Accelerating into the Abyss: Financial Dependence and the Great Depression*

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This paper gives new evidence for the importance of bank suspensions during the Great Depression. I establish that more financially dependent manufacturing industries exhibited steeper declines in output relative to peers. This differential is largest in states that were most affected by banking suspensions, and the same pattern also holds for value added, number of establishments, and employment. I use Altonji et al. ratios and instrument bank suspensions to argue that omitted variable bias and reverse causality are not likely. The results show that bank suspensions could explain a third of the decline in manufacturing output during the Great Depression.

Keywords: Financial Intermediation, Bank Failures, Economic Growth, Great Depression.

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1. Introduction

Between 1929 and 1932, US output declined by a third. During the same period, the number of suspended banks increased fourfold. Yet whether the suspensions had a causal effect on output remains in dispute. The question is relevant even today, especially in light of the large role played by financial distress in the global crisis that erupted after 2008. If policy makers are to learn the correct lessons from past downturns, then it is essential to establish the facts of the case.

Did bank suspensions indeed turn a "garden variety" recession into the Great Depression? They did according to Friedman and Schwartz (1963), who argue that banking panics were the main mechanism propagating the Depression. Suspended banks reduced the money supply and that, in the view of Friedman and Schwartz, depressed economic activity. In contrast, Bernanke (1983) emphasizes the loss of information about clients of the suspended banks and the resulting rise in the cost of credit intermediation. Although these two works do not stress the same transmission mechanism, both consider bank suspensions to be crucial factors in deepening the downturn. The empirical basis of these two interpretations can be criticized as presuming that temporal correlation and succession imply causation. After all, banks could have become suspended because their clients were suffering. The importance of contemporaneous bank suspensions was therefore challenged by two broad bodies of literature: one that directly argued against a role of bank suspensions, and one that simply proposed that other factors may have also mattered for the downturn.

The skeptics from the real business cycle literature have directly argued that bank suspensions ought not to have mattered. Prominent works in this vein include Cole and Ohanian (1999, 2001) as well as Chari, Kehoe, and McGrattan (2003). Given Calomiris and Mason's (2003) evidence that reductions in the supply of loans led to output declines in a cross section of US states, Temin's (2008) critique of the methodology used by Kehoe and Prescott (2007) to study economic depressions, and the latters' reply, it is clear that the debate has not yet been resolved. For this reason, one of the objectives of this work is to confirm that bank suspensions had a causal effect on decline in output and several other measures of real economic activity.

Moreover, the more alternative factors contributed to the downturn, the less importance could be attributed to bank suspensions themselves. Besides the aforementioned real business cycle literature that argues that negative productivity shocks can explain the Depression, Temin (1976) writes in favor of a contraction in aggregate demand, which Romer (1993) later relates to a rise in uncertainty following the stock market crash of 1929. Kindleberger (1973) and Eichengreen (1992) instead emphasize the role of disruptions in international trade and international finance. Eggertsson (2008) advances the view that Roosevelt's abandonment of Hoover-era policy dogma – the gold standard, a balanced budget, and small government – made the real interest rate fall by creating expectations of a higher future money supply; this caused the recovery which, rather than resulting from the end to bank suspensions in March 1933, just coincided with it. Given the wealth of competing explanations, each of which aspires to explain a part of the downturn, another objective of this work is to ascertain that the magnitude of the effect of bank suspensions on the real economy was substantial enough to conclude that bank suspensions mattered for the downturn.

1.1. Summary of the Findings

In this paper, I present new evidence that bank suspensions played a crucial role in making the Great Depression "great". I demonstrate that, during the Depression, financially dependent industries suffered bigger declines in output than did their peers. Furthermore, the difference is greatest in states most affected by bank suspensions. My sample is a panel of state-industry observations from the interwar period, so it covers more than just the Great Depression. This approach avoids the potentially confounding effects of both domestic and international shocks specific to the years 1929-33. The sampled industries accounted for 40 percent of manufacturing output in 1929.

The paper measures the causal effect of bank suspensions on output by using a novel instrumental variable. This is the state-specific fraction of population rural in 1910, which captures the demand shock for agricultural products during World War I (WWI), but predates any potential reaction to this shock. The clients of banks were more leveraged and more prone to bankruptcy in states that expanded food production to meet the temporary demand from Europe during WWI. I demonstrate that, via higher indebtedness of rural residents and via weaker balance sheets of banks in rural areas, the instrument is able to predict bank suspensions throughout the interwar period. The causal estimates reported in this paper show that the difference in the effect of bank suspensions on output of industries more and less reliant on external financing is large and that, under certain assumptions, the results could be interpreted to mean that bank suspensions accounted for a third of the decline in manufacturing output observed during the Great Depression.

My findings are based on two measures of financial dependence: the "external dependence" measure of Rajan and Zingales (1998), which is standard in the literature, and *inverse interest cover*, a novel measure of dependence that is appropriate for deep recessions. The "external dependence" of Rajan and Zingales (1998) serves as an indicator of technology-driven investment needs. Because for many firms investment was not a priority during the Great Depression, I verify that the reported findings all hold even when an alternative measure is used. I chose to use *inverse interest cover* as a measure of financial dependence that is suitable for periods when capacities are underutilized and new investment is not a priority. After collecting industry-level data from the 1920s, I calculate the inverse interest cover on the eve of the Great Depression. This metric reflects the difference between earnings (before interest, taxes, depreciation, and amortization; a.k.a. EBITDA) and interest payments due in the same year. The inverse interest cover is, first of all, an indicator of short-term financing needs: industries with lower interest cover are forced to borrow more, if not for investment then for the rollover of their debt. Second, it is a measure of how difficult it is to obtain credit in recessions. As a flow-side measure of assets and liabilities, this metric is also indicative of bankruptcy risk; in recessions, lenders redirect credit away from high-risk borrowers (Bernanke, Gertler, and Gilchrist 1996). Industries with a low interest cover are thus vulnerable to bank suspensions not only because they need more credit but also because they are underprivileged in accessing what credit remains after bank suspensions. Even though both results using external dependence and those using inverse interest cover suggest that bank suspensions affected financially dependent industries the most, I argue that the two measures should not be treated as substitutes. Instead, I present evidence that the two measures are complements which describe different aspects of financial dependence and outline different channels by which bank suspensions reduce output.

Several more robustness checks and one extension verify that bank suspensions mattered. Throughout the regression analysis I include controls to assure that the observed variation in the post-bank suspension performance of industries of different financial dependence is not driven by their cyclicality related to consumer demand. I also employ four tests that confirm the robustness of the effect of bank suspensions estimated using IV analysis. They verify that the effect is not limited to the more rural states, but that it is present and of comparable size also in the rest of the country. Moreover, they check that it is not driven by any problem to the exclusion restriction. In an extension, I present evidence that bank suspensions severely affected not only output, but also several other measures of manufacturing performance; namely, value added, number of establishments, and employment. In addition, that they affected both the 18 sampled industries, with the same pattern of decline across industries with different financial dependence as found for output growth, and the aggregate of the manufacturing sector in each state.

1.2. Related Literature

This paper is closely related to that of Calomiris and Mason (2003). Using instruments of loan supply, they find a causal effect of changes in loans and deposits – the results of bank distress – on production income in a cross section of US states during the 1930–1932 period. Although I also use data from a cross section of US states, my approach differs in two main respects. First, I add variation across industries to the analysis and demonstrate that the extent to which financially dependent industries perform worse than peers depends on the incidence of bank suspensions within a state. This is an additional piece of evidence that bank suspensions contributed significantly to the decline in manufacturing output, while the link through financial dependence also speaks of an effect through decline in loans. Second, I measure the causal effect of bank suspensions (and not of supply of bank loans) on output. Reduced supply of loans could, indeed, be the most important consequence of bank suspensions that leads to output declines. Yet bank suspensions can reduce output in several other ways: by discouraging demand for loans from less creditworthy borrowers, as well as by reducing demand for goods through the uncertainty they create about the future availability of credit and the future level of income. For this reason, measuring a causal effect of bank suspensions has distinct importance for policy making.

My paper also relates to a pair of recent works that apply a measure of financial dependence to study the performance of US firms and industries during the interwar period. Nanda and Nicholas (2014) show that bank distress had a more negative effect on the innovation of firms that are financially dependent. As a proxy for external dependence, they introduce the ratio of pre-Depression notes payable *divided by* fixed assets. Lee and Mezzanotti (2017) look at a sample of 29 US cities over the period 1929–1933. These authors similarly find that, in cities where bank suspension rates were higher, the financially dependent industries (measured as in Nanda and Nicholas 2014) were more affected in terms of employment, output, and establishment growth. Lee and Mezzanotti instrument bank suspensions using a measure of religious fragmentation. My paper is different in at least two respects. First, my sample covers all of the United States and results verify that bank suspensions had important real effects on the whole country during the Great Depression *and* during the rest of the interwar period. Second, I introduce into use a new measure of financial dependence – inverse interest cover – that has been designed specifically for deep recessions when new investment is a low priority. My measure of financial dependence follows a similar logic to that of Benmelech, Frydman, and Papanikolaou (2018), who show that firms whose bonds were maturing in periods and locations where banks were suspended suffered sharper declines in employment because of the inability to refinance them.

This paper belongs to the large body of literature on bank suspensions and failures of the Great Depression.[1] Wicker (1996) gives a detailed description of the contemporary banking crises, and there are many papers that shed light on the causes of bank suspensions.[2] Most of them agree that the bank suspensions of the Great Depression were not entirely different than the other interwar bank suspensions[3], although panics and contagion became a more prominent during Depression-era suspensions[4]. Although Carlson and Rose (2015) present evidence that bank suspensions caused declines in lending, evidence that bank suspensions caused declines in output is mixed. Bernanke (1983) and Friedman and Schwartz (1963) argue that the Great Depression bank suspensions had real consequences. So do Calomiris and Mason (2003), Richardson and Troost (2009), Ziebarth (2013), Nanda and Nicholas (2014), Lee and Mezzanotti (2017), Cohen, Hachem, and Richardson (2018), and Benmelech, Frydman, and Papanikolaou (2018). For the period preceding the Depression, Kupiec and Ramirez (2013) use the vector autoregression method and find evidence of real effects of bank suspensions; Frydman, Hilt, and Zhou (2015) document real effects of the Panic of 1907. Most works arguing *against* any significant real effects of Depression-era banking crises are in the real business cycle literature,[5] though there are exceptions. For instance, Rosenbloom and Sundstrom (1999) demonstrate that regional variation in manufacturing performance during the Depression can be largely attributed to regional differences in trend of employment growth and industrial composition.

[2] Notable works include Alston, Grove and Wheelock (1994), Calomiris (1989b), Calomiris and Mason (1997, 2001), Carlson and Mitchener (2006), Friedman and Schwartz (1963), Grossman (1994), Richardson (2007a), Temin (1976), Wheelock (1995), and White (1984).

[3] Much research suggests that the bank failures in these two periods were not entirely different. Calomiris and Mason (1997) find that few solvent banks failed in the Chicago banking panic of 1932; they also show that most pre-1933 bank failures were driven by their poor fundamentals and not by liquidity crises or contagion (Calomiris and Mason, 2001). Temin (1976) claims that many Depression-era suspensions were caused by falling agricultural income, the reason behind most suspensions in the 1920s. Along these lines, White (1984) shows that banks failing in the year 1930 shared many characteristics with banks failing in previous years.

[4] Friedman and Schwartz (1963) argue that declining borrower income led to most failures in the 1920s whereas bank runs led to most failures in the Depression. Heitfield, Richardson, and Shirley (2017) emphasize the role of the panic in the initial banking crises of the Great Depression, while Calomiris, Jaremski, and Wheelock (2019) stress the importance of inter-bank contagion.

[5] See especially Cole and Ohanian (1999, 2001) and Chari, Kehoe, and McGrattan (2003). In a cross section of US states, Cole and Ohanian (2001) find no significant relationship between personal income and deposits in suspended banks.

More broadly, my paper relates to research that attempts to explain both the downturn and the recovery from the Great Depression. The initial downturn, starting in 1928, is considered to be initiated by the tightening of the Federal Reserve policy (see e.g. Hamilton 1987 and Romer 1993) intended to counter the stock market boom and the outflows of gold to France because of undervalued franc. The resulting stock market crash in October 1929 led to a decline in consumption. While Mishkin (1978) relates it to the fall in financial wealth caused by the stock market crash in 1929, Romer (1990) associates it to the uncertainty that this crash created. The further output decline after late 1930 is usually associated to a series of banking panics (see e.g. Romer 1993 and Calomiris 1993). Besides the effect of bank suspensions themselves (Friedman and Schwartz 1963, Bernanke 1983), Hansen and Ziebarth (2017) relate the downturn to financial distress that could have resulted both from bank suspensions and a shortage of funds available for lending, Benmelech and Bergman (2017) document the contemporaneous freeze of the credit markets, and Mitchener and Richardson (2019) stress the importance of interbank contagion as an amplifier of the reduction in lending. Eggertsson (2008) is a recent proponent of a long-standing view that abandonment of the gold standard and creation of inflationary expectations following Roosevelt's inauguration led to the recovery, which coincided with an end to bank suspensions. Hausman, Rhode, and Wieland (2019) show that an important source of recovery in 1933 was the increase in farmers' consumption enabled by the rise in dollar prices of tradable farm products post-devaluation.

Similar mechanisms by which financial distress is transmitted to the real economy have been documented with more recent data. Rajan and Zingales (1998) pioneer the measure of external financial dependence to study the relationship between financial development and growth. In cross-country studies, Kroszner, Laeven, and Klingebiel (2007) and Dell'Ariccia, Detragiache, and Rajan (2008) use this measure to document significant real effects of banking crises. Goetz and Gozzi (2010) demonstrate that metropolitan areas where banks experienced greater shock to financing during the 2008 crisis suffered from larger reductions in employment and establishments, especially where the local industries were more financially dependent. Contemporary studies have been able to use detailed data on bank–firm relationships. Iyer and

Peydro (2011) show how a major bank failure leads to interbank contagion and fewer loans extended by the banking sector. Similarly, Schnabl (2012) studies the 1998 Russian default as a negative liquidity shock to international banks and demonstrates that this shock led to a decline in interbank lending and in the lending to firms by affected banks. Iyer et al. (2014) establish that: (a) banks that relied more on interbank finance prior to the 2007–2009 crisis made greater reductions in lending activity during the crisis; and (b) smaller and younger client firms could not substitute for that lost lending by borrowing from another bank. Other papers demonstrate that such reductions in loans had real effects. Paravisini et al. (2015) show that firms borrowing from banks more affected by the 2008 reversals of capital flows reduced their exports to a greater extent. Duchin, Ozbas, and Sensoy (2010) report that investment declines during the subprime mortgage crisis were highest for firms more vulnerable – as measured by low cash reserves, high short-term debt, and/or external financial dependence – to a shrinking supply of credit. Almeida et al. (2011) show that firms whose debt was maturing during credit crisis, after the third quarter of 2007, cut their investment by more than their peers whose debt was due after 2008.

The theoretical counterparts to this paper build on the concept of "financial accelerator", which dates to Bernanke and Gertler (1989). A borrower whose net worth declines must pay a higher external financing premium, which in turn reduces the borrower's spending and output. Kiyotaki and Moore (1997) argue that further amplification can result from the interaction between credit constraints and asset prices that determine agent's net worth. Thus any initial effect of bank suspensions during the Great Depression on the net worth of borrowers would be amplified by this financial accelerator. Moreover, the accelerator mechanism would become even more pronounced in recessions (Brunnermeier and Sannikov 2014). During the Depression, the usual financial accelerator also interacted with ongoing disruptions of the financial system. Yet the earlier theoretical work had little to say about the role of bank suspensions themselves. That all changed with the recent crisis, which spurred considerable research that models the banking sector as the recession's initial impulse (for a survey of this research, see Gertler and Kiyotaki 2010). In all of this theoretical work, the difference between an agent's assets and liabilities is the key determinant of that agent's economic destiny. Agents with a lower net worth will always face higher external financing premia. They will also be perceived as risky in recessions, and lenders – in a "flight to quality" – will confer their credit elsewhere (Bernanke, Gertler, and Gilchrist 1996).

The paper proceeds as follows: Section 2 places the US interwar bank suspensions in a historical context. Section 3 describes the data and its sources. Section 4 is devoted to explaining the method used, presenting the main results that estimate the effect of bank suspensions on output growth, and discussing their implications. Section 5 explores the robustness of my IV estimates. Section 6 presents an extension –measuring the effect of bank suspensions on growth in value added, number of establishments, and employment – while Section 7 concludes. In the Appendix, Subsection A.1. formally presents the evidence against reverse causality using selection on unobservables, shows the results of the first stage regressions, and demonstrates that the instruments are strong, while Subsection A.2. presents two further robustness checks.

2. Historical Background: The Prevalence of the US Interwar Bank Suspensions

The interwar years can be divided into three periods with regard to the prevalence of bank suspensions: the 1920s, the Great Depression, and the years that followed. In the 1920s, 1–4 percent of commercial banks were suspended each year and bank suspensions were widespread in rural areas (Alston, Grove, and Wheelock 1994). The Depression era corresponded to roughly a fivefold increase in the fraction of suspended institutions. Friedman and Schwartz (1963) identify four banking crises in the Great Depression, three of which took place in 1930 and 1931. They argue that the first crisis, acceleration of bank suspensions from November 1930 to January 1931, turned a bad recession into the Great Depression. The suspensions of two large banks, Caldwell and Company in Tennessee and the Bank of United States in New York, also hurt their business partners in neighboring states and severely undermined depositor confidence throughout the country. A more vulnerable banking system soon experienced more shocks. The second crisis consisted of a series of regional crises and urban panics from April to August 1931 (Wicker 1996). The most prominent ones were in Toledo (Ohio) and Chicago (Illinois), the latter panics caused by the combination of declining real estate values and too many mortgages in banks' portfolios (Postel-Vinay 2016). The third crisis, in September and October 1931, was triggered by Britain leaving the gold standard (Wicker 1996); it was more intense than the previous crises and also nationwide. That crisis was apparently stopped only by the reactive establishment of the National Credit Corporation. Soon renamed the Reconstruction Finance Corporation, this entity boosted confidence as it made loans to troubled banks. For this reason, there were fewer bank suspensions in 1932 than in 1931.

The fourth crisis, which followed in 1933, was an outlier among all interwar banking crises: it was the worst crisis with the best aftermath. During its buildup, the Federal Reserve and the Reconstruction Finance Corporation failed to agree on which should be the "lender of last resort" for a group of distressed Michigan banks (Wicker 1996). Hence Michigan's governor declared a statewide banking holiday on February 14, 1933. Depositors from Michigan then attempted to obtain funds from contiguous states, whose concerned residents naturally followed. The ultimate result was a cascade of more than 30 statewide moratoria. Payment suspensions in most parts of the country were already a fait accompli by the time Franklin D. Roosevelt, the new president, declared a national "bank holiday" on March 6. Half of the banks, which together held some 11 percent of all deposits, were not allowed to reopen on March 15. The government's guarantee for the banks that were reopened restored confidence in the banking system. Massive amounts of hoarded currency were redeposited, and industrial production quickly recovered. Following the Great Depression was a much calmer period during which fewer than 1 percent of banks were suspended in any given year. In effect, the banking system was consolidated by the guarantees of the newly established Federal Deposit Insurance Corporation and a decade of widespread failures (see Walter 2005).

3. Data

My data set consists of observations that vary across periods, states, and industries. This section gives the definitions and sources for that data: the measures of manufacturing performance, bank suspensions, its instrument, and two measures of financial dependence. Tables 1 and 2 provide the summary of my data.

3.1. Measures of Manufacturing Performance

Over the course of the Great Depression, US gross domestic product (GDP) fell by 29 percent (Kendrick 1961). The decline in manufacturing output, which accounted for about a third of GDP at the time, was more pronounced than that of overall output. In this paper, I use four biannual growth rates of indicators of manufacturing performance specific to state, industry, and period. The indicators are manufacturing output, examined in the main body of the paper, and value added by manufacturing, number of manufacturing establishments, and employment in manufacturing, examined in an extension. An example unit of observation is the growth of one of these four indicators of the glass industry in Arizona between 1923 and 1925. The U.S. Bureau of the Census' Biennial Census of Manufactures was the original source of these data. I use a modified version of Rosenbloom and Sundstrom's (1999) data set, which includes 21 of the 31 largest employers among the manufacturing industries (as ranked in 1929). According to the authors, the remaining 10 industries were excluded only because it was not possible to construct a reasonably consistent time series, given the changes in the definition and constituent sub-industries over the period. In order to match them with my other variables, I exclude two of these industries and merge another two, resulting in 18 industries.[6] Their growth rates are obtained for eight biannual periods between 1921 and 1937 – namely, 1921–1923, 1923–1925, 1925–1927, 1927–1929, 1929–1931, 1931–1933, 1933–1935, and 1935–1937. The sample ends in 1937 because the redefinitions of industries that were introduced in 1939 make comparison with the preceding years difficult (Rosenbloom and Sundstrom, 1999). Panel A of Table 1 summarizes the growth rates in our four indicators over the interwar period, and over its sub-periods

[6] In particular, I exclude the rayon industry (because it contains only a handful of observations) and the cigar industry (because it is an outlier in terms of external dependence). I also merge the lumber and the planing mills industries because I need to map them to a single score of external dependence. The 18 industries used are: boots, bread, canning, chemicals, confectionery, cotton goods, furniture, glass, iron, lumber and planing mill, meat packing, motor vehicle parts, motor vehicles, nonferrous metal, paper, petroleum refining, printing, rubber.

characterized by differences in the incidence of bank suspensions. My sample covers the lion's share of US interwar production: few but large, the sampled industries accounted for some 40 percent of the total US manufacturing output in 1929 (U.S. Bureau of the Census 1929). Importantly, the pattern which holds for all indicators is by far the worst performance in the 1929-1931 period, the interval in which three out of four major banking crises of the Great Depression took place.

TABLE 1—DESCRIPTIVE STATISTICS

	Biannual period	N	Mean	Standard deviation
Panel A. State-industry-period level data				
Output growth$_{sit}$ (%)	1921-1937	2,194	12.61	36.53
	1921-1929	1,097	17.52	33.31
	1929-1931	279	-26.09	35.56
	1931-1933	266	-8.06	27.31
	1933-1937	552	32.36	26.50
Value added growth$_{sit}$ (%)	1921-1937	2,194	14.34	43.93
	1921-1929	1,097	21.56	44.28
	1929-1931	279	-30.31	26.92
	1931-1933	266	4.32	48.69
	1933-1937	552	27.38	30.71
Number of establishments growth$_{sit}$ (%)	1921-1937	2,194	1.47	28.05
	1921-1929	1,097	2.94	21.73
	1929-1931	279	-20.95	22.49
	1931-1933	266	-1.08	47.33
	1933-1937	552	11.10	22.48
Employment growth$_{sit}$ (%)	1921-1937	2,194	7.18	27.72
	1921-1929	1,097	9.78	26.69
	1929-1931	279	-23.10	23.15
	1931-1933	266	0.44	24.86
	1933-1937	552	20.58	19.85
Panel B. State-period level data				
Deposits suspended$_{st}$ (%)	1921-1937	384	4.57	9.38
	1921-1929	192	2.37	4.66
	1929-1931	48	7.50	7.16
	1931-1933	48	19.21	17.01
	1933-1937	96	0.17	0.71
Panel C. State level data				
Rural population in 1910 (%)		48	60.83	22.46

Notes: The observations with few establishments per state are excluded so as to leave at least 95 percent of an industry's product value in a given year. Output values and cost of materials used for calculation of the growth in output and value added were deflated using the "CPI for All Urban Consumers, All Items" from the U.S. Bureau of Labor Statistics of the Department of Labor (2010). The observations of growth rates of all four indicators of manufacturing performance below the 1st percentile and above the 99th percentile of the respective growth rate were recoded to the values of the 1st and the 99th percentile (i.e., winsorized).

Sources: For output growth, Censuses of Manufactures and author's calculations; for deposits suspended, "All Bank Statistics 1896–1955", "Banking and Monetary Statistics 1914–1941", and author's calculations; for rural population in 1910, ICPSR (197?) dataset and author's calculations.

3.2. Bank Suspensions

If financing matters, then output should decline when banks close. I use deposits in suspended banks as the preferred indicator of credit disintermediation. A measure of bank suspension rather than bank failure is chosen because, even when suspensions do not ultimately lead to failure, depositors are still denied immediate access to their funds.[7] In this, I follow the related papers in the literature: Calomiris and Mason (2003) use changes in bank deposits (p.939), while Bernanke (1983) and Benmelech, Frydman, and Papanikolaou (2018) use deposits in suspended banks and the terms failure and suspension interchangeably. Percentage of deposits in all banks suspended is calculated as the total deposits of suspended banks, obtained from the "Banking and Monetary Statistics 1914-1941", published in 1943 by the U.S. Board of Governors of the Federal Reserve System (FRS), *divided by* the deposits of all banks,[8] obtained from the "All Bank Statistics" (FRS 1959). Here an example unit of observation is the percentage of bank deposits suspended in California in 1928 and 1929. Descriptive statistics of deposit suspensions by period are given in Panel B of Table 1.

The suspensions of the March 1933 banking holiday present a challenge for the analysis. This was by far the largest episode of interwar suspensions, but it was accompanied by the federal government's guarantees for surviving banks and measures to implement their capital. Even though the funds in suspended banks were inaccessible, the surviving banks had less reason to constrain lending in the prevailing atmosphere of renewed trust and redepositing of previously hoarded currency. The suspensions of March 1933 were thus an entirely different phenomenon from the preceding ones. Proportional to their size, the effects of suspensions in 1933 should

[7] Suspensions and failures are not the same, and many suspensions in the Great Depression did not result in failures. Researchers initially adopted deposit suspensions (including failures) as a proxy for bank distress – rather than adopting bank failures – possibly because the former measure was available across all states (Anari, Kolari, and Mason, 2005). Anari, Kolari, and Mason (2005) emphasize that suspensions *not* resulting in failures were usually associated with funds being recovered more rapidly and at a higher rate. They argue that, for this reason, the use of suspensions (including failures) would overstate the prevalence of bank failures. The distinction is most relevant to the spring of 1933. I use suspensions because they are a broader measure of financial distress that should yield disintermediation with effects on the real economy. Following a suspension, depositors are immediately denied access to their funds. Moreover, suspensions are enough in themselves to induce uncertainty and the expectation of future bank closings, thereby initiating several of the mechanisms by which banks closing affect the real economy, described in Subsection 4.1.

[8] All banks include commercial banks and mutual savings banks (FRS, 1959). Commercial banks in turn include all national banks and two categories of state commercial banks: incorporated and unincorporated ("private").

have been smaller compared to the effects of earlier suspensions. Several prominent papers in the preceding literature also treated the 1933 bank suspensions in a special way: while Calomiris and Mason (2003) did not include 1933 in their analysis, Bernanke (1983) recoded 1933 suspensions at 15 percent of their value. The latter approach scaled them down to the size of those of October 1931, the worst month for bank suspensions before the bank holiday.

In this paper, I do not ex-ante exclude the 1933 bank suspensions from the analysis, acknowledging that they also should have had effects on the real economy. However, in order to be able to use them in the same panel regressions with "unsupervised" bank suspensions from other periods, I first adjust their size. Instead of reducing them to an assumed fraction like Bernanke does, I first investigate which transformation would make the effect of bank suspensions in the 1932-33 biannual period most comparable in size to that of suspensions in the rest of our interwar sample. Using the transformation identified in this way, I then recode the 1932-33 suspensions before using them in any further analysis.

3.3. Determinant of Bank Suspensions

I use a novel instrument of bank suspensions: the fraction of population rural in 1910. The instrument captures the nation-wide WWI demand shock for agricultural products, but predates any potentially endogeneous reaction to that shock. It is obtained from the ICPSR's dataset 3, where the Decennial Census of the United States in 1910 served as the original source; it is measured as the fraction of population residing in places of fewer than 2500 persons, and is summarized in Panel C of Table 1. By using the fraction of population in rural settlements in 1910 as a state-specific measure of banking system's sensitivity to financial distress, I follow Temin (1989) in acknowledging the role of international disequilibria created by WWI in generating the Great Depression – as well as Calomiris and Mason (2003), who instrument loan supply by using the ratio of bank-owned real estate, that primarily originated from farm foreclosures, to bank loans. The instrument is expected to predict bank suspensions through two related channels: via higher indebtedness of rural residents, and via weaker balance sheets of banks in rural areas. The connection between the fraction of population rural in 1910 and bank suspensions – via both of these channels – is documented in Subsection 4.6, where the exclusion restriction is also discussed.

