Firm Networks in the Great Depression*

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Abstract

We study how firms allocate resources across their constituent establishments in response to local economic shocks in the context of the Great Depression. Using establishment-level data from the Census of Manufactures, we find that establishments are affected by local shocks in the regions where the other establishments making up the same firm are located. In particular, establishment employment is positively affected by positive shocks to the local supply of credit to other establishments that make up the same firm. These results show the important role of firms in the geographic propagation of local economic shocks.

Keywords: Great Depression, Firms, Establishments, Spillovers.

JEL Codes: N12, L6, G30, E30.

Introduction

Firms play a critical role in the allocation of resources in the aggregate through their individual decisions of how to allocate resources to their constituent business units. Some of these decisions, such as where to build a new plant or what product lines to invest in, can have important long-term consequences. Other decisions can have important short-run consequences that determine how the firm's units respond to local business cycle shocks. These decisions, when firms are geographically dispersed, can play a critical role in setting the geographic contours of business cycles. This is a particular puzzle in the Great Depression during which manufacturing employment was highly synchronized across regions (Rosenbloom and Sundstrom, 1999). Yet markets in many ways were highly fragmented geographically due to, for example, legal limits on bank branching across state lines.

We empirically establish the critical role played by firms in generating spatial *spillovers* of local shocks during the Depression. To motivate our empirical strategy, consider the following concrete example: the Alpha Portland Cement Company. One of its nine constituent establishments was located in La Salle, Illinois, about 100 miles from Chicago. In June 1932, Chicago experienced a terrible banking panic. During the week of June 20th alone, 26 Chicago banks failed, and 40 failed during the whole month, even while there was no similar spike in failures at the state or national level. This event would presumably have affected the Alpha establishment located in Illinois directly by, for example, reducing its customers' access to credit. The question we address empirically is whether other establishments located in places as far away as Alabama and New York that made up the Alpha Portland Cement Company were also affected by this event through their connections to the Illinois establishment.[1]

Consider now the firm's decision to allocate resources across its constituent establishments when facing a working capital constraint.[2] The working capital constraint limits the total wage bill of a firm to a fraction of total revenue. We allow for the possibility that the revenue earned by different establishments has different degrees of *pledgeability* as collateral for working capital loans. This assumption can be motivated by thinking of working capital more broadly as including trade credit extended to local wholesalers. In this way, establishments in a given region might be exposed to changes in the local availability of funding for their wholesalers. Holding fixed the total amount of financial resources available in the case of a binding working capital constraint, a firm's constituent network of establishments is valuable in that it allows the firm to exploit local shocks by transferring (financial) resources between establishments located in different regions.

We distinguish between two different shocks: (1) investment opportunity and (2) cashflow. The first type of shock changes the optimal ratio of labor between establishments located

[1]In this paper, we take the existence and structure of these networks as given and do not consider their endogenous formation though this is clearly an interesting and important question. Decker et al. (2016) present a model that could be applied to understand the formation of networks in our setting. It focuses on entry into new markets as a way to diversity market-specific demand shocks.

[2]The exact details of the model are in the appendix.

in different regions holding fixed the amount of resources a firm has. We highlight that only when different establishments that make up a firm are treated differently by local credit markets do investment opportunity shocks generate a negative correlation between employment at establishments within a firm. The second type of shock, which can be interpreted as a shock to the pledgeability of a particular establishment's revenue, changes the total amount of resources a firm has access to while leaving the optimal labor ratio across establishments fixed. As compared to investment opportunity shocks, we find, like Giroud and Mueller (2019), that cashflow shocks always generate a positive correlation in employment across establishments making up the same firm.

With this theoretical motivation, we construct an establishment-level dataset of 25 industries from the Census of Manufactures taken in 1929, 1931, 1933, and 1935. These industries represent just under 20% of all manufacturing revenue at this time. We link the establishments into their constituent firms. We focus on "horizontal" firm networks. That is, networks made up of establishments that are part of the same industry selling a "similar" product that is geographically differentiated.[3] As an example, the cement industry is comprised of firms operating geographically dispersed establishments producing nearly identical physical products but differentiated spatially due to the high transportation costs.

We exploit the fact that before the Banking Act of 1933, the regional Federal Reserve banks had a degree of autonomy in how they set monetary policy and, most critically, discount lending policy in their region. These differences were not simply differences in discount rates, but also potentially in terms of what kinds of assets could be used as collateral at the discount window. These differences in policy, which in many cases were ideologically driven, had major impacts on the supply of credit in a local area. For example, work by Richardson and Troost (2009) show how policy differences between the St. Louis and Atlanta Federal

[3] There are, of course, other types of within firm connections such as one in which an establishment produces an intermediate good used by another establishment in the same firm. This vertical dimension between establishments within a firm introduces a whole other set of bargaining and hold-up issues that we avoid by focusing on this particular type of network structure.

Reserve banks led to dramatic differences in the number of bank failures across the state of Mississippi following the collapse of the bank Caldwell & Co. in November 1930.

A long literature has attempted to identify the effect of local credit shocks on local economic conditions during the Great Depression. For example, Calomiris and Mason (2003) and Lee and Mezzanotti (2017) both identify negative effects from local credit market breakdowns. Amir-Ahmadi et al. (2021) estimate structural VARs using data on the 12 reserve districts and exploiting the variation in discount rates to identify the effects of monetary policy on economic activity. Using a number of measures of local credit supply including the quantity of bills discounted by the regional Federal Reserve branch and the number of banks terminally suspended, we find a similar result that an expansion in the local supply of credit increases employment. As an example, a 10% increase in the quantity of bills discounted by the regional Fed is associated with an increase in employment of 0.5%.

What is novel about our results is that these shocks to the local supply of credit have spillover effects. Consistent with an interpretation of these credit shocks operating as cash-flow shocks, the "other" effects of the credit shocks are of the same (positive) sign. They are also of a similar magnitude as the "own" effect emphasizing the quantitative significance of these spillovers. Besides using standard methods of statistical inference, we quantify the statistical uncertainty using a variation of a Fisher randomization test. The idea of this test is to generate the distribution of estimates assuming firms were randomly assembled from establishments and to compare the actual estimate to this distribution.

As an extension, we also examine whether local *demand* shocks spillover inside of a firm. We take local retail sales as a proxy for local demand similar to how Giroud and Mueller (2019) treat housing price shocks in the Great Recession (Mian et al., 2013). We find similar to the results for the local credit supply measures. That is, increases in retail sales at other establishments within the same firm have positive spillovers. This suggests that changes in retail sales also operate like cashflow shocks relaxing a firm's overall budget constraint.

This result is somewhat surprising since a change in relative demand across regions would presumably change the optimal relative allocation of resources across regions.

The existence of these spillovers highlights the critical role played by firms in shaping the geography of the Great Depression. Consider again the case of the banking panic in Chicago in June 1932. Based on our results, not only would have the establishments making up Alpha Portland Cement Company located in the area been negatively affected by the panic, but also the establishments in that firm located in Alabama over 700 miles away. From the viewpoint of our model, this spreading out of the effects of the local credit shock does not reflect an amplification of the initial shock, but rather a form of risk sharing within a firm. Establishments directly impacted by a shock to local credit supply are better off in terms of higher employment if they are connected to an establishment not located in the same area. For this reason, our results do not help to explain the depth of the Great Depression, but they do help to explain the regional comovement in economic activity.

Besides relating to the literature on the Great Depression, our paper also relates to a literature in corporate finance that studies the functioning of firms' so-called internal capital markets. Theory has identified costs and benefits of these "markets" relative to external, arms-length capital markets. For example, Stein (1997) highlights the benefits of these markets by allowing firms to engage in "winner picking" for particularly productive projects. On the other hand, Scharfstein and Stein (2000) emphasize the costs in the form of potential rent seeking by managers of the various projects. An ample literature has attempted to empirically identify these costs and benefits (Shin and Stulz, 1998; Lamont, 1997; Rajan et al., 2000; Schoar, 2002; Maksimovic and Phillips, 2002; Gomes and Livdan, 2004).[4] Our model focuses on the positive aspects of these internal capital markets as there are no (internal) inefficiencies in a firm's allocation of resources. Instead, the existence of this network allows a firm to insure against local shocks that directly affect its constituent establishments.

[4]See Stein (2003) and Phillips and Maksimovic (2007) for more thorough literature reviews.

Historical Background

In this section, we sketch out the institutional background of firm finance and the role of regional Federal Reserve banks during the Depression to provide support for how we conceptualized the decision faced by a firm in allocating its resources across its constituent establishments. The central assumption we make is that, while the firm faces a single working capital constraint, the revenue generated by each of its establishments has a different degree of *pledgeability*. The notion of pledgeability captures the idea that as a function of local credit conditions, one dollar of revenue might be worth more or less as collateral for a working capital loan.

In general, goods were sold under the "cash discount open account system" (Willis and Steiner, 1926, p. 214). In this framework, a seller (in our case, the manufacturer) quotes terms of sale. These terms generally included a certain period in which full payment is required and another shorter period (usually 10 days) during which a specified cash discount (say 2%) is granted. These terms were usually standard within an industry, though across industries there was considerable variation, based on characteristics of the industry such as the seasonality of demand or perishability of goods. Wholesalers, when not offering cash as payment, would offer "paper" promising payment at some future date. Manufacturers, accepting that paper, could have it "discounted" to get cash immediately from a local bank. That paper in turn could be "rediscounted" by the regional Federal Reserve banks to provide cash to the bank itself through the discount window.[5] In this way, the regional Federal Reserve banks helped to provide credit to support the working capital needs of businesses.

The dominant banking theory of the time drew a distinction between loans for "commercial" operations, which *were* eligible for support from Reserve banks through rediscounting,

[5]In the case when the local or country bank was not a member of the Federal Reserve, there was an additional credit intermediation step between the local banks and their so-called correspondent banks, which were actually members of the system. The correspondent banks would be the ones who discounted the paper of the local banks and then rediscounted it at the discount window.

and loans for "investment", which *were not*. Commercial banks were only supposed to be involved in the former, while investment banks were only involved in the latter. Commercial banks often went so far as to conduct annual "clean ups" of their portfolios to show they were not involved in investment lending (White, 2003). For a loan to be considered commercial, it needed to either be "employed to acquire readily salable good" or used "to further some definite stage of production, distribution or manufacture" (Willis and Steiner, 1926, p. 152). A loan's commercial purpose was critical for determining whether it was "eligible" or "acceptable" to be rediscounted by the Federal Reserve. While Reserve banks had "discretion in taking the steps necessary to satisfy itself as to eligibility" of a note (Willis and Steiner, 1926, p. 178), the criteria for eligibility should "not [be] the character of the borrower's business but the use of the proceeds of the particular instrument in question" (Willis and Steiner, 1926, p. 219). This view empathized the function (and term) of the paper as the key criteria for whether it was eligible.[6] For example, the paper of a utility to purchase coal would be eligible, and it should not matter the identity of the utility.

Even on this seemingly straightforward question of whether funds were being used for commercial as opposed to investment purposes, the policies of the Reserve banks allowed for virtually unlimited discretion in practice. Rather than tests based on financial statements for whether banks had used and would use the proceeds of a discount loan for commercial use, instead use was indicated simply by showing "a reasonable excess of quick assets over current liabilities" (Willis and Steiner, 1926, p. 206). With the Banking Act of 1933 (also known as the Glass-Steagall Act), Congress ended the figment that discount lending had to be for commercial purposes and granted the Federal Reserve emergency authority to lend to member banks on any "good" asset. It made this authority permanent with the Banking Act of 1935. At the same time, the Banking Act of 1933 required the Reserve banks to keep themselves appraised of whether loans were being used for improper (that is, speculative) uses and to take this information into account when deciding whether or not to rediscount

[6]This policy was, in effect, the "Real Bills Doctrine" of monetary policy

(Whitney, 1934).

Beyond discretion on whether a discount loan was for the proper use, regional Federal Reserve banks could and did exercise discretion over the strictness of their lending standards. In a 1931 questionnaire, Atlanta and Boston admitted to varying their standards based on individual circumstances of the borrowers (Whitney, 1934). This was partially driven by the fact that the Board of Governors specifically ruled that paper could not be rediscounted "in any case where the ultimate payment of the note is dependent upon the success of the transaction giving rise to the note" (Willis and Steiner, 1926, p. 218). In the view of Willis and Steiner (1926, p. 219), this ruling was inconsistent with a definition of eligible paper based solely on its function. As a practical matter, this inconsistency did not matter since the Reserve banks were under no obligation to rediscount all eligible paper. Reserve banks could refuse to rediscount paper on the grounds that the member banks had not conformed to the policies of the Federal Reserve (Whitney, 1934). As Willis and Steiner (1926, p. 206) put it, "while some [Reserve banks] are strict, others have taken almost all eligible paper offered them." One way in which Reserve banks controlled the volume of discount lending was by requiring additional or "marginal" collateral. For example, through the summer of 1931, the St. Louis Fed required *double* collateral that consisted of the eligible paper required by law plus an equal amount of U.S. government securities. As Westerfield (1932) stated, "in the requirement of additional collateral there [was] no uniformity among the Federal Reserve banks."

While policies like requiring marginal collateral seem like somewhat *ad hoc* ways to control the volume of discount lending, eight of the twelve Reserve banks considered managing the volume directly through these tools a superior policy to indirectly managing the volume by managing discount rates (Whitney, 1934). The Reserve banks felt that member banks did not often pass on changes in the discount rates to customers, and, for that reason, changes in the discount rate did not necessarily have the effects intended by the Reserve banks. This suggests that perhaps a better measure of the stance of monetary policy at the regional level during this period is the total volume of discount loans rather than the discount rate itself.

A specifically important question for us was how the Reserve banks treated paper involving a borrower that was part of a much larger firm, not necessarily headquartered in the region. Our model assumes, in effect, that the Fed did not treat establishments that were part of the same firm as necessarily identical. It is hard to get a clear answer on whether this was actually the case. For example, the Board "require[d] that the statement of a borrower who has closely affiliated or subsidiary corporations or firms shall be accompanied by separate statements of the latter, unless his own statement clearly indicates that the 'note is both eligible from a legal standpoint and acceptable from a credit standpoint'" (Willis and Steiner, 1926, p. 181). This seems to suggest the regional Feds needed to consider the firm as a whole and not the individual establishment itself. But the same policy of the Board "authorized the Reserve banks to waive the requirement until May 1, 1925, in any case where separate financial statements have not been issued prior thereto" (Willis and Steiner, 1926, p. 181), which points in the other direction.

