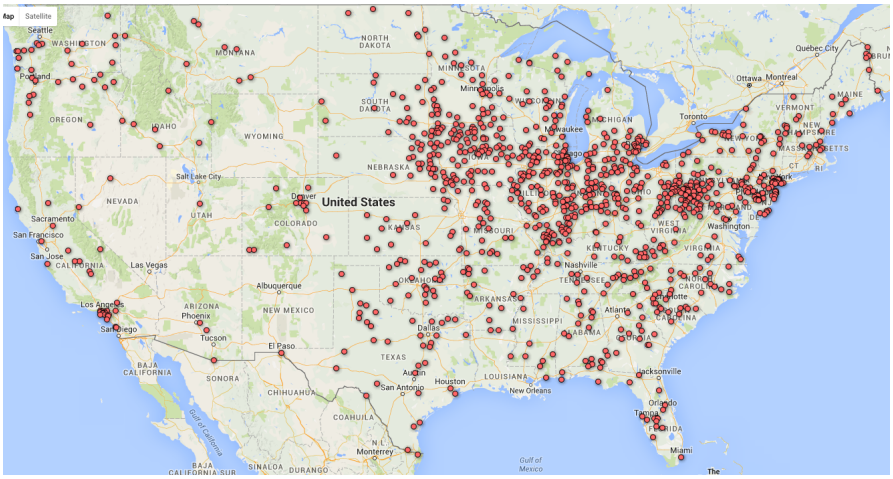
There is evidence that banking deregulation is associated with faster economic growth (Jayaratne and Strahan (1996)). And while our results favor a political economy interpretation, it remains plausible that faster economic growth induced by deregulation could have also increase the demand for finance. Why such an increase in the demand for finance should be disproportionately felt in those areas most affected by the Depression-era failures is not clear. And the dynamic response to deregulation (column 2) weighs against this demand mechanism. To model the dynamics, we interact the log number of failures with dummy variables indicating the year of deregulation, as well as dummy variables for up to three years afterwards. We also create a dummy variable that equals one for the period

six years after the state deregulates, interacting the failures variable with this post-deregulation dummy. In addition, we include dummy variables for the three years preceding deregulation, as well as interaction terms with the log number of failures.

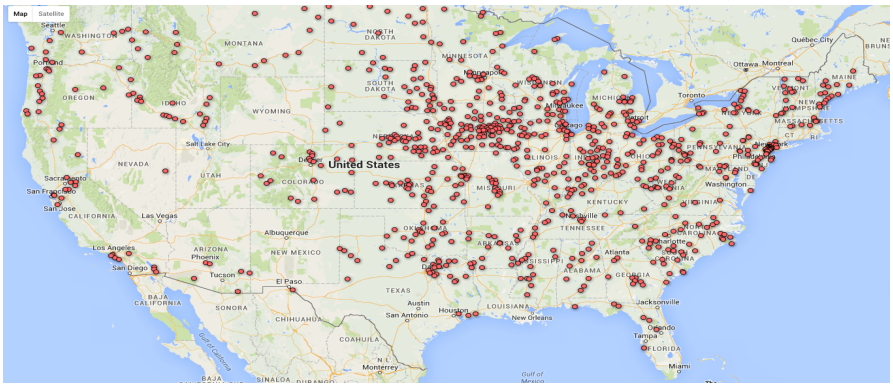
In each of the three years before the state deregulates, there is evidence of anticipatory entry. The interaction terms with the log number of bank failures are positive and significant, suggesting greater entry in those areas left most under banked by the Depression. But the period immediately after deregulation sees the greatest attenuation of persistence. For example, the interaction term three years after the year of deregulation is about thirty four percent larger than the corresponding term three years before. While we cannot exclude the possibility that an increase in the demand for finance, concentrated primarily in those areas with larger Depression-era failures might explain these findings, the relatively rapid attenuation of the historic bank failures variable in the period immediately after deregulation favors a political economy mechanism behind persistence.