3.4. Measures of Financial Dependence

I verify that, in states with severe bank suspensions, output of financially dependent industries contracted more than output of their peers. My findings are based on two distinct measures of industry-specific financial dependence. One of them is the *external dependence* of Rajan and Zingales (1998), standard in the literature, while the other one is the *inverse interest cover*, a measure which I argue should be especially suitable for deep recessions.

3.4.1. External Dependence.—The external dependence is equal to the fraction of capital expenditure that is not covered by the cash flow from operations. By measuring dependence for purposes of investment, it is well suited for periods of expansion such as the twenties and the years following the Great Depression. Both of these periods are included in the 1921-1937 sample. Yet also in recessions in general, and the Great Depression in particular, the external dependence ought to matter: to the extent that growth is still hindered by lack of investment, to the extent that firms dependent on banks for investment use the relationship with their financial intermediary also for financing daily business, and to the extent that prior investment that turns out to be irreversible burdens a company during crises (Caggese 2007).

Standard and Poor's Compustat data are used to construct the indicator of external dependence (Compustat 2010). The sample covers US publicly traded companies in the 1950-2007 period. I calculate the measure as the industry average of firm-level data, following the methodology of Rajan and Zingales (1998). The industries from the 1929 Census of Manufactures were first mapped into 1987 Standard Industry Classification (SIC) industries. This matched my data on output growth to the Compustat sample. To make the regression results easier for interpretation, the measure of external dependence was also linearly transformed to set its median to zero and maximum to one, as shown in equation (1) below.

$$T(ED) = (ED - ED_{median}) / (ED_{maximum} - ED_{median}) \quad (1)$$

Panel A of Table 2 summarizes the resulting indicator with a distinct value of external dependence (ED) for each of the sampled industries. The Compustat sample ought to allow us to capture well the technology-driven demand for external financing. Rajan and Zingales argue that external dependence is an inherent property of an industry determined by its technology. They reason that in modern times the supply of finance for large US companies is almost unconstrained; their use of external finance directly relates to their demand. Yet the actual level of debt across industries depends on both the demand for credit and the availability of credit. The indebtedness would thus be changing over the interwar years with the aggregate availability of

TABLE 2 — INDUSTRY CHARACTERISTICS

Panel A.				
External Dependence (ED)	canning	1.00	petroleum refining	0.00
	motor vehicles	0.80	rubber	-0.04
	iron	0.72	motor vehicle parts	-0.08
	meat packing	0.68	bread	-0.20
	lumber and planing mills	0.48	nonferrous metal	-0.36
	paper	0.44	furniture	-0.40
	chemicals	0.24	confectionery	-1.00
	cotton goods	0.04	boots	-2.52
	glass	0.00	printing	-3.00
Panel B.				
Inverse Interest Cover (IIC)	rubber	1.00	paper	-0.06
	boots	0.33	iron	-1.18
	bread	0.27	motor vehicle parts	-1.18
	canning	0.27	motor vehicles	-1.18
	confectionery	0.27	nonferrous metal	-1.18
	meat packing	0.27	chemicals	-1.55
	cotton goods	0.09	petroleum refining	-1.55
	furniture	0.06	printing	-1.58
	lumber and planing mill	0.06	glass	-2.03
Panel C.				
Durability (Durable, Semidurable or Perishable)	furniture	dur	rubber	semidur
	glass	dur	bread	perish
	iron	dur	canning	perish
	lumber and planing mill	dur	chemicals	perish
	motor vehicles	dur	confectionery	perish
	motor vehicle parts	dur	meat packing	perish
	nonferrous metal	dur	paper	perish
	boots	semidur	petroleum refining	perish
	cotton goods	semidur	printing	perish

Sources: For external dependence, Compustat and author's judgment to match the 1929 Census of Manufactures industries with 1987 Standard Industry Classification (SIC) industries; for inverse interest cover, yearly IRS "Statistics of Income" publications and author's calculations; for durability, category definitions from Shaw (1947) and author's judgment in allocating industries to each category.

loans. Size, age and other firm characteristics would also affect the supply constraint. But, the inherent need for external financing determined by technology would remain unaltered. By resulting from a sample of large companies in recent times, our indicator of external dependence ought to capture this need. That I use data from the second half of the twentieth century does leave one concern: the pattern of external dependence across industries could have changed since the interwar years. This paper thus assumes persistence over time in external dependence, just like Mitchener and Wheelock (2013) and Nanda and Nicholas (2014).[9] Notice moreover that applying the US indicator of modern external dependence to the interwar period resembles Rajan and Zingales (1998). They apply it across countries, and the modern companies around the world could resemble their counterparts from the US past by the financing constraints they face and the technology they use.

3.4.2. Inverse Interest Cover.—While the external dependence measures dependence for purposes of investment, much of productive capacity was idle during the Depression. Hence I use another measure of financial dependence that should better predict acute financing needs during deep recessions. In particular, I collected data on the financing structure of US industries in the 1920s to construct an indicator of short-term borrowing needs on the eve of the Great Depression. This indicator is the *interest cover ratio*, which is equal to EBITDA divided by interest expense due in the same year. The ratio is an indicator, first, of pressing current financing needs. When fewer earnings remain after interest payments, a firm will more likely have to borrow. Investment is far indeed from an enterprise's first concern in severe recessions; however, both borrowing to finance working capital and the rolling over of debt remain important. The short maturities of the interwar loans rendered debt rollover a frequently inescapable necessity. The interwar commercial banks specialized in short-term loans by "law and tradition" (White 2000), and the maturity of such loans was seldom more than six months.

[9] Nanda and Nicholas (2014) also show that the correlation between a proxy measure of financial dependence (bank notes payable over fixed assets) for a sample of firms in the 1920s and 1930s and the Rajan and Zingales (1998) measure from the 1980s equals 0.28. But, their proxy measure from the interwar period is not suitable to be used here; the difficulty to match the industries from my sample to the industry groups for which their proxy is calculated would result in a significant reduction of the sample that I use and a measure with little variation across industries.

Second, the interest cover reflects the difficulty of obtaining credit in recessions. That difficulty stems from earnings and interest payments being flow-side measures of (respectively) assets and liabilities. Borrowers with low net worth are viewed as risky prospects during recessions and therefore credit is allocated elsewhere in a flight to quality (Bernanke, Gertler, and Gilchrist 1996). This is a clear advantage of interest cover over external dependence as a measure of sensitivity to bank suspensions. Externally dependent firms may instead be privileged when supply of credit shrinks because banks know them well from periods of expansion. But the output of industries with a low interest cover should be severely affected by bank suspensions; such industries need more credit even as they are at a disadvantage in accessing the credit that remains after banks are suspended. While inverse interest cover is a novel indicator of financial dependence, its logic could still be related to Benmelech, Frydman, and Papanikolaou (2018), who measure the refinancing need by the fraction of bonds maturing within a period, or Almeida et al. (2011), who do the same by the presence of maturing debt.

Unlike external dependence, which is considered an inherent property of each industry driven by its technology, interest cover (IC) indicator could vary across industries for a myriad of historical reasons. I therefore do not assume its stability over time, but construct it as a seven-year industry group average over the period 1922–1928. The ratio captures variation in financing needs across industries at the onset of the Great Depression, and I use it in regressions that cover the 1927–1937 period. Having chosen the indicator for periods of severe recessions, it is thus applied for the years during and surrounding the Great Depression. The data are from the U.S. Internal Revenue Service's yearly "Statistics of Income" publications for the years 1922–1928. The 18 industries were matched to the 10 industry groups used by the "Statistics of Income", so the resulting indicator variable can take any of 10 distinct values. To facilitate interpretation of the regression results, the IC indicator was linearly transformed as shown in equation (2). It was rescaled to set its median to zero and maximum to one. Moreover, because interest cover is an inverse measure of financial dependence, it was inverted to obtain *inverse interest cover* (IIC). Panel B of Table 2 lists the values of the inverse interest cover and the corresponding industry groups.

$$IIC = T(IC) = (IC_{median} - IC) / (IC_{median} - IC_{minimum}) \quad (2)$$

3.4.3. The Difference between the Two Measures of Financial Dependence.—I expect that both measures of financial dependence confirm that, in the presence of bank suspensions, financially dependent industries suffered more than their peers. But, I argue that this should be interpreted as evidence that two different aspects of financial dependence both hindered growth when banks were suspended. One of these aspects – difficulty to finance investment, rely on a close relationship with a financial intermediary in general, or bear the burden of prior investment that turned out to be irreversible – ought to be captured by external dependence while the other one – the inability to finance working capital and debt rollover – ought to be captured by inverse interest cover. To demonstrate that the two indicators should indeed be seen as complements, rather than substitutes, I first note that values of the two measures (presented in Panel A and Panel B of Table 2) have very little in common for the sampled industries. This is evident from Figure 1, the plot of inverse interest cover against external dependence, and is verified by their correlation coefficient of merely 0.046.

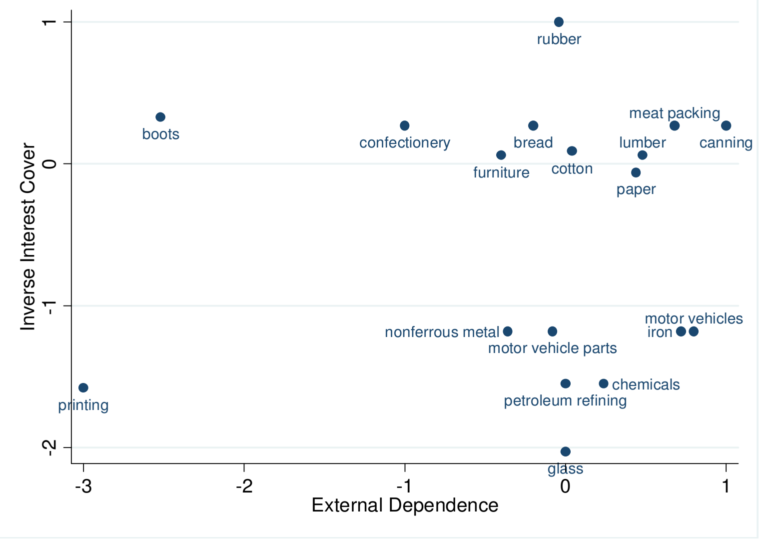

FIGURE 1. RELATIONSHIP BETWEEN THE TWO MEASURES OF FINANCIAL DEPENDENCE

This figure presents a plot of the interest cover measure of financial dependence (IIC) against the external dependence measure of financial dependence (ED).

Moreover, in Section 6, which examines the effect of bank suspensions on manufacturing value added, employment, and number of establishments, I present further evidence that the two measures of financial dependence ought to describe distinct channels by which bank suspensions affect output.

4. Method and Main Results

This section starts with a discussion of the expected difference in the effect of bank suspensions across industries, locations, and periods, and explains the importance of this difference for the chosen method of identification. It then proceeds to test for an effect of bank suspensions on manufacturing output, using graphical analysis for the period of the Great Depression and using regression analysis for the wider interwar period.

4.1. Industry and Location Specific Differences in the Effect of Bank Suspensions

We can expect bank suspensions to cause a decline in output in several different ways. Perhaps the most prominent channel would operate through a reduction in lending to firms. There is much evidence that bank suspensions of the Great Depression led to declines in lending. Observers at the time associated the Depression's banking crises with the "pressure by banks on customers for repayment of loans and [the] refusal by banks to grant new loans" (National Industrial Conference Board 1932). Bernanke (1983) observes that the "shrinkage of credit shared the rhythm of the banking crises." For this reason, we expect the firms in financially dependent industries – in the need for credit both for investment and everyday business – to suffer the most when banks are suspended.

But, bank suspensions can also reduce output in a number of other ways. First, reduced lending to consumers would suppress demand that is normally financed by consumer credit. Second, bank suspensions would create uncertainty about the future availability of credit and the future level of income. The former would lead firms to invest less and keep more cash reserves while the latter would lead consumers to delay irreversible purchases (Romer 1990). Importantly, while the reduction in lending to producers would impact financially dependent industries the most, the impact of bank suspensions through other channels should not depend on the extent of financial dependence.

We further expect all the aforementioned channels by which bank suspensions affect output to be the strongest in the proximity of suspended banks. Wicker (1996, 32) states that a number of bank closings "resulted in the closing of many other banks, partly because of affiliate and correspondent relationships, and partly because of the spread of fear among depositors, particularly in territory near the location of the banks." Similarly, Carlson and Rose (2015) show that the Chambers of Commerce in 1934 blamed mostly the failures of local banks during the Depression for the difficulties in obtaining credit. Declines in lending ought to affect the most the clients of the suspended banks – typically coming from the county of the bank – who subsequently face a higher cost of credit at surviving banks which are unfamiliar with them. The mechanisms involving uncertainty caused by expected future bank suspensions should also result in output declines even beyond the surroundings of the suspended banks. Nonetheless, evidence from the Great Depression suggests that the "spread of fear among depositors" was due mainly to proximate bank suspensions (Wicker 1996). Given that the effect of bank suspensions should be expressed the most in their proximity, we expect two patterns to emerge in states with many suspended banks: declining output, and the most severe declines occurring in financially dependent industries. This conclusion will form the basis of the chosen method of identification.

4.2. Difference in the Effect of 1933 Suspensions and Those in Other Periods on Output Growth

Moreover, we can expect the effect of 1933 bank suspensions and those from other years to be different. In order to measure this difference, I make use of the regression model shown in equation (3). As the dependent variable, g_{sit}, it uses the growth rate of output of industry *i* in state *s* over the biannual period *t*. As explanatory variables, it includes two interactions of bank suspensions in state *s* and over the biannual period *t* ($suspend_{st}$) with period dummies. Dummy variable $non\text{-}1932\text{-}33_t$ is equal to zero in the two year period 1931-33, while $1932\text{-}33_t$ is equal to zero in all other periods. The additional control variable is the one-period lagged output growth ($g_{si,t-1}$). Its inclusion ought to be especially important for the correct estimation of the effect of bank suspensions in the 1931-33 period. This is because the recovery that marked most of the year 1933 is expected to be stronger in those states which suffered the most in the first half of the Depression (1929-31).

$$g_{sit} = \beta_0 + \beta_1 (suspend_{st})(non\text{-}1932\text{-}33_t) + \beta_2 (suspend_{st})(1932\text{-}33_t) + \beta_3 (g_{si,t-1}) + a_s + a_i + u_{sit} \quad (3)$$

I expect that bank suspensions in the 1931-1933 two-year period, and those in all other periods, correspond to a contemporaneous reduction in output growth in the same state. Moreover, because of the uniqueness of the 1933 bank suspensions, in particular the government's efforts to alleviate their consequences, the effect of suspensions in the 1931-1933 period should be smaller. This is exactly what we find. Column (1) of Table 3 reports these results. One percent of deposit suspensions over the 1931-33 period corresponds to 1.2 percent lower growth in the same state, while over other periods it corresponds to 3.8 percent lower growth.

TABLE 3 — THE EFFECT OF BANK *SUSPENSIONS* IN 1933 AND OTHER PERIODS

Dependent variable is output growth $_{sit}$	(1)	(2)	(3)
Fraction of 1933 suspensions included, x	x= 100%	x= 11%	x= 0%
Non-193233$_t$ X Deposits suspended$_{st}$	-3.79	-3.82	-3.73
	(0.29)***	(0.29)***	(0.29)***
193233$_t$X Deposits suspended $_{st}$	-1.22	-3.81	-4.04
	(0.10)***	(0.49)***	(0.63)***
Lagged output growth $_{sit}$	-0.23	-0.21	-0.20
	(0.02)***	(0.02)***	(0.02)***
Observations	2048	2048	2048
R^2	0.25	0.24	0.22
Fixed effects			
State	Yes	Yes	Yes
Industry	Yes	Yes	Yes

Notes: Constants were calculated but were not reported. Regressions are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Heteroskedasticity-robust standard errors are in parentheses. If state-time, state-industry, or industry-time clustered standard errors are used instead, the range of p-value (as marked by asterisks) does not change for any of the coefficients.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, *** Significant at the 10 percent level.

In order to make the effect of bank suspensions of the 1931-1933 period comparable to that of suspensions in other periods, we need to transform them for use in all further analysis. But, the biannual structure of our data means that the bank suspensions of the 1931-1933 mix the pre-holiday suspensions and the suspensions resulting from the banking holiday. For this reason, we start by transforming the 1932-1933 bank suspensions to be equal to $(suspend_{s,1932}) + x(suspend_{s,1933})$, where the fraction of the 1933 bank suspensions, x, is chosen so that β_1 becomes equal to β_2 in equation (3).[10] A gradual reduction of the fraction of the 1933 bank suspensions, x, from 100% to 0%, in increments of 1%, results in values of β_2 that start with lower and end with higher absolute value than β_1, while the two coefficients become equal when x=11%. Column (2) of Table 3 shows the results for output growth, g_{sit}, and x=11%, while column (3) shows the results when x=0%.

Judging based on the existing literature, the uncovered fraction of bank suspensions in 1933 that makes their effect on output similar to that of suspensions in other periods is not at all surprising. This is because Bernanke (1983), working with monthly data, judged the suspensions of March 1933 comparable to those of October 1931, recoding them to 15% of their actual value. In a similar vein, examining a number of indicators of real economic activity, Wicker (1996) concluded that the 1933 panic "did not have effects completely out of line" with those in the earlier banking crises of the Great Depression. But in addition to confirming what was earlier argued with less quantitative evidence, I also show that a reduction of 1933 suspensions to such a fraction makes them comparable not only to the other crises of the Great Depression but also to the bank suspensions of the whole interwar period.

There are however two more reasons to deepen the analysis of the effect of bank suspensions in the 1931-1933 period, prior to deciding on their appropriate transformation. Firstly, it is difficult to argue that the results suggest a single fraction for transforming 1933 suspensions, x, since the values of both β_1 and β_2 can be known only within a confidence interval. Secondly, the

[10] While some 85 percent of total deposit suspensions in 1932 and 1933 occurred in 1933, the pre-holiday suspensions of 1933 – from January and February – account for not more than 6% of the total suspensions in 1933. Given that monthly data were not available on state level, it was not possible to exclude the pre-holiday suspensions of 1933 from the transformation imposed on all the suspensions of 1933.

government intervention associated to the bank holiday of 1933 ought to have resolved the uncertainty created by some of the 1932 suspensions too, terminating the mechanisms, outlined in Subsection 4.1, by which the actions of the surviving banks, firms, and consumers further limit the real economic activity in the aftermath of bank suspensions. Compared to other biannual periods in our sample, the suspensions of the first of the two years, in this case 1932, ought to have had somewhat lower impact on the real economy. In order to address these concerns, I proceed as follows. I allow for both the fraction of the 1932 and the 1933 suspensions, $x1932$ and $x1933$, to vary. For each combination of the two fractions, measured in increments of one percent, I measure the fraction of overlap of the 95% confidence intervals of β_1 and β_2 from equation (3).[11] The higher is the fraction of overlap, the closer are the estimated effects of 1931-1933 bank suspensions on output growth to the effects of bank suspensions in all other periods.

Figure 2 plots the fraction of overlap of the confidence intervals for β_1 and β_2, shown on the vertical axis, as a function of $x1932$, shown on the depth axis, and $x1933$, shown on the primary horizontal axis. A complete overlap of the two confidence intervals is achieved in somewhat less than five percent of combinations of $x1932$ *and* $x1933$. What appears as the highest mountain range in Figure 2 with height equal to one (i.e. 100% overlap) extends from the maximum values of $x1932$ and minimal values of $x1933$ towards lower values of $x1932$ and higher values of $x1933$. But, but the decline in $x1932$ along this reef is much faster than the rise in $x1933$. In fact, for values of $x1933$ higher than 37% there exists no $x1932$ that would create the complete overlap of the two confidence intervals, and for values of $x1933$ higher than 51% there exists no $x1932$ that would lead to even a fractional overlap.

We now proceed to explain the key points of the area with 100% overlap in Figure 2, as well as the direction of extension of this reef. The widest area of the reef shown in Figure 2 extends between points with coordinates ($x1932$, $x1933$)=(0.89, 0.00) and ($x1932$, $x1933$)=(1.00,0.15).

[11] The fraction is calculated as the length of overlap divided by the length of the shorter of the two confidence intervals.

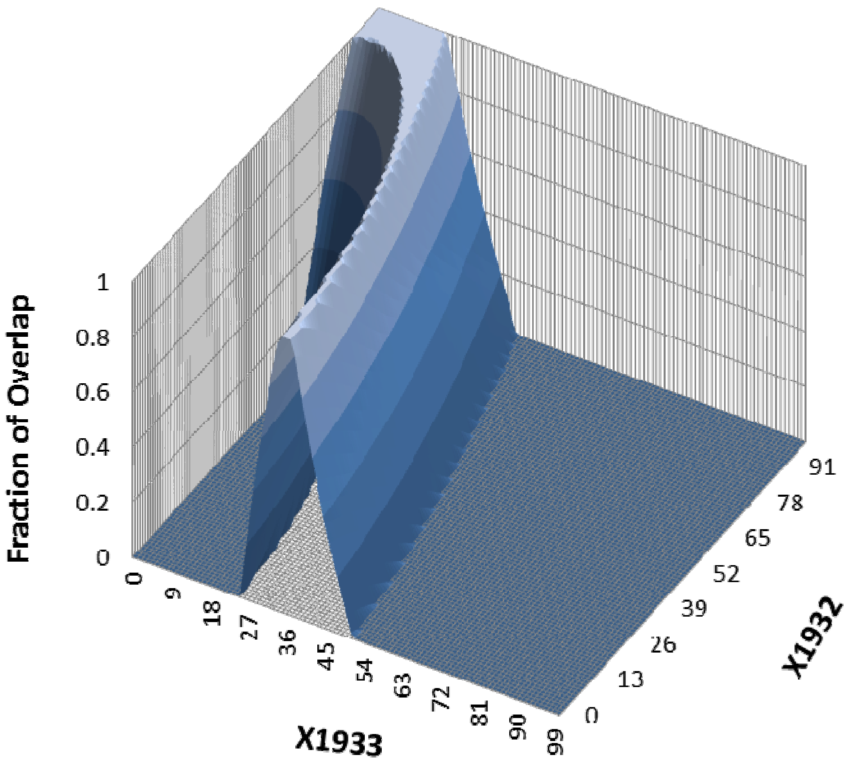

FIGURE 2. THE EQUALITY OF THE EFFECT OF BANK SUSPENSIONS IN THE 1931-1933 BIANNUAL PERIOD ON OUTPUT GROWTH WITH THE EFFECT OF THOSE IN OTHER INTERWAR PERIODS, AS A FUNCTION OF THE FRACTIONS OF 1932 AND 1933 BANK SUSPENSIONS

This figure presents the values of the fraction of overlap of the 95% confidence intervals of β_1 and β_2 from equation (3) as a function of the fractions of 1932 and 1933 bank suspensions used in constructing the measure of suspensions for the biannual period 1931-1933. A high fraction of overlap means that a given combination of the fractions of 1932 and 1933 bank suspensions gives an effect of 1931-33 bank suspensions on output growth similar to that from the other biannual suspensions in the interwar period.

The point with coordinates (*x1932*, *x1933*) = (0.89, 0.00) firstly suggests that not all of 1932 bank suspensions should be included. Namely, in all other biannual periods of our sample the whole second year would also be available for the suspensions of the first of the two years to have non-immediate effects through the uncertainty they created, while the suspensions of 1932 would have effects only in the first quarter of 1933, before the government intervention recovered confidence. This ought to have made the effect of the 1932 suspensions somewhat smaller, just as the estimated *x1932,* being less than one, suggests. It is more difficult to understand the estimated fraction of the 1933 bank suspensions of zero. The examination of the monthly series of manufacturing production, presented in the Subsection A.2.1. of the Appendix, instead showed that the immediate effects of the banking crisis of February and March 1933 were severe. The index of manufacturing production declined by 9 percent in the first quarter of 1933, and reached its lowest level in March. Given such immediate effects of suspensions in early 1933 – even though a strong recovery ensued countrywide after mid-March 1933 – one could expect that the states characterized by more suspensions before the recovery should have seen their companies suffer relatively more on average, also net of the recovery.

Such reasoning however assumes that the forces of recovery had an equal effect across all states, which need not have been the case. In fact, there are at least two reasons to expect that the recovery of the banking sector, following the March 1933 banking holiday, was instead larger in those states with more holiday suspensions, and one more reason for the recovery of manufacturing to be greater in states which experienced more suspensions. Firstly, because both the suspensions of 1933 and the amount of currency hoarded during the whole Depression should have been driven by the weakness of the local banking system, the amount of initially hoarded and subsequently redeposited currency ought to have been greater in states with more 1933 suspensions. Secondly, it would not be a surprise if the measures of government intervention post-March 1933, such as the Reconstruction Finance Corporation's repurchases of capital obligations (including preferred stock) to implement the capital of the reopened banks (Wicker, 1996, pp. 147-8), were especially targeted towards states with more prior bank suspensions. Thirdly, Hausman, Rhode, and Wieland (2019) show that, after devaluation increased dollar prices of tradable farm products in 1933, consumption rose more in rural states – states with more prior bank suspensions – which ought to have helped the local manufactures.

The evidence presented in Figure 3 suggests that the recovery of the banking sector after the banking holiday was indeed more expressed in states with more March 1933 suspensions. The upper two diagrams examine the correlation between the fraction of suspended deposits of all banks in 1933 (on the horizontal axis) with the fraction of change in their deposits (on the vertical axis of the diagram on the left) and capital (on the vertical axis of the diagram on the right), across the US states and over the year following the banking holiday. The period over which the change in deposits and bank capital is measured – June 30th 1933 to June 30th 1934 – is determined by the frequency of the available data. The upper two diagrams demonstrate that both the redepositing of currency, and the rebuilding of bank capital, were more expressed in those states with more March 1933 suspensions. That the 1933 recovery was indeed special is

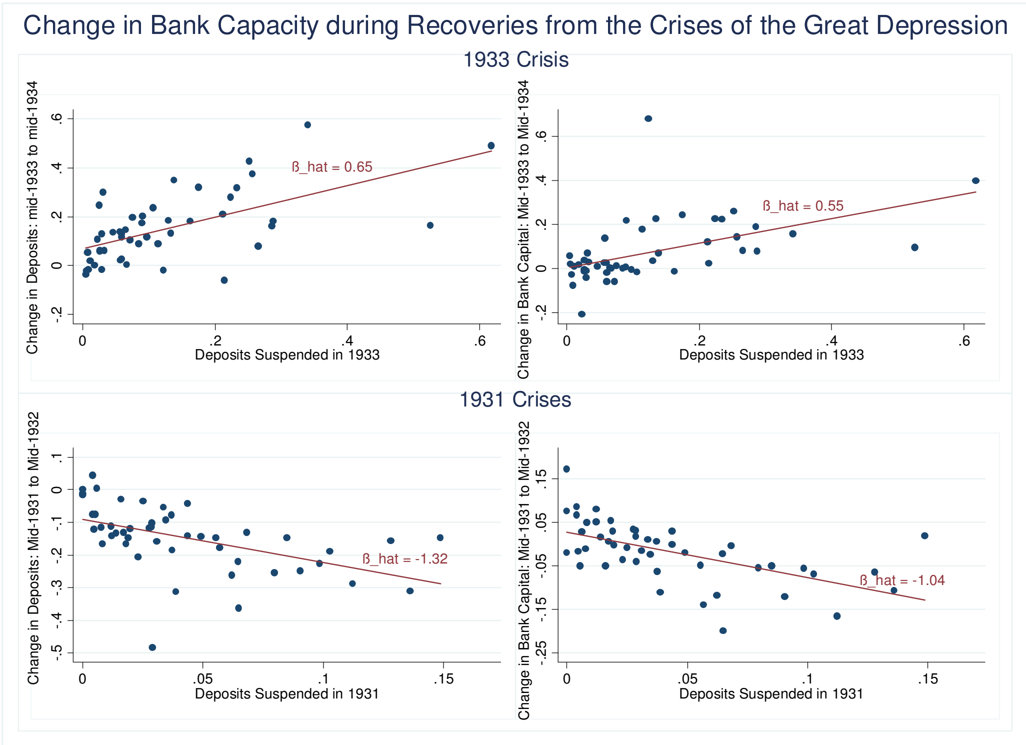

FIGURE 3. THE CHANGE IN DEPOSITS AND BANK CAPITAL ACROSS US STATES FOLLOWING THE CRISES OF 1933 AND 1931

This figure relates the changes in deposits and bank capital (on the vertical axes) of all banks over the one year periods starting in mid-1933, and mid-1931, with the fraction of deposits suspended (on the horizontal axes) in 1933 and 1931, respectively. The observation points are state-specific. The upper two diagrams correspond to the year 1933, while the lower two diagrams correspond to the year 1931. The vertical axes of the diagrams on the left measure the change in deposits, while the vertical axes of the diagrams on the right measure the change in bank capital. Bank capital is measured as the sum of "Capital" and "Surplus and other capital accounts" from "All Bank Statistics 1896–1955". The horizontal axes of all diagrams measure the fraction of deposits held by suspended banks, calculated as the amount of deposits in all banks suspended during a year divided by the amount of deposits held by all banks in that state around June 30th of the previous year. The slopes of the OLS regression lines fitted for all four diagrams are strongly statistically significant, positive for 1933 and negative for 1931.

illustrated by comparison with the period following the crises of 1931: the lower two diagrams correlate the fraction of suspended deposits in 1931 with the fraction of change in deposits and bank capital over the one year period starting in mid-1931. The year 1931 was chosen because all three of the other major banking crises of the Great Depression occurred within it. In sharp contrast to 1933, the states with more suspensions in 1931 were also those with greater decline in deposits and bank capital over the subsequent period, just as one would expect to be in the absence of strong forces of recovery that would counteract these suspensions.[12]

From Figure 3 we conclude that the power of the banks to positively affect the real economy was not as shrunk over 1933 as the variation across states in the size of bank suspensions would suggest. Given that the states characterized by more bank suspensions in 1933 were also those where the banking sector subsequently recovered more, it should not come as a surprise that an effect of 1933 suspensions would be difficult to identify within our biannual sample and that, for the same reason, the estimated fraction of the 1933 bank suspensions, *x1933*, that makes the effect of the suspensions in the 1931-1933 period the most similar to the effect in other periods would be zero, as in the first point that we analyzed, (*x1932*, *x1933*) = (0.89, 0.00).