However the regional Reserve bank treated a borrower that was part of a larger firm, it was the case that if local banks were unwilling to accept the paper from a wholesaler for discount, manufacturers ended up being in effect lenders to their wholesalers. In this way, even manufacturers that were part of a larger firm were potentially sensitive to local credit conditions and the decisions made by the regional Federal Reserve banks. In this way, it was not so much the credit quality of the manufacturer that mattered but the quality of their wholesaler. This is why we are willing to make the simplifying assumption that the manufacturer itself, in effect, faced the working capital constraint of its wholesaler. As a consequence, it is plausible that there were differences in the pledgeability of revenue across establishments within a firm.

These differences across Reserve banks in terms of what counted as eligible paper and the amount of collateral required had enormous consequences for the banking system during the early years of the Great Depression. Richardson and Troost (2009) show how the stricter lending policies of the St. Louis Fed relative to the Atlanta Fed led to much higher rates of bank failures in the part of Mississippi covered by the St. Louis Fed following the collapse of the bank Caldwell & Co. in November 1930. Extending that work, Jalil (2014) finds that bank failures were lower in the Atlanta region along its whole border with other Federal Reserve regions in the first years of the Depression. Carlson et al. (2011) highlight another example where the aggressive discount lending actions of the Atlanta Fed were essential to stopping a panic in Florida in 1929.

Data

We use establishment-level data from the Census of Manufactures (CoM) covering 25 industries taken in 1929, 1931, 1933, and 1935. This data source provides a detailed picture of manufacturing establishments over the course the first half of the Depression.[7] Table 1 provides some summary statistics on our sample by industry.[8] While originally collected for a variety of purposes, we would argue that the sample as a whole reflects the broad contours of the manufacturing sector in this period. As we document in the appendix, the sample covers a sizable portion of manufacturing establishments (about 10% of the total), wage earners (about 18%), and revenue (about 20%), all non-trivial fractions.[9] Second, we have a variety of types of industries from "high tech" ones of the day such as aircraft and radios, to durable goods producers such as cement and steel, to non-durable good producers such as ice cream and manufactured ice. Finally, Benguria et al. (2020) conduct some formal tests

[7]The CoM was also taken in 1937 but the establishment-level schedules do not still exist as far as we know. In fact, these 4 years are the only years between 1880 and 1963 for which the establishment-level schedules are still extant.

[8]For a detailed discussion on the representativeness and quality of our sample, we refer to the paper by Benguria et al. (2020). The source as a whole is discussed in greater detail in Vickers and Ziebarth (2018).

[9]We thank David Donaldson, Richard Hornbeck, and James Lee for providing the transcribed published tables that we use to benchmark our sample.

comparing the industry-level characteristics of industries in the sample to those not included. They find no statistically significant differences in number of wage earners, revenue, wages, and revenue per worker. They also show that the sample is not tilted toward counties with relatively higher bank failure rates or larger declines in retail sales between 1929 and 1933.

While providing in many respects more detailed information than the modern CoM, this source does have a few important limitations that shape our empirical analysis. First, the CoM in these years lacks information on investment and the value of capital. These pieces of data are available in the modern CoM and the 19th-century ones. This limitation prevents us from focusing on these variables as much of the literature on the internal capital markets has. The second major data limitation of the CoM from this period is a dearth of any information on the financial position of the establishments or their parent firms. The CoM provides nothing about debt outstanding, when that debt matures, equity, or surplus held in reserve. This precludes us from examining whether our results differ based on the financial strength of the firms.

Because of these limitations, we will focus on quarterly establishment-level employment as our dependent variable. This is similar to the dependent variable in Giroud and Mueller (2019). The CoM asked for a breakdown of employees into wage and salary earners. As discussed in Vickers and Ziebarth (2018), this distinction between salary versus wage earners is similar to the modern CoM distinction between non-production and production workers. For the wage earner category, the CoM furthermore asked for this count at a monthly frequency. We aggregate these monthly counts to the quarter to smooth out some of the high-frequency fluctuations. We will also make use of the information provided on total wages paid over the course of the whole year in weighting establishments within a firm.

Unlike the modern CoM, the Census Bureau at this time did not provide establishment or firm identifiers. This makes it nontrivial to link the same establishment or firm over time. It also makes it difficult to link the group of establishments that make up the same firm in the cross-section. Therefore, we construct establishment and firm links "by hand." In the case of establishments, we use mainly the address of an establishment, which should remain fixed over time, and, to "break ties," we use names of the establishment and its parent firm.[10] For firm identifiers, we use mainly the name of the parent company listed on the establishment schedules supplemented with external sources on particular industries. As one small point, a firm is defined as having multiple establishments if, at any point in a year, it has more than 1 establishment in operation. This status could change from census to census if establishments within a firm open or close, but MP status is fixed within a particular year.

Given the limitations in the linking process, it is important to identify what types of linking errors could bias our results and in what direction. To look ahead, our main regressions will use quarterly variation in employment as the dependent variable and the economic conditions of the regions where other establishments making up the same firm are located as the key independent variable. So an error in linking an establishment *within* a particular year to its parent firm can potentially generate biases in the regressions. This means that changes in a firm's name over time will not be problematic since we really only need the name to be consistent within a year.[11] What is potentially problematic are common-sounding firm names that make it difficult for us to tell if two seemingly similar names are actually referring to the same firm. In these cases, the likely linking error is to "overgroup" establishments creating overly large firms. Errors of this type would make it more difficult for us to identify the effects of firm network linkages since, by assumption, these links do not exist between establishments incorrectly grouped into a firm.[12]

One limitation of the constructed firm identifiers is that they identify establishments comprising a firm located *within a single industry*. Because we do not have the whole universe

[10]For the cement industry, Chicu et al. (2013) were able to construct establishment identifiers using directories from the portland cement trade group, the Cement Institute.

[11]This issue will potentially affect the estimated standard errors since we will cluster on firm (among other variables). However, these errors will not affect the point estimates.

[12]To get a sense of the magnitude of the potential bias here, we conduct a placebo test where we randomly assign establishments to firms and rerun the regressions. We then compare our estimated effect to the counterfactual distribution of estimates.

of manufacturing establishments, we are not able to identify establishments owned by a particular firm that fall outside of the industries in our sample. For example, while we have information on establishments that do the final assembly of automobiles, we do not have information on all of the industries that produce inputs into the production of cars (though we have some like tire producers). At this time, the Ford Motor Company was highly vertically integrated, even attempting to run its own rubber plantation in Brazil (Grandin, 2010). All of these other far-flung establishments owned by Ford will not be in our sample. We do not think this lack of information on establishments outside of these industries is necessarily problematic. Even if all of our results focusing on the horizontal allocation of resources are really just reflections of vertical relationships within the firm, it is still the case that firm networks are important. It is just a different type of connection that matters. In any case, understanding the decision of how a firm allocates its resources in the presence of these vertical relationships is interesting in its own right and something we leave for future work.

Table 2 reports the importance of MP establishments across our set of industries pooling all 4 years. There is considerable variation across the industries in the relative importance of establishments that are part of MP firms. The fraction of MP establishments ranges from 2% in macaroni all the way to 68% in bone black. The range is even larger if we consider revenue or employment percentages that range from 2% in macaroni to almost 79% in blast furnaces. Finally, we note that, for all the industries, MP establishments command more than a proportional share of employment and revenue relative to their share of establishments. This suggests differences, at least in terms of the average size, between these two types of firms.

One final point to keep in mind is that the industries differ in their degree of "aggregation" and whether establishments produce multiple products. The Census Bureau at the time did not use a detailed hierarchical system like SIC codes to organize industries. Some of the industries, such as manufactured ice, macaroni, cement, sugar refining, malt, bone black, and cane sugar, are very narrowly defined and consistent over time with establishments in these industries tending to make only one product with little product differentiation. On the other hand, the remaining industries are closer to 3-digit SIC codes with many establishments producing a variety of products. For example, establishments in the agricultural implements industry made reapers, tractors, and thrashers, among other pieces of machinery. In fact, we actually created the radio industry ourselves by identifying establishments that manufactured radios from the broader industry of producers of electrical equipment. Given we are not using these industry categories to define, for example, the set of competitors, the broadness of the categories is not particularly problematic. What is potentially problematic is that industry fixed effects will not really control for what we want them to control for, such as industry-specific seasonal patterns.[13]

Empirical Strategy

Our empirical strategy uses geographic and temporal variation in local economic conditions to identify the effects of firm networks, similar to the work of Giroud and Mueller (2019). Although we have mapped establishments to the counties where they are located, the data on retail sales and credit supply is not as geographically fine. When working with data from this period, there is a tradeoff between geographic detail and sampling frequency. Datasets at a higher frequency tend to be reported at larger geographic units. In our case, the data on credit supply and retail sales are available by quarter, but only geographically at the level of the Federal Reserve district.[14] While it would be helpful to have geographically finer data, there is a risk in using too fine of a geographic unit if that unit does not actually reflect the "true" extent of an establishment's product or credit market.

To measure spillovers of conditions faced by *other* establishments inside the firm, we con-

[13]That said, it is not obvious how this bias would lead us to overestimate (or underestimate for that matter) the magnitude of spillovers from other establishments making up the same firm.

[14]Details regarding the source and construction of this retail sales index are discussed in Park and Richardson (2011).

struct a measure of "other" conditions that summarizes the conditions for all the other establishments that make up a firm. We have to decide on how to weight the local economic conditions at the various establishments inside a firm. Following Giroud and Mueller (2019), we construct an employment-weighted average of the economic conditions in regions where other establishments part of the same firm are located. Concretely, define the weighted "other" measure of either local credit supply or demand X_{it}^{Other} for establishment i that is part of firm f at time t as

$$X_{it}^{\text{Other}} = \sum_{j \in f, j \neq i} \frac{E_j}{\sum_{j \in f, j \neq i} E_j} X_{jt},$$

where E_j is the employment *in the previous quarter* at establishment j and the sum is over all establishments in firm f except for establishment i. We use lagged employment to eliminate a simultaneity issue that would arise if we used contemporaneous employment. As a consequence, we effectively drop observations from the first quarter of each year since the biennial nature of the census means we do not have a previous quarter for these establishments. We experiment with different weights to construct the other variable including weighting by revenue and equally weighting establishments.

Letting Emp_{it} be log quarterly employment[15] at establishment i and time t, we estimate the following regression on the set of MP establishments:

$$Emp_{it} = \beta^{\text{Own}} \cdot X_{it}^{\text{Own}} + \beta^{\text{Other}} \cdot X_{it}^{\text{Other}} + \text{Controls}_{it} + \varepsilon_{it},$$

where X_{it}^{Own} is either the retail sales index or a measure of local credit supply in establishment i's own region at time t. We estimate the specifications with the following sets of fixed effects in Controls_{it}: (1) Federal Reserve district specific seasonal trends and industry-specific seasonal trends (Fed $\times$ Quarter $+$ Ind $\times$ Quarter), as well as Federal Reserve district, industry,

[15]Technically, we add 1 before taking the log to handle cases of 0 employment in a given month. This happens in a minor number of cases.

and quarter fixed effects and (2) Federal Reserve district by industry-specific seasonal trends (Fed $\times$ Ind $\times$ Quarter), as well as Federal Reserve district, industry, and quarter fixed effects.

A Placebo Test

To quantify the extent of statistical uncertainty in the regressions, we propose the following placebo test. First, we randomly assign establishments to firms. Then we calculate the other conditions based on this random assignment, and finally, we rerun the regression specifications. In particular, for a given year and industry, we take as given the number of firms and the number of establishments for each firm. Let the set of establishments be denoted by E and the set of firms F. Define the assignment rule $f : E \to F$ as the (surjective) function that assigns to each establishment its firm. We then draw a random permutation of the establishments σ and apply the rule $f \circ \sigma$ to randomly reassign establishments to firms maintaining the same distribution of firm sizes. With this artificially generated dataset, we rerun our main specifications and collect the point estimates of the own and other effects. We repeat this process 100 times and compare our actual estimate to the distribution of generated effects.

This placebo test can be thought of as a test of the quality of our matching of establishments to firms. If our firm matching was no better than random, then the actual effects should not be much different than the effects based on an actually randomly generated set of firms. If we assume that the errors in our firm matching are relatively small, then we think of this test as akin to a Fisher randomization test for the existence of firm network effects. The basic idea of these randomization tests is to randomly permute treatment and control status and compare the generated density of treatment effects to the estimated one.[16] In our case, we think of

[16] A slightly different version of our test would be to simply permute the values of the "other" variables between establishments. This version would be identical to our version in the case when firms consisted of, at most, 2 establishments. When firms consist of more than 2 establishments as is the case in reality, our test considers a wider set of hypotheticals since we do not require the generated distribution of the other variable to have the same marginal distribution as the actual one.

the firm network as the treatment that we randomly permute between establishments. The upshot of such a test is that it allows for conducting statistical inference without having to rely on asymptotic approximations in the form of the central limit theorem.

Unlike the simplest Fisher randomization tests, there are many plausible counterfactual distributions of the esteemed effects that could be constructed in our setting. In one direction, we could construct a distribution with more "balancing" on covariates. In particular, we could restrict the set of permutations to also respect the geographic distribution of establishments within firms. For example, if an industry was composed of two firms with two establishments each with an establishment in both regions A and B, then this balancing requirement would impose the restriction that any artificially generated firm would have establishments in both regions. In the other direction, one could imagine, for each iteration, drawing a random number of firms and a distribution of establishments across those firms completely at random. We choose what we consider a middle route in terms of the constraints we impose on the marginal distribution of firm characteristics in constructing the randomly generated distributions. On the one hand, we do want to rule out what we think are implausible hypotheticals such as an industry made up of one firm owning all the establishments. At the same time, we do not want to rule out all hypotheticals that change the distribution of establishment observables across firms.

