Bank Distress and Manufacturing: Evidence from the Great Depression

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August 2017

Abstract

Using newly digitized data from the US Censuses of Manufactures, we examine the importance of bank distress in explaining the decline in the economic activity during the Great Depression. Our research design compares the within-MSA behavior of industries that are more or less dependent on external finance across areas that experienced different levels of bank distress between 1929 and 1933. We show that employment, value added, and establishment count contracted relatively more in industries more dependent on external finance than other industries in response to bank distress. Using an instrumental variable design and a set of placebo tests, we confirm the causal interpretation of our results. Lastly, we document that the credit shock appeared to have some persistent effect on industry composition. Our estimates confirm that that disruption in the banking sector had a sizable impact on the manufacturing sector.

*We thank James Feigenbaum, Claudia Goldin, Samuel Hanson, Victoria Ivashina, Josh Lerner, Robert Margo, Kris Mitchener, Diana Moreira, Ramana Nanda, Tom Nicholas, Nathan Nunn, Hugues Pirotte, Daniel Pollmann, David Scharfstein, Adi Sunderam, and seminar participants at Harvard University and the EABH Workshop for New Scholars in Financial History for helpful comments. We thank the Lab for Economic and Policy Applications at Harvard University, the Harvard Business School Historical Collections at Baker Library and Kellogg School of Management for financial support. All errors are our own. Affiliation: * Associate, Cornerstone Research; † Assistant Professor of Finance, Kellogg School of Management, Northwestern University. Email: jlee@cornerstone.com, filippo.mezzanotti@kellogg.northwestern.edu.

1 Introduction

After nearly a decade of expansion in the 1920s, the US manufacturing sector contracted by more than 30 percent from 1929 to 1933 (Figure 1). At the same time, overall US gross domestic product fell by 28 percent and the national unemployment rate rose from 3 to 25 percent.[1] By almost any measure, the real economy suffered. During those same years, more than 7,600 banks—32 percent of the 1929 total—were suspended, and total deposits fell by 16 percent (Figure 1). Never before—or since—had the financial sector seen so many institutions close (Figure 2).

With our newly digitized data, we are interested in understanding whether the manufacturing decline was a result of the disruption in the banking sector that has characterized the Great Depression. In fact, starting with Bernanke (1983), economists have argued that banks' failure can have a dramatic impact on economic activity. Distressed banks increase the cost of credit intermediation by reducing the net worth of lenders and borrowers (Bernanke, 1983) and by destroying valuable lending relationships (Petersen and Rajan, 1994). We use microeconomic data to test these hypotheses in their original context and make two additional extensions on the existing literature. First, our data, which cover about 70 percent of US manufacturing business during the Great Depression, are more representative of the American manufacturing sector than data used by previous work examining US manufacturing during the Great Depression. This enhances the external validity of our results and allows us to revisit some outstanding questions in the literature. Second, because our data extend beyond 1933, we are able to explore the extent to which the effects of credit disruptions persisted after the crisis.

For our research design, we exploit cross-city variation in bank distress and cross-city, cross-industry variation in the manufacturing activity to identify the causal effect of credit market disruption on the real economy. We use the manufacturing sector as our measure of

[1] Gross domestic product data are from the US Bureau of Economic Analysis. Employment data are from Margo (1993).

real economic activity because it comprised 30 percent of the US economy at the time and our data allow us to measure outcomes across space, industry, and time (Lee, 2015). We use suspensions of local banks as our measure of credit market disruption because at the time of the Great Depression financing remained a fairly local practice due to technological and regulatory constraints (Mitchener and Wheelock, 2013).[2]

In our main specification, we test whether from 1929 to 1933 manufacturing industries with high levels of dependence on external finance experienced larger declines in economic activity than industries with low levels of external finance dependence in cities with larger bank suspension rates. Our particular measures of economic activity are the number of establishments, total employment, and value added, all measured at the MSA-industry-year level. In the most restrictive specification, we estimate the regression controlling for a full set of city-by-time and industry-by-time fixed effects. These controls allow us to flexibly account for any contemporaneous shocks at the city or industry level that may have both affected manufacturing activity and been correlated with local bank distress. This helps isolate the impact of local bank distress on manufacturing activity in ways previous research, which often relied on only spatial variation, did not.

Our results show that the banking sector had a significant impact on the real economy. In particular, we find that industries that are more dependent on external finance contracted relatively more than less dependent industries in response to bank distress. On the employment dimension, industries more dependent on external finance contracted 14 percent more than industries with less external-finance dependence in response to a one-standard-deviation increase in the share of local banks suspended. The results are similar in magnitude and statistical significance for value added and the number of establishments. In addition, using these estimates to conduct a simple counterfactual exercise, we conclude that bank distress

[2] From the regulatory side, consider the role of the McFadden Act, which allowed states to prohibit banks from operating branches beyond state lines (Nestor, 1992).

was an important factor in explaining the aggregate drop in manufacturing activity during the Great Depression.

The results are robust to a battery of tests. First, we find no change in the estimates when we augment our specification with controls for non-financial industry characteristics interacted with time and our bank shock variable. The non-financial industry controls are industry capital intensity, skill intensity, contract intensity, and average establishment size. Second, we implement a placebo test in which we find that in the decade leading up to 1929, industries more dependent on external finance did not struggle more in cities where bank distress was stronger during the Great Depression. Instead, the relative decline we find in the 1929-1933 period is specific to that period. This evidence is consistent with the causal interpretation of our result and help exclude the importance of pre-Depression trend or other unobservable industry or city factors in explaining our inference.

Lastly, we also show that our results are robust when using two, alternative instruments for the level at bank distress in the local market. Our first–and preferred–IV uses across-city variation in the level of religious fragmentation as a proxy for social trust (Nanda and Nicholas, 2014). The intuition is that a bank run–all else equal–should be more likely in a city with lower level of trust. Our second IV instead uses the boom-and-bust dynamic of agriculture-land values in the inter-war period (Calomiris and Mason, 2003, Mladjan, 2017). While neither of the two instruments is perfect, the fact that both deliver consistent results provides reassuring evidence for the quality of our research design.

Beyond these robustness checks, we also show that some of the effects of the credit market distress remained visible after the distress had subsided in 1937. In particular, the number of manufacturing establishments had not reached its pre-Depression levels by 1937. Employment and value added had recovered. Interestingly, this microeconomic finding is consistent with the aggregate data: while employment and value added in the manufacturing sector had reached their 1929 levels by the end of the 1930s, the number of establishments recovered only after WWII. This result suggests that credit shocks can have persistent effects on industry structure. This evidence can help better characterizing the welfare implication of credit shocks.

The main contribution of this paper is to use new, microeconomic data and a rigorous research design to confirm that bank distress was an important cause of the sharp contraction in manufacturing activity during the Great Depression (Bernanke, 1983). Because of the financial disruption, manufacturing contracted in terms of value added, employment, and the number of establishments. These effects were both economically and statistically meaningful. Furthermore, while the effects of the credit shocks appear to have been short-lived across the employment and value added metrics, the decline in the number of establishments persisted into the late 1930s.

Relative to the previous literature on this topic, we show three improvements. First, because our data cover approximately 70% of manufacturing activity, our results are more representative of the effects of bank distress during the Great Depression on the overall American economy than the results of prior work. Prior work used more selected samples, such as the manufacturing industry in Mississippi, where most of the firms are small and operating in low-tech industries (Ziebarth, 2013), or on large public companies (Benmelech et al., 2017). This difference in coverage is not just relevant to evaluate the external validity of these estimates, but in this case, it appears to have an impact on the interpretation of the findings. For instance, Ziebarth (2013) finds no effect on plant-level employment, while Benmelech et al. (2017) finds a strong negative decline. If large firms are particularly fragile and there is some substitution in employment between large and small firms, as these results may suggest, one could argue that the effects for large firms are over-stating the overall impact of credit disruption on aggregate employment. Our analyses–which use industry-level data and therefore are less affected by general equilibrium concerns–confirm the importance of the credit channel for employment during the Great Depression.

Second, our exploration of the persistence of the results provides novel evidence on the medium-term effects of credit shocks. This not only contributes to the understanding of the

Great Depression, but also is important in the broader context of the literature that looks at the real cost of financial disruption. This area has been active recently, with several studies of the recent crisis in the US and Europe.[3] Our results suggest that a complete welfare analysis requires a more systematic exploration of the persistence of credit-shock effects.

Lastly, this paper contributes to the body of research that in recent years has used microeconomic data to explore the causes and consequences of the Great Depression. In this area, Cole and Ohanian (2007), Eichengreen and Mitchener (2004), and Richardson and Troost (2009) have empirically examined the causes of the depression. On the consequences side, in addition to the papers already cited (Ziebarth, 2013, Benmelech et al., 2017), other works have documented a large effect of bank failure on innovation (Nanda and Nicholas, 2014), income at the city level (Calomiris and Mason, 2003), and industry output (Mladjan, 2017). Other papers have studied the effect of the Great Depression on social and other economic outcomes such as crime (Fishback et al., 2010), the labor market (Moulton, 2016), or intergenerational mobility (Feigenbaum, 2015).

The remainder of the paper proceeds as follows. Section 2 describes the data. Section 3 explains the empirical strategy. Section 4 discusses results and performs robustness checks. Section 5 addresses persistence and Section 6 concludes.

2 Data

To carry out our analysis, we use three main data sources. The first is the US Census of Manufactures.[4] In particular, we use the city-industry level records from the 1929 and

[3] Many recent papers have looked at the effects of the 2008–2009 financial crisis and 2010–2011 Euro crisis, finding that a shock to the credit market can have sizable effects on the real economy. These results were confirmed both in US (Benmelech et al. 2016; Chodorow-Reich 2014; Duygan-Bump et al. 2015; Greenstone et al. 2014) and Europe (Bentolila et al. 2013; Bottero et al. (2015); Carpinelli and Crosignani (2015); Cingano et al. 2016). Outside the recent crisis, similar results were found by Ashcraft (2003), Gilje (2013), and Peek and Rosengren (2000).

[4] Lee (2015) is the first paper to use the harmonized city–industry level data from the Census. The data were digitized by the author with the support of the HBS Historical Library. The same data around the Civil War period were also used by Feigenbaum et al. (2017).