The second corner point of the widest area of the reef with 100% overlap shown in Figure 2, (*x1932*, *x1933*) = (1.00, 0.15), also allows for an intuitive explanation. The *x1933* = 0.15 is exactly equal to the transformation of March 1933 suspensions (which represent 94% of suspensions in 1933) that Bernanke (1983) used, and has the familiar interpretation. The direction of extension of the reef with 100% overlap, from the two points discussed towards lower values of *x1932* and higher values of *x1933,* but with a maximum *x1933* of 37%, corresponds to an increase in the total national transformed suspensions for the 1932-33 period from 1.6% (*x1932 * National suspensions in 1932* = 100% * 1.6%) to 3.2% (*x1933 * National suspensions in 1933* = 37% * 8.6%). This suggests that, as *x1932* declines and *x1933* rises along the reef, the increasing lack of negative correlation between bank suspensions and output growth in the cross-section of states, resulting from the pattern of recovery post-March 1933, is compensated by identification coming from time series variation; thus, an ever greater national

[12] The uniqueness of the changes in the banking system during 1933 is also evident if, instead of focusing on the recovery phase of the crisis, illustrated in Figure 3, we consider both the decline and the recovery together. In particular, the set of diagrams that plot the changes in deposits and bank capital over the two year period from June 30th 1932 to June 30th 1934, against the 1933 bank suspensions, and over the period from June 30th 1930 to June 30th 1932, against the 1931 bank suspensions, has all four slopes negative. But the slopes for 1933 are around four times smaller in absolute value compared to those in 1931 – equal to around -0.5 compared to -2.0 – and the one corresponding to the change in deposits is statistically insignificant. The evidence from such a set of diagrams is therefore also indicative of a stronger recovery of the banking sector in those states with more 1933 bank suspensions.

level of transformed suspensions is required while moving along the reef. But while the variation across states in the recovery of the banking sector after March 1933 appears to be an important omitted variable for explaining the recovery of the real economy, the fact that changes in deposits and capital could be calculated over intervals that begin and end in the middle of a year makes them unsuitable for inclusion into our regressions that use biannual observations that begin and end in the end of a year.

For this reason, I instead recode the 1932-33 suspensions to (*x1932*, *x1933*) = (1.00, 0.15), the second of the two points discussed. The recoded suspensions are then used in any further – both ordinary least squares (OLS) and instrumental variable (IV) – analysis that involves output growth. Such an adjustment rests on the assumption that the ratio of the OLS coefficients for bank suspensions from different periods is the same as the ratio of their true causal effects. The choice of this point ought to be more conservative than the one from the opposite edge of the reef in Figure 2, (*x1932*, *x1933*) = (0.89, 0.00). This is because preliminary analysis suggested that the estimated effect of bank suspensions is reduced when 1933 bank suspensions are included in any regression. But, importantly, the results using (0.89, 0.00) result in very little quantitative change of estimated coefficients in all regressions that involve output growth, and never lead to a change in their interpretation. Before proceeding with regression analysis that uses a wider interwar period as the sample, the following subsection uses graphical analysis to focus on the period of the Great Depression.

4.3. Graphical Analysis

The graphical analysis presented in this subsection tests for an effect of bank suspensions on output growth during the Great Depression. Figure 4 plots the distribution of growth in output over the Great Depression for the sampled industries, where the observation points are specific to state and industry. The three upper graphs plot output growth conditional on external dependence (ED) of Rajan and Zingales (1998), while the three lower graphs plot it conditional on inverse interest cover (IIC). Irrespective of which measure of financial dependence is used, two

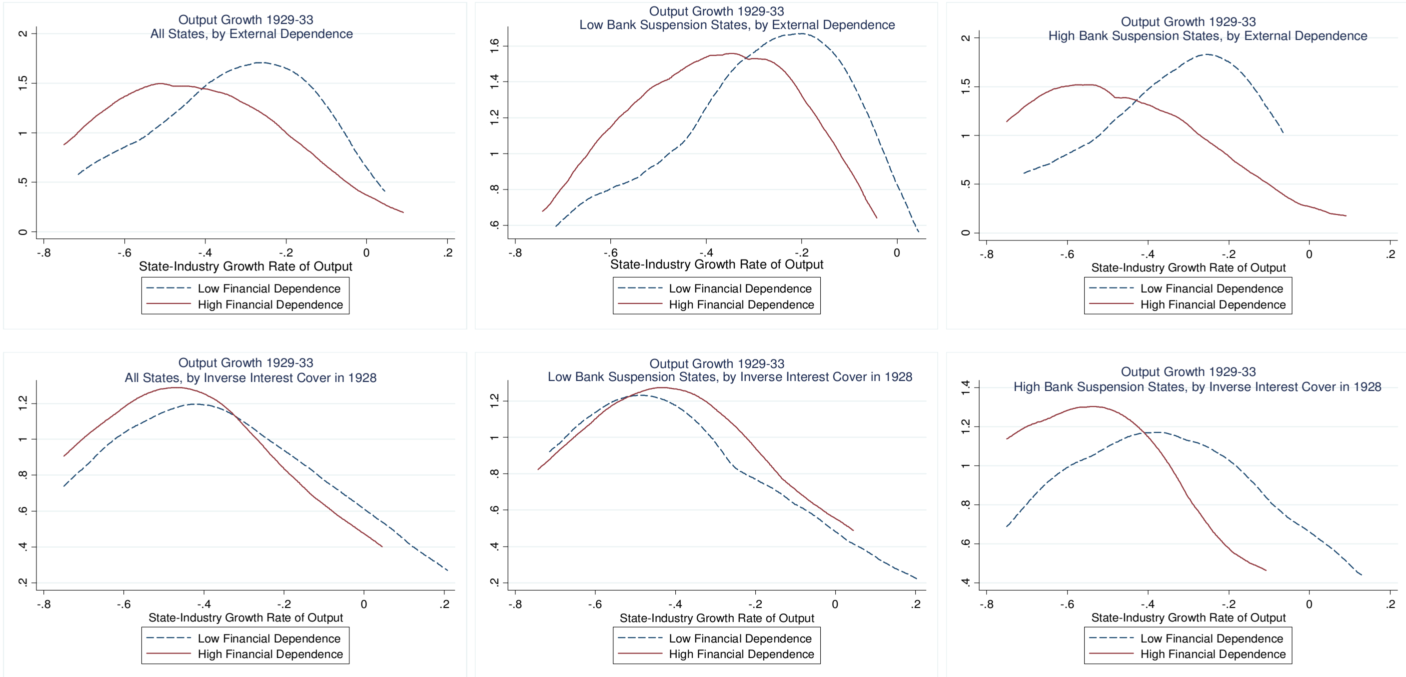

FIGURE 4. DISTRIBUTION OF OUTPUT GROWTH DURING THE GREAT DEPRESSION – CONDITIONAL ON FINANCIAL DEPENDENCE AND INTENSITY OF BANK SUSPENSIONS

This figure presents distributions of output growth during the Great Depression. The observations points are state-and-industry-specific. The upper three diagrams condition growth on the external dependence measure, while the lower three diagrams condition growth on the inverse interest cover measured in 1928. Diagrams on the left plot output growth distributions for all states, diagrams in the middle for the third of states with the smallest fraction of suspended banks, while diagrams on the right for the third of the states with the largest fraction of suspended banks.

relationships are evident. First, the Great Depression – a period with many bank suspensions – is associated with worse performance by financially dependent industries. The upper-left (conditional on ED) and lower-left (conditional on IIC) graphs in the figure illustrate this trend by using observations from all states; the growth distribution of financially dependent industries appears shifted toward lower values. Here the "high" and "low" categories of financial dependence correspond (respectively) to the top and the bottom third of industries. Second, financially dependent industries suffered substantially more in states with more intense bank suspensions, as evidenced in the remaining four graphs of Figure 4. The upper-middle (using ED) and lower-middle (using IIC) graphs plot industry growth in the third of states with the lowest percentage of deposit suspensions, while the upper-right (using ED) and lower-right (using IIC) graphs plot industry growth in the third of states with the highest percentage of deposit suspensions. Here the states are ranked by the intensity of suspensions over the 1929-33 period after first recoding the suspensions of 1932-33 to ($x1932$, $x1933$) = (1.00, 0.15).[13] Using the measure of external dependence we can observe that, even though in both groups of states financially dependent performed worse than their peers, the difference is larger in states with intense bank suspensions. If instead using the measure of inverse interest cover, we can observe a similar pattern: no difference in growth rates in low suspension states becomes a clear underperformance of the financially dependent in high suspension states. Moreover, both measures of financial dependence confirm that most of the worst performers in suspension-intensive states are financially dependent: the upper-right and the lower-right graphs show that the low-growth tails of the distributions are especially thick for financially dependent industries.

4.4. Regression Design and OLS Results

Regression results confirm that the pattern evident in Figure 4 holds not only in the Great Depression but also in the longer interwar period. Moreover, that the pattern of output decline

[13] Using ($x1932$, $x1933$) = (0.86, 0.00) instead replaces only one state from the low failures and two states from the high failures category, and results in no changes that would alter the interpretation of any of the figures presented in this subsection.

across industries is robust to controlling for industry-specific cyclicality. The regressions establish that, in states and periods when banks are suspended, manufacturing output falls and especially in financially dependent industries.

4.4.1. Regression Design.—I test for this two-trait pattern via the following main regression model:

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_2(suspend_{st}) + \beta_3(findep_i) + \beta_4(g_{si,t-1}) + a_s + u_{sit}. \quad (4)$$

The dependent variable is g_{sit}, the growth rate of manufacturing output of industry i in state s over the biannual period t. Biannual frequency – unlike, say, monthly – allows time for the non-immediate effects of suspensions to become evident. In this regression there are four explanatory variables: the percentage of deposits in suspended banks in state s during period t ($suspend_{st}$), the financial dependence of industry i ($findep_i$), their interaction, and the dependent variable itself lagged for one biannual period ($g_{si,t-1}$). A necessary control in standard cross-country growth models would be the state-specific initial level of output. Instead of it, all specifications in this paper include state fixed effects (a_s); they not only control for differences between states in the initial level of output, but also capture other unobserved state characteristics that affect output growth. I expect that the coefficients β_1 and β_2 will both be negative. For states and periods in which 1 percent of deposits are suspended, the β_2 coefficient represents the predicted percent-reduction in growth of an industry with median-level financial dependence and the *sum* of coefficients β_1 and β_2 represents the predicted percent-reduction in growth of the maximally dependent industry (since our measures of financial dependence were linearly transformed to have a median of zero and maximum of one). I also expect the differences between industries with different levels of financial dependence not to be important in the absence of bank suspensions, thus even if β_3 is statistically significant it should be economically unimportant. Finally, I expect the coefficient β_4 to be negative, capturing the rebounds during the business cycle, such as the recovery from the Depression.

To test the robustness of the findings, several other specifications are estimated; they differ in terms of fixed effects and control variables included. The second specification, equation (5), adds industry fixed effects (a_i) in order to prevent any bias in the estimates of β_1 resulting from factors that vary at the industry level. The third specification, equation (6), retains industry and further adds time fixed effects (a_t) in order to control for any omitted factors that affect output and operate simultaneously across all states. Time fixed effects help control for unobservables, but they are expected also to capture the part of the effect of bank suspensions on output that results from the variation over time in the national level of suspensions. I therefore interpret the estimates derived from any specification that includes time fixed effects as a *lower* bound on the true association between bank suspensions and growth. The fourth specification in equation (7) includes not only industry but also state-time fixed effects. Because bank suspensions vary across states and periods, the latter are included to prevent any bias in β_1, the coefficient which measures the difference between the effect of bank suspensions on industries at the median and maximum levels of financial dependence. However, equation (7) cannot be used to estimate the effect of bank suspensions on the median dependent industry itself, measured by β_2, since state-time fixed effects fully absorb them. These three additional specifications are stated formally as follows:

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_2(suspend_{st}) + \beta_4(g_{si,t-1}) + a_s + a_i + u_{sit}; \quad (5)$$

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_2(suspend_{st}) + \beta_4(g_{si,t-1}) + a_s + a_i + a_t + u_{sit}; \quad (6)$$

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_4(g_{si,t-1}) + a_i + a_{st} + u_{sit}. \quad (7)$$

Three more specifications are used to verify that the observed pattern of decline in output across industries with different levels of financial dependence is indeed driven by bank suspensions, rather than a *nation-wide* decline in consumer demand which could have also occurred for reasons other than bank suspensions. The most notable proponents of falling consumer demand as the driver of Depression-era output declines are Mishkin (1978) and Romer

(1990). Romer argues that the general uncertainty about future income, which was caused by the collapse of stock prices in October 1929 and their continued gyrations during 1930, led to a decline in the consumption of durable goods. Consumers stop purchasing durables in times of uncertainty because, if such purchases are irreversible, then it makes more sense to wait until realizing the actual level of future income so that one can choose the optimal level of consumption. Mishkin, too, emphasizes the reduced consumption of durables but argues that the decline in financial wealth caused by the stock market crash led consumers to postpone purchases of both durables and housing so as to remain solvent. Given that the consumption of durables had increased substantially during the 1920s (Olney 1987), an abrupt decline in consumer demand for them could have indeed deepened the Depression.

These three specifications are given in equations (8), (9), and (10) below. They enrich the preceding ones with controls for industry-specific cyclicality. These controls consist of interactions between industry group dummies (with industries classified into durables, semidurables, and perishables, as shown in Panel C of Table 2) and a measure of growth in total output, g_t (sourced from Shaw 1947).

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_2(suspend_{st}) + \beta_4\,(g_{si,t-1}) + \\ + \beta_5(durable)g_t + \beta_6(semidurable)g_t + \beta_7(perishable)g_t + a_s + a_i + u_{sit}; \quad (8)$$

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_2(suspend_{st}) + \beta_4\,(g_{si,t-1}) + \\ + \beta_5(durable)g_t + \beta_6(semidurable)g_t + a_s + a_i + a_t + u_{sit}. \quad (9)$$ [14]

$$g_{sit} = \beta_0 + \beta_1(findep_i)(suspend_{st}) + \beta_4\,(g_{si,t-1}) + \\ + \beta_5(durable)g_t + \beta_6(semidurable)g_t + a_i + a_{st} + u_{sit}. \quad (10)$$

The chosen method thus estimates the cyclicality of each industry group within the interwar period that includes the Great Depression. The advantage of such an approach, compared to

[14] Interaction with perishable is omitted in specification corresponding to equation (9), including time, and specification corresponding to equation (10), including state-time fixed effects because, if all three interactions using durability variables would be included, their sum would equal *National output growth*$_t$ (g_t) which varies only by periods.

using cyclicality betas estimated prior to the Great Depression[15], is that it allows for a change of cyclicality over time; this is especially important if cyclicality is changed by extreme crises. The approach nevertheless has two limitations. Firstly, cyclicality estimates from a period with many bank suspensions could themselves in part result from bank suspensions. In particular, industry groups must have different average dependence on banks which would also influence how deep their nationwide output would fall during the Great Depression. Secondly, including in the regression a measure of growth in the overall nationwide output (g_t) – output which may itself decline due to bank suspensions – makes it harder to find any effect of bank suspensions on the state-industry specific output. But, to the extent that our measures of cyclicality are not capturing all the variation across industries caused by bank suspensions, the interaction effect between bank suspensions and industry specific financial dependence, β_1, should still be negative and significant.

4.4.2. OLS Results.— Ordinary least-squares (OLS) regression estimates of equations (4)-(10) are presented in Table 4. They confirm that the same pattern evident in Figure 4 for the period of the Great Depression also holds when considering the whole interwar period: bank suspensions are associated with lower output growth of industries on average and, as also hypothesized, with even lower growth among the more financially dependent industries.

The results of regressions that use external dependence of Rajan and Zingales (1998) are given in Panel A of Table 4. The first four columns report the results of equations (4)–(7). All four specifications confirm that following bank suspensions financially dependent (i.e. high-ED) industries contract more than their peers. The estimates of β_1 are statistically and economically significant, and are also stable across regressions. On average, the most dependent industry grows 1.0 to 1.2 percent less than the median dependent industry in states and periods with 1 percent of suspended deposits. The degree to which median dependent industries are expected to

[15] This alternative approach was also tried, and resulted in results which were qualitatively identical. It consisted of estimating cyclicality betas for each industry group from the period 1889–1928 (excluding the disturbances from the WWI years of 1914–1920 also made no qualitative difference to the result), removing the predicted cyclical variation from our dependent variable, and then running the regressions based on previous specifications. But, since Gormley and Matsa (2014) show that directly adjusting variables may result in inconsistent estimates, this test was not chosen to serve as the main one.

contract when banks are suspended is also statistically and economically significant, but it varies across specifications. The estimates of β_2 change from −3.9 in the first two specifications (equations 4 and 5) to −0.8 in the specification that includes time fixed effects (equation 6). It would be unduly bold to associate the whole effect estimated in the first two specification (equations 4 and 5) to bank suspensions because time fixed effects control for omitted determinants of output that prevail across all states. Nevertheless, it would be just as misleading to claim that time fixed effects do not capture any effects resulting from bank suspensions. At best one can predict a fairly broad range within the extent of which output growth falls (in the event of bank suspensions). Yet the pattern of decline across industries (measured by β_1) comports well with there being significant real effects of bank suspensions, which result in part from a decline in lending and which affect the financially dependent more. Although externally dependent industries contract more when banks are suspended, the estimate of β_3 in column (1) suggests that they perform marginally better than other industries in the absence of such suspensions. Industries with the maximum score of external dependence (of one) grew 3 basis points more than those with the median score of external dependence (of zero). The estimates of β_4 are negative and both statistically and economically significant: one percent higher growth in the previous period is associated with one fifth to one quarter of a percent of lower growth in the current period.

Columns (5), (6), and (7) of Panel A report the regression results of equations (8), (9), and (10) that use external dependence and include controls for cyclicality of industry-specific output. They show that, even after controlling for cyclicality, the externally dependent are still doing worse than others in the presence of bank suspensions. The coefficient for the interaction between external dependence and deposits suspended remains negative and significant but somewhat decreases; this suggests that external dependence and product durability, and thus cyclicality, were positively associated. A part of the variation in growth across industries with different levels of external dependence could thus be explained by their comovement with national output even in the absence of bank suspensions in the same state. Nevertheless, a substantial part of the effect of same-state bank suspensions remains: the most externally

TABLE 4 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (OLS RESULTS)

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A. (1921-1937): External Dependence (ED)			
ED$_i$ X Deposits	-0.98	-1.00	-0.99	-1.19	-0.63	-0.68	-0.91
	(0.27)***	(0.27)***	(0.29)***	(0.29)***	(0.28)**	(0.28)**	(0.28)***
Deposits suspended$_{st}$	-3.93	-3.95	-0.78		-0.28	-0.42	
	(0.27)***	(0.27)***	(0.25)***		(0.20)	(0.22)*	
ED$_i$	0.03						
	(0.01)***						
Lagged output growth$_{sit}$	-0.20	-0.23	-0.24	-0.23	-0.21	-0.26	-0.25
	(0.02)***	(0.02)***	(0.03)***	(0.03)***	(0.02)***	(0.02)***	(0.03)***
Durable$_i$ X National output growth$_t$					1.94	1.27	1.22
					(0.06)***	(0.07)***	(0.07)***
Semidurable$_i$ X National output growth$_t$					1.00	0.33	0.38
					(0.11)***	(0.12)***	(0.12)***
Perishable$_i$ X National output growth$_t$					0.68		
					(0.06)***		
Observations	2048	2048	2048	2048	2048	2048	2048
R^2	0.20	0.26	0.48	0.57	0.56	0.57	0.64
				Panel B. (1927-1937): Inverse Interest Cover (IIC)			
IIC$_i$ X Deposits	-0.19	-0.26	-0.36	-0.48	-0.83	-0.86	-0.99
	(0.34)	(0.35)	(0.34)	(0.33)	(0.33)**	(0.33)***	(0.32)***
Deposits suspended$_{st}$	-3.88	-3.91	-0.58		-0.36	-0.44	
	(0.31)***	(0.31)***	(0.26)**		(0.22)*	(0.23)*	
IIC$_i$	-0.04						
	(0.01)***						
Lagged output growth$_{sit}$	-0.05	-0.08	-0.17	-0.16	-0.20	-0.24	-0.23
	(0.03)*	(0.03)**	(0.04)***	(0.04)***	(0.02)***	(0.03)***	(0.04)***
Durable$_i$ X National output growth$_t$					1.96	1.21	1.19
					(0.07)***	(0.09)***	(0.09)***
Semidurable$_i$ X National output growth$_t$					0.77	0.02	0.07
					(0.11)***	(0.11)	(0.13)
Perishable$_i$ X National output growth$_t$					0.77		
					(0.06)***		
Observations	1277	1277	1277	1277	1277	1277	1277
R^2	0.22	0.26	0.47	0.54	0.56	0.57	0.62
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for that column in both panels is reported below Panel B. *National output growth*$_t$ is the growth in the sum of output of consumer and producer durables, semidurables, perishables and construction materials, in the whole country. Interaction with perishable is omitted in specifications with time and state-time fixed effects because, if all three interactions using durability variables would be included, their sum would equal *National output growth*$_t$ which varies only by periods. Heteroskedasticity-robust standard errors are in parentheses. Standard errors clustered by state-time, state-industry, and industry-time were also calculated, and their magnitude is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

dependent are always associated to at least 0.6 percent lower output growth than the median dependent industries, in the presence of one percent of deposit suspensions. At the same time, the coefficient for the bank suspensions themselves, β_2, declines. But, this could have however been expected because regressions include a measure of decline in overall nationwide output – which should itself decline due to bank suspensions – making it hard to identify an effect of bank suspensions after the adjustment for cyclicality.

The results of regressions that use inverse interest cover (IIC) are given in Panel B. In sign, they agree with those for external dependence; bank suspensions are associated with lower output growth, and even lower growth among the more dependent industries. But, the estimates of β_1 in columns (1)–(4), which do not control for cyclicality, are statistically insignificant. While the finding that median dependent industries are expected to contract is statistically significant across all specifications, the size of the measured effect varies. The estimates of β_2 change from −3.9 in the first specification (equation 4, column 1), to −0.6 in the specification that includes time fixed effects (equation 6, column 3), but a fraction of this difference ought to be attributed to the effect of bank suspensions themselves. It is however not clear how large a fraction and the estimated range of output decline associated to bank suspensions is thus broad. Although worse covered industries contract more when banks are suspended, the estimates of β_3 suggest they perform much like the others in the absence of suspensions. The worst covered industries grew merely 4 basis points less than median covered industries. This finding rejects the concern that IIC is just a proxy for low profitability, leading to lower growth independently of bank suspensions.

Columns (5), (6), and (7) of Panel B report the estimates of equations (8), (9), and (10) that use inverse interest cover and include controls for cyclicality. Importantly, the coefficient for the interaction between inverse interest cover and deposits suspended is negative, statistically significant, and large in absolute value. It suggests that industries with higher dependence (as measured by IIC) produced more perishable and thus less cyclical products than their peers. So controlling for the cyclicality of output actually reaffirms the finding that the more financially dependent industries suffered more than their peers from bank suspensions. This is so in spite of the fact that IIC does not vary across the full range of 18 sampled industries, but only over 10 industry groups, measuring dependence, albeit a different type of it, less precisely. The evidence from columns (5)–(7) of both panels of Table 4 thus confirms that much of the decline in output was caused by bank suspensions rather than a nationwide decline in consumer demand caused by other reasons. Overall, the pattern of output contraction across industries, observed in Table 4 and robust to controlling for industry-specific cyclicality, is suggestive of important real effects of bank suspensions, at least some of which worked through decline in lending to firms.

The robustness of the results to several other ways of estimating standard errors was also analyzed. In OLS regressions, the baseline estimate of standard errors is heteroskedasticity-robust. The results proved to be very robust to state-time and state-industry clustering, while some of the interaction coefficients lose significance when industry-time clustered standard errors are used.[16]

[16] Clustering of standard errors is needed if there is a reason to believe that observations within a group could be correlated. For instance, output growth of various industries in the same state and in the same period could be correlated for a number of reasons other than the exposure to the same bank suspensions, for instance because of the same local demand, the same business infrastructure, etc. This is a reason to estimate state-time clustered standard errors. Moreover, that bank suspensions vary only across states and periods creates a special need to pursue state-time clustering. Similarly, using state-industry clusters would be needed if observations that correspond to the same state and the same industry are correlated, which means correlation over time of an industry's production within a state, which could well be the case. Finally, using industry-time clusters would be needed if observations that correspond to the same industry in the same period are correlated, which means correlation across states of an industry's production in a period. This could also be the case, in particular to the extent that individual industries depend for their supply or demand from the national market. Even though the need to cluster is reduced by the inclusion of a whole range of fixed effects, the state-time, state-industry, and industry-time clustered standard errors are nevertheless estimated for robustness. The results are very robust to state-time and state-industry clustering: no estimate of β_1 becomes insignificant, while the estimates of β_2 marginally lose significance only in column (5), thus in one of the specifications in which the control for cyclicality must anyhow capture a part of the effect of bank suspensions. The results are more sensitive to clustering by industry-time. While the significance of the estimates of β_2 is never affected, some estimates of $\beta_1$become marginally insignificant. In particular, in columns (5) and (6) of Panel A (for ED measure, leaving (1)-(4) and (7) significant), and in columns (5)-(7) of Panel B (for IIC measure, leaving none significant). But, clustering by industry-time leaves fewer independent observations, especially for the shorter sample used with IIC, which makes it ex-ante harder to detect a statistically significant relationship (Angrist and Pischke, 2009, p. 321).

4.5. Omitted Variables Bias

Without further analysis, we cannot rule out the possibility that omitted variables, or reverse causality, are (in part) driving the OLS results. The coefficient for bank suspensions, β_2, would still be negative if a source of variation which is omitted from the regression were simultaneously causing bank suspensions and output declines. For instance, economic events in the past of a state could act as determinants of both the current growth rates of output and vulnerability of the banking system. It is more difficult to imagine how an omitted variable could lead to a negative coefficient for the interaction term between financial dependence and bank suspensions, β_1. The most likely reason for β_1 to be negative should be a causal link between bank suspensions and output declines, in either direction.

I argue that the stability of the coefficients, presented in Table 4, suggests that omitted variable bias is not likely to be a major concern. As a wealth of fixed effects and cyclicality controls are added, the β_1 coefficient for ED_i *X Deposits* $suspended_{st}$ declines from -0.98 in column (1) to -0.91 in column (7), thus for merely seven percent; the coefficient for IIC_i *X Deposits* $suspended_{st}$ even increases. The coefficient β_2 for *Deposits* $suspended_{st}$ is on the other hand only sensitive to controls that themselves should capture a part of the effect of bank suspensions themselves – time fixed effects and changes in national output growth – which can therefore not be interpreted as evidence against an effect of bank suspensions on manufacturing output. The same argument over omitted variable bias is formally presented using Altonji, Elder, and Taber (2005) ratios is the Appendix Subsection A.1.1.

4.6. Reverse Causality

Altonji et al. ratios thus indicate that OLS coefficients are not likely to be driven by omitted variable bias. But, the concern of reverse causality – which could result in both β_1 and β_2 being negative – remains. Banks that would be suspended because output declined would lead to a negative β_2. In a similar vein, β_1 would be negative if banks are even more likely to be suspended when their financially dependent clients, on whom they rely for business, suffer. In order to establish that bank suspensions indeed *caused* output contractions, I proceed in two steps. First, I present narrative evidence from the Great Depression that speaks in favor of bank suspensions driving output declines. Second, I use an instrumental variable to demonstrate that bank suspensions caused output to decline.