Results

Sensitivity to Other Local Credit Conditions

We examine whether there were spillovers of local credit conditions similar. In some sense, the closest analog to a cashflow shock (a change in the parameter κ) in our model is a change in the haircut regional Federal Reserves applied to collateral presented by banks for discount loans. Unfortunately, we do not have systematic evidence on these haircuts. Instead, we assume that we can proxy for haircut using the total volume of discount loans. Such an assumption is consistent, at least, with the behavior of the St. Louis and Atlanta reserve banks during the Caldwell crisis in the early 1930s. In particular, Richardson and Troost (2009) show that St. Louis not only restricted discount loans but also increased the haircut it applied while Atlanta did just the opposite.

Table 3 shows that the total volume of bills discounted in an establishment's own region has a strong positive association with employment. A 10% increase in bills discounted is associated with about a 0.46% increase in employment in the baseline specification when weighting by employment, 0.30% when weighting by revenue, and 0.65% when equally weighting establishments. We do not ascribe a causal interpretation to this relationship. It could be that changes in monetary policy as reflected in the quantity of bills discounted are really driving employment. It could also be that, in line with the Real Bills Doctrine, monetary policy is simply responding in a pro-cyclical fashion to the changes in the demand for credit as proxied by employment. The table also shows that this "own" effect is robust to whether we include a richer set of fixed effects that includes interactions between Federal Reserve district, industry, and calendar quarter.

More interesting than the own effect is the fact that the effects of the volume of discount lending by the regional Federal Reserve banks spills over through establishment networks inside of firms. Column 1 shows that a 10% increase in the quantity of bills discounted in other areas where the establishment's parent firm is located is associated with a statistically significant increase in employment of 0.44%, a non-trivial amount. It is plausible to interpret the other effect causally rather than as a reflection of reverse causality. There is no reason to think that employment in one region could affect credit conditions in another geographic region.

The estimated other effect is robust to how we weight other establishments and whether we include additional controls.[17] The fact that the own and other effects have the same sign is consistent with the interpretation that a change in the volume of bills discounted functions like a cashflow shock. Figure 1 shows the results of the placebo test where we plot the distribution of simulated other effects for total bills discounted. We find that the actual effect is greater than all the simulated effects and approximately 5 times greater than the mode of the simulated effects. We note that the distribution is not symmetric at 0 as might have been expected given we are randomly creating firms.

We now examine a different measure of local credit conditions as proxied by the volume of distressed loans held by private banks.[18] Increases in this variable reflect a *decline* in the local (private) supply of credit. Like the quantity of bills discounted, we do not argue that changes in this measure are completely exogenous. Table 4 shows that an increase in distressed loans in an establishment's own area is associated with lower levels of employment. Of more interest is that there is a negative spillover of distressed loans of similar magnitude to the own effect. These results are robust to weighting and including additional controls.[19] The magnitudes of these effects are smaller than for the effects of the volume of discount lending though similar to those found in other papers that address the effects of bank failures, e.g., (Lee and Mezzanotti, 2017). Figure 2 shows the simulated other effects. We find that the actual effect is larger in magnitude (more negative) than 95% of the simulated effects with a magnitude 33% greater than the mode of the simulated effects.

Finally, Table 5 reports the spillover effects of the number of banks that were terminally suspended in the quarter. We again find consistent evidence for negative spillovers. Based on Column 1, a 10% increase in the number of banks that were terminally suspended in an establishment's own region decreases its employment by 0.69% while a 10% increase in the other version of this variable decreases employment by 0.44%. The magnitude and statistical

[17] Note that these data on only go through 1933.

[18] We add one to this measure before applying the log transformation to this variable.

[19] In the appendix, we show that results are similar if we use other measures of local credit supply including, among other things, the discount rate and deposits in suspended banks.

significance of these results are stable as we vary the weights used to construct the other variable and include the additional controls. Figure 3 shows the simulated other effects and that the actual effect is greater in magnitude than 89% of the simulated effects.

Sensitivity to Other Local Demand Conditions

As an extension, we now consider whether there were spillovers of local demand conditions. We prefer to focus on the credit variables for a few reasons. First of all, it is not totally clear to what extent some of these industries such as automobile manufacturers are really dependent on *local* as opposed to national demand. We attempt to address this concern later by focusing only on the industries that have a statistically significant own effect. Second, and more critically, it is very hard to interpret these changes in local retail sales as exogenous. Now as we argued earlier, it is not absolutely critical that the variation in the own variable be exogenous, but if the variation is exogenous, that makes the key identification assumption that the variation in the other variable is exogenous highly plausible. We believe, based on the narrative evidence provided, that it is more plausible to think that the variation in the credit-related variables is exogenous as compared to the retail sales index. Therefore, we find the results using the credit variables as more likely causal than the results using the retail sales variable.

Table 6 reports the estimates of the effects of own and other retail sales for a variety of weighting variables and specifications. The specifications in the odd columns include Federal Reserve district-specific seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and quarter-of-year fixed effects. To attempt to address the issue of endogeneity of retail sales, as an alternative specification, we estimate in the even columns a fully saturated model, which includes a fixed effect for every possible interaction between year, quarter, industry, and Federal Reserve district. In this model, the own effect will be absorbed by the fixed effects.

Focusing on the results using employment as the weighting variable in the first two columns, we find that the baseline association between employment and the own retail index has the "correct" positive sign, meaning increases in retail sales in an establishment's own region are associated with higher levels of employment. Again we do not want to attribute a causal interpretation to this association. Next, we find that an establishment responds to the demand conditions of establishments in other locations that are part of the same firm. The other effect is almost 1.5 times larger than the own effect. Column 2 shows that this strong spillover is still present in a fully saturated model in which the own effect is absorbed by the fixed effects, making it slightly difficult to judge the magnitude of this coefficient.

The remaining columns 3-6 run the same specifications using different weights. Columns 3 and 4 use revenue as the weight while columns 5-6 equally weight establishments. The results also show a strong statistically significant positive effect of other retail conditions though the magnitude of this is slightly smaller in the case of weighting by revenue. These results show that the choice of the weight variable in constructing the other conditions is not crucial for the results. Neither is whether we include a full set of fixed effects absorbing the own effect in the process.

Figure 4 shows the results of the placebo test in which we randomly permute establishments between firms. We plot the distribution of generated regression effects based on 100 simulations as well as the actual estimates for the other demand conditions from Column 1 of Table 6. Note that we do not permute an establishment's own local conditions. So all of the variation in simulated effects comes from the variation in other conditions and its correlation with the own effects. We find that the mode of the simulated effects is close to (though not symmetric around) 0. This is sensible since the other variable should be uncorrelated with employment and any common region shocks should be absorbed by the region fixed effects. The actual estimate of the other variable we find is greater than all the simulated estimates, emphasizing its high level of statistical and economic significance.

As noted above, obviously, not all of these industries sold their goods locally so to what extent changes in local retail sales are really relevant differs by industry. There are a number of ways to identify whether an industry depends on local or aggregate demand, including such measures as the value-to-transportation cost ratio. We take a more "data-driven" approach to identify these locally "sensitive" industries defined as ones that have a statistically significant association at the 5% level with retail sales in our base specification pooling all years. Clearly for regressions focusing on this group of industries, the statistical and economic significance of the own effect is not of interest since we are screening for a significant value here, but again this is not our parameter of interest.

Table 7 redoes the same set of specifications for the industries that we find to be sensitive to local demand. Note this sensitivity is defined using all establishments, not simply MP establishments. By restricting attention to this set of industries, we lose about a quarter of the sample from the previous regressions. Even with this smaller sample size, across specifications and choices of the weighting variable, we still find strong positive spillovers of other conditions, and, if anything, stronger than those including all the industries.

How then should we interpret these results through the lens of the model? The fact that the effect of other conditions has the same sign as the own conditions suggests that changes in retail sales act more like a cashflow shock than an investment opportunity shock. As we mentioned, it is possible for an investment opportunity shock to generate this pattern, but, in most cases, this type of shock would lead to the own and other effects having opposite signsdue to differences across establishments in the pledgeability of revenue.

Conclusion

"Big" firms matter, and they matter, in particular, for business cycle fluctuations. As one historical example, there is evidence that a shock impacting labor costs driven by a unionization push in the automobile industry and, in particular, the Big 3 automakers caused the recession in 1937 (Hausman, 2016). We studied one dimension of bigness as defined by whether a firm owns multiple establishments. To identify the effects of these networks of establishments during the Depression, we collected an establishment-level dataset from the CoM and linked establishments to their parent firms. We found that local shocks to credit and demand spilled over between geographically separated establishments that make up the same firm.

Going forward, as we mentioned in the introduction, one salient feature of the Depression was the geographic synchronicity in, for example, manufacturing employment across regions of the county. This was despite the fact that there were many ways in which markets were anything but integrated including limits on bank branching. In light of that, our results suggest that these firm networks can provide a micro-foundation for how local shocks can end up looking like aggregate ones and potentially explain the geographic synchronicity. In ongoing work, we are working to extend the static model in a dynamic and quantitative direction. The goal of this richer model is to quantify what fraction of the spatial correlation in economic outcomes over this period is due to these firm networks. We leave this for future work.

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Table 1: Summary Statistics of the Sample Industries

Industry	Establishments	Log Employees	Durable
Beverages	5,153	1.115	0
Ice cream	3,156	1.308	0
Ice, Manufactured	3,584	1.405	0
Macaroni	312	2.043	0
Malt	28	2.850	0
Sugar, Cane	70	2.399	0
Sugar, Refining	21	6.345	0
Cotton Goods	1,280	5.115	0
Linoleum	7	6.526	1
Matches	21	4.675	0
Planing Mills	4,816	2.238	1
Bone Black	65	2.943	0
Soap	282	2.077	0
Petroleum Refining	389	4.000	0
Rubber Tires	90	5.043	1
Cement	172	4.770	1
Concrete Products	2,429	1.462	1
Glass	263	4.935	1
Blast Furnaces	105	5.217	1
Steel Works	486	5.764	1
Agricultural Implements	280	3.215	1
Aircraft and Parts	131	3.330	1
Motor Vehicles	245	4.678	1
Cigars and Cigarettes	49	3.768	0
Radio Equipment	334	3.906	1

Notes: All statistics are calculated for 1929. The "Establishments" column is the total number of establishments. The "Log Employees" column is the average number of log employees across establishments. The "Durable" column is whether we coded an industry's product as durable.

Table 2: Relative Importance of MP Establishments by Industry

	Percentage in an MP firm of...		
Industry	Revenue	Employment	Establishments
Beverages	10	6	3
Ice cream	24	23	7
Ice, Manufactured	60	55	44
Macaroni	2	2	2
Malt	21	28	32
Sugar, Cane	27	38	16
Sugar, Refining	40	40	33
Cotton Goods	43	43	31
Linoleum	38	43	57
Matches	48	47	29
Planing Mills	15	14	9
Bone Black	74	70	68
Soap	56	59	9
Petroleum Refining	78	77	48
Rubber Tires	11	12	7
Cement	65	68	58
Concrete Products	22	19	10
Glass	56	52	37
Blast Furnaces	79	74	57
Steel Works	77	71	43
Agricultural Implements	74	75	13
Aircraft and Parts	55	63	15
Motor Vehicles	74	60	28
Cigars and Cigarettes	62	49	31
Radio Equipment	38	41	10

Notes: These numbers are percentages of industry totals in 1929 by MP status which is defined based on whether a firm operates more than 1 establishment at any point in a given year.

Table 3: Effects of Own and Other Bills Discounted

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Bills Discounted	0.058	0.054	0.036	0.044	0.076	0.071
	(0.014)	(0.014)	(0.011)	(0.011)	(0.014)	(0.014)
Other Bills Discounted	0.031	0.034	0.054	0.044	0.012	0.017
	(0.013)	(0.013)	(0.008)	(0.008)	(0.013)	(0.013)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	30058	30040	30212	30195	30354	30337

Notes: These data are at a quarterly frequency at the Federal Reserve district. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. The additional controls include Federal Reserve district by industry seasonal trends, as well as Fed district, industry, and quarter fixed effects. We restrict attention to establishments that are part of a MP firm. These data only go through 1933.

Table 4: Effects of Own and Other Distressed Loans

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Distressed Loans	-0.011	-0.014	-0.011	-0.013	-0.014	-0.015
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Other Distressed Loans	-0.031	-0.027	-0.030	-0.028	-0.027	-0.025
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	20884	20864	21087	21068	21139	21120

Notes: These data are at a quarterly frequency at the Federal Reserve district. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. The additional controls include Federal Reserve district by industry seasonal trends, as well as Fed district, industry, and quarter fixed effects. We restrict attention to establishments that are part of a MP firm. An increase in this credit measure should be interpreted as a *decrease* in local (private) credit supply.

Table 5: Effects of Own and Other Number of Banks Terminally Suspended

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Banks Terminally Suspended	-0.053	-0.056	-0.051	-0.052	-0.060	-0.060
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
Other Banks Terminally Suspended	-0.060	-0.058	-0.060	-0.061	-0.050	-0.052
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	20884	20864	21087	21068	21139	21120

Notes: These data are at a quarterly frequency at the Federal Reserve district. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. The additional controls include Federal Reserve district by industry seasonal trends, as well as Fed district, industry, and quarter fixed effects. We restrict attention to establishments that are part of a MP firm. An increase in this credit measure should be interpreted as a *decrease* in local (private) credit supply.