1933 Censuses.[5] We limit our sample to the 29 metropolitan areas (MSA) with industry level data in both 1929 and 1933 (Table 1). These MSAs are well distributed across the major manufacturing centers of the country at the time, as shown in Figure 4—with the Eastern seaboard and Upper Midwest well represented. For these 29 cities, the Census reports information on economic activity at the industry level, where the industry definition is equivalent to a 4-digit SIC code.[6] In particular, the data report for each city-industry-year triplet, the number of establishments, value added, and employment. The other industry-level characteristics we use are discussed as we introduce them into the analyses.

As mentioned earlier, our data are representative of the American manufacturing sector at the time. Combined, the 29 MSAs in our sample comprise approximately 70 percent of 1929 US manufacturing, as measured by value added. Furthermore, because our sample consists of urban areas, it contains a wide variety of different industries present in the sector at the time, including the more productive, more technologically advanced industries. This improves the external validity of our findings relative to the literature.[7]

Onto these manufacturing data we merge bank data from the FDIC, which have been extensively used for research of the Great Depression. The bank data report for each year from 1920-1936 the number of banks and number of bank suspensions in each US county.[8] We aggregate these county level data to the MSA level using the county-to-city mappings

[5]The city-industry records from the 1931 Census of Manufactures cannot be located by the US Census Bureau, the National Archives, or the Library of Congress.

[6]The 283 industries are based on 1929 industry classifications. In order to have a consistent set of industry classifications across years, we map all industry classifications from 1933 into 1929 classifications. We do this using the year-to-year industry mappings and national industry employment data found in each Census. For our regression analysis, we also use the higher level industry groupings provided in the 1929 Census. In particular, we place each industry into one of 34 groups. These groups are roughly pre-cursors to the 3-digit SIC codes, which is the way we refer to this classification going forward.

[7]For instance, Ziebarth (2013) focuses on manufacturing in Mississippi – a state that was mostly rural and agricultural at the time – while Benmelech et al. (2017) focuses on large, public companies around the country. While these works already provide very interesting insights on the Great Depression, our sample has the potential to give a much more representative view of the status of the American manufacturing during this period.

[8]A bank suspension is defined by the FDIC as the closure of a bank to the public either temporarily or permanently by supervisory authorities or by the banks' board of directors on account of financial difficulties (see Reserve, 1937).

provided in the Censuses of Manufactures.[9] Consistent with Nanda and Nicholas (2014), we create our primary measure of bank distress as the fraction of all 1929 banks in the city that were suspended between 1930 and 1933:

$$Shock_c = \frac{\sum_{t=1930}^{1933} SuspendedBank_{ct}}{Banks_{c1929}}. \tag{1}$$

Table 1 reports the rate of suspensions across our sample together with other MSA-level information, showing a large dispersion in suspension rates across the country. The same dispersion is well documented by Figure 3, which graphically reports suspensions across our sample of MSAs between 1921 and 1936.

Lastly, our identification strategy examines whether the bank suspensions affected industries within the same city differently based on pre-Depression dependence on external finance. Because the data available for this period do not allow us to measure directly the amount of industry investment funded by external sources (Rajan and Zingales, 1998), we use the same procedure and data used in the work of Nanda and Nicholas (2014). In particular, we use their hand-collected, firm-level data on the fraction of assets that are financed by bank loans based on the 1920s Moody's *Manual of Industrial,* which they then aggregated to the industry level for 16 different industries. We then manually map these 16 industries into our 3-digit SIC classifications to create our final score. The final measure used in the paper is an indicator variable equal to 1 if an industry's external-finance dependence measure is above the median measure of 0.06. We use an indicator rather than a continuous measure to avoid specifying a parametric relationship between external-finance dependence and our manufacturing outcomes.

[9]Each city in the Census of Manufactures is a set of contiguous counties. We simply sum the county level banking data across each city's component counties to construct the city's banking data. Results are robust to using city definitions from different Census years. We use city definitions from a later year—1977—in our main analysis because we want to capture bank lending opportunities that may have existed outside the 1929 boundaries. Using only the 1929 central cities counties does not significantly change results.

3 Empirical Strategy

In Figure 5, we document a strong, negative correlation between the percentage change in manufacturing value added between 1929-1933 and the contemporaneous rate of bank suspension. In particular, using the raw MSA-level data, we find a 30% negative correlation between these two quantities (Table 1). While this correlation is consistent with the hypothesis that the banking crisis during the Depression led to a contraction in manufacturing activity, its sign or magnitude may also result from omitted variable biases or reverse causality. In this section, we discuss the empirical strategy we employ to provide causal evidence of the effect of bank distress on the real economy.

Our main specification is a triple difference model, which compares changes in manufacturing activity between 1929 and 1933 in industries with high dependence on external finance to those with low dependence across cities characterized by different rates of bank suspension. There are two reasons why we compare industries across pre-Depression level of dependence on external finance. Theoretically, industries that are more dependent on external finance should be relatively more affected by any change in local financial conditions (Rajan and Zingales, 1998). At the extreme, an industry that is completely independent of external-finance should be unaffected with respect to credit supply shocks. Empirically, comparing across industries of high and low external-finance dependence introduces an extra layer of variation in the data that allows us to tighten our identification by exploiting within-city variation. In practice, we implement this feature by introducing a full set of MSA-by-year fixed effects.

This feature relaxes the identification assumptions relative to other works that only use spatial variation in their identification of the effects of bank distress (e.g. Calomiris and Mason, 2003). In fact, this estimator does not require manufacturing industries–in absence of the banking shock–to have evolved similarly across cities. Instead, our setup only requires this counterfactual condition to hold for the difference in growth between high and low-dependence industries. Importantly, the ability to exploit only within-MSA variation also reduces the concerns of reverse causality. More discussion on this issue is then provided below, where we introduce our instrumental-variable model.

It is also important to highlight that the specification is saturated by a very detailed set of fixed-effects to non-parametrically control for omitted variables at MSA and industry levels. In fact, on top of the city-by-year fixed effects that were previously discussed, we include industry group (3-digit SIC) by year fixed effects in all our specifications. This set of controls should absorb all unobservable industry variation that may affect manufacturing. All of this considered, our main OLS specification is:

$$\ln Y_{cit} = \beta HFin_{j(i)} Post_t Shock_c + \gamma HFin_{j(i)} Shock_c + \alpha_{ct} + \alpha_{j(i)t} + \varepsilon_{cit} \qquad (2)$$

where Y_{cit} denotes manufacturing outcome $Y = \{$establishments, value added, employment$\}$ in MSA c and industry i at time t; $HFin_{j(i)}$ is an indicator equal to 1 if the 3-digit SIC industry group j, which is a function of the observation's industry i, is of high external finance dependence (Nanda and Nicholas, 2014); $Post_t$ is an indicator equal to 1 if the year is 1933 (the other year is 1929, for which $Post_t = 0$); $Shock_c$ is the fraction of 1929 banks in city c suspended between 1930 and 1933, as presented in equation (1); α_{ct} are city-year fixed effects; $\alpha_{j(i)t}$ are industry group j-year fixed effects; and ε_{cit} is the error term. The sample is all city-industry-year triplets in the 29 cities.[10] Lastly, we cluster standard errors at the city level, which is the level of the financial shock treatment. If financial distress during the Depression affected manufacturing, we expect a relatively larger decline by industries more dependent on external finance in cities more affected by bank suspensions. In the context of the model, this is equivalent to test $\beta < 0$.

In the result section, we also present and discuss three extra robustness tests. First, we augment our OLS model with an extra set of industry-level controls interacted with both

[10] We consider all the industries that were surveyed in both Censuses. Furthermore, we do not introduce any other data filter. In particular, we include all city-industry-year triplets in these cities, even if the city-industry is nonzero in the data in only year.

the time dummy and the bank-suspension measure. With this horse-race test, we want to exclude that our external dependence measure is capturing other dimensions of firms' business that are unrelated to finance but that may still affect manufacturing during a downturn. For instance, industries that are more dependent on external finance may also be more contract intensive (Nunn, 2007), where input purchases occur in a more relationship-based fashion. Our claim that the decline in manufacturing was caused by bank distress may be confounded by this correlation, because more contract-intense firms may still reduce their activity in presence of weak demand even if the credit market is not an issue for companies.[11]

In particular, we consider four, nonfinancial dimensions. First, as in the example, we control for the level of contract intensity using the data from Nunn (2007).[12] Second, we look at capital intensity, which is measured by the industry-level ratio between capital stock and value added.[13] Third, we consider skill intensity at the industry level, proxied by the share of non-production workers in the industry. Lastly, we control for average establishment size in the industry. To make these measures comparable to external-finance dependence, we transform them into an indicator variable, which is equal to one if the industry is above the median of that dimension. The Appendix provides a very detailed discussion on the construction of these variables.

Second, we implement a battery of placebo tests using Census data from the pre-Great Depression period. One concern with our results is that industries that are more dependent on external finance may have always been performing poorly in MSAs characterized by bank distress during the Great Depression. For instance, the textile sector in Chicago–an industry with a large need for external financing in a city with one of the highest distress levels registered during 1929-1933–may have been struggling relative to the rest of the economy

[11]One example is that weak demand increases counter-party risks in transactions and therefore it imposes a larger cost for companies where contracts are more important.

[12]Nunn (2007)measures the share of an industry's inputs that are neither sold on an organized exchange nor listed in a reference price trade journal. The higher the fraction, the more contract-intensive the industry because input purchases are more likely to occur in a relationship-specific way.

[13]We obtain capital stock at the 4-digit SIC industry level from the 1919 Census of Manufactures. The 1919 Census of Manufactures was the last pre-Depression Census of Manufactures to report total capital stock at the industry level.

for the whole previous decade. If this was the case, then our results may have been simply picking up secular trends across industries and cities rather than the real effect of financial disruption during the Great Depression.

We implement this test by replicating our main specification using a different set of manufacturing outcomes. Instead of using the 1933 as post-crisis period and the 1929 as pre-crisis period, we estimate the equation (2) fixing 1929 as a fictional post-crisis period and using 1927, 1925 and 1923 as the fictional pre-crisis period. Consistent with the quality of our empirical setting, we expect to find that areas characterized by greater bank distress during the Great Depression did not experience an under-performance in those industries that were more dependent on external finance.