4.6.1. Narrative Evidence on the Causal Effect of Bank Suspensions on Output.—Several pieces of narrative evidence suggest that bank suspensions were not caused by output declines. Following the tightening of the Federal Reserve policy in 1928, and the 1929 stock market crash, the output did shrink by a quarter by October 1930, which should have weakened the banks. But, it is unlikely that the initial wave of bank suspensions in November 1930 was caused by anticipation of future declines in output; the previous recession of 1920-22 did not cause banks to be suspended (Bernanke 1983). Heitfield, Richardson, and Wang (2017) instead show that the initial banking crisis was a contagious panic which spread through the lines of geographic proximity and proximity in the interbank network. After the rise in the incidence of bank suspensions that followed, the problems of the financial system would typically occur *before* declines in output. Moreover, a wealth of reasons for bank suspensions unrelated to decline in U.S. output were documented. Bank runs became a more important cause of bank suspensions at that time, and interbank contagion was important (Richardson 2007b; Calomiris, Jaremski and Wheelock 2019); clearing houses, which would earlier contain runs by suspending convertibility of deposits into currency, now in vain expected the Federal Reserve to act instead. Similarly, reasons for the most famous runs – such as the scandal at the Bank of the United States, failure of Austrian Kreditanstalt, and Britain's abandoning of the gold standard – were not related to output declines. Changes in the loans outstanding also do not appear to have been caused by movements in output but by bank suspensions. Before the first banking crisis at the end of 1930, credit outstanding did not change much. But when banks started being suspended, loans outstanding started to fall following the "rhythm of the banking crises" (Bernanke, 1983).

4.6.2. The Channels through Which Our Instrument Predicts Bank Suspensions.—As an additional, quantitative, piece of evidence that bank suspensions caused output to decline I use an instrumental variable. This is the fraction of population rural in 1910, described in Subsection 3.3. It captures the cross-state exposure to the WWI demand shock for agricultural products but, by predating it, is not affected by any endogenous reaction to this shock. Figure 5 below illustrates that my instrument statistically significantly predicts suspensions in the whole period (upper plot), as well as the 1920s (lower-left plot) and the 1930s (lower-right plot) separately. I further argue that it does so via two related channels: higher rural indebtedness and weaker balance sheets of rural banks.

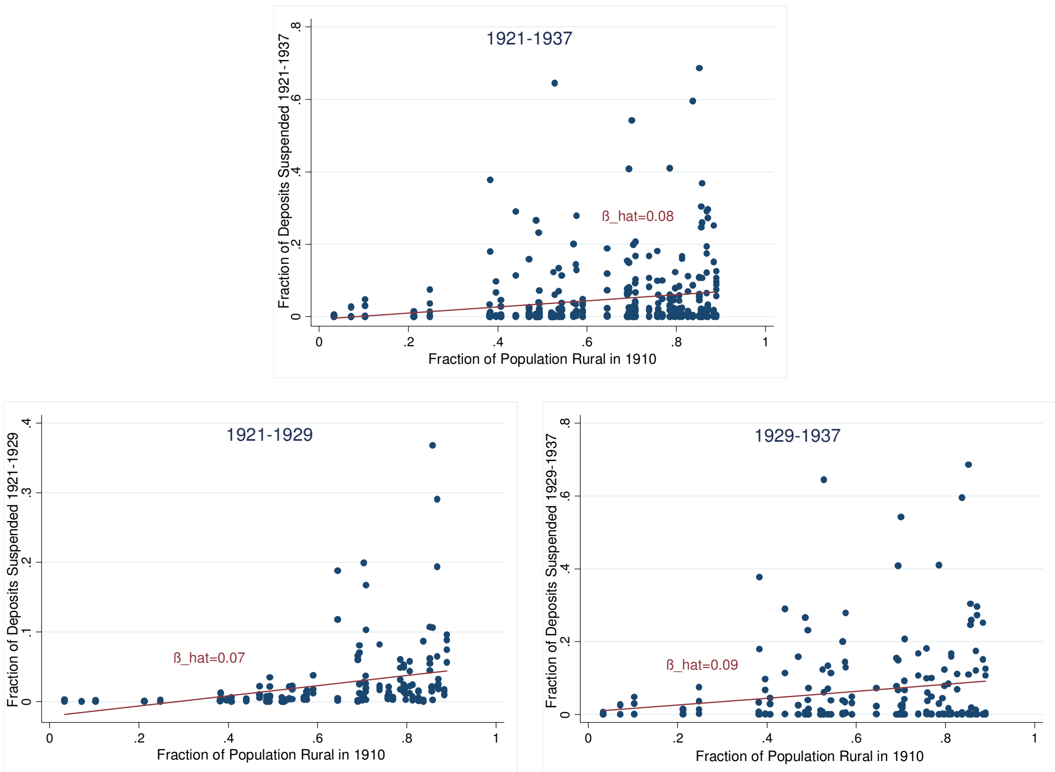

FIGURE 5. THE FRACTION OF POPULATION RURAL IN 1910 AS A PREDICTOR OF BANK SUSPENSIONS

This figure examines the ability of the fraction of population rural in 1910 (on the horizontal axes) to predict the fraction of deposits suspended (on the vertical axes). The observation points are state and biannual-period specific. The upper diagram covers the whole interwar sample (1921-1937), the lower-left diagram covers the 1920s (1921-1929), while the lower-right diagram covers the Great Depression and the 1930s (1929-1937). The OLS relationships identified in the whole sample and the two sub-samples are all statistically significant at least at the 5% confidence level, while the coefficient estimate of the effect of fraction rural in 1910 on the fraction of deposits suspended is reported in each of the subplots.

The fraction of population rural in 1910 firstly proxies for indebtedness in rural areas, one of the reasons for interwar bank suspensions. American farmers saw increased demand for their products while the Europeans were fighting during WWI. They responded by borrowing to improve and expand their farms. The states in which there was greater expansion in agricultural land in the 1910s also had more leveraged farmers in the years that followed. But, when the Europeans returned to their ploughs after WWI, the American farmers saw their income fall. Meeting mortgage payments suddenly became difficult. Higher rural indebtedness then lead to more local bank suspensions following defaults on loans to farmers: such loans made a greater fraction of banks' assets, and their repayment a greater fraction of liquid funds for banks, in areas with greater rural indebtedness. Defaults themselves, frequent after the 1920-21 recession, could have resulted from any shock to income of rural borrowers, primarily related to declining prices of agricultural products.

Secondly, the fraction of population rural in 1910 also proxies for weak balance sheets of banks in rural areas, another reason for interwar bank suspensions; when banks' assets are small relative to their liabilities, they may not be sold for sufficient value to cover the latter when demanded. The failure of banks to meet their obligations is more likely following bank runs – while these are in turn more likely on weaker banks – or following defaults on loans initiated by shocks to local agricultural income. This logic is similar to the one in Calomiris and Mason (2003) who used real estate owned relative to loans as a loan supply instrument. They argued that reductions in the value of bank assets coming from loan foreclosures in the 1920s were an important source of variation in bank loan supply, while here I argue that weaker balance sheets of banks also led to bank suspensions. Moreover, additional suspensions ought to have resulted in areas with both high rural indebtedness and weak balance sheets of rural banks. Where indebtedness was higher, the shocks to banks' balance sheets from defaults on loans ought to have been greater, and where balance sheets were weak, banks' ability to compensate for such shocks was smaller, resulting in even more suspensions from the interaction of these two factors.

The two outlined channels have different testable implications. For our instrument to predict bank suspensions via higher interwar rural indebtedness, the states with a higher fraction of rural

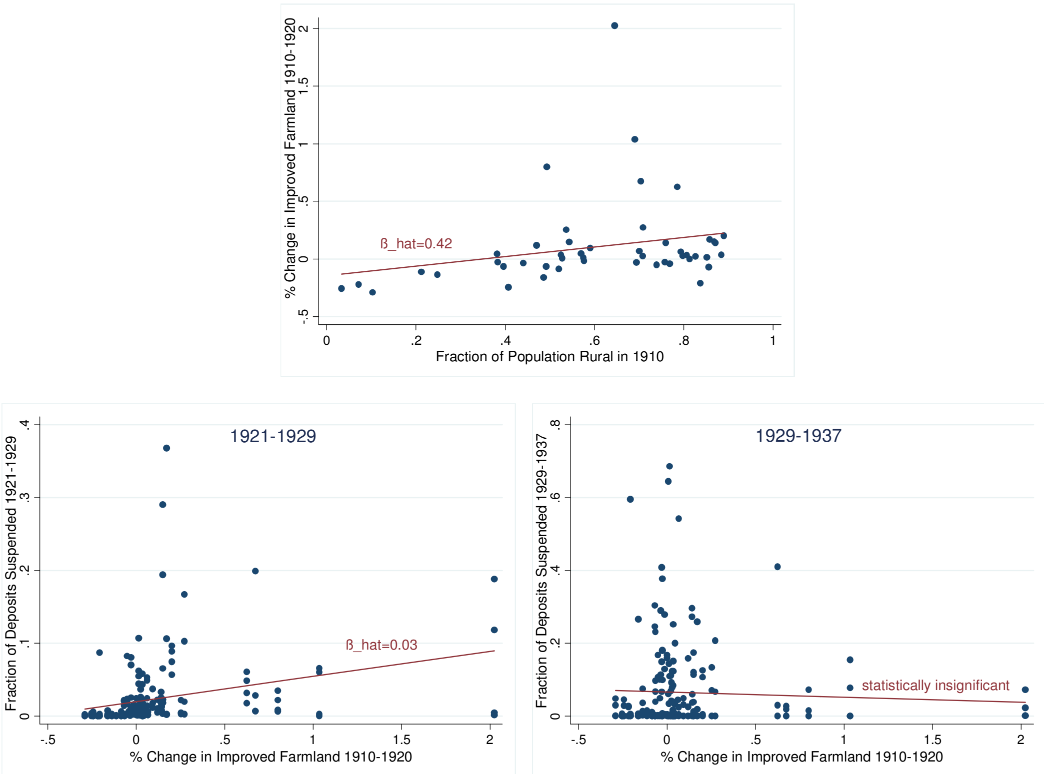

FIGURE 6. THE FRACTION OF POPULATION RURAL IN 1910 AS A PREDICTOR OF BANK SUSPENSIONS THROUGH THE CHANNEL OF RURAL INDEBTEDNESS

This figure examines the ability of the fraction of population rural in 1910 to predict the fraction of deposits suspended, through higher rural indebtedness proxied by the percentage change in improved farmland over the 1910s. The upper diagram examines the ability of the fraction of population rural in 1910 (on the x axis) to predict the percentage change in improved farmland from 1910 to 1920 (on the y axis). The observation points are state specific and the OLS relationship is positive and statistically significant at the 10% confidence level. The lower two diagrams in turn examine the ability of the percentage change in improved farmland from 1910 to 1920 (on the x axis) to predict the fraction of deposits suspended (on the y axis): the lower-left diagram does so over the 1920s (1921-1929), while the lower-right diagram does so over the Great Depression and the 1930s (1929-1937). The observation points are state and period specific and the OLS relationship identified for the 1920s is positive and statistically significant at the 1% confidence level, while the one for the Great Depression and the 1930s is statistically insignificant.

residents in 1910 ought to be those with a greater rise in improved farmland in the 1910s. This is because expansion and improvement of farms was associated with a rise in indebtedness to finance it, as well as a subsequent rise in defaults (Atack and Passel, 1994; Alston, 1983, p.898)[17]. The upper plot in Figure 6 above confirms that the increase in improved farmland over the 1910s was indeed greater in states with a greater fraction of rural population in 1910.

But, the lower two plots in Figure 6 demonstrate that the changes in improved farmland over the 1910s are able to statistically significantly predict bank suspensions only in the 1920s (lower-left plot), while in the 1930s (lower-right plot) the relationship is insignificant. This suggests that bank suspensions that resulted from defaults on agricultural loans were potentially important in the 1920s, while other reasons for bank suspensions were more important in the 1930s. That agrees with Alston (1983), who finds that excessive growth in improved farmland during WWI significantly predicts defaults on agricultural loans in the 1920s, but not in the 1930s.

For our instrument to also predict bank suspensions via weaker balance sheets of rural banks, the states with a higher fraction of rural residents in 1910 ought to have been those with a greater rise in farm value over the 1910s as well as a greater fall in farm value over the subsequent period. The swing in farm value resulted in weaker balance sheets in the 1920s and the 1930s, when banks acquired real estate during foreclosures, as a less valuable substitute for the loans not paid back (Atack and Passel, 1994). Just as declining farm values made farmers less creditworthy borrowers, cutting them away from credit and so making foreclosures more likely, the foreclosures seem to have themselves led to a further decline in farm values (Alston, 1983, p.894), additionally weakening banks' balance sheets. The process evidently continued into the 1930s, given that the interwar foreclosure rates peaked in 1932 and 1933 (Alston, 1983, p.888).

Figure 7 below confirms that states with more rural residents in 1910 are statistically significantly associated to both a greater rise in farm values over the 1910s (upper plot), and a greater fall in farm values subsequently: during the 1920s (lower-left plot), and over the 1930-1935 period (lower-right plot).[18]

[17] The change in farm values, on the other hand, ought to be a less perfect indicator of rural indebtedness. This is because higher indebtedness to expand farmland may not result in higher farm values if the supply of farmland is very elastic. Also, expansion may occur on marginal lands (Atack and Passel, 1994) which may have lower productivity and may lead to a lower average value of farmland.

[18] This swing of farm values in states more rural in 1910 stopped in the second half of the 1930s, but without a statistically significant recovery. Thus rural banks' balance sheets ought to have remained more vulnerable even in the second half of the 1930s.

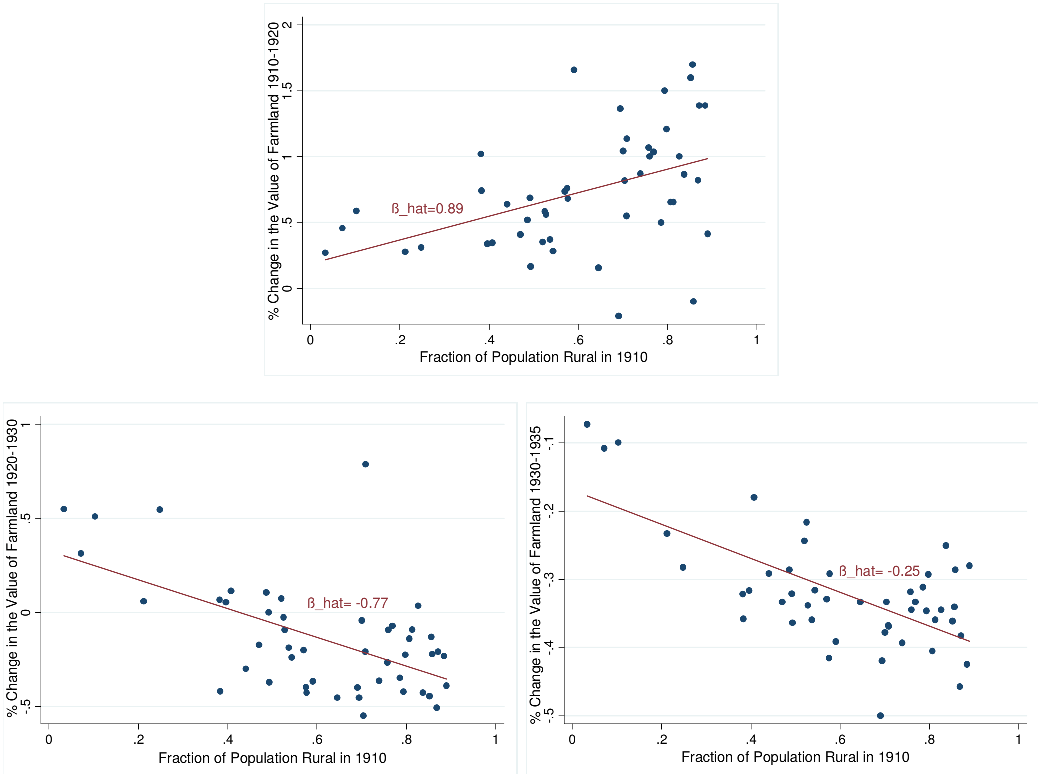

FIGURE 7. THE FRACTION OF POPULATION RURAL IN 1910 AS A PREDICTOR OF BANK SUSPENSIONS THROUGH THE CHANNEL OF WEAKER BALANCE SHEETS OF BANKS – STEP 1: THE FRACTION OF POPULATION RURAL IN 1910 AS A PREDICTOR OF WEAKER BALANCE SHEETS

This figure examines the ability of the fraction of population rural in 1910 to predict the percentage change in the value of farmland, as a proxy for the state of balance sheets of banks exposed to loans to farmers. The observations are state specific. The upper diagram examines the ability of the fraction of population rural in 1910 (on the x axis) to predict the percentage change in the value of farmland from 1910 to 1920 (on the y axis); the OLS relationship is positive and statistically significant at the 1% confidence level. The lower-left diagram examines the ability of the fraction of population rural in 1910 (on the x axis) to predict the percentage change in the value of farmland from 1920 to 1930 (on the y axis); the OLS relationship is now negative and statistically significant at the 1% confidence level. The lower-right diagram examines the ability of the fraction of population rural in 1910 (on the x axis) to predict the percentage change in the value of farmland from 1930 to 1935 (on the y axis); the OLS relationship is also negative and statistically significant at the 1% confidence level.

Moreover, Figure 8 demonstrates that the decline in farm values over the 1920s is able to statistically significantly predict bank suspensions in both the 1920s (upper-left plot) and the 1930s (upper-right plot). That the decline in farm values over the 1920s had not only a contemporaneous but also a delayed effect can be explained by rural banks, with balance sheets weakened during the 1920s, that may have managed to survive until the greater income shocks and more frequent bank runs led to their suspension during the Great Depression. Moreover, the lower plot shows that the further decline in farm values over the 1930-1935 period is able to statistically significantly predict bank suspensions in the 1930s. The evidence therefore suggests that bank suspensions resulting from weak balance sheets of banks were important throughout the sampled period.

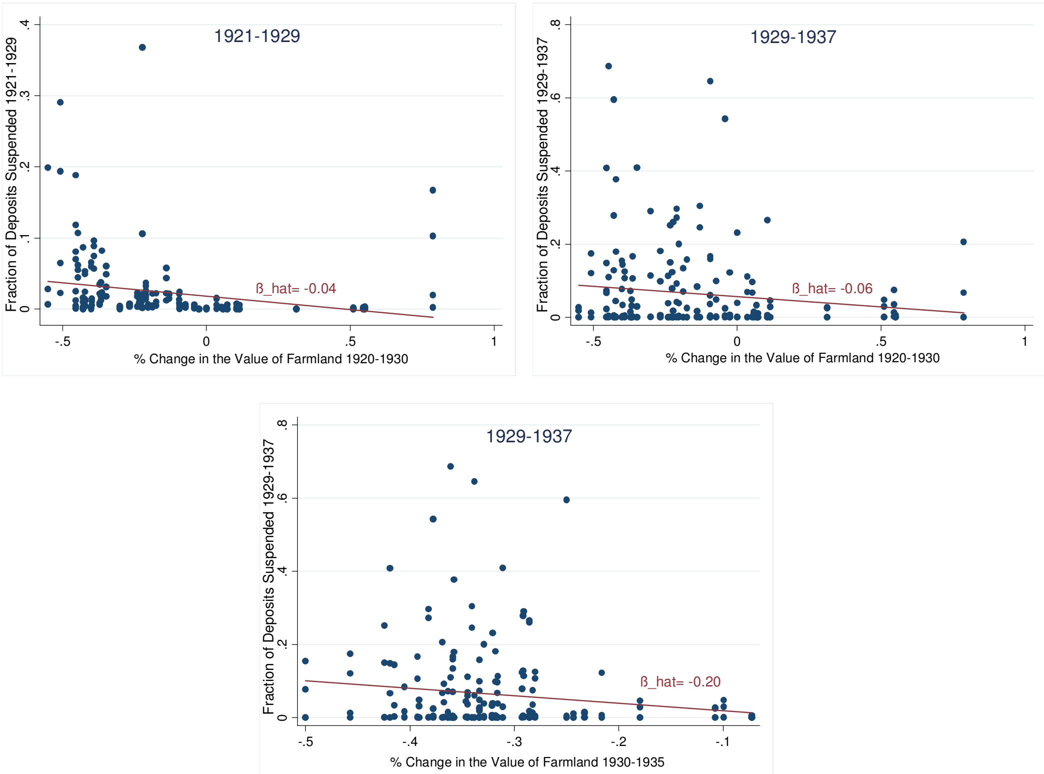

FIGURE 8. THE FRACTION OF POPULATION RURAL IN 1910 AS A PREDICTOR OF BANK SUSPENSIONS THROUGH THE CHANNEL OF WEAKER BALANCE SHEETS OF BANKS – STEP 2: WEAKER BALANCE SHEETS AS A PREDICTOR OF BANK SUSPENSIONS

This figure examines the ability of the percentage change in the value of farmland, my proxy for the state of balance sheets of banks exposed to loans to farmers, to predict bank suspensions. The observations are state and period specific. The upper two diagrams examine the ability of the percentage change in the value of farmland from 1920 to 1930 (on the x axis) to predict bank suspensions (on the y axis); the upper-left diagram does so over the 1920s (1921-1929), while the upper-right diagram does so over the Great Depression and the 1930s (1929-1937). The two OLS relationships are negative and statistically significant at the 1% and 5% confidence level, respectively. The lower diagram examines the ability of the percentage change in the value of farmland from 1930 to 1935 (on the x axis) to predict bank suspensions over the Great Depression and the 1930s (1929-1937) (on the y axis); the OLS relationship is negative and statistically significant at the 5% confidence level.

Overall, the findings suggest that the fraction of population rural in 1910 is a powerful predictor of bank suspensions throughout the interwar period. As one variable predating WWI, it manages to capture two channels by which the WWI demand shock for food caused bank suspensions: through higher rural indebtedness in the 1920s, and through weaker balance sheets of banks in both the 1920s and the 1930s.

4.6.3. The Areas in Which Our Instrument Predicts Bank Suspensions.—The evidence presented above testifies that the fraction of population rural in 1910 is a significant predictor of interwar bank suspensions in the whole sample of states. But, if the instrument is effective only in the more rural states, that would be its important limitation. This is because the IV estimate of the effect of bank suspensions on output could potentially be very different in the majority of other, less rural, states. To address this concern, I examined the power of the instrument to predict bank suspensions in a restricted sample that excludes Mountain and Great Plain states[19]. The excluded states are the more rural states in which, at the same time, both the expansion of improved farmland during the 1910s (more than 40 percent higher than in other states) and the subsequent rise in foreclosure rates (Alston, 1983, p. 890) were greater than elsewhere. The analysis showed that what we found for the complete sample also fully holds in the restricted sample. Firstly, the fraction of population rural in 1910 is also able to statistically significantly predict bank suspensions in the sample that excludes Mountain and Great Plain states. Secondly, it does so via higher rural indebtedness in the 1920s and via weaker balance sheets of banks in both the 1920s and the 1930s. The evidence suggests that our instrument can predict bank suspensions even in the rural areas of the less rural US states. It also confirms that the IV estimate of the effect of bank suspensions on output obtained using our instrument is based on the whole of the United States, not just the more rural states.

4.6.4. The Exclusion Restriction.—I further argue that the fraction of population rural in 1910 would not predict lower growth rates of manufacturing output other than through bank suspensions. The shocks to rural income that lead to bank suspensions in areas with high rural indebtedness and weak balance sheets of rural banks – unlike the shocks to manufacturing income – do not directly affect the growth rates of local manufacturing. There is however an indirect effect of such shocks, and it proceeds through lower local demand for all manufacturing products and higher supply of local labor to local manufacturing. There indeed must have been

[19] In the subsample of Mountain and Great Plain states I included Arizona, Colorado, Idaho, Kansas, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, South Dakota, Texas, Utah, and Wyoming.

less demand for manufacturing output whenever the income of farmers declined. Yet farmers whose agricultural income declined would subsequently be willing to supply their labor to local manufacturing at a lower cost. With the markets for goods better integrated than the labor markets, the net effect on manufacturing production ought to have been favorable. Hence any effect of bank suspensions on manufacturing output that would be found using the fraction of rural population in 1910 as an instrument would constitute a *lower* bound on the true effect.

4.6.5. Two-Stage Least Squares Results.—The instrument of bank suspensions described in Subsection 3.3, the fraction of population rural in 1910 ($rural1910_s$), varies only *across states*. Therefore, it is not able to explain the variation in suspensions *over time*. For the same reason, it cannot be used in specifications that include state fixed effects. To overcome these difficulties, I interact $rural1910_s$ with the period-specific national level of deposit suspensions ($USsuspend_t$). The compound instrument so obtained both varies over time and survives the inclusion of state fixed effects. The rationale behind this approach is that any national shock that propagates across states should lead to more suspensions in states with more rural residents.

The interaction of $USsuspend_t$ with our state-specific determinant of suspensions introduces additional concerns regarding the exclusion restriction, but these are addressed by the fixed effects that we use. Firstly, some manufacturing shocks at the national-level, while unrelated to agricultural markets, could be related to $USsuspend_t$, but would be captured by time fixed effects. Secondly, the interaction of the fraction of population rural in 1910 with the national deposit suspensions ($rural1910_s X USsuspend_t$) could potentially be related to state and period specific shocks to manufacturing that are unrelated to agriculture. These are shocks that hit manufacturing in rural states in times of high national bank suspensions, but are not bank suspensions themselves. While these do not appear as a likely concern, they are controlled for by the inclusion of state-time fixed effects.

Table 5 reports the IV estimates of the causal effect of bank suspensions on output growth of manufacturing industries.[20] The IV results agree with the OLS findings described previously: bank suspensions reduced output, and even more so the output of financially dependent industries. The IV estimates actually predict an even stronger effect of bank suspensions on industries with high financial dependence then does the OLS analysis. The results of regressions that use external dependence (ED) of Rajan and Zingales (1998) are given in Panel A, while those that use the inverse interest cover (IIC) are given in Panel B. The first four columns of each panel report the results of equations (4)-(7), while the following three columns report the results of equations (8)-(10), where the latter ones include controls for cyclicality of industry-specific output.

The β_1 coefficient for the ED_i *X Deposits* $suspended_{st}$, reported in Panel A of Table 5, is more than twice as large in the IV than in the OLS specifications, stable in magnitude, and statistically significant in all regressions. Even the estimates that adjust for cyclicality, reported in columns (5)–(7), are only marginally smaller. Judging based on all seven specifications, the most externally dependent industries contract between 2.4 and 3.1 percent more than median dependent industries as a result of 1 percent of deposit suspensions. The stability of the interaction coefficient to the addition of a broad range of fixed effects, and controls for industry-specific cyclicality, suggests that it is unlikely that there is an omitted variable correlated with the instrument that may be biasing the results. The estimate of the first and second specification (equations 4 and 5, shown in columns 1 and 2) predicts a 7.9 percent decline in growth of the median dependent industry, given by β_2, in response to 1 percent of suspended deposits. This

[20] Appendix Subsection A.1.2 includes tests of our instrument's strength and explains the design of first-stage regressions. Appendix Table A2 demonstrates that our instrument is a strong predictor of state-specific rates of deposit suspensions. The model I use is exactly identified because one instrument is used for one endogenous regressor – deposit suspensions – which avoids any possible bias from overidentification; Table A3 reports the results of first-stage regressions.