Table 6: Effects of Own and Other Local Demand

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Retail Index	0.178		0.372		0.155	
	(0.068)		(0.039)		(0.072)	
Other Retail Index	0.347	0.363	0.140	0.193	0.369	0.368
	(0.062)	(0.067)	(0.023)	(0.027)	(0.067)	(0.073)
Fully Saturated?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	39372	38980	39557	39160	39736	39338

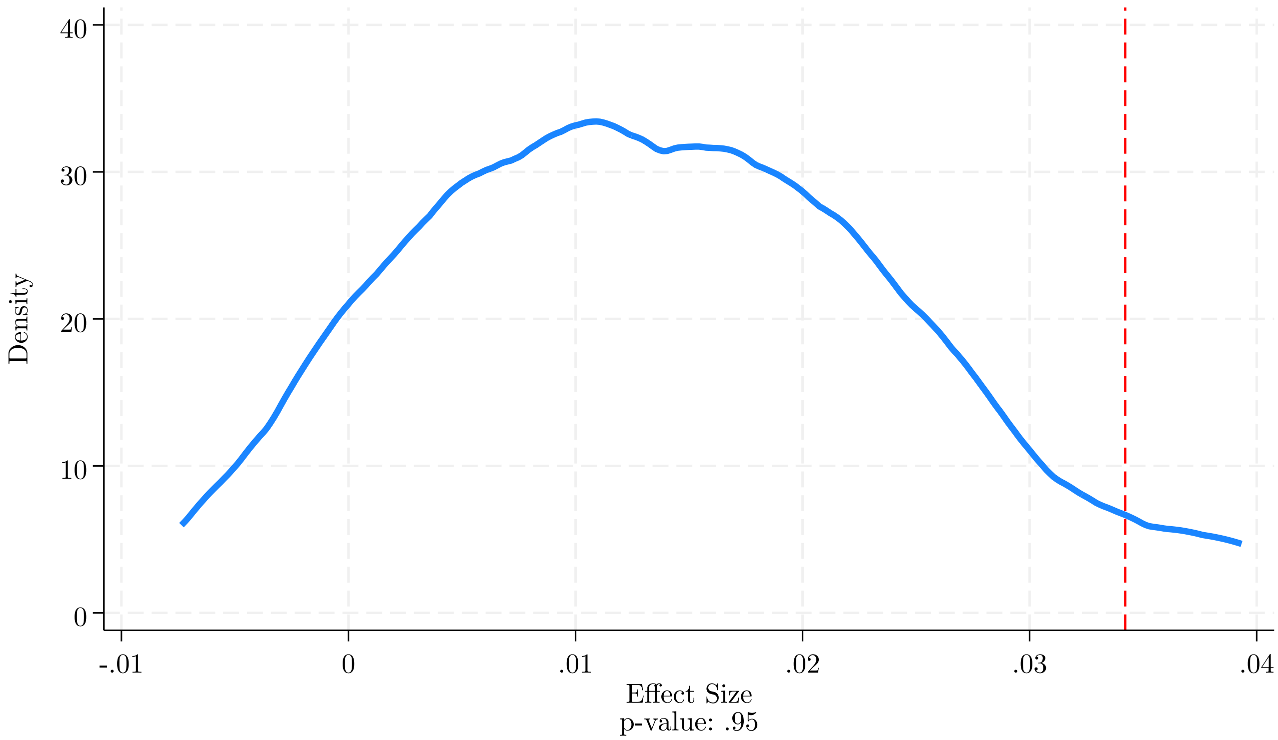
Notes: These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district. The base specification includes Federal Reserve district by industry-specific seasonal trends, as well as Fed district, industry, and quarter fixed effects. Weights are all based on lagged values. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, quarter, year, and industry. Including all of these fixed effects absorbs the own effect. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm.

Table 7: Effects of Own and Other Local Demand: Sensitive Industries

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Retail Index	0.028		0.442		0.043	
	(0.079)		(0.043)		(0.079)	
Other Retail Index	0.561	0.456	0.129	0.134	0.547	0.448
	(0.073)	(0.079)	(0.025)	(0.029)	(0.074)	(0.080)
Fully Saturated?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	30462	30371	30603	30513	30755	30664

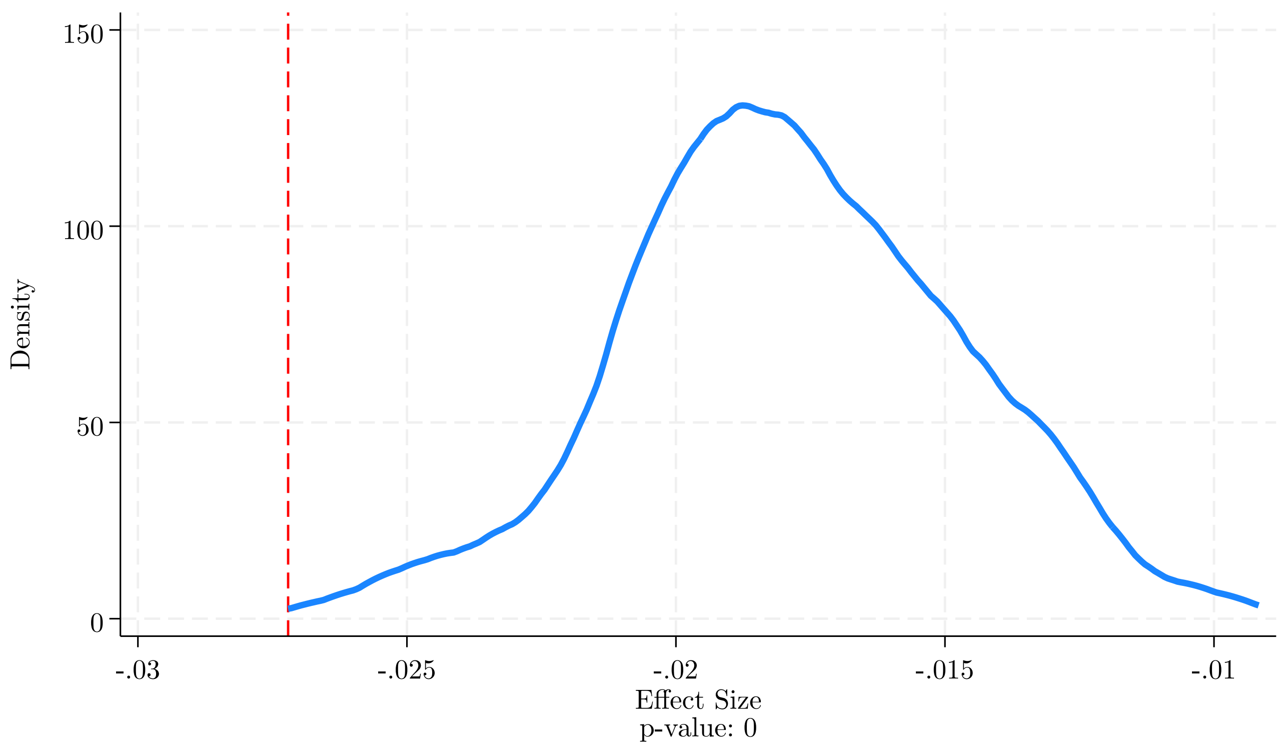
Notes: These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district. The base specification includes Federal Reserve district by industry-specific seasonal trends, as well as Fed district, industry, and quarter fixed effects. Weights are all based on lagged values. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, quarter, year, and industry. Including all of these fixed effects absorbs the own effect. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm. Sensitive industries are those industries with a statistically significant correlation with the own effect at the 5% level in the base specification.

Figure 1: Placebo Test of Effects of Other Bills Discounted



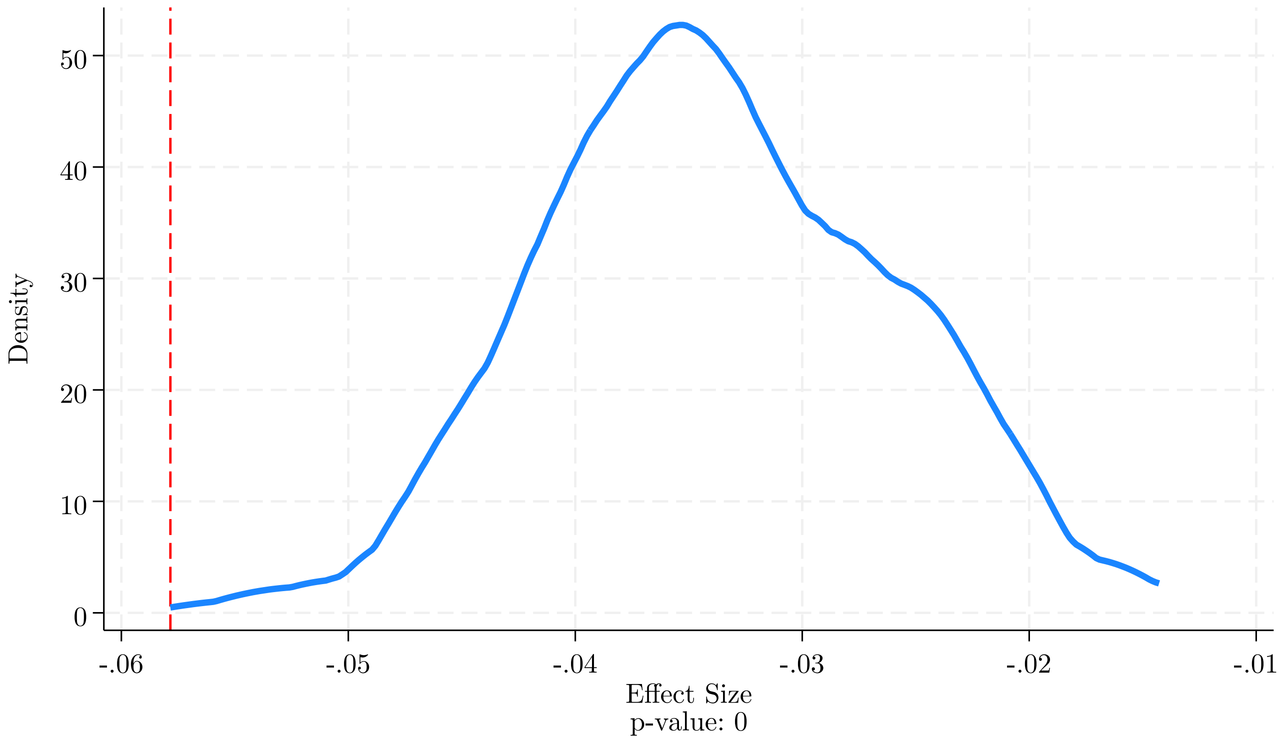
Notes: This figure compares the actual estimates from Column 1 in Table 3 denoted by the red line with estimates from using randomly generated firms. The distribution is 100 simulations where, for each simulation, we randomly permute establishments between firms holding fixed the marginal distribution of establishments per firm.

Figure 2: Placebo Test of Effects of Other Distressed Loans



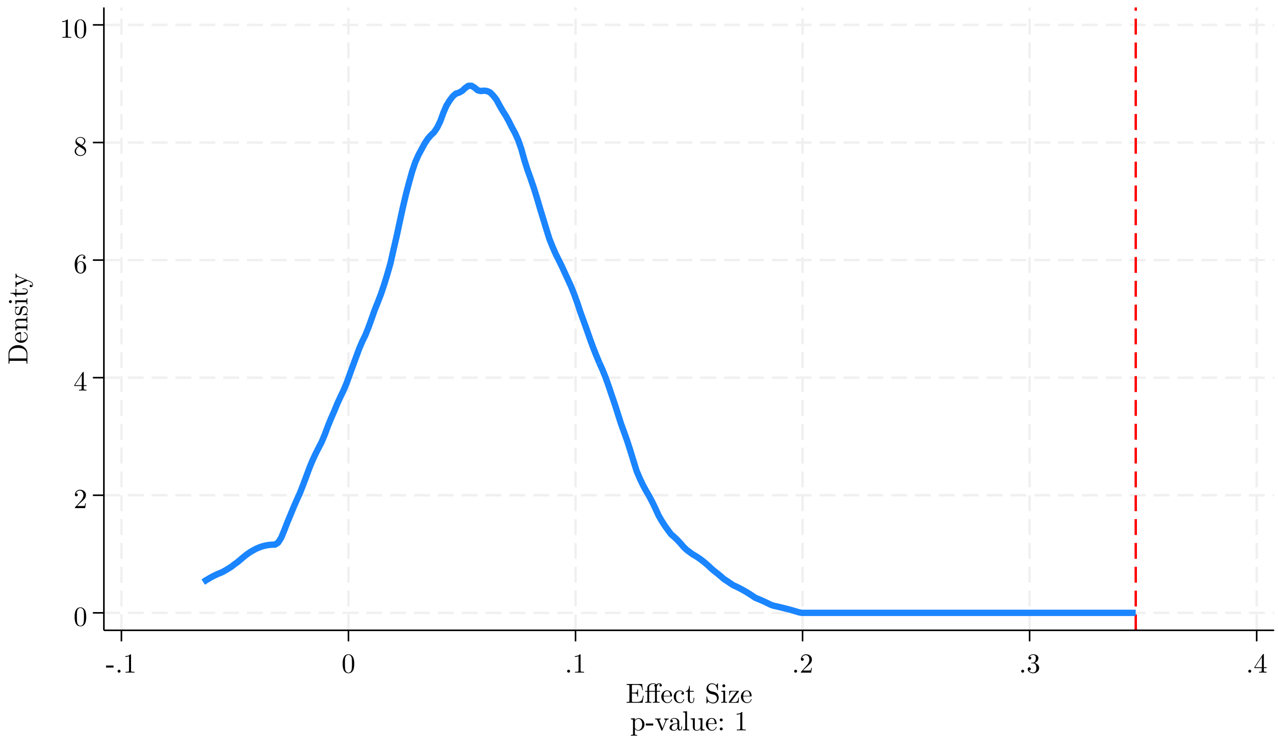
Notes: This figure compares the actual estimates from Column 1 in Table 4 denoted by the red line with estimates from using randomly generated firms. The distribution is 100 simulations where, for each simulation, we randomly permute establishments between firms holding fixed the marginal distribution of establishments per firm.

Figure 3: Placebo Test of Effects of Other Number of Banks Terminally Suspended



Notes: This figure compares the actual estimates from Column 1 in Table 5 denoted by the red line with estimates from using randomly generated firms. The distribution is 100 simulations where, for each simulation, we randomly permute establishments between firms holding fixed the marginal distribution of establishments per firm.

Figure 4: Placebo Test of Effects of Other Retail Sales



Notes: This figure compares the actual estimates from Column 1 in Table 6 denoted by the red line with estimates from using randomly generated firms. The distribution is 100 simulations where, for each simulation, we randomly permute establishments between firms holding fixed the marginal distribution of establishments per firm.