Third, we implement an instrumental variable model to further address issues related to reverse causality. In principle, the presence of both MSA-by-year and industry group-by-year fixed effects already reduces the concern of reverse causality. These controls at least exclude the two main channels through which reverse causality could influence our results. First, the presence of time-varying MSA fixed effects excludes the possibility that our results may simply be capturing the fact that bank distress at the city level was caused by an aggregate decline in local manufacturing. Second, the presence of time-varying industry-group fixed effects also excludes the possibility that the estimates are just reflecting an industry specific shock, that brought down both the manufacturing and the banking sector. Based on this observation, our results could not be explained by a larger shock to demand in industries more dependent on external finance.

However, we cannot rule out that more nuanced versions of reverse causality may still play a role in explaining our results.[14] The only way to fully address this issue in this context is to

[14]One example is the following. If the demand shocks to industries that are more dependent on external finance are also city specific–that is, they vary at the city-industry level, not just the industry level–then our main parameter of interest may be biased. This could happen if two industries highly dependent on external finance-–say automobiles and textiles—experienced differential demand shocks and the importance of the two industries varied greatly across two cities—say with Detroit, MI having a relatively larger share of its manufacturing sector in the automobile industry than Providence, RI, which had a relatively larger share of its manufacturing sector in the textiles industry. Bank distress in the city with the more negatively shocked high-external-dependence industry could then result from the negative industry shock. These are

implement an IV model. Therefore, we instrument the level of bank distress at the MSA level with two arguably suitable instruments: the level of religious fragmentation at the MSA level and the growth in land prices during the interwar boom period. Importantly, we use the two instruments separately and we find consistent results across the two models. The validity of these instruments relies on two assumptions. First, the instrument has to predict the increase in bank distress during the Great Depression. Second, the instrument–conditional on the other controls–has to affect manufacturing only through the banking sector. While the first assumption can be examined empirically, the second is fundamentally untestable. In the next section, before presenting the results from these analyses, we will explain in detail the nature of these instruments and provide direct and indirect evidence in favor of their validity.

4 Results

4.1 OLS model

Table 2 shows our primary results, which estimate equation (2) for our three manufacturing outcomes: establishments, employment, and value added. All columns contain industry group-time fixed effects. Even-numbered columns contain city fixed effects and odd-numbered columns contain city-time fixed effects. We normalize the bank suspension rate to have mean zero and unit standard deviation so that the estimated β coefficients can be interpreted as the difference in the conditional growth rate between industries of high and low external-finance dependence following a one- standard-deviation increase in the bank suspension rate.

Consistent with the credit-supply hypothesis, we find that the estimated β coefficients are negative, highly statistically significant, and economically meaningful. The number of establishment in industries with greater dependence on external finance contracted by 11 log

strong assumptions, though, since they require high correlation between demand and supply shock across city and industry pairs.

points more than industries with less external finance dependence following a one-standard deviation in the bank suspension rate on the establishment measure. The decline in the employment measure was 14 log points, and for value added, 15 log points. The results are similar when we have simple MSA fixed effects (odd columns) and when we add MSA by year fixed effects (even columns).

To better appreciate the magnitude of the results, it may be useful to compare the relative behavior of the two industry groups for a representative MSA. For instance, if we consider a city in the top decile in terms of bank suspension,[15] our estimate translates into a 7.5 percentage point larger decline in the value added for high dependent industries. The same holds for employment - with highly dependent industries experiencing a 7 percentage point larger decline than low-dependence industries - and establishment, where the difference between the two groups waswith an effect of about 5.5 percentage point.

By comparing the different interaction terms, we can also explore the conditional drop in manufacturing activity for each group in the representative city.[16] We find that both economic activity declined for every industry group during the Great Depression, but the drop for industries less dependent on external finance was relatively small. For instance, our estimates imply that - in the top decile of the shock - value added dropped by only 3 percentage points in the low-dependence industries, while the decline was geater than 10 percentage points for the more dependent industries. The same intuition on both the absolute and relative behavior of the two groups is confirmed graphically in Figure 6, where we plot the bin scatter of the regression coefficient on bank suspension separately for the two groups.

Thus, manufacturing industries relatively more dependent on external finance declined markedly more than industries relatively less dependent on external finance following bank shocks during the US Great Depression. As a last step in this analysis, we use our micro-

[15]In our sample, this would be the city of Chicago, which had about a 50% suspension rate (Table 1).

[16]In order to do this, we need to consider the specification without MSA by year fixed effects, for which the interaction between the crisis dummy and the shock is estimated.

estimates to examine the extent to which bank suspension can explain the aggregate drop in manufacturing experienced in the US during the Great Depression (Figure 1). In particular, we combine the coefficients from Table 2 with the pre-Depression share of high-external-finance industries and data on the overall drop in aggregate manufacturing employment. The idea - which we discuss in detail in the Appendix 4.4.1 - is that we can use our estimated coefficients to measure the drop in manufacturing due to the credit shock, which can then be compared with the overall drop that we measure in the raw data. We conclude that at least 5 percent of the overall decline in manufacturing employment during the US Great Depression can be attributed to bank distress. As we discuss in the Appendix, this point estimate is likely to be a lower bound of the effect. In fact, to compute this coefficient, we assume in the model that firms with low dependence on external finance were completely unaffected by the credit contraction and we do not account for the fact that credit disruption is also likely to affect aggregate industry activity.

4.2 Robustness: finance vs. other industry characteristics

These results confirm that bank distress had a causal, negative impact on the manufacturing sector during the Great Depression. The estimated effect was both economically large and statistically significant. One key component of our identification strategy is the ability to sort industries in relationship with to their dependence on external finance (Rajan and Zingales, 1998; Nanda and Nicholas, 2014). While our approach follows the literature on this area, one concern is that our measure of financial dependence may be spuriously capturing other dimension of heterogeneity across industries.

To address this issue, we run a battery of horse-race regressions between our measure of dependence on external finance and other industry characteristics that may have affected manufacturing activity during the Great Depression. In particular, we explore four dimensions: contract and capital intensity, dependence of the industry on high-skill workers and average size of the establishments. Each of these measures – which are transformed in a categorical variable splitting at the median across industries – is then added to the equation (2) interacted with the 1933 dummy and the rate of bank suspension.

The results of these tests are reported in the three panels of Table 3. In each panel, we focus on one of our three main outcomes and we report the horse-race results for each of the alternative measures both one at the time and all together.[17] Our findings are striking: across all the specifications, the coefficient on the triple interaction with external dependence remains significant. Furthermore, its magnitude of the effects remains generally unaffected, suggesting that our measure of external dependence is indeed orthogonal to the other measures.

From these analyses, we also find that–similarly to external dependence–capital intensity also seems to predict a higher decline in manufacturing activity in MSA characterized by greater bank distress. For instance, a one-standard-deviation increase in bank suspension led to a 9% higher decline in employment for highly capital-intensive industries relative to the less capital-intensive ones. Results for the other outcomes are also similar. Overall, this result is consistent with a credit-supply explanation, which predicts that–all else equal–more capital-intensive industries which require greater levels of capital investment will decline by more than less capital intensive industries when the banking sector experiences distress.

All in all, this battery of tests confirms the quality of our previous analyses. In fact, our measure of dependence to external finance does not appear to be capturing any other difference across industries. Furthermore, the results on capital intensity seem to confirm the hypothesis that bank distress led to the disruption in the manufacturing sector during the Great Depression.

[17] The table only reports the triple interaction across all the covariates for the sake of clarity. However, the regression is also run with all the lower level interactions, as in the other cases.

4.3 Robustness: placebo tests

In our next robustness check, we estimate a battery of placebo tests using data from the pre-Depression period. Our main specification compares high vs. low externally dependent industries across MSAs characterized by different levels of exposure to bank distress during the Great Depression. In this setting, the main identification assumption is that – in the absence of the shock – the industry dependence would not have predicted differential growth across cities characterized by different levels of bank distress. While this assumption is intrinsically untestable, we can provide evidence that supports it by exploring the growth in manufacturing in the pre-Depression period.

Specifically, these tests can exclude that our results are just reflecting the presence of differential trends across industries and cities before 1929. We implement these tests by replicating our main specification (equation 2) using data from years in which bank distress should not have predicted any differential change in economic activity. In particular, rather than using measures of manufacturing activity from the Censuses of 1929 and 1933 as outcomes, we rerun our main specification across three time periods: 1923-1929, 1925-1929, and 1927-1929. Importantly, in these analyses industries and cities are still sorted in terms of dependence on external finance and bank distress the same way as in the main specification. In other words, bank distress is always measured in 1929-1933, as before.[18]

The results from these tests are presented in Table (4). In columns (1), (4) and (7), we replicate across the three outcomes our main specification using data from 1922 and 1929. Across all outcomes, we can reject that before the Great Depression industries more dependent on external finance were performing worse than low dependence industries in high-distress cities. In particular, the relative growth across the two groups of industries does not seem to be correlated in any way with the level of bank disruption during the Great Depression. In every specification, the main coefficient of interest is both non-significant

[18]Note that, as reported in Figure (3), bank distress is very low during all the 1920s and there is essentially no heterogeneity in suspensions levels across cities.

and small in magnitude relative to our main results. The same results can be found when using the data from the 1925 and 1929 Censuses (columns 2, 5, and 8) and 1927 and 1929 (columns 3, 6, and 9). In every case, we find no differential growth across MSA suspension rates and dependence on external finance.

Overall, this set of placebo tests excludes that differential trends in manufacturing activity can explain the relative drop in employment, valueadded and establishments that we registered during the Great Depression. Across all the specifications and periods considered, we cannot find any evidence that would be consistent with this hypothesis.

4.4 Robustness: instrumental variables estimators

4.4.1 Religious Fragmentation Instrument

We next turn to estimating our model with an instrumental variable for bank suspension rates for 1929-1933. Our first and favorite instrumental variable is a measure of city trust: religious fragmentation. Like Nanda and Nicholas (2014), we construct religious fragmentation using the 1906 US Census of Religious Bodies. This survey collected all the churces capacity of 91 different religious denominations in each county as of 1906. We aggregate the county data to the city level using the same method as before—summing across the counties within each city. We next construct a Herfindahl Index of religious concentration. Our instrument measure of religious fragmentation is then one minus the Herfindahl index:

$$RelFrag_c = 1 - \sum_r h_{cr}^2 \tag{3}$$

where h_{cr} is the share of denomination r in city c.