TABLE 5 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS)

Dependent variable is output $growth_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: (*x1932*, *x1933*) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. (1921-1937): External Dependence (ED)						
ED_i X Deposits	-2.63	-2.70	-3.14	-2.89	-2.42	-2.50	-2.42
	(0.52)***	(0.51)***	(0.57)***	(0.42)***	(0.54)***	(0.54)***	(0.43)***
Deposits $suspended_{st}$	-7.85	-7.89	-0.30		0.08	-0.57	
	(0.83)***	(0.84)***	(0.90)		(0.56)	(0.78)	
ED_i	0.07						
	(0.01)***						
Lagged output $growth_{sit}$	-0.25	-0.28	-0.28	-0.27	-0.22	-0.29	-0.28
	(0.04)***	(0.04)***	(0.04)***	(0.03)***	(0.02)***	(0.04)***	(0.03)***
$Durable_i$ X National output $growth_t$					1.94	1.16	1.14
					(0.09)***	(0.07)***	(0.07)***
$Semidurable_i$ X National output $growth_t$					1.23	0.47	0.52
					(0.15)***	(0.13)***	(0.13)***
$Perishable_i$ X National output $growth_t$					0.79		
					(0.07)***		
Observations	2048	2048	2048	2048	2048	2048	2048
Clusters	334	334	334		334	334	
Centered R^2	0.02	0.07	0.40	0.53	0.50	0.52	0.61
F-statistics							
Cragg-Donald	502.58	495.89	53.64	1040.30	138.59	54.75	958.65
Kleibergen-Paap	27.60	26.95	5.47	105.87	15.06	5.48	102.39
	Panel B. (1927-1937): Inverse Interest Cover (IIC)						
IIC_i X Deposits	-1.37	-1.55	-1.43	-1.77	-2.12	-2.22	-2.61
	(0.56)**	(0.57)***	(0.53)***	(0.54)***	(0.53)***	(0.55)***	(0.54)***
Deposits $suspended_{st}$	-7.77	-7.87	-1.21		0.05	-0.98	
	(0.72)***	(0.73)***	(0.86)		(0.88)	(0.75)	
IIC_i	-0.01						
	(0.02)						
Lagged output $growth_{sit}$	-0.11	-0.14	-0.18	-0.19	-0.22	-0.26	-0.26
	(0.05)**	(0.05)***	(0.04)***	(0.04)***	(0.03)***	(0.04)***	(0.04)***
$Durable_i$ X National output $growth_t$					2.15	1.28	1.29
					(0.19)***	(0.11)***	(0.09)***
$Semidurable_i$ X National output $growth_t$					0.78	-0.06	-0.00
					(0.12)***	(0.11)	(0.12)
$Perishable_i$ X National output $growth_t$					0.87		
					(0.11)***		
Observations	1277	1277	1277	1277	1277	1277	1277
Clusters	206	206	206		206	206	
Centered R^2	0.05	0.07	0.46	0.53	0.54	0.55	0.60
F-statistics							
Cragg-Donald	421.58	414.09	44.71	733.19	49.69	44.42	686.61
Kleibergen-Paap	35.92	36.42	7.49	166.09	7.19	7.39	152.45
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes

Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. *National output growth*$_t$ is the growth in the sum of output of consumer and producer durables, semidurables, perishables and construction materials, in the whole country. Interaction with perishable is omitted in specifications with time and state-time fixed effects because, if all three interactions using durability variables would be included, their sum would equal *National output growth*$_t$ which varies only by periods. State-time clustered standard errors are in parentheses in the columns (1), (2), (3), (5) and (6) while heteroskedasticity-robust standard errors are in parentheses in columns (4) and (7), the logic for which is explained in the main text and Subsection A.1.2 of the Appendix. The robustness of the results to several other ways of clustering standard errors is discussed in the main text. The corresponding first-stage regressions are reported in Table A3 of the Appendix.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

value, too, is greater than the corresponding OLS estimate of 3.9 percent. However, the statistical significance of the IV estimate of the effect on the median covered industry is more sensitive (than its OLS counterpart) to the inclusion of time fixed effects. In particular, the IV estimate loses its size and becomes statistically insignificant in the third specification (equation 6, shown in column 3). But, the time fixed effects are themselves expected to capture at least a part of the bank suspension effect. Similarly, the lack of statistical significance of β_2 in columns (5) and (6) could be explained by the difficulty to identify an effect of bank suspensions after the adjustment for cyclicality: these regressions include a measure of decline in overall nationwide output, which should itself decline due to bank suspensions. The IV results confirm the OLS finding that, in the *absence* of bank suspensions, industries with different ED levels do not exhibit economically significant differences in growth (as measured by β_3, coefficient for ED_i in column 1). If anything, high-ED industries grow somewhat more when there are no suspensions.

For the inverse interest cover (IIC), the IV results reported in Panel B of Table 5 agree with those of IV regressions that use the external dependence (ED). Bank suspensions cause output declines, and the high-IIC industries are the most affected. The IV estimate of β_1, coefficient for the $IIC_i XDeposits\ suspended_{st}$, is not only negative but, unlike its OLS counterpart reported in Table 4, always large and statistically significant. As a result of 1 percent of deposit suspensions, the most dependent industries contract between 1.4 and 2.6 percent more than median dependent industries. The estimates of β_1 are also robust to controlling for industry-specific cyclicality. That the estimates adjusted for cyclicality, reported in columns (5)–(7) of Panel B, are larger than unadjusted estimates in columns (1)–(4) suggests that industries with higher dependence (as measured by IIC) produced less cyclical products than their peers. Furthermore, the estimates of β_2 in the specifications without time fixed effects, reported in columns (1) and (2), predict a 7.8 percent decline in growth of the median covered industry in response to 1 percent of suspended deposits. This too is larger than a β_2 of around 3.9 percent in the OLS regressions. Just like the IV estimates of β_2 in specifications that use ED measure in Panel A, it is sensitive to the inclusion of time fixed effects and controls for cyclicality. But, this comes as no surprise because time fixed effects in columns (3) and (4) should in part capture the effect of suspensions themselves, and the national output as control in columns (5)–(7) should itself decline due to bank suspensions. Finally, the IV results confirm the OLS finding that industries with different levels of dependence do not exhibit significant differences in growth – measured by β_3 – in the absence of suspensions.

The IV estimates require a different estimation of standard errors compared to the OLS ones. In particular, using heteroskedasticity-robust standard errors would give unwarranted strength to our instruments. This is because the first stage regression for *Deposits suspended*$_{st}$, a variable which varies only by states and periods, would be run over states, periods, *and industries*, just like the first stage regressions for *Findep*$_i$*XDeposits suspended*$_{st}$ and the second stage regression. To obtain realistic test statistics, it is therefore necessary to cluster standard errors at the state-time level in regressions reported in columns (1), (2), (3), (5), and (6). When state-time fixed effects are used in regression equations reported in columns (4) and (7), clustering standard errors by state and period becomes, for this reason, unnecessary because the first stage regression for *Deposits suspended*$_{st}$ is not estimated any more[21]. The heteroskedasticity-robust standard errors are thus reported in place of the clustered ones in columns (4) and (7). Similar to the OLS estimates, the IV results are very robust to state-time and state-industry clustering, while some of

[21] It also becomes infeasible because the number of state-time clusters becomes insufficient to match the number of variables.

the coefficients on interaction terms lose significance when industry-time clustered standard errors are invoked.[22]

4.7. Interpretation of the Results

The results using both measures of financial dependence show that bank suspensions strongly reduced output in the same state, and especially the output of the local financially dependent industries. The size of the overall impact depends on how much of the time fixed effect we associate to bank suspensions. The finding that bank suspensions had the greatest effect on financially dependent industries suggests that at least part of this effect operated via reduced lending to firms. Furthermore, the estimated variation in output decline across industries with different levels of financial dependence, measured by β_1, is both very stable across regressions that include different sets of fixed effects and robust to controlling for industry-specific cyclicality of output. All the results obtained using external dependence (ED) are confirmed using inverse interest cover (IIC). But, results using IIC do more than verify that at least a part of the causal effect of bank suspensions operated through reduction in lending. Because IIC describes a different aspect of financial dependence than ED, the results also reveal that industries in need of banks to finance working capital and depth rollover were especially vulnerable when banks were suspended.

It is striking that the IV estimates of the effect of bank suspensions on the median dependent industry – and of the difference between the effect on median and maximum dependent industries – are larger than the OLS estimates. If reverse causality (whereby difficulties in manufacturing lead to bank suspensions) applied here, then the use of instruments would be expected instead to *reduce* the coefficients obtained. That being said, there are two factors that

[22] Clustering at both state-industry and industry-time level confirms that bank suspensions caused manufacturing output decline: the level of statistical significance, measured by the range of p-value for the coefficient for deposits suspensions, β_2, never changes. In terms of the coefficient β_1 for the interaction between external dependence and deposit suspensions, it is also robust to both clustering at state-industry and industry-time level. The coefficient β_1 for the interaction between inverse interest cover and deposit suspensions is robust to clustering at the state-industry level, but becomes insignificant after clustering at industry-time level in specifications that do not control for cyclicality, and marginally insignificant in specifications which include cyclicality controls. The estimates of the coefficients for the measure of financial dependence, β_3, and lagged growth of output, β_4, are robust to all described types of clustering.

could make the IV parameter estimates larger. First, our instrument should have more predictive power in rural areas where manufacturing establishments on average had less access to nonbank financing than did their urban counterparts, even if the largest firms in a state were able to borrow from banks in a major financial center. Second, deposit suspensions can be interpreted as measuring financial distress with some error. This imprecision arises because expectations of future bank suspensions – which reduce output by altering the behavior of banks, firms, and consumers – are created not only by bank suspensions within a state (my explanatory variable) but also by those in the rest of the country. Since my instrument measures the banking system's fragility, both by predicting higher indebtedness of their rural clients and weaker balance sheets of banks in rural localities, and since banks that are more fragile should be more affected by bank suspensions within *and* outside the state, it follows that the IV inference should in fact be expected to yield a greater effect of bank suspensions, closer to the true effect, than the effect implied by OLS correlations. The instrumenting should be correcting the measurement error both in the coefficient β_1, increased when fragile banks constrain lending to avoid bank runs inspired by suspensions in the rest of the country, and in the coefficient β_2, increased when firms and consumers in states and periods of local banking fragility invest less, keep more cash reserves, and delay irreversible purchases (Romer, 1990). It is also worth emphasizing that the IV results are quantitatively significant: they suggest that bank suspensions could explain a third of the decline in manufacturing output during the Great Depression.[23]

But, can such a large estimated effect of bank suspensions on output stand in agreement with the contemporaneous financing practices? At least for large firms, there ought to have been alternatives: on the eve of the Depression corporate bonds rather than bank loans were the most important source of their financing (Jacoby and Saulnier, 1947), and there was also the option of

[23] The manufacturing sector then accounted for about a third of US output. If we assign half of the time fixed effect to the effect of suspensions, then the effect on the median dependent industry predicted by the IV regressions is 3.75 percent decline in growth for each 1 percent of deposit suspensions. Given that my instruments have greater predictive power in the rural part of the sample, where there are fewer alternatives to banks, it is conservative to use 2.5 percent in the rest of the calculation. In the two years that saw three of the Depression's four banking crises – namely, 1930 and 1931 – more than 5 percent of deposits were suspended in total. The prediction is then for a 12.5 percent decline in output (i.e., 2.5×5) as a consequence of bank suspensions. At the same time, the biannual growth rate of manufacturing output plunged from 8 percent in the 1920s to −32 percent in the Great Depression, a 40 percentage point reversal. So 12.5 percent of that decline, around a third of the total, can be linked with suspensions in the deepening phase of the Depression (1930 and 1931). The year 1932 confirms this result in that its deposit suspensions are comparable to the average amount for 1930 and 1931.

using the equity markets. But, equity financing was rare even among large firms: in a sample of around a thousand of large firms listed in Moody Manual of Investments, Benmelech, Frydman, and Papanikolaou (2018) find that less than a fifth were listed in the NYSE. Moreover, the public bond market collapsed from 1930 to 1934 (Hickman, 1960), and the equity markets also dried up (Benmelech and Bergman, 2017) after the stock market crash in 1929, both speaking in favor of feasibility of a large effect of bank suspensions, as estimated in this paper.

5. The Robustness of IV Estimates

In this section, I confirm the robustness of my instrumental variable estimates using several tests.

5.1. IV Estimates in the Restricted Sample that Excludes Mountain and Great Plain States

One could be concerned that the IV results using the fraction of population rural in 1910 as the instrument could be driven by the effects of bank suspensions in more rural states. This would firstly limit our ability to generalize the results to the rest of the country. Secondly, if the instrument is going to separate less developed from more developed states, this could jeopardize the exclusion restriction; growth rates of manufacturing in some periods could be lower in less developed states for reasons other than bank suspensions. To address these concerns, I re-estimate the IV regressions using a restricted sample which excludes the 14 Mountain and Great Plain states.

Subsection 4.6.3. already demonstrated that our instrument statistically significantly predicts bank suspensions in the restricted sample that excludes Mountain and Great Plain states – through higher rural indebtedness in the 1920s and weaker balance sheets of banks in both the 1920s and the 1930s. Table 6 below instead gives the second stage results; it demonstrates that bank suspensions affected output growth also in the sample that excludes the more rural, Mountain and Great Plain, states.

TABLE 6 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS) IN THE RESTRICTED SAMPLE THAT EXCLUDES MOUNTAIN AND GREAT PLAIN STATES

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: (*x1932*, *x1933*) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. (1921-1937): External Dependence (ED)						
ED$_i$ X Deposits	-1.99	-2.06	-2.37	-2.29	-1.72	-1.77	-1.76
	(0.42)***	(0.42)***	(0.45)***	(0.39)***	(0.44)***	(0.42)***	(0.39)***
Deposits suspended$_{st}$	-7.47	-7.50	-0.86		0.59	-0.34	
	(0.81)***	(0.82)***	(0.67)		(0.47)	(0.53)	
ED$_i$	0.06						
	(0.01)***						
Lagged output growth$_{sit}$	-0.25	-0.29	-0.26	-0.25	-0.22	-0.28	-0.27
	(0.04)***	(0.04)***	(0.04)***	(0.03)***	(0.02)***	(0.03)***	(0.03)***
Durable$_i$ X					2.04	1.19	1.19
National output growth$_t$					(0.08)***	(0.07)***	(0.07)***
Semidurable$_i$ X					1.21	0.39	0.49
National output growth$_t$					(0.14)***	(0.12)***	(0.13)***
Perishable$_i$ X					0.85		
National output growth$_t$					(0.07)***		
Observations	1848	1848	1848	1848	1848	1848	1848
Clusters	272	272	272		272	272	
Centered R^2	0.07	0.12	0.49	0.55	0.56	0.59	0.64
F-statistics							
Cragg-Donald	640.79	631.72	88.56	1087.40	172.40	87.38	996.70
Kleibergen-Paap	24.22	23.83	8.10	98.41	15.70	7.85	94.40
	Panel B. (1927-1937): Inverse Interest Cover (IIC)						
IIC$_i$ X Deposits	-0.98	-1.10	-0.79	-1.02	-1.62	-1.70	-1.91
	(0.49)**	(0.50)**	(0.42)*	(0.50)***	(0.45)***	(0.46)***	(0.49)***
Deposits suspended$_{st}$	-7.40	-7.48	-1.43		0.07	-0.58	
	(0.72)***	(0.73)***	(0.66)**		(0.66)	(0.54)	
IIC$_i$	-0.02						
	(0.02)						
Lagged output growth$_{sit}$	-0.11	-0.14	-0.17	-0.16	-0.22	-0.25	-0.25
	(0.05)**	(0.18)**	(0.04)***	(0.04)***	(0.03)***	(0.04)***	(0.04)***
Durable$_i$ X					2.11	1.28	1.27
National output growth$_t$					(0.15)***	(0.11)***	(0.09)***
Semidurable$_i$ X					0.79	-0.03	0.05
National output growth$_t$					(0.11)***	(0.11)	(0.12)
Perishable$_i$ X					0.84		
National output growth$_t$					(0.09)***		
Observations	1154	1154	1154	1154	1154	1154	1154
Clusters	170	170	170		170	170	
Centered R^2	0.07	0.10	0.49	0.54	0.58	0.59	0.63
F-statistics							
Cragg-Donald	449.86	439.28	62.29	827.64	65.22	60.80	772.24
Kleibergen-Paap	43.93	43.10	10.22	175.12	9.46	10.00	158.06
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No

Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: All regressions whose results are reported in this table are run over the restricted sample that excludes the Mountain and Great Plain states, namely Arizona, Colorado, Idaho, Kansas, Montana, Nebraska, Nevada, New Mexico, North Dakota, Oklahoma, South Dakota, Texas, Utah, and Wyoming. Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. *National output growth*$_t$ is the growth in the sum of output of consumer and producer durables, semidurables, perishables and construction materials, in the whole country. Interaction with perishable is omitted in specifications with time and state-time fixed effects because, if all three interactions using durability variables would be included, their sum would equal *National output growth*$_t$ which varies only by periods. State-time clustered standard errors are in parentheses in the columns (1), (2), (3), (5) and (6) while heteroskedasticity-robust standard errors are in parentheses in columns (4) and (7), the logic for which is explained in the main text and Subsection A.1.2. of the Appendix.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

The IV estimates from the restricted sample show the same pattern as those from the full sample: bank suspensions caused decline in output, and even more so decline in the output of financially dependent industries. This is confirmed by both measures of financial dependence. The suspensions in the areas of the less rural US states where banks are still exposed to rural debtors, predicted by our instrument in the restricted sample, appear to have also had important real effects. Importantly, the size of the effect is still large, even though both the effect on the median dependent industry, given by β_2, and even more so the difference between the effect on the median and the maximum dependent industry, given by β_1, are somewhat smaller (e.g. in regression model shown in column 1: β_2 of -7.5 instead of -7.9, and β_1 of -2.0 instead of -2.6, for ED measure of dependence; β_2 of -7.4 instead of -7.8, and β_1 of -1.0 instead of -1.4, for IIC measure of dependence). The pattern of decrease of the interaction coefficient β_1 in regressions using ED, and increase of the same in regressions using IIC, after controlling for cyclicality of industry-specific output in columns (5)–(7) is also the same as in the unrestricted sample. These findings indicate that the effect of bank suspensions estimated in the full sample is not driven by the more rural and less developed states, but is also present and comparable in magnitude in the remaining US states.

5.2. IV Estimates with Controls for the Structure of the Banking System

Table 6 above illustrates that the IV estimates are not solely based on the effects of bank suspensions in rural states, but also those in the rest of the country. The exclusion of the less economically developed states also lowers the concern that our instrument selects less developed states where growth rates could be lower for reasons other than bank suspensions. Note also that none of our estimates could be driven by merely the difference in the average growth rate of output between states with different levels of development: all regression specifications include state fixed effects which capture such differences. But if less developed states grow less than others only in some periods, when banks also happen to fail, only state-time fixed effects control for such differences, and we already illustrated that the estimates of β_1 in columns (4) and (7) are robust. The inclusion of state-time fixed effects makes it impossible to estimate the coefficient β_2. To address the concern that β_2 could be driven by our instrument selecting states where manufacturing grows less in some periods, especially because small, rural banks are more likely to fail in the face of economic pressure, Table 7 presents regression estimates that control for differences in the structure of the banking system across all states. Banking structure controls include a dummy variable indicator for the presence of state deposit insurance (sourced from Calomiris, 1989a), measures of population per bank (population sourced from Flood, 1998; the number of banks sourced from "All Bank Statistics 1896–1955", FRS 1959) and area per bank (area sourced from ICPSR 197?; the number of banks sourced from FRS 1959) – population per bank and area per bank with two distinct values for 1921-1929 and 1930-1937 – and the ratio of branch bank offices to the total number of bank offices in 1920 (sourced from the "Banking and Monetary Statistics 1914–1941", FRS 1943). All indicators are interacted with a binary variable for the pre Great Depression period and another one for the Great Depression period. Their inclusion, especially in the first stage, makes sure that our instrument, the fraction of population rural in 1920s, does not predict bank suspensions because of coinciding with weaker banking structure areas. The results presented in Table 7 show that the coefficient β_2 is indeed robust to controlling for the period-variant effects of the banking structure. The size of the coefficient does

TABLE 7 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS), CONTROLLING FOR BANKING STRUCTURE

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)				
	Baseline estimates: (1)–(3)			Estimates adjusted for cyclicality: (4)–(5)	
	(1)	(2)	(3)	(4)	(5)
	Panel A. (1921-1937): External Dependence (ED)				
ED$_i$ X Deposits	-2.64	-2.71	-3.11	- 2.43	-2.52
	(0.51)***	(0.51)***	(0.55)***	(0.54)***	(0.53)***
Deposits suspended$_{st}$	-6.33	-6.30	-0.69	0.07	-0.11
	(0.97)***	(0.97)***	(0.61)	(0.51)	(0.53)
ED$_i$	0.07				
	(0.01)***				
Lagged output growth control $_{sit}$	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	Yes	Yes
Banking structure controls$_{st}$ X preGD$_t$	Yes	Yes	Yes	Yes	Yes
Banking structure controls$_{st}$ X GD$_t$	Yes	Yes	Yes	Yes	Yes
Observations	2048	2048	2048	2048	2048
Clusters	334	334	334	334	334
Centered R^2	0.17	0.23	0.41	0.51	0.52
F-statistics					
Cragg-Donald	345.37	343.20	140.86	212.66	140.60
Kleibergen-Paap	27.86	27.84	12.42	22.15	12.37
	Panel B. (1927-1937): Inverse Interest Cover (IIC)				
IIC$_i$ X Deposits	-1.48	-1.66	-1.47	-2.21	-2.27
	(0.55)**	(0.56)***	(0.56)***	(0.56)***	(0.57)***
Deposits suspended$_{st}$	-6.16	-6.21	-1.55	-0.72	-1.04
	(0.82)***	(0.82)***	(0.62)**	(0.54)	(0.55)*
IIC$_i$	-0.01				
	(0.02)				
Lagged output growth control $_{sit}$	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	Yes	Yes
Banking structure controls$_{st}$ X preGD$_t$	Yes	Yes	Yes	Yes	Yes
Banking structure controls$_{st}$ X GD$_t$	Yes	Yes	Yes	Yes	Yes
Observations	1277	1277	1277	1277	1277
Clusters	206	206	206	206	206
Centered R^2	0.24	0.27	0.46	0.55	0.55
F-statistics					
Cragg-Donald	252.36	249.38	99.79	105.91	97.80
Kleibergen-Paap	28.64	28.50	12.52	13.38	12.50
Fixed effects					
State	Yes	Yes	Yes	Yes	Yes

Industry	No	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	Yes
State X Time	No	No	No	No	No

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Lagged output growth is controlled for like in Table 4. Banking structure controls include a dummy variable indicator for the presence of state deposit insurance (sourced from Calomiris, 1989a), measures of population per bank (population sourced from Flood, 1998; the number of banks sourced from FRS 1959) and area per bank (area sourced from ICPSR 197?; the number of banks sourced from FRS 1959) – population per bank and area per bank with two distinct values for 1921-1929 and 1930-1937 – and the ratio of branch bank offices to the total number of bank offices in 1920 (sourced from the "Banking and Monetary Statistics 1914–1941", FRS 1943). All indicators are interacted with a dummy for the pre Great Depression period and another dummy for the Great Depression period. State-time clustered standard errors are in parentheses.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

decline for a fifth, compared to the estimates in Table 5, but they remain large and statistically significant. Moreover, unlike in Table 5, in Panel B of Table 7, which uses IIC as the measure of dependence, they remain significant even after the inclusion of time fixed effects. The estimates of β_1 are also robust in all specifications. Table 7 therefore provides yet another piece of evidence that the estimates obtain using our instrument are not likely to be driven by reverse causality.

5.3. IV Estimates with Controls for Weather Shocks and Growth in Food Prices

The results presented in Table 8 below control for one more set of variables that could potentially endanger the exclusion restriction; these are weather shocks and the growth in prices of agricultural products. Weather shocks and changes in agricultural prices, by influencing the income of farmers, could affect both bank suspensions and the output of rural manufacturing. This is so because farmers are both debtors of rural banks and buyers of at least a part of the local manufacturing products. Because the effect of weather and food prices on both bank suspensions and demand for local manufacturing is greater in states with a greater fraction of rural population, the interactions of weather shocks and growth in food prices with our instrument, fraction of population rural in 1910, are included as additional control variables. State-and-period-specific weather shocks consist of shocks in rainfall and temperature, and are

TABLE 8 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS), CONTROLLING FOR WEATHER SHOCKS AND GROWTH IN AGRICULTURAL PRICES

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)				
	Baseline estimates: (1)–(3)			Estimates adjusted for cyclicality: (4)–(5)	
	(1)	(2)	(3)	(4)	(5)
	Panel A. (1921-1937): External Dependence (ED)				
ED$_i$ X Deposits suspended$_{st}$	-2.67 (0.51)***	-2.76 (0.51)***	-3.25 (0.62)***	-2.46 (0.56)***	-2.54 (0.56)***
Deposits suspended$_{st}$	-9.43 (1.89)***	-9.24 (1.85)***	2.91 (2.77)	1.96 (1.34)	1.62 (1.99)
ED$_i$	0.07 (0.01)***				
Lagged output growth$_{sit}$	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	Yes	Yes
Weather shocks$_{st}$	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$	Yes	Yes	No	Yes	No
Weather shocks$_{st}$ X Rural1910$_s$	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$ X Rural1910$_s$	Yes	Yes	Yes	Yes	Yes
Observations	2048	2048	2048	2048	2048
Clusters	334	334	334	334	334
F-statistics					
Cragg-Donald	99.34	98.04	8.60	28.51	9.00
Kleibergen-Paap	14.22	14.02	1.58	3.23	1.66
	Panel B. (1927-1937): Inverse Interest Cover (IIC)				
IIC$_i$ X Deposits	-1.48 (0.53)**	-1.67 (0.55)**	-1.43 (0.52)***	-1.66 (0.80)**	-2.31 (0.56)***
Deposits suspended$_{st}$	-5.76 (2.23)***	-5.99 (2.29)***	-1.58 (3.53)	8.10 (12.44)	-2.95 (4.00)
IIC$_i$	-0.01 (0.02)				
Lagged output growth control $_{sit}$	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	Yes	Yes
Weather shocks$_{st}$	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$	Yes	Yes	No	Yes	No
Weather shocks$_{st}$ X Rural1910$_s$	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$ X Rural1910$_s$	Yes	Yes	Yes	Yes	Yes
Observations	1277	1277	1277	1277	1277
Clusters	206	206	206	206	206
F-statistics					
Cragg-Donald	31.47	30.85	2.52	1.80	2.65
Kleibergen-Paap	3.79	3.71	0.36	0.28	0.38

Fixed effects					
State	Yes	Yes	Yes	Yes	Yes
Industry	No	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	Yes
State X Time	No	No	No	No	No

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Lagged output growth and cyclicality are controlled for like in Table 4. Weather shocks, obtained from the National Climatic Data Center (2009), include precipitation and temperature shocks which are calculated as the difference between the weather conditions in the current year and the average of conditions in the previous five years. Growth in agricultural prices is calculated as growth in the price index for food, "CPI for All Urban Consumers: Food", sourced from the U.S. Bureau of Labor Statistics of the Department of Labor (2010). State-time clustered standard errors are in parentheses.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

calculated as the differences between the weather conditions in the current year and the average of conditions in the previous five years. They are obtained from the National Climatic Data Center (2009). The growth in agricultural prices is calculated as growth in the price index for food, "CPI for All Urban Consumers: Food", sourced from the U.S. Bureau of Labor Statistics of the Department of Labor (2010); it is intended to additionally capture those shocks to agricultural income which are unrelated to weather.

Since location-specific weather shocks and national price shocks interacted with fraction of rural population are state-period variant, none of the specifications in Table 8 can include state-period fixed effects. The results presented in Table 8 show that both of our coefficients of interest, β_1 and β_2, are robust to controlling for shocks to agricultural income. This applies to both specifications without and with cyclicality controls, presented in columns (1)–(3) and (4)–(5) respectively. While the coefficient for the interaction between financial dependence and bank suspensions, β_1, is statistically significant in all specifications, the coefficient for bank suspensions themselves, β_2, loses significance only after inclusion of time fixed effects in column (3), while its estimates in columns (4) and (5) are not informative of the effect of suspensions because they control for decline in the overall nationwide output, itself in part driven by bank suspensions.

5.4. IV Estimates with Controls for Industries that Use Agricultural Inputs

One third of the industries in our sample – namely (leather) boots, bread, canning, confectionery, cotton goods, and meat packing – uses agricultural inputs which could at least in part come from farms in the same state. At the same time, our instrument predicts bank suspensions in areas with high rural indebtedness and weak balance sheets of rural banks. The shocks to rural income that lead to bank suspensions in such areas would coincide with lower availability or higher prices of agricultural inputs. For instance, local weather shocks or national shocks in food prices would at the same time represent shocks to rural income which cause bank suspensions and supply shocks for those six manufacturing industries. In case those six industries are at the same time more financially dependent than their peers, the pattern of output decline across industries identified using IV regressions, measured by the coefficient for the interaction between financial dependence and bank suspensions, β_1, could be driven by shocks to the supply of agricultural inputs rather that bank suspensions. In order to address this concern, I proceed in two steps. Firstly, I control for these industries by re-estimating our original regression specifications after including interaction variables – between weather shocks and growth in food prices on the one hand, and a dummy variable for industries that use agricultural inputs, $agric_i$, on the other hand; the effect of weather shocks and changes in food prices ought to be different on industries that use agricultural inputs. The results of these regressions are reported in Table 9. Secondly, I re-estimate our original specifications in a restricted sample that excludes those six industries which use agricultural inputs; and report these results in Table 10.