Online Appendix

A Model of Firm Networks

In this section we introduce our model of firm networks. Firms operate establishments in different regions. Each establishment has a constant returns to scale technology in labor. The firm faces a working capital constraint that limits the total wage bill of the firm to a function of the firm's total revenue. The model is simplified in a number of dimensions. First, we take as given the network structure rather than derive as a profit maximizing choice of the firm. Second, and related, we assume that there are no inefficiencies in the operation of these networks. Third, the model is static, eliminating the possibility of a firm "saving its way out" of the working capital constraint. Fourth, the model is partial equilibrium in that we take the supply of factors of production as given and, thereby, prices as well. Finally, and related, we assume that prices are perfectly flexible.[20]

Setup

Demand and Production Within region i, monopolistically competitive establishments produce a continuum of non-tradeable goods indexed by ω. For simplicity, we assume that each producer faces a constant elasticity demand curve $p_i(\omega) = \tilde{z}_i y_i(\omega)^{-\frac{1}{\sigma}}$ where $\sigma > 1$ and $\tilde{z}_i$ is a demand shifter. This type of demand curve could be micro-founded with a Dixit-Stiglitz household demand structure. Each establishment operates a linear production technology in labor $y_i(\omega) = a_i l_i(\omega)$ where a_i is aggregate productivity in region i. Then an establishment's

[20] In ongoing work, we are developing a fully general equilibrium model that models the labor supply and consumption decisions of households. This richer model allows us to explore the quantitative implications of firm networks though it still abstracts away a number of these issues. For this paper, we are only interested in establishing some qualitative results.

revenue $s_i(\omega)$ is

$$s_i(\omega) = p_i(\omega)y_i(\omega) = \tilde{z}_i l_i(\omega)^{\frac{\sigma-1}{\sigma}}.$$

From now on, we will suppress the index ω for notational convenience.

We assume that labor is freely mobile so that wages are equalized across regions and we normalize the wage to 1. Define $z_i = \frac{\sigma-1}{\sigma}\tilde{z}_i$ as the *investment opportunity* shock. Then profits are $\pi_i(l_i) = \frac{\sigma}{\sigma-1} z_i l_i^{\frac{\sigma-1}{\sigma}} - l_i$. It is easy to check that the first-best, profit-maximizing level of labor input is $l_i^{FB} = z_i^{\sigma}$. This expression provides the foundation for why we call z_i an investment opportunity shock since the ratio of the first-best labor inputs across regions i and j is determined by z_i/z_j.

The Working Capital Constraint We now introduce a working capital constraint (WCC) that can be interpreted as requiring a firm to borrow its total wage bill up front securing that loan by posting collateral based on its (future) revenue. We introduce the parameter κ_i to capture differences in the pledgeability of an *establishment*'s revenue (Holmström and Tirole, 1998), and, hence, the value of that establishment's revenue for the firm's liquidity position overall. We will later provide narrative evidence for this assumption as well as a number of empirical regularities that are consistent with it.

Assuming the firm owns N establishments in the N separate regions,[21] it maximizes total profits

$$\max_{\{l_i\}_{i=1}^N} \sum_{i=1}^{N} \pi_i(l_i)$$

[21] As noted earlier, we take the network structure as given.

subject to its working capital constraint:

$$\sum_{i=1}^{N} l_i \leq \sum_{i=1}^{N} \kappa_i s_i. \tag{1}$$

It will be useful to rewrite this problem in terms of what we call the *free cashflow* function, $\mathrm{FCF}_i(l_i) = \kappa_i s_i - w_i l_i$. Define l_i^{Max} as the labor input that maximizes free cashflow. This labor choice will solve the first order condition:

$$\kappa_i \mathrm{MPL}_i = 1,$$

where $\mathrm{MPL}_i = z_i l_i^{-\frac{1}{\sigma}}$ is the marginal product of labor for establishment i. In the case when $\kappa_i < \frac{\sigma-1}{\sigma} < 1$, $l_i^{Max} = \kappa_i^{1/\sigma} l_i^{FB} < l_i^{FB}$, so there is a disconnect between maximizing an establishment's profits and its free cashflow. We can then rewrite the firm's problem as

$$\max_{\{l_i\}_{i=1}^{N}} \sum_{i=1}^{N} \left[(1-\kappa_i)s_i + \mathrm{FCF}_i(l_i)\right],$$

subject to

$$\sum_{i=1}^{N} \mathrm{FCF}_i(l_i) \geq 0.$$

Rewriting the constraint in this way provides a straightforward method for determining whether the WCC binds. To do this, we evaluate the free cashflow function at the first best labor choice for each establishment i, $\mathrm{FCF}_i(l_i^{FB}) = z_i^{\sigma}\left(\frac{\sigma}{\sigma-1}\kappa_i - 1\right)$ and check whether the sum of the free cashflow generated by each establishment is non-negative. In general, the WCC evaluated at the first-best levels of labor is

$$\sum_{i=1}^{N} \alpha_i(0)\kappa_i \geq \frac{\sigma-1}{\sigma} \tag{2}$$

where $\alpha_i(0) = \frac{z_i^\sigma}{\sum_{i=1}^N z_i^\sigma}$.

With this test, it is easy to see that a single establishment firm will be liquidity constrained if and only if $\kappa_i < (\sigma - 1)/\sigma$. Hence, whether a SP firm's labor choice is distorted from the first best only depends on κ_i not on the investment opportunity shock, z_i. This is why we call a change in κ_i a *cashflow* shock.[22] In the case of a MP firm, whether the firm is liquidity constrained overall depends on a weighted average of κ_i where the weights depend on z_i.

Our choice to focus on a constraint to working capital or trade credit rather the investment capital is motivated by our reading of the literature. It is often assumed that the main channel through which credit matters is through the investment spending channel. For example, Bernanke et al. (1999) show in a quantitative business cycle model, how credit contractions lead to declines in aggregate demand and recessions through declines in investment. At least for the Depression, that theory is hard to square with the micro evidence that finds the *local* businesses are affected by *local* credit (Ziebarth, 2015). Based on the investment spending channel, local businesses should decrease their investment immediately in response to a credit contraction, but there is no reason to believe that the businesses immediately affected by this reduction in investment spending should be *located in the same region.* Ben Bernanke made exactly this point in commenting on the cross-sectional relationship between state-level income growth and bank failures provided by Cole and Ohanian (2000): "[I]f financial distress reduces the demand for automobiles in Alabama, output in Michigan rather than in Alabama will be most affected." Of course in the long-run, a credit starved business which is not able to invest will experience a decline in output relative to the case with abundant credit, but it seems hard to build a business cycle theory around this long-run outcome. So why do numerous papers such as Lee and Mezzanotti (2017) observe a casual relationship between local credit supply and local economic outcomes?[23]

[22]As we discuss below, we also make this definition to draw a parallel to a related paper (Giroud and Mueller, 2019) that studies the effects of "cashflow" shocks.

[23]They find that this relationship depends on the financial dependence of an industry. The fact that this relationship between local credit and local economic outcomes is not uniform casts doubt on a version of the Friedman-Schwartz hypothesis that focuses on how bank failures and suspensions prevent households from

Our assumption about pledgeability differences across establishments within a firm can be motivated either as a direct effect of how multi-plant firms were treated, or as a consequence of historical banking practices. In particular, as we discuss in detail in the history section, banks play an important role in facilitating purchases of goods by local wholesaler through the trade credit channel. In normal circumstances, manufacturers could obtain cash from banks in exchange for commercial paper; however, if credit was constrained, this channel could be foreclosed and thus manufacturers would bear the risk of the commercial paper themselves.

Labor Choices When the WCC Binds

SP Firm Case We first derive the second best outcome in the case of a single establishment (SP) firm facing a binding WCC. In this case, the optimal labor choice is the one that sets the free cashflow equal to 0, l^0:

$$l_{SP}^{SB} = \left(\frac{\sigma}{\sigma - 1}\kappa z\right)^{\sigma} = \left(\frac{\sigma}{\sigma - 1}\kappa\right)^{\sigma} l^{FB}.$$

We have dropped the i subscript for notational simplicity. As we showed above, the WCC binds if $\frac{\sigma}{\sigma-1}\kappa < 1$, so we see that in this case, $l_{SP}^{SB} < l^{FB}$. We now derive benchmark elasticities of labor with respect to the investment specific shock z and the cashflow shock, κ:

$$\frac{\partial \log l_{SP}^{SB}}{\partial \log \kappa} = \frac{\partial \log l_{SP}^{SB}}{\partial \log z} = \sigma. \tag{3}$$

accessing their deposits to use for purchases.

MP Firm Case We now turn to the case of a firm owning multiple establishments. First, the WCC can be rewritten as:

$$\text{FCF}_i(l_i) = -\sum_{j \neq i} \text{FCF}_j(l_j). \tag{4}$$

This defines a negative relationship between labor input at establishment i and that at some other establishment $-i$, holding fixed the remaining establishments' labor inputs.[24]

The first order condition (FOC) for the choice of establishment i's labor input is

$$\frac{(1-\kappa_i)\text{MPL}_i(l_i)}{\text{FCF}'_i(l_i)} = \frac{1}{N-1}\sum_{j \neq i} \frac{(1-\kappa_j)\text{MPL}_j(l_j)}{\text{FCF}'_j(l_j)}. \tag{5}$$

We can rewrite this in units of the first best labor input, $\bar{l} = l/l^{FB}$:

$$\frac{(1-\kappa_i)\bar{l}_i^{-\frac{1}{\sigma}}}{\kappa_i \bar{l}_i^{-\frac{1}{\sigma}} - 1} = \frac{1}{N-1}\sum_{j \neq i} \frac{(1-\kappa_j)\bar{l}_j^{-\frac{1}{\sigma}}}{\kappa_j \bar{l}_j^{-\frac{1}{\sigma}} - 1}.$$

Since the FCF function is everywhere concave, this relationship defines a positive relationship between l_i and l_{-i}, holding fixed the other establishments' labor inputs.[25] In the case when there is no cross-subsidization meaning for all i, $\text{FCF}_i(l_i) = 0$, and, therefore, $l_i = l_i^0$, MP establishments will operate just like SP establishments in terms of their responses to local investment opportunity shocks.

[24] Note that in this case, it will never be the case that one establishment is operating at its first best scale while the others are not. Assume for contradiction that one establishment was operating at its efficient scale while another was not. Then a marginal change in the labor use of the undistorted establishment would have second order effects on the profits that establishment earns while there would be a first order effect in reallocating some additional resources to the distorted establishments. Hence, this allocation of labor would not maximize firm profits.

[25] For the case of two establishments, we can show that an unique solution to these equations exists. Note first that if $l_i = l_i^{FB}$, then the FOC implies that $l_{-i} = l_{-i}^{FB}$. However, on the other hand if $l_i = l_i^{FB}$, then $-\text{FCF}_i(l_i^{FB}) > 0$ so $l_{-i} < l_i^{FB}$ by the WCC. Therefore, we know that the FOC curve is about the WCC curve when $l_i = l_i^{FB}$. For the case where $l_i = l_i^{Max}$, then the FOC implies that $l_{-i} = l_{-i}^{Max}$. From the WCC, we know that $-\text{FCF}_i(l_i^{Max}) < 0$ so $l_{-i} > l_{-i}^{Max}$. Therefore, we know that the WCC curve is above the FOC curve when $l_i = l_i^{Max}$. Since the curves are continuous and monotonic, there is a unique solution to this problem.

Comparative Statics

We now derive comparative statics with respect to "own" local conditions z_i and κ_i as well as with respect to "other" conditions z_{-i} and κ_{-i}. To provide intuition, Figure 5 shows the the effect of an increase in the investment opportunity shock for establishment i, z_i when initially establishment i is subsidizing establishment $-i$. In terms of $\bar{l}$, the FOC does not depend directly on z_i so the FOC curve does not change. Instead only the WCC curve is affected and rotates about the no cross-subsidization point. Figure 6 shows the effect of a negative cashflow shock in region i, which is a decrease in κ_i, again starting from a point where establishment i is subsidizing establishment $-i$. What these figures highlight is the extent to which the comparative statics depend on whether a particular establishment is initially subsidizing or being subsidized by the other establishments in the firm.

Let λ be the Lagrange multiplier on the working capital constraint (eqn. 4). It represents the shadow value of a marginal unit of working capital to the firm. To derive analytical expressions for the comparative statics, we calculate the optimal input choice for each establishment within a firm as a function of the multiplier:[26]

$$l_i^* = \left((\kappa_i^{-1} + \lambda) \sum_{j=1}^{N} \frac{\alpha_j(\lambda)}{\kappa_j^{-1} + \lambda} \right)^{\sigma} l_{SP}^{SB}. \tag{6}$$

We define the weights $\alpha_j(\lambda) = \frac{w_j l_j^*}{\sum_{k=1}^{N} w_k l_k^*}$ and l_i^* is an implicit function of λ.[27] The form for $\alpha_j(\lambda)$ shows why in defining the condition for whether a firm is constrained overall (eqn. 2), we used the notation $\alpha_j(0)$ since it corresponds to evaluating $\alpha_j(\lambda)$ at $\lambda = 0$. Relative labor inputs are then given by

$$\frac{l_i^*}{l_{-i}^*} = \left(\frac{(\kappa_i^{-1} + \lambda) z_i}{(\kappa_{-i}^{-1} + \lambda) z_{-i}} \right)^{\sigma}. \tag{7}$$

[26] We provide details on this derivation in the appendix.
[27] In the Appendix, we collect all proofs and derivations for the model.

Holding fixed λ, a decrease in κ_i making establishment i's revenue less pledgeable would decrease its relative labor input. This shows that establishments that face relatively tight financing constraints will be subsidized by other establishments that make up the firm relative to the SP firm case. However, a decrease in κ_i will also affect the value of λ. So to understand the effects on labor inputs we need to calculate the total differential.