The use of group fragmentation as a measure of trust is common in academic research. Alesina and LaFerrara (2002) show that group identity is one of the four strongest determinants of trust. In financial decision-making, Guiso et al. (2008) find trust to be an important factor in individuals' decisions to invest in the stock market. Similarly, Botazzi et al. (2012) show that trust plays a role in venture capital investment. Following the interpretation of Nanda and Nicholas (2014), we expect religious fragmentation to be positively related to the bank-suspension rate. The intuition is that higher fragmentation makes bank runs more likely if there is any uncertainty over banks' illiquid assets.[19] Consistent with this intuition, we confirm that higher fragmentation predicts an higher rate of bank suspension at the MSA level during the Great Depression (Figure 7).[20] The correlation between the two quantities is not just economically large–a 10 percent increase in fragmentation leads to a 5 percent increase in suspension rate–but also highly significant statistically, confirming that this instrument is a good candidate for being an IV (Table 8).

In addition of predicting the endogenous variable, we argue that this instrument is also likely to satisfy the exclusion restriction—namely, that conditional on the fixed effects, the only way in which our religious-fragmentation index affects the relative change in manufacturing across external-finance dependence is through bank suspension. Because of our fixed effects, religious fragmentation affecting any city-year or industry group-year level characteristic directly does not threaten the exclusion restriction. For example, our instrument is still valid if fragmentation is correlated with some city-level characteristic such as the quality of institutions, which may influence how the real economy responds to a negative city shock. The only condition that needs to hold is that – within a city – higher fragmentation is not systematically correlated with lower manufacturing activity in high-dependence relative to low-dependence industries. While this is impossible to rule out completely, we believe that this instrument represents a step forward in relaxing our identification assumptions.

However, as a last step, Table 8 also provides suggestive evidence of the exclusion restriction. In particular, religious fragmentation does not appear to be strongly related to

[19]Because our instrument relies on bank runs causing suspensions, it requires a sufficiently high fraction of all suspensions to be due to runs rather than insolvency. Only if this is the case will we find a first-stage relationship between our trust index and bank suspensions.

[20]This plot is not the first stage regression used in the analysis. That regression contains fixed effects. We show this picture for clarity—so we can display each city as a separate data point. The first stage results that include fixed effects are reported below.

pre-Depression banking sector size, the manufacturing growth rate, or the share of manufacturing in industries of high external-finance dependence. While this evidence is neither necessary nor sufficient in proving the exclusion restriction, it does show that cities with greater religious fragmentation are not systematically different than cities with less religious fragmentation on dimensions other than bank suspensions.

Following this discussion on the plausibility of the instrument, Table 5 reports the results obtained instrumenting for bank suspensions with our religious fragmentation measure.[21] Our results are generally consistent with the OLS. Across our three main outcomes, we find that in the manufacturing sector, industries that are more dependent on external finance contracted relatively more in MSAs with more bank suspensions. These differences are still statistically significant, but - relative to the OLS - they are slightly larger in absolute magnitude. In particular, the increase in size goes from a 10 percent increase for value added to a 50 percent increase for employment. The more likely explanation for this increase is measurement error: if our initial measure of bank suspension were noisy, classical measurement error would attenuate the OLS estimates and the IV approach would reduce this bias. Because our suspension rate measure collapses together suspensions of varying types and time lengths, noise is likely to play some role in explaining the difference in size. However, we cannot exclude that the local effect estimated using the instrument is larger relative to the average effect estimated with the OLS (Angrist and Imbens, 1994).[22]

[21]In practice, we implement the estimation using Limited Information Maximum Likelihood method (LIML) built into the standard instrumental variable estimator in Stata.

[22]This second explanation is related to the LATE interpretation of an IV estimates. In fact, if the treatment effect is heterogeneous across cities, then our instrument identifies a parameter that is a weighted average of individual city effects, where the weights depend positively on the sensitivity of the instrumented variable to the instrument. In our case, this means that the LATE assigns greater weight to cities in which religious fragmentation makes bank suspensions more likely. Such a weighting would make the estimated IV effect larger than the estimated OLS effect if the cities in which religious fragmentation is associated with more bank suspensions were also cities in which non-financial firms were weaker pre-Depression. This seems plausible if cities with weak bank balance sheets were also cities with weak non-bank balance sheets. In this case, the onset of a negative panic in the banking sector would lead to both greater bank suspensions and non-financial firm decline. This will increase the size of the estimated effect of bank suspensions on non-financial firm decline when that effect weights these weak cities more heavily. Hence, the LATE estimate will be larger than the OLS estimate.

This evidence confirms the causal interpretation of our estimates and therefore the importance of the credit shock in explaining the drop in economic activity during the Great Depression. In the next Section, we further confirm this interpretation using an alternative instrument.

4.4.2 Land Value Instrument

As a further robustness test to our OLS results, we implement another IV model with an alternative instrument: 1910-1920 growth in agricultural land values. Previous examinations of the impact of bank distress on the real economy during the US Great Depression have used real-estate-related instruments for bank distress. Calomiris and Mason (2003) used the share of bank assets in real estate in 1929. The authors asserted that a larger holding of real estate in 1929 was a good predictor of 1929-1933 bank distress for two reasons. First, real estate is a relatively illiquid asset compared to financial securities, so the more real estate a bank held, the more likely it was to suffer during a run or another negative shock. Second, real estate values—particularly those of agricultural real estate— increased markedly in 1910s and then declined during the 1920s as demand for agricultural goods from World War I subsided. Consequently, banks with large real estate portfolios—especially in agriculture—suffered large losses. In turn, these losses left these banks with a weak balance sheet at the onset of the Great Depression.

Following this logic, we use the 1910-1920 growth in agricultural land values around the MSA as an instrument for bank suspension. The intuition is that banks in areas that experienced a larger increase in prices in the 1910s are more likely to have increased their exposure to real estate during this time and consequently to have been negatively affected by the general decrease in agricultural land values in the following decade. Furthermore, Mladjan (2017) claims that states with the largest increases in agricultural land prices from 1910-1920 had the largest decreases in agricultural land prices from 1920-1929, therefore exacerbating the negative effect on banks' balance sheets.[23]

Because banks in an MSA may have had exposure to agricultural land in counties near but not technically within the city boundaries, we include in our calculations all counties within specified distances (20, 25, and 30 miles) of our city centroids. We then calculate for each city a weighted average of the 1910-1920 agricultural land value growth rates for the counties within the specified distance, where the weights are the number of county acres in farmland in 1910:

$$Growth_c = \frac{\sum_{p \in d} g_p l_p}{\sum_{p \in d} l_p} \tag{4}$$

where g_p is the growth rate in agricultural land values from 1910-1920 for county p; l_p the number of acres of agricultural land in county p as of 1910; and d is the distance—20, 25, or 30 miles—from the city centroid.

In order for it to be a valid instrument, the usual assumptions must hold. First, we need the instrument to predict the endogenous variable. Figure 8 shows a graphical representation of the first stage. As expected, cities with larger increases in agricultural land value from 1910 to 1920 experienced more bank suspensions from 1930-1933. Second, the instrument must affect the manufacturing sector only through an increase in bank distress during the Great Depression. Similarly to the case of religious fragmentation, this instrument can be correlated with city-time and industry-time characteristics and still produce unbiased estimates. However, the growth rate in agricultural land values from 1910 to 1920 must be uncorrelated with the relative growth in manufacturing activity between high- and low-dependence industries.

The results from this IV model– which are presented in Table 6– are also re-assuring.

[23] Unlike the religious fragmentation instrument, however, this land value growth instrument operates through a balance sheet channel rather than a bank run channel. That is, while greater religious fragmentation leads to more bank suspensions because a lack of community trust makes bank runs more likely, large growth in agricultural land values from 1910-1920 leads to more bank suspensions because bank balance sheet are weaker at the outset of the 1929 financial crisis. Hence, this instrument operates through the insolvency rather than the illiquidity channel. Nevertheless, its affect on bank suspensions is likely to increase them.

In general, we confirm that higher bank distress led to a decline in manufacturing activity: in every specification, we find that the coefficient on the triple interaction is negative and significant. In this case too, the magnitude of the coefficient increases relative to the OLS results. Furthermore, we find consistent results across different definitions of the instrument. In particular, the effect is similar when we use 20, 25, or 30 miles as definition of relevant real estate market. Hence, when using a real estate related instrument for 1930-1933 bank distress, we confirm our OLS and religious fragmentation IV robustness findings.

5 Persistence

Whether that decline was short-lived or persisted is a question we are uniquely able to answer with our newly digitized, city-industry-year level manufacturing data, which extend into the late 1930s. Moreover, with information on three different measures of manufacturing—the number of establishments, employment, and value added—we can further test whether the persistence was concentrated along certain dimensions of the sector.

Learning the answers to these questions is important for understanding the total costs of a financial crisis and designing government policy. Failing to account for persistence in the negative effects could lead to an underestimation of financial-crisis costs. This, in turn, could lead to ill-informed policy decisions—particularly in the immediate post-crisis period. For example, if the negative effects on the real economy of a credit-supply shock dissipate quickly, then policymakers might be wise to intervene less in the economy once the credit market has returned to pre-crisis health. In the context of the Great Depression, with bank-suspension rates in our sample cities returning to 1920s levels by 1935 (Figure 3), a finding of non-persistence could support a perhaps less-aggressive role for government policy in the late 1930s. Either way, investigating the persistence of negative credit-market effects on the real economy will enhance the understanding of the costs of financial crises as well as any government policy that could seek to address those costs.

We test for persistence by running the same OLS specification in equation (2), except instead of interacting our high external finance and bank shock variables with an indicator for 1933, we use 1937 data and interact them with an indicator for 1937:

$$\ln Y_{cit} = \beta_{1937} HFin_{j(i)} 1\left[t = 1937\right] Shock_c + \gamma HFin_{j(i)} Shock_c + \alpha_{j(i)t} + \alpha_{ct} + \varepsilon_{cit} \quad (5)$$

Thus, we are now comparing the differential growth rates between industries high and low external finance dependence in cities with large and small bank suspensions from 1929 to 1937 instead of from 1929 to 1933. If there is any persistence in our results, we expect $\beta_{1937} < 0$.