Figures and Tables

PANEL A OF FIGURE 1. TOWNS WITH FAILED NATIONAL BANKS, 1930-1934.



PANEL B OF FIGURE 1. TOWNS WITHOUT FAILED NATIONAL BANKS, 1930-1934.



PANEL C OF FIGURE 1. COMBINED SAMPLE OF TOWNS, DARKER COLORS INDICATE TOWNS WITH FAILED NATIONAL BANKS, 1930-1936.

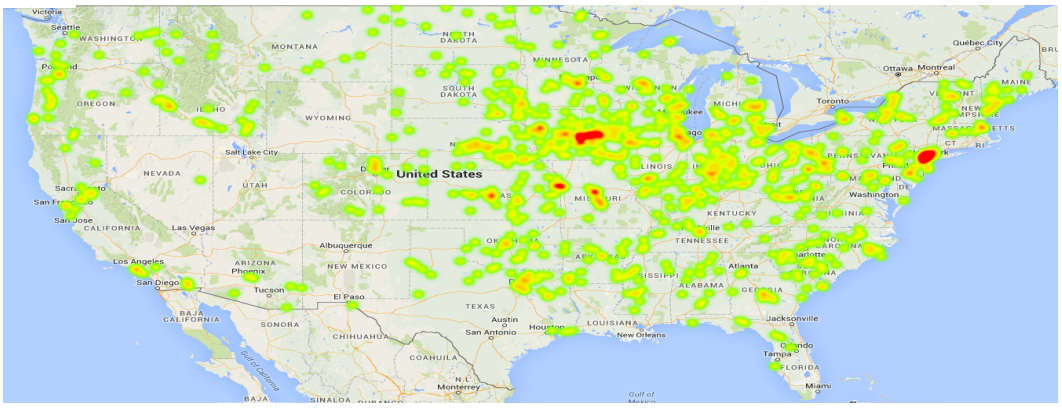
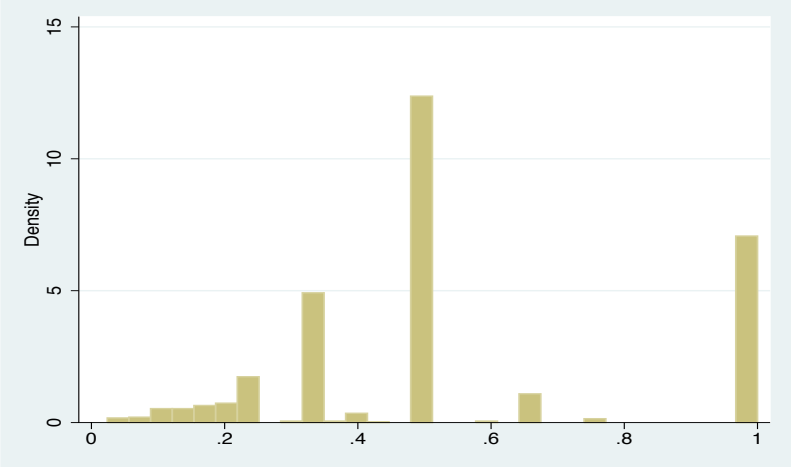
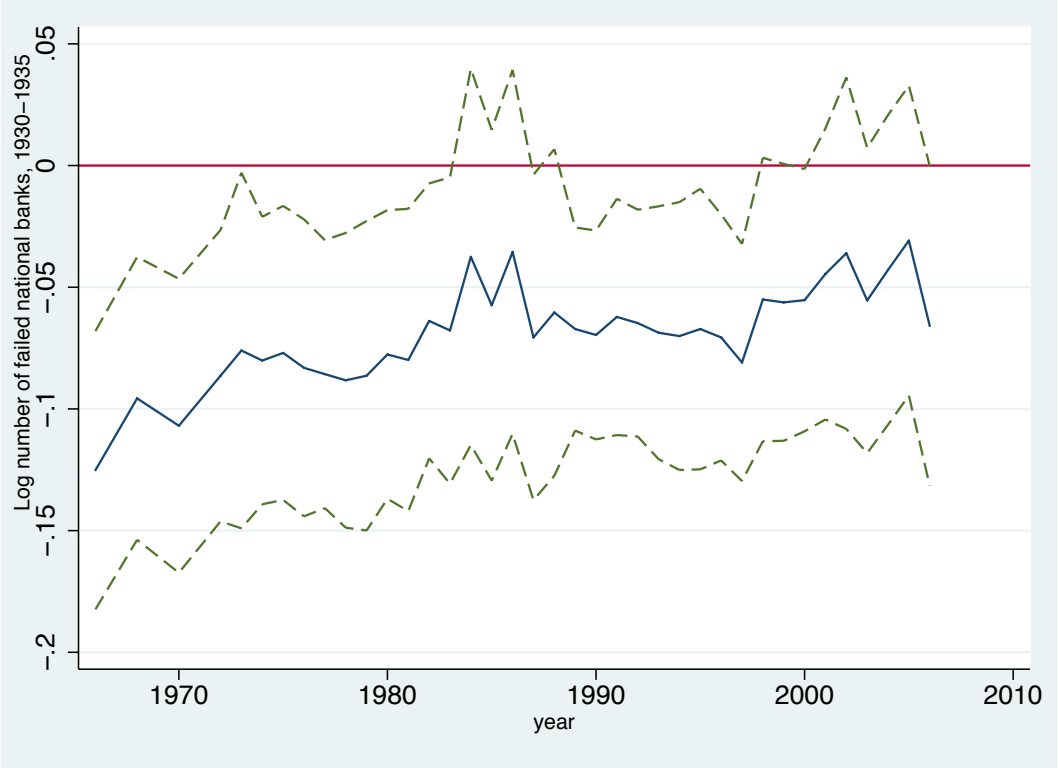