We focus on the case when the WCC is just binding. In that case, the total differential of labor demand at establishment i when $\lambda = 0^+$ is given by:

Proposition 1 (Total differential of labor demand)

$$d \log l_i = \sigma d \log z_i + \frac{\sigma^2}{B}(1-\kappa_i)(\kappa_i - \kappa_{-i}) \left(d \log z_i - d \log z_{-i}\right). \tag{8}$$

The proof along with the definition of the constant $B > 0$ is in the appendix. From this, we can deduce the comparative statics of the investment opportunity shocks:

Proposition 2 (Comparative statics of investment opportunity shocks)

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log z_i} = \sigma \left(1 + \frac{\sigma}{B}(1-\kappa_i)(\kappa_i - \kappa_{-i})\right), \tag{9}$$

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log z_{-i}} = -\frac{\sigma^2}{B}(1-\kappa_i)(\kappa_i - \kappa_{-i}). \tag{10}$$

Both the "own" (eqn. 9) and "other" elasticities (eqn. 10) are affected by the sign of relative pledgeability $\kappa_i - \kappa_{-i}$. In the case of the own elasticity, this relative pledgeability determines whether the response of an establishment in a MP firm is greater or smaller than for a standalone firm, which is σ. For the "other" elasticity, this relative pledgeability determines whether establishment i grows or shrinks in response to an "other" investment opportunity shock. Sensibly, the MP firm responds the same to an aggregate demand shock that affects both z_i and z_{-i} equally as a SP firm does to a "local" demand shock. In particular, a 1%

increase in aggregate demand will increase employment at each establishment by σ percent.

We now turn to the "own" and "other" elasticities with respect to cashflow shocks.

Proposition 3 (Comparative statics of cashflow shocks)

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log \kappa_i} = \sigma \delta_i(0), \tag{11}$$

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log \kappa_{-i}} = \sigma \delta_{-i}(0) \tag{12}$$

where $\delta_i(0) = \frac{z_i^\sigma \kappa_i}{\sum_{i=1}^N z_i^\sigma \kappa_i}$.

Eqn. 11 shows that the effects of an "own" cashflow shocks are dampened relative to the case of a firm consisting of a single establishment, in which case, the response is σ. The size of this dampening depends on the establishment's relative size within the firm. The second equation, which is the elasticity with respect to an "other" cashflow shock, shows how the firm "spreads" out the effects of the cashflow shock across all of its constituent establishments. In general, we interpret investment opportunity shocks as generating a negative correlation between employment across establishments within a firm while cashflow shocks generate a positive correlation.[28]

This result for the cashflow shock is similar to the one in Giroud and Mueller (2019).[29] The difference between their cashflow shocks and ours is that their shock is a "pure wealth" shock that relaxes the WCC, but does not affect "price" of labor in terms of its effects on the availability of working capital within the firm. Our cashflow shock, on the other hand, affects the marginal value of labor as measured by the free cashflow generated by establishment i. While this difference between their cashflow shock and ours does not make a difference for the qualitative effects of such a cashflow shock, the difference does matter for the qualitative

[28]Note that, for cashflow shocks, there is no necessary reason why the own effect should be larger or smaller than the other effect. What matters for the relative size of the own and other effects is the relative size of $z_i^\sigma \kappa_i$ across regions.

[29]In the appendix, we consider their setup in more detail.

effects of an investment opportunity shock. In their model, the relative change in labor inputs across establishments, even when the WCC is binding, only depends on investment opportunities across regions. Compare this to the expression for relative labor demand in our case (eqn. 7). For us, the allocation of labor across establishments also depends on the relative values of pledgeability.

Additional Model Details

Setup and micro-foundations

The model is in partial equilibrium. A firm is a collection of establishments all indexed by $i = \{1 \ldots N\}$. With a slight abuse of notation we will identify an establishment i with the region where it is located.

Microfoundation from the Demand Side

We assume that firms operate in a monopolistically competitive environment at the regional level. In each region, consumers have demand for differentiated goods, indexed by ω, produced by a set Ω_i of establishments in region i. We specify the demand curve using an aggregator that exhibits constant elasticity of substitution across goods:

$$C_i = \left[\int_{\Omega_i} c_i(\omega)^{1-\frac{1}{\sigma}} d\omega \right]^{\frac{\sigma}{\sigma-1}}, \tag{13}$$

If firms operate a linear technology in labor, $y_i(\omega) = a_i l_i(\omega)$, then standard Dixit-Stiglitz algebra yields the following equilibrium revenue for a firm in region i:

$$s_i = p_i y_i = P_i C_i^{\frac{1}{\sigma}} a_i l_i^{\frac{\sigma-1}{\sigma}}, \tag{14}$$

where P_i is the price index over all differentiated varieties ω consumed in region i. In partial equilibrium, it is convenient to group firm productivity a_i and local demand conditions $P_iC_i^{1/\sigma}$ as a local shock $\tilde{z}_i$ that applies to all establishments operating in region i. In partial equilibrium, this model is isomorphic to an economy where firms operate a decreasing returns to scale technology with labor as the sole factor input. The models are equivalent when the labor productivity is defined as

$$z_i = \frac{\sigma - 1}{\sigma}\tilde{z}_i = \frac{\sigma - 1}{\sigma}P_iC_i^{\frac{1}{\sigma}}a_i. \tag{15}$$

Proof of Result for Optimal Labor Choice

Lemma 1 *The Lagrange multiplier on the working capital constraint λ solves*

$$\frac{\sigma - 1}{\sigma(1+\lambda)} = \sum_{i=1}^{N} \frac{\alpha_i(\lambda)}{\kappa_i^{-1} + \lambda},$$

where $\alpha_i(\lambda) = \frac{w_i l_i(\lambda)}{\sum_{i=1}^{N} w_i l_i(\lambda)}$.

To prove this, we start with the FOC for l_i

$$(1 + \lambda\kappa_i)z_i l_i^{-1/\sigma} = (1+\lambda)w_i.$$

Next we multiply both sides by l_i and factor out a κ_i to get

$$\frac{\sigma - 1}{\sigma}\kappa_i y_i = (1+\lambda)\frac{w_i l_i}{\kappa_i^{-1} + \lambda}.$$

Summing over i and using the fact that $\sum_{i=1}^{N} \kappa_i y_i = \sum_{i=1}^{N} w_i l_i$, we find

$$\frac{\sigma - 1}{\sigma(1+\lambda)}\sum_{i=1}^{N} w_i l_i = \sum_{i=1}^{N} \frac{w_i l_i}{\kappa_i^{-1} + \lambda}.$$

Finally, we divide through by $\sum_{i=1}^{N} w_i l_i$ and define $\alpha_i = \frac{w_i l_i}{\sum_{i=1}^{N} w_i l_i}$ to arrive at the claim of the lemma.

We can now prove the result in the paper for the optimal labor choice as a function of λ. First, the FOC for l_i can be written as

$$l_i = l_{SP}^{SB} \left(\frac{\kappa_i^{-1} + \lambda}{1+\lambda} \frac{\sigma - 1}{\sigma} \right)^{\sigma}.$$

Then we substitute for $\frac{\sigma-1}{\sigma(1+\lambda)}$ using the lemma and multiply through by $\kappa_i^{-1} + \lambda$ to arrive at our result.

Proofs of Comparative Statics Results

From the FOC for l_i, we have

$$l_i = \left(\frac{z_i}{w_i} \right)^{\sigma} \left(\frac{1 + \lambda \kappa_i}{1 + \lambda} \right)^{\sigma}.$$

Replacing l_i in the working capital constraint with this expression, we get

$$\frac{\sigma}{\sigma - 1} \sum_{i=1}^{N} \kappa_i w_i^{1-\sigma} z_i^{\sigma} \left(\frac{1 + \lambda \kappa_i}{1 + \lambda} \right)^{\sigma - 1} = \sum_{i=1}^{N} w_i^{1-\sigma} z_i^{\sigma} \left(\frac{1 + \lambda \kappa_i}{1 + \lambda} \right)^{\sigma}.$$

Recall that $\alpha_i(\lambda) = \frac{w_i l_i}{\sum_{i=1}^{N} w_i l_i} = \frac{w_i^{1-\sigma} z_i^{\sigma} (1+\lambda\kappa_i)}{\sum_{i=1}^{N} w_i^{1-\sigma} z_i^{\sigma} (1+\lambda\kappa_i)}$. Then, by the lemma, we know

$$\sum_{i=1}^{N} \frac{\kappa_i}{1 + \kappa_i} \alpha_i(\lambda) = \frac{\sigma - 1}{\sigma} \frac{1}{1 + \lambda}.$$

This equation defines implicitly the value for λ. We can then use the implicit function theorem to calculate the derivative of λ with respect to the investment opportunity shock z_i and the cashflow shock κ_i.

For the case of two establishments and taking the limit of the derivative as $\lambda \to 0^+$:

$$\lim_{\lambda\to 0^+} \frac{\partial \lambda}{\partial \kappa_i} = -B^{-1}\left(1 + \frac{w_i^{1-\sigma} z_i^{\sigma}}{w_{-i}^{1-\sigma} z_{-i}^{\sigma}}\right),$$

$$\lim_{\lambda\to 0^+} \frac{\partial \lambda}{\partial z_i} = -\sigma B^{-1} \frac{\kappa_i - \kappa_{-i}}{z_i},$$

where $B = \sum_{i=1}^{2} \kappa_i(1-\kappa_i)\left(\frac{w_i^{1-\sigma} z_i^{\sigma}}{w_{-i}^{1-\sigma} z_{-i}^{\sigma}} + 1\right) + \sigma(\kappa_i - \kappa_{-i})^2 > 0$. Hence, if κ_i increases, the financing constraint is relaxed and λ falls. On the other hand, if z_i increases, the financing constraint is relaxed if and only if $\kappa_i > \kappa_{-i}$, i.e. the revenue of establishment i's is relatively more pledgeable.

Once we have expressions for the derivative of the multiplier with respect to the various parameters, it is relatively straightforward to derive the comparative statics for the labor choices. From the above expression for l_i, we have

$$l_i = \left(\frac{z_i}{w_i}\right)^{\sigma} \left(\frac{1+\lambda\kappa_i}{1+\lambda}\right)^{\sigma}.$$

Differentiating with respect to z_i, we find

$$\frac{\partial l_i}{\partial z_i} = \sigma \frac{l_i}{z_i}\left(1 - \frac{1-\kappa_i}{(1+\lambda)(1+\lambda\kappa_i)} z_i \frac{\partial \lambda}{\partial z_i}\right).$$

Taking the limit as $\lambda \to 0^+$ again and using the result for $\lim_{\lambda\to 0^+} \frac{\partial \lambda}{\partial z_i}$, we have

$$\lim_{\lambda\to 0^+} \frac{\partial \log l_i}{\partial \log z_i} = \sigma^2 \left[\sigma^{-1} + B^{-1}(\kappa_i - \kappa_{-i})(1-\kappa_i)\right].$$

We derive the elasticity with respect to "other" demand shocks in a similar way:

$$\lim_{\lambda\to 0^+} \frac{\partial \log l_i}{\partial \log z_{-i}} = -\sigma^2 B^{-1}(\kappa_i - \kappa_{-i})(1-\kappa_i).$$

The process to calculate the comparative statics with respect to κ is similar, but involves a bit more algebra. First, we have

$$\frac{\partial \log l_i}{\partial \log \kappa_i} = \kappa_i \sigma \left(\frac{\lambda + \kappa_i \frac{\partial \lambda}{\partial \kappa_i}}{1 + \lambda \kappa_i} - \frac{\partial \lambda}{\partial \kappa_i} \frac{1}{1 + \lambda} \right).$$

Taking the limit as $\lambda \to 0^+$, we get

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log \kappa_i} = -\sigma \kappa_i (1 - \kappa_i) \lim_{\lambda \to 0^+} \frac{\partial \lambda}{\partial \kappa_i}.$$

Substituting in for $\lim_{\lambda \to 0^+} \frac{\partial \lambda}{\partial \kappa_i}$ gives our result. Now for the "other" shock, we start with

$$\frac{\partial \log l_i}{\partial \log \kappa_{-i}} = \sigma \kappa_{-i} \frac{\partial \lambda}{\partial \kappa_{-i}} \left(\frac{\kappa_i}{1 + \lambda \kappa_i} - \frac{1}{1 + \lambda} \right).$$

Taking the limit as $\lambda \to 0^+$, we get

$$\lim_{\lambda \to 0^+} \frac{\partial \log l_i}{\partial \log \kappa_{-i}} = \sigma \kappa_{-i} \kappa_i \lim_{\lambda \to 0^+} \frac{\partial \lambda}{\partial \kappa_{-i}}.$$

Substituting in for $\lim_{\lambda \to 0^+} \frac{\partial \lambda}{\partial \kappa_{-i}}$ gives our result.

Comparison to the Model of Giroud and Mueller (2019)

We now highlight similarities and differences between our model with the one in the paper by Giroud and Mueller (2019) (GM). The key difference between the two models is in the formulation of the "cashflow" shock. Fixing ideas, a firm maximizes its profits from a set of N establishments given by

$$\sum_{i=1}^{N} p_i y_i - w_i l_i,$$

subject to a working capital constraint:

$$\sum_{i=1}^{N} w_i l_i \leq \sum_{i=1}^{N} C_i.$$

The term C_i is the "cashflow" generated by establishment i and the total cashflows across all establishments limits the the total wage bill of the firm. Taking prices and wages as exogenous, the first order condition for l_j is

$$p_j y_j'(l_j) = (1+\lambda) w_j,$$

where λ is the Lagrange multiplier on the cashflow constraint.

GM consider the comparative statics of shocks to C_k. These can be calculated by differentiating the FOC and the working capital constraint.

$$\frac{\partial \lambda}{\partial C_k} = \tilde{w}_j \frac{\partial l_j}{\partial C_k},$$
$$\sum_{i=1}^{N} w_i \frac{\partial l_i}{\partial C_k} = 1,$$

where $\tilde{w}_j = \frac{p_j}{w_j} y_j''(l_j)$. Define $\omega_j = \frac{\tilde{w}_j}{\sum_{i=1}^{N} \tilde{w}_i} > 0$ is a weight that does not depend on k, then

$$\frac{\partial l_j}{\partial C_k} = \omega_j.$$

This shows that it does not matter the source of the cashflow shock. Each establishment that makes up a firm "shares" in the effects of the cashflow shock in proportion to its importance as measured by ω. It does not matter which establishment actually "directly" experiences the shock.