The results in Tables 9 and 10 suggest that our IV results, obtained using the fraction of population rural in 1910 as the instrument, are robust to controlling for supply shocks to those manufacturing industries in our sample that use agricultural inputs. The results in Table 9 show that, after controlling for a differential effect of weather shocks and growth in food prices on industries that use agricultural inputs, the variation across industries with different levels of external dependence becomes somewhat less expressed (i.e. β_1 declines in absolute value), while the variation across industries with different levels of inverse interest cover becomes somewhat

TABLE 9 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS), CONTROLLING FOR SUPPLY SHOCKS TO THE SIX INDUSTRIES WHICH USE AGRICULTURAL INPUTS

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. (1921-1937): External Dependence (ED)						
ED$_i$ X Deposits	-2.50	-2.61	-3.17	-2.92	-2.26	-2.35	-2.26
	(0.52)***	(0.52)***	(0.57)***	(0.42)***	(0.52)***	(0.52)***	(0.40)***
Deposits suspended$_{st}$	-10.01	-9.98	-0.32		-1.03	-0.62	
	(1.28)***	(1.27)***	(0.95)		(0.80)	(0.83)	
ED$_i$	0.07						
	(0.01)***						
Agricultural industries$_i$	-0.04						
	(0.11)						
Lagged output growth$_{sit}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	No	Yes	Yes	Yes
Weather shocks$_{st}$	Yes	Yes	Yes	No	Yes	Yes	No
Food price growth$_t$	Yes	Yes	No	No	Yes	No	No
Weather shocks$_{st}$ X Agricultural industries$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$ X Agricultural industries$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2048	2048	2048	2048	2048	2048	2048
Clusters	334	334	334		334	334	
F-statistics							
Cragg-Donald	294.68	293.02	48.52	1039.40	71.13	49.41	932.61
Kleibergen-Paap	38.24	38.11	4.99	107.46	6.64	4.97	107.29
	Panel B. (1927-1937): Inverse Interest Cover (IIC)						
IIC$_i$ X Deposits	-2.03	-2.26	-2.35	-2.70	-1.87	-1.94	-2.36
	(0.69)***	(0.69)***	(0.64)***	(0.59)***	(0.54)***	(0.55)***	(0.52)***
Deposits suspended$_{st}$	-8.85	-8.95	-1.07		-0.19	-0.92	
	(1.25)***	(1.24)***	(0.93)		(1.15)	(0.86)	
IIC$_i$	-0.00						
	(0.02)						
Agricultural industries$_i$	0.21						
	(0.13)*						
Lagged output growth$_{sit}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	No	Yes	Yes	Yes
Weather shocks$_{st}$	Yes	Yes	Yes	No	Yes	Yes	No
Food price growth$_t$	Yes	Yes	No	No	Yes	No	No
Weather shocks$_{st}$ X Agricultural industries$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Food price growth$_t$ X Agricultural industries$_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1277	1277	1277	1277	1277	1277	1277
Clusters	206	206	206		206	206	
F-statistics							

Cragg-Donald	177.74	175.90	37.86	538.94	32.00	37.98	541.74
Kleibergen-Paap	29.13	28.87	6.43	126.93	4.50	6.38	130.30
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Lagged output growth is and cyclicality are controlled for as in Table 4. Weather shocks, obtained from the National Climatic Data Center (2009), include precipitation and temperature shocks which are calculated as the difference between the weather conditions in the current year and the average of conditions in the previous five years. Growth in agricultural prices is calculated as growth in the price index for food, "CPI for All Urban Consumers: Food", sourced from the U.S. Bureau of Labor Statistics of the Department of Labor (2010). State-time clustered standard errors are in parentheses in the columns (1), (2), (3), (5) and (6) while heteroskedasticity-robust standard errors are in parentheses in columns (4) and (7), the logic for which is explained in the main text and Subsection A.1.2 of the Appendix.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

more expressed (i.e. β_1 increases in absolute value), but the same pattern identified by our baseline IV estimates in Table 5 is fully preserved: bank suspensions caused declines in output, especially severe for financially dependent industries. This pattern is also evident even in the restricted sample that excludes the six industries that use agricultural inputs, Table 10. In fact, the estimates of β_1 are even greater in absolute value, besides those for inverse interest cover when cyclicality is controlled for, but even then they retain statistical significance.

TABLE 10 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE (IV RESULTS) IN THE RESTRICTED SAMPLE THAT EXCLUDES THE SIX INDUSTRIES WHICH USE AGRICULTURAL INPUTS

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A. (1921-1937): External Dependence (ED)						
ED$_i$ X Deposits	-3.75	-3.75	-4.52	-3.80	-2.64	-2.87	-2.19
	(0.74)***	(0.75)***	(0.87)***	(0.63)***	(0.77)***	(0.76)***	(0.59)***
Deposits suspended$_{st}$	-8.75	-8.76	0.93		2.44	1.32	
	(1.04)***	(1.04)***	(1.29)		(0.85)***	(1.12)	
ED$_i$	0.08						
	(0.01)***						
Lagged output growth$_{sit}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	No	Yes	Yes	Yes
Observations	1165	1165	1165	1165	1165	1165	1165
Clusters	316	316	316		316	316	
F-statistics							
Cragg-Donald	267.16	264.94	29.99	502.54	83.97	30.42	400.14
Kleibergen-Paap	27.57	27.12	4.52	48.31	14.55	4.48	49.05
	Panel B. (1927-1937): Inverse Interest Cover (IIC)						
IIC$_i$ X Deposits	-2.66	-2.76	-3.39	-2.93	-0.77	-0.87	-0.99
	(0.76)***	(0.78)***	(0.74)***	(0.60)***	(0.49)	(0.47)*	(0.48)**
Deposits suspended$_{st}$	-9.49	-9.58	0.17		2.18	0.90	
	(1.03)***	(1.04)***	(1.48)		(1.46)	(1.09)	
IIC$_i$	0.02						
	(0.02)						
Lagged output growth$_{sit}$	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cyclicality controls$_{it}$	No	No	No	No	Yes	Yes	Yes
Observations	723	723	723	723	723	723	723
Clusters	196	196	196		196	196	
F-statistics							
Cragg-Donald	270.64	264.30	24.04	442.68	30.61	25.18	393.91
Kleibergen-Paap	38.35	38.65	6.03	108.42	6.56	6.44	107.76
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: All regressions whose results are reported in this table are run over the restricted sample that excludes the six industries which use a substantial amount of agricultural inputs: boots, bread, canning, confectionery, cotton goods, and meat packing. Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Lagged output growth is and cyclicality are controlled for as in Table 4. State-time clustered standard errors are in parentheses in the columns (1), (2), (3), (5) and (6) while heteroskedasticity-robust standard errors are in parentheses in columns (4) and (7), the logic for which is explained in the main text and Subsection A.1.2 of the Appendix.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

6. The Effect of Bank Suspensions on Several Other Indicators of Manufacturing Performance

In this section, I extend my analysis to demonstrate that bank suspensions not only affected manufacturing output, as shown thus far, but also manufacturing value added, employment, and the number of establishments. In doing this, I also find additional evidence that the two measures of financial dependence, ED and IIC, describe distinct channels by which bank suspensions affected the real economy.

6.1. Graphical Analysis of the Effect of Bank Suspensions on Several Other Indicators of Manufacturing Performance

The graphical analysis presented in this subsection tests for an effect of bank suspensions on growth in value added, number of establishments, and employment during the Great Depression, and further explores the difference between the two measures of financial dependence introduced in Subsection 3.4. It consists of three figures that analyze three distinct indicators of manufacturing performance, similar to what Figure 4 does for output growth in Subsection 4.3. Figure 9 plots the distribution of growth in value added over the Great Depression. The observations points are state-and-industry-specific. While its subplots appear somewhat different to those in Figure 4, they do confirm everything that has been said for growth in output: financially dependent industries contracted more during the Great Depression, especially in states with more intense suspensions. Moreover, the evidence presented in Figures 4 and 9 suggests that two different aspects of financial dependence affected growth following bank suspensions: both the difficulty to finance new or reverse prior investment, measured by external dependence, and the difficulty to finance working capital and debt rollover, measured by inverse interest cover.

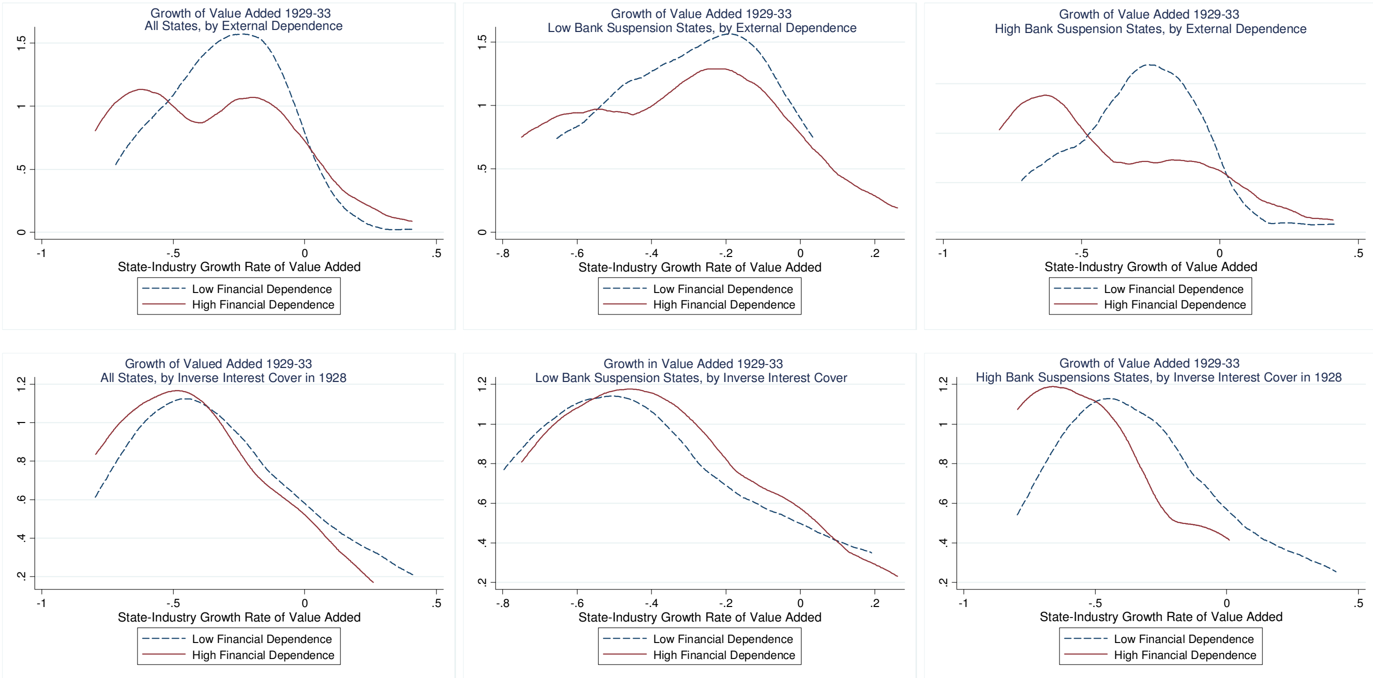

FIGURE 9. DISTRIBUTION OF GROWTH IN VALUE ADDED DURING THE GREAT DEPRESSION – CONDITIONAL ON FINANCIAL DEPENDENCE AND INTENSITY OF BANK SUSPENSIONS

This figure presents distributions of growth in value added during the Great Depression. The observations points are state-and-industry-specific. The upper three diagrams condition growth in value added on the external dependence measure, while the lower three diagrams condition growth in value added on the inverse interest cover measured in 1928. Diagrams on the left plot output growth distributions for all states, diagrams in the middle for the third of states with the smallest fraction of suspended banks, while diagrams on the right for the third of the states with the largest fraction of suspended banks.

Figures 10 and 11, plotting the distribution of growth in the number of establishments and employment, respectively, not only verify that bank suspensions mattered during the Great Depression, but also help us confirm that the two measures of financial dependence describe distinct channels by which bank suspensions affected output. Several predictions can be made in order to outline the difference between these channels. When banks are suspended, we can expect that more externally dependent industries – by construction of the measure of dependence more reliant on external financing for investment purposes – would experience a sharper decline in investment than their peers. But the same need not hold for industries with different values of inverse interest cover. Unfortunately, the data on investment which would be needed to test this are not available from the Censi of Manufactures in the period.

We can also expect that, when banks are suspended, firms with high dependence, measured using the interest cover indicator, are more likely to exit the market. Here is why: when a firm has high inverse interest cover, it is more likely to be in need of external financing for debt rollover and working capital. On the level of an industry, the inability to satisfy these needs following bank suspensions could lead to a rise in bankruptcies or to a severe reduction in capacity. Facing difficulty to roll over debt, a firm is more likely to become bankrupt. But, bankruptcies themselves did not rise much during the Depression and "most businesses that exited did not default or file for bankruptcy" (Hansen and Ziebarth, 2017). Bankruptcy could indeed be avoided by redirecting the available funds from working capital to debt rollover. The redirection of funds to debt rollover, in times when total external funding also declined, causes however yet another difficulty for financing working capital. Unable to finance production, firms would produce less and, in more severe cases, choose to close some or all of its establishments. For this reason, we can expect that in states with high intensity of bank suspensions the industries with high financial dependence measured using the interest cover indicator ought to experience a higher reduction in the number of establishments than their peers. On the other hand, we have no reason to expect that industries with different levels of external dependence would exhibit the same pattern. While they need the external financing for investment, not investing could force them out of the market only in more distant future.

The patterns illustrated in Figure 10, plotting the distribution of growth in the number of establishments, confirm this prediction. The upper three diagrams show no observable difference in the growth in the number of establishments conditional on external dependence between the high and the low dependence industries. Since in conditions of excess capacity investing to build new establishments should not have been a priority, this comes as no surprise. But the lower three diagrams, which plot the growth in the number of establishments conditional on inverse interest cover, exhibit a very different pattern. The lower-left graph, which includes all states, shows that more financially dependent industries experienced a sharper decline in the number of establishments than their peers. The lower-middle and lower-right graph moreover show that high financial dependence industries, measured using inverse interest cover, did particularly badly in the third of states with the highest percentage of bank deposit suspensions during the Great Depression. While the high-growth tail of highly financially dependent extends far to the right in low-suspension states, it ends abruptly at much smaller values for high-suspension states; the opposite holds for low-dependence industries. In addition, the comparison of the upper right and the lower right graph of Figure 10, in particular the growths rates in the number of establishments shown on the x-axis, reveals that the number of establishments shrunk significantly more for highly dependent industries ranked by inverse interest cover on the lower right plot than for highly dependent ranked by external dependence on the upper right plot.

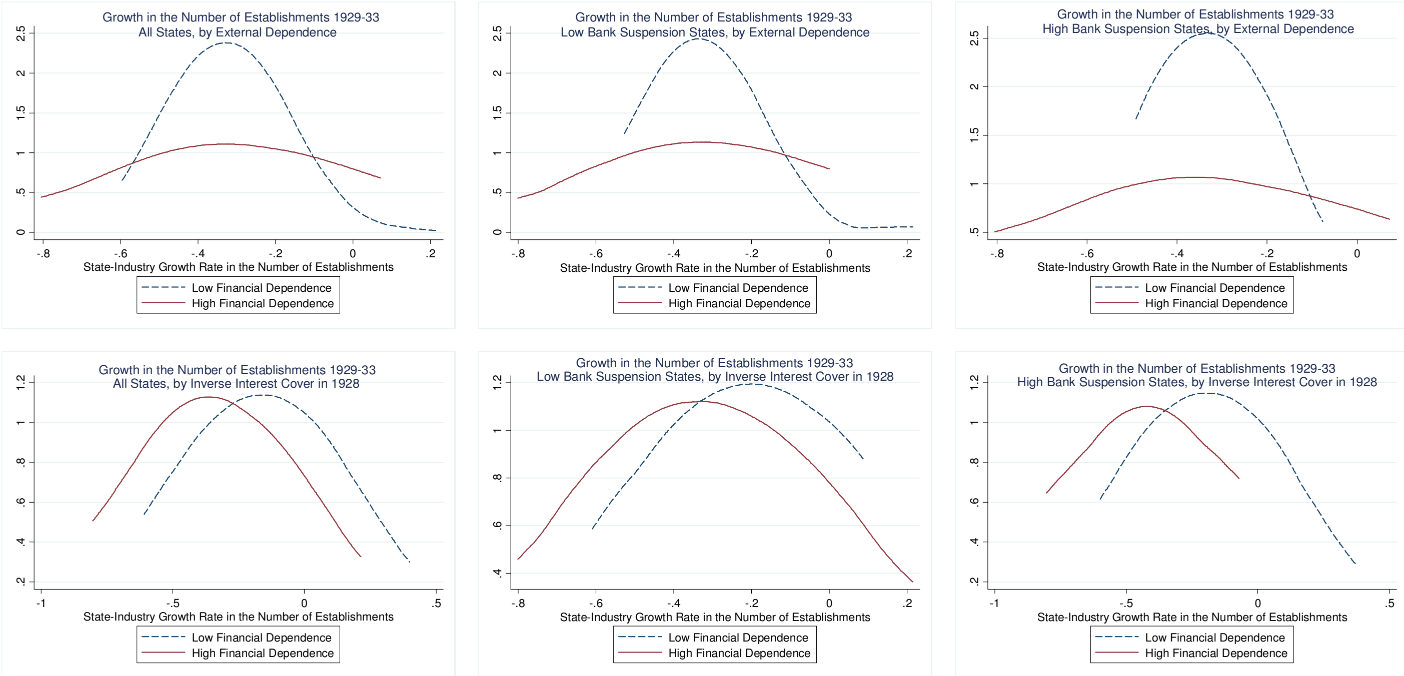

FIGURE 10. DISTRIBUTION OF GROWTH IN THE NUMBER OF ESTABLISHMENTS DURING THE GREAT DEPRESSION – CONDITIONAL ON FINANCIAL DEPENDENCE AND INTENSITY OF BANK SUSPENSIONS

This figure presents distributions of growth in the number of establishments during the Great Depression. The observations points are state-and-industry-specific. The upper three diagrams condition growth in the number of establishments on the external dependence measure, while the lower three diagrams condition growth in the number of establishments on the inverse interest cover measured in 1928. Diagrams on the left plot output growth distributions for all states, diagrams in the middle for the third of states with the smallest fraction of suspended banks, while diagrams on the right for the third of the states with the largest fraction of suspended banks.

Figure 11, plotting the distribution of growth in employment, confirms the findings from the previous figures for growth in other measures of manufacturing performance. High financial dependence industries, as measured using both indicators of financial dependence, saw their employment contract more during the Great Depression than that of the low-dependent ones, and even more so in states with particularly intense bank suspensions. Moreover, the patterns of employment growth also suggest that the ED and IIC measures of financial dependence outline different channels by which bank suspensions affected manufacturing. In particular, a decline in employment could result from a decline in output of the existing establishments, and the closing of some establishments. The measure of external dependence should capture the decline in employment for the first of the two reasons, because without investment the contemporaneous depreciation would reduce productive capacity of the existing establishments and fewer employees could be needed for their operation. But, the inverse interest cover ought to capture both the decline in employment of the existing establishments and the lost employment in closed establishments, since the inability to finance working capital could also force firms to close at least some of their establishments. As postulated, the difference in employment growth shown in Figure 11 is much larger between industries with different levels of inverse interest cover, as shown in the lower-right plot, than between industries with different levels of external dependence, as shown in the upper-right plot.

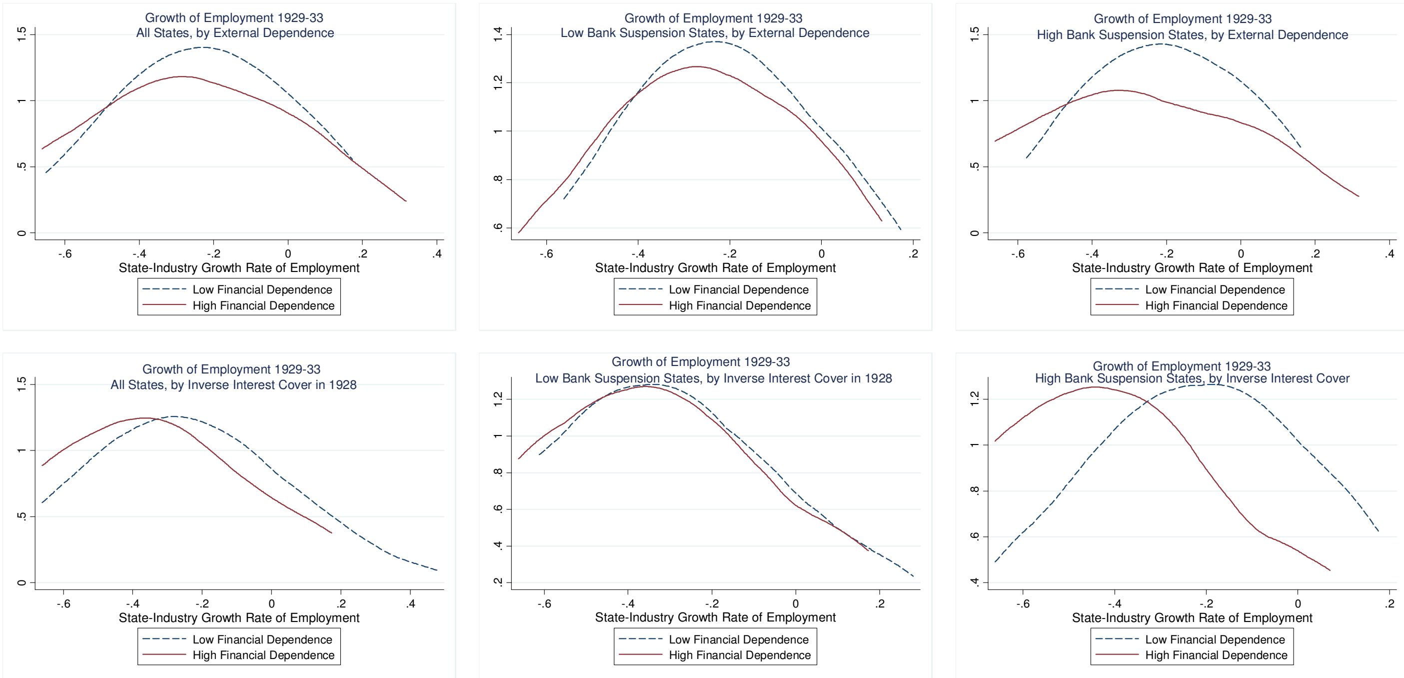

FIGURE 11. DISTRIBUTION OF GROWTH IN EMPLOYMENT DURING THE GREAT DEPRESSION – CONDITIONAL ON FINANCIAL DEPENDENCE AND INTENSITY OF BANK SUSPENSIONS

This figure presents distributions of growth in employment during the Great Depression. The observations points are state-and-industry-specific. The upper three diagrams condition growth in employment on the external dependence measure, while the lower three diagrams condition growth in employment on the inverse interest cover measured in 1928. Diagrams on the left plot output growth distributions for all states, diagrams in the middle for the third of states with the smallest fraction of suspended banks, while diagrams on the right for the third of the states with the largest fraction of suspended banks.

Overall, the evidence presented in Figures 4, 9, 10, and 11 is in line with my expectations that (i) the financially dependent did worse than their peers during the Great Depression and (ii) they underperformed even more in states characterized by high bank suspension intensity. In addition, the evidence from Figures 10 and 11 suggests that the two measures of financial dependence should not be considered substitutes, but complements which describe different ways is which bank suspensions cause output to decline.

6.2. Regression Analysis of the Effect of Bank Suspensions on Several Other Indicators of Manufacturing Performance

In this subsection, I confirm using regression analysis that bank suspensions had important real effects on manufacturing also measured by value added, the number of establishments, and employment. Moreover, that they affected not only the 18 sampled industries, but the whole manufacturing sector.

Table 11 below summarizes the measures of manufacturing performance which are used in this extension. Panel A presents the descriptive statistics for the state-specific growth in output, value added, the number of establishments, and employment of the whole manufacturing sector; Panel B presents the same variables for the 18-industry sample used in other parts of this paper and further differentiated by industry. U.S. Census Bureau's Biennial Census of Manufactures served as the source for all data. While those in Panel A were sourced directly, those in Panel B were taken from the dataset used by Rosenbloom and Sundstrom (1999).

The results of regressions which measure the effect of bank suspensions on several state-period-specific indicators of manufacturing performance of the whole manufacturing sector are shown in Table 12. Panel A gives the OLS results, while Panel B gives the IV estimates. The odd numbered columns correspond to regressions that include state fixed effects, while even numbered columns correspond to those that include both state and time fixed effects.

TABLE 11 — DESCRIPTIVE STATISTICS OF ADDITIONAL MEASURES OF MANUFACTURING PERFORMANCE

	Biannual period	N	Mean	Standard deviation
Panel A. State-period level data				
Output growth$_{st}$ (%)	1921-1937	384	0.12	0.33
	1929-1931	48	-0.35	0.11
	1931-1933	48	-0.08	0.14
Value added growth$_{st}$ (%)	1921-1937	384	0.11	0.31
	1929-1931	48	-0.31	0.11
	1931-1933	48	-0.08	0.13
Number of establishments growth$_{st}$ (%)	1921-1937	384	-0.00	0.16
	1929-1931	48	-0.20	0.09
	1931-1933	48	-0.19	0.04
Employment growth$_{st}$ (%)	1921-1937	384	0.03	0.19
	1929-1931	48	-0.27	0.07
	1931-1933	48	-0.07	0.09
Panel B. State-industry-period level data				
Output growth$_{sit}$ (%)	1921-1937	2194	0.13	0.37
	1929-1931	279	-0.26	0.36
	1931-1933	266	-0.08	0.27
Value added growth$_{sit}$ (%)	1921-1937	2194	0.12	0.34
	1929-1931	279	-0.28	0.24
	1931-1933	266	-0.00	0.32
Number of establishments growth$_{sit}$ (%)	1921-1937	2194	-0.00	0.18
	1929-1931	279	-0.18	0.16
	1931-1933	266	-0.10	0.19
Employment growth$_{sit}$ (%)	1921-1937	2194	0.06	0.23
	1929-1931	279	-0.22	0.17
	1931-1933	266	-0.01	0.19

Source: Censuses of Manufactures, Rosenbloom and Sundstrom (1999), and author's calculations.

The OLS results in Panel A of Table 12 confirm that bank suspensions were associated with a decline of all indicators of performance of the whole manufacturing sector. In specifications with state fixed effects, in odd numbered columns, one percent of suspended deposits was associated with more than 1.5 percent decline in the growth of output and value added, as well as more than 1.0 percent decline in the growth in number of establishments and employment[24]. The results thus show that the reduction in output caused by bank suspensions did not only come from a lower production in the existing establishments, but also from either shutting down of the existing establishments, or lack of formation of new ones, or both. Moreover, that lower

[24] The difference in the size of the effect on output growth and employment is in line with the difference between the effect on output growth estimated in the main part of this paper, and the effect on employment measured for the Great Depression period in Benmelech, Frydman, and Papanikolaou (2017).

TABLE 12 — THE EFFECT OF BANK SUSPENSIONS ON SEVERAL MEASURES OF MANUFACTURING PERFORMANCE OF THE WHOLE MANUFACTURING SECTOR (STATE-PERIOD SAMPLE)

	Panel A. OLS Results							
Dependent variable is:	Output growth$_{st}$ (%)		Value added growth$_{st}$ (%)		No. of establishments growth$_{st}$ (%)		Employment growth$_{st}$ (%)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Deposits suspended$_{st}$	-1.78	0.04	-1.54	0.07	-1.15	-0.18	-1.12	0.02
	(0.37)***	(0.23)	(0.37)***	(0.19)	(0.22)***	(0.10)*	(0.28)***	(0.13)
Lagged dependent variable$_{st}$	-0.26	-0.29	-0.29	-0.24	-0.12	-0.28	-0.25	-0.15
	(0.07)***	(0.10)***	(0.07)***	(0.09)***	(0.04)***	(0.07)***	(0.05)***	(0.07)**
Observations	384	384	384	384	384	384	384	384
R^2	0.20	0.66	0.20	0.70	0.23	0.78	0.22	0.79
	Panel B. IV Results							
Deposits suspended$_{st}$	-8.28	-1.70	-7.01	-0.35	-4.20	-2.47	-4.95	0.19
	(1.33)***	(1.11)	(1.14)***	(0.90)	(0.60)***	(0.86)***	(0.77)***	(0.51)
Lagged dependent variable$_{st}$	-0.35	-0.31	-0.35	-0.24	-0.08	-0.20	-0.36	-0.15
	(0.10)***	(0.10)***	(0.10)***	(0.09)***	(0.07)	(0.12)*	(0.09)***	(0.07)**
Observations	384	384	384	384	384	384	384	384
Fixed effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Constants were calculated but were not reported. The sample consists of measures of total manufacturing activity per state, while the observations vary by state and period. Regressions are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Heteroskedasticity-robust standard errors are in parentheses.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

production caused by bank suspensions did not only result in lower production per existing employee, but also in a reduction of the number of employees. The IV estimates of specifications that use state fixed effects in Panel B follow the same pattern across indicators, but they are around four times larger. The whole manufacturing sector appears to be more sensitive to bank suspensions predicted using the fraction of population rural in 1910 as the instrument than the limited sample of 18 industries used elsewhere in the paper, where the IV estimates were roughly twice as large as the OLS ones. The inclusion of time fixed effects – just as expected if bank suspensions are prevalent across all states in certain periods – weakens both the OLS and IV estimates of the effect of bank suspensions. But, the IV estimate of the effect of suspensions on the growth in the number of establishments remains large and statistically significant even after time fixed effects are included. That the estimate of the effect of bank suspensions on the number of establishments is the most robust to the inclusion of time fixed effects could suggest that the closing of some establishments caused by bank suspensions is in part substituted by an increase in production (reflected in output, value added, and the number of employees) of the surviving establishments in the same state, be it those belonging to the same company or its local competitors.