FIGURE 2. THE FRACTION OF FAILED BANKS



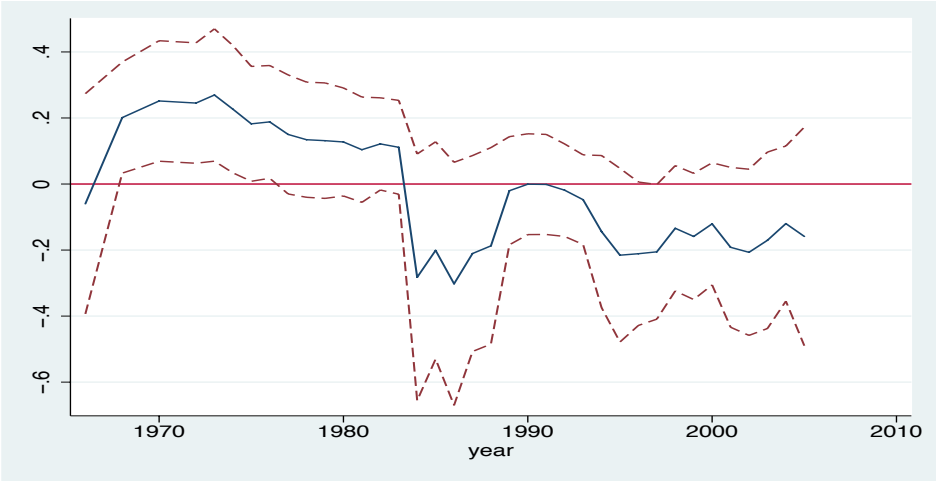
For the sample of towns with failed banks, this figure plots the ratio of banks that subsequently failed to the number of banks present in 1929.

FIGURE 3. THE IMPACT OF THE LOG NUMBER OF FAILED NATIONAL BANKS, 1930-1934, ON THE LOG NUMBER OF BANKS, 1966-2005.