Before estimating β_{1937}, however, we make two comments about identification. First, to identify β_{1937} requires an additional assumption beyond the assumptions required to identify β in equation (2)—namely, that any city-level shocks from 1933 to 1937 not caused by the original 1929-1933 bank shocks are uncorrelated with the 1929-1933 bank shock. A post-1933 shock correlated with but not caused by the 1929-1933 bank shock would cause β_{1937} to include the effects from the post-1933 shock in addition to the 1929-1933 bank shock effects. This would bias our estimates of β_{1937}.[24]

Secondly, even if β_{1937} remains unbiased, it could be attenuated by post-1933 targeted government intervention. If politicians devoted recovery resources to cities with high levels of bank distress and within those cities to manufacturing industries that declined the most from 1930 to 1933, then the recovery resources might have mitigated persistent effects of local bank distress. If, on the other hand, politicians distributed resources orthogonally to 1930-1933 bank distress, then recovery resources would simply add noise to our model, which, all else equal, would just increase measurement error and decrease precision. In any case, a direct government intervention is likely to bias us against finding any evidence of

[24]A post-1933 shock correlated with the earlier bank shock but also caused by the bank shock, on the other hand, would not pose a problem. For this case, the post-1933 shock would be attributable to the bank shock. In that case, β_{1937} would remain unbiased.

persistence.

With these two comments in mind, we estimate equation (5) in Table 7. In the odd columns, we explore these tests using the standard OLS model. In general, we do not find strong evidence of persistence on the value added and employment outcomes. On the establishment outcome, however, we do see the negative effects of the 1930-1933 banking shock present in 1937. The magnitude of the effect is similar to the effect in 1933, which suggests little recovery in the number of manufacturing establishments between 1933 and 1937. We confirm our results using our religious-fragmentation robustness instrument. These results are reported in the same Table 7 in the odd columns.[25] Combined, the OLS and IV robustness results using 1937 outcomes suggest that the negative effects of the 1930-1933 local-credit-supply shocks on the manufacturing sector persisted, at least on the number of establishment.

One very interesting fact is that a very similar pattern can be found in the national aggregate data, which are presented in Figure 1. In particular, this Figure shows that both manufacturing employment and value added recovered to their pre-Depression level by the end of the 30s. Instead, the number of establishments in 1939 was still more than 20% lower than in 1929. Hence, aggregate data patterns are consistent with our sample micro data estimates: the creation of new manufacturing plants simply did not recover at the same rates as employment and output. This pattern seems consistent to some fixed-cost model, where re-opening a new establishment is relatively more expensive than scaling up an operating facility.

These persistent, negative effects of financial distress on establishments are particularly interesting and potentially important for policy. However, their interpretation in terms of welfare is non trivial. On the one hand, to the extent that establishments do indeed proxy for capital investment, their slower recovery suggests that policymakers may want to

[25] We obtain a more precise estimate on the employment outcome using the IV approach, but the magnitudes of the coefficient and the precision of the estimate are both less than the corresponding figures on the establishment outcome.

design policies to encourage investment in the aftermath of a financial crisis even if other recovery policy measures are easing. This effect would be even reinforced if the number of establishment proxied for the level of entrepreneurship in the area. In turn, this could have an important impact on long-term growth and innovation (Chinitz, 1961). On the other hand, the strong recovery in employment and value added combined with a slower increase in the number of establishments seem to support a cleansing effect of the crisis (Caballero and Hammour, 1994). Further research may be required to better interpret these findings in terms of welfare. Nevertheless, our results confirm that financial shocks may affect the growth patterns

6 Conclusion

Exploiting the comprehensive outcomes of newly digitized city-industry-year level data covering US manufacturing during the country's largest financial crisis, the Great Depression, we show that distress in the financial sector has a significant, negative, and—along certain dimensions—persistent effect on the real economy. We estimate that the unprecedented wave of bank suspensions from 1929 to 1933 led to contemporaneous decline in manufacturing employment, establishments, and value added. Such a finding provides a micro-founded channel through which bank distress induced a contraction in real activity during the Depression era. Moreover, our findings that the negative effects of financial distress lingered into the late 1930s for establishments, which in the Census of Manufactures data did not achieve pre-Depression levels until 1947, are among the first micro-evidence on the persistence of financial shocks. Thus, with our new data, which allow for a uniquely rigorous identification strategy, we have shown that US manufacturing, which comprised nearly a third of the country's economic activity in 1929, suffered greatly as a result of financial-sector distress, and that the effects persisted along the establishment channel.

We encourage future researchers to investigate in greater detail the nature of the recovery in the financial and non-financial sectors following the Great Depression—and financial crises, generally—using similar microeconometric techniques and new data. Since Calomiris and Mason (2003), scholars have increasingly exploited micro level data in historical contexts to document the causes of financial crises and answer macroeconomic questions more generally. A similar approach on recoveries would be equally informative, particularly for policymakers seeking ex-ante and ex-post policies to mitigate economic damage caused by financial crises. Our analysis into the persistence of the negative effects of Depression-era bank suspensions is a start, but as more micro-level data from the Great Depression era and the 2008-2009 financial crisis become available, scholars can augment our understanding of how economies rebound from credit-supply shocks.

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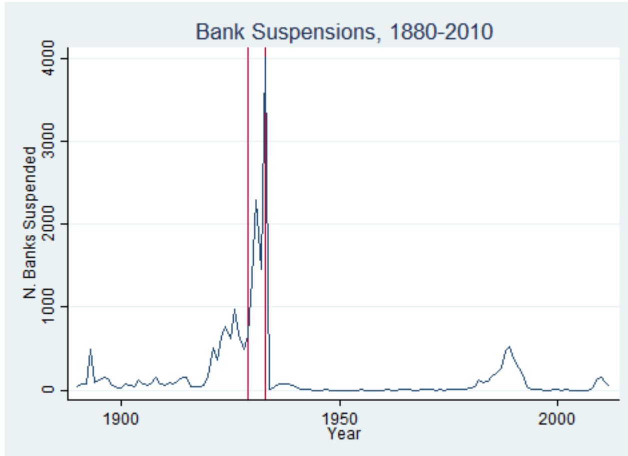
Figure 1: US Manufacturing Establishments, Employment, and Value Added, and US Bank Deposits, 1923-1939

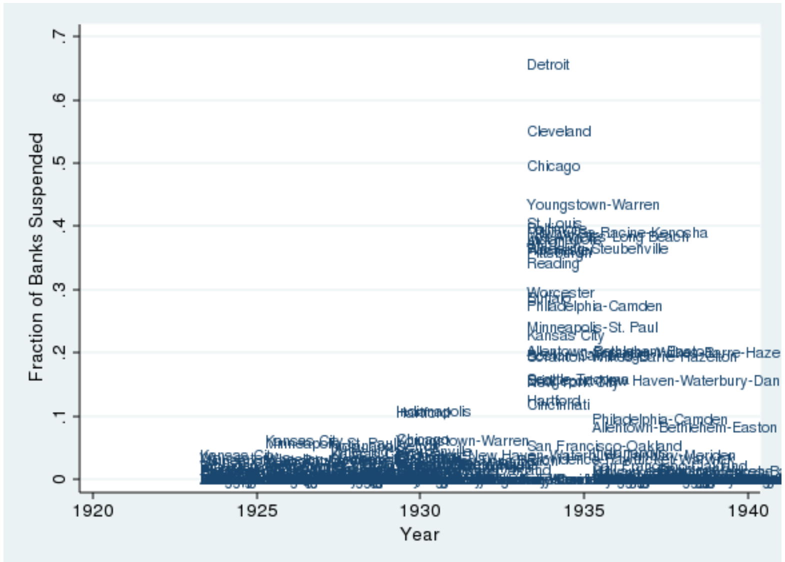
Notes: Each line reflects the natural log of the indicated outcome, less the log of the 1929 value. All manufacturing data are from the US Census of Manufactures, 1923-1929. Deposit data are from the FDIC.

Figure 2: US Bank Suspensions, 1880-2010



Notes: A bank suspension is defined by the FDIC as the temporary or permanent closure of a bank by supervisory authorities or the bank's board of directors on account of financial difficulties. Data are constructed combining two sources. For the older part of the series (pre-1970), the data come from the "Historical Statistics of the United States: Colonial Times to 1970", Table bf01. For the more recent part of the series, the data come from the FDIC website—in particular the "Failures and Assistance Transactions" page.

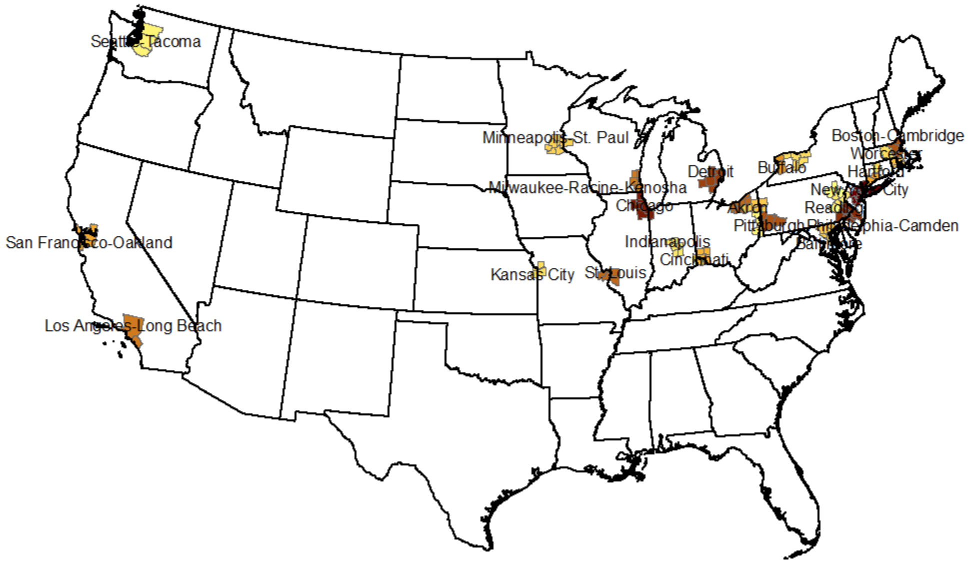
Figure 3: US Bank Suspensions Rates, by City, 1921-1936