Tables 13–16 provide additional evidence that bank suspensions affected several indicators of manufacturing performance in the same 18 industry sample, with state-industry-period-specific observations, used in other parts of this paper. The four tables corresponding to different measures of manufacturing activity follow the same structure. Columns (1)–(4) report the results of OLS regressions, while columns (5)–(8) those of IV regressions. Panel A uses the external dependence (ED), while Panel B uses the inverse interest cover (IIC) measure of financial dependence. The groups of both the OLS and the IV regressions corresponding to any of the two measure of financial dependence include a total of four specifications each. OLS specifications in columns (1), (2) differ only by the set of fixed effects, where the first includes only state fixed effects, while the second includes both state and period fixed effects. The specifications in columns (3) and (4) are like those in the previous two columns, but also include controls for cyclicality of industry specific output. The specifications in columns (5), (6), (7), and (8) contain the same variables as those presented in the first four columns, but results from IV analysis.

The results in Table 13 have the growth in state-industry-period specific output in our sample of 18 manufacturing industries as the dependent variable. While the specifications for output growth with only state fixed effects were already reported elsewhere in the paper, those with both state and period fixed effects have results almost identical to those with state, period, and industry fixed effects previously estimated. But, they are reported in Table 13 in a form identical to Tables 14–16 that correspond to the remaining three indicators of manufacturing activity, for ease of comparison. The main characteristics of these results are as follows. Analysis using both measures shows that bank suspensions reduced output growth, especially that of more financially

TABLE 13 — THE EFFECT OF BANK SUSPENSIONS ON THE GROWTH IN OUTPUT CONDITIONAL ON FINANCIAL DEPENDENCE

	Panel A. (1921-1937): External Dependence (ED)							
Dependent variable is Output growth$_{sit}$ (%)	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ED$_i$ X Deposits suspended$_{st}$	-0.98 (0.27)***	-0.95 (0.30)***	-0.62 (0.29)**	-0.65 (0.29)**	-2.63 (0.52)***	-3.04 (0.57)***	-2.39 (0.54)***	-2.43 (0.54)***
Deposits suspended$_{st}$	-3.93 (0.27)***	-0.76 (0.25)***	-0.25 (0.21)	-0.44 (0.22)**	-7.85 (0.83)***	-0.16 (0.90)	-0.37 (0.57)	-0.40 (0.77)
ED$_i$	0.03 (0.01)***	0.04 (0.01)***	0.02 (0.01)***	0.02 (0.01)***	0.07 (0.01)***	0.08 (0.01)***	0.06 (0.01)***	0.06 (0.01)***
Lagged dependent variable$_{sit}$	-0.20 (0.02)***	-0.18 (0.03)***	-0.17 (0.02)***	-0.20 (0.02)***	-0.25 (0.04)***	-0.22 (0.04)***	-0.19 (0.02)***	-0.23 (0.03)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2048	2048	2048	2048	2048	2048	2048	2048
Clusters					334	334	334	334
R^2	0.20	0.43	0.51	0.52				
F-statistics								
Cragg-Donald					502.58	53.71	139.53	54.76
Kleibergen-Paap					27.60	5.47	15.26	5.48
	Panel B. (1927-1937): Inverse Interest Cover (IIC)							
	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IIC$_i$ X Deposits suspended$_{st}$	-0.19 (0.34)	-0.28 (0.33)	-0.72 (0.32)**	-0.73 (0.32)**	-1.37 (0.56)**	-1.24 (0.51)**	-1.93 (0.52)***	-1.97 (0.54)***
Deposits suspended$_{st}$	-3.88 (0.31)***	-0.57 (0.26)**	-0.36 (0.23)	-0.44 (0.23)*	-7.77 (0.72)***	-0.99 (0.84)	0.01 (0.84)	-0.78 (0.74)
IIC$_i$	-0.04 (0.01)***	-0.04 (0.01)***	-0.03 (0.01)**	-0.03 (0.01)**	-0.01 (0.02)	-0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Lagged dependent variable$_{sit}$	-0.05 (0.03)*	-0.12 (0.04)***	-0.16 (0.03)***	-0.17 (0.04)***	-0.11 (0.05)**	-0.13 (0.04)***	-0.18 (0.03)***	-0.19 (0.04)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	1277	1277	1277	1277	1277	1277	1277	1277
Clusters					206	206	206	206
R^2	0.22	0.44	0.52	0.53				
F-statistics								
Cragg-Donald					421.58	45.18	50.54	44.91
Kleibergen-Paap					35.92	7.53	7.26	7.43
Fixed effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Constants were calculated but were not reported. The sample consists of the 18 industries used in the rest of this paper, while the observations vary by state, industry, and period. Regressions in Panel A are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37, while those in Panel B are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Heteroskedasticity-robust standard errors are in parentheses for OLS regressions, while state-time clustered standard errors are in parentheses for IV regressions, the logic for which is explained in the main text and Subsection A.1.2 of the Appendix. The robustness of results to several other ways of clustering standard errors is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

dependent industries. Cyclicality controls weaken the results for the ED measure of dependence, while they strengthen those for the IIC measure of dependence. The IV analysis strengthens the size of all estimates, especially for the IIC measure of dependence. One percent of deposit suspensions results in two (for IIC) to two and a half (for ED) percent greater reduction in output growth for the most dependent industries compared to the median dependent ones (after controlling for cyclicality), and it results in almost eight percent lower output growth of the median dependent industry (without controlling for time fixed effects). While the inclusion of period fixed effects always reduces the estimated effect on the median dependent industry, some estimates do retain statistical significance.

The results in Table 14 have the growth in state-industry-period specific value added in our sample of 18 manufacturing industries as the dependent variable. The effect of bank suspensions on the growth in value added of the median dependent industry, measured by β_2, is similar to that on the growth in output reported in Table 13; the IV estimates suggest that one percent of deposit suspensions results in around seven and a half percent lower growth in value added of the median dependent industry (without controlling for time fixed effects). But, the sensitivity of the effect of suspensions across industries with varying levels of financial dependence, measured by β_1, is one half of the one for output growth when considering the ED measures of dependence; one percent of deposit suspensions results in one to one and a half percent greater reduction in growth in value added for the most dependent industries compared to the median dependent ones, before controlling for cyclicality which makes it insignificant. The difference across industries becomes statistically insignificant when considering the IIC measure of financial dependence. Considering both β_1 and β_2, this results in a smaller reduction in value added than in output of the most dependent industries as a result of bank suspensions. This may suggest that, when output drops, cost cutting is able to lead to a smaller decline in value added than in output, and that measures to cut costs could have been the most expressed among the most severely challenged industries – the financially dependent ones. The results for the IIC measure also appear to suggest that the financially dependent exhibited sharper cost-cutting when exposed to bank suspensions: not only β_1 but also β_3 from Panel B, support this interpretation. While β_1, the

TABLE 14 — THE EFFECT OF BANK SUSPENSIONS ON THE GROWTH IN VALUE ADDED CONDITIONAL ON FINANCIAL DEPENDENCE

	Panel A. (1921-1937): External Dependence (ED)							
Dependent variable is Value added growth$_{sit}$ (%)	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ED_i X Deposits suspended$_{st}$	-0.55	-0.53	-0.20	-0.21	-1.00	-1.49	-0.74	-0.76
	(0.23)**	(0.24)**	(0.23)	(0.23)	(0.47)**	(0.51)**	(0.49)	(0.49)
Deposits suspended$_{st}$	-3.89	-0.91	-0.35	-0.59	-7.52	-0.95	0.07	-0.97
	(0.29)***	(0.28)***	(0.25)	(0.26)**	(0.79)***	(0.94)	(0.78)	(0.88)
ED_i	0.01	0.02	-0.00	-0.00	0.02	0.04	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)**	(0.01)***	(0.01)	(0.01)
Lagged dependent variable$_{sit}$	-0.27	-0.21	-0.25	-0.22	-0.29	-0.21	-0.24	-0.22
	(0.03)***	(0.03)***	(0.02)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2048	2048	2048	2048	2048	2048	2048	2048
Clusters					334	334	334	334
R^2	0.19	0.35	0.40	0.40				
F-statistics								
Cragg-Donald					501.19	53.99	139.16	55.00
Kleibergen-Paap					27.78	5.49	15.46	5.50
	Panel B. (1927-1937): Inverse Interest Cover (IIC)							
	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IIC_i X Deposits suspended$_{st}$	-0.25	-0.22	-0.17	-0.18	0.18	0.33	-0.26	-0.22
	(0.28)	(0.25)	(0.26)	(0.26)	(0.58)	(0.51)	(0.53)	(0.52)
Deposits suspended$_{st}$	-3.53	-0.38	-0.37	-0.24	-7.09	-1.48	-3.51	-1.37
	(0.29)***	(0.26)	(0.24)	(0.23)	(0.66)***	(0.89)*	(1.28)***	(0.83)*
IIC_i	-0.09	-0.08	-0.08	-0.07	-0.09	-0.09	-0.07	-0.07
	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***	(0.02)***
Lagged dependent variable$_{sit}$	-0.21	-0.22	-0.27	-0.23	-0.25	-0.22	-0.26	-0.23
	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.03)***	(0.03)***	(0.04)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	1277	1277	1277	1277	1277	1277	1277	1277
Clusters					206	206	206	206
R^2	0.20	0.35	0.40	0.40				
F-statistics								
Cragg-Donald					426.13	45.27	51.33	45.01
Kleibergen-Paap					33.75	7.54	7.56	7.44
Fixed effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Constants were calculated but were not reported. The sample consists of the 18 industries used in the rest of this paper, while the observations vary by state, industry, and period. Regressions in Panel A are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37, while those in Panel B are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Heteroskedasticity-robust standard errors are in parentheses for OLS regressions, while state-time clustered standard errors are in parentheses for IV regressions, the logic for which is explained in the main text and Subsection A.1.2 of the Appendix. The robustness of results to several other ways of clustering standard errors is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

coefficient for the measure of financial dependence interacted with bank suspensions, is statistically insignificant, β_3, the coefficient for the measure of financial dependence itself, is also negative, but greater in absolute value, in regressions which use value added than in those that use growth in output. This is exactly what one would expect to find if cost cutting allowed to absorb the immediate impact of bank suspensions on output, in states and periods with intense suspensions, but left consequences on productive abilities of the financially dependent industries – such as through a closing of some establishments, as Table 15 will demonstrate. The reduced productive abilities then resulted in lower growth rate in value added of the financially dependent in the subsequent years, and their lower average growth rate in value added during the interwar period, measured by β_3.

In a similar vein, the results in Table 15 have the growth in state-industry-period specific number of establishments in our sample of 18 manufacturing industries as the dependent variable. While the estimates of the effect on the median dependent industry from both panels, measured by β_2, confirm that bank suspensions reduced the growth in the number of establishments, the differential effect on industries with varying levels of financial dependence, measured by β_1, is insignificant for the ED measure and large and significant for the IIC measure. This difference ought to be telling about the mechanism by which bank suspensions affected manufacturing. The insignificant interaction effect using the ED measure, constructed as an indicator of financial dependence for investment purposes, suggests that the fall in output of the more dependent industries, evident in Table 13, did not result from a lack of formation of new establishments for which new investment is needed. Instead, the significant interaction effect using the IIC measure, constructed as an indicator of short-term borrowing needs, suggests that the fall in output of the more dependent industries at least in part resulted from shutting down of the existing establishments due to a shortage of funds to keep them running. The size of β_1 for the IIC indicator (with IV estimates of -1.0 to -1.3) increases after controlling for cyclicality of industry-specific output, the pattern also evident in estimates of β_1 for output growth, which are for a third larger. The size of the estimate of the effect of bank suspensions on the median dependent industries, measured by β_2, varies from -3 to -5 for the IV estimates, and is

TABLE 15 — THE EFFECT OF BANK SUSPENSIONS ON THE GROWTH IN THE NUMBER OF ESTABLISHMENTS CONDITIONAL ON FINANCIAL DEPENDENCE

	Panel A. (1921-1937): External Dependence (ED)							
Dependent variable is Number of establishments growth$_{sit}$ (%)	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ED_i X Deposits suspended$_{st}$	-0.43	-0.41	-0.35	-0.33	-0.26	-0.33	-0.09	-0.16
	(0.32)	(0.30)	(0.32)	(0.31)	(0.56)	(0.57)	(0.58)	(0.58)
Deposits suspended$_{st}$	-1.71	-0.16	-0.45	-0.08	-3.93	-2.99	-4.30	-3.02
	(0.15)***	(0.20)	(0.18)**	(0.19)	(0.45)***	(1.16)***	(0.91)***	(1.18)***
ED_i	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)*	(0.01)	(0.01)	(0.01)*	(0.01)*
Lagged dependent variable$_{sit}$	-0.24	-0.24	-0.23	-0.23	-0.24	-0.23	-0.24	-0.23
	(0.04)***	(0.03)***	(0.04)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***	(0.03)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2048	2048	2048	2048	2048	2048	2048	2048
Clusters					334	334	334	334
R^2	0.14	0.24	0.19	0.25				
F-statistics								
Cragg-Donald					474.77	53.72	146.80	54.75
Kleibergen-Paap					26.24	5.46	17.14	5.48
	Panel B. (1927-1937): Inverse Interest Cover (IIC)							
	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IIC_i X Deposits suspended$_{st}$	-0.59	-0.61	-0.79	-0.84	-0.98	-1.01	-1.25	-1.34
	(0.34)*	(0.33)*	(0.35)**	(0.34)**	(0.56)*	(0.55)*	(0.59)**	(0.59)***
Deposits suspended$_{st}$	-2.27	-0.21	-0.21	-0.17	-4.91	-3.63	-4.02	-3.57
	(0.18)***	(0.23)	(0.22)	(0.22)	(0.49)***	(1.10)***	(1.25)***	(1.08)***
IIC_i	-0.03	-0.02	-0.02	-0.01	-0.01	-0.01	-0.00	-0.00
	(0.02)*	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Lagged dependent variable$_{sit}$	-0.29	-0.29	-0.31	-0.26	-0.30	-0.27	-0.29	-0.24
	(0.05)***	(0.04)***	(0.05)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***	(0.04)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	1277	1277	1277	1277	1277	1277	1277	1277
Clusters					206	206	206	206
R^2	0.18	0.29	0.29	0.31				
F-statistics								
Cragg-Donald					411.81	44.78	49.85	44.69
Kleibergen-Paap					33.37	7.51	7.26	7.42
Fixed effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Constants were calculated but were not reported. The sample consists of the 18 industries used in the rest of this paper, while the observations vary by state, industry, and period. Regressions in Panel A are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37, while those in Panel B are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Heteroskedasticity-robust standard errors are in parentheses for OLS regressions, while state-time clustered standard errors are in parentheses for IV regressions, the logic for which is explained in the main text and Subsection A.1.2. of the Appendix. The robustness of results to several other ways of clustering standard errors is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

therefore almost for a half smaller than the corresponding estimate for growth in output or value added. But, in comparison to the corresponding estimates for any other measures of manufacturing performance, both statistical significance and size of the IV estimate of the coefficient β_2, are very robust to the inclusion of time fixed effects and controls for cyclicality. This suggests that the local bank suspensions had a greater effect on the local number of establishments than on any other indicator of manufacturing performance. In terms of the size of the estimated effect of bank suspensions, on both median and maximum dependent industries, it is smaller on the growth in the number of establishments than on output or value added. This suggests that the decline in output that resulted from bank suspensions was not merely associated to a reduction in the number of establishments, but also a fall in production per establishment.

Finally, the results in Table 16 have the growth in state-industry-period specific employment in our sample of 18 manufacturing industries as the dependent variable. The estimates of the effect on the median dependent industry, measured by β_2 which could be as large as -5.5, are of comparable magnitude to those on the growth in the number of establishments, thus almost twice as small as those for growth in output or value added. But, in comparison to the corresponding estimates for the growth in the number of establishments, they are less robust to controlling for time fixed effects – which suggests that same-state effects of bank suspensions on employment are weaker – and to controlling for cyclicality. The estimates of the differential effect on financially dependent industries, measured by β_1, and for both measures of financial dependence, are also smaller than those for growth in output or value added; the IV estimates range from around -1 to -1.5. The smaller absolute value of both β_1 and β_2 suggests that the decline in output that resulted from bank suspensions was not merely associated to a reduction in employment, but also to a fall in production per employee. While the statistical significance of β_1 for the ED measure is confirmed when controlling for cyclicality of industry-specific output, β_1 for the IIC measure becomes significant when controlling for cyclicality.

TABLE 16 — THE EFFECT OF BANK SUSPENSIONS ON THE GROWTH IN EMPLOYMENT CONDITIONAL ON FINANCIAL DEPENDENCE

Panel A. (1921-1937): External Dependence (ED)

Dependent variable is Employment growth$_{sit}$ (%)	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ED$_i$ X Deposits suspended$_{st}$	-0.53	-0.52	-0.26	-0.27	-1.17	-1.45	-0.88	-0.91
	(0.20)***	(0.23)**	(0.21)	(0.22)	(0.36)***	(0.40)***	(0.37)**	(0.38)**
Deposits suspended$_{st}$	-2.58	-0.48	-0.21	-0.23	-5.31	-0.29	-0.94	-0.38
	(0.18)***	(0.17)***	(0.15)	(0.15)	(0.58)***	(0.81)	(0.56)*	(0.71)
ED$_i$	0.02	0.02	0.01	0.00	0.03	0.04	0.02	0.02
	(0.01)***	(0.01)***	(0.01)	(0.01)	(0.01)***	(0.01)***	(0.01)**	(0.01)**
Lagged dependent variable$_{sit}$	-0.19	-0.17	-0.18	-0.17	-0.23	-0.18	-0.19	-0.17
	(0.03)***	(0.03)***	(0.02)***	(0.03)***	(0.04)***	(0.04)***	(0.02)***	(0.03)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	2048	2048	2048	2048	2048	2048	2048	2048
Clusters					334	334	334	334
R^2	0.17	0.35	0.42	0.44				
F-statistics								
Cragg-Donald					505.74	53.77	143.72	54.82
Kleibergen-Paap					27.65	5.47	16.20	5.49

Panel B. (1927-1937): Inverse Interest Cover (IIC)

	OLS Results: (1) – (4)				IV Results: (5) – (8)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IIC$_i$ X Deposits suspended$_{st}$	0.07	0.04	-0.32	-0.30	-0.47	-0.32	-0.89	-0.85
	(0.26)	(0.25)	(0.24)	(0.25)	(0.40)	(0.36)	(0.36)**	(0.36)**
Deposits suspended$_{st}$	-2.56	-0.23	-0.10	-0.13	-5.36	-0.68	-1.37	-0.47
	(0.22)***	(0.19)	(0.16)	(0.16)	(0.53)***	(0.73)	(0.84)	(0.66)
IIC$_i$	-0.03	-0.03	-0.02	-0.02	-0.02	-0.02	-0.00	-0.00
	(0.01)***	(0.01)***	(0.01)**	(0.01)*	(0.01)	(0.01)	(0.01)	(0.01)
Lagged dependent variable$_{sit}$	-0.07	-0.10	-0.19	-0.13	-0.12	-0.10	-0.18	-0.13
	(0.03)**	(0.04)**	(0.03)***	(0.04)***	(0.05)**	(0.04)**	(0.03)***	(0.04)***
Cyclicality controls	No	No	Yes	Yes	No	No	Yes	Yes
Observations	1277	1277	1277	1277	1277	1277	1277	1277
Clusters					206	206		
R^2	0.20	0.40	0.47	0.49				
F-statistics								
Cragg-Donald					427.32	45.37	48.62	45.24
Kleibergen-Paap					35.33	7.60	7.03	7.53
Fixed effects								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Constants were calculated but were not reported. The sample consists of the 18 industries used in the rest of this paper, while the observations vary by state, industry, and period. Regressions in Panel A are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37, while those in Panel B are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for each column in both panels is reported below Panel B. Heteroskedasticity-robust standard errors are in parentheses for OLS regressions, while state-time clustered standard errors are in parentheses for IV regressions, the logic for which is explained in the main text and Subsection A.1.2 of the Appendix. The robustness of results to several other ways of clustering standard errors is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

Similar to the findings for the growth in output, discussed with Tables 4 and 5, all results in Tables 13–16 are fully robust to clustering at state-period level, those for the effect on the median dependent industry, measured by β_2, are very robust to all types of clustering in various specifications, while those for the differential effect on the financially dependent industries, measured by β_1, and more sensitive to clustering at the industry-period level.[25] But, clustering at the state-industry level actually leads a number of β_1, coefficients for the interaction terms, to obtain statistical significance: both IV with cyclicality controls for growth in value added (ED measure), all OLS and the IV with state and period fixed effects for growth in the number of establishments (ED measure), and all OLS with cyclicality controls for growth in employment (both ED and IIC measures). Given that state-industry clustering allows for correlation across different periods within a cluster, this could be interpreted to suggest that bank suspensions may not have led to an instantaneous decline in growth of a number indicators of manufacturing activity, especially that of establishments, but that over the whole interwar period the cumulative effects of the investment lost because of bank suspensions was evident. Importantly, even when the coefficient β_1 for the growth in the number of establishments becomes statistically significant for the ED measure, its magnitude in IV regressions is never more than a third of the corresponding coefficient for IIC, confirming that the decline in the growth of establishments resulting from bank suspensions primarily occurred through closing of the existing ones rather than a lack of formation of the new ones.

Overall, the results presented in Section 6.2 testify that bank suspensions reduced not only the growth of output but also three other performance indicators of manufacturing: value added,

[25] While the IV estimates in Tables 13–16 already report standard errors clustered at the state-period level, all OLS results are robust to clustering standard errors at the state-period level. The coefficients for the interaction terms, β_1, are more sensitive to clustering by industry-time: the interactions using IIC for growth in output and in the number of establishments, and those in IV regressions using ED for growth in value added. But, clustering by industry-period makes it harder to find a statistically significant relationship because it leaves fewer independent observations, especially within the smaller sample used with the IIC measure (Angrist and Pischke, 2009, p. 321). In terms of the effect on the median dependent industry, measured by the coefficient β_2, nothing changes in specifications which do not control for cyclicality for growth in output and the number of establishments, while it loses significance in the IV specification with both state and time fixed effects for growth in value added (using the IIC measure) when clustering at state-industry level.

number of establishments, and employment. Moreover, the results for all indicators confirm that the more financially dependent industries suffered more than their peers in the presence of bank suspensions: both measures of financial dependence confirm this for growth in output and employment, and at least one measure confirms it for growth in value added and the number of establishments. In addition, this section provided evidence that the effect of bank suspensions is evident not only within our sample of 18 manufacturing industries, but also when considering the whole manufacturing sector.

These findings are also in agreement with those of the recent literature on the topic. The smaller effect of bank suspensions on employment growth than on output growth agrees with the findings of Benmelech, Frydman, and Papanikolaou (2018), who estimate that inability to access credit – measured as firm-specific need to refinance maturing bonds in counties where national banks were suspended – accounted for 10% to 33% of the total decline in employment of large firms over the Great Depression, a smaller fraction than our estimate for manufacturing output. The findings also agree with Ziebarth (2013) who documents a larger decline in employment in the part of Mississippi that experienced more banking failures. Moreover, Lee and Mezzanotti (2017) also find a somewhat larger effect of bank suspensions on value added, than on employment or the number of establishments, in a city-industry sample of 29 US cities and 16 industry groups with unique values of external dependence, covering the 1929-1933 period.

7. Conclusion

In the view of Ben Bernanke, truly understanding the Great Depression is the "holy grail" of macroeconomics. This paper attempts to take one step toward such understanding by evaluating the effect of bank suspensions on output. I use an interwar panel of US states and manufacturing industries and exploit differences among industries with respect to their financial dependence. I demonstrate that bank suspensions reduced output, and even more so the output of financially dependent industries located in the same state where banks were suspended. This suggests that at least a part of the effect of bank suspensions proceeded through a decline in lending to firms. The results are based on two measures of financial dependence: the standard measure of dependence for the purposes of investment, the external dependence of Rajan and Zingales (1998), and a new measure of financial dependence that reflects short-term financing needs, the *inverse interest cover*. I argue that the latter measure is appropriate for recessions, when new investment is not a priority yet borrowing – to finance working capital and debt rollover – remains important. I give evidence that the two measures describe different ways in which bank suspensions caused output to decline: via reduced investment (ED measure) and via inability to finance the existing operations (IIC measure).

To establish that bank suspensions indeed *caused* output to decline, I use a novel state-specific instrument of bank suspensions, the fraction of population rural in 1910. It is chosen to capture the variation across states in the demand shock for food during WWI, but to predate any endogenous reaction to this shock. I provide evidence that my instrument is able to predict bank suspensions throughout the interwar period and through two channels – the higher indebtedness of rural residents and weaker balance sheets in rural areas. While the identification is not able to ascertain which fraction of the time fixed effects should be associated to bank suspensions, and which to other competing factors, the measured gap in the effect of bank suspensions on the output of industries with different levels of financial dependence is substantial. I interpret this to mean that the effect on the median dependent industry must be large, too. The findings therefore suggest that bank suspensions could likely be blamed for around a third of the Depression-era decline in manufacturing output. Several robustness checks confirm that the estimated effect is representative not only of the rural states, where my instrument should have more predictive power, but also of the rest of the United States. Moreover, that it is not driven by any problem related to the exclusion restriction. An extension finally gives evidence that bank suspensions affected not only output, but also value added, number of establishments, and employment, both of the 18-industry sample and of the whole manufacturing sector.

How does this paper change our view of the Great Depression? Although many argued bank suspensions deepened the Depression, whether this was the case and, if so, to what extent, remains a contested topic. This paper gives new evidence that bank suspensions could explain an important part of the downturn. It does so by showing that bank suspensions caused declines in output, as well as value added, number of establishments, and employment. Moreover, it offers a plausible identification based on demonstrating that financially dependent industries were the most affected by local bank suspensions. The pattern of output decline across industries indicates also that the transmission mechanism operated at least in part through reduced lending to firms, both for purposes of investment and for maintaining the existing operations. By identifying bank suspensions as the cause of the reduction in the real economic activity, my findings raise the importance of their prevention as a goal for policy makers.

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APPENDIX

A.1. Appendix to Analysis of Omitted Variable Bias and Reverse Causality

A.1.1. Omitted Variable Bias: Altonji et al. Ratios

In this section of the Appendix and in Subsection 4.5, I address the concern over omitted variable bias, while the potential problem of reverse causality is approached in Subsection 4.6. The coefficient for bank suspensions, β_2, would still be negative if a source of variation which is omitted from the regression were simultaneously causing bank suspensions and output declines. For instance, economic events in the past of a state could act as determinants of both the current growth rates of output and vulnerability of the banking system. Yet it is difficult to imagine how an omitted variable could lead to a negative coefficient for the interaction term between financial dependence and bank suspensions, β_1. For β_1 to be negative, there ought to be a causal link between bank suspensions and output declines, in either direction. The following test nevertheless examines the stability of both β_1 and β_2.

To verify that omitted variables are unlikely to bias our coefficients of interest, I proceed to assess potential bias from unobservables by using the selection on observables (similar to Altonji, Elder, and Taber 2005). The Altonji et al. ratios are presented in Appendix Table A1; they measure to what extent coefficients for the variables of interest change as more controls are added, and are calculated as follows. I first estimate equation (4) with only state fixed effects, considered a *restricted* set of control variables, to obtain β_{iR}. I then estimate the equations (5), (6), (7), that include more fixed effects, and equation (10), which also includes controls for industry-specific cyclicality, considered a *full* set of control variables, to obtain β_{iF}. The Altonji et al. ratio is then given by $\beta_{iF} / (\beta_{iR} - \beta_{iF})$. The higher is the numerator β_{iF}, the more of the effect is left after including a wealth of controls, and the harder would it be for the unobservables to reduce it to zero if included. The lower is the denominator, the smaller is the change in the estimated coefficient as controls are added, and the more powerful would have to be the unobservables relative to the included controls to explain away the whole effect.