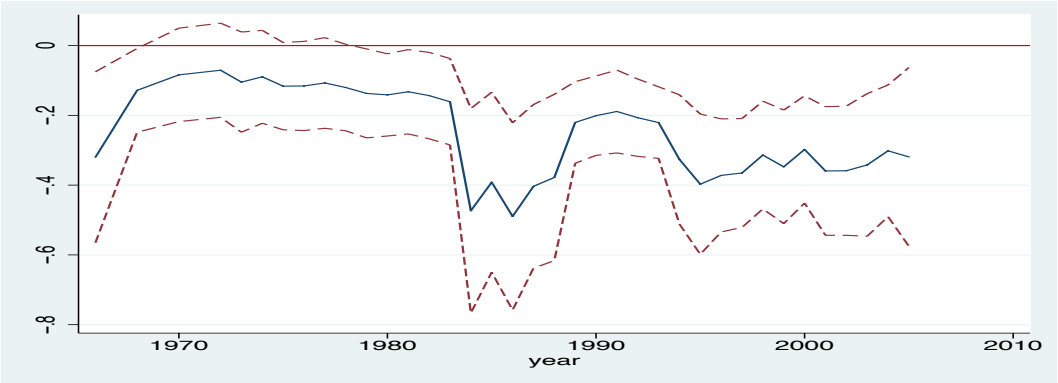
This figure reports the coefficient and 95 percent confidence bands for the following series of cross-section regressions: $\sum_{t=1966}^{t=2005} \log(number\ of\ banks_{it}) = \beta_t \log(number\ of failed\ banks_i) + X_i\theta_t + e_i$. The controls include the log total deposits and assets in the town, as well as the log number of banks in the town, all observed in 1929, along with state fixed effects.

PANEL A OF FIGURE 4. THE IMPACT OF ALL BANKS FAILING BY 1936 ON THE LOG NUMBER OF BANKS, 1966-2005.



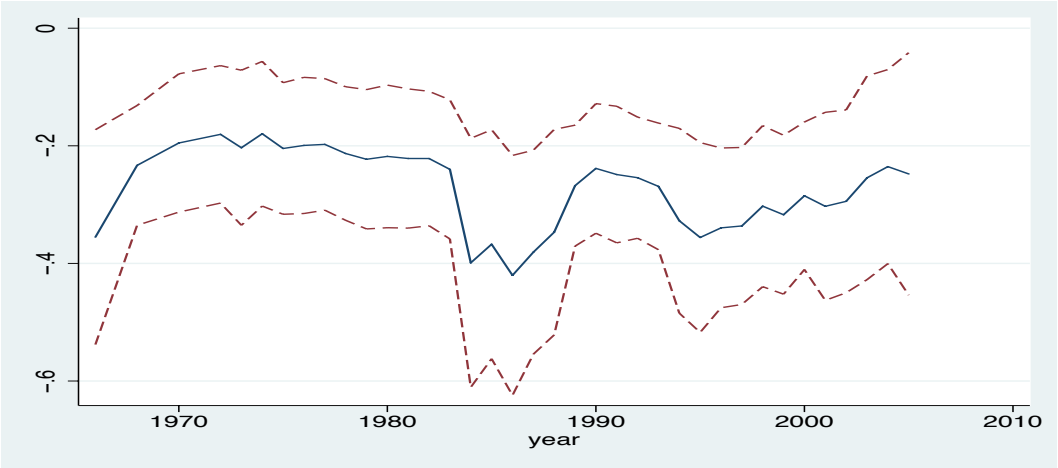
This figure reports the "all_i" coefficient and 95 percent confidence bands for the following series of cross-section regressions: $\sum_{t=1966}^{t=2005} \log(number\ of\ banks_{it}) = \beta_t\ all_{\ i} + \gamma_{1t} one_i + \gamma_{2t} two_i + \gamma_{3t} three_i + X_i\theta_t + e_i$. The controls include the log total deposits and assets in the town, as well as the log number of banks in the town, all observed in 1929, along with state fixed effects. all_i is an indicator that equals one if all the banks failed in the town by 1936 and 0 otherwise. "one_i" is an indicator that equals one if one bank were left in town by 1936 and 0 otherwise. . "two_i" is an indicator that equals one if two banks were left in town by 1936 and 0 otherwise. . "$three_i$" is an indicator that equals one if three banks were left in town by 1936 and 0 otherwise.