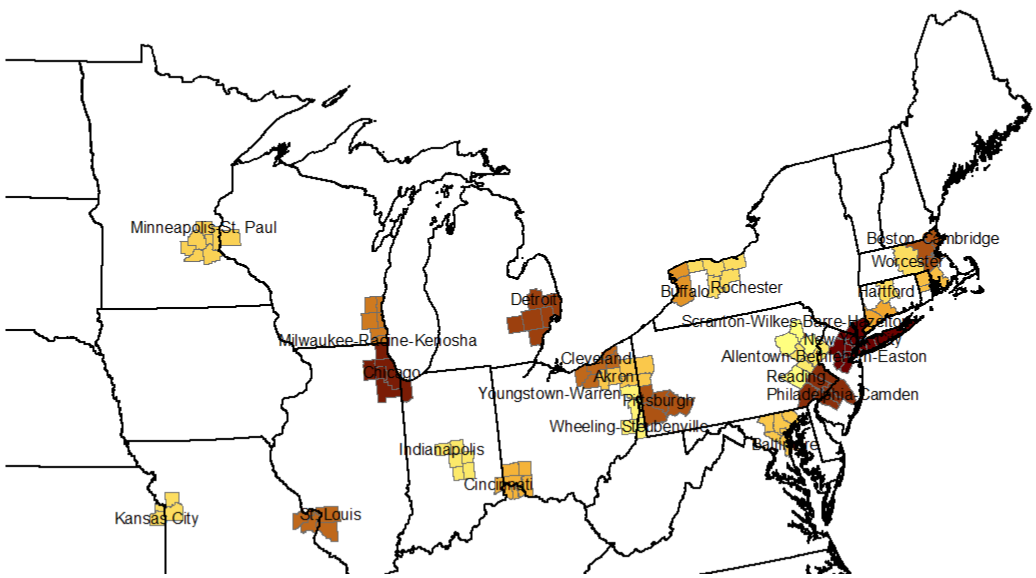
Notes: Each dot represents the fraction of the indicated city's banks suspended in the two years leading up to the shown data point. All data are from the FDIC. Cities are defined as contiguous counties based on definitions from the US Census Bureau.

Figure 4: Manufacturing Value Added in 29 Sample Cities, 1929

Panel A: All 29 Sample Cities

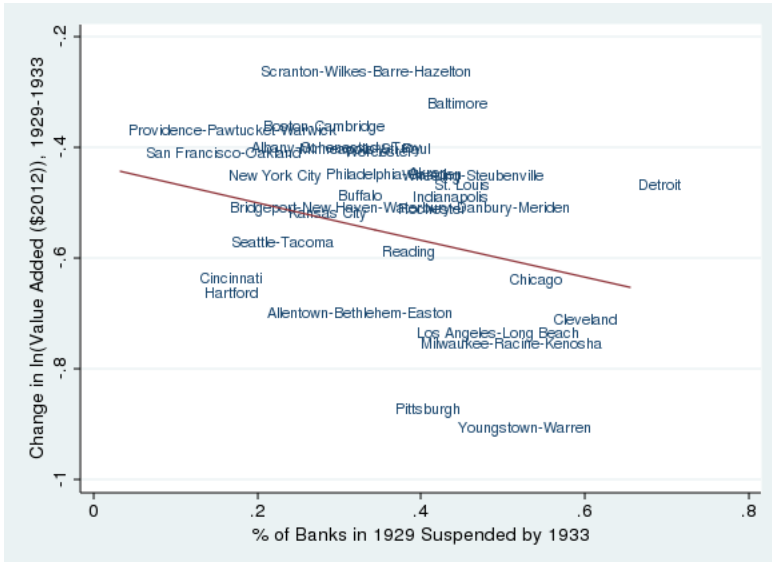


Panel B: Northeast Zoom



Notes: Manufacturing data and city boundaries are from the US Census of Manufactures. The light gray lines are county boundaries within cities.

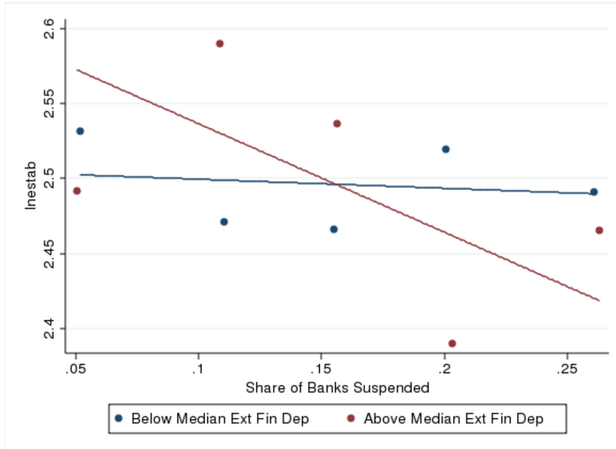
Figure 5: Change in Manufacturing Value Added from 1929-1933, by 1930-1933 Bank Suspension Rates



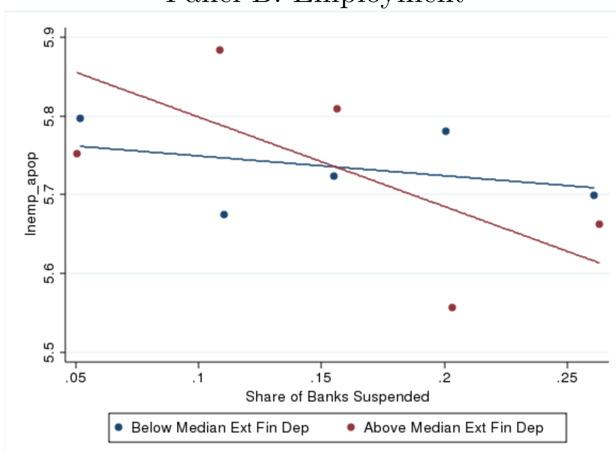
Notes: Bank data are from the FDIC. Manufacturing data are from the US Census of Manufactures.

Figure 6: Difference in 1929-1933 Manufacturing Growth, by External Finance Dependence

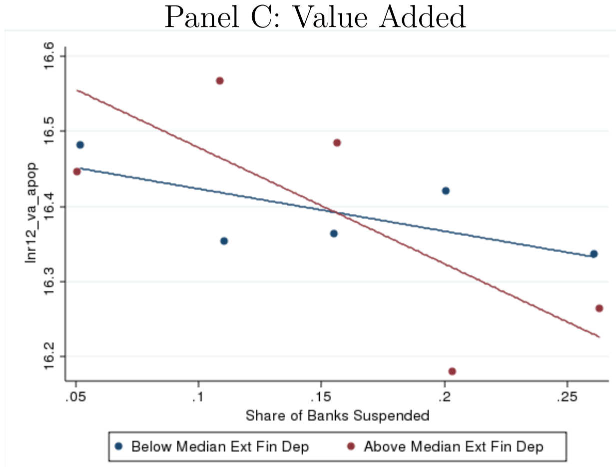
Panel A: Establishments



Panel B: Employment

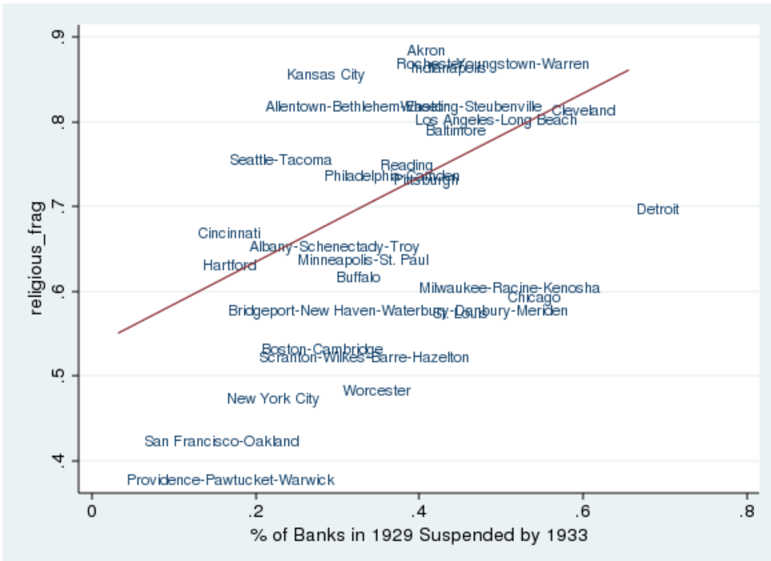


Panel C: Value Added



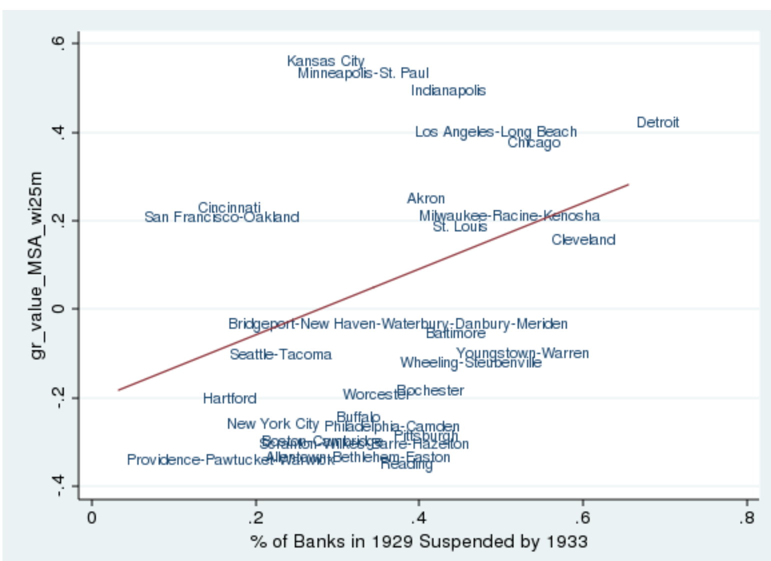
Notes: Each red (blue) dot represents the average outcome value among data points in the given quintile of the share of banks suspended among high (low) external finance dependence industries, after residualizing for city and year. The red (blue) line represents the linear fit to all red (blue) data points. Manufacturing data are from the US Census of Manufactures. External finance dependence data are from Moody's *Manual of Industrials* and Nanda and Nicholas (2014). Bank data are from the FDIC.

Figure 7: First-Stage: Religious Fragmentation, by Fraction of 1929 Banks Suspended by 1933



Notes: Religious fragmentation is defined by equation (3) in the text. Religious data are from the US Census of Religious Bodies, 1906. Bank data are from the FDIC.

Figure 8: Real Estate First-Stage: Change in Nearby Agricultural Land Values from 1910-1920, by Fraction of 1929 Banks Suspended by 1933



Notes: Each dot reflects the change in agricultural land values from 1910-1920 among counties within 25 miles of the indicated city center. Each county is weighted by its 1910 acres of farmland. Agricultural land price data are from the US Census of Agriculture. Bank data are from the FDIC.