APPENDIX TABLE A1 — SELECTION ON UNOBSERVABLES

Fixed effects in restricted set	Fixed effects in full set	Cyclicality controls	Explanatory variables: ED_i X Deposits $suspended_{st}$	Explanatory variables: Deposits $suspended_{st}$
Panel A. (1921-1937): External Dependence (ED)				
State	State, industry	No	(Negative)	(Negative)
State	State, industry, year	No	(Negative)	0.25
State	State-year and industry	No	(Negative)	/
State	State-year and industry	Yes	14.49	/
Panel B. (1927-1937): Inverse Interest Cover (IIC)				
State	State, industry	No	(Negative)	(Negative)
State	State, industry, year	No	(Negative)	0.18
State	State-year and industry	No	(Negative)	/
State	State-year and industry	Yes	(Negative)	/

Notes: This table shows the relative strength of selection on unobservables that is needed to fully explain the observed effect of bank suspensions on output growth by using the methodology of Altonji, Elder and Taber (2005). All regressions also include a measure of financial dependence, when fixed effects allow for it, and the same state-industry growth lagged for one period. Fields marked with (Negative) signify that the corresponding Altonji et al. ratio is negative which suggests OLS estimates are downward biased.

Source: Author's calculations.

Panel A of Appendix Table A1 presents the Altonji et al. ratios for coefficients from OLS regressions that use external dependence as the measure of financial dependence, while Panel B presents the ratios from regressions that use inverse interest cover. Two patterns emerge. Firstly, the interactions between measures of financial dependence and *Deposits* $suspended_{st}$ are robust to the inclusion of fixed effects and cyclicality controls. For the variable ED_i *X Deposits* $suspended_{st}$, three out of four of Altonji et al. ratios are negative, which means that our OLS estimates appear to be biased downwards, representing a lower bound estimate. The only positive Altonji et al. ratio is obtained when cyclicality controls are included, but it is high and equal to 14.49. This suggests that covariance of unobserved factors and ED_i *X Deposits* $suspended_{st}$ would have to be at least 14.49 times as high as the covariance of the included controls for unobserved factors to explain the full OLS effect found. For comparison, Altonji, Elder, and Taber (2005) interpreted a ratio of 3.55 as evidence that unobservables should not be able to explain the effect. For the variable IIC_i *X Deposits* $suspended_{st}$, all four ratios are negative: adding fixed effects and controlling for cyclicality makes the estimated effect of bank suspensions on highly financially dependent industries, as measured by IIC, larger. Secondly, the corresponding ratios for *Deposits suspended*$_{st}$ show that the estimated effect is only sensitive to the inclusion of times fixed effects, in which case the ratios are rather low (0.25 for ED and 0.18 for IIC). This should however not be interpreted as evidence against an effect of bank suspensions: time fixed effects must have absorbed a part of the effect of bank suspensions themselves, especially in the years with intense suspensions, widespread across states. Overall, the negative and high Altonji et al. ratios suggest that omitted variable bias is unlikely, but they cannot resolve the question of how much of the time fixed effects, or the change in growth in national output used to control for cyclicality in equations (8) and (9), should be associated to the effect of bank suspensions themselves.

A.1.2. Instrumental Variables: Tests of Instrument Strength and First Stage Results

In this subsection of the Appendix, I further examine the instrument used in the main body of the paper. I test that the instrument is strong, explain the design of first stage regressions, and present their results. Table A2 shows how the original instrument – state-specific fraction of rural population in 1910, *rural1910*$_s$, in column (1) – as well as the compound instrument – *rural1910*$_s$ multiplied by the period-specific national level of deposit suspensions, *USsuspend*$_t$, in column (2) – predict state-specific deposit suspensions whose 1932-1933 value was previously transformed. My instrument is a strong predictor of bank suspensions. All F-statistics of the instrument are above 10. This holds for regressions over both samples (1921-1937 sample used with external dependence and 1927-1937 sample used with inverse interest cover) and both for specifications with and without state and period fixed effects. The sign of coefficient estimate is always as expected: states with a greater fraction of rural population in 1910 experienced more bank suspensions in the interwar period. Even though these estimates enable us to evaluate how well the instrument predicts bank suspensions, note that they do not represent first stage regressions.

APPENDIX TABLE A2 — DETERMINANTS OF BANK SUSPENSIONS

Dependent variable is *deposits suspended*$_{st}$	(1)	(2)
Panel A. 1921-1937 sample		
Fraction of population rural in 1910$_s$	0.07 (0.01)***	
Fraction of population rural in 1910$_s$ X US deposits suspended$_t$		2.90 (0.83)***
Observations	384	384
R^2	0.07	0.38
F-test of determinants	27.05	12.13
Panel B. 1927-1937 sample		
Fraction of population rural in 1910$_s$	0.07 (0.02)***	
Fraction of population rural in 1910$_s$ X US deposits suspended$_t$		3.31 (0.91)***
Observations	240	240
R^2	0.05	0.47
F-test of the determinant	11.88	13.11
Fixed effects	None	State and Time

Notes: Explanatory variables are the state specific fraction of rural population in 1910 as well as the same variable interacted with the period-specific national percentage of deposits suspended. Panel A gives regressions run over the 1921-1937 period that corresponds to the sample used in regressions with external dependence. Panel B gives regressions run over the 1927-1937 period that corresponds to the sample used in regressions with inverse interest cover. Constants were calculated but were not reported. Standard errors are in parentheses.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

There can actually be one or two first stage regressions, depending on the number of endogenous variables. The latter in turn varies with the fixed effects each specification includes. There are two endogenous variables in the first three specifications, given by equations (4) – (6), as well as two out of three specifications that include cyclicality controls, given by equations (8) and (9). These are deposit suspensions and financial dependence interacted with deposit suspensions. I need two first stage regressions to match them. Figure A1 lists the endogenous regressors and the instruments used for specifications given by equations (4) – (6), (8), and (9). To explain financial dependence interacted with deposit suspensions, I interact the compound instrument with financial dependence. Both instruments however need to be used in the first stage regression for each of the two endogenous variables.

Endogenous variables in equations (4) – (6), (8), and (9)	Instruments
$(suspend_{st})$ $(findep_i)(suspend_{st})$	$(rural1910_s)(USsuspend_t)$ $(findep_i)(rural1910_s)(USsuspend_t)$

APPENDIX FIGURE A1. FIRST STAGE REGRESSIONS FOR EQUATIONS (4) – (6), (8), AND (9)

This figure presents the endogenous variables used in equations (4) – (6), (8) and (9), as well as the instrumental variables which are used to identify their causal effect.

Specifications given by equations (4) – (6), (8), and (9) require several alterations to the usual instrumental variables (IV) analysis. Tests of instrument strength need to be changed and standard errors need to be appropriately clustered. I use a compound statistic to judge the strength of instruments. It takes into account the *F*-statistics of excluded instruments in each of the two first stage regressions. This is Cragg-Donald statistic and, in case of heteroskedasticity robust or clustered standard errors, Kleibergen-Paap statistic, both reported in Table 5 that presents IV regression results. Notice further that some of the variables do not span all dimensions of my dataset. Deposit suspensions and the compound instrument not interacted with a measure of financial dependence vary over states and periods only. Both the first and the second stage regressions are however run over states, periods, and industries. Such a structure of one of the first stage regressions can give unwarranted strength to our instruments. To obtain realistic test statistics, I cluster standard errors at the state-time level.

Unlike the previous specifications, the specifications given by equations (7) and (10) include just one endogenous regressor. This is financial dependence interacted with deposit suspensions. The effect of deposits suspended themselves cannot be estimated; they vary by state and period while specifications given by equations (7) and (10) include state-period fixed effects. A single first stage regression with one instrument (listed in Figure A2) is thus required. Note that, when state-time fixed effects are introduced in equations (7) and (10), the number of state-time clusters becomes insufficient to match the number of variables. The heteroskedasticity robust standard errors estimator is hence invoked in place of the clustered one. Notice also that specifications given by equations (4) – (6), (8), and (9), with two excluded instruments to match two endogenous regressors, and specifications given by equations (7) and (10), with one excluded instrument to match one endogenous regressor, are all exactly identified models; this avoids any potential bias that may arise from overidentification.

Endogenous variables in equations (7) and (10)	Instruments
$(findep_i)(suspend_{st})$	$(findep_i)(rural1910_s)(USsuspend_t)$

APPENDIX FIGURE A2. FIRST STAGE REGRESSIONS FOR EQUATIONS (7) AND (10)

This figure presents the endogenous variable used in equations (7) and (10) as well as the instrumental variable which is used to identify its causal effect.

Appendix Table A3 shows the results of first-stage regressions. Panel A corresponds to IV regressions reported in Panel A of Table 5 which use ED as the measure of financial dependence, while Panel B corresponds to IV regressions reported in Panel B of Table 5 which use IIC as the measure of financial dependence. The results of first-stage regressions, presented in Appendix Table A3, are in line with those on how the compound instruments predict state-specific deposit suspensions, presented in Appendix Table A2. The *F*-statistics of excluded instruments presented in Appendix Table A3 suggest that instruments are sufficiently strong. This is confirmed by Cragg-Donald and Kleibergen-Paap statistics reported in Table 5. Some concern remains only in specifications run over the longer sample (1921-1937, corresponding to the ED measure of dependence) which use time fixed effects (reported in columns 3 and 6 of Table 5) when using the stricter, Kleibergen-Paap, clustered standard errors statistic.

APPENDIX TABLE A3 — FIRST STAGE REGRESSIONS

	Deposits suspended in 1932 and 1933 recoded to their fractions: (*x1932*, *x1933*) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. (1921-1937): External Dependence (ED)							
First Stage Regression 1:	Endogenous variable is ED_i X Deposits $suspended_{st}$						
ED_i X $Rural1910_s$ X US deposits $suspended_t$	3.01 (0.40)***	3.00 (0.41)***	2.97 (0.41)***	3.00 (0.29)***	2.96 (0.40)***	2.94 (0.40)***	2.95 (0.29)***
$Rural1910_s$ X US deposits $suspended_t$	0.10 (0.08)	0.09 (0.08)	0.55 (0.21)***		0.29 (0.18)	0.52 (0.20)***	
ED_i	0.003 (0.002)*						
Lagged output $growth_{sit}$	-0.004 (0.004)	-0.004 (0.004)	-0.008 (0.008)	-0.011 (0.008)	-0.004 (0.004)	-0.009 (0.008)	-0.011 (0.008)
$Durable_i$ X National output $growth_t$					0.003 (0.006)	-0.008 (0.006)	-0.012 (0.010)
$Semidurable_i$ X National output $growth_t$					0.029 (0.022)	0.018 (0.022)	0.023 (0.031)
$Perishable_i$ X National output $growth_t$					0.012 (0.009)		
Observations	2048	2048	2048	2048	2048	2048	2048
Clusters	334	334	334		334	334	
Centered R^2	0.56	0.56	0.56	0.67	0.56	0.56	0.67
F-statistic							
of excluded instruments	28.54	27.78	32.78	105.87	30.60	34.54	102.39
Angrist-Pischke, of excluded instruments	55.40	54.10	51.81	105.87	53.30	51.98	102.39
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. State-year clustered standard errors are in parentheses in columns (1)-(3) and (5)-(6); heteroskedasticity robust standard errors are in parentheses in columns (4) and (7).

Source: Author calculations.

*** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

APPENDIX TABLE A3 — FIRST STAGE REGRESSIONS

	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. (1921-1937): External Dependence (ED)							
First Stage Regression 2:	Endogenous variable is Deposits suspended_{st}						
ED_i X Rural1910_s X US deposits suspended_t	0.09 (0.07)	0.09 (0.07)	0.12 (0.07)*		0.06 (0.07)	0.078 (0.070)	
Rural1910_s X US deposits suspended_t	3.14 (0.32)***	3.14 (0.32)***	2.23 (0.68)***		2.54 (0.48)***	2.24 (0.69)***	
ED_i	-0.001 (0.000)**						
Lagged output growth_{sit}	-0.003 (0.004)	-0.003 (0.004)	0.001 (0.003)		-0.003 (0.004)	0.002 (0.002)	
Durable_i X National output growth_t					-0.043 (0.019)**	-0.024 (0.007)***	
Semidurable_i X National output growth_t					-0.030 (0.018)*	-0.010 (0.016)	
Perishable_i X National output growth_t					0.021 (0.019)		
Observations	2048	2048	2048		2048	2048	
Clusters	334	334	334		334	334	
Centered R^2	0.54	0.54	0.56		0.55	0.56	
F-statistic of excluded instruments	48.59	48.30	5.71		14.93	5.29	
Angrist-Pischke, of excluded instruments	96.80	96.30	10.66		28.65	10.58	
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. State-year clustered standard errors are in parentheses in columns (1)-(3) and (5)-(6); heteroskedasticity robust standard errors are in parentheses in columns (4) and (7).

Source: Author calculations.

*** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

APPENDIX TABLE A3 — FIRST STAGE REGRESSIONS

	Deposits suspended in 1932 and 1933 recoded to their fractions: ($x1932$, $x1933$) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. (1927-1937): Inverse Interest Cover (IIC)							
First Stage Regression 1:	Endogenous variable is IIC_i X Deposits $suspended_{st}$						
IIC_i X $Rural1910_s$	3.21	3.22	3.18	3.27	3.13	3.11	3.21
X US deposits $suspended_t$	(0.41)***	(0.41)***	(0.41)***	(0.25)***	(0.39)***	(0.39)***	(0.26)***
$Rural1910_s$	-0.09	-0.08	0.46		0.45	0.46	
X US deposits $suspended_t$	(0.06)	(0.06)	(0.26)*		(0.27)	(0.26)*	
IIC_i	0.004						
	(0.002)						
Lagged output $growth_{sit}$	-0.001	-0.001	-0.005	-0.004	-0.004	-0.007	-0.005
	(0.003)	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
$Durable_i$ X					0.051	0.034	0.024
National output $growth_t$					(0.022)**	(0.012)***	(0.011)**
$Semidurable_i$ X					0.013	-0.004	-0.004
National output $growth_t$					(0.010)	(0.009)	(0.015)
$Perishable_i$ X					0.019		
National output $growth_t$					(0.014)		
Observations	1277	1277	1277	1277	1277	1277	1277
Clusters	206	206	206		206	206	
Centered R^2	0.58	0.58	0.58	0.67	0.59	0.59	0.67
F-statistic							
of excluded instruments	31.80	32.47	31.07	166.09	32.17	31.67	152.45
Angrist-Pischke, of excluded instruments	61.00	62.34	61.99	166.09	63.56	62.72	152.45
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. State-year clustered standard errors are in parentheses in columns (1)-(3) and (5)-(6); heteroskedasticity robust standard errors are in parentheses in columns (4) and (7).

Source: Author calculations.

*** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

APPENDIX TABLE A3 — FIRST STAGE REGRESSIONS

	Deposits suspended in 1932 and 1933 recoded to their fractions: (*x1932*, *x1933*) = (1.00, 0.15)						
	Baseline estimates: (1)–(4)				Estimates adjusted for cyclicality: (5)–(7)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. (1927-1937): Inverse Interest Cover (IIC)							
First Stage Regression 2:	Endogenous variable is Deposits suspended_{st}						
IIC_i X Rural1910_s X US deposits suspended_t	-0.20 (0.13)	-0.21 (0.13)	-0.15 (0.11)		-0.14 (0.11)	-0.12 (0.11)	
Rural1910_s X US deposits suspended_t	3.00 (0.28)***	3.01 (0.28)***	2.28 (0.61)***		2.17 (0.59)***	2.29 (0.62)***	
IIC_i	0.001 (0.001)						
Lagged output growth_{sit}	-0.004 (0.005)	-0.004 (0.005)	0.001 (0.004)		-0.000 (0.005)	0.003 (0.004)	
Durable_i X National output growth_t					-0.060 (0.028)**	-0.027 (0.008)***	
Semidurable_i X National output growth_t					-0.045 (0.026)*	-0.012 (0.016)	
Perishable_i X National output growth_t					-0.036 (0.029)		
Observations	1277	1277	1277		1277	1277	
Clusters	206	206	206		206	206	
Centered R^2	0.59	0.59	0.60		0.60	0.60	
F-statistic of excluded instruments	58.78	58.21	10.19		9.51	9.51	
Angrist-Pischke, of excluded instruments	116.04	114.65	14.21		13.73	14.13	
Fixed effects							
State	Yes	Yes	Yes	No	Yes	Yes	No
Industry	No	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	Yes	No	No	Yes	No
State X Time	No	No	No	Yes	No	No	Yes

Notes: Constants were calculated but were not reported. State-year clustered standard errors are in parentheses in columns (1)-(3) and (5)-(6); heteroskedasticity robust standard errors are in parentheses in columns (4) and (7).

Source: Author calculations.

*** Significant at the 1 percent level; ** Significant at the 5 percent level; * Significant at the 10 percent level.

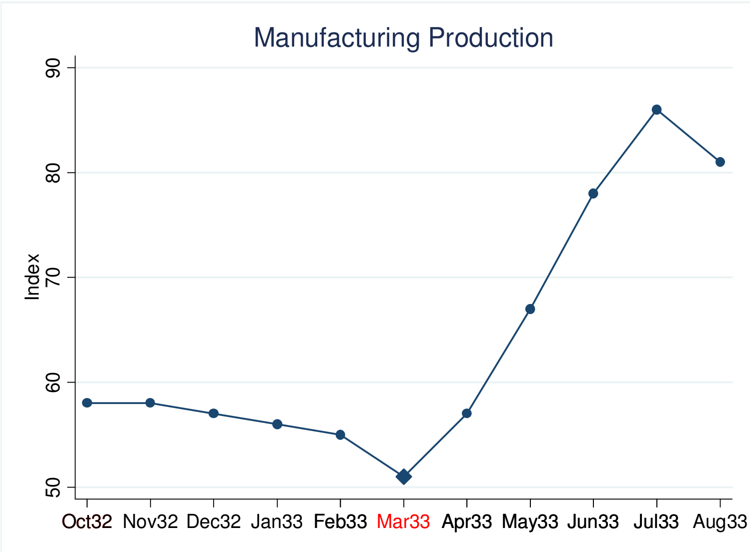
A.2. Further Robustness Checks

In this section of the Appendix, I first focus on the months surrounding the bank suspensions of 1933 – which differed from other interwar suspensions owing to the government intervention that followed them – to confirm that they also had severe real effects. I then show that results are also robust to controlling for the state–industry–specific average of establishment size (a measure of the agency costs of lending used by Temin 2000).

A.2.1. The Bank Suspensions of March 1933

In this subsection I present graphical evidence that bank suspensions of the first quarter of 1933 had important real effects, although in a yearly series their effect would be "hidden" by a recovery that ensued. The plot in Appendix Figure A3 clearly indicates that the 1933 bank suspensions had economically significant immediate effects. The figure shows the monthly index of manufacturing production in the period surrounding the banking crisis of February and March 1933. The same pattern is exhibited by monthly indices of industrial production, factory employment, freight car loadings, and department store sales. The initial effect of the February and March closings appears severe, and the rate of decline is greatest during the crisis: from January to March 1933, manufacturing production declined by 9 percent. The index reaches its lowest value in March. Bank suspensions seem to cause a decline in output, just as in all the other periods examined in this paper. Yet in those other periods there was no government intervention and so bank suspensions created uncertainty about the future of surviving banks. Immediately after the 1933 national banking holiday, however, a strong recovery[26] ensued. So even though it appears the 1933 bank suspensions had immediate negative effects, those effects were soon countered by the government's dispelling of uncertainty about the stability of surviving banks and perhaps also by other factors that in themselves facilitated recovery.

[26] That recovery could be explained in at least two ways. First, government action restored the public's trust in the banking system. The Reconstruction Finance Corporation was empowered to provide the reopened banks with aid that included advancing them capital. What followed was the redepositing of $600 million by the end of March, or 10 percent of the currency in circulation (Wicker 1996, 136). Second, recovery after March 1933 could also have been the consequence of other factors. For instance, Eggertsson (2008) argues that Roosevelt's abandonment of Hoover-era policy dogma – the gold standard, a balanced budget, and small government – made the real interest rate fall by creating expectations of a higher future money supply. Hausman, Rhode, and Wieland (2019) emphasize that post-devaluation increase in dollar prices of tradable farm products was an important driver of recovery through a sharp rise in farmers' consumption.



APPENDIX FIGURE A3. MANUFACTURING PRODUCTION AROUND MARCH 1933

This figure plots the monthly time series of the index of manufacturing and industrial production over the 10-month period centered at March 1933. Source: Manufacturing and Industrial Production, Adjusted for seasonal variation, 1935-1939=100, from "Federal Reserve Bulletin", Washington DC, August 1940, p.765.

A.2.2. Financial Dependence and Industry-Average Establishment Size

In this subsection, I demonstrate that results are robust to controlling for average establishment size across industries, a proxy for agency costs of lending used in Temin (2000). Bernanke (1983) postulates that the rise in the cost of credit intermediation, resulting from bank suspensions of the Great Depression, should have hurt small firms much more than the large ones. But, using a cross section of industries from that period Temin (2000) demonstrates that, if anything, the industries with larger average establishment size did worse, i.e. the opposite from the expected. I attempt to resolve this conundrum by noticing that, even if the large firms experienced a smaller rise in the external financing premium, the industries with many large firms could have still suffered more if they were at the same time more financially dependent. This implies that controlling for firm size could reveal that financial dependence mattered even more then my results thus far show. OLS regression results in Appendix Table A4 thus compare the effects of bank suspensions on industries with varying average establishment size and financing needs.

In the absence of a measure of average firm size, I follow Temin (2000) in using the average establishment size, sourced from the Censi of Manufactures, as a proxy for firm size. State-specific average establishment size per industry is estimated over the 1919-1929 period for each state and expressed in units of 10 million dollars of output. Panel A of Appendix Table A4 uses external dependence (ED) as a measure of financial dependence, while Panel B uses inverse interest cover (IIC). The first three regressions (summarized in columns 1, 2 and 3) include state fixed effects, while the remaining three regressions (summarized in columns 4, 5 and 6) include both state and time fixed effects. Within each set of regressions that include the same fixed effects, the first regressions (summarized in columns 1 and 4) estimate the effect of bank suspensions conditional only on a measure financial dependence. The following regressions (summarized in columns 2 and 5) also condition the effect of suspensions on state-industry-specific average establishment size. The remaining regressions (summarized in columns 3 and 6), in addition to all the aforementioned variables, also control for cyclicality of industry-group's output.

Importantly, the estimated coefficients for interaction terms between financial dependence and deposit suspensions (β_1) and for deposit suspensions themselves (β_2) – across the specifications that involve different controls, different fixed effects, and different measures of financial dependence – are almost the same as in regressions without controls for industry-specific establishment size (reported in Table 4). Just as Temin (2000) finds, in the presence of bank suspensions the industries with higher average establishment size performed, if any different, then worse than their peers. While the effect of size when banks are suspended is not statistically significant in regressions that use ED measure of financial dependence, it is negative and both economically and statistically significant in regressions that use the IIC measure.

APPENDIX TABLE A4 — THE EFFECT OF BANK SUSPENSIONS ON OUTPUT GROWTH CONDITIONAL ON FINANCIAL DEPENDENCE AND INDUSTRY-AVERAGE ESTABLISHMENT SIZE (OLS RESULTS)

Dependent variable is output growth$_{sit}$	Deposits suspended in 1932 and 1933 recoded to their fractions: (*x1932*, *x1933*) = (1.00, 0.15)					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. (1921-1937): External Dependence (ED)					
ED$_i$ X Deposits suspended$_{st}$	-0.98 (0.27)***	-0.97 (0.28)***	-0.62 (0.29)**	-0.95 (0.30)***	-0.95 (0.31)***	-0.65 (0.29)**
SIZE$_{si}$ X Deposits suspended$_{st}$		-0.13 (0.57)	-0.07 (0.35)		-0.11 (0.38)	-0.14 (0.35)
Deposits suspended$_{st}$	-3.93 (0.27)***	-3.91 (0.30)***	-0.24 (0.22)	-0.77 (0.25)***	-0.75 (0.26)***	-0.42 (0.23)*
ED$_i$	0.03 (0.01)***	0.03 (0.01)***	0.02 (0.01)***	0.04 (0.01)***	0.03 (0.01)***	0.02 (0.01)***
SIZE$_{si}$		0.02 (0.03)	0.02 (0.02)		0.02 (0.02)	0.03 (0.02)
Lagged output growth$_{sit}$	-0.20 (0.02)***	-0.20 (0.02)***	-0.18 (0.02)***	-0.18 (0.02)***	-0.18 (0.03)***	-0.20 (0.03)***
Durable$_i$ X National output growth$_t$			1.89 (0.06)***			1.14 (0.07)***
Semidurable$_i$ X National output growth$_t$			0.94 (0.11)***			0.21 (0.11)*
Perishable$_i$ X National output growth$_t$			0.74 (0.05)***			
Observations	2043	2043	2043	2043	2043	2043
R^2	0.20	0.20	0.51	0.43	0.43	0.52
	Panel B. (1927-1937): Inverse Interest Cover (IIC)					
IIC$_i$ X Deposits suspended$_{st}$	-0.19 (0.34)	-0.27 (0.36)	-0.81 (0.34)**	-0.28 (0.33)	-0.37 (0.35)	-0.83 (0.34)**
SIZE$_{si}$ X Deposits suspended$_{st}$		-1.22 (0.73)*	-1.25 (0.56)**		-1.23 (0.58)**	-1.30 (0.56)**
Deposits suspended$_{st}$	-3.88 (0.31)***	-3.72 (0.32)***	-0.21 (0.24)	-0.57 (0.26)**	-0.41 (0.28)	-0.28 (0.25)
IIC$_i$	-0.04 (0.01)***	-0.04 (0.01)***	-0.02 (0.01)*	-0.04 (0.01)***	-0.04 (0.01)***	-0.02 (0.01)***
SIZE$_{si}$		0.03 (0.03)	0.04 (0.03)		0.03 (0.03)	0.04 (0.03)
Lagged output growth$_{sit}$	-0.05 (0.03)*	-0.06 (0.03)*	-0.17 (0.03)***	-0.12 (0.04)***	-0.13 (0.04)***	-0.18 (0.04)***
Durable$_i$ X National output growth$_t$			1.90 (0.07)***			1.11 (0.08)***
Semidurable$_i$ X National output growth$_t$			0.73 (0.11)***			-0.05 (0.12)
Perishable$_i$ X National output growth$_t$			0.79 (0.06)***			
Observations	1272	1272	1272	1272	1272	1272
R^2	0.22	0.23	0.53	0.44	0.44	0.53
Fixed effects						
State	Yes	Yes	Yes	Yes	Yes	Yes
Time	No	No	No	Yes	Yes	Yes

Notes: Constants were calculated but were not reported. Regressions in Panel A use external dependence (ED) as the measure of financial dependence and are run over eight biannual periods: 1921-23, 1923-25, 1925-27, 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Regressions in Panel B use inverse interest cover (IIC) as the measure of financial dependence and are run over five biannual periods: 1927-29, 1929-31, 1931-33, 1933-35 and 1935-37. Deposit suspensions in the biannual period 1931-1933 are first transformed to a sum of 100% of the 1932 suspensions and 15% of the 1933 suspensions, based on the results of the Subsection 4.2. The set of fixed effects used in regressions for that column in both panels is reported below Panel B. $SIZE_{si}$ denotes state-specific average establishment size per industry, estimated over the 1919-1929 period for each state and expressed in units of 10 million dollars of output. *National output growth*$_t$ is the growth in the sum of output of consumer and producer durables, semidurables, perishables and construction materials, in the whole country. Interaction with perishable is omitted in specifications with time fixed effects because, if all three interactions using durability variables would be included, their sum would equal *National output growth*$_t$ which varies only by periods. Heteroskedasticity-robust standard errors are in parentheses. Standard errors clustered by state-time, state-industry, and industry-time were also calculated, and their magnitude is discussed in the main text.

Source: Author's calculations.

*** Significant at the 1 percent level, ** Significant at the 5 percent level, * Significant at the 10 percent level.

No estimate loses statistical significance after clustering standard error at state-period or state-industry level. Like in Table 4, the estimates of the coefficient on the interaction effect (β_1) are more sensitive to industry-period clustering, but using industry-period clusters leaves fewer independent observations, making it harder to find significance. Moreover, this sensitivity does not appear to be related to controlling for establishment size. Our original findings are therefore robust to controlling for the effect of state-industry average establishment size. But, the negative effect on output of larger average establishment size in the presence of bank suspensions, documented by Temin (2000), remains however unexplained.