Theoretically, GM's cashflow shock is closest to what we call a "cashflow" shock. In fact, the comparative statics for our cashflow shock are qualitatively identical to theirs. The difference between our formulation and theirs is in the comparative statics for our investment opportunity shock. For this shock, we find that positive co-movement in employment across establishments within a firm depends on relative pledgeability across establishments and, in the case, that there are no differences, then there is no correlation in employment between establishments. In their paper, they dismiss interpreting the shock they focus on–a shock to local housing prices–in this way since they state that such a shock would generate a negative correlation between employment across a firm's constituent establishments. Our richer model with differences in pledgeablity across establishments shows that this is not necessarily the case. In particular, as equations (8) and (9) in our paper show if $\kappa_i - \kappa_{-i} < 0$ (and this difference is not too large), then, in fact, we can generate a positive co-movement in employment across the different regions. (This is at the "cost" of having a dampened response to an own shock, but the response is still positive.) In fact, GM's model is a special case of our model when $\kappa_i = \kappa_{-i}$ and, in this case, employment in other regions does not change.

Comparing MP to Non-MP Establishments

Given that, in effect, our results are based on comparing firms with establishment networks of different size and scope, it is reasonable to wonder about how similar these groups of establishments are. Rather than focus on comparing establishments in slightly bigger firms to those in slightly smaller firms, we instead for simplicity compare MP to non-MP establishments. Any ways in which MP establishments differ from non-MP establishments beyond simply being part of an MP firm are potential confounders. While it is impossible to rule out everything, we can test whether that are differences between these groups in terms of observable characteristics such as revenue, employment, labor productivity, and the labor share. Figure 7 shows the differences in means by industry. We scale the difference by the industry-specific standard deviation of the dependent variable and adjust the standard errors accordingly. With this scaling, the estimated differences are measured in units of the industry standard deviation.

Across all industries except for malt, MP establishments are larger in terms of employment and revenue. The magnitudes of these differences are all less than 2 standard deviations with the motor vehicle, cigars and cigarettes, aircraft and parts, and soap industries as outliers. On the other hand, along the total wage bill in the revenue dimension, non-MP and MP establishments do not appear that dissimilar across industries. Taken as a whole, MP establishments tend to have a smaller ratio of wages to revenue, but the difference is neither large in the statistical (only 6 industries have statistically significant differences) nor economic sense (all the differences are less than 0.5 standard deviations in magnitude).

We also examine differences in labor productivity as another measure of the production technology being used by an establishment. Modern evidence in a paper by Schoar (2002) finds that MP establishments or, more precisely, conglomerate firms are more productive on average than stand-alone firms. On the balance, we find a similar pattern with higher labor productivity for MP establishments across most industries.[30] This suggests MP establishments are not (too) different in terms of technology relative to non-MP establishments in their industry. There is more direct evidence on this technology question for some industries. For example, in cement and ice, differences between establishments were not due to fundamentally different production processes. It was simply a function of the scale of the machinery employed. In cement, it was the size of the kiln, and, for ice, the horsepower of the compressors. On the other hand, there is qualitative evidence from various sources that in some particular industries there were differences in technology such as automobiles (Bresnahan and Raff, 1991) and macaroni (Alexander, 1997). These papers are silent on whether these technology choices were correlated with whether an establishment was part of

[30]This is not an altogether surprising finding given the labor share results since the labor share is equal to the inverse of labor productivity times the establishment-specific wage. So if this wage did not vary across establishments within an industry, then the labor share and the inverse of labor productivity should be proportional.

an MP firm.

Finally, we examine the geographic distribution of MP and non-MP establishments by Federal Reserve district. Figure 8 shows the marginal distribution of establishments across the 13 Federal Reserve districts by MP status. We plot the ratio of the number of non-MP to MP establishments in a given region relative to the ratio at the national level. So a value of 1 for this ratio means a particular region has the same ratio as in the aggregate. While there are some deviations from the national ratio such as in the Minneapolis region, it is clear that in all Federal Reserve districts, there are both MP and non-MP establishments.

Robustness Checks: Alternative Credit Supply Measures

In the paper, we used the quantity of discount loans, the number of banks terminally suspended, and the amount of loans at distressed banks as measures of local credit supply. Here we offer results for an additional set of credit supply measures.

As for an alternative measure of public credit supply, Table 8 shows the results using the discount rate rather than the quantity of discount loans. Somewhat surprisingly, the own effect tends to be positive though statistically insignificant except for the case of revenue weighting meaning increases in the discount rate, presumably tighter monetary policy, are associated with increases in employment. This is perhaps due to reverse causality and a reflection of the Real Bills doctrine, which called for a pro-cyclical monetary policy and held sway on policymakers at this time. Similarly, the other effects, which are economically and statistically significant across the different weighting variables, are also positive. Without having a consistent estimate for the own effect, it is hard to interpret the other effects. If we used revenue for weighting, then the discount rate looks like an investment opportunity shock. For the other weight variables, the discount rate looks like a cashflow shock though not in the direction we would have thought.

As for alternative measures of private credit supply, Tables 9, 10, and 11 shows the results using the quantity of deposits in suspended banks, the number of banks temporarily suspended, and the amount of loans in liquidation. Across all these variables and choices of the weighting variable, we find statistically and economically significant negative own and other effects, which is consistent with our interpretation that increases in these variables reflect *decreases* in credit supply. We conclude that the choice of the private credit supply variable is not critical for our results.

Robustness Checks: Local Demand

Tail Trimming

The first set of robustness checks considers whether outliers in the employment distribution are driving our main results on demand spillovers. More precisely, we calculate percentiles by year and industry and trim within that cell. Figure 9 shows the effects from our baseline specification as we vary the percentage of observations in the tails that we trim. We find that results are relatively robust to the amount of tail trimming. We lose a little bit of statistical significance of the own effect, but the point estimates of the own and other effects are effectively unchanged.

Effects by Year

We now examine whether the spillover effects of retail sales varied by year. One might imagine that the extent of these spillovers depended on developments in external capital markets. A large part of the value of internal networks is derived from the presence of binding borrowing constraints. If firms are free to borrow as much as they like, then there is very little incentive to pool financial resources among establishments and we would not expect to see spillovers between establishments within the same firm.

Figure 10 shows the own and other effects of retail sales by year. Like in the paper, we construct the other effect using employment as the weight. We find that the own effect is quite stable across the 4 years with perhaps a slightly smaller effect in 1931. Now while we can not reject the null that the other effects do not differ by year, we take the evidence as suggesting that something changed after 1929. There were no spillovers in that year and, if anything, the spillovers were in the opposite direction of the own effect. However, from 1931 onward, which was a period marked by severe stresses in the external capital markets, there is clear evidence for positive spillovers.

We view these results by year as similar to those in Matvos and Seru (2014), who examined the functioning of internal capital markets during the Great Recession. They find that these internal capital markets acted as a substitute for external financing during this period of time when it was difficult to tap external capital markets. In this way, having an internal capital market, which we take as parallel to a firm's network of establishments, can be quite valuable during times of financial stress, a conclusion that our (tentative) results endorse.

Table 8: Effects of Own and Other Discount Rate

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Discount Rate	0.034	0.031	-0.024	-0.013	0.018	0.018
	(0.020)	(0.019)	(0.017)	(0.017)	(0.020)	(0.020)
Other Discount Rate	0.038	0.043	0.097	0.087	0.054	0.056
	(0.020)	(0.019)	(0.017)	(0.017)	(0.020)	(0.020)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	39385	39364	39582	39560	39756	39736

Notes: These data are at a quarterly frequency by Federal Reserve district. An increase in distressed loans should be interpreted as a decrease in local credit supply. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm.

Table 9: Effects of Own and Other Deposits in Suspended Banks

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Deposits Suspended	-0.014	-0.018	-0.014	-0.017	-0.016	-0.018
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Other Deposits Suspended	-0.025	-0.021	-0.025	-0.022	-0.022	-0.020
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	20884	20864	21087	21068	21139	21120

Notes: These data are at a quarterly frequency by Federal Reserve district. An increase in the quantity of deposits in suspended banks should be interpreted as a decrease in local credit supply. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm.

Table 10: Effects of Own and Other Number of Banks Temporarily Suspended

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Banks Temporarily Suspended	-0.045	-0.040	-0.044	-0.035	-0.049	-0.042
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
Other Banks Temporarily Suspended	-0.048	-0.052	-0.044	-0.053	-0.037	-0.044
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	20884	20864	21087	21068	21139	21120

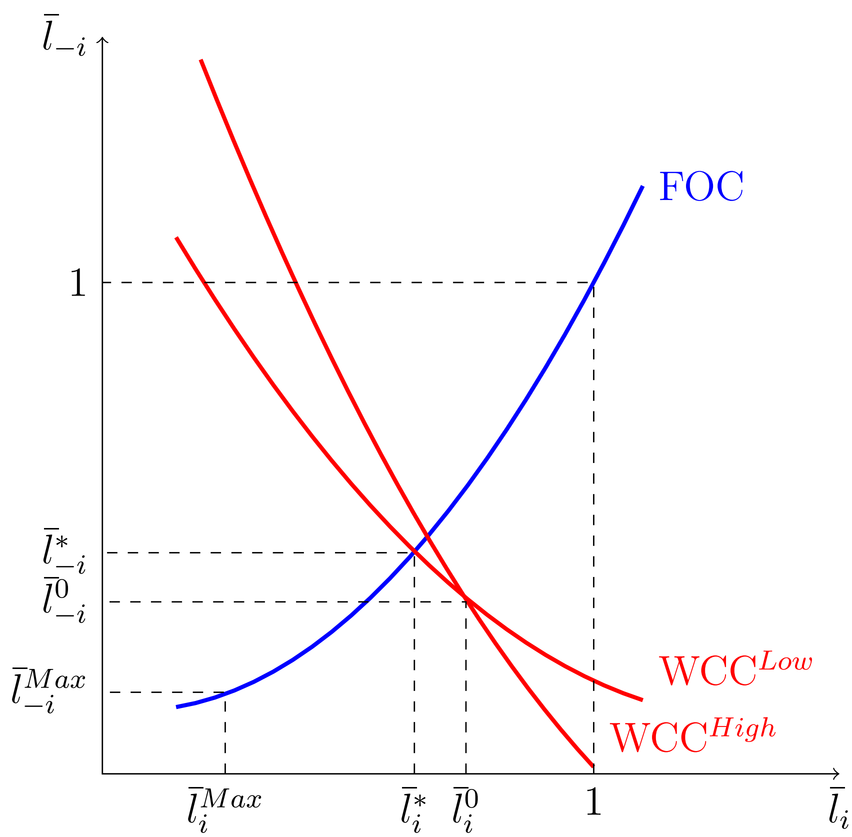
Notes: These data are at a quarterly frequency by Federal Reserve district. An increase in the number of temporarily suspended banks should be interpreted as a decrease in local credit supply. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm.

Table 11: Effects of Own and Other Loans in Liquidation

	Log Wage Earners					
	(1)	(2)	(3)	(4)	(5)	(6)
Own Loans in Liquidation	-0.012	-0.015	-0.012	-0.015	-0.014	-0.016
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Other Loans in Liquidation	-0.028	-0.025	-0.029	-0.026	-0.026	-0.024
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Additional Controls?	No	Yes	No	Yes	No	Yes
Weight	Employment	Employment	Revenue	Revenue	Equally	Equally
Observations	20884	20864	21087	21068	21139	21120

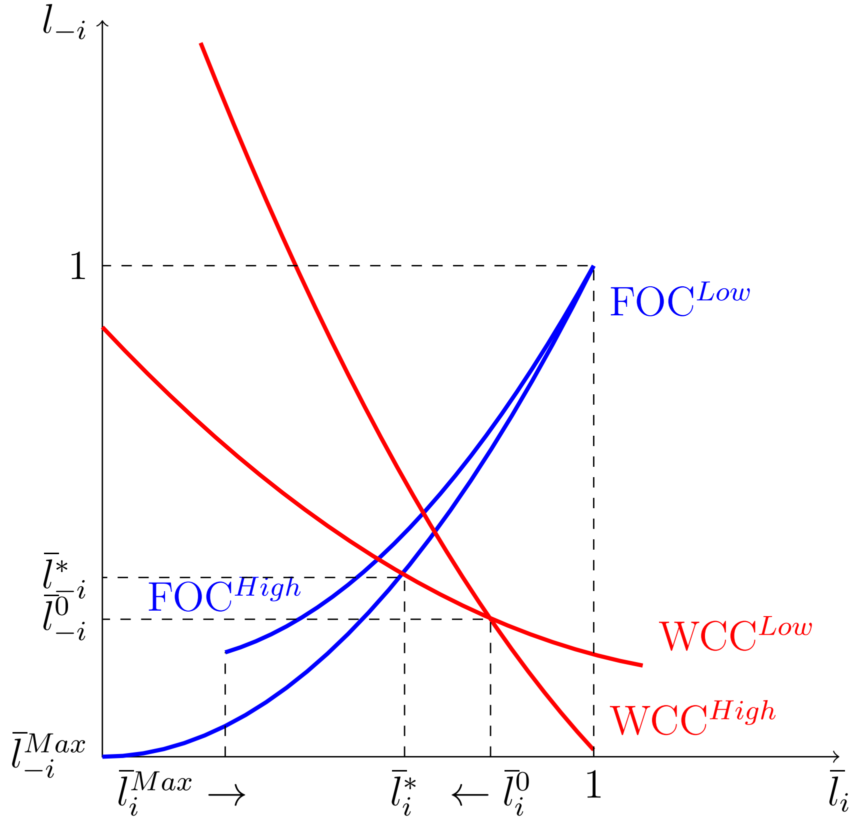
Notes: These data are at a quarterly frequency by Federal Reserve district. An increase in the quantity of loans in liquidation should be interpreted as a decrease in local credit supply. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Standard errors are clustered at the firm level. We restrict attention to establishments that are part of an MP firm.