Table 1: City Bank Suspension Rate and City Manufacturing Outcomes, 1929-1933

MSA	Bank Suspension(1929)	Value Added ($2012 M)		
		1929	1933	% Drop
Detroit, MI	76.6%	16,384	10,262	-37.4%
Cleveland, OH	55.0%	9,797	4,812	-50.9%
Chicago, IL	49.7%	33,955	17,931	-47.2%
Youngstown-Warren, OH	43.5%	4,257	1,720	-59.6%
St. Louis, MO	40.5%	8,302	5,204	-37.3%
Baltimore, MD	39.7%	4,570	3,319	-27.4%
Milwaukee-Racine-Kenosha, WI	39.0%	7,332	3,454	-52.9%
Los Angeles-Long Beach, CA	38.5%	8,115	3,898	-52.0%
Indianapolis, IN	38.0%	2,530	1,551	-38.7%
Akron, OH	37.5%	3,749	2,401	-36.0%
Wheeling-Steubenville, WV	36.6%	1,866	1,188	-36.3%
Rochester, NY	36.2%	3,368	2,022	-40.0%
Pittsburgh, PA	35.8%	11,413	4,772	-58.2%
Reading, PA	34.3%	1,663	924	-44.4%
Worcester, MA	29.6%	3,245	2,161	-33.4%
Buffalo, NY	28.8%	6,575	4,041	-38.5%
Philadelphia-Camden, PA	27.4%	19,073	12,181	-36.1%
Kansas City, MO	22.9%	3,063	1,825	-40.4%
Minneapolis-St. Paul, MN	24.2%	3,662	2,448	-33.2%
Allentown-Bethlehem-Easton, PA	20.2%	2,545	1,265	-50.3%
Boston-Cambridge, MA	19.7%	12,975	9,040	-30.3%
Scranton-Wilkes-Barre-Hazelton, PA	19.4%	1,236	951	-23.0%
Seattle-Tacoma, WA	15.8%	2,158	1,219	-43.5%
Bridgeport-New Haven-Waterbury-Danbury-Meriden, CT	15.7%	5,921	3,563	-39.8%
New York City, NY	15.5%	60,098	38,317	-36.2%
Cincinnati, OH	11.9%	5,850	3,102	-47.0%
Hartford, CT	12.5%	3,047	1,572	-48.4%
San Francisco-Oakland, CA	5.4%	6,170	4,101	-33.5%
Providence-Pawtucket-Warwick, RI	3.2%	5,820	4,023	-30.9%
Average	29.7%	8,922	5,285	-40.8%
Correlation Bank Drop and Value Added Change		29.2%		

The sample is all city in the 29 cities with industry level data reported in both the 1929 and 1933 US Censuses of Manufactures. Bank shock is defined at the city level as the fraction of the city's 1929 banks suspended by 1933, according to FDIC. Deposit shock is defined at the city level as the fraction of the city's 1929 deposit that were held by a suspended banks by 1933, according to FDIC. Correlations are simple raw correlations.

Table 2: Difference in Manufacturing Outcomes, by City Bank Suspension Rate and Industry External Finance Dependence, 1929-1933

	$\ln(Estab)$		$\ln(Emp)$		$\ln(VA)$	
	(1)	(2)	(3)	(4)	(5)	(6)
$HiFin_j Post_t Shock_c$	-0.112**	-0.114**	-0.142**	-0.141**	-0.151***	-0.153***
	(0.047)	(0.048)	(0.063)	(0.063)	(0.054)	(0.054)
$HiFin_j Shock_c$	-0.027	-0.028	-0.024	-0.025	-0.028	-0.028
	(0.027)	(0.027)	(0.064)	(0.063)	(0.066)	(0.065)
$Post_c Shock_c$	-0.005		-0.024		-0.066**	
	(0.015)		(0.023)		(0.027)	
City FE	Y		Y		Y	
CityXTime FE		Y		Y		Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y
R^2	0.326	0.327	0.319	0.320	0.341	0.342
N	4,733	4,733	4,733	4,733	4,733	4,733

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Table 3: Difference in Manufacturing Outcomes, Controlling for Non-Finance Industry Characteristics, 1929-1933

	$\ln(Estab)$					
	(1)	(2)	(3)	(4)	(5)	(6)
$HiFin_j Post_t Shock_c$	-0.114**	-0.107**	-0.120**	-0.115**	-0.123**	-0.125**
	(0.048)	(0.047)	(0.045)	(0.050)	(0.047)	(0.054)
$Contr_j Post_t Shock_c$		-0.009				-0.017
		(0.038)				(0.039)
$Skill_j Post_t Shock_c$			-0.046			-0.048
			(0.045)			(0.051)
$Capital_j Post_t Shock_c$				-0.067		-0.098**
				(0.043)		(0.044)
$Size_j Post_t Shock_c$					0.051	0.055
					(0.073)	(0.085)
CityXTime FE	Y	Y	Y	Y	Y	Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y
R^2	0.327	0.327	0.333	0.328	0.349	0.337
N	4,733	4,733	4,733	4,733	4,733	4,733

	ln $(Estab)$					
	(1)	(2)	(3)	(4)	(5)	(6)
$HiFin_j Post_t Shock_c$	-0.114**	-0.107**	-0.120**	-0.115**	-0.123**	-0.125**
	(0.048)	(0.047)	(0.045)	(0.050)	(0.047)	(0.054)
$Contr_j Post_t Shock_c$		-0.009				-0.017
		(0.038)				(0.039)
$Skill_j Post_t Shock_c$			-0.046			-0.048
			(0.045)			(0.051)
$Capital_j Post_t Shock_c$				-0.067		-0.098**
				(0.043)		(0.044)
$Size_j Post_t Shock_c$					0.051	0.055
					(0.073)	(0.085)
CityXTime FE	Y	Y	Y	Y	Y	Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y
R^2	0.327	0.327	0.333	0.328	0.349	0.337
N	4,733	4,733	4,733	4,733	4,733	4,733

	ln (VA)					
	(1)	(2)	(3)	(4)	(5)	(6)
$HiFin_j Post_t Shock_c$	-0.153***	-0.144***	-0.149***	-0.163***	-0.128**	-0.129**
	(0.054)	(0.049)	(0.053)	(0.058)	(0.055)	(0.060)
$Contr_j Post_t Shock_c$		-0.047				-0.065
		(0.046)				(0.041)
$Skill_j Post_t Shock_c$			0.006			-0.069
			(0.063)			(0.062)
$Capital_j Post_t Shock_c$				-0.129***		-0.132**
				(0.044)		(0.049)
$Size_j Post_t Shock_c$					-0.044	-0.041
					(0.110)	(0.129)
CityXTime FE	Y	Y	Y	Y	Y	Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y
R^2	0.342	0.343	0.344	0.359	0.417	0.419
N	4,733	4,733	4,733	4,733	4,733	4,733

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. Contract intensity ($Contr_j$) is taken from Nunn (2007) and pertains to 4-digit SIC code industries in 1963. It is then recoded as a dummy, which takes the value of one for all industries whose contract intensity is above the median level. Capital Intensity($Capital_j$) is obtained from the 1919 Census of Manufactures. The 1919 Census of Manufactures is the last pre-Depression Census of Manufactures to report total capital stock at the industry level. We divide total capital stock by industry value added to construct our industry-level capital intensity. We then recode the variable as a dummy , which, as with the other industry-level variables,takes the value of one if the industry level is above the median level. Skill Intensity ($Skill_j$) is constructed directly from the 1929 Census of Manufactures at the 4-digit SIC industry level. It is the fraction of an industry's employees that is not production workers. It is coded as a dummy, which also takes the value of one if the industry level is above the median level. Average establishment size ($Size_j$) is also constructed directly from the 1929 Census of Manufactures at the 4-digit SIC industry level. It is the total number of employees in the industry divided by the total number of establishments in the industry. We code it as a dummy, which also takes the value of one if the industry level is above the median industry level. We report only the triple interactions, but all regressions include all lower-level interactions as well. Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Table 4: Placebo Tests

		$\ln(Estab)$			$\ln(Emp)$			$\ln(VA)$	
Period	1923-1929	1925-1929	1927-1929	1923-1929	1925-1929	1927-1929	1923-1929	1925-1929	1927-1929
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$HighFin_j 1\{Yr = 29\} Shock_c$	0.028	-0.032	-0.027	0.076	0.042	0.005	0.037	0.022	0.010
	(0.040)	(0.041)	(0.037)	(0.061)	(0.057)	(0.042)	(0.053)	(0.065)	(0.039)
$HighFin_j Shock_c$	-0.056	0.005	-0.001	-0.101	-0.067	-0.030	-0.065	-0.050	-0.038
	(0.046)	(0.046)	(0.041)	(0.092)	(0.093)	(0.076)	(0.089)	(0.097)	(0.077)
CityXTime FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.355	0.365	0.366	0.361	0.385	0.373	0.383	0.401	0.395
N	4,558	4,431	4,255	4,558	4,431	4,255	4,558	4,431	4,255

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. In column (1), (4) and (7), we employ data from 1923 and 1929 for the placebo test; in columns (2), (5) and (8) we use data from 1925 and 1929; in columns (3), (6) and (9) we use data from 1927 and 1929. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. The Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Table 5: Difference in Manufacturing Outcomes, by City Bank Suspension Rate and Industry External Finance Dependence, 1929-1933, Instrumenting for City Bank Suspension Rate with Religious Fragmentation

	$\ln(Estab)$		$\ln(Emp)$		$\ln(VA)$	
	(1)	(2)	(3)	(4)	(5)	(6)
$HighFin_j Post_t Shock_c$	-0.209**	-0.205**	-0.224**	-0.223**	-0.167*	-0.160*
	(0.099)	(0.099)	(0.098)	(0.097)	(0.092)	(0.092)
$HighFin_j Shock_c$	0.106	0.100	0.217	0.215	0.160	0.154
	(0.070)	(0.066)	(0.145)	(0.143)	(0.141)	(0.137)
$Post_t Shock_c$	-0.044		-0.066*		-0.129**	
	(0.033)		(0.038)		(0.050)	
City FE	Y		Y		Y	
CityXTime FE		Y		Y		Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y
R^2	0.324	0.327	0.317	0.318	0.339	0.341
N	4,733	4,733	4,733	4,733	4,733	4,733