PANEL B OF FIGURE 4. THE IMPACT OF ONE SURVIVING BANK BY 1936 ON THE LOG NUMBER OF BANKS, 1966-2005.



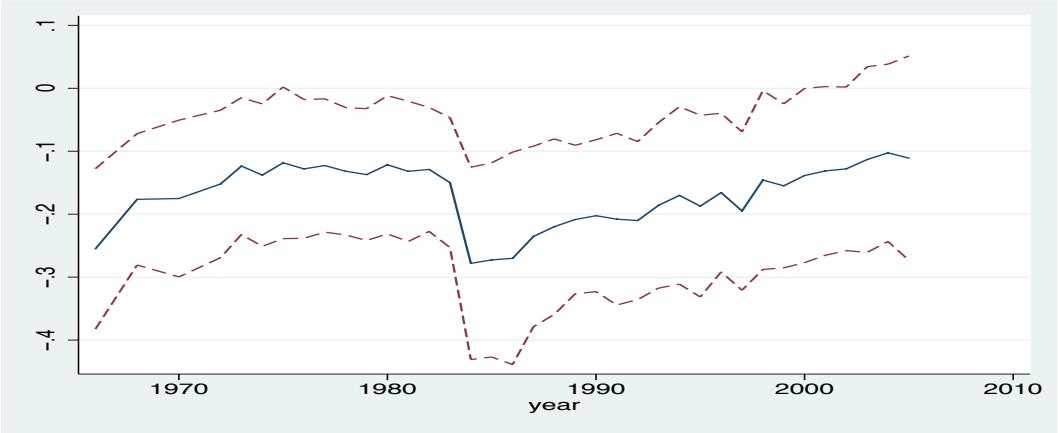
This figure reports the "one_i" coefficient and 95 percent confidence bands for the following series of cross-section regressions: $\sum_{t=1966}^{t=2005} \log(number\ of\ banks_{it}) = \beta_t\ all_{\ i} + \gamma_{1t} one_i + \gamma_{2t} two_i + \gamma_{3t} three_i + X_i\theta_t + e_i$. The controls include the log total deposits and assets in the town, as well as the log number of banks in the town, all observed in 1929, along with state fixed effects. all_i is an indicator that equals one if all the banks failed in the town by 1936 and 0 otherwise. "one_i" is an indicator that equals one if one bank were left in town by 1936 and 0 otherwise. . "two_i" is an indicator that equals one if two banks were left in town by 1936 and 0 otherwise. . "$three_i$" is an indicator that equals one if three banks were left in town by 1936 and 0 otherwise.

PANEL C OF FIGURE 4. THE IMPACT OF TWO SURVIVING BANSK BY 1936 ON THE LOG NUMBER OF BANKS, 1966-2005.



This figure reports the "two_i" coefficient and 95 percent confidence bands for the following series of cross-section regressions: $\sum_{t=1966}^{t=2005} \log(number\ of\ banks_{it}) = \beta_t\ all_{\ i} + \gamma_{1t} one_i + \gamma_{2t} two_i + \gamma_{3t} three_i + X_i\theta_t + e_i$. The controls include the log total deposits and assets in the town, as well as the log number of banks in the town, all observed in 1929, along with state fixed effects. all_i is an indicator that equals one if all the banks failed in the town by 1936 and 0 otherwise. "one_i" is an indicator that equals one if one bank were left in town by 1936 and 0 otherwise. . "two_i" is an indicator that equals one if two banks were left in town by 1936 and 0 otherwise. . "$three_i$" is an indicator that equals one if three banks were left in town by 1936 and 0 otherwise.