Figure 5: Comparative Statics of an Investment Opportunity Shock



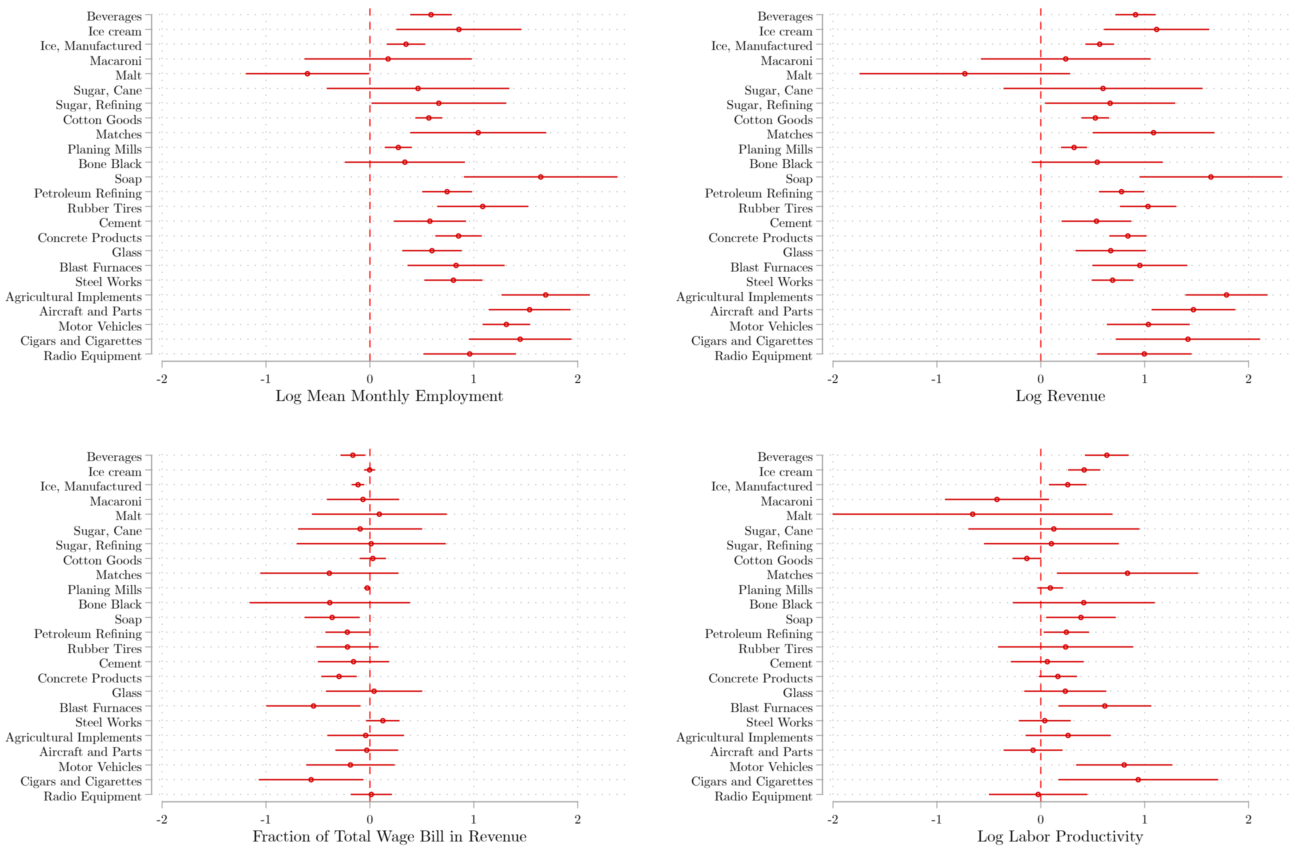
Notes: A positive investment opportunity shock in region i is an increase in a_i. Establishment i is subsidizing establishment $-i$ at the initial optimal choice. The WCC constraint rotates about the point $(\bar{l}^0_i, \bar{l}^0_{-I})$ where neither establishment generates any cashflow. The FOC curve is independent of a_i so it remains fixed.

Figure 6: Comparative Statics of a Cashflow Shock



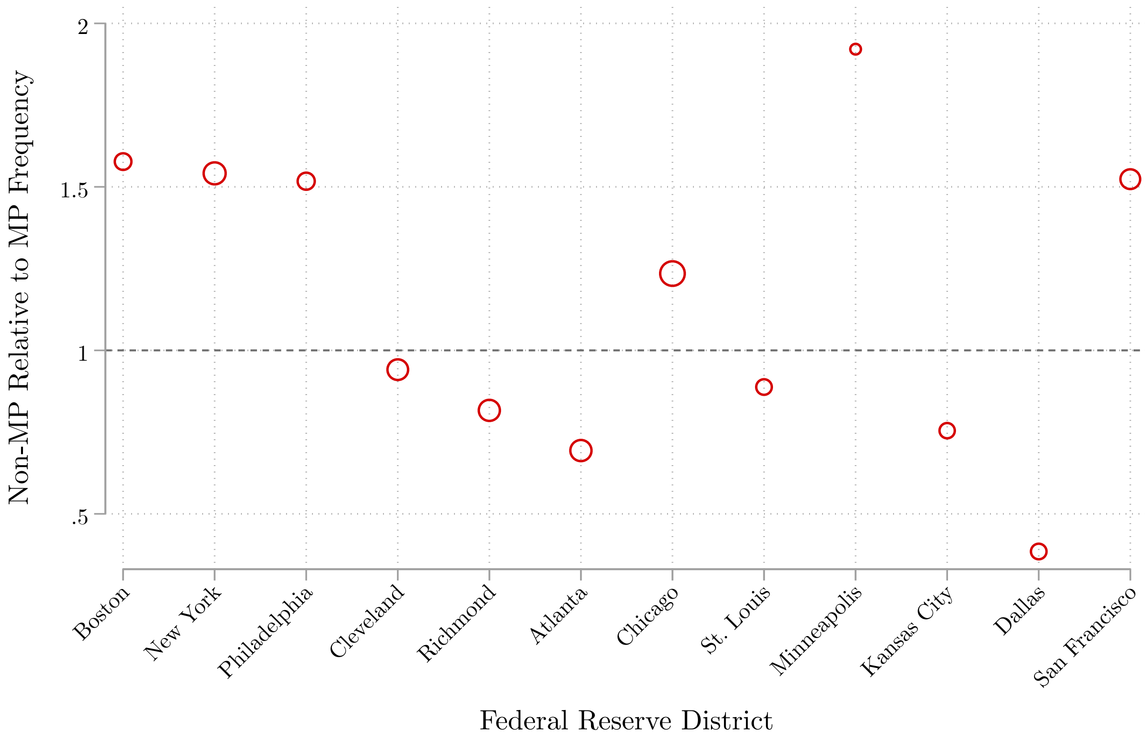
Notes: A positive cashflow shock in region i is a decrease in κ_i. Establishment i is subsidizing establishment $-i$ at the initial optimal choice. The WCC constraint rotates about the point $(\bar{l}^0_i, \bar{l}^0_{-I})$ where neither establishment generates any cashflow. There are two effects on the FOC curve. First, the domain over which it is defined shrinks since $\bar{l}^{Max}_i$ increases. In addition, for all values of $\bar{l}_i$ where both FOC curves are defined, the new FOC curve is higher.

Figure 7: Comparison of MP to non-MP Establishments in 1929



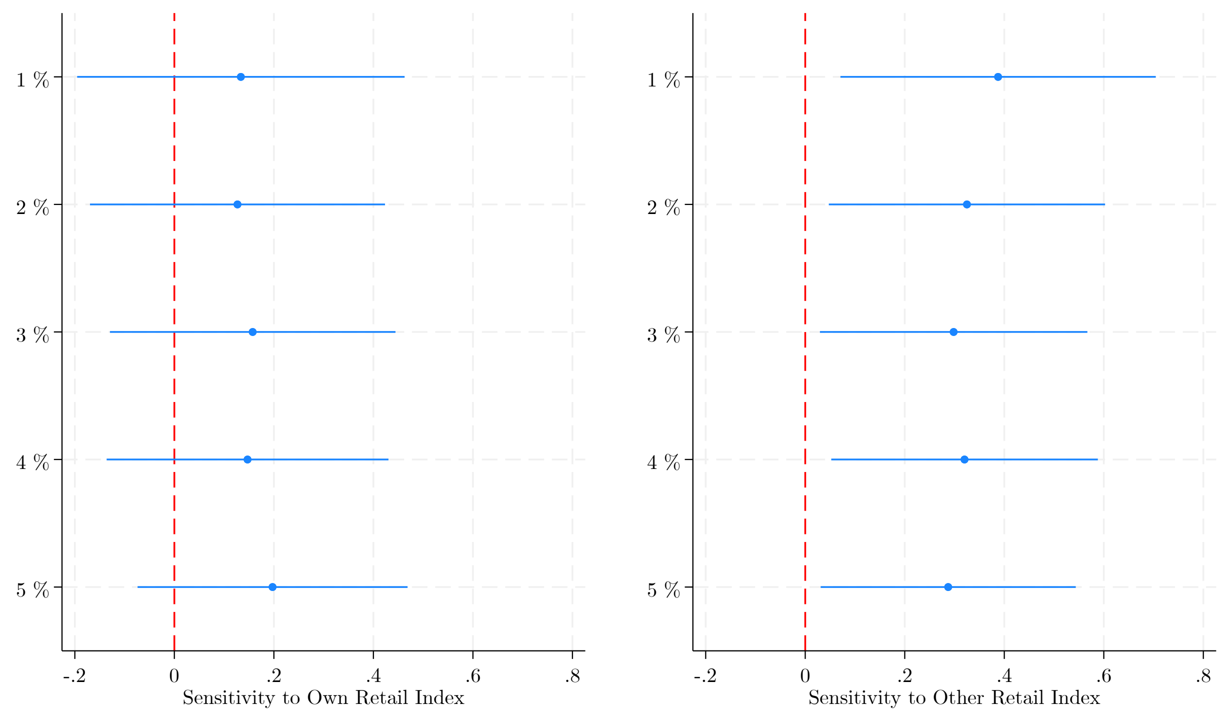
Notes: The figure reports the mean difference between MP and non-MP establishments in 1929. Labor productivity is measured as the ratio of total revenue relative to total of wage earners. Mean employment is the average monthly employment in 1929. Each variable is log transformed besides the fraction of the total wage bill in revenue. Coefficients and standard errors are scaled by the standard deviation of the dependent variable in the given industry. Standard errors are clustered at the firm level.

Figure 8: Geographic Distribution of MP and non-MP Establishments



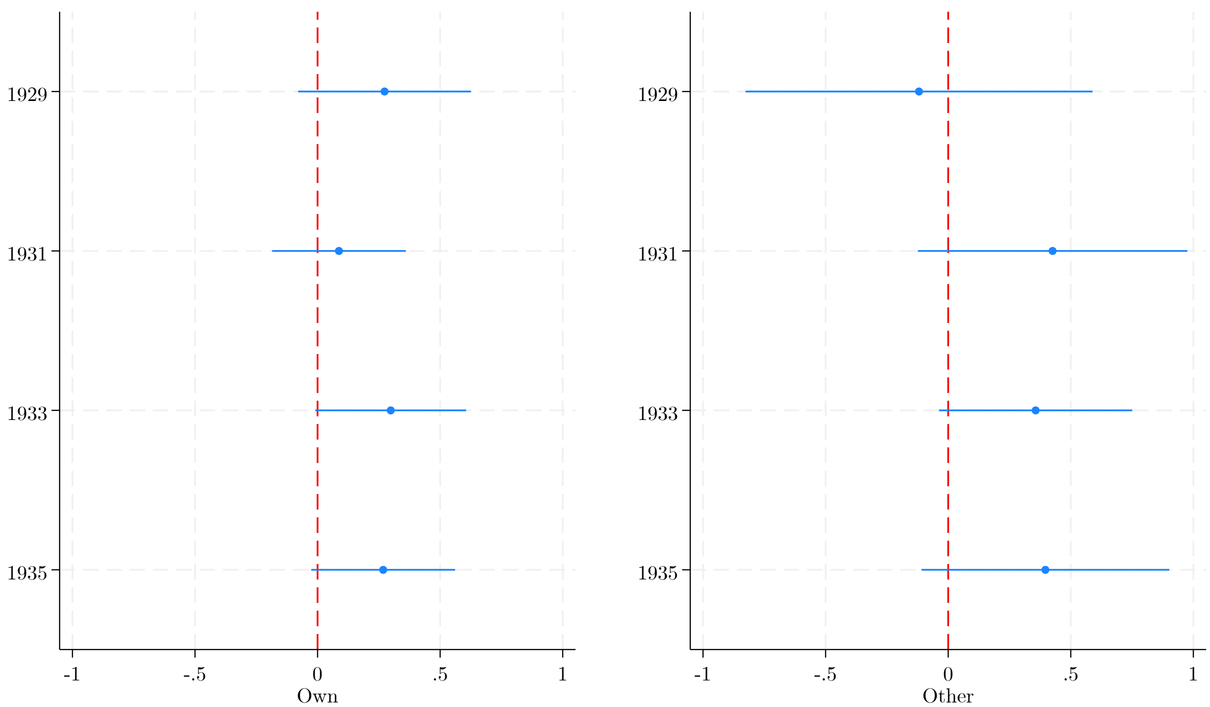
Notes: The relative frequency is the number of MP establishments to non-MP establishments in 1929 scaled by the aggregate ratio of MP to non-MP establishments. So a value of 1 means that the ratio in a given district is equal to the national ratio. The size of the dots represents the number of observations in that Federal Reserve district.

Figure 9: Effects of Trimming the Tails of the Employment Distribution



Notes: The percentiles of the employment distribution are calculated by year and industry. These data are at a quarterly frequency. The retail index is defined at the Federal Reserve district level. The variable MP is an indicator for whether an establishment is part of a multi-plant firm. The base specification includes Federal Reserve district seasonal trends and industry-specific seasonal trends, as well as Fed district, industry, and calendar quarter fixed effects. Sensitive industries are those industries with a statistically significant correlation with the own effect at the 5% level in the base specification. The "Fully Saturated" model includes fixed effects for all possible interactions between Federal Reserve region, quarter, year, and industry. Including all of these fixed effects absorbs the own effect. Standard errors are clustered at the firm level.

Figure 10: Effects of Own and Other Retail Sales by Year



Notes: The year-by-year regressions include Federal Reserve district fixed effects and industry-specific seasonal trends. Standard errors are clustered at the firm level. To construct the other retail sales index, we weight establishments by employment.