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. This is instrumented using the Trust index, constructed from the Census of Religion Bodies (1906), as described in the paper. The Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Table 6: Difference in Manufacturing Outcomes, by City Bank Suspension Rate and Industry External Finance Dependence, 1929-1933, Instrumenting for City Bank Suspension Rate with Regional Land Value Growth from 1910-1920

		$\ln(Estab)$			$\ln(Emp)$			$\ln(VA)$	
	20$m.$	25$m.$	30$m.$	20$m.$	25$m.$	30$m.$	20$m.$	25$m.$	30$m.$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$HighFin_j Post_t Shock_c$	-0.372**	-0.381***	-0.418**	-0.301*	-0.289**	-0.311*	-0.241*	-0.233*	-0.236*
	(0.159)	(0.142)	(0.175)	(0.158)	(0.139)	(0.164)	(0.138)	(0.123)	(0.134)
$HighFin_j Shock_c$	-0.077	-0.056	-0.057	-0.211	-0.197	-0.215	-0.306*	-0.290*	-0.328*
	(0.062)	(0.060)	(0.067)	(0.141)	(0.146)	(0.172)	(0.164)	(0.160)	(0.198)
CityXTime FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
IndGrpXTime FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
R^2	0.320	0.323	0.322	0.316	0.317	0.316	0.337	0.338	0.336
N	4,733	4,733	4,733	4,733	4,733	4,733	4,733	4,733	4,733

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. This is instrumented using the Land Growth between 1910 and 1920, constructed from the Census data. Land Growth is computed, as described in the paper, over three different distances from the city: 20 miles, 25 miles and 30 miles. Every outcome is presented with each instrument.Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Table 7: Persistence Regressions 1929-1933 and 1929-1937, (OLS and IV with Religious Fragmentation)

	$\ln(Estab)$			$\ln(Emp)$			$\ln(VA)$		
	OLS	*IV*	*IV&Contr*	*OLS*	*IV*	*IV&Contr*	*OLS*	*IV*	*IV&Contr*
β_{1933}	-0.114**	-0.205**	-0.213**	-0.141**	-0.223**	-0.236**	-0.153***	-0.160*	-0.170*
	(0.048)	(0.099)	(0.099)	(0.063)	(0.097)	(0.092)	(0.054)	(0.092)	(0.090)
β_{1937}	-0.118*	-0.300**	-0.304**	-0.088	-0.256*	-0.231*	-0.065	-0.158	-0.141
	(0.059)	(0.123)	(0.121)	(0.068)	(0.143)	(0.136)	(0.063)	(0.129)	(0.126)

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is all city-industry-year triplets in the 29 cities with industry level data collected in both the 1929 and 1933 US Censuses of Manufactures. β_t represents the coefficient on the triple interaction between the dummy for high dependence on external finance, the City Bank Suspension Rate and a dummy that takes value one for the period t. All variables definition is the same as in previous tables. The *OLS* columns have the standard least-squares regression with city-time and industry group-time fixed effect. The *IV* columns are estimated using the Religious Fragmentation index as instrumental variable. The *IV&Contr* columns are estimated with Religious Fragmentation index as instrumental variable and controlling for the other relevant factors in the analysis, as in the previous Table. The Standard errors are clustered at the city level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Appendix

Industry-level measure

To ensure that our external finance dependence measure is capturing true external finance dependence and not some other industry characteristic, we estimate our main equation with four separate, non-external finance dependence industry characteristics interacted with the bank suspension rate and an indicator for the year being 1933. In this section, we provide further details on how we have created these measures.

The first measure we consider is contract intensity. It is taken from Nunn (2007) and pertains to 4-digit SIC code industries in 1963. We map this 1963 measure back to our 1929 4-digit SIC code industries using information on Census-to-Census industry mappings published in the Censuses of Manufactures from 1929 to 1963. The measure is the fraction of an industry's inputs that are neither sold on an organized exchange nor listed in a reference price trade journal. The higher the fraction, the more contract-intensive the industry because input purchases are more likely to occur in a relationship-specific—and therefore contract-inducing—way. We create an indicator for whether an industry is above the median contract intensity value across all industries. We then interact this indicator with the interaction of our bank suspension measure and an indicator for the year being 1933—just as we did with our external finance dependence measure.

Our second non-external finance dependence industry measure that we interact with the bank suspension rate and an indicator for year 1933 is capital intensity. We obtain this at the 4-digit SIC industry level from the 1919 Census of Manufactures. The 1919 Census of Manufactures was the last pre-Depression Census of Manufactures to report total capital stock at the industry level. We divide total capital stock by industry value added to construct our industry-level capital intensity measure. We map the 1919 industries into 1929 industries using the industry mapping described above. We then interact the bank suspension rate and an indicator for year 1933 with an indicator for whether an industry is above the median capital intensity, as we did with our other two industry measures.

Our third non-external finance dependence industry measure is skill intensity. We construct this directly from the 1929 Census of Manufactures at the 4-digit SIC industry level. We calculate it as the fraction of an industry's employees that is not production workers. Such workers are either "proprietors and firm members" or "salaried officers and employees." We consider these workers skilled. Hence, the more of these workers an industry has relative to its production workers, the more skill-intensive the industry. As with our other industry measures, we interact an indicator for whether this measure is above the median measure value with the bank suspension rate and an indicator for year 1933.

Our fourth and final non-external finance dependence industry measure is establishment size. As with the skill intensity measure, we construct this directly from the 1929 Census of Manufactures at the 4-digit SIC industry level. We calculate it as the number of industry employees per establishment. Hence, higher levels mean larger establishments, on average. We interact an indicator for whether this measure is above the median measure value with the bank suspension rate and an indicator for year 1933. Combined with the contract intensity, capital intensity, and skill intensity measures, this establishment size measure thus provides a further test that our external finance dependence measure is indeed capturing industry dependence on external finance and not some other characteristic.

Aggregate Effects of Financial Distress

In the paper, we claim that the credit supply shock caused at least 5 percent of the drop in manufacturing employment during the Great Depression. We arrive at this estimate using the following procedure.

First, we define the unconditional percentage point decline in employment as α^H for industries with high external finance dependence and α^L for industries with low external finance dependence. We say unconditional because α^j, with $j = \{H, L\}$, is the drop in employment coming from all sources—the credit supply channel and others. Second, we define the share of employment in high external finance dependence industries as θ. In our sample, θ is approximately 40 percent. Hence, the total unconditional drop in employment is $\theta\alpha^H + (1-\theta)\alpha^L$. Using the values of α^H and α^L that we see in our data, we obtain the total unconditional drop in employment 33 percent. Third, we define as β^j with $j = \{H, L\}$ the drop in employment coming from only the credit supply channel. This is the decline we estimate in equation (2). Thus, the the drop in employment due to the credit shock is $\theta\beta^H + (1-\theta)\beta^L$. We divide this by the total unconditional drop to obtain the fraction of the total unconditional drop coming from the credit shock:

$$\sigma = \frac{\theta\beta^H + (1-\theta)\beta^L}{\theta\alpha^H + (1-\theta)\alpha^L}$$

We assume $\beta^L = 0$. This means that low external finance industries do not experience any drop from the credit supply shock. We view this as a conservative assumption, since the estimated coefficient for the low dependent group - which can be recovered when we do not introduce MSA by year fixed effects - is generally negative.[26] Furthermore, these baseline coefficients are by themselves likely to underestimate the impact of the shock, since part of the effect of bank distress is capture by the industry trends.

Using the $\hat{\beta^H} = -0.043$ we obtain at the median shock level, this yields $\sigma = \frac{\theta\hat{\beta^H}+(1-\theta)\beta^L}{\theta\alpha^H+(1-\theta)\alpha^L} = \frac{(0.40)\cdot(-0.043)}{(-0.33)} = 0.05$.[27] Hence, the fraction of the total drop in manufacturing employment

[26] The alternative assumption would be $\beta^L > 0$. This could be plausible if a general equilibrium effect existed of declines in high external finance dependence industries leading to expansions of low external finance dependence industries within a city, even after controlling for industry group demand. We are not aware of any evidence of this—including in our data. Even if that were the case however—that $\beta^L > 0$—the magnitude of the effect would have to be large enough to overpower the negative effect of $\beta^H < 0$, which we have precisely estimated. We think such a large effect is unlikely. Hence, we proceed conservatively under the assumption that $\beta^L = 0$—even though the evidence we have suggests $\beta^L < 0$.

[27] The median and mean shocks are very similar, so results are not sensitive to using the median rather than the mean.

during the Great Depression that can be explained by financial distress is at least 5 percent. As we discussed before, this estimate is likely to be a lower bound for the effect of the shock.

We understand the limitations of this exercise, but believe it is a useful starting point for quantifying aggregate effects. In particular, because our data do not capture the roughly 30 percent of US manufacturing activity taking place outside of the 29 cities in our sample, this aggregate calculation is somewhat of a partial equilibrium exercise. If plants in high relative to low external finance dependence industries differentially moved out of our sample cities and into smaller cities or more rural areas, then our calculation could overstate the aggregate manufacturing decline. With manufacturing activity increasingly concentrating in metropolitan areas over the twentieth century and the threat to our calculation requiring plant migration to areas outside of our sample to be differential by external finance dependence, we think this is not a significant concern.

Other Tables and Figures

Table 8: City Characteristics 1929 and Religious Fragmentation

	$Shock_c$	$log(Banks_{29})$	$Man.Growth_{27-29}$	$Man.Growth_{23-29}$	$\%High\,Fin_{29}$
$Rel.Frag.$	0.513***	-0.468	-0.396	-0.566	-0.069
	(0.129)	(1.204)	(0.608)	(0.594)	(0.096)
$Constant$	-0.052	4.869***	0.981**	1.144***	0.705***
	(0.093)	(0.892)	(0.373)	(0.369)	(0.067)
R^2	0.264	0.008	0.015	0.031	0.023
N	29	29	29	29	29

Notes: Each column represents a different city-industry-year level regression of the indicated manufacturing outcome on the indicated covariates. The sample is city level. Manufacturing outcomes are constructed from the US Censuses of Manufactures. High external finance dependence industries are those with above the median (0.06) fraction of total assets in bank loans as of the mid-1920s, according to Moody's *Manuals of Industrials*. Bank shock is defined at the city level as the fraction of the city's 1929 banks that were suspended by 1933, according to FDIC data. Robust errors are applied. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.