PANEL D OF FIGURE 4. THE IMPACT OF THREE SURVIVING BANSK BY 1936 ON THE LOG NUMBER OF BANKS, 1966-2005.



This figure reports the "$three_i$" coefficient and 95 percent confidence bands for the following series of cross-section regressions: $\sum_{t=1966}^{t=2005} \log(number\ of\ banks_{it}) = \beta_t\ all_{\ i} + \gamma_{1t} one_i + \gamma_{2t} two_i + \gamma_{3t} three_i + X_i\theta_t + e_i$. The controls include the log total deposits and assets in the town, as well as the log number of banks in the town, all observed in 1929, along with state fixed effects. all_i is an indicator that equals one if all the banks failed in the town by 1936 and 0 otherwise. "one_i" is an indicator that equals one if one bank were left in town by 1936 and 0 otherwise. . "two_i" is an indicator that equals one if two banks were left in town by 1936 and 0 otherwise. . "$three_i$" is an indicator that equals one if three banks were left in town by 1936 and 0 otherwise.

TABLE 1. SUMMARY STATISTICS, 1929

	Towns without failed national banks, 1930-1934		
	Total deposits in town/number of banks	Total assets in town/number of banks	Number of banks
Mean	30989.94	7.70E+06	2.01
Median	3180.33	4.80E+05	1
25th percentile	1518	2.70E+05	1
75th percentile	8716	1.10E+06	2
Minimum	0	0	1
Maximum	8.70E+06	1.10E+09	43
Standard Deviation	3.30E+05	6.10E+07	2.85
	Towns with failed national banks, 1930-1934		
Mean	18183.62	1.10E+11	3.51
Median	5784	8.70E+05	2
25th percentile	2281	4.60E+05	2
75th percentile	14126.83	1.70E+06	3
Minimum	--	71661	1
Maximum	7.50E+05	1.10E+14	214
Standard Deviation	49915.07	3.50E+12	10.35

TABLE 2. DEREGULATION AND PERSISTENCE

VARIABLES	(1) Log number of banks	(2) Log number of banks
Log number of failed national banks, 1930-1936	-0.164***	-0.149***
	(0.0308)	(0.0319)
Log number of failed national banks, 1930-1936*period after branching deregulation	0.208***	
	(0.0405)	
Log number of failed national banks, 1930-1936*year of branching deregulation		0.0884***
		(0.0261)
Log number of failed national banks, 1930-1936*one year after branching deregulation		0.106***
		(0.0278)
Log number of failed national banks, 1930-1936*two years after branching deregulation		0.124***
		(0.0270)
Log number of failed national banks, 1930-1936*three years after branching deregulation		0.127***
		(0.0279)
Log number of failed national banks, 1930-1936*one year before branching deregulation		0.111***
		(0.0258)
Log number of failed national banks, 1930-1936*two years before branching deregulation		0.104***
		(0.0248)
Log number of failed national banks, 1930-1936*three years before branching deregulation		0.0944***
		(0.0261)
Log number of failed national banks, 1930-1936*six years and beyond after branching deregulation		0.214***
		(0.0592)
Observations	74,546	62,293
R-squared	0.822	0.838

The dependent variable in this table is the log number of banks, observed in an annual panel from 1966-2005. The other controls include the log total deposits, and assets in the town, observed in 1929, as well as the log number of banks, again observed in 1929.. The indicator variable "period after branching deregulation" equals one in the years that the state permits bank branching and 0 otherwise. The panel also includes state and year fixed effects, and standard errors (in parentheses) are clustered at the state level *** p<0.01,** p<0.05,* p<0.1